NATIONAL BUREAU OF STANDARDS REPORT

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Draft of
Part III, Sections 1,2
for

MANUAL ON EXPERIMENTAL STATISTICS
FOR ORDNANCE ENGINEERS

A Report to

OFFICE OF ORDNANCE RESEARCH
DEPARTMENT OF THE ARMY

NBS

U. S. DEPARTMENT OF COMMERCE
NATIONAL BUREAU OF STANDARDS

THE NATIONAL BUREAU OF STANDARDS

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MANUAL ON EXPERIMENTAL STATISTICS
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Prepared by

Statistical Engineering Laboratory

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NOTICE

This report is a preliminary draft of Part III, sections 1 and 2 for the Manual on Experimental Statistics for Ordnance Engineers.

At the time of printing, it has been noted that certain portions of the text should be revised in the final draft. No known inaccuracies exist in the present draft, but improvements in arrangement and exposition of some of the material may be made at a later time.

Table of Contents

III THE PLANNING AND ANALYSIS OF COMPARATIVE EXPERIMENTS (THE DESIGN OF EXPERIMENTS)

1. General Considerations

In comparing several treatments, processes, etc., with regard to some characteristic, the procedures used may be relatively unimportant when the differences are sufficiently large. When variation, and, or experimental error approach the order of magnitude of the differences, then procedures become important, if very large experiments are to be avoided. The experimental techniques must be refined, and, or the overall experimentation carefully planned so as to minimize the effect of variability. It is when the differences are of approximately the same order of magnitude as the variations that it is most important that the experiment be well planned.

There are certain principles of scientific experimentation which should always be followed. Although the principles are not necessarily statistical, they are important considerations in a well planned experiment. Many books have been written on the topic and the books by Wilson [1] and Churchman [2] are recommended for reading. We shall content ourselves here with a statement of some of the principles.

i) There must be a clearly defined objective. It is often advisable to write up a dummy report before the experiment has been run. In this way one can check to see if his experiment will really allow him to attain his stated objective.

ii) The experiment must be free from conscious or unconscious bias on the part of the experimenter. This is accomplished mainly by randomization.

Suppose we wish to test for differences in "muzzle velocity" of ammunition stored under different conditions. Suppose we have two testing devices. Even though we had reason to believe the testing devices were equally good, rather than fire one set of shells in one device, and the other set in the second device, a more reasonable procedure would be to allocate each shell randomly to one or the other devices (limiting one half of each set to a given device). Unsuspected systematic differences or biases may always occur, and for this reason it is wise to allocate "treatments", "methods", etc., using the randomization technique. "Randomization is somewhat analogous to insurance in that it is a precaution against disturbances that may or may not occur, and that may or may not be serious if they do occur. It is generally advisable to take the trouble to randomize even when it is not expected that there will be any serious bias from failure to randomize". */

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*/ Cochran and Cox [3, page 8].

iii) There must be a measure of variation or of experimental error. Two methods or treatments may appear to differ, when in reality the observed difference has occurred by chance. In order to decide whether there is a real difference or not, we must have a measure of the amount of variation we could expect from chance alone. Proper randomization assures such a measure. Indeed tests of significance and confidence limits can be constructed using only the fact that proper randomization has been applied in the experiment.

iv) The experiment should have sufficient precision to accomplish its purpose. Two methods are available for increasing the precision of an experiment. One is to increase the sample size. The other is by eliminating or reducing technical and other errors. Errors can be reduced by improvement of techniques, and especially when the number of repetitions is large by blocking techniques. If we have some knowledge as to the amount of error (e.g., from previous or similar experiences) then we have some idea of the size necessary for our experiment.

v) The experiment should have sufficient scope. If we are interested in several factors, then the experiment should be large enough to include all of the factors. Experimenting with one factor at a time is costly and frequently misleading.

Another way in which our experiment's scope may be limited is by studying too small a range of the variable (or variables) under consideration.

Blocking

When the experimental material is not homogeneous, or when the conditions are variable throughout the entire experiment, then blocking is a technique that can be used to improve the precision. It consists essentially of dividing the experiment up into smaller chunks (blocks), so that in each block, the conditions, experimental material, etc., are more homogeneous.

Suppose we are required to compare the effect of five different treatments of a plastic material. Plastic qualities vary considerably from batch to batch and even in a given sheet. Thus, to get a good comparison of the five treatment effects, we should cut the plastic sheet into more or less homogeneous areas, and subdivide each area into five parts. The five treatments could then be allocated to the five parts of a given area. Each set of five parts may be termed a block. In this case, had we had four or six

treatments, we could as well have had blocks of four or six units. This is not always the case.

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

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

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

Blocking is sometimes referred to as restricted randomization in that we do not assign treatments to experimental units completely at random. The choice of an experimental plan to take into consideration size of blocks, allocations of treatments, ease of analysis, etc., is part of a large subject called - "design of experiments", about which many books have been written. In the following sections, we shall attempt to explain the use of some of the more useful and simpler plans.

2. Comparing the Performance of Several Items, Products or Processes ("Treatments")

2.1 Planning

2.1.1. Completely Randomized Plans

This plan is the simplest, and is the best one when the experimental material and conditions during the experiment are homogeneous. If there are N available experimental units, and we wish to assign $n_1, n_2, \ldots, n_t$ $(N = n_1 + n_2 + \ldots + n_t)$ experimental units respectively to each of the t treatments or products, then we proceed to assign the products to the treatments at random. As an example, suppose we have three types of ammunition of a given size and calibre, and we wish to test to see which has the highest velocity. We have n_1, n_2, n_3 shells respectively, of the three types. If the conditions under which the shells are fired are assumed to be the same for each shell, i.e., wind velocities, barrel conditions, etc., then the simplest and best plan is to choose the shells at random and fire them in that order. (It is of course obvious that if we fired all the shells of one type first, and then all the shells of the next type, etc., that we would have absolutely no insurance against possible influences which we had not been anticipating, i.e., if the wearing of the gun-barrel from firing affected the velocity or atmospheric conditions such as wind velocity had changed. Randomization affords insurance against unforeseen disturbances).

The results of a completely randomized plan can be exhibited in a table such as the one below.

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

Figure 2.1.1

2.1.2. Randomized Block Plans

In comparing a number of treatments, it is clearly desirable that all other conditions shall be kept as nearly constant as possible. Frequently the number of repeated tests may be too great to be carried out under similar conditions. In such cases one may be able to divide the experiment in several homogeneous groups, in each of which every treatment is observed exactly once. Such an experimental plan is called a "randomized block design".

There are many situations where a randomized block plan can be profitably utilized. A testing scheme may take several days to complete. If one expects some systematic differences between days, one might plan to "observe" each item on each day, or

conduct one test per day on each item. A day would then represent a block. It may be that several persons will be conducting the tests or making the observations, and differences between persons are expected. The tests or observations made by a given person may be planned so as to represent a block.

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

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

The results of a randomized block experiment can be exhibited in a two-way table such as the one below (assuming we have b blocks and t treatments).

Block	Treatment				Total	Mean
	1	2	...	t		
1					B_1	
2					B_2	
⋮					⋮	
b					B_b	
Total	T_1	T_2	...	T_t	G	
Mean						

Figure 2.1.2

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

2.1.3. Incomplete Randomized Block Plans

In many situations it is not possible to make a test of each "treatment" in a homogeneous block. In these cases, an incomplete block plan may often be used. The blocks are chosen so that in any block, the experimental conditions are reasonably homogeneous. It may be that a number of treatments are to be compared, and not all of the treatments can be observed on the same day. If we expect differences from day to day, then we may regard each day as a block.

Another example of the incomplete blocks might be the following: Five synthetic materials are to be tested for their wearing qualities as soles in shoes. Since the two feet of an individual travel the same distance, carry the same load, etc., conditions for each of the two feet are approximately the same. The natural block in this case consists of the two feet of an individual, and we may therefore compare two kinds of synthetic material on a given individual.

In order that valid and efficient estimates of "treatment" effects may be made, suitable experimental plans must be used and followed rigorously. We shall discuss two kinds of randomized incomplete block plans - balanced incomplete block designs and chain block designs. The former have the

advantage of easy analysis and the important property that all differences between "treatment" effects are estimated with the same precision. The latter class has an advantage when we wish a plan which keeps the number of duplicate observations on treatments down to a minimum. This sort of plan is very useful when the difference in treatments considered worth detecting is larger than the amount of experimental error. Experimental error may be thought of as the difference between an observed treatment and the average of a large number of similar observations under similar conditions.

Other classes of designs are often useful when the above classes do not meet the desires of the experimenter with regard to number of blocks, size of blocks, number of treatments, etc. An important, and very large class of plans is included in the class called the "partially balanced incomplete block designs". Experiments using these plans are slightly more complicated to analyze, and we shall not discuss them.

<u>Balanced Incomplete Block Plans.</u>

We shall define r, b, t, k, λ, as follows:

r = number of replications (number of times each treatment appears in the plan)

b = number of blocks in the plan

t = number of "treatments"

k = number of treatments which appear in every block

λ = number of blocks which contain any treatment pair.

Using the above nomenclature, it is possible to enumerate the situations in which it is combinatorially possible to construct a balanced incomplete block design. The possibilities are listed in Table 2.1.3 for $4 \leq t \leq 10$, $r \leq 10$. For the remaining plans with $r \leq 10$, see "Experimental Designs" by W. G. Cochran and G. M. Cox, second edition, pp. 520-544.

If one desires to estimate and to make tests concerning block effects in addition to treatment effects, then consideration should be given to the plans where $b = t$, i.e., the number of blocks equals the number of treatments. In these plans, called symmetrical balanced incomplete block designs, differences between block effects are estimated with equal precision for all pairs of blocks.

To use a given plan from Table 2.1.3, one should proceed as follows:

i) Rearrange the blocks at random. (In a number of the plans given, the blocks are arranged in groups. In these plans, rearrange the blocks at random within their respective groups).

ii) Randomize the positions of the treatment letters within each block.

iii) Assign the treatments at random to the letters in the plan.

For analysis, the results of a balanced incomplete block design may be exhibited in a table such as the one below. (Plan 7):

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

Figure 2.1.3

TABLE 2.1.3. INDEX TO PLANS

t	k	r	b	λ	E(1)	Plan
4	2	3	6	1	2/3	1
	3	3	4	2	8/9	*
5	2	4	10	1	5/8	2
	3	6	10	3	5/6	*
	4	4	5	3	15/16	*
6	2	5	15	1	3/5	3
	3	5	10	2	4/5	4
	3	10	20	4	4/5	5
	4	10	15	6	9/10	6
	5	5	6	4	24/25	*
7	2	6	21	1	7/12	*
	3	3	7	1	7/9	7
	4	4	7	2	7/8	8
	6	6	7	5	35/36	*
8	2	7	28	1	4/7	9
	4	7	14	3	6/7	10
	7	7	8	6	48/49	*
9	2	8	36	1	9/16	*
	3	4	12	1	3/4	10a
	4	8	18	3	27/32	11
	5	10	18	5	9/10	12
	6	8	12	5	15/16	13
	8	8	9	7	63/64	*
10	2	9	45	1	5/9	14
	3	9	30	2	20/27	15
	4	6	15	2	5/6	16
	5	9	18	4	8/9	17
	6	9	15	5	25/27	18
	9	9	10	8	80/81	*

*) These plans are formed by forming all possible combinations of the t treatments in blocks of size k. The number of blocks b serves as a check that no block has been missed.

(1) The constant E will be used in the analysis.

BALANCED INCOMPLETE BLOCK PLANS*

Plan 1. t=4, k=2, r=3, b=6, λ=1, E=2/3.

Block Treatments

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

Plan 2. t=5, k=2, r=3, b=6, λ=1, E=5/8.

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

Plan 3. t=6, k=2, r=5, b=15, λ=1, E=3/5.

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

Plan 4. t=6, k=3, r=5, b=10, λ=2, E=4/5.

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

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*) In a number of the plans given, the blocks are arranged in groups. In setting up the experiment, it may be well to make the groups as homogeneous as possible. That is, there should be more difference between blocks in different groups than between blocks in the same group.

Plan 5. t=6, k=3, r=10, b=20, λ=4, E=4/5.

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

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

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

Plan 6. t=6, k=4, r=10, b=15, λ=6, E=9/10.

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

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

Plan 7. t=7, k=3, r=3, b=7, λ=1, E=7/9.

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

Plan 8. t=7, k=4, r=4, b=7, λ=2, E=7/8.

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

Plan 9. t=8, k=2, r=7, b=28, λ=1, E=4/7.

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

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

Plan 10. t=8, k=4, r=7, b=14, λ=3, E=6/7.

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

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

Plan 10a. t=9, k=3, r=4, b=12, λ=1, E=3/4.

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

Plan 11. t=9, k=4, r=8, b=18, λ=3, E=27/32.

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

Plan 12. t=9, k=5, r=10, b=18, λ=5, E=9/10.

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

Plan 13. t=9, k=6, r=8, b=12, λ=5, E=15/16.

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

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

Plan 14. t=10, k=2, r=9, b=45, λ=1, E=5/9.

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

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

Plan 15. t=10, k=3, r=9, b=30, λ=2, E=20/27.

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

Plan 16. t=10, k=4, r=6, b=15, λ=2, E=5/6.

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

Plan 17. t=10, k=5, r=9, b=18, λ=4, E=8/9.

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

Plan 18. t=10, k=6, r=9, b=15, λ=5, E=25/27.

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

Chain Block Plans.

The chain block plan is very useful when we can make only a few more observations than we have treatments and important differences in treatment effects are larger than the experimental error. Basically the design is one in

which some of the treatments are observed twice, and some are observed once. Schematically, we can write the plan as follows:

Blocks

1	2	...	b-1	b
A_1'	A_2'	...	A_{b-1}'	A_b'
A_2''	A_3''	...	A_b''	A_1''
x	x			x
x	x			x
⋮	⋮			⋮
x	x	...		x

A_1' represents either a treatment or a group of treatments and A_1'' represents the same treatment or group of treatments. The x's represent treatments for which we have only one observation. We need not have the same number of these in each block. N-t-b+1 is the number of degrees of freedom for our estimate of the variance of the experimental error .

Frequently, for a given number of blocks and a given number of treatments t, there are several different chain block plans available. Two examples of chain block plans are given below, numbers in each block represent treatments.

Blocks 1	2	3	4
1 2	3 4	5 6	7 8
3 4	5 6	7 8	1 2
9 13	10	11	12

Blocks 1	2	3
1 2 3	4 5 6	7 8 9
4 5 6	7 8 9	1 2 3
10	11	

Schematically, the plans may be written

Blocks 1	2	3	4
A'_1	A'_2	A'_3	A'_4
A''_2	A''_3	A''_4	A''_1
x	x	x	x
x			

Blocks 1	2	3
A'_1	A'_2	A'_3
A''_2	A''_3	A''_1
x	x	

The degrees of freedom for error in the above plans are 5 and 7 respectively. The user should have little difficulty in producing a chain block plan suitable to his own needs.

To use a given chain block plan, the numbers in a given block should be permuted at random. The numbers may then be allocated to the treatments at random.

For purposes of analysis, the observations in the plan should be recorded in the form of the unrandomized plan.

The parameters of the plan are:

b = number of blocks in the plan

k_i = number of observations in the i^{th} block

t = number of treatments

m = number of treatments in a group A_i' or A_i''

N = total number of observations.

2.1.4. Latin and Youden Square Plans.

Latin and Youden Squares are useful when it is desirable to allow for two kinds of non-homogeneity. Suppose we wish to compare four materials with regard to their wearing qualities. Suppose further that we have a wear-testing machine which can handle four samples simultaneously. Two sources of inhomogeneity may be the variations from run to run, and the variation among the four positions on the wear machine. A 4x4 Latin Square in our situation will enable us to allow for both sources of inhomogeneity if we can make four runs. The Latin Square plan is as follows (The four materials are labelled A, B, C, D).

4x4 Latin Square

Run	Position number			
	(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 use of Latin Square plans is restricted by the fact that the number of rows, columns and treatments must always be the same, i.e., in this example the number of runs, positions and treatments must be the same.

Examples of Latin Squares from 3x3 to 11x11 are given in Table 2.1.4a. In the case of the 4x4 Latin Square, four are given, and when a 4x4 Latin Square is needed, one of the four should be selected at random. The procedures to be followed in using a given Latin Square is as follows:

(i) Permute the columns at random.

(ii) Permute the rows at random.

(iii) Assign letters randomly to the treatments.

(If squares of 5x5 and higher are used very frequently, then, strictly speaking, each time we use one we should choose a square at random from the set of all possible squares. Fisher and Yates [4] give complete representations for 5x5 and 6x6 squares).

The results of a Latin Square experiment can be exhibited in a two way table like the plan.

The treatment totals, and row and column totals of the Latin Square plan are each directly comparable without adjustment.

Youden Squares - The Youden Square, like the Latin Square, is used when one wishes to allow for two kinds of inhomogeneity. The conditions for its use are, however, less restrictive. The use of the Latin Square plans is restricted by the fact that the number of rows, columns and treatments must always be the same. Youden Squares have the same number of rows and treatments, but a fairly wide choice in the number of columns is allowed.

In the previous section on Latin Squares a problem was discussed in which we wished to test four materials with regard to their wearing qualities. There were two sources of inhomogeneity, the variation among the four positions on the machine, and the variations from run to run. In order to use the Latin Square plan, we were forced to make 4 runs. For this example, Youden Square arrangements are available in Table 2.1.4b for 3, 5, 6, 7, 8, 9, and 10 runs. Table 2.1.4b gives a number of plans for $t \leq 11$ where t is the number of treatments to be compared. Cochran and Cox, "Experimental Designs" give additional plans. In all the plans given, the analysis is essentially the same, and all differences between treatment effects are estimated with the same precision.

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

(i) Permute the rows at random.

(ii) Permute the columns at random.

(iii) Assign letters at random to the treatments.

The results of a Youden Square plan experiment can be exhibited in a two-way table like the plan.

In many instances where there are two sources of inhomogeneity, a suitable Latin or Youden square may not exist. Suppose we have t treatments, b levels of one source of inhomogeneity, k levels of the other source of inhomogeneity. For a large number of sets of values of t,b,k, plans or arrangements exist which enable the experimenter to allow for the two sources of heterogeneity, in a fairly simple manner. The analysis and interpretation is more complicated than for the plans given, and in case these plans are considered desirable, a statistician should be consulted.

<u>TABLE 2.1.4a LATIN SQUARE ARRANGEMENTS</u>

(include 3x3, 4 4x4's, 5x5, ..., to 11x11. See Tables by Fisher and Yates .)

YOUDEN SQUARE PLANS

1. t=b=3, k=r=4,5,6,7,8,9 or 10

	1	2	3	4	5	6	7	8	9	10
(1)	A	B	C	A	B	C	A	B	C	A
(2)	B	C	A	C	A	B	B	C	A	C
(3)	C	A	B	B	C	A	C	A	B	B

2. t=b=4, k=r=5,7,8 or 9

	1	2	3	4	5	6	7	8	9
(1)	A	B	C	D	A	B	C	D	A
(2)	B	A	D	C	C	D	A	B	D
(3)	C	D	A	B	D	C	B	A	B
(4)	D	C	B	A	B	A	D	C	C

3. t=b=4, k=r=6

	1	2	3	4	5	6
(1)	A	B	C	A	B	C
(2)	B	A	D	B	A	D
(3)	C	D	A	C	D	A
(4)	D	C	B	D	C	B

4. t=b=4, k=r=10

	1	2	3	4	5	6	7	8	9	10
(1)	A	B	C	D	A	A	B	C	D	A
(2)	B	A	D	C	C	B	A	D	C	C
(3)	C	D	A	B	D	C	D	A	B	D
(4)	D	C	B	A	B	D	C	B	A	B

5. t=b=5, k=r=6,9 or 10

	1	2	3	4	5	6	7	8	9	10
(1)	A	B	C	D	E	A	B	C	D	E
(2)	B	C	D	E	A	D	E	A	B	C
(3)	C	D	E	A	B	B	C	D	E	A
(4)	D	E	A	B	C	E	A	B	C	D
(5)	E	A	B	C	D	C	D	E	A	B

6. t=b=5, k=r=8

	1	2	3	4	5	6	7	8
(1)	A	B	C	D	A	B	C	D
(2)	B	C	D	E	B	C	D	E
(3)	C	D	E	A	C	D	E	A
(4)	D	E	A	B	D	E	A	B
(5)	E	A	B	C	E	A	B	C

7. t=b=6, k=r=7

	1	2	3	4	5	6	7
(1)	A	B	C	D	E	F	A
(2)	B	C	F	A	D	E	C
(3)	C	F	B	E	A	D	E
(4)	D	E	A	B	F	C	D
(5)	E	A	D	F	C	B	F
(6)	F	D	E	C	B	A	B

8. t=b=6, k=r=10

	1	2	3	4	5	6	7	8	9	10
(1)	A	B	C	D	E	A	B	C	D	E
(2)	B	C	F	A	D	B	C	F	A	D
(3)	C	F	B	E	A	C	F	B	E	A
(4)	D	E	A	B	F	D	E	A	B	F
(5)	E	A	D	F	C	E	A	D	F	C
(6)	F	D	E	C	B	F	D	E	C	B

9. t=b=7, k=r=3 or 9

	1	2	3	4	5	6	7	8	9
(1)	A	B	D	A	B	D	A	B	D
(2)	B	C	E	B	C	E	B	C	E
(3)	C	D	F	C	D	F	C	D	F
(4)	D	E	G	D	E	G	D	E	G
(5)	E	F	A	E	F	A	E	F	A
(6)	F	G	B	F	G	B	F	G	B
(7)	G	A	C	G	A	C	G	A	C

10. t=b=7, k=r=4

	1	2	3	4
(1)	A	C	D	E
(2)	B	D	E	F
(3)	C	E	F	G
(4)	D	F	G	A
(5)	E	G	A	B
(6)	F	A	B	C
(7)	G	B	C	D

11. t=b=7, k=r=8

	1	2	3	4	5	6	7	8
(1)	A	B	C	D	E	F	G	A
(2)	B	E	A	G	F	D	C	C
(3)	C	F	G	B	D	A	E	G
(4)	D	G	E	F	C	B	A	F
(5)	E	D	B	C	A	G	F	E
(6)	F	C	D	A	G	E	B	D
(7)	G	A	F	E	B	C	D	B

12. t=b=11, k=r=5

	1	2	3	4	5
(1)	A	B	C	D	E
(2)	G	A	F	J	C
(3)	I	H	A	F	B
(4)	K	I	G	A	D
(5)	J	K	E	H	A
(6)	H	G	B	C	K
(7)	B	F	D	K	J
(8)	F	C	K	E	I
(9)	C	D	J	I	H
(10)	E	J	I	B	G
(11)	D	E	H	G	F

13. t=b=11, k=r=6

	1	2	3	4	5	6
(1)	A	F	J	D	I	E
(2)	B	C	I	G	D	F
(3)	C	J	B	F	E	H
(4)	D	E	G	C	K	J
(5)	E	H	D	K	B	I
(6)	F	G	H	I	J	K
(7)	G	K	E	A	F	B
(8)	H	B	A	J	G	D
(9)	I	A	C	E	H	G
(10)	J	I	K	B	C	A
(11)	K	D	F	H	A	C

TABLE 2.1.4b YOUDEN SQUARE ARRANGEMENTS

t=b	k=r	E	Plan
3	4	15/16	First 4 Columns of 1
	5	24/25	First 5 Columns of 1
	6	1	First 6 Columns of 1
	7	48/49	First 7 Columns of 1
	8	63/64	First 8 Columns of 1
	9	1	First 9 Columns of 1
	10	99/100	Plan 1
4	3	8/9	*
	5	24/25	First 5 Columns of 2
	6	8/9	Plan 3
	7	48/49	First 7 Columns of 2
	8	1	First 8 Columns of 2
	9	80/81	Plan 2
	10	24/25	Plan 4
5	4	15/16	*
	6	35/36	First 6 Columns of 5
	8	15/16	Plan 6
	9	80/81	First 9 Columns of 5
	10	1	Plan 5
6	5	24/25	*
	7	48/49	Plan 7
	10	24/25	Plan 8
7	3	7/9	First 3 Columns of 9
	4	7/8	Plan 10
	6	35/36	*
	8	63/64	Plan 11
	9	7/9	Plan 9
8	7	48/49	*
9	8	63/64	*
10	9	80/81	*
11	5	22/25	Plan 12
	6	11/12	Plan 13

*) Construct from a txt Latin Square by omission of the last column. See Table 2.1.4a of Latin Squares.

2.2 Analysis

2.2.1. Completely Randomized Plans - See Part I, section 2.4.

2.2.2. Randomized Block Plans

Our analysis of a randomized block experiment depends on a number of assumptions. We assume that each of our observations is the sum of three components. If we let Y_{ij} be the observation on the i^{th} treatment in the j^{th} block, then

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

where β_j is a term peculiar to a given block, and is constant regardless of which treatments occur in the block. It is the amount by which the response of a given treatment in the j^{th} block differs from the response of the same treatment averaged over all blocks, assuming no experimental error.

φ_i is a term peculiar to the i^{th} treatment, and is constant regardless of the block in which the treatment occurs. It may be regarded as the average value of the i^{th} treatment averaged over all blocks in the experiment if there were no experimental error.

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

In the following analysis, we assume that the results of the experiment are tabulated as in Figure 2.1.2.

Estimation of the treatment effects φ_i

The treatment effect φ_i is estimated by the mean of the observations on the i^{th} treatment. That is, the estimate of φ_i is $t_i = T_i/b$.

Testing and estimating differences in treatment effects

Suppose we wish to test whether or not there is a difference in treatment effects. We may proceed as follows:

i) Select α, the significance level of the test.

ii) Look up $q_{1-\alpha}(t,\nu)$ in Table IV, where $\nu = (b-1)(t-1)$.

iii) Compute $S_t = (T_1^2 + T_2^2 + \ldots + T_t^2)/b - G^2/tb$.

iv) Compute $S_b = (B_1^2 + B_2^2 + \ldots + B_b^2)/t - G^2/tb$.

v) Compute $S = \sum_{i=1}^{t} \sum_{j=1}^{b} Y_{ij}^2 - G^2/tb$, i.e., compute the sum of the squares of all the observations, and subtract G^2/tb.

vi) Compute $s^2 = (S - S_b - S_t)/(b-1)(t-1)$.

vii) Compute $w = q_{1-\alpha}s/\sqrt{b}$

viii) If the difference between any two estimated treatment effects exceeds w, decide that the treatment effects differ. Otherwise, decide that the experiment gives no reason to believe the treatment effects differ.

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

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

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

<u>Estimation of Block Effects</u> β_j

The block effect β_j is estimated by the mean of the observations in the j^{th} block minus the grand mean. That is, the estimate of β_j, the j^{th} block effect is $b_j = B_j/t - G/rt$.

<u>Testing and estimating differences in block effects</u>

There are occasions when one is as interested in block effects as in treatment effects. If we wish to test to see if the blocks have different effects, we may proceed as follows:

i) Choose α, the significance level of the test.

ii) Look up $q_{1-\alpha}(b,\nu)$ in Table IV, where $\nu = (b-1)(t-1)$.

iii), iv), v), vi), - Same steps as in testing and estimating differences in treatment effects.

vii) Compute $w' = q_{1-\alpha} s/\sqrt{t}$.

viii) If the difference between any two estimated block effects exceeds w', decide that the block effects differ. Otherwise, decide that the experiment gives no reason to believe the block effects differ.

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

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

2.2.3. Incomplete Randomized Block Plans

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

The analysis we will describe is what is sometimes called the intra-block analysis.

Balanced Incomplete Block Plans

Estimating treatment effects

We shall assume the observations have been exhibited in a table such as Figure 2.1.3. The treatment effects cannot be estimated directly from the treatment averages, but must be adjusted for possible block effects. The estimate of φ_i the effect of the i^{th} treatment is $t_i = Q_i/Er + G/rt$, where $Q_i = T_i$ - (Sum of the totals of those blocks in which treatment i occurs)/k.

Testing and estimating differences in treatment effects

If we wish to see whether there is a difference in the treatment effects, we may proceed as follows:

i) Choose α, the significance level of the test.

ii) Look up $q_{1-\alpha}(t,\nu)$ in Table IV, entering the table with t and ν where $\nu = tr-t-b+1$.

iii) Compute Q_i and t_i for each treatment (The sum of the Q_i should equal zero).

iv) Compute $S_t = (Q_1^2 + Q_2^2 + \ldots + Q_t^2)/Er$

v) Compute $S_b = (B_1^2 + B_2^2 + \ldots + B_b^2)/k$

vi) Compute $S = \Sigma Y_{ij}^2 - G^2/rt$, i.e., compute the sum of the squares of all the observations and subtract G^2/rt.

vii) Compute $s^2 = (S - S_t - S_b)/(tr-t-b+1)$

viii) Compute $w = qs/(Er)^{1/2}$

ix) If the difference between any two estimated treatment effects exceeds w, decide that the treatment effects differ. Otherwise, decide that the experiment gives no reason to believe the treatment effects differ.

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

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

Estimation of Block Effects

Like the treatment effects, block effects cannot be estimated directly from block averages, but must be adjusted according to which treatments occur in them. We shall discuss estimation of the block effects only in those cases where $b=t$, i.e., the number of blocks equals the number of treatments. In these cases we have what is called a symmetrical balanced incomplete block plan. If it is desired to estimate or test block effects in a balanced incomplete block plan which is not symmetric, a statistician should be consulted.

The estimate b_j of the j^{th} block effect β_j is Q'_j/Er where $Q'_j = B_j$ - (Sum of treatment totals of all treatments occurring in the j^{th} block)/r.

<u>Testing and estimating differences in block effects</u>

In the case of symmetrical balanced incomplete block plans, we can test to see whether there is a difference in the block effects as follows:

i) Choose α, the significance level of the test.

ii) Look up $q_{1-\alpha}(b,\nu)$ in Table IV, where $\nu = tr-t-b+1$.

iii) Compute Q'_i and b_i for each block (the sum of the Q'_i should be zero).

iv) Compute $S'_b = (Q'^2_1 + Q'^2_2 + \ldots + Q'^2_t)/Er$.

v) Compute $S'_t = (T^2_1 + T^2_2 + \ldots + T^2_t)/r$.

vi) Compute $S = \Sigma Y^2_{ij} - G^2/rt$, i.e., compute the sum of the squares of all the observations and subtract G^2/rt.

vii) Compute $s^2 = (S - S'_t - S'_b)/(tr-t-b+1)$.

Since $S'_t + S'_b = S_t + S_b$, this should give the same value for s^2 as in (vii) of the section on estimating and testing differences in treatment effects.

viii) Compute $w' = q_{1-\alpha}s/(Er)^{1/2}$.

ix) If the difference between any two estimated block effects exceeds w', decide that the block effects differ. Otherwise, decide that the experiment gives no reason to believe the block effects differ.

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

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

Chain Block Plans

We shall assume the observations have been tabled in the schematic form below:

Blocks

1	2	...	b-1	b
A'_1	A'_2	...	A'_{b-1}	A'_b
A''_2	A''_3	...	A''_b	A''_1
x	x		x	x
⋮	⋮		⋮	⋮
x	x	...	x	x

Estimating treatment and block effects

Since the method of estimating treatment effects requires calculation of the estimated block effects, we shall first compute the block effects. The procedure is as follows:

i) Compute the sum of the observations for each of the groups A_i', A_i''. Call the totals X_i', X_i''.

ii) Compute $D_i = X_i' - X_i''$,

$$G' = X_1' + X_2' + \ldots + X_b'$$

$$G'' = X_1'' + X_2'' + \ldots + X_b''$$

G''' = sum of all observations on treatments which occur only once.

$$G = G' + G'' + G'''$$

The results may be tabulated in the form below:

X_1'	X_2'	...	X_{b-1}'	X_b'	G'
X_1''	X_2''	...	X_{b-1}''	X_b''	G''
D_1	D_2	...	D_{b-1}	D_b	

iii) Compute $L_1 = (b-1)(D_1-D_2)+(b-3)(D_b-D_3)+(b-5)(D_{b-1}-D_4)+\ldots$ where the sum is over $b/2$ terms if b is even, and $(b-1)/2$ terms if b is odd.

iv) Compute $H = (G'' - G')/mb$.

v) If there are m treatments in each group A_i' or A_i'', then we may estimate the first block effect as follows: $b_1 = L_1/2mb$.

vi) Compute:

$$b_2 = b_1 + D_2/m + H$$

$$b_3 = b_2 + D_3/m + H$$

$$\vdots$$

$$b_b = b_{b-1} + D_1/m + H.$$

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

vii) The estimated treatment effects t_i are computed as follows:

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

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

Testing and estimating differences in treatment effects

To test for differences in treatment effects, we may proceed as follows:

i) Choose α, the significance level of the test.

ii) Look up $F_{1-\alpha}(t-1, N-b-t-1)$ in Table III.

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

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

v) From each of the observations in A_1' subtract the observation on the same treatment in A_1''. If the differences are $d_{11}, d_{12}, \ldots, d_{1m}$, compute $S_1 = (d_{11}^2 + d_{12}^2 + \ldots + d_{1m}^2)/2 - D_1^2/2m$. Compute the comparable quantities $S_2, S_3, \ldots, S_b$.

vi) Compute $S_e = S' + S_1 + S_2 + \ldots + S_b$, and $s^2 = S_e/(N-b-t-1)$.

vii) Compute S = sum of squares of all the observations minus G^2/N.

viii) Compute $S_t = S - S_b - S_e$.

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

x) If $F > F_{1-\alpha}$, conclude that the treatments differ. Otherwise, conclude that the experiment gives no reason for us to believe that the treatments differ.

2.2.4. Latin and Youden Square Plans

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

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

where ρ_j is a term peculiar to the j^{th} row, and is constant regardless of column or treatment effects.

K_m is a term peculiar to the m^{th} column and is defined similarly to ρ_j.

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

ϵ_{ijm} is the experimental error involved in the given observation.

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

In the following analysis, we assume the data have been exhibited in a two way table like the plan. We put

T_i = Sum of the observations in the i^{th} treatment.

R_i = Sum of the observations in the i^{th} row.

C_i = Sum of the observations in the i^{th} column.

G = Sum of all the observations.

Latin Square Plans

Estimation of treatment effects

The estimate t_i of the i^{th} treatment effect φ_i can be estimated directly by the treatment average T_i/r, where r is the number of times the treatment occurs (also the number of treatments, the number of rows, and the number of columns).

Testing and estimating differences in treatment effects

If we wish to test whether there are differences in treatment effects, we may proceed as follows:

i) Choose α, the significance level of the test.

ii) Look up $q_{1-\alpha}(r,\nu)$ in Table IV, where $\nu = (r-2)(r-1)$.

iii) Compute $S_t = (T_1^2 + T_2^2 + \ldots + T_r^2)/r - G^2/r^2$.

iv) Compute $S_r = (R_1^2 + R_2^2 + \ldots + R_r^2)/r - G^2/r^2$.

v) Compute $S_c = (C_1^2 + C_2^2 + \ldots + C_r^2)/r - G^2/r^2$.

vi) Compute S = sum of squares of all the observations $- G^2/r^2$.

vii) Compute $s^2 = (S - S_t - S_r - S_c)/(r-2)(r-1)$.

viii) Compute $w = qs/r^{1/2}$.

ix) If the difference between any two estimated treatment effects exceeds w, decide that the treatment effects differ. Otherwise, decide that the experiment gives no reason to believe the treatment effects differ.

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

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

<u>Estimation of row (column) effects</u>

The row (column) effects can be directly estimated by subtracting G/r^2 from the row (column) averages. That is, we estimate ρ_i by $r_i = R_i/r - G/r^2$, and K_i by $c_i = C_i/r - G/r^2$.

<u>Testing and estimating differences in row (column) effects</u>

If it is desired to test for differences in row (column) effects, we can use the following procedure:

i) to vii) Same as procedure for testing and estimating differences in treatment effects.

viii) Compute $w_r = qs/r^{1/2}$.

ix) If the difference between any two estimated row effects r_i exceeds w_r decide that the treatment effects differ. Otherwise, decide that the experiment gives no reason to believe the treatment effects differ.

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

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

(For a similar set of statements about the columns, replace r_i, r_j, ρ_i, ρ_j, by c_i, c_j, K_i, K_j).

Youden Squares

As in the case of the incomplete block plan, the analysis we present is what is sometimes called the intra-block analysis.

Estimation of treatment effects

The estimate t_i of the i^{th} treatment effect φ_i is $t_i = Q_i/Er + G/r^2$ where, $Q_i = T_i - (n_{i1}R_1 + n_{i2}R_2 + \ldots + n_{ib}R_b)/r$, n_{ij} is the number of times the i^{th} treatment occurs in the j^{th} row.

Testing and estimating differences in treatment effects

If we wish to test whether there are differences in treatment effects, we may proceed as follows:

i) Choose α, the significance level of the test.

ii) Look up $q_{1-\alpha}(t,\nu)$ in Table IV, where $\nu = (b-2)(r-1)$.

iii) Compute $S_t = (Q_1^2 + Q_2^2 + \ldots + Q_t^2)/Er$.

iv) Compute $S_r = (R_1^2 + R_2^2 + \ldots + R_b^2)/k - G^2/bk$.

v) Compute $S_c = (C_1^2 + C_2^2 + \ldots + C_r^2)/b - G^2/bk$.

vi) Compute S = sum of squares of all observations minus G^2/bk.

vii) Compute $s^2 = (S - S_t - S_r - S_c)/(b-2)(r-1)$.

viii) Compute $w = qs/(Er)^{1/2}$.

ix) If the difference between any two estimated treatment effects exceeds w, decide that the treatment effects differ. Otherwise, decide that the experiment gives no reason to believe the treatment effects differ.

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

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

Estimation of column effects

The column effects can be estimated directly from the column means, i.e., the estimate of the i^{th} column effect is $c_i = C_i/b - G/bk$.

Testing and estimating differences in column effects

If we wish to test whether there are differences in column effects we can proceed as follows:

i) Choose α, the significance level of the test.

ii) Look up $q_{1-\alpha}(k, \nu)$ in Table IV, where $\nu = (b-2)(r-1)$.

iii) to vii) Same as "Testing and estimating differences in treatment effects".

viii) Compute $w_c = q_{1-\alpha} s/\sqrt{b}$.

ix) If the difference between any two estimated column effects exceeds w_c, decide that the column effects differ. Otherwise, decide that the experiment gives no reason to believe the column effects differ.

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

$$c_i - c_j - w_c \leq K_i - K_j \leq c_i - c_j + w_c \ .$$

Estimation of row effects

The estimate of the j^{th} row effect ρ_j is $r_j = Q'_j/Er$, where

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

and as before n_{ij} is the number of times the i^{th} treatment occurs in the j^{th} row.

Testing and estimating differences in row effects

If we wish to test whether there are differences in row effects, we may proceed as follows:

i) Choose α, the significance level of the test.

ii) to vii) Same as "Testing and estimating differences in treatment effects."

viii) Compute $w_r = q_{1-\alpha}s/\sqrt{k}$.

ix) If the difference between any two estimated row effects exceeds w_r, decide that the row effects differ. Otherwise, decide that the experiment gives no reason to believe the row effects differ.

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

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

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U. S. DEPARTMENT OF COMMERCE
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Optics and Metrology. Photometry and Colorimetry. Optical Instruments. Photographic Technology. Length. Engineering Metrology.

Heat and Power. Temperature Physics. Thermodynamics. Cryogenic Physics. Rheology and Lubrication. Engine Fuels.

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