Fractional Factorial Experiments 15

15.1 Introduction

Fractional factorial experiments are used frequently in industry, especially in various stages of product development and in process and quality improvement. In a *fractional factorial experiment* only a fraction of the treatment combinations are observed. This has the advantage of saving time and money in running the experiment, but has the disadvantage that each main-effect and interaction contrast will be confounded with one or more other main-effect or interaction contrasts and cannot be estimated separately. Two factorial contrasts that are confounded are referred to as being *aliased*. The term "confounded" is generally reserved for the indistinguishability of a treatment contrast and a block contrast (as described in Chaps. 13 and 14).

We look at two methods of obtaining fractional factorial designs that can be analyzed in a straightforward manner. The first method, described in Sects. 15.2–15.5, is to select one block from one of the single-replicate designs in Chap. 13 as the fraction to be used. The second method of choosing a fraction, described in Sect. 15.6, is popular in industry and uses the concept of an *orthogonal array*. In Sect. 15.7, we aim to identify the treatment factor levels whose effects are least affected by noise variable fluctuations; the intention being to reduce the sensitivity of a product or manufacturing process to uncontrolled variation.

In Sect. 15.8, very small screening designs are introduced. These designs allow one to search among many factors for the few factors which have substantially large main effects. The use of SAS and R software in analyzing fractional factorial experiments is explored in Sects. 15.9 and 15.10, respectively.

15.2 Fractions from Block Designs; Factors with 2 Levels

15.2.1 Half-Fractions of 2^p Experiments; 2^{p-1} Experiments

We start with a very small example to illustrate the ideas of fractional factorial experiments. Suppose that an experiment is to be run with three treatment factors A, B, and C, each having two levels. There are no blocking factors, so the experiment will be run as a completely randomized design. However, only four observations can be taken.

We can obtain a fractional factorial design with just 4 of the 8 total treatment combinations by selecting at random one of the blocks of a single-replicate design with two blocks of size 4. For illustration, consider the block design that confounds the ABC contrast, given in Table 15.1. Suppose

A. Dean et al., *Design and Analysis of Experiments*,
Springer Texts in Statistics, DOI 10.1007/978-3-319-52250-0_15

Table 15.1 2^3 experiment in 2 blocks of 4, confounding *ABC*

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

Table 15.2 Contrasts for the $\frac{1}{2}$-fraction (001, 010, 100, 111) of a 2^3 experiment, and the corresponding alias scheme

	A	*B*	*C*	*AB*	*AC*	*BC*	*ABC*	Alias scheme
001	−1	−1	1	1	−1	−1	1	$I = ABC$
010	−1	1	−1	−1	1	−1	1	$A = BC$
100	1	−1	−1	−1	−1	1	1	$B = AC$
111	1	1	1	1	1	1	1	$C = AB$

we select the second block, which is (001, 010, 100, 111) and is defined by the equation

$$a_1 + a_2 + a_3 = 1 \bmod 2.$$

The four treatment combinations constitute a $\frac{1}{2}$-*fraction* or $\frac{1}{2}$-*replicate* of a 2^3 experiment, called a 2^{3-1} design, and the *ABC* contrast is called the *defining contrast* for the fraction. We write

$$I = ABC,$$

which is called the *defining relation* for the fraction.

With only $n = 4$ observations, there are only $n - 1 = 3$ total degrees of freedom. This means that it is not possible to estimate each of the six remaining contrasts (A, B, C, AB, AC, BC) even if no estimate of σ^2 is required. If we look at the contrasts for a 2^3 experiment (shown in Table 13.2, p. 436) and cross out the rows corresponding to the unobserved treatment combinations, just the contrast coefficients shown in the left part of Table 15.2 remain.

Table 15.2 shows several interesting features. First, the column corresponding to *ABC* is not a contrast. The coefficients are the coefficients that one would use in obtaining the sum of the four observations, which is a multiple of the mean. So, *ABC* is confounded with the mean, and the *ABC* contrast cannot be measured. The notation $I = ABC$ of the defining relation indicates the equivalence of *ABC* and the sum of the observations, since *I* corresponds to a list of coefficients all equal to +1.

Secondly, we see from Table 15.2 that the contrast coefficients for *A* and *BC* are identical, the contrasts for *B* and *AC* are identical, and the contrasts for *C* and *AB* are identical. The main effect of *A* and the interaction *BC* are said to be *aliased*, as are *B* and *AC*, and *C* and *AB*. We write

$$A = BC, \quad B = AC, \quad C = AB.$$

Thus, there are three estimable contrasts in the fraction, but each measures more than one factorial effect. For example, using the cell-means model

$$Y_{ijk} = \mu + \tau_{ijk} + \epsilon_{ijk}\,,$$

the "$A = BC$" contrast with divisor $v/2$ (where v is the number of treatment combinations in the fraction) is obtained by multiplying the τ_{ijk}'s by the coefficients in the column labeled *A* in Table 15.2, that is,

$$\frac{1}{2}[-\tau_{001} - \tau_{010} + \tau_{100} + \tau_{111}]. \tag{15.2.1}$$

This is an estimable contrast with least squares estimate

$$\frac{1}{2}[-y_{001} - y_{010} + y_{100} + y_{111}].$$

It can be verified that this "$A = BC$" contrast in (15.2.1) can be written as

$$\begin{aligned}&\tfrac{1}{4}[-\tau_{000} - \tau_{001} - \tau_{010} - \tau_{011} + \tau_{100} + \tau_{101} + \tau_{110} + \tau_{111}]\\ &\quad + \tfrac{1}{4}[+\tau_{000} - \tau_{001} - \tau_{010} + \tau_{011} + \tau_{100} - \tau_{101} - \tau_{110} + \tau_{111}]\\ &= [-\overline{\tau}_{0..} + \overline{\tau}_{1..}] + \tfrac{1}{2}[\overline{\tau}_{.00} - \overline{\tau}_{.01} - \overline{\tau}_{.10} + \overline{\tau}_{.11}]\,.\end{aligned}$$

Thus, what is being estimated is the sum of the A contrast and the BC contrast, and we could refer to the "$A = BC$" contrast as the $A + BC$ contrast. However, for simplicity, we often refer to it as the A contrast, remembering the role of BC from the list of aliased contrasts.

If in a hypothesis test or half-normal probability plot the main effect of factor A is nonsignificant, then there are two possibilities. One is that neither A nor its alias, BC, is significantly different from zero. The alternative is that the two contrasts have equal and opposite effects and cancel each other out. Since the former is much more likely, this is the assumption that is usually made. If the main effect of A appears to be significant, then it is not clear whether the observed effect is due to the main effect of A or to the BC interaction or the combination of both effects. Because of the aliasing problem, fractional factorial experiments are most often run as *screening experiments*. The word "screening" means that the experimenter is trying to determine which of a large number of factors affect the response (see Sect. 15.8).

The list of aliased contrasts is called the *aliasing scheme* for the design. We generally write this as in the right hand side of Table 15.2, with the first row showing the defining relation and the following rows listing the aliased contrasts. The number of rows in the aliasing scheme is the same as the number of observations in the design.

It can be verified that, if Block I of the single replicate design in Table 15.1 were to be used as the $\frac{1}{2}$-fraction instead of Block II, exactly the same aliasing scheme would result except that, in each pair of aliased contrasts, the coefficients would differ from each other in sign. The fraction would then consist of the four treatment combinations 000, 011, 101, 110, and the A contrast would be

$$\begin{aligned}&\tfrac{1}{2}[-\tau_{000} - \tau_{011} + \tau_{101} + \tau_{110}]\\ &= \tfrac{1}{4}[-\tau_{000} - \tau_{001} - \tau_{010} - \tau_{011} + \tau_{100} + \tau_{101} + \tau_{110} + \tau_{111}]\\ &\quad - \tfrac{1}{4}[+\tau_{000} - \tau_{001} - \tau_{010} + \tau_{011} + \tau_{100} - \tau_{101} - \tau_{110} + \tau_{111}]\\ &= [-\overline{\tau}_{0..} + \overline{\tau}_{1..}] - \tfrac{1}{2}[\overline{\tau}_{.00} - \overline{\tau}_{.01} - \overline{\tau}_{.10} + \overline{\tau}_{.11}]\,,\end{aligned}$$

which we could refer to as the $A - BC$ contrast. This difference in signs can be highlighted by including this information in the aliasing scheme, that is, $I = -ABC$, $A = -BC$, $B = -AC$, and $C = -AB$.

The entire aliasing scheme in the right hand side of Table 15.2 can be deduced from the defining relation without writing out the contrasts. Using the contrast names, we can multiply the defining relation by A to obtain

$$A \times I = A \times ABC\,.$$

Treating I as a multiplicative identity so that $A \times I = A$, and reducing superscripts modulo 2 so that $A^2BC = BC$, we obtain $A = BC$. The other two rows of the scheme can be obtained in a similar fashion. From now on, we will avoid writing out the contrasts and use the contrast names and the defining relation to obtain the aliasing scheme.

Half-fractions of 2^p experiments have $\frac{1}{2}2^p$ treatment combinations and are called 2^{p-1} experiments. They are almost always obtained by selecting one block from a block design that confounds the highest-order interaction. Thus for $p = 4$, for example, the fraction satisfies either

$$a_1 + a_2 + a_3 + a_4 = 0 \text{ (mod 2)}$$

and has defining relation $I = ABCD$, or it satisfies

$$a_1 + a_2 + a_3 + a_4 = 1 \text{ (mod 2)}$$

and has defining relation $I = -ABCD$. For $p = 5$, the fraction satisfies either

$$a_1 + a_2 + a_3 + a_4 + a_5 = 0 \text{ (mod 2)}$$

and has defining relation $I = -ABCDE$, or it satisfies

$$a_1 + a_2 + a_3 + a_4 + a_5 = 1 \text{ (mod 2)}$$

and has defining relation $I = ABCDE$. Notice that the sign of the contrast in the defining relation is positive if the equation contains an even number of a_i's and is set equal to 0 (mod 2). It is also positive if the equation contains an odd number of a_i's and also is set equal to 1 (mod 2). Otherwise, the sign is negative. This always holds, even for the more complicated fractions of 2^p experiments discussed in the following sections.

For most purposes, we do not need to know whether the contrasts listed in the defining relation differ in sign. Consequently, unless they are needed, we shall usually ignore the signs in the aliasing scheme.

15.2.2 Resolution and Notation

The defining relation contains a set of contrasts such as AB, ABC, etc. that are aliased with the mean; these contrasts are often called *words*. The number of letters in the shortest word in the defining relation is called the *resolution* of the design.

The design in Table 15.2 is a Resolution III design, since the only word in the defining relation is ABC, which has three letters. In all Resolution III designs, main-effect contrasts are aliased with 2-factor interaction contrasts. In a Resolution IV design, the defining relation contains only words with 4 or more letters. Some main effects are then aliased with 3-factor interactions and 2-factor interactions aliased with other 2-factor interactions. In a Resolution V design, such as that in Table 15.4, some main effects are aliased with 4-factor interactions, and 2-factor interactions are aliased with 3-factor interactions. This is summarized in Table 15.3.

Since the main-effects and low-order interactions are usually the most important factorial effects to be measured, it is generally beneficial to select a design with as high resolution as can be found. The designs in Table 15.60 (at the end of the chapter) all satisfy this requirement.

A $1/2^q$ fraction of a 2^p experiment is usually referred to as a 2^{p-q} *fractional factorial experiment*. The resolution number is sometimes added as a subscript. A resolution III design, for example, can be written as a 2^{p-q}_{III} design.

Table 15.3 Resolution numbers of fractional factorial experiments

Resolution	Main effects aliased with	2-factor interactions aliased with
III	2-factor interactions and/or higher	Main effects and/or interactions
IV	3-factor interactions and/or higher	2-factor interactions and/or higher
V	4-factor interactions and/or higher	3-factor interactions and/or higher

15.2.3 A Real Experiment—Soup Experiment

L.B. Hare, in the (1988) issue of the *Journal of Quality Technology*, described an experiment on a dry soup mix filling process that was run at Thomas J. Lipton, Inc. The company was concerned about keeping the weight of the mix as uniform as possible. They found that most of the variability was due to the uneven flow of the "intermix," which is a mixture of vegetable oil, salt, and other ingredients, during the mixing process. The researchers prepared a list of five treatment factors that they thought might be influential in controlling the mixing process. The factors and their levels (subsequently coded 0, 1) were:

A: number of mixer ports through which vegetable oil was added (two levels, 1 and 3);
B: temperature of mixer jacket (two levels; ambient temperature, presence of cooling water);
C: mixing time (two levels; 60 and 80 sec);
D: batch weight (two levels; 1500 and 2000 lb);
E: delay between mixing and packaging (two levels; 1 day and 7 days).

This was a screening experiment, since the researchers had little idea of which factors were going to turn out to be important in affecting the variability of the soup mix weight. They decided to run a $\frac{1}{2}$-fraction to investigate the five factors, and follow up the experiment with a more detailed study of the important factors later. They chose a Resolution V design with defining relation $I = ABCDE$, which allowed them to include all main effects and two-factor interactions in the model. The corresponding block design, which confounds $ABCDE$, is listed in Table 13.29. The experimenters chose the second block, as it contained the treatment combination that represented the normal operating conditions prior to the experiment. These were 00010, that is, one port, presence of cooling water, 60 s mix, 2000 lb batch weight, and a one day delay before packaging.

The experiment was designed so that it could be run with very little disruption to the daily production routine. Sets of 5 samples were taken every 15 min during the production run for each treatment combination and weighed. The response variable was a measure of variation based on these weights. The randomized design and the responses obtained are shown in Table 15.4. Also shown in the table are the contrasts for the main effects. As in Chap. 13, the contrast has coefficient -1 when the corresponding factor is at its low level and coefficient $+1$ when it is at its high level. The contrast coefficients for the interactions are the products of the corresponding main-effect contrast coefficients.

The experimenters included all the main effects and 2-factor interactions in the model. Since there were $16 - 1 = 15$ degrees of freedom in total and 15 contrasts to estimate (5 main effects and 10 two-factor interactions), there were no degrees of freedom available to estimate the error variability. The experimenters calculated all the contrast estimates and prepared a normal probability plot to find the important contrasts. For example, the contrast estimate for the main effect of E is

$$\begin{aligned}\hat{E} = (&0.78 + 1.10 - 1.70 - 1.28 + 0.97 + 1.47 - 1.85 - 2.10 + 0.76 \\ &+ 0.62 - 1.09 - 1.13 + 1.25 + 0.98 - 1.36 - 1.18)/8 \;=\; -0.47.\end{aligned}$$

Table 15.4 Design, data (measure of weight variability), and main-effect contrasts for the soup experiment. Defining relation $I = ABCDE$

Levels of A, B, C, D, E	y_{ijklm}	Contrasts				
		A	B	C	D	E
01011	0.78	−1	1	−1	1	1
11111	1.10	1	1	1	1	1
10000	1.70	1	−1	−1	−1	−1
11100	1.28	1	1	1	−1	−1
00001	0.97	−1	−1	−1	−1	1
01101	1.47	−1	1	1	−1	1
00010	1.85	−1	−1	−1	1	−1
10110	2.10	1	−1	1	1	−1
00111	0.76	−1	−1	1	1	1
10011	0.62	1	−1	−1	1	1
01110	1.09	−1	1	1	1	−1
01000	1.13	−1	1	−1	−1	−1
11001	1.25	1	1	−1	−1	1
10101	0.98	1	−1	1	−1	1
11010	1.36	1	1	−1	1	−1
00100	1.18	−1	−1	1	−1	−1

Source Hare (1988). Reprinted with Permission from Journal of Quality Technology © 1998 ASQ, www.asg.org

Table 15.5 Contrast estimates (with divisor $v/2 = 8$) for the soup experiment

Contrasts	A	B	C	D	E	AB	AC	
Estimates	0.145	−0.088	0.038	−0.038	−0.470	−0.015	0.095	
Contrasts	AD	AE	BC	BD	BE	CD	CE	DE
Estimates	0.030	−0.153	0.068	−0.163	0.405	0.073	0.135	−0.315

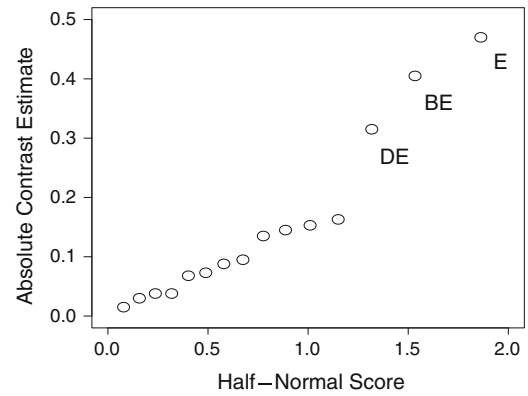
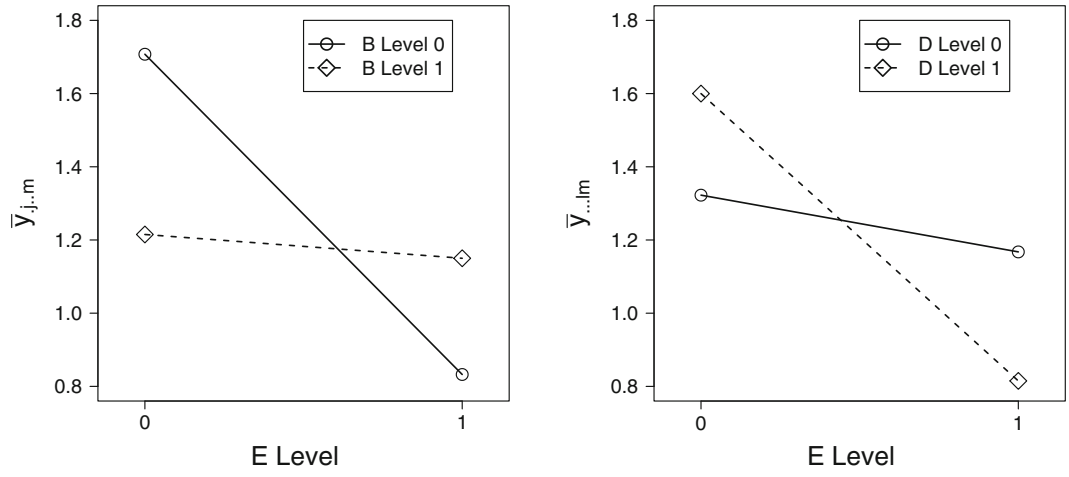


Fig. 15.1 Soup experiment: half-normal probability plot of contrast absolute estimates (using divisor $v/2 = 8$)

The main effect and two-factor interaction contrast estimates are shown in Table 15.5. In Fig. 15.1, we present a half-normal probability plot (Sect. 7.5.2) which shows the absolute values of the contrast estimates plotted against their half-normal scores. It can be seen that the most important contrasts appear to be E, BE, and DE.



(a) *BE* interaction (b) *DE* interaction

Fig. 15.2 Interaction plots for the soup experiment

Interaction plots of the two interactions *BE* and *DE* are shown in Fig. 15.2. The response is a measure of weight variability, and the experimenters wanted to reduce this as much as possible. The estimate of the *E* contrast is negative, indicating that the low level of *E* (one-day delay before packaging) is more variable that the high level (seven-day delay). The two interaction plots also indicate that a seven-day delay before packaging would be beneficial using the ambient temperature (low level of *B*) and the large batch size (2000 lb, high level of *D*). The interaction plots also indicate that if a seven-day delay is not feasible, then it is better to use cooling water and a small batch size (high *B*, low *D*). The production management of the company agreed to a seven-day delay, and the researchers decided to investigate these three factors (*B*, *D*, and *E*) in more detail in a followup experiment, together with factor *C*, whose interaction with *E* was the next largest effect.

15.2.4 Quarter-Fractions of 2^p Experiments; 2^{p-2} Experiments

We can obtain a $\frac{1}{4}$-fraction of a 2^p experiment by selecting at random one of the blocks from a single-replicate confounded design with 4 blocks of size 2^{p-2}. The defining relation is then the set of three contrasts that were confounded to obtain the block design.

For example, suppose a 2^5 experiment was to be run as a completely randomized design, but only eight observations could be taken, and the main effects and interaction *AE* were of particular interest. Table 13.29 (p. 471) lists a design in 4 blocks of 8. Suppose we switch *E* and *D* in the listed design, then we obtain a design in 4 blocks of 8 which confounds the three interactions *ABD*, *CDE*, and *ABCE*. We can then select one block for the $\frac{1}{4}$-fraction; for example, if we select the block that satisfies

$$a_1 + a_2 + a_4 = 1 \pmod 2 \quad \text{and} \quad a_3 + a_4 + a_5 = 0 \pmod 2,$$

the treatment combinations in the resulting fraction are

$$00011 \;\; 00110 \;\; 01000 \;\; 01101 \;\; 10000 \;\; 10101 \;\; 11011 \;\; 11110\,.$$

Table 15.6 Aliasing scheme (ignoring signs) for a $\frac{1}{4}$-fraction of a 2^5 experiment with the defining relation $I = ABD = CDE = ABCE$

I	=	ABD	=	CDE	=	$ABCE$
A	=	BD	=	$ACDE$	=	BCE
B	=	AD	=	$BCDE$	=	ACE
C	=	$ABCD$	=	DE	=	ABE
D	=	AB	=	CE	=	$ABCDE$
E	=	$ABDE$	=	CD	=	ABC
AC	=	BCD	=	ADE	=	BE
AE	=	BDE	=	ACD	=	BC

The defining relation for the fraction is

$$I = ABD = CDE = ABCE\,.$$

(If we work out the contrast coefficients for this fraction, we find that the coefficients for ABD are all $+1$, while those for CDE and $ABCE$ are all -1. Thus, if the signs of the contrasts were taken into account, the defining relation would be $I = ABD = -CDE = -ABCE$.) The other seven rows of the aliasing scheme are obtained by multiplying the defining relation by each of the contrast names in turn. The resulting aliasing scheme (ignoring signs) is shown in Table 15.6.

Only one factorial effect from each row of the aliasing scheme (and none from the defining relation) can be entered into the model for analyzing the experiment. So, for example, we could include all main effects and the two 2-factor interactions AC and AE in the model. (Notice that, if we had not made the switch of labels E and D, then AE would have been aliased with B.)

If the D effect, for example, is insignificant, the interactions AB, CE, and $ABCDE$ are also regarded as insignificant. But if the analysis shows that D has a significant effect on the response, it is unknown whether the effect is due to the main effect of D, or to AB, or to CE, or to $ABCDE$ (although this latter effect is the least likely), or to some combination of all four. The design is useful for screening when it is believed that most main effects and interactions will be negligible but one or two factors will possibly have an important effect on the response.

This design is clearly ideal if all of the interactions are negligible, or if all interactions except exactly one of AC, BE, AE, and BC are thought to be negligible. In the first case, two degrees of freedom are available to estimate σ^2. In the second case, all of the main effects and the one interaction can be measured, and one degree of freedom remains to estimate σ^2. If all main effects and, say, the CD interaction were required to be estimated, then a different block design should be chosen since, in this design $CD = E$. For example, a suitable design could be obtained by interchanging A and D in the list of confounded contrasts. In other words, the design obtained by confounding ABD, ACE, and $BCDE$ will give a $\frac{1}{4}$-fraction in which CD is not aliased with main effects.

A list of useful $\frac{1}{4}$-fractions is given in Table 15.60 at the end of the chapter.

Example 15.2.1 `Sludge experiment`

S.R. Schmidt and R.G. Launsby, in their textbook *Understanding Industrial Designed Experiments*, include an article by J. Brickell and K. Knox on the operation of a biological treatment system (known as an activated sludge system) used in wastewater treatment plants. The details of the system are given in the article. The response, Y, is the removal of "biochemical oxygen demand," which is related to the quality of water. The water quality increases as more biochemical oxygen demand is removed, so the

response Y is to be maximized. The experiment described in the article investigates the effect of five factors on Y:

A: Reactor biomass concentration (3000 and 6000 mg/l),
B: Clarifier biomass concentration (8000 and 12000 mg/l),
C: Waste sludge flow rate (78.5 and 940 m^3/d),
D: Biological growth rate constant (0.040 and 0.075 d^{-1})
E: Fraction of food to biomass (0.4 and 0.8 kg/kg).

Since this experiment was to be run in a water treatment plant, it was necessary to keep the number of observations small, and a $\frac{1}{4}$-fraction was selected with defining relation $I = ABD = CDE = ABCE$. This gives the aliasing scheme of Table 15.6.

The experimenters selected the fraction whose treatment combinations, written as $a_1a_2a_3a_4a_5$, satisfied

$$\begin{aligned} a_1 + a_2 \quad + a_4 \quad &= 1 \pmod 2 \\ a_3 + a_4 + a_5 &= 1 \pmod 2 \end{aligned}$$

The design, prior to randomization, is shown in Table 15.7 together with the responses obtained.

The experimenters included all main effects and the 2-factor interactions AC and BC in their model. The contrast estimates (with divisors $v/2 = 4$) are listed in Table 15.8, and a half-normal probability plot of the seven contrast estimates is shown in Fig. 15.3. There are too few contrast estimates in total to be able to draw good conclusions from the half-normal probability plot. Nevertheless, the most important effect appears to be the main effect of C and, perhaps to a lesser extent, E. Now, C is aliased with DE, and E is aliased with CD. A followup experiment investigating the effects of C, D, and E would certainly be advisable.

If we try to draw conclusions from the results of the present experiment, and if we are willing to assume that the main effects are the dominant effects in any alias sets, it would seem advisable to set C and possibly B at their high levels in order to maximize the response, and to set E and possibly D at their low levels. On the other hand, if we assume that the interactions in the alias sets might be

Table 15.7 $\frac{1}{4}$-fraction of a 2^5 experiment and data from the sludge experiment

Levels of A, B, C, D, E	y_{ijklm}	Contrasts						
		A	B	C	D	E	AC	BC
00010	195	−1	−1	−1	1	−1	1	1
00111	496	−1	−1	1	1	1	−1	−1
01001	87	−1	1	−1	−1	1	1	−1
01100	1371	−1	1	1	−1	−1	−1	1
10001	102	1	−1	−1	−1	1	−1	1
10100	1001	1	−1	1	−1	−1	1	−1
11010	354	1	1	−1	1	−1	−1	−1
11111	775	1	1	1	1	1	1	1

Source Brickell and Knox (1992). Copyright © 1992 Air Academy Press. Reprinted with permission

Table 15.8 Contrast estimates (with divisor $v/2 = 4$) for the sludge experiment

Contrast	A	B	C	D	E	AC	BC
Estimate	20.75	198.25	726.25	−185.25	−365.25	−66.25	126.25

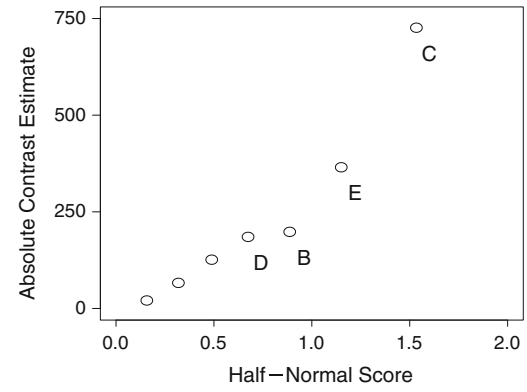
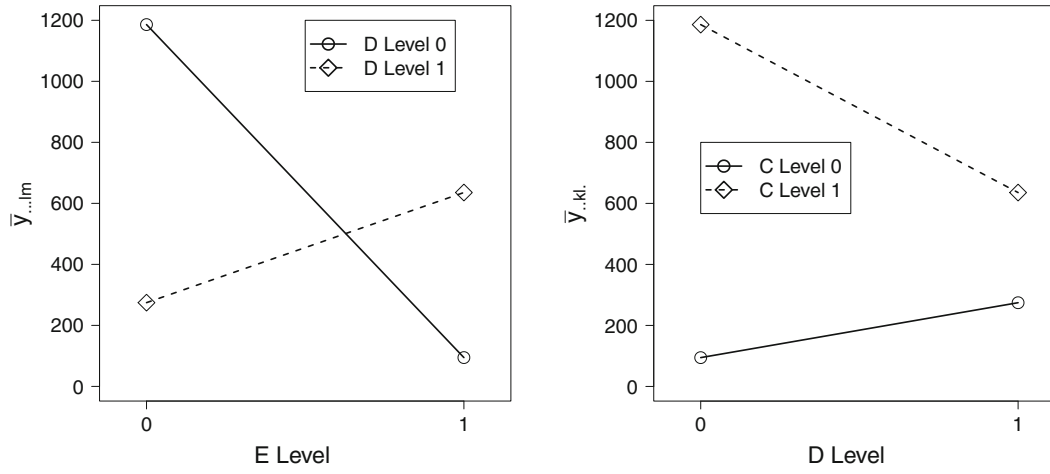


Fig. 15.3 Half-normal probability plot of contrast absolute estimates for the sludge experiment



(a) *DE* interaction (b) *CD* interaction

Fig. 15.4 Interaction plots for the sludge experiment

important, we would examine the *DE* and *CD* interaction plots (see Fig. 15.4). The *DE* plot suggests that *D* and *E* should both be at their low levels, and the *CD* plot suggests that *C* should be at its high level and *D* at its low level. Since the recommendations from the interaction plots agree with those from the main effect comparisons, we would feel comfortable in recommending that the process be set at the cheaper of 01100 or 11100. Notice that the first of these was included among the experimental runs (and happened to give rise to the largest observed yield), whereas the second was not included. In either case, the experiment should be re-run at the chosen setting to confirm the results.

The authors of the article point out that other considerations, such as cost, come into play before any system can be changed. In an actual water treatment plant, it would be expensive to change the levels of *D* (biological growth rate constant) and *E* (fraction of food to biomass) from their current settings. Also, increasing the waste sludge flow rate (*C*) increases cost. A followup experiment could verify that the above recommendations were correct, and also could examine intermediate values of *C*. □

15.2.5 Smaller Fractions of 2^p Experiments

Smaller fractions of a 2^p experiment can be obtained in exactly the same way as the $\frac{1}{2}$-fractions and $\frac{1}{4}$-fractions of the preceding subsections. For a $1/2^s$ fraction, the first step is to find a design with 2^s blocks of size 2^{p-s} that confounds negligible interactions. One block is selected at random. The aliasing scheme is then checked to ensure that as few important contrasts as possible are aliased with each other. If the aliasing scheme is not suitable, then an attempt is made to obtain a better design by interchanging letters in the confounding scheme, or by investigating different confounding schemes. A list of useful $\frac{1}{4}$-, $\frac{1}{8}$- and $\frac{1}{16}$-fractions of 2^p experiments is given in Table 15.60 at the end of the chapter.

Example 15.2.2 `Welding experiment`

An experiment was discussed by A.K. Shahani in *The Statistician* in 1970 which involved a (1/1024) fraction of a 2^{21} experiment (that is, a 2^{21-16} experiment). The experiment, which required only $v = 2^5 = 32$ observations, was designed by Dr. Shahani for Bristol Aerojet Ltd and concerned the "pull strength" of welds resulting from a certain welding process. The company wished to discover which settings of the 21 factors would give welds with pull strength exceeding a given size. Of the 21 factors, only a few were expected to have important effects on the pull strength, and this allowed the use of such a highly fractionated design.

All 21 factors were easy to manipulate, and the engineers selected two reasonable settings for each factor (coded 0 and 1). For some of the factors, the two levels chosen were at equal distances on each side of the current operating conditions. For others, such as factors A, D, and W, the low levels were at the current operating conditions and could not be lowered further. If we label the factors A, B, ..., W omitting I and O, the contrasts selected for confounding were as follows:

$$\begin{array}{llll} ABV & ACW & ADT & AES \\ BCU & ABEN & ACDQ & ACEP \\ ADEM & BCER & BDEL & CDEK \\ ABCEH & ABDEJ & ACDEG & BCDEF \end{array}$$

The defining relation consists of these 16 contrasts together with all their possible products. Since the shortest word in the defining relation is of length 3, the design is Resolution III. Although each main effect is aliased with several two-factor interactions, the main effects are not aliased with each other.

The 32 treatment combinations and their responses are shown in Table 15.9. (We have corrected typing errors that occurred in the original paper in the two treatment combinations in the second row of our table). The responses are in coded units, details of which were not given in the original paper.

This experiment has too many factors to be able to analyze it easily by hand. The main-effect contrast estimates (with divisors $v/2 = 16$), obtained from a computer package, are shown in Table 15.10. Under the current operating conditions, it was known that the error standard deviation σ was about 60 units. The experimenters were willing to assume that this would not change appreciably under different operating conditions and therefore calculated the standard error of a main effect contrast $\Sigma_i c_i \tau_i$ to be

$$\sqrt{\text{Var}(\Sigma_i c_i \hat{\tau}_i)} = \sigma\sqrt{\Sigma c_i^2} = 60\sqrt{32/16^2} = 21.21.$$

Without assuming that the coded responses follow a normal distribution, the experimenters then deemed any contrast whose estimated absolute value turned out to be several times larger than 21.21 to be important.

Table 15.9 Treatment combinations and responses for the 2^{21-16} welding experiment

Treatment combination	Response	Treatment combination	Response
00000111100000011111	430	10000100000111000100	422
01000100010100111001	336	11000011011011100010	380
00100000110001111010	438	10100011010110010001	96
01100101011011111100	394	11100110111100000111	319
00010001011100101011	334	10010010111011000 1100	202
01010100110110111 0001	322	11010111010001010 1010	238
00110110000110101 10010	184	10110101101010110 1001	188
01110011100111001 0100	348	11110000000000100 1111	−234
00001000011111010 1111	384	10001011111000111 0100	338
01001101110101000 1001	404	11001110010010101 0010	370
00101111001110100 1010	542	10101100101001001 0001	114
01101010100100110 1100	316	11101001000011011 0111	432
00011110100011110 0111	256	10011101000100011 1100	206
01011011001001100 0001	82	11011000101110001 1010	106
00111001110010000 0010	528	10111010010101101 1001	110
01111100011000010 0100	35	11111111111111111 1111	370

Source Shahani (1970). Copyright © 1970 Blackwell Publishers. Reprinted with permission

Table 15.10 Contrast estimates for the welding experiment (with divisor 16)

Contrast	A	B	C	D	E	F	G
Estimate	−104.8	−34.6	−39.4	−152.5	12.3	37.4	5.3
Contrast	H	J	K	L	M	N	P
Estimate	101.9	70.5	48.4	−23.4	43.5	100.1	32.9
Contrast	Q	R	S	T	U	V	W
Estimate	16.6	3.0	42.1	−8.1	7.1	72.8	−69.0

The contrast estimates whose absolute values exceed 63.63 are those for the main effects of A, D, H, J, N, V, and W. The estimates for main effects of M, K, and S are all around 2 standard errors, with those for C and F a little smaller.

Since there are 32 observations, a total of 31 orthogonal contrasts can be measured. Thus there are 10 sets of confounded interaction contrasts that can be measured in addition to the 21 main-effect contrasts. The identification of these contrast sets requires writing out the entire aliasing scheme—a daunting task! A proper analysis of the main effects also requires knowledge about which interactions are aliased with which main effects. A followup experiment to separate out the most likely aliased effects would be needed.

Assuming, temporarily, that the process can be improved by considering the main effects only, the contrast estimates (high level minus low level) suggest that factors A, D, and W (whose contrast estimates are negative) should be set at their low levels and factors H, J, K, N, V, M, and S (whose contrast estimates are positive) should be set at their high levels. As mentioned above, A, D, and W were already set at the lowest possible values in the original process, and therefore further experimentation with these factors is unnecessary. The other seven factors were discussed by the engineers and new (higher) settings selected for these, resulting in an improved process that met the pull strength requirements. The author of the article commented that the research and development department should give consideration to a further experiment involving these seven factors in which main effects and two-factor

interactions could all be measured. He suggested the use of a 2^{7-1} experiment, which would require 64 observations. Fewer observations would require aliasing some of the 2-factor interactions (see Table 15.60). □

15.3 Fractions from Block Designs; Factors with 3 Levels

15.3.1 One-Third Fractions of 3^p Experiments; 3^{p-1} Experiments

To obtain a fraction of a 3^p experiment, we use the same idea that we used for 2^p experiments. We select one block at random from a confounded single-replicate design with 3^s blocks of size 3^{p-s} with a suitable confounding scheme. For example, suppose a $\frac{1}{3}$-fraction of a 3^4 experiment is required (that is, a total of $3^{4-1} = 27$ observations). The highest-order interaction in a 3^4 experiment that can be confounded is the 4-factor interaction. Therefore, the maximum number of letters in a word in the defining relation of the fraction is also four. For a Resolution IV design, when 3- and 4-factor interactions are negligible, the main-effect contrasts can be estimated, but the 2-factor interactions will be aliased. This is the best design available and, unless a larger budget can be obtained to allow more observations, some aliasing among the low-order interactions will have to be tolerated.

Suppose the selected single-replicate confounded design is that which confounds the pair of contrasts $(ABCD; A^2B^2C^2D^2)$ from the 4-factor interaction. The block design is constructed using the equations

$$a_1 + a_2 + a_3 + a_4 = 0,\ 1, \text{ or } 2 \pmod 3$$

as in Sect. 14.2.3, and one block is selected at random for the $\frac{1}{3}$-fraction. Since there are 27 treatment combinations to be observed, the aliasing scheme has 27 rows. Seven rows from the aliasing scheme are given in Table 15.11. The remaining rows contain main effects or 2-factor interaction contrasts (such as AB^2 or A^2B) that are aliased only with higher-order interactions. The 27 rows of the aliasing scheme include one for effects aliased with the mean, together with 13 additional pairs of rows, such as the pair of rows involving AB and A^2B^2 which represent contrasts from the same two-factor interaction. The two rows containing AB and A^2B^2 indicate, for example, that the pair of contrasts $(AB; A^2B^2)$ is aliased with the pairs of contrasts $(CD; C^2D^2)$ and $(ABC^2D^2; A^2B^2CD)$. Use of this design is illustrated in Example 15.3.1.

Example 15.3.1 `Refinery experiment`

John (1971) describes an experiment of Vance (1962) to find a set of operating conditions to optimize the quality of lube oil treated at a refinery. There were four factors of interest, called here A, B, C, and D, and three equally spaced levels were selected for each of these so that quadratic trends could be measured.

Table 15.11 Seven rows from the aliasing scheme for a $\frac{1}{3}$-fraction of a 3^4 experiment with the defining relation $I = ABCD = A^2B^2C^2D^2$

I	=	$ABCD$	=	$A^2B^2C^2D^2$				
AB	=	A^2B^2CD	=	C^2D^2	A^2B^2	=	CD	=	ABC^2D^2
AC	=	A^2BC^2D	=	B^2D^2	A^2C^2	=	BD	=	AB^2CD^2
AD	=	A^2BCD^2	=	B^2C^2	A^2D^2	=	BC	=	AB^2C^2D

Table 15.12 $\frac{1}{3}$-fraction of a 3^4 experiment and data from the refinery experiment

Treatment combination	y_{ijkl}	A_L	A_Q	B_L	B_Q	C_L	C_Q	D_L	D_Q
0000	4.2	−1	1	−1	1	−1	1	−1	1
0012	5.9	−1	1	−1	1	0	−2	1	1
0021	8.2	−1	1	−1	1	1	1	0	−2
0102	13.1	−1	1	0	−2	−1	1	1	1
0111	16.4	−1	1	0	−2	0	−2	0	−2
0120	30.7	−1	1	0	−2	1	1	−1	1
0201	9.5	−1	1	1	1	−1	1	0	−2
0210	22.2	−1	1	1	1	0	−2	−1	1
0222	31.0	−1	1	1	1	1	1	1	1
1002	7.7	0	−2	−1	1	−1	1	1	1
1011	16.5	0	−2	−1	1	0	−2	0	−2
1020	14.3	0	−2	−1	1	1	1	−1	1
1101	11.0	0	−2	0	−2	−1	1	0	−2
1110	29.0	0	−2	0	−2	0	−2	−1	1
1122	55.0	0	−2	0	−2	1	1	1	1
1200	8.5	0	−2	1	1	−1	1	−1	1
1212	37.4	0	−2	1	1	0	−2	1	1
1221	66.3	0	−2	1	1	1	1	0	−2
2001	11.4	1	1	−1	1	−1	1	0	−2
2010	21.1	1	1	−1	1	0	−2	−1	1
2022	57.9	1	1	−1	1	1	1	1	1
2100	13.5	1	1	0	−2	−1	1	−1	1
2112	51.6	1	1	0	−2	0	−2	1	1
2121	76.5	1	1	0	−2	1	1	0	−2
2202	31.0	1	1	1	1	−1	1	1	1
2211	74.5	1	1	1	1	0	−2	0	−2
2220	85.1	1	1	1	1	1	1	−1	1

Sources John (1971). Copyright © 1971 P.W.M. John. Reprinted with permission

Since this was a preliminary experiment, a $\frac{1}{3}$-fraction of Resolution IV was thought to be adequate. The experimenters used a design with defining relation $I = ABCD = A^2B^2C^2D^2$. Part of the aliasing scheme is shown in Table 15.11. We see from row 2 that two degrees of freedom from the AB interaction are aliased with two degrees of freedom from each of the CD and $ABCD$ interactions. The other two degrees of freedom from each of these interactions are aliased with 3-factor interactions. (For example, the pair $(AB^2; A^2B)$ is aliased with the pairs $(A^2CD; AC^2D^2)$ and $(BC^2D^2; B^2CD)$). A similar confounding pattern occurs with AC and BD and also with AD and BC.

The treatment combinations can be obtained from the equation

$$a_1 + a_2 + a_3 + a_4 = 0 \text{ (mod 3)}$$

and are shown in Table 15.12, prior to randomization, together with the data collected. Also shown are the linear and quadratic contrast coefficients for the main effects. The objective of the experiment was to select factor levels that would increase the response (a measure of quality).

The analysis of variance is complicated by the aliasing of pairs of degrees of freedom for two-factor interactions. We have not tried to separate these but have listed the contributions of the pairs of

Table 15.13 Analysis of variance for the refinery experiment

Source of variation	Degrees of freedom	Sum of squares	Mean square
A	2	4496.29	2248.14
A_L	1	4399.22	4399.22
A_Q	1	97.07	97.07
B	2	2768.69	1384.35
B_L	1	2647.49	2647.49
B_Q	1	121.20	121.20
C	2	5519.79	2759.89
C_L	1	5516.00	5516.00
C_Q	1	3.79	3.79
D	2	283.37	141.68
D_L	1	213.56	213.56
D_Q	1	69.81	69.81
AB, CD	6	339.00	56.50
AC, BD	6	1384.24	230.71
AD, BC	6	753.38	125.56
Total	26	15,544.66	

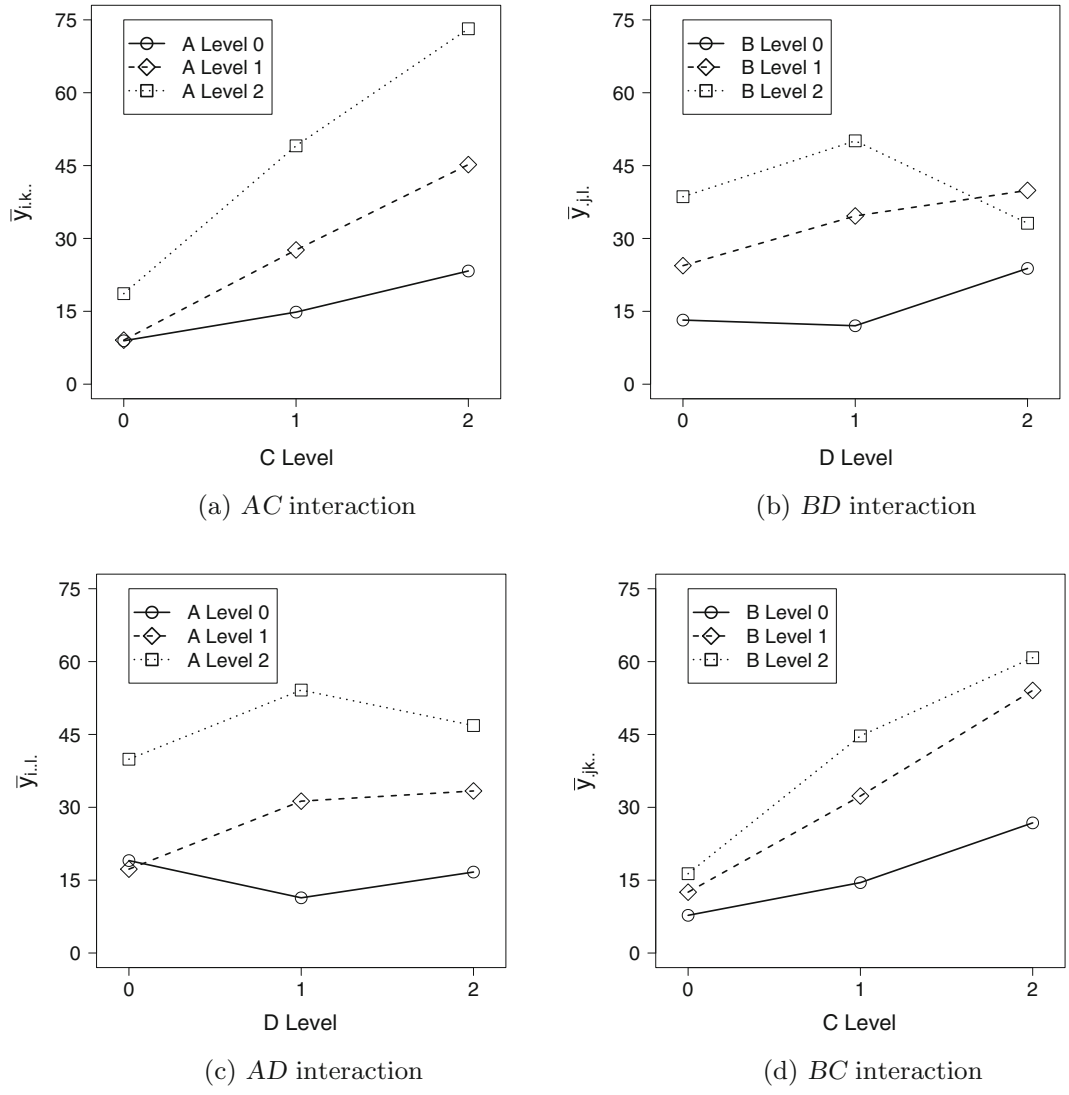
interactions on the same line of the analysis of variance table shown in Table 15.13. Without information concerning negligible interactions, we are unable to obtain an estimate for the error variance. The most important interactions appear to be the AC or BD interactions, and the BC or AD interactions; the corresponding interaction plots are shown in Fig. 15.5. In each case, the plots indicate that in order to increase the response, factors A, B, and C should all be set at their high levels, cost permitting, and factor D should be set at its middle level. They also indicate that since the lines are not too far from parallel, it would be reasonable to examine the main-effect contrasts.

Normalized linear and quadratic main-effect contrast estimates are obtained as

$$\frac{1}{d}\sum_i\sum_j\sum_k\sum_l c_{ijkl}\hat{\tau}_{ijkl} = \frac{1}{d}\sum_i\sum_j\sum_k\sum_l c_{ijkl}y_{ijkl}\,,$$

where the c_{ijkl}'s are the contrast coefficients in Table 15.12 and the divisor d is the square root of the sum of squares of the coefficients (that is, $\sqrt{18}$ for the linear contrasts and $\sqrt{54}$ for the quadratic contrasts). These estimates are listed in Table 15.14, and a half-normal probability plot of the estimates is shown in Fig. 15.6. Eight estimates are too few to make a good judgment, but the most important effects appear to be the linear trends in C, A, and B (in that order). All of these contrast estimates are positive, suggesting that the high levels should be selected in order to increase the response. This agrees with the conclusions from the interaction plots as well as the observed data in Table 15.12. Note that we could have examined interactions more closely by including individual interaction contrast estimates in the half-normal probability plot. We have not done this because of the complicated confounding of the interactions.

Since we have no estimate for error, we are unable to test any hypotheses. However, had the experimenters believed, prior to the experiment, that some or all of the interactions were negligible, then tests would have been done for the remaining interactions and the main effects. The sums of squares for testing the linear and quadratic main-effect contrasts are the squares of the corresponding normalized contrast estimates in Table 15.14. For example, the sums of squares for testing the



(a) AC interaction (b) BD interaction

(c) AD interaction (d) BC interaction

Fig. 15.5 Interaction plots for the refinery experiment

hypothesis that the linear trend of factor A is negligible, against the alternative hypothesis that it is not negligible, is

$$ss(A_{\mathrm{L}}) = 66.327^2 = 4399.22\,.$$

The sums of squares for the main effects of factors A, B, C, and D can be obtained either by adding their respective linear and quadratic contrast sums of squares, or by using the rules of Chap. 7 with $r = 1/3$ (since this is a one-third fraction). For example,

$$ssA = ss(A_{\mathrm{L}}) + ss(A_{\mathrm{Q}}) = 66.327^2 + 9.852^2 = 4399.22 + 97.07 = 4496.29\,,$$

or

$$ssA = 9\Sigma\bar{y}_{i\ldots}^2 - 27\bar{y}_{\ldots.}^2 = 28766.30 - 24270.01 = 4496.29\,. \qquad \square$$

Table 15.14 Normalized contrast estimates for the refinery experiment

	$\hat{A}_{\mathrm{L}}$	$\hat{A}_{\mathrm{Q}}$	$\hat{B}_{\mathrm{L}}$	$\hat{B}_{\mathrm{Q}}$	$\hat{C}_{\mathrm{L}}$	$\hat{C}_{\mathrm{Q}}$	$\hat{D}_{\mathrm{L}}$	$\hat{D}_{\mathrm{Q}}$
Estimate	66.33	9.85	51.45	−11.01	74.27	−1.95	14.61	−8.36

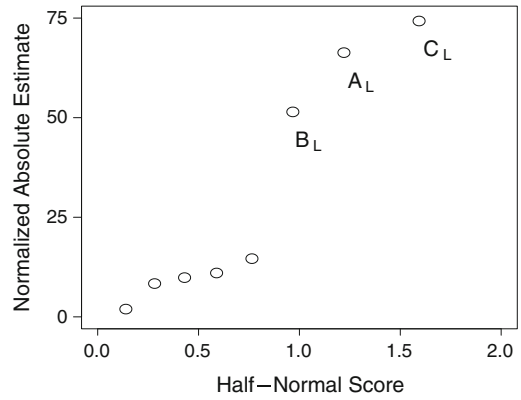


Fig. 15.6 Half-normal probability plot for the refinery experiment contrast absolute estimates

15.3.2 One-Ninth Fractions of 3^p Experiments; 3^{p-2} Experiments

As an example of a $\frac{1}{9}$-fraction, we take the sixth block of the 3^4 single-replicate confounded design shown in Table 14.5. The list of confounded interactions in the block design provides the list of interactions in the defining relation for the 3^{4-2} fractional factorial design. In the block design of Table 14.5, the confounded contrasts are $(AB^2C; A^2BC^2)$, $(ABD; A^2B^2D^2)$, $(AC^2D^2; A^2CD)$, and $(BCD^2; B^2C^2D)$ so, in the fraction, which consists of 9 treatment combinations, the defining relation is

$$\begin{array}{rrrr} I & = AB^2C & = A^2BC^2 \\ = ABD & = A^2CD & = B^2C^2D \\ = A^2B^2D^2 & = BCD^2 & = AC^2D^2\,. \end{array}$$

This design has Resolution III (since the shortest word has 3 letters), and main-effect contrasts will be aliased with 2-factor interaction contrasts. The nine observations provide 8 degrees of freedom, which is sufficient to estimate the four main effects (with two degrees of freedom each). Therefore, the design would be useful if all two-factor interactions were believed to be negligible. Since there are no degrees of freedom available for estimating σ^2, a half-normal probability plot of normalized contrast estimates would be drawn as was done in Fig. 15.6.

15.4 Fractions from Block Designs; Other Experiments

15.4.1 $2^p \times 4^q$ Experiments

The simplest way to design a fractional factorial experiment when all factors have four levels, or when some factors have two levels and the others have four levels, is to use pseudofactors. For example, suppose we require a design for a $2^3 \times 4$ experiment with eight observations. A design in four blocks of size 8 is shown in Table 14.9 (p. 483). The confounded contrasts are FGJ_1J_2, GHJ_2, and FHJ_1, where J_1 and J_2 are the two 2-level pseudofactors making up the 4-level factor J. Suppose Block I is

Table 15.15 Aliasing scheme for a $\frac{1}{4}$-fraction of a $2^3 \times 4$ experiment

I	$=$	FGJ_1J_2	$=$	GHJ_2	$=$	FHJ_1
F	$=$	GJ_1J_2	$=$	$FGHJ_2$	$=$	HJ_1
G	$=$	FJ_1J_2	$=$	HJ_2	$=$	$FGHJ_1$
H	$=$	$FGHJ_1J_2$	$=$	GJ_2	$=$	FJ_1
J_1	$=$	FGJ_2	$=$	GHJ_1J_2	$=$	FH
J_2	$=$	FGJ_1	$=$	GH	$=$	FHJ_1J_2
J_1J_2	$=$	FG	$=$	GHJ_1	$=$	FHJ_2
FJ_2	$=$	GJ_1	$=$	FGH	$=$	HJ_1J_2

selected from the design to give a $\frac{1}{4}$-fraction of a $2^3 \times 4$ experiment, then the defining relation is

$$I = FGJ_1J_2 = GHJ_2 = FHJ_1$$

and the design is Resolution III. The aliasing scheme, shown in Table 15.15, indicates that the F contrast, for example, is aliased with one contrast from each of the GJ, $FGHJ$ and HJ interactions. There are 3 contrasts (J_1, J_2 and J_1J_2) for the 4-level factor J. The aliasing scheme shows that J is aliased with contrasts from the FGJ, GHJ, FH, GH, FHJ, FG interactions.

An experiment involving pseudofactors will be illustrated in Example 15.5.1 in Sect. 15.5.

15.4.2 $2^p \times 3^q$ Experiments

Suppose that a $\frac{1}{6}$-fraction of a $2^3 \times 3^3$ experiment is required, which has a total of 36 observations. Again, we follow the idea of selecting one block from a block design. So we first need a block design in $b = 6$ blocks, each of size 36. Following the procedure of Example 14.4.1, p. 484, we select, for example, (i) a block design in two blocks of size 4 from the 2^3 experiment with factors A, B, C, confounding the ABC interaction, and (ii) a block design in three blocks of size 9 from the 3^3 experiment with factors E, F, G, confounding the pair of contrasts (DE^2F; D^2EF^2). Combining the treatment combinations in the blocks of these designs as in Example 14.4.1 leads to a design with six blocks in which the five contrasts ABC, (DE^2F; D^2EF^2), and ($ABCDE^2F$; $ABCD^2EF^2$) are confounded. Then, if one of the six blocks is selected for the $\frac{1}{6}$-fraction, we have a Resolution III design with defining relation

$$I = ABC = DE^2F = D^2EF^2 = ABCDE^2F = ABCD^2EF^2\,,$$

and the contrasts ABC, (DE^2F; D^2EF^2), and ($ABCDE^2F$; $ABCD^2EF^2$) are aliased with the mean.

The aliasing scheme for the fraction has 36 rows and includes the following three rows:

$$\begin{aligned}
A &= BC = ADE^2F = AD^2EF^2 = BCDE^2F = BCD^2EF^2\,,\\
B &= AC = BDE^2F = BD^2EF^2 = ACDE^2F = ACD^2EF^2\,,\\
C &= AB = CDE^2F = CD^2EF^2 = ABDE^2F = ABD^2EF^2\,.
\end{aligned}$$

Thus, the 2-level factors A, B, and C are aliased with 2-factor interactions between the 2-level factors plus some higher-order interactions. For example, the A contrast is aliased with the contrasts BC, (ADE^2F; AD^2EF^2), and ($BCDE^2F$; BCD^2EF^2).

A similar aliasing happens for the 3-level factors. For example,

$$\begin{aligned} D &= ABCD = D^2E^2F = EF^2 = ABCD^2E^2F = ABCEF^2, \\ D^2 &= ABCD^2 = E^2F = DEF^2 = ABCE^2F = ABCDEF^2, \end{aligned}$$

so the pairs of contrasts $(D; D^2)$, $(ABCD; ABCD^2)$, $(DEF^2; D^2E^2F)$, $(EF^2; E^2F)$, and $(ABCEF^2; ABCE^2F)$ are aliased with one another.

Finally, there is aliasing of interactions involving both 2- and 3-level factors, for example

$$\begin{aligned} AD &= BCD = AD^2E^2F = AEF^2 = BCD^2E^2F = BCEF^2, \\ AD^2 &= BCD^2 = AE^2F = ADEF^2 = BCE^2F = BCDEF^2, \end{aligned}$$

so the pairs of contrasts $(AD; AD^2)$, $(BCD; BCD^2)$, $(ADEF^2; AD^2E^2F)$, $(AEF^2; AE^2F)$, and $(BCEF^2; BCE^2F)$ are aliased with one another.

The design would be useful mainly when most of the interactions were expected to be negligible.

15.5 Blocked Fractional Factorial Experiments

If experimental conditions are not constant over the entire experiment, it may be necessary to arrange a fractional factorial experiment in blocks. For example, consider the soup experiment in Sect. 15.2.3 (p. 499), for which the experimenters used the resolution V 2^{5-1} fraction with defining relation $I = ABCDE$. Suppose the experimenters had decided that the experimental conditions could be kept fairly stable over the course of 8 observations but not 16. The treatment combinations would then have been divided into two blocks of size 8. If the fraction is divided into $b = 2$ blocks, then $b - 1 = 1$ contrast *and its alias* must be confounded. If CDE, for example, is selected for confounding, then the aliased pair of contrasts $CDE = AB$ is confounded with blocks, and neither of these contrasts can be measured. Rather than confound a 2-factor interaction, an alternative might be to select the Resolution IV design with defining relation $I = ABDE$ and to confound the aliased pair of contrasts $BCE = ACD$. Then, all two-factor interactions can be estimated, although six of them will be in aliased pairs. The choice between these two designs is the choice of losing information on one 2-factor interaction completely while aliasing the others with high-order interactions, or aliasing three pairs of 2-factor interactions.

For each fractional factorial design listed Table 15.60, at the end of the chapter, a suggestion (shown in parentheses) is given for selecting an interaction to be confounded when running the corresponding fraction in two blocks. (The aliases of this interaction can be obtained by multiplication with the interaction names in the defining relation, as usual).

Example 15.5.1 `Flour experiment`

M.G. Tuck, S.M. Lewis, and J.I.L. Cottrell describe a series of four experiments in the 1993 issue of *Applied Statistics* that were carried out at Spillers Milling Ltd. in order to identify a flour that would give a "high loaf volume and be tolerant to fluctuations in the bread making process". In the the third experiment in the series, four flour formulations were investigated (four levels of factor A, coded 0, 1, 2, 3), together with four noise factors each at two levels. The noise factors were amount of yeast (factor N, low or high), proof time (factor S, short or long), degree of mixing and moulding (factor Q, "undermixing, little water, heavy pressure" or "overmixing, much water, little pressure"), and dough time delay (factor T, short or long). Thus, this was a 4×2^4 experiment. A $\frac{1}{2}$ fraction with $v = 32$ treatment combinations was selected and divided into two blocks of size 16, representing the number of observations that could be taken per day.

The 4-level factor A can be written in terms of two pseudofactors A_1 and A_2, with the level correspondence $0 = 00$, $1 = 01$, $2 = 10$, $3 = 11$. The researchers selected the first block of the

Table 15.16 A blocked $\frac{1}{2}$-fraction of a 4×2^4 experiment for the flour experiment

Block (day)	Treatment combination	Av. specific volume	Contrasts						
			A_1	A_2	A_1A_2	N	S	Q	T
I	000011	436	−1	−1	1	−1	−1	1	1
	000110	507	−1	−1	1	−1	1	1	−1
	001001	434	−1	−1	1	1	−1	−1	1
	001100	508	−1	−1	1	1	1	−1	−1
	010010	436	−1	1	−1	−1	−1	1	−1
	010111	508	−1	1	−1	−1	1	1	1
	011000	404	−1	1	−1	1	−1	−1	−1
	011101	510	−1	1	−1	1	1	−1	1
	100010	440	1	−1	−1	−1	−1	1	−1
	100111	517	1	−1	−1	−1	1	1	1
	101000	442	1	−1	−1	1	−1	−1	−1
	101101	501	1	−1	−1	1	1	−1	1
	110011	458	1	1	1	−1	−1	1	1
	110110	536	1	1	1	−1	1	1	−1
	111001	464	1	1	1	1	−1	−1	1
	111100	532	1	1	1	1	1	−1	−1
II	000000	567	−1	−1	1	−1	−1	−1	−1
	000101	549	−1	−1	1	−1	1	−1	1
	001010	391	−1	−1	1	1	−1	1	−1
	001111	418	−1	−1	1	1	1	1	1
	010001	458	−1	1	−1	−1	−1	−1	1
	010100	499	−1	1	−1	−1	1	−1	−1
	011011	381	−1	1	−1	1	−1	1	1
	011110	451	−1	1	−1	1	1	1	−1
	100001	499	1	−1	−1	−1	−1	−1	1
	100100	483	1	−1	−1	−1	1	−1	−1
	101011	368	1	−1	−1	1	−1	1	1
	101110	456	1	−1	−1	1	1	1	−1
	110000	475	1	1	1	−1	−1	−1	−1
	110101	597	1	1	1	−1	1	−1	1
	111010	414	1	1	1	1	−1	1	−1
	111111	452	1	1	1	1	1	1	1

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$\frac{1}{2}$-fraction with defining relation $I = A_1A_2NSQT$. The aliased pair of contrasts selected for confounding to provide two blocks were $NQ = A_1A_2ST$. Provided that the 16 combinations of design and noise factor levels were observed in a completely random order in each block, we can analyze this as an ordinary blocked fractional factorial experiment, without distinguishing between design and noise factors. The distinction between these two types of factors in made in Example 15.7.1, where flour sensitivity to the noise variable fluctuations is investigated.

The treatment combinations in each block are shown, prior to randomization, in Table 15.16, together with the main-effect contrast coefficients. All of the contrasts, apart from the confounded contrasts NQ and A_1A_2ST, are orthogonal to the block contrast, and the estimates of their aliased pairs can be

Table 15.17 Contrast estimates (with divisor 16) for the flour experiment

$\widehat{A_1}$	$\widehat{A_2}$	$\widehat{A_1A_2}$	$\widehat{N}$	$\widehat{S}$	$\widehat{Q}$	$\widehat{T}$
11.06	3.69	24.06	−52.44	59.81	−47.06	0.56
$\widehat{A_1N}$	$\widehat{A_2N}$	$\widehat{A_1A_2N}$	$\widehat{A_1S}$	$\widehat{A_2S}$	$\widehat{A_1A_2S}$	
5.44	7.56	−11.56	4.44	14.56	−2.31	
$\widehat{A_1Q}$	$\widehat{A_2Q}$	$\widehat{A_1A_2Q}$	$\widehat{A_1T}$	$\widehat{A_2T}$	$\widehat{A_1A_2T}$	
3.06	9.19	−17.19	9.19	9.56	−15.81	

Table 15.18 Analysis of variance for the flour experiment

Source of variation	Degrees of freedom	Sum of squares	Mean square	Ratio	p-value
Block	1	957.03	–		
A	3	5719.84	1906.62	3.06	0.0737
A_1	1	979.03	979.03	1.57	0.2363
A_2	1	108.78	108.78	0.17	0.6843
A_1A_2	1	4632.03	4632.03	7.42	0.0198
N	1	21997.53	21997.53	35.26	0.0001
S	1	28620.28	28620.28	45.88	0.0001
Q	1	17719.03	17719.03	28.40	0.0002
T	1	2.53	2.53	0.00	0.9504
AN	3	1763.59	587.86	0.94	0.4533
AS	3	1896.84	632.28	1.01	0.4235
AQ	3	3113.59	1037.87	1.66	0.2318
AT	3	3407.09	1135.69	1.82	0.2017
Error	11	6862.34	623.85		
Total	31	92059.72			

calculated without block adjustments. The response variable for each treatment combination was the average specific volume of three loaves, and the observed values (ml/100g) are listed in Table 15.16.

Multiplying the responses by the A_1 contrast coefficients and then dividing by $v/2 = 16$, we obtain the estimate of the difference in the effect of the high and low levels of A_1. Translating back to the original levels of factor A, this contrast compares the average of the flours 2 and 3 with the average of flours 0 and 1. The contrast estimate is $(7634 - 7457)/16 = 11.0625$ units.

The contrast for the interaction of the noise variable N with the pseudofactor A_1 is obtained by multiplying the A_1 and N contrast coefficients in Table 15.16 and dividing by $v/2 = 16$ to obtain the same standard error as the main-effect contrasts. Thus the contrast has coefficients

$$\begin{bmatrix} 1, & 1, -1, -1, & 1, & 1, -1, -1, -1, -1, & 1, & 1, -1, -1, & 1, & 1, \\ 1, & 1, -1, -1, & 1, & 1, -1, -1, -1, -1, & 1, & 1, -1, -1, & 1, & 1 \end{bmatrix}$$

with divisor 16, and the contrast estimate is $(7589 - 7502)/16 = 5.4375$. All of the main-effect and 2-factor interaction contrast estimates are shown in Table 15.17, and the analysis of variance table is shown in Table 15.18.

Selecting individual significance levels of $\alpha^* = 0.01$ for each hypothesis test, (for an overall Type I error probability of at most $\alpha = 0.09$), we compare the F-ratios in Table 15.18 with either

$F_{1,11,0.01} = 9.65$ or $F_{3,11,0.01} = 6.22$ as appropriate. The interactions of the flour formulations with the noise variables are not significantly different from zero, but the noise variables N, S, Q themselves do have a large effect on the specific volume. Although the flours are not significantly different in terms of average specific volume, the contrast A_1A_2 appears to be the most important of the three flour contrasts investigated. This contrast compares the average of flours 0 and 3 with the average of flours 1 and 2. The first pair give the higher average specific volume. Before the experiment took place, the experimenters had expected flour 3 (coded 11) to be the best. The difference of averages contrast, which compares flour 3 with the average of the other three flours, has least squares estimate

$$\begin{aligned}&\bar{y}_{11\ldots} - \tfrac{1}{3}(\bar{y}_{00\ldots} + \bar{y}_{01\ldots} + \bar{y}_{10\ldots})\\ &= 491.000 - \tfrac{1}{3}(476.250 + 455.875 + 463.250)\\ &= 25.875\,.\end{aligned}$$

A preplanned 95% confidence interval for this contrast is given by

$$\begin{aligned}&\bar{y}_{11\ldots} - \frac{1}{3}(\bar{y}_{00\ldots} + \bar{y}_{01\ldots} + \bar{y}_{10\ldots}) \pm t_{11,0.025}\sqrt{mse\left(\frac{1}{8} + 3\left(\frac{1}{9}\right)\left(\frac{1}{8}\right)\right)}\\ &= 25.875 \pm 2.201\sqrt{(623.849)(0.1667)}\\ &= 25.875 \pm 22.443\\ &= (3.432, 48.318).\end{aligned}$$

At the 95% confidence level, it does appear that flour 3 (coded 11) has specific volume between 3.4 and 48.3 units larger than the average of the other three flours. (We can draw this conclusion only because the contrast was preplanned. Otherwise, we would need to use Scheffé's method of multiple comparisons with $t_{11,0.025}$ replaced by $\sqrt{3F_{3,11,0.05}} = 3.24$, and the interval would include zero). □

15.6 Fractions from Orthogonal Arrays

15.6.1 2^p Orthogonal Arrays

The simplest type of orthogonal array is that shown in Table 15.19, consisting of a set of $2^p - 1$ orthogonal contrasts. The first column has the first half of its 8 entries equal to -1 and the second half equal to $+1$. The second column has the first quarter of its entries equal to -1, the second quarter equal to $+1$, the third quarter equal to -1 again and the fourth quarter equal to $+1$ again. The third column is divided into eighths with alternating -1's and $+1$'s. If the columns had been longer, the next column would have been divided into sixteenths, and so on. These are the "independent" columns. The fourth, fifth and sixth columns of Table 15.19 are the products of corresponding coefficients in the first three columns taken in pairs, and the last column is the triple product of the first three columns. The result is a table with $2^p = 8$ rows and $2^p - 1 = 7$ columns in which any pair of columns are orthogonal.

The p independent columns of an orthogonal array define the treatment combinations for a 2^p design. As usual, a contrast coefficient of -1 in a column corresponds to level 0 in the corresponding factor, and a contrast coefficient of $+1$ in a column corresponds to level 1. If all eight treatment combinations of Table 15.19 are used for a 2^3 experiment, then the orthogonal array defines a full factorial experiment, and no aliasing of contrasts occurs.

Now suppose that only 4 observations can be taken in a 2^3 experiment. Instead of proceeding as in Sect. 15.2.1 and choosing a defining relation, we could first construct an orthogonal array with $n = 4$

Table 15.19 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

Table 15.20 An orthogonal array for four observations

−1	−1	1
−1	1	−1
1	−1	−1
1	1	1

Table 15.21 $\frac{1}{2}$-fractions of a 2^3 experiment obtained from orthogonal arrays

Design d_1				Design d_2			
TC	A	B	C	TC	A	B	C
001	−1	−1	1	011	−1	1	1
010	−1	1	−1	000	−1	−1	−1
100	1	−1	−1	110	1	1	−1
111	1	1	1	101	1	−1	1

rows and $n - 1 = 3$ columns. One is shown in Table 15.20, where the first column has the first half of its entries -1, and the second half $+1$, the second column is divided into quarters, and the third column is the product of the first two. Since we have 3 factors, suppose we label the columns in order as A, B, C. The three columns then show the parts of the A, B, and C contrasts corresponding to a $\frac{1}{2}$-fraction. However, the third column is also the product of the first two columns, so it not only represents the C contrast but also the interaction between A and B. Consequently, C is aliased with AB. Similarly, the first column is the product of the last two columns, so A is aliased with BC. Similarly, again, B is aliased with AC. The defining relation must be $I = ABC$ in order to produce this aliasing scheme.

The coefficients in the contrasts tell us which treatment combinations are represented, and the design is "Design d_1" shown in Table 15.21. Notice that this is the same design that would have been produced from the equation $a_1 + a_2 + a_3 = 1 \pmod 2$. We could obtain the $\frac{1}{2}$-fraction corresponding to $a_1 + a_2 + a_3 = 0 \pmod 2$, by multiplying any one of the columns by -1 (see Design d_2 in Table 15.21, where the second column has been multiplied by -1).

Thus, we have arrived back at the same type of design that we studied in Sect. 15.2, and this will often (but not always) be the case. The main difference in procedure is that when we start with an orthogonal array, we are starting with an unlabeled list of contrasts which can be labeled in any way we please. The labeling then determines the defining relation and the design.

Any columns in an orthogonal array can be multiplied by -1 and we still obtain an orthogonal array, although the treatment combinations may not be identical, or they may be identical but in a different order (try multiplying the B and C columns for the designs in Table 15.21 by -1 and see whether the same design results).

Now we return to the orthogonal array of Table 15.19, which is reproduced in Table 15.22 with column headings indicating which columns are products of which other columns. For example, column

Table 15.22 An orthogonal array for 8 observations

Columns						
1	2	3	12	13	23	123
−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	−1	1	1
1	−1	1	−1	1	−1	−1
1	1	−1	1	−1	−1	−1
1	1	1	1	1	1	1

7 is the product of columns 1, 2, and 3. We consider using this array for a 2^5 experiment instead of a 2^3 experiment. Since there are only 8 rows, we will be looking for a $\frac{1}{4}$-replicate (that is, a 2^{5-2} fractional factorial experiment).

Suppose that we label the first 5 columns as A, B, C, D, E. Since the product of the first two columns gives column 4 and the product of the first and third columns gives column 5, aliasing would occur between D and AB, and between E and AC. Consequently, ABD and ACE must be in the defining relation, together with their product, so we have

$$I = ABD = ACE = BCDE\,.$$

The rest of the aliasing scheme can be written out also, and we would see that A is aliased with BD and CE, that B is aliased with AD, and that C is aliased with AE. The sixth column, which is the product of columns 2 and 3, and also of columns 4 and 5, can be labeled BC or DE, and these two interactions are aliased. The seventh column is $ABC = CD = BE = ADE$. The eight treatment combinations are deduced from the −1's and +1's in the first five columns; that is,

$$00011,\ 00110,\ 01001,\ 01100,\ 10000,\ 10101,\ 11010,\ 11111\,.$$

Different sets of treatment combinations corresponding to the same defining relation (but with different signs in the aliasing scheme) can be obtained by multiplying one or more columns of Table 15.22 by −1.

There is nothing special about labeling the first five columns of Table 15.22 as A, B, C, D, E. Any five columns could have been chosen. Different choices may lead to different aliasing schemes, and sometimes these aliasing schemes may not be equally good. Table 15.62 at the end of the chapter lists orthogonal arrays for various-sized experiments. Some useful column labelings for various fractional factorial experiments are suggested in the table.

The standard notation, used by industrial statisticians and engineers, for an orthogonal array is the letter L with subscript equal to the number of runs. Sometimes, the largest Resolution III design that can be used with the array is added in brackets. The orthogonal array in Table 15.20 provides a Resolution III design with 4 observations for 3 or fewer two-level factors and would be written as $L_4(2^3)$. Similarly, the design of Table 15.22 would be written as $L_8(2^7)$. Occasionally, an orthogonal array for 2^p experiments will be written using factor levels rather than contrast coefficients. The orthogonality could then be checked by ensuring that in every pair of columns, all possible pairs of factor levels (00, 01, 10, and 11) appear the same number of times (see, for example, the factor levels shown together with designs d_1 and d_2 of Table 15.21).

Table 15.23 Treatment factors and their levels for the wafer experiment

Factors	Prior level	Experimental levels	
		Low (0)	High (1)
A (rotation method)	Oscillating	Continuous	Oscillating
B (wafer batch)		668G4	678D4
C (deposition temperature)	1215 °C	1210 °C	1220 °C
D (deposition time)	Low	High	Low
E (arsenic flow rate)	57%	55%	59%
F (acid etch temp.)	1200 °C	1180 °C	1215 °C
G (acid flow rate)	12%	10%	14%
H (nozzle position)	4	2	6

Example 15.6.1 `Wafer experiment`

R. Kackar and A. Shoemaker (*AT&T Technical Journal*, 1986) describe an experiment that they helped to run at AT&T to try to reduce the variability of the thickness of an "epitaxial layer" deposited onto silicon wafers during the manufacture of integrated circuit devices.

The wafers were mounted on a seven-sided "susceptor" with two wafers (one above the other) on each side. The susceptor rotated inside a heated bell jar as chemical vapors were introduced via a nozzle near the top of the jar. The chemicals were deposited on the wafers, and the bell jar was cooled when the thickness of the deposited layer was close to the target of 14.5 μm.

The engineers identified eight factors that might affect the variability of the thickness of the epitaxial layer. These are shown in Table 15.23 together with the operating factor levels prior to the experiment and the levels selected for the experiment.

The experimenters decided to take 16 observations. The 16 treatment combinations were selected via the orthogonal array $L_{16}(2^{15})$ shown in Table 15.24. The orthogonal array is constructed as described earlier in this section. The labels in the row headings of the table identify which columns are products of which other columns. The assignment of factors to columns chosen by the experimenters is indicated in the foot of the table. The experiment is a 2^{8-4} experiment, and the defining relation is generated by 4 confounded interactions. Notice, from the heading and the foot of Table 15.24, that D must be aliased with ABC, F must be aliased with ABE, G with ACE, and H with BCE. Thus, the defining relation includes $ABCD$, $ABEF$, $ACEG$, $BCEH$, and all their possible products (a total of $2^4 = 16$ terms in the defining relation):

$$\begin{aligned} I &= ABCD = ABEF = CDEF \\ &= ACEG = BDEG = BCFG = ADFG \\ &= BCEH = ADEH = ACFH = BDFH \\ &= ABGH = CDGH = EFGH = ABCDEFGH \end{aligned}$$

This is a Resolution IV design, and there is considerable aliasing between 2-factor interactions. For example, the contrast listed in column labelled 12 in Table 15.24 not only measures the 2-factor interaction AB, but also measures its aliased 2-factor interactions CD, EF, GH (and some higher-order interactions).

There were 70 measurements taken for each treatment combination (5 measurements on each of the 2 wafers on the 7 sides of the receptor). From these, two different response variables were calculated—the average of the 70 measurements (which we denote by x) and the log sample variance of the 70

Table 15.24 An orthogonal array for 16 observations: An $L_{16}(2^{15})$

							Columns							
1	2	12	3	13	23	123	4	14	24	124	34	134	234	1234
−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	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	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	−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	−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	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	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	−1
1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
A	B		C			D	E			F		G	H	

Table 15.25 Treatment combinations and response variables for the wafer experiment

Treatment combination	Average response $x_{ijklmnpq}$	Log variance response $v_{ijklmnpq}$
0 0 0 0 0 0 0 0	14.821	−0.4425
0 0 0 0 1 1 1 1	14.888	−1.1989
0 0 1 1 0 0 1 1	14.037	−1.4307
0 0 1 1 1 1 0 0	13.880	−0.6505
0 1 0 1 0 1 0 1	14.165	−1.4230
0 1 0 1 1 0 1 0	13.860	−0.4969
0 1 1 0 0 1 1 0	14.757	−0.3267
0 1 1 0 1 0 0 1	14.921	−0.6270
1 0 0 1 0 1 1 0	13.972	−0.3467
1 0 0 1 1 0 0 1	14.032	−0.8563
1 0 1 0 0 1 0 1	14.843	−0.4369
1 0 1 0 1 0 1 0	14.415	−0.3131
1 1 0 0 0 0 1 1	14.878	−0.6154
1 1 0 0 1 1 0 0	14.932	−0.2292
1 1 1 1 0 0 0 0	13.907	−0.1190
1 1 1 1 1 1 1 1	13.914	−0.8625

Source Kackar and Shoemaker (1986).

measurements (which we denote by v). The treatment combinations, corresponding to the orthogonal array in Table 15.24, together with the two response variables, are shown in Table 15.25.

The experimenters first analyzed the log variance response. The contrast estimates (high level minus low level) for this response variable are shown in Table 15.26. The contrast estimates for factors A and H are considerably larger in absolute value than those for the other factors. Consequently, A and H

Table 15.26 Contrast estimates for log sample variance response variable

Contrast	*A*	*B*	*C*	*D*	*E*	*F*	*G*	*H*
Estimate	0.352	0.122	0.105	−0.249	−0.012	−0.072	−0.101	−0.566

Table 15.27 Contrast estimates for the mean response variable

Contrast	*A*	*B*	*C*	*D*	*E*	*F*	*G*	*H*
Estimate	−0.055	0.056	−0.109	−0.836	−0.067	0.060	−0.098	0.142

should be investigated for reducing variability in the response. Since the log variance response is to be reduced, and the contrast estimate for A is positive while that for H is negative, we would want to set A at its low level (continuous rotation) and H at its high level (position 6). All other factors can be set at their current operating conditions.

The second requirement of the experimenters was to achieve an average thickness of 14.5 μm. Contrast estimates for the average response are shown in Table 15.27. Not surprisingly, factor D, deposition time, has by far the largest effect on the mean response, and the experimenters were able to adjust this factor in order to meet the target.

As with any good experiment, the experimenters wished to confirm their results. Their confirmation experiment investigated two treatment combinations. The first treatment combination consisted of the prior operating levels of factors A and C–H, with factor B at level 1, and the second treatment combination was the same except that the levels of A and H were changed as discussed above. The confirmation experiment showed that the variance of the thickness had been reduced by a factor 2.5—quite a remarkable result! □

15.6.2 $2^p \times 4^q$ Orthogonal Arrays

The orthogonal arrays of Sect. 15.6.1 can be used when one or more factors have 4 levels. Each 4-level factor requires 3 independent columns to represent 3 orthogonal contrasts. For example, the orthogonal array in Table 15.22 could be used for a $2^3 \times 4$ experiment as follows. The first three columns (which are independent) could be labeled A, B, and C. If the 4th column is labeled D_1, then D_1 is aliased with AB. If the 7th column is labeled D_2, then D_2 is aliased with ABC. The product of the coefficients in the 4th and 7th columns gives the coefficients in the 3rd column, so D_1D_2, the remaining contrast for the 4-level factor, is aliased with C, and the defining relation is

$$I = ABD_1 = ABCD_2 = CD_1D_2 \,.$$

This is a Resolution II design, which should be avoided if possible, since it confounds two main effects (C and D). A better design is to assign D_2 to the 5th column, where it is aliased with AC. The product of the 4th and 5th columns gives the 6th column, so that D_1D_2 is aliased with BC. The defining relation is

$$I = ABD_1 = ACD_2 = BCD_1D_2 \,,$$

which is Resolution III. The 7th column of Table 15.22 corresponds to the ABC contrast, which is aliased with AD_1D_2, CD_1, and BD_2 contrasts. The complete aliasing scheme is

$$
\begin{aligned}
I &= ABD_1 &&= ACD_2 &&= BCD_1D_2\\
A &= BD_1 &&= CD_2 &&= ABCD_1D_2\\
B &= AD_1 &&= ABCD_2 &&= CD_1D_2\\
C &= ABCD_1 &&= AD_2 &&= BD_1D_2\\
D_1 &= AB &&= ACD_1D_2 &&= BCD_2\\
D_2 &= ABD_1D_2 &&= AC &&= BCD_1\\
D_1D_2 &= ABD_2 &&= ACD_1 &&= BC\\
ABC &= CD_1 &&= BD_2 &&= AD_1D_2
\end{aligned}
$$

The design would be useful for an experiment where all interactions were expected to be negligible, in which case one degree of freedom would be available to estimate σ^2.

15.6.3 3^p Orthogonal Arrays

The orthogonal arrays for 2^p experiments introduced in Sect. 15.6.1 have the property that any pair of columns in the array are orthogonal (that is, the sum of the products of corresponding coefficients is zero). An examination of the arrays in Tables 15.21, 15.22 and 15.24 reveals that this orthogonality arises because every pair of coefficients $(-1, -1)$, $(-1, 1)$, $(1, -1)$ and $(1, 1)$ occurs equally often in every pair of columns. We could rewrite the array to contain the factor labels 0, 1 instead of the contrast coefficients -1, 1, and every pair of labels would occur the same number of times in every pair of columns. This is the way that orthogonal arrays are defined for 3^p experiments.

An orthogonal array with 9 treatment combinations is shown as columns 1–4 in Table 15.28 for four factors, each having 3 levels. If any pair of columns is selected, it can be verified that each of the nine pairs of levels (0, 0), (0, 1), (0, 2), (1, 0), (1, 1), (1, 2), (2, 0), (2, 1), (2, 2) occurs once. The first column consists of three copies of each of 0, 1, and 2. The second column consists of 0, 1, and 2, in order, repeated three times. The third column is obtained from the sum of the coefficients in the first two columns reduced modulo 3 (thereby ensuring that any factor assigned to the 3rd column will be aliased with the interaction between the first two factors). The fourth column is obtained from twice the sum of columns 2 and 3 (ensuring that any factor assigned to the fourth column will be aliased with the interaction of factors assigned to columns 2 and 3 and with the interaction of the first two factors). It is not possible to find more than four orthogonal columns with only 9 observations.

Table 15.28 A 3^p orthogonal array for 9 observations

Columns				Contrasts							
1	2	3	4								
0	0	0	0	−1	1	−1	1	−1	1	−1	1
0	1	1	1	−1	1	0	−2	0	−2	0	−2
0	2	2	2	−1	1	1	1	1	1	1	1
1	0	1	2	0	−2	−1	1	0	−2	1	1
1	1	2	0	0	−2	0	−2	1	1	−1	1
1	2	0	1	0	−2	1	1	−1	1	0	−2
2	0	2	1	1	1	−1	1	1	1	0	−2
2	1	0	2	1	1	0	−2	−1	1	1	1
2	2	1	0	1	1	1	1	0	−2	−1	1
A	B	C	D	A_L	A_Q	B_L	B_Q	C_L	C_Q	D_L	D_Q

In Table 15.28, a pair of orthogonal contrasts is given corresponding to each of the four columns in the orthogonal array. It can be verified that this set of 8 contrasts is orthogonal. As for 2^p experiments, a 3^p orthogonal array with n rows can have at most $n-1$ orthogonal columns of contrast coefficients, and therefore can accommodate at most $(n-1)/2$ three-level factors. An experiment is discussed in Sect. 15.7.1 that uses part of the orthogonal array for three-level factors and 27 observations listed in Table 15.65.

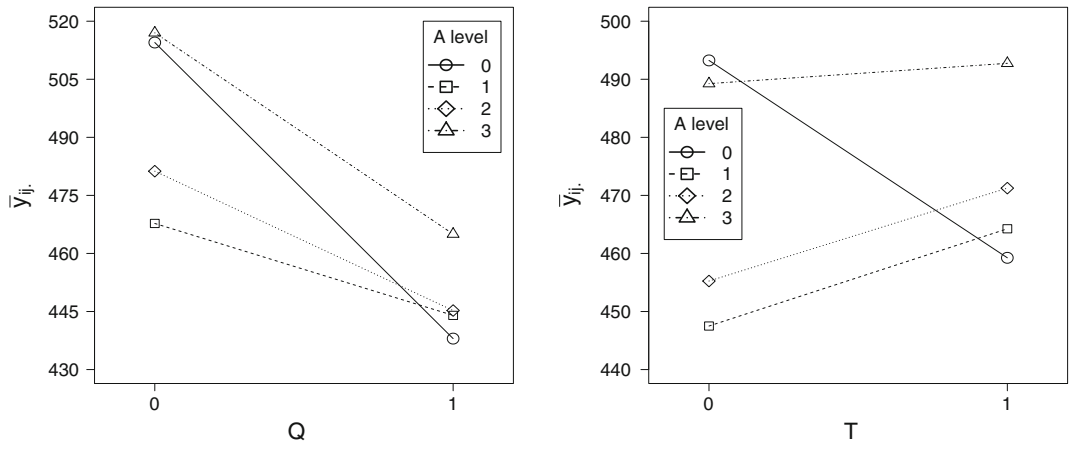
15.7 Design for the Control of Noise Variability

Design for the control of noise variability is sometimes known as *robust design* or *parameter design* and refers to the procedure of developing or designing a product in such a way that it performs consistently, and as intended, under the variety of conditions of its use throughout its life. The ideas apply equally well to the design of high quality manufacturing systems and other organizational processes. Experimentation involves *design factors* (also known as *control factors*) which are possible inexpensive to control in the design of the product, and *noise factors* which may affect the performance of a product but which are difficult or impossible to control when the product is in use.

Experiments that involve both design and noise factors are often known colloquially as *Taguchi experiments*. Dr. Taguchi was a Japanese quality consultant who advocated the use of quality improvement techniques, including the design of experiments, to the Japanese engineering and industrial communities from the 1950s. One of his fundamental contributions is the principle that reduction of variation is generally the most difficult task from an engineering perspective and so should be the focus of attention during the design of a product.

There are two different types of designs for such experiments—"mixed arrays" and "product arrays". *Mixed arrays* are ordinary fractional factorial designs in which the difference between the design and noise factors is ignored at the design stage except to ensure that the design-by-noise interactions are estimable and not confounded with blocks or part of the defining relation of a fraction. For a mixed array, all of the treatment combinations (composed of both design and noise factors) are observed in a random order. This complete randomization allows the design×noise interactions to be studied and to try to identify the particular levels of the design factors that are least affected by changing the levels of the noise factors. An example of a mixed array is the design used for the flour experiment of Example 15.5.1 which had a single design factor at 4 levels and four two-level noise factors. Analysis of the design×noise interactions for this experiment is illustrated in Example 15.7.1 assuming a complete randomization of the order of treatment combinations.

Product arrays, on the other hand, are designed and analyzed differently from the usual factorial experiments. They are composed of two fractional factorial or full factorial experiments, one for the design factors and one for the noise factors. Then every combination of design factors is observed in conjunction with every combination of noise factors. In product arrays, first the order of the design factor combinations is randomized. Then, for each design factor combination in turn, observations are taken across all of the noise factor combinations in a random or non-random order. (Occasionally, the order of randomization is reversed). This restricted randomization means that the usual analysis of design×noise interactions is not valid. Instead, for each design factor combination, the average and log sample variance of the responses are calculated across the different noise factor combinations. The average response and the log variance response are then taken as two totally separate sets of data values and analyzed separately. The objective of the experiment is to find out which factors affect the log sample variance response the most, and which factors most affect the average response. Design factor combinations are then sought that give a low variance across the noise factor combinations and also that give an average response close to the target value. Finally, confirmatory observations are taken.



(a) AQ interaction (b) AT interaction

Fig. 15.7 Design×noise interaction plots for the flour experiment

The reason for taking the log of the sample variance before analysis is that the assumptions of the linear model with $\ln(s^2)$ as the response are more closely satisfied than taking s^2 itself as the response. (See Sect. 5.6.2 for a discussion of transformations).

The design of the wafer experiment of Example 15.6.1 was a product array, where the levels of the single noise factor "position" were the 14 locations on the susceptor and the two responses, log sample variance response and average response, were analyzed separately. An example of a product array with factors at three levels will be illustrated in Sect. 15.7.1 and the computer analysis discussed in Sects. 15.9.2 and 15.10.2 using SAS and R software, respectively.

Example 15.7.1 `Flour experiment, continued`

One purpose of the flour experiment in Example 15.5.1, p. 513, was to find which of the four flours (factor A, coded 0, 1, 2, 3) was least variable under the different levels of the four noise variables: amount of yeast (factor N), proof time (factor S), degree of mixing and moulding (factor Q), and dough time delay (factor T), each at 2 levels. If the treatment combinations in Table 15.16 were observed in a completely random order within each block, this experiment can be analyzed as a mixed array. The analysis of variance is shown in Table 15.18. The interactions of A with the noise variables can provide information on which flours are least sensitive to noise fluctuations.

Although none of the design×noise factor interactions were significantly different from zero, we illustrate the search for robust design factor levels by looking at the two largest such interactions (AQ and AT). If we plot the average response (specific volume) for each level of A versus the levels of Q or T on the horizontal axis, we obtain the interaction plots in Fig. 15.7. From the AT interaction plot, we can see that flours 1, 2, and 3 are much more stable than flour 0 in terms of the resulting average specific volume of loaves. This is also apparent, to a lesser extent, in the AQ interaction plot. Thus, flour 0 is not as robust as the other three flours and should probably be ruled out of consideration for general use.

If we now examine the average specific volume of loaves baked over the two levels of T, we see that flour 3 seems to be the best. a similar result is obtained by averaging over the levels of Q. In this experiment, the AQ and AT interactions were not significantly different from zero, but if they had been larger, this analysis would have pointed to flour 3 being preferable both in terms of robustness to fluctuating noise factors and of leading to loaves with high specific volume.

15.7.1 A Real Experiment—Inclinometer Experiment

A collaborative study involving statisticians and mechanical engineers was described by S. Lewis, B. Hodgson, R. New, and C. Sexton in the 1989 *Proceedings of the Institute of Mechanical Engineers International Conference on Engineering Design*. The experiment sought to improve the performance of an inclinometer, which is an instrument that records the angle of tilt of an object such as a crane jib. The design of the inclinometer is described in the article as follows:

"The basic design of the product is composed in four parts: a bob-weight and flexure, a flanged flywheel and a copper-plated disc (PCB). All are attached to a shaft supported in low-friction bearings. When the object to which the flywheel is attached is tilted, the bob-weight assembly moves to stay perpendicular to the earth, causing the PCB to rotate relative to the casing. The main performance difficulty of the inclinometer is that it does not immediately register the true angle of tilt. Spurious swing of the disc is produced by movement of the object."

The purpose of the experiment was to vary the relative sizes of the parts of the inclinometer to find a combination of factors that would reduce the swing. The engineers identified 7 design factors (A–G) that could be altered and that might affect the swing. Three levels were selected for each factor so that linear and quadratic trends could be investigated. The levels of factors A–F were equally spaced. The factors were:

A: Flexure length (30.00, 31.25, 32.5) B: Flexure thickness (0.05, 0.275, 0.5)
C: Flexure width (4.0, 5.0, 6.0) D: Flange thickness (1.0, 3.5, 6.0)
E: Flange width (6.0, 10.5, 15.0) F: Bob-weight length (12.0, 20.0, 28.0)
G: Copper plating thickness (0.0175, 0.035, 0.07)

All measurements are in millimeters, and the levels of all factors are coded 0, 1, and 2. For the experiment, it was possible to produce the factor levels exactly as specified, but in mass production variability naturally creeps in. The experimenters decided to build the production variability into the experiment as noise factors as follows (measured in mm, except where stated):

H: Flexure length $(-0.25, +0.25)$ P: Flexure thickness $(-0.005, +0.005)$
J: Flange thickness $(-0.025, +0.025)$ K: Flange width $(-0.025, +0.025)$
L: Copper plating thickness $(-0.005, +0.005)$
M: Tolerance on bob weight mass $(-9.0, +9.0 \times (1/100)g)$
N: Maximum horizontal amplitude of vibration (5, 25)

The two levels of each noise factor were coded as 0 and 1. Thus, the entire experiment was a $3^7 \times 2^7$ factorial experiment, where the 3-level factors were the design factors and the 2-level factors were the noise factors. The treatment combination 0000000 of the design factors in conjunction with the combination 0000000 of the noise factors would have flexure length (A) of $(30.00 - 0.25)$ mm $=$ 29.75 mm, flexure thickness of $(0.050 - 0.005)$ mm $= 0.045$ mm, and so on.

The objective of the experiment was to select the combinations of the design factors that gave the least amount of swing. In terms of producing a product of consistently high quality, it was also important that the variability of the amount of swing also remain low across the different noise combinations.

The experimenters selected a product array formed from a $\frac{1}{81}$ fraction of the 3^7 design-treatment combinations and a $\frac{1}{16}$ fraction of the 2^7 noise combinations. This gave a total of $27 \times 8 = 216$ observations. For the 3^{7-4} fractional factorial experiment, seven columns of the orthogonal array $L_{27}(3^{13})$ were selected. These are indicated in Table 15.65 (p. 562). For the 2^{7-4} fractional factorial experiment, the orthogonal array $L_8(2^7)$ shown in Table 15.22 (p. 518) was used with the noise factors

Table 15.29 Maximum angle of swing for the inclinometer experiment. Combinations of design factors A–G are in rows, and combinations of noise factors H–N are in columns

Noise factors	H	0	0	0	0	1	1	1	1		
	P	0	0	1	1	0	0	1	1		
	J	0	0	1	1	1	1	0	0		
	K	0	1	0	1	0	1	0	1		
	L	0	1	0	1	1	0	1	0		
	M	0	1	1	0	0	1	1	0		
	N	0	1	1	0	1	0	0	1		
Design factors											
$ABCDEFG$										Mean	$\ln(s^2)$
0 0 0 0 0 0 0		0.62	3.54	3.56	0.62	3.09	0.71	0.73	3.20	2.01	0.73
0 0 1 1 1 1 1		0.59	3.11	3.11	0.59	2.98	0.63	0.64	3.02	1.83	0.53
0 0 2 2 2 2 2		0.59	3.01	3.02	0.59	2.97	0.61	0.62	3.00	1.80	0.50
0 1 0 1 1 2 2		0.51	2.65	2.65	0.50	2.53	0.53	0.54	2.56	1.56	0.21
0 1 1 2 2 0 0		0.18	0.96	0.96	0.18	0.89	0.19	0.20	0.90	0.56	−1.85
0 1 2 0 0 1 1		1.88	9.58	9.55	1.85	9.30	1.92	1.94	9.48	5.69	2.80
0 2 0 2 2 1 1		0.19	1.03	1.03	0.19	0.97	0.21	0.21	0.93	0.60	−1.72
0 2 1 0 0 2 2		1.85	9.46	9.42	1.82	9.19	1.90	1.92	9.35	5.61	2.77
0 2 2 1 1 0 0		0.52	2.73	2.72	0.51	2.61	0.55	0.56	2.64	1.61	0.27
1 0 0 1 2 1 2		0.29	1.56	1.56	0.29	1.45	0.31	0.32	1.47	0.91	−0.87
1 0 1 2 0 2 0		0.95	4.98	4.93	0.94	4.79	0.99	1.00	4.82	2.93	1.48
1 0 2 0 1 0 1		1.16	6.09	6.09	1.13	5.70	1.21	1.26	5.93	3.57	1.87
1 1 0 2 0 0 1		0.26	1.45	1.45	0.25	1.30	0.29	0.30	1.30	0.83	−1.05
1 1 1 0 1 1 2		1.15	5.99	5.92	1.13	5.69	1.19	1.22	5.84	3.51	1.84
1 1 2 1 2 2 0		0.85	4.31	4.30	0.84	4.23	0.86	0.88	4.28	2.57	1.21
1 2 0 0 1 2 0		1.10	5.74	5.67	1.07	5.43	1.14	1.17	5.57	3.36	1.75
1 2 1 1 2 0 1		0.29	1.55	1.55	0.28	1.45	0.31	0.32	1.47	0.90	−0.88
1 2 2 2 0 1 2		0.91	4.64	4.66	0.90	4.56	0.94	0.95	4.57	2.77	1.35
2 0 0 2 1 2 1		0.39	2.05	2.06	0.39	1.96	0.41	0.42	1.97	1.21	−0.30
2 0 1 0 2 0 2		0.67	3.61	3.57	0.65	3.27	0.72	0.74	3.41	2.08	0.79
2 0 2 1 0 1 0		1.42	7.31	7.38	1.41	7.14	1.48	1.51	7.24	4.36	2.27
2 1 0 0 2 1 0		0.69	3.66	3.60	0.67	3.37	0.73	0.74	3.47	2.12	0.82
2 1 1 1 0 2 1		1.18	6.04	6.06	1.17	5.90	1.21	1.23	5.95	3.59	1.88
2 1 2 2 1 0 2		0.37	1.95	1.95	0.37	1.87	0.39	0.40	1.88	1.15	−0.40
2 2 0 1 0 0 2		0.39	2.15	2.16	0.38	1.94	0.44	0.44	1.96	1.23	−0.25
2 2 1 2 1 1 0		0.44	2.29	2.29	0.43	2.21	0.46	0.47	2.22	1.35	−0.07
2 2 2 0 2 2 1		1.84	9.35	9.19	1.79	9.06	1.85	1.89	9.28	5.53	2.74

Sources Lewis, Hodgson, New, and Sexton (1989).

Table 15.30 Analysis of variance for $\ln(s^2)$ response for the inclinometer experiment

Source of variation	Degrees of freedom	Sum of squares	Mean square	Ratio	p-value
A	2	0.6316	0.3158		
Linear A	1	0.5798	0.5798	22.73	0.0005
Quadratic A	1	0.0519	0.0519	2.03	0.1794
B	2	0.1358	0.0679		
Linear B	1	0.0581	0.0581	2.28	0.1571
Quadratic B	1	0.0777	0.0777	3.05	0.1064
C	2	9.8448	4.9224		
Linear C	1	9.8241	9.8241	385.18	0.0001
Quadratic C	1	0.0207	0.0207	0.81	0.3852
D	2	18.8987	9.4493		
Linear D	1	18.3769	18.3769	720.53	0.0001
Quadratic D	1	0.5217	0.5217	20.46	0.0007
E	2	7.0366	3.5183		
Linear E	1	7.0044	7.0044	274.63	0.0001
Quadratic E	1	0.0322	0.0322	1.26	0.2829
F	2	9.5150	4.7575		
Linear F	1	9.4043	9.4043	368.73	0.0001
Quadratic F	1	0.1106	0.1106	4.34	0.0593
G	2	0.0354	0.0177	0.69	0.5184
Error	12	0.3061	0.0255		
Total	26	46.4039			

Table 15.31 Contrast estimates (log var response) for the nonnegligible contrasts

Lin A	Lin C	Lin D	Quad D	Lin E	Lin F
0.359	1.478	−2.021	0.590	−1.248	1.446

assigned to the columns in the order $H, P, K, -J, -L, -M, N$, where the minus signs indicate that the column was multiplied by -1 (thus reversing the high and low levels).

The maximum absolute angle of swing was ascertained for each of the selected combinations of design- and noise- factor levels, and these are shown in Table 15.29. The noise-factor combinations label the columns, and the design-factor combinations label the rows. The last two columns of the table show the average and log sample variance of the observations for the design combinations calculated across the noise combinations.

Consider first using the log sample variance $\ln(s^2)$ of the observations as the response variable. The analysis of variance table is shown in Table 15.30. We have included the information needed for testing the hypotheses of negligible linear and quadratic trends in each of the factors except for G. The levels of G are not equally spaced, and therefore the correct trend contrast coefficients are not those shown in Table A.2.

If we test the hypotheses of negligible contrasts for each trend contrast shown in Table 15.30 at individual significance levels $\alpha^* = 0.01$ and test the hypothesis of no effect of factor G at level $\alpha^* = 0.01$ (for an overall level of at most $\alpha = 0.13$), we reject the hypotheses of negligible linear trends in factors A, C, D, E, F and of a negligible quadratic trend in factor D. Factors B and G show very little effect on log variance response, so these factors (flexure thickness and copper plating thickness) cannot be employed to achieve less variability in the swing in the inclinometer. The contrast estimates for the nonnegligible contrasts are shown in Table 15.31. From the signs on the contrast estimates we

Table 15.32 Analysis of variance for average response for the inclinometer experiment

Source of variation	Degrees of freedom	Sum of squares	Mean square	Ratio	p-value
A	2	0.1288	0.0644		
Linear A	1	0.1023	0.1023	0.53	0.4813
Quadratic A	1	0.0264	0.0264	0.14	0.7183
B	2	0.2899	0.1449		
Linear B	1	0.2850	0.2850	1.47	0.2486
Quadratic B	1	0.0049	0.0049	0.03	0.8768
C	2	12.9528	6.4764		
Linear C	1	12.8863	12.8863	66.48	0.0001
Quadratic C	1	0.0665	0.0665	0.34	0.5689
D	2	24.6042	12.3021		
Linear D	1	22.9193	22.9193	118.23	0.0001
Quadratic D	1	1.6850	1.6850	8.69	0.0122
E	2	9.0561	4.5280		
Linear E	1	7.9385	7.9385	40.95	0.0001
Quadratic E	1	1.1177	1.1177	5.77	0.0334
F	2	11.5710	5.7855		
Linear F	1	11.2476	11.2476	58.02	0.0001
Quadratic F	1	0.3234	0.3234	1.67	0.2208
G	2	0.6725	0.3362	1.73	0.2179
Error	12	2.3262	0.1938		
Total	26	61.6014			

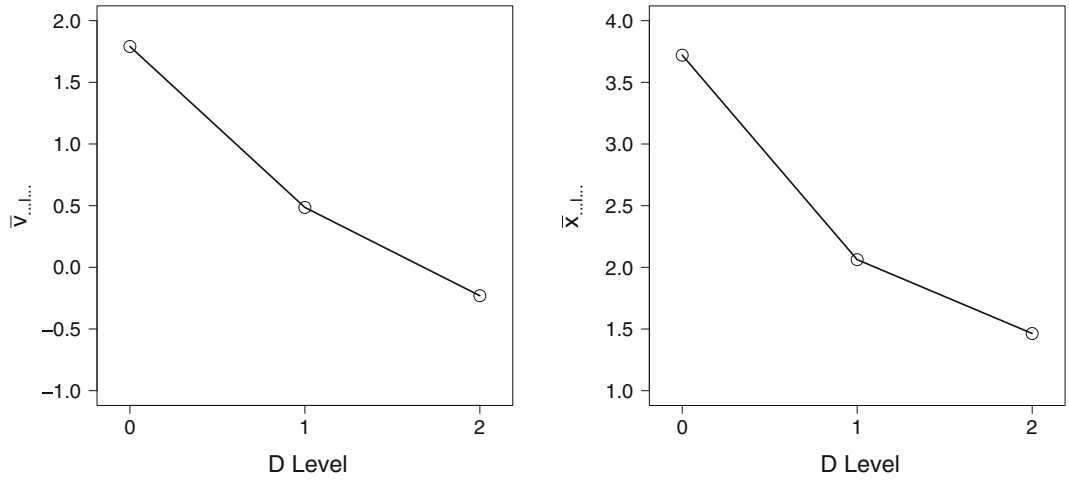
Table 15.33 Contrast estimates (average response)

Lin C	Lin D	Quad D	Lin E	Quad E	Lin F
1.692	−2.257	1.060	−1.328	0.863	1.581

see that in order to reduce the variability of the swing, factors A, C, and F should be set at their low levels, while E should be set at its high level and, from the main effect plot in Fig. 15.8(a) D should also be set at its high level.

In order to reduce the size of the swing, we need to use as response variable the average swing for each design combination (averaged over the noise combinations). These are listed in Table 15.29. The analysis of variance (shown in Table 15.32) identifies the linear trends of factors C, D, E, and F as having large effects on the swing. The contrast estimates are shown in Table 15.33. The signs of the estimates suggest that factors D and E should be set at their high levels and factors C and F at their low levels. Since this agrees with the conclusions of the analysis of variability, it is possible to reduce the size and the variability of the swing simultaneously.

Plots of the least squares estimates of the effect of the levels of factor D for both log sample variance and average response are shown in Fig. 15.8. The conclusions of the experiment are that the dimensions of the flexure and bob-weight (A, C, F) should be decreased, while the dimensions of the flange (D, E) should be increased. The experimenters comment in the article that the results match what would be expected by engineering principles. The SAS and R commands for analyzing this product array experiment are discussed in Examples 15.9.1 and 15.10.1, respectively.



(a) Log sample variance response (b) Average response

Fig. 15.8 Plots of the effect of the levels of factor D for the inclinometer experiment, where $x_{ijklmnp}$ denotes average response and $v_{ijklmnp}$ denotes the log sample variance $\ln(s^2)$ for corresponding design factor combinations

15.8 Small Screening Designs: Orthogonal Main Effect Plans

15.8.1 Saturated Designs

A design is called *saturated* if it uses only n treatment combinations, observed once each, to estimate $n - 1$ factorial contrasts plus a mean. Saturated designs were studied in Sect. 7.5, p. 219, under the heading "one observation per cell". Due to the lack of degrees of freedom for estimating σ^2, half-normal probability plots and the Voss-Wang method of simultaneous confidence intervals were used to identify contrasts with large effects. Similarly, the soup fractional factorial experiment in Sect. 15.2.3, p. 499, used a saturated design with $n = 16$ observations to measure a mean, 5 main effects and 10 two-factor interactions, using a half-normal probability plot for identifying unusually large contrasts.

In the extreme case, a saturated design may have only n observations for measuring the main effects of $p = n - 1$ factors plus a mean, in which case interactions cannot be measured separately from the main effects. If the $n - 1$ main effect contrasts are orthogonal, such designs are known as *orthogonal main-effect plans*. For example, the two designs in Table 15.21, p. 517, are saturated orthogonal main-effect plans for measuring the main effects of three factors, and the design of Table 15.22 is a saturated orthogonal main effect plan for measuring the main effects of 7 two-level factors. The design discussed in Sect. 15.3.2, p. 511, is an orthogonal main effect plan for four 3-level factors and $n = 9$ observations, and so is the design of Table 15.28, p. 523.

Orthogonal main-effect plans can be used in the early stages of experimentation with the objective of finding the factors with large main effects, and with the intention of investigating their interactions later. Implicitly, such a strategy assumes that any factors which interact will also have large main effects and so will not be screened out in the initial experiment. This assumption may not, of course, be true. For example, in the soup experiment of Sect. 15.2.3, the largest effects by far were the BE interaction and the E main effect, but the main effect of B was extremely small. So, if only the main effects had been estimated in a screening experiment, factor B would not have been selected for follow up and the large BE interaction would not have been detected. Nevertheless, it does appear that in many experiments both factor main effects do tend to appear large when the corresponding two factors

interact. So, in an initial screening experiment with very few observations, experimenters will often be content to measure main effects only.

Until now, for two-level factors, and the number of observations being a power of 2, we have constructed orthogonal main effect plans as in Sect. 15.6.1, p. 516. For $n = 2^q$, we wrote down q columns where the c^{th} column has $n/2^{q-c}$ alternating sets of 2^{q-c} entries of -1s and $+1$s ($c = 1, \ldots, q$) as, for example, in Table 15.22, p. 518. The remaining columns were formed as products of corresponding coefficients in the first q columns taken in pairs, then in triples, and so on. We then labeled the columns in terms of main effect and interaction contrasts and determined the aliasing scheme from the relationship among the contrast coefficients. If all $n-1$ columns are used to measure main effects, the design is a saturated orthogonal main effect plan.

If n is not a power of 2, the above strategy cannot be used for obtaining an orthogonal main effect plan. In a paper in *Biometrika* in 1946, R.L. Plackett and J.P. Burman provided a different method of constructing saturated orthogonal main effect plans with n observations for $p = n-1$ two-level factors. This method can be used for many values of n which are multiples of 4 (including powers of 2). When n is a power of 2, these designs are likely to have similar aliasing properties to those discussed above. Otherwise, they are very different and, although all pairs of main-effect contrasts are orthogonal, we cannot write down a defining relation. Fractional factorial designs without defining relations are called *non-regular*, while designs that do have defining relations are called *regular*. In non-regular designs, main effect contrasts are correlated with interaction contrasts, but they may not be completely aliased with them.

Plackett and Burman's construction of orthogonal main effect plans uses a cyclic method. They listed the first row of each orthogonal array, called the *generator*. The entire orthogonal array is obtained from the generator by repeatedly cycling it to the right to obtain subsequent rows, and then appending a row of -1's. Such designs are known as *Plackett–Burman designs*. Some generators for cyclically generated orthogonal main-effect plans for $n = 8, 12, 16, 20$ and 24 are listed in Table 15.63, and further generators are given in Plackett and Burman's paper. Interestingly, for $n = 28$ (and some larger sizes), there is no generator that can construct an orthogonal main effect plan by cycling in this way, and Plackett and Burman used a different method of construction for these sizes.

As an example of a Plackett–Burman design, if we take the generator for $n = 12$ from Table 15.63, we have

$$1 \quad 1 \quad 1 \; -1 \quad 1 \quad 1 \; -1 \quad 1 \; -1 \; -1 \; -1,$$

and this gives us the first row of the orthogonal array. Cycling this to the right, and wrapping the end round to the beginning, we get the second row of the array as

$$-1 \quad 1 \quad 1 \quad 1 \; -1 \quad 1 \quad 1 \; -1 \quad 1 \; -1 \; -1 \, .$$

By repeatedly cycling row by row, we obtain 11 distinct rows. The 12th row of -1's is then added so that each factor appears $n/2$ times at its high level and $n/2$ times at its low level. The resulting array is shown in Table 15.34. The rows should be randomly ordered before the design is used.

We can check that all pairs of columns are orthogonal, so main effects can be estimated independently of each other. But the design is non-regular and has no defining relation. (Notice that we can also create the array in Table 15.34 by cycling to the left; rows 2–10 will just be in reverse order).

Some non-regular orthogonal main effect plans also exist for factors with more than 2 levels. For example, the orthogonal array $L_{18}(3^6 \times 6)$ of Table 15.64, at the end of the chapter, is a saturated orthogonal main effects plan for $n = 18$ observations for 6 three-level factors (whose main effects require 2 degrees of freedom each) and one six-level factor (whose main effect requires 5 degrees of

Table 15.34 A Plackett–Burman saturated orthogonal main-effect plan for 11 factors and 12 observations

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	−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	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	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	−1	−1	−1	−1

freedom). Also, the orthogonal array $L_{27}(3^{13})$ shown in Table 15.65 is a saturated orthogonal main effects plan with $n = 27$ observations for 13 three-level factors.

Example 15.8.1 `Lactic acid experiment`

B. Naveena, M. Altaf, K. Bhadriah, and G. Reddy, in their 2005 paper in *Bioresource Technology* used a Plackett–Burman design with $n = 16$ observations to screen the main effects of 15 factors. They wanted to find out which factors most affect the amount of "L(+) lactic acid" produced from wheat bran using microbial metabolism in solid state fermentation. They explained that L(+) lactic acid is used widely, including in foods, anti-inflammatory drugs, and synthesis of biodegradable products. The purpose of the experiment was to take a step towards the use of renewable cheap raw material

Table 15.35 Plackett–Burman design and data (g. lactic acid per 10g. wheat bran) for the lactic acid experiment

A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	*K*	*L*	*M*	*N*	*P*	*y*
1	1	1	1	−1	1	−1	1	1	−1	−1	1	−1	−1	−1	1.4659
−1	1	1	1	1	−1	1	−1	1	1	−1	−1	1	−1	−1	1.4284
−1	−1	1	1	1	1	−1	1	−1	1	1	−1	−1	1	−1	1.1367
−1	−1	−1	1	1	1	1	−1	1	−1	1	1	−1	−1	1	2.3041
1	−1	−1	−1	1	1	1	1	−1	1	−1	1	1	−1	−1	2.2765
−1	1	−1	−1	−1	1	1	1	1	−1	1	−1	1	1	−1	1.2114
−1	−1	1	−1	−1	−1	1	1	1	1	−1	1	−1	1	1	1.8786
1	−1	−1	1	−1	−1	−1	1	1	1	1	−1	1	−1	1	0.9304
1	1	−1	−1	1	−1	−1	−1	1	1	1	1	−1	1	−1	0.9730
−1	1	1	−1	−1	1	−1	−1	−1	1	1	1	1	−1	1	1.1137
1	−1	1	1	−1	−1	1	−1	−1	−1	1	1	1	1	−1	0.9961
−1	1	−1	1	1	−1	−1	1	−1	−1	−1	1	1	1	1	1.0268
1	−1	1	−1	1	1	−1	−1	1	−1	−1	−1	1	1	1	1.7966
1	1	−1	1	−1	1	1	−1	−1	1	−1	−1	−1	1	1	1.2928
1	1	1	−1	1	−1	1	1	−1	−1	1	−1	−1	−1	1	0.8937
−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	1.0449

Sources Naveena et al., *Bioresource Technology*, 2005, published by Elsevier

Table 15.36 Contrast estimates for the lactic acid experiment

A	B	C	D	E	F	G	H
−0.065	−0.370	−0.044	−0.076	0.238	0.428	0.349	−0.016
I	J	K	L	M	N	P	
0.276	0.036	−0.331	0.287	−0.026	−0.143	0.088	

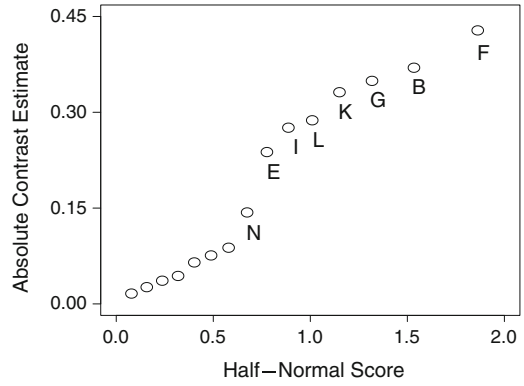


Fig. 15.9 Half-normal probability plot of normalized contrast absolute estimates for the lactic acid experiment

(here, wheat bran) for large scale production of L(+) lactic acid. The fifteen factors that were studied consisted of three physical factors (which we label A–C), a buffer (D), and eleven nutrients (E–N, P).

The Plackett–Burman design that was used can be obtained by cycling from the generator of Table 15.63. The design and the responses (grams of lactic acid per 10 grams of wheat bran) are shown in Table 15.35 before randomization. The 15 main effect contrast estimates (with divisor 1.0) and a corresponding half-normal probability plot are shown in Table 15.36 and Fig. 15.9.

We can see that with only $n = 16$ observations, it is possible to identify 7 (or possibly 8) of the 15 factors as likely to be the most influential in the lactic acid production. In order of size of contrast estimate, the 8 factors with the largest main effects are those labeled F, B, G, K, L, I, E, and N, which are all nutrients except for B which is a physical factor. These 8 factors can then be followed up in a later experiment, and their interactions examined. Since the main effect estimates for the five factors F, G, L, I, and E are all positive, the experimenters suggested that these should be examined at higher levels in the next experiment.

The estimate for the main effect of nutrient K is negative, and the experimenters commented that the wheat bran is already rich in this nutrient, so this nutrient may not need to be added in future experiments. The low level of factor N had already been set at zero, so a negative main effect estimate suggests that this nutrient, too, need not be added in future. The factor B main effect estimate is also negative and it was suggested that this be retained at its low level. Thus, main effects of only 5 factors and their interactions need to be followed up in future and this could be done in a full factorial experiment with 32 observations, or a resolution V half fraction with 16 observations. □

It is not necessary to assign all of the columns of an orthogonal main effect plan to factors. For example, the design of Table 15.35 could have been used to measure the main effects of factors A–L only and, if so, this would no longer be a saturated design—there would be 3 degrees of freedom for error. Similarly, seven of the 13 columns of the orthogonal main effect plan $L_{27}(3^{13})$ of Table 15.65 was used as a non-saturated orthogonal main effect plan for the seven 3-level design factors in the

inclinometer experiment of Sect. 15.7.1. The orthogonal array $L_{18}(3^7 \times 2)$ indicated in the right hand side of Table 15.64 is a non-saturated orthogonal main effect plan with two degrees of freedom for error.

15.8.2 Supersaturated Designs

A design is called *supersaturated* if the number of factorial effects to be estimated, plus the mean, exceeds the number of observations. In some sense, all the regular fractional factorial designs in the earlier sections are supersaturated if an insufficient number of interactions can be assumed to be negligible. For instance, in the sludge experiment of Example 15.2.1, p. 502, there were only $n = 8$ observations but, ideally, 5 main effects, 10 interactions and a mean were of interest. Similarly, in the refinery experiment of Example 15.3.1, p. 507, there were 4 main effects (requiring 2 degrees of freedom each), 6 two-factor interactions (requiring 4 degrees of freedom each) and a mean of interest – a total of 33 factorial effects, but only $n = 27$ observations could be taken. In both of these examples, and others like them, contrasts were either in the defining relation and could not be measured at all, or were measurable within a set of aliased contrasts. Aliased contrast estimators were completely correlated and non-aliased contrast estimators were independent.

However, the word *supersaturated* is not usually applied to fractions with alias schemes. Rather, the term is usually reserved for designs in which, although there are fewer observations than contrasts of interest plus the mean, *some information can be gained on all contrasts*. To achieve this, one must give up the idea of independent estimates and allow some or all contrast estimators to be correlated. In the extreme case of fewer observations than the number of factors ($n < p$), it is usually necessary to estimate main effects only and to postpone consideration of interactions among important factors to a later date.

Since the contrast estimates will be correlated, we would like the correlations to be as small as possible. The correlation between contrast estimators can be calculated using the information about the covariance of two contrast estimators from Sect. 6.7.2, and dividing by the square root of the product of their variances to obtain the formula for correlation. Following Sect. 6.7.2, p. 172, the estimators of the two contrasts $\Sigma c_i \tau_i$ and $\Sigma k_s \tau_s$, with one observation per treatment combination in the design, have correlation

$$\text{Corr}\left(\sum_{i=1}^{n} c_i Y_i \sum_{s=1}^{n} k_s Y_s\right) = \frac{\sum_{i=1}^{n}\sum_{s=1}^{n} c_i k_s}{\sqrt{\sum c_i^2 \sum k_s^2}}\,.$$

For two-level factors, each of the n contrast coefficients is -1 or $+1$, so

$$\text{Corr}\left(\sum_{i=1}^{n} c_i Y_i \sum_{s=1}^{n} k_s Y_s\right) = \frac{1}{n}\sum_{i=1}^{n}\sum_{s=1}^{n} c_i k_s\,. \tag{15.8.1}$$

In general, our recommendation is only to consider using a supersaturated design if the largest correlation between two columns is at most 1/3 and if there are likely to be very few large main effects, say at most $n/3$ (effect sparsity), (see Example 15.8.2 for problems that may be encountered in supersaturated designs when there are many large effects.)

Table 15.37 A supersaturated design with $n_{ssd} = n/2 = 6$ observations for measuring main effects of up to $n-2 = 10$ factors, obtained from a Plackett–Burman design with $n = 12$ rows

−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	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

One Method of Construction

There have been a few simple methods of construction proposed for supersaturated designs with $n < p$, and we describe one here that selects rows from a non-regular orthogonal main effect plan. This method was proposed by D. Lin in his article in *Technometrics*, 1993, and results in $n_{ssd} = n/2$ observations for investigating main effects of $p = n - 2$ two-level factors. The method uses an approach similar to that used in forming regular fractions from orthogonal arrays in Sect. 15.6. It starts with an orthogonal main effect plan with n rows and $n - 1$ columns containing an equal number of -1 and $+1$, with n being a multiple of 4. It then selects one column, called a *branching column*, and takes the $n_{ssd} = n/2$ rows which have $+1$ in the branching column. The $n_{ssd} = n/2$ rows which have -1 could be taken instead.

In passing, we note that, when a column of $+1$s (representing the mean) is appended to a saturated orthogonal main effect plan with n observations and $n - 1$ two-level factors, the resulting $n \times n$ array is often referred to as a *Hadamard matrix*. So, a supersaturated design constructed as above is often referred to as being constructed from a Hadamard matrix using a branching column.

All of the orthogonal main effect plans in Table 15.63 with number of rows n being a multiple of 4, but not a power of 2, can be used to construct supersaturated designs using a branching column in this way. When n is a power of 2, this technique may lead to complete aliasing between main effect contrasts. For example, in the design of the lactic acid experiment of Table 15.35, if we were select the last column (labeled P) as a branching column, and keep just those rows which have -1 in the branching column, then the main effect contrast of A would be identical to that of L, and the main effect contrast of C would be identical to that of D, and so on, giving a Resolution II design.

Complete aliasing between main effect contrasts does not occur using the cyclic generators of Table 15.63 when n is a multiple of 4 but not a power of 2. For example, suppose we generate the Plackett–Burman design in Table 15.34 using the generator for $n = 12$ from Table 15.63. If we select the last column as the branching column, and keep only the rows that have $+1$ in this column, we obtain the supersaturated design in Table 15.37 with $n_{ssd} = 6$ observations for measuring main effects of up to $n - 2 = 10$ factors. The treatment combinations in the supersaturated design can be identified from the rows of Table 15.37, with -1 representing the low level and $+1$ representing the high level of each factor. The rows should be randomly ordered before the design is used.

Using (15.8.1), we can verify that all pairs of contrast estimators have correlation 0.33 or -0.33. However, there is some hidden aliasing which is not easy to notice. If we take the contrasts for B, D and H and multiply the corresponding coefficients together, we obtain contrast C. There are several other hidden aliases of this type, too. Thus, this design should only be used in a setting where at most $n_{ssd}/3 = 2$ main effects are expected to be large. Otherwise, there will be problems with masking of effects. Example 15.8.2 shows that this design is successful in identifying two large main effects, but not three. Similar issues hold for all very small designs.

Analysis of a Supersaturated Design

In a supersaturated design, there are too many factors (and too few observations) to fit a model by least squares that contains all main effects so, typically, smaller models are investigated. But, even so, contrast estimates are not likely to be independent which means that half normal probability plots cannot be used here. Even when there are few large main effects, any analysis of supersaturated designs is tricky due to the hidden aliases. Various sophisticated methods of analysis have been researched, including certain "penalized regression" techniques which are outside the scope of this book. One simple method is to compare the fit of all possible regression models (Chap. 8) containing first one variable, then two variables, then three variables, and so on, up to $p = n - 1$ variables; a technique called *all-subsets regression*. There are still decisions to be made on how to select the best of these models, and most software packages will present options for these. Here, we will compare only the R^2 values for models (see (8.6.1) in Sect. 8.6.1), but alternative more sophisticated methods would be preferable. Research is still continuing today on the best methods of selecting influential factors (or variables) when the number of observations is so small. With so few observations, the selected model should be taken only as a possible selection of the most influential factors, and not as a model to explain the data. The potentially influential factors that are identified must be followed up in a future experiment; some of the identified factors may have only appeared to have had large effects due to the hidden aliasing.

Example 15.8.2 `Identifying influential factors`

In this example, we examine how the contrast correlations, that are an unavoidable part of a supersaturated design, might affect the identification of the factors that influence the response. Let us start with an example using the orthogonal main effects plan with $n = 12$ observations in Table 15.34 and label the columns to be the main effect contrasts of the ten factors $A, B, \ldots, J$. This same design is shown in columns 3–12 of Table 15.39, p. 537, together with two sets of responses listed as y_{BD} and y_{BDH} in the first two columns. The data y_{BD} in the first column were created by assuming that the contrast main effect values for B and D are 16 and 24, respectively, and all other main effect values are randomly selected from a $N(0, 2)$ distribution and random errors from a N(0,1) distribution. Using data y_{BD}, we calculate the contrast estimates by multiplying the contrast coefficient by the corresponding response and dividing by $v/2 = 6$, we obtain the estimates in Table 15.38. Since we used an orthogonal main effects plan (Plackett–Burman design), these are independent estimates. It can be seen that factors B and D clearly have the largest main effects and these would be selected as the only two influential factors. An all subsets regression will also identify these two factors as the most influential, and the regression model is

$$\hat{y} = 34.70 + 7.62x_2 + 12.62x_4\,,$$

where x_2 and x_4 are the coded levels ± 1 of factors B and D and, when multiplied by 2, the corresponding parameter estimates match the contrast estimates in Table 15.38. (The difference of a factor of 2 is due to the fact that the levels ± 1 are 2 apart and are treated as actual values rather than coded levels for the regression model).

Table 15.38 Main effect contrast estimates obtained from the Plackett–Burman design in Example 15.8.2

A	B	C	D	E	F	G	H	J	K
−1.84	15.24	1.37	25.24	−0.23	0.08	−1.30	−0.78	1.55	−0.05

Now, taking the column labeled K in Table 15.39 as the branching column and keeping only those rows corresponding to $+1$ in this column, we obtain the supersaturated design with $n_{ssd} = 6$ observations in Table 15.37.

An all-subsets regression would again select factors B and D. A regression model containing these two factors has R^2, so accounts for 97.7% of the variability in the data. The fitted model is

$$\hat{y} = 34.62 + 7.08x_2 + 11.97x_4 \,,$$

which is very similar to that obtained from the orthogonal main effects plan. However, with so few observations, the model should not be used for prediction; it should only be used as a guide to which factors are to be followed up in a future experiment. At the follow-up stage, a predictive model can be fitted, and the interaction between B and D can be examined. Also, notice that the number of large main effects in this example is, as recommended, not more than $n_{ssd}/3 = 2$.

If we try to use this design in a situation where there are, say, three large main effects, the contrast correlations will most likely prevent the correct factors from being selected. For example, the data set y_{BDH} in Table 15.39 was created to correspond to true values of the main effects of B, D and H equal to 16, 24, 14, respectively, and these influential factors will be selected when using the orthogonal main effects plan. But from the supersaturated design, only factor D can be detected, and an all-subsets regression will identify C as a second influential factor. This is due to the hidden aliasing in which the main effect contrast of C is defined by the main effect contrasts of B, D and H, as described above. □

Example 15.8.2 illustrates that supersaturated designs can be successful in detecting the important factors *provided that there is only a small number* of these (say, at most $n_{ssd}/3$) compared with the number of observations, n_{ssd}. Analysis of the data in Example 15.8.2 is illustrated in Sects. 15.9.3 and 15.10.3, for the SAS and R software respectively.

15.8.3 Saturated Orthogonal Main Effect Plans Plus Interactions

Saturated orthogonal main effect plans which are equivalent to regular fractions do not give scope for estimating interactions independently of main effects. One can draw interaction plots as we did in Chaps. 6 and 7, but since the interactions are aliased with main effects, one cannot separate out the information. However, non-regular saturated orthogonal main effect plans for n not a power of 2 do, in general, offer the ability to gain some information on a few 2-factor interactions.

For example, if we take the Plackett–Burman design of Table 15.34, p. 531, and add the AB and AC interaction contrasts, we obtain the set of contrasts in Table 15.39. Notice that neither interaction column is identical to any main effect column and all correlations of interaction columns with main effect columns are 0.333 or -0.333. After adding interaction contrasts to the plan, such designs are supersaturated and so the same cautions and methods of analysis discussed in the previous section apply. Much more has been written on the topic of estimating interactions for non-regular fractions. A summary can be found in the paper of Xu et al. (2009).

An alternative possibility of measuring not only added interactions but also quadratic trends in main effects is given by the definitive screening designs outlined in the next section.

Table 15.39 Main effect, AB, and AC interaction contrasts for the saturated orthogonal main-effect plan of Table 15.34, together with the responses for Example 15.8.2

y_{BD}	y_{BDH}	A	B	C	D	E	F	G	H	I	J	K	AB	AC
28.99	35.99	1	1	1	−1	1	1	−1	1	−1	−1	−1	1	1
57.32	50.33	−1	1	1	1	−1	1	1	−1	1	−1	−1	−1	−1
39.40	46.40	−1	−1	1	1	1	−1	1	1	−1	1	−1	1	−1
40.83	47.84	−1	−1	−1	1	1	1	−1	1	1	−1	1	1	1
13.35	6.35	1	−1	−1	−1	1	1	1	−1	1	1	−1	−1	−1
28.17	35.17	−1	1	−1	−1	−1	1	1	1	−1	1	1	−1	1
13.98	20.98	1	−1	1	−1	−1	−1	1	1	1	−1	1	−1	1
54.51	61.51	1	1	−1	1	−1	−1	−1	1	1	1	−1	1	−1
32.85	25.85	−1	1	1	−1	1	−1	−1	−1	1	1	1	−1	−1
39.78	32.78	1	−1	1	1	−1	1	−1	−1	−1	1	1	−1	1
52.08	45.08	1	1	−1	1	1	−1	1	−1	−1	−1	1	1	−1
15.15	8.15	−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	−1	1	1

Table 15.40 Design and data for the vaccine experiment

A_L	B_L	C_L	D_L	E_L	F_L	Response
0	1	−1	−1	−1	−1	3.6
0	−1	1	1	1	1	28.2
1	0	−1	1	1	−1	22.0
−1	0	1	−1	−1	1	4.2
−1	−1	0	1	−1	−1	2.7
1	1	0	−1	1	1	27.5
−1	1	1	0	1	−1	2.5
1	−1	−1	0	−1	1	31.9
1	−1	1	−1	0	−1	14.9
−1	1	−1	1	0	1	8.5
1	1	1	1	−1	0	30.7
−1	−1	−1	−1	1	0	2.9
0	0	0	0	0	0	14.4

Source Erler et al. (2013), *Biotechnology Letters*, © 2012 Springer Science + Business Media Dordrecht

15.8.4 Definitive Screening Designs

Definitive screening designs were introduced by Jones and Nachtsheim in the *Journal of Quality Technology* in 2011. These designs have three-level factors, and allow estimation of linear and quadratic main effect trends as well as linear×linear trends in the interactions.

Jones and Nachtsheim list definitive screening design plans for $p = 4, \ldots, 12$ factors (for example, the design for $p = 6$ is shown in Table 15.40). The rows need to be randomly ordered before the designs are used. The designs given for $p = 4$, 6, 8, and 10 allow the main effect linear trends to be estimated independently of all linear and quadratic main effect trends, and independently of the linear×linear interaction contrasts. For other cases, the linear trend contrasts are not quite orthogonal, but they can still be estimated independently of the quadratic main effects and the linear×linear interaction trend contrasts. The quadratic trends are not orthogonal to each other nor to the linear×linear trends, but they are not completely confounded (—in Table 15.40, for example, each linear×linear contrast is a function of the quadratic trends for all factors not involved in the interaction). Because of the correlations, all contrast estimates must be least squares estimates which are adjusted for other effects in the model.

Definitive screening designs are supersaturated in the sense that, for p factors, they use only $2p + 1$ observations to measure a total of $p(p + 3)/2$ trend contrasts plus a mean (e.g. if there are $p = 4$ factors, a definitive screening design uses 9 observations to measure 8 linear and quadratic main effect trend contrasts, 6 interaction linear×linear trend contrasts and a mean). Like supersaturated designs, definitive screening designs may be analyzed using all-subsets regression or a more sophisticated "penalized regression" technique. As in Chap. 7, if the selected model contains linear×linear interaction terms, the constituent linear main effect terms should be included too.

Example 15.8.3 `Vaccine experiment`

An experiment, described by Erler, de Mas, Ramsey, and Henderson in *Biotechnology Letters* (2013), was run to study the effect of $p = 6$ factors on a particular chemical reaction related to a candidate vaccine product. The experimenters were interested in the linear and quadratic main effects of the factors and the linear×linear interactions, so there were 28 contrasts of interest. They used the definitive screening design from the paper of Jones and Nachtsheim (2011), which is shown in Table 15.40, plus some extra repeats of the final treatment combination (which we are not using here).

The response is a measure of "extent of polymerization" and is shown in the last column of Table 15.40. The data were collected in a random order. An all-subsets regression run with all linear terms x_i (with values as in Table 15.40), quadratic terms x_i^2, and linear×linear terms $x_i x_j$, selects the linear trends in A, F, and D as being the most influential, with the possible addition of the linear$C$×linear$D$ trend. The regression model involving these four effects, together with the linear C main effect is

$$\hat{y} = 14.92 + 10.62x_1 + 1.16x_3 + 3.90x_4 + 5.46x_6 + 1.98x_3x_4\,,$$

and this accounts for 98% of the variability in the data. □

15.9 Using SAS Software

15.9.1 Fractional Factorials

The analysis of a fractional factorial experiment by computer is identical to that of a single-replicate factorial experiment (Sect. 7.6, p. 225) except that only one effect should be entered into the model from each line of the aliasing scheme (and none from the defining relation). If two aliased effects are entered into the model, the Type I sum of squares and degrees of freedom will be zero for the second effect entered, and the Type III sum of squares and degrees of freedom will be zero for both effects.

In Table 15.41 we show a straightforward program for analyzing the sludge experiment of Example 15.2.1. The cell-means model in terms of the treatment combinations `TC` is used. (Variables `A`–`E` are created for later use.) Using `PROC GLM`, the analysis of variance is generated in the usual way by the `MODEL` statement, while the contrast estimates are obtained by the `ESTIMATE` statements, using the contrast coefficients listed in Table 15.7 (p. 503). The output is shown in Fig. 15.10. The main effects are each aliased with 2-factor (and 3-factor) interactions (see p. 503). The 2-factor interactions AC and BC are aliased with BE and AE, respectively. Since there is only one observation on each of the observed treatment combinations, the cell-means model leaves no degrees of freedom for error—this is why the p-values and values of test statistics and standard errors are either missing or meaningless. The inclusion of the `DIVISOR=4` options in the `ESTIMATE` statements ensures that all the contrasts listed in Table 15.7 will be divided by $v/2 = 4$ and give the same estimates as those in Table 15.8 (p. 503).

Table 15.41 SAS program for the sludge experiment—cell-means model

```
DATA SLUDGE;
 INPUT  A  B  C  D  E  Y;
 TC = 10000*A + 1000*B + 100*C + 10*D + E;
 LINES;
 0 0 0 1 0  195
 0 0 1 1 1  496
 0 1 0 0 1  87
 0 1 1 0 0  1371
 1 0 0 0 1  102
 1 0 1 0 0  1001
 1 1 0 1 0  354
 1 1 1 1 1  775
;
PROC GLM;
 CLASS TC;
 MODEL Y = TC;
 ESTIMATE 'A' TC -1 -1 -1 -1  1  1  1  1 / DIVISOR = 4;
 ESTIMATE 'B' TC -1 -1  1  1 -1 -1  1  1 / DIVISOR = 4;
 ESTIMATE 'C' TC -1  1 -1  1 -1  1 -1  1 / DIVISOR = 4;
 ESTIMATE 'D' TC  1  1 -1 -1 -1 -1  1  1 / DIVISOR = 4;
 ESTIMATE 'E' TC -1  1  1 -1  1 -1 -1  1 / DIVISOR = 4;
 ESTIMATE 'AC' TC 1 -1  1 -1 -1  1 -1  1 / DIVISOR = 4;
 ESTIMATE 'BC' TC 1 -1 -1  1  1 -1 -1  1 / DIVISOR = 4;
```

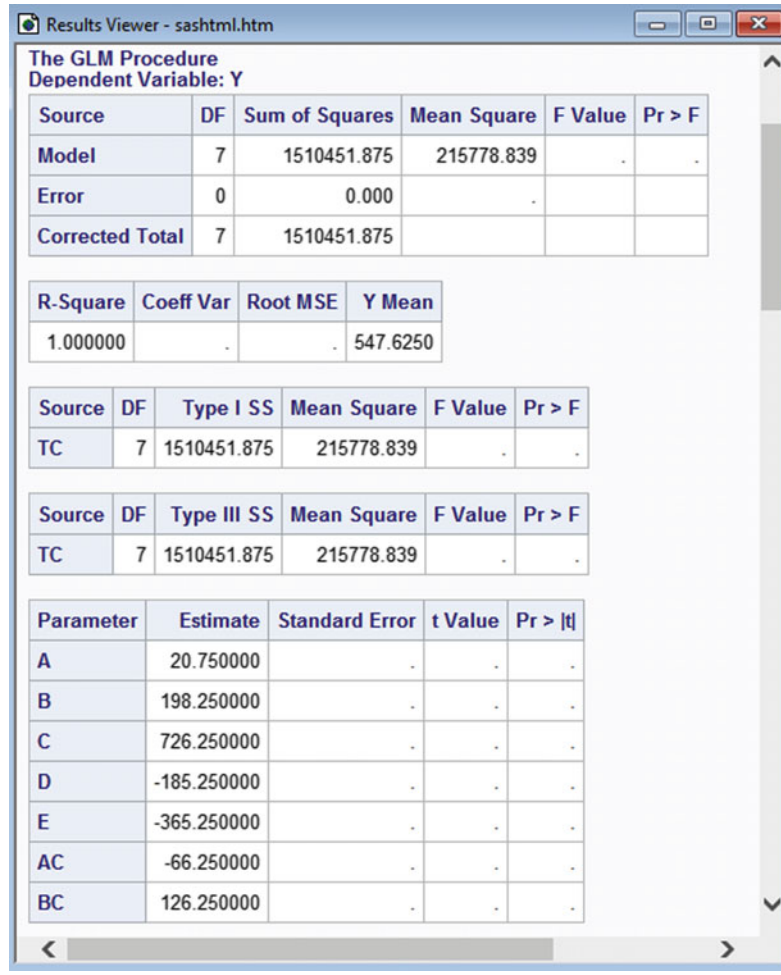
In Table 15.42, we show the SAS program for the equivalent model written in terms of main-effect and interaction parameters. The `ESTIMATE` statements for the main effects need no divisors, as they are automatically divided by 4 (the number of observations on each of the high and low levels). However, the `ESTIMATE` statements for the interaction contrasts include the option `DIVISOR=2`, to increase the actual divisor by a factor of 2. Without this option, the interaction estimates would be calculated with divisor 2 (the number of observations on each combination of levels of the two factors). The main-effect and interaction sums of squares are shown in Fig. 15.11. The output from the `ESTIMATE` statements is identical to that obtained from the cell-means model.

Again, there are no degrees of freedom for error, since a term has been included in the model from every row of the aliasing scheme. If all 2-factor interactions can be assumed to be negligible, then AC and BC would be omitted from the model, leaving 2 degrees of freedom for error.

Consider what would happen if two aliased terms were entered into the model. The defining relation for the $\frac{1}{4}$-fraction was stated in Example 15.2.1 to be $I = ABD = CDE = ABCE$. Consequently, A is aliased with BD. Adding BD into the model subsequent to A would give Type I sum of squares and degrees of freedom for BD equal to zero. This is because BD adds no more information if A is already in the model. The Type III sums of squares would be zero for both A and BD, since each would be added into the model assuming that the other is already in the model.

15.9.2 Design for the Control of Noise Variability

We now turn to the analysis of experiments involving design and noise factors, often known as Taguchi experiments. These were discussed in Sect. 15.7. There are two approaches to the analysis. The first approach involves the analysis of the mean and variance of the response observed for each design-treatment combination, calculated over the levels of the noise factors. The second approach involves


Results Viewer - sashtml.htm

The GLM Procedure
Dependent Variable: Y

Source	DF	Sum of Squares	Mean Square	F Value	Pr > F
Model	7	1510451.875	215778.839	.	.
Error	0	0.000	.		
Corrected Total	7	1510451.875			

R-Square	Coeff Var	Root MSE	Y Mean
1.000000	.	.	547.6250

Source	DF	Type I SS	Mean Square	F Value	Pr > F
TC	7	1510451.875	215778.839	.	.

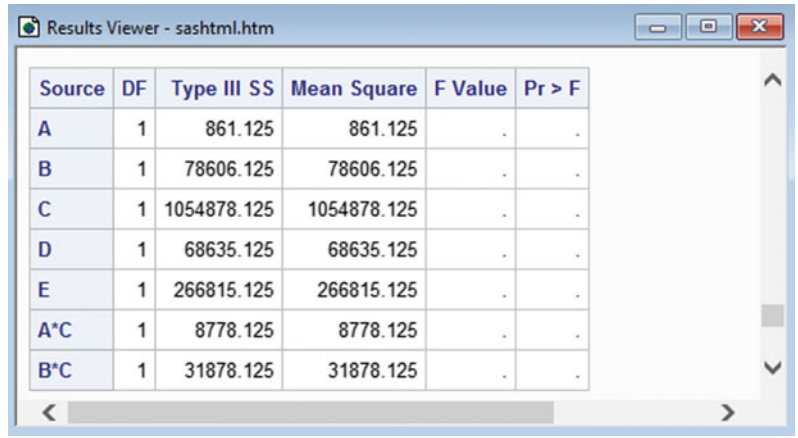
Source	DF	Type III SS	Mean Square	F Value	Pr > F
TC	7	1510451.875	215778.839	.	.

Parameter	Estimate	Standard Error	t Value	Pr > |t|
A	20.750000	.	.	.
B	198.250000	.	.	.
C	726.250000	.	.	.
D	-185.250000	.	.	.
E	-365.250000	.	.	.
AC	-66.250000	.	.	.
BC	126.250000	.	.	.

Fig. 15.10 Output from SAS program for the sludge experiment—cell-means model

Table 15.42 SAS program for the sludge experiment—five-way model

```
PROC GLM;
 CLASSES A B C D E;
 MODEL Y= A B C D E A*C B*C;
 ESTIMATE 'A' A -1 1;
 ESTIMATE 'B' B -1 1;
 ESTIMATE 'C' C -1 1;
 ESTIMATE 'D' D -1 1;
 ESTIMATE 'E' E -1 1;
 ESTIMATE 'AC' A*C 1 -1 -1 1 / DIVISOR=2;
 ESTIMATE 'BC' B*C 1 -1 -1 1 / DIVISOR=2;
```


Results Viewer - sashtml.htm

Source	DF	Type III SS	Mean Square	F Value	Pr > F
A	1	861.125	861.125	.	.
B	1	78606.125	78606.125	.	.
C	1	1054878.125	1054878.125	.	.
D	1	68635.125	68635.125	.	.
E	1	266815.125	266815.125	.	.
A*C	1	8778.125	8778.125	.	.
B*C	1	31878.125	31878.125	.	.

Fig. 15.11 Output from the SAS program for the sludge experiment—five-way model

the study of design-by-noise interactions. The first approach requires every noise combination to be observed with every design combination (that is, a product array), and the second approach requires randomization of all observed combinations of noise and design factors taken together.

Example 15.9.1 `Inclinometer experiment—product-array approach`

The inclinometer experiment was described in Sect. 15.7.1, and the data are shown in Table 15.29 (p. 526). Since this is a product array, the analysis will be done on the average and log variance of the responses over the levels of the noise factors, and these do not need to be identified in the SAS program input. Thus, the SAS program in Table 15.43 reads in the data corresponding to each combination of levels of the seven design factors (A–G) without identifying the levels of the noise factors. The average `AVY` and the log sample variance `LNVAR` of the observations for each design-treatment combination is computed and added to the data set. Since only 27 (i.e., 3^3) of the 3^7 design combinations are observed, we have a 3^{7-4} fractional factorial experiment with two possible response variables.

Two analyses are requested in Table 15.43. The first uses the response variable `LNVAR`. An analysis of variance table is requested via the `PROC GLM` statement for the model that includes main effects but no interactions. The least squares means for the levels of the design variables are requested via the `LSMEANS` statement, and these can be used to prepare plots such as those shown in Fig. 15.8 (p. 529). Linear and quadratic trends in each design factor can be tested via `ESTIMATE` or `CONTRAST` statements, only two of which are shown in the program. The final section of the program in Table 15.43 uses the response variable `AVY`. The output is similar to that shown in Tables 15.30, 15.31, 15.32 and 15.33, pp. 527–528. □

For a mixed array, in which observed combinations of noise and design factors taken together are randomized, the analysis is done in a similar way to that of Chap. 7. In the SAS program, for the mixed-array approach, it is necessary to input the levels of the noise factors as well as those of the design factors. The model should include the main effects of the design and noise factors and at least the design-by-noise interactions. The design-by-noise interactions help in identifying those design factors whose levels give the most stable response as the noise factor levels change. Interaction plots for the significant design-by-noise interactions are made in the same way as those described in Sect. 6.8.3. If the noise factor levels are placed on the horizontal axis as in Fig. 15.7, p. 525, the levels of the design factor(s) that are most robust to the noise level fluctuations are those whose average responses result in lines closest to horizontal across the noise factor levels (cf. level 3 of factor A in Fig. 15.7(b), p. 525).

Table 15.43 SAS program for the product array analysis of the inclinometer experiment

```
DATA INCLP;
  INPUT A B C D E F G  Y1 Y2 Y3 Y4 Y5 Y6 Y7 Y8;
  AVY = (Y1 + Y2 + Y3 + Y4 + Y5 + Y6 + Y7 + Y8)/8;
  VAR = ((Y1*Y1 + Y2*Y2 + Y3*Y3 + Y4*Y4 + Y5*Y5 + Y6*Y6
                + Y7*Y7 + Y8*Y8) - 8*AVY*AVY)/7;
LNVAR = LOG(VAR);
  LINES;
  0 0 0 0 0 0 0 0.62 3.54 3.56 0.62 3.09 0.71 0.73 3.20
  0 0 1 1 1 1 1 0.59 3.11 3.11 0.59 2.98 0.63 0.64 3.02
  : : : : : : :  :    :    :    :    :    :    :    :
  2 2 2 0 2 2 1 1.84 9.35 9.19 1.79 9.06 1.85 1.89 9.28
;
* Analysis of the log sample variance;
PROC GLM;
  CLASS A B C D E F G;
  MODEL LNVAR = A B C D E F G;
  ESTIMATE 'Lin A'  A -1  0  1;
  ESTIMATE 'Quad A' A  1 -2  1;
  :       :  :  :  :  :  :
  ESTIMATE 'Quad F' F  1 -2  1;
  CONTRAST 'Lin A'  A -1  0  1;
  CONTRAST 'Quad A' A  1 -2  1;
  :       :  :  :  :  :  :
  CONTRAST 'Quad F' F  1 -2  1;
  LSMEANS A B C D E F G;
;
* Analysis of the sample mean;
PROC GLM;
  CLASS A B C D E F G;
  MODEL AVY = A B C D E F G;
  ESTIMATE 'Lin A'  A -1  0  1;
  ESTIMATE 'Quad A' A  1 -2  1;
  :       :  :  :  :  :  :
  ESTIMATE 'Quad F' F  1 -2  1;
  CONTRAST 'Lin A'  A -1  0  1;
  CONTRAST 'Quad A' A  1 -2  1;
  :       :  :  :  :  :  :
  CONTRAST 'Quad F' F  1 -2  1;
  LSMEANS A B C D E F G;
```

15.9.3 Analysis of Small Screening Designs

For a small design with n_d observations, one method of searching for the k most influential factors, for any given k ($1 \leq k \leq n_d - 1$), is to select the factors that would result in a linear regression model with largest value of R^2. This is a simple method of analysis and is not guaranteed to select the best set of k factors, especially when the estimates are highly correlated. Nevertheless, we saw in Example 15.8.2 that, for simple problems, the method can work reasonably well and solutions for different values of k can be compared. In the SAS software, this method is easily achieved by adding an option to the model statement as follows:

Table 15.44 Results of the RSQUARE option

Number in model	R-Square	Variables in model	Number in model	R-Square	Variables in model
1	0.6597	D	1	0.7111	C
1	0.3808	E	1	0.5778	D
1	0.3452	H	1	0.2694	E
2	0.9774	B D	2	0.9693	C D
2	0.8730	E I	2	0.8670	C G
2	0.8672	C H	2	0.7970	C H
3	0.9985	B D G	3	0.9957	A C G
3	0.9871	B D I	3	0.9957	C D G
3	0.9871	B D E	3	0.9957	A C D
3	0.9871	D E I	3	0.9957	A D G

```
PROC REG;
MODEL Y = A B C D E F G H I J / SELECTION = RSQUARE BEST = 6;
```

The SELECTION=RSQUARE options asks SAS to check R^2 for linear models containing k variables, for all $k = 1, 2, 3, \ldots, n_d - 1$. The BEST=6 options asks SAS to list the 6 sets of factors producing models with the 6 largest R^2.

For example, suppose we set BEST=4, for the supersaturated design obtained from Table 15.39 with rows corresponding to $+1$ in the branching column K, and use the data y_{BD}. Then, we would obtain the output in the left part of Table 15.44 (where we have deleted the fourth selection for the one- and two-factor models). From this table, we can see that the factors B and D, taken together, account for 97.7% of the variation in the data and little is gained from adding a third factor.

The right part of Table 15.44 shows the results of running the RSQUARE option using the data set y_{BDH} of Table 15.39. From this, one would most likely select factors C and D (incorrectly). Notice that the sets of $k = 3$ best factors indicate that selection of any three of A, C, D, G seems to be equivalent. This anomaly can be explained by the hidden aliasing which can be discovered by running PROC REG without any options, which says:

```
F =  -A + D - E
G =   A - C - D
H =  -B - C - D
I =  -B - D + E
J =  -A + B + C + D - E
```

and we see that the contrasts for A, C, D and G are linearly related. The correct selection of factors for these data is B, D and H. A linear model containing the main effects of three factors has an R^2 of only 0.971, which is still high but unlikely to be the set of three factors chosen. Thus, when there are many large effects as compared with the number of observations, their detection is difficult and, perhaps, impossible.

15.10 Using R Software

15.10.1 Fractional Factorials

The analysis of a fractional factorial experiment by computer is identical to that of a single-replicate factorial experiment (Sect. 7.7, p. 230) except that only one effect should be entered into the model from each line of the aliasing scheme (and none from the defining relation). If two aliased effects are

Table 15.45 R program and output for the sludge experiment—cell-means model

```
> sludge.data = read.table("data/sludge.txt", header = T) # Read A:E,y
> # Create variable TC within sludge.data:
> sludge.data = within(sludge.data, {TC = 10000*A+1000*B+100*C+10*D+E})
> # Create factor variables within sludge.data:
> sludge.data = within(sludge.data,
+ {fA = factor(A); fB = factor(B); fC = factor(C);
+  fD = factor(D);  fE = factor(E); fTC = factor(TC)})
> head(sludge.data, 3)

  A B C D E   y   TC  fTC fE fD fC fB fA
1 0 0 0 1 0 195   10   10  0  1  0  0  0
2 0 0 1 1 1 496  111  111  1  1  1  0  0
3 0 1 0 0 1  87 1001 1001  1  0  0  1  0

> # Analysis of variance: cell means model
> modelTC = lm(y ~ fTC, data = sludge.data)
> anova(modelTC)

Analysis of Variance Table
Response: y
          Df  Sum Sq Mean Sq F value Pr(>F)
fTC        7 1510452  215779
Residuals  0       0

> # Contrast estimates: cell means model
> lsmTC = lsmeans(modelTC, ~ fTC)
> contrast(lsmTC, list(A=c(-1,-1,-1,-1, 1, 1, 1, 1)/4,
+                      B=c(-1,-1, 1, 1,-1,-1, 1, 1)/4,
+                      C=c(-1, 1,-1, 1,-1, 1,-1, 1)/4,
+                      D=c( 1, 1,-1,-1,-1,-1, 1, 1)/4,
+                      E=c(-1, 1, 1,-1, 1,-1,-1, 1)/4,
+                      AC=c( 1,-1, 1,-1,-1, 1,-1, 1)/4,
+                      BC=c( 1,-1,-1, 1, 1,-1,-1, 1)/4))

contrast estimate  SE df t.ratio p.value
A           20.75 NaN  0     NaN     NaN
B          198.25 NaN  0     NaN     NaN
C          726.25 NaN  0     NaN     NaN
D         -185.25 NaN  0     NaN     NaN
E         -365.25 NaN  0     NaN     NaN
AC         -66.25 NaN  0     NaN     NaN
BC         126.25 NaN  0     NaN     NaN
```

entered into the model, the Type I sum of squares and degrees of freedom will be zero for the second effect entered, and the Type III sum of squares and degrees of freedom will be zero for both effects.

In Table 15.45 we show a straightforward program and output for analyzing the sludge experiment of Example 15.2.1. The cell-means model in terms of the factor variable `fTC` for the treatment combinations `TC` is used. (Factor variables `fA`–`fE` are created for later use.) The linear models function `lm` fits the model, and the `anova` function generates the analysis of variance table. Then the contrast estimates of interest are generated by a single `contrast` statement, using least squares means generated

Table 15.46 R program (continued) for the sludge experiment—five-way model

```
> # Analysis of variance: factorial effects model
> modelFE = lm(y ~ fA + fB + fC + fD + fE + fA:fC + fB:fC, data = sludge.data)
> anova(modelFE)

Analysis of Variance Table

Response: y
          Df  Sum Sq Mean Sq F value Pr(>F)
fA         1     861     861
fB         1   78606   78606
fC         1 1054878 1054878
fD         1   68635   68635
fE         1  266815  266815
fA:fC      1    8778    8778
fB:fC      1   31878   31878
Residuals  0       0

> # Contrast estimates: factorial effects model
> lsmA = lsmeans(modelFE, ~ fA); contrast(lsmA, list(A = c(-1, 1)))
> lsmB = lsmeans(modelFE, ~ fB); contrast(lsmB, list(B = c(-1, 1)))
> lsmC = lsmeans(modelFE, ~ fC); contrast(lsmC, list(C = c(-1, 1)))
> lsmD = lsmeans(modelFE, ~ fD); contrast(lsmD, list(D = c(-1, 1)))
> lsmE = lsmeans(modelFE, ~ fE); contrast(lsmE, list(E = c(-1, 1)))
> lsmAC = lsmeans(modelFE, ~ fA:fC); contrast(lsmAC, list(AC = c(1,-1,-1,1)/2))
> lsmBC = lsmeans(modelFE, ~ fB:fC); contrast(lsmBC, list(BC = c(1,-1,-1,1)/2))
```

by the `lsmeans` function and saved as `lsmTC`, and using the contrast coefficients listed in Table 15.7 (p. 503). Nicely formatted output is obtained by providing the contrasts as a list, including a name (i.e. `A`, `B`, etc.) for each.

The main effects are each aliased with 2-factor (and 3-factor) interactions (see p. 503). The 2-factor interactions *AC* and *BC* are aliased with *BE* and *AE*, respectively. Since there is only one observation on each of the observed treatment combinations, the cell-means model leaves no degrees of freedom for error—this is why the standard errors, test statistics and *p*-values generated by `contrast` are "not a number" (`NaN`). The inclusion of the divisor 4 each contrast ensures that all the contrasts listed in Table 15.7 will be divided by $v/2 = 4$ and give the same estimates as those in Table 15.8 (p. 503).

In Table 15.46, we show a continuation of the `R` program of Table 15.45, illustrating the analysis using a factorial effects model, providing selected output. Main effect and interaction contrast estimates are computed using the `contrast` statement of the least squares means function `lsmeans`. All of the contrasts as specified use coefficients $c_{ijk} = \pm 1/4$. For the main effects, the specified coefficients ± 1 are automatically divided by 4, averaging over the four combinations of the other two factors. For the two-factor interaction contrasts, the coefficients $\pm 1/2$ are specified and are automatically divided by 2, averaging over the two observations on each combination of these two factors. The main-effect and interaction sums of squares are shown in Table 15.46. The contrast estimates (not shown) are identical to those obtained from the cell-means model.

Again, there are no degrees of freedom for error, since a term has been included in the model from every row of the aliasing scheme. If all 2-factor interactions can be assumed to be negligible, then *AC* and *BC* would be omitted from the model, leaving 2 degrees of freedom for error.

Consider what would happen if two aliased terms were entered into the model. The defining relation for the $\frac{1}{4}$-fraction was stated in Example 15.2.1 to be $I = ABD = CDE = ABCE$. Consequently, A is aliased with BD. Adding BD into the model subsequent to A would give Type I sum of squares and degrees of freedom for BD equal to zero. This is because BD adds no more information if A is already in the model. The Type III sums of squares would be zero for both A and BD, since each would be added into the model assuming that the other is already in the model.

15.10.2 Design for the Control of Noise Variability

We now turn to the analysis of experiments involving design and noise factors, often known as Taguchi experiments. These were discussed in Sect. 15.7. There are two approaches to the analysis. The first approach involves the analysis of the mean and variance of the response observed for each design-treatment combination, calculated over the levels of the noise factors. The second approach involves the study of design-by-noise interactions. The first approach requires every noise combination to be observed with every design combination (that is, a product array), and the second approach requires randomization of all observed combinations of noise and design factors taken together.

Example 15.10.1 `Inclinometer experiment—product-array approach`

The inclinometer experiment was described in Sect. 15.7.1, and the data are shown in Table 15.29 (p. 526). Since this is a product array, the analysis will be done on the average and log variance of the responses over the levels of the noise factors, and these do not need to be identified in the `R` program input. Thus, the `R` program in Table 15.47 reads in the data corresponding to each combination of levels of the seven design factors (A–G) without identifying the levels of the noise factors. The average `Avy` and the log sample variance `LnVar` of the observations for each design-treatment combination is computed and added to the data set. Since only 27 (i.e., 3^3) of the 3^7 design combinations are observed, we have a 3^{7-4} fractional factorial experiment with two possible response variables.

Two analyses are requested in Table 15.47. The first uses the response variable `LnVar`. After fitting the linear model that includes main effects but no interactions via the `lm` function, an analysis of variance table is requested via the `anova` statement. The least squares means for the levels of each design factor are requested by the `lsmeans` function, and these can be used to prepare plots such as those shown in Fig. 15.8 (p. 529). Also, the `contrast` function uses the saved least squares means to estimate and test the linear and quadratic trends for each design factor. This use of `lsmeans` is only shown in the program for design factor A but can be used to obtain least squares means for the remaining design factors and the trend contrast estimates for B–F, (since they each have equally-spaced levels). A regression model is also fit, including linear and quadratic terms for each design factor, to generate the sum of squares associated with the linear and quadratic contrasts. This approach also provides the correct linear and quadratic trend contrast sums of squares for G, even though the levels of G are not equally spaced. The final section of the program in Table 15.47 uses the response variable `Avy`. The output is similar to that shown in Tables 15.30, 15.31, 15.32 and 15.33, pp. 527–528. □

For a mixed array, in which observed combinations of noise and design factors taken together are randomized, the analysis is done in a similar way to that of Chap. 7. The model should include the main effects of the design and noise factors and at least the design-by-noise interactions. The design-by-noise interactions help in identifying those design factors whose levels give the most stable response as the noise factor levels change. Interaction plots for the significant design-by-noise interactions are made in the same way as those described in Sect. 6.9.3. If the noise factor levels are placed on the horizontal axis as in Fig. 15.7, the levels of the design factor(s) that are most robust to the noise level fluctuations are those whose average responses result in lines closest to horizontal across the noise factor levels (cf. level 3 of factor A in Fig. 15.7(b), p. 525).

Table 15.47 R program for the product array analysis of the inclinometer experiment

```
# Read data from file. Header: A B C D E F G y1 y2 y3 y4 y5 y6 y7 y8
ipa.data = read.table("data/inclinometer.product.txt", header = T)
# Create factor variables
ipa.data = within(ipa.data,
{fA = factor(A); fB = factor(B); fC = factor(C); fD = factor(D);
fE = factor(E); fF = factor(F); fG = factor(G)})
# Compute Avy and LnVar of data at each design treatment combo
ipa.data = within(ipa.data, {Avy = (y1 + y2 + y3 + y4 + y5 + y6 + y7 + y8)/8
    LnVar = log(((y1*y1 + y2*y2 + y3*y3 + y4*y4 + y5*y5 + y6*y6
                  + y7*y7 + y8*y8) - 8*Avy^2)/7)})
# Remove variables y1:y8
ipa.data = within(ipa.data, {remove(y1, y2, y3, y4, y5, y6, y7, y8)})
head(ipa.data,3)

# Analysis of log sample variance
model1 = lm(LnVar ~ fA + fB + fC + fD + fE + fF + fG, data = ipa.data)
anova(model1)
# Least square means and contrast estimates for A (similarly for B--F)
library(lsmeans)
lsmA1 = lsmeans(model1, ~ fA)
lsmA1
contrast(lsmA1, list(Alin = c(-1, 0, 1), Aquad = c(1, -2, 1)))
# Regression for log sample variance (to get contrast SS's)
model2 = lm(LnVar ~ A + I(A^2) + B + I(B^2) + C + I(C^2) + D + I(D^2)
          + E + I(E^2) + F + I(F^2) + G + I(G^2), data = ipa.data)
anova(model2)

# Analysis of sample mean
model3 = lm(Avy ~ fA + fB + fC + fD + fE + fF + fG, data = ipa.data)
anova(model3)
# Least square means and contrast estimates for A (similarly for B--F)
# library(lsmeans)
lsmA3 = lsmeans(model3, ~ fA)
lsmA3
contrast(lsmA3, list(Alin = c(-1, 0, 1), Aquad = c(1, -2, 1)))
# Regression for sample mean (to get contrast SS's, including for G)
model4 = lm(Avy ~ A + I(A^2) + B + I(B^2) + C + I(C^2) + D + I(D^2)
         + E + I(E^2) + F + I(F^2) + G + I(G^2), data = ipa.data)
anova(model4)
```

15.10.3 Analysis of Small Screening Designs

For a small design with n_d observations, one method of searching for the k most influential factors, for any given k ($1 \leq k \leq n_d - 1$), is to select the factors that would result in a linear regression model with largest value of R^2. This is a simple method of analysis and is not guaranteed to select the best set of k factors, especially when the estimates are highly correlated. Nevertheless, we saw in Example 15.8.2 that, for simple problems, the method can work reasonably well and solutions for different values of k can be compared. In the R software, the function `regsubsets` from the `leaps` package fits regression models containing all subsets of p of the factors up to `nvmax` and prints out the `nbest` models found for each value of p. Table 15.48 shows the output obtained from the R

Table 15.48 Results of the all subsets variable selection via `regsubsets`

```
> ssd <- data.frame(A = c(-1, -1,  1, -1,  1,  1),
+                   B = c(-1,  1, -1,  1, -1,  1),
+                   C = c(-1, -1,  1,  1,  1, -1),
+                   D = c( 1, -1, -1, -1,  1,  1),
+                   E = c( 1, -1, -1,  1, -1,  1),
+                   F = c( 1,  1, -1, -1,  1, -1),
+                   G = c(-1,  1,  1, -1, -1,  1),
+                   H = c( 1,  1,  1, -1, -1, -1),
+                   I = c( 1, -1,  1,  1, -1, -1),
+                   J = c(-1,  1, -1,  1,  1, -1),
+ y = c(40.83, 28.17, 13.98, 32.85, 39.78, 52.08))

> library(leaps)
> regsubsets.outssd <- regsubsets(y ~ A + B + C + D + E +
+   F + G + H + I + J, data = ssd, nbest = 3,
+   nvmax = 3, method = "exhaustive")
Warning message:
In leaps.setup(x, y, wt = wt, nbest = nbest, nvmax = nvmax,
force.in = force.in,  :
  5  linear dependencies found

> summary.outssd <- summary(regsubsets.outssd)
> as.data.frame(summary.outssd$outmat)
        A B C D E F G H I J
1  ( 1 )       *
1  ( 2 )         *
1  ( 3 )               *
2  ( 1 )   *   *
2  ( 2 )         *       *
2  ( 3 )     *         *
3  ( 1 )   *   *     *
3  ( 2 )   *     *       *
3  ( 3 )       * *       *

> round(summary.outssd$rsq, 4)
[1] 0.6597 0.3808 0.3452 0.9774 0.8730 0.8672 0.9985
[8] 0.9871 0.9871

> alias(aov(y ~ A + B + C + D + E + F + G + H + I + J, ssd))
Model :
y ~ A + B + C + D + E + F + G + H + I + J
Complete :
  (Intercept)  A  B  C  D  E
F  0          -1  0  0  1 -1
G  0           1  0 -1 -1  0
H  0           0 -1 -1 -1  0
I  0           0 -1  0 -1  1
J  0          -1  1  1  1 -1
```

software `regsubsets` command, for the supersaturated design obtained from Table 15.39 with rows corresponding to +1 in the branching column K, and the data y_{BD}.

The `regsubsets` call fits all possible models containing up to `nvmax = 3` factors and displays the `nbest = 3` of each size. The statement

```
as.data.frame(summary.outssd$outmat)
```

produces a list of these best models with stars indicating which factors have been fitted in the model. The R^2 values for the listed models are obtained from the `round(summary.outssd$rsq, 4)` command, where the number 4 specifies the number of decimal places. The fourth entry in the list of R^2 values corresponds to the model containing B and D; we see that the $R^2 = 0.9774$, and little is gained by including other factors.

Notice the warning message in the output that says there were 5 linear dependencies found. These are the hidden aliases that can be identified by running

```
alias(aov(y   A + B + C + D + E + F + G + H + I + J, ssd))
```

and observing that

```
F =  -A + D - E
G =   A - C - D
H =  -B - C - D
I =  -B - D + E
J =  -A + B + C + D - E
```

If we try to use this same design for the data set y_{BDH} from Table 15.39, we obtain

```
         A B C D E F G H I J
1  ( 1 )    *
1  ( 2 )      *
1  ( 3 )        *
2  ( 1 )    * *
2  ( 2 )    *       *
2  ( 3 )    *         *
3  ( 1 )    * *     *
3  ( 2 ) *    *     *
3  ( 3 ) *  *       *
```

with associated R^2 values of `0.7111 0.5778 0.2694 0.9693 0.8670 0.7970 0.9957 0.9957 0.9957`. From this, one would most likely select factors C and D (incorrectly). Notice that the sets of $k = 4$ best factors indicate that selection of any three of A, C, D, G seems to be equivalent. This anomaly can be explained by the hidden aliasing which can be discovered with the `alias` command as shown above and we see that the contrasts for A, C, D and G are linearly related. The correct selection of factors for these data is B, D and H. A linear model containing the main effects of three factors has an R^2 of only 0.971, which is still high but unlikely to be the set of three factors chosen. Thus, when there are many large effects as compared with the number of observations, their detection is difficult and, perhaps, impossible.

Exercises

1. **Decontamination experiment—beta particles, continued**

   Suppose that only the first block of the data (beta particles) had been obtained in the decontamination experiment described in Exercise 8 of Chap. 13 (p. 466). The design would then have been a $\frac{1}{2}$-fraction of a 2^4 experiment with defining relation $I = ABCD$. The half fraction is shown in Table 15.49. Analyze the data and compare your conclusions with those of the full experiment in

Table 15.49 Block I of the decontamination experiment

Treatment combinations (Response)							
1010	1111	0110	0000	1100	0101	0011	1001
(716)	(686)	(498)	(1437)	(527)	(579)	(1433)	(906)

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

Table 15.50 Yields (in pounds) of mangold roots for Block III of the mangold experiment

Treatment combinations (Yield)							
00101	11001	01011	01110	10010	11100	00000	10111
(896)	(1284)	(996)	(860)	(1184)	(984)	(740)	(1468)

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

Exercise 8 of Chap. 13. Explain the circumstances under which a half fraction would be preferred to a single-replicate factorial experiment.

2. **Mangold experiment, continued**

The mangold experiment in Sect. 13.5, p. 447, was a single replicate confounded design for a 2^5 experiment in $b = 4$ blocks of size 8. 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). All of the 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.
Suppose that the data from only the third block had been available, so that we have a $\frac{1}{4}$-fraction. The data are reproduced in Table 15.50.

(a) Write down the aliasing scheme for this fractional factorial experiment.
(b) Analyze the data. What conclusions can you draw?
(c) Comparing your conclusions with those of Sect. 13.5, what extra information do you gain by running the single-replicate design instead of the fraction?
(d) When would you recommend that an experimenter consider using a fractional factorial design rather than a single-replicate design?

3. **Dye experiment, continued**

The dye experiment was discussed in Sect. 14.2.4 (p. 478). There were three factors: the concentration of inorganic material M in the free water in the reaction mixture (factor A at three equally spaced levels), the volume of free water in the reaction mixture (factor B at three equally spaced levels), and the concentration of inorganic material N in the free water in the reaction mixture (factor C at three equally spaced levels). The data for the first replicate of the original experiment were given in Table 14.6 (p. 479) and the first block is reproduced in Table 15.51. The design for

Table 15.51 Volume of dyestuff for Block I of the dye experiment

Treatment combinations (Yield)								
000	021	012	110	101	122	220	211	202
(74)	(130)	(56)	(110)	(166)	(227)	(195)	(146)	(90)

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

Table 15.52 Yields of sugar beet for Block III of the sugar-beet experiment

Treatment combinations (Yield)								
202	020	210	111	001	122	221	012	100
(2198)	(2093)	(2354)	(2268)	(1926)	(2152)	(2349)	(2025)	(2106)

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

the first replicate was a single-replicate design that confounded (AB^2C^2; A^2BC). Analyze the data of Block I as though it had come from a $\frac{1}{3}$-fraction. State your conclusions.

4. **Sugar beet experiment, continued**

The sugar beet experiment described in Exercise 6 of Chap. 14 concerned the effects of three standard fertilizers, nitrogen, phosphate, and potassium (factors N, P, and K), each at three equally spaced levels, on sugar beet yield. The experiment was run as a single-replicate confounding the contrasts (NP^2K; N^2PK^2). Suppose the only data available were those of Block III, reproduced in Table 15.52.

(a) If the only data available were those from Block III, write out the aliasing scheme for the design.
(b) Analyze the data from Block III as though they came from a $\frac{1}{3}$-fraction. State your conclusions.

5. **Flour experiment, continued**

Suppose that the data from Block II of the 4×2^4 experiment in Table 15.16 (p. 514) had been lost, so that only Block I remained. This would then constitute a $\frac{1}{4}$-fraction.

(a) Write out the aliasing scheme for the design. What is the resolution number. Is this a good design?
(b) Bearing in mind the purpose of the experiment, can you find a better $\frac{1}{4}$-fraction? If so, write out the design and its aliasing scheme.
(c) Analyze the data from Block I of Table 15.16. What can you conclude?

6. **Handwheel experiment**

E.N. Corlett and G. Gregory describe an experiment in the 1960 issue of *Applied Statistics* that was concerned with finding the design of a machine tool handwheel that would maximize the accuracy on the part of the operator in the setting of the machine tool handwheel. The apparatus consisted of an optical dividing head with a dial mounted onto a mandrel to which was connected the handwheel spindle. The spindle was provided with an adjustable friction brake. The operator first offset the dial by 15° and then moved the handwheel so that a line on the dial was brought "into coincidence with a fixed line on the dividing head, making the final adjustment by means of a series of taps by hand on the handwheel rim."

Seven factors, each at two levels (coded 0 and 1) were investigated as follows.

A: Handwheel diameter (5.5 in., 10 in.)
B: Dial diameter (4 in., 8 in.)
C: Thickness of the dial line (0.008 in., 0.064 in.)
D: Friction of the spindle (7.5 lb.-in., 45 lb.-in.)

Table 15.53 Log variance of observations for the handwheel experiment

Block I		Block II	
ABCDEFG	$\ln(s^2)$	*ABCDEFG*	$\ln(s^2)$
0 0 0 0 0 0 0	0.7044	1 1 0 0 0 0 0	0.0561
1 0 1 0 0 0 0	0.5907	0 1 1 0 0 0 0	0.3615
0 0 1 0 0 0 1	−0.0297	1 1 1 0 0 0 1	−0.1158
1 0 0 0 0 0 1	0.3914	0 1 0 0 0 0 1	−0.1952
0 1 0 1 0 0 0	0.0792	1 0 0 1 0 0 0	0.4585
1 1 1 1 0 0 0	0.3228	0 0 1 1 0 0 0	0.2531
0 1 1 1 0 0 1	−0.1599	1 0 1 1 0 0 1	0.2727
1 1 0 1 0 0 1	−0.0996	0 0 0 1 0 0 1	0.6861
0 0 0 1 1 0 0	0.5878	1 1 0 1 1 0 0	−0.0074
1 0 1 1 1 0 0	0.3577	0 1 1 1 1 0 0	−0.2328
0 0 1 1 1 0 1	0.1847	1 1 1 1 1 0 1	−0.1046
1 0 0 1 1 0 1	0.5706	0 1 0 1 1 0 1	−0.2069
0 1 0 0 1 0 0	−0.1805	1 0 0 0 1 0 0	0.8051
1 1 1 0 1 0 0	−0.3224	0 0 1 0 1 0 0	0.4634
0 1 1 0 1 0 1	−0.1433	1 0 1 0 1 0 1	0.2904
1 1 0 0 1 0 1	0.1354	0 0 0 0 1 0 1	0.4692
Block III		Block IV	
ABCDEFG	$\ln(s^2)$	*ABCDEFG*	$\ln(s^2)$
0 1 0 0 0 1 0	−0.6760	1 0 0 0 0 1 0	0.5457
1 1 1 0 0 1 0	−0.3824	0 0 1 0 0 1 0	0.0846
0 1 1 0 0 1 1	−0.2996	1 0 1 0 0 1 1	0.4453
1 1 0 0 0 1 1	−0.4539	0 0 0 0 0 1 1	0.2361
0 0 0 1 0 1 0	0.2970	1 1 0 1 0 1 0	−0.5069
1 0 1 1 0 1 0	0.1646	0 1 1 1 0 1 0	−0.3299
0 0 1 1 0 1 1	0.3878	1 1 1 1 0 1 1	−0.3245
1 0 0 1 0 1 1	0.2168	0 1 0 1 0 1 1	−0.3233
0 1 0 1 1 1 0	0.0148	1 0 0 1 1 1 0	0.4199
1 1 1 1 1 1 0	−0.4898	0 0 1 1 1 1 0	0.2957
0 1 1 1 1 1 1	0.1308	1 0 1 1 1 1 1	0.2278
1 1 0 1 1 1 1	−0.1829	0 0 0 1 1 1 1	0.4269
0 0 0 0 1 1 0	0.0182	1 1 0 0 1 1 0	−0.4798
1 0 1 0 1 1 0	0.2070	0 1 1 0 1 1 0	−0.0669
0 0 1 0 1 1 1	0.1101	1 1 1 0 1 1 1	−0.0584
1 0 0 0 1 1 1	0.2642	0 1 0 0 1 1 1	−0.6856

Source Corlett and Gregory (1960). Copyright © 1960 Blackwell Publishers. Reprinted with permission

E: Level of operator's elbow relative to height of handwheel
(Level with center of spindle, 6 in. above spindle center)
F: Previous experience of operator (Practiced, Nonpracticed)
G: Knowledge of accuracy of previous setting (Feedback, No feedback)

The response variable was $\ln(s^2)$, where s^2 was the sample variance of 25 repeated observations for a particular treatment combination. It was estimated that each set of 25 repeated observations would take about 15 min to complete, including setup time. In a morning or afternoon session of four hours, therefore, sixteen observations could be taken. The experiment was to last over two

days, which meant that a 2^{7-1} fractional factorial experiment was required, divided into 4 blocks of 16.

The highest-order interaction was selected for the defining relation of the fraction, that is, $I = ABCDEFG$. Only two operators were used for the experiment, one for each level of practice. The difference between these operators was not of interest, only the interaction of the level of practice with the other factors. Rather unusually, then, the main effect of F was selected as one of the contrasts for confounding. All the 2-factor interactions and most of the 3-factor interactions were thought to be of interest. Unlikely 3-factor interactions included ACG and BDE, which were also chosen for confounding with blocks. The complete set of confounded contrasts was $F, ACG, ACFG$ together with its set of aliases $ABCDEG$, $BDEF$, BDE. All other main-effect, 2-factor, and 3-factor interaction contrasts could be estimated.

The data obtained from the experiment are shown in Table 15.53.

(a) Write out the aliasing scheme for the design.
(b) Using a computer package, estimate the (estimable) main-effect and interaction contrasts.
(c) Prepare a half-normal probability plot of the contrast estimates and identify the most important main effects and interactions.
(d) The authors of the article point out that if the responses are normally distributed and n is large (where n is the number of repeated observations, 25 in this experiment), then the response variable $\ln(s^2)$ has approximately constant variance equal to $2/(n-1)$. Calculate the standard error for each of the contrasts estimated in part (c). Using Bonferroni's method with an individual significance level of 0.001 for each test (giving an overall level of at most 0.06), which main effects and interactions are significantly different from zero? Do these results agree with the results from part (c)? Discuss why or why not.
(e) Draw interaction plots of the important interactions and discuss recommended settings for the six factors A, B, C, D, E, and G for the practiced and nonpracticed operators individually.
(f) Would you recommend further experimentation? If so, which factors and which settings would you recommend? Can you suggest a suitable design?

7. **Paint experiment**

(a) Suppose that you need to design an experiment involving 6 factors (A, B, C, D, E, F) at 2 levels each (64 treatment combinations) and that only 8 observations can be taken. You decide to sacrifice information on the ABF, $ACDF$, and $ABCE$ contrasts. Write out the defining relation and the two rows of the aliasing scheme showing the aliasing of A and the aliasing of AC.
(b) Explain what aliasing means.
(c) An experiment was run in Germany by S. Eibl, U. Kess, and F. Pukelsheim (*Journal of Quality Technology*, 1992) on the thickness of a paint coating. Prior to the experiment, the thickness achieved was around 2 mm, much higher than the target 0.8 mm. They selected the following six factors, each at two levels:

A: belt speed	B: tube width	C: pump pressure
D: paint viscosity	E: tube height	F: heating temperature

They used the $\frac{1}{8}$-fraction with the aliasing scheme in part (a), and they decided to ignore all interactions for this first experiment. Since they wanted to monitor the variation of the thickness, they took four observations on each of the 8 treatment combinations in the fraction.

Table 15.54 2^{10-6} resolution III fraction and pore diameter (nm) for the anatase experiment of Exercise 8

A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	Pore diameter
0	0	0	0	1	0	0	1	0	0	6.4
1	0	0	0	0	0	1	1	1	1	5.7
0	1	0	0	0	1	0	1	1	1	6.1
1	1	0	0	1	1	1	1	0	0	7.6
0	0	1	0	0	1	1	0	0	1	3.5
1	0	1	0	1	1	0	0	1	0	3.5
0	1	1	0	1	0	1	0	1	0	3.7
1	1	1	0	0	0	0	0	0	1	6.5
0	0	0	1	0	1	1	0	1	0	10.1
1	0	0	1	1	1	0	0	0	1	3.6
0	1	0	1	1	0	1	0	0	1	15.6
1	1	0	1	0	0	0	0	1	0	12.3
0	0	1	1	1	0	0	1	1	1	12.1
1	0	1	1	0	0	1	1	0	0	17.3
0	1	1	1	0	1	0	1	0	0	14.9
1	1	1	1	1	1	1	1	1	1	15.3

Source Olsen et al. (2014), *Journal of Porous Materials*, 21, © 2014, Springer Science + Business Media New York. With permission of Springer

The data are shown in Table 16.1, p. 570, where the two levels of each factor are coded as -1 and 1 and shown for factors *A*–*F* in the columns labeled z_A–z_F.
Calculate the analysis of variance table and contrast estimates using response variable `LNVAR` (the log variance). What do you conclude?

(d) Assuming that the order of observations was completely randomized, calculate the analysis of variance table and also contrast estimates of interest, using the 32 observations separately (without combining them into an average). Remembering that the goal is to reduce the thickness, what conclusions would you draw from this particular experiment?

(e) The experimenters decided to run a followup experiment with at most 16 observations. You can use any of the original 6 factors and you can change the levels from their original settings. The ultimate goal is to achieve a coating of 0.8 mm. Suggest a followup experiment.

8. **Anatase experiment**

R.E. Olsen and coauthors described several experiments in the *Journal of Porous Materials* (2014). These concerned the study of anatase (a form of titanium dioxide) as a catalyst support. Specific catalyst support properties are required, such as certain surface area, pore volume, and pore diameter. Samples were prepared by mixing various chemicals with water in specified orders and speeds to produce a "slurry". For half the observations, the slurry was (i) dried, (ii) rinsed with distilled water and (iii) calcinated; this was called the DRC procedure. For the remaining observations, (ii) and (iii) were interchanged to give the DCR procedure.

In the first of their experiments, all factors had two levels, coded here as 0 or 1. These factors were *A* Mixing order, *B* speed of water addition (slow, fast), *C* amount of water (7 ml, 25 ml), *D* rinsing order (DRC, DCR), *E* drying time (3 h, 24 h), *F* drying temperature (25 °C, 100 °C), *G* calcination ramp rate (2 °C/min, 20 °C/min), *H* calcination temperature (400 °C, 700 °C), *I* calcination time (2 h, 20 h), *J* amount of an aluminium compound added (5, 22). A 2^{10-6} resolution III fraction was

run. Several responses were measured, including the pore diameters which are listed together with the fraction in Table 15.54. The observations were taken in a random order (not shown here).

(a) Part of the aliasing scheme for the design is:

$$E = ACD,\ F = BCD\ , G = ACD,\ H = CD,\ I = ABCD,\ J = ABC\ .$$

Write out the defining relation for the fraction (a total of 63 contrasts):

(b) Fifteen factorial effects can be included in the model. This can include the 10 main effects. Suggest five interaction contrasts to include (these cannot be aliased with main effects nor with each other.)

(c) Estimate the 15 contrasts in your model and plot a half normal probability plot.

(d) From part (c), which factors seem to affect the pore diameter the most. Suppose you were going to follow up these factors and their interactions in more detail in a later experiment. How would you design such an experiment if you could take 16 observations?

9. **Flour early experiment**

The flour experiment was introduced in Example 15.5.1, p. 513. In Table 15.55, we show part of the design for an early experiment (the first in a series of four experiments). Six ingredients, A, B, C, D, E, F, added to the flour were to be investigated in the experiment. In addition, there were three noise factors: Factor P (which was a combination of factors N and S in Example 15.5.1) had two levels ("high yeast with long proof time" or "low yeast with short proof time"), Factor Q, (as in Example 15.5.1, two levels "undermixing, little water, heavy pressure" or "overmixing, much water, little pressure"), and Factor R (two levels, underbake or overbake).
A crossed array was selected. The noise array was a $\frac{1}{2}$-fraction with defining relation $I = PQR$. Each of the four noise combinations was run on a single day, so that the experiment ran over four days. The design array was a $\frac{1}{4}$-fraction with defining relation $I = ABCD = BCEF = ADEF$, and this was run on each day. Thus the noise contrasts are confounded with days and cannot be analyzed. However, the object of the experiment was to examine the average yield (specific volume, ml/100 g) and the variance of the yield for the design factors across the noise factors.

(a) Calculate the average yield and the log variance of the yield for each design-treatment combination.

(b) Analyze the two sets of data separately. What recommendations would you make if the objective is to reduce the variability and increase the specific volume?

10. **Injection molding experiment**

S.R. Schmidt and R.G. Launsby in their book *Understanding Industrial Designed Experiments* describe an experiment on the effect of six factors on the shrinkage of a part produced by injection molding. The six factors were injection velocity (factor A), cooling time (factor B), barrel zone temperature (factor C), mold temperature (factor D), hold pressure (factor E), and back pressure (factor F). Each factor had two levels coded 0 and 1.

There were two responses of interest, the length and width of the part after shrinkage. The purpose of the experiment was to find settings of the six variables that would enable the parts to be "on target," that is, a post-shrinkage length of 14.5 units and width of 9.35 units.

Table 15.55 Specific volume for part of experiment 1 of the flour early experiment

Design combinations	Noise combinations			
	Day 1 (111)	Day 2 (101)	Day 3 (000)	Day 4 (011)
000000	519	446	337	415
000011	503	468	343	418
001101	567	471	355	424
001110	552	489	361	425
010101	534	466	356	431
010110	549	461	354	427
011000	560	480	345	437
011011	535	477	363	418
100100	558	483	376	418
100111	551	472	349	426
101001	576	487	358	434
101010	569	494	357	444
110001	562	474	358	404
110010	569	494	348	400
111100	568	478	367	463
111111	551	500	373	462

Source Tuck et al. (1993). Copyright © 1993 Blackwell Publishers. Reprinted with permission

The orthogonal array in Table 15.22, p. 518, was selected with columns columns 1–6 labeled A, B, D, C, E, F, and columns 5 and 6 multiplied by -1. One degree of freedom (corresponding to column 7) is available to measure σ^2 or one of the two-factor interactions. Five parts were measured at each treatment combination, and the lengths and widths are recorded in Table 15.56.

(a) Write down the defining relation for the $\frac{1}{8}$-fraction and the aliasing scheme. The investigators assumed that all the interactions were negligible. If they had not done so, which interactions could have been measured?
(b) For the length data, calculate the average response and the standard deviation of the response for each treatment combination.
(c) Can you recommend which factors should be investigated more thoroughly in order to find a setting that would give the required length and also factors that could be set to reduce the variability?
(d) Repeat parts (a) and (b) for the width data.
(e) Can you make any overall recommendation?
(f) Write down the assumptions on the model that would need to be true in order to interpret the analysis of variance. Are these assumptions likely to be valid for this experiment?

11. **Spectrometer experiment, continued**

Read the details of the spectrometer experiment in Exercise 10 of Chap. 7. You will need to have access to your solutions to that exercise to answer this question.

Suppose that you are consultant for a different company and that they wish to run a similar experiment, with the same five factors, but with a total of 64 observations. To keep things simple, you might recommend that factors A and C be examined at 2 levels each rather than 3 levels in your first experiment (even though you may suspect that some of the factors have quadratic trends).

Table 15.56 Lengths and widths of parts after shrinkage in the injection molding experiment

Treatment combinations						Length (Deviation from 14.5) $\times 10^4$					Width (Deviation from 9.35) $\times 10^4$				
A	*B*	*C*	*D*	*E*	*F*										
0	0	0	0	0	0	0	5	0	0	5	75	60	70	85	90
0	0	0	1	1	1	75	90	70	65	65	50	40	40	40	45
0	1	1	0	0	1	45	50	45	45	45	45	45	45	50	40
0	1	1	1	1	0	100	105	105	110	105	130	130	125	135	135
1	0	1	0	1	0	105	110	105	120	100	55	60	60	55	60
1	0	1	1	0	1	45	55	65	50	50	80	65	50	40	45
1	1	0	0	1	1	150	140	155	50	145	100	80	85	90	85
1	1	0	1	0	0	55	65	55	55	60	65	60	65	65	60

Source Schmidt and Launsby (1992). Copyright © 1992 Air Academy Press. Reprinted with permission

Table 15.57 Analysis of variance for the industrial experiment

Source of variation	Degrees of freedom	Sum of squares	Mean square	Ratio	*p*-value
A	1	262.205	262.205	54.57	0.0857
B	1	11.045	11.045	2.30	0.3712
C	1	981.245	981.245	204.21	0.0445
D	1	5.120	5.120	1.07	0.4899
E	1	1568.000	1568.000	326.33	0.0352
F	1	8.820	8.820	1.84	0.4048
Error	1	4.805	4.805		
Total	7	2841.240			

Thus, you have a 2^5 experiment. List 5 interactions that you are particularly interested in studying. You should use information from your answer to (a) and (b) of Exercise 10 of Chap. 7 in choosing the interactions. Design a factorial experiment in 4 blocks of size 8. State exactly how you chose your design. Write out at least three of the treatment combinations in two of the blocks and explain how you obtained them.

12. **Design of industrial experiment**

Suppose that you are asked to design an experiment for 6 treatment factors each having two levels. Only 64 observations can be taken in total, and these should be divided into 8 blocks of size 8. Suppose that you decide to confound the interaction contrasts *ABD*, *DEF*, and *ACDF*.

(a) Can all the other interaction contrasts be estimated?
(b) What does the statement "*ABD* is confounded" mean?
(c) How would you obtain the 8 blocks? Write out two blocks as an example.
(d) Suppose that the budget is cut before the experiment can take place, and only 8 observations can be taken in total. How would you decide which 8 observations to take? What can be estimated?
(e) Suppose that you were fairly sure that all interactions involving 4 factors or more were negligible and that neither *D* nor *F* interacts with any of the other factors. Suppose that the analysis of variance table obtained from the results of the experiment is as in Table 15.57. What would you investigate in a followup experiment? Give your reasons.

Table 15.58 Plackett–Burman design and data for Exercise 14

A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	y
−1	−1	−1	−1	1	−1	1	−1	1	1	1	1	−1	−1	1	−1	−1	1	1	134.2
1	−1	−1	−1	−1	1	−1	1	−1	1	1	1	1	−1	−1	1	−1	−1	1	250.0
1	1	−1	−1	−1	−1	1	−1	1	−1	1	1	1	1	−1	−1	1	−1	−1	59.3
−1	1	1	−1	−1	−1	−1	1	−1	1	−1	1	1	1	1	−1	−1	1	−1	155.0
−1	−1	1	1	−1	−1	−1	−1	1	−1	1	−1	1	1	1	1	−1	−1	1	293.4
1	−1	−1	1	1	−1	−1	−1	−1	1	−1	1	−1	1	1	1	1	−1	−1	174.4
−1	1	−1	−1	1	1	−1	−1	−1	−1	1	−1	1	−1	1	1	1	1	−1	182.3
−1	−1	1	−1	−1	1	1	−1	−1	−1	−1	1	−1	1	−1	1	1	1	1	211.9
1	−1	−1	1	−1	−1	1	1	−1	−1	−1	−1	1	−1	1	−1	1	1	1	13.6
1	1	−1	−1	1	−1	−1	1	1	−1	−1	−1	−1	1	−1	1	−1	1	1	95.5
1	1	1	−1	−1	1	−1	−1	1	1	−1	−1	−1	−1	1	−1	1	−1	1	158.0
1	1	1	1	−1	−1	1	−1	−1	1	1	−1	−1	−1	−1	1	−1	1	−1	366.4
−1	1	1	1	1	−1	−1	1	−1	−1	1	1	−1	−1	−1	−1	1	−1	1	164.8
1	−1	1	1	1	1	−1	−1	1	−1	−1	1	1	−1	−1	−1	−1	1	−1	98.9
−1	1	−1	1	1	1	1	−1	−1	1	−1	−1	1	1	−1	−1	−1	−1	1	45.8
1	−1	1	−1	1	1	1	1	−1	−1	1	−1	−1	1	1	−1	−1	−1	−1	170.1
−1	1	−1	1	−1	1	1	1	1	−1	−1	1	−1	−1	1	1	−1	−1	−1	107.3
−1	−1	1	−1	1	−1	1	1	1	1	−1	−1	1	−1	−1	1	1	−1	−1	283.6
−1	−1	−1	1	−1	1	−1	1	1	1	1	−1	−1	1	−1	−1	1	1	−1	130.2
1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	373.5

13. Suppose that you wish to run an experiment with four treatment factors (A, B, C, D) each having three levels. The only likely interactions are AB, AC, and ABC. The experiment needs to be run in blocks of size at most $k = 9$.

    (a) Design a 3^4 experiment in $b = 3^2$ blocks of size $k = 3^2$ confounding ABD and AB^2CD. What else is confounded? Are you happy with this design? Why or why not?
    (b) Show how you would obtain the nine blocks and show one of the blocks as an illustration.
    (c) Write out the degrees of freedom column for the analysis of variance table. (Read the question information again before you do this.)
    (d) Suppose that the blocks are randomly ordered. After the first block is run, the budget for the experiment is cut, so only nine observations are available. The design is now a 3^{4-2} fractional factorial design. Write down the defining relation for the design. Is this design going to be useful in examining the main effects and the interactions of interest? Why or why not?

14. **Plackett–Burman and supersaturated design**

    Table 15.58 shows a hypothetical set of data from a Plackett–Burman design with 20 observations and 19 factors. The data were simulated from a model in which factors C, J, K and P all have large main effects, all other main effects are drawn from a $N(0, 4.5)$ distribution and the random errors have a $N(0, 3)$ distribution. There are no interactions.

    (a) Using the data from Table 15.58, either fit a full main effects model or run an all-subsets regression to verify that the four important factors can be detected.
    (b) What are the estimates of the main effects of the four important factors?

(c) Using the *first* column of the design in Table 15.58 as a branching column, create a supersaturated design corresponding to +1 in the branching column.
(d) The supersaturated design in part (c) has 10 observations which may not be quite sufficient to be able to detect four large main effects. Run an all-subsets regression using the 10 observations. How many of the four important factors can be detected and what are their main effect estimates?

15. **Anatase experiment**

The anatase experiment was described in Exercise 8. After the initial experiment, several further experiments were run. Table 15.59 shows one of the definitive screening designs (where factor *D* of Exercise 8 has been set at level DCR, and factor *A* held constant). The response shown is pore diameter (nm), The randomized order of the observations is shown in the original paper. The design allows linear and quadratic trends in the main effects of some factors and some of the linear×linear interactions to be measured.

(a) Use an all subsets regression to find a model that explains much of the variability in the data (R^2 at least 0.90). Remember to include the main effects of any factors that are involved in large linear×linear interactions.
(b) If you were to design a follow-up experiment, which factors (and interactions) would you examine? Suggest a suitable design if you could take 16 more observations.

Table 15.59 Definitive screening design and pore diameter (nm) for the anatase experiment of Exercise 15

B	*C*	*E*	*F*	*G*	*H*	*I*	*J*	Pore diameter
0	−1	1	1	−1	1	1	1	9.0
0	1	−1	−1	1	−1	−1	−1	10.3
−1	0	−1	1	1	1	1	−1	14.6
1	0	1	−1	−1	−1	−1	1	16.9
−1	−1	0	1	1	−1	−1	0	6.6
1	1	0	−1	−1	1	1	−1	13.3
1	−1	1	0	1	1	−1	−1	15.3
−1	1	−1	0	−1	−1	1	1	7.8
−1	−1	1	−1	0	−1	1	−1	11.0
1	1	−1	1	0	1	−1	1	14.0
1	−1	−1	−1	1	0	1	1	11.2
−1	1	1	1	−1	0	−1	−1	9.3
−1	1	1	−1	1	1	0	1	8.5
1	−1	−1	1	−1	−1	0	−1	10.0
1	1	1	1	1	−1	1	0	18.6
−1	−1	−1	−1	−1	1	−1	0	11.8
0	0	0	0	0	0	0	0	12.5

Sources Olsen et al. (2014), *Journal of Porous Materials*, 21,

Table 15.60 2^{p-s} fractions of 2^p experiments. For each defining relation, s independent generators are underlined, and s corresponding equations are given. To obtain the $v = 2^{p-s}$ treatment combinations in the fraction, list all v combinations of levels a_i of the $p - s$ factors not determined by the equations, then use the equations modulo 2 to complete each treatment combination. For two blocks, confound the effect in parentheses and its aliases

2^{p-s}	v	Defining relation	Equations
2^{5-2}_{III}	8	$I = \underline{ABCE} = \underline{ABD} = CDE$	$a_4 = a_1 + a_2$
		(AC)	$a_5 = a_1 + a_2 + a_3$
2^{6-2}_{IV}	16	$I = \underline{ABCD} = \underline{CDEF} = ABEF$	$a_4 = a_1 + a_2 + a_3$
		(ACE)	$a_6 = a_3 + a_4 + a_5$
2^{7-2}_{IV}	32	$I = \underline{ABCDE} = \underline{ABFG} = CDEFG$	$a_5 = a_1 + a_2 + a_3 + a_4$
		(AEF)	$a_7 = a_1 + a_2 + a_6$
2^{8-2}_{V}	64	$I = \underline{ABCDE} = \underline{DEFGH} = ABCFGH$	$a_5 = a_1 + a_2 + a_3 + a_4$
		(CEF)	$a_8 = a_4 + a_5 + a_6 + a_7$
2^{6-3}_{III}	8	$I = \underline{BCD} = \underline{ABE} = ACDE$	$a_4 = a_2 + a_3$
		$= \underline{ABCF} = ADF = CEF$	$a_5 = a_1 + a_2$
		$= BDEF \quad (AC)$	$a_6 = a_1 + a_2 + a_3$
2^{7-3}_{IV}	16	$I = \underline{ABCD} = \underline{CDEF} = ABEF$	$a_4 = a_1 + a_2 + a_3$
		$= \underline{ACEG} = BDEG = ADFG$	$a_6 = a_3 + a_4 + a_5$
		$= BCFG \quad (ACF)$	$a_7 = a_1 + a_3 + a_5$
2^{8-3}_{IV}	32	$I = \underline{ABCD} = \underline{CDEF} = ABEF$	$a_4 = a_1 + a_2 + a_3$
		$= \underline{ACEGH} = BDEGH = ADFGH$	$a_6 = a_3 + a_4 + a_5$
		$= BCFGH \quad (ABG)$	$a_8 = a_1 + a_3 + a_5 + a_7$
2^{9-3}_{IV}	64	$I = \underline{CDEF} = \underline{ACEGH} = ADFGH$	$a_4 = a_1 + a_2 + a_3$
		$= \underline{ABCDJ} = ABEFJ = BDEGHJ$	$a_8 = a_1 + a_3 + a_5 + a_7$
		$= BCFGHJ \quad (ACF)$	$a_9 = a_1 + a_2 + a_3 + a_4$
2^{7-4}_{III}	8	$I = \underline{ABCD} = \underline{BCE} = ADE$	$a_4 = a_1 + a_2 + a_3$
		$= \underline{ACF} = BDF = ABEF$	$a_5 = a_2 + a_3$
		$= CDEF = \underline{ABG} = CDG$	$a_6 = a_1 + a_3$
		$= ACEG = BDEG = BCFG$	$a_7 = a_1 + a_2$
		$= ADFG = EFG = ABCDEFG$	
2^{8-4}_{IV}	16	$I = \underline{ABCD} = \underline{CDEF} = ABEF$	$a_4 = a_1 + a_2 + a_3$
		$= \underline{ADFG} = BCFG = ACEG$	$a_6 = a_3 + a_4 + a_5$
		$= BDEG = \underline{ABGH} = CDGH$	$a_7 = a_1 + a_4 + a_6$
		$= ABCDEFGH = EFGH = BDFH$	$a_8 = a_1 + a_2 + a_7$
		$= ACFH = BCEH = ADEH$	
		(ADH)	
2^{9-4}_{IV}	32	$I = \underline{ABCD} = \underline{CDEF} = ABEF$	$a_4 = a_1 + a_2 + a_3$
		$= \underline{ADFGH} = BCFGH = ACEGH$	$a_6 = a_3 + a_4 + a_5$
		$= BDEGH = \underline{ABGJ} = CDGJ$	$a_7 = a_1 + a_4 + a_5 + a_6$
		$= ABCDEFGH = EFGJ = BDFHJ$	$a_8 = a_1 + a_2 + a_7$
		$= ACFHJ = BCEHJ = ADEHJ$	
		(ABH)	

Table 15.61 3^{p-s} fractions of 3^p experiments. For each defining relation, s independent generators are underlined, and s corresponding equations are given. To obtain the $v = 3^{p-s}$ treatment combinations in the fraction, list all v combinations of levels a_i of the $p - s$ factors not determined by the equations, then use the equations modulo 3 to complete each treatment combination

3^{p-s}	v	Defining relation	Equations
3^{4-2}_{III}	9	$I = \underline{AB^2C} = A^2BC^2$	$a_3 = 2a_1 + a_2$
		$= \underline{ABD} = A^2CD = B^2C^2D$	$a_4 = 2a_2 + 2a_3$
		$= A^2B^2D^2 = BCD^2 = AC^2D^2$	
3^{5-2}_{III}	27	$I = \underline{ABC^2D^2} = A^2B^2CD$	$a_4 = a_1 + a_2 + 2a_3$
		$= \underline{ADE^2} = A^2BC^2E^2 = B^2CD^2E^2$	$a_5 = a_1 + a_4$
		$= A^2D^2E = BC^2DE = AB^2CE$	
3^{6-2}_{IV}	81	$I = \underline{ABC^2D^2} = A^2B^2CD$	$a_4 = a_1 + a_2 + 2a_3$
		$= \underline{ACEF} = A^2BD^2EF = B^2C^2DEF$	$a_6 = 2a_1 + 2a_3 + 2a_5$
		$= A^2C^2E^2F^2 = BCD^2E^2F^2 = AB^2DE^2F^2$	
3^{6-3}_{III}	27	$I = \underline{ABC^2D^2} = A^2B^2CD$	$a_4 = a_1 + a_2 + 2a_3$
		$= \underline{BDE^2} = AB^2C^2E^2 = A^2CD^2E^2$	
		$= B^2D^2E = AC^2DE = A^2BCE$	$a_5 = a_2 + a_4$
		$= \underline{CDF^2} = ABF^2 = A^2B^2C^2D^2F^2$	$a_6 = a_3 + a_4$
		$= BCD^2E^2F^2 = AB^2DE^2F^2 = A^2C^2E^2F^2$	
		$= B^2CEF^2 = AD^2EF^2 = A^2BC^2DEF^2$	
		$= C^2D^2F = ABCDF = A^2B^2F$	
		$= BC^2E^2F = AB^2CD^2E^2F = A^2DE^2F$	
		$= B^2C^2DEF = ACEF = A^2BD^2EF$	
3^{7-3}_{IV}	81	$I = \underline{ABC^2D^2} = A^2B^2CD$	$a_4 = a_1 + a_2 + 2a_3$
		$= \underline{ACEF} = A^2BD^2EF = B^2C^2DEF$	$a_6 = 2a_1 + 2a_3 + 2a_5$
		$= A^2C^2E^2F^2 = BCD^2E^2F^2 = AB^2DE^2F^2$	
		$= \underline{BC^2E^2FG} = AB^2CD^2E^2FG = A^2DE^2FG$	$a_7 = 2a_2 + a_3$
		$= ABF^2G = A^2B^2C^2D^2F^2G = CDF^2G$	$+ a_5 + 2a_6$
		$= A^2BCEG = B^2D^2EG = AC^2DEG$	
		$= B^2CEF^2G^2 = AD^2EF^2G^2 = A^2BC^2DEF^2G^2$	
		$= AB^2C^2E^2G^2 = A^2CD^2E^2G^2 = BDE^2G^2$	
		$= AB^2FG^2 = C^2D^2FG^2 = ABCDFG^2$	

Table 15.62 Orthogonal arrays with 2^p observations and useful column labelings. Assign factors in alphabetical order

No. of factors	8 observations—Design of Table 15.22															
	Columns															
	1	2	3	12	13	23	123									
3–6	*A*	*B*	*C*	*E*	*F*	*G*	*D*									
No. of factors	16 observations—Design of Table 15.24															
	Columns															
	1	2	12	3	13	23	123	4	14	24	124	34	134	234	1234	
4–5	*A*	*B*		*C*				*D*							*E*	
6–15	*A*	*B*	*K*	*C*	*L*	*M*	*D*	*E*	*N*	*P*	*F*	*Q*	*G*	*H*	*J*	
No. of factors	32 observations															
	Columns															
	1	2	12	3	13	23	123	4	14	24	124	34	134	234	1234	
4–22	*A*	*B*		*C*			*M*	*D*			*N*		*P*	*Q*	*G*	
	Columns															
	5	15	25	125	35	135	235	1235	45	145	245	1245	345	1345	2345	12345
4–22	*E*			*R*		*S*	*T*	*H*		*U*	*V*	*J*	*W*	*K*	*L*	*F*

Table 15.63 Generators for cyclically generated orthogonal main-effect plans. These are saturated designs for factors each at two levels, for n observations with n divisible by 4 but not a power of 2. To generate a design, systematically cycle the generator to the right to obtain $n - 1$ rows; then include a final row of -1's

n	Generator																						
8	1	1	1	−1	1	−1	−1																
12	1	1	1	−1	1	1	−1	1	−1	−1	−1												
16	1	1	1	1	−1	1	−1	1	1	−1	−1	1	−1	−1	−1								
20	1	1	1	1	−1	1	−1	1	−1	−1	−1	−1	1	1	−1	1	1	−1	−1				
24	1	1	1	1	1	−1	1	−1	1	1	−1	−1	1	1	−1	−1	1	−1	1	−1	−1	−1	−1
28	Does not exist																						
32	−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															
36	−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	1	1	1											

Obtained via a computer program described by Dean and Draper (1999). Further generators are given in Plackett and Burman (1946)

Table 15.64 Orthogonal arrays for 18 observations: $L_{18}(3^6 \times 6)$ and $L_{18}(3^7 \times 2)$

								For $L_{18}(3^7 \times 2)$ replace 6-level column by the two columns below
0	0	0	0	0	0	0	0	0
0	1	2	2	0	1	1	1	0
0	2	1	2	1	0	2	2	0
0	1	1	0	2	2	3	0	1
0	2	0	1	2	1	4	1	1
0	0	2	1	1	2	5	2	1
1	1	1	1	1	1	0	0	0
1	2	0	0	1	2	1	1	0
1	0	2	0	2	1	2	2	0
1	2	2	1	0	0	3	0	1
1	0	1	2	0	2	4	1	1
1	1	0	2	2	0	5	2	1
2	2	2	2	2	2	0	0	0
2	0	1	1	2	0	1	1	0
2	1	0	1	0	2	2	2	0
2	0	0	2	1	1	3	0	1
2	1	2	0	1	0	4	1	1
2	2	1	0	0	1	5	2	1

Sources $L_{18}(3^6 \times 6)$ from http://neilsloane.com/oadir/

Table 15.65 A 3^p orthogonal array for 27 observations: $L_{27}(3^{13})$

Columns												
1	2	3	4	5	6	7	8	9	10	11	12	13
0	0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	1	1	1	1	1	1	1	1	1
0	0	0	0	2	2	2	2	2	2	2	2	2
0	1	1	1	0	0	1	1	1	2	2	0	2
0	1	1	1	1	1	2	2	2	0	0	1	0
0	1	1	1	2	2	0	0	0	1	1	2	1
0	2	2	2	0	0	2	2	2	1	1	0	1
0	2	2	2	1	1	0	0	0	2	2	1	2
0	2	2	2	2	2	1	1	1	0	0	2	0
1	0	1	2	0	1	0	1	2	1	2	2	0
1	0	1	2	1	2	1	2	0	2	0	0	1
1	0	1	2	2	0	2	0	1	0	1	1	2
1	1	2	0	0	1	1	2	0	0	1	2	2
1	1	2	0	1	2	2	0	1	1	2	0	0
1	1	2	0	2	0	0	1	2	2	0	1	1
1	2	0	1	0	1	2	0	1	2	0	2	1
1	2	0	1	1	2	0	1	2	0	1	0	2
1	2	0	1	2	0	1	2	0	1	2	1	0
2	0	2	1	0	2	0	2	1	2	1	1	0
2	0	2	1	1	0	1	0	2	0	2	2	1
2	0	2	1	2	1	2	1	0	1	0	0	2
2	1	0	2	0	2	1	0	2	1	0	1	2
2	1	0	2	1	0	2	1	0	2	1	2	0
2	1	0	2	2	1	0	2	1	0	2	0	1
2	2	1	0	0	2	2	1	0	0	2	1	1
2	2	1	0	1	0	0	2	1	1	0	2	2
2	2	1	0	2	1	1	0	2	2	1	0	0
A	*B*			*C*			*D*	*E*	*F*	*G*		