CHAPTER 13

THE RANDOMIZED BLOCKS DESIGN

13.1 Local control

13.1.1 The problem of "reduction of error" or of trying to ensure that experimental error is kept within reasonable bounds is one which attracts so much attention, particularly in field experiments, that the beginner frequently confuses himself into thinking that "randomization reduces error". On the contrary, what randomization effectively achieves is the conversion of systematic errors into independent random errors; the magnitude of the variability between plots is unaffected.

13.1.2 The laboratory experimentalist is fortunate in that he can usually choose the material upon which he wishes to test a number of different treatments so that it is very uniform and the differences observed are likely to be chiefly treatment differences. The field experimentalist has no such advantage, since the fertility of the soil can vary considerably over quite small areas without taking into account major differences such as those of soil type. Thus in a simple random design laid out as in Figure 12.3 with treatments scattered over the whole experimental area, the variation in yields within a treatment is likely to be considerable unless the fertility gradient is fairly regular and happens to be parallel to the longer sides of the plots, especially if the gradient is parallel to the length of the area. The C.V. could be of the order of 20%–30%, and since fertility gradients can seldom be predicted with any very great accuracy, it is necessary to seek some alternative design, in which the possibility of such a high error is reduced. *The simple random design is therefore very seldom used in field experiments.* Even when the variate is not yield, but, for example, incidence of disease, there is usually some systematic effect (even if it is quite independent of soil fertility) associated with position on the ground such that there can be considerable differences between plots at different ends of the experimental area.

13.1.3 The author has heard the suggestion made that the experimental area should be stripped of its soil to a given depth by bulldozers, the soil to be replaced after thorough mixing in a big pile. Quite apart from the question of expense, such a proposal would, of course, grossly contravene the principle of realism! It is perhaps not so immediately obvious that there would still in all probability be systematic effects due to position, apart from any question of soil heterogeneity, which would contribute to experimental error.

This is well illustrated by pot experiments. It should be mentioned that pot experiments are, of course, a long way from field experiments in respect of realism and can have very high errors owing to the few plants which can be placed in a pot. They can nevertheless be valuable for small-scale investigations, and since the conditions are quite artificial anyway, it is natural to mix the soil as thoroughly as possible before placing it in the pots. In this way soil heterogeneity can be virtually excluded as a source of error, but in a greenhouse there can be marked effects due to position arising from variations in light, draught, etc. Even when pots are placed in a compact mass out in the open, it is possible for effects of position to be revealed for no obvious or visible reason.

13.1.4 The possibility of high experimental error with a simple random design may be countered by dividing the experimental area into a number of equal rectangular areas called **blocks**, within which the treatments allotted to a particular block in accordance with the design are randomized, on the principle that plots are likely to vary less within the smaller area of a block than over the whole experimental area. This is, in another form, the first characteristic of soil heterogeneity mentioned in § 12.1.4 and is known as the principle of **local control.** Differences between blocks are eliminated from experimental error by means of certain D.F. in the analysis of variance allocated for this purpose; the experimental error then consists solely of variation of plots within blocks.

13.1.5 The word "control" refers to the control of error, i.e. keeping it within bounds. In view of the definition of experimental error as "variation over which the experimenter has no direct control" (§ 2.8.9), it should be explained that it is possible for the experimenter to use devices (such as local control) which are likely to make the error less than if the devices had not been used, but that once the design and lay-out have been decided upon experimental error arises from causes which are beyond his control, even though, naturally, careful technique will help to keep the error low. The point is that despite local control and the highest degree of care, residual error will always be present, and a high C.V. is not necessarily a reflection on the manner in which the experiment has been conducted, though it may suggest that a better design or lay-out could have been used.

13.2 Randomized blocks design

13.2.1 The simplest example of the use of local control is in the **randomized blocks design.** Here the experimental area is divided into a number of blocks

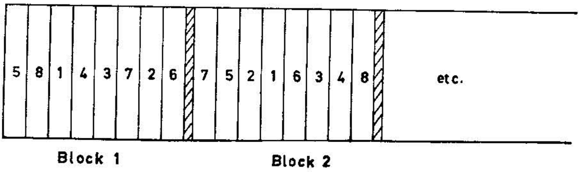

Figure 13.1: A randomized blocks design with 8 treatments.

equal to the number of replications required, and each block contains a single replication of the experiment. In other words each block contains a number of plots equal to the number of treatments and *each treatment appears once and once only in each block*; the order of treatments within a block is randomized.

Thus, although there is an element of balancing in the randomized blocks design, the essential element of randomization is not lacking.

13.2.2 In accordance with the principle of local control the variation of plots within a block under uniform treatment is usually less than over the whole experimental area, so that comparisons between treatments, being made within blocks, will be, in general, subject to less error than in the simple random design. Any major effects of soil heterogeneity over the area will thus be eliminated from experimental error. As compared with the simple random design, the randomization of treatments to plots is subject to the restriction that no treatment can appear more than once in a block; this is the origin of the alternative name "completely random design" for the simple random design.

13.2.3 The randomized blocks design uses the same principle in experimental design as the stratified sample does in sampling theory, the object being reduction of error, whether experimental or sampling. Thus in a simple random design with 6 replications of 8 treatments we have 8 samples of 6 plots each, chosen at random from the finite population of 48 plots, whereas in the corresponding randomized blocks design one plot in each sample is taken from each block or stratum. In this case we have stratified to the maximum extent, since there are as many strata as there are sampling units in each sample.

The two theories do not completely coincide, however. Thus it is an essential condition for a stratified sample that at least one S.U. be drawn from each stratum. In a randomized blocks design, however, it is quite possible for one of the stratified samples (i.e. the plots of one treatment) to have a unit missing in one stratum (i.e. the treatment has a plot missing in one block), without affecting the principle underlying the reduction of error. Such a "missing plot" can easily occur as the result of a mishap in the field, but, as we shall see later, it destroys the basic symmetry of the design and causes the analysis to become more difficult.

The reduction of error brought about by a randomized blocks design may be explained on the basis that each treatment shares more or less equally in the fertility of the area by virtue of being applied to one plot in each block. While this explanation is correct for a randomized blocks design with all data intact, it is inadequate so far as the principle of local control on its own is concerned. Two of the principles of the randomized block design—local control and the occurrence of each treatment once and once only in each block—are involved in the explanation above, but only the former is strictly relevant in respect of reduction of error.

13.3 Partitioning of the Total S.S. in a randomized blocks design

13.3.1 Let us consider a rectangular array of the yields from 5 replications of 4 treatments, using the letter y (for yield) in place of x.

Table 13.1: Symbolic table of yields in a randomized blocks design

Blocks	Treatments 1	2	3	4	Block means
1	y_{11}	y_{21}	y_{31}	y_{41}	y_{01}
2	y_{12}	y_{22}	y_{32}	y_{42}	y_{02}
3	y_{13}	y_{23}	y_{33}	y_{43}	y_{03}
4	y_{14}	y_{24}	y_{34}	y_{44}	y_{04}
5	y_{15}	y_{25}	y_{35}	y_{45}	y_{05}
Treatment means	y_{10}	y_{20}	y_{30}	y_{40}	$\bar{y}$

We are now interested in the means of the horizontal arrays, $y_{01}, y_{02}, \ldots y_{05}$, i.e. *the block means,* for we wish to eliminate from our estimate of error variance the systematic component of variation represented by differences between blocks.

From our previous result ([10.16] or [10.17]) we have the partitioning

$$\sum_i \sum_j (y_{ij} - \bar{y})^2 = 5 \sum_i (y_{i0} - \bar{y})^2 + \sum_i \sum_j (y_{ij} - y_{i0})^2, \qquad [13.1]$$

where the L.H.S. represents the Total S.S. and the first term on the R.H.S. the Treatments S.S. The final term, which in the simple random design represented the Error S.S., is now a mixture of block and error effects and it is required to separate them.

13.3.2 To achieve this let us apply [10.15] to $\sum_i (y_{i1} - y_{i0})^2$, i.e. the sum of squared residuals for Block 1 only, obtaining

$$\sum_i (y_{i1} - y_{i0})^2 = 4(y_{01} - \bar{y})^2 + \sum_i (y_{i1} - y_{i0} - y_{01} + \bar{y})^2.$$

Summing over all blocks, we have

$$\sum_i \sum_j (y_{ij} - y_{i0})^2 = 4 \sum_j (y_{0j} - \bar{y})^2 + \sum_i \sum_j (y_{ij} - y_{i0} - y_{0j} + \bar{y})^2,$$

and, substituting in [13.1],

$$\sum_i \sum_j (y_{ij} - \bar{y})^2 = 5 \sum_i (y_{i0} - \bar{y})^2 + 4 \sum_j (y_{0j} - \bar{y})^2 + \sum_i \sum_j (y_{ij} - y_{i0} - y_{0j} + \bar{y})^2.$$

The second term on the R.H.S. represents deviations of block means from the general mean. The Total S.S. has therefore been partitioned into three S.S.'s representing, in order, deviations due to treatment means, block means, and residuals. Since the only controlled sources of variation in the design are blocks and treatments, the residual variation must represent experimental error.

More generally, if we have r replications of t treatments the partitioning is

$$\sum_i \sum_j (y_{ij} - \bar{y})^2 = r \sum_i (y_{i0} - \bar{y})^2 + t \sum_j (y_{0j} - \bar{y})^2 + \sum_i \sum_j (y_{ij} - y_{i0} - y_{0j} + \bar{y})^2. \qquad [13.2]$$

13.3.3 This partitioning may be written down mechanically by using the methods explained in § 10.6.8. The Treatments and Blocks S.S.'s have their usual form and the contents of the last bracket are obtained by a subtraction process involving the contents of the other brackets, viz.

$$y_{ij} - y_{io} - y_{oj} + \bar{y} = y_{ij} - \bar{y} - (y_{io} - \bar{y}) - (y_{oj} - \bar{y}). \qquad [13.3]$$

13.3.4 The partitioning [13.2] is again purely algebraic, being true for any quantities y_{ij} in a rectangular array. If the y_{ij} are statistical variates, the subdivision of the Total S.S., being exact, is orthogonal, and there is the usual partitioning of D.F. as well. Since there are t treatments, there are $t - 1$ D.F. for treatments; since there are r blocks, there are $r - 1$ D.F. for blocks. This leaves $rt - 1 - (t - 1) - (r - 1) = (r - 1)(t - 1)$ D.F. for error, and the analysis of variance is as follows:

Table 13.2: Symbolic analysis of variance for a randomized blocks design

Source of variation	D.F.	S.S.	M.S.
Blocks	$r - 1$	$t\sum_j(y_{oj} - \bar{y})^2$	
Treatments	$t - 1$	$r\sum_i(y_{io} - \bar{y})^2 = A$	$\frac{A}{t-1}$
Error	$(r - 1)(t - 1)$	$\sum_i\sum_j(y_{ij} - y_{io} - y_{oj} + \bar{y})^2 = B$	$\frac{B}{(r-1)(t-1)}$
Total	$rt - 1$	$\sum_i\sum_j(y_{ij} - \bar{y})^2$	

If the errors are independently distributed, the M.S.'s for treatments and error are, on the null hypothesis of no treatment effects, both estimates of the error variance (σ^2). If we make the assumption that the errors are normally as well as independently distributed, all three component S.S.'s are statistically independent, so that the usual F-test of the null hypothesis may be made by means of $\dfrac{\text{Treatments M.S.}}{\text{Error M.S.}}$.

13.3.5 So far as the Blocks S.S. is concerned, it fulfils its purpose by accounting for the variation between blocks and removing it from the Error S.S. There is really no question of any test of significance of $\dfrac{\text{Blocks M.S.}}{\text{Error M.S.}}$; we are prepared to assume that differences between blocks exist. The orthogonality of the three S.S.'s ensures (on the additivity assumptions discussed in § 13.5) that the Error M.S. remains an unbiased estimate of σ^2 whether or not block and treatment effects exist. It also ensures that the Treatments S.S. represents deviations due to treatment effects only (except for the inevitable random variation) irrespective of any block effects. We shall discuss this aspect of the design more fully in § 13.6.

13.4 Statistical analysis of a randomized blocks design

The calculations of the S.S.'s for the analysis of variance follow the rule set out in § 10.3.7. The Blocks S.S. is computed as

$$\frac{1}{t}(Y_{01}^2 + Y_{02}^2 + \cdots + Y_{0r}^2) - C.F., \qquad [13.4]$$

where Y_{0j} = total of j^{th} block. Similarly the Treatments S.S. is obtained by

$$\frac{1}{r}(Y_{10}^2 + Y_{20}^2 + \cdots + Y_{t0}^2) - C.F., \qquad [13.5]$$

where Y_{i0} = total for i^{th} treatment. Notice carefully that the divisor for *blocks* = no. of *treatments*, and *vice versa.* The Error S.S. is usually obtained by subtraction for the reason set out in § 10.3.4.

Example 13.1 The following represents the field plan and yields (in lb.) in a fertilizer experiment with potatoes laid out in 4 randomized blocks:

Block 1

F 331	*B* 286	*E* 312
C 311	*D* 280	*A* 177

Block 2

D 292	*F* 323	*A* 185
C 294	*B* 278	*E* 322

Block 3

F 313	*C* 266	*A* 182
E 319	*D* 284	*B* 258

Block 4

C 291	*B* 253	*E* 328
A 193	*D* 233	*F* 319

The treatments are: A = control
B = sulphate of ammonia (N)
$C = N +$ superphosphate (P)
$D = N +$ lime (L)
$E = N + P +$ potash (K)
F = special NPK mixture

Analyse the data, presenting results in tons per acre, given that each plot $= \frac{1}{85}$ acre (1 ton = 2,000 lb.).

Computation sheet (basic analysis)

Block	Treatment (A) *A*	*B*	*C*	*D*	*E*	*F*	Block totals
1	177	286	311	280	312	331	1,697
2	185	278	294	292	322	323	1,694
3	182	258	266	284	319	313	1,622
4	193	253	291	233	328	319	1,617
Treatment totals	737	1,075	1,162	1,089	1,281	1,286	6,630 (B)

$$\text{C.F.} = 1{,}831{,}537\cdot 5 \text{ (C)}$$

$$\text{Total S.S.} = \begin{array}{r} 1{,}886{,}776 \\ \underline{1{,}831{,}538} \\ 55{,}238 \end{array}$$

$$\text{Blocks S.S.} = \begin{array}{r} 1{,}832{,}503 \\ \underline{1{,}831{,}538} \\ 965 \end{array} \text{ (D)}$$

$$\text{Treatments S.S.} = \begin{array}{r} 1{,}882{,}429 \\ \underline{1{,}831{,}538} \\ 50{,}891 \end{array} \text{ (E)}$$

Analysis of variance

Source	D.F.	S.S.	M.S.	*F*
Blocks ..	3	965		
Treatments ..	5	50,891	10,178	**
Error ..	15 (F)	3,382 (G)	225·5	
Total ..	23	55,238		

$$\text{S.E. of a single yield} = 15\cdot 02 \text{ (H)}$$

$$\text{C.V.} = \frac{15\cdot 02}{6630} \times 24 \times 100 = 5\cdot 49\%$$

$$\text{S.E. of a single treatment total} = 30\cdot 04 \text{ (I)}$$

$$\text{Least significant differences (2 treatment totals)} = \sqrt{8 \times 225\cdot 2} \times t \text{ (15 D.F.) (J)}$$

$$= 42\cdot 474 \times \begin{cases} 2\cdot 131 \\ 2\cdot 947 \end{cases}$$

$$= \begin{array}{l} 90\cdot 5 \ (5\%) \\ 125\cdot 2 \ (1\%) \end{array}$$

$$\text{Conversion factor, treatment totals to means in tons per acre} = \frac{85}{4 \times 2000} = 0\cdot 010625 \text{ (K)}$$

Notes on basic analysis

(A) The very first step in any analysis, but especially in field experiments, is to check the data, particularly the yields. Neatly typed statements of the yields in an experiment too often turn out to be actually riddled with errors. If possible, the biometrician should insist on access to the field note-books and draw up his own independent data sheet to check with that provided. This cannot, of course, eliminate errors made in weighing or in recording weights in the field note-book; any mistakes at this stage cannot be checked later, which emphasizes the necessity for neat and accurate recording in the field. What can be picked up, however, are mistakes in addition when two or more weighings are necessary for the produce of one plot, or in subtraction when the weight of a bag or pan has to be deducted. Everyone has the greatest of confidence in his own ability to prepare an accurate data sheet from information provided, but experience shows that it is only rarely that such confidence is well founded; even simple copying mistakes are frequently made, and *no one is immune.* An independent check of the data sheet will therefore save the gnashing of teeth which will occur when the whole analysis has to be recalculated because of some mistake in the data.

There is, of course, no chance of making any such check on the data provided here, but a check should be made on the data as entered in the computation sheet, because even in this simple operation mistakes can occur. For example, the G.T. in the computation sheet could be checked against the G.T. of the yields in the data sheet.

The first step in the computations proper is to draw up a rectangular table of yields classified according to block and treatment. Attention should be paid to the headings to guard against the possibility of mistaking the blocks for the treatments, and *vice versa.*

(B) *It is imperative that the G.T. be checked as the sum of the block totals as well as the sum of the treatment totals.*

(C) $\text{C.F.} = \dfrac{(6630)^2}{24}$.

(D) $\dfrac{1}{6}(1697^2 + \cdots + 1617^2) - C.F.$ (Formula [13.4]).

(E) $\dfrac{1}{4}(737^2 + \cdots + 1286^2) - C.F.$ (Formula [13.5]).

Calculate the component S.S.'s in the order of appearance in the analysis of variance table. (See Notes C and D, Example 10.3.)

(F) Check that the D.F. for error is the product of the D.F. for blocks and treatments as well as fitting in with the addition.

(G) By subtraction.

(H) $15 \cdot 02 = \sqrt{225 \cdot 5}$.

(I) This and the subsequent calculations are illustrative of the preference for working with totals mentioned in § 11.6.1. The S.E. of a single treatment total will be later converted into the S.E. of a single treatment mean for presentation purposes. Here $30 \cdot 04 = \sqrt{(4 \times \text{Error M.S.})} = 2 \times 15 \cdot 02$.

(J) The S.E. of the difference of two treatment totals $= \sqrt{(2 \times 4 \times \text{Error M.S.})}$. Least significant differences are calculated as explained in § 11.4.5.

(K) In field experiments it is necessary to convert the data in experimental units of measurement (here lb. per plot) to some standard units such as bushels per acre, tons per morgen, etc., as may be appropriate for the particular crop. This enables the results to be presented in a form which will make them more readily appreciated by workers with the crop against the yard-stick of their experience.

To calculate a factor for converting a treatment total in experimental units of measurement to a treatment mean in the standard units, we work as follows. Suppose that k_1 of the units of area used in the experiment = one standard unit of area, and that k_2 experimental units of weight = one standard unit of weight.

Then conversion factor

$$= \frac{1}{\text{Area of } r \text{ plots in exptl. units}} \times \frac{k_1}{k_2},$$

i.e. we express the area of land from which each treatment total has been derived in standard units, divide by this, and at the same time convert the experimental units of weight into standard units.

Here $k_1 = 1$ (since the plot size is given in acres), $k_2 = 2000$, and the area of 4 plots $= \frac{4}{85}$ acre. Hence conversion factor $= \frac{85}{4} \times \frac{1}{2000}$. To avoid inaccuracy in the conversion, a fair number of significant digits should be obtained.

It is possible, but it is not usually done, to convert all plot yields to standard units at the start and to perform the analysis on the converted yields. This method involves many more conversions, however.

The form of the analysis down to this point is the same for any randomized blocks design, except that, for an unclassified set of treatments where F has proved non-significant, it is not essential to calculate the least significant differences. The subsequent analysis and presentation of data will vary according to the nature of the treatments and the results of tests of significance.

Computation sheet (continued)

Treatment comparisons: (L)

B v. *A*	**
C v. *B*	N.S. (nearly *)
D v. *B*	N.S.
E v. *C*	* (nearly **)
F v. *E*	N.S.

Presentation of results

TREATMENT MEANS IN TONS PER ACRE

Control	7·83 (M)	S.E. = ± 0·32 (N)
N	11·42	L.S.D.'s: 0·96 (5%) (O)
NP	12·35	1·33 (1%)
NL	11·57	
NPK	13·61	
Special *NPK* mixture	13·66	
Mean	11·74	

S.E. as % of mean yield = 5·49%

(P) All fertilizer treatments have yielded significantly better than the control, the gain due to nitrogen alone being 3·59 tons per acre, significant at the 1% level. Phosphate in the presence of nitrogen has increased the yield by 0·93 tons per acre (very nearly significant at the 5% level), and the addition of potash to the nitrogen and phosphate shows a further increase of 1·26 tons per acre (very nearly significant at the 1% level). The response to lime in the presence of nitrogen is negligible, as is the difference between the *NPK* special mixture and the ordinary *NPK* application.

Notes on additional analysis

(L) The treatments in this experiment comprise, of course, a classified set, so that the over-all *F*-test is actually irrelevant and could have been omitted. Here the arrangement of treatments is such that the nomination of 5 comparisons offers no difficulties, in that it is difficult to suggest natural alternatives; the absence of any directive in this respect is therefore no handicap. (The nomination should, in fact, have been made at least prior to any inspection of the yields, and preferably when the experiment was designed.)

The comparisons are intended to test the following: (1) Nitrogen *v.* No nitrogen, (2) Phosphate in the presence of nitrogen, (3) Lime in the presence of nitrogen, (4) Potash in the presence of nitrogen and phosphate, (5) The special *NPK* mixture *v.* the ordinary *NPK* application.

The set of comparisons is obviously not orthogonal, and so there is no point in working out S.S.'s and using the *F*-test. No group comparisons are involved and the least significant differences suffice for all tests (§ 11.4.5). The methods of § 11.8 are inappropriate, because there is no question of the treatments other than control being regarded on an equal footing as possible equivalent treatments (Cf. Example 14.1, Note I.).

(M) $7{\cdot}83 = 737 \times 0{\cdot}010625$,
$11{\cdot}42 = 1075 \times 0{\cdot}010625$, etc.

(N) $0{\cdot}32 = 30{\cdot}04 \times 0{\cdot}010625$.

(O) $0{\cdot}96 = 90{\cdot}5 \times 0{\cdot}010625$,
$1{\cdot}33 = 125{\cdot}2 \times 0{\cdot}010625$.

In the calculations under Notes M, N, and O the conversion factor should be left in the machine as a constant multiplier.

(P) The presentation of the results should include a list of treatment means and the S.E. of a single treatment mean in standard units. The inclusion of the C.V. is generally desirable and inclusion of the least significant differences optional. (However, least significant differences are best omitted in the case of an unclassified set of treatments with *F* non-significant.) A brief summary of the results concludes the analysis. This should be in terms of the actual treatments applied and not in terms of *A*, *B*, *C*, *D*, etc.; thus "Nitrogen is better than Control", not "*B* is better than *A*". Contractions such as *N*, *P*, *K*, are not inadmissible for the sake of brevity. The inclusion of levels of significance in the summary is debatable, but is justifiable on account of the increased flexibility it gives; it would otherwise be a little difficult to deal with the effect of phosphate in this experiment, for example.

Sometimes the treatment means in a set such as this one are expressed as a percentage of the control, or sometimes of the general mean. This amounts to a simple proportional sum. Thus, to express means as a percentage of the control, they are multiplied here by $100/7{\cdot}83$; the same applies to the S.E. and least significant differences.

13.5 Statistical model for the randomized blocks design

13.5.1 For better understanding of the assumptions underlying the analysis, it is necessary to consider the statistical model for the design. In accordance with §§ 12.8.3 and 12.8.7, only the infinite model will be discussed. The yield, y_{ij}, for the i^{th} treatment in the j^{th} replication may be assumed made up of

$$\mu_i + \mu'_j + \epsilon_{ij},$$

where μ_i is a component of yield common to all plots of the i^{th} treatment, μ'_j is a component common to all plots in the j^{th} block, and ϵ_{ij} is N.I.D. $(0, \sigma^2)$. Clearly, there are superfluous parameters in this model since any constant can be added to μ_i provided the same constant is subtracted from μ'_j without altering the expected value of y_{ij}; any solution to the problem of estimating these parameters would therefore be indeterminate. Working on the same lines as in § 9.7.6, we arrive at the model

$$y_{ij} = \mu + \beta_j + \tau_i + \epsilon_{ij}, \qquad [13.6]$$

with $\Sigma\beta_j = 0$ and $\Sigma\tau_i = 0$, where the block effects β_j and the treatment effects τ_i are both regarded as fixed effects.

13.5.2 In this model is incorporated the assumption that each plot within a block has, on the null hypothesis ($\tau_i = 0$), the same mean, but that this mean may vary from block to block. The treatment effect is a purely additive component; not only is it independent of the magnitude of the error component, but also of β_j, i.e. the treatment effects (or relative performances of the treatments) do not vary from block to block. The former assumption of additivity is the same as was made in the simple random design, but *when we speak of the assumption of additivity in a randomized blocks design it is the constancy of the treatment effects over the different blocks which is usually implied.* In practice there is always likely to be some degree of non-additivity if the blocks differ in their average levels of fertility. Thus a treatment difference may actually show up more prominently in a block of high fertility than in one of low fertility or *vice versa*; in Model [13.6], however, treatment differences are assumed constant from block to block. Where treatment and block effects are non-additive, we say that they **interact**. The usual assumptions, that the errors are normally and independently distributed about zero mean with constant variance, are also made, with the same justification as in the simple random design. In the randomized blocks design there is the possibility that the error variance may be greater for a high-yielding block than for a low-yielding block.

13.5.3 In Model [13.6] both treatment and block effects are taken as fixed. In field experiments, at least, fixed treatment effects will usually be appropriate, but the possibility of assuming random block effects needs consideration. With fixed block effects interest is restricted only to the particular blocks in the experiment; with random block effects we would have the alternative model

$$y_{ij} = \mu + \eta_j + \tau_i + \epsilon_{ij}, \qquad [13.7]$$

where η_j is a random variate with mean zero and everything else is as before, and it would seem possible to interpret the results on the wider basis that the blocks in the experiment comprise a random sample from a hypothetical infinite population of blocks. Again, however, we are up against the problem of the verisimilitude of this hypothetical infinite population, when in a normal experiment what we actually have is a number of blocks alongside one another and arbitrarily selected. If we were to place the blocks in a number of localities with a view to achieving wide generality of our results, the position looks better, even though a proper random selection of localities would seldom be made.

13.5.4 Provided it is possible to assume additivity of block and treatment effects, it makes no difference to the analysis of variance which of the two models [13.6] or [13.7] is adopted or is appropriate. The only difference is in the interpretation, or the degree of generality attaching to the results. In view of certain difficulties when non-additivity is present, to be discussed in §13.5.5, it is advisable to see that the blocks of a field experiment do not occur on markedly different areas, even though differences between the block means will be eliminated from the Error S.S.; in other words there is no detraction from the desirability of selecting as uniform an experimental area as possible (see § 12.11.1).

*13.5.5 Should additivity not hold, the presence of interaction manifests itself as an additional component in the Error S.S. With random block and random treatment effects or with random block and fixed treatment effects this same component appears in the Treatments S.S. in such a way that the usual F-test of treatments against error is unaffected. On the other hand, where both block and treatment effects must be considered fixed, no component due to interaction appears in the Treatments S.S., and the Error M.S. is therefore no longer appropriate to test the null hypothesis $\tau_i = 0$.

13.5.6 Where blocks are placed in different localities, non-additivity is almost certain owing to the probable variation in relative performance of the treatments at the different sites; in addition, the error variance is likely to change from site to site. In these circumstances, however, contradictory as it may seem, tests of significance of the treatment effects may be of no very great use. For instance, there is little value in showing that Variety A is superior to Variety B on the average over the selected sites, if at a number of these sites the reverse is true. In the presence of non-additivity, therefore, we are forced back to the consideration of results on individual sites or groups of sites, and this means that each site should have a self-contained experiment. With estimates of error variance available at each site it is possible to study whether the error variances are homogeneous over the different sites, as would have to be assumed in the analysis of an experiment with one block at each site. The picture therefore changes from a single experiment with its blocks dispersed over a series of sites, to a series of separate experiments on different sites.

13.6 Orthogonality of the randomized blocks design

13.6.1 Mention has already been made in § 13.3 of the orthogonality of the three component S.S.'s in the analysis of variance and how it ensures that the S.S. of the block totals and the S.S. of the treatment totals represent, respectively, deviations due to block effects only and due to treatment effects only (in addition to random deviations). We say that blocks and treatments, or block effects and treatment effects, are orthogonal (to one another).

13.6.2 The randomized blocks design, since each variate-value belongs simultaneously to a particular block and a particular treatment, is called a design with two criteria of classification (treatments and blocks), as compared with the single criterion of the simple random design (treatments only). This means that the data from a randomized blocks design can be arranged in a **two-way table** such as that given symbolically in §13.3.1, or numerically at the beginning of the computation sheet of Example 13.1. The orthogonality of two criteria of classification needs further study.

13.6.3 Yates defined orthogonality as "*that property of a design which* *ensures that the different classes of effects to which the experimental material is subject shall be capable of direct and separate estimation without any entanglement*". Applying this to the estimation of treatment and block effects in a randomized blocks design, the beginner may think that "without any entanglement" refers to the fact that estimates of the treatment effects are not subject to errors arising from block differences because comparisons are made within blocks; in other words he may confuse "orthogonality" with "reduction of error".

13.6.4 In order to understand this definition we must give attention to the estimation of the block and treatment effects. Let the estimates of μ, β_j, and τ_i in Model [13.6] be denoted by m, b_j, and t_i respectively. (The notation $\hat{\mu}$, $\hat{\beta}_j$, $\hat{\tau}_i$ is also common.) Intuitively we look to the G.T., block totals, and treatment totals, respectively, to evaluate these estimates. In the notation of Table 13.1 and Formulae [13.4] and [13.5], the expected value of Y_{10} is the sum of

$$\begin{array}{ll}
\mu + \beta_1 + \tau_1 & (y_{11}) \\
\mu + \beta_2 + \tau_1 & (y_{12}) \\
\mu + \beta_3 + \tau_1 & (y_{13}) \\
\mu + \beta_4 + \tau_1 & (y_{14}) \\
\mu + \beta_5 + \tau_1 & (y_{15}) \\
\hline
= 5\mu + \sum_j \beta_j + 5\tau_1. &
\end{array} \qquad [13.8]$$

Similarly, the expected value of Y_{20} is $5\mu + \sum_j \beta_j + 5\tau_2$, and so on. To estimate the parameters, we equate the numerical values of these quantities to their expectations, i.e.

$$\left.\begin{array}{l}
5m + \sum_j b_j + 5t_1 = Y_{10}, \\
5m + \sum_j b_j + 5t_2 = Y_{20}, \text{ etc.}
\end{array}\right\} \qquad [13.9]$$

Immediately, we obtain by simple subtraction

$$5(t_1 - t_2) = Y_{10} - Y_{20}$$

or

$$t_1 - t_2 = y_{10} - y_{20}, \text{ etc.}$$

Thus the comparison of any two treatments can be made, i.e. the estimate of the difference of their effects can be computed, free from any entanglement with the block effects.

A parallel situation exists with respect to the estimation of block effects. The expected value of Y_{01} is the sum of

$$\begin{array}{ll}
\mu + \beta_1 + \tau_1 & (y_{11}) \\
\mu + \beta_1 + \tau_2 & (y_{21}) \\
\mu + \beta_1 + \tau_3 & (y_{31}) \\
\mu + \beta_1 + \tau_4 & (y_{41}) \\
\hline
= 4\mu + 4\beta_1 + \sum_i \tau_i. &
\end{array} \qquad [13.10]$$

Proceeding as before, we obtain the following equations for the estimation of the β_j:

$$\left.\begin{array}{l}
4m + 4b_1 + \sum_i t_i = Y_{01}, \\
4m + 4b_2 + \sum_i t_i = Y_{02}, \text{ etc.}
\end{array}\right\} \qquad [13.11]$$

These immediately give

$$4(b_1 - b_2) = Y_{01} - Y_{02}$$

or
$$b_1 - b_2 = y_{01} - y_{02}, \text{ etc.,}$$

and so the difference of any two block effects can be estimated free from any entanglement with treatment effects. This is what Yates means by "direct and separate estimation".

13.6.5 The immediate reason for this very desirable state of affairs lies in the nature of the randomized blocks design. Each treatment occurs once and once only in each block, and so any treatment total is subject to exactly the same block effects, since in Model [13.6] the block effects are constant for all plots within a block; this means that comparisons between treatment means or totals are unaffected by the block effects. Likewise, since each block contains each treatment once and once only, each block total is subject to the same treatment effects, so that block differences are unaffected by any treatment differences. This sort of superficial examination, i.e. to see that each member of the one classification occurs equally over all categories of the other, is all that is needed to establish the fact of the orthogonality of the two classifications.

Because of this orthogonality between blocks and treatments we may refer to the orthogonality of the randomized blocks design, or speak of it as an orthogonal design.

13.6.6 Should the orthogonality of the design be broken, e.g. by some field mishap such as a missing plot in one block, then the S.S. calculated from the block totals (even allowing for the unequal numbers of plots in blocks) will not reflect only block effects since all blocks do not contain the same treatments; it will therefore reflect a mixture of both block and treatment effects, though primarily the former (in addition, of course, to random errors) and is called the S.S. for *blocks ignoring treatments.* Similarly the S.S. calculated from the treatment totals will be a mixture of both treatment and block effects (the S.S. for *treatments ignoring blocks*), since each treatment does not come from the same blocks, and so the test of significance of treatment effects is vitiated. Also, owing to the non-orthogonality, these S.S.'s do not "add up", i.e. the S.S.'s calculated from the block totals and from the treatment totals do not sum to what the combined S.S. for these two effects should be, i.e. they do not add to the Total S.S. minus the Error S.S. The unfortunate consequence of this is that the Error S.S. cannot be obtained by subtraction after calculating the S.S.'s for blocks ignoring treatments and treatments ignoring blocks. Furthermore, just as the treatment and block effects are intermingled in the S.S.'s, so [13.9] and [13.11] are no longer such that the t's and b's can be so rapidly disentangled by such an obvious and direct method of elimination. In short, the simple analysis of variance procedure breaks down, and the analysis becomes more complicated.

13.6.7 However, although non-orthogonality complicates the estimation of the block and treatment effects, it does not affect the capacity of the design to reduce error as compared with a simple random design. In a non-orthogonal design it is still possible to obtain an Error S.S. which is orthogonal to (independent of) the non-random effects in the experiment, so that the Error S.S. contains no block or treatment effects, for example. Otherwise, of course, it would not be an Error S.S. The elimination of block effects from error is not dependent therefore on the orthogonality of the design. (See also § 13.2.3.)

13.6.8 Although we were able in § 13.6.4 to obtain estimates of differences of treatment effects or differences of block effects in the randomized blocks design, we did not actually obtain the t_i and b_j themselves. If we assemble, for example, the equations for all possible differences between estimates of treatment effects, viz.

$$\begin{aligned} t_1 - t_2 &= y_{10} - y_{20} \\ t_1 - t_3 &= y_{10} - y_{30} \\ t_1 - t_4 &= y_{10} - y_{40} \\ t_2 - t_3 &= y_{20} - y_{30} \\ t_2 - t_4 &= y_{20} - y_{40} \\ t_3 - t_4 &= y_{30} - y_{40}, \end{aligned}$$

it is apparent that the last 3 equations do not help in the solution, because the last equation, for example, can be obtained as the difference of the second and third. The maximum number of independent equations is 3, and the first 3 comprise such an independent set. In these 3 equations, however, there are 4 unknown t's, which means that the equations are indeterminate—there is any number of solutions. To obtain a single solution it is necessary to apply some linear restriction to the t's. In view of the restriction selected for the parameters themselves ($\Sigma\tau_i = 0$), it is natural to choose the same restriction for their estimates, viz. $\Sigma t_i = 0$. Similarly for the b_j. The values of m, t_i, and b_j will then be estimates of μ, τ_i, and β_j under the restrictions $\Sigma\tau_i = \Sigma\beta_j = 0$, but it must be remembered that these latter restrictions are themselves arbitrarily imposed and could be changed. Addition of the first 3 equations gives

$$3t_1 - t_2 - t_3 - t_4 = 3y_{10} - y_{20} - y_{30} - y_{40},$$

or

$$4t_1 - \Sigma t_i = 4y_{10} - \Sigma y_{i0},$$

i.e.

$$4t_1 = 4y_{10} - 4\bar{y},$$

i.e.

$$t_1 = y_{10} - \bar{y}.$$

Similar solutions are obtained for the other t's, and in general $t_i = y_{i0} - \bar{y}$, which is the estimate of τ_i. In an exactly similar way the b_j may be obtained as $y_{0j} - \bar{y}$ on adoption of the restriction $\Sigma b_j = 0$, and these are the estimates of the β_j.

13.6.9 It remains to evaluate m, the estimate of μ. For this we turn to the grand total (Y) or to the general mean of the experiment ($\bar{y}$). In our little example, the G.T. is an estimate of

$$20\mu + 5\,\Sigma\tau + 4\,\Sigma\beta.$$

For estimation of μ we have the equation

$$20m + 5\,\Sigma t + 4\,\Sigma b = Y, \qquad [13.12]$$

from which, if $\Sigma t = \Sigma b = 0$, we obtain

$$m = \tfrac{1}{20} Y = \bar{y}.$$

The set of $r + t + 1$ (here 10) equations [13.9], [13.11], and [13.12] is referred to as the **normal equations** for the design. The estimates of the parameters obtained from these equations are such that the sum of squared residuals

$$\sum_i \sum_j (y_{ij} - m - t_i - b_j)^2 \qquad [13.13]$$

is a minimum. The method of evaluating m, t_i, and b_j is known as "**fitting constants**", and the criterion used for **goodness of fit** is that the sum of squared residuals is a minimum, i.e., if we substitute any other values for m, t_i, and b_j than those obtained from the normal equations, the sum of squared residuals will be greater. This is again the principle of least squares, and in this sense the solution to the normal equations provides the best fitting constants, i.e. the best estimates of the parameters μ, τ_i, and β_j. The normal equations may actually be derived mathematically starting from the criterion that $\sum_i \sum_j (y_{ij} - m - t_i - b_j)^2$ shall be a minimum, instead of by the intuitive approach we have used here.

Since there are $r + t + 1$ normal equations for the estimation of only $(r - 1) + (t - 1) + 1 = r + t - 1$ linearly independent parameters (in view of the linear restrictions imposed on the τ_i and β_j), it would seem that we have more equations than unknowns. Actually, however, the sum of all the equations [13.9] gives [13.12], as does the sum of all the equations [13.11]. There are therefore only $r + t - 1$ independent equations.

13.6.10 If we immediately put $\Sigma t_i = \Sigma b_j = 0$ in the normal equations, they reduce to:

$$\left.\begin{aligned} 20m &= Y \\ 5m + 5t_i &= Y_{i0} \text{ (4 equations)} \\ 4m + 4b_j &= Y_{0j} \text{ (5 equations)} \end{aligned}\right\} \qquad [13.14]$$

From these we may obtain $m = \bar{y}$, $t_i = y_{i0} - \bar{y}$, and $b_j = y_{0j} - \bar{y}$, even more directly than above. The orthogonality of the design ensures that for a suitable choice of linear restrictions for the t_i and b_j the normal equations will reduce to a set in which the equation for any one constant does not involve the others, i.e. there are no simultaneous equations to solve. It is true that the equations for t_i and b_j involve m, but then m is given directly from the first equation. In a non-orthogonal design the evaluation of the constants always involves the solution of a number of simultaneous equations.

Consideration of Equations [13.14], as compared with [13.9] and [13.11], might suggest that the orthogonality of blocks and treatments depends on the fact that we adopt particular linear restrictions $\Sigma t_i = \Sigma b_j = 0$. This is not true, however. The real reason for the orthogonality is as was explained in §§ 13.6.4 and 13.6.5. The choice of $\Sigma t_i = \Sigma b_j = 0$ does, however, ensure the orthogonality of m to the other constants. It also ensures that $m = \bar{y}$ and so is an estimate of μ, the hypothetical true mean of all plots in the experiment.

13.6.11 It is of interest to relate our new ideas on orthogonality with those discussed in Chapter 11. From one point of view Yates's definition of § 13.6.3 does not cover the orthogonality of treatment comparisons, because this depends on the particular set of treatments being tested and it is useful to preserve a distinction between *design* and *arrangement of treatments* within a design. It is true that the decision as to which treatments shall be tested in an experiment is one in which the biometrician must assist and that this is commonly referred to as part of the process of designing the experiment. Nevertheless, whatever the arrangement of treatments in a randomized blocks design, it is still a randomized blocks and not some other type of design, and it may be better to think of the arrangement of treatments in a design as part of the planning, rather than the designing, of an experiment.

The orthogonality of treatment comparisons and the orthogonality of a design in the sense explained in this chapter are, however, essentially the same. In both cases a S.S. is being orthogonally subdivided—in the one case the Treatments S.S., in the other the Total S.S.

*13.6.12 To illustrate this further, let us for convenience study a small randomized blocks design with 3 replications of 4 treatments, the yields from which are

$$y_{11} \quad y_{12} \quad y_{13} \;\vdots\; y_{21} \quad y_{22} \quad y_{23} \;\vdots\; y_{31} \quad y_{32} \quad y_{33} \;\vdots\; y_{41} \quad y_{42} \quad y_{43},$$

the vector of yields, which we may denote simply as $\boldsymbol{y}$. The general mean is in vector notation

$$\bar{y} = \left[\begin{array}{ccc|ccc|ccc|ccc} \frac{1}{12} & \frac{1}{12} & \frac{1}{12} & \frac{1}{12} & \frac{1}{12} & \frac{1}{12} & \frac{1}{12} & \frac{1}{12} & \frac{1}{12} & \frac{1}{12} & \frac{1}{12} & \frac{1}{12} \end{array}\right]\boldsymbol{y},$$

(The partitioning lines are purely to help the eye and divide the yields of the 4 treatments from one another.) The mean of a treatment, say the first, is

$$y_{10} = \left[\begin{array}{ccc|ccc|ccc|ccc} \frac{1}{3} & \frac{1}{3} & \frac{1}{3} & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \end{array}\right]\boldsymbol{y}.$$

The mean of a block, say the first, is

$$y_{01} = \left[\begin{array}{ccc|ccc|ccc|ccc} \frac{1}{4} & 0 & 0 & \frac{1}{4} & 0 & 0 & \frac{1}{4} & 0 & 0 & \frac{1}{4} & 0 & 0 \end{array}\right]\boldsymbol{y}.$$

Hence, subtracting corresponding coefficients, we obtain

$$y_{10} - \bar{y} = \left[\begin{array}{ccc|ccc|ccc|ccc} \frac{1}{4} & \frac{1}{4} & \frac{1}{4} & -\frac{1}{12} & -\frac{1}{12} & -\frac{1}{12} & -\frac{1}{12} & -\frac{1}{12} & -\frac{1}{12} & -\frac{1}{12} & -\frac{1}{12} & -\frac{1}{12} \end{array}\right]\boldsymbol{y}$$

and

$$y_{01} - \bar{y} = \left[\begin{array}{ccc|ccc|ccc|ccc} \frac{1}{6} & -\frac{1}{12} & -\frac{1}{12} & \frac{1}{6} & -\frac{1}{12} & -\frac{1}{12} & \frac{1}{6} & -\frac{1}{12} & -\frac{1}{12} & \frac{1}{6} & -\frac{1}{12} & -\frac{1}{12} \end{array}\right]\boldsymbol{y}.$$

Both these vectors have coefficients summing to zero and are obviously orthogonal, from which it follows that the respective S.S.'s, $r\sum_i(y_{i0} - \bar{y})^2$ and $t\sum_j(y_{0j} - \bar{y})^2$, are orthogonal, and that treatment effects are orthogonal to block effects.

13.7 Direct calculation of the residual S.S.

13.7.1 If in the sum of squared residuals [13.13], we substitute the symbolic values of the constants found by solving the normal equations, we have

$$\begin{aligned}
&\sum_i \sum_j (y_{ij} - m - t_i - b_j)^2 \\
&= \sum_i \sum_j \{y_{ij} - \bar{y} - (y_{i0} - \bar{y}) - (y_{0j} - \bar{y})\}^2 \\
&= \sum_i \sum_j (y_{ij} - y_{i0} - y_{0j} + \bar{y})^2,
\end{aligned}$$

the Error S.S. of [13.2]. The analysis of variance, therefore, is such that the Error S.S. is a minimum in the sense implied in § 13.6.9, and this is true of any design. The sum of squared residuals or the Error S.S. represents the remaining variation in the yields when the effects represented by m, t_i, and b_j, have been accounted for, i.e. it represents variability not under the control of the experimenter, or random error. Any error residual,

$$e_{ij} = y_{ij} - y_{i0} - y_{0j} + \bar{y} = y_{ij} - (m + t_i + b_j),$$

is the deviation of the actual plot yield y_{ij} from its expected value as estimated from the data in accordance with the model. Equation [13.3] earlier also demonstrates this.

13.7.2 It is of interest to work out the numerical values of the error residuals for Example 13.1. Table 13.3 shows for each plot the actual yield, the expected yield as estimated from the data, and the difference of these, the error residual.

Table 13.3: Error residuals in a randomized blocks design

Block	Treatment						Totals	y_{0j}	$y_{0j} - \bar{y}$
	A	B	C	D	E	F			
1	177	286	311	280	312	331	1697	282·83	6·58
	190·83	275·33	297·08	278·83	326·83	328·08			
	−13·83	10·67	13·92	1·17	−14·83	2·92	0·02		
2	185	278	294	292	322	323	1694	282·33	6·08
	190·33	274·83	296·58	278·33	326·33	327·58			
	−5·33	3·17	−2·58	13·67	−4·33	−4·58	0·02		
3	182	258	266	284	319	313	1622	270·33	−5·92
	178·33	262·83	284·58	266·33	314·33	315·58			
	3·67	−4·83	−18·58	17·67	4·67	−2·58	0·02		
4	193	253	291	233	328	319	1617	269·50	−6·75
	177·50	262·00	283·75	265·50	313·50	314·75			
	15·50	−9·00	7·25	−32·50	14·50	4·25	0·00		
Totals	737	1075	1162	1089	1281	1286	6630	—	−0·01
$\sum_i e_{ij}$	0·01	0·01	0·01	0·01	0·01	0·01	0·06		
y_{i0}	184·25	268·75	290·50	272·25	320·25	321·50	—	276·25	—
$y_{i0} - \bar{y}$	−92·00	−7·50	14·25	−4·00	44·00	45·25	0·00	—	—

To exemplify the construction of this table, the expected yield for Treatment A in Block 1

$$\begin{aligned}
&= m + t_A + b_1 \\
&= \bar{y} + (y_{A0} - \bar{y}) + (y_{01} - \bar{y}) \\
&= 276{\cdot}25 + (-92{\cdot}00) + 6{\cdot}58 \\
&= 190{\cdot}83.
\end{aligned}$$

The corresponding error residual is therefore $177 - 190{\cdot}83 = -13{\cdot}83$.

The additivity assumptions on which the statistical model are based should be clear as a result of this numerical illustration. In data where a non-additive model would be appropriate, the residuals e_{ij} would represent non-random (combined with random) deviations from the expected yields under the additive scheme. Otherwise they are random or error deviations.

13.7.3 It is easily proved that the residuals sum to zero over all plots and also over any one treatment or any one block, i.e. $\sum_i \sum_j e_{ij} = \sum_i e_{ij} = \sum_j e_{ij} = 0$. For example,

$$\begin{aligned} &\sum_i \sum_j (y_{ij} - y_{i0} - y_{0j} + \bar{y}) \\ &= \sum_i \sum_j [y_{ij} - \bar{y} - (y_{i0} - \bar{y}) - (y_{0j} - \bar{y})] \\ &= \sum_i \sum_j (y_{ij} - \bar{y}) - r \sum_i (y_{i0} - \bar{y}) - t \sum_j (y_{0j} - \bar{y}), \end{aligned}$$

and all three terms in this are zero by Theorem 5.9. The small discrepancies in Table 13.3 are due to rounding errors. In the light of this it is clear that, if we were allotting values of e_{ij} arbitrarily to the various plots, only the plots in (say) the first 3 rows and the first 5 columns could be so dealt with. Because the e_{ij} in each row and in each column have to sum to zero, the remaining e_{ij} can be filled in by subtraction (apart from the rounding errors). In other words there are $5 \times 3 = 15$ D.F. for error in this particular design, in consequence of the fact that $4 + 6 - 1 = 9$ linearly independent parameters (cf. § 13.6.9) have been estimated from the 24 plot yields.

13.7.4 The S.S. of the error residuals $\sum_i \sum_j e_{ij}^2$ may be computed and works out as 3381·5, the discrepancy being due to rounding errors. It can now be appreciated how much more convenient it is to calculate the Error S.S. by subtraction!

13.8 Randomization of a randomized blocks design

13.8.1 The usual method is to consider each plot in turn and allot a treatment number to it at random, with the restriction that the same treatment number must not occur more than once in a block. Pairs of random numbers are drawn, each being divided by the number of treatments, to give a series of remainders corresponding to random treatment numbers.

Suppose we have 6 treatments *1, 2, 3, 4, 5, 6*, or we might have called them *A, B, C, D, E, F*, in which case we call *A 1*, *B 2*, and so on. Start at random in a table of random numbers and divide each pair by 6, noting remainders and skipping numbers 96, 97, 98, 99, to avoid bias (cf. § 3.13.6). If the first remainder is, say, 3, we can interpret this as that Treatment *1* takes up position 3 in the block, or we might equally well say that Treatment *3* occupies position 1. Let us adopt the latter interpretation, and suppose that the random numbers scanned are 69, 26, 69, 82, 89, 15, 87, 16, 59, 22, 40, 66, 35, 84, 57, 54, 30, etc.; 69 gives 3, 26 gives 2, 69 repeats 3, 82 gives 4, 89 gives 5, 15 repeats 3, as does 87, 16 repeats 4, 59 repeats 5, 22 repeats 4, as does 40, 66 gives 6, so that the last place in the block is occupied by Treatment *1*, and the order for the first block is 3, 2, 4, 5, 6, 1. We may then start the second block, which will be 5, 6, 3, etc.... There is no need to randomize the order of the blocks so drawn.

13.8.2 Random permutations of numbers have been prepared which save a good deal of the labour of the above process. For example, there are tables of random permutations of numbers from 1 to 9, which can be used as the treatment order in the blocks of a design with 9 treatments. For designs with fewer than 9 treatments unwanted numbers are discarded. Naturally a random start must be made in these tables. Tables for 9 and 16 numbers appear in Cochran and Cox and for 9 and 20 numbers in Fisher and Yates. For a long time there was a considerable resistance against the publication of such tables on the grounds that the randomization process would not be able to give rise to all possible randomizations, but only to the limited set published in the tables. Their publication is evidence of some departure from the strict standpoint of § 12.8.2, but where a large series of similar designs is being considered, it would be advisable to use the method of § 13.8.1.

13.8.3 It must be carefully noted that each block must be independently randomized. It might be thought that the number of replications in a randomized blocks design could be doubled by dividing each plot into two and harvesting each half separately. Of course, there is bound to be a "catch" somewhere in such a simple method of "doubling" the replications; the "catch" is that the "extra" replications have the same randomization as the original, and hence cannot validly be considered as additional replications.

13.9 Field lay-out of the randomized blocks design

13.9.1 It is useful here to preserve a distinction between lay-out and design. Considerations of lay-out are obviously part of the design of an experiment in the wider sense of that term, perhaps better indicated as the planning of an experiment, but there are many possible different lay-outs corresponding to a single design.

13.9.2 An obvious objective in any randomized blocks design is to see that the plots within a block vary in fertility as little as possible, because it is this variation which will be the main contributor to experimental error. If a fertility gradient is present, the direction of which is known, it would in theory be sufficient to have long narrow plots running right across the block parallel to the gradient (§ 12.11.8). In that case the shape of the blocks is immaterial, except to this extent that, if the block is itself long and narrow and has its longer side parallel to the gradient, the plots will tend to be excessively long and narrow unless a plot such as that mentioned in § 12.11.10 is required.

13.9.3 Where, as commonly, there are only hazy ideas about the direction of possible fertility gradients, it is advisable to avoid long narrow blocks on the grounds that, if the fertility gradient happens to be parallel to the longer side of the block, the variability of plots within the block will be considerable, *unless the plots themselves run lengthwise through the block*, which, as seen in § 13.9.2, may not be desirable. In Figure 13.2, for example, if the fertility gradient is in the direction of arrow (1), there will be considerable variability

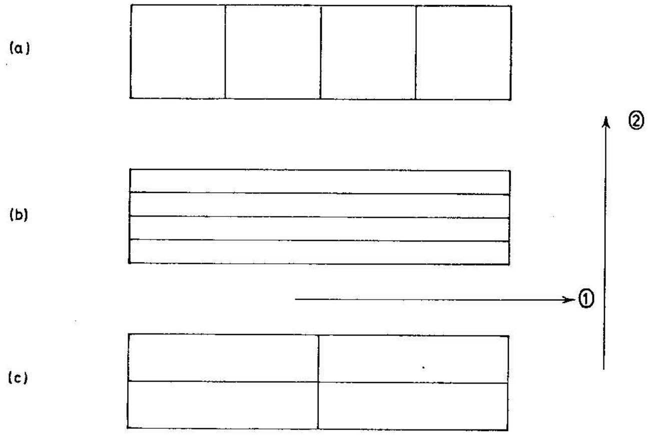

Figure 13.2: Arrangement of 4 plots in a long narrow block.

due to differences in soil fertility between the plots of diagram (*a*), but very little between those of diagram (*b*). On the other hand, if the direction of the gradient corresponds to arrow (2), the variability between the plots of diagram (*a*) will be negligible, whereas there will be much greater variability between the plots of diagram (*b*). However, owing to the small width of the block, the variability in the latter case will not be so great as in diagram (*a*) with arrow (1). In diagram (*c*), where the plots are perhaps more normal in shape, the situation is intermediate between the other two, but, if the fertility gradient is in the direction of arrow (1), it is clear that the error will be fairly high, and to avoid this possibility it is preferable that squarish or compact blocks be used so that the dependence on a successful guess at the direction of the fertility gradient for obtaining a moderate error is largely avoided.

13.9.4 The actual shape of block adopted must depend to some extent on the shape of plot, which in turn depends on all the considerations discussed in § 12.11. There is also sometimes the question of fitting a design into a limited area of a given shape.

13.9.5 *The shape of block and the arrangement of plots within a block should be identical for all blocks*; it then follows from § 12.11.2 *that all blocks should be similarly oriented*, especially if they do happen to be oblong. The reason here is similar to before, namely that the variation of plots within a block, assuming a consistent pattern of fertility over the experimental area, could differ markedly if an oblong block were to be changed in shape without altering the orientation of plots by means of a different arrangement of plots within the block, or if the block were to be turned through an angle. The latter possibility is already prohibited by the necessity for similar orientation of plots, but may be accepted in circumstances where the experiment is sited on contoured land as explained in § 12.11.11.

13.9.6 *Once the shape of plots, internal arrangement of plots within a block, and the orientation of the blocks have been decided upon, it makes no difference* *to the experimental error how the blocks are placed on the ground, provided we assume a consistent fertility pattern over the area.* Thus the lay-out (*a*) of Figure 13.3 will be approximately equal in efficiency to lay-out (*b*) whatever the direction of the fertility gradient.

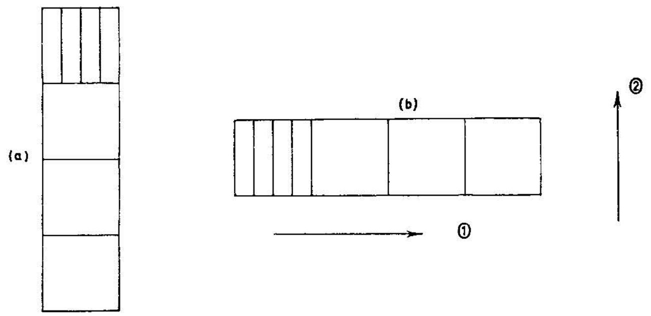

Figure 13.3: Randomized blocks lay-outs with different arrangements of similarly oriented blocks.

It is sometimes stated that the objective in laying out a randomized blocks design is to make the blocks as different from one another in average fertility as possible. This argument is correct if we are concerned with a set of plots already decided upon for the experiment, for then obviously the greater we can make the Blocks S.S., the smaller will be the Error M.S. Otherwise this approach is misleading, and in any case it ignores the remarks of § 13.5.4. As already explained, the error of a randomized blocks design depends on the internal arrangement of plots within a block in relation to the fertility pattern of the area, and is fixed for a given orientation. In Figure 13.3, if the fertility gradient is in the direction of arrow (1), the blocks in diagram (*a*) will be at more or less the same fertility level, whereas those in diagram (*b*) will differ considerably; hence the Blocks S.S. for diagram (*a*) will be much less than for diagram (*b*). Nevertheless the Error M.S.'s should be approximately the same for the two lay-outs, the reason being that the plots in diagram (*b*) vary more *over the whole lay-out* than those of diagram (*a*), and it is this extra variability which is eliminated by the blocks. Within the blocks the variability is identical. With the fertility gradient in the direction of arrow (2), the position is, of course, reversed.

In actual fact, for reasons set out in § 13.5.4, it is desirable for the blocks to differ as little as possible, so that, with the fertility gradient in the direction of arrow (1), lay-out (*a*) would be preferable for this reason. Usually, however, little notice is taken of this if a uniform area has been selected for the experiment, and in any case the direction of the fertility gradient will probably be unknown.

13.9.7 For designs with oblong plots running across the blocks as in Figure 13.3, there is one consideration, however, which may affect the choice of lay-out (*a*) or (*b*), and this is that the lay-out in diagram (*b*) or in Figure 13.1 is as explained in § 12.11.10 much more convenient where machinery is to be used.

13.10 Comparison with the simple random design

13.10.1 Although the two lay-outs of Figure 13.3 are of equal efficiency for any given direction of fertility gradient, their *efficiency relative to a simple random design on the same plots* does vary. When the gradient is in the direction of arrow (1), for example, the plots in lay-out (*b*) differ considerably in fertility, so that a simple random design on the same plots would have been much less efficient than the randomized blocks design. The blocks in lay-out (*a*), however, are of more or less equal fertility and a simple random design on the same plots would have been equally efficient. It follows that the efficiency of a randomized blocks design relative to a simple random design on the same plots can vary from equal efficiency to much greater efficiency according as the blocks happen to remove effects of soil heterogeneity from error or not.

13.10.2 Clearly, therefore, *the randomized blocks design can never in theory have a higher error than a simple random design on the same plots*, though in practice due to sampling fluctuations this can happen to a slight extent. It is also clear that, if fertility gradients had characteristics similar to arrows and could be accurately predicted, there would be no reason to prefer a randomized blocks design. Faced with the situation where there will probably be a gradient but of unknown or uncertain direction, the experimenter does not take a chance by adopting a simple random design when a randomized blocks design will almost certainly have a lower error and when the error with a simple random design could be excessively high. By using a randomized blocks design he is guarding against this extreme eventuality and is making sure of keeping the S.E. of his experiment within reasonable bounds. In short, the randomized blocks design is an insurance against unfavourable fertility gradients which might cause the error of a simple random design to be very large. It is for this reason that actually no very great attention is usually paid to the arrangement of plots within a block as discussed in §§ 13.9.2 and 13.9.3, since the error will be kept within reasonable bounds in any case; thus the consideration mentioned in § 13.9.7 would probably take priority over an alternative arrangement which might give a somewhat lower S.E.

13.10.3 The premium to be paid for this insurance policy is, among other things, the loss of D.F. from error to blocks. This means that, in the theoretical case where the error variances of a randomized blocks design and a simple random design on the same plots are equal, the efficiency of the randomized blocks design is actually less because of this loss. In a design of any size, however, the loss of D.F. from error is usually trivial and is, in any case, almost invariably more than counterbalanced by the greatly reduced error.

13.10.4 For any given randomized blocks design it is possible to calculate what the experimental error would have been without the blocks, i.e. with a simple random design on the same plots. Unless, however, the use of a simple random design is contemplated, which is very unlikely, this calculation is of only theoretical interest. Similarly, the success of a randomized blocks design should not be gauged in relation to the corresponding simple random design unless the use of the latter was seriously contemplated. It is quite pointless to gloat over a large Blocks M.S. (compared to the Error M.S.), because what the error would have been with a simple random design is ordinarily irrelevant. It is not necessary to have a fire to establish the value of insurance!

13.10.5 Normally a field experiment should be laid down in a single day and harvested in a single day. Failure to do this may render these operations liable to a long delay due to the intervention of bad weather conditions, and this may cause serious damage to the experiment. For this reason it may be inadvisable to lay down or harvest the plots of each treatment in turn, even though this procedure is especially convenient when laying down the experiment. If some treatments were laid down and then rain intervened, the remaining treatments would be laid down under different conditions and this could upset the experiment.

There are, of course, a number of obvious exceptions to the above. For example, if one treatment is early planting and another late planting, the experiment cannot be laid down on a single day; similarly at harvest, if the treatments mature at different times, it might prejudice the experiment to harvest them all simultaneously.

In the randomized blocks design there does exist up to a point the possibility of completing some blocks on one day, some on another, on the grounds that differences between blocks are eliminated. In a simple random design, provided the laying down is not done treatment by treatment, differences due to this cause would contribute to error. However, the possibility of an interaction between treatments and blocks cannot be ignored, and this idea must not be carried too far. In any case it is more convenient as a rule to lay down all the plots of one treatment before starting another, especially when a drill is used. At harvest it is usually better to work block by block. If the whole experiment cannot be harvested in a single day, then be sure to stop work only after completing a block.

13.10.6 The randomized blocks design is one step in complexity above the simple random design. Thus, when it is necessary to examine the assumption of constant variance in a simple random design, an estimate of variance can be obtained for each treatment separately by simple methods, but this cannot be done in a randomized blocks design. Mishaps to the plot yields usually upset the orthogonality of a randomized blocks design so that the ordinary analysis of variance procedure breaks down. An exception to this occurs when a whole treatment is missing, such as when it fails completely or is unable to be applied for some reason. In this case the analysis is performed as usual with the missing treatment omitted. A mistake in the field such as mixing up the treatments of two plots *within a block* affects only the randomization and is immaterial (cf. § 12.8.10); when, however, a treatment is applied twice in one block and not at all in another, then the orthogonality is upset. Of course, the data from the blocks concerned could be rejected and the analysis carried out as usual on the remaining blocks, but this means the sacrifice of much information which can be extracted only by means of a more complex analysis.

The analysis of variance procedure is affected only in respect of minor details when mishaps occur in a simple random design.

13.11 The randomized blocks design in other than field experiments

13.11.1 Although we have discussed the randomized blocks design in relation to field experiments, its application is quite general. Whenever the experimental material can be divided into groups homogeneous within themselves, but differing from group to group, such groups may serve as blocks. In animal experiments, for example, we might take litters as groups in the expectation that differences within litters would be less than those between litters. In that case we would seem to be limited in the number of treatments by the minimum size of the litters available. With cattle, even twins are comparatively rare, triplets almost impossible. In any case we usually try to find identical twins, since non-identical twins are not so much more homogeneous than calves from different mothers.

13.11.2 This difficulty of the limitation of the number of treatments by the size of the blocks available can sometimes be met by a special type of alternative design. Another possibility is to make up blocks consisting of unrelated experimental units which are alike, or thought to be alike, in respect of some quality or factor which may influence yield. In milking experiments with dairy cattle, for example, it is probable that animals grouped as being alike on the basis of uniformity yields (which can easily be taken for a few weeks before the experiment commences) would also be alike under uniform treatment during the experiment itself, and will therefore form effective blocks. The basis of the blocking may even be a rather crude classification such as weight, if it were thought that weight might influence yield. There is very little to be lost—only a few D.F. for error—and there are prospects of a reduced error if the blocking proves effective. The factor on which the blocking is based will, of course, vary according to the nature of the experiment. Although initial weights would be perhaps a dubious prospect in respect of milk yields of dairy cattle, they would probably be excellent for an experiment in which weight gains of cattle were of interest. Blocks formed on the basis mentioned in this paragraph are sometimes referred to as "outcome groups" in animal experiments, but the term blocks, although originating in field experiments, has been adopted as a general term.

13.11.3 Nor are randomized blocks designs restricted to agricultural experimentation. Suppose it is intended to test certain treatments on an industrial product which is manufactured in batches which show more variation from batch to batch than within batches. The extra variation between batches can be eliminated from the experiment by using the batches as blocks. It follows that in these circumstances we would be much more likely to think in terms of Model [13.7] in view of the fact that we would probably be interested in a general result rather than one obtained for a few particular batches and that it is perfectly feasible to visualize an infinite population of batches in this case. Also the variation between batches may itself be of interest in the light of the desirability of maximum uniformity of production.

13.11.4 Example 9.6 was actually, of course, a randomized blocks design. "Student's" method of paired differences is the special case of a randomized blocks design with 2 treatments only. The remarks of § 10.4.4 in respect of the equality of variances and independence of errors therefore apply to the randomized blocks design for this special case only.

*13.12 Variations of the randomized blocks design

13.12.1 Some variations of the basic design occur which can be called designs in randomized blocks, but it is better to think of *the* randomized blocks design as only the design discussed hitherto.

13.12.2 Perhaps the commonest variation is where each treatment occurs twice in a single block. In cases where it is suspected that interaction between blocks and treatments will occur, the repetition of the treatments within a block provides an estimate of pure error against which this interaction can be tested. Differences between units receiving the same treatment within a block can be due only to chance error, and it is from such differences that we can estimate the pure error variance. The disadvantage of this design is that the block size is double that of the ordinary design, which will probably mean a greater error due to the increased variability within the larger blocks. The orthogonality of blocks and treatments still holds good, since each treatment still occurs an equal number of times in each block.

13.12.3 As an example we may consider an experiment with cattle in which it has been decided to use different breeds as blocks. It is quite likely that treatment differences will vary for the different breeds, i.e. that there may be an interaction between blocks (breeds) and treatments. If so, we would be interested in such an interaction, and to assess its importance we could place two animals in each block on the same treatment. Full appreciation of the ideas behind this type of design will scarcely be possible until the study of factorial experiments in Chapter 19.

13.13 Usefulness and limitations of the randomized blocks design

13.13.1 The randomized blocks design is probably the most commonly used experimental design. Certainly, if we include all the possible variations on the simple theme of using blocks as a form of local control, which is the basis of so many designs, there is no question about it.

13.13.2 The randomized blocks design nevertheless has a certain limitation to its usefulness. If the number of treatments is large, the size of block has to be large and the result may be that the variability of the experimental units within a block is so great that the blocks are not exerting a sufficiently tight control over experimental error. This is certainly true in field experiments, where a block size of about 16 plots is looked upon as a reasonable maximum. Notice that block size refers to the number of plots per block. Within limits physical size of block (for a given number of plots per block) is immaterial, because the larger the plot the lower the C.V. (§ 12.11.4).

13.13.3 The solution to this limitation, which can be a serious matter (for example, to a plant breeder who may have some hundreds of crosses to test), lies in the formulation of **incomplete block designs**, special arrangements in which not all treatments occur in each block. Only one special case of such a design will be discussed in this book (Chapter 22).

EXERCISES

13.1 Analyse the following yields of grain in gm. per 16-foot row (each row 7 in. apart) in an experiment with 8 varieties of oats.

	Blocks				
Variety	1	2	3	4	5
1	296	357	340	331	348
2	402	390	431	340	320
3	437	334	426	320	296
4	303	319	310	260	242
5	469	405	442	487	394
6	345	342	358	300	308
7	324	339	357	352	220
8	488	374	401	338	320

Present the results in lb. per acre given that 1 lb. = 453·59 gms. and 1 acre = 4,840 sq. yds. (N.B.: The plot width is 7 in. Competition effects are assumed negligible.)

13.2 The yields of teff in gms. of dry material per pot in an experiment carried out at the Natal Agricultural Research Institute were as under (W. J. Fölscher's data):

	Blocks			
Treatment	1	2	3	4
Control	78·0	63·5	68·0	76·0
S_1	95·0	73·5	78·0	70·0
S_2	84·5	88·0	81·0	86·5
R_1	65·0	77·5	74·0	60·5
M_1	77·0	69·5	71·0	65·0
$(S + L)_1$	90·0	88·0	93·0	82·0

$$\left.\begin{aligned} S_1 &= \text{superphosphate} \\ R_1 &= \text{rock phosphate} \\ M_1 &= \text{metaphosphate} \end{aligned}\right\} \text{applied at 300 lb. per acre just prior to sowing}$$

S_2 = super at 300 lb. per acre applied 1 month before sowing

$(S + L)_1 = S_1 + 2$ tons lime per acre.

Analyse the data.

13.3 The following are the plan and yields from a trial with 10 strains of carrots conducted at Ukulinga Experiment Station in 1953. Strains 1–5 were selections of the variety Chantenay, and strains 6–10 selections of the variety Cape Market. Analyse the data, presenting results in tons per morgen given that the yields are in lb. per plot of 18 in. × 22 ft., and that 1 ton = 2,000 lb., 1 morgen = 10,244 sq. yd.

Block 1

10	*9*	*7*	*6*	*2*	*4*	*5*	*1*	*8*	*3*
27·7	36·7	32·6	30·6	33·4	22·2	30·2	30·0	30·1	32·9

Block 2

9	*5*	*4*	*1*	*10*	*6*	*2*	*3*	*7*	*8*
35·5	33·0	25·2	28·0	34·3	30·0	29·5	29·0	31·7	29·7

(Blocks 3 and 4 overleaf)

Block 3

7	*3*	*2*	*5*	*9*	*4*	*1*	*8*	*10*	*6*
30·2	31·2	31·9	30·1	35·7	24·8	28·3	27·6	31·7	28·5

Block 4

1	*6*	*4*	*9*	*2*	*8*	*3*	*5*	*7*	*10*
31·8	31·8	22·3	32·4	29·8	29·5	25·8	27·8	30·8	27·7

(Data from Department of Horticultural Science, University of Natal.)

A FIRST COURSE IN BIOMETRY FOR AGRICULTURE STUDENTS.

A.A. RAYNER.

The following errata in addition to those on the printed slip, have been noticed during one year's use of the book as a class text:-

Page 115: In Table 7.1 the entry for N = 4, x = 3 should be $\frac{4}{16}$ ($= \frac{1}{4}$) not $\frac{1}{8}$.

Page 119: In Formula 7.2 the last term should be $x^3/3!$, not $x^2/3!$.

Page 197: Note H: For 1025.3/64 read 1052.3/64.

Page 248: (4 lines under the analysis of variance table): For 225.2 read 225.5

Page 293: (6 lines from foot of page): For "than" read "that".

Page 299: (first line of Example 15.5): For "Example 9.4" read "Example 15.4".

Page 300: The second P(10) should be P(11).

Page 308: Example 15.10: Some rounding errors occur in the last 6 entries in the χ^2 column:

For	0.17	read	0.18
	0.05		0.06
	6.54		6.56
	3.08		3.10

Page 309: Line 2: The P value for Heterogeneity should read: "$0.98 > P > 0.95$".

Page 315: Note H: For "souce" read "source".

Page 340: Note B: Delete the second and third lines of this note and replace by the following: "The C.F. for the S.P. (101.3256) must lie between the C.F.'s for the two S.S.'s (256.2560 and 40.0649). Also the uncorrected S.P. (103.5921) usually (i.e. when the variate-values are all positive and $\bar{x}_1$ and $\bar{x}_2$ are are reasonably different) lies between the two uncorrected S.S.'s (262.5634 and 40.9593), as here. Gross errors ......".

Page 342: The entries in the last five lines of the table have in the printing got out of line with the columns higher up. The "0" in the ξ_1 line should be under the "141" in the n_1^* line, etc.

Page 367: (lines 10, 12 and 13): For 0.25919 read 0.25906.
(line 10): For 7751.29 read 7755.29.
(line 13): For 44.887 read 44.896

Page 394: Figure 17.6: x-axis should be calibrated 0, 50, 100, 150, 200, 250.

Page 411: (Example 18.2): In the table Linear effect of phosphate, the totals for X_{i0} and Y_{i0} should be interchanged, viz. 1442.5 for X_{i0}, and 1047.5 for Y_{i0}.

Page 504: Line 16: For "litte" read "little".

Page 534: Just above analysis of variance table: The calculation for [NK]' should read "$-18 + 150 - 148 = -16$".

Page 541: Formula 23.7: The denominator of the second term in the curly brackets should be $r(r - 1)(t - 1)$.

I am grateful to the many sharp-eyed students this year who detected errors. I should also be grateful to any reader who detects any further errors.

A.A. RAYNER.
29/10/1969.