

PRINCIPLES AND PROPERTIES OF EXPERIMENT DESIGN

by

W. T. Federer

BU-394-M

November, 1971

Abstract

One aspect of any scientific investigation involving the collection of data is the design of the investigation. One part of this aspect relates to the selection of the experiment (experimental) design for the investigation, where the experiment design is defined to be the arrangement of the entities (treatments) being investigated in the experimental area or space. The principles involved in experiment design and the properties associated with experiment designs are studied in the present paper. The principles of replication, randomization, blocking, confounding, orthogonality, balancing, sensitivity, and augmentation are defined and discussed together with some resulting properties. A diagram of relationships among principles and some related properties is presented.

The presentation is mostly nonmathematical, but there is a pressing need for a rigorous and precise formulation of all principles and properties in mathematical form. The formulation, the equivalences, and the consequences of all definitions, need considerable study and precise formulation. Only in this manner is it possible to completely understand all the ramifications of a formulation and its relation to other topics. This study represents a first step in this direction. Several specific studies are underway.

PRINCIPLES AND PROPERTIES OF EXPERIMENT DESIGN

by

W. T. Federer

BU-394-M

November, 1971

1. INTRODUCTION

Sir Ronald A. Fisher laid down the principles of experiment (experimental) design as we know them today. Although they were indicated in his 1926 paper on "The Arrangement of Field Experiments," they were not completely enunciated until the appearance of his "Design of Experiments" book in 1935. Despite the importance and greatness of this contribution, little work, relative to the needs, has been done on principles and properties of experiment design. R. C. Bose and co-workers [e.g., 1939, 1947, 1952] have been the main exceptions although there are others (see references). It would appear that a concentrated research effort on the principles and properties of experiment design is needed to fully understand equivalences, alternatives, and ramifications of currently available literature on this subject.

Over the past 30 years, it would appear that many researchers first constructed designs and then determined the properties associated with their experiment design. Also, present textbooks on experiment design give little or no attention and/or space to this important topic. A realistic introduction to a textbook on experiment design would include considerable material and discussion on the principles of designing experiments and on the properties of the various experiment designs that may be constructed and used. Then, using these principles and properties, classes of experiment design with the specified characteristics would be constructed.

It should be noted that considerable research on estimators of parameters and on tests of significance have proceeded in this manner. That is, an estimator

or a test of significance was obtained and then the investigator searched until he found some property associated with the technique. Alternatively, an investigator would develop the technique and its properties simultaneously. These methods of approach are to be expected when a subject is developing, but they become less and less frequent as the subject becomes more developed as Statistics is today. Given this situation, we should see more emphasis in the future on principles and properties than previously. Since this is believed to be the situation, it is necessary to thoroughly investigate principles and properties and to determine what additional ones are necessary, which ones should be modified, and the interrelations among them. This paper, then, represents a start in this direction.

The principles of experiment design as expounded by Fisher [1935] and Yates [1933] are listed below:

replication

randomization

blocking

confounding

orthogonality (perhaps a condition rather than a principle)

balancing (" ")

sensitivity (" " property " " " ")

An additional principle that may be added to the above seven is augmentation. Efficiency could also be added but it is considered to be a property rather than a principle. Also, specific experiment designs may have the orthogonal property even though the principle (condition) of orthogonality was utilized in constructing the design. This illustrates the dual role of some of the above listed principles. A similar type of argument also holds in estimation. For example,

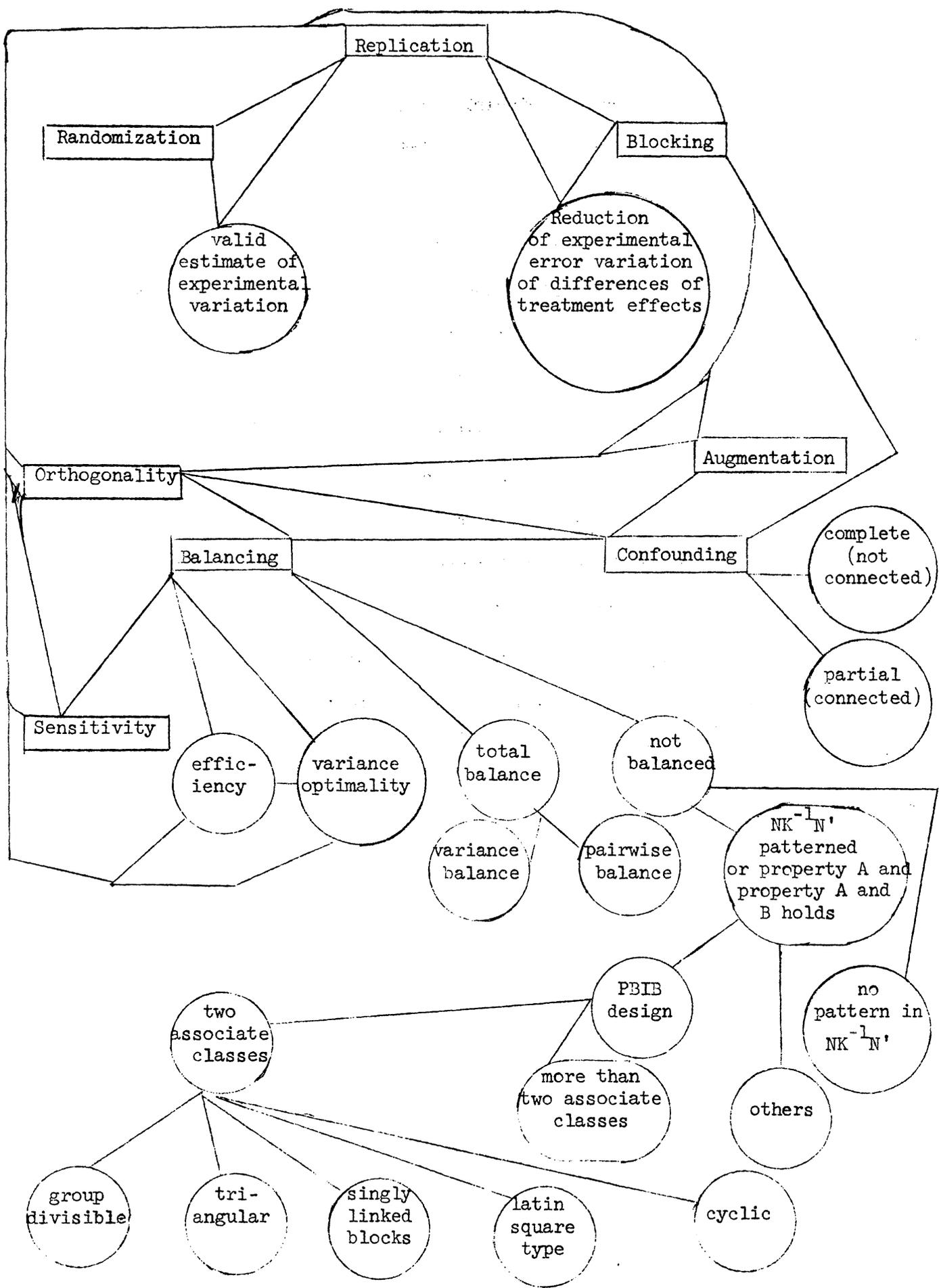
the maximum likelihood estimation principle is used to obtain estimators that have the maximum likelihood properties of efficiency, sufficiency, consistency, etc.

The part of Figure 1 above the dotted line is reported to have hung on the wall of Sir Ronald A. Fisher's office when he was at the Rothamsted Experimental Station. The two items circled in this part of Figure 1 may more properly be considered as consequences rather than as properties.

The relationship among the above eight principles and several of the associated properties are given in Figure 1. The principles are listed in the rectangles and the related properties are given in circles. The relationships are indicated by lines between the rectangles and circles. Some properties have been omitted; these may be added to Figure 1 by the reader as he sees fit.

A discussion of each of the principles of experiment design and some of the resulting properties of designs obtained via the principles is given in the following sections. An attempt is made to obtain deeper insight into the various principles.

Figure 1. Relationships among principles (in rectangles) and properties (in circles) of experiment design.



2. THE REPLICATION PRINCIPLE

Replication is defined to be the repetition of a treatment or level of a factor on other experimental units where an experimental unit is the smallest entity to which one treatment is applied. Replication should not be confused with duplicate, triplicate, etc. observations on the same experimental unit as for example, readings, weighings, etc. Likewise, if the experimental unit consists of a number of sampling or observational units (e.g., a group of animals comprising the experimental unit such that the group receives one treatment), the individuals do not constitute replication of a treatment but merely multiple observations of a response.

Fisher [1935] states that the principle of replication has two distinct consequences or purposes. These purposes are achieved jointly with the randomization principle described in the next section. The first, and perhaps more important, purpose of replication is to supply an estimate of the error variance of treatment effects or of contrasts of treatment effects. If one considers replication in its most general sense (e.g., in a linear regression problem with r observations the intercept and slope effects are replicated r times), there is, as Fisher [1935] states, no other method for doing this. As stated previously, it is necessary that randomization be utilized to obtain a valid estimate of the error variation.

The second purpose of replication is to reduce the error variance of a treatment effect or mean, say σ^2/r , which decreases inversely as the number of replicates is increased, where the number of replicates refers to the number of experimental units to which one treatment is applied. (In some cases such as the randomized complete block design with each treatment included once in each block, one replicate for the set of v treatments is equivalent to one complete block; in this case the words replicate and block are sometimes used interchangeably).

This decrease takes place in the manner described provided that the error variations in each block comes from a common population with parameter σ^2 , i.e., homoscedasticity holds.

As may be noted from Figure 1 replication is related to several other principles and properties. Replicated experiments, when properly randomized, have the property of yielding a valid estimate of the error variation, are more sensitive, and are more efficient than nonreplicated experiments. Experiment designs for which all v treatments have the same number of replicates are called equi-replicate designs.

3. THE RANDOMIZATION PRINCIPLE

As Fisher [1935] states, the process of randomization, which is the allocation of the treatments to the experimental units according to the laws of chance restricted only by the constraints of the experiment design, forms the physical basis for the validity of all statistical procedures including tests of significance and interval estimation. The two main consequences of utilizing the randomization principle in experimentation are to provide:

- (i) unbiased estimates of means, treatment effects, or contrasts among treatments,
- (ii) an unbiased estimate(s) of the error variance(s) associated with the estimated contrasts.

After selecting the experimental units for the experiment, there will be variation among them. In order to assess treatment differences uncontaminated with additional effects or sources of controllable variation, it is essential that whatever residual variation that remains, should affect treatment responses according to the laws of chance. Then, and only then, will the use of statistical procedures be valid for making inferences.

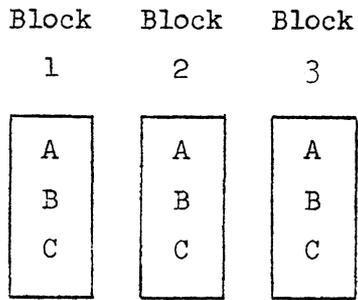
4. THE BLOCKING PRINCIPLE

The Fisherian principle of experiment design known as blocking, local control, grouping, or stratification may be defined as a subdivision of the total experimental material into subsets of the experimental material such that the subsets contain all of the experimental material. An attempt is made to block in such a manner that the variation among units within a block is smaller than the variation among units in different blocks. Optimum blocking is achieved when the variation among units within blocks is minimal, and conversely, the variation among block means will be maximal.

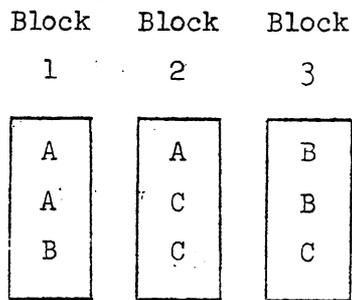
To be more specific, suppose that there are $N = \sum_{i=1}^v r_i$ experimental units to which v different treatments are to be applied with each treatment being allocated to r_i experimental units. (An experimental unit is the smallest unit of material to which one treatment is applied.) If the treatment, (for $i = 1, 2, \dots, v$) is randomly allocated to r_i of the $N = \sum r_i$ units without replacement, then there are $N! / \prod_{i=1}^v (r_i)!$ possible ways of assigning the v treatments to the N experimental units. Any restriction on the allocation of the treatments to the N experimental units results in a restriction of the total number of different permutations possible and thus represents a form of blocking.

We shall now illustrate one aspect of the principle of blocking in several experiment designs. First consider the following examples $v = 3$ and 4 treatments in blocks of size $k = 3$ and 4 :

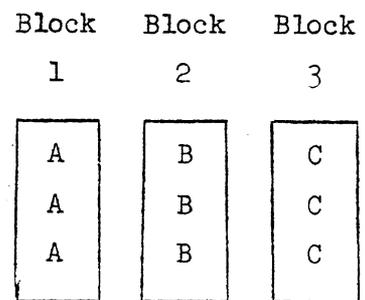
Example 4-1:



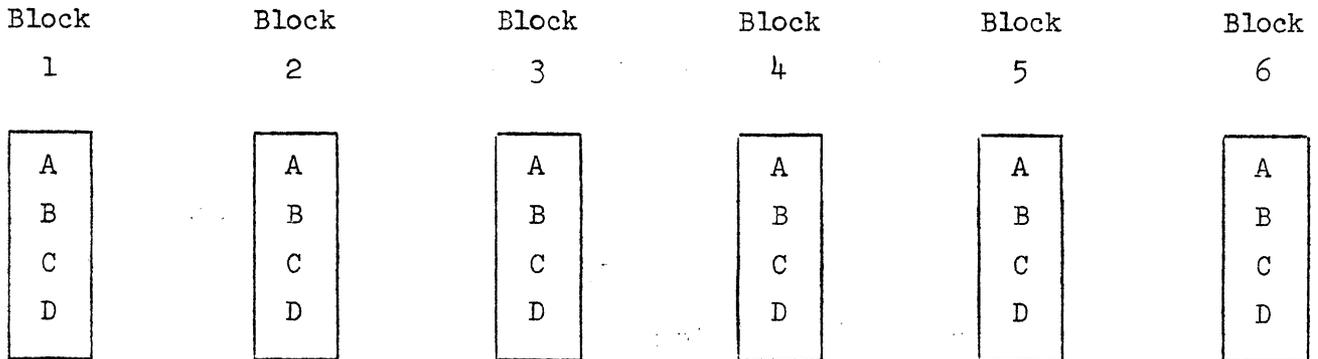
Example 4-2:



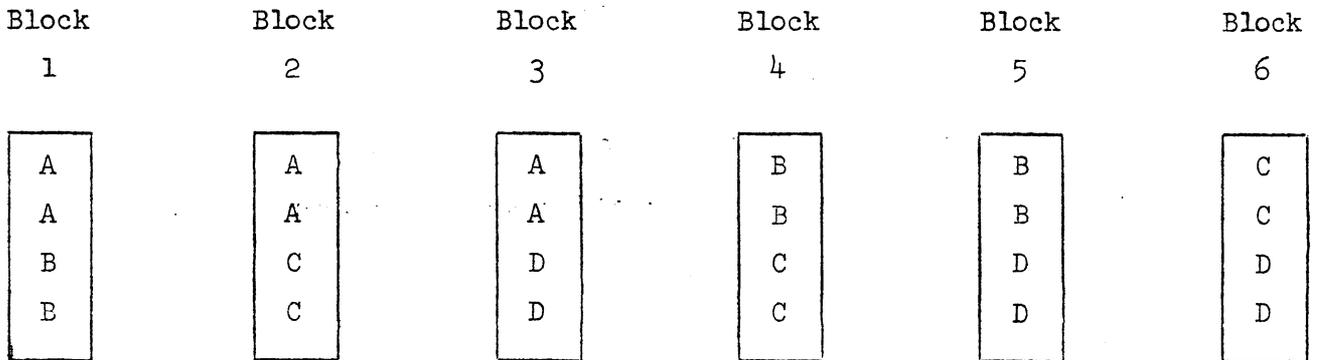
Example 4-3:



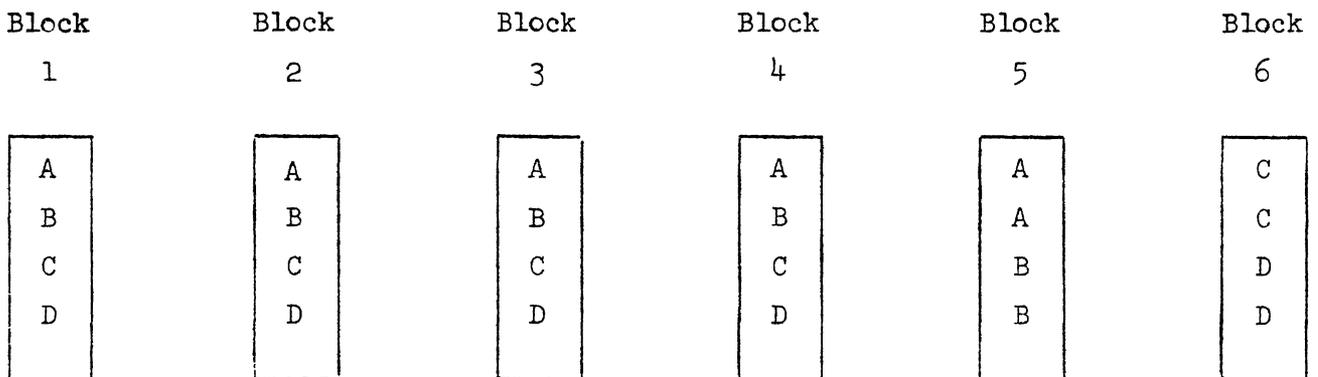
Example 4-4:



Example 4-5:



Example 4-6:



If there were nine experimental units the allocation of treatments in examples 4-1, 4-2, and 4-3 results in identical blocking and in identical removal of heterogeneity from the experimental material. If the three treatments A, B, and C are randomly allocated the three experimental units in each of the blocks in example 4-1 the resulting design is a randomized complete blocks design. If the sets of treatments are randomly allocated to the blocks and if the treatments within each block are randomly allocated to the experimental units within the block in example 4-2, the resulting design is a ternary balanced design (see Tocher [1952]). In example 4-3, there is a complete entanglement (confounding) of the treatment effects and the block effects.

Likewise, for the plans given in examples 4-4, 4-5, and 4-6, the blocking is identical but associations among the four letters A, B, C, and D are different for the three examples. With randomization of letters to experimental units within each block and randomization of blocks to groups of experimental material, example 4-4 is a randomized complete block design, example 4-5 is balanced incomplete block design, and example 4-6 is a partially balanced incomplete block design of the group divisible type. This last design, example 4-6, is more efficient (smaller variances of differences of effects) than the balanced incomplete block design in example 4-5.

As another example of one-way elimination of heterogeneity and blocking consider that there is a linear gradient running through the experimental material, and that there are four treatments. Since the gradient is linear and if the experimental units are equally spaced, the coefficients for the linear term are -3, -1, 1, and 3. Then if the sequence of treatments ABCD is obtained, the contrast $A + D - B - C$ is uncorrelated, the contrast $A + C - B - D$ is partly correlated, and the contrast $A + B - C - D$ is completely correlated with the linear regression coefficient. Thus by restricting the randomization a specified contrast(s) may be estimated independently of the linear gradient in the experimental material.

Another method of blocking on a linear gradient is to form subgroups or blocks and orders within blocks to obtain a design with two-way elimination of heterogeneity as follows:

Block 1				Block 2				Block 3				Block 4			
order															
1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4
A	B	C	D	B	C	D	A	C	D	A	B	D	A	B	C

Rearranged the above may be pictured as a form of a latin square arrangement:

Blocks (columns)				
orders within blocks (rows)	1	2	3	4
1	A	B	C	D
2	B	C	D	A
3	C	D	A	B
4	D	A	B	C

The two-way blocking is on blocks (columns) and orders within blocks (rows).

Other examples of two-way elimination of heterogeneity arrangements are:

Example 4-7

v = 2 treatments in
4 rows and 4 columns

	column			
row	1	2	3	4
1	A	B	A	B
2	B	B	A	A
3	A	A	B	B
4	B	A	B	A

Example 4-8

v = 2 treatments in
2 rows and 6 columns

	column					
row	1	2	3	4	5	6
1	A	A	A	B	B	B
2	B	B	B	A	A	A

Example 4-9

v = 3 treatments in
4 rows and 4 columns

	column			
row	1	2	3	4
1	A	B	C	A
2	B	C	A	A
3	C	A	A	B
4	A	A	B	C

Three-way elimination of heterogeneity designs represent blocking on three sources of variation. The magic latin square and the latin cube are two representatives of this class of designs. For v = 3 in the latin cube and v = 4 in the magic latin square, the arrangements are:

Example 4-10 (latin cube, v = 3)

first tier

	column		
row	1	2	3
1	A	B	C
2	B	C	A
3	C	A	B

second tier

	column		
row	1	2	3
1	B	C	A
2	C	A	B
3	A	B	C

third tier

	column		
row	1	2	3
1	C	A	B
2	A	B	C
3	B	C	A

Example 4-11

magic latin square (v = 4)

	column			
row	1	2	3	4
	<u>square 1</u>		<u>square 2</u>	
1	A	B	C	D
2	C	D	A	B
	<u>square 3</u>		<u>square 4</u>	
3	B	A	D	C
4	D	C	B	A

Thus from the above examples, it is apparent that blocking can be preformed to remove or to control n sources of heterogeneity. It is advisable to use minimum blocking to remove the heterogeneity from experimental material. This is important in designs utilizing randomization and statistical estimation procedures in that the smaller the number of degrees of freedom for blocking the larger will be the number of degrees of freedom associated with the error variance in the design. It is possible to utilize one form of blocking to control two or more sources of variation under some circumstances. This form of blocking should be utilized whenever possible, provided that these sources of variation do not interact with the treatments in the experiment. When this is the case, these sources may need to be considered as additional treatment design factors.

Blocking may be performed in a random manner resulting in equality of variation within blocks and between blocks. Or, it may be performed to minimize variation within blocks and to maximize variation between blocks. Under the latter situation designs blocked in this manner will be efficient and have minimum variance among all other designs.

5. THE CONFOUNDING PRINCIPLE

Confounding is defined to be the partial or complete mixing up of subsets of one set of factors with a second set of factors. From the estimation of parameters point of view, there is a partial or complete correlation of estimates of subsets or set effects from the two factors being considered. From a combinatorial point of view, say for a prime-powered factorial treatment design in blocked experiment, some points of the Projective Geometry for the treatment design may coincide exactly with the points occupied by blocks. That is, for some subsets of the two factors the possible combinations are limited to the

cases where the subsets coincide. For example, consider a 2^3 factorial in blocks of size four in the following two arrangements: (the notation is of the standard form used by Federer [1955], Kempthorne [1952], e.g.):

Arrangement I					Arrangement II			
block					block			
1	2	3	4		1	2	3	4
000	111	000	111		000	111	000	100
011	100	011	100		011	100	110	010
101	010	101	010		101	010	001	101
110	001	110	001		110	001	111	011
(ABC) ₀	(ABC) ₁	(ABC) ₀	(ABC) ₁		(ABC) ₀	(ABC) ₁	(AB) ₀	(AB) ₁

In the first arrangement, the contrast of blocks 1 + 3 versus 2 + 4 is identical to the ABC contrast which is $(ABC)_1$ versus $(ABC)_0$. Therefore, the ABC effect from the 2^3 treatment set is completely mixed up (completely confounded) with the contrast among blocks involving blocks 1 + 3 versus blocks 2 + 4. In arrangement II, treatment effect ABC can be estimated from blocks 3 and 4 and treatment effect AB can be estimated from blocks 1 and 2. Since not all blocks can be used to obtain estimates of effects ABC and AB free of blocks, these two effects are partially mixed up (partially confounded) with blocks effects. The remaining five treatment effects A, B, C, AC, and BC are completely free of block effects, and hence are unconfounded with block effects.

The confounding principle may be utilized in many ways for designing experiments. In undesigned experiments, confounding is of common occurrence; even in designed experiments involving humans and animals, it is of frequent occurrence. As may be noted from Figure 1, confounding is related to several of the other principles and properties of experiment design.

6. THE ORTHOGONALITY PRINCIPLE

Orthogonality is usually defined in terms of the sets of parameters of two or more factors or variables. One definition is the following. If the yield of an observation equals a treatment effect τ_i plus a random error component ϵ_{ij} plus stratification effects ρ_j plus an overall mean μ and if the difference between the arithmetic means of any two treatments in a blocked experiment contains only differences between treatment parameters, plus some random error components, then the treatment effects are said to be orthogonal to the mean and to the stratification effects ρ_j in the experiment. This means that

$$\bar{y}_{i..} - \bar{y}_{i'..} = \tau_i - \tau_{i'} + f(\epsilon_{ij}),$$

where $f(\epsilon_{ij})$ is some function of the residual variations. If there are other parameters on the right hand side of the above equation, then treatment effects would not be orthogonal to those effects appearing on the right hand side of the equation.

Likewise, if N is the design matrix between two factors, and if the matrix NN' can be written in the form $NN' - K = DJ^*$ where K is a matrix such that any row of K is a multiple of any other row of K , D is a diagonal matrix with the number of replicates on the treatments in the diagonals, and J^* is the matrix having the number of occurrences of each treatment in every row, then the two factors are said to be orthogonal to one another. To illustrate suppose that one has 3 treatments (A,B,C) in a blocked design of the form:

block 1	block 2	block 3	block 4
A	A	A	A
A	A	A	A
B	B	B	B
C	C	C	C

The design matrix N is

treatments	blocks				total
	1	2	3	4	
A	2	2	2	2	8
B	1	1	1	1	4
C	1	1	1	1	4

$$\text{Then } NN' = \begin{pmatrix} 16 & 8 & 8 \\ 8 & 4 & 4 \\ 8 & 4 & 4 \end{pmatrix} = \begin{pmatrix} 8 & 0 & 0 \\ 0 & 4 & 0 \\ 0 & 0 & 4 \end{pmatrix} \begin{pmatrix} 2 & 1 & 1 \\ 2 & 1 & 1 \\ 2 & 1 & 1 \end{pmatrix} = \begin{pmatrix} 8 & 0 & 0 \\ 0 & 4 & 0 \\ 0 & 0 & 4 \end{pmatrix} \begin{pmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{pmatrix} \begin{pmatrix} 2 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{pmatrix}$$

and any row is a multiple of any other row of the matrix.

From a combinatorial viewpoint, if each of the levels of one factor (say treatments) appears λ_i times at all levels of the second factor (say blocks), then the two sets of factors are said to be orthogonal to each other. In the above, $\lambda_A = 2$, $\lambda_B = 1$, and $\lambda_C = 1$. The ratio $\lambda_A : \lambda_B : \lambda_C$ stays constant in every block in orthogonal designs.

Some properties that result from the use of the orthogonality principle (or condition) is that orthogonal designs have maximal efficiency and ease of analysis. They represent the base for the relative efficiencies of other designs.

7. THE BALANCING PRINCIPLE

As with orthogonality some statisticians may consider balancing to be a condition rather than a principle as is done here. Despite this, the balancing principle may be utilized in many ways to achieve experiment designs with properties such as ease of statistical analysis and/or optimality with respect to variance. In the following we shall consider balance from several viewpoints. There are several criteria for balance and these have been used in various ways in experimentation and in statistical theory.

One concept of balance, which we may denote as pairwise-balance, is the following. If each and every pair of v treatments occur together in the b blocks of size k an equal number of times, say λ , and each treatment appears in r of the b blocks, then the arrangement is said to be balanced. This is a combinatorial concept of balance.

A second concept for defining balance is that if N is the design matrix for blocks and treatments and if $NN' + c_1J$ or $NN' + c_2J^*$ is a diagonal matrix, where J is a matrix with all elements equal to one, c_1 and c_2 are scalars, and J^* is as defined in the preceding section, then the design is balanced. This concept has not been widely used by statisticians. We could call this diagonal balance.

A third way of defining balance is to say that if the variance of a difference of effects for every pair of treatments is equal, then the design is balanced. A more suitable terminology would be variance balance.

A fourth way of defining balance is to say that if $NN' = C_1I + C_2J$, where I is the identity matrix and the other symbols are defined above, then the design is balanced. This is a matrix formulation of balance.

Not all of these definitions are equivalent. For example, consider the following balanced incomplete block plan for $v = 7$ treatments in $b = 7$ blocks of size $k = 3$ each with $r = 3$ and $\lambda = 1$.

blocks

1	2	3	4	5	6	7
1	2	3	4	5	6	7
2	3	4	5	6	7	1
4	5	6	7	1	2	3

If treatment 7 (or any other treatment or any two treatments) is deleted, the resulting plan is still pairwise balanced but not variance balanced. Designs can be constructed where the reverse is true. Also, the plan in example 4-5 satisfies all four concepts above even though it is not considered to be one of the classical balanced incomplete block plans, since treatments occur twice or zero time in a block instead of once or zero time in a block.

A fifth concept of balancing appears in double-reversal or double change-over designs wherein treatments are applied in sequence and each treatment follows and is followed by every other treatment an equal number of times in a plan balanced for residual effects.

These many and varied uses of the balancing principle (condition) in experiment design illustrate its importance. Experiment designs can be constructed to be balanced in one of the senses defined above. These designs have various properties. For example, in the classical balanced incomplete block design wherein any treatment occurs only once or zero time in a block and in $r < b$ of the incomplete blocks of size k and with every other treatment a constant, λ , number of times, these designs are simple to analyze, all treatments contrasts involving two treatments have equal variances, and this design is the most efficient design among all non-orthogonal designs. (Homoscedasticity is assumed here.)

Experiment designs with the balanced incomplete block design property are those which satisfy either or both of the pairwise-balance and variance balance concepts above. Many published papers relate to various methods of constructing balanced incomplete block designs and upon existence of certain designs. A concentrated attack upon the entire balancing concept has not been made in the literature to date.

8. THE SENSITIVITY PRINCIPLE

It would appear that Fisher [1935], in his first chapter, considers sensitivity to be a principle rather than a property or condition. He states that one experiment is more sensitive than a second if it allows detection of a smaller difference; also, he talks of the sensitivity of a single experiment. In comparing the sensitivity of two procedures, use of standard units, e.g., (difference of two treatment means = $\bar{y}_1 - \bar{y}_2$) / (standard error of the difference of two means = $s \sqrt{\frac{1}{n_1} + \frac{1}{n_2}}$), is desirable; this is especially true when different characteristics in the same experiment are being compared.

To illustrate the sensitivity principle in experimentation, consider the lady-tasting-tea example given in Chapter II of Fisher [1935]. The first procedure involves the presentation of eight cups of tea in a random order to the lady for tasting with the single item of information that four of the eight cups had the milk infusion added first and the remaining four had the tea infusion added first. By chance alone, the lady could correctly identify all cups correctly once in 70 trials on the average. Now, if the second procedure consists of presenting the eight cups of tea in a random order and if the infusion of milk or tea first to each cup is determined by a chance mechanism that allows equal chances for either tea or milk to be added first, by chance alone the lady could identify all cups of tea correctly only once in 256 trials on the average. Thus both procedures involve the same amount of experimental material (and replication) but the sensitivity of the second procedure relative to the first is 256/70, or about 3.7 times.

As a second illustration, different amounts of nitrogen fertilizer of zero, 150 lbs., and 300 lbs. per acre were added to field plots of sugar cane. If one computed lsd (least significant) units equal to $(\bar{y}_2 - \bar{y}_1) / t_{.05} s \sqrt{\frac{1}{n_2} + \frac{1}{n_1}}$ where

\bar{y}_1 . is the mean amount of nitrogen in the leaves for plants with no fertilizer added, \bar{y}_2 . is the mean amount of nitrogen in the leaves of plants where 150 lbs./A of nitrogen fertilizer were added, $s_{\bar{y}_2 - \bar{y}_1}$ is the standard error of the difference of two means with f degrees of freedom, and $t_{.05, f}$ is the tabulated value of the t -statistic at the .05 level for f degrees of freedom, then it was found that there was one lsd unit between the means of the zero and 150 lbs. and one between the 150 lbs. and 300 lbs. treatments. If, on the other hand, this same procedure was used for the amount of nitrogen in the part of the stalk 8 to 10 nodes from the top, there were six lsd units between the successive treatment means. Hence, for purposes of assessing the amount of nitrogen in plants as affected by amount added to the plots, measurements in the leaves are only 1/6 as sensitive as measurements in the stalk area 8 to 10 nodes below the top of the plant. Thus, a measurement of stalk nitrogen was six times more sensitive than a measurement of leaf nitrogen.

Sometimes a measuring device is too variable; a reduction of variability in measurements increases the sensitivity of experiments. An example might be rounding to the nearest inch when differences should have been measured in mm. Also, blocking of experimental material is often effective in making experiments more sensitive. For example, in swine nutrition trials, it was found that blocking on litter size halved the experimental error variance. Similarly, control of variation in initial weights of cattle by covariance, as these weights affected final weights, also halved the experimental error variance.

9. THE AUGMENTATION PRINCIPLE

The augmentation principle involves the addition of new treatments to an experiment either in the present stage or in succeeding stages. For example,

additional treatments may be included with a set of v treatments arranged in a standard blocked design wherein the blocks are enlarged to include the new or additional treatments (see e.g., Federer [1956]). This form of augmentation has many ramifications in experiment design. Another form of augmentation is related to the sequential selection of treatments wherein the results for the original set at the i^{th} stage are used to determine the augmented or additional treatments at the $(i+1)^{\text{st}}$ stage. This principle has been utilized in various ways in experimentation but has not been mathematically formalized.

10. THE EFFICIENCY PROPERTY

Fisher [1935] has defined the amount of information on a contrast to be the reciprocal of the variance of the contrast and the efficiency of a design and/or a procedure is the ratio of the amount of information solicited to the amount available in an investigation. Thus, one experiment design is said to be more efficient than a second design if the ratio of the two respective amounts of information is greater than unity, i.e., $1/s_1^2 / 1/s_2^2 = s_2^2/s_1^2 > 1$. A design is said to be amount-of-information optimal if the amount of information is greater than or equal to that for all other possible designs. Thus, orthogonal designs are more efficient than non-orthogonal designs when all possible contrasts are considered.

The proportional loss in information due to estimating a variance is $2/(f+3)$; the proportional amount of information in the estimated variance with f degrees of freedom relative to the parameter is $1 - 2/(f+3) = (f+1)/(f+3)$. Hence, if the degrees of freedom in the estimated error variances for two designs are different, then this needs to be taken into account in computing the relative efficiencies of two designs. For

example, the amount of information for design one would be $(f_1+1)/s_1^2(f_1+3)$ and the amount of information on design two is $(f_2+1)/s_2^2(f_2+3)$; then the relative efficiency of design one to design two is:

$$\begin{aligned} & \frac{(f_1+1)/s_1^2(f_1+3)}{(f_2+1)/s_2^2(f_2+3)} \\ &= \left(\frac{f_1+1}{f_1+3} \right) \left(\frac{f_2+3}{f_2+1} \right) \frac{s_2^2}{s_1^2}, \end{aligned}$$

where s_1^2 is the error variance of a contrast with f_1 degrees of freedom from design (procedure) one and s_2^2 is the error variance of the same contrast with f_2 degrees of freedom from design (procedure) two. Whenever f_1 and f_2 exceed 20 the term $(f_1+1)(f_2+3)/(f_1+3)(f_2+1)$ is close to unity and hence can be neglected; also when $f_1 = f_2$ this term equals unity.

11. SOME COMMENTS

The preceding has been presented with the idea that it would motivate individuals to work on various aspects of principles, conditions, and properties of experiment design that are unresolved. Two such studies are underway. A. Hedayat, Cornell University, is in the process of writing up an investigation of the various aspects of balancing. A second study on orthogonality has been completed by Eccleston [1971]. Tocher [1952] and Pearce [1963, 1968, 1970] have reported results on the efficiency property. Kempthorne [1952] and co-workers have investigated many aspects of the randomization principle. Hedayat [1971] has investigated the connectedness property and has extended the concept from a local to a global one. Also, a bibliography on properties of designs has been prepared by Federer [1970]. Other studies of this nature need to be made. At

some point it will be necessary to perform a concentrated study of all principles and properties of experiment design simultaneously. After a complete study of their interrelationships, it should be possible to ascertain which new principles and properties need to be established and which ones need extension.

12. REFERENCES

1. Bose, R. C. [1947]. Presidential Address. Proc. 34th Indian Sci. Congress, p. 12.
2. Bose, R. C. and Nair, K. R. [1939]. Partially balanced incomplete block designs. Sankhyā 4:337-372.
3. Bose, R. C. and Shimamoto, T. [1952]. Classification and analysis of partially balanced incomplete block designs with two associate classes. J. American Stat. Assoc. 47:151-184.
4. Eccleston, J. A. [1971]. On characterization of orthogonal designs. Biometrics Unit Mimeo Series No. BU-396-M, Cornell University, October.
5. Federer, W. T. [1955]. Experimental Design. Macmillan, New York.
6. Federer, W. T. [1956]. Augmented (or hoonuiaku) designs. Hawaiian Planters' Record 55:191-208.
7. Federer, W. T. [1970]. A bibliography on properties of experimental design, 1950-1967. Biometrics Unit Mimeo Series No. BU-346-M, Cornell University, November.
8. Fisher, R. A. [1926]. The arrangement of field experiments. J. Ministry Agri. 33:505-513.
9. Fisher, R. A. [1935]. The Design of Experiments (7th edition in 1966). Oliver and Boyd Ltd., Edinburgh.
10. Hedayat, A. [1971]. A set of conditions which every locally connected design must satisfy. Biometrics Unit Mimeo Series No. BU-392-M, Cornell University, October.
11. Jones, J. M. [1959]. On a property of incomplete blocks. J. Royal Stat. Soc., Series B, 21:172-179.
12. Kempthorne, O. [1952]. The Design and Analysis of Experiments. John Wiley and Sons, Inc., New York.

13. Pearce, S. C. [1963]. The use and classification of non-orthogonal designs (with discussion). J. Royal Stat. Soc., Series A, 126:353-377.
14. Pearce, S. C. [1968]. The mean efficiency of equi-replicate designs. Biometrika 55 (2): 251-253.
15. Pearce, S. C. [1970]. The efficiency of block designs in general. Biometrika 57 (2):339-346.
16. Tocher, D. [1952]. The design and analysis of block experiments. J. Royal Stat. Soc., Series B, 14:45-100.
17. Yates, F. [1933]. The principles of orthogonality and confounding in replicated experiments. J. Agri. Sci. 23:108-145.
18. Yates, F. [1937]. The design and analysis of factorial experiments. Imperial Bureau Soil Sci., Tech. Comm. 35:1-95.