Chapter 4

Blocking

4.1 Types of block

If the plots are not all reasonably similar, we should group them into *blocks* in such a way that plots within each block are alike.

There are three main types of block.

4.1.1 Natural discrete divisions

These divisions in the experimental units are already present. If the experimental units are new-born animals then litters make natural blocks. In an experiment on people or animals, the two sexes make obvious blocks. In testing tags on cows' ears, the ears are the experimental units and cows are the blocks. In an industrial process, a block could be a batch of chemical or ore used for part of the process.

Sometimes there is more than one type of natural discrete block. If the experimental units are half-leaves of tobacco plants then whole leaves make one sort of block while the plants make another. In a consumer experiment, such as Example 1.9, testers and weeks are both natural blocks. In an experiment in the laboratory, technicians, benches and days may all be blocks.

If an experiment is carried out on plots that had previously been used for another experiments then you should consider whether to deem the previous treatments to be blocks. This is because the previous treatments may have left some residue that may affect the responses in the new experiment. This type of block is particularly important in experiments on trees, which may have to be used for different experiments year after year.

Example 4.1 (Irrigated rice) Rice is usually grown on irrigated land. Figure 4.1 shows 32 plots in a rice paddy to be used for an experiment. Irrigation channels

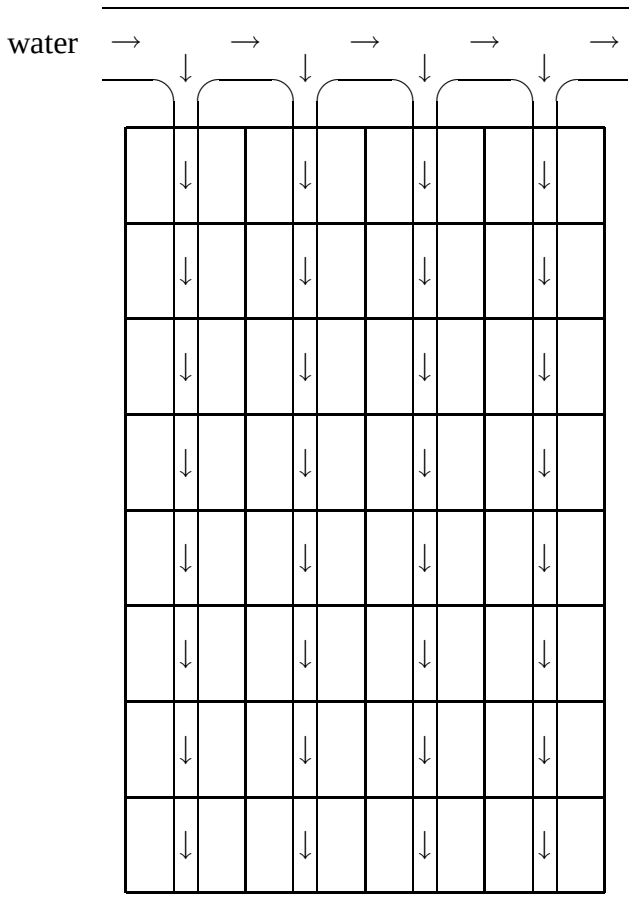

Figure 4.1: Irrigation channels in the rice experiment

branch off the main irrigation channel, each one watering a long strip of plots. These strips, or irrigation groupings, should be considered as blocks.

4.1.2 Continuous gradients

If an experiment is spread out in time or space then there will probably be continuous underlying trends but no natural boundaries. In such cases the plots can be divided into blocks of plots which are contiguous in time or space. To some extent the positioning of the block boundaries is arbitrary.

In an experiment on people or animals, age, weight and state of health are continuous variables which are often suitable for determining blocks. To be in the same block two people do not have to have exactly the same weight, but weight ranges can be chosen so that blocks have a suitable size. Similarly, severity of disease can be used to block patients in a clinical trial.

Example 4.2 (Laboratory measurement of samples) Consider the technician measuring soil samples in Question 2.2. His experimental units follow one another in

time. As time goes on, he may get more accurate, or he may get tired. Outside factors, such as temperature or humidity, may change. Dividing up the experimental units into three or four blocks of consecutive plots should remove these unnecessary sources of variation from the conclusions of the experiment.

Example 4.3 (Field trial) The plots in a agricultural field trial may cover quite a large area, encompassing changes in fertility. Sometimes it is possible to form natural blocks by marking out a stony area, a shady area and so on. More often it is simply assumed that plots close to each other are more likely to repond similarly than plots far apart, so small compact areas are chosen as blocks.

In Example 4.1, the distance from the main irrigation channel gives a continuous source of variability that should also be used for blocking, but now there is some freedom to choose how large a distance each block should cover.

4.1.3 Choice of blocking for trial management

Some aspects of trial management force differences between the plots. As far as possible, these differences should match (some of) the block boundaries.

In a clinical trial patients may have to be divided into groups to be attended to by different doctors or nurses. These groups should be blocks.

In a laboratory experiment, technicians may be thought of as natural blocks if their times and places of work are already fixed. However, if technicians can be allocated to tasks as part of the management of the experiment, then it may be possible to adjust their work so that, for example, the number of samples analysed by one person in one session is equal to the number of treatments.

There are many experiments where one or more treatment factors can be applied only to large areas: see Example 1.5. These large areas form a sort of block.

Example 4.3 revisited (Field trial) In the developed world, most agricultural operations are by tractor. Typically a tractor is driven as far as possible in a straight line before being turned round. This suggests that blocks in field trials should be long thin areas corresponding to a few passes of the tractor.

4.1.4 How and when to block

If possible,

(i) blocks should all have the same size;

(ii) blocks should be big enough to allow each treatment to occur at least once in each block.

Natural discrete blocks should always be used once they have been recognized. If possible, choose plots and blocks to satisfy (i).

Example 4.4 (Piglets) If the experimental units are piglets then litters are natural blocks. Litters are not all of the same size, typically being in the range 8–12, depending on the breed. It would be sensible to use only some fixed number, say 9, of piglets from each litter. Then you need an objective rule for which pigets to choose from the larger litters, such the heaviest piglets. Alternatively, if larger blocks are needed, start with more sows than necessary and use only those litters large enough to give, say, 10 piglets.

Natural blocks have an upper limit on their size, so it may be impossible to satisfy (ii). In the cows' ears example, blocks have size 2 no matter how many treatments there are.

Blocks should always be used for management. Then all trial operations—sowing, harvesting, interim applications of treatments, measuring—are done block-by-block, in case of interruptions, improvements in technique, replacement of staff, etc. This ensures that any extra variation caused by changing conditions is already accounted for by the blocking. Management blocks can usually be chosen to satisfy both (i) and (ii).

To eliminate the effects of a continuous trend, blocks can also be chosen to satisfy both (i) and (ii). Usually such blocking is helpful, but it may be better not to use this sort of block if doing so would make the number of residual degrees of freedom very small: see Example 4.6.

As noted in Example 4.3, the requirements of blocking for trial management may conflict with those of blocking to remove a continuous trend. You may have to decide which is more important for the experiment at hand.

We have also noted examples where more than one sort of block is needed. This point will be developed further in Chapters 6 and 8.

4.2 Orthogonal block designs

For the rest of this chapter we suppose that Ω consists of b blocks of equal size k. We thus have a *block factor* B which is defined by

$$B(\omega) = \text{the block containing } \omega.$$

The *block subspace* V_B consist of those vectors in V which take a constant value on each block. For $j = 1, \ldots, b$, let $\mathbf{v}_j$ be the vector whose entry on plot ω is equal to

$$\begin{cases} 1 & \text{if } \omega \text{ is in block } j; \\ 0 & \text{otherwise.} \end{cases}$$

Then $\mathbf{v}_j \cdot \mathbf{v}_j = k$, while $\mathbf{v}_j \cdot \mathbf{v}_l = 0$ if $j \neq l$. Therefore $\{\mathbf{v}_j : j = 1, \ldots, b\}$ is an orthogonal basis for V_B, and $\dim V_B = b$.

Now $\mathbf{u}_0 = \sum_{j=1}^{b} \mathbf{v}_j \in V_B$, so $V_0 \subset V_B$. Just as we defined W_T, we put

$$W_B = \{\mathbf{v} \in V_B : \mathbf{v} \text{ is orthogonal to } V_0\} = V_B \cap V_0^{\perp}.$$

Definition A block design is *orthogonal* if the spaces W_T and W_B are orthogonal to each other.

Theorem 4.1 *Let s_{ij} be the number of times that treatment i occurs in block j, for $i = 1, \ldots, t$ and $j = 1, \ldots, b$. Then the block design is orthogonal if and only if $s_{ij} = r_i/b$ for $i = 1, \ldots, t$ and $j = 1, \ldots, b$.*

Proof First note that $s_{ij} = \mathbf{u}_i \cdot \mathbf{v}_j$.

Since W_T is orthogonal to V_0, $W_T \perp W_B$ if and only if $W_T \perp V_B$, which happens if and only if

$$\left(\sum_{i=1}^{t} a_i \mathbf{u}_i \right) \cdot \mathbf{v}_j = 0 \quad \text{for } j = 1, \ldots, b$$

whenever $\sum_i a_i r_i = 0$; that is, so $\sum_i a_i s_{ij} = 0$ whenever $\sum_i a_i r_i = 0$.

If $s_{ij} = r_i/b$ for each i then $\sum_i a_i s_{ij} = \sum_i (a_i r_i)/b$, which is zero whenever $\sum_i a_i r_i = 0$. This is true for all j, so $W_T \perp W_B$.

Conversely, suppose that $W_T \perp W_B$. Fix i different from 1, and put $a_1 = 1/r_1$, $a_i = -1/r_i$ and $a_l = 0$ if $l \notin \{1, i\}$. Then $\sum_l a_l r_l = 0$ and $\sum_l a_l s_{lj} = s_{1j}/r_1 - s_{ij}/r_j$ so

$$\frac{s_{1j}}{r_1} = \frac{s_{ij}}{r_i}$$

for all j. This is true for all i, including $i = 1$, so counting the plots in block j gives

$$k = \sum_{i=1}^{t} s_{ij} = \frac{s_{1j}}{r_1} \sum_{i=1}^{t} r_i = \frac{s_{1j}}{r_1} N = \frac{s_{1j}}{r_1} bk.$$

Therefore $s_{1j} = r_1/b$ and hence $s_{ij} = r_i/b$ for all i. ∎

Definition A *complete-block design* has blocks of size t, with each treatment occurring once in each block.

Corollary 4.2 *Complete-block designs are orthogonal.*

We consider only orthogonal block designs for the remainder of this chapter.

4.3 Construction and randomization

Construct and randomize an orthogonal block design as follows.

(i) Apply treatment i to r_i/b plots in block 1, for $i = 1, \ldots, t$, and randomize, just as for a completely randomized design.

(ii) Repeat for each block, using a fresh randomization each time, independent of the preceding randomizations.

5	4	6	7	|	8	6	2	0	|	8	0	2	2	4	|	2	7	4	1
2	1	3	4	|	4	3	2	1	|	4	1	2	X	3	|	2	4	3	1

|	5	9	8	5	7	|	4	7	4	7	2	6	|	9	3	3	1	5	|
|	1	4	3	X	2	|	2	4	X	X	1	3	|	4	2	X	1	3	|

6	4	1	6	3
4	3	1	X	2

Table 4.1: Stream of random digits, used to randomize the design in Example 4.5

Judge 1				
Tasting	1	2	3	4
Wine	2	1	3	4

Judge 2				
Tasting	1	2	3	4
Wine	4	3	2	1

Judge 3				
Tasting	1	2	3	4
Wine	4	1	2	3

Judge 4				
Tasting	1	2	3	4
Wine	2	4	3	1

Judge 5				
Tasting	1	2	3	4
Wine	1	4	3	2

Judge 6				
Tasting	1	2	3	4
Wine	2	4	1	3

Judge 7				
Tasting	1	2	3	4
Wine	4	2	1	3

Judge 8				
Tasting	1	2	3	4
Wine	4	3	1	2

Table 4.2: Randomized plan in Example 4.5

Example 4.5 (Wine tasting) Four wines are tasted and evaluated by each of eight judges. A plot is one tasting by one judge; judges are blocks. So there are eight blocks and 32 plots. Plots within each judge are identified by order of tasting.

The systematic design is the same for each judge.

Judge j				
Tasting	1	2	3	4
Wine	1	2	3	4

To randomize this design we need eight independent random permutations of four objects. Here we use the method described at the end of Section 2.2, using a stream of random digits and taking as many as are needed for each successive block. The random digits are shown in the top row of Table 4.1 and the randomized plan in Table 4.2.

4.4 Models for block designs

Recall that $Y_\omega = \tau_{T(\omega)} + Z_\omega$, where Z_ω is the effect of plot ω. There are two common models for how the blocks affect Z_ω.

In the first model, the blocks affect the expectation but not the covariance. Thus

$$\mathbb{E}(Z_\omega) = \zeta_{B(\omega)},$$

where $\zeta_{B(\omega)}$ is an unknown constant depending on the block $B(\omega)$ containing ω. However, the covariance still has its simplest form; that is

$$\mathrm{cov}(Z_\alpha, Z_\beta) = \begin{cases} \sigma^2 & \text{if } \alpha = \beta \\ 0 & \text{otherwise.} \end{cases}$$

This is called the *fixed-effects model*.

In the second model the blocks make no contribution to the expectation, so that $\mathbb{E}(Z_\omega) = 0$. However, the covariance between the responses on plots α and β depends on whether $\alpha = \beta$, α and β are different but in the same block, or α and β are in different blocks. Thus

$$\mathrm{cov}(Z_\alpha, Z_\beta) = \begin{cases} \sigma^2 & \text{if } \alpha = \beta \\ \rho_1\sigma^2 & \text{if } \alpha \neq \beta \text{ but } B(\alpha) = B(\beta) \\ \rho_2\sigma^2 & \text{if } B(\alpha) \neq B(\beta). \end{cases}$$

Of course, $1 \geq \rho_1$ and $1 \geq \rho_2$. Usually we expect that $\rho_1 > \rho_2$, because plots in the same block should respond in a more alike manner than plots in different blocks. This is called the *random-effects model*.

Let $\mathbf{J}_B$ be the $N \times N$ matrix whose (α, β)-entry is equal to

$$\begin{cases} 1 & \text{if } B(\alpha) = B(\beta) \\ 0 & \text{otherwise.} \end{cases}$$

Then, in the random-effects model,

$$\begin{aligned} \mathrm{Cov}(\mathbf{Y}) &= \sigma^2\mathbf{I} + \rho_1\sigma^2(\mathbf{J}_B - \mathbf{I}) + \rho_2\sigma^2(\mathbf{J} - \mathbf{J}_B) \\ &= \sigma^2[(1-\rho_1)\mathbf{I} + (\rho_1 - \rho_2)\mathbf{J}_B + \rho_2\mathbf{J}]. \end{aligned}$$

Some natural discrete classifications with a small number of possibilities (such as sex) are best considered as fixed. For example, 20-year-old human males might always be heavier than 20-year-old human females and we might want to find out how much heavier. Most other classifications are just a nuisance and are best thought of as random. For example, plots at the top end of the field may do better than plots at the bottom end in wet years and worse in dry years, but, on the whole, plots at the top end will tend to perform more similarly to each other than to plots at the bottom end.

4.5 Analysis: fixed effects

The expectation part of the fixed-effects model is that

$$\mathbb{E}(Y_\omega) = \tau_{T(\omega)} + \zeta_{B(\omega)}. \tag{4.1}$$

In vector terms, this is

$$\mathbb{E}(\mathbf{Y}) = \tau + \zeta,$$

where $\tau \in V_T$ and $\zeta \in V_B$. Equation (2.4) shows that $\tau = \tau_0 + \tau_T$, where $\tau_0 = \overline{\tau} u_0 \in V_0$ and $\underline{\tau}_T = \tau - \overline{\tau} u_0 \in W_T$. Similarly, $\zeta = \zeta_0 + \zeta_B$, where $\zeta_0 = \overline{\zeta} u_0 \in V_0$ and $\zeta_B = \zeta - \overline{\zeta} u_0 \in W_B$. Thus

$$\mathbb{E}(\mathbf{Y}) = (\tau_0 + \zeta_0) + \tau_T + \zeta_B$$

with $\tau_0 + \zeta_0$ in V_0, τ_0 in W_T and ζ_B in W_B.

Now, τ_0 and ζ_0 are both multiples of the all-1 vector $\mathbf{u}_0$ and so they cannot be distinguished, either in the model (4.1) or from the data. This can be seen in another way. We could replace τ_i by $(\tau_i + c)$ for some constant c, for all i, and replace ζ_j by $(\zeta_j - c)$, for all j, without changing Equation (4.1). This implies that neither τ_0 nor ζ_0 can be estimated. However, we can estimate treatment contrasts, and we can estimate block contrasts.

The definition of the sum of two vector subspaces gives

$$V_T + V_B = \{\mathbf{v} + \mathbf{w} : \mathbf{v} \in V_T,\ \mathbf{w} \in V_B\}.$$

Thus Equation (4.1) can be rewritten as

$$\mathbb{E}(\mathbf{Y}) \in V_T + V_B.$$

Suppose that $\mathbf{x} \in W_T$. Then $\mathbf{x} \in V_T + V_B$. Applying Theorem 2.5 with $V_T + V_B$ in place of V_T shows that $\mathbf{x} \cdot \mathbf{Y}$ is the best linear unbiased estimator of $\mathbf{x} \cdot (\tau_0 + \zeta_0 + \tau_T + \zeta_B)$. Now, $\mathbf{x} \cdot \tau_0 = \mathbf{x} \cdot \zeta_0 = 0$ because $\mathbf{x} \in V_0^\perp$, and $\mathbf{x} \cdot \zeta_B = 0$ because $\mathbf{x} \in W_B^\perp$: that is why we restrict attention to orthogonal designs throughout this chapter. Therefore

$$\mathbf{x} \cdot (\tau_0 + \zeta_0 + \tau_T + \zeta_B) = \mathbf{x} \cdot \tau_T,$$

whose best linear unbiased estimator is $\mathbf{x} \cdot \mathbf{Y}$ with variance $\|\mathbf{x}\|^2 \sigma^2$. Similarly, if $\mathbf{z} \in W_B$ then $\mathbf{z} \cdot \mathbf{Y}$ is the best linear unbiased estimator of $\mathbf{z} \cdot \zeta_B$, with variance $\|\mathbf{z}\|^2 \sigma^2$.

Likewise, we have $P_{W_T}(\tau_0 + \zeta_0) = \mathbf{0}$ because $\tau_0 + \zeta_0$ is orthogonal to W_T. Similarly, $P_{W_T}(\zeta_B) = 0$ because ζ_B is in W_B, which is orthogonal to W_T. Now Theorem 2.4 shows that

$$\mathbb{E}\left(P_{W_T}(\mathbf{Y})\right) = P_{W_T}\left(\mathbb{E}\,\mathbf{Y}\right) = P_{W_T}\left(\tau_0 + \zeta_0 + \tau_T + \zeta_B\right) = \tau_T$$

and that $\mathbb{E}\left(P_{W_B}(\mathbf{Y})\right) = \zeta_B$.

Put $W_E = (V_T + V_B)^\perp$. This is going to be the residual subspace: the reason for the notation E will be explained in Chapter 10. Then V is the following direct sum of orthogonal subspaces:

$$V = V_0 \oplus W_T \oplus W_B \oplus W_E.$$

We have constructed both W_T and W_B to be orthogonal to V_0. The subspaces W_T and W_B are orthogonal to each other because we have assumed that the design is orthogonal. Finally, we have constructed W_E to be orthogonal to the previous three subspaces because $V_T + V_B = V_0 \oplus W_T \oplus W_B$.

Just as in Section 2.11, the orthogonal decomposition of V leads to (orthogonal) decompositions of the dimension, expectation, data and sum of squares, as follows:

$$\begin{array}{lrcccccccc}
 & V & = & V_0 & \oplus & W_T & \oplus & W_B & \oplus & W_E \\
\text{dimension} & N = bk = & & 1 & + & (t-1) & + & (b-1) & + & [b(k-1)-(t-1)] \\
\text{expectation} & \mathbb{E}(\mathbf{Y}) & = & (\tau_0+\zeta_0) & + & \tau_T & + & \zeta_B & + & \mathbf{0} \\
\text{data} & \mathbf{y} & = & \bar{y}\mathbf{u}_0 & + & \mathbf{y}_T & + & \mathbf{y}_B & + & \text{residual} \\
\text{sum of squares} & \displaystyle\sum_{\omega\in\Omega} y_\omega^2 & = & \dfrac{\mathsf{sum}^2}{N} & + & \mathrm{SS}(\text{treatments}) & + & \mathrm{SS}(\text{blocks}) & + & \mathrm{SS}(\text{residual})
\end{array}$$

where

$$\mathbf{y}_T = \sum_{i=1}^{t} \mathsf{mean}_{T=i}\mathbf{u}_i - \bar{y}\mathbf{u}_0,$$

$$\mathbf{y}_B = \sum_{j=1}^{b} \mathsf{mean}_{B=j}\mathbf{v}_j - \bar{y}\mathbf{u}_0,$$

$$\mathrm{SS}(\text{treatments}) = \sum_{i=1}^{t} \frac{\mathsf{sum}^2_{T=i}}{r_i} - \frac{\mathsf{sum}^2}{N},$$

$$\mathrm{SS}(\text{blocks}) = \sum_{j=1}^{b} \frac{\mathsf{sum}^2_{B=j}}{k} - \frac{\mathsf{sum}^2}{N},$$

$$\begin{aligned}
\mathrm{SS}(\text{residual}) &= \text{sum of squares of the residuals} \\
&= \sum_{\omega\in\Omega} y_\omega^2 - \mathrm{SS}(\text{mean}) - \mathrm{SS}(\text{treatments}) - \mathrm{SS}(\text{blocks}),
\end{aligned}$$

and $\mathsf{sum}_{B=j}$ and $\mathsf{mean}_{B=j}$ are the total and mean respectively of the values of y_ω for ω in block j.

Hence we obtain the anova table shown in Table 4.3. Of course, this is really two anova tables in one. The theoretical anova table, which tells us what to do, can omit the columns for mean square and variance ratio, but must show the column for EMS, which shows us which variance ratios to calculate. The anova table given by the actual data, in which the formulae are replaced by their values, does not need to show the EMS column, but may well include a final column headed 'F-probability', which gives the probability of obtaining a variance ratio at least as big as the one obtained in the table, under the null hypothesis of zero effect for that line, and assuming normality.

Use the variance ratio

$$\frac{\text{MS(treatments)}}{\text{MS(residual)}}$$

to test for treatment differences, and the variance ratio

$$\frac{\text{MS(blocks)}}{\text{MS(residual)}}$$

to test for block differences (if you are interested in them). Both tests are one-sided.

4.6 Analysis: random effects

Put $\mathbf{C} = \text{Cov}(\mathbf{Y})$. Then we have

$$\mathbf{C} = \sigma^2[(1-\rho_1)\mathbf{I} + (\rho_1-\rho_2)\mathbf{J}_B + \rho_2\mathbf{J}].$$

If plot ω is in block j then the ω-row of $\mathbf{J}_B$ is just $\mathbf{v}_j$. Hence if $\mathbf{x}$ is any vector in V then the ω-entry in $\mathbf{J}_B\mathbf{x}$ is equal to $\mathbf{v}_j \cdot \mathbf{x}$. In particular, if $\mathbf{x} = \mathbf{u}_0$ then $\mathbf{v}_j \cdot \mathbf{x} = k$ for all j and so $\mathbf{J}_B\mathbf{u}_0 = k\mathbf{u}_0$. Since $\mathbf{I}\mathbf{u}_0 = \mathbf{u}_0$ and $\mathbf{J}\mathbf{u}_0 = N\mathbf{u}_0$, we see that

$$\mathbf{C}\mathbf{u}_0 = \sigma^2[(1-\rho_1) + k(\rho_1-\rho_2) + N\rho_2]\mathbf{u}_0,$$

so that $\mathbf{u}_0$ is an eigenvector of $\mathbf{C}$ with eigenvalue ξ_0, where

$$\xi_0 = \sigma^2[(1-\rho_1) + k(\rho_1-\rho_2) + N\rho_2].$$

If $\mathbf{x} \in V_B$ then $\mathbf{x} = \sum_j \lambda_j \mathbf{v}_j$ for some scalars $\lambda_1, \ldots, \lambda_b$; hence $\mathbf{v}_j \cdot \mathbf{x} = k\lambda_j$ and so $\mathbf{J}_B\mathbf{x} = k\mathbf{x}$. Hence if $\mathbf{x} \in W_B = V_B \cap V_0^\perp$ then

$$\mathbf{C}\mathbf{x} = \sigma^2[(1-\rho_1) + k(\rho_1-\rho_2)]\mathbf{x},$$

and so $\mathbf{x}$ is an eigenvalue of $\mathbf{C}$ with eigenvalue ξ_1, where

$$\xi_1 = \sigma^2[(1-\rho_1) + k(\rho_1-\rho_2)].$$

Finally, if $\mathbf{x} \in V_B^\perp \subset V_0^\perp$ then $\mathbf{J}_B\mathbf{x} = \mathbf{0}$ and $\mathbf{J}\mathbf{x} = \mathbf{0}$ so $\mathbf{C}\mathbf{x} = \xi_2\mathbf{x}$, where $\xi_2 = \sigma^2(1-\rho_1)$.

Thus the eigenspaces of $\mathbf{C}$ (the strata) are V_0, W_B and $V_B^\perp$, with dimensions 1, $b-1$ and $N-b$ and eigenvalues ξ_0, ξ_1 and ξ_2 respectively. Usually we expect that $\xi_1 > \xi_2$.

Theorem 2.10 shows that the appropriate anova table is that shown in Table 4.4. The arithmetic calculations are identical to those for the fixed-effects model. Assess treament differences just as before. For the effects of blocks, do a two-sided test using

$$\frac{\text{MS(blocks)}}{\text{MS(residual)}}.$$

If $\text{MS(blocks)} >> \text{MS(residual)}$ then the choice of blocks was good: do it similarly next time. If $\text{MS(blocks)} << \text{MS(residual)}$ then

source	sum of squares	degrees of freedom	mean square	EMS	variance ratio
V_0 mean	$\frac{\mathsf{sum}^2}{N}$	1	$\mathrm{SS}(\text{mean})$	$\lVert\tau_0+\zeta_0\rVert^2+\sigma^2$	$\frac{\mathrm{MS}(\text{mean})}{\mathrm{MS}(\text{residual})}$
W_B blocks	$\sum_j \frac{\mathsf{sum}^2_{B=j}}{k} - \frac{\mathsf{sum}^2}{N}$	$b-1$	$\frac{\mathrm{SS}(\text{blocks})}{b-1}$	$\frac{\lVert\zeta_B\rVert^2}{b-1}+\sigma^2$	$\frac{\mathrm{MS}(\text{blocks})}{\mathrm{MS}(\text{residual})}$
W_T treatments	$\sum_i \frac{\mathsf{sum}^2_{T=i}}{r_i} - \frac{\mathsf{sum}^2}{N}$	$t-1$	$\frac{\mathrm{SS}(\text{treatments})}{t-1}$	$\frac{\lVert\tau_T\rVert^2}{t-1}+\sigma^2$	$\frac{\mathrm{MS}(\text{treatments})}{\mathrm{MS}(\text{residual})}$
residual	$\leftarrow \ldots\ldots$ by subtraction $\ldots\ldots \rightarrow$		$\frac{\mathrm{SS}(\text{residual})}{\mathrm{df}(\text{residual})}$	σ^2	–
Total	$\sum_\omega y_\omega^2$	N			

Table 4.3: Anova table for blocks and unstructured treatments under the fixed-effects model

stratum		source	df	EMS	VR
V_0	mean	mean	1	$\|\tau_0\|^2+\xi_0$	–
W_B	blocks	blocks	$b-1$	ξ_1	–
$V_B^\perp$	plots	treatments	$t-1$	$\frac{\|\tau_T\|^2}{t-1}+\xi_2$	$\frac{\text{MS(treatments)}}{\text{MS(residual)}}$
		residual	$b(k-1)-(t-1)$	ξ_2	–
Total			N		

Table 4.4: Anova table for blocks and unstructured treatments under the random-effects model

either $\xi_1 < \xi_2$ because plots within a block compete (for example, if all plots in a chamber in a greenhouse share a single system of circulating liquid nutrients)

or $\xi_1 < \xi_2$ and there is a better way of blocking

or trial management has not been by block

or the scientist is fiddling the data (and is not expecting you to notice very low values of the variance ratio).

4.7 Why use blocks?

If we should use blocks and do not, what happens?

If the blocks contribute fixed effects then ζ_B is almost certainly not zero. If the treatments are not allocated orthogonally to blocks then ζ_B will not be orthogonal to W_T. The estimator of τ_T is $P_{W_T}\mathbf{Y}$, whose expectation is $\tau_T + P_{W_T}\zeta_B$. Thus treatment estimators are biased. It is most likely that ζ_B is also not orthogonal to $V_T^\perp$, so the estimator of σ^2 will also be biased. In fact, Theorem 2.4(ii) shows that the expectation of this estimator will be

$$\left\|P_{V_T^\perp}\zeta_B\right\|^2 \Big/ (N-t)+\sigma^2,$$

so that the variance will be overestimated.

If the blocks contribute random effects then treatment estimators are unbiased but their variances are larger than they need be: on average, ξ_2 will be replaced by

$$\frac{(b-1)\xi_1+(N-b)\xi_2}{N-1}.$$

If we do use blocks in the design but forget to include them in the analysis, what happens?

Now the treatment estimators are unbiased, but in both models our estimates of their variances are too high, so we may fail to detect genuine treatment differences. For fixed effects, the expectation of the estimator of σ^2 is equal to

$$\|\zeta_B\|^2/(N-t)+\sigma^2;$$

for random effects, the expectation of the estimator of ξ_2 is equal to

$$\frac{(b-1)\xi_1+(N-b-t+1)\xi_2}{N-t}.$$

4.8 Loss of power with blocking

The following example, which is taken from a case where the manufacturer tried to sue the statisticians for using blocks, shows the only circumstances where blocking may be a disadvantage: there are no natural block boundaries, there are a small number of residual degrees of freedom, and the purpose of the experiment is (arguably) hypothesis testing rather than estimation.

Example 4.6 (Pasture grass) A new additive is claimed to vastly improve the quality of pasture grass. Are farmers wasting their money in buying it?

There are two treatments: the new additive, and nothing.

Plots must be large enough for several sheep to graze freely. Hence the replication cannot be large: replication 3 is chosen.

Should the design be completely randomized or in three randomized complete blocks?

Put

$$\begin{aligned}\tau_1 &= \text{response to nothing}\\ \tau_2 &= \text{response to new additive.}\end{aligned}$$

The null hypothesis is $H_0 : \tau_1 = \tau_2 = \bar{\tau} = \left(\frac{\tau_1+\tau_2}{2}\right)$. Now,

$$\tau_T = \begin{cases} \tau_1 - \bar{\tau} = \dfrac{\tau_1-\tau_2}{2} & \text{on nothing}\\ \tau_2 - \bar{\tau} = \dfrac{\tau_2-\tau_1}{2} & \text{on new additive}\end{cases}$$

Using the model for the completely randomized design from Section 2.13, but writing ξ in place of ξ_1 (to avoid confusion with the next model), we obtain the

following anova table.

stratum	source	df	EMS
mean	mean	1	$6\overline{\tau}^2+\xi_0$
plots	treatments	1	$\frac{6}{4}(\tau_1-\tau_2)^2+\xi$
	residual	4	ξ
Total		6	

Now we consider the complete-block design. There are no natural block boundaries, so the random-effects model is appropriate, and we obtain the following anova table.

stratum	source	df	EMS
mean	mean	1	$6\overline{\tau}^2+\xi_0$
blocks	blocks	2	ξ_1
plots	treatments	1	$\frac{3}{2}(\tau_1-\tau_2)^2+\xi_2$
	residual	2	ξ_2
Total		6	

The completely randomized design mixes up the 5 degrees of freedom orthogonal to V_0, so

$$5\xi = 2\xi_1 + 3\xi_2$$

so $\xi > \xi_2$ if $\xi_1 > \xi_2$.

The variance of the estimator of $\tau_1 - \tau_2$ is

$$\begin{cases} \frac{2}{3}\xi & \text{in the completely randomized design} \\ \frac{2}{3}\xi_2 & \text{in the complete-block design} \end{cases}$$

so the complete-block design gives smaller variance and so is better for estimation.

For hypothesis testing, we consider the one-sided alternative that τ_2 is bigger then τ_1. To test at the 5% significance level we need the 0.95 point of the t-distribution, which is 2.920 on 2 degrees of freedom and 2.132 on 4 degrees of freedom. To have 90% power of detecting that $\tau_2 > \tau_1$, we also need the 0.90 points of these distributions, which are 1.886 and 1.533 respectively. The argument in

Section 2.12 shows that to have probability at least 0.9 of detecting that $\tau_2 > \tau_1$ when doing a one-sided test at the 5% significance level we need

$$\begin{cases} \tau_2 - \tau_1 > (2.132 + 1.533) \times \sqrt{\frac{2}{3}\xi} & \text{in the completely randomized design} \\ \tau_2 - \tau_1 > (2.920 + 1.886) \times \sqrt{\frac{2}{3}\xi_2} & \text{in the complete-block design.} \end{cases}$$

Thus

$$\begin{aligned} \text{the block design is better} \quad &\Longleftrightarrow \quad 4.806\sqrt{\xi_2} < 3.665\sqrt{\xi} \\ &\Longleftrightarrow \quad \xi > 1.720\xi_2 \\ &\Longleftrightarrow \quad \xi_1 > 2.8\xi_2. \end{aligned}$$

Typically we have $\xi_1 \approx 1.5\xi_2$ for such a trial, so smaller differences can be detected by the unblocked design. A scientist who is more interested in proving that the new additive is better (than in accurately estimating how much better) might complain if the experiment is conducted in blocks rather than in a completely randomized design.

Questions for Discussion

4.1 The plan in Figure 4.2 is the field layout of an experiment conducted in 1935 at Rothamsted Experimental Station (an agricultural research station founded in 1843). Each plot had a notice on it showing the block number and the plot number. These are the top two numbers given in each plot in the plan. The purpose of the experiment was to compare various types of fumigant, in single and double doses, for their ability to control eelworms in the soil where oats were being grown. A "control" treatment (i.e. no fumigant) was included. In the plan, each plot shows, in order below the plot number, the level of a factor called Fumigant, then the dose, then the type of chemical. In the spring, 400 gm of soil were sampled from each plot, and the number of eelworm cysts in each sample counted and recorded. The oats were sown, fumigated, grown and harvested. After harvest the plots were sampled again in the same way, and the number of cysts recorded. The variable *logcount* was calculated as

$$\begin{aligned} \textit{logcount} \;=\; &\log(\text{number of eelworm cysts at harvest}) \\ &- \log(\text{number of eelworm cysts in spring before treatment}), \end{aligned}$$

where the logarithms are to base e. This variable is shown at the bottom of each plot in the plan.

(i) How many treatments were there?

(ii) How were the plots divided into blocks?

(iii) After sampling soil from plot 4 of block III, which plot should the scientist sample next?

(iv) Devise a better way of numbering the plots.

(v) Why do you think logarithms were used to present the data in the form *logcount*?

4.2 Ignoring the factorial structure of the treatments, calculate the analysis-of-variance table and the table of means for the data *logcount* in the eelworm experiment.

4.3 Redo Question 1.2 under the assumption that the professor has 10 pills of Wakey-Wakey and 10 of Zizzaway.

There is only one observation room, so only one pill can be tested per day. Your plan should show which student should take which pill on which day.

What information should you give the professor about the plan?

		I 1 1 0 *Z* 0.549	I 2 2 2 *K* −0.011	I 3 2 1 *N* 0.457	I 4 2 1 *M* 0.599	I 5 2 1 *S* 0.341	I 6 1 0 *Z* 0.784				
		I 7 1 0 *Z* 0.759	I 8 2 2 *M* 0.365	I 9 2 2 *S* 0.277	I 10 2 1 *K* 0.107	I 11 1 0 *Z* 1.187	I 12 2 2 *N* 0.740				
II 1 2 1 *K* 0.771	II 2 1 0 *Z* 0.873	II 3 2 1 *S* 0.803	II 4 2 2 *K* 0.609	III 1 2 2 *K* 0.414	III 2 1 0 *Z* 0.521	III 3 2 1 *K* 0.191	III 4 2 1 *M* 1.088	IV 1 2 2 *M* 0.739	IV 2 2 2 *S* 0.268	IV 3 2 2 *K* 0.574	
II 5 1 0 *Z* 1.269	II 6 2 2 *N* 1.067	II 7 2 2 *S* 0.888	II 8 2 1 *N* 1.665	III 5 1 0 *Z* 2.170	III 6 2 2 *N* 2.325	III 7 2 2 *S* 0.499	III 8 1 0 *Z* 1.719	IV 4 1 0 *Z* 1.482	IV 5 2 1 *K* 0.791	IV 6 2 1 *N* 1.316	
II 9 2 2 *M* 0.812	II 10 1 0 *Z* 1.081	II 11 2 1 *M* 1.355	II 12 1 0 *Z* 1.618	III 9 2 1 *S* 1.247	III 10 2 1 *N* 1.792	III 11 1 0 *Z* 1.807	III 12 2 2 *M* 1.826	IV 7 2 1 *M* 1.457	IV 8 1 0 *Z* 0.616	IV 9 1 0 *Z* 1.398	
								IV 10 1 0 *Z* 2.138	IV 11 2 2 *N* 1.992	IV 12 2 1 *S* 1.271	

Figure 4.2: Field layout for the experiment in Question 4.1