13 Confounded Two-Level Factorial Experiments

13.1 Introduction

In Chaps. 6 and 7 we discussed factorial experiments arranged as completely randomized designs, and in Chaps. 10 and 11 we looked at factorial experiments arranged as block designs. Factorial experiments that involve several treatment factors tend to be large. Even a modest experiment with four factors having 2, 2, 3, and 3 levels has a total of 36 treatment combinations. Since experimenters generally are working to a restricted budget and since observations cost time and money, many factorial experiments are *single-replicate* experiments (one observation per treatment combination). In this chapter we consider single-replicate experiments arranged in blocks where every treatment factor has two levels. This will be extended in Chap. 14 to cover treatment factors with more than two levels. Then, in Chap. 15, we will look at experiments in which only a fraction of the treatment combinations can be observed.

In Sect. 13.2, we discuss alternative codings of treatment combinations, and the general problem of confounding together with its implications for analysis. Methods of designing single-replicate experiments so that information is lost on as few lower-order treatment contrasts as possible are the main focus of Sects. 13.3 and 13.4, followed by an example in Sect. 13.5. Section 13.6 describes plans for the design of confounded experiments.

In Sects. 13.7–13.10 we return to the subject of multi-replicate factorial experiments in blocks and compare the traditional incomplete block designs with the multiple use of single-replicate confounded designs. Analysis of confounded factorial experiments by the computer packages SAS and R is considered briefly in Sects. 13.11 and 13.12.

13.2 Single Replicate Factorial Experiments

13.2.1 Coding and Notation

A factorial experiment that involves two treatment factors each having two levels is known as a 2×2, or 2^2, experiment. Similarly, an experiment with two factors each having 3 levels is known as a 3×3, or 3^2, experiment. A $2^4 \times 3^2$ experiment has six treatment factors, the first four having two levels each, and the last two having three levels each. Other factorial experiments are described in a similar manner. A factorial experiment is called *symmetric* if all factors have the same number of levels. Otherwise, it is called *asymmetric*. In this chapter we will deal only with symmetric 2^p experiments. Other situations will be discussed in Chap. 14.

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The levels of a two-level treatment factor are often referred to as the "low" and "high" levels, and in Chaps. 6 and 7 we coded these as 1 and 2. The codings 0 and 1, or -1 and $+1$, are also commonly used. A 2^2 experiment then has four treatment combinations coded as (11, 12, 21, 22) or as (00, 01, 10, 11) or as $(-1-1, -1+1, +1-1, +1+1)$. A fourth standard coding for the treatment combinations is ((1), b, a, ab), where the letter a or b appears if the corresponding factor A or B is at its high level, and is absent if the corresponding factor is at its low level. The symbol (1) means that both factors are at their low level.

Coding is a matter of personal choice. Although we have coded the levels as 1 and 2 until now, the other three codings are more usual when talking about single-replicate factorial experiments. We will code the levels as 0 and 1 throughout this and the next two chapters.

13.2.2 Confounding

A factorial experiment with v treatment combinations uses $v-1$ degrees of freedom to measure all of the main effects and interactions. In a single-replicate experiment, there are only v observations and $v-1$ total degrees of freedom. Thus, the experiment is not large enough to allow measurement of all of the factorial effects and also estimation of the error variance. Three ways around this problem for statistical inference in completely randomized designs were discussed in Sects. 6.7 and 7.5.

The problem is worse when the experiment is to be run as a block design. If there are b blocks in the design, $b-1$ of the total degrees of freedom are used to measure the block differences, leaving only $(v-1)-(b-1) = v-b$ degrees of freedom available for measuring the treatment contrasts and the error variance. The result of this is that $b-1$ of the treatment contrasts can no longer be measured. They cannot be distinguished from block contrasts and are said to be *confounded* with blocks. Such a design is useful only when at most $v-b$ treatment contrasts are to be measured.

Care is required in designing this type of experiment. If v treatment combinations are arbitrarily divided into b blocks of size v/b, the important treatment contrasts will not necessarily be estimable. The estimable contrasts are those that are orthogonal to the confounded contrasts. This means that the experiment should be designed in such a way that the confounded contrasts belong only to interactions that are expected to be negligible. Fortunately, in some cases this is not difficult to achieve, and we will examine these cases in this chapter. For a 2^p experiment, we will restrict attention to designs with $b = 2^s$ blocks of size $k = 2^{p-s}$.

13.2.3 Analysis

The standard block–treatment model for a single-replicate factorial experiment arranged as an incomplete block design has the same form as model (11.4.2), p. 356, used for incomplete block designs in Chap. 11; that is,

$$\begin{gathered} Y_{hi} = \mu + \theta_h + \tau_i + \epsilon_{hi}\,, \\ \epsilon_{hi} \sim N(0, \sigma^2)\,, \\ \epsilon_{hi}\text{'s are mutually independent}\,, \\ h = 1, \ldots, b; \quad i = 1, \ldots, v; \quad (h, i) \text{ is in the design.} \end{gathered} \tag{13.2.1}$$

As usual, Y_{hi} is the random variable representing the observation on treatment combination i in block h (if it appears in the design), ϵ_{hi} is the corresponding error random variable, μ is a constant, τ_i is the effect of the ith treatment combination, and θ_h is the effect of the hth block. The block$\times$treatment interaction is assumed to be negligible.

Table 13.1 Outline analysis of variance table for single-replicate factorial experiments constructed by the methods of this chapter

Source of variation	Degrees of freedom	Sum of squares
Blocks	$b-1$	$ss\theta = \frac{1}{k}\Sigma B_h^2 - \frac{1}{v}G^2$
	$\vdots$	$\vdots$
$\Sigma c_i \tau_i$	1	$ssc = \frac{(\Sigma\Sigma c_i y_{hi})^2}{\Sigma c_i^2}$
(at most $v-b$ of these)		
	$\vdots$	$\vdots$
Error	df (by subtraction)	ssE (by subtraction)
Total	$v-1$	$sstot = \Sigma\Sigma y_{hi}^2 - \frac{1}{v}G^2$

Analysis of all single-replicate designs described in this chapter is straightforward. Because of the way in which the designs will be constructed, contrasts in the important main effects and interactions will be completely orthogonal to block contrasts. As a consequence, these contrasts will have no adjustment for blocks, and their estimates and sums of squares can be calculated in exactly the same way as for completely randomized designs (see Chaps. 6 and 7). An outline of an analysis of variance table is shown in Table 13.1. The maximum number of degrees of freedom available for estimating main effects and interactions is $v-b$ if no estimate of the error variance is required; otherwise, it is $v-b-1$. The unadjusted sum of squares for blocks $ss\theta$ can be calculated either as the total of all the confounded contrast sums of squares or by the usual formula, which was given in Table 11.7, p. 358, as

$$ss\theta = \frac{1}{k}\Sigma_h B_h^2 - \frac{1}{v}G^2 , \tag{13.2.2}$$

where B_h is the total of the observations in the hth block and G is the grand total of all the observations.

13.3 Confounding Using Contrasts

13.3.1 Contrasts

Treatment contrasts for factorial experiments were discussed in Sects. 6.3, 7.2.4, and 7.3. When there are two factors, A and B, each having two levels, there are $v=4$ treatment combinations in total, and it is possible to find a set of three orthogonal contrasts, one for the main effects of each of A and B and one for their interaction. The coefficient lists $[c_{00}, c_{01}, c_{10}, c_{11}]$ for these contrasts are

$$\begin{aligned} &\text{For } A: && [-1, -1, \ \ 1, \ \ 1\,], \\ &\text{For } B: && [-1, \ \ 1, -1, \ \ 1\,], \\ &\text{For } AB: && [\ \ 1, -1, -1, \ \ 1\,]. \end{aligned}$$

Each coefficient for the AB interaction is the product of the corresponding coefficients for the main effects of A and B. Similarly, for three factors, the coefficient lists for the seven contrasts are shown in Table 13.2 written as columns. The interaction coefficients are, again, the product of corresponding main-effect coefficients. The row labels in Table 13.2 are the treatment combinations (in lexicographical order) whose observations are to be multiplied by the contrast coefficients when estimating the contrast. The seven contrasts are orthogonal. This can be verified by multiplying together corresponding digits

Table 13.2 Contrasts for a 2^3 experiment

	A	B	C	AB	AC	BC	ABC
000	-1	-1	-1	1	1	1	-1
001	-1	-1	1	1	-1	-1	1
010	-1	1	-1	-1	1	-1	1
011	-1	1	1	-1	-1	1	-1
100	1	-1	-1	-1	-1	1	1
101	1	-1	1	-1	1	-1	-1
110	1	1	-1	1	-1	-1	-1
111	1	1	1	1	1	1	1

in any two columns and showing that the sum of the products is zero. A table of orthogonal contrasts, such as Table 13.2, is sometimes called an *orthogonal array*.

Such contrasts in a 2^p experiment all have the same variance, since $\Sigma c_i^2 = v = 2^p$ for all contrasts and $\text{Var}(\Sigma c_i \hat{\tau}_i) = \Sigma c_i^2 \sigma^2 = v\sigma^2$. The main effect of A is often measured by the A contrast divided by $v/2$, so that it compares the average of all treatment combinations at the high level of A with the average of the treatment combinations at the low level. If the interaction contrast coefficients are also divided by $v/2$, the contrast estimators all have variance $\sum c_i^2 \sigma^2 = 4\sigma^2/v$, and the AB interaction, for example, then compares the average response when factors A and B are at the same level with the average response when they are at different levels. We shall use either $v/2$ or 1 for the divisor for all contrasts in 2^p experiments both here and in Chap. 15.

13.3.2 Experiments in Two Blocks

We start with an example. Suppose that a single-replicate 2^3 experiment is to be run in two blocks of size four. Suppose also that the experimenter knows that one of the factors, say factor A, does not interact with either of the other two factors. This means that the interactions AB, AC, and ABC may be assumed to be negligible and that the contrasts labeled A, B, C, and BC in Table 13.2 are the only contrasts to be measured.

Since there will be $b = 2$ blocks, it follows that $b - 1 = 1$ degree of freedom will be used to measure block differences and one treatment contrast will be confounded with blocks. Without too much difficulty, we can ensure that the confounded contrast is one of the negligible contrasts. For example, we can confound the negligible ABC contrast by placing in one block those treatment combinations corresponding to -1 in the ABC contrast, and placing in the second block those treatment combinations corresponding to $+1$ in the same contrast. Referring to Table 13.2, we can see that the design in Table 13.3 results. The ABC contrast is now identical to a block contrast that compares Block I with Block II, and consequently, the ABC contrast is confounded with blocks. The other two negligible contrasts, AB and AC, provide two degrees of freedom to estimate σ^2. Since all the nonnegligible factorial contrasts are orthogonal to AB, AC, and ABC, they can be measured as though there were no blocks present. Block design randomization (see Sect. 11.2.2) needs to be carried out before the design in Table 13.3 can be used in practice.

Table 13.3 2^3 experiment in 2 blocks of 4, confounding ABC

Block I	000	011	101	110
Block II	001	010	100	111

Table 13.4 Data for the field experiment (*ABCD* is confounded)

Block I		Block II	
TC	Yield	TC	Yield
0000	58	0001	55
0011	51	0010	45
0101	44	0100	42
0110	50	0111	36
1001	43	1000	53
1010	50	1011	55
1100	41	1101	41
1111	44	1110	48

Source Data adapted from *Experimental Designs*, Second Edition, by W.G. Cochran and G.M. Cox, 1957, John Wiley & Sons, New York

A similar method of confounding can be used for any 2^p experiment in $b = 2$ blocks of size $k = 2^{p-1}$. All factorial contrasts except for the one confounded contrast can be estimated.

Example 13.3.1 `Field experiment`

The data shown in Table 13.4 form part of the results of a field experiment on the yield of beans using various types of fertilization. The experiment was conducted at Rothamsted Experimental Station in 1936 and was reported by W.G. Cochran and G.M. Cox in their book *Experimental Designs*. There were four treatment factors each at two levels. Factor A was the amount of dung (0 or 10 tons) spread per acre, factors B, C, and D were the amounts of nitrochalk (0 and 45 lb), superphosphate (0 and 67 lb), and muriate of potash (0 and 112 lb), respectively, per acre. The experimental area was divided into two possibly dissimilar blocks of land, each of which was subdivided into eight plots (experimental units). Since this was a single-replicate experiment with $2^4 = 16$ treatment combinations (TC) divided into $b = 2$ blocks of size $k = 8$, one treatment contrast had to be confounded. The experimenters chose to confound the *ABCD* contrast, since the four-factor interaction was of least interest.

The *ABCD* contrast is shown below, and it can be verified that the treatment combinations corresponding to contrast coefficient $+1$ appear in Block I of Table 13.4, while those corresponding to coefficient -1 appear in Block II. All the other factorial contrasts are orthogonal to the *ABCD* contrast, so they can all be estimated without adjusting for the block effects. We take as examples the B and BC contrasts shown below.

TC	0000	0001	0010	0011	0100	0101	0110	0111
y_{hijkl}	58	55	45	51	42	44	50	36
B	-1	-1	-1	-1	1	1	1	1
BC	1	1	-1	-1	-1	-1	1	1
$ABCD$	1	-1	-1	1	-1	1	1	-1
TC	1000	1001	1010	1011	1100	1101	1110	1111
y_{hijkl}	53	43	50	55	41	41	48	44
B	-1	-1	-1	-1	1	1	1	1
BC	1	1	-1	-1	-1	-1	1	1
$ABCD$	-1	1	1	-1	1	-1	-1	1

Using rule 10 of Sect. 7.3, the least squares estimate of the B contrast is $\overline{y}_{..1..} - \overline{y}_{..0..} = \frac{1}{8}\Sigma c_{ijkl} y_{hijkl}$, where the sum is to be taken over the four subscripts, and the contrast coefficients c_{ijkl} are given in standard order above. Multiplying the contrast coefficients by the data values, we obtain

$$\text{For } B : \tfrac{1}{8}\sum c_{ijkl}y_{hijkl} \quad = \quad -8.00.$$

Similarly, if we divide the *BC* contrast shown above by the same divisor $v/2$, we obtain the contrast estimate

$$\text{For } BC : \tfrac{1}{8}\sum c_{ijkl}y_{hijkl} \quad = \tfrac{1}{8}(y_{..00.} - y_{..01.} - y_{..10.} + y_{..11.}) \quad = \quad 2.25.$$

Using (6.7.53), the sum of squares for testing the hypothesis that the main effect of B is negligible is

$$ssB = \frac{\left(\frac{1}{8}\sum c_{ijkl}y_{hijkl}\right)^2}{\sum\left(\frac{1}{8}c_{ijkl}\right)^2} = \frac{(-8.00)^2}{\frac{16}{64}} = 256.0.$$

Similarly, the sum of squares for testing the hypothesis that the interaction between B and C is negligible is

$$ss(BC) = \frac{(2.25)^2}{\frac{16}{64}} = 20.25.$$

An alternative way to calculate the sums of squares is to use the method of Sect. 7.3. Following the rules in that section, we obtain

$$\begin{aligned} ssB &= acd\sum_j \overline{y}^2_{..j..} - abcd\overline{y}^2_{.....} \\ &= 8\left[(51.25)^2 + (43.25)^2\right] - 16(47.25)^2 \quad = \quad 256.0\,, \end{aligned}$$

and

Table 13.5 Analysis of variance for the field experiment

Source of variation	Degrees of freedom	Sum of squares	Contrast estimate (divisor $v/2$)
Block (*ABCD*)	1	2.25	
A	1	2.25	−0.75
B	1	256.00	−8.00
C	1	0.25	0.25
D	1	20.25	−2.25
AB	1	6.25	1.25
AC	1	81.00	4.50
AD	1	0.00	0.00
BC	1	20.25	2.25
BD	1	12.25	−1.75
CD	1	1.00	0.50
ABC	1	16.00	−2.00
ABD	1	16.00	2.00
ACD	1	20.25	2.25
BCD	1	121.00	−5.50
Total	15	575.00	

$$
\begin{aligned}
ss(BC) &= ad\sum_j\sum_k \overline{y}^2_{..jk.} - acd\sum_j \overline{y}^2_{..j..} - abd\sum_k \overline{y}^2_{...k.} + abcd\overline{y}^2_{.....} \\
&= 4\left[(52.25)^2 + (50.25)^2 + (42.00)^2 + (44.50)^2\right] \\
&\quad - 8\left[(51.25)^2 + (43.25)^2\right] - 8\left[(47.125)^2 + (47.375)^2\right] \\
&\quad + 16(47.25)^2 \;=\; 20.25.
\end{aligned}
$$

The complete analysis of variance table is shown in Table 13.5. The important contrasts can be identified using one of the methods of Sect. 7.5. A half-normal probability plot of the 14 contrast estimates is shown in Fig. 13.1. Note that we have not included the confounded *ABCD* contrast in the half-normal probability plot. Although it is not the case here, the block effect is usually expected to be large and may draw attention away from the important treatment contrasts.

The contrasts B, *BCD*, and *AC* may or may not be important, since their plotted points roughly line up with those of the seven smallest absolute estimates, but not with those of the 11 smallest absolute estimates. Suppose they are considered important. We notice that the contrast estimate for B is negative, suggesting that the addition of nitrochalk decreased the yield of beans when averaged over the levels of A, C, and D. The interaction plot for *BCD* is shown in Fig. 13.2 and that for *AC* in Fig. 13.3. We see from Fig. 13.2 that the *BCD* interaction can be characterized by the fact that the *CD* interaction changes as B changes from its low level to its high level. If the objective of the experiment is to increase yield, then comparison of the two plots in Fig. 13.2 suggests that B should be set at its low level, unless

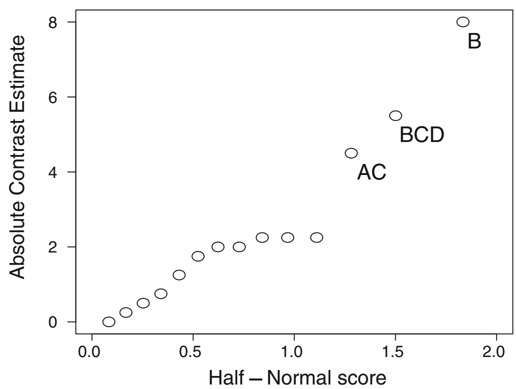
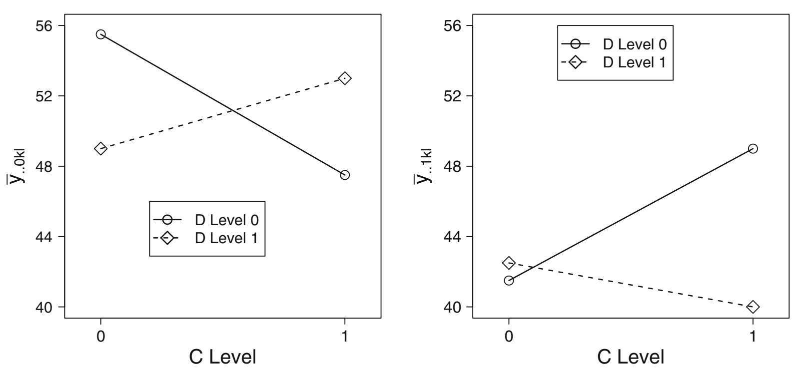


Fig. 13.1 Half-normal probability plot for the contrast absolute estimates of the field experiment



(a) B level 0 **(b)** B level 1

Fig. 13.2 *BCD* interaction plot for the field experiment

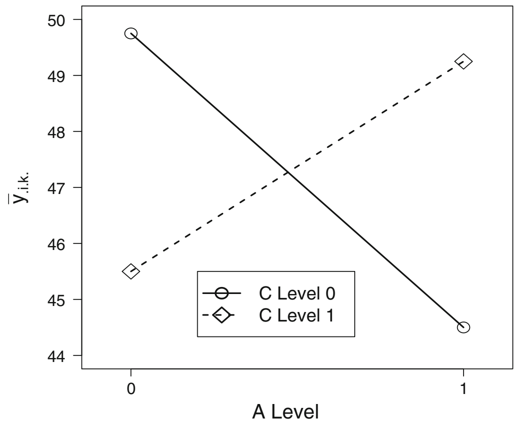


Fig. 13.3 *AC* interaction plot for the field experiment

the high level of C and the low level of D are used. This tends to agree with the earlier observation that the contrast estimate for B is negative, suggesting that the low level is better. If B is set at its low level, the left-hand graph of Fig. 13.2 suggests that both C and D should be at their low levels. The AC interaction plot in Fig. 13.3 shows that either both C and A should be at their low levels or both C and A should be at their high levels. The contrast estimators for A and D are both negative, suggesting that on average the low level is better, although the difference in yield is minor. More importantly, the low levels in this experiment are cheaper. Therefore, all the evidence points towards not adding any fertilizer ingredients in the quantities studied in the experiment. A followup experiment could be run with the same four factors but with an increased "high" level of C and lower "high" levels of A, B, and D. Since it is possible that the response is quadratic for each of the factors, a 3^4 experiment could be run.

Suppose the experimenters had known ahead of time that factors A and D do not interact, so that interactions AD, ABD and ACD could have been assumed negligible; then the corresponding terms would have been omitted from the model. There would then have been 3 degrees of freedom for estimating the error variance. The error sum of squares would have been the total of the sums of squares for AD, ABD, and ACD listed in Table 13.5, so that

$$\hat{\sigma}^2 = msE = \frac{1}{3}(ss(AD) + ss(ABD) + ss(ACD)) = \frac{1}{3}(36.25) = 12.0833.$$

The analysis of variance table would then have been as shown in Table 13.6, and we see that at an overall significance level of at most $\alpha = 11(0.005) = 0.055$, none of the contrasts would have been judged as significantly different from zero, since $F_{1,3,0.005} = 55.55$.

Confidence intervals could be calculated for each contrast at an overall confidence level of at least 94.5%, using the Bonferroni method (formula (4.4.21)) with each interval at individual level 99.5%, with error degrees of freedom $df = 3$ and with $r_i = 8$ being the number of observations averaged over to obtain the estimate. For example, a confidence interval for the difference in the high and low levels of B is

$$\begin{aligned} \beta_1^* - \beta_0^* &\in \left(\frac{1}{8} \Sigma c_{ijkl} y_{hijkl} \pm t_{3,0.0025} \sqrt{msE\,(16/64)} \right) \\ &= (-8 \pm 7.4532 \times 1.7381) \;=\; (-20.954, 4.954)\,. \end{aligned}$$

Table 13.6 Analysis of variance for the field experiment

Source of variation	Degrees of freedom	Sum of squares	Mean square	Ratio	p-value
Block	1	2.25	2.25	–	–
A	1	2.25	2.25	0.186	0.6952
B	1	256.00	256.00	21.186	0.0193
C	1	0.25	0.25	0.021	0.8947
D	1	20.25	20.25	1.676	0.2861
AB	1	6.25	6.25	0.517	0.5240
AC	1	81.00	81.00	6.703	0.0811
BC	1	20.25	20.25	1.676	0.2861
BD	1	12.25	12.25	1.014	0.3882
CD	1	1.00	1.00	0.083	0.7923
ABC	1	16.00	16.00	1.324	0.3332
BCD	1	121.00	121.00	10.014	0.0507
Error	3	36.25	12.0833		
Total	15	575.00			

We remind the reader that if both of the above analyses are done, that is, if the interactions AD, ABD, and ACD are dropped from the model after examining the half-normal probability plot, then it is no longer meaningful to talk about the significance levels of the tests or the confidence interval levels (see Sect. 6.5.6, p. 170). □

13.3.3 Experiments in Four Blocks

We can extend the method of confounding that we used for two blocks to obtain $b = 4 = 2^2$ blocks. We then need to use two contrasts to divide up the treatment combinations. For example, suppose that in a 2^4 experiment, all interactions except for the two-factor interactions are thought to be negligible. We can select one of the negligible interactions to produce two blocks of size 8 and then select a second interaction to subdivide each of these two blocks into two smaller blocks, giving a total of 4 blocks of size 4. Since $b - 1 = 3$ degrees of freedom are needed to measure blocks, a third treatment contrast must also be confounded. We require this third contrast to be among the negligible contrasts, so care must be taken as to which pair of contrasts is initially selected for confounding. The choice of $ABCD$ and ABC, for example, is a very poor choice even if these high-order interactions may be thought to be negligible. We can see this by examining the design and the third confounded contrast as follows.

The two contrasts and the corresponding treatment combinations (TC) are shown in Table 13.7. The treatment combinations are divided into 2 blocks of size 8 according to the coefficients in the $ABCD$ contrast. Each of these blocks is then subdivided into 2 blocks of size 4 according to the coefficients in the ABC contrast. The $b = 4$ blocks of size $k = 4$ are therefore determined by the pairs of coefficients $(ABCD, ABC) = (-1, -1)$ or $(-1, 1)$ or $(1, -1)$ or $(1, 1)$ in the two contrasts. The resulting design is shown in Table 13.7 (prior to randomization).

Examination of the blocks in the design shows that all of the treatment combinations in Block I and Block IV have the fourth digit equal to 1, and all of those in Blocks II and III have the fourth digit equal to 0. This means that the high and low levels of factor D cannot be compared within the same block, and therefore the contrast for the main effect of D must be the third contrast confounded with blocks.

Table 13.7 2^4 experiment in 4 blocks of 4, confounding *ABCD*, *ABC*, *D*

TC	0000	0001	0010	0011	0100	0101	0110	0111
ABCD	1	−1	−1	1	−1	1	1	−1
ABC	−1	−1	1	1	1	1	−1	−1
TC	1000	1001	1010	1011	1100	1101	1110	1111
ABCD	−1	1	1	−1	1	−1	−1	1
ABC	1	1	−1	−1	−1	−1	1	1
Block	Contrast coefficients (*ABCD*, *ABC*)	Treatment combinations						
I	(−1, −1)	0001	0111	1011	1101			
II	(−1, 1)	0010	0100	1000	1110			
III	(1, −1)	0000	0110	1010	1100			
IV	(1, 1)	0011	0101	1001	1111			

We could have predicted this outcome, since if corresponding coefficients of the two contrasts *ABCD* and *ABC* shown in Table 13.7 are multiplied together, the coefficients of the D contrast results. Notice that in symbols, we can write

$$(ABCD)(ABC) = A^2B^2C^2D = D,$$

where any letter with exponent 2 is ignored. The squared coefficients of any 2^p factorial contrast are all $+1$, so multiplying the D contrast by C^2, say, is the same as multiplying the contrast coefficients by $+1$ and $C^2D = D$. Multiplication of the contrast names in this way gives a quick, easy method of checking which third contrast is confounded without writing out the contrasts and without writing out the design.

The above design is not suitable for the stated experiment. Suppose that the contrasts *ABD* and *BCD* were selected for confounding instead. The third confounded contrast would then be $(ABD)(BCD) = AB^2CD^2 = AC$. This, too, is not suitable since all two-factor interactions were to have been measured. Unfortunately, there is no choice that will meet the specifications of this particular experiment. The number of blocks and the block sizes are too small to measure everything that is required. At least one two-factor interaction would have to be sacrificed, or a larger experiment must be run.

Example 13.3.2 2^5 `experiment in 4 blocks of 8`

In Sect. 13.5 we will describe a 2^5 experiment that was run in $b = 4$ blocks of size $k = 8$. In designing such an experiment, one needs to select two contrasts for confounding and to check that the third confounded contrast is acceptable. As in the discussion above, selecting the 5-factor interaction for confounding will generally be a poor choice, since no matter which other interaction is selected for the second confounded interaction, a 2-factor interaction or main effect will be among the confounded contrasts. For example,

$$(ABCD)(ABCDE) = E \quad \text{and} \quad (ABC)(ABCDE) = DE.$$

If an experimenter knew ahead of time that factors D and E do not interact, then the second choice might be acceptable. In general, though, most experimenters would prefer not to confound low-order interactions. So, a selection of a 3-factor interaction and a 4-factor interaction with as few letters in common as possible will generally be the best choice. For example,

$$(ABCD)(CDE) = ABE.$$

There are many selections of this type, and the experimenter would wish to avoid confounding any 3-factor interaction that might be of some interest. The selection made in Sect. 13.5 is ABD, BCE, and their product $ACDE$. If the treatment combinations are written out in standard order together with the contrast coefficients for ABD and BCE, it can be verified that the pairs of contrast coefficients give the four blocks shown in Table 13.11 (p. 448). □

13.3.4 Experiments in Eight Blocks

If an experiment is required in $b = 2^3$ blocks, then three contrasts must be selected for confounding. A single-replicate design in eight blocks confounds $b - 1 = 7$ treatment contrasts in total, including the three contrasts initially selected and all products of these. For example, suppose that a 2^6 experiment is required in $b = 2^3 = 8$ blocks of 8, and that the two-factor interactions are of interest together with the four three-factor interactions ACE, ACD, ADE, and CDE (these are the four 3-factor interactions that do not contain B or F). A suitable choice might be to confound the interactions BCD, ABE, and ADF. The other four confounded contrasts would be

$$\begin{aligned}(BCD)(ABE) &= ACDE,\\ (BCD)(ADF) &= ABCF,\\ (ABE)(ADF) &= BDEF,\\ (BCD)(ABE)(ADF) &= CEF.\end{aligned}$$

The list of seven confounded contrasts is called the *confounding scheme* for the design. The reader is invited to write out the three selected contrasts and verify that the design in Table 13.10 (p. 446) results. The fact that $ACDE$, $ABCF$, $BDEF$, and ABE are also confounded can be verified by showing that the coefficients of each of these four contrasts are constant for all treatment combinations within each block.

Note that the same design will be obtained for any initial selection of three of the above seven confounded contrasts, *provided that no selected contrast is a product of the other two*. For example, suppose that $ABCF$, ABE, and CEF are initially selected. The selected contrasts $ABCF$ and ABE divide the treatment combinations into four blocks, but CEF does not subdivide these blocks further, since it is the third contrast automatically confounded in the four blocks, i.e., $(ABCF)(ABE) = CEF$. A different third contrast needs to be chosen, and any of the remaining four contrasts will do. Three selected contrasts satisfying the requirement that no selected contrast be a product of the other two is called a set of three *independent* contrasts.

13.3.5 Experiments in More Than Eight Blocks

The same ideas can be used for 2^p experiments in 2^s blocks of size $k = 2^{p-s}$ by selecting s independent contrasts to subdivide the treatment combinations into blocks; s contrasts are independent if none can be obtained as the product of two or more of the other $s - 1$ contrasts. Although the multiplication of contrast names is a convenient method to determine the list of confounded contrasts, it becomes harder to use the contrasts themselves for constructing the design as the number of factors increases.

Table 13.8 2^3 experiment in 2 blocks of 4, confounding *ABC*

Block I	000	011	101	110
Block II	001	010	100	111

In the next section, we present a method of constructing block designs for single-replicate factorial experiments that avoids writing out the contrasts.

13.4 Confounding Using Equations

13.4.1 Experiments in Two Blocks

The design in Table 13.8 (given previously in Table 13.3) was constructed by allocating the treatment combinations to blocks in such a way that the contrast that compares Block I with Block II is identical to the *ABC* contrast. The *ABC* contrast is confounded with blocks and cannot be estimated, but all of the other factorial contrasts are estimable because they are orthogonal to the confounded contrast.

If the treatment combinations in the two blocks of the design are examined closely, an interesting property becomes apparent. All the treatment combinations in the first block have an even number of 1's, and all those in the second block have an odd number of 1's. We could, in fact, have predicted this property. The *ABC* contrast is the product of the A, B, and C contrasts (see Sect. 13.3.1), and so the only way to achieve a coefficient -1 in the *ABC* contrast is for there to be an odd number of -1's among the corresponding coefficients in the A, B, and C contrasts. This means that there must be an odd number of 0's and an even number of 1's in the corresponding treatment combination. Thus, if we want to confound *ABC* without writing out the contrasts, we can simply allocate a treatment combination $a_1a_2a_3$ to Block I if it has an even number of 1's among its digits, and to Block II if it has an odd number of 1's. Equivalently, we allocate $a_1a_2a_3$ to Block I if $a_1 + a_2 + a_3$ is an even number and to Block II if $a_1 + a_2 + a_3$ is an odd number. Instead of writing "$a_1 + a_2 + a_3$ is an even number," we write "$a_1 + a_2 + a_3 = 0 \pmod 2$" and instead of writing "$a_1 + a_2 + a_3$ is an odd number," we write "$a_1 + a_2 + a_3 = 1 \pmod 2$". Working mod 2, or modulo 2, means that we subtract 2 repeatedly from the number until we reach either 0 or 1, or equivalently, we divide by 2 and take the remainder which is either 0 or 1. For example, $5 = 1 \pmod 2$, but $8 = 0 \pmod 2$. We call the pair of equations "$a_1 + a_2 + a_3 = 0 \pmod 2$" and "$a_1 + a_2 + a_3 = 1 \pmod 2$" the *confounding equations*. The design of Tables 13.3 and 13.8 is constructed using the following rule:

$$\begin{aligned}&\text{Block I: Treatment combinations with } a_1 + a_2 + a_3 = 0 \pmod 2\,,\\ &\text{Block II: Treatment combinations with } a_1 + a_2 + a_3 = 1 \pmod 2\,.\end{aligned}$$

If the *AC* contrast were to be confounded in the 2^3 experiment instead of the *ABC* contrast, only the first and third digits of each treatment combination would be used to allocate it to a block. This is because only the first and third digits of the treatment combination govern the coefficients in the *AC* contrast. We would use the pair of confounding equations $a_1 + a_3 = 0 \pmod 2$ and $a_1 + a_3 = 1 \pmod 2$, and the design would be constructed using the rule

$$\begin{aligned}&\text{Block I: Treatment combinations with } a_1 + a_3 = 0 \pmod 2\,,\\ &\text{Block II: Treatment combinations with } a_1 + a_3 = 1 \pmod 2\,,\end{aligned}$$

giving the design of Table 13.9. It can be verified that *AC* is indeed confounded with blocks in this design, since the coefficients of the *AC* contrast (shown in Table 13.2) are all equal to $+1$ for the

Table 13.9 2^3 experiments in 2 blocks of 4, confounding *AC*

Block I	000	010	101	111
Block II	001	011	100	110

treatment combinations in Block I and −1 for those in Block II. All other contrasts are estimable because they are orthogonal to the confounded *AC* contrast.

We may now generalize to 2^p experiments in 2 blocks of size 2^{p-1}. If the interaction $A^{z_1}B^{z_2}C^{z_3}\cdots P^{z_p}$ is to be confounded with blocks, where $z_i = 1$ if the factor is present in the interaction and $z_i = 0$ if it is not, the blocks of the design are

$$\begin{aligned}
&\text{Block I: Treatment combinations with} \\
&\qquad z_1a_1 + z_2a_2 + z_3a_3 + \cdots + z_pa_p = 0 \pmod 2\,, \\
&\text{Block II: Treatment combinations with} \\
&\qquad z_1a_1 + z_2a_2 + z_3a_3 + \cdots + z_pa_p = 1 \pmod 2\,.
\end{aligned}$$

13.4.2 Experiments in More Than Two Blocks

We obtain designs with 4, 8, 16, ... blocks by using more than one pair of confounding equations. We label the pairs of confounding equations as L_1, L_2, etc. For example, the (unsatisfactory) design

Block	Treatment Combinations
I	0000 0110 1010 1100
II	0011 0101 1001 1111
III	0001 0111 1011 1101
IV	0010 0100 1000 1110

from Table 13.7 for a 2^4 experiment in 4 blocks of size 4 was produced by confounding the *ABCD* and *ABC* contrasts. Using the *ABCD* contrast to produce two blocks is equivalent to using the pair of confounding equations $L_1 = a_1 + a_2 + a_3 + a_4 = 0 \pmod 2$ and $L_1 = 1 \pmod 2$, and using the *ABC* contrast is equivalent to using the pair of confounding equations $L_2 = a_1 + a_2 + a_3 = 0 \pmod 2$ and $L_2 = 1 \pmod 2$. Thus there are four possible values for the pair (L_1, L_2), and it can be verified that the blocks of the design satisfy

$$\begin{aligned}
&\text{Block I:} && L_1 = a_1 + a_2 + a_3 + a_4 = 0;\ L_2 = a_1 + a_2 + a_3 = 0 \pmod 2\,; \\
&\text{Block II:} && L_1 = a_1 + a_2 + a_3 + a_4 = 0;\ L_2 = a_1 + a_2 + a_3 = 1 \pmod 2\,; \\
&\text{Block III:} && L_1 = a_1 + a_2 + a_3 + a_4 = 1;\ L_2 = a_1 + a_2 + a_3 = 0 \pmod 2\,; \\
&\text{Block IV:} && L_1 = a_1 + a_2 + a_3 + a_4 = 1;\ L_2 = a_1 + a_2 + a_3 = 1 \pmod 2\,.
\end{aligned}$$

We already know from Sect. 13.3.3 that a third contrast, namely contrast *D*, is confounded in this design. If we add the two confounding equations used to create each block, we have, for Block I,

$$\begin{array}{rl}
L_1 = & a_1 + a_2 + a_3 + a_4 = 0 \pmod 2 \\
+\,L_2 = & a_1 + a_2 + a_3 \phantom{{}+a_4} = 0 \pmod 2 \\
\hline
L_1 + L_2 = & 2a_1 + 2a_2 + 2a_3 + a_4 = 0 \pmod 2
\end{array}$$

If all coefficients are reduced modulo 2, the sum gives $a_4 = 0 \pmod 2$, which indicates that the *D* contrast is also confounded.

As has been demonstrated, there is a correspondence between the contrasts, the contrast names, and the confounding equations. The contrast names are the most convenient for checking the total list of confounded contrasts, and the equations are the most convenient for constructing the design.

Design construction can be done in several ways. One way is to examine each of the 2^p treatment combinations and allocate them to blocks according to their values obtained in the left sides of the confounding equations. Another way is to identify the treatment combinations that make the confounding equations equal to zero. These will form Block I of the design. The other blocks of the design are obtained by adding (modulo 2) to the treatment combinations in Block I any treatment combination that has not yet appeared in a block. This is illustrated in the following example.

Example 13.4.1 2^6 `experiment in 8 blocks of 8`

A confounding scheme was found in Sect. 13.3.4 for a 2^6 experiment in 8 blocks of 8 by selecting contrasts *BCD*, *ABE*, and *ADF* for confounding. It was shown that contrasts *ACDE*, *ABCF*, *BDEF*, and *CEF* were also confounded. Writing out the equations for the three selected contrasts, we have

$$\begin{aligned} L_1 &= \phantom{a_1 + {}} a_2 + a_3 + a_4 \phantom{{} + a_5 + a_6} = 0 \text{ or } 1 \pmod 2, \\ L_2 &= a_1 + a_2 \phantom{{} + a_3 + a_4} + a_5 \phantom{{} + a_6} = 0 \text{ or } 1 \pmod 2, \\ L_3 &= a_1 \phantom{{} + a_2 + a_3} + a_4 \phantom{{} + a_5} + a_6 = 0 \text{ or } 1 \pmod 2. \end{aligned}$$

The equations corresponding to the other four confounded contrasts are obtained by setting $L_1 + L_2$, $L_1 + L_3$, $L_2 + L_3$, and $L_1 + L_2 + L_3$ equal to 0 or 1 (mod 2).

The design is shown in Table 13.10. It can be constructed systematically as follows. The first treatment combination 000000 gives 0 for each of L_1, L_2, L_3 and is allocated to Block I. The second treatment combination 000001 gives values 0, 0, 1 for the three L_i and is allocated to Block II, and so on.

Alternatively, one can look for the eight treatment combinations that give zero for each of L_1, L_2, and L_3 and construct Block I first. Solving $L_1 = 0$, $L_2 = 0$, and $L_3 = 0$ each for the last a_i gives $a_4 = a_2 + a_3$ (mod 2), $a_5 = a_1 + a_2$ (mod 2) and $a_6 = a_1 + a_4$ (mod 2), respectively. For each one

Table 13.10 2^6 experiment in 8 blocks of 8, confounding *ABE*, *ADF*, *BCD*, *CEF*, *ABCF*, *ACDE*, *BDEF*

Block	L_1, L_2, L_3	Treatment Combinations			
I	0, 0, 0	000000	001101	010111	011010
		100011	101110	110100	111001
II	0, 0, 1	000001	001100	010110	011011
		100010	101111	110101	111000
III	0, 1, 0	000010	001111	010101	011000
		100001	101100	110110	111011
IV	0, 1, 1	000011	001110	010100	011001
		100000	101101	110111	111010
V	1, 0, 0	000101	001000	010010	011111
		100110	101011	110001	111100
VI	1, 0, 1	000100	001001	010011	011110
		100111	101010	110000	111101
VII	1, 1, 0	000111	001010	010000	011101
		100100	101001	110011	111110
VIII	1, 1, 1	000110	001011	010001	011100
		100101	101000	110010	111111

of of the eight combinations a_1, a_2, a_3 of factors A, B, and C, the corresponding values of a_4, a_5, and a_6 can thus be computed to obtain one of the eight treatment combinations of Block I. For example, if $a_1 = a_2 = a_3 = 1$, then $a_4 = a_2 + a_3 = 1 + 1 = 0$ (mod 2), $a_5 = a_1 + a_2 = 1 + 1 = 0$ (mod 2), and $a_6 = a_1 + a_4 = 1 + 0 = 1$ (mod 2), so the treatment combination 111001 is in Block I.

Each of the other blocks is obtained by adding a new treatment combination to those in Block I—"new" meaning not yet in a block. For example, the treatment combination 000001 is not in Block I and if 000001 is added modulo 2 to the eight treatment combinations in Block I, then Block II results. For example, 000001 is added to 111001 of Block I by adding corresponding digits and reducing modulo 2; that is,

$$000001 + 111001 = 111002 = 111000 \text{ (mod 2)},$$

so 111000 is also in Block II.

A third block can be obtained by taking any treatment combination not in the first two blocks, and adding it to each treatment combination in Block I. Proceeding in this fashion, blocks are constructed until each treatment combination has been allocated to some block. □

13.5 A Real Experiment—Mangold Experiment

O. Kempthorne in his book *Design and Analysis of Experiments* describes an experiment run at Rothamsted Agricultural Station to investigate the effects of five different fertilizers on the growth of mangold roots. The five factors were Sulphate of ammonia (factor A at levels 0 or 0.6 cwt per acre), Superphosphate (factor B at levels 0 or 0.5 cwt per acre), Muriate of potash (factor C at levels 0 or 1.0 cwt per acre), Agricultural salt (factor D at levels 0 or 5 cwt per acre), and Dung (factor E at levels 0 or 10 tons per acre). The experimental area was divided into $b = 4$ blocks of size $k = 8$. All 3-, 4-, and 5-factor interactions were expected to be negligible. The two three-factor interactions *ABD*, *BCE*, and their product *ACDE* were selected for confounding.

The division of the 32 treatment combinations into the four blocks was then determined from the confounding equations corresponding to *ABD* and *BCE*; that is,

$$\begin{aligned} L_1 &= a_1 + a_2 \qquad + a_4 \qquad = 0 \text{ or } 1 \text{ (mod 2)}\,, \\ L_2 &= \qquad a_2 + a_3 \qquad + a_5 = 0 \text{ or } 1 \text{ (mod 2)}\,. \end{aligned}$$

The blocks can be formed by systematically working through all 32 treatment combinations and assigning them to the blocks according to the values of L_1 and L_2 and the rule

$$\begin{aligned} &\text{Block I:} && L_1 = 0(\text{mod } 2) && \text{and} && L_2 = 0(\text{mod } 2)\,, \\ &\text{Block II:} && L_1 = 0(\text{mod } 2) && \text{and} && L_2 = 1(\text{mod } 2)\,, \\ &\text{Block III:} && L_1 = 1(\text{mod } 2) && \text{and} && L_2 = 0(\text{mod } 2)\,, \\ &\text{Block IV:} && L_1 = 1(\text{mod } 2) && \text{and} && L_2 = 1(\text{mod } 2)\,. \end{aligned}$$

Alternatively, we can notice that $L_1 = 0$ gives $a_4 = a_1 + a_2$ (mod 2) and $L_2 = 0$ gives $a_5 = a_2 + a_3$ (mod 2). Computing these for each combination of levels $a_1a_2a_3$ of the first three factors gives Block I as

Block I: 00000 10010 00101 10111
11100 01110 11001 01011

Any treatment combination that is not in Block I can be added to the treatment combinations in Block I to obtain a second block. So, if we add 00001, for example, we obtain

Block II: 00001 10011 00100 10110
11101 01111 11000 01010

Any treatment combination not in Blocks I and II can now be added to the treatment combinations in Block I to obtain a third block. Since 00010, for example, has not appeared in Blocks I or II, we can add it to the treatment combinations in Block I to obtain Block III. Block IV can then be obtained by adding a treatment combination, say 00011, that has not appeared in the previous three blocks.

Block III: 00010 10000 00111 10101
11110 01100 11011 01001
Block IV: 00011 10001 00110 10100
11111 01101 11010 01000

The order of the treatment combinations was randomized within each block, and the order of the blocks was randomized. The final design, together with the resulting yields (in pounds) of mangold roots, is shown in Table 13.11.

The contrasts for the main effects and interactions are obtained as usual. The contrast for comparing the high and low levels of superphosphate (B), for example, is $\sum_i c_i \tau_i$, where τ_i is the effect of the ith treatment combination, and the coefficient c_i is the ith element of

$$\tfrac{1}{16}[-1,\ -1,\ -1,\ -1,\ -1,\ -1,\ -1,\ -1,\ \ 1,\ 1,\ 1,\ 1,\ 1,\ 1,\ 1,\ 1,$$
$$-1,\ -1,\ -1,\ -1,\ -1,\ -1,\ -1,\ -1,\ \ 1,\ 1,\ 1,\ 1,\ 1,\ 1,\ 1,\ 1\,].$$

Table 13.11 Yields (in pounds) of mangold roots for the mangold experiment

Block	Treatment Combinations (Yield)			
I	01101	00011	10100	11111
	(844)	(1104)	(1156)	(1508)
	11010	00110	10001	01000
	(1312)	(1000)	(1176)	(888)
II	00001	01111	00100	10011
	(1248)	(1100)	(784)	(1376)
	11101	10110	11000	01010
	(1356)	(1376)	(1008)	(964)
III	00101	11001	01011	01110
	(896)	(1284)	(996)	(860)
	10010	11100	00000	10111
	(1184)	(984)	(740)	(1468)
IV	10101	11110	00111	11011
	(1328)	(1292)	(1008)	(1324)
	01001	01100	00010	10000
	(1008)	(692)	(780)	(1108)

Source Design and Analysis of Experiments, by O. Kempthorne, 1976. Reprinted by permission of Krieger Publishing company Inc.

Equivalently, $c_i = -1/16$ if the ith treatment combination has B at the low level, and $c_i = 1/16$ otherwise. There is only one observation on each treatment combination, so a least squares estimate of τ_i is just y_i (the observation on the ith treatment combination). The least squares estimate of the contrast for comparing the high and low levels of B is

$$\sum_i c_i \hat{\tau}_i = -312/16 = -19.5\,.$$

The contrast estimates for each of the main effects and also the interactions are shown in Table 13.12.

To test the hypothesis that adding superphosphate has no effect on the yield of mangold roots, we test the null hypothesis $H_0^B : \sum_i c_i \tau_i = 0$ against the alternative hypothesis $H_A^B : \sum_i c_i \tau_i \neq 0$ using the rules of Sect. 7.3; that is,

$$\text{reject } H_0^B \quad \text{if} \quad \frac{ssB}{msE} = \frac{(\sum_i c_i \hat{\tau}_i)^2}{\left(\sum_i c_i^2\right) msE} = \frac{(\sum_i c_i y_i)^2}{(32/16^2)\; msE} > F_{1,13,\alpha/m}\,.$$

If the 3-, 4-, and 5-factor interactions are assumed to be negligible, the total of their contrast sums of squares, apart from *ABD*, *BCE*, and *ACDE*, which are confounded with blocks, forms the error sum of squares with 13 degrees of freedom, $(10 - 2 = 8$ from the 3-factor interactions, $5 - 1 = 4$ from the 4-factor interactions, and 1 from the 5-factor interaction). There are $m = 15$ contrasts of interest, so if each individual contrast is tested at significance level $\alpha/m = 0.005$, the overall significance level would be at most $\alpha = 0.075$. For the main effect of B,

$$\frac{(\sum_i c_i y_i)^2}{(32/16^2)\; msE} = \frac{8(-19.5)^2}{msE} = \frac{3042.00}{6791.23} < F_{1,13,0.005} = t^2_{13,0.0025} = 11.37\,,$$

Table 13.12 Analysis of variance for the mangold experiment (*ABD*, *BCE*, *ACDE* are confounded)

Source of variation	Degrees of freedom	Sum of squares	Mean square	Ratio	Contrast estimate	p-values
Block	3	52832	17610.67	–	–	–
A	1	887112	887112.00	130.63	333.0	0.0001
B	1	3042	3042.00	0.45	−19.5	0.5150
C	1	722	722.00	0.11	9.5	0.7496
D	1	144722	144722.00	21.31	134.5	0.0005
E	1	262088	262088.00	38.59	181.0	0.0001
AB	1	338	338.00	0.05	6.5	0.8269
AC	1	48050	48050.00	7.08	77.5	0.0196
AD	1	16562	16562.00	2.44	45.5	0.1424
AE	1	288	288.00	0.04	−5.0	0.8400
BC	1	6272	6272.00	0.92	−28.0	0.3541
BD	1	5832	5832.00	0.86	27.0	0.3710
BE	1	98	98.00	0.01	−3.5	0.9062
CD	1	30752	30752.00	4.53	62.0	0.0530
CE	1	882	882.00	0.13	−10.5	0.7244
DE	1	13778	13778.00	2.03	−41.5	0.1779
Error	13	88286	6791.23			
Total	31	1561656				

so we fail to reject the null hypothesis H_0^B and conclude that there is no evidence to suggest a difference in yield due to the high and low levels of superphosphate. The ratio for each of the other hypothesis tests is given in Table 13.12, and we see that the only hypotheses that would be rejected at overall significance level 0.075 are the hypotheses of no effect of A or D or E.

The block sum of squares is the sum of the ABD, BCE, and $ACDE$ contrast sums of squares, or alternatively, it can be calculated as

$$ss\theta = \frac{1}{8}\sum_h B_h^2 - \frac{1}{32}G^2 = 52832.0$$

as in (13.2.2), where $B_h = y_{h.....}$ and $G = y_{......}$. The block mean square is more than twice the size of the error mean square, indicating that the creation of blocks in this experiment was useful for reducing the error variability, assuming that the effects confounded with blocks are negligible.

Since none of the interactions appear to be significantly different from zero, the main effects can be investigated. The contrast estimates of the significant main effects are all positive, suggesting that the high levels of A, D, and E increase the yield of mangold roots significantly. Confidence intervals for all the main-effect contrasts can be obtained via Bonferroni's method using formula (4.4.21), but with error degrees of freedom $df = 13$. If we select the confidence level to match the α level of the hypothesis tests, we obtain a set of simultaneous confidence intervals with overall confidence level of at least 92.5% as follows:

$$\begin{aligned}
\text{For } A: &\ \left(\overline{y}_{.1....} - \overline{y}_{.0....} \pm t_{13,0.0025}\sqrt{32\hat{\sigma}^2/16^2}\right) \\
&= (333.0 \pm 3.372\sqrt{6791.23/8}) \\
&= (333.0 \pm 98.25) = (234.75,\ 431.25)\,; \\
\text{For } B: &\ (-19.5 \pm 98.25) = (-117.75,\ 78.75)\,; \\
\text{For } C: &\ (9.5 \pm 98.25) = (-88.75,\ 107.75)\,; \\
\text{For } D: &\ (134.5 \pm 98.25) = (36.25,\ 232.75)\,; \\
\text{For } E: &\ (181.0 \pm 98.25) = (82.75,\ 279.25)\,.
\end{aligned}$$

At a somewhat higher overall significance level, the hypothesis of no interaction between A and C would have been rejected. The AC interaction plot is shown in Fig. 13.4. This plot agrees with the earlier observation that A should be set at its high level. Unless the cost is high, the plot suggests that C should also be set at its high level. Factor B does not seem to affect the yield much and can be set at its low (zero) level. As stated earlier, D and E should be at their high levels.

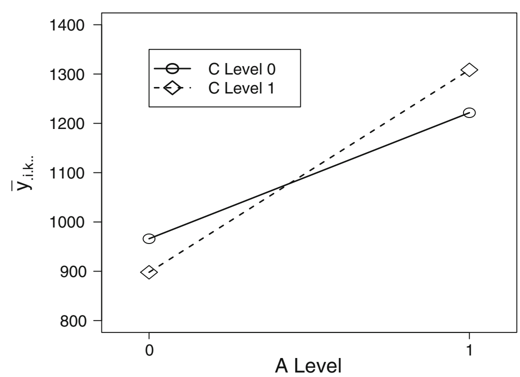


Fig. 13.4 *AC* interaction plot for the mangold experiment

After an experiment of this type, it is good policy to run a *confirmatory experiment* verifying that the selected levels are, indeed, a good combination. Here the recommendation is to use treatment combination 10111. Certainly, in the main experiment, this treatment combination gave the highest yield in Block III, but it cannot easily be compared with the other observations because of the large block differences. A few more observations on this particular treatment combination would help to verify that it is consistently a good choice.

13.6 Plans for Confounded 2^p Experiments

Suggestions for confounding schemes useful for constructing designs for 2^p experiments in $b = 2^s$ blocks of size $k = 2^{p-s}$ are given in Table 13.29 at the end of this chapter. These have been chosen to allow all main effects to be estimable and as many two-factor interactions as possible. The first block of each design can be obtained from the given relations on the factor levels, and the other blocks can be obtained by adding (modulo 2) new treatment combinations to those in Block I. This process was illustrated in Example 13.4.1, p. 446.

If one or more of the listed confounded contrasts is an important contrast in the experiment being designed, the factors should be relabeled. For example, suppose that a design is required for a 2^6 experiment in 8 blocks of 8. The design listed in Table 13.29 confounds *BCD*, *ABE*, *ACDE*, *ADF*, *ABCF*, *BDEF*, and *CEF* (this is also the design of Table 13.10). Suppose that the experimenter wished to estimate the contrasts *ABC* and *BCD*. The design, as listed, confounds *BCD*. However, if the experimenter were to switch the labels *B* and *E* of the actual treatment factors, then the important contrasts would be called *ACE* and *CDE*, both of which can be estimated in the listed design. Equivalently, the experimenter could switch the labels *B* and *E* of the listed design, so as to confound *CDE*, *ABE*, *ABCD*, *ADF*, *ACEF*, *BDEF*, and *BCF*, leaving *ABC* and *BCD* unconfounded.

13.7 Multireplicate Designs

When the experiment is large enough that each treatment combination can be observed $r > 1$ times, we have a choice of several different ways to design the experiment. If the block size can be chosen to be as large as the number of treatment combinations, then a randomized block design (Chap. 10) can be used.

If an incomplete block design is required, we could choose to use one of the standard incomplete block designs described in Chap. 11, such as a balanced incomplete block design. Alternatively, we could take a single-replicate design, confounding one or more interaction contrasts, and repeat the design r times. Alternatively, again, we could piece together r different single-replicate designs, confounding different contrasts in each. The confounding of a contrast in some but not all replicates is called *partial confounding*.

The choice between these three types of incomplete block designs involves tradeoffs concerning what is to be estimated and how accurately. The analysis of a standard incomplete block design of Chap. 11 requires that *every* contrast estimate be adjusted for blocks. Thus, while all contrasts are estimable, their least squares estimators have higher variances than they would if they were unadjusted for blocks—there is some *loss of information* on each contrast. If a balanced incomplete block design is used, then there is the same loss of information on every treatment contrast.

A repeated single-replicate design, on the other hand, loses information completely on the confounded contrasts—they are not estimable and are said to be *completely confounded*—but all contrasts orthogonal to these are unconfounded and can be estimated with no block adjustment and so with no loss of information. The estimator of any unconfounded contrast is the same as it would be in a complete block design, and with the same variance formula. The use of smaller blocks should, however, result in a smaller error variance σ^2.

The designs with partial confounding fall between these two extremes, allowing all contrasts to be estimable but with different levels of adjustment and loss of information. Complete and partial confounding are illustrated in Sects. 13.8–13.9 and are compared via an example in Sect. 13.10.

13.8 Complete Confounding: Repeated Single-Replicate Designs

If the number of treatment combinations v is divisible by the block size k, the v/k blocks of a single-replicate design can be repeated r times to give an incomplete block design with $b = rv/k$ blocks. The contrasts that are confounded in the single-replicate design are also confounded in the r-replicate design—they cannot be estimated and are said to be *completely confounded*. For all other contrasts, the estimators and corresponding variance formulae are as in a complete block design—no block adjustments are needed.

13.8.1 A Real Experiment—Decontamination Experiment

An experiment was described by M.K. Barnett and F.C. Mead, Jr. in the journal *Applied Statistics* in 1956 to explore the effect of four factors on the efficiency of a decontamination process for the removal of radioactive isotopes from liquid waste. The four treatment factors were:

A: The amount of aluminum sulphate added to the liquid waste (two levels, 0.4 g and 2.5 g per liter, coded 0, 1).

B: The amount of barium chloride added to the liquid waste (two levels, 0.4 g and 2.5 g per liter, coded 0, 1).

C: The amount of carbon added to the liquid waste (two levels, 0.08 g and 0.4 g per liter, coded 0, 1).

D: Final pH of liquid waste (two levels, 6 and 10, coded 0, 1) achieved by adding sodium hydroxide or hydrochloric acid.

The experimental units were portions of a typical laboratory waste of pH 8.3 and in which the principal radioactivity was attributable to salts of radium, thorium, and actinium. The measurements

Table 13.13 Repeated single-replicate design for a 2^4 experiment in 2×2 blocks of size 8, confounding *ABCD*. Data for the decontamination experiment are shown in parentheses

Block	Treatment combinations (response)							
I	1010	1111	0110	0000	1100	0101	0011	1001
	(183)	(350)	(188)	(881)	(225)	(298)	(1039)	(466)
II	0010	0001	0111	1000	1101	0100	1110	1011
	(650)	(1180)	(238)	(191)	(420)	(289)	(135)	(781)
III	0101	1001	1100	0000	1010	1111	0110	0011
	(273)	(890)	(370)	(834)	(193)	(389)	(163)	(1146)
IV	0001	1110	1000	0100	0111	1011	0010	1101
	(1193)	(156)	(257)	(178)	(254)	(775)	(494)	(429)

Source Barnett and Mead (1956). Copyright © 1956 Blackwell Publishers. Reprinted with permission taken after the experimental decontamination process were the counts per minute per milliliter of alpha and beta particles. Here, we will only reproduce the data for the alpha particles (see Table 13.13).

Only 8 of the 16 treatment combinations could be examined per day. Four days were available for the experiment, allowing each treatment combination to be measured twice during the course of the experiment. The experimenters anticipated day-to-day variations in the observations and decided to run a block design with $b = 4$ blocks of size $k = 8$. In fact, an unforeseen change of operators became necessary at the end of the first day. Since a block design had been used, any shift in the observations due to the operator change was absorbed into the block differences and did not affect the comparisons of the treatment combinations.

The experimenters first selected a single-replicate design in $b^* = 2$ blocks of size $k = 8$ that confounded the 4-factor interaction contrast *ABCD* (which they thought unlikely to exist in this experiment). By using this single-replicate design twice, they obtained a design with $r = 2$ observations per treatment combination and with $b = 2b^* = 4$ blocks in which all contrasts except for *ABCD* could be measured without adjustments for blocks. (The single-replicate design selected is that listed in Table 13.29.) The treatment combinations were randomly ordered within each block, and the final design is shown in Table 13.13.

The chosen model included all main effects and all 2-factor and 3-factor interactions. The block×treatment interaction was assumed to be negligible. Thus, the model was

$$\begin{aligned} Y_{hijkl} &= \mu + \theta_h + \alpha_i + \beta_j + \gamma_k + \delta_l \\ &\quad + (\alpha\beta)_{ij} + (\alpha\gamma)_{ik} + (\alpha\delta)_{il} + (\beta\gamma)_{jk} + (\beta\delta)_{jl} + (\gamma\delta)_{kl} \\ &\quad + (\alpha\beta\gamma)_{ijk} + (\alpha\beta\delta)_{ijl} + (\alpha\gamma\delta)_{ikl} + (\beta\gamma\delta)_{jkl} + \epsilon_{hijkl}\,, \\ &\epsilon_{hijkl} \sim N(0, \sigma^2) \text{ and mutually independent,} \\ &h = 1, 2, 3, 4; \quad i = 0, 1; \quad j = 0, 1; \quad k = 0, 1; \quad l = 0, 1; \\ &\qquad (h, i, j, k, l) \text{ in the design.} \end{aligned}$$

The analysis of variance table is shown in Table 13.14. There are $b = 4$ blocks in the design, but *ABCD* is the only treatment contrast confounded. The sum of squares for *ABCD* is included in the block sum of squares. The sums of squares for the other interactions and main effects can be obtained from rules 4 or 12 in Sect. 7.3. For example, the sum of squares for testing the hypothesis of negligible *AC* interaction is

$$\begin{aligned} ss(AC) &= \sum_i \sum_k \frac{y_{.i.k.}^2}{8} - \sum_i \frac{y_{.i...}^2}{16} - \sum_k \frac{y_{...k.}^2}{16} + \frac{y_{.....}^2}{32} \\ &= \frac{1}{8}(5126^2 + 4172^2 + 3248^2 + 2962^2) - \frac{1}{16}(9298^2 + 6210^2) \\ &\quad - \frac{1}{16}(8374^2 + 7134^2) + \frac{1}{32}(15508^2) \\ &= 13{,}944.5. \end{aligned}$$

Alternatively, we can calculate the sum of squares for the *AC* contrast as follows. The contrast coefficients, which are 1 if the levels of factors *A* and *C* are both high or both low, and −1 otherwise, are

$$[1,\ 1,\ -1,\ -1,\ 1,\ 1,\ -1,\ -1,\ -1,\ -1,\ 1,\ 1,\ -1,\ -1,\ 1,\ 1\,].$$

Table 13.14 Analysis of variance for the decontamination alpha-particle experiment

Source of variation	Degrees of freedom	Sum of squares	Ratio	p-value
Blocks	3	28262.500	–	–
A	1	297992.000	40.25	0.0001
B	1	1444150.125	195.07	0.0001
C	1	48050.000	6.49	0.0232
D	1	700336.125	94.60	0.0001
AB	1	570846.125	77.11	0.0001
AC	1	13944.500	1.88	0.1915
AD	1	22366.125	3.02	0.1041
BC	1	15.125	0.00	0.9646
BD	1	252050.000	34.05	0.0001
CD	1	24531.125	3.31	0.0902
ABC	1	38226.125	5.16	0.0394
ABD	1	144.500	0.02	0.8909
ACD	1	66.125	0.01	0.9260
BCD	1	5618.000	0.76	0.3984
Error	14	103645.000		
Total	31	3550243.500		

Then,

$$ss(AC) = \frac{\left(\sum_i \sum_j \sum_k \sum_l c_{ijkl}\overline{y}_{.ijkl}\right)^2}{\left(\sum_i \sum_j \sum_k \sum_l c_{ijkl}^2/r\right)} = \frac{(334)^2}{16/2} = 13{,}944.5.$$

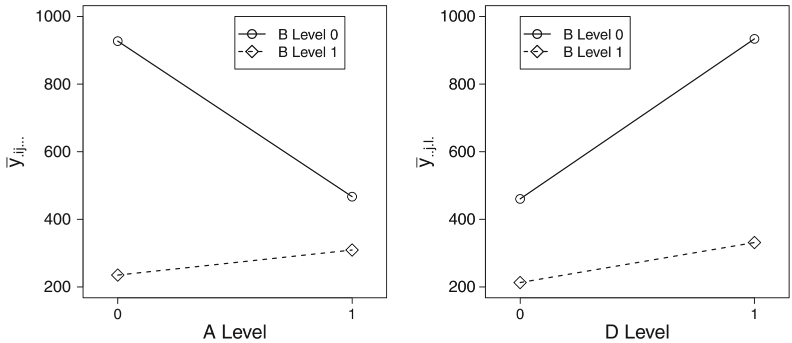
The error sum of squares is obtained by subtracting the sums of squares for the main effects and interactions from the total sum of squares, where the latter is

$$sstot = \sum_h \sum_i \sum_j \sum_k \sum_l y_{hijkl}^2 - \frac{G^2}{32}.$$

Similarly, the error degrees of freedom are obtained by subtraction.

There are fourteen hypothesis tests to be done. If each is done at a significance level 0.01, the overall level is at most $\alpha = 0.14$. At this level $F_{1,14,.01} = 8.86$, and the significant effects are the AB and BD interactions (and the main effects of A, B, and D averaged over the levels of the other factors). The hypotheses of negligible C main effect and ABC interaction would be rejected at a slightly higher significance level.

Interaction plots of the two most important interactions (AB and BD) are shown in Fig. 13.5. Figure 13.5(a) suggests that B should be set at its high level to achieve a lower radioactivity. The interaction is caused by the fact that the benefit of setting B at its high level is more marked when A is at its low level than at its high level. If B is at its high level, there is a slight preference for A to be at its low level to achieve a lower radioactivity. On the other hand, the system is more stable when A is at its high level, meaning that the radioactivity is not so sensitive to the level of B. So the choice for the setting of A is not completely obvious. Figure 13.5(b) shows a similar picture. Again B should be at its high level with a preference for D at its low level (which also produces the more stable system).



(a) AB interaction (b) BD interaction

Fig. 13.5 Interaction plots for the decontamination alpha-particle experiment

The main effect of C is not significant, but the data suggest that it should be set at its high level unless this increases the cost substantially.

Thus, the results of the analysis of variance table suggest that suitable treatment combinations for the decontamination process are 0110 or 1110. These are the same two treatment combinations that would be selected from a perusal of the data in Table 13.13. The benefit of the investigation of main effects and interactions is that it suggests directions for future experiments (raising the level of B further while keeping D fairly low and perhaps raising A a little). It also allows the chemical analysts to better understand the nature of the chemical system (see the original article of Barnett and Mead). Finally, it helps to ensure that the treatment combination that appears to be the best from the data is not just the result of error variability or a spurious observation (outlier).

13.9 Partial Confounding

Partial confounding is the term applied to a design that is formed from the combination of the blocks from different single-replicate designs with different confounding schemes. A contrast that is confounded in some replicates but not in others is said to be *partially confounded* with blocks. A partially confounded contrast can be estimated using only the data of those replicates in which it is unconfounded. Thus, the variance of the contrast estimator is inversely proportional to the number of replicates in which it is estimable.

For example, the design in Table 13.15 for a 2^3 experiment in 8 blocks of size 4 has four observations on each treatment combination. It is made up of four single-replicate designs; the first confounds the contrast from the ABC interaction, while the second confounds the AB contrast, the third confounds AC, and the fourth BC. This means that the ABC contrast is estimable from the second, third, and fourth single-replicate designs, but not the first, and the AB contrast is estimable from the first, third, and fourth single-replicate designs, but not the second. Similarly, the AC and BC contrasts are estimable from three of the four replicates. The main-effect contrasts A, B, and C are estimable from all four replicates. Consequently, all factorial treatment contrasts can be estimated, but the variance associated with each partially confounded contrast will be larger than that associated with each of the unconfounded contrasts by a factor of four-thirds. The benefit of using a partially confounded design instead of a repeated single-replicate design is that each treatment contrast is estimable, yet all totally unconfounded contrasts are still estimated with the maximum possible precision.

Table 13.15 Design and data (in parentheses) for the coil experiment

Confounded	Block	Treatment combination (response)			
ABC	I	000 (2208)	110 (2133)	101 (2459)	011 (3096)
	II	100 (2196)	010 (2086)	001 (3356)	111 (2776)
AB	III	000 (2004)	110 (2112)	001 (3073)	111 (2631)
	IV	100 (2179)	010 (2073)	101 (3474)	011 (3360)
AC	V	001 (2839)	100 (2189)	011 (3522)	110 (2095)
	VI	000 (1916)	101 (2979)	010 (2151)	111 (2500)
BC	VII	100 (2056)	000 (2010)	011 (3209)	111 (3066)
	VIII	010 (1878)	110 (2156)	001 (3423)	101 (2524)

Example 13.9.1 `Coil experiment`

C. R. Hicks, in his textbook *Fundamental Concepts in the Design of Experiments*, describes an experiment to examine the variability of outside diameters of coils of wire. There were three treatment factors of interest, as follows:

A: Two winding machines, coded 0, 1.
B: Two wire stocks, coded 0, 1.
C: Two positions on the coil, coded 0, 1.

Only four of the $v = 8$ treatment combinations could be measured at any one time. Consequently, the experiment was divided into blocks of size $k = 4$. A total of $n = 32$ observations could be taken, and a block design of $b = 8$ blocks of size $k = 4$ was needed.

It is easily verified that no balanced incomplete block design of this size exists (since $r = 4$ and $\lambda = 12/7$). A cyclic design could have been used, but cyclic designs do not have orthogonal factorial structure in general. A partially confounded design was selected, consisting of four single-replicate designs, each confounding a different interaction (*ABC*, *AB*, *AC*, and *BC*). The design and responses are shown in Table 13.15.

We use the standard model for 3 treatment factors and one block factor with no block×treatment interaction.

$$\begin{aligned} Y_{hijk} &= \mu + \theta_h + \tau_{ijk} + \epsilon_{hijk} \\ &= \mu + \theta_h + \alpha_i + \beta_j + \gamma_k + (\alpha\beta)_{ij} + (\alpha\gamma)_{ik} \\ &\quad + (\beta\gamma)_{jk} + (\alpha\beta\gamma)_{ijk} + \epsilon_{hijk}\,, \\ \epsilon_{hijk} &\sim N(0, \sigma^2) \text{ and mutually independent}\,, \\ h &= 1, \ldots, 8; \quad i = 0, 1; \quad j = 0, 1; \quad k = 0, 1; \\ &(h, i, j, k) \text{ in the design.} \end{aligned}$$

Since the main effect contrasts are not confounded in any part of the design, these can be estimated using all of the data. In addition, these contrasts are orthogonal to block contrasts, so no block adjustments are needed. The contrast for comparing the first two levels of factor B (averaged over the levels of A and C), for example, is then

$$\beta_1^* - \beta_0^*\,, \quad \text{where } \beta_j^* = \beta_j + \overline{(\alpha\beta)}_{.j} + \overline{(\beta\gamma)}_{j.} + \overline{(\alpha\beta\gamma)}_{.j.}\,,$$

and has least squares estimate

$$\hat{\beta}_1^* - \hat{\beta}_0^* = \overline{y}_{..1.} - \overline{y}_{..0.} = \frac{y_{..1.}}{16} - \frac{y_{..0.}}{16} = 2552.75 - 2555.31 = -2.56.$$

To four decimal places, this estimate is -2.5625. Equivalently, in terms of the treatment combinations the contrast is $\sum_i \sum_j \sum_k c_{ijk}\tau_{ijk}$, where the coefficients c_{ijk} in standard order and divisor $v/2$ are given by

$$\frac{1}{4}[-1,\ -1,\ \ 1,\ \ 1,\ -1,\ -1,\ \ 1,\ \ 1\ ].$$

with least squares estimate $\sum_i \sum_j \sum_k c_{ijk}\overline{y}_{.ijk} = -2.5625$. The variance associated with this contrast is

$$\frac{\sum_i \sum_j \sum_k c_{ijk}^2 \sigma^2}{4} = \frac{8\sigma^2}{16 \times 4} = \frac{\sigma^2}{8}.$$

The test of the hypothesis $H_0 : \{\beta_1^* - \beta_0^* = 0\}$ against the alternative hypothesis $H_A : \{\beta_1^* - \beta_0^* \neq 0\}$ is similar to (4.3.15) (p. 77); that is,

$$\text{reject } H_0 \text{ if } \frac{ssc_B}{msE} = \frac{(\overline{y}_{..1.} - \overline{y}_{..0.})^2}{msE/8} > F_{1,df,\alpha/m},$$

where *df* is the error degrees of freedom obtained from the analysis of variance table, which is shown in Table 13.16, m is the number of hypotheses to be tested, and $ssc_B = 8(-2.5625)^2 = 52.53$. To test the equivalent hypothesis $H_0^B : \{\beta_0^* = \beta_1^*\}$ using the rules of Sect. 7.3, we obtain the same value of ssB; that is,

$$ssB = acr \sum_j \overline{y}_{..j.}^2 - abcr\overline{y}_{....}^2 = 52.53.$$

The sum of squares for each of the other main effects can be calculated in a similar fashion. The sum of squares for the interactions can be calculated similarly, except that only three of the four replicates are used. For example, the *BC* interaction contrast can be estimated only from the first three replicates (that is, from 24 observations, not 32). Thus, the contrast $\frac{1}{2}[(\beta\gamma)_{00}^* - (\beta\gamma)_{01}^* - (\beta\gamma)_{10}^* + (\beta\gamma)_{11}^*]$

Table 13.16 Analysis of variance for the coil experiment

Source of variation	Degrees of freedom	Sum of squares	Mean square	Ratio	*p*-value
Blocks (adj)	7	303,414.14	43,344.88	-	
Blocks (unadj)	7	439,777.72	-	-	
A	1	224,282.53	224,282.53	3.97	0.0627
B	1	52.53	52.53	0.00	0.9760
C	1	6,886,688.28	6,886,688.28	121.79	0.0001
AB (adj)	1	737.04	737.04	0.01	0.9104
AC (adj)	1	416,066.67	416,066.67	7.36	0.0148
BC (adj)	1	2,667.04	2,667.04	0.05	0.8307
ABC (adj)	1	70,742.04	70,742.04	1.25	0.2789
Error	17	961,283.11	56,546.07		
Total	31	9,002,296.97			

(where $(\beta\gamma)^*_{jk} = (\beta\gamma)_{jk} + \overline{(\alpha\beta\gamma)}_{.jk}$) has least squares estimate

$$\begin{aligned}&\frac{1}{2}\left[\overline{y}_{..00} - \overline{y}_{..01} - \overline{y}_{..10} + \overline{y}_{..11}\right]\\ &\quad = \frac{1}{2}[2080.86 - 3029.76 - 2099.38 + 3006.11] \;=\; -21.085,\end{aligned}$$

where only *six* observations y_{hijk} are used in the calculation of $\overline{y}_{..jk}$ for each combination of B and C. (Use of 4 decimal places gives the more accurate value of -21.0833). In terms of the treatment combination effects τ_{ijk}, the contrast coefficients c_{ijk} in standard order and divisor $v/2$ are

$$\frac{1}{4}[1,\ -1,\ -1,\ \ 1,\ \ 1,\ -1,\ -1,\ \ 1\,],$$

and the contrast estimate is

$$\sum\sum\sum c_{ijk}(\overline{y}_{.ijk}) = -21.0833,$$

where only the *three* observations from the first three single-replicate designs are used in the calculation of each $\overline{y}_{.ijk}$. The variance associated with this contrast is

$$\frac{\sum_i \sum_j \sum_k c^2_{ijk}\sigma^2}{3} = \frac{8\sigma^2}{16 \times 3} = \frac{\sigma^2}{6}\,.$$

To test the hypothesis $H_0^{BC} : [(\beta\gamma)^*_{00} - (\beta\gamma)^*_{01} - (\beta\gamma)^*_{10} + (\beta\gamma)^*_{11} = 0]$, we reject H_0^{BC} if

$$\frac{\left(\frac{1}{2}\left[\overline{y}_{..00} - \overline{y}_{..01} - \overline{y}_{..10} + \overline{y}_{..11}\right]\right)^2}{(4/(2^2 \times 6)) \times msE} = \frac{(-21.0833)^2}{msE/6} = \frac{2667.04}{56546.07} \;<\; F_{1,17,\alpha/m}\,.$$

The complete analysis of variance table is shown in Table 13.16. The adjusted mean square for blocks, which is 43,344.88, is smaller than the mean square for error. Thus, blocking was not helpful in this experiment. □

13.10 Comparing the Multireplicate Designs

For some block sizes, we have a choice of possible designs for a multireplicate factorial experiment. For example, suppose that a design is required with blocks of size $k = 4$ for a factorial experiment involving 3 factors, each having two levels (so $v = 8$). Practical considerations dictate that at most $b = 14$ blocks can be used. The contrasts of interest are all of the main-effect and interaction contrasts with coefficients ± 1 as listed in Table 13.2 (p. 436). Three possible ways of designing the experiment with 14 blocks are as follows.

Design Possibility 1

The first possibility is to use a balanced incomplete block design, since one exists with $\lambda = 3$ and $r = 7$, $b = 14$, $v = 8$, $k = 4$. The design (before randomization) is given in Table 13.17, where the blocks are shown as columns. The treatment labels 1–8 of the design are randomly assigned to the $v = 8$ treatment combinations (000, 001, . . ., 111) to obtain a balanced incomplete block design suitable for a factorial experiment.

Table 13.17 A balanced incomplete block design with 8 treatment labels and 14 blocks of size 4

Blocks													
I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV
1	5	1	3	1	2	1	2	1	3	1	2	1	2
2	6	2	4	3	4	4	3	2	4	3	4	4	3
3	7	7	5	6	5	6	5	5	7	5	6	5	6
4	8	8	6	8	7	7	8	6	8	7	8	8	7

If we let the effect of treatment combination *ijk* be denoted by τ_{ijk}, then following Sect. 11.4.3, the least squares estimator of a contrast $\Sigma\Sigma\Sigma c_{ijk}\tau_{ijk}$ is $\Sigma\Sigma\Sigma c_{ijk}\widehat{\tau}_{ijk} = \frac{k}{\lambda v}\Sigma\Sigma\Sigma c_{ijk}Q_{ijk}$, where $Q_{ijk} = T_{ijk} - \frac{1}{k}\sum_h n_{hijk}B_h$, and T_{ijk} is the total of the observations on treatment combination *ijk*, B_h is the total of the observations in the hth block, and n_{hijk} is 1 if treatment combination *ijk* is in block h, and otherwise n_{hijk} is 0. Taking all contrast coefficients c_{ijk} equal to $+1$ or -1, the variance of the least squares estimator, $\Sigma\Sigma\Sigma c_{ijk}\widehat{\tau}_{ijk}$, is

$$\text{Var}\left(\sum_i\sum_j\sum_k c_{ijk}\hat{\tau}_{ijk}\right) = \frac{k}{\lambda v}\sum_i\sum_j\sum_k c_{ijk}^2\,\sigma^2 = \frac{8\sigma^2}{6}\,, \tag{13.10.3}$$

and this is the same for all main-effect and interaction contrasts for a 2^3 experiment.

Design Possibility 2

For the same experiment, suppose that the 3-factor interaction *ABC* is expected to be negligible and is of no interest. Then the balanced incomplete block design discussed above is not ideal, because *ABC* contrast is measured with the same precision as the main-effect and 2-factor interaction contrasts. Suppose, instead, we decide to confound the *ABC* contrast in each of seven replicates. Using the equations

$$a_1 + a_2 + a_3 = 0 \text{ or } 1 \pmod 2,$$

the following single-replicate design in two blocks would be obtained:

Block I: 000 011 101 110
Block II: 001 010 100 111

The design in $b = 14$ blocks is obtained by repeating these two blocks $r = 7$ times. Since *ABC* is confounded in every replicate, it is not estimable—it cannot be measured. *ABC* is said to be *completely confounded*. All other orthogonal contrasts (including the main-effect and 2-factor interaction contrasts) are unconfounded, so can be estimated without adjusting for blocks.

Let $\Sigma\Sigma\Sigma c_{ijk}\tau_{ijk}$ be a contrast (with all coefficients ±1) measuring a 2-factor interaction or a main effect. Then its least squares estimator is $\Sigma\Sigma\Sigma c_{ijk}\overline{Y}_{.ijk}$, where the average is taken over the 7 replicates or repeated pairs of blocks. The corresponding variance is

$$\text{Var}\left(\Sigma\Sigma\Sigma c_{ijk}\hat{\tau}_{ijk}\right) = \text{Var}\left(\Sigma\Sigma\Sigma c_{ijk}\overline{Y}_{.ijk}\right) = \frac{8\sigma^2}{7}. \tag{13.10.4}$$

Table 13.18 Partial confounding of *ABC*, *AB*, *AC*, *BC* contrasts in a design with 14 blocks of size 4 for 3 factors at two levels each

Replicates	Confounded	Blocks	Treatment combinations			
I–IV	*ABC*	I, III, V, VII	000	011	101	110
		II, IV, VI, VIII	001	010	100	111
V	*AB*	IX	000	001	110	111
		X	100	101	010	011
VI	*AC*	XI	000	010	101	111
		XII	001	011	100	110
VII	*BC*	XIII	000	011	100	111
		XIV	001	010	101	110

Comparing (13.10.4) with (13.10.3), we see that the effect of losing all of the information on the *ABC* contrast is to reduce the variance of all other factorial contrasts from $8\sigma^2/6$ to $8\sigma^2/7$.

Design Possibility 3

Instead of repeating the same design seven times as was done above, we could try to spread the loss of information due to confounding across several of the interaction contrasts by using partial confounding. Suppose that we take four copies of the two blocks that confound *ABC*, together with one pair of blocks that confounds *AB*, one pair that confounds *AC*, and one pair that confounds *BC*. This seven-replicate design is shown in Table 13.18.

The *ABC* contrast is confounded in replicates I–IV, but can be estimated (without block adjustments) from replicates V–VII (that is, from Blocks IX–XIV, or three pairs of blocks). The variance of the *ABC* contrast estimator is then $8\sigma^2/3$, compared with $8\sigma^2/6$ in the balanced incomplete block design—it is completely confounded in the second design.

Each 2-factor interaction contrast can be estimated without block adjustments from the six replicates, or pairs of blocks, in which it is not confounded. The variances of their least squares estimators are then $8\sigma^2/6$, the same as in the balanced incomplete block design, but worse than the value $8\sigma^2/7$ for the design with *ABC* completely confounded.

The main effects can be estimated from all seven replicates. The variances of their contrast estimators are all $8\sigma^2/7$, the same as for the second design, but better than the value of $8\sigma^2/6$ for the balanced incomplete block design.

Summary

A summary of the variances of the least squares estimators of the factorial contrasts is given in Table 13.19. No one design is the best for all seven factorial effects. The choice of design would depend upon the importance of estimating the *ABC* contrast relative to the 2-factor and main-effect contrasts.

Table 13.19 Variances of contrast estimators $\Sigma\Sigma\Sigma c_{ijk}\tau_{ijk}$ (with all coefficients ± 1) measuring a 2-factor interaction or a main effect for three design possibilities for $v = 8, r = 7, b = 14, k = 4$

Contrast	Design 1	Design 2	Design 3
	BIBD	Complete Confounding of *ABC*	Partial Confounding of *ABC*, *AB*, *AC*, *BC*
ABC	$\frac{8\sigma^2}{6}$	not estimable	$\frac{8\sigma^2}{3}$
AB, *AC*, *BC*	$\frac{8\sigma^2}{6}$	$\frac{8\sigma^2}{7}$	$\frac{8\sigma^2}{6}$
A, B, C	$\frac{8\sigma^2}{6}$	$\frac{8\sigma^2}{7}$	$\frac{8\sigma^2}{7}$

We have not exhausted all the possible designs that can be obtained by partial confounding. For example, one could confound the two-factor interactions in two pairs of blocks and the three-factor interaction in one pair of blocks, or alternatively, one could confound each of the seven factorial contrasts in turn in one pair of blocks. (This latter option gives design possibility 1.) In every case, the smaller contrast variances will coincide with the contrasts that are confounded less often, the contrast variance being $8\sigma^2/r$ if the contrast is unconfounded in r replicates.

13.11 Using SAS Software

Analyzing factorial experiments with confounding using the SAS software is straightforward for the types of designs discussed in this chapter. The SAS statements required for the analysis are the same as those outlined in Chaps. 6, 7, and 10. Using the `GLM` procedure, the blocking factor is listed in the `MODEL` statement first, so that the Type I sums of squares for factorial effects are appropriately adjusted for blocks.

Any effect that is completely confounded—including effects confounded in a single replicate design—should *not* be included in the `MODEL` statement. If included after the blocking factor, a completely confounded effect would show zero degrees of freedom under the Type I and Type III sums of squares. The corresponding degree of freedom would already be accounted for under block effects. Partially confounded effects, however, should be included in the model statement as illustrated in the following example.

Table 13.20 SAS program for analysis of an experiment with partial confounding—the coil experiment

```
DATA COIL;
 INPUT BLOCK A B C Y;
 LINES;
  1   0 0 0   2208
  1   1 1 0   2133
  1   1 0 1   2459
  1   0 1 1   3096
  2   1 0 0   2196
  :   : : :     :
  8   1 1 0   2156
  8   0 0 1   3423
  8   1 0 1   2524
;
PROC GLM;
 CLASS BLOCK A B C;
 MODEL Y = BLOCK A B C A*B A*C B*C A*B*C;
 ESTIMATE 'A' A -1 1;
 ESTIMATE 'B' B -1 1;
 ESTIMATE 'C' C -1 1;
 ESTIMATE 'AB' A*B 1 -1 -1 1 / DIVISOR=2;
 ESTIMATE 'AC' A*C 1 -1 -1 1 / DIVISOR=2;
 ESTIMATE 'BC' B*C 1 -1 -1 1 / DIVISOR=2;
 ESTIMATE 'ABC' A*B*C -1 1 1 -1 1 -1 -1 1 / DIVISOR=4;
```

Fig. 13.6 SAS program partial output illustrating partial confounding—the coil experiment

Results Viewer - SAS Output

The GLM Procedure
Dependent Variable: Y

Source	DF	Sum of Squares	Mean Square	F Value	Pr > F
Model	14	8041013.854	574358.132	10.16	<.0001
Error	17	961283.115	56546.066		
Corrected Total	31	9002296.969			

Source	DF	Type I SS	Mean Square	F Value	Pr > F
BLOCK	7	439777.719	62825.388	1.11	0.4003
A	1	224282.531	224282.531	3.97	0.0627
B	1	52.531	52.531	0.00	0.9760
C	1	6886688.281	6886688.281	121.79	<.0001
A*B	1	737.042	737.042	0.01	0.9104
A*C	1	416066.667	416066.667	7.36	0.0148
B*C	1	2667.042	2667.042	0.05	0.8307
A*B*C	1	70742.042	70742.042	1.25	0.2789

Source	DF	Type III SS	Mean Square	F Value	Pr > F
BLOCK	7	303414.135	43344.876	0.77	0.6225

Parameter	Estimate	Standard Error	t Value	Pr > |t|
A	-167.437500	84.0729338	-1.99	0.0627
B	-2.562500	84.0729338	-0.03	0.9760
C	927.812500	84.0729338	11.04	<.0001
AB	11.083333	97.0790619	0.11	0.9104
AC	-263.333333	97.0790619	-2.71	0.0148
BC	-21.083333	97.0790619	-0.22	0.8307
ABC	-108.583333	97.0790619	-1.12	0.2789

Example 13.11.1 `Partial confounding—Coil experiment, continued`

Table 13.20 contains a SAS program for analysis of the coil experiment data. Corresponding output is given in Fig. 13.6. The coil experiment was a four-replicate 2^3 experiment with partial confounding—each of the four interaction effects was confounded in one of the four replicates. In the `GLM` procedure, the blocking factor is entered into the `MODEL` statement first. As a result, the Type I sum of squares for blocks is unadjusted for treatment effects, whereas the Type I sums of squares for each treatment interaction effect is adjusted for block effects. The Type III sum of squares for blocks is adjusted for treatment effects, so can be used to assess the usefulness of blocking in this experiment.

The divisors used in the `ESTIMATE` statements of the `GLM` procedure cause use of divisor $v/2 = 4$ for the contrast coefficients c_{ijk} for each contrast. Thus, all contrasts would have been estimated with the same variance had there been no partial confounding. Because each interaction contrast is confounded in one of the four replicates, the variance of each interaction contrast estimator is larger than each main effect contrast estimator by a factor of four-thirds (i.e. $97.079^2/84.073^2 = 1.333$). □

13.12 Using R Software

Analyzing factorial experiments with confounding using the R software is straightforward for the types of designs discussed in this chapter. The R statements required for the analysis are the same as those outlined in Chaps. 6, 7, and 10. Using the linear models function `lm`, the blocking factor is entered into the model first, so that the Type I sums of squares for factorial effects are appropriately adjusted for blocks.

Any effect that is completely confounded—including effects confounded in a single replicate design—should *not* be included in the model. If included after the blocking factor, a completely confounded effect would show zero degrees of freedom under the Type I and Type III sums of squares. The corresponding degree of freedom would already be accounted for under block effects. Partially confounded effects, however, should be included in the model statement as illustrated in the following example.

Table 13.21 R program for analysis of an experiment with partial confounding—the coil experiment

```
coil.data = read.table("data/coil.txt", header=T)
head(coil.data, 3)
# Create factor variables
coil.data = within(coil.data,
   {fBlock = factor(Block); fA = factor(A);
    fB = factor(B); fC = factor(C) })

# Analysis of variance
options(contrasts = c("contr.sum", "contr.poly"))
model1 = lm(y ~ fBlock + fA*fB*fC, data=coil.data)
anova(model1)
drop1(model1, ~., test="F")


# Contrast estimates, confidence intervals, and tests
library(lsmeans)
lsmA = lsmeans(model1, ~ fA)
summary(contrast(lsmA, list(A=c(-1, 1))), infer=c(T,T))
lsmB = lsmeans(model1, ~ fB)
summary(contrast(lsmB, list(B=c(-1, 1))), infer=c(T,T))
lsmC = lsmeans(model1, ~ fC)
summary(contrast(lsmC, list(C=c(-1, 1))), infer=c(T,T))
lsmAB = lsmeans(model1, ~ fB:fA)
summary(contrast(lsmAB, list(AB=c(1,-1,-1, 1)/2)), infer=c(T,T))
lsmAC = lsmeans(model1, ~ fC:fA)
summary(contrast(lsmAC, list(AC=c(1,-1,-1, 1)/2)), infer=c(T,T))
lsmBC = lsmeans(model1, ~ fC:fB)
summary(contrast(lsmBC, list(BC=c(1,-1,-1, 1)/2)), infer=c(T,T))
lsmABC = lsmeans(model1, ~ fC:fB:fA)
summary(contrast(lsmABC, list(ABC=c(-1,1,1,-1,1,-1,-1, 1)/4)),
        infer=c(T,T))
```

Table 13.22 R program partial output illustrating partial confounding—the coil experiment

```
> model1 = lm(y ~ fBlock + fA*fB*fC, data=coil.data)
> anova(model1)

Analysis of Variance Table
Response: y
          Df  Sum Sq Mean Sq F value  Pr(>F)
fBlock     7  439778   62825    1.11   0.400
fA         1  224283  224283    3.97   0.063
fB         1      53      53    0.00   0.976
fC         1 6886688 6886688  121.79 3.6e-09
fA:fB      1     737     737    0.01   0.910
fA:fC      1  416067  416067    7.36   0.015
fB:fC      1    2667    2667    0.05   0.831
fA:fB:fC   1   70742   70742    1.25   0.279
Residuals 17  961283   56546

> # Contrast estimates
> library(lsmeans)
> lsmB = lsmeans(model1, ~ fB)
> summary(contrast(lsmB, list(B=c(-1, 1))), infer=c(T,T))
 contrast estimate     SE df lower.CL upper.CL t.ratio p.value
 B         -2.5625 84.073 17  -179.94   174.82   -0.03  0.9760

Results are averaged over the levels of: fBlock, fA, fC
Confidence level used: 0.95

> lsmBC = lsmeans(model1, ~ fC:fB)
> summary(contrast(lsmBC, list(BC=c(1,-1,-1, 1)/2)), infer=c(T,T))
 contrast estimate     SE df lower.CL upper.CL t.ratio p.value
 BC        -21.083 97.079 17   -225.9   183.74  -0.217  0.8307

Results are averaged over the levels of: fBlock, fA
Confidence level used: 0.95

> lsmABC = lsmeans(model1, ~ fC:fB:fA)
> summary(contrast(lsmABC, list(ABC=c(-1,1,1,-1,1,-1,-1, 1)/4)), infer=c(T,T))

 contrast estimate     SE df lower.CL upper.CL t.ratio p.value
 ABC       -108.58 97.079 17   -313.4   96.236  -1.119  0.2789

Results are averaged over the levels of: fBlock
Confidence level used: 0.95
```

Example 13.12.1 `Partial confounding—Coil experiment, continued`

Table 13.21 contains an R program for analysis of the coil experiment data. Some of the corresponding output is given in Table 13.22. The coil experiment was a four-replicate 2^3 experiment with partial confounding—each of the four interaction effects was confounded in one of the four replicates. In the second block of code in Table 13.21, the `lm` function fits the standard model involving block effects and factorial effects, saving the results as `model1`. Subsequently, the functions `anova` and `drop1` generate the type 1 and type 3 analysis of variance tables, respectively, the first of which is shown in the top of Table 13.22. The blocking factor is entered into the model first. As a result, the type 1 sum of squares for blocks is unadjusted for treatment effects, whereas the Type I sums of squares for each

Table 13.23 Projectile experiment data, confounding *ABCD*

	Day 1			Day 2	
Run	TC	y_{1ijkl}	Run	TC	y_{2ijkl}
1	0000	97	13	0001	75
7	0011	26	11	0010	39
5	0101	53	9	0100	68
3	0110	15	15	0111	−16
6	1001	145	10	1000	151
4	1010	100	16	1011	97
2	1100	150	14	1101	141
8	1111	54	12	1110	66

Source Johnson and Leone (1977). Copyright © 1977 Johnson and Leone. Reprinted with permission

treatment interaction effect is adjusted for block effects. The Type III sum of squares for blocks (not shown) is adjusted for treatment effects, so can be used to assess the usefulness of blocking in this experiment.

In the third block of code, for each factorial effect contrast, the `summary` and `contrast` functions of `lsmeans` are coupled to compute the contrast estimate, standard error, t-test, and 95% confidence interval. Since each main effect contrast is averaged over the four combinations of two other factors, specifying a pair of coefficients ± 1 yields contrast coefficients c_{ijk} with divisor $v/2 = 4$. Likewise, each two-factor interaction contrast is averaged over the two levels of the third factor, so specifying four coefficients $\pm 1/2$ again yields contrast coefficients c_{ijk} with divisor $v/2 = 4$. The three factor interaction contrast coefficients c_{ijk} have divisor $v/2 = 4$ as directly specified. Output for the main effect of B, the *BC* interaction, and the *ABC* interaction contrasts are shown in the bottom of Table 13.22.

Since all contrasts considered here have contrast coefficients $c_{ijk} = \pm 1/4$, all contrasts would have been estimated with the same variance had there been no partial confounding. Because each interaction contrast is confounded in one of the four replicates, the variance of each interaction contrast estimator is larger than that of each main effect contrast estimator by a factor of four-thirds. Correspondingly, the squares of the standard errors are in this ratio. □

Exercises

1. Construct a single-replicate 2^3 design confounding *AB* with blocks. In other words, list the treatment combinations block by block.

2. Construct a single replicate 2^5 design confounding *ABC* and *CDE*. Determine the other effect that is confounded.

3. **Projectile experiment**

   N.L. Johnson and F.C. Leone, in their 1977 book *Statistics and Experimental Design in Engineering and the Physical Sciences*, described a single-replicate 2^4 experiment concerning the performance of a new rifle under test. Under study were the effects on projectile velocity of the factors charge weight (A), projectile weight (B), propellant web (C), and weapon (D), where two rifles were used. The design included two blocks each of size eight, corresponding to the two days on which data were collected, confounding *ABCD*. The coded velocity data are given in Table 13.23.

(a) Fit a model including block effects, treatment main effects, and 2-factor interactions. Use residual plots to check the standard model assumptions.
(b) Conduct the analysis of variance, and discuss the results.
(c) Construct simultaneous confidence intervals for any interesting treatment contrasts using an appropriate method of multiple comparisons.
(d) Reanalyze the data using the Voss–Wang method, including all estimable treatment effects in the analysis.

4. **Field experiment, continued**

(a) For the field experiment of Example 13.3.1, p. 437, verify that the sum of squares and the contrast estimate for *BD* are as shown in Table 13.5, p. 438.
(b) Draw the *BD* interaction plot. Does this plot also suggest that *B* and *D* should be at their low levels?
(c) Suppose that the experimenters had expected all of the 3-factor interactions to be negligible and had omitted the corresponding terms from the model (instead of those involving *AD*). Reanalyze the experiment accordingly. What would have been concluded?
(d) Apply the Voss–Wang method to analyze the data of the field experiment. Relevant information is given in Table 13.5, p. 438.

5. Suggest a confounding scheme for a 2^6 experiment in 8 blocks of 8, assuming that all 2-factor interactions are to be estimated, as are the 3-factor interactions involving both A and F. List all effects confounded. List the treatment combinations in the design block by block.

6. Suggest a confounding scheme for a 2^8 experiment in 16 blocks of 16, assuming that all 2-factor and 3-factor interactions are to be estimated. List all effects confounded. List the treatment combinations in Block I and in two other blocks.

7. **Mangold experiment, continued**

(a) For the mangold experiment of Sect. 13.5, verify that the sum of squares and the contrast estimate for *CD* are as shown in Table 13.12, p. 449.
(b) Draw the *CD* interaction plot. Does this plot agree with the factor levels suggested in Sect. 13.5 for increasing the yield?
(c) Check that the assumption of normality of the error variables is satisfied. Also check that the variances of the errors appear to be equal for each level of the four factors.
(d) Draw a half-normal probability plot of all of the contrast estimates (including the higher-order interactions). Does it appear that the experimenters made the correct assumptions of negligible higher-order interactions?

8. **Decontamination experiment—Beta particles**

An experiment was described by M. K. Barnett and F. C. Mead, Jr. in the journal *Applied Statistics* in 1956 to explore the effect of four factors on the efficiency of a decontamination process for the removal of radioactive isotopes from liquid waste. The measurements taken after the decontamination process were the counts per minute per milliliter of alpha and beta particles. Data for the alpha particles and further description of the experiment were given in Sect. 13.8.1, p. 452. We consider here part of the data for the beta particles, shown in Table 13.24. The four treatment factors were:

Table 13.24 Randomized design for a 2^4 experiment in 2 blocks of size 8, confounding *ABCD*. Data for the decontamination beta-particle experiment are shown in parentheses.

Block	Treatment combinations (response)							
I	1010	1111	0110	0000	1100	0101	0011	1001
	(716)	(686)	(498)	(1437)	(527)	(579)	(1433)	(906)
II	0010	0001	0111	1000	1101	0100	1110	1011
	(1024)	(1364)	(475)	(574)	(664)	(579)	(507)	(1130)

Source Barnett and Mead (1956). Copyright © 1956 Blackwell Publishers. Reprinted with permission

A : 0.4 g and 2.5 g per liter of aluminum sulphate (coded 0, 1);

B : 0.4 g and 2.5 g per liter of barium chloride (coded 0, 1);

C : 0.08 g and 0.4 g per liter of carbon (coded 0, 1);

D : Final pH of liquid waste (6 and 10, coded 0, 1).

The experimenters selected a design in $b = 2$ blocks of $k = 8$ that confounded the four-factor interaction contrast *ABCD*. The fitted model included all main-effects and all 2-factor and 3-factor interactions. The block×treatment interaction was assumed to be negligible.

(a) Use a half-normal probability plot to identify the important contrasts.
(b) Use the method of Voss–Wang to check your selection in part (a).
(c) Draw an interaction plot of any interaction that appears to be nonnegligible by either analysis.
(d) Looking at the results of your analysis, which settings of the factors would you recommend for reducing the beta particle counts?
(e) Suppose that the experimenters had believed before the experiment that the three-factor interactions were all negligible. What would the analysis of variance table have looked like? Would your recommendations have been any different?

9. **Penicillin experiment**

An experiment is described in Example 9.2 of the book *Design and Analysis of Industrial Experiments* edited by O. L. Davies that investigates the effects of various factors on the yield of penicillin in surface culture experiments. The five factors of interest were added to the nutrient medium, which was inoculated with a spore suspension of *P. Chrysogenum*. The spores rise to the surface, causing the growth of mycelium accompanied by the formation of penicillin. The factors and their levels were corn steep liquor (factor A, 2% and 3% strength), lactose (factor B, 2% and 3% strength), precursor (factor C, 0% and 0.05%), sodium nitrate (factor D, 0% and 0.3%), and glucose (factor E, 0% and 0.5%). Only 16 of the 32 treatment combinations could be carried out at one time, and the experimenters decided to observe 16 treatment combinations in one week and the remaining 16 in the following week. Large week-to-week variations were known to exist, and therefore the experiment was designed as a block design with two blocks, confounding the 5-factor interaction *ABCDE*. The observed yields of penicillin are shown in Table 13.25. Prior to the experiment, it was believed that all that all 3- and 4-factor interactions would be negligible, and also that the *CE* interaction would be important.

(a) Analyze the data, assuming that all 3- and 4-factor interactions are negligible. Do not forget to check the assumptions on the model.

Table 13.25 Data for the penicillin experiment

Block I		Block II	
Treatment combination	Yield	Treatment combination	Yield
00000	142	00001	106
00011	101	00010	148
00101	113	00100	185
00110	200	00111	130
01001	88	01000	129
01010	146	01011	140
01100	200	01101	166
01111	145	01110	215
10001	106	10000	114
10010	108	10011	114
10100	162	10101	88
10111	83	10110	164
11000	109	11001	98
11011	72	11010	195
11101	79	11100	172
11110	118	11111	110

Source Data adapted from *The Design and Analysis of Industrial Experiments* Second edition, 1979. Editor O. L. Davies. Published by Longman Group Ltd

(b) The experimenters decided to use logarithms of the data. Does your assumption check in part (a) confirm that this should be done? If so, reanalyze the data and state your conclusions.
(c) Using the logarithms of the data, draw a half-normal probability plot of the contrast estimates without using any knowledge that the higher-order interactions are likely to be negligible. Do your conclusions remain the same? Which analysis do you prefer? Why?

10. **Peas experiment**

The following experiment was run at Biggelswade, in England, and reported by F. Yates in his 1935 paper *Complex Experiments*. The three treatment factors were the standard fertilizers, nitrogen, phosphate, and potassium (factors N, P, and K) each at two levels. The experimental area was divided into $b = 6$ blocks of $1/70$ of an acre. Each block was large enough for four plots on which a certain variety of pea was sown, and the fertilizer combinations shown in Table 13.26 were added. The design consists of three identical single-replicate designs each of which confounds the 3-factor interaction NPK. Each block has been separately randomized.

(a) Estimate the treatment contrasts for all main effects and interactions.
(b) Calculate the analysis of variance table for this experiment and test all relevant hypotheses. State the overall significance level.
(c) Draw interaction plots for any important interactions. Give a set of 95% confidence intervals for the main-effect contrasts, if appropriate.
(d) State your overall recommendations about the fertilizers in this experiment. Would you recommend a followup experiment? If so, what would you investigate?

11. **Field experiment, continued**
The field experiment was described in Example 13.3.1, p. 437. There were four treatment factors

Table 13.26 Data for the peas experiment

Block	Treatment combinations (Yield)		Block	Treatment combinations (Yield)	
I	011 (49.5)	000 (46.8)	II	100 (62.0)	001 (45.5)
	110 (62.8)	101 (57.0)		111 (48.8)	010 (44.2)
III	100 (59.8)	001 (55.5)	IV	110 (52.0)	101 (49.8)
	111 (58.5)	010 (56.0)		000 (51.5)	011 (48.8)
V	010 (62.8)	100 (69.5)	VI	101 (57.2)	011 (53.2)
	111 (55.8)	001 (55.0)		110 (59.0)	000 (56.0)

Source Yates (1935). Copyright © 1935 Blackwell Publishers. Reprinted with permission

Table 13.27 Data for the field experiment, by block and treatment combination (TC)

Block I		Block II		Block III		Block IV	
TC	y_{1ijkl}	TC	y_{2ijkl}	TC	y_{3ijkl}	TC	y_{4ijkl}
0000	58	0001	55	0000	57	0001	50
0011	51	0010	45	0011	56	0010	39
0101	44	0100	42	0101	43	0100	47
0110	50	0111	36	0110	39	0111	43
1001	43	1000	53	1001	52	1000	42
1010	50	1011	55	1010	52	1011	44
1100	41	1101	41	1100	42	1101	34
1111	44	1110	48	1111	54	1110	52

Source Experimental Designs, Second Edition, by W.G. Cochran and G.M. Cox, Copyright © 1957, John Wiley & Sons, New York. Adapted by permission of John Wiley & Sons, Inc

(A, B, C, and D) at two levels each, and the $v = 16$ treatment combinations were observed twice. Each of the $r = 2$ sets of treatment combinations were divided into blocks of size 8. The first two blocks, which confounded the *ABCD* interaction, were shown in Table 13.4, p. 437. The complete design, which is shown in Table 13.27, consisted of two such single-replicate designs.

(a) Calculate the analysis of variance table for this experiment. Now that $r = 2$, there is an estimate for error variability. Test any hypotheses of interest. Are the results similar to those obtained from the first two blocks only?

(b) Draw any interaction plots of interest. If the yield is to be increased, what recommendations would you make about the levels of the factors?

12. Construct a four-replicate 2^3 design in eight blocks of size four, partially confounding each interaction effect. Compare the variance of each interaction contrast with that of each main effect, using divisor $v/2 = 4$ for each contrast.

13. **Catalytic reaction experiment**

J.R. Bainbridge, in his 1951 article in the journal *Industrial and Engineering Chemistry*, described a factorial experiment conducted at a small plant carrying out a catalytic gaseous synthesis reaction to remove the product as a liquid solution. A 2-replicate 2^3 experiment was conducted to study the effects of converter reaction temperature (factor A), throughput rate through the converter (factor B), and the concentration of the active ingredient in the makeup gas (factor C) on each of several response variables, including the strength of the product solution (y_{hijk}). The design was composed of four blocks of size four, with the *ABC* interaction completely confounded. The design

Table 13.28 Data for the catalytic reaction experiment

Run	Block	TC	y_{hijk}	Run	Block	TC	y_{hijk}
1	1	011	89.5	9	3	010	86.2
2	1	101	84.2	10	3	100	81.8
3	1	110	85.2	11	3	001	90.4
4	1	000	89.9	12	3	111	83.6
5	2	010	85.1	13	4	110	75.3
6	2	111	83.5	14	4	000	84.6
7	2	001	90.8	15	4	011	86.7
8	2	100	81.8	16	4	101	82.2

Source Reprinted with permission from Bainbridge (1951). Copyright © 1951 *American Chemical Society*

and data are provided in Table 13.28, including the run order. (The observations in Table 13.28 are "uncoded," each value being 80 plus one-tenth the coded value given by Bainbridge.)

(a) Based on the run order, discuss how the design was probably randomized.
(b) Fit an appropriate model, and use residual plots to check the standard model assumptions.
(c) Conduct the analysis of variance, and discuss the results.
(d) Using a simultaneous confidence level of 95% for all six factorial effects, construct confidence intervals for those effects found to be significant in the analysis of variance.

14. **Catalytic reaction experiment, continued**

In the experiment described in Exercise 13, the covariate "makeup gas purity" was measured. The covariate values were 17, 12, 10, 10, 13, 14, 10, 16, 12, 13, 13, 11, 16, 11, 12, and 11, corresponding to runs 1–16, respectively. Repeat Exercise 13, but for an analysis of covariance.

15. **Design of a follow-up experiment**

An experiment was run in 2007 by Joanne Sklodowski, Josh Svenson, Adam Dallas, Tim Degenero and Paul Cotellesso to examine the compressive strength of various mortar mixes. They examined the effects of four factors: Amount of water (Factor A, 0.75lb and 0.85lb), Sand type (Factor B, play sand and medium grain sand), Temperature of water (Factor C, 58 and 96 °C), Cure time (Factor D, 4 and 6 days). The type and age of cement and the mixing time were held fixed throughout the experiment.
The ingredients were mixed and poured into a cylindrical mold. After the allotted curing time, the cylinder was crushed on a compression machine. The maximum pressure exerted before the cylinder failed was recorded in pounds per square inch (psi).

(a) The analysis of the experiment showed large effects of BCD, B, C, AB. Design a follow-up experiment to examine the interactions AB, BC, BD, CD and BCD, as well as all main effects. You can only take $r = 1$ observation on each treatment combination and you need to run the experiment in 4 blocks of 4. Write out two of the four blocks of your design and state how to find the other two.
(b) Write down a model for this experiment. Write out the "Degrees of Freedom" columns of the analysis of variance table.

16. **Design of an experiment**

(a) Design an experiment with five factors A, B, C, D, E, each having two levels, in 4 blocks of 8 and $r = 1$ observation per treatment combination. Make sure that you can estimate all main effects, all two-factor interactions, as well as all three-factor interactions that involve *both* D and E.

(b) Write out the 8 treatment combinations in Block I, and indicate how to find the treatment combinations in the other blocks. Illustrate this with two treatment combinations in the second block.

(c) Write out the degrees of freedom column for the analysis of variance table corresponding to the model that you would fit.

Table 13.29 Confounding schemes for 2^p experiments in $b = 2^s$ blocks of size $k = 2^{p-s}$. For each design, s independent generators are underlined, and s corresponding equations are given. To obtain Block I of a design, list all k combinations of the first a_i's shown, then use the equations modulo 2 to complete each treatment combination

2^p	b	k	Confounded Contrasts	Block I
2^3	2	4	$\underline{ABC}$	a_1, a_2 $a_3 = a_1 + a_2$
2^4	2	8	$\underline{ABCD}$	a_1, a_2, a_3 $a_4 = a_1 + a_2 + a_3$
2^4	4	4	$\underline{AC}, \underline{ABD}, BCD$	a_1, a_2 $a_3 = a_1$ $a_4 = a_1 + a_2$
2^5	2	16	$\underline{ABCDE}$	a_1, a_2, a_3, a_4 $a_5 = a_1 + a_2 + a_3 + a_4$
2^5	4	8	$\underline{ABCD}, \underline{ABE}, CDE$	a_1, a_2, a_3 $a_4 = a_1 + a_2 + a_3$ $a_5 = a_1 + a_2$
2^5	8	4	$\underline{AC}, \underline{BD}, ABCD,$ $\underline{ABE}, BCE, ADE, CDE$	a_1, a_2 $a_3 = a_1$ $a_4 = a_2$ $a_5 = a_1 + a_2$
2^6	2	32	$\underline{ABCDEF}$	a_1, a_2, a_3, a_4, a_5 $a_6 = a_1 + a_2 + a_3 + a_4 + a_5$
2^6	4	16	$\underline{ABCD}, \underline{CDEF}, ABEF$	a_1, a_2, a_3, a_5 $a_4 = a_1 + a_2 + a_3$ $a_6 = a_3 + a_4 + a_5$
2^6	8	8	$\underline{BCD}, \underline{ABE}, ACDE,$ $\underline{ABCF}, ADF, CEF, BDEF$	a_1, a_2, a_3 $a_4 = a_2 + a_3$ $a_5 = a_1 + a_2$ $a_6 = a_1 + a_2 + a_3$
2^7	4	32	$\underline{ABCDE}, \underline{ABFG}, CDEFG$	a_1, a_2, a_3, a_4, a_6 $a_5 = a_1 + a_2 + a_3 + a_4$ $a_7 = a_1 + a_2 + a_6$
2^7	8	16	$\underline{ABCD}, \underline{CDEF}, ABEF,$ $\underline{ACEG}, BDEG, ADFG, BCFG$	a_1, a_2, a_3, a_5 $a_4 = a_1 + a_2 + a_3$ $a_6 = a_3 + a_4 + a_5$ $a_7 = a_1 + a_3 + a_5$
2^7	16	8	$\underline{ABC}, \underline{CDE}, ABDE,$ $\underline{BDF}, ACDF, BCEF, AEF,$ $\underline{ADG}, BCDG, ACEG, BEG,$ $ABFG, CFG, ABCDEFG, DEFG$	a_1, a_2, a_4 $a_3 = a_1 + a_2$ $a_5 = a_3 + a_4$ $a_6 = a_2 + a_4$ $a_7 = a_1 + a_4$