CHAPTER 5

Introduction to Factorial Designs

CHAPTER OUTLINE

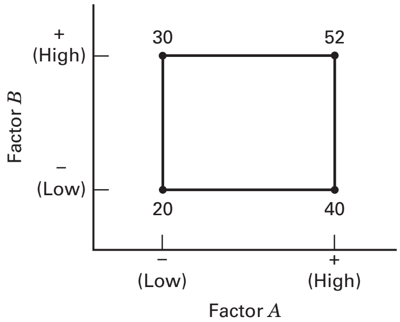
The supplemental material is on the textbook website www.wiley.com/college/montgomery.

CHAPTER LEARNING OBJECTIVES

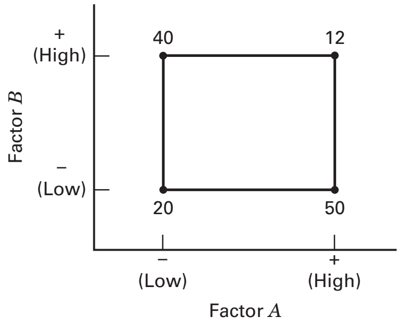
1. Learn the definitions of main effects and interactions.
2. Learn about two-factor factorial experiments.
3. Learn how the analysis of variance can be extended to factorial experiments.
4. Know how to check model assumptions in a factorial experiment.
5. Understand how sample size decisions can be evaluated for factorial experiments.
6. Know how factorial experiments can be used for more than two factors.
7. Know how the blocking principle can be extended to factorial experiments.
8. Know how to analyze factorial experiments by fitting response curves and surfaces.

5.1 Basic Definitions and Principles

Many experiments involve the study of the effects of two or more factors. In general, **factorial designs** are most efficient for this type of experiment. By a factorial design, we mean that in each complete trial or replicate of the experiment, all possible combinations of the levels of the factors are investigated. For example, if there are a levels of



■ **FIGURE 5.1 A two-factor factorial experiment, with the response (y) shown at the corners**



■ **FIGURE 5.2 A two-factor factorial experiment with interaction**

factor A and b levels of factor B, each replicate contains all ab treatment combinations. When factors are arranged in a factorial design, they are often said to be **crossed**.

The effect of a factor is defined to be the change in response produced by a change in the level of the factor. This is frequently called a **main effect** because it refers to the primary factors of interest in the experiment. For example, consider the simple experiment in Figure 5.1. This is a two-factor factorial experiment with both design factors at two levels. We have called these levels "low" and "high" and denoted them "–" and "+," respectively. The main effect of factor A in this two-level design can be thought of as the difference between the average response at the low level of A and the average response at the high level of A. Numerically, this is

$$A = \frac{40 + 52}{2} - \frac{20 + 30}{2} = 21$$

That is, increasing factor A from the low level to the high level causes an **average response increase** of 21 units. Similarly, the main effect of B is

$$B = \frac{30 + 52}{2} - \frac{20 + 40}{2} = 11$$

If the factors appear at more than two levels, the above procedure must be modified because there are other ways to define the effect of a factor. This point is discussed more completely later.

In some experiments, we may find that the difference in response between the levels of one factor is not the same at all levels of the other factors. When this occurs, there is an **interaction** between the factors. For example, consider the two-factor factorial experiment shown in Figure 5.2. At the low level of factor B (or B^-), the A effect is

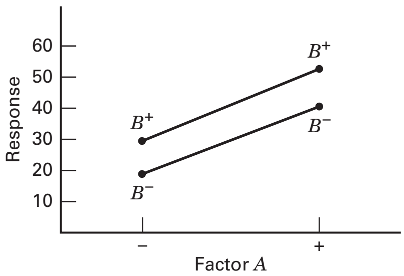
$$A = 50 - 20 = 30$$

and at the high level of factor B (or B^+), the A effect is

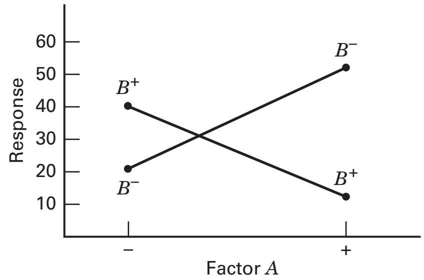
$$A = 12 - 40 = -28$$

Because the effect of A depends on the level chosen for factor B, we see that there is interaction between A and B. The magnitude of the interaction effect is the average *difference* in these two A effects, or $AB = (-28 - 30)/2 = -29$. Clearly, the interaction is large in this experiment.

These ideas may be illustrated graphically. Figure 5.3 plots the response data in Figure 5.1 against factor A for both levels of factor B. Note that the B^- and B^+ lines are approximately parallel, indicating a lack of interaction between factors A and B. Similarly, Figure 5.4 plots the response data in Figure 5.2. Here we see that the B^- and B^+ lines are not parallel. This indicates an interaction between factors A and B. Two-factor interaction graphs such as these are frequently very useful in interpreting significant interactions and in reporting results to nonstatistically



■ **FIGURE 5.3 A factorial experiment without interaction**



■ **FIGURE 5.4 A factorial experiment with interaction**

trained personnel. However, they should not be utilized as the sole technique of data analysis because their interpretation is subjective and their appearance is often misleading.

There is another way to illustrate the concept of interaction. Suppose that both of our design factors are **quantitative** (such as temperature, pressure, time). Then a **regression model representation** of the two-factor factorial experiment could be written as

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_{12} x_1 x_2 + \epsilon$$

where y is the response, the β's are parameters whose values are to be determined, x_1 is a variable that represents factor A, x_2 is a variable that represents factor B, and ϵ is a random error term. The variables x_1 and x_2 are defined on a **coded scale** from -1 to $+1$ (the low and high levels of A and B), and $x_1 x_2$ represents the interaction between x_1 and x_2.

The parameter estimates in this regression model turn out to be related to the effect estimates. For the experiment shown in Figure 5.1 we found the main effects of A and B to be $A = 21$ and $B = 11$. The estimates of β_1 and β_2 are one-half the value of the corresponding main effect; therefore, $\hat{\beta}_1 = 21/2 = 10.5$ and $\hat{\beta}_2 = 11/2 = 5.5$. The interaction effect in Figure 5.1 is $AB = 1$, so the value of interaction coefficient in the regression model is $\hat{\beta}_{12} = 1/2 = 0.5$. The parameter β_0 is estimated by the average of all four responses, or $\hat{\beta}_0 = (20 + 40 + 30 + 52)/4 = 35.5$. Therefore, the fitted regression model is

$$\hat{y} = 35.5 + 10.5x_1 + 5.5x_2 + 0.5x_1 x_2$$

The parameter estimates obtained in the manner for the factorial design with all factors at two levels ($-$ and $+$) turn out to be **least squares estimates** (more on this later).

The interaction coefficient ($\hat{\beta}_{12} = 0.5$) is small relative to the main effect coefficients $\hat{\beta}_1$ and $\hat{\beta}_2$. We will take this to mean that interaction is small and can be ignored. Therefore, dropping the term $0.5x_1x_2$ gives us the model

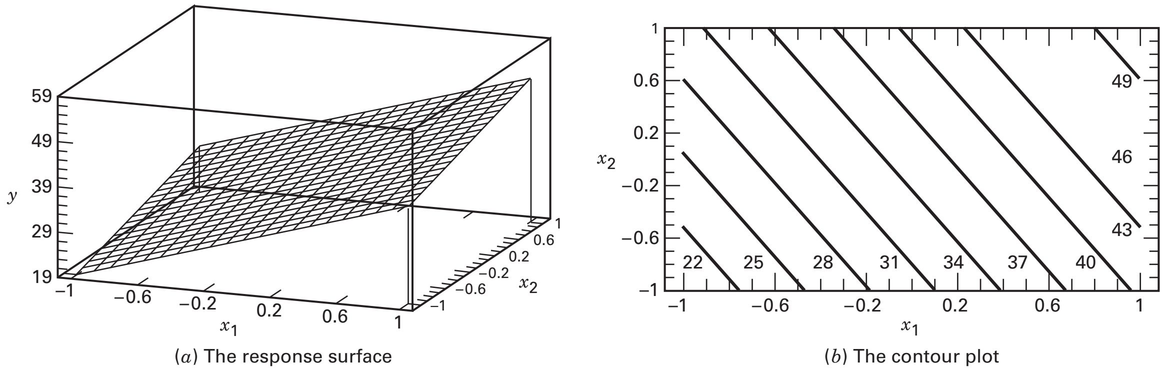
$$\hat{y} = 35.5 + 10.5x_1 + 5.5x_2$$

Figure 5.5 presents graphical representations of this model. In Figure 5.5*a* we have a plot of the plane of y-values generated by the various combinations of x_1 and x_2. This three-dimensional graph is called a **response surface plot**. Figure 5.5*b* shows the contour lines of constant response y in the x_1, x_2 plane. Notice that because the response surface is a plane, the contour plot contains parallel straight lines.

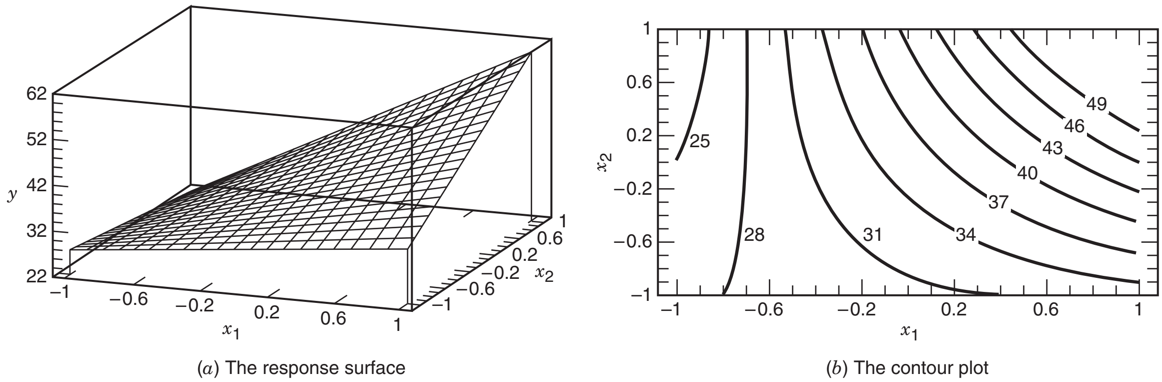
Now suppose that the interaction contribution to this experiment was not negligible; that is, the coefficient β_{12} was not small. Figure 5.6 presents the response surface and contour plot for the model

$$\hat{y} = 35.5 + 10.5x_1 + 5.5x_2 + 8x_1 x_2$$

(We have let the interaction effect be the average of the two main effects.) Notice that the significant interaction effect "twists" the plane in Figure 5.6*a*. This twisting of the response surface results in curved contour lines of constant response in the x_1, x_2 plane, as shown in Figure 5.6*b*. Thus, **interaction is a form of curvature** in the underlying response surface model for the experiment.



■ **FIGURE 5.5** **Response surface and contour plot for the model $\hat{y} = 35.5 + 10.5x_1 + 5.5x_2$**



■ **FIGURE 5.6** **Response surface and contour plot for the model $\hat{y} = 35.5 + 10.5x_1 + 5.5x_2 + 8x_1x_2$**

The response surface model for an experiment is extremely important and useful. We will say more about it in Section 5.5 and in subsequent chapters.

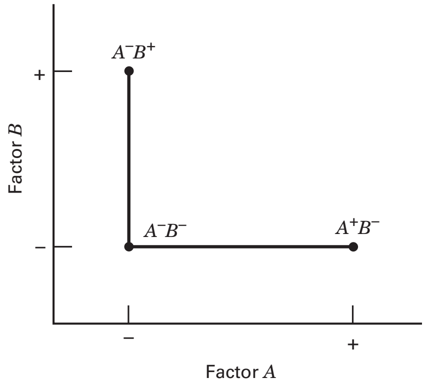
Generally, when an interaction is large, the corresponding main effects have little practical meaning. For the experiment in Figure 5.2, we would estimate the main effect of A to be

$$A = \frac{50 + 12}{2} - \frac{20 + 40}{2} = 1$$

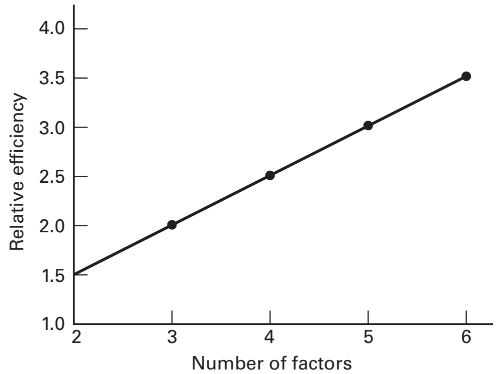
which is very small, and we are tempted to conclude that there is no effect due to A. However, when we examine the effects of A at *different levels of factor B*, we see that this is not the case. Factor A has an effect, but it *depends on the level of factor B*. That is, knowledge of the AB interaction is more useful than knowledge of the main effect. A significant interaction will often **mask** the significance of main effects. These points are clearly indicated by the interaction plot in Figure 5.4. In the presence of significant interaction, the experimenter must usually examine the levels of one factor, say A, with levels of the other factors fixed to draw conclusions about the main effect of A.

5.2 The Advantage of Factorials

The advantage of factorial designs can be easily illustrated. Suppose we have two factors A and B, each at two levels. We denote the levels of the factors by A^-, A^+, B^-, and B^+. Information on both factors could be obtained by varying the factors one at a time, as shown in Figure 5.7. The effect of changing factor A is given by $A^+B^- - A^-B^-$, and the



■ **FIGURE 5.7 A one-factor-at-a-time experiment**



■ **FIGURE 5.8 Relative efficiency of a factorial design to a one-factor-at-a-time experiment (two-level factors)**

effect of changing factor B is given by $A^-B^+ - A^-B^-$. Because experimental error is present, it is desirable to take two observations, say, at each treatment combination and estimate the effects of the factors using average responses. Thus, a total of six observations are required.

If a factorial experiment had been performed, an additional treatment combination, A^+B^+, would have been taken. Now, using just *four* observations, two estimates of the A effect can be made: $A^+B^- - A^-B^-$ and $A^+B^+ - A^-B^+$. Similarly, two estimates of the B effect can be made. These two estimates of each main effect could be averaged to produce average main effects that are *just as precise* as those from the single-factor experiment, but only four total observations are required and we would say that the relative efficiency of the factorial design to the one-factor-at-a-time experiment is $(6/4) = 1.5$. Generally, this relative efficiency will increase as the number of factors increases, as shown in Figure 5.8.

Now suppose interaction is present. If the one-factor-at-a-time design indicated that A^-B^+ and A^+B^- gave better responses than A^-B^-, a logical conclusion would be that A^+B^+ would be even better. However, if interaction is present, this conclusion may be *seriously in error*. For an example, refer to the experiment in Figure 5.2.

In summary, note that factorial designs have several advantages. They are more efficient than one-factor-at-a-time experiments. Furthermore, a factorial design is necessary when interactions may be present to avoid misleading conclusions. Finally, factorial designs allow the effects of a factor to be estimated at several levels of the other factors, yielding conclusions that are valid over a range of experimental conditions.

5.3 The Two-Factor Factorial Design

5.3.1 An Example

The simplest types of factorial designs involve only two factors or sets of treatments. There are a levels of factor A and b levels of factor B, and these are arranged in a factorial design; that is, each replicate of the experiment contains all ab treatment combinations. In general, there are n replicates.

As an example of a factorial design involving two factors, an engineer is designing a battery for use in a device that will be subjected to some extreme variations in temperature. The only design parameter that he can select at this point is the plate material for the battery, and he has three possible choices. When the device is manufactured and is shipped to the field, the engineer has no control over the temperature extremes that the device will encounter, and he knows from experience that temperature will probably affect the effective battery life. However, temperature can be controlled in the product development laboratory for the purposes of a test.

■ **TABLE 5.1**
Life (in hours) Data for the Battery Design Example

Material Type	**Temperature (°F)**					
	15		**70**		**125**	
1	130	155	34	40	20	70
	74	180	80	75	82	58
2	150	188	136	122	25	70
	159	126	106	115	58	45
3	138	110	174	120	96	104
	168	160	150	139	82	60

The engineer decides to test all three plate materials at three temperature levels—15, 70, and 125°F—because these temperature levels are consistent with the product end-use environment. Because there are two factors at three levels, this design is sometimes called a **3^2 factorial design**. Four batteries are tested at each combination of plate material and temperature, and all 36 tests are run in random order. The experiment and the resulting observed battery life data are given in Table 5.1.

In this problem, the engineer wants to answer the following questions:

1. What effects do material type and temperature have on the life of the battery?
2. Is there a choice of material that would give *uniformly long life regardless of temperature*?

This last question is particularly important. It may be possible to find a material alternative that is not greatly affected by temperature. If this is so, the engineer can make the battery **robust** to temperature variation in the field. This is an example of using statistical experimental design for **robust product design**, a very important engineering problem.

This design is a specific example of the general case of a two-factor factorial. To pass to the general case, let y_{ijk} be the observed response when factor A is at the ith level ($i = 1, 2, \ldots, a$) and factor B is at the jth level ($j = 1, 2, \ldots, b$) for the kth replicate ($k = 1, 2, \ldots, n$). In general, a two-factor factorial experiment will appear as in Table 5.2. The order in which the abn observations are taken is selected at random so that this design is a **completely randomized design**.

The observations in a factorial experiment can be described by a model. There are several ways to write the model for a factorial experiment. The **effects model** is

$$y_{ijk} = \mu + \tau_i + \beta_j + (\tau\beta)_{ij} + \epsilon_{ijk} \begin{cases} i = 1, 2, \ldots, a \\ j = 1, 2, \ldots, b \\ k = 1, 2, \ldots, n \end{cases} \tag{5.1}$$

where μ is the overall mean effect, τ_i is the effect of the ith level of the row factor A, β_j is the effect of the jth level of column factor B, $(\tau\beta)_{ij}$ is the effect of the interaction between τ_i and β_j, and ϵ_{ijk} is a random error component. Both factors are assumed to be **fixed**, and the treatment effects are defined as deviations from the overall mean, so $\sum_{i=1}^{a} \tau_i = 0$ and $\sum_{j=1}^{b} \beta_j = 0$. Similarly, the interaction effects are fixed and are defined such that $\sum_{i=1}^{a} (\tau\beta)_{ij} = \sum_{j=1}^{b} (\tau\beta)_{ij} = 0$. Because there are n replicates of the experiment, there are abn total observations.

■ **TABLE 5.2**
General Arrangement for a Two-Factor Factorial Design

		Factor B			
		1	**2**	...	b
Factor A	1	$y_{111}, y_{112}, \ldots, y_{11n}$	$y_{121}, y_{122}, \ldots, y_{12n}$		$y_{1b1}, y_{1b2}, \ldots, y_{1bn}$
	2	$y_{211}, y_{212}, \ldots, y_{21n}$	$y_{221}, y_{222}, \ldots, y_{22n}$		$y_{2b1}, y_{2b2}, \ldots, y_{2bn}$
	⋮				
	a	$y_{a11}, y_{a12}, \ldots, y_{a1n}$	$y_{a21}, y_{a22}, \ldots, y_{a2n}$		$y_{ab1}, y_{ab2}, \ldots, y_{abn}$

Another possible model for a factorial experiment is the **means model**

$$y_{ijk} = \mu_{ij} + \epsilon_{ijk} \begin{cases} i = 1, 2, \ldots, a \\ j = 1, 2, \ldots, b \\ k = 1, 2, \ldots, n \end{cases}$$

where the mean of the ijth cell is

$$\mu_{ij} = \mu + \tau_i + \beta_j + (\tau\beta)_{ij}$$

We could also use a **regression model** as in Section 5.1. Regression models are particularly useful when one or more of the factors in the experiment are quantitative. Throughout most of this chapter we will use the effects model (Equation 5.1) with an illustration of the regression model in Section 5.5.

In the two-factor factorial, both row and column factors (or treatments), A and B, are of equal interest. Specifically, we are interested in **testing hypotheses** about the equality of row treatment effects, say

$$\begin{aligned} &H_0{:}\tau_1 = \tau_2 = \cdots = \tau_a = 0 \\ &H_1{:}\text{at least one } \tau_i \neq 0 \end{aligned} \tag{5.2a}$$

and the equality of column treatment effects, say

$$\begin{aligned} &H_0{:}\beta_1 = \beta_2 = \cdots = \beta_b = 0 \\ &H_1{:}\text{at least one } \beta_j \neq 0 \end{aligned} \tag{5.2b}$$

We are also interested in determining whether row and column treatments *interact*. Thus, we also wish to test

$$\begin{aligned} &H_0{:}(\tau\beta)_{ij} = 0 \quad \text{for all } i, j \\ &H_1{:}\text{ at least one } (\tau\beta)_{ij} \neq 0 \end{aligned} \tag{5.2c}$$

We now discuss how these hypotheses are tested using a **two-factor analysis of variance**.

5.3.2 Statistical Analysis of the Fixed Effects Model

Let $y_{i..}$ denote the total of all observations under the ith level of factor A, $y_{.j.}$ denote the total of all observations under the jth level of factor B, $y_{ij.}$ denote the total of all observations in the ijth cell, and $y_{...}$ denote the grand total of all the observations. Define $\bar{y}_{i..}, \bar{y}_{.j.}, \bar{y}_{ij.}$, and $\bar{y}_{...}$ as the corresponding row, column, cell, and grand averages. Expressed mathematically,

$$
\begin{aligned}
y_{i..} &= \sum_{j=1}^{b}\sum_{k=1}^{n} y_{ijk} & \bar{y}_{i..} &= \frac{y_{i..}}{bn} & i &= 1, 2, \ldots, a \\
y_{.j.} &= \sum_{i=1}^{a}\sum_{k=1}^{n} y_{ijk} & \bar{y}_{.j.} &= \frac{y_{.j.}}{an} & j &= 1, 2, \ldots, b \\
y_{ij.} &= \sum_{k=1}^{n} y_{ijk} & \bar{y}_{ij.} &= \frac{y_{ij.}}{n} & i &= 1, 2, \ldots, a \\
& & & & j &= 1, 2, \ldots, b \\
y_{...} &= \sum_{i=1}^{a}\sum_{j=1}^{b}\sum_{k=1}^{n} y_{ijk} & \bar{y}_{...} &= \frac{y_{...}}{abn}
\end{aligned}
\tag{5.3}
$$

The **total corrected sum of squares** may be written as

$$
\begin{aligned}
\sum_{i=1}^{a}\sum_{j=1}^{b}\sum_{k=1}^{n} (y_{ijk} - \bar{y}_{...})^2 &= \sum_{i=1}^{a}\sum_{j=1}^{b}\sum_{k=1}^{n} [(\bar{y}_{i..} - \bar{y}_{...}) + (\bar{y}_{.j.} - \bar{y}_{...}) \\
&\quad + (\bar{y}_{ij.} - \bar{y}_{i..} - \bar{y}_{.j.} + \bar{y}_{...}) + (y_{ijk} - \bar{y}_{ij.})]^2] \\
&= bn \sum_{i=1}^{a} (\bar{y}_{i..} - \bar{y}_{...})^2 + an \sum_{j=1}^{b} (\bar{y}_{.j.} - \bar{y}_{...})^2 \\
&\quad + n \sum_{i=1}^{a}\sum_{j=1}^{b} (\bar{y}_{ij.} - \bar{y}_{..} - \bar{y}_{.j.} - \bar{y}_{...})^2 \\
&\quad + \sum_{i=1}^{a}\sum_{j=1}^{b}\sum_{k=1}^{n} (y_{ijk} - \bar{y}_{ij.})^2
\end{aligned}
\tag{5.4}
$$

because the six cross products on the right-hand side are zero. Notice that the total sum of squares has been partitioned into a sum of squares due to "rows," or factor A, (SS_A); a sum of squares due to "columns," or factor B, (SS_B); a sum of squares due to the interaction between A and B, (SS_{AB}); and a sum of squares due to error, (SS_E). This is the fundamental ANOVA equation for the two-factor factorial. From the last component on the right-hand side of Equation 5.4, we see that there must be at least two replicates ($n \geq 2$) to obtain an error sum of squares.

We may write Equation 5.4 symbolically as

$$SS_T = SS_A + SS_B + SS_{AB} + SS_E \tag{5.5}$$

The number of degrees of freedom associated with each sum of squares is

Effect	Degrees of Freedom
A	$a - 1$
B	$b - 1$
AB interaction	$(a - 1)(b - 1)$
Error	$ab(n - 1)$
Total	$abn - 1$

We may justify this allocation of the $abn - 1$ total degrees of freedom to the sums of squares as follows: The main effects A and B have a and b levels, respectively; therefore, they have $a - 1$ and $b - 1$ degrees of freedom as shown. The interaction degrees of freedom are simply the number of degrees of freedom for cells (which is $ab - 1$) minus the number of degrees of freedom for the two main effects A and B; that is, $ab - 1 - (a - 1) - (b - 1) = (a - 1)(b - 1)$. Within each of the ab cells, there are $n - 1$ degrees of freedom between the n replicates; thus, there are $ab(n - 1)$ degrees of freedom for error. Note that the number of degrees of freedom on the right-hand side of Equation 5.5 adds to the total number of degrees of freedom.

Each sum of squares divided by its degrees of freedom is a **mean square**. The expected values of the mean squares are

$$E(MS_A) = E\left(\frac{SS_A}{a-1}\right) = \sigma^2 + \frac{bn\sum_{i=1}^{a}\tau_i^2}{a-1}$$

$$E(MS_B) = E\left(\frac{SS_B}{b-1}\right) = \sigma^2 + \frac{an\sum_{j=1}^{b}\beta_j^2}{b-1}$$

$$E(MS_{AB}) = E\left(\frac{SS_{AB}}{(a-1)(b-1)}\right) = \sigma^2 + \frac{n\sum_{i=1}^{a}\sum_{j=1}^{b}(\tau\beta)_{ij}^2}{(a-1)(b-1)}$$

and

$$E(MS_E) = E\left(\frac{SS_E}{ab(n-1)}\right) = \sigma^2$$

Notice that if the null hypotheses of no row treatment effects, no column treatment effects, and no interaction are true, then MS_A, MS_B, MS_{AB}, and MS_E all estimate σ^2. However, if there are differences between row treatment effects, say, then MS_A will be larger than MS_E. Similarly, if there are column treatment effects or interaction present, then the corresponding mean squares will be larger than MS_E. Therefore, to test the significance of both main effects and their interaction, simply divide the corresponding mean square by the error mean square. Large values of this ratio imply that the data do not support the null hypothesis.

If we assume that the model (Equation 5.1) is adequate and that the error terms ϵ_{ijk} are normally and independently distributed with constant variance σ^2, then each of the ratios of mean squares MS_A/MS_E, MS_B/MS_E, and MS_{AB}/MS_E is distributed as F with $a - 1$, $b - 1$, and $(a - 1)(b - 1)$ numerator degrees of freedom, respectively, and $ab(n - 1)$ denominator degrees of freedom,[1] and the critical region would be the upper tail of the F distribution. The test procedure is usually summarized in an **analysis of variance table**, as shown in Table 5.3.

Computationally, we almost always employ a statistical software package to conduct an ANOVA. However, manual computing of the sums of squares in Equation 5.5 is straightforward. One could write out the individual elements of the ANOVA identity

$$y_{ijk} - \bar{y}_{...} = (\bar{y}_{i..} - \bar{y}_{...}) + (\bar{y}_{.j.} - \bar{y}_{...}) + (\bar{y}_{ij.} - \bar{y}_{i..} - \bar{y}_{.j.} + \bar{y}_{...}) + (y_{ijk} - \bar{y}_{ij.})$$

and calculate them in the columns of a spreadsheet. Then each column could be squared and summed to produce the ANOVA sums of squares. Computing formulas in terms of row, column, and cell totals can also be used. The total sum of squares is computed as usual by

$$SS_T = \sum_{i=1}^{a}\sum_{j=1}^{b}\sum_{k=1}^{n} y_{ijk}^2 - \frac{y_{...}^2}{abn} \tag{5.6}$$

[1] The F-test may be viewed as an approximation to a randomization test, as noted previously.

■ **TABLE 5.3**

The Analysis of Variance Table for the Two-Factor Factorial, Fixed Effects Model

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0
A treatments	SS_A	$a-1$	$MS_A = \frac{SS_A}{a-1}$	$F_0 = \frac{MS_A}{MS_E}$
B treatments	SS_B	$b-1$	$MS_B = \frac{SS_B}{b-1}$	$F_0 = \frac{MS_B}{MS_E}$
Interaction	SS_{AB}	$(a-1)(b-1)$	$MS_{AB} = \frac{SS_{AB}}{(a-1)(b-1)}$	$F_0 = \frac{MS_{AB}}{MS_E}$
Error	SS_E	$ab(n-1)$	$MS_E = \frac{SS_E}{ab(n-1)}$	
Total	SS_T	$abn-1$		

The sums of squares for the main effects are

$$SS_A = \frac{1}{bn}\sum_{i=1}^{a} y_{i..}^2 - \frac{y_{...}^2}{abn} \tag{5.7}$$

and

$$SS_B = \frac{1}{an}\sum_{j=1}^{b} y_{.j.}^2 - \frac{y_{...}^2}{abn} \tag{5.8}$$

It is convenient to obtain the SS_{AB} in two stages. First we compute the sum of squares between the ab cell totals, which is called the sum of squares due to "subtotals":

$$SS_{\text{Subtotals}} = \frac{1}{n}\sum_{i=1}^{a}\sum_{j=1}^{b} y_{ij.}^2 - \frac{y_{...}^2}{abn}$$

This sum of squares also contains SS_A and SS_B. Therefore, the second step is to computeSS_{AB} as

$$SS_{AB} = SS_{\text{Subtotals}} - SS_A - SS_B \tag{5.9}$$

We may compute SS_E by subtraction as

$$SS_E = SS_T - SS_{AB} - SS_A - SS_B \tag{5.10}$$

or

$$SS_E = SS_T - SS_{\text{Subtotals}}$$

EXAMPLE 5.1 The Battery Design Experiment

Table 5.4 presents the effective life (in hours) observed in the battery design example described in Section 5.3.1. The row and column totals are shown in the margins of the table, and the circled numbers are the cell totals.

Using Equations 5.6 through 5.10, the sums of squares are computed as follows:

$$\begin{aligned} SS_T &= \sum_{i=1}^{a}\sum_{j=1}^{b}\sum_{k=1}^{n} y_{ijk}^2 - \frac{y_{...}^2}{abn} \\ &= (130)^2 + (155)^2 + (74)^2 + \cdots \\ &\quad + (60)^2 - \frac{(3799)^2}{36} = 77{,}646.97 \end{aligned}$$

$$SS_{\text{Material}} = \frac{1}{bn}\sum_{i=1}^{a} y_{i..}^2 - \frac{y_{...}^2}{abn}$$
$$= \frac{1}{(3)(4)}[(998)^2 + (1300)^2 + (1501)^2] - \frac{(3799)^2}{36} = 10{,}683.72$$
$$SS_{\text{Temperature}} = \frac{1}{an}\sum_{j=1}^{b} y_{.j.}^2 - \frac{y_{...}^2}{abn}$$
$$= \frac{1}{(3)(4)}[(1738)^2 + (1291)^2 + (770)^2] - \frac{(3799)^2}{36} = 39{,}118.72$$
$$SS_{\text{Interaction}} = \frac{1}{n}\sum_{i=1}^{a}\sum_{j=1}^{b} y_{ij.}^2 - \frac{y_{...}^2}{abn} - SS_{\text{Material}} - SS_{\text{Temperature}}$$
$$= \frac{1}{4}[(539)^2 + (229)^2 + \cdots + (342)^2] - \frac{(3799)^2}{36} - 10{,}683.72 - 39{,}118.72 = 9613.78$$

and

$$SS_E = SS_T - SS_{\text{Material}} - SS_{\text{Temperature}} - SS_{\text{Interaction}}$$
$$= 77{,}646.97 - 10{,}683.72 - 39{,}118.72 - 9613.78 = 18{,}230.75$$

The ANOVA is shown in Table 5.5. Because $F_{0.05,4,27} = 2.73$, we conclude that there is a significant interaction between material types and temperature. Furthermore, $F_{0.05,2,27} = 3.35$, so the main effects of material type and temperature are also significant. Table 5.5 also shows the P-values for the test statistics.

To assist in interpreting the results of this experiment, it is helpful to construct a graph of the average responses at each treatment combination. This graph is shown in Figure 5.9. The significant interaction is indicated by the lack of parallelism of the lines. In general, longer life is attained at low temperature, regardless of material type. Changing from low to intermediate temperature, battery life with material type 3 may actually increase, whereas it decreases for types 1 and 2. From intermediate to high

■ **TABLE 5.4**
Life Data (in hours) for the Battery Design Experiment

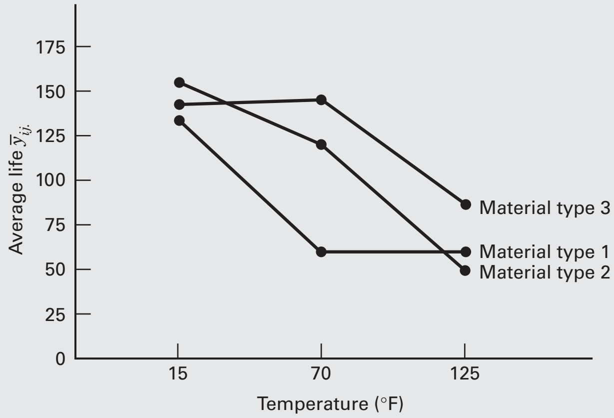
	Temperature (°F)									
Material Type	**15**			**70**			**125**			$y_{i..}$
	130	155		34	40		20	70		
1	74	180	539	80	75	229	82	58	230	998
	150	188		136	122		25	70		
2	159	126	623	106	115	479	58	45	198	1300
	138	110		174	120		96	104		
3	168	160	576	150	139	583	82	60	342	1501
$y_{.j.}$		1738			1291			770		$3799 = y_{...}$

■ **TABLE 5.5**
Analysis of Variance for Battery Life Data

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	P-Value
Material types	10,683.72	2	5,341.86	7.91	0.0020
Temperature	39,118.72	2	19,559.36	28.97	< 0.0001
Interaction	9,613.78	4	2,403.44	3.56	0.0186
Error	18,230.75	27	675.21		
Total	77,646.97	35			

temperature, battery life decreases for material types 2 and 3 and is essentially unchanged for type 1. Material type 3 seems to give the best results if we want less loss of effective life as the temperature changes.

■ **FIGURE 5.9 Material type–temperature plot for Example 5.1**



Multiple Comparisons. When the ANOVA indicates that row or column means differ, it is usually of interest to make comparisons between the individual row or column means to discover the specific differences. The multiple comparison methods discussed in Chapter 3 are useful in this regard.

We now illustrate the use of Tukey's test on the battery life data in Example 5.1. Note that in this experiment, interaction is significant. When interaction is significant, comparisons between the means of one factor (e.g., A) may be obscured by the AB interaction. One approach to this situation is to fix factor B at a specific level and apply Tukey's test to the means of factor A at that level. To illustrate, suppose that in Example 5.1 we are interested in detecting differences among the means of the three material types. Because interaction is significant, we make this comparison at just one level of temperature, say level 2 (70°F). We assume that the best estimate of the error variance is the MS_E from the ANOVA table, utilizing the assumption that the experimental error variance is the same over all treatment combinations.

The three material type averages at 70°F arranged in ascending order are

$$\bar{y}_{12.} = 57.25 \qquad \text{(material type 1)}$$
$$\bar{y}_{22.} = 119.75 \qquad \text{(material type 2)}$$
$$\bar{y}_{32.} = 145.75 \qquad \text{(material type 3)}$$

and

$$\begin{aligned} T_{0.05} &= q_{0.05}(3, 27)\sqrt{\frac{MS_E}{n}} \\ &= 3.50\sqrt{\frac{675.21}{4}} \\ &= 45.47 \end{aligned}$$

where we obtained $q_{0.05}(3, 27) \simeq 3.50$ by interpolation in Appendix Table V. The pairwise comparisons yield

$$3 \text{ vs. } 1: \quad 145.75 - 57.25 = 88.50 > T_{0.05} = 45.47$$
$$3 \text{ vs. } 2: \quad 145.75 - 119.75 = 26.00 < T_{0.05} = 45.47$$
$$2 \text{ vs. } 1: \quad 119.75 - 57.25 = 62.50 > T_{0.05} = 45.47$$

This analysis indicates that at the temperature level 70°F, the mean battery life is the same for material types 2 and 3 and that the mean battery life for material type 1 is significantly lower in comparison to both types 2 and 3.

If interaction is significant, the experimenter could compare *all ab* cell means to determine which ones differ significantly. In this analysis, differences between cell means include interaction effects as well as both main effects. In Example 5.1, this would give 36 comparisons between all possible pairs of the nine cell means.

Computer Output. Figure 5.10 presents condensed computer output for the battery life data in Example 5.1. Figure 5.10a contains Design-Expert output and Figure 5.10b contains JMP output. Note that

$$\begin{aligned} SS_{\text{Model}} &= SS_{\text{Material}} + SS_{\text{Temperature}} + SS_{\text{Interaction}} \\ &= 10{,}683.72 + 39{,}118.72 + 9613.78 \\ &= 59{,}416.22 \end{aligned}$$

with eight degrees of freedom. An F-test is displayed for the model source of variation. The P-value is small ($<$ 0.0001), so the interpretation of this test is that at least one of the three terms in the model is significant. The tests on the individual model terms (A, B, AB) follow. Also,

$$R^2 = \frac{SS_{\text{Model}}}{SS_{\text{Total}}} = \frac{59{,}416.22}{77{,}646.97} = 0.7652$$

That is, about 77 percent of the variability in the battery life is explained by the plate material in the battery, the temperature, and the material type–temperature interaction. The residuals from the fitted model are displayed on the Design-Expert computer output and the JMP output contains a plot of the residuals versus the predicted response. We now discuss the use of these residuals and residual plots in model adequacy checking.

5.3.3 Model Adequacy Checking

Before the conclusions from the ANOVA are adopted, the adequacy of the underlying model should be checked. As before, the primary diagnostic tool is **residual analysis**. The residuals for the two-factor factorial model with interaction are

$$e_{ijk} = y_{ijk} - \hat{y}_{ijk} \qquad \textbf{(5.11)}$$

and because the fitted value $\hat{y}_{ijk} = \bar{y}_{ij.}$ (the average of the observations in the ijth cell), Equation 5.11 becomes

$$e_{ijk} = y_{ijk} - \hat{y}_{ij.} \qquad \textbf{(5.12)}$$

The residuals from the battery life data in Example 5.1 are shown in the Design-Expert computer output (Figure 5.10a) and in Table 5.6. The normal probability plot of these residuals (Figure 5.11) does not reveal anything particularly troublesome, although the largest negative residual (−60.75 at 15°F for material type 1) does stand out somewhat from the others. The standardized value of this residual is $-60.75/\sqrt{675.21} = -2.34$, and this is the only residual whose absolute value is larger than 2.

Figure 5.12 plots the residuals versus the fitted values $\hat{y}_{ijk}$. This plot was also shown in the JMP computer output in Figure 5.10*b*. There is some mild tendency for the variance of the residuals to increase as the battery life increases. Figures 5.13 and 5.14 plot the residuals versus material types and temperature, respectively. Both plots indicate mild inequality of variance, with the treatment combination of 15°F and material type 1 possibly having larger variance than the others.

From Table 5.6 we see that the 15°F-material type 1 cell contains both extreme residuals (−60.75 and 45.25). These two residuals are primarily responsible for the inequality of variance detected in Figures 5.12, 5.13 and 5.14. Reexamination of the data does not reveal any obvious problem, such as an error in recording, so we accept these responses as legitimate. It is possible that this particular treatment combination produces slightly more erratic battery life than the others. The problem, however, is not severe enough to have a dramatic impact on the analysis and conclusions.

Response: Life In Hours
ANOVA for Selected Factorial Model
Analysis of Variance Table [Partial Sum of Squares]

Source	Sum of Squares	DF	Mean Square	*F* Value	Prob > *F*	
Model	59416.22	8	7427.03	11.00	< 0.0001	significant
A	*10683.72*	*2*	*5341.86*	*7.91*	*0.0020*	
B	*39118.72*	*2*	*19559.36*	*28.97*	*< 0.0001*	
AB	*9613.78*	*4*	*2403.44*	*3.56*	*0.0186*	
Residual	18230.75	27	675.21			
Lack of Fit	*0.000*	*0*				
Pure Error	*18230.75*	*27*	*675.21*			
Cor Total	77646.97	35				

Std. Dev.	25.98	R-Squared	0.7652
Mean	105.53	Adj R-Squared	0.6956
C.V.	24.62	Pred R-Squared	0.5826
PRESS	32410.22	Adeq Precision	8.178

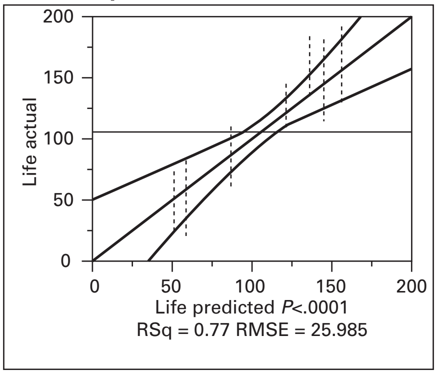
Diagnostics Case Statistics

Standard Order	Actual Value	Predicted Value	Residual	Leverage	Student Residual	Cook's Distance	Outlier *t*
1	130.00	134.75	−4.75	0.250	−0.211	0.002	−0.207
2	74.00	134.75	−60.75	0.250	−2.700	0.270	−3.100
3	155.00	134.75	20.25	0.250	0.900	0.030	0.897
4	180.00	134.75	45.25	0.250	2.011	0.150	2.140
5	150.00	155.75	−5.75	0.250	−0.256	0.002	−0.251
6	159.00	155.75	3.25	0.250	0.144	0.001	0.142
7	188.00	155.75	32.25	0.250	1.433	0.076	1.463
8	126.00	155.75	−29.75	0.250	−1.322	0.065	−1.341
9	138.00	144.00	26.00	0.250	−0.267	0.003	−0.262
10	168.00	144.00	24.00	0.250	1.066	0.042	1.069
11	110.00	144.00	−34.00	0.250	−1.511	0.085	−1.550
12	160.00	144.00	16.00	0.250	0.711	0.019	0.704
13	34.00	57.25	−23.25	0.250	−1.033	0.040	−1.035
14	80.00	57.25	22.75	0.250	1.011	0.038	1.011
15	40.00	57.25	−17.25	0.250	−0.767	0.022	−0.761
16	75.00	57.25	17.75	0.250	0.789	0.023	0.783
17	136.00	119.75	16.25	0.250	0.722	0.019	0.716
18	106.00	119.75	−13.75	0.250	−0.611	0.014	−0.604
19	122.00	119.75	2.25	0.250	0.100	0.000	0.098
20	115.00	119.75	−4.75	0.250	−0.211	0.002	−0.207
21	174.00	145.75	28.25	0.250	1.255	0.058	1.269
22	150.00	145.75	4.25	0.250	0.189	0.001	0.185
23	120.00	145.75	−25.75	0.250	−1.144	0.048	−1.151
24	139.00	145.75	−6.75	0.250	−0.300	0.003	−0.295
25	20.00	57.50	−37.50	0.250	−1.666	0.103	−1.726
26	82.00	57.50	24.50	0.250	1.089	0.044	1.093
27	70.00	57.50	12.50	0.250	0.555	0.011	0.548
28	58.00	57.50	0.50	0.250	0.022	0.000	0.022
29	25.00	49.50	−24.50	0.250	−1.089	0.044	−1.093
30	58.00	49.50	8.50	0.250	0.378	0.005	0.372
31	70.00	49.50	20.50	0.250	0.911	0.031	0.908
32	45.00	49.50	−4.50	0.250	−0.200	0.001	−0.196
33	96.00	85.50	10.50	0.250	0.467	0.008	0.460
34	82.00	85.50	−3.50	0.250	−0.156	0.001	−0.153
35	104.00	85.50	18.50	0.250	0.822	0.025	0.817
36	60.00	85.50	−25.50	0.250	−1.133	0.048	−1.139

(*a*)

■ **FIGURE 5.10** **Computer output for Example 5.1. (a) Design-Expert output; (b) JMP output**

Response Life
Whole Model
Actual by Predicted Plot



Summary of Fit

RSquare	0.76521
RSquare Adj	0.695642
Root Mean Square Error	25.98486
Mean of Response	105.5278
Observations (or Sum Wgts)	36

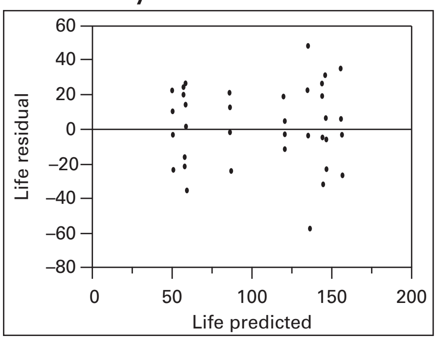
Analysis of Variance

Source	DF	Sum of Squares	Mean Square	*F* Ratio
Model	8	59416.222	7427.03	10.9995
Error	27	18230.750	675.21	Prob > F
C.Total	35	77646.972		<.001

Effect Tests

Source	Nparm	DF	Sum of Squares	*F* Ratio	Prob > *F*
Material Type	2	2	10683.722	7.9114	0.0020
Temperature	2	2	39118.722	28.9677	<.0001
Material Type Temperature	4	4	9613.778	3.5595	0.0186

Residual by Predicted Plot

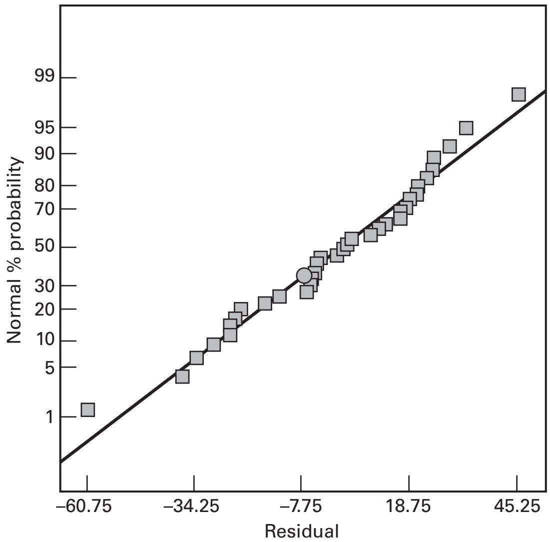


(*b*)

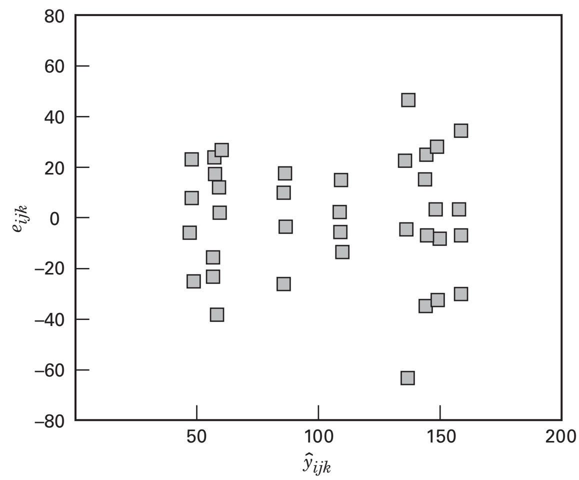
■ **FIGURE 5.10** (*Continued*)

■ **TABLE 5.6**
Residuals for Example 5.1

	Temperature (°F)					
Material Type	**15**		**70**		**125**	
1	−4.75	20.25	−23.25	−17.25	−37.50	12.50
	−60.75	45.25	22.75	17.75	24.50	0.50
2	−5.75	32.25	16.25	2.25	−24.50	20.50
	3.25	−29.75	−13.75	−4.75	8.50	−4.50
3	−6.00	−34.00	28.25	−25.75	10.50	18.50
	24.00	16.00	4.25	−6.75	−3.50	−25.50



■ **FIGURE 5.11** **Normal probability plot of residuals for Example 5.1**



■ **FIGURE 5.12** **Plot of residuals versus $\hat{y}_{ijk}$ for Example 5.1**

5.3.4 Estimating the Model Parameters

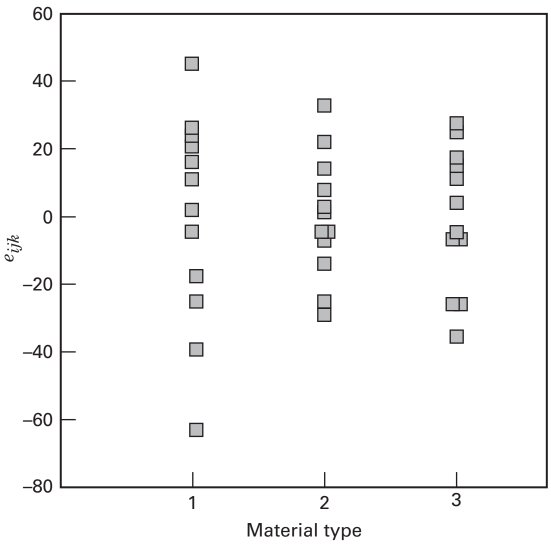
The parameters in the effects model for two-factor factorial

$$y_{ijk} = \mu + \tau_i + \beta_j + (\tau\beta)_{ij} + \epsilon_{ijk} \tag{5.13}$$

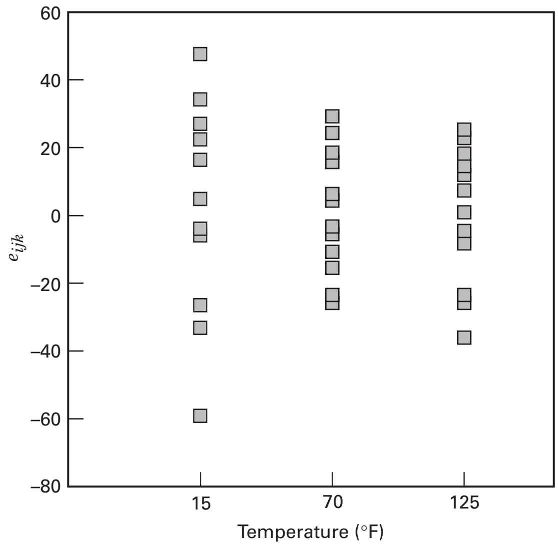
may be estimated by least squares. Because the model has $1 + a + b + ab$ parameters to be estimated, there are $1 + a + b + ab$ normal equations. Using the method of Section 3.9, we find that it is not difficult to show that the normal equations are

$$\mu{:}abn\hat{\mu} + bn\sum_{i=1}^{a}\hat{\tau}_i + an\sum_{j=1}^{b}\hat{\beta}_j + n\sum_{i=1}^{a}\sum_{j=1}^{b}(\widehat{\tau\beta})_{ij} = y_{...} \tag{5.14a}$$

$$\tau_i{:}bn\hat{\mu} + bn\hat{\tau}_i + n\sum_{j=1}^{b}\hat{\beta}_j + n\sum_{j=1}^{b}(\widehat{\tau\beta})_{ij} = y_{i..} \qquad i = 1, 2, \ldots, a \tag{5.14b}$$



■ **FIGURE 5.13 Plot of residuals versus material type for Example 5.1**



■ **FIGURE 5.14 Plot of residuals versus temperature for Example 5.1**

$$\beta_j\colon an\hat{\mu} + n\sum_{i=1}^{a}\hat{\tau}_i + an\hat{\beta}_j + n\sum_{i=1}^{a}(\widehat{\tau\beta})_{ij} = y_{.j.} \qquad j = 1, 2, \ldots, b \tag{5.14c}$$

$$(\tau\beta)_{ij}\colon n\hat{\mu} + n\hat{\tau}_i + n\hat{\beta}_j + n(\widehat{\tau\beta})_{ij} = y_{ij.} \qquad \begin{cases} i = 1, 2, \ldots, a \\ j = 1, 2, \ldots, b \end{cases} \tag{5.14d}$$

For convenience, we have shown the parameter corresponding to each normal equation on the left-hand side in Equations 5.14.

The effects model (Equation 5.13) is an overparameterized model. Notice that the a equations in Equation 5.14b sum to Equation 5.14a and that the b equations of Equation 5.14c sum to Equation 5.14a. Also summing Equation 5.14d over j for a particular i will give Equation 5.14b, and summing Equation 5.14d over i for a particular j will give Equation 5.14c. Therefore, there are $a + b + 1$ **linear dependencies** in this system of equations, and no unique solution will exist. In order to obtain a solution, we impose the constraints

$$\sum_{i=1}^{a}\hat{\tau}_i = 0 \tag{5.15a}$$

$$\sum_{j=1}^{b}\hat{\beta}_j = 0 \tag{5.15b}$$

$$\sum_{i=1}^{a}(\widehat{\tau\beta})_{ij} = 0 \qquad j = 1, 2, \ldots, b \tag{5.15c}$$

and

$$\sum_{j=1}^{b}(\widehat{\tau\beta})_{ij} = 0 \qquad i = 1, 2, \ldots, a \tag{5.15d}$$

Equations 5.15a and 5.15b constitute two constraints, whereas Equations 5.15c and 5.15d form $a + b - 1$ independent constraints. Therefore, we have $a + b + 1$ total constraints, the number needed.

Applying these constraints, the normal equations (Equations 5.14) simplify considerably, and we obtain the solution

$$\begin{aligned}
\hat{\mu} &= \overline{y}_{...} \\
\hat{\tau}_i &= \overline{y}_{i..} - \overline{y}_{...} \qquad i = 1, 2, \ldots, a \\
\hat{\beta}_j &= \overline{y}_{.j.} - \overline{y}_{...} \qquad j = 1, 2, \ldots, b \\
\widehat{(\tau\beta)}_{ij} &= \overline{y}_{ij.} - \overline{y}_{i..} - \overline{y}_{.j.} + \overline{y}_{...} \qquad \begin{cases} i = 1, 2, \ldots, a \\ j = 1, 2, \ldots, b \end{cases}
\end{aligned} \tag{5.16}$$

Notice the considerable intuitive appeal of this solution to the normal equations. Row treatment effects are estimated by the row average minus the grand average; column treatments are estimated by the column average minus the grand average; and the *ij*th interaction is estimated by the *ij*th cell average minus the grand average, the *i*th row effect, and the *j*th column effect.

Using Equation 5.16, we may find the **fitted value** y_{ijk} as

$$\begin{aligned}
\hat{y}_{ijk} &= \hat{\mu} + \hat{\tau}_i + \hat{\beta}_j + \widehat{(\tau\beta)}_{ij} \\
&= \overline{y}_{...} + (\overline{y}_{i..} - \overline{y}_{...}) + (\overline{y}_{.j.} - \overline{y}_{...}) \\
&\quad + (\overline{y}_{ij.} - \overline{y}_{i..} - \overline{y}_{.j.} + \overline{y}_{...}) \\
&= \overline{y}_{ij.}
\end{aligned}$$

That is, the *k*th observation in the *ij*th cell is estimated by the average of the *n* observations in that cell. This result was used in Equation 5.12 to obtain the residuals for the two-factor factorial model.

Because constraints (Equations 5.15) have been used to solve the normal equations, the model parameters are not uniquely estimated. However, certain important **functions** of the model parameters *are* estimable, that is, uniquely estimated regardless of the constraint chosen. An example is $\tau_i - \tau_u + \overline{(\tau\beta)}_{i.} - \overline{(\tau\beta)}_{u.}$, which might be thought of as the "true" difference between the *i*th and the *u*th levels of factor *A*. Notice that the true difference between the levels of any main effect includes an "average" interaction effect. It is this result that disturbs the tests on main effects in the presence of interaction, as noted earlier. In general, any function of the model parameters that is a linear combination of the left-hand side of the normal equations is **estimable**. This property was also noted in Chapter 3 when we were discussing the single-factor model. For more information, see the supplemental text material for this chapter.

5.3.5 Choice of Sample Size

Computer software can be used to assist in determining an appropriate same size in a factorial experiment. For example, consider the battery life experiment in Example 5.1. There are two factors, one quantitative and one qualitative, each at three levels. Suppose that the experimenter is unsure about the required number of replicates, but wants to be sure that if the effect sizes are one standard deviation in magnitude, they have a high probability of being detected (power).

JMP can be used to assist in answering this sample size question. Table 5.7 contains output from the JMP Design Evaluation tool for this experiment, assuming three replicates (upper portion of the table) and four replicates (lower portion). In this analysis, we have assumed that the model regression coefficients are one standard deviation in magnitude. Because temperature is quantitative, we have included both linear and quadratic components of that factor. The qualitative factor material type has two degrees of freedom, which are represented by the two material type model terms. Both designs have reasonable power. With three replicates, the interaction effects and the quadratic temperature effects have power below 0.9, while with four replicates the power for the interaction term is also above 0.9 and the power for the quadratic effect of temperature has increased from 0.645 to 0.78. This is probably adequate, so a design with four replicates is a reasonable choice.

■ **TABLE 5.7**
Power Analysis from JMP for Example 5.1

Power Analysis

Significance Level 0.05
Anticipated RMSE 1

Term	Anticipated Coefficient	Power
Intercept	1	0.814
Material type 1	1	0.937
Material type 2	-1	0.937
Temperature	1	0.981
Material type*Temperature 1	1	0.814
Material type*Temperature 2	-1	0.814
Temperature*Temperature	1	0.645

Power Analysis

Significance Level 0.05
Anticipated RMSE 1

Term	Anticipated Coefficient	Power
Intercept	1	0.917
Material type 1	1	0.984
Material type 2	-1	0.984
Temperature	1	0.997
Material type*Temperature 1	1	0.917
Material type*Temperature 2	-1	0.917
Temperature*Temperature	1	0.78

5.3.6 The Assumption of No Interaction in a Two-Factor Model

Occasionally, an experimenter feels that a **two-factor model without interaction** is appropriate, say

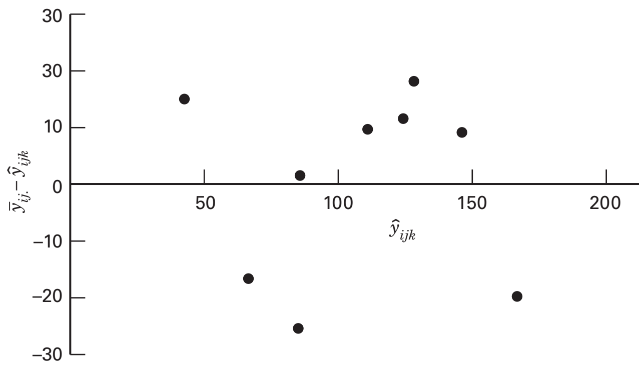
$$y_{ijk} = \mu + \tau_i + \beta_j + \epsilon_{ijk} \qquad \begin{cases} i = 1, 2, \ldots, a \\ j = 1, 2, \ldots, b \\ k = 1, 2, \ldots, n \end{cases} \tag{5.17}$$

We should be very careful in dispensing with the interaction terms, however, because the presence of significant interaction can have a dramatic impact on the interpretation of the data.

The statistical analysis of a two-factor factorial model without interaction is straightforward. Table 5.8 presents the analysis of the battery life data from Example 5.1, assuming that the no-interaction model (Equation 5.17) applies. As noted previously, both main effects are significant. However, as soon as a residual analysis is performed for these data, it becomes clear that the no-interaction model is inadequate. For the two-factor model without interaction, the fitted values are $\hat{y}_{ijk} = \overline{y}_{i..} + \overline{y}_{.j.} - \overline{y}_{...}$. A plot of $\hat{y}_{ij.} - \hat{y}_{ijk}$ (the cell averages minus the fitted value for that cell) versus the fitted value $\hat{y}_{ijk}$ is shown in Figure 5.15. Now the quantities $\overline{y}_{ij.} - \hat{y}_{ijk}$ may be viewed as the differences between the observed cell means and the estimated cell means assuming no interaction. Any pattern in these quantities

■ **TABLE 5.8**
Analysis of Variance for Battery Life Data Assuming No Interaction

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0
Material types	10,683.72	2	5,341.86	5.95
Temperature	39,118.72	2	19,559.36	21.78
Error	27,844.52	31	898.21	
Total	77,646.96	35		



■ **FIGURE 5.15** **Plot of $\bar{y}_{ij.} - \hat{y}_{ijk}$ versus $\hat{y}_{ijk}$, battery life data**

is suggestive of the presence of interaction. Figure 5.15 shows a distinct pattern as the quantities $\bar{y}_{ij.} - \bar{y}_{ijk}$ move from positive to negative to positive to negative again. This structure is the result of interaction between material types and temperature.

5.3.7 One Observation per Cell

Occasionally, one encounters a two-factor experiment with only a **single replicate**, that is, only one observation per cell. If there are two factors and only one observation per cell, the effects model is

$$y_{ij} = \mu + \tau_i + \beta_j + (\tau\beta)_{ij} + \epsilon_{ij} \qquad \begin{cases} i = 1, 2, \ldots, a \\ j = 1, 2, \ldots, b \end{cases} \tag{5.18}$$

The analysis of variance for this situation is shown in Table 5.9, assuming that both factors are fixed.

From examining the expected mean squares, we see that the error variance σ^2 is *not estimable*; that is, the two-factor interaction effect $(\tau\beta)_{ij}$ and the experimental error cannot be separated in any obvious manner. Consequently, there are no tests on main effects unless the interaction effect is zero. If there is no interaction present, then $(\tau\beta)_{ij} = 0$ for all i and j, and a plausible model is

$$y_{ij} = \mu + \tau_i + \beta_j + \epsilon_{ij} \qquad \begin{cases} i = 1, 2, \ldots, a \\ j = 1, 2, \ldots, b \end{cases} \tag{5.19}$$

If the model (Equation 5.19) is appropriate, then the residual mean square in Table 5.9 is an unbiased estimator of σ^2, and the main effects may be tested by comparing MS_A and MS_B to MS_{Residual}.

A test developed by Tukey (1949a) is helpful in determining whether interaction is present. The procedure assumes that the interaction term is of a particularly simple form, namely,

$$(\tau\beta)_{ij} = \gamma\tau_i\beta_j$$

■ **TABLE 5.9**
Analysis of Variance for a Two-Factor Model, One Observation per Cell

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	Expected Mean Square
Rows (A)	$\sum_{i=1}^{a} \frac{y_{i.}^2}{b} - \frac{y_{..}^2}{ab}$	$a - 1$	MS_A	$\sigma^2 + \frac{b \sum \tau_i^2}{a-1}$
Columns (B)	$\sum_{j=1}^{b} \frac{y_{.j}^2}{a} - \frac{y_{..}^2}{ab}$	$b - 1$	MS_B	$\sigma^2 + \frac{a \sum \beta_j^2}{b-1}$
Residual or AB	Subtraction	$(a-1)(b-1)$	MS_{Residual}	$\sigma^2 + \frac{\sum\sum(\tau\beta)_{ij}^2}{(a-1)(b-1)}$
Total	$\sum_{i=1}^{a}\sum_{j=1}^{b} y_{ij}^2 - \frac{y_{..}^2}{ab}$	$ab - 1$		

where γ is an unknown constant. By defining the interaction term this way, we may use a regression approach to test the significance of the interaction term. The test partitions the residual sum of squares into a single-degree-of-freedom component due to nonadditivity (interaction) and a component for error with $(a-1)(b-1)-1$ degrees of freedom. Computationally, we have

$$SS_N = \frac{\left[\sum_{i=1}^{a}\sum_{j=1}^{b} y_{ij}y_{i.}y_{.j} - y_{..}\left(SS_A + SS_B + \frac{y_{..}^2}{ab}\right)^2\right]^2}{abSS_A SS_B} \tag{5.20}$$

with one degree of freedom, and

$$SS_{\text{Error}} = SS_{\text{Residual}} - SS_N \tag{5.21}$$

with $(a-1)(b-1)-1$ degrees of freedom. To test for the presence of interaction, we compute

$$F_0 = \frac{SS_N}{SS_{\text{Error}}/[(a-1)(b-1)-1]} \tag{5.22}$$

If $F_0 > F_{\alpha,1,(a-1)(b-1)-1}$, the hypothesis of no interaction must be rejected.

EXAMPLE 5.2

The impurity present in a chemical product is affected by two factors—pressure and temperature. The data from a single replicate of a factorial experiment are shown in Table 5.10. The sums of squares are

$$SS_A = \frac{1}{b}\sum_{i=1}^{a} y_{i.}^2 - \frac{y_{..}^2}{ab}$$

$$= \frac{1}{5}[23^2 + 13^2 + 8^2] - \frac{44^2}{(3)(5)} = 23.33$$

$$SS_B = \frac{1}{a}\sum_{j=1}^{b} y_{.j}^2 - \frac{y_{..}^2}{ab}$$

$$= \frac{1}{3}[9^2 + 6^2 + 13^2 + 6^2 + 10^2] - \frac{44^2}{(3)(5)} = 11.60$$

$$SS_T = \sum_{i=1}^{a}\sum_{j=1}^{b} y_{ij}^2 - \frac{y_{..}^2}{ab}$$

$$= 166 - 129.07 = 36.93$$

and

$$SS_{\text{Residual}} = SS_T - SS_A - SS_B$$

$$= 36.93 - 23.33 - 11.60 = 2.00$$

The sum of squares for nonadditivity is computed from Equation 5.20 as follows:

$$\sum_{i=1}^{a}\sum_{j=1}^{b} y_{ij}y_{i.}y_{.j} = (5)(23)(9) + (4)(23)(6) + \cdots$$
$$+ (2)(8)(10) = 7236$$

$$SS_N = \frac{\left[\sum_{i=1}^{a}\sum_{j=1}^{b} y_{ij}y_{i.}y_{.j} - y_{..}\left(SS_A + SS_B + \frac{y_{..}^2}{ab}\right)\right]^2}{abSS_ASS_B}$$
$$= \frac{[7236 - (44)(23.33 + 11.60 + 129.07)]^2}{(3)(5)(23.33)(11.60)}$$
$$= \frac{[20.00]^2}{4059.42} = 0.0985$$

and the error sum of squares is, from Equation 5.21,

$$SS_{\text{Error}} = SS_{\text{Residual}} - SS_N = 2.00 - 0.0985 = 1.9015$$

The complete ANOVA is summarized in Table 5.11. The test statistic for nonadditivity is $F_0 = 0.0985/0.2716 = 0.36$, so we conclude that there is no evidence of interaction in these data. The main effects of temperature and pressure are significant.

■ **TABLE 5.10**
Impurity Data for Example 5.2

	Pressure					
Temperature (°F)	**25**	**30**	**35**	**40**	**45**	$y_{i.}$
100	5	4	6	3	5	23
125	3	1	4	2	3	13
150	1	1	3	1	2	8
$y_{.j}$	9	6	13	6	10	$44 = y_{..}$

■ **TABLE 5.11**
Analysis of Variance for Example 5.2

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	*P*-Value
Temperature	23.33	2	11.67	42.97	0.0001
Pressure	11.60	4	2.90	10.68	0.0042
Nonadditivity	0.0985	1	0.0985	0.36	0.5674
Error	1.9015	7	0.2716		
Total	36.93	14			

In concluding this section, we note that the two-factor factorial model with one observation per cell (Equation 5.19) looks exactly like the randomized complete block model (Equation 4.1). In fact, the Tukey single-degree-of-freedom test for nonadditivity can be directly applied to test for interaction in the randomized block model. However, remember that the **experimental situations** that lead to the randomized block and factorial models are very different. In the factorial model, *all ab* runs have been made in random order, whereas in the randomized block model, randomization occurs only *within the block*. The blocks are a randomization restriction. Hence, the manner in which the experiments are run and the interpretation of the two models are quite different.

5.4 The General Factorial Design

The results for the two-factor factorial design may be extended to the general case where there are a levels of factor A, b levels of factor B, c levels of factor C, and so on, arranged in a factorial experiment. In general, there will be $abc \ldots n$ total observations if there are n replicates of the complete experiment. Once again, note that we must have at least two replicates ($n \geq 2$) to determine a sum of squares due to error if all possible interactions are included in the model.

If all factors in the experiment are fixed, we may easily formulate and test hypotheses about the main effects and interactions using the ANOVA. For a fixed effects model, test statistics for each main effect and interaction may be constructed by dividing the corresponding mean square for the effect or interaction by the mean square error. All of these F-tests will be upper-tail, one-tail tests. The number of degrees of freedom for any main effect is the number of levels of the factor minus one, and the number of degrees of freedom for an interaction is the product of the number of degrees of freedom associated with the individual components of the interaction.

For example, consider the **three-factor analysis of variance model**:

$$y_{ijkl} = \mu + \tau_i + \beta_j + \gamma_k + (\tau\beta)_{ij} + (\tau\gamma)_{ik} + (\beta\gamma)_{jk} + (\tau\beta\gamma)_{ijk} + \epsilon_{ijkl} \quad \begin{cases} i = 1, 2, \ldots, a \\ j = 1, 2, \ldots, b \\ k = 1, 2, \ldots, c \\ l = 1, 2, \ldots, n \end{cases} \tag{5.23}$$

Assuming that A, B, and C are fixed, the **analysis of variance table** is shown in Table 5.12. The F-tests on main effects and interactions follow directly from the expected mean squares.

■ **TABLE 5.12**

The Analysis of Variance Table for the Three-Factor Fixed Effects Model

Source of Variation	Sum of Square	Degrees of Freedom	Mean Squares	Expected Mean Square	F_0
A	SS_A	$a-1$	MS_A	$\sigma^2 + \dfrac{bcn\sum\tau_i^2}{a-1}$	$F_0 = \dfrac{MS_A}{MS_E}$
B	SS_B	$b-1$	MS_B	$\sigma^2 + \dfrac{acn\sum\beta_j^2}{b-1}$	$F_0 = \dfrac{MS_B}{MS_E}$
C	SS_C	$c-1$	MS_C	$\sigma^2 + \dfrac{abn\sum\gamma_k^2}{c-1}$	$F_0 = \dfrac{MS_C}{MS_E}$
AB	SS_{AB}	$(a-1)(b-1)$	MS_{AB}	$\sigma^2 + \dfrac{cn\sum\sum(\tau\beta)_{ij}^2}{(a-1)(b-1)}$	$F_0 = \dfrac{MS_{AB}}{MS_E}$
AC	SS_{AC}	$(a-1)(c-1)$	MS_{AC}	$\sigma^2 + \dfrac{bn\sum\sum(\tau\gamma)_{ik}^2}{(a-1)(c-1)}$	$F_0 = \dfrac{MS_{AC}}{MS_E}$
BC	SS_{BC}	$(b-1)(c-1)$	MS_{BC}	$\sigma^2 + \dfrac{an\sum\sum(\beta\gamma)_{jk}^2}{(b-1)(c-1)}$	$F_0 = \dfrac{MS_{BC}}{MS_E}$
ABC	SS_{ABC}	$(a-1)(b-1)(c-1)$	MS_{ABC}	$\sigma^2 + \dfrac{n\sum\sum\sum(\tau\beta\gamma)_{ijk}^2}{(a-1)(b-1)(c-1)}$	$F_0 = \dfrac{MS_{ABC}}{MS_E}$
Error	SS_E	$abc(n-1)$	MS_E	σ^2	
Total	SS_T	$abcn-1$			

Usually, the analysis of variance computations would be done using a statistics software package. However, manual computing formulas for the sums of squares in Table 5.12 are occasionally useful. The total sum of squares is found in the usual way as

$$SS_T = \sum_{i=1}^{a}\sum_{j=1}^{b}\sum_{k=1}^{c}\sum_{l=1}^{n} y_{ijkl}^2 - \frac{y_{....}^2}{abcn} \tag{5.24}$$

The sums of squares for the main effects are found from the totals for factors $A(y_{i...})$, $B(y_{.j..})$, and $C(y_{..k.})$ as follows:

$$SS_A = \frac{1}{bcn}\sum_{i=1}^{a} y_{i...}^2 - \frac{y_{....}^2}{abcn} \tag{5.25}$$

$$SS_B = \frac{1}{acn}\sum_{j=1}^{b} y_{.j..}^2 - \frac{y_{....}^2}{abcn} \tag{5.26}$$

$$SS_C = \frac{1}{abn}\sum_{k=1}^{c} y_{..k.}^2 - \frac{y_{....}^2}{abcn} \tag{5.27}$$

To compute the two-factor interaction sums of squares, the totals for the $A \times B, A \times C$, and $B \times C$ cells are needed. It is frequently helpful to collapse the original data table into three two-way tables to compute these quantities. The sums of squares are found from

$$\begin{aligned} SS_{AB} &= \frac{1}{cn}\sum_{i=1}^{a}\sum_{j=1}^{b} y_{ij..}^2 - \frac{y_{....}^2}{abcn} - SS_A - SS_B \\ &= SS_{\text{Subtotals}(AB)} - SS_A - SS_B \end{aligned} \tag{5.28}$$

$$\begin{aligned} SS_{AC} &= \frac{1}{bn}\sum_{i=1}^{a}\sum_{k=1}^{c} y_{i.k.}^2 - \frac{y_{....}^2}{abcn} - SS_A - SS_C \\ &= SS_{\text{Subtotals}(AC)} - SS_A - SS_C \end{aligned} \tag{5.29}$$

and

$$\begin{aligned} SS_{BC} &= \frac{1}{an}\sum_{j=1}^{b}\sum_{k=1}^{c} y_{.jk.}^2 - \frac{y_{....}^2}{abcn} - SS_B - SS_C \\ &= SS_{\text{Subtotals}(BC)} - SS_B - SS_C \end{aligned} \tag{5.30}$$

Note that the sums of squares for the two-factor subtotals are found from the totals in each two-way table. The three-factor interaction sum of squares is computed from the three-way cell totals $\{y_{ijk.}\}$ as

$$SS_{ABC} = \frac{1}{n}\sum_{i=1}^{a}\sum_{j=1}^{b}\sum_{k=1}^{c} y_{ijk.}^2 - \frac{y_{....}^2}{abcn} - SS_A - SS_B - SS_C - SS_{AB} - SS_{AC} - SS_{BC} \tag{5.31a}$$

$$= SS_{\text{Subtotals}(ABC)} - SS_A - SS_B - SS_C - SS_{AB} - SS_{AC} - SS_{BC} \tag{5.31b}$$

The error sum of squares may be found by subtracting the sum of squares for each main effect and interaction from the total sum of squares or by

$$SS_E = SS_T - SS_{\text{Subtotals}(ABC)} \tag{5.32}$$

EXAMPLE 5.3 The Soft Drink Bottling Problem

A soft drink bottler is interested in obtaining more uniform fill heights in the bottles produced by his manufacturing process. The filling machine theoretically fills each bottle to the correct target height, but in practice, there is variation around this target, and the bottler would like to understand the sources of this variability better and eventually reduce it.

The process engineer can control three variables during the filling process: the percent carbonation (A), the operating pressure in the filler (B), and the bottles produced per minute or the line speed (C). The pressure and speed are easy to control, but the percent carbonation is more difficult to control during actual manufacturing because it varies with product temperature. However, for purposes of an experiment, the engineer can control carbonation at three levels: 10, 12, and 14 percent. She chooses two levels for pressure (25 and 30 psi) and two levels for line speed (200 and 250 bpm). She decides to run two replicates of a factorial design in these three factors, with all 24 runs taken in random order. The response variable observed is the average deviation from the target fill height observed in a production run of bottles at each set of conditions. The data that resulted from this experiment are shown in Table 5.13. Positive deviations are fill heights above the target, whereas negative deviations are fill heights below the target. The circled numbers in Table 5.13 are the three-way cell totals $y_{ijk.}$

The total corrected sum of squares is found from Equation 5.24 as

$$SS_T = \sum_{i=1}^{a}\sum_{j=1}^{b}\sum_{k=1}^{c}\sum_{l=1}^{n} y_{ijkl}^2 - \frac{y_{....}^2}{abcn}$$

$$= 571 - \frac{(75)^2}{24} = 336.625$$

■ **TABLE 5.13**
Fill Height Deviation Data for Example 5.3

	Operating Pressure (B)								
	25 psi				**30 psi**				
Percent	**Line Speed (C)**				**Line Speed (C)**				
Carbonation (A)	**200**		**250**		**200**		**250**		$y_{i...}$
10	−3, −1	−4	−1, 0	−1	−1, 0	−1	1, 1	2	−4
12	0, 1	1	2, 1	3	2, 3	5	6, 5	11	20
14	5, 4	9	7, 6	13	7, 9	16	10, 11	21	59
$B \times C$ Totals $y_{.jk.}$	6		15		20		34		$75 = y$
$y_{.j..}$	21				54				

$A \times B$ Totals $y_{ij..}$

A \ B	25	30
10	−5	1
12	4	16
14	22	37

$A \times C$ Totals $y_{i.k.}$

A \ C	200	250
10	−5	1
12	6	14
14	25	34

and the sums of squares for the main effects are calculated from Equations 5.25, 5.26, and 5.27 as

$$SS_{\text{Carbonation}} = \frac{1}{bcn}\sum_{i=1}^{a} y_{i...}^2 - \frac{y_{....}^2}{abcn}$$

$$= \frac{1}{8}[(-4)^2 + (20)^2 + (59)^2] - \frac{(75)^2}{24} = 252.750$$

$$SS_{\text{Pressure}} = \frac{1}{acn}\sum_{j=1}^{b} y_{.j..}^2 - \frac{y_{....}^2}{abcn}$$

$$= \frac{1}{12}[(21)^2 + (54)^2] - \frac{(75)^2}{24} = 45.375$$

and

$$SS_{\text{Speed}} = \frac{1}{abn}\sum_{k=1}^{c} y_{..k.}^2 - \frac{y_{....}^2}{abcn}$$

$$= \frac{1}{12}[(26)^2 + (49)^2] - \frac{(75)^2}{24} = 22.042$$

To calculate the sums of squares for the two-factor interactions, we must find the two-way cell totals. For example, to find the carbonation–pressure or AB interaction, we need the totals for the $A \times B$ cells $\{y_{ij..}\}$ shown in Table 5.13. Using Equation 5.28, we find the sums of squares as

$$SS_{AB} = \frac{1}{cn}\sum_{i=1}^{a}\sum_{j=1}^{b} y_{ij..}^2 - \frac{y_{....}^2}{abcn} - SS_A - SS_B$$

$$= \frac{1}{4}[(-5)^2 + (1)^2 + (4)^2 + (16)^2 + (22)^2 + (37)^2]$$

$$-\frac{(75)^2}{24} - 252.750 - 45.375$$

$$= 5.250$$

The carbonation–speed or AC interaction uses the $A \times C$ cell totals $\{y_{i.k.}\}$ shown in Table 5.13 and Equation 5.29:

$$SS_{AC} = \frac{1}{bn}\sum_{i=1}^{a}\sum_{k=1}^{c} y_{i.k.}^2 - \frac{y_{....}^2}{abcn} - SS_A - SS_C$$

$$= \frac{1}{4}[(-5)^2 + (1)^2 + (6)^2 + (14)^2 + (25)^2 + (34)^2]$$

$$-\frac{(75)^2}{24} - 252.750 - 22.042$$

$$= 0.583$$

The pressure–speed or BC interaction is found from the $B \times C$ cell totals $\{y_{.jk.}\}$ shown in Table 5.13 and Equation 5.30:

$$SS_{BC} = \frac{1}{an}\sum_{j=1}^{b}\sum_{k=1}^{c} y_{.jk.}^2 - \frac{y_{....}^2}{abcn} - SS_B - SS_C$$

$$= \frac{1}{6}[(6)^2 + (15)^2 + (20)^2 + (34)^2] - \frac{(75)^2}{24}$$

$$-45.375 - 22.042$$

$$= 1.042$$

The three-factor interaction sum of squares is found from the $A \times B \times C$ cell totals $\{y_{ijk.}\}$, which are circled in Table 5.13. From Equation 5.31a, we find

$$SS_{ABC} = \frac{1}{n}\sum_{i=1}^{a}\sum_{j=1}^{b}\sum_{k=1}^{c} y_{ijk.}^2 - \frac{y_{....}^2}{abcn} - SS_A - SS_B - SS_C$$

$$-SS_{AB} - SS_{AC} - SS_{BC}$$

$$= \frac{1}{2}[(-4)^2 + (-1)^2 + (-1)^2 + \cdots + (16)^2 + (21)^2]$$

$$-\frac{(75)^2}{24} - 252.750 - 45.375 - 22.042$$

$$-5.250 - 0.583 - 1.042$$

$$= 1.083$$

Finally, noting that

$$SS_{\text{Subtotals}(ABC)} = \frac{1}{n}\sum_{i=1}^{a}\sum_{j=1}^{b}\sum_{k=1}^{c} y_{ijk.}^2 - \frac{y_{....}^2}{abcn} = 328.125$$

we have

$$SS_E = SS_T - SS_{\text{Subtotals}(ABC)}$$

$$= 336.625 - 328.125$$

$$= 8.500$$

The ANOVA is summarized in Table 5.14. We see that the percentage of carbonation, operating pressure, and line speed significantly affect the fill volume. The carbonation–pressure interaction F ratio has a P-value of 0.0558, indicating some interaction between these factors.

The next step should be an analysis of the residuals from this experiment. We leave this as an exercise for the reader but point out that a normal probability plot of the residuals and the other usual diagnostics do not indicate any major concerns.

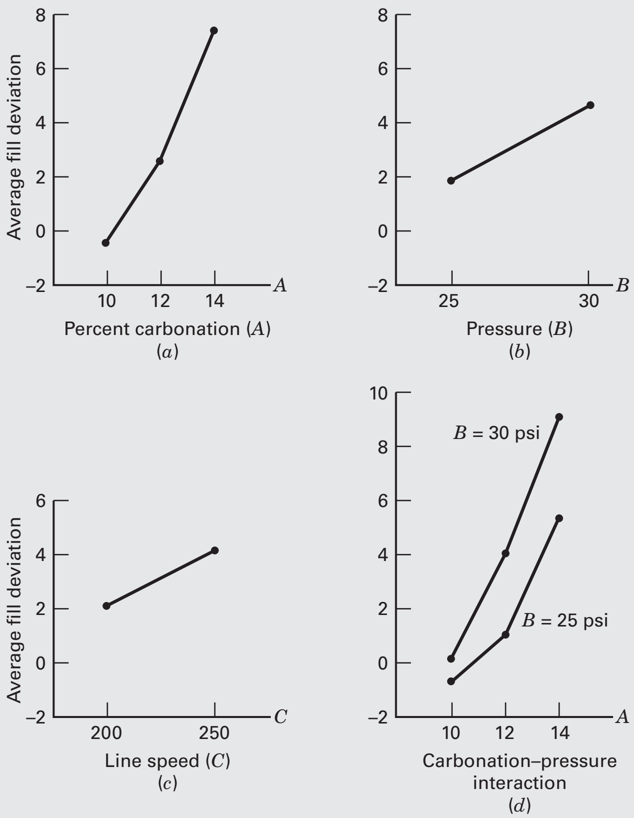
To assist in the practical interpretation of this experiment, Figure 5.16 presents plots of the three main effects and the AB (carbonation–pressure) interaction. The main effect plots are just graphs of the marginal response averages at the levels of the three factors. Notice that all three variables have *positive* main effects; that is, increasing the variable moves the average deviation from the fill target upward. The interaction between carbonation and pressure is fairly small, as shown by the similar shape of the two curves in Figure 5.16*d*.

Because the company wants the average deviation from the fill target to be close to zero, the engineer decides to recommend the low level of operating pressure (25 psi) and the high level of line speed (250 bpm, which will maximize the production rate). Figure 5.17 plots the average observed deviation from the target fill height at the three different carbonation levels for this set of operating conditions.

■ **TABLE 5.14**
Analysis of Variance for Example 5.3

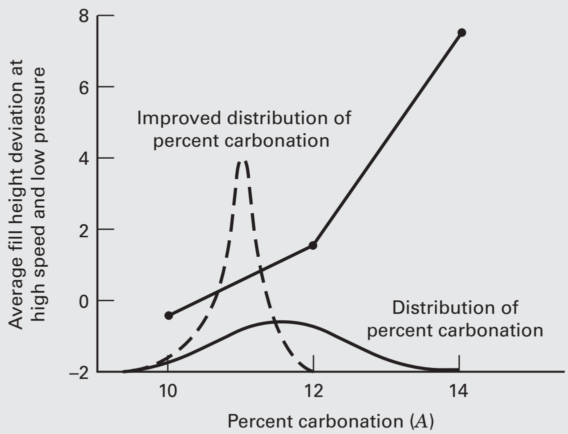
Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	*P*-Value
Percent carbonation (*A*)	252.750	2	126.375	178.412	<0.0001
Operating pressure (*B*)	45.375	1	45.375	64.059	<0.0001
Line speed (*C*)	22.042	1	22.042	31.118	0.0001
AB	5.250	2	2.625	3.706	0.0558
AC	0.583	2	0.292	0.412	0.6713
BC	1.042	1	1.042	1.471	0.2485
ABC	1.083	2	0.542	0.765	0.4867
Error	8.500	12	0.708		
Total	336.625	23			

Now the carbonation level cannot presently be perfectly controlled in the manufacturing process, and the normal distribution shown with the solid curve in Figure 5.17 approximates the variability in the carbonation levels presently experienced. As the process is impacted by the values of the carbonation level drawn from this distribution, the fill heights will fluctuate considerably. This variability in the fill heights could be reduced if the distribution of the carbonation level values followed the normal distribution shown with the dashed line in Figure 5.17. Reducing the standard deviation of the carbonation level distribution was ultimately achieved by improving temperature control during manufacturing.



■ **FIGURE 5.16 Main effects and interaction plots for Example 5.3. (*a*) Percentage of carbonation (*A*). (*b*) Pressure (*B*). (*c*) Line speed (*C*). (*d*) Carbonation–pressure interaction**

■ **FIGURE 5.17 Average fill height deviation at high speed and low pressure for different carbonation levels**



We have indicated that if all the factors in a factorial experiment are fixed, test statistic construction is straightforward. The statistic for testing any main effect or interaction is always formed by dividing the mean square for the main effect or interaction by the mean square error. However, if the factorial experiment involves one or more **random factors**, the test statistic construction is not always done this way. We must examine the expected mean squares to determine the correct tests. We defer a complete discussion of experiments with random factors until Chapter 13.

5.5 Fitting Response Curves and Surfaces

The ANOVA always treats all of the factors in the experiment as if they were qualitative or categorical. However, many experiments involve at least one quantitative factor. It can be useful to fit a **response curve** to the levels of a quantitative factor so that the experimenter has an equation that relates the response to the factor. This equation might be used for interpolation, that is, for predicting the response at factor levels between those actually used in the experiment. When at least two factors are quantitative, we can fit a **response surface** for predicting y at various combinations of the design factors. In general, **linear regression methods** are used to fit these models to the experimental data. We illustrated this procedure in Section 3.5.1 for an experiment with a single factor. We now present two examples involving factorial experiments. In both examples, we will use a computer software package to generate the regression models. For more information about regression analysis, refer to Chapter 10 and the supplemental text material for this chapter.

EXAMPLE 5.4

Consider the battery life experiment described in Example 5.1. The factor temperature is quantitative, and the material type is qualitative. Furthermore, there are three levels of temperature. Consequently, we can compute a linear and a quadratic temperature effect to study how temperature affects the battery life. Table 5.15 presents condensed output from Design-Expert for this experiment and assumes that temperature is quantitative and material type is qualitative.

The ANOVA in Table 5.15 shows that the "model" source of variability has been subdivided into several components. The components "A" and "A^2" represent the linear and quadratic effects of temperature, and "B" represents the main effect of the material type factor. Recall that material type is a qualitative factor with three levels. The terms "AB" and "A^2B" are the interactions of the linear and quadratic temperature factor with material type.

■ **TABLE 5.15**
Design-Expert Output for Example 5.4

Response: Life In Hours

ANOVA for Response Surface Reduced Cubic Model

Analysis of Variance Table [Partial Sum of Squares]

Source	Sum of Squares	DF	Mean Square	F Value	Prob > F	
Model	59416.22	8	7427.03	11.00	<0.0001	significant
A	*39042.67*	*1*	*39042.67*	*57.82*	<0.0001	
B	*10683.72*	*2*	*5341.86*	*7.91*	*0.0020*	
A^2	*76.06*	*1*	*76.06*	*0.11*	*0.7398*	
AB	*2315.08*	*2*	*1157.54*	*1.71*	*0.1991*	
A^2B	*7298.69*	*2*	*3649.35*	*5.40*	*0.0106*	
Residual	18230.75	27	675.21			
Lack of Fit	*0.000*	*0*				
Pure Error	*18230.75*	*27*	*675.21*			
Cor Total	77646.97	35				

Std. Dev.	25.98	R-Squared	0.7652
Mean	105.53	Adj R-Squared	0.6956
C.V.	24.62	Pred R-Squared	0.5826
PRESS	32410.22	Adeq Precision	8.178

Term	Coefficient Estimate	DF	Standard Error	95% Cl Low	95% Cl High	VIF
Intercept	107.58	1	7.50	92.19	122.97	
A-Temp	−40.33	1	5.30	−51.22	−29.45	1.00
B[1]	−50.33	1	10.61	−72.10	−28.57	
B[2]	12.17	1	10.61	−9.60	33.93	
A^2	−3.08	1	9.19	−21.93	15.77	1.00
AB[1]	1.71	1	7.50	−13.68	17.10	
AB[2]	−12.79	1	7.50	−28.18	2.60	
A^2B[1]	41.96	1	12.99	15.30	68.62	
A^2B[2]	−14.04	1	12.99	−40.70	12.62	

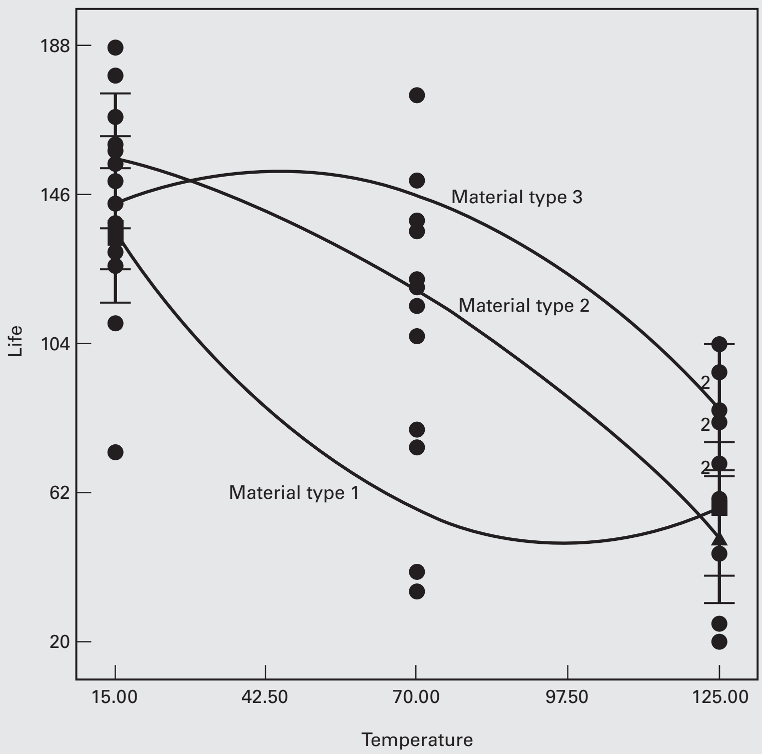
Final Equation in Terms of Coded Factors:

$$
\begin{aligned}
\text{Life} = {} & \\
& +107.58 \\
& -40.33 \quad *A \\
& -50.33 \quad *B[1] \\
& +12.17 \quad *B[2] \\
& -3.08 \quad *A^2 \\
& +1.71 \quad *AB[1] \\
& -12.79 \quad *AB[2] \\
& +41.96 \quad *A^2B[1] \\
& -14.04 \quad *A^2[2]
\end{aligned}
$$

■ **TABLE 5.15** ***(Continued)***

Final Equation in Terms of Actual Factors:

Material Type	1
Life =	
+169.38017	
−2.50145	$*$Temp
+0.012851	$*$Temp^2
Material Type	2
Life =	
+159.62397	
−0.17335	$*$Temp
+0.41627	$*$Temp^2
Material Type	3
Life =	
+132.76240	
+0.90289	$*$Temp
−0.01248	$*$Temp^2



■ **FIGURE 5.18** **Predicted life as a function of temperature for the three material types, Example 5.4**

The P-values indicate that A^2 and AB are not significant, whereas the A^2B term is significant. Often we think about removing nonsignificant terms or factors from a model, but in this case, removing A^2 and AB and retaining A^2B will result in a model that is not **hierarchical**. The **hierarchy principle** indicates that if a model contains a high-order term (such as A^2B), it should also contain all of the lower order terms that compose it (in this case A^2 and AB). Hierarchy promotes a type of internal consistency in a model, and many statistical model builders rigorously follow the principle. However, hierarchy is not always a good idea, and many models actually work better as prediction equations without including the nonsignificant terms that promote hierarchy. For more information, see the supplemental text material for this chapter.

The computer output also gives model coefficient estimates and a final prediction equation for battery life in coded factors. In this equation, the levels of temperature are $A = -1, 0, +1$, respectively, when temperature is at the low, middle, and high levels (15, 70, and 125°C). The variables $B[1]$ and $B[2]$ are coded **indicator variables** that are defined as follows:

	Material Type 1	2	3
$B[1]$	1	0	−1
$B[2]$	0	1	−1

There are also prediction equations for battery life in terms of the actual factor levels. Notice that because material type is a qualitative factor there is an equation for predicted life as a function of temperature for each material type. Figure 5.18 shows the response curves generated by these three prediction equations. Compare them to the two-factor interaction graph for this experiment in Figure 5.9.

If several factors in a factorial experiment are quantitative a **response surface** may be used to model the relationship between y and the design factors. Furthermore, the quantitative factor effects may be represented by single-degree-of-freedom polynomial effects. Similarly, the interactions of quantitative factors can be partitioned into single-degree-of-freedom components of interaction. This is illustrated in the following Example 5.5.

EXAMPLE 5.5

The effective life of a cutting tool installed in a numerically controlled machine is thought to be affected by the cutting speed and the tool angle. Three speeds and three angles are selected, and a 3^2 factorial experiment with two replicates is performed. The coded data are shown in Table 5.16. The circled numbers in the cells are the cell totals $\{y_{ij.}\}$.

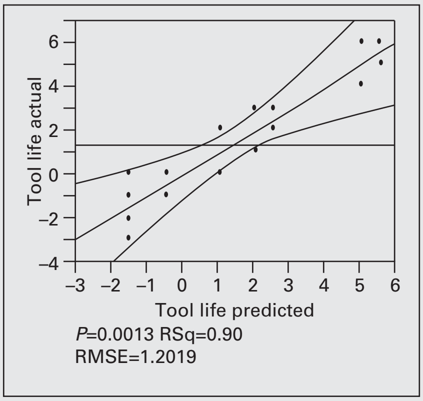
Table 5.17 shows the JMP output for this experiment. This is a classical ANOVA, treating both factors as categorical. Notice that design factors tool angle and speed as well

■ **TABLE 5.16**
Data for Tool Life Experiment

Total Angle (degrees)	Cutting Speed (in/min) 125		150		175		$y_{i..}$
15	−2, −1	−3	−3, 0	−3	2, 3	5	−1
20	0, 2	2	1, 3	4	4, 6	10	16
25	−1, 0	−1	5, 6	11	0, −1	−1	9
$y_{.j.}$	−2		12		14		$24 = y_{...}$

■ **TABLE 5.17**
JMP ANOVA for the Tool Life Experiment in Example 5.5

Response Tool Life
Whole Model
Actual by Predicted Plot



Summary of Fit

RSquare	0.895161
RSquare Adj	0.801971
Root Mean Square Error	1.20185
Mean of Response	1.333333
Observations (or Sum Wgts)	18

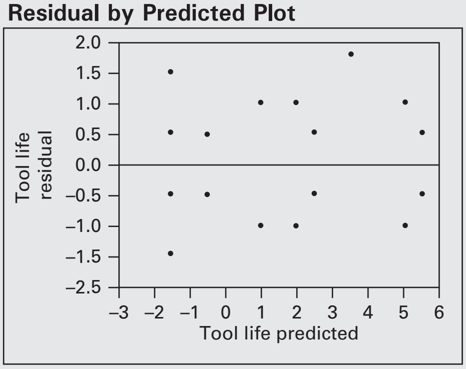
Analysis of Variance

Source	DF	Sum of Squares	Mean Square	*F* Ratio
Model	8	111.00000	13.8750	9.6058
Error	9	13.00000	1.4444	Prob > *F*
C. Total	17	124.00000		0.0013

Effect Tests

Source	Nparm	DF	Sum of Squares	*F* Ratio	Prob > *F*
Angle	2	2	24.333333	8.4231	0.0087
Speed	2	2	25.333333	8.7692	0.0077
Angle*Speed	4	4	61.333333	10.6154	0.0018

Residual by Predicted Plot



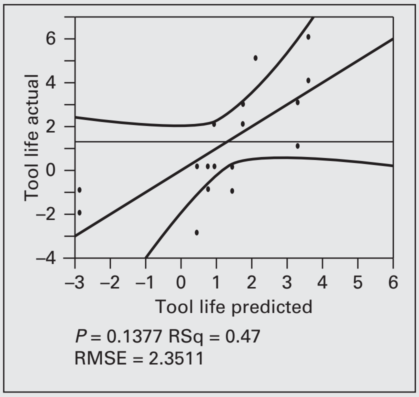
as the angle–speed interaction are significant. Since the factors are quantitative, and both factors have three levels, a **second-order model** such as

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_{12} x_1 x_2 + \beta_{11} x_1^2 + \beta_{22} x_2^2 + \epsilon$$

where x_1 = angle and x_2 = speed could also be fit to the data. The JMP output for this model is shown in Table 5.18. Notice that JMP "centers" the predictors when forming the interaction and quadratic model terms. The second-order model doesn't look like a very good fit to the data; the value of R^2 is only 0.465 (compared to $R^2 = 0.895$ in the categorical variable ANOVA) and the only significant factor is the linear term in speed for which the P-value is 0.0731. Notice that the mean square for error in the second-order model fit is 5.5278, considerably larger than it was in the classical categorical variable ANOVA of Table 5.17. The JMP output in Table 5.18 shows the **prediction profiler**, a graphical display showing the response variable life as a function of each design factor, angle and speed. The prediction profiler is very useful for optimization. Here it has been set to the levels of angle and speed that result in maximum predicted life.

■ **TABLE 5.18**
JMP Output for the Second-Order Model, Example 5.5

Response Tool Life
Actual by Predicted Plot



Summary of Fit

RSquare	0.465054
RSquare Adj	0.242159
Root Mean Square Error	2.351123
Mean of Response	1.333333
Observations (or Sum Wgts)	18

Analysis of Variance

Source	DF	Sum of Squares	Mean Square	F Ratio
Model	5	57.66667	11.5333	2.0864
Error	12	66.33333	5.5278	Prob > F
C. Total	17	124.00000		0.1377

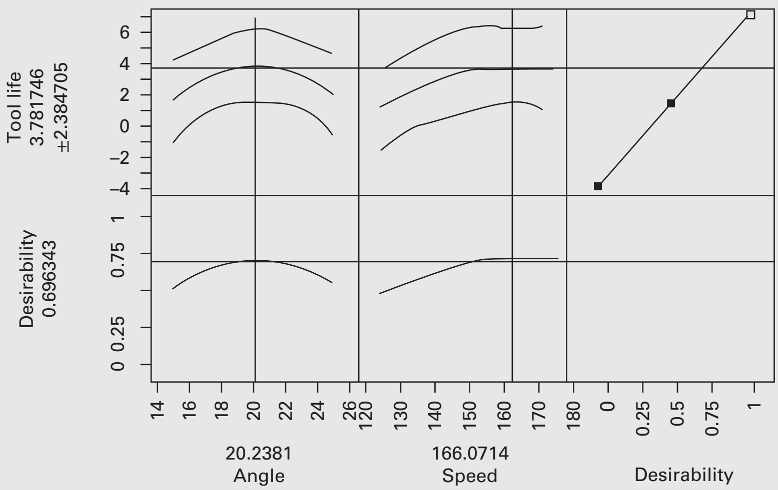
Parameter Estimates

Term	Estimate	Std. Error	t Ratio	Prob > $\|t\|$
Intercept	−8	5.048683	−1.58	0.1390
Angle	0.1666667	0.135742	1.23	0.2431
Speed	0.0533333	0.027148	1.96	0.0731

■ **TABLE 5.18** (*Continued*)

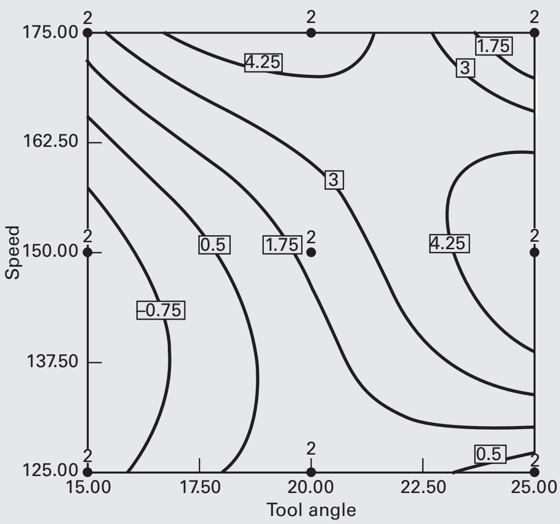
(Angle-20)*(Speed-150)	−0.008	0.00665	−1.20	0.2522
(Angle-20)*(Angle-20)	−0.08	0.047022	−1.70	0.1146
(Speed-150)*(Speed-150)	−0.0016	0.001881	−0.85	0.4116

Prediction Profiler

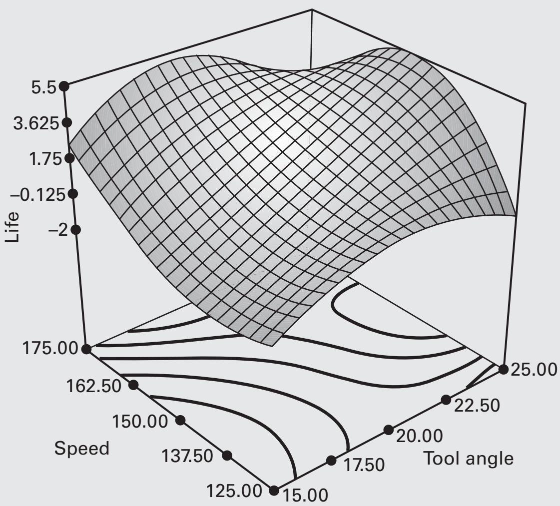


Part of the reason for the relatively poor fit of the second-order model is that only one of the four degrees of freedom for interaction are accounted for in this model. In addition to the term $\beta_{12}x_1x_2$, there are three other terms that could be fit to completely account for the four degrees of freedom for interaction, namely $\beta_{112}x_1^2x_2$, $\beta_{122}x_1x_2^2$, and $\beta_{1122}x_1^2x_2^2$.

JMP output for the second-order model with the additional higher-order terms is shown in Table 5.19. While these higher-order terms are components of the two-factor interaction, the final model is a reduced quartic. Although there are some large *P*-values, all model terms have been retained to ensure hierarchy. The prediction profiler



■ **FIGURE 5.19** **Two-dimensional contour plot of the tool life response surface for Example 5.5**



■ **FIGURE 5.20** **Three-dimensional tool life response surface for Example 5.5** indicates that maximum tool life is achieved around an angle of 25 degrees and speed of 150 in/min.

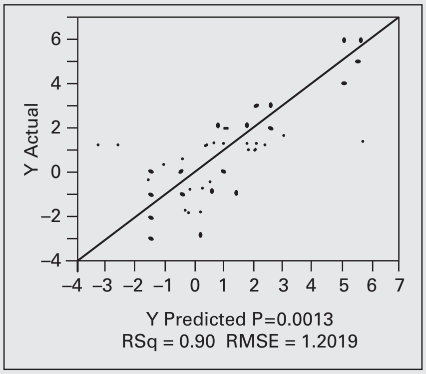
Figure 5.19 is the contour plot of tool life for this model and Figure 5.20 is a three-dimensional response surface plot. These plots confirm the estimate of the optimum operating conditions found from the JMP prediction profiler. Exploration of response surfaces is an important use of designed experiments, which we will discuss in more detail in Chapter 11.

■ **TABLE 5.19**

JMP Output for the Expanded Model in Example 5.5

Response Y

Actual by Predicted Plot



Summary of Fit

RSquare	0.895161
RSquare Adj	0.801971
Root Mean Square Error	1.20185
Mean of Response	1.333333
Observations (or Sum Wgts)	18

Analysis of Variance

Source	**DF**	**Sum of Squares**	**Mean Square**	***F* Ratio**
Model	8	111.00000	13.8750	9.6058
Error	9	13.00000	1.4444	**Prob > *F***
C. Total	17	124.00000		0.0013*

Parameter Estimates

Term	**Estimate**	**Std Error**	***t* Ratio**	**Prob > |*t*|**
Intercept	−24	4.41588	−5.43	0.0004*
Angle	0.7	0.120185	5.82	0.0003*
Speed	0.08	0.024037	3.33	0.0088*
(Angle-20)*(Speed-150)	−0.008	0.003399	−2.35	0.0431*
(Angle-20)*(Angle-20)	2.776e-17	0.041633	0.00	1.0000
(Speed-150)*(Speed-150)	0.0016	0.001665	0.96	0.3618
(Angle-20)*(Speed-150)*(Angle-20)	−0.0016	0.001178	−1.36	0.2073
(Speed-150)*(Speed-150)*(Angle-20)	−0.00128	0.000236	−5.43	0.0004*
(Angle-20)*(Speed-150)*(Angle-20)*(Speed-150)	−0.000192	8.158a-5	−2.35	0.0431*

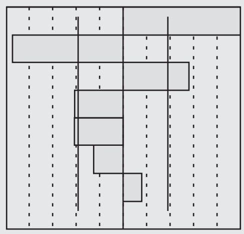
■ **TABLE 5.19** ***(Continued)***

Effect Tests

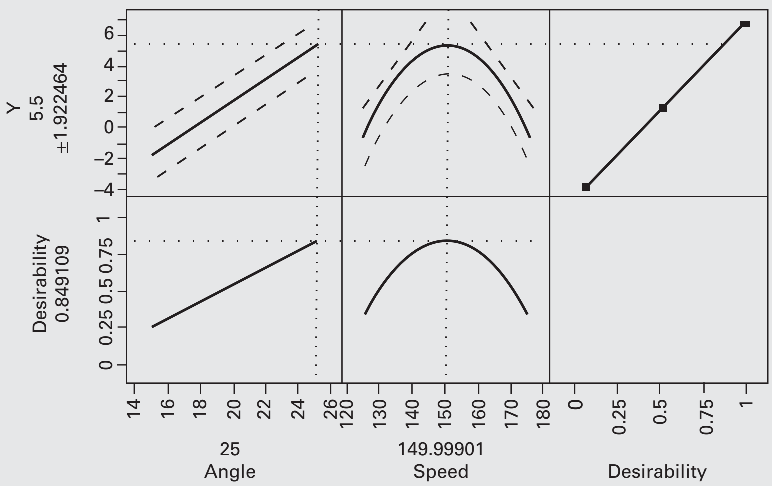
Source	Nparm	DF	Sum of Squares	*F* Ratio	Prob > *F*
Angle	1	1	49.000000	33.9231	0.0003*
Speed	1	1	16.000000	11.0769	0.0088*
Angle*Speed	1	1	8.000000	5.5385	0.0431*
Angle*Angle	1	1	6.4198e-31	0.0000	1.0000
Speed*Speed	1	1	1.333333	0.9231	0.3618
Angle*Speed*Angle	1	1	2.666667	1.8462	0.2073
Speed*Speed*Angle	1	1	42.666667	29.5385	0.0004*
Angle*Speed*Angle*Speed	1	1	8.000000	5.5385	0.0431*

Sorted Parameter Estimates

Term	Estimate	Std Error	*t* Ratio	Prob > |*t*|
Angle	0.7	0.120185	5.82	0.0003*
(Speed-150)*(Speed-150)*(Angle-20)	−0.00128	0.000236	−5.43	0.0004*
Speed	0.08	0.024037	3.33	0.0088*
(Angle-20)*(Speed-150)*(Angle-20)*(Speed-150)	−0.000192	8.158a-5	−2.35	0.0431*
(Angle-20)*(Speed-150)	−0.008	0.003399	−2.35	0.0431*
(Angle-20)*(Speed-150)*(Angle-20)	−0.0016	0.001178	−1.36	0.2073
(Speed-150)*(Speed-150)	0.0016	0.001665	0.96	0.3618
(Angle-20)*(Angle-20)	2.776e-17	0.041633	0.00	1.0000



Prediction Profiler



5.6 Blocking in a Factorial Design

We have discussed factorial designs in the context of a completely randomized experiment. Sometimes, it is not feasible or practical to completely randomize all of the runs in a factorial. For example, the presence of a nuisance factor may require that the experiment be run in blocks. We discussed the basic concepts of blocking in the context of a single-factor experiment in Chapter 4. We now show how blocking can be incorporated in a factorial. Some other aspects of blocking in factorial designs are presented in Chapters 7, 8, 9, and 13.

Consider a factorial experiment with two factors (A and B) and n replicates. The linear statistical model for this design is

$$y_{ijk} = \mu + \tau_i + \beta_j + (\tau\beta)_{ij} + \epsilon_{ijk} \qquad \begin{cases} i = 1, 2, \ldots, a \\ j = 1, 2, \ldots, b \\ k = 1, 2, \ldots, n \end{cases} \tag{5.33}$$

where τ_i, β_j, and $(\tau\beta)_{ij}$ represent the effects of factors A, B, and the AB interaction, respectively. Now suppose that to run this experiment a particular raw material is required. This raw material is available in batches that are not large enough to allow *all abn* treatment combinations to be run from the *same* batch. However, if a batch contains enough material for ab observations, then an alternative design is to run each of the n replicates using a separate batch of raw material. Consequently, the batches of raw material represent a randomization restriction or a **block**, and a single replicate of a complete factorial experiment is run within each block. The effects model for this new design is

$$y_{ijk} = \mu + \tau_i + \beta_j + (\tau\beta)_{ij} + \delta_k + \epsilon_{ijk} \qquad \begin{cases} i = 1, 2, \ldots, a \\ j = 1, 2, \ldots, b \\ k = 1, 2, \ldots, n \end{cases} \tag{5.34}$$

where δ_k is the effect of the kth block. Of course, within a block the order in which the treatment combinations are run is completely randomized.

The model (Equation 5.34) assumes that interaction between blocks and treatments is negligible. This was assumed previously in the analysis of randomized block designs. If these interactions do exist, they cannot be separated from the error component. In fact, the error term in this model really consists of the $(\tau\delta)_{ik}$, $(\beta\delta)_{jk}$, and $(\tau\beta\delta)_{ijk}$ interactions. The ANOVA is outlined in Table 5.20. The layout closely resembles that of a factorial design, with the

■ TABLE 5.20
Analysis of Variance for a Two-Factor Factorial in a Randomized Complete Block

Source of Variation	Sum of Squares	Degrees of Freedom	Expected Mean Square	F_0
Blocks	$\frac{1}{ab}\sum_k y_{..k}^2 - \frac{y_{...}^2}{abn}$	$n-1$	$\sigma^2 + ab\sigma_\delta^2$	
A	$\frac{1}{bn}\sum_i y_{i..}^2 - \frac{y_{...}^2}{abn}$	$a-1$	$\sigma^2 + \frac{bn\sum \tau_i^2}{a-1}$	$\frac{MS_A}{MS_E}$
B	$\frac{1}{an}\sum_j y_{.j.}^2 - \frac{y_{...}^2}{abn}$	$b-1$	$\sigma^2 + \frac{an\sum \beta_j^2}{b-1}$	$\frac{MS_B}{MS_E}$
AB	$\frac{1}{n}\sum_i\sum_j y_{ij.}^2 - \frac{y_{...}^2}{abn} - SS_A - SS_B$	$(a-1)(b-1)$	$\sigma^2 + \frac{n\sum\sum(\tau\beta)_{ij}^2}{(a-1)(b-1)}$	$\frac{MS_{AB}}{MS_E}$
Error	Subtraction	$(ab-1)(n-1)$	σ^2	
Total	$\sum_i\sum_j\sum_k y_{ijk}^2 - \frac{y_{...}^2}{abn}$	$abn-1$		

error sum of squares reduced by the sum of squares for blocks. Computationally, we find the sum of squares for blocks as the sum of squares between the n block totals $\{y_{..k}\}$. The ANOVA in Table 5.20 assumes that both factors are fixed and that blocks are random. The ANOVA estimator of the variance component for blocks σ_δ^2, is

$$\sigma_\delta^2 = \frac{MS_{Blocks} - MS_E}{ab}$$

In the previous example, the randomization was restricted to within a batch of raw material. In practice, a variety of phenomena may cause randomization restrictions, such as time and operators. For example, if we could not run the entire factorial experiment on one day, then the experimenter could run a complete replicate on day 1, a second replicate on day 2, and so on. Consequently, each day would be a block.

EXAMPLE 5.6

An engineer is studying methods for improving the ability to detect targets on a radar scope. Two factors she considers to be important are the amount of background noise, or "ground clutter," on the scope and the type of filter placed over the screen. An experiment is designed using three levels of ground clutter and two filter types. We will consider these as fixed-type factors. The experiment is performed by randomly selecting a treatment combination (ground clutter level and filter type) and then introducing a signal representing the target into the scope. The intensity of this target is increased until the operator observes it. The intensity level at detection is then measured as the response variable. Because of operator availability, it is convenient to select an operator and keep him or her at the scope until all the necessary runs have been made. Furthermore, operators differ in their skill and ability to use the scope. Consequently, it seems logical to use the operators as blocks. Four operators are randomly selected. Once an operator is chosen, the order in which the six treatment combinations are run is randomly determined. Thus, we have a 3×2 factorial experiment run in a randomized complete block. The data are shown in Table 5.21.

The linear model for this experiment is

$$y_{ijk} = \mu + \tau_i + \beta_j + (\tau\beta)_{ij} + \delta_k + \epsilon_{ijk} \quad \begin{cases} i = 1, 2, 3 \\ j = 1, 2 \\ k = 1, 2, 3, 4 \end{cases}$$

where τ_i represents the ground clutter effect, β_j represents the filter type effect, $(\tau\beta)_{ij}$ is the interaction, δ_k is the block effect, and ϵ_{ijk} is the NID$(0, \sigma^2)$ error component. The sums of squares for ground clutter, filter type, and their interaction are computed in the usual manner. The sum of squares due to blocks is found from the operator totals $\{y_{..k}\}$ as follows:

$$\begin{aligned} SS_{\text{Blocks}} &= \frac{1}{ab}\sum_{k=1}^{n} y_{..k}^2 - \frac{y_{...}^2}{abn} \\ &= \frac{1}{(3)(2)}[(572)^2 + (579)^2 + (597)^2 + (530)^2] \\ &\quad - \frac{(2278)^2}{(3)(2)(4)} \\ &= 402.17 \end{aligned}$$

■ **TABLE 5.21**
Intensity Level at Target Detection

Operators (blocks)	**1**		**2**		**3**		**4**	
Filter Type	**1**	**2**	**1**	**2**	**1**	**2**	**1**	**2**
Ground clutter								
Low	90	86	96	84	100	92	92	81
Medium	102	87	106	90	105	97	96	80
High	114	93	112	91	108	95	98	83

■ **TABLE 5.22**
Analysis of Variance for Example 5.6

Source of Variation	Sum of Square	Degrees of Freedom	Mean Squares	F_0	P-Value
Ground clutter (G)	335.58	2	167.79	15.13	0.0003
Filter type (F)	1066.67	1	1066.67	96.19	<0.0001
GF	77.08	2	38.54	3.48	0.0573
Blocks	402.17	3	134.06		
Error	166.33	15	11.09		
Total	2047.83	23			

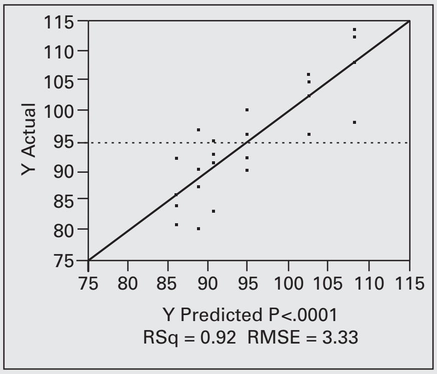
The complete ANOVA for this experiment is summarized in Table 5.22. The presentation in Table 5.22 indicates that all effects are tested by dividing their mean squares by the mean square error. Both ground clutter level and filter type are significant at the 1 percent level, whereas their interaction is significant only at the 10 percent level. Thus, we conclude that both ground clutter level and the type of scope filter used affect the operator's ability to detect the target, and there is some evidence of mild interaction between these factors. The ANOVA estimate of the variance component for blocks is

$$\hat{\sigma}_\delta^2 = \frac{MS_{Blocks} - MS_E}{ab} = \frac{134.06 - 11.09}{(3)(2)} = 20.50$$

The JMP output for this experiment is shown in Table 5.23. The residual maximum likelihood (REML) estimate of the variance component for blocks is shown in this output, and because this is a balanced design, the REML and ANOVA estimates agree. JMP also provides the confidence intervals on both variance components σ^2 and σ_δ^2.

■ **TABLE 5.23**
JMP Output for Example 5.6

Whole Model
Actual by Predicted Plot



Summary of Fit

RSquare	0.917432
RSquare Adj	0.894497
Root Mean Square Error	3.329998
Mean of Response	94.91667
Observations (or Sum Wgts)	24

■ **TABLE 5.23** (*Continued*)

REML Variance Component Estimates

Random Effect	Var Ratio	Var Component	Std Error	95% Lower	95% Upper	Pct of Total
Operators (Blocks)	1.8481964	20.494444	18.255128	−15.28495	56.273839	64.890
Residual		11.088889	4.0490897	6.0510389	26.561749	35.110
Total		31.583333				100.000

−2 LogLikelihood = 118.73680261

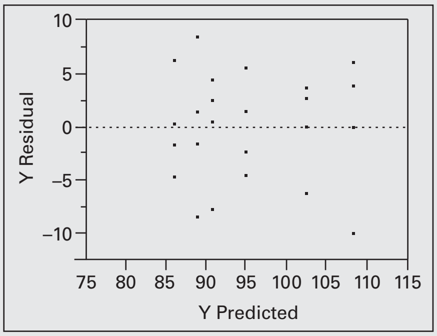
Covariance Matrix of
Variance Component Estimates

Random Effect	Operators (Blocks)	Residual
Operators (Blocks)	333.24972	−2.732521
Residual	−2.732521	16.395128

Fixed Effect Tests

Source	Nparm	DF	DFDen	*F* Ratio	Prob > *F*
Clutter	2	2	15	15.1315	0.0003*
Filter Type	1	1	15	96.1924	<.0001*
Clutter*Filter Type	2	2	15	3.4757	0.0575

Residual by Predicted Plot



In the case of two randomization restrictions, each with p levels, if the number of treatment combinations in a k-factor factorial design exactly equals the number of restriction levels, that is, if $p = ab \ldots m$, then the factorial design may be run in a $p \times p$ Latin square. For example, consider a modification of the radar target detection experiment of Example 5.6. The factors in this experiment are filter type (two levels) and ground clutter (three levels), and operators

■ **TABLE 5.24**
Radar Detection Experiment Run in a 6 × 6 Latin Square

	Operator					
Day	**1**	**2**	**3**	**4**	**5**	**6**
1	$A(f_1g_1 = 90)$	$B(f_1g_2 = 106)$	$C(f_1g_3 = 108)$	$D(f_2g_1 = 81)$	$F(f_2g_3 = 90)$	$E(f_2g_2 = 88)$
2	$C(f_1g_3 = 114)$	$A(f_1g_1 = 96)$	$B(f_1g_2 = 105)$	$F(f_2g_3 = 83)$	$E(f_2g_2 = 86)$	$D(f_2g_1 = 84)$
3	$B(f_1g_2 = 102)$	$E(f_2g_2 = 90)$	$G(f_2g_3 = 95)$	$A(f_1g_1 = 92)$	$D(f_2g_1 = 85)$	$C(f_1g_3 = 104)$
4	$E(f_2g_2 = 87)$	$D(f_2g_1 = 84)$	$A(f_1g_1 = 100)$	$B(f_1g_2 = 96)$	$C(f_1g_3 = 110)$	$F(f_2g_3 = 91)$
5	$F(f_2g_3 = 93)$	$C(f_1g_3 = 112)$	$D(f_2g_1 = 92)$	$E(f_2g_2 = 80)$	$A(f_1g_1 = 90)$	$B(f_1g_2 = 98)$
6	$D(f_2g_1 = 86)$	$F(f_2g_3 = 91)$	$E(f_2g_2 = 97)$	$C(f_1g_3 = 98)$	$B(f_1g_2 = 100)$	$A(f_1g_1 = 92)$

are considered as blocks. Suppose now that because of the setup time required, only six runs can be made per day. Thus, days become a second randomization restriction, resulting in the 6 × 6 Latin square design, as shown in Table 5.24. In this table we have used the lowercase letters f_i and g_j to represent the ith and jth levels of filter type and ground clutter, respectively. That is, f_1g_2 represents filter type 1 and medium ground clutter. Note that now six operators are required, rather than four as in the original experiment, so the number of treatment combinations in the 3 × 2 factorial design exactly equals the number of restriction levels. Furthermore, in this design, each operator would be used only once on each day. The Latin letters $A, B, C, D, E,$ and F represent the $3 \times 2 = 6$ factorial treatment combinations as follows: $A = f_1g_1, B = f_1g_2, C = f_1g_3, D = f_2g_1, E = f_2g_2,$ and $F = f_2g_3$.

The five degrees of freedom between the six Latin letters correspond to the main effects of filter type (one degree of freedom), ground clutter (two degrees of freedom), and their interaction (two degrees of freedom). The linear statistical model for this design is

$$y_{ijkl} = \mu + \alpha_i + \tau_j + \beta_k + (\tau\beta)_{jk} + \theta_l + \epsilon_{ijkl} \qquad \begin{cases} i = 1, 2, \ldots, 6 \\ j = 1, 2, 3 \\ k = 1, 2 \\ l = 1, 2, \ldots, 6 \end{cases} \tag{5.35}$$

where τ_j and β_k are effects of ground clutter and filter type, respectively, and α_i and θ_l represent the randomization restrictions of days and operators, respectively. To compute the sums of squares, the following two-way table of treatment totals is helpful:

Ground Clutter	Filter Type 1	Filter Type 2	$y_{.j..}$
Low	560	512	1072
Medium	607	528	1135
High	646	543	1189
$y_{..k.}$	1813	1583	$3396 = y_{....}$

■ **TABLE 5.25**
Analysis of Variance for the Radar Detection Experiment Run as a 3 × 2 Factorial in a Latin Square

Source of Variation	Sum of Squares	Degrees of Freedom	General Formula for Degrees of Freedom	Mean Square	F_0	*P*-Value
Ground clutter (*G*)	571.50	2	$a - 1$	285.75	28.86	<0.0001
Filter type (*F*)	1469.44	1	$b - 1$	1469.44	148.43	<0.0001
GF	126.73	2	$(a - 1)(b - 1)$	63.37	6.40	0.0071
Days (rows)	4.33	5	$ab - 1$	0.87		
Operators (columns)	428.00	5	$ab - 1$	85.60		
Error	198.00	20	$(ab - 1)(ab - 2)$	9.90		
Total	2798.00	35	$(ab)^2 - 1$			

Furthermore, the row and column totals are

Rows ($y_{.jkl}$):	563	568	568	568	565	564
Columns ($y_{ijk.}$):	572	579	597	530	561	557

The ANOVA is summarized in Table 5.25. We have added a column to this table indicating how the number of degrees of freedom for each sum of squares is determined.

5.7 Problems

5.1 An interaction effect in the model from a factorial experiment involving quantitative factors is a way of incorporating curvature into the response surface model representation of the results.

(a) **True**

(b) **False**

5.2 A factorial experiment may be conducted as a RCBD by running each replicate of the experiment in a unique block.

(a) **True**

(b) **False**

5.3 If an interaction effect in a factorial experiment is significant, the main effects of the factors involved in that interaction are difficult to interpret individually.

(a) **True**

(b) **False**

5.4 A biomedical researcher has conducted a two-factor factorial experiment as part of the research to develop a new product. She performed the statistical analysis using a computer software package. A portion of the output is shown below:

ANOVA for Selected Factorial Model
Analysis of variance table [Partial sum of squares]

Source	Sum of Squares	DF	Mean Square	F Value	Prob > *F*
Model	874.00	5	174.80	3.28	0.0904
A	*776.00*	?	*388.00*	*7.27*	*0.0249*
B	*5.33*	*1*	*5.33*	*0.10*	*0.7625*
AB	*92.67*	*2*	*46.33*	*0.87*	*0.4663*
Pure Error	320.00	?	53.33		
Cor Total	1194.00	11			

(a) Interpret the *F*-statistic in the "Model" row of the ANOVA. Specifically, what hypotheses are being tested?

(b) What conclusions should be drawn regarding the individual model effects?

(c) How many levels of factor A were used in this experiment?

(d) How many replicates were run?

5.5 Consider the following incomplete ANOVA table:

Source	SS	DF	MS	F
A	?	1	50.00	?
B	80.00	?	40.00	?
AB	30.00	2	15.00	?
Error	?	12	?	
Total	172.00	17		

(a) Complete the ANOVA calculations.

(b) Provide an interpretation of this experiment.

(c) The pure error estimate of the standard deviation of the sample observations is 1.

True

False

5.6 The following output was obtained from a computer program that performed a two-factor ANOVA on a factorial experiment.

```
Two-way ANOVA: y versus, A, B

Source          DF       SS       MS        F      P
A                1    0.322        ?        ?      ?
B                ?   80.554  40.2771     4.59      ?
Interaction      ?        ?        ?        ?      ?
Error           12  105.327   8.7773
Total           17  231.551
```

(a) Fill in the blanks in the ANOVA table. You can use bounds on the P-values.

(b) How many levels were used for factor B?

(c) How many replicates of the experiment were performed?

(d) What conclusions would you draw about this experiment?

5.7 The following output was obtained from a computer program that performed a two-factor ANOVA on a factorial experiment.

```
Two-way ANOVA: y versus A, B
Source          DF       SS        MS     F      P
A                1        ?    0.0002     ?      ?
B                ?  180.378         ?     ?      ?
Interaction      3    8.479         ?     ?  0.932
Error            8  158.797         ?
Total           15  347.653
```

(a) Fill in the blanks in the ANOVA table. You can use bounds on the P-values.

(b) How many levels were used for factor B?

(c) How many replicates of the experiment were performed?

(d) What conclusions would you draw about this experiment?

5.8 The yield of a chemical process is being studied. The two most important variables are thought to be pressure and temperature. Three levels of each factor are selected, and a factorial experiment with two replicates is performed. The yield data are as follows:

	Pressure (psig)		
Temperature (°C)	**200**	**215**	**230**
150	90.4	90.7	90.2
	90.2	90.6	90.4
160	90.1	90.5	89.9
	90.3	90.6	90.1
170	90.5	90.8	90.4
	90.7	90.9	90.1

(a) Analyze the data and draw conclusions. Use $\alpha = 0.05$.

(b) Prepare appropriate residual plots and comment on the model's adequacy.

(c) Under what conditions would you operate this process?

5.9 An engineer suspects that the surface finish of a metal part is influenced by the feed rate and the depth of cut. He selects three feed rates and four depths of cut. He then conducts a factorial experiment and obtains the following data:

Feed Rate (in/min)	Depth of Cut (in)			
	0.15	**0.18**	**0.20**	**0.25**
	74	79	82	99
0.20	64	68	88	104
	60	73	92	96
	92	98	99	104

0.25	86	104	108	110
	88	88	95	99
	99	104	108	114
0.30	98	99	110	111
	102	95	99	107

(a) Analyze the data and draw conclusions. Use $\alpha = 0.05$.

(b) Prepare appropriate residual plots and comment on the model's adequacy.

(c) Obtain point estimates of the mean surface finish at each feed rate.

(d) Find the P-values for the tests in part (a).

5.10 For the data in Problem 5.9, compute a 95 percent confidence interval estimate of the mean difference in response for feed rates of 0.20 and 0.25 in/min.

5.11 An article in *Industrial Quality Control* (1956, pp. 5–8) describes an experiment to investigate the effect of the type of glass and the type of phosphor on the brightness of a television tube. The response variable is the current necessary (in microamps) to obtain a specified brightness level. The data are as follows:

Glass Type	Phosphor Type 1	Phosphor Type 2	Phosphor Type 3
	280	300	290
1	290	310	285
	285	295	290
	230	260	220
2	235	240	225
	240	235	230

(a) Is there any indication that either factor influences brightness? Use $\alpha = 0.05$.

(b) Do the two factors interact? Use $\alpha = 0.05$.

(c) Analyze the residuals from this experiment.

5.12 Johnson and Leone (*Statistics and Experimental Design in Engineering and the Physical Sciences*, Wiley, 1977) describe an experiment to investigate warping of copper plates. The two factors studied were the temperature and the copper content of the plates. The response variable was a measure of the amount of warping. The data are as follows:

Temperature (°C)	Copper Content (%) 40	60	80	100
50	17, 20	16, 21	24, 22	28, 27
75	12, 9	18, 13	17, 12	27, 31
100	16, 12	18, 21	25, 23	30, 23
125	21, 17	23, 21	23, 22	29, 31

(a) Is there any indication that either factor affects the amount of warping? Is there any interaction between the factors? Use $\alpha = 0.05$.

(b) Analyze the residuals from this experiment.

(c) Plot the average warping at each level of copper content and compare them to an appropriately scaled t distribution. Describe the differences in the effects of the different levels of copper content on warping. If low warping is desirable, what level of copper content would you specify?

(d) Suppose that temperature cannot be easily controlled in the environment in which the copper plates are to be used. Does this change your answer for part (c)?

5.13 The factors that influence the breaking strength of a synthetic fiber are being studied. Four production machines and three operators are chosen and a factorial experiment is run using fiber from the same production batch. The results are as follows:

Operator	Machine 1	Machine 2	Machine 3	Machine 4
1	109	110	108	110
	110	115	109	108
2	110	110	111	114
	112	111	109	112
3	116	112	114	120
	114	115	119	117

(a) Analyze the data and draw conclusions. Use $\alpha = 0.05$.

(b) Prepare appropriate residual plots and comment on the model's adequacy.

5.14 A mechanical engineer is studying the thrust force developed by a drill press. He suspects that the drilling speed and the feed rate of the material are the most important factors. He selects four feed rates and uses a high and low drill speed chosen to represent the extreme operating conditions.

He obtains the following results. Analyze the data and draw conclusions. Use $\alpha = 0.05$.

	Feed Rate			
Drill Speed	0.015	0.030	0.045	0.060
125	2.70	2.45	2.60	2.75
	2.78	2.49	2.72	2.86
200	2.83	2.85	2.86	2.94
	2.86	2.80	2.87	2.88

5.15 An experiment is conducted to study the influence of operating temperature and three types of faceplate glass in the light output of an oscilloscope tube. The following data are collected:

	Temperature		
Glass Type	100	125	150
	580	1090	1392
1	568	1087	1380
	570	1085	1386
	550	1070	1328
2	530	1035	1312
	579	1000	1299
	546	1045	867
3	575	1053	904
	599	1066	889

(a) Use $\alpha = 0.05$ in the analysis. Is there a significant interaction effect? Does glass type or temperature affect the response? What conclusions can you draw?

(b) Fit an appropriate model relating light output to glass type and temperature.

(c) Analyze the residuals from this experiment. Comment on the adequacy of the models you have considered.

5.16 Consider the experiment in Problem 5.8. Fit an appropriate model to the response data. Use this model to provide guidance concerning operating conditions for the process.

5.17 Use Tukey's test to determine which levels of the pressure factor are significantly different for the data in Problem 5.8.

5.18 An experiment was conducted to determine whether either firing temperature or furnace position affects the baked density of a carbon anode. The data are shown below:

	Temperature (°C)		
Position	800	825	850
	570	1063	565
1	565	1080	510
	583	1043	590
	528	988	526
2	547	1026	538
	521	1004	532

Suppose we assume that no interaction exists. Write down the statistical model. Conduct the ANOVA and test hypotheses on the main effects. What conclusions can be drawn? Comment on the model's adequacy.

5.19 Derive the expected mean squares for a two-factor analysis of variance with one observation per cell, assuming that both factors are fixed.

5.20 Consider the following data from a two-factor factorial experiment. Analyze the data and draw conclusions. Perform a test for nonadditivity. Use $\alpha = 0.05$.

	Column Factor			
Row Factor	1	2	3	4
1	36	39	36	32
2	18	20	22	20
3	30	37	33	34

5.21 The shear strength of an adhesive is thought to be affected by the application pressure and temperature. A factorial experiment is performed in which both factors are assumed to be fixed. Analyze the data and draw conclusions. Perform a test for nonadditivity.

	Temperature (°F)		
Pressure (lb/in^2)	250	260	270
120	9.60	11.28	9.00
130	9.69	10.10	9.57
140	8.43	11.01	9.03
150	9.98	10.44	9.80

5.22 Consider the three-factor model

$$y_{ijk} = \mu + \tau_i + \beta_j + \gamma_k + (\tau\beta)_{ij} + (\beta\gamma)_{jk} + \epsilon_{ijk} \quad \begin{cases} i = 1, 2, \ldots, a \\ j = 1, 2, \ldots, b \\ k = 1, 2, \ldots, c \end{cases}$$

Notice that there is only one replicate. Assuming all the factors are fixed, write down the ANOVA table, including the expected mean squares. What would you use as the "experimental error" to test hypotheses?

5.23 The percentage of hardwood concentration in raw pulp, the vat pressure, and the cooking time of the pulp are being investigated for their effects on the strength of paper. Three levels of hardwood concentration, three levels of pressure, and two cooking times are selected. A factorial experiment with two replicates is conducted, and the following data are obtained:

	Cooking Time 3.0 Hours		
	Pressure		
Percentage of Hardwood Concentration	**400**	**500**	**650**
2	196.6	197.7	199.8
	196.0	196.0	199.4
4	198.5	196.0	198.4
	197.2	196.9	197.6
8	197.5	195.6	197.4
	196.6	196.2	198.1

	Cooking Time 4.0 Hours		
	Pressure		
Percentage of Hardwood Concentration	**400**	**500**	**650**
2	198.4	199.6	200.6
	198.6	200.4	200.9
4	197.5	198.7	199.6
	198.1	198.0	199.0
8	197.6	197.0	198.5
	198.4	197.8	199.8

(a) Analyze the data and draw conclusions. Use $\alpha = 0.05$.

(b) Prepare appropriate residual plots and comment on the model's adequacy.

(c) Under what set of conditions would you operate this process? Why?

5.24 The quality control department of a fabric finishing plant is studying the effect of several factors on the dyeing of cotton–synthetic cloth used to manufacture men's shirts. Three operators, three cycle times, and two temperatures were selected, and three small specimens of cloth were dyed under each set of conditions. The finished cloth was compared to a standard, and a numerical score was assigned. The results are as follows. Analyze the data and draw conclusions. Comment on the model's adequacy.

	Temperature					
	300°C			**350°C**		
	Operator			**Operator**		
Cycle Time	**1**	**2**	**3**	**1**	**2**	**3**
	23	27	31	24	38	34
40	24	28	32	23	36	36
	25	26	29	28	35	39
	36	34	33	37	34	34
50	35	38	34	39	38	36
	36	39	35	35	36	31
	28	35	26	26	36	28
60	24	35	27	29	37	26
	27	34	25	25	34	24

5.25 In Problem 5.8, suppose that we wish to reject the null hypothesis with a high probability if the difference in the true mean yield at any two pressures is as great as 0.5. If a reasonable prior estimate of the standard deviation of yield is 0.1, how many replicates should be run?

5.26 The yield of a chemical process is being studied. The two factors of interest are temperature and pressure. Three levels of each factor are selected; however, only nine runs can be made in one day. The experimenter runs a complete replicate of the design on each day. The data are shown in the following table. Analyze the data, assuming that the days are blocks.

	Day 1 Pressure			**Day 2 Pressure**		
Temperature	**250**	**260**	**270**	**250**	**260**	**270**
Low	86.3	84.0	85.8	86.1	85.2	87.3
Medium	88.5	87.3	89.0	89.4	89.9	90.3
High	89.1	90.2	91.3	91.7	93.2	93.7

5.27 Consider the data in Problem 5.12. Analyze the data, assuming that replicates are blocks.

5.28 Consider the data in Problem 5.13. Analyze the data, assuming that replicates are blocks.

5.29 An article in the *Journal of Testing and Evaluation* (Vol. 16, no. 2, pp. 508–515) investigated the effects of cyclic loading and environmental conditions on fatigue crack growth at a constant 22 MPa stress for a particular material. The data from this experiment are shown below (the response is crack growth rate):

	Environment		
Frequency	**Air**	**H_2O**	**Salt H_2O**
	2.29	2.06	1.90
10	2.47	2.05	1.93
	2.48	2.23	1.75
	2.12	2.03	2.06
	2.65	3.20	3.10
1	2.68	3.18	3.24
	2.06	3.96	3.98
	2.38	3.64	3.24
	2.24	11.00	9.96
0.1	2.71	11.00	10.01
	2.81	9.06	9.36
	2.08	11.30	10.40

(a) Analyze the data from this experiment (use $\alpha = 0.05$).

(b) Analyze the residuals.

(c) Repeat the analyses from parts (a) and (b) using ln (y) as the response. Comment on the results.

5.30 An article in the *IEEE Transactions on Electron Devices* (Nov. 1986, pp. 1754) describes a study on polysilicon doping. The experiment shown below is a variation of their study. The response variable is base current.

	Anneal Temperature (°C)		
Polysilicon Doping (ions)	**900**	**950**	**1000**
1×10^{20}	4.60	10.15	11.01
	4.40	10.20	10.58
2×10^{20}	3.20	9.38	10.81
	3.50	10.02	10.60

(a) Is there evidence (with $\alpha = 0.05$) indicating that either polysilicon doping level or anneal temperature affects base current?

(b) Prepare graphical displays to assist in interpreting this experiment.

(c) Analyze the residuals and comment on model adequacy.

(d) Is the model

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_{22} x_2^2 + \beta_{12} x_1 x_2 + \epsilon$$

supported by this experiment (x_1 = doping level, x_2 = temperature)? Estimate the parameters in this model and plot the response surface.

5.31 An experiment was conducted to study the life (in hours) of two different brands of batteries in three different devices (radio, camera, and portable DVD player). A completely randomized two-factor factorial experiment was conducted and the following data resulted.

	Device		
Brand of Battery	**Radio**	**Camera**	**DVD Player**
A	8.6	7.9	5.4
	8.2	8.4	5.7
B	9.4	8.5	5.8
	8.8	8.9	5.9

(a) Analyze the data and draw conclusions, using $\alpha = 0.05$.

(b) Investigate model adequacy by plotting the residuals.

(c) Which brand of batteries would you recommend?

5.32 I have recently purchased new golf clubs, which I believe will significantly improve my game. Below are the scores of three rounds of golf played at three different golf courses with the old and the new clubs.

	Course		
Clubs	**Ahwatukee**	**Karsten**	**Foothills**
Old	90	91	88
	87	93	86
	86	90	90
New	88	90	86
	87	91	85
	85	88	88

(a) Conduct an analysis of variance. Using $\alpha = 0.05$, what conclusions can you draw?

(b) Investigate model adequacy by plotting the residuals.

5.33 A manufacturer of laundry products is investigating the performance of a newly formulated stain remover. The new formulation is compared to the original formulation with respect to its ability to remove a standard tomato-like stain in a test article of cotton cloth using a factorial experiment. The other factors in the experiment are the number of times the test article is washed (1 or 2) and whether or not a detergent booster is used. The response variable is the stain shade after washing (12 is the darkest, 0 is the lightest). The data are shown in the following table.

	Number of Washings		Number of Washings	
	1		2	
Formulation	**Booster**		**Booster**	
	Yes	**No**	**Yes**	**No**
New	6, 5	6, 5	3, 2	4, 1
Original	10, 9	11, 11	10, 9	9, 10

(a) Conduct an analysis of variance. Using $\alpha = 0.05$, what conclusions can you draw?

(b) Investigate model adequacy by plotting the residuals.

5.34 Bone anchors are used by orthopedic surgeons in repairing torn rotator cuffs (a common shoulder tendon injury among baseball players). The bone anchor is a threaded insert that is screwed into a hole that has been drilled into the shoulder bone near the site of the torn tendon. The torn tendon is then sutured to the anchor. In a successful operation, the tendon is stabilized and reattaches itself to the bone. However, bone anchors can pull out if they are subjected to high loads. An experiment was performed to study the force required to pull out the anchor for three anchor types and two different foam densities (the foam simulates the natural variability found in real bone). Two replicates of the experiment were performed. The experimental design and the pullout force response data are as follows.

	Foam Density	
Anchor Type	**Low**	**High**
A	190, 200	241, 255
B	185, 190	230, 237
C	210, 205	256, 260

(a) Analyze the data from this experiment.

(b) Investigate model adequacy by constructing appropriate residual plots.

(c) What conclusions can you draw?

5.35 An experiment was performed to investigate the keyboard feel on a computer (crisp or mushy) and the size of the keys (small, medium, or large). The response variable is typing speed. Three replicates of the experiment were performed. The experimental design and the data are as follow.

	Keyboard Feel	
Key Size	**Mushy**	**Crisp**
Small	31, 33, 35	36, 40, 41
Medium	36, 35, 33	40, 41, 42
Large	37, 34, 33	38, 36, 39

(a) Analyze the data from this experiment.

(b) Investigate model adequacy by constructing appropriate residual plots.

(c) What conclusions can you draw?

5.36 An article in *Quality Progress* (May 2011, pp. 42–48) describes the use of factorial experiments to improve a silver powder production process. This product is used in conductive pastes to manufacture a wide variety of products ranging from silicon wafers to elastic membrane switches. Powder density (g/cm^2) and surface area (cm^2/g) are the two critical characteristics of this product. The experiments involved three factors—reaction temperature, ammonium percent, and stirring rate. Each of these factors had two levels and the design was replicated twice. The design is shown below.

Ammonium (%)	Stir Rate (RPM)	Temperature (°C)	Density	Surface Area
2	100	8	14.68	0.40
2	100	8	15.18	0.43
30	100	8	15.12	0.42
30	100	8	17.48	0.41
2	150	8	7.54	0.69
2	150	8	6.66	0.67
30	150	8	12.46	0.52
30	150	8	12.62	0.36
2	100	40	10.95	0.58
2	100	40	17.68	0.43

30	100	40	12.65	0.57
30	100	40	15.96	0.54
2	150	40	8.03	0.68
2	150	40	8.84	0.75
30	150	40	14.96	0.41
30	150	40	14.96	0.41

(a) Analyze the density response. Are any interactions significant? Draw appropriate conclusions about the effects of the significant factors on the response.

(b) Prepare appropriate residual plots and comment on model adequacy.

(c) Construct contour plots to aid in practical interpretation of the density response.

(d) Analyze the surface area response. Are any interactions significant? Draw appropriate conclusions about the effects of the significant factors on the response.

(e) Prepare appropriate residual plots and comment on model adequacy.

(f) Construct contour plots to aid in practical interpretation of the surface area response.

5.37 **Continuation of Problem 5.36**. Suppose that the specifications require that surface area must be between 0.3 and 0.6 cm^2/g and that density must be less than 14 g/cm^3. Find a set of operating conditions that will result in a product that meets these requirements.

5.38 An article in *Biotechnology Progress* (2001, Vol. 17, pp. 366–368) described an experiment to investigate nisin extraction in aqueous two-phase solutions. A two-factor factorial experiment was conducted using factors A = concentration of PEG and B = concentration of Na_2SO_4. Data similar to that reported in the paper are shown below.

A	*B*	**Extraction (%)**
13	11	62.9
13	11	65.4
15	11	76.1
15	11	72.3
13	13	87.5
13	13	84.2
15	13	102.3
15	13	105.6

(a) Analyze the extraction response. Draw appropriate conclusions about the effects of the significant factors on the response.

(b) Prepare appropriate residual plots and comment on model adequacy.

(c) Construct contour plots to aid in practical interpretation of the density response.

5.39 Reconsider the experiment in Problem 5.9. Suppose that this experiment had been conducted in three blocks, with each replicate a block. Assume that the observations in the data table are given in order, that is, the first observation in each cell comes from the first replicate, and so on. Reanalyze the data as a factorial experiment in blocks and estimate the variance component for blocks. Does it appear that blocking was useful in this experiment?

5.40 Reconsider the experiment in Problem 5.11. Suppose that this experiment had been conducted in three blocks, with each replicate a block. Assume that the observations in the data table are given in order, that is, the first observation in each cell comes from the first replicate, and so on. Reanalyze the data as a factorial experiment in blocks and estimate the variance component for blocks. Does it appear that blocking was useful in this experiment?

5.41 Reconsider the experiment in Problem 5.13. Suppose that this experiment had been conducted in two blocks, with each replicate a block. Assume that the observations in the data table are given in order, that is, the first observation in each cell comes from the first replicate, and so on. Reanalyze the data as a factorial experiment in blocks and estimate the variance component for blocks. Does it appear that blocking was useful in this experiment?

5.42 Reconsider the three-factor factorial experiment in Problem 5.23. Suppose that this experiment had been conducted in two blocks, with each replicate a block. Assume that the observations in the data table are given in order, that is, the first observation in each cell comes from the first replicate, and so on. Reanalyze the data as a factorial experiment in blocks and estimate the variance component for blocks. Does it appear that blocking was useful in this experiment?

5.43 Reconsider the three-factor factorial experiment in Problem 5.24. Suppose that this experiment had been conducted in three blocks, with each replicate a block. Assume that the observations in the data table are given in order, that is, the first observation in each cell comes from the first replicate, and so on. Reanalyze the data as a factorial experiment in blocks and estimate the variance component for blocks. Does it appear that blocking was useful in this experiment?

5.44 Reconsider the bone anchor experiment in Problem 5.34. Suppose that this experiment had been conducted in two blocks, with each replicate a block. Assume that the observations in the data table are given in order, that is, the first observation in each cell comes from the first replicate, and so on. Reanalyze the data as a factorial experiment in blocks and estimate the variance component for blocks. Does it appear that blocking was useful in this experiment?

5.45 Reconsider the keyboard experiment in Problem 5.35. Suppose that this experiment had been conducted in three blocks, with each replicate a block. Assume that the observations in the data table are given in order, that is, the first observation in each cell comes from the first replicate, and so on. Reanalyze the data as a factorial experiment in blocks and estimate the variance component for blocks. Does it appear that blocking was useful in this experiment?

5.46 The C. F. Eye Care company manufactures lenses for transplantation into the eye following cataract surgery. An engineering group has conducted an experiment involving two factors to determine their effect on the lens polishing process. The results of this experiment are summarized in the following ANOVA display:

Source	DF	SS	MS	*F*	*P*-Value
Factor *A*	?	?	0.0833	0.05	0.952
Factor *B*	?	96.333	96.3333	57.80	<0.001
Interaction	2	12.167	6.0833	3.65	?
Error	6	10.000	?	?	
Total	11	118.667			

Answer the following questions about this experiment.

(a) The sum of squares for factor *A* is____.

(b) The number of degrees of freedom for factor *A* in the experiment is____.

(c) The number of degrees of freedom for factor *B* is____.

(d) The mean square for error is____.

(e) An upper bound for the *P*-value for the interaction test statistic is____.

(f) The engineers used ____ levels of the factor *A* in this experiment.

(g) The engineers used ____ levels of the factor *B* in this experiment.

(h) There are ____ replicates of this experiment.

(i) Would you conclude that the effect of factor *B* depends on the level of factor *A*?

Yes

No

(j) An estimate of the standard deviation of the response variable is ____.

5.47 Reconsider the lens polishing experiment in Problem 5.46. Suppose that this experiment had been conducted as a randomized complete block design. The sum of squares for blocks is 4.00. Reconstruct the ANOVA given this new information. What impact does the blocking have on the conclusions from the original experiment?

5.48 In Problem 4.58 you met physics PhD student Laura Van Ertia who had conducted a single-factor experiment in her pursuit of the unified theory. She is at it again, and this time she has moved on to a two-factor factorial conducted as a completely randomized design. From her experiment, Laura has constructed the following incomplete ANOVA display:

Source	SS	DF	MS	*F*
A	350.00	2	?	?
B	300.00	?	150	?
AB	200.00	?	50	?
Error	150.00	18		
Total	1000.00			

(a) How many levels of factor *B* did she use in the experiment?

(b) How many degrees of freedom are associated with interaction?

(c) The error mean square is ____.

(d) The mean square for factor *A* is ____.

(e) How many replicates of the experiment were conducted? ____

(f) What are your conclusions about interaction and the two main effects?

(g) An estimate of the standard deviation of the response variable is ____.

(h) If this experiment had been run in blocks there would have been ____ degrees of freedom for blocks.

5.49 **Continuation of Problem 5.48**. Suppose that Laura did actually conduct the experiment in Problem 5.48 as a randomized complete block design. Assume that the block sum of squares is 60.00. Reconstruct the ANOVA display under this new set of assumptions.

5.50 Consider the following ANOVA for a two-factor factorial experiment:

Source	DF	SS	MS	F	P
A	2	8.0000	4.00000	2.00	0.216
B	1	8.3333	8.33333	4.17	0.087
Interaction	2	10.6667	5.33333	2.67	0.148
Error	6	12.0000	2.00000		
Total	11	39.0000			

In addition to the ANOVA, you are given the following data totals. Row totals (factor A) = 18, 10, 14; column totals (factor B) = 16, 26; cell totals = 10, 8, 2, 8, 4, 10, and replicate totals = 19, 23. The grand total is 42. The original experiment was a completely randomized design. Now suppose that the experiment had been run in two complete blocks. Answer the following questions about the ANOVA for the blocked experiment.

(a) The block sum of squares is ____.

(b) There are____ degrees of freedom for blocks.

(c) The error sum of squares is now ____.

(d) The interaction effect is now significant at 1 percent.

Yes

No

5.51 Consider the following incomplete ANOVA table:

Source	**SS**	**DF**	**MS**	***F***
A	50.00	1	50.00	?
B	80.00	2	40.00	?
AB	30.00	2	15.00	?
Error	?	12	?	
Total	172.00	17		

In addition to the ANOVA table you know that the experiment has been replicated three times and that the totals of the three replicates are 10, 12, and 14 respectively. The original experiment was run as a completely randomized design. Answer the following questions:

(a) The pure error estimate of the standard deviation of the sample observations is 1.

Yes

No

(b) Suppose that the experiment had been run in blocks, so that it is an randomized complete block design. The number of degrees of freedom for blocks would be ____.

(c) The block sum of squares is ____.

(d) The error sum of squares in the randomized complete block design is now ____.

(e) For the randomized complete block design, what is the estimate of the standard deviation of the sample observations?

5.52 Consider the following incomplete ANOVA table:

Source	**SS**	**DF**	**MS**	***F***
A	50.00	1	50.00	?
B	80.00	2	40.00	?
AB	30.00	2	15.00	?
Blocks	10.00	1	?	
Error	?	?	?	
Total	185.00	11		

(a) The pure error estimate of the standard deviation of the sample observations is 1.73.

True

False

(b) Suppose that the experiment had not been run in blocks; that is, it is now a CRD. The number of degrees of freedom for error would now be ____________________.

(c) The error mean square in the CRD would be ____________________.

(d) The F-test statistic for interaction in the CRD is significant at $\alpha = 0.05$.

True

False

CHAPTER 6

The 2^k Factorial Design

CHAPTER OUTLINE

The supplemental material is on the textbook website www.wiley.com/college/montgomery.

CHAPTER LEARNING OBJECTIVES

1. Learn about the 2^k series of factorial designs.
2. Know how to compute main effects and interactions for 2^k factorial designs.
3. Learn how the analysis of variance can be used for 2^k factorial designs.
4. Know how to represent the results from a 2^k factorial design as a regression model.
5. Know how to use graphical and analytical methods to analyze unreplicated 2^k factorial designs.
6. Understand the basics of design optimality: *D*-optimality, *I*-optimality, and *G*-optimality, and why factorial designs are generally optimal designs.
7. Know how to use design optimality criteria in constructing designs.
8. Know the value of adding center runs to 2^k factorial designs.
9. Know why we work with coded variables in analyzing 2^k factorial designs.

6.1 Introduction

Factorial designs are widely used in experiments involving several factors where it is necessary to study the joint effect of the factors on a response. Chapter 5 presented general methods for the analysis of factorial designs. However, several special cases of the general factorial design are important because they are widely used in research work and also because they form the basis of other designs of considerable practical value.

The most important of these special cases is that of k factors, each at only two levels. These levels may be **quantitative**, such as two values of temperature, pressure, or time; or they may be **qualitative**, such as two machines, two operators, the "high" and "low" levels of a factor, or perhaps the presence and absence of a factor. A complete replicate of such a design requires $2 \times 2 \times \cdots \times 2 = 2^k$ observations and is called a **2^k factorial design**.

This chapter focuses on this extremely important class of designs. Throughout this chapter, we assume that (1) the factors are fixed, (2) the designs are completely randomized, and (3) the usual normality assumptions are satisfied.

The 2^k design is particularly useful in the early stages of experimental work when many factors are likely to be investigated. It provides the smallest number of runs with which k factors can be studied in a complete factorial design. Consequently, these designs are widely used in **factor screening experiments** (where the experiments is intended in discovering the set of **active** factors from a large group of factors). It is also easy to develop effective blocking schemes for these designs (Chapter 7) and to fix them in fractional versions (Chapter 8).

Because there are only two levels for each factor, we assume that the response is approximately linear over the range of the factor levels chosen. In many factor screening experiments, when we are just starting to study the process or the system, this is often a reasonable assumption. In Section 6.8, we will present a simple method for checking this assumption and discuss what action to take if it is violated. The book by Mee (2009) is a useful supplement to this chapter and Chapters 7 and 8.

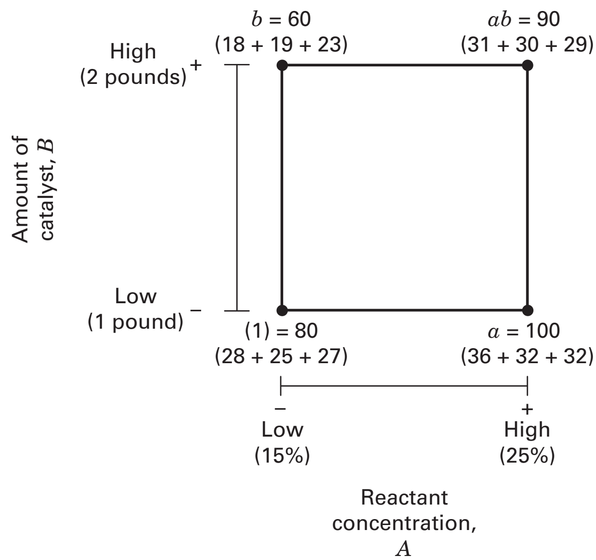
6.2 The 2^2 Design

The first design in the 2^k series is one with only two factors, say A and B, each run at two levels. This design is called a **2^2 factorial design**. The levels of the factors may be arbitrarily called "low" and "high." As an example, consider an investigation into the effect of the concentration of the reactant and the amount of the catalyst on the conversion (yield) in a chemical process. The objective of the experiment was to determine if adjustments to either of these two factors would increase the yield. Let the reactant concentration be factor A and let the two levels of interest be 15 and 25 percent. The catalyst is factor B, with the high level denoting the use of 2 pounds of the catalyst and the low level denoting the use of only 1 pound. The experiment is replicated three times, so there are 12 runs. The order in which the runs are made is random, so this is a **completely randomized experiment**. The data obtained are as follows:

Factor		Treatment	Replicate			
A	***B***	**Combination**	**I**	**II**	**III**	**Total**
$-$	$-$	A low, B low	28	25	27	80
$+$	$-$	A high, B low	36	32	32	100
$-$	$+$	A low, B high	18	19	23	60
$+$	$+$	A high, B high	31	30	29	90

The four treatment combinations in this design are shown graphically in Figure 6.1. By convention, we denote the effect of a factor by a capital Latin letter. Thus, "A" refers to the effect of factor A, "B" refers to the effect of factor B, and "AB" refers to the AB interaction. In the 2^2 design, the low and high levels of A and B are denoted by "$-$" and "$+$," respectively, on the A and B axes. Thus, $-$ on the A axis represents the low level of concentration (15%), whereas $+$ represents the high level (25%), and $-$ on the B axis represents the low level of catalyst, and $+$ denotes the high level.

The four treatment combinations in the design are also represented by lowercase letters, as shown in Figure 6.1. We see from the figure that the high level of any factor in the treatment combination is denoted by the corresponding lowercase letter and that the low level of a factor in the treatment combination is denoted by the absence of the corresponding letter. Thus, a represents the treatment combination of A at the high level and B at the low level, b represents



■ **FIGURE 6.1 Treatment combinations in the 2^2 design**

A at the low level and B at the high level, and ab represents both factors at the high level. By convention, (1) is used to denote both factors at the low level. This notation is used throughout the 2^k series.

In a two-level factorial design, we may define the average effect of a factor as the change in response produced by a change in the level of that factor averaged over the levels of the other factor. Also, the symbols (1), a, b, and ab now represent the *total* of the response observation at all n replicates taken at the treatment combination, as illustrated in Figure 6.1. Now the effect of A at the low level of B is $[a - (1)]/n$, and the effect of A at the high level of B is $[ab - b]/n$. Averaging these two quantities yields the **main effect** of A:

$$A = \frac{1}{2n}\{[ab - b] + [a - (1)]\}$$

$$= \frac{1}{2n}[ab + a - b - (1)] \tag{6.1}$$

The average main effect of B is found from the effect of B at the low level of A (i.e., $[b - (1)]/n$) and at the high level of A (i.e., $[ab - a]/n$) as

$$B = \frac{1}{2n}\{[ab - a] + [b - (1)]\}$$

$$= \frac{1}{2n}[ab + b - a - (1)] \tag{6.2}$$

We define the **interaction effect** AB as the average difference between the effect of A at the high level of B and the effect of A at the low level of B. Thus,

$$AB = \frac{1}{2n}\{[ab - b] - [a - (1)]\}$$

$$= \frac{1}{2n}[ab + (1) - a - b] \tag{6.3}$$

Alternatively, we may define AB as the average difference between the effect of B at the high level of A and the effect of B at the low level of A. This will also lead to Equation 6.3.

The formulas for the effects of A, B, and AB may be derived by another method. The effect of A can be found as the difference in the average response of the two treatment combinations on the right-hand side of the square in

Figure 6.1 (call this average $\overline{y}_{A+}$ because it is the average response at the treatment combinations where A is at the high level) and the two treatment combinations on the left-hand side (or $\overline{y}_{A-}$). That is,

$$\begin{aligned} A &= \overline{y}_{A+} - \overline{y}_{A-} \\ &= \frac{ab+a}{2n} - \frac{b+(1)}{2n} \\ &= \frac{1}{2n}[ab+a-b-(1)] \end{aligned}$$

This is exactly the same result as in Equation 6.1. The effect of B, Equation 6.2, is found as the difference between the average of the two treatment combinations on the top of the square ($\overline{y}_{B+}$) and the average of the two treatment combinations on the bottom ($\overline{y}_{B-}$), or

$$\begin{aligned} B &= \overline{y}_{B+} - \overline{y}_{B-} \\ &= \frac{ab+b}{2n} - \frac{a+(1)}{2n} \\ &= \frac{1}{2n}[ab+b-a-(1)] \end{aligned}$$

Finally, the interaction effect AB is the average of the right-to-left diagonal treatment combinations in the square [ab and (1)] minus the average of the left-to-right diagonal treatment combinations (a and b), or

$$\begin{aligned} AB &= \frac{ab+(1)}{2n} - \frac{a+b}{2n} \\ &= \frac{1}{2n}[ab+(1)-a-b] \end{aligned}$$

which is identical to Equation 6.3.

Using the experiment in Figure 6.1, we may estimate the average effects as

$$\begin{aligned} A &= \frac{1}{2(3)}(90+100-60-80) = 8.33 \\ B &= \frac{1}{2(3)}(90+60-100-80) = -5.00 \\ AB &= \frac{1}{2(3)}(90+80-100-60) = 1.67 \end{aligned}$$

The effect of A (reactant concentration) is positive; this suggests that increasing A from the low level (15%) to the high level (25%) will increase the yield. The effect of B (catalyst) is negative; this suggests that increasing the amount of catalyst added to the process will decrease the yield. The interaction effect appears to be small relative to the two main effects.

In experiments involving 2^k designs, it is always important to examine the **magnitude** and **direction** of the factor effects to determine which variables are likely to be important. The **analysis of variance** can generally be used to confirm this interpretation (t-tests could be used too). Effect magnitude and direction should always be considered along with the ANOVA, because the ANOVA alone does not convey this information. There are several excellent statistics software packages that are useful for setting up and analyzing 2^k designs. There are also special time-saving methods for performing the calculations manually.

Consider determining the sums of squares for A, B, and AB. Note from Equation 6.1 that a **contrast** is used in estimating A, namely

$$\text{Contrast}_A = ab + a - b - (1) \tag{6.4}$$

We usually call this contrast the **total effect** of A. From Equations 6.2 and 6.3, we see that contrasts are also used to estimate B and AB. Furthermore, these three contrasts are **orthogonal**. The sum of squares for any contrast can be computed from Equation 3.29, which states that the sum of squares for any contrast is equal to the contrast squared divided by the number of observations in each total in the contrast times the sum of the squares of the contrast coefficients. Consequently, we have

$$SS_A = \frac{[ab + a - b - (1)]^2}{4n} \tag{6.5}$$

$$SS_B = \frac{[ab + b - a - (1)]^2}{4n} \tag{6.6}$$

and

$$SS_{AB} = \frac{[ab + (1) - a - b]^2}{4n} \tag{6.7}$$

as the sums of squares for A, B, and AB. Notice how simple these equations are. We can compute sums of squares by only squaring one number.

Using the experiment in Figure 6.1, we may find the sums of squares from Equations 6.5, 6.6, and 6.7 as

$$SS_A = \frac{(50)^2}{4(3)} = 208.33$$
$$SS_B = \frac{(-30)^2}{4(3)} = 75.00 \tag{6.8}$$

and

$$SS_{AB} = \frac{(10)^2}{4(3)} = 8.33$$

The total sum of squares is found in the usual way, that is,

$$SS_T = \sum_{i=1}^{2}\sum_{j=1}^{2}\sum_{k=1}^{n} y_{ijk}^2 - \frac{y_{...}^2}{4n} \tag{6.9}$$

In general, SS_T has $4n - 1$ degrees of freedom. The error sum of squares, with $4(n - 1)$ degrees of freedom, is usually computed by subtraction as

$$SS_E = SS_T - SS_A - SS_B - SS_{AB} \tag{6.10}$$

For the experiment in Figure 6.1, we obtain

$$\begin{aligned} SS_T &= \sum_{i=1}^{2}\sum_{j=1}^{2}\sum_{k=1}^{3} y_{ijk}^2 - \frac{y_{...}^2}{4(3)} \\ &= 9398.00 - 9075.00 = 323.00 \end{aligned}$$

and

$$\begin{aligned} SS_E &= SS_T - SS_A - SS_B - SS_{AB} \\ &= 323.00 - 208.33 - 75.00 - 8.33 \\ &= 31.34 \end{aligned}$$

using SS_A, SS_B, and SS_{AB} from Equations 6.8. The complete ANOVA is summarized in Table 6.1. On the basis of the P-values, we conclude that the main effects are statistically significant and that there is no interaction between these factors. This confirms our initial interpretation of the data based on the magnitudes of the factor effects.

It is often convenient to write down the treatment combinations in the order (1), a, b, ab. This is referred to as **standard order** (or Yates's order, for Frank Yates who was one of Fisher coworkers and who made many important

■ **TABLE 6.1**
Analysis of Variance for the Experiment in Figure 6.1

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_q	*P*-Value
A	208.33	1	208.33	53.15	0.0001
B	75.00	1	75.00	19.13	0.0024
AB	8.33	1	8.33	2.13	0.1826
Error	31.34	8	3.92		
Total	323.00	11			

contributions to designing and analyzing experiments). Using this standard order, we see that the contrast coefficients used in estimating the effects are

Effects	(1)	*a*	*b*	*ab*
A	−1	+1	−1	+1
B	−1	−1	+1	+1
AB	+1	−1	−1	+1

Note that the contrast coefficients for estimating the interaction effect are just the product of the corresponding coefficients for the two main effects. The contrast coefficient is always either +1 or −1, and a **table of plus and minus signs** such as in Table 6.2 can be used to determine the proper sign for each treatment combination. The column headings in Table 6.2 are the main effects (*A* and *B*), the *AB* interaction, and *I*, which represents the total or average of the entire experiment. Notice that the column corresponding to *I* has only plus signs. The row designators are the treatment combinations. To find the contrast for estimating any effect, simply multiply the signs in the appropriate column of the table by the corresponding treatment combination and add. For example, to estimate *A*, the contrast is $-(1) + a - b + ab$, which agrees with Equation 6.1. Note that the contrasts for the effects *A*, *B*, and *AB* are **orthogonal**. Thus, the 2^2 (and all 2^k designs) is an **orthogonal design**. The ±1 coding for the low and high levels of the factors is often called the **orthogonal coding** or the **effects coding**.

■ **TABLE 6.2**
Algebraic Signs for Calculating Effects in the 2^2 Design

Treatment Combination	Factorial Effect			
	I	*A*	*B*	*AB*
(1)	+	−	−	+
a	+	+	−	−
b	+	−	+	−
ab	+	+	+	+

The Regression Model. In a 2^k factorial design, it is easy to express the results of the experiment in terms of a **regression model**. Because the 2^k is just a factorial design, we could also use either an effects or a means model, but the regression model approach is much more natural and intuitive. For the chemical process experiment in Figure 6.1, the regression model is

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \epsilon$$

where x_1 is a **coded variable** that represents the reactant concentration, x_2 is a coded variable that represents the amount of catalyst, and the β's are regression coefficients. The relationship between the **natural variables**, the reactant concentration and the amount of catalyst, and the coded variables is

$$x_1 = \frac{\text{Conc} - (\text{Conc}_{\text{low}} + \text{Conc}_{\text{high}})/2}{(\text{Conc}_{\text{high}} - \text{Conc}_{\text{low}})/2}$$

and

$$x_2 = \frac{\text{Catalyst} - (\text{Catalyst}_{\text{low}} + \text{Catalyst}_{\text{high}})/2}{(\text{Catalyst}_{\text{high}} - \text{Catalyst}_{\text{low}})/2}$$

When the natural variables have only two levels, this coding will produce the familiar ± 1 notation for the levels of the coded variables. To illustrate this for our example, note that

$$\begin{aligned} x_1 &= \frac{\text{Conc} - (15 + 25)/2}{(25 - 15)/2} \\ &= \frac{\text{Conc} - 20}{5} \end{aligned}$$

Thus, if the concentration is at the high level (Conc = 25%), then $x_1 = +1$; if the concentration is at the low level (Conc = 15%), then $x_1 = -1$. Furthermore,

$$\begin{aligned} x_2 &= \frac{\text{Catalyst} - (1 + 2)/2}{(2 - 1)/2} \\ &= \frac{\text{Catalyst} - 1.5}{0.5} \end{aligned}$$

Thus, if the catalyst is at the high level (Catalyst = 2 pounds), then $x_2 = +1$; if the catalyst is at the low level (Catalyst = 1 pound), then $x_2 = -1$.

The fitted regression model is

$$\hat{y} = 27.5 + \left(\frac{8.33}{2}\right) x_1 + \left(\frac{-5.00}{2}\right) x_2$$

where the intercept is the grand average of all 12 observations, and the regression coefficients $\hat{\beta}_1$ and $\hat{\beta}_2$ are one-half the corresponding factor effect estimates. The regression coefficient is one-half the effect estimate because a regression coefficient measures the effect of a one-unit change in x on the mean of y, and the effect estimate is based on a two-unit change (from -1 to $+1$). This simple method of estimating the regression coefficients results in **least squares** parameter estimates. We will return to this topic again in Section 6.7. Also see the **supplemental material** for this chapter.

How Much Replication is Necessary? A standard question that arises in almost every experiment is how much replication is necessary? We have discussed this in previous chapters, but there are some aspects of this topic that are particularly useful in 2^k designs, which are used extensively for **factor screening**. That is, studying a group of k factors to determine which ones are active. Recall from our previous discussions that the choice of an appropriate sample size in a designed experiment depends on how large the effect of interest is, the power of the statistical test, and the choice of type I error. While the size of an important effect is obviously problem-dependent, in many practical situations experimenters are interested in detecting effects that are at least as large as twice the error standard deviation (2σ). Smaller effects are usually of less interest because changing the factor associated with such a small effect often results in a change in response that is very small relative to the background noise in the system. Adequate power is also problem-dependent, but in many practical situations achieving power of at least 0.80 or 80% should be the goal.

We will illustrate how an appropriate choice of sample size can be determined using the 2^2 chemical process experiment. Suppose that we are interested in detecting effects of size 2σ. If the basic 2^2 design is replicated twice for a total of 8 runs, there will be 4 degrees of freedom for estimating a model-independent estimate of error (pure error). If the experimenter uses a significance level or Type I error rate of $\alpha = 0.05$, this design results in a power of 0.572 or 57.2%. This is too low, and the experimenter should consider more replication. There is another alternative that could be useful in screening experiments, use a higher type I error rate. In screening experiments Type I errors (thinking a factor is active when it really isn't) usually does not have the same impact than a Type II error (failing to identify an active factor). If a factor is mistakenly thought to be active, that error will be discovered in further work and so the consequences of this type I error is usually small. However, failing to identify an active factor is usually very problematic because that factor is set aside and typically never considered again. So in screening experiments experimenters are often willing to consider higher Type I error rates, say 0.10 or 0.20.

Suppose that we use $\alpha = 0.10$ in our chemical process experiment. This would result in power of 75%. Using $\alpha = 0.20$ increases the power to 89%, a very reasonable value. The other alternative is to increase the sample size by using additional replicates. If we use three replicates there will be 8 degrees of freedom for pure error and if we want to detect effects of size 2σ with $\alpha = 0.05$, this design will result in power of 85.7%. This is a very good value for power, so the experimenters decided to use three replicates of the 2^2 design.

Software packages can be used to produce the power calculations given above. The boxed display below shows the power calculations from JMP. The model has both main effects and the two-factor interaction and the effects of size 2σ is chosen by setting the square root of mean square error (Anticipated RMSE) to 1 and setting the size of each anticipated model coefficient to 1.

Evaluate Design

Model

Intercept

X1

X2

X1*X2

Power Analysis

Significance Level	0.05
Anticipated RMSE	1

Term	Anticipated Coefficient	Power
Intercept	1	0.857
X1	1	0.857
X2	1	0.857
X1*X2	1	0.857

Residuals and Model Adequacy. The regression model can be used to obtain the predicted or fitted value of y at the four points in the design. The residuals are the differences between the observed and fitted values of y. For example, when the reactant concentration is at the low level ($x_1 = -1$) and the catalyst is at the low level ($x_2 = -1$), the predicted yield is

$$\hat{y} = 27.5 + \left(\frac{8.33}{2}\right)(-1) + \left(\frac{-5.00}{2}\right)(-1) = 25.835$$

There are three observations at this treatment combination, and the residuals are

$$\begin{aligned} e_1 &= 28 - 25.835 = 2.165 \\ e_2 &= 25 - 25.835 = -0.835 \\ e_3 &= 27 - 25.835 = 1.165 \end{aligned}$$

The remaining predicted values and residuals are calculated similarly. For the high level of the reactant concentration and the low level of the catalyst,

$$\hat{y} = 27.5 + \left(\frac{8.33}{2}\right)(+1) + \left(\frac{-5.00}{2}\right)(-1) = 34.165$$

and

$$\begin{aligned} e_4 &= 36 - 34.165 = 1.835 \\ e_5 &= 32 - 34.165 = -2.165 \\ e_6 &= 32 - 34.165 = -2.165 \end{aligned}$$

For the low level of the reactant concentration and the high level of the catalyst,

$$\hat{y} = 27.5 + \left(\frac{8.33}{2}\right)(-1) + \left(\frac{-5.00}{2}\right)(+1) = 20.835$$

and

$$\begin{aligned} e_7 &= 18 - 20.835 = -2.835 \\ e_8 &= 19 - 20.835 = -1.835 \\ e_9 &= 23 - 20.835 = 2.165 \end{aligned}$$

Finally, for the high level of both factors,

$$\hat{y} = 27.5 + \left(\frac{8.33}{2}\right)(+1) + \left(\frac{-5.00}{2}\right)(+1) = 29.165$$

and

$$\begin{aligned} e_{10} &= 31 - 29.165 = 1.835 \\ e_{11} &= 30 - 29.165 = 0.835 \\ e_{12} &= 29 - 29.165 = -0.165 \end{aligned}$$

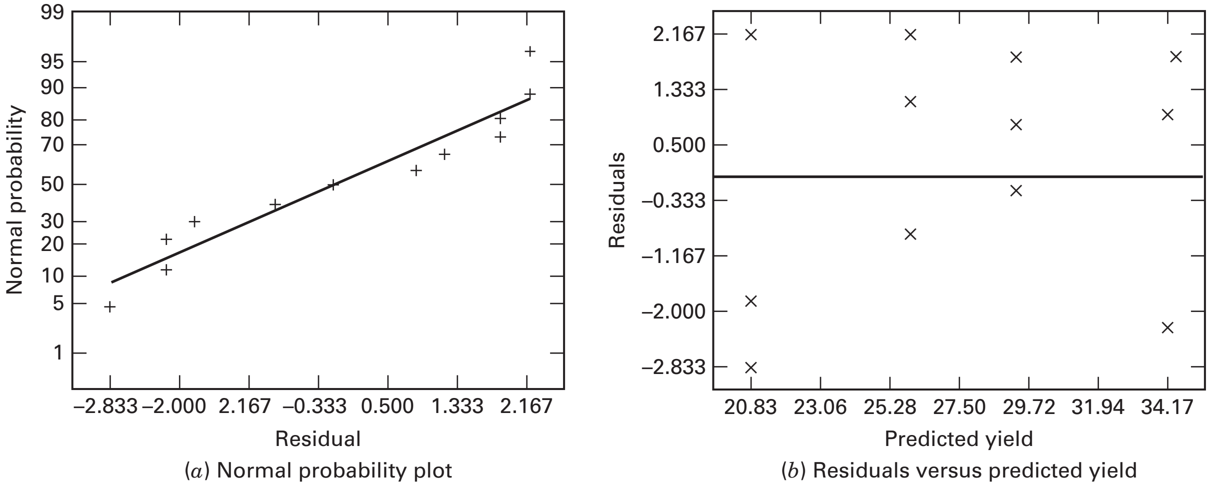
Figure 6.2 presents a normal probability plot of these residuals and a plot of the residuals versus the predicted yield. These plots appear satisfactory, so we have no reason to suspect that there are any problems with the validity of our conclusions.

The Response Surface. The regression model

$$\hat{y} = 27.5 + \left(\frac{8.33}{2}\right)x_1 + \left(\frac{-5.00}{2}\right)x_2$$

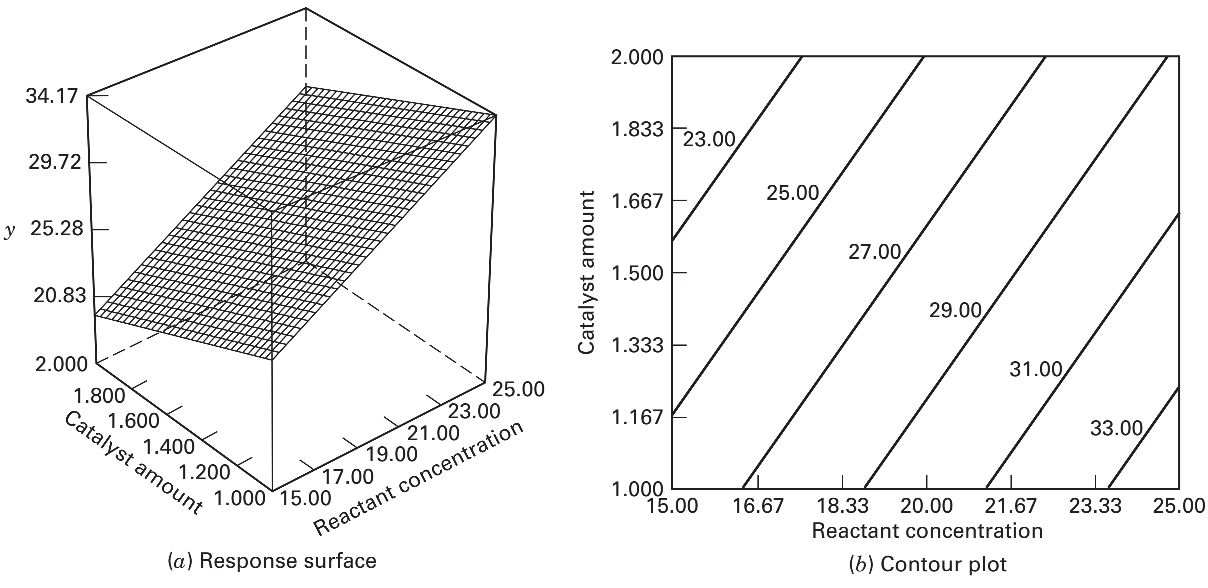
can be used to generate response surface plots. If it is desirable to construct these plots in terms of the **natural factor levels**, then we simply substitute the relationships between the natural and coded variables that we gave earlier into the regression model, yielding

$$\begin{aligned} \hat{y} &= 27.5 + \left(\frac{8.33}{2}\right)\left(\frac{\text{Conc} - 20}{5}\right) + \left(\frac{-5.00}{2}\right)\left(\frac{\text{Catalyst} - 1.5}{0.5}\right) \\ &= 18.33 + 0.8333\ \text{Conc} - 5.00\ \text{Catalyst} \end{aligned}$$



■ **FIGURE 6.2** **Residual plots for the chemical process experiment**

Figure 6.3*a* presents the three-dimensional response surface plot of yield from this model, and Figure 6.3*b* is the contour plot. Because the model is **first-order** (that is, it contains only the main effects), the fitted response surface is a plane. From examining the contour plot, we see that yield increases as reactant concentration increases and catalyst amount decreases. Often, we use a fitted surface such as this to find a **direction of potential improvement** for a process. A formal way to do so, called the **method of steepest ascent**, will be presented in Chapter 11 when we discuss methods for systematically exploring response surfaces.



■ **FIGURE 6.3** **Response surface plot and contour plot of yield from the chemical process experiment**

6.3 The 2^3 Design

Suppose that three factors, A, B, and C, each at two levels, are of interest. The design is called a **2^3 factorial design**, and the eight treatment combinations can now be displayed geometrically as a cube, as shown in Figure 6.4*a*. Using the "+ and −" orthogonal coding to represent the low and high levels of the factors, we may list the eight runs in the 2^3 design as in Figure 6.4*b*. This is sometimes called the **design matrix**. Extending the label notation discussed in Section 6.2, we write the treatment combinations in standard order as (1), a, b, ab, c, ac, bc, and abc. Remember that these symbols also represent the *total* of all n observations taken at that particular treatment combination.

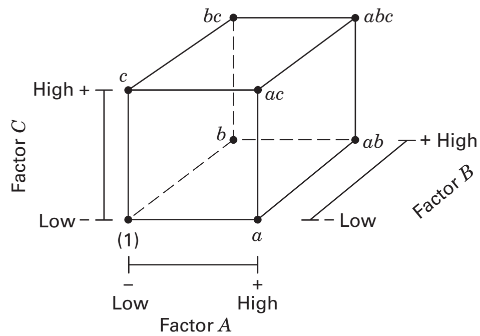
Three different notations are widely used for the runs in the 2^k design. The first is the + and − notation, often called the **geometric coding** (or the **orthogonal coding** or the **effects coding**). The second is the use of lowercase letter labels to identify the treatment combinations. The final notation uses 1 and 0 to denote high and low factor levels, respectively, instead of + and −. These different notations are illustrated below for the 2^3 design:

Run	A	B	C	Labels	A	B	C
1	−	−	−	(1)	0	0	0
2	+	−	−	a	1	0	0
3	−	+	−	b	0	1	0
4	+	+	−	ab	1	1	0
5	−	−	+	c	0	0	1
6	+	−	+	ac	1	0	1
7	−	+	+	bc	0	1	1
8	+	+	+	abc	1	1	1

There are seven degrees of freedom between the eight treatment combinations in the 2^3 design. Three degrees of freedom are associated with the main effects of A, B, and C. Four degrees of freedom are associated with interactions: one each with AB, AC, and BC and one with ABC.

Consider estimating the main effects. First, consider estimating the main effect A. The effect of A when B and C are at the low level is $[a - (1)]/n$. Similarly, the effect of A when B is at the high level and C is at the low level is $[ab - b]/n$. The effect of A when C is at the high level and B is at the low level is $[ac - c]/n$. Finally, the effect of

■ **FIGURE 6.4 The 2^3 factorial design**



(*a*) Geometric view

Run	Factor A	B	C
1	−	−	−
2	+	−	−
3	−	+	−
4	+	+	−
5	−	−	+
6	+	−	+
7	−	+	+
8	+	+	+

(*b*) Design matrix

A when both B and C are at the high level is $[abc - bc]/n$. Thus, the average effect of A is just the average of these four, or

$$A = \frac{1}{4n}[a - (1) + ab - b + ac - c + abc - bc] \tag{6.11}$$

This equation can also be developed as a contrast between the four treatment combinations in the right face of the cube in Figure 6.5a (where A is at the high level) and the four in the left face (where A is at the low level). That is, the A effect is just the average of the four runs where A is at the high level ($\bar{y}_{A^+}$) minus the average of the four runs where A is at the low level ($\bar{y}_{A^-}$), or

$$\begin{aligned} A &= \bar{y}_{A^+} - \bar{y}_{A^-} \\ &= \frac{a + ab + ac + abc}{4n} - \frac{(1) + b + c + bc}{4n} \end{aligned}$$

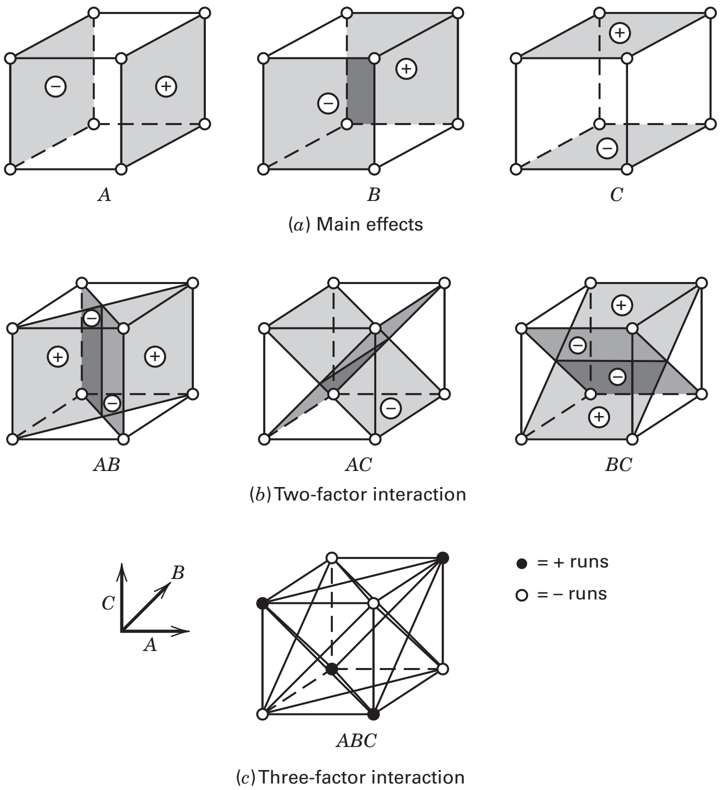
This equation can be rearranged as

$$A = \frac{1}{4n}[a + ab + ac + abc - (1) - b - c - bc]$$

which is identical to Equation 6.11.

In a similar manner, the effect of B is the difference in averages between the four treatment combinations in the front face of the cube and the four in the back. This yields

$$\begin{aligned} B &= \bar{y}_{B^+} - \bar{y}_{B^-} \\ &= \frac{1}{4n}[b + ab + bc + abc - (1) - a - c - ac] \end{aligned} \tag{6.12}$$



■ **FIGURE 6.5 Geometric presentation of contrasts corresponding to the main effects and interactions in the 2^3 design**

The effect of C is the difference in averages between the four treatment combinations in the top face of the cube and the four in the bottom, that is,

$$\begin{aligned} C &= \bar{y}_{C^+} = \bar{y}_{C^-} \\ &= \frac{1}{4n}[c + ac + bc + abc - (1) - a - b - ab] \end{aligned} \tag{6.13}$$

The two-factor interaction effects may be computed easily. A measure of the AB interaction is the difference between the average A effects at the two levels of B. By convention, one-half of this difference is called the AB interaction. Symbolically,

B	**Average A Effect**
High (+)	$\dfrac{[(abc - bc) + (ab - b)]}{2n}$
Low (−)	$\dfrac{\{(ac - c) + [a - (1)]\}}{2n}$
Difference	$\dfrac{[abc - bc + ab - b - ac + c - a + (1)]}{2n}$

Because the AB interaction is one-half of this difference,

$$AB = \frac{[abc - bc + ab - b - ac + c - a + (1)]}{4n} \tag{6.14}$$

We could write Equation 6.14 as follows:

$$AB = \frac{abc + ab + c + (1)}{4n} - \frac{bc + b + ac + a}{4n}$$

In this form, the AB interaction is easily seen to be the difference in averages between runs on two diagonal planes in the cube in Figure 6.5*b*. Using similar logic and referring to Figure 6.5*b*, we find that the AC and BC interactions are

$$AC = \frac{1}{4n}[(1) - a + b - ab - c + ac - bc + abc] \tag{6.15}$$

and

$$BC = \frac{1}{4n}[(1) + a - b - ab - c - ac + bc + abc] \tag{6.16}$$

The ABC interaction is defined as the average difference between the AB interaction at the two different levels of C. Thus,

$$\begin{aligned} ABC &= \frac{1}{4n}\{[abc - bc] - [ac - c] - [ab - b] + [a - (1)]\} \\ &= \frac{1}{4n}[abc - bc - ac + c - ab + b + a - (1)] \end{aligned} \tag{6.17}$$

As before, we can think of the ABC interaction as the difference in two averages. If the runs in the two averages are isolated, they define the vertices of the two tetrahedra that comprise the cube in Figure 6.5*c*.

In Equations 6.11 through 6.17, the quantities in brackets are **contrasts** in the treatment combinations. A table of plus and minus signs can be developed from the contrasts, which is shown in Table 6.3. Signs for the main effects are determined by associating a plus with the high level and a minus with the low level. Once the signs for the main effects have been established, the signs for the remaining columns can be obtained by multiplying the appropriate preceding columns row by row. For example, the signs in the AB column are the product of the A and B column signs in each row. The contrast for any effect can be obtained easily from this table.

Table 6.3 has several interesting properties: (1) Except for column I, every column has an equal number of plus and minus signs. (2) The sum of the products of the signs in any two columns is zero. (3) Column I multiplied times any column leaves that column unchanged. That is, I is an **identity element**. (4) The product of any two columns yields a column in the table. For example, $A \times B = AB$, and

$$AB \times B = AB^2 = A$$

■ **TABLE 6.3**
Algebraic Signs for Calculating Effects in the 2^3 Design

Treatment Combination	Factorial Effect							
	I	*A*	*B*	*AB*	*C*	*AC*	*BC*	*ABC*
(1)	+	−	−	+	−	+	+	−
a	+	+	−	−	−	−	+	+
b	+	−	+	−	−	+	−	+
ab	+	+	+	+	−	−	−	−
c	+	−	−	+	+	−	−	+
ac	+	+	−	−	+	+	−	−
bc	+	−	+	−	+	−	+	−
abc	+	+	+	+	+	+	+	+

We see that the exponents in the products are formed by using **modulus 2** arithmetic. (That is, the exponent can only be 0 or 1; if it is greater than 1, it is reduced by multiples of 2 until it is either 0 or 1.) All of these properties are implied by the **orthogonality** of the 2^3 design and the contrasts used to estimate the effects.

Sums of squares for the effects are easily computed because each effect has a corresponding single-degree-of-freedom contrast. In the 2^3 design with n replicates, the sum of squares for any effect is

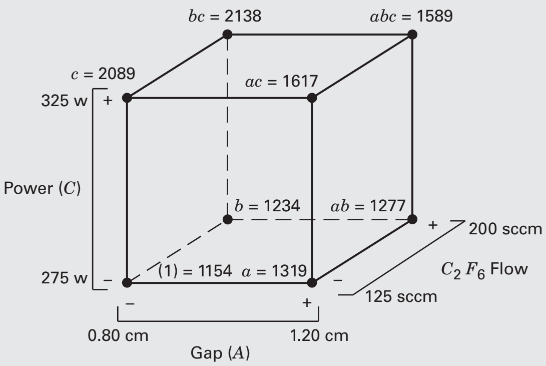
$$SS = \frac{(\text{Contrast})^2}{8n} \tag{6.18}$$

EXAMPLE 6.1 Plasma Etching

A 2^3 factorial design was used to develop a nitride etch process on a single-wafer plasma etching tool. The design factors are the gap between the electrodes, the gas flow (C_2F_6 is used as the reactant gas), and the RF power applied to the cathode (see Figure 3.1 for a schematic of the plasma etch tool). Each factor is run at two levels, and the design is replicated twice. The response variable is the etch rate for silicon nitride (Å/m). The etch rate data are shown in Table 6.4, and the design is shown geometrically in Figure 6.6.

■ **TABLE 6.4**
The Plasma Etch Experiment, Example 6.1

Run	Coded Factors			Etch Rate		Total	Factor Levels		
	A	*B*	*C*	Replicate 1	Replicate 2			Low (−1)	High (+1)
1	−1	−1	−1	550	604	(1) = 1154	*A* (Gap, cm)	0.80	1.20
2	1	−1	−1	669	650	*a* = 1319	*B* (C_2F_6 flow, SCCM)	125	200
3	−1	1	−1	633	601	*b* = 1234	*C* (Power, W)	275	325
4	1	1	−1	642	635	*ab* = 1277			
5	−1	−1	1	1037	1052	*c* = 2089			
6	1	−1	1	749	868	*ac* = 1617			
7	−1	1	1	1075	1063	*bc* = 2138			
8	1	1	1	729	860	*abc* = 1589			



■ **FIGURE 6.6** **The 2^3 design for the plasma etch experiment for Example 6.1**

Using the totals under the treatment combinations shown in Table 6.4, we may estimate the factor effects as follows:

$$A = \frac{1}{4n}[a - (1) + ab - b + ac - c + abc - bc]$$
$$= \frac{1}{8}[1319 - 1154 + 1277 - 1234 + 1617 - 2089 + 1589 - 2138]$$
$$= \frac{1}{8}[-813] = -101.625$$

$$B = \frac{1}{4n}[b + ab + bc + abc - (1) - a - c - ac]$$
$$= \frac{1}{8}[1234 + 1277 + 2138 + 1589 - 1154 - 1319 - 2089 - 1617]$$
$$= \frac{1}{8}[59] = 7.375$$

$$C = \frac{1}{4n}[c + ac + bc + abc - (1) - a - b - ab]$$
$$= \frac{1}{8}[2089 + 1617 + 2138 + 1589 - 1154 - 1319 - 1234 - 1277]$$
$$= \frac{1}{8}[2449] = 306.125$$

$$AB = \frac{1}{4n}[ab - a - b + (1) + abc - bc - ac + c]$$
$$= \frac{1}{8}[1277 - 1319 - 1234 + 1154 + 1589 - 2138 - 1617 + 2089]$$
$$= \frac{1}{8}[-199] = -24.875$$

$$AC = \frac{1}{4n}[(1) - a + b - ab - c + ac - bc + abc]$$
$$= \frac{1}{8}[1154 - 1319 + 1234 - 1277 - 2089 + 1617 - 2138 + 1589]$$
$$= \frac{1}{8}[-1229] = -153.625$$

$$BC = \frac{1}{4n}[(1) + a - b - ab - c - ac + bc + abc]$$
$$= \frac{1}{8}[1154 + 1319 - 1234 - 1277 - 2089 - 1617 + 2138 + 1589]$$
$$= \frac{1}{8}[-17] = -2.125$$

and

$$ABC = \frac{1}{4n}[abc - bc - ac + c - ab + b + a - (1)]$$
$$= \frac{1}{8}[1589 - 2138 - 1617 + 2089 - 1277 + 1234 + 1319 - 1154]$$
$$= \frac{1}{8}[45] = 5.625$$

The largest effects are for power ($C = 306.125$), gap ($A = -101.625$), and the power–gap interaction ($AC = -153.625$).

The sums of squares are calculated from Equation 6.18 as follows:

$$SS_A = \frac{(-813)^2}{16} = 41{,}310.5625$$
$$SS_B = \frac{(59)^2}{16} = 217.5625$$
$$SS_C = \frac{(2449)^2}{16} = 374{,}850.0625$$
$$SS_{AB} = \frac{(-199)^2}{16} = 2475.0625$$
$$SS_{AC} = \frac{(-1229)^2}{16} = 94{,}402.5625$$
$$SS_{BC} = \frac{(-17)^2}{16} = 18.0625$$

and

$$SS_{ABC} = \frac{(45)^2}{16} = 126.5625$$

The total sum of squares is $SS_T = 531{,}420.9375$ and by subtraction $SS_E = 18{,}020.50$. Table 6.5 summarizes the effect estimates and sums of squares. The column labeled "percent contribution" measures the percentage contribution of each model term relative to the total sum of squares. The percentage contribution is often a rough but effective guide to the relative importance of each model term. Note that the main effect of C (Power) really dominates this process, accounting for over 70 percent of the total variability, whereas the main effect of A (Gap) and the AC interaction account for about 8 and 18 percent, respectively.

The ANOVA in Table 6.6 may be used to confirm the magnitude of these effects. We note from Table 6.6 that the main effects of Gap and Power are highly significant (both have very small P-values). The AC interaction is also highly significant; thus, there is a strong interaction between Gap and Power.

■ **TABLE 6.5**

Effect Estimate Summary for Example 6.1

Factor	Effect Estimate	Sum of Squares	Percent Contribution
A	−101.625	41,310.5625	7.7736
B	7.375	217.5625	0.0409
C	306.125	374,850.0625	70.5373
AB	−24.875	2475.0625	0.4657
AC	−153.625	94,402.5625	17.7642
BC	−2.125	18.0625	0.0034
ABC	5.625	126.5625	0.0238

■ **TABLE 6.6**

Analysis of Variance for the Plasma Etching Experiment

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	P-Value
Gap (*A*)	41,310.5625	1	41,310.5625	18.34	0.0027
Gas flow (*B*)	217.5625	1	217.5625	0.10	0.7639
Power (*C*)	374,850.0625	1	374,850.0625	166.41	0.0001
AB	2475.0625	1	2475.0625	1.10	0.3252
AC	94,402.5625	1	94,402.5625	41.91	0.0002
BC	18.0625	1	18.0625	0.01	0.9308
ABC	126.5625	1	126.5625	0.06	0.8186
Error	18,020.5000	8	2252.5625		
Total	531,420.9375	15			

Replication of the 2^3 Design. The experimenter in the plasma etching experiment of Example 6.1 used two replicates of the 2^3 design. This will provide 8 degrees of freedom for pure error. Suppose that effects of size 2σ are of interest, the experimenter wants to consider all main effects and interactions (the full factorial model) and use $\alpha = 0.05$. The JMP power calculations are shown below:

Evaluate Design

Model

Intercept
X1
X2
X3
X1*X2
X1*X3
X2*X3
X1*X2*X3

Power Analysis

Significance Level	0.05
Anticipated RMSE	1

Term	**Anticipated Coefficient**	**Power**
Intercept	1	0.937
X1	1	0.937
X2	1	0.937
X3	1	0.937
X1*X2	1	0.937
X1*X3	1	0.937
X2*X3	1	0.937
X1*X2*X3	1	0.937

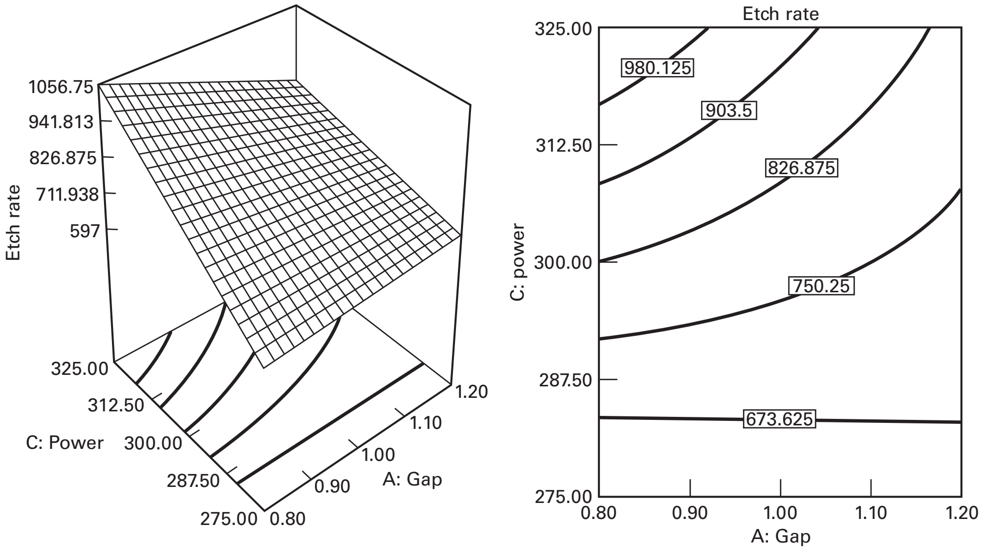
The power of this design is 93.7%. Even if the experimenter decides to use $\alpha = 0.01$ the power is still 72%. Two replicates of the 2^3 design is a good choice for this experiment.

The Regression Model and Response Surface. The regression model for predicting etch rate is

$$\begin{aligned}\hat{y} &= \hat{\beta}_0 + \hat{\beta}_1 x_1 + \hat{\beta}_3 x_3 + \hat{\beta}_{13} x_1 x_3 \\ &= 776.0625 + \left(\frac{-101.625}{2}\right) x_1 + \left(\frac{306.125}{2}\right) x_3 + \left(\frac{-153.625}{2}\right) x_1 x_3\end{aligned}$$

where the coded variables x_1 and x_3 represent A and C, respectively. The x_1x_3 term is the AC interaction. Residuals can be obtained as the difference between observed and predicted etch rate values. We leave the analysis of these residuals as an exercise for the reader.

Figure 6.7 presents the response surface and contour plot for etch rate obtained from the regression model. Notice that because the model contains interaction, the contour lines of constant etch rate are curved (or the response surface is a "twisted" plane). It is desirable to operate this process so that the etch rate is close to 900 Å/m. The contour plot shows that several combinations of gap and power will satisfy this objective. However, it will be necessary to control both of these variables very precisely.



(*a*) The response surface (*b*) The contour plot

■ **FIGURE 6.7 Response surface and contour plot of etch rate for Example 6.1**

Computer Solution. Many statistics software packages are available that will set up and analyze two-level factorial designs. The output from one of these computer programs, Design-Expert, is shown in Table 6.7. In the upper part of the table, an ANOVA for the **full model** is presented. The format of this presentation is somewhat different from the ANOVA results given in Table 6.6. Notice that the first line of the ANOVA is an overall summary for the full model (all main effects and interactions), and the model sum of squares is

$$SS_{\text{Model}} = SS_A + SS_B + SS_C + SS_{AB} + SS_{AC} + SS_{BC} + SS_{ABC} = 5.134 \times 10^5$$

Thus, the statistic

$$F_0 = \frac{MS_{\text{Model}}}{MS_E} = \frac{73{,}342.92}{2252.56} = 32.56$$

is testing the hypotheses

$$H_0: \beta_1 = \beta_2 = \beta_3 = \beta_{12} = \beta_{13} = \beta_{23} = \beta_{123} = 0$$
$$H_1: \text{at least one } \beta \neq 0$$

Because F_0 is large, we would conclude that at least one variable has a nonzero effect. Then each individual factorial effect is tested for significance using the F-statistic. These results agree with Table 6.6.

Below the full model ANOVA in Table 6.7, several R^2 statistics are presented. The ordinary R^2 is

$$R^2 = \frac{SS_{\text{Model}}}{SS_{\text{Total}}} = \frac{5.134 \times 10^5}{5.314 \times 10^5} = 0.9661$$

and it measures the proportion of total variability explained by the model. A potential problem with this statistic is that it always increases as factors are added to the model, even if these factors are not significant. The **adjusted** R^2 statistic, defined as

$$R^2_{\text{Adj}} = 1 - \frac{SS_E/df_E}{SS_{\text{Total}}/df_{\text{Total}}} = 1 - \frac{18{,}020.50/8}{5.314 \times 10^5/15} = 0.9364$$

is a statistic that is adjusted for the "size" of the model, that is, the number of factors. The adjusted R^2 can actually decrease if nonsignificant terms are added to a model. The PRESS statistic is a measure of how well the model will predict new data. (PRESS is actually an acronym for prediction error sum of squares, and it is computed as the sum

■ TABLE 6.7
Design-Expert Output for Example 6.1

Response: Etch rate
ANOVA for Selected Factorial Model
Analysis of variance table [Partial sum of squares]

Source	Sum of Squares	DF	Mean Square	*F* Value	*Prob > F*
Model	5.134E + 005	7	73342.92	32.56	< 0.0001
A	*41310.56*	*1*	*41310.56*	*18.34*	*0.0027*
B	*217.56*	*1*	*217.56*	*0.097*	*0.7639*
C	3.749*E + 005*	*1*	3.749*E + 005*	*166.41*	< 0.0001
AB	*2475.06*	*1*	*2475.06*	*1.10*	*0.3252*
AC	*94402.56*	*1*	*94402.56*	*41.91*	*0.0002*
BC	*18.06*	*1*	*18.06*	*8.019E-003*	*0.9308*
ABC	*126.56*	*1*	*126.56*	*0.056*	*0.8186*
Pure Error	18020.50	8	2252.56		
Cor Total	5.314E + 005	15			

Std. Dev.	47.46	R-Squared	0.9661
Mean	776.06	Adj R-Squared	0.9364
C.V.	6.12	Pred R-Squared	0.8644
PRESS	72082.00	Adeq Precision	14.660

Factor	Coefficient Estimated	DF	Standard Error	95% CI Low	95% CI High	VIF
Intercept	776.06	1	11.87	748.70	803.42	
A-Gap	−50.81	1	11.87	−78.17	−23.45	1.00
B-Gas flow	3.69	1	11.87	−23.67	31.05	1.00
C-Power	153.06	1	11.87	125.70	180.42	1.00
AB	−12.44	1	11.87	−39.80	14.92	1.00
AC	−76.81	1	11.87	−104.17	−49.45	1.00
BC	−1.06	1	11.87	−28.42	26.30	1.00
ABC	2.81	1	11.87	−24.55	30.17	1.00

Final Equation in Terms of Coded Factors:

Etch rate	=
+776.06	
−50.81	* *A*
+3.69	* *B*
+153.06	* *C*
−12.44	* *A* * *B*
−76.81	* *A* * *C*
+1.06	* *B* * *C*
+2.81	* *A* * *B* * *C*

Final Equation in Terms of Actual Factors:

Etch rate	=
−6487.33333	
+5355.41667	* Gap
+6.59667	* Gas flow
+24.10667	* Power
−6.15833	* Gap * Gas flow
−17.80000	* Gap * Power
−0.016133	* Gas flow * Power
+0.015000	* Gap * Gas flow * Power

■ **TABLE 6.7** (*Continued*)

Response: Etch rate
ANOVA for Selected Factorial Model
Analysis of variance table [Partial sum of squares]

Source	Sum of Squares	DF	Mean Square	F Value	Prob > F
Model	5.106E + 005	3	1.702E + 005	97.91	< 0.0001
A	*41310.56*	*1*	*41310.56*	*23.77*	*0.0004*
C	*3.749E + 005*	*1*	*3.749E + 005*	*215.66*	< 0.0001
AC	*94402.56*	*1*	*94402.56*	*54.31*	< 0.0001
Residual	20857.75	12	1738.15		
Lack of Fit	*2837.25*	*4*	*709.31*	*0.31*	*0.8604*
Pure Error	*18020.50*	*8*	*2252.56*		
Cor Total	5.314E + 005	15			

Std. Dev.	41.69	R-Squared	0.9608
Mean	776.06	Adj R-Squared	0.9509
C.V.	5.37	Pred R-Squared	0.9302
PRESS	37080.44	Adeq Precision	22.055

Factor	Coefficient Estimated	DF	Standard Error	95% CI Low	95% CI High	VIF
Intercept	776.06	1	10.42	753.35	798.77	
A-Gap	−50.81	1	10.42	−73.52	28.10	1.00
C-Power	153.06	1	10.42	130.35	175.77	1.00
AC	−76.81	1	10.42	−99.52	−54.10	1.00

Final Equation in Terms of Coded Factors:

Etch rate	=
+776.06	
−50.81	* *A*
+153.06	* *C*
−76.81	* *A* * *C*

Final Equation in Terms of Actual Factors:

Etch rate	=
−5415.37500	
+4354.68750	* Gap
+21.48500	* Power
−15.36250	* Gap * Power

Diagnostics Case Statistics

Standard Order	Actual Value	Predicted Value	Residual	Leverage	Student Residual	Cook's Distance	Outlier *t*	Run Order
1	550.00	597.00	−47.00	0.250	−1.302	0.141	−1.345	9
2	604.00	597.00	7.00	0.250	0.194	0.003	0.186	6
3	669.00	649.00	20.00	0.250	0.554	0.026	0.537	14
4	650.00	649.00	1.00	0.250	0.028	0.000	0.027	1
5	633.00	597.00	36.00	0.250	0.997	0.083	0.997	3
6	601.00	597.00	4.00	0.250	0.111	0.001	0.106	12
7	642.00	649.00	−7.00	0.250	−0.194	0.003	−0.186	13
8	635.00	649.00	−14.00	0.250	−0.388	0.013	−0.374	8
9	1037.00	1056.75	−19.75	0.250	−0.547	0.025	−0.530	5
10	1052.00	1056.75	−4.75	0.250	−0.132	0.001	−0.126	16
11	749.00	801.50	−52.50	0.250	−1.454	0.176	−1.534	2
12	868.00	801.50	66.50	0.250	1.842	0.283	2.082	15
13	1075.00	1056.75	18.25	0.250	0.505	0.021	0.489	4
14	1063.00	1056.75	6.25	0.250	0.173	0.002	0.166	7
15	729.00	801.50	−72.50	0.250	−2.008	0.336	−2.359	10
16	860.00	801.50	58.50	0.250	1.620	0.219	1.755	11

of the squared prediction errors obtained by predicting the ith data point with a model that includes all observations *except* the ith one.) A model with a small value of PRESS indicates that the model is likely to be a good predictor. The "Prediction R^2" statistic is computed as

$$R^2_{\text{Pred}} = 1 - \frac{\text{PRESS}}{SS_{\text{Total}}} = 1 - \frac{72{,}082.00}{5.314 \times 10^5} = 0.8644$$

This indicates that the full model would be expected to explain about 86 percent of the variability in new data.

The next portion of the output presents the regression coefficient for each model term and the **standard error** of each coefficient, defined as

$$se(\hat{\beta}) = \sqrt{V(\hat{\beta})} = \sqrt{\frac{MS_E}{n2^k}} = \sqrt{\frac{MS_E}{N}} = \sqrt{\frac{2252.56}{2(8)}} = 11.87$$

The standard errors of all model coefficients are equal because the design is **orthogonal**. The 95 percent confidence intervals on each regression coefficient are computed from

$$\hat{\beta} - t_{0.025,N-p}se(\hat{\beta}) \leq \beta \leq \hat{\beta} + t_{0.025,N-p}se(\hat{\beta})$$

where the degrees of freedom on t are the number of degrees of freedom for error; that is, N is the total number of runs in the experiment (16), and p is the number of model parameters (8). The full model in terms of both the coded variables and the natural variables is also presented.

The last part of the display in Table 6.7 illustrates the output following the removal of the nonsignificant interaction terms. This **reduced model** now contains only the main effects A, C, and the AC interaction. The **error** or **residual** sum of squares is now composed of a **pure error** component arising from the replication of the eight corners of the cube and a **lack-of-fit** component consisting of the sums of squares for the factors that were dropped from the model (B, AB, BC, and ABC). Once again, the regression model representation of the experimental results is given in terms of both coded and natural variables. The proportion of total variability in etch rate that is explained by this model is

$$R^2 = \frac{SS_{\text{Model}}}{SS_{\text{Total}}} = \frac{5.106 \times 10^5}{5.314 \times 10^5} = 0.9608$$

which is smaller than the R^2 for the full model. Notice, however, that the adjusted R^2 for the reduced model is actually slightly larger than the adjusted R^2 for the full model, and PRESS for the reduced model is considerably smaller, leading to a larger value of R^2_{Pred} for the reduced model. Clearly, removing the nonsignificant terms from the full model has produced a final model that is likely to function more effectively as a predictor of new data. Notice that the confidence intervals on the regression coefficients for the reduced model are shorter than the corresponding confidence intervals for the full model.

The last part of the output presents the residuals from the reduced model. Design-Expert will also construct all of the residual plots that we have previously discussed.

Other Methods for Judging the Significance of Effects. The analysis of variance is a formal way to determine which factor effects are nonzero. Several other methods are useful. Below, we show how to calculate the **standard error of the effects**, and we use these standard errors to construct **confidence intervals** on the effects. Another method, which we will illustrate in Section 6.5, uses **normal probability plots** to assess the importance of the effects.

The standard error of an effect is easy to find. If we assume that there are n replicates at each of the 2^k runs in the design, and if $y_{i1}, y_{i2}, \ldots, y_{in}$ are the observations at the ith run, then

$$S_i^2 = \frac{1}{n-1}\sum_{j=1}^{n}(y_{ij} - \bar{y}_i)^2 \qquad i = 1, 2, \ldots, 2^k$$

is an estimate of the variance at the ith run. The 2^k variance estimates can be combined to give an overall variance estimate:

$$S^2 = \frac{1}{2^k(n-1)} \sum_{i=1}^{2^k} \sum_{j=1}^{n} (y_{ij} - \overline{y}_i)^2 \tag{6.19}$$

This is also the variance estimate given by the error mean square in the analysis of variance. The *variance* of each effect estimate is

$$\begin{aligned} V(\text{Effect}) &= V\left(\frac{\text{Contrast}}{n2^{k-1}}\right) \\ &= \frac{1}{(n2^{k-1})^2} V(\text{Contrast}) \end{aligned}$$

Each contrast is a linear combination of 2^k treatment totals, and each total consists of n observations. Therefore,

$$V(\text{Contrast}) = n2^k\sigma^2$$

and the variance of an effect is

$$V(\text{Effect}) = \frac{1}{(n2^{k-1})^2} n2^k\sigma^2 = \frac{1}{n2^{k-2}}\sigma^2$$

The estimated standard error would be found by replacing σ^2 by its estimate S^2 and taking the square root of this last expression:

$$se(\text{Effect}) = \frac{2S}{\sqrt{n2^k}} \tag{6.20}$$

Notice that the standard error of an effect is twice the standard error of an estimated regression coefficient in the regression model for the 2^k design (see the Design-Expert computer output for Example 6.1). It would be possible to test the significance of any effect by comparing the effect estimates to its standard error:

$$t_0 = \frac{\text{Effect}}{se(\text{Effect})}$$

This is a t statistic with $N - p$ degrees of freedom.

The $100(1 - \alpha)$ percent confidence intervals on the effects are computed from Effect $\pm\, t_{\alpha/2,N-p} se(\text{Effect})$, where the degrees of freedom on t are just the error or residual degrees of freedom ($N - p$ = total number of runs − number of model parameters).

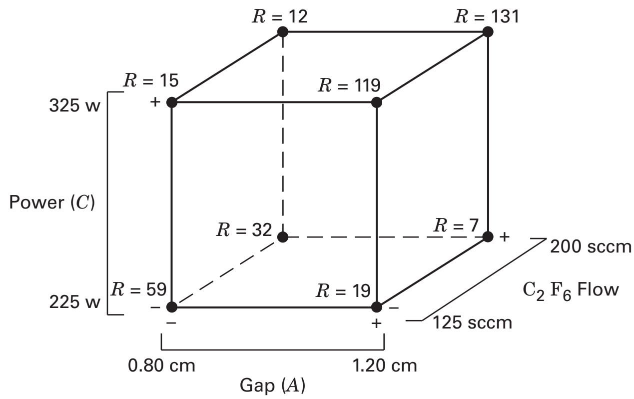
To illustrate this method, consider the plasma etching experiment in Example 6.1. The mean square error for the full model is $MS_E = 2252.56$. Therefore, the standard error of each effect is (using $S^2 = MS_E$)

$$se(\text{Effect}) = \frac{2S}{\sqrt{n2^k}} = \frac{2\sqrt{2252.56}}{\sqrt{2(2^3)}} = 23.73$$

Now $t_{0.025,8} = 2.31$ and $t_{0.025,8} se(\text{Effect}) = 2.31(23.73) = 54.82$, so approximate 95 percent confidence intervals on the factor effects are

$$\begin{aligned} A &: -101.625 \pm 54.82 \\ B &: \quad 7.375 \pm 54.82 \\ C &: \quad 306.125 \pm 54.82 \\ AB &: \quad -24.875 \pm 54.82 \\ AC &: -153.625 \pm 54.82 \\ BC &: \quad -2.125 \pm 54.82 \\ ABC &: \quad 5.625 \pm 54.82 \end{aligned}$$

This analysis indicates that A, C, and AC are important factors because they are the only factor effect estimates for which the approximate 95 percent confidence intervals do not include zero.



■ **FIGURE 6.8** **Ranges of etch rates for Example 6.1**

Dispersion Effects. The process engineer working on the plasma etching tool was also interested in **dispersion effects;** that is, do any of the factors affect variability in etch rate from run to run? One way to answer the question is to look at the **range** of etch rates for each of the eight runs in the 2^3 design. These ranges are plotted on the cube in Figure 6.8. Notice that the ranges in etch rates are much larger when both Gap and Power are at their high levels, indicating that this combination of factor levels may lead to more variability in etch rate than other recipes. Fortunately, etch rates in the desired range of 900 Å/m can be achieved with settings of Gap and Power that avoid this situation.

6.4 The General 2^k Design

The methods of analysis that we have presented thus far may be generalized to the case of a **2^k factorial design**, that is, a design with k factors each at two levels. The statistical model for a 2^k design would include k main effects, $\binom{k}{2}$ two-factor interactions, $\binom{k}{3}$ three-factor interactions, . . . , and one k-factor interaction. That is, the complete model would contain $2^k - 1$ effects for a 2^k design. The notation introduced earlier for treatment combinations is also used here. For example, in a 2^5 design *abd* denotes the treatment combination with factors A, B, and D at the high level and factors C and E at the low level. The treatment combinations may be written in **standard order** by introducing the factors one at a time, with each new factor being successively combined with those that precede it. For example, the standard order for a 2^4 design is (1), $a, b, ab, c, ac, bc, abc, d, ad, bd, abd, cd, acd, bcd$, and $abcd$.

The general approach to the statistical analysis of the 2^k design is summarized in Table 6.8. As we have indicated previously, a computer software package is usually employed in this analysis process.

■ **TABLE 6.8**
Analysis Procedure for a 2^k Design

1. Estimate factor effects
2. Form initial model
   a. If the design is replicated, fit the full model
   b. If there is no replication, form the model using a normal probability plot of the effects
3. Perform statistical testing
4. Refine model
5. Analyze residuals
6. Interpret results

The sequence of steps in Table 6.8 should, by now, be familiar. The first step is to estimate factor effects and examine their signs and magnitudes. This gives the experimenter preliminary information regarding which factors and interactions may be important and in which directions these factors should be adjusted to improve the response. In forming the initial model for the experiment, we usually choose the **full model**, that is, all main effects and interactions, provided that at least one of the design points has been replicated (in the next section, we discuss a modification to this step). Then in step 3, we use the analysis of variance to formally test for the significance of main effects and interaction. Table 6.9 shows the general form of an analysis of variance for a 2^k factorial design with n replicates. Step 4, refine the model, usually consists of removing any nonsignificant variables from the full model. Step 5 is the usual residual analysis to check for model adequacy and assumptions. Sometimes model refinement will occur after residual analysis if we find that the model is inadequate or assumptions are badly violated. The final step usually consists of graphical analysis—either main effect or interaction plots, or response surface and contour plots.

Although the calculations described above are almost always done with a computer, occasionally it is necessary to manually calculate an effect estimate or sum of squares for an effect. To estimate an effect or to compute the sum of squares for an effect, we must first determine the contrast associated with that effect. This can always be done by using a table of plus and minus signs, such as Table 6.2 or Table 6.3. However, this is awkward for large values of k and

■ **TABLE 6.9**
Analysis of Variance for a 2^k Design

Source of Variation	Sum of Squares	Degrees of Freedom
k **main effects**		
A	SS_A	1
B	SS_B	1
$\vdots$	$\vdots$	$\vdots$
K	SS_K	1
$\binom{k}{2}$ **two-factor interactions**		
AB	SS_{AB}	1
AC	SS_{AC}	1
$\vdots$	$\vdots$	$\vdots$
JK	SS_{JK}	1
$\binom{k}{3}$ **three-factor interactions**		
ABC	SS_{ABC}	1
ABD	SS_{ABD}	1
$\vdots$	$\vdots$	$\vdots$
IJK	SS_{IJK}	1
$\vdots$	$\vdots$	$\vdots$
$\binom{k}{k}$ k**-factor interaction**		
$ABC \cdots K$	$SS_{ABC\cdots K}$	1
Error	SS_E	$2^k(n-1)$
Total	SS_T	$n2^k - 1$

we can use an alternate method. In general, we determine the contrast for effect $AB \cdots K$ by expanding the right-hand side of

$$\text{Contrast}_{AB\cdots K} = (a \pm 1)(b \pm 1) \cdots (k \pm 1) \tag{6.21}$$

In expanding Equation 6.21, ordinary algebra is used with "1" being replaced by (1) in the final expression. The sign in each set of parentheses is negative if the factor is included in the effect and positive if the factor is not included.

To illustrate the use of Equation 6.21, consider a 2^3 factorial design. The contrast for AB would be

$$\begin{aligned}\text{Contrast}_{AB} &= (a - 1)(b - 1)(c + 1)\\ &= abc + ab + c + (1) - ac - bc - a - b\end{aligned}$$

As a further example, in a 2^5 design, the contrast for $ABCD$ would be

$$\begin{aligned}\text{Contrast}_{ABCD} &= (a - 1)(b - 1)(c - 1)(d - 1)(e + 1)\\ &= abcde + cde + bde + ade + bce\\ &\quad + ace + abe + e + abcd + cd + bd\\ &\quad + ad + bc + ac + ab + (1) - a - b - c\\ &\quad - abc - d - abd - acd - bcd - ae\\ &\quad - be - ce - abce - de - abde - acde - bcde\end{aligned}$$

Once the contrasts for the effects have been computed, we may estimate the effects and compute the sums of squares according to

$$AB \cdots K = \frac{2}{n2^k}(\text{Contrast}_{AB\cdots K}) \tag{6.22}$$

and

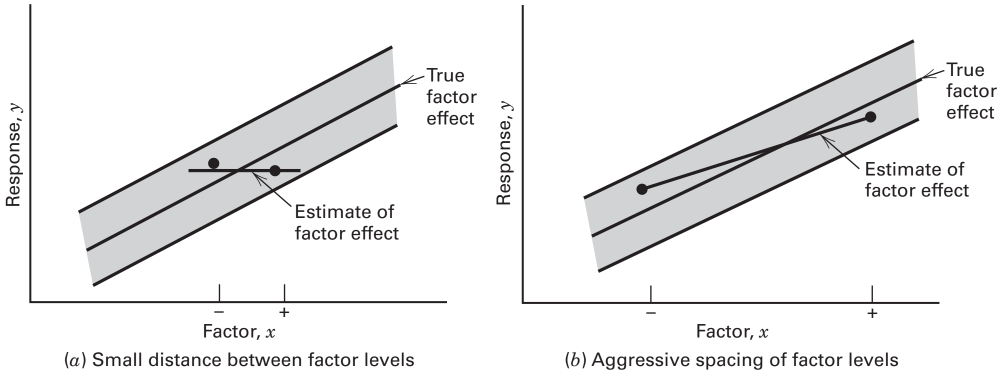
$$SS_{AB\cdots K} = \frac{1}{n2^k}(\text{Contrast}_{AB\cdots K})^2 \tag{6.23}$$

respectively, where n denotes the number of replicates. There is also a tabular algorithm due to Frank Yates that can occasionally be useful for manual calculation of the effect estimates and the sums of squares. Refer to the **supplemental text material** for this chapter.

6.5 A Single Replicate of the 2^k Design

For even a moderate number of factors, the total number of treatment combinations in a 2^k factorial design is large. For example, a 2^5 design has 32 treatment combinations, a 2^6 design has 64 treatment combinations, and so on. Because resources are usually limited, the number of replicates that the experimenter can employ may be restricted. Frequently, available resources only allow a **single replicate** of the design to be run, unless the experimenter is willing to omit some of the original factors.

An obvious risk when conducting an experiment that has only one run at each test combination is that we may be fitting a model to noise. That is, if the response y is highly variable, misleading conclusions may result from the experiment. The situation is illustrated in Figure 6.9*a*. In this figure, the straight line represents the true factor effect. However, because of the random variability present in the response variable (represented by the shaded band), the experimenter actually obtains the two measured responses represented by the dark dots. Consequently, the estimated factor effect is close to zero, and the experimenter has reached an erroneous conclusion concerning this factor. Now if there is less variability in the response, the likelihood of an erroneous conclusion will be smaller. Another way to ensure that reliable effect estimates are obtained is to increase the distance between the low (−) and high (+) levels of the factor, as illustrated in Figure 6.9*b*. Notice that in this figure, the increased distance between the low and high factor levels results in a reasonable estimate of the true factor effect.



■ **FIGURE 6.9 The impact of the choice of factor levels in an unreplicated design**

The single-replicate strategy is often used in screening experiments when there are relatively many factors under consideration. Because we can never be entirely certain in such cases that the experimental error is small, a good practice in these types of experiments is to spread out the factor levels aggressively. You might find it helpful to reread the guidance on choosing factor levels in Chapter 1.

A single replicate of a 2^k design is sometimes called an **unreplicated factorial**. With only one replicate, there is no internal estimate of error (or "pure error"). One approach to the analysis of an unreplicated factorial is to assume that certain high-order interactions are negligible and combine their mean squares to estimate the error. This is an appeal to the **sparsity of effects principle**; that is, most systems are dominated by some of the main effects and low-order interactions, and most high-order interactions are negligible.

While the effect sparsity principle has been observed by experimenters for many decades, only recently has it been studied more objectively. A paper by Li, Sudarsanam, and Frey (2006) studied 113 response variables obtained from 43 published experiments from a wide range of science and engineering disciplines. All of the experiments were full factorials with between three and seven factors, so no assumptions had to be made about interactions. Most of the experiments had either three or four factors. The authors found that about 40 percent of the main effects in the experiments they studied were significant, while only about 11 percent of the two-factor interactions were significant. Three-factor interactions were very rare, occurring only about 5 percent of the time. The authors also investigated the absolute values of factor effects for main effects, two-factor interactions, and three-factor interactions. The median of main effect strength was about four times larger than the median strength of two-factor interactions. The median strength of two-factor interactions was more than two times larger than the median strength of three-factor interactions. However, there were many two- and three-factor interactions that were larger than the median main effect. Another paper by Bergquist, Vanhatalo, and Nordenvaad (2011) also studied the effect of the sparsity question using 22 different experiments with 35 responses. They considered both full factorial and fractional factorial designs with factors at two levels. Their results largely agree with those of Li et al. (2006), with the exception that three-factor interactions were less frequent, occurring only about 2 percent of the time. This difference may be partially explained by the inclusion of experiments with indications of curvature and the need for transformations in the Li et al. (2006) study. Bergquist et al. (2011) excluded such experiments. Overall, both of these studies confirm the validity of the sparsity of effects principle.

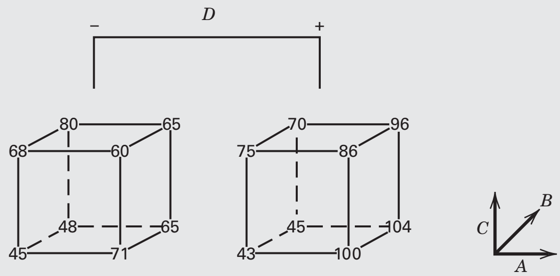
When analyzing data from unreplicated factorial designs, occasionally real high-order interactions occur. The use of an error mean square obtained by pooling high-order interactions is inappropriate in these cases. A method of analysis attributed to Daniel (1959) provides a simple way to overcome this problem. Daniel suggests examining a **normal probability plot** of the estimates of the effects. The effects that are negligible are normally distributed, with mean zero and variance σ^2 and will tend to fall along a straight line on this plot, whereas significant effects will have nonzero means and will not lie along the straight line. Thus, the preliminary model will be specified to contain those effects that are apparently nonzero, based on the normal probability plot. The apparently negligible effects are combined as an estimate of error.

EXAMPLE 6.2 A Single Replicate of the 2^4 Design

A chemical product is produced in a pressure vessel. A factorial experiment is carried out in the pilot plant to study the factors thought to influence the filtration rate of this product. The four factors are temperature (A), pressure (B), concentration of formaldehyde (C), and stirring rate (D). Each factor is present at two levels. The design matrix and the response data obtained from a single replicate of the 2^4 experiment are shown in Table 6.10 and Figure 6.10. The 16 runs are made in random order. The process engineer is interested in maximizing the filtration rate. Current process conditions give filtration rates of around 75 gal/h. The process also currently uses the concentration of formaldehyde, factor C, at the high level. The engineer would like to reduce the formaldehyde concentration as much as possible but has been unable to do so because it always results in lower filtration rates.

We will begin the analysis of these data by constructing a normal probability plot of the effect estimates. The table of plus and minus signs for the contrast constants for the 2^4 design are shown in Table 6.11. From these contrasts, we may estimate the 15 factorial effects and the sums of squares shown in Table 6.12.

The normal probability plot of these effects is shown in Figure 6.11. All of the effects that lie along the line are negligible, whereas the large effects are far from the line. The important effects that emerge from this analysis are the main effects of A, C, and D and the AC and AD interactions.



■ **FIGURE 6.10 Data from the pilot plant filtration rate experiment for Example 6.2**

■ **TABLE 6.10**
Pilot Plant Filtration Rate Experiment

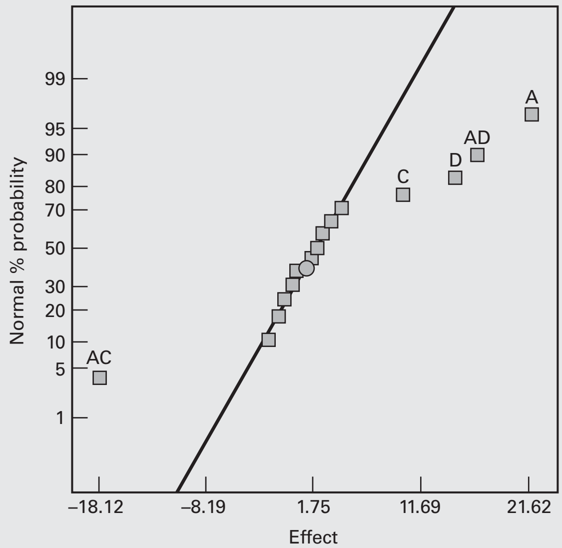
Run Number	Factor *A*	Factor *B*	Factor *C*	Factor *D*	Run Label	Filtration Rate (gal/h)
1	−	−	−	−	(1)	45
2	+	−	−	−	*a*	71
3	−	+	−	−	*b*	48
4	+	+	−	−	*ab*	65
5	−	−	+	−	*c*	68
6	+	−	+	−	*ac*	60
7	−	+	+	−	*bc*	80
8	+	+	+	−	*abc*	65
9	−	−	−	+	*d*	43
10	+	−	−	+	*ad*	100
11	−	+	−	+	*bd*	45
12	+	+	−	+	*abd*	104
13	−	−	+	+	*cd*	75
14	+	−	+	+	*acd*	86
15	−	+	+	+	*bcd*	70
16	+	+	+	+	*abcd*	96

■ **TABLE 6.11**
Contrast Constants for the 2^4 Design

	A	*B*	*AB*	*C*	*AC*	*BC*	*ABC*	*D*	*AD*	*BD*	*ABD*	*CD*	*ACD*	*BCD*	*ABCD*
(1)	−	−	+	−	+	+	−	−	+	+	−	+	−	−	+
a	+	−	−	−	−	+	+	−	−	+	+	+	+	−	−
b	−	+	−	−	+	−	+	−	+	−	+	+	−	+	−
ab	+	+	+	−	−	−	−	−	−	−	−	+	+	+	+
c	−	−	+	+	−	−	+	−	+	+	−	−	+	+	−
ac	+	−	−	+	+	−	−	−	−	+	+	−	−	+	+
bc	−	+	−	+	−	+	−	−	+	−	+	−	+	−	+
abc	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−
d	−	−	+	−	+	+	−	+	−	−	+	−	+	+	−
ad	+	−	−	−	−	+	+	+	+	−	−	−	−	+	+
bd	−	+	−	−	+	−	+	+	−	+	−	−	+	−	+
abd	+	+	+	−	−	−	−	+	+	+	+	−	−	−	−
cd	−	−	+	+	−	−	+	+	−	−	+	+	−	−	+
acd	+	−	−	+	+	−	−	+	+	−	−	+	+	−	−
bcd	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−
abcd	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+

■ **TABLE 6.12**
Factor Effect Estimates and Sums of Squares for the 2^4 Factorial in Example 6.2

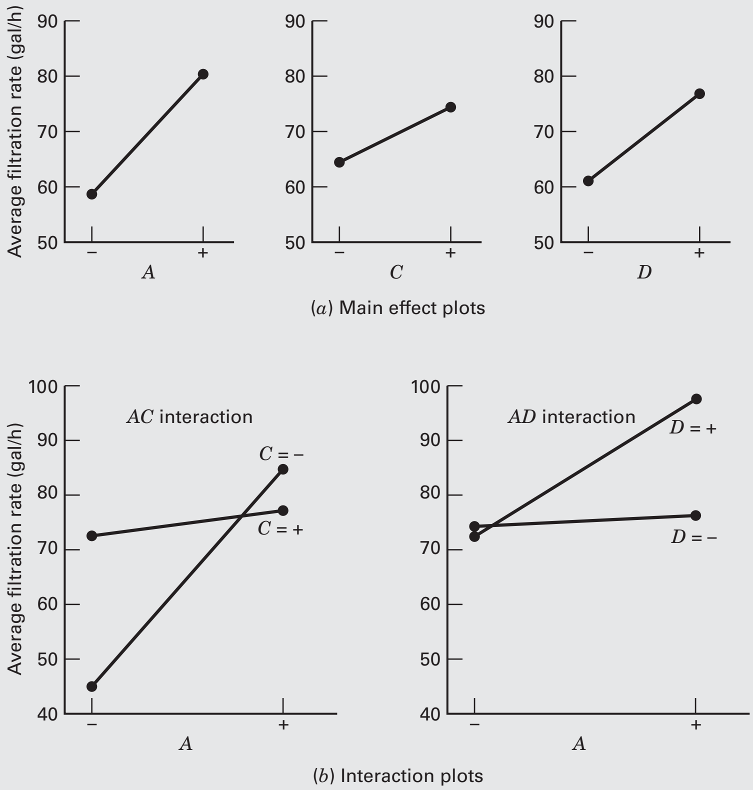
Model Term	Effect Estimate	Sum of Squares	Percent Contribution
A	21.625	1870.56	32.6397
B	3.125	39.0625	0.681608
C	9.875	390.062	6.80626
D	14.625	855.563	14.9288
AB	0.125	0.0625	0.00109057
AC	−18.125	1314.06	22.9293
AD	16.625	1105.56	19.2911
BC	2.375	22.5625	0.393696
BD	−0.375	0.5625	0.00981515
CD	−1.125	5.0625	0.0883363
ABC	1.875	14.0625	0.245379
ABD	4.125	68.0625	1.18763
ACD	−1.625	10.5625	0.184307
BCD	−2.625	27.5625	0.480942
ABCD	1.375	7.5625	0.131959



■ **FIGURE 6.11** **Normal probability plot of the effects for the 2^4 factorial in Example 6.2**

The main effects of *A*, *C*, and *D* are plotted in Figure 6.12*a*. All three effects are positive, and if we considered only these main effects, we would run all three factors at the high level to maximize the filtration rate. However, it is always necessary to examine any interactions that are important. Remember that main effects do not have much meaning when they are involved in significant interactions.

The *AC* and *AD* interactions are plotted in Figure 6.12*b*. These interactions are the key to solving the problem. Note from the *AC* interaction that the temperature effect is very small when the concentration is at the high level and very large when the concentration is at the low level, with the best results obtained with low concentration and high temperature. The *AD* interaction indicates that stirring rate *D* has little effect at low temperature but a large positive effect at high temperature. Therefore, the best filtration rates would appear to be obtained when *A* and *D* are at the high level and *C* is at the low level. This would allow the reduction of the formaldehyde concentration to a lower level, another objective of the experimenter.



■ **FIGURE 6.12 Main effect and interaction plots for Example 6.2**

The use of normal probability plot is not without criticism. If none of the effects are very large (say larger than 2σ), then the plot may be ambiguous and hard to interpret. If there are few effects, in say an eight-run design, the plot may be of little help.

Design Projection. Another interpretation of the effects in Figure 6.11 is possible. Because *B* (pressure) is not significant and all interactions involving *B* are negligible, we may discard *B* from the experiment so that the design becomes a 2^3 factorial in *A*, *C*, and *D* with two replicates. This is easily seen from examining only columns *A*, *C*, and

■ **TABLE 6.13**
Analysis of Variance for the Pilot Plant Filtration Rate Experiment in *A*, *C*, and *D*

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	*P*-Value
A	1870.56	1	1870.56	83.36	< 0.0001
C	390.06	1	390.06	17.38	< 0.0001
D	855.56	1	855.56	38.13	< 0.0001
AC	1314.06	1	1314.06	58.56	< 0.0001
AD	1105.56	1	1105.56	49.27	< 0.0001
CD	5.06	1	5.06	< 1	
ACD	10.56	1	10.56	< 1	
Error	179.52	8	22.44		
Total	5730.94	15			

D in the design matrix shown in Table 6.10 and noting that those columns form two replicates of a 2^3 design. The analysis of variance for the data using this simplifying assumption is summarized in Table 6.13. The conclusions that we would draw from this analysis are essentially unchanged from those of Example 6.2. Note that by projecting the single replicate of the 2^4 into a replicated 2^3, we now have both an estimate of the *ACD* interaction and an estimate of error based on what is sometimes called **hidden replication**.

The concept of projecting an unreplicated factorial into a replicated factorial in fewer factors is very useful. In general, if we have a single replicate of a 2^k design, and if $h(h < k)$ factors are negligible and can be dropped, then the original data correspond to a full two-level factorial in the remaining $k - h$ factors with 2^h replicates.

Diagnostic Checking. The usual diagnostic checks should be applied to the residuals of a 2^k design. Our analysis indicates that the only significant effects are $A = 21.625$, $C = 9.875$, $D = 14.625$, $AC = -18.125$, and $AD = 16.625$. If this is true, the estimated filtration rates are given by

$$\hat{y} = 70.06 + \left(\frac{21.625}{2}\right)x_1 + \left(\frac{9.875}{2}\right)x_3 + \left(\frac{14.625}{2}\right)x_4 - \left(\frac{18.125}{2}\right)x_1x_3 + \left(\frac{16.625}{2}\right)x_1x_4$$

where 70.06 is the average response, and the coded variables x_1, x_3, x_4 take on values between -1 and $+1$. The predicted filtration rate at run (1) is

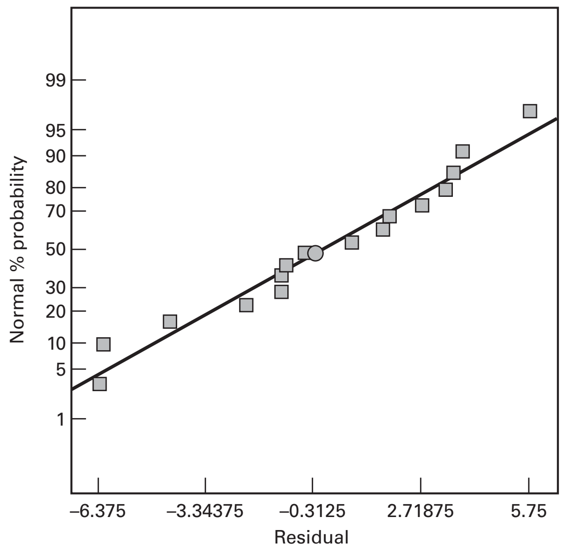
$$\begin{aligned}\hat{y} &= 70.06 + \left(\frac{21.625}{2}\right)(-1) + \left(\frac{9.875}{2}\right)(-1) + \left(\frac{14.625}{2}\right)(-1) \\ &\quad - \left(\frac{18.125}{2}\right)(-1)(-1) + \left(\frac{16.625}{2}\right)(-1)(-1) \\ &= 46.22\end{aligned}$$

Because the observed value is 45, the residual is $e = y - \hat{y} = 45 - 46.25 = -1.25$. The values of y, $\hat{y}$, and $e = y - \hat{y}$ for all 16 observations are as follows:

	y	$\hat{y}$	$e = y - \hat{y}$
(1)	45	46.25	−1.25
a	71	69.38	1.63
b	48	46.25	1.75
ab	65	69.38	−4.38
c	68	74.25	−6.25
ac	60	61.13	−1.13
bc	80	74.25	5.75
abc	65	61.13	3.88
d	43	44.25	−1.25
ad	100	100.63	−0.63
bd	45	44.25	0.75
abd	104	100.63	3.38
cd	75	72.25	2.75
acd	86	92.38	−6.38
bcd	70	72.25	−2.25
abcd	96	92.38	3.63

A normal probability plot of the residuals is shown in Figure 6.13. The points on this plot lie reasonably close to a straight line, lending support to our conclusion that *A*, *C*, *D*, *AC*, and *AD* are the only significant effects and that the underlying assumptions of the analysis are satisfied.

■ **FIGURE 6.13 Normal probability plot of residuals for Example 6.2**



The Response Surface. We used the interaction plots in Figure 6.12 to provide a practical interpretation of the results of this experiment. Sometimes we find it helpful to use the response surface for this purpose. The response surface is generated by the regression model

$$\hat{y} = 70.06 + \left(\frac{21.625}{2}\right)x_1 + \left(\frac{9.875}{2}\right)x_3 + \left(\frac{14.625}{2}\right)x_4 - \left(\frac{18.125}{2}\right)x_1x_3 + \left(\frac{16.625}{2}\right)x_1x_4$$

Figure 6.14*a* shows the response surface contour plot when stirring rate is at the high level (i.e., $x_4 = 1$). The contours are generated from the above model with $x_4 = 1$, or

$$\hat{y} = 77.3725 + \left(\frac{38.25}{2}\right)x_1 + \left(\frac{9.875}{2}\right)x_3 - \left(\frac{18.125}{2}\right)x_1x_3$$

Notice that the contours are curved lines because the model contains an interaction term.

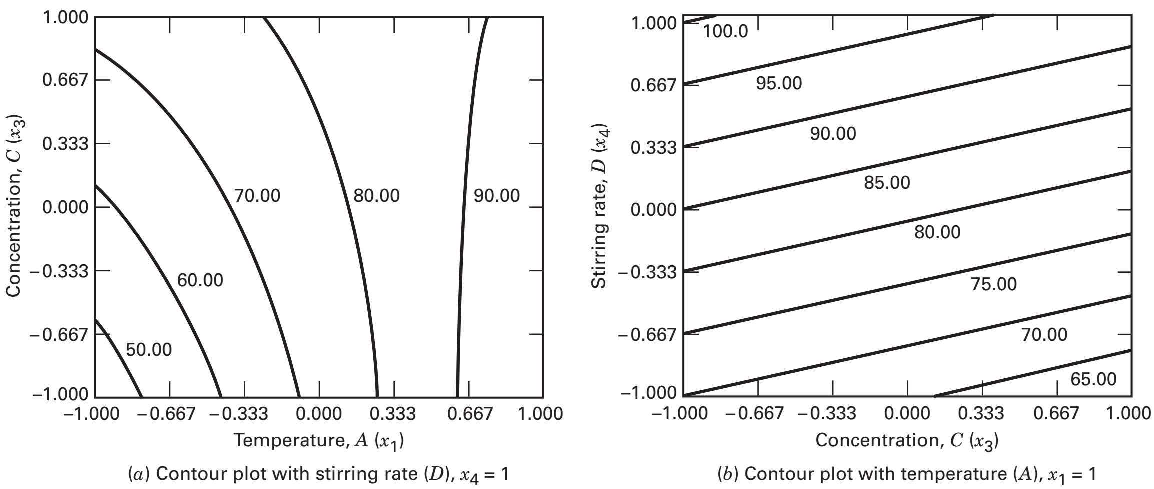
Figure 6.14*b* is the response surface contour plot when temperature is at the high level (i.e., $x_1 = 1$). When we put $x_1 = 1$ in the regression model, we obtain

$$\hat{y} = 80.8725 - \left(\frac{8.25}{2}\right)x_3 + \left(\frac{31.25}{2}\right)x_4$$

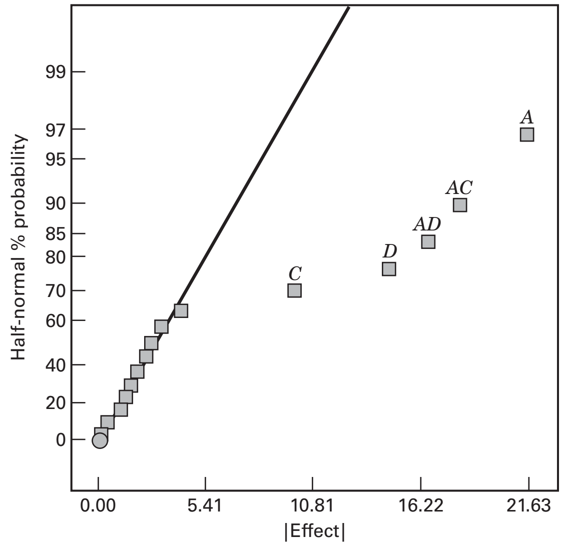
These contours are parallel straight lines because the model contains only the main effects of factors C (x_3) and D (x_4).

Both contour plots indicate that if we want to maximize the filtration rate, variables A (x_1) and D (x_4) should be at the high level and that the process is relatively robust to concentration C. We obtained similar conclusions from the interaction graphs.

The Half-Normal Plot of Effects. An alternative to the normal probability plot of the factor effects is the **half-normal plot**. This is a plot of the **absolute value** of the effect estimates against their cumulative normal probabilities. Figure 6.15 presents the half-normal plot of the effects for Example 6.2. The straight line on the half-normal plot always passes through the origin and should also pass close to the fiftieth percentile data value. Many analysts feel that the half-normal plot is easier to interpret, particularly when there are only a few effect estimates such as when the experimenter has used an eight-run design. Some software packages will construct both plots.



■ **FIGURE 6.14 Contour plots of filtration rate, Example 6.2**



■ **FIGURE 6.15 Half-normal plot of the factor effects from Example 6.2**

Other Methods for Analyzing Unreplicated Factorials. A widely used analysis procedure for an unreplicated two-level factorial design is the normal (or half-normal) plot of the estimated factor effects. However, unreplicated designs are so widely used in practice that many formal analysis procedures have been proposed to overcome the subjectivity of the normal probability plot. Hamada and Balakrishnan (1998) compared some of these methods. They found that the method proposed by Lenth (1989) has good power to detect significant effects. It is also easy to implement, and as a result it appears in several software packages for analyzing data from unreplicated factorials. We give a brief description of **Lenth's method**.

Suppose that we have m contrasts of interest, say $c_1, c_2, \ldots, c_m$. If the design is an unreplicated 2^k factorial design, these contrasts correspond to the $m = 2^k - 1$ factor effect estimates. The basis of Lenth's method is to estimate the variance of a contrast from the smallest (in absolute value) contrast estimates. Let

$$s_0 = 1.5 \times \text{median}(|c_j|)$$

and

$$PSE = 1.5 \times \text{median}(|c_j| : |c_j| < 2.5s_0)$$

PSE is called the "pseudostandard error," and Lenth shows that it is a reasonable estimator of the contrast variance when there are only a few active (significant) effects. The *PSE* is used to judge the significance of contrasts. An individual contrast can be compared to the **margin of error**

$$ME = t_{0.025,d} \times PSE$$

where the degrees of freedom are defined as $d = m/3$. For inference on a group of contrasts, Lenth suggests using the **simultaneous margin of error**

$$SME = t_{\gamma,d} \times PSE$$

where the percentage point of the t distribution used is $\gamma = 1 - (1 + 0.95^{1/m})/2$.

To illustrate Lenth's method, consider the 2^4 experiment in Example 6.2. The calculations result in $s_0 = 1.5 \times |-2.625| = 3.9375$ and $2.5 \times 3.9375 = 9.84375$, so

$$PSE = 1.5 \times |1.75| = 2.625$$
$$ME = 2.571 \times 2.625 = 6.75$$
$$SME = 5.219 \times 2.625 = 13.70$$

Now consider the effect estimates in Table 6.12. The *SME* criterion would indicate that the four largest effects (in magnitude) are significant because their effect estimates exceed *SME*. The main effect of *C* is significant according to the *ME* criterion, but not with respect to *SME*. However, because the *AC* interaction is clearly important, we would probably include *C* in the list of significant effects. Notice that in this example, Lenth's method has produced the same answer that we obtained previously from examination of the normal probability plot of effects.

Several authors [see Loughin and Nobel (1997), Hamada and Balakrishnan (1998), Larntz and Whitcomb (1998), Loughin (1998), and Edwards and Mee (2008)] have observed that Lenth's method results in values of ME and SME that are too conservative and have little power to detect significant effects. Simulation methods can be used to calibrate his procedure. Larntz and Whitcomb (1998) suggest replacing the original *ME* and *SME* multipliers with **adjusted multipliers** as follows:

Number of Contrasts	7	15	31
Original *ME*	3.764	2.571	2.218
Adjusted *ME*	2.295	2.140	2.082
Original *SME*	9.008	5.219	4.218
Adjusted *SME*	4.891	4.163	4.030

These are in close agreement with the results in Ye and Hamada (2000).

The JMP software package implements Lenth's method as part of the screening platform analysis procedure for two-level designs. In their implementation, *P*-values for each factor and interaction are computed from a "real-time" simulation. This simulation assumes that none of the factors in the experiment are significant and calculates the observed value of the Lenth statistic 10,000 times for this null model. Then *P*-values are obtained by determining where the observed Lenth statistics fall relative to the tails of these simulation-based reference distributions. These *P*-values can be used as guidance in selecting factors for the model. Table 6.14 shows the JMP output from the screening analysis platform for the resin filtration rate experiment in Example 6.2. Notice that in addition to the Lenth statistics, the JMP output includes a half-normal plot of the effects and a "Pareto" chart of the effect (contrast) magnitudes. When the factors are entered into the model, the Lenth procedure would recommend including the same factors in the model that we identified previously.

The final JMP output for the fitted model is shown in Table 6.15. The **Prediction Profiler** at the bottom of the table has been set to the levels of the factors that maximize filtration rate. These are the same settings that we determined earlier by looking at the contour plots.

In general, the Lenth method is a clever and very useful procedure. However, we recommend using it as a **supplement** to the usual normal probability plot of effects, not as a replacement for it.

Bisgaard (1998–1999) has provided a nice graphical technique, called a **conditional inference chart**, to assist in interpreting the normal probability plot. The purpose of the graph is to help the experimenter in judging significant effects. This would be relatively easy if the standard deviation σ were known, or if it could be estimated from the data. In unreplicated designs, there is no internal estimate of σ, so the conditional inference chart is designed to help the experimenter evaluate effect magnitude for a *range* of standard deviation values. Bisgaard bases the graph on the result that the standard error of an effect in a two-level design with N runs (for an unreplicated factorial, $N = 2^k$) is

$$\frac{2\sigma}{\sqrt{N}}$$

where σ is the standard deviation of an individual observation. Then ± 2 times the standard error of an effect is

$$\pm\frac{4\sigma}{\sqrt{N}}$$

■ **TABLE 6.14**

JMP Screening Platform Output for Example 6.2

Response Y

Summary of Fit

RSquare	1
RSquare Adj	-
Root Mean Square Error	-
Mean of Response	70.0625
Observations (or Sum Wgts)	16

Sorted Parameter Estimates

Term	Estimate	Relative Std Error	Pseudo t-Ratio	Pseudo t-Ratio	Pseudo p-Value
Temp	10.8125	0.25	8.24		0.0004*
Temp*Conc	−9.0625	0.25	−6.90		0.0010*
Temp*StirR	8.3125	0.25	6.33		0.0014*
StirR	7.3125	0.25	5.57		0.0026*
Conc	4.9375	0.25	3.76		0.0131*
Temp*Pressure*StirR	2.0625	0.25	1.57		0.1769
Pressure	1.5625	0.25	1.19		0.2873
Pressure*Conc*StirR	−1.3125	0.25	−1.00		0.3632
Pressure*Conc	1.1875	0.25	0.90		0.4071
Temp*Pressure*Conc	0.9375	0.25	0.71		0.5070
Temp*Conc*StirR	−0.8125	0.25	−0.62		0.5630
Temp*Pressure*Conc*StirR	0.6875	0.25	0.52		0.6228
Conc*StirR	−0.5625	0.25	−0.43		0.6861
Pressure*StirR	−0.1875	0.25	−0.14		0.8920
Temp*Pressure	0.0625	0.25	0.05		0.9639

No error degrees of freedom, so ordinary tests uncomputable. Relative Std Error corresponds to residual standard error of 1.

Pseudo t-Ratio and p-Value calculated using Lenth PSE = 1.3125 and DFE = 5

Effect Screening

The parameter estimates have equal variances.

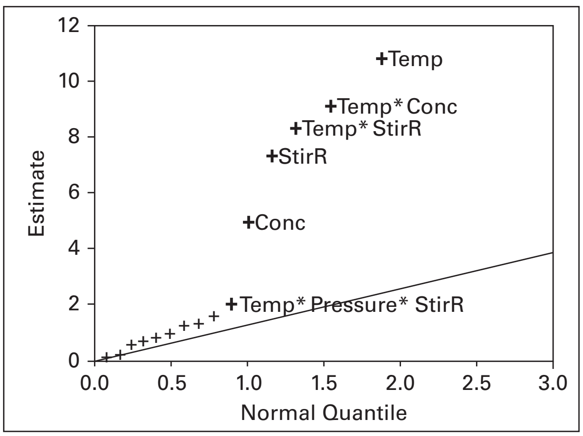
The parameter estimates are not correlated.

Lenth PSE

1.3125

Orthog t Test used Pseudo Standard Error

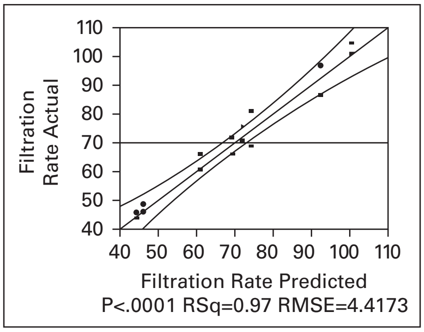
Normal Plot



Blue line is Lenth's PSE, from the estimates population

■ TABLE 6.15
JMP Output for the Fitted Model Example 6.2

Response Filtration Rate Actual by Predicted Plot



Summary of Fit

RSquare	0.965952
RSquare Adj	0.948929
Root Mean Square Error	4.417296
Mean of Response	70.0625
Observations (or Sum Wgts)	16

Analysis of Variance

Source	DF	Sum of Squares	Mean Square	F Ratio
Model	5	5535.8125	1107.16	56.7412
Error	10	195.1250	19.51	**Prob > F**
C. Total	15	5730.9375		<.0001*

Lack of Fit

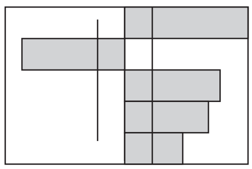
Source	DF	Sum of Squares	Mean Square	F Ratio
				0.3482
Lack of Fit	2	15.62500	7.8125	**Prob > F**
Pure Error	8	179.50000	22.4375	0.7162
Total Error	10	195.12500		**Max RSq**
				0.9687

Parameter Estimates

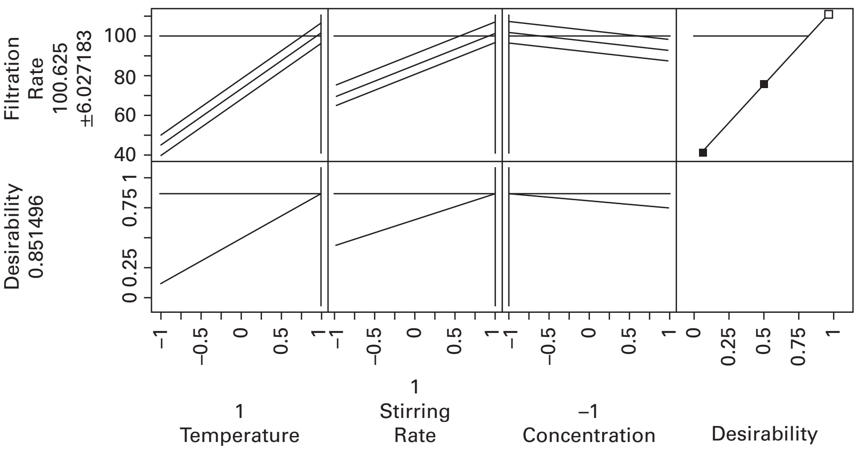
Term	Estimate	Std Error	t Ratio	Prob>|t|
Intercept	70.0625	1.104324	63.44	<.0001*
Temperature	10.8125	1.104324	9.79	<.0001*
Stirring Rate	7.3125	1.104324	6.62	<.0001*
Concentration	4.9375	1.104324	4.47	0.0012*
Temperature *Stirring Rate	8.3125	1.104324	7.53	<.0001*
Temperature *Concentration	−9.0625	1.104324	−8.21	<.0001*

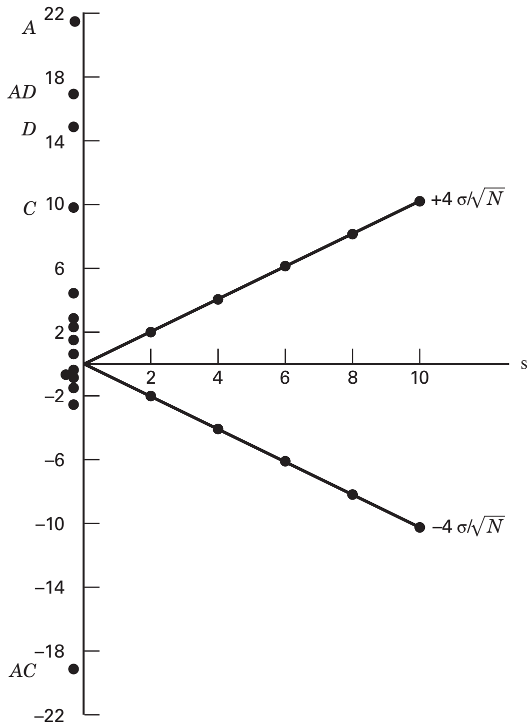
Sorted Parameter Estimates

Term	Estimate	Std Error	t Ratio	Prob < |t|
Temperature	10.8125	1.104324	9.79	<. 0001*
Temperature *Concentration	−9.0625	1.104324	−8.21	<. 0001*
Temperature *Stirring Rate	8.3125	1.104324	7.53	<. 0001*
Stirring Rate	7.3125	1.104324	6.62	<. 0001*
Concentration	4.9375	1.104324	4.47	0.0012*



Prediction Profiler





■ **FIGURE 6.16** **A conditional inference chart for Example 6.2**

Once the effects are estimated, plot a graph as shown in Figure 6.16, with the effect estimates plotted along the vertical or y-axis. In this figure, we have used the effect estimates from Example 6.2. The horizontal, or x-axis, of Figure 6.16 is a standard deviation (σ) scale. The two lines are at

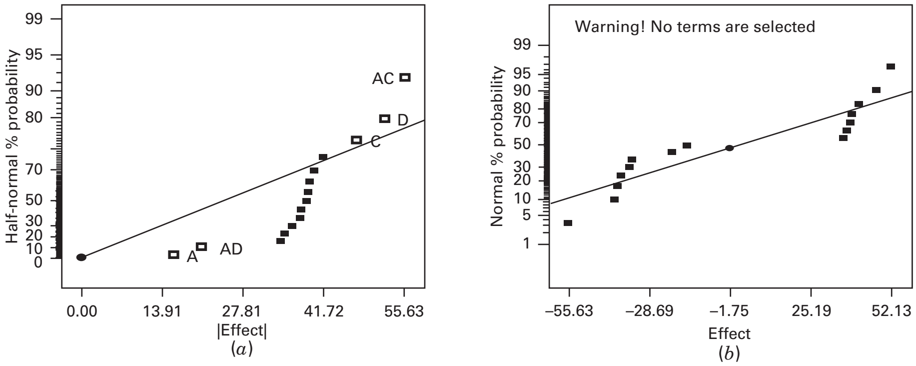
$$y = +\frac{4\sigma}{\sqrt{N}} \quad \text{and} \quad y = -\frac{4\sigma}{\sqrt{N}}$$

In our example, $N = 16$, so the lines are at $y = +\sigma$ and $y = -\sigma$. Thus, for any given value of the standard deviation σ, we can read off the distance between these two lines as an approximate 95 percent confidence interval on the negligible effects.

In Figure 6.16, we observe that if the experimenter thinks that the standard deviation is between 4 and 8, then factors A, C, D, and the AC and AD interactions are significant. If he or she thinks that the standard deviation is as large as 10, factor C may not be significant. That is, for any given assumption about the magnitude of σ, the experimenter can construct a "yardstick" for judging the approximate significance of effects. The chart can also be used in reverse. For example, suppose that we were uncertain about whether factor C is significant. The experimenter could then ask whether it is reasonable to expect that σ could be as large as 10 or more. If it is unlikely that σ is as large as 10, then we can conclude that C is significant.

Effect of Outliers in Unreplicated Designs. Experimenters often worry about the impact of outliers in unreplicated designs, concerned that the outlier will invalidate the analysis and render the results of the experiment useless. This usually isn't a major concern. The reason for this is that the effect estimates are reasonably robust to outliers. To see this, consider an unreplicated 2^4 design with an outlier for (say) the cd treatment combination. The effect of any factor, say for example A, is

$$A = \overline{y}_{A+} - \overline{y}_{A-}$$



■ **FIGURE 6.17** **The effect of outliers. (*a*) Half-normal probability plot (*b*) Normal probability plot**

and the *cd* response appears in only one of the averages, in this case $\bar{y}_{A^-}$. The average $\bar{y}_{A^-}$ is an average of eight observations (half of the 16 runs in the 2^4), so the impact of the outlier *cd* is damped out by averaging it with the other seven runs. This will happen with all of the other effect estimates. As an illustration, consider the 2^4 design in the resin filtration rate experiment of Example 6.2. Suppose that the run $cd = 375$ (the correct response was 75). Figure 6.17*a* shows the half-normal plot of the effects. It is obvious that the correct set of important effects is identified on the graph. However, the half-normal plot gives an indication that an outlier may be present. Notice that the straight line identifying the nonsignificant effects does not point toward the origin. In fact, the reference line from the origin is not even close to the collection of nonsignificant effects. A full normal probability plot would also have provided evidence of an outlier. The normal probability plot for this example is shown in Figure 6.17*b*. Notice that there are two distinct lines on the normal probability plot, not a single line passing through the nonsignificant effects. This is usually a strong indication that an outlier is present.

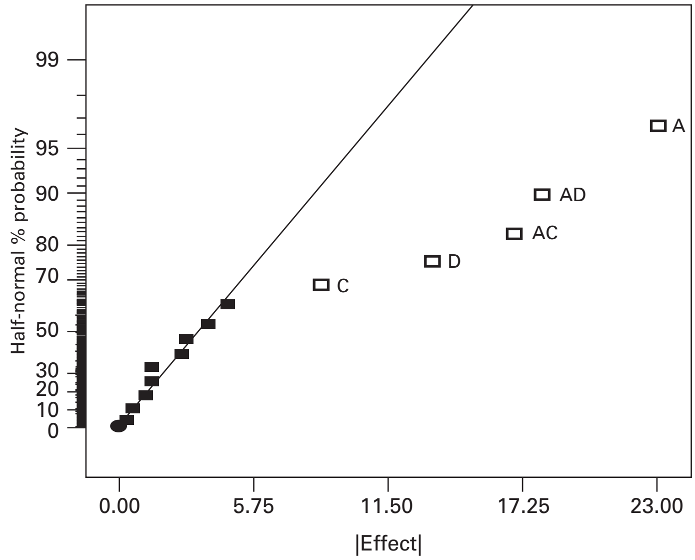
The illustration here involves a very severe outlier (375 instead of 75). This outlier is so dramatic that it would likely be spotted easily just by looking at the sample data or certainly by examining the residuals.

What should we do when an outlier is present? If it is a simple data recording or transposition error, an experimenter may be able to correct the outlier, replacing it with the right value. One suggestion is to replace it by an estimate (following the tactic introduced in Chapter 4 for blocked designs). This will preserve the orthogonality of the design and make interpretation easy. Replacing the outlier with an estimate that makes the highest order interaction estimate zero (in this case, replacing *cd* with a value that makes ABCD = 0) is one option. Discarding the outlier and analyzing the remaining observations is another option. This same approach would be used if one of the observations from the experiment is missing. Exercise 6.32 asks the reader to follow through with this suggestion for Example 6.2.

Modern computer software can analyze the data from 2^k designs with missing values because they use the method of least squares to estimate the effects, and least squares does not require an orthogonal design. The impact of this is that the effect estimates are no longer uncorrelated as they would be from an orthogonal design. The normal probability plotting technique requires that the effect estimates be uncorrelated with equal variance, but the degree of correlation introduced by a missing observation is relatively small in 2^k designs where the number of factors k is at least four. The correlation between the effect estimates and the model regression coefficients will not usually cause significant problems in interpreting the normal probability plot.

Figure 6.18 presents the half-normal probability plot obtained for the effect estimates if the outlier observation $cd = 375$ in Example 6.2 is omitted. This plot is easy to interpret, and exactly the same significant effects are identified as when the full set of experimental data was used. The correlation between design factors in this situation is ± 0.0714. It can be shown that the correlation between the model regression coefficients is larger, that is ± 0.5, but this still does not lead to any difficulty in interpreting the half-normal probability plot.

■ **FIGURE 6.18 Analysis of Example 6.2 with an outlier removed**

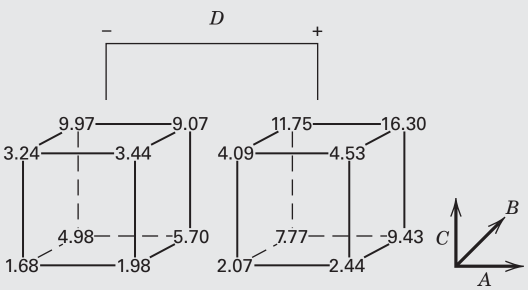


6.6 Additional Examples of Unreplicated 2^k Designs

Unreplicated 2^k designs are widely used in practice. They may be the most common variation of the 2^k design. This section presents four interesting applications of these designs, illustrating some additional analysis that can be helpful.

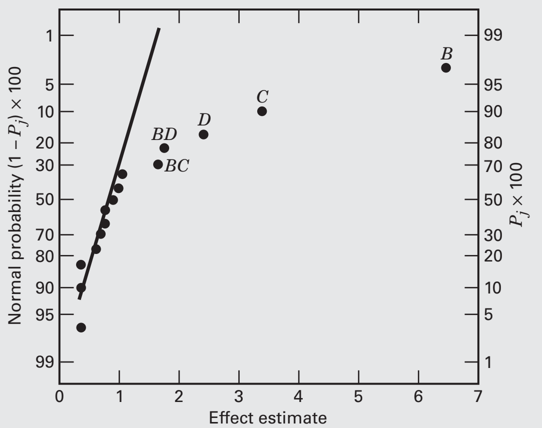
EXAMPLE 6.3 Data Transformation in a Factorial Design

Daniel (1976) describes a 2^4 factorial design used to study the advance rate of a drill as a function of four factors: drill load (A), flow rate (B), rotational speed (C), and the type of drilling mud used (D). The data from the experiment are shown in Figure 6.19.

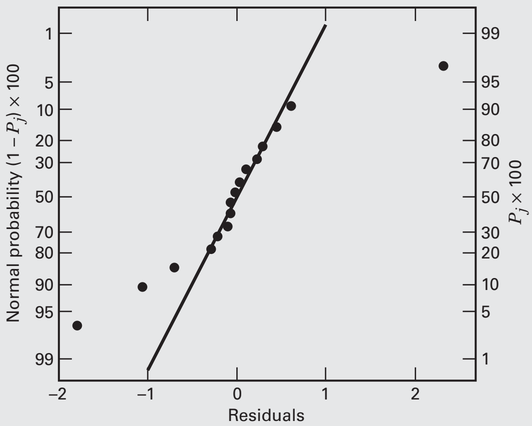


■ **FIGURE 6.19 Data from the drilling experiment of Example 6.3**

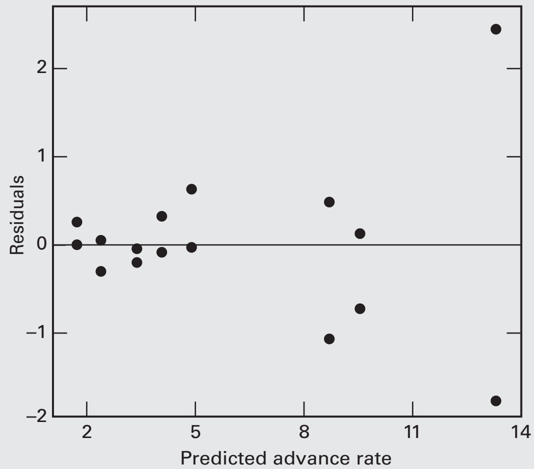
The normal probability plot of the effect estimates from this experiment is shown in Figure 6.20. Based on this plot, factors B, C, and D along with the BC and BD interactions



■ **FIGURE 6.20 Normal probability plot of effects for Example 6.3**



■ **FIGURE 6.21 Normal probability plot of residuals for Example 6.3**

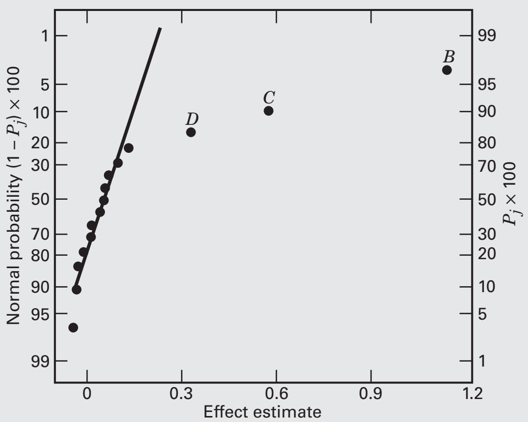


■ **FIGURE 6.22 Plot of residuals versus predicted advance rate for Example 6.3**

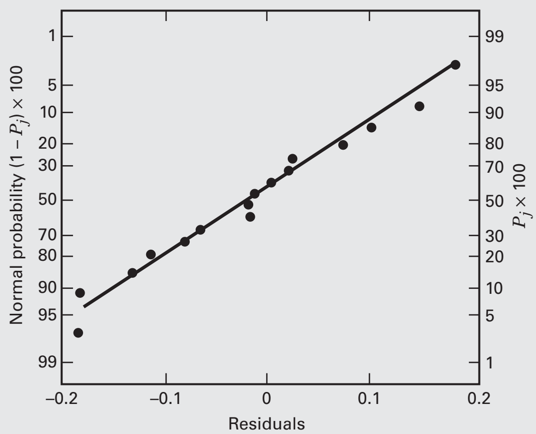
require interpretation. Figure 6.21 is the normal probability plot of the residuals and Figure 6.22 is the plot of the residuals versus the predicted advance rate from the model containing the identified factors. There are clearly problems with normality and equality of variance. A data transformation is often used to deal with such problems. Because the response variable is a rate, the log transformation seems a reasonable candidate.

Figure 6.23 presents a normal probability plot of the effect estimates following the transformation $y^* = \ln y$. Notice that a much simpler interpretation now seems possible because only factors B, C, and D are active. That is, expressing the data in the correct metric has simplified its structure to the point that the two interactions are no longer required in the explanatory model.

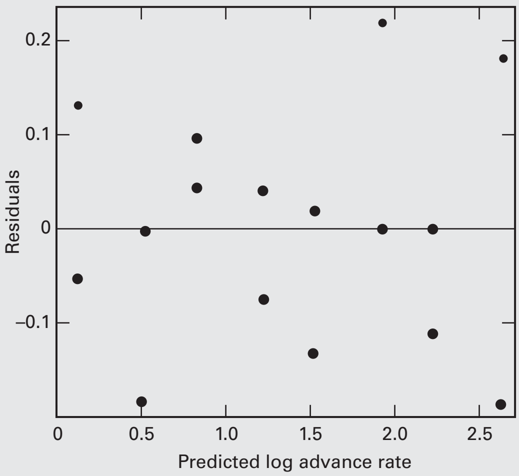
Figures 6.24 and 6.25 present, respectively, a normal probability plot of the residuals and a plot of the residuals versus the predicted advance rate for the model in the log scale containing B, C, and D. These plots are now satisfactory. We conclude that the model for $y* = \ln y$



■ **FIGURE 6.23 Normal probability plot of effects for Example 6.3 following log transformation**



■ **FIGURE 6.24 Normal probability plot of residuals for Example 6.3 following log transformation**



■ **FIGURE 6.25 Plot of residuals versus predicted advance rate for Example 6.3 Following log transformation**

requires only factors B, C, and D for adequate interpretation. The ANOVA for this model is summarized in Table 6.16. The model sum of squares is

$$\begin{aligned} SS_{\text{Model}} &= SS_B + SS_C + SS_D \\ &= 5.345 + 1.339 + 0.431 \\ &= 7.115 \end{aligned}$$

and $R^2 = SS_{\text{Model}}/SS_T = 7.115/7.288 = 0.98$, so the model explains about 98 percent of the variability in the drill advance rate.

■ **TABLE 6.16**

Analysis of Variance for Example 6.3 Following the Log Transformation

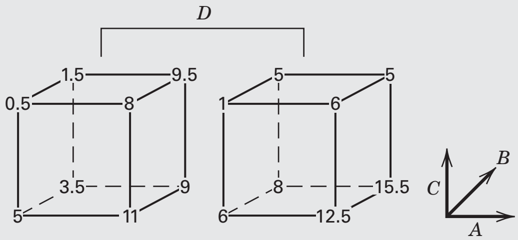
Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	P-Value
B (Flow)	5.345	1	5.345	381.79	< 0.0001
C (Speed)	1.339	1	1.339	95.64	< 0.0001
D (Mud)	0.431	1	0.431	30.79	< 0.0001
Error	0.173	12	0.014		
Total	7.288	15			

EXAMPLE 6.4 Location and Dispersion Effects in an Unreplicated Factorial

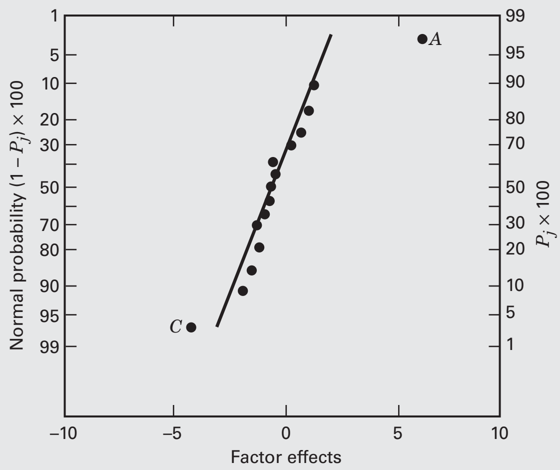
A 2^4 design was run in a manufacturing process producing interior sidewall and window panels for commercial aircraft. The panels are formed in a press, and under present conditions the average number of defects per panel in a press load is much too high. (The current process average is 5.5 defects per panel.) Four factors are investigated using a single replicate of a 2^4 design, with each replicate corresponding to a single press load. The factors are temperature (A), clamp time (B), resin flow (C), and press closing time (D). The data for this experiment are shown in Figure 6.26.

A normal probability plot of the factor effects is shown in Figure 6.27. Clearly, the two largest effects are $A = 5.75$ and $C = -4.25$. No other factor effects appear to be large, and A and C explain about 77 percent of the total variability. We therefore conclude that lower temperature (A) and higher resin flow (C) would reduce the incidence of panel defects.

Factors	Low (–)	High (+)
A = Temperature (°F)	295	325
B = Clamp time (min)	7	9
C = Resin flow	10	20
D = Closing time (s)	15	30

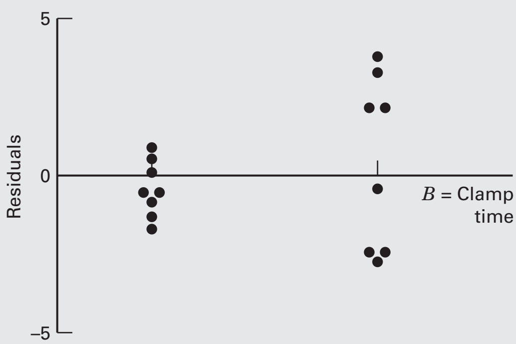


■ FIGURE 6.26 Data for the panel process experiment of Example 6.4



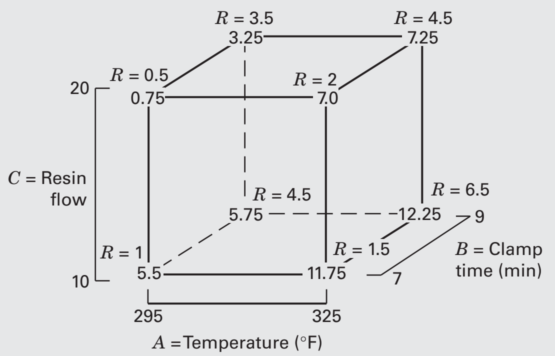
■ FIGURE 6.27 Normal probability plot of the factor effects for the panel process experiment of Example 6.4

Careful residual analysis is an important aspect of any experiment. A normal probability plot of the residuals showed no anomalies, but when the experimenter plotted the residuals versus each of the factors A through D, the plot of residuals versus B (clamp time) presented the pattern shown in Figure 6.28. This factor, which is unimportant insofar as the average number of defects per panel is concerned, is very important in its effect on process variability, with the lower clamp time resulting in less variability in the average number of defects per panel in a press load.



■ FIGURE 6.28 Plot of residuals versus clamp time for Example 6.4

The dispersion effect of clamp time is also very evident from the **cube plot** in Figure 6.29, which plots the average number of defects per panel and the range of the number of defects at each point in the cube defined by factors A, B, and C. The average range when B is at the high level (the back face of the cube in Figure 6.29) is $\overline{R}_{B+} = 4.75$ and when B is at the low level, it is $\overline{R}_{B-} = 1.25$.



■ FIGURE 6.29 Cube plot of temperature, clamp time, and resin flow for Example 6.4

As a result of this experiment, the engineer decided to run the process at low temperature and high resin flow to reduce the average number of defects, at low clamp time to reduce the variability in the number of defects per panel, and at low press closing time (which had no effect on either location or dispersion). The new set of operating conditions resulted in a new process average of less than one defect per panel.

The residuals from a 2^k design provide much information about the problem under study. Because residuals can be thought of as observed values of the noise or error, they often give insight into process variability. We can systematically examine the residuals from an unreplicated 2^k design to provide information about process variability.

Consider the residual plot in Figure 6.28. The standard deviation of the eight residuals where B is at the low level is $S(B^-) = 0.83$, and the standard deviation of the eight residuals where B is at the high level is $S(B^+) = 2.72$. The statistic

$$F_B^* = \ln \frac{S^2(B^+)}{S^2(B^-)} \tag{6.24}$$

has an approximate normal distribution if the two variances $\sigma^2(B^+)$ and $\sigma^2(B^-)$ are equal. To illustrate the calculations, the value of F_B^* is

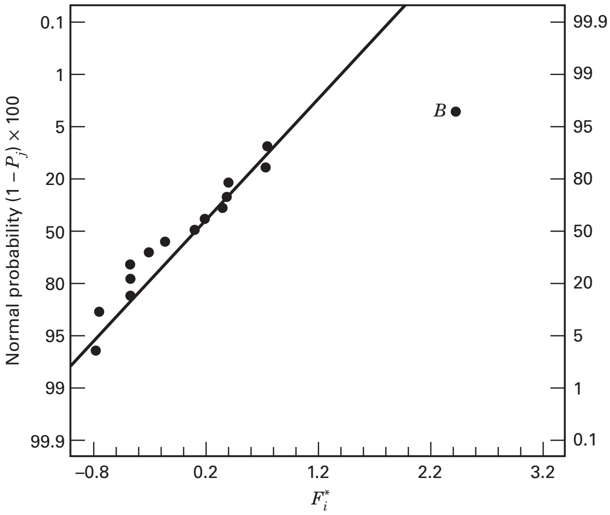
$$\begin{aligned} F_B^* &= \ln \frac{S^2(B^+)}{S^2(B^-)} \\ &= \ln \frac{(2.72)^2}{(0.83)^2} \\ &= 2.37 \end{aligned}$$

Table 6.17 presents the complete set of contrasts for the 2^4 design along with the residuals for each run from the panel process experiment in Example 6.4. Each column in this table contains an equal number of plus and minus signs, and we can calculate the standard deviation of the residuals for each group of signs in each column, say $S(i^+)$ and $S(i^-)$, $i = 1, 2, \ldots, 15$. Then

$$F_i^* = \ln \frac{S^2(i^+)}{S^2(i^-)} \quad i = 1, 2, \ldots, 15 \tag{6.25}$$

■ **TABLE 6.17**
Calculation of Dispersion Effects for Example 6.4

Run	A	B	AB	C	AC	BC	ABC	D	AD	BD	ABD	CD	ACD	BCD	$ABCD$	Residual
1	−	−	+	−	+	+	−	−	+	+	−	+	−	−	+	−0.94
2	+	−	−	−	−	+	+	−	−	+	+	+	+	−	−	−0.69
3	−	+	−	−	+	−	+	−	+	−	+	+	−	+	−	−2.44
4	+	+	+	−	−	−	−	−	−	−	−	+	+	+	+	−2.69
5	−	−	+	+	−	−	+	−	+	+	−	−	+	+	−	−1.19
6	+	−	−	+	+	−	−	−	−	+	+	−	−	+	+	0.56
7	−	+	−	+	−	+	−	−	+	−	+	−	+	−	+	−0.19
8	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	2.06
9	−	−	+	−	+	+	−	+	−	−	+	−	+	+	−	0.06
10	+	−	−	−	−	+	+	+	+	−	−	−	−	+	+	0.81
11	−	+	−	−	+	−	+	+	−	+	−	−	+	−	+	2.06
12	+	+	+	−	−	−	−	+	+	+	+	−	−	−	−	3.81
13	−	−	+	+	−	−	+	+	−	−	+	+	−	−	+	−0.69
14	+	−	−	+	+	−	−	+	+	−	−	+	+	−	−	−1.44
15	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−	3.31
16	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	−2.44
$S(i^+)$	2.25	2.72	2.21	1.91	1.81	1.80	1.80	2.24	2.05	2.28	1.97	1.93	1.52	2.09	1.61	
$S(i^-)$	1.85	0.83	1.86	2.20	2.24	2.26	2.24	1.55	1.93	1.61	2.11	1.58	2.16	1.89	2.33	
F_i^*	0.39	2.37	0.34	−0.28	−0.43	−0.46	−0.44	0.74	0.12	0.70	−0.14	0.40	−0.70	0.20	−0.74	



■ **FIGURE 6.30 Normal probability plot of the dispersion effects F_i^* for Example 6.4**

is a statistic that can be used to assess the magnitude of the **dispersion effects** in the experiment. If the variance of the residuals for the runs where factor i is positive equals the variance of the residuals for the runs where factor i is negative, then F_i^* has an approximate normal distribution. The values of F_i^* are shown below each column in Table 6.15.

Figure 6.30 is a normal probability plot of the dispersion effects F_i^*. Clearly, B is an important factor with respect to process dispersion. For more discussion of this procedure, see Box and Meyer (1986) and Myers, Montgomery, and Anderson-Cook (2016). Also, in order for the model residuals to properly convey information about dispersion effects, the **location model** must be correctly specified. Refer to the supplemental text material for this chapter for more details and an example.

EXAMPLE 6.5 Duplicate Measurements on the Response

A team of engineers at a semiconductor manufacturer ran a 2^4 factorial design in a vertical oxidation furnace. Four wafers are "stacked" in the furnace, and the response variable of interest is the oxide thickness on the wafers. The four design factors are temperature (A), time (B), pressure (C), and gas flow (D). The experiment is conducted by loading four wafers into the furnace, setting the process variables to the test conditions required by the experimental design, processing the wafers, and then measuring the oxide thickness on all four wafers. Table 6.18 presents the design and the resulting thickness measurements. In this table, the four columns labeled "Thickness" contain the oxide thickness measurements on each individual wafer, and the last two columns contain the sample average and sample variance of the thickness measurements on the four wafers in each run.

The proper analysis of this experiment is to consider the individual wafer thickness measurements as **duplicate measurements** and not as replicates. If they were really replicates, each wafer would have been processed individually on a single run of the furnace. However, because all four wafers were processed together, they received the treatment factors (that is, the levels of the design variables) *simultaneously*, so there is much less variability in the individual wafer thickness measurements than would have been observed if each wafer was a replicate. Therefore, the **average** of the thickness measurements is the correct response variable to initially consider.

Table 6.19 presents the effect estimates for this experiment, using the average oxide thickness $\bar{y}$ as the response variable. Note that factors A and B and the AB interaction have large effects that together account for nearly 90 percent of the variability in average oxide thickness. Figure 6.31 is a normal probability plot of the effects. From examination of this display, we would conclude that factors A, B, and C and the AB and AC interactions are important. The analysis of variance display for this model is shown in Table 6.20.

The model for predicting average oxide thickness is

$$\hat{y} = 399.19 + 21.56x_1 + 9.06x_2 - 5.19x_3 + 8.44x_1x_2 - 5.31x_1x_3$$

The residual analysis of this model is satisfactory.

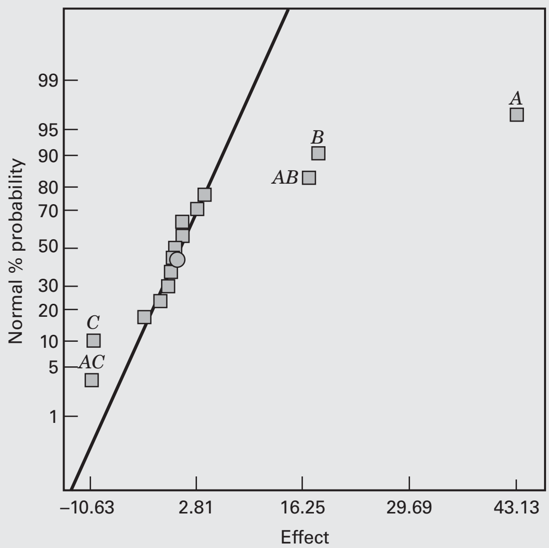
The experimenters are interested in obtaining an average oxide thickness of 400 Å, and product specifications require that the thickness must lie between 390 and 410 Å. Figure 6.32 presents two contour plots of average thickness, one with factor C (or x_3), pressure, at the low level (that is, $x_3 = -1$) and the other with C (or x_3) at the high level

■ **TABLE 6.18**
The Oxide Thickness Experiment

Standard Order	Run Order	A	B	C	D	Thickness				$\bar{y}$	s^2
1	10	−1	−1	−1	−1	378	376	379	379	378	2
2	7	1	−1	−1	−1	415	416	416	417	416	0.67
3	3	−1	1	−1	−1	380	379	382	383	381	3.33
4	9	1	1	−1	−1	450	446	449	447	448	3.33
5	6	−1	−1	1	−1	375	371	373	369	372	6.67
6	2	1	−1	1	−1	391	390	388	391	390	2
7	5	−1	1	1	−1	384	385	386	385	385	0.67
8	4	1	1	1	−1	426	433	430	431	430	8.67
9	12	−1	−1	−1	1	381	381	375	383	380	12.00
10	16	1	−1	−1	1	416	420	412	412	415	14.67
11	8	−1	1	−1	1	371	372	371	370	371	0.67
12	1	1	1	−1	1	445	448	443	448	446	6
13	14	−1	−1	1	1	377	377	379	379	378	1.33
14	15	1	−1	1	1	391	391	386	400	392	34
15	11	−1	1	1	1	375	376	376	377	376	0.67
16	13	1	1	1	1	430	430	428	428	429	1.33

■ **TABLE 6.19**
Effect Estimates for Example 6.5, Response Variable Is Average Oxide Thickness

Model Term	Effect Estimate	Sum of Squares	Percent Contribution
A	43.125	7439.06	67.9339
B	18.125	1314.06	12.0001
C	−10.375	430.562	3.93192
D	−1.625	10.5625	0.0964573
AB	16.875	1139.06	10.402
AC	−10.625	451.563	4.12369
AD	1.125	5.0625	0.046231
BC	3.875	60.0625	0.548494
BD	−3.875	60.0625	0.548494
CD	1.125	5.0625	0.046231
ABC	−0.375	0.5625	0.00513678
ABD	2.875	33.0625	0.301929
ACD	−0.125	0.0625	0.000570753
BCD	−0.625	1.5625	0.0142688
$ABCD$	0.125	0.0625	0.000570753



■ **FIGURE 6.31** **Normal probability plot of the effects for the average oxide thickness response, Example 6.5**

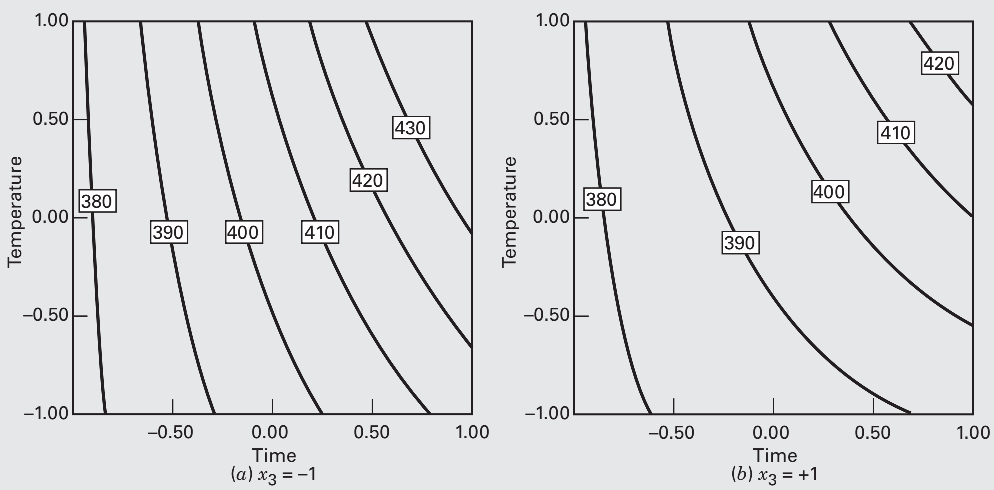
■ **TABLE 6.20**

Analysis of Variance (from Design-Expert) for the Average Oxide Thickness Response, Example 6.5

Source	Sum of Squares	DF	Mean Square	*F* Value	Prob > *F*
Model	10774.31	5	2154.86	122.35	<0.000
A	*7439.06*	*1*	*7439.06*	*422.37*	*<0.000*
B	*1314.06*	*1*	*1314.06*	*74.61*	*<0.000*
C	*430.56*	*1*	*430.56*	*24.45*	*0.0006*
AB	*1139.06*	*1*	*1139.06*	*64.67*	*<0.000*
AC	*451.46*	*1*	*451.56*	*25.64*	*0.0005*
Residual	176.12	10	17.61		
Cor Total	10950.44	15			

Std. Dev.	4.20	R-Squared	0.9839
Mean	399.19	Adj R-Squared	0.9759
C.V.	1.05	Pred R-Squared	0.9588
PRESS	450.88	Adeq Precision	27.967

Factor	Coefficient Estimate	DF	Standard Error	95% CI Low	95% CI High
Intercept	399.19	1	1.05	396.85	401.53
A-Time	21.56	1	1.05	19.22	23.90
B-Temp	9.06	1	1.05	6.72	11.40
C-Pressure	−5.19	1	1.05	−7.53	−2.85
AB	8.44	1	1.05	6.10	10.78
AC	−5.31	1	1.05	−7.65	−2.97



■ **FIGURE 6.32** **Contour plots of average oxide thickness with pressure (x_3) held constant**

■ **TABLE 6.21**
Analysis of Variance (from Design-Expert) of the Individual Wafer Oxide Thickness Response

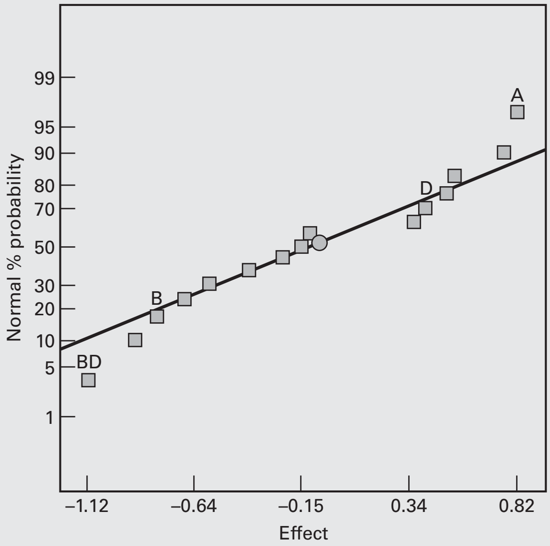
Source	Sum of Squares	DF	Mean Square	*F* Value	Prob > *F*
Model	43801.75	15	2920.12	476.75	<0.0001
A	*29756.25*	*1*	*29756.25*	*4858.16*	*<0.0001*
B	*5256.25*	*1*	*5256.25*	*858.16*	*<0.0001*
C	*1722.25*	*1*	*1722.25*	*281.18*	*<0.0001*
D	*42.25*	*1*	*42.25*	*6.90*	*0.0115*
AB	*4556.25*	*1*	*4556.25*	*743.88*	*<0.0001*
AC	*1806.25*	*1*	*1806.25*	*294.90*	*<0.0001*
AD	*20.25*	*1*	*20.25*	*3.31*	*0.0753*
BC	*240.25*	*1*	*240.25*	*39.22*	*<0.0001*
BD	*240.25*	*1*	*240.25*	*39.22*	*<0.0001*
CD	*20.25*	*1*	*20.25*	*3.31*	*0.0753*
ABD	*132.25*	*1*	*132.25*	*21.59*	*<0.0001*
ABC	*2.25*	*1*	*2.25*	*0.37*	*0.5473*
ACD	*0.25*	*1*	*0.25*	*0.041*	*0.8407*
BCD	*6.25*	*1*	*6.25*	*1.02*	*0.3175*
ABCD	*0.25*	*1*	*0.25*	*0.041*	*0.8407*
Residual	294.00	48	6.12		
Lack of Fit	*0.000*	*0*			
Pure Error	*294.00*	*48*	*6.13*		
Cor Total	44095.75	63			

(that is, $x_3 = +1$). From examining these contour plots, it is obvious that there are many combinations of time and temperature (factors A and B) that will produce acceptable results. However, if pressure is held constant at the low level, the operating "window" is shifted toward the left, or lower, end of the time axis, indicating that lower cycle times will be required to achieve the desired oxide thickness.

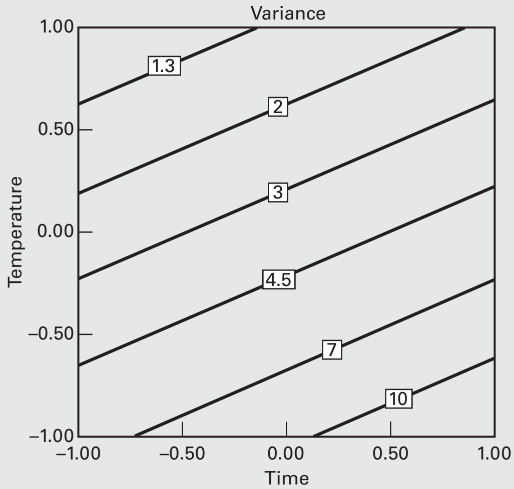
It is interesting to observe the results that would be obtained if we **incorrectly** consider the individual wafer oxide thickness measurements as replicates. Table 6.21 presents a full-model ANOVA based on treating the experiment as a replicated 2^4 factorial. Notice that there are many significant factors in this analysis, suggesting a much more complex model than the one that we found when using the average oxide thickness as the response. The reason for this is that the estimate of the error variance in Table 6.21 is too small ($\hat{\sigma}^2 = 6.12$). The residual mean square in Table 6.21 reflects a combination of the variability between wafers *within* a run and variability *between* runs. The estimate of error obtained from Table 6.20 is much larger, $\hat{\sigma}^2 = 17.61$, and it is primarily a measure of the between-run variability. This is the best estimate of error to use in judging the significance of process variables that are changed from run to run.

A logical question to ask is: What harm results from identifying too many factors as important, as the incorrect analysis in Table 6.21 would certainly do. The answer is that trying to manipulate or optimize the unimportant factors would be a waste of resources, and it could result in adding unnecessary variability to *other* responses of interest.

When there are duplicate measurements on the response, these observations almost always contain useful information about some aspect of process variability. For example, if the duplicate measurements are multiple tests by a gauge on the same experimental unit, then the duplicate measurements give some insight about *gauge capability*. If the duplicate measurements are made at different locations on an experimental unit, they may give some information about the *uniformity* of the response variable across that unit. In our example, because we have one observation on each of the four experimental units that have undergone processing together, we have some information about the *within-run* variability in the process. This information is contained in the variance of the oxide thickness measurements from the four wafers in each run. It would be of interest to determine whether any of the process variables influence the within-run variability.



■ **FIGURE 6.33 Normal probability plot of the effects using ln (s^2) as the response, Example 6.5**



■ **FIGURE 6.34 Contour plot of s^2 (within-run variability) with pressure at the low level and gas flow at the high level**

Figure 6.33 is a normal probability plot of the effect estimates obtained using $\ln(s^2)$ as the response. Recall from Chapter 3 that we indicated that the log transformation is generally appropriate for modeling variability. There are not any strong individual effects, but factor *A* and *BD* interaction are the largest. If we also include the main effects of *B* and *D* to obtain a hierarchical model, then the model for $\ln(s^2)$ is

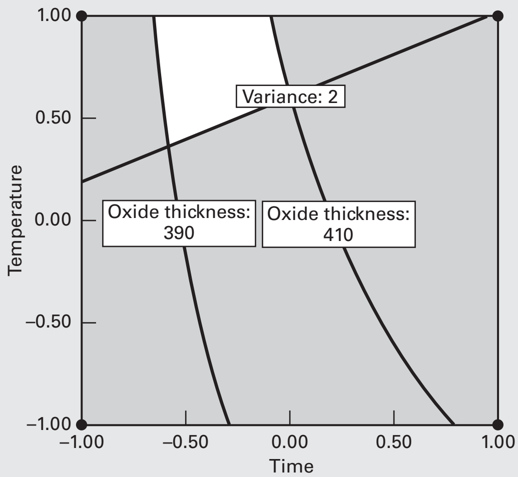
$$\widehat{\ln(s^2)} = 1.08 + 0.41x_1 - 0.40x_2 + 0.20x_4 - 0.56x_2x_4$$

The model accounts for just slightly less than half of the variability in the $\ln(s^2)$ response, which is certainly not spectacular as empirical models go, but it is often difficult to obtain exceptionally good models of variances.

Figure 6.34 is a contour plot of the predicted variance (not the log of the predicted variance) with pressure x_3 at the low level (recall that this minimizes cycle time) and gas flow x_4 at the high level. This choice of gas flow gives the lowest values of predicted variance in the region of the contour plot.

The experimenters here were interested in selecting values of the design variables that gave a mean oxide thickness within the process specifications and as close to 400 Å as possible, while simultaneously making the within-run variability small, say $s^2 \leq 2$. One possible way to find a suitable set of conditions is to overlay the contour plots in Figures 6.32 and 6.34. The overlay plot is shown in Figure 6.35, with the specifications on mean oxide thickness and the constraint $s^2 \leq 2$ shown as contours. In this plot, pressure is held constant at the low level and gas flow is held constant at the high level. The open region near the upper left center of the graph identifies a feasible region for the variables time and temperature.

This is a simple example of using contour plots to study two responses simultaneously. We will discuss this problem in more detail in Chapter 11.



■ **FIGURE 6.35 Overlay of the average oxide thickness and s^2 responses with pressure at the low level and gas flow at the high level**

EXAMPLE 6.6 Credit Card Marketing

An article in the *International Journal of Research in Marketing* ("Experimental Design on the Front Lines of Marketing: Testing New Ideas to Increase Direct Mail Sales," 2006, Vol. 23, pp. 309–319) describes an experiment to test new ideas to increase direct mail sales by the credit card division of a financial services company. They want to improve the response rate to its credit card offers. They know from experience that the interest rates are an important factor in attracting potential customers, so they have decided to focus on factors involving both interest rates and fees. They want to test changes in both introductory and long-term rates, as well as the effects of adding an account-opening fee and lowering the annual fee. The factors tested in the experiment are as follows:

Factor	(−) Control	(+) New Idea
A: Annual fee	Current	Lower
B: Account-opening fee	No	Yes
C: Initial interest rate	Current	Lower
D: Long-term interest rate	Low	High

The marketing team used columns *A* through *D* of the 2^4 factorial test matrix shown in Table 6.22 to create 16 mail packages. The +/− sign combinations in the 11 interaction (product) columns are used solely to facilitate the statistical analysis of the results. Each of the 16 test combinations was mailed to 7500 customers, and 2837 customers responded positively to the offers.

Table 6.23 is the JMP output for the screening analysis. Lenth's method with simulated *P*-values is used to identify significant factors. All four main effects are significant, and one interaction (*AB*, or Annual Fee × Account Opening Fee). The prediction profiler indicates the settings of the four factors that will result in the maximum response rate. The lower annual fee, no account opening fee, the lower long-term interest rate and either value of the initial interest rate produce the best response, 3.39 percent. The optimum conditions occur at one of the actual test combinations because all four design factors were treated as qualitative. With continuous factors, the optimal conditions are usually not at one of the experimental runs.

■ **TABLE 6.22**
The 2^4 Factorial Design Used in the Credit Card Marketing Experiment, Example 6.6

Test Cell	Annual-Fee *A*	Account-Opening Fee *B*	Initial Interest Rate *C*	Long-Term Interest Rate *D*	(Interactions) *AB*	*AC*	*AD*	*BC*	*BD*	*CD*	*ABC*	*ABD*	*ACD*	*BCD*	*ABCD*	Orders	Response Rate
1	−	−	−	−	+	+	+	+	+	+	−	−	−	−	+	184	2.45%
2	+	−	−	−	−	−	−	+	+	+	+	+	+	−	−	252	3.36%
3	−	+	−	−	−	+	+	−	−	+	+	+	−	+	−	162	2.16%
4	+	+	−	−	+	−	−	−	−	+	−	−	+	+	+	172	2.29%
5	−	−	+	−	+	−	+	−	+	−	+	−	+	+	−	187	2.49%
6	+	−	+	−	−	+	−	−	+	−	−	+	−	+	+	254	3.39%
7	−	+	+	−	−	−	+	+	−	−	−	+	+	−	+	174	2.32%
8	+	+	+	−	+	+	−	+	−	−	+	−	−	−	−	183	2.44%
9	−	−	−	+	+	+	−	+	−	−	−	+	+	+	−	138	1.84%
10	+	−	−	+	−	−	+	+	−	−	+	−	−	+	+	168	2.24%
11	−	+	−	+	−	+	−	−	+	−	+	−	+	−	+	127	1.69%
12	+	+	−	+	+	−	+	−	+	−	−	+	−	−	−	140	1.87%
13	−	−	+	+	+	−	−	−	−	+	+	+	−	−	+	172	2.29%
14	+	−	+	+	−	+	+	−	−	+	−	−	+	−	−	219	2.92%
15	−	+	+	+	−	−	−	+	+	+	−	−	−	+	−	153	2.04%
16	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	152	2.03%

■ **TABLE 6.23**

JMP Output for Example 6.6

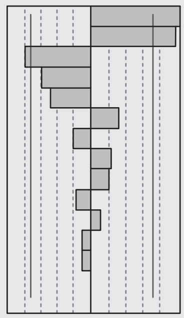
Response Response Rate

Summary of Fit

RSquare	1
RSquare Adj	.
Root Mean Square Error	.
Mean of Response	2.36375
Observations (or Sum Wgts)	16

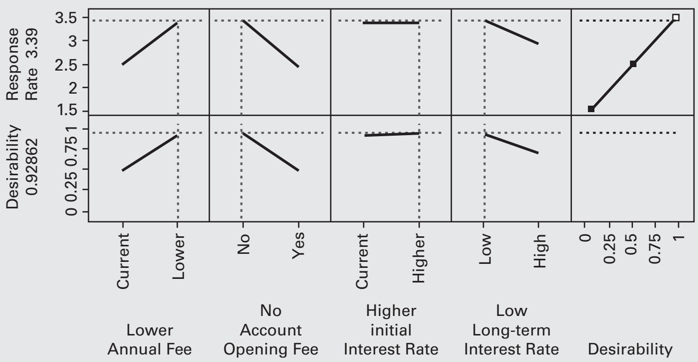
Sorted Parameter Estimates

Term	Estimate	Relative Std Error	Pseudo t-Ratio	Pseudo p-Value
Account Opening Fee[No]	0.25875	0.25	3.63	0.0150*
Long-term Interest Rate[Low]	0.24875	0.25	3.49	0.0174*
Annual Fee[Current]	−0.20375	0.25	−2.86	0.0354*
Annual Fee[Current]*Account Opening Fee[No]	−0.15125	0.25	−2.12	0.0872
initial Interest Rate[Current]	−0.12625	0.25	−1.77	0.1366
initial Interest Rate[Current]*Long-term Interest Rate[Low]	0.07875	0.25	1.11	0.3194
Annual Fee[Current]*Long-term Interest Rate[Low]	−0.05375	0.25	−0.75	0.4846
Account Opening Fee[No]*initial Interest Rate[Current]*Long-term Interest Rate[Low]	0.05375	0.25	0.75	0.4846
Account Opening Fee[No]*Long-term Interest Rate[Low]	0.05125	0.25	0.72	0.5042
Annual Fee[Current]*Account Opening Fee[No]*Long-term Interest Rate[Low]	−0.04375	0.25	−0.61	0.5661
Annual Fee[Current]*Account Opening Fee[No]*initial Interest Rate[Current]	0.02625	0.25	0.37	0.7276
Annual Fee[Current]*Account Opening Fee[No]*initial Interest Rate[Current]*Long-term Interest Rate[Low]	−0.02625	0.25	−0.37	0.7276
Account Opening Fee[No]*initial Interest Rate[Current]	−0.02375	0.25	−0.33	0.7524
Annual Fee[Current]*initial Interest Rate[Current]*Long-term Interest Rate[Low]	−0.00375	0.25	−0.05	0.9601
Annual Fee[Current]*initial Interest Rate[Current]	0.00125	0.25	0.02	0.9867



No error degrees of freedom, so ordinary tests uncomputable.
Relative Std Error corresponds to residual standard error of 1.
Pseudo t-Ratio and p-Value calculated using Lenth PSE = 0.07125
and DFE=5

Prediction Profiler



6.7 2^k Designs are Optimal Designs

Two-level factorial designs have many interesting and useful properties. In this section, a brief description of some of these properties is given. We have remarked in previous sections that the model regression coefficients and effect estimates from a 2^k design are least squares estimates. This is discussed in the supplemental text material for this chapter and presented in more detail in Chapter 10, but it is useful to give a proof of this here.

Consider a very simple case of the 2^2 design with one replicate. This is a four-run design, with treatment combinations (1), a, b, and ab. The design is shown geometrically in Figure 6.1. The model we fit to the data from this design is

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_{12} x_1 x_2 + \varepsilon$$

where x_1 and x_2 are the main effects of the two factors on the ± 1 scale and $x_1 x_2$ is the two-factor interaction. We can write out each one of the four runs in this design in terms of this model as follows:

$$\begin{aligned}
(1) &= \beta_0 + \beta_1(-1) + \beta_2(-1) + \beta_{12}(-1)(-1) + \epsilon_1 \\
a &= \beta_0 + \beta_1(1) + \beta_2(-1) + \beta_{12}(1)(-1) + \epsilon_2 \\
b &= \beta_0 + \beta_1(-1) + \beta_2(1) + \beta_2(-1)(1) + \epsilon_3 \\
ab &= \beta_0 + \beta_1(1) + \beta_2(1) + \beta_{12}(1)(1) + \epsilon_4
\end{aligned}$$

It is much easier if we write these four equations in matrix form:

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \epsilon, \text{ where } \mathbf{y} = \begin{bmatrix} (1) \\ a \\ b \\ ab \end{bmatrix}, \mathbf{X} = \begin{bmatrix} 1 & -1 & -1 & 1 \\ 1 & 1 & -1 & -1 \\ 1 & -1 & 1 & -1 \\ 1 & 1 & 1 & 1 \end{bmatrix}, \boldsymbol{\beta} = \begin{bmatrix} \beta_0 \\ \beta_1 \\ \beta_2 \\ \beta_{12} \end{bmatrix}, \text{ and } \epsilon = \begin{bmatrix} \varepsilon_1 \\ \varepsilon_2 \\ \varepsilon_3 \\ \varepsilon_4 \end{bmatrix}$$

The least squares estimates of the model parameters are the values of the β's that minimize the sum of the squares of the model errors, ϵ_i, $i = 1, 2, 3, 4$. The least squares estimates are

$$\hat{\boldsymbol{\beta}} = (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{y} \tag{6.26}$$

where the prime (′) denotes a transpose and $(\mathbf{X}'\mathbf{X})^{-1}$ is the inverse of $\mathbf{X}'\mathbf{X}$. We will prove this result later in Chapter 10. For the 2^2 design, the quantities $\mathbf{X}'\mathbf{X}$ and $\mathbf{X}'\mathbf{y}$ are

$$\mathbf{X}'\mathbf{X} = \begin{bmatrix} 1 & 1 & 1 & 1 \\ -1 & 1 & -1 & 1 \\ -1 & -1 & 1 & 1 \\ 1 & -1 & -1 & 1 \end{bmatrix} \begin{bmatrix} 1 & -1 & -1 & 1 \\ 1 & 1 & -1 & -1 \\ 1 & -1 & 1 & -1 \\ 1 & 1 & 1 & 1 \end{bmatrix} = \begin{bmatrix} 4 & 0 & 0 & 0 \\ 0 & 4 & 0 & 0 \\ 0 & 0 & 4 & 0 \\ 0 & 0 & 0 & 4 \end{bmatrix}$$

and

$$\mathbf{X}'\mathbf{y} = \begin{bmatrix} 1 & 1 & 1 & 1 \\ -1 & 1 & -1 & 1 \\ -1 & -1 & 1 & 1 \\ 1 & -1 & -1 & 1 \end{bmatrix} \begin{bmatrix} (1) \\ a \\ b \\ ab \end{bmatrix} = \begin{bmatrix} (1) + a + b + ab \\ -(1) + a - b + ab \\ -(1) - a + b + ab \\ (1) - a - b + ab \end{bmatrix}$$

The $\mathbf{X}'\mathbf{X}$ matrix is diagonal because the 2^2 design is orthogonal. The least squares estimates are as follows:

$$\begin{aligned}
\hat{\boldsymbol{\beta}} &= (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{y} \\
&= \begin{bmatrix} 4 & 0 & 0 & 0 \\ 0 & 4 & 0 & 0 \\ 0 & 0 & 4 & 0 \\ 0 & 0 & 0 & 4 \end{bmatrix}^{-1} \begin{bmatrix} (1) + a + b + ab \\ -(1) + a - b + ab \\ -(1) - a + b + ab \\ (1) - a - b + ab \end{bmatrix}
\end{aligned}$$

$$
= \begin{bmatrix} \dfrac{(1)+a+b+ab}{4} \\ \dfrac{-(1)+a-b+ab}{4} \\ \dfrac{-(1)-a+b+ab}{4} \\ \dfrac{(1)-a-b+ab}{4} \end{bmatrix}
$$

The least squares estimates of the model regression coefficients are exactly equal to one-half of the usual effect estimates.

It turns out that the variance of any model regression coefficient is easy to find:

$$
\begin{aligned} V(\hat{\beta}) &= \sigma^2(\text{diagonal element of } (\mathbf{X}'\mathbf{X})^{-1}) \\ &= \frac{\sigma^2}{4} \end{aligned} \tag{6.27}
$$

All model regression coefficients have the same variance. Furthermore, there is no other four-run design on the design space bounded by ± 1 that makes the variance of the model regression coefficients smaller. In general, the variance of any model regression coefficient in a 2^k design where each design point is replicated n times is $V(\hat{\beta}) = \sigma^2/(n2^k) = \sigma^2/N$, where N is the total number of runs in the design. This is the minimum possible variance for the regression coefficient.

For the 2^2 design, the determinant of the $\mathbf{X}'\mathbf{X}$ matrix is

$$
|(\mathbf{X}'\mathbf{X})| = 256
$$

This is the maximum possible value of the determinant for a four-run design on the design space bounded by ± 1. It turns out that the volume of the joint confidence region that contains all the model regression coefficients is inversely proportional to the square root of the determinant of $\mathbf{X}'\mathbf{X}$. Therefore, to make this joint confidence region as small as possible, we would want to choose a design that makes the determinant of $\mathbf{X}'\mathbf{X}$ as large as possible. This is accomplished by choosing the 2^2 design.

In general, a design that minimizes the variance of the model regression coefficients is called a ***D*-optimal design**. The D terminology is used because these designs are found by selecting runs in the design to maximize the determinant of $\mathbf{X}'\mathbf{X}$. The 2^k design is a D-optimal design for fitting the first-order model or the first-order model with interaction. Many computer software packages, such as JMP, Design-Expert, and Minitab, have algorithms for finding D-optimal designs. These algorithms can be very useful in constructing experimental designs for many practical situations. We will make use of them in subsequent chapters.

Now consider the variance of the predicted response in the 2^2 design

$$
V[\hat{y}(x_1 x_2)] = V(\hat{\beta}_0 + \hat{\beta}_1 x_1 + \hat{\beta}_2 x_2 + \hat{\beta}_{12} x_1 x_2)
$$

The variance of the predicted response is a function of the point in the design space where the prediction is made (x_1 and x_2) and the variance of the model regression coefficients. The estimates of the regression coefficients are independent because the 2^2 design is orthogonal and they all have variance $\sigma^2/4$, so

$$
\begin{aligned} V[\hat{y}(x_1, x_2)] &= V(\hat{\beta}_0 + \hat{\beta}_1 x_1 + \hat{\beta}_2 x_2 + \hat{\beta}_{12} x_1 x_2) \\ &= \frac{\sigma^2}{4}(1 + x_1^2 + x_2^2 + x_1^2 x_2^2) \end{aligned}
$$

The maximum prediction variance occurs when $x_1 = x_2 = \pm 1$ and is equal to σ^2. To determine how good this is, we need to know the best possible value of prediction variance that we can attain. It turns out that the smallest possible value of the maximum prediction variance over the design space is $p\sigma^2/N$, where p is the number of model parameters and N is the number of runs in the design. The 2^2 design has $N = 4$ runs and the model has $p = 4$ parameters, so the model that we fit to the data from this experiment minimizes the maximum prediction variance over the design region. A design that has this property is called a ***G*-optimal design**. In general, 2^k designs are G-optimal designs for fitting the first-order model or the first-order model with interaction.

We can evaluate the prediction variance at any point of interest in the design space. For example, when we are at the center of the design where $x_1 = x_2 = 0$, the prediction variance is

$$V[\hat{y}(x_1 = 0, x_2 = 0)] = \frac{\sigma^2}{4}$$

When $x_1 = 1$ and $x_2 = 0$, the prediction variance is

$$V[\hat{y}(x_1 = 1, x_2 = 0)] = \frac{\sigma^2}{2}$$

An alternative to evaluating the prediction variance at a lot of points in the design space is to consider the **average prediction variance** over the design space. One way to calculate this average prediction variance is

$$I = \frac{1}{A}\int_{-1}^{1}\int_{-1}^{1} V[\hat{y}(x_1, x_2)]dx_1 dx_2$$

where A is the area (in general the volume) of the design space. To compute the average, we are integrating the variance function over the design space and dividing by the area of the region.

Sometimes I is called the **integrated variance** criterion. Now for a 2^2 design, the area of the design region is $A = 4$, and

$$\begin{aligned} I &= \frac{1}{A}\int_{-1}^{1}\int_{-1}^{1} V[\hat{y}(x_1, x_2)]dx_1 dx_2 \\ &= \frac{1}{4}\int_{-1}^{1}\int_{-1}^{1} \sigma^2 \frac{1}{4}(1 + x_1^2 + x_2^2 + x_1^2 x_2^2)dx_1 dx_2 \\ &= \frac{4\sigma^2}{9} \end{aligned}$$

It turns out that this is the smallest possible value of the average prediction variance that can be obtained from a four-run design used to fit a first-order model with interaction on this design space. A design with this property is called an ***I*-optimal design**. In general, 2^k designs are I-optimal designs for fitting the first-order model or the first-order model with interaction. The JMP software will construct I-optimal designs. This can be very useful in constructing designs when response prediction is the goal of the experiment.

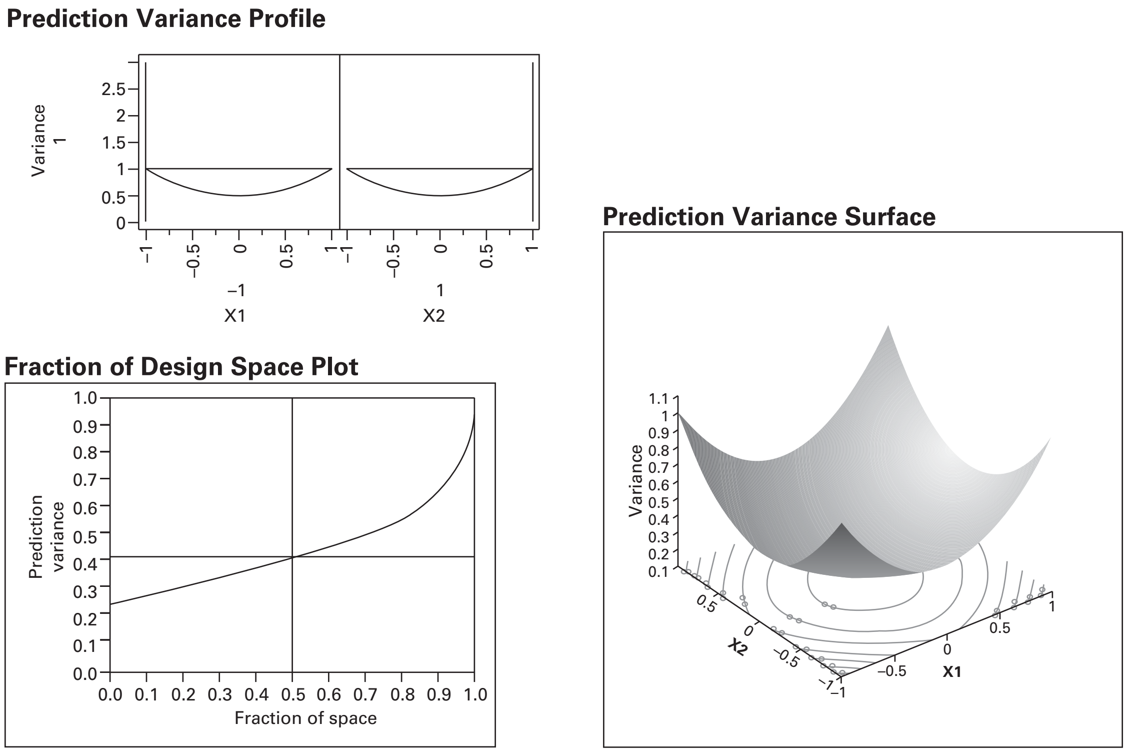
It is also possible to display the prediction variance over the design space graphically. Figure 6.36 is output from JMP illustrating three possible displays of the prediction variance from a 2^2 design. The first graph is the **prediction variance profiler**, which plots the **unscaled prediction variance**

$$UPV = \frac{V[\hat{y}(x_1, x_2)]}{\sigma^2}$$

against the levels of each design factor. The "crosshairs" on the graphs are adjustable, so that the unscaled prediction variance can be displayed at any desired combination of the variables x_1 and x_2. Here, the values chosen are $x_1 = -1$

Custom Design
Design

Run	X1	X2
1	1	1
2	1	−1
3	−1	−1
4	−1	1



■ **FIGURE 6.36** **JMP prediction variance output for the 2^2 design**

and $x_2 = +1$, for which the unscaled prediction variance is

$$\begin{aligned} UPV &= \frac{V[\hat{y}(x_1, x_2)]}{\sigma^2} \\ &= \frac{\frac{\sigma^2}{4}(1 + x_1^2 + x_2^2 + x_1^2 x_2^2)}{\sigma^2} \\ &= \frac{\frac{\sigma^2}{4}(4)}{\sigma^2} \\ &= 1 \end{aligned}$$

The second graph is a **fraction of design space (FDS) plot**, which shows the unscaled prediction variance on the vertical scale and the fraction of design space on the horizontal scale. This graph also has an adjustable crosshair that is shown at the 50 percent point on the fraction of design space scale. The crosshairs indicate that the unscaled prediction variance will be at most 0.425 σ^2 (remember that the unscaled prediction variance divides by σ^2, that's why the point on the vertical scale is 0.425) over a region that covers 50 percent of the design region. Therefore, an FDS plot gives a simple display of how the prediction variance is distributed throughout the design region. An ideal FDS plot would be flat with a small value of the unscaled prediction variance. FDS plots are an ideal way to compare designs in terms of their potential prediction performance.

The final display in the JMP output is a surface plot of the unscaled prediction variance. The contours of constant prediction variance for the 2^2 are circular; that is, all points in the design space that are at the same distance from the center of the design have the same prediction variance.

Optimal design tools in software can be used to aid the experimenter in constructing designs when the requirements of the experiment are such that a standard design isn't available. For example, consider a situation where an experimenter is interested in three continuous factors, each at two levels, and wants to be sure that all main effects and two-factor interactions can be estimated. It is also desirable to have replication so that formal statistical testing can be conducted. A logical design choice would seem to be the 2^3 factorial with two replicates, requiring 16 runs. However, the experimental budget can only accommodate 12 runs. There isn't a standard design available with this sample size, so an optimal design is a reasonable alternative in this situation.

The left side of the display below shows a 12-run *D*-optimal design created using the optimal design tool in JMP. The right-hand side contains some estimation efficiency information. The first thing we notice is that the relative standard error of the model regression coefficients are all equal, but they are not $1/\sqrt{12} = 0.289$, as they would be for a 12-run orthogonal design (the relative standard error is the standard error of the model parameter apart from the unknown constant σ). This is because the *D*-optimal design is not orthogonal. The main effects are orthogonal to each other but not to all of the two-factor interactions. Every main effect is correlated with the two-factor interaction not including that factor and the correlation is 0.33. However, all model coefficients have the same relative standard error, so this *D*-optimal design is an **equi-variance design**, meaning all parameters are estimated with the same precision. This design is not exactly *D*-optimal; it's *D*-efficiency is 94.28%. The reason that this design isn't *D*-optimal is that it isn't orthogonal. There isn't an orthogonal design with 12 runs available for this problem situation. The length of the confidence intervals on each model parameter (apart from the intercept) is increased by 6.1% relative to what the length would be if a 12-run orthogonal design could be used. The power of this design using $\alpha = 0.10$ is 84.6%.

Run	X1	X2	X3
1	1	1	-1
2	1	1	1
3	1	1	-1
4	-1	1	1
5	1	-1	1
6	1	-1	-1
7	-1	1	-1
8	-1	-1	1
9	-1	-1	-1
10	-1	1	1
11	1	-1	1
12	-1	-1	-1

Estimation Efficiency

Term	Fractional Increase in CI Length	Relative Std Error of Estimate
Intercept	0.061	0.306
X1	0.061	0.306
X2	0.061	0.306
X3	0.061	0.306
X1*X2	0.061	0.306
X1*X3	0.061	0.306
X2*X3	0.061	0.306
X1*X2*X3	0.061	0.306

6.8 The Addition of Center Points to the 2^k Design

A potential concern in the use of two-level factorial designs is the assumption of **linearity** in the factor effects. Of course, perfect linearity is unnecessary, and the 2^k system will work quite well even when the linearity assumption holds only very approximately. In fact, we have noted that if **interaction terms** are added to a main effect or first-order model, resulting in

$$y = \beta_0 + \sum_{j=1}^{k} \beta_j x_j + \sum\sum_{i<j} \beta_{ij} x_i x_j + \epsilon \tag{6.28}$$

then we have a model capable of representing some curvature in the response function. This curvature, of course, results from the twisting of the plane induced by the interaction terms $\beta_{ij}x_ix_j$.

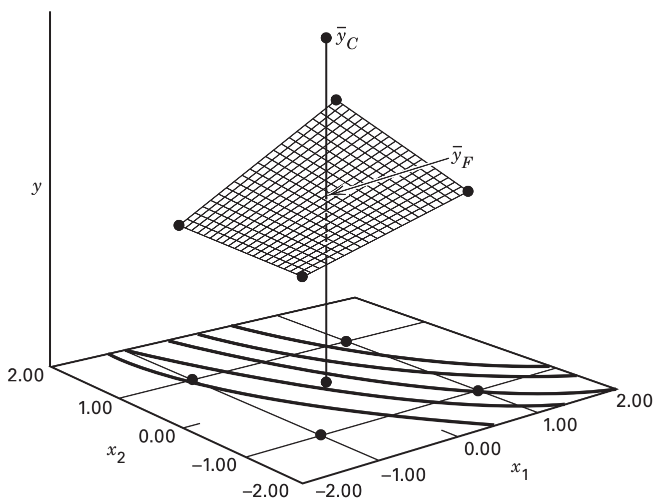
In some situations, the curvature in the response function will not be adequately modeled by Equation 6.28. In such cases, a logical model to consider is

$$y = \beta_0 + \sum_{j=1}^{k} \beta_j x_j + \sum\sum_{i<j} \beta_{ij} x_i x_j + \sum_{j=1}^{k} \beta_{jj} x_j^2 + \epsilon \tag{6.29}$$

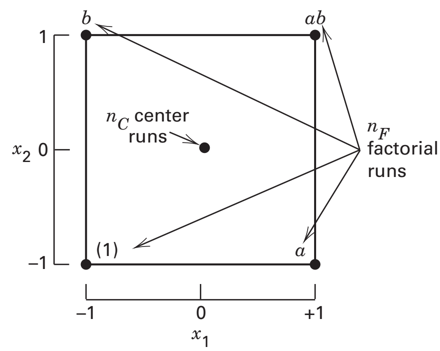
where the β_{jj} represent pure second-order or **quadratic effects**. Equation 6.29 is called a **second-order response surface model**.

In running a two-level factorial experiment, we usually anticipate fitting the first-order model in Equation 6.28, but we should be alert to the possibility that the second-order model in Equation 6.29 is more appropriate. There is a method of replicating certain points in a 2^k factorial that will provide protection against curvature from second-order effects as well as allow an independent estimate of error to be obtained. The method consists of adding **center points** to the 2^k design. These consist of n_C replicates run at the points $x_i = 0 (i = 1, 2, \ldots, k)$. One important reason for adding the replicate runs at the design center is that center points do not affect the usual effect estimates in a 2^k design. When we add center points, we assume that the k factors are **quantitative**.

To illustrate the approach, consider a 2^2 design with one observation at each of the factorial points $(-, -)$, $(+, -)$, $(-, +)$, and $(+, +)$ and n_C observations at the center point $(0, 0)$. Figures 6.37 and 6.38 illustrate the situation.



■ **FIGURE 6.37** **A 2^2 design with center points**



■ **FIGURE 6.38** **A 2^2 design with center points**

Let $\bar{y}_F$ be the average of the four runs at the four factorial points, and $\bar{y}_C$ be the average of the n_C runs at the center point. If the difference $\bar{y}_F - \bar{y}_C$ is small, then the center points lie on or near the plane passing through the factorial points, and there is no quadratic curvature. On the other hand, if $\bar{y}_F - \bar{y}_C$ is large, then quadratic curvature is present. A single-degree-of-freedom **sum of squares for pure quadratic curvature** is given by

$$SS_{\text{Pure quadratic}} = \frac{n_F n_C (\bar{y}_F - \bar{y}_C)^2}{n_F + n_C} \tag{6.30}$$

where, in general, n_F is the number of factorial design points. This sum of squares may be incorporated into the ANOVA and may be compared to the error mean square to test for pure quadratic curvature. More specifically, when points are added to the center of the 2^k design, the test for curvature (using Equation 6.30) actually tests the hypotheses

$$H_0: \sum_{j=1}^{k} \beta_{jj} = 0$$

$$H_1: \sum_{j=1}^{k} \beta_{jj} \neq 0$$

Furthermore, if the factorial points in the design are unreplicated, one may use the n_C center points to construct an estimate of error with $n_C - 1$ degrees of freedom. A t-test can also be used to test for curvature. Refer to the **supplemental text material** for this chapter.

EXAMPLE 6.7

We will illustrate the addition of center points to a 2^k design by reconsidering the pilot plant experiment in Example 6.2. Recall that this is an unreplicated 2^4 design. Refer to the original experiment shown in Table 6.10. Suppose that four center points are added to this experiment, and at the points $x_1 = x_2 = x_3 = x_4 = 0$ the four observed filtration rates were 73, 75, 66, and 69. The average of these four center points is $\bar{y}_C = 70.75$, and the average of the 16 factorial runs is $\bar{y}_F = 70.06$. Since $\bar{y}_C$ and $\bar{y}_F$ are very similar, we suspect that there is no strong curvature present.

Table 6.24 summarizes the analysis of variance for this experiment. In the upper portion of the table, we have fit the full model. The mean square for pure error is calculated from the center points as follows:

$$MS_E = \frac{SS_E}{n_C - 1} = \frac{\sum_{\text{Center points}} (y_i - \bar{y}_c)^2}{n_C - 1} \tag{6.31}$$

Thus, in Table 6.22,

$$MS_E = \frac{\sum_{i=1}^{4} (y_i - 70.75)^2}{4 - 1} = \frac{48.75}{3} = 16.25$$

The difference $\bar{y}_F - \bar{y}_C = 70.06 - 70.75 = -0.69$ is used to compute the pure quadratic (curvature) sum of squares in the ANOVA table from Equation 6.30 as follows:

$$SS_{\text{Pure quadratic}} = \frac{n_F n_C (\bar{y}_F - \bar{y}_C)^2}{n_F + n_C}$$
$$= \frac{(16)(4)(-0.69)^2}{16 + 4} = 1.51$$

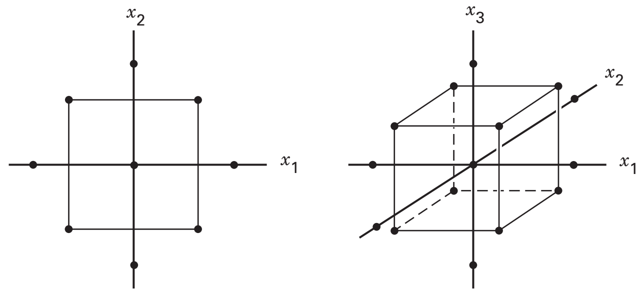
The ANOVA indicates that there is no evidence of second-order curvature in the response over the region of exploration. That is, the null hypothesis $H_0: \beta_{11} + \beta_{22} + \beta_{33} + \beta_{44} = 0$ cannot be rejected. The significant effects are A, C, D, AC, and AD. The ANOVA for the reduced model is shown in the lower portion of Table 6.24. The results of this analysis agree with those from Example 6.2, where the important effects were isolated using the normal probability plotting method.

■ **TABLE 6.24**
Analysis of Variance for Example 6.6

ANOVA for the Full Model

Source of Variation	Sum of Squares	DF	Mean Square	F	Prob > F
Model	5730.94	15	382.06	23.51	0.0121
A	*1870.56*	*1*	*1870.56*	*115.11*	*0.0017*
B	*39.06*	*1*	*39.06*	*2.40*	*0.2188*
C	*390.06*	*1*	*390.06*	*24.00*	*0.0163*
D	*855.56*	*1*	*855.56*	*52.65*	*0.0054*
AB	*0.063*	*1*	*0.063*	*3.846E-003*	*0.9544*
AC	*1314.06*	*1*	*1314.06*	*80.87*	*0.0029*
AD	*1105.56*	*1*	*1105.56*	*68.03*	*0.0037*
BC	*22.56*	*1*	*22.56*	*1.39*	*0.3236*
BD	*0.56*	*1*	*0.56*	*0.035*	*0.8643*
CD	*5.06*	*1*	*5.06*	*0.31*	*0.6157*
ABC	*14.06*	*1*	*14.06*	*0.87*	*0.4209*
ABD	*68.06*	*1*	*68.06*	*4.19*	*0.1332*
ACD	*10.56*	*1*	*10.56*	*0.65*	*0.4791*
BCD	*27.56*	*1*	*27.56*	*1.70*	*0.2838*
ABCD	*7.56*	*1*	*7.56*	*0.47*	*0.5441*
Pure quadratic					
Curvature	1.51	1	1.51	0.093	0.7802
Pure error	48.75	3	16.25		
Cor total	5781.20	19			
Model	5535.81	5	1107.16	59.02	<0.000
A	*1870.56*	*1*	*1870.56*	*99.71*	*<0.000*
C	*390.06*	*1*	*390.06*	*20.79*	*0.0005*
D	*855.56*	*1*	*855.56*	*45.61*	*<0.000*
AC	*1314.06*	*1*	*1314.06*	*70.05*	*<0.000*
AD	*1105.56*	*1*	*1105.56*	*58.93*	*<0.000*
Pure quadratic					
curvature	1.51	1	1.51	0.081	0.7809
Residual	243.87	13	18.76		
Lack of fit	*195.12*	*10*	*19.51*	*1.20*	*0.4942*
Pure error	*48.75*	*3*	*16.25*		
Cor total	5781.20	19			

■ **FIGURE 6.39** **Central composite designs**



(*a*) Two factors (*b*) Three factors

In Example 6.6, we concluded that there was no indication of quadratic effects; that is, a first-order model in A, C, D, along with the AC and AD interaction, is appropriate. However, there will be situations where the quadratic terms (x_i^2) will be required. To illustrate for the case of $k = 2$ design factors, suppose that the curvature test is significant so that we will now have to assume a second-order model such as

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_{12} x_1 x_2 + \beta_{11} x_1^2 + \beta_{22} x_2^2 + \epsilon$$

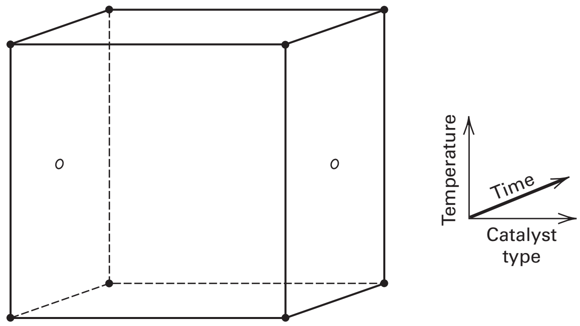
Unfortunately, we cannot estimate the unknown parameters (the β's) in this model because there are six parameters to estimate and the 2^2 design and center points in Figure 6.38 have only five independent runs.

A simple and highly effective solution to this problem is to augment the 2^k design with four **axial runs**, as shown in Figure 6.39*a* for the case of $k = 2$. The resulting design, called a **central composite design**, can now be used to fit the second-order model. Figure 6.39*b* shows a central composite design for $k = 3$ factors. This design has $14 + n_C$ runs (usually $3 \le n_C \le 5$) and is a very efficient design for fitting the 10-parameter second-order model in $k = 3$ factors.

Central composite designs are used extensively in building second-order response surface models. These designs will be discussed in more detail in Chapter 11.

We conclude this section with a few additional useful suggestions and observations concerning the use of center points.

1. When a factorial experiment is conducted in an ongoing process, consider using the current operating conditions (or recipe) as the center point in the design. This often assures the operating personnel that at least some of the runs in the experiment are going to be performed under familiar conditions, and so the results obtained (at least for these runs) are unlikely to be any worse than are typically obtained.
2. When the center point in a factorial experiment corresponds to the usual operating recipe, the experimenter can use the observed responses at the center point to provide a rough check of whether anything "unusual" occurred during the experiment. That is, the center point responses should be very similar to the responses observed historically in routine process operation. Often operating personnel will maintain a control chart for monitoring process performance. Sometimes the center point responses can be plotted directly on the control chart as a check of the manner in which the process was operating during the experiment.
3. Consider running the replicates at the center point in nonrandom order. Specifically, run one or two center points at or near the beginning of the experiment, one or two near the middle, and one or two near the end. By spreading the center points out in time, the experimenter has a rough check on the stability of the process during the experiment. For example, if a trend has occurred in the response while the experiment was performed, plotting the center point responses versus time order may reveal this.
4. Sometimes experiments must be conducted in situations where there is little or no prior information about process variability. In these cases, running two or three center points as the first few runs in the experiment can be very helpful. These runs can provide a preliminary estimate of variability. If the magnitude of the variability seems reasonable, continue; on the contrary, if larger than anticipated (or reasonable!) variability

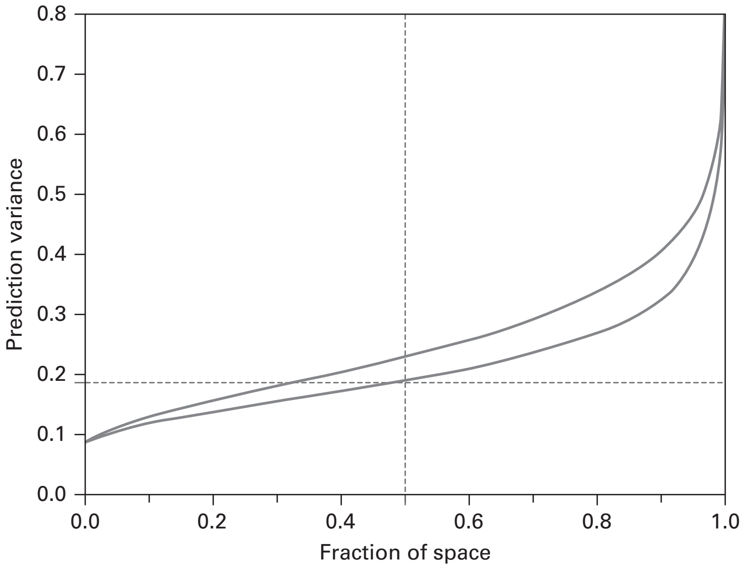


■ **FIGURE 6.40** **A 2^3 factorial design with one qualitative factor and center points**

is observed, stop. Often it will be very profitable to study the question of why the variability is so large before proceeding with the rest of the experiment.

5. Usually, center points are employed when all design factors are **quantitative**. However, sometimes there will be one or more qualitative or categorical variables and several quantitative ones. Center points can still be employed in these cases. To illustrate, consider an experiment with two quantitative factors, time and temperature, each at two levels, and a single qualitative factor, catalyst type, also with two levels (organic and nonorganic). Figure 6.40 shows the 2^3 design for these factors. Notice that the center points are placed in the opposed faces of the cube that involve the quantitative factors. In other words, the center points can be run at the high- and low-level treatment combinations of the qualitative factors as long as those subspaces involve only quantitative factors.

It is interesting to note that adding center runs to a 2^k design is never a D-optimal design strategy. To illustrate, recall the 12-run D-optimal design for three factors that we constructed at the end of Section 6.7. The D-efficiency of that design was 94.28%. The D-efficiency of the 2^3 design with four center points is only 70.64%. Furthermore, in the 12-run D-optimal design the relative standard error of the model parameters was 0.306, while in the design with four center points it is 0.354. As one would expect, the D-optimal design results in model parameters that are more precisely estimated. The fraction of design space plot in Figure 6.41 compares the prediction variance



■ **FIGURE 6.41** **Fraction of design space plot comparing a 12-run *D*-optimal design (lower curve) to a 2^3 design with four center points (upper curve)** performance of the two designs. The lower curve in this figure is the FDS curve for the D-optimal design. Clearly, the D-optimal design outperforms the 2^3 design with four center points in terms of the ability to predict the response over almost all of the design space. However, the D-optimal design does not have the capability to detect potential curvature in the response function. The trade-off between the two designs is a decision that the experimenter needs to consider carefully.

6.9 Why We Work with Coded Design Variables

The reader will have noticed that we have performed all of the analysis and model fitting for a 2^k factorial design in this chapter using **coded** design variables, $-1 \leq x_i \leq +1$, and not the design factors in their **original** units (sometimes called actual, natural, or **engineering** units). When the engineering units are used, we can obtain different numerical results in comparison to the coded unit analysis, and often the results will not be as easy to interpret.

To illustrate some of the differences between the two analyses, consider the following experiment. A simple DC-circuit is constructed in which two different resistors, 1 and 2Ω, can be connected. The circuit also contains an ammeter and a variable-output power supply. With a resistor installed in the circuit, the power supply is adjusted until a current flow of either 4 or 6 amps is obtained. Then the voltage output of the power supply is read from a voltmeter. Two replicates of a 2^2 factorial design are performed, and Table 6.25 presents the results. We know that Ohm's law determines the observed voltage, apart from measurement error. However, the analysis of these data via empirical modeling lends some insight into the value of coded units and the engineering units in designed experiments.

Tables 6.26 and 6.27 present the regression models obtained using the design variables in the usual coded variables (x_1 and x_2) and the engineering units, respectively. Minitab was used to perform the calculations. Consider first the coded variable analysis in Table 6.26. The design is orthogonal and the coded variables are also orthogonal. Notice that both main effects (x_1 = current) and (x_2 = resistance) are significant as is the interaction. In the coded variable analysis, the magnitudes of the model coefficients are directly comparable; that is, they all are dimensionless, and they measure the effect of changing each design factor over a one-unit interval. Furthermore, they are all estimated with the same precision (notice that the standard error of all three coefficients is 0.053). The interaction effect is smaller than either main effect, and the effect of current is just slightly more than one-half the resistance effect. This suggests that over the range of the factors studied, resistance is a more important variable. Coded variables are very effective for determining the **relative size** of factor effects.

■ **TABLE 6.25**
The Circuit Experiment

I (Amps)	R (Ohms)	x_1	x_2	V (Volts)
4	1	−1	−1	3.802
4	1	−1	−1	4.013
6	1	1	−1	6.065
6	1	1	−1	5.992
4	2	−1	1	7.934
4	2	−1	1	8.159
6	2	1	1	11.865
6	2	1	1	12.138

■ **TABLE 6.26**
Regression Analysis for the Circuit Experiment Using Coded Variables

```
The regression equation is
V = 7.50 + 1.52 × 1 + 2.53 × 2 + 0.458 × 1 × 2

Predictor          Coef         StDev          T          P
Constant        7.49600       0.05229     143.35      0.000
x 1             1.51900       0.05229      29.05      0.000
x 2             2.52800       0.05229      48.34      0.000
x 1 x 2         0.45850       0.05229       8.77      0.001

S = 0.1479      R-Sq = 99.9%          R-Sq(adj) = 99.8%

Analysis of Variance

Source              DF          SS          MS          F          P
Regression           3      71.267      23.756    1085.95      0.000
Residual  Error      4       0.088       0.022
Total                7      71.354
```

■ **TABLE 6.27**
Regression Analysis for the Circuit Experiment Using Engineering Units

```
The regression equation is
V = -0.806 + 0.144 I + 0.471 R + 0.917 IR

Predictor           Coef        StDev          T          P
Constant         -0.8055       0.8432      -0.96      0.394
I                 0.1435       0.1654       0.87      0.434
R                 0.4710       0.5333       0.88      0.427
IR                0.9170       0.1046       8.77      0.001

S = 0.1479      R-Sq = 99.9%     R-Sq(adj) = 99.8%

Analysis of Variance

Source              DF          SS          MS          F          P
Regression           3      71.267      23.756    1085.95      0.000
Residual Error       4       0.088       0.022
Total                7      71.354
```

Now consider the analysis based on the engineering units, as shown in Table 6.27. In this model, only the interaction is significant. The model coefficient for the interaction term is 0.9170, and the standard error is 0.1046. We can construct a t statistic for testing the hypothesis that the interaction coefficient is unity:

$$t_0 = \frac{\hat{\beta}_{IR} - 1}{se(\hat{\beta}_{IR})} = \frac{0.9170 - 1}{0.1046} = -0.7935$$

■ **TABLE 6.28**

Regression Analysis for the Circuit Experiment (Interaction Term Only)

```
The regression equation is
V = 1.00 IR

Predictor           Coef          Std. Dev.         T           P
Noconstant
IR               1.00073           0.00550     181.81       0.000

S = 0.1255
Analysis of Variance

Source              DF            SS          MS          F          P
Regression           3        71.267      23.756    1085.95      0.000
Residual Error       4         0.088       0.022
Total                7        71.354
```

The *P*-value for this test statistic is $P = 0.76$. Therefore, we cannot reject the null hypothesis that the coefficient is unity, which is consistent with Ohm's law. Note that the regression coefficients are not dimensionless and that they are estimated with differing precision. This is because the experimental design, with the factors in the engineering units, is not orthogonal.

Because the intercept and the main effects are not significant, we could consider fitting a model containing only the interaction term *IR*. The results are shown in Table 6.28. Notice that the estimate of the interaction term regression coefficient is now different from what it was in the previous engineering-units analysis because the design in engineering units is not orthogonal. The coefficient is also virtually unity.

Generally, the engineering units are not directly comparable, but they may have physical meaning as in the present example. This could lead to possible simplification based on the underlying mechanism. In almost all situations, the coded unit analysis is preferable. It is fairly unusual for a simplification based on some underlying mechanism (as in our example) to occur. The fact that coded variables let an experimenter see the relative importance of the design factors is useful in practice.

6.10 Problems

6.1 In a 2^4 factorial design, the number of degrees of freedom for the model, assuming the complete factorial model, is

(a) 7 **(b)** 5 **(c)** 6

(d) 11 **(e)** 12 **(f)** none of the above

6.2 A 2^3 factorial is replicated twice. The number of pure error or residual degrees of freedom are

(a) 4 **(b)** 12 **(c)** 15

(d) 2 **(e)** 8 **(f)** none of the above

6.3 A 2^3 factorial is replicated twice. The ANOVA indicates that all main effects are significant but the interactions are not significant. The interaction terms are dropped from the model. The number of residual degrees of freedom for the reduced model are

(a) 12 **(b)** 8 **(c)** 16

(d) 14 **(e)** 10 **(f)** none of the above

6.4 A 2^3 factorial is replicated three times. The ANOVA indicates that all main effects are significant but two of the interactions are not significant. The interaction terms are dropped from the model. The number of residual degrees of freedom for the reduced model are

(a) 12 **(b)** 14 **(c)** 6

(d) 10 **(e)** 8 **(f)** none of the above

6.5 An engineer is interested in the effects of cutting speed (A), tool geometry (B), and cutting angle (C) on the life (in hours) of a machine tool. Two levels of each factor are chosen, and three replicates of a 2^3 factorial design are run. The results are as follows:

				Replicate		
A	**B**	**C**	**Treatment Combination**	**I**	**II**	**III**
−	−	−	(1)	22	31	25
+	−	−	*a*	32	43	29
−	+	−	*b*	35	34	50
+	+	−	*ab*	55	47	46
−	−	+	*c*	44	45	38
+	−	+	*ac*	40	37	36
−	+	+	*bc*	60	50	54
+	+	+	*abc*	39	41	47

(a) Estimate the factor effects. Which effects appear to be large?

(b) Use the analysis of variance to confirm your conclusions for part (a).

(c) Write down a regression model for predicting tool life (in hours) based on the results of this experiment.

(d) Analyze the residuals. Are there any obvious problems?

(e) On the basis of an analysis of main effect and interaction plots, what coded factor levels of A, B, and C would you recommend using?

6.6 Reconsider part (c) of Problem 6.5. Use the regression model to generate response surface and contour plots of the tool life response. Interpret these plots. Do they provide insight regarding the desirable operating conditions for this process?

6.7 Find the standard error of the factor effects and approximate 95 percent confidence limits for the factor effects in Problem 6.5. Do the results of this analysis agree with the conclusions from the analysis of variance?

6.8 Plot the factor effects from Problem 6.5 on a graph relative to an appropriately scaled t distribution. Does this graphical display adequately identify the important factors? Compare the conclusions from this plot with the results from the analysis of variance.

6.9 A router is used to cut locating notches on a printed circuit board. The vibration level at the surface of the board as it is cut is considered to be a major source of dimensional variation in the notches. Two factors are thought to influence vibration: bit size (A) and cutting speed (B). Two bit sizes ($\frac{1}{16}$ and $\frac{1}{8}$ in.) and two speeds (40 and 90 rpm) are selected, and four boards are cut at each set of conditions shown below. The response variable is vibration measured as the resultant vector of three accelerometers (x, y, and z) on each test circuit board.

			Replicate			
A	**B**	**Treatment Combination**	**I**	**II**	**III**	**IV**
−	−	(1)	18.2	18.9	12.9	14.4
+	−	*a*	27.2	24.0	22.4	22.5
−	+	*b*	15.9	14.5	15.1	14.2
+	+	*ab*	41.0	43.9	36.3	39.9

(a) Analyze the data from this experiment.

(b) Construct a normal probability plot of the residuals, and plot the residuals versus the predicted vibration level. Interpret these plots.

(c) Draw the AB interaction plot. Interpret this plot. What levels of bit size and speed would you recommend for routine operation?

6.10 Reconsider the experiment described in Problem 6.5. Suppose that the experimenter only performed the eight trials from replicate I. In addition, he ran four center points and obtained the following response values: 36, 40, 43, 45.

(a) Estimate the factor effects. Which effects are large?

(b) Perform an analysis of variance, including a check for pure quadratic curvature. What are your conclusions?

(c) Write down an appropriate model for predicting tool life, based on the results of this experiment. Does this model differ in any substantial way from the model in Problem 6.5, part (c)?

(d) Analyze the residuals.

(e) What conclusions would you draw about the appropriate operating conditions for this process?

6.11 An experiment was performed to improve the yield of a chemical process. Four factors were selected, and two replicates of a completely randomized experiment were run. The results are shown in the following table:

	Replicate			Replicate	
Treatment Combination	**I**	**II**	**Treatment Combination**	**I**	**II**
(1)	90	93	*d*	98	95
a	74	78	*ad*	72	76
b	81	85	*bd*	87	83
ab	83	80	*abd*	85	86
c	77	78	*cd*	99	90
ac	81	80	*acd*	79	75
bc	88	82	*bcd*	87	84
abc	73	70	*abcd*	80	80

(a) Estimate the factor effects.

(b) Prepare an analysis of variance table and determine which factors are important in explaining yield.

(c) Write down a regression model for predicting yield, assuming that all four factors were varied over the range from -1 to $+1$ (in coded units).

(d) Plot the residuals versus the predicted yield and on a normal probability scale. Does the residual analysis appear satisfactory?

(e) Two three-factor interactions, *ABC* and *ABD*, apparently have large effects. Draw a cube plot in the factors *A*, *B*, and *C* with the average yields shown at each corner. Repeat using the factors *A*, *B*, and *D*. Do these two plots aid in data interpretation? Where would you recommend that the process be run with respect to the four variables?

6.12 A bacteriologist is interested in the effects of two different culture media and two different times on the growth of a particular virus. He or she performs six replicates of a 2^2 design, making the runs in random order. Analyze the bacterial growth data that follow and draw appropriate conclusions. Analyze the residuals and comment on the model's adequacy.

	Culture Medium			
Time (h)	**1**		**2**	
	21	22	25	26
12	23	28	24	25
	20	26	29	27
	37	39	31	34
18	38	38	29	33
	35	36	30	35

6.13 An industrial engineer employed by a beverage bottler is interested in the effects of two different types of 32-ounce bottles on the time to deliver 12-bottle cases of the product. The two bottle types are glass and plastic. Two workers are used to perform a task consisting of moving 40 cases of the product 50 feet on a standard type of hand truck and stacking the cases in a display. Four replicates of a 2^2 factorial design are performed, and the times observed are listed in the following table. Analyze the data and draw appropriate conclusions. Analyze the residuals and comment on the model's adequacy.

	Worker			
Bottle Type	**1**		**2**	
Glass	5.12	4.89	6.65	6.24
	4.98	5.00	5.49	5.55
Plastic	4.95	4.43	5.28	4.91
	4.27	4.25	4.75	4.71

6.14 In Problem 6.13, the engineer was also interested in potential fatigue differences resulting from the two types of bottles. As a measure of the amount of effort required, he measured the elevation of the heart rate (pulse) induced by the task. The results follow. Analyze the data and draw conclusions. Analyze the residuals and comment on the model's adequacy.

	Worker			
Bottle Type	**1**		**2**	
Glass	39	45	20	13
	58	35	16	11
Plastic	44	35	13	10
	42	21	16	15

6.15 Calculate approximate 95 percent confidence limits for the factor effects in Problem 6.14. Do the results of this analysis agree with the analysis of variance performed in Problem 6.14?

6.16 An article in the *AT&T Technical Journal* (March/April 1986, Vol. 65, pp. 39–50) describes the application of two-level factorial designs to integrated circuit manufacturing. A basic processing step is to grow an epitaxial layer on polished silicon wafers. The wafers mounted on a susceptor are positioned inside a bell jar, and chemical vapors are introduced. The susceptor is rotated, and heat is applied until the epitaxial layer is thick enough. An experiment was run using two factors: arsenic flow rate (*A*) and deposition time (*B*). Four replicates were run, and the epitaxial layer thickness was measured (μm). The data are shown in Table P6.1.

■ **TABLE P6.1**
The 2^2 Design for Problem 6.16

		Replicate					Factor Levels	
A	*B*	I	II	III	IV		Low (−)	High (+)
−	−	14.037	16.165	13.972	13.907	*A*	55%	59%
+	−	13.880	13.860	14.032	13.914			
−	+	14.821	14.757	14.843	14.878	*B*	Short	Long
+	+	14.888	14.921	14.415	14.932		(10 min)	(15 min)

(a) Estimate the factor effects.

(b) Conduct an analysis of variance. Which factors are important?

(c) Write down a regression equation that could be used to predict epitaxial layer thickness over the region of arsenic flow rate and deposition time used in this experiment.

(d) Analyze the residuals. Are there any residuals that should cause concern?

(e) Discuss how you might deal with the potential outlier found in part (d).

6.17 ***Continuation of Problem 6.16***. Use the regression model in part (c) of Problem 6.16 to generate a response surface contour plot for epitaxial layer thickness. Suppose it is critically important to obtain layer thickness of 14.5 μm. What settings of arsenic flow rate and decomposition time would you recommend?

6.18 ***Continuation of Problem 6.17***. How would your answer to Problem 6.17 change if arsenic flow rate was more difficult to control in the process than the deposition time?

6.19 A nickel–titanium alloy is used to make components for jet turbine aircraft engines. Cracking is a potentially serious problem in the final part because it can lead to nonrecoverable failure. A test is run at the parts producer to determine the effect of four factors on cracks. The four factors are pouring temperature (A), titanium content (B), heat treatment method (C), and amount of grain refiner used (D). Two replicates of a 2^4 design are run, and the length of crack (in mm $\times$ 10^{-2}) induced in a sample coupon subjected to a standard test is measured. The data are shown in Table P6.2.

■ **TABLE P6.2**
The Experiment for problem 6.19

					Replicate	
A	*B*	*C*	*D*	Treatment Combination	I	II
−	−	−	−	(1)	7.037	6.376
+	−	−	−	*a*	14.707	15.219
−	+	−	−	*b*	11.635	12.089
+	+	−	−	*ab*	17.273	17.815
−	−	+	−	*c*	10.403	10.151
+	−	+	−	*ac*	4.368	4.098
−	+	+	−	*bc*	9.360	9.253
+	+	+	−	*abc*	13.440	12.923
−	−	−	+	*d*	8.561	8.951
+	−	−	+	*ad*	16.867	17.052
−	+	−	+	*bd*	13.876	13.658
+	+	−	+	*abd*	19.824	19.639
−	−	+	+	*cd*	11.846	12.337
+	−	+	+	*acd*	6.125	5.904
−	+	+	+	*bcd*	11.190	10.935
+	+	+	+	*abcd*	15.653	15.053

(a) Estimate the factor effects. Which factor effects appear to be large?

(b) Conduct an analysis of variance. Do any of the factors affect cracking? Use $\alpha = 0.05$.

(c) Write down a regression model that can be used to predict crack length as a function of the significant main effects and interactions you have identified in part (b).

(d) Analyze the residuals from this experiment.

(e) Is there an indication that any of the factors affect the variability in cracking?

(f) What recommendations would you make regarding process operations? Use interaction and/or main effect plots to assist in drawing conclusions.

6.20 ***Continuation of Problem 6.19***. One of the variables in the experiment described in Problem 6.19, heat treatment method (C), is a categorical variable. Assume that the remaining factors are continuous.

(a) Write two regression models for predicting crack length, one for each level of the heat treatment method variable. What differences, if any, do you notice in these two equations?

(b) Generate appropriate response surface contour plots for the two regression models in part (a).

(c) What set of conditions would you recommend for the factors A, B, and D if you use heat treatment method $C = +$?

(d) Repeat part (c) assuming that you wish to use heat treatment method $C = -$.

6.21 An experimenter has run a single replicate of a 2^4 design. The following effect estimates have been calculated:

$$
\begin{array}{lll}
A = 76.95 & AB = -51.32 & ABC = -2.82 \\
B = -67.52 & AC = 11.69 & ABD = -6.50 \\
C = -7.84 & AD = 9.78 & ACD = 10.20 \\
D = -18.73 & BC = 20.78 & BCD = -7.98 \\
 & BD = 14.74 & ABCD = -6.25 \\
 & CD = 1.27 &
\end{array}
$$

(a) Construct a normal probability plot of these effects.

(b) Identify a tentative model, based on the plot of the effects in part (a).

6.22 The effect estimates from a 2^4 factorial design are as follows: ABCD = −1.5138, ABC = −1.2661, ABD = −0.9852, ACD = −0.7566, BCD = −0.4842, CD = −0.0795, BD = −0.0793, AD = 0.5988, BC = 0.9216, AC = 1.1616, AB = 1.3266, D = 4.6744, C = 5.1458, B = 8.2469, and A = 12.7151. Are you comfortable with the conclusions that all main effects are active?

6.23 The effect estimates from a 2^4 factorial experiment are listed here. Are any of the effects significant? ABCD = −2.5251, BCD = 4.4054, ACD = −0.4932, ABD = −5.0842, ABC = −5.7696, CD = 4.6707, BD = −4.6620, BC = −0.7982, AD = −1.6564, AC = 1.1109, AB = −10.5229, D = −6.0275, C = −8.2045, B = −6.5304, and A = −0.7914.

6.24 Consider a variation of the bottle filling experiment from Example 5.3. Suppose that only two levels of carbonation are used so that the experiment is a 2^3 factorial design with two replicates. The data are shown in Table P6.3.

■ **TABLE P6.3**
Fill Height Experiment from Problem 6.24

	Coded Factors			Fill Height Deviation			Factor Levels	
Run	***A***	***B***	***C***	**Replicate 1**	**Replicate 2**		**Low (−1)**	**High (+1)**
1	−	−	−	−3	−1	A (%)	10	12
2	+	−	−	0	1	B (psi)	25	30
3	−	+	−	−1	0	C (b/m)	200	250
4	+	+	−	2	3			
5	−	−	+	−1	0			
6	+	−	+	2	1			
7	−	+	+	1	1			
8	+	+	+	6	5			

(a) Analyze the data from this experiment. Which factors significantly affect fill height deviation?

(b) Analyze the residuals from this experiment. Are there any indications of model inadequacy?

(c) Obtain a model for predicting fill height deviation in terms of the important process variables. Use this model to construct contour plots to assist in interpreting the results of the experiment.

(d) In part (a), you probably noticed that there was an interaction term that was borderline significant. If you did not include the interaction term in your model, include it now and repeat the analysis. What difference did this make? If you elected to include the interaction term in part (a), remove it and repeat the analysis. What difference does the interaction term make?

6.25 I am always interested in improving my golf scores. Since a typical golfer uses the putter for about 35–45 percent of his or her strokes, it seems reasonable that improving one's putting is a logical and perhaps simple way to improve a golf score ("The man who can putt is a match for any man."—Willie Parks, 1864–1925, two time winner of the British Open). An experiment was conducted to study the effects of four factors on putting accuracy. The design factors are length of putt, type of putter, breaking putt versus straight putt, and level versus downhill putt. The response variable is distance from the ball to the center of the cup after the ball comes to rest. One golfer performs the experiment, a 2^4 factorial design with seven replicates was used, and all putts are made in random order. The results are shown in Table P6.4.

■ **TABLE P6.4**
The Putting Experiment from Problem 6.25

Design Factors				Distance from Cup (replicates)						
Length of Putt (ft)	**Type of Putter**	**Break of Putt**	**Slope of Putt**	**1**	**2**	**3**	**4**	**5**	**6**	**7**
10	Mallet	Straight	Level	10.0	18.0	14.0	12.5	19.0	16.0	18.5
30	Mallet	Straight	Level	0.0	16.5	4.5	17.5	20.5	17.5	33.0
10	Cavity back	Straight	Level	4.0	6.0	1.0	14.5	12.0	14.0	5.0
30	Cavity back	Straight	Level	0.0	10.0	34.0	11.0	25.5	21.5	0.0
10	Mallet	Breaking	Level	0.0	0.0	18.5	19.5	16.0	15.0	11.0
30	Mallet	Breaking	Level	5.0	20.5	18.0	20.0	29.5	19.0	10.0
10	Cavity back	Breaking	Level	6.5	18.5	7.5	6.0	0.0	10.0	0.0
30	Cavity back	Breaking	Level	16.5	4.5	0.0	23.5	8.0	8.0	8.0
10	Mallet	Straight	Downhill	4.5	18.0	14.5	10.0	0.0	17.5	6.0
30	Mallet	Straight	Downhill	19.5	18.0	16.0	5.5	10.0	7.0	36.0
10	Cavity back	Straight	Downhill	15.0	16.0	8.5	0.0	0.5	9.0	3.0
30	Cavity back	Straight	Downhill	41.5	39.0	6.5	3.5	7.0	8.5	36.0
10	Mallet	Breaking	Downhill	8.0	4.5	6.5	10.0	13.0	41.0	14.0
30	Mallet	Breaking	Downhill	21.5	10.5	6.5	0.0	15.5	24.0	16.0
10	Cavity back	Breaking	Downhill	0.0	0.0	0.0	4.5	1.0	4.0	6.5
30	Cavity back	Breaking	Downhill	18.0	5.0	7.0	10.0	32.5	18.5	8.0

(a) Analyze the data from this experiment. Which factors significantly affect putting performance?

(b) Analyze the residuals from this experiment. Are there any indications of model inadequacy?

6.26 Semiconductor manufacturing processes have long and complex assembly flows, so matrix marks and automated 2d-matrix readers are used at several process steps throughout factories. Unreadable matrix marks negatively affect factory run rates because manual entry of part data is required before manufacturing can resume. A 2^4 factorial experiment was conducted to develop a 2d-matrix laser mark on a metal cover that protects a substrate-mounted die. The design factors are $A =$ laser power (9 and 13 W), $B =$ laser pulse frequency (4000 and 12,000 Hz), $C =$ matrix cell size (0.07 and 0.12 in.), and $D =$ writing speed (10 and 20 in./sec), and the response variable is the unused error correction (UEC). This is a measure of the unused portion of the redundant information embedded in the 2d-matrix. A UEC of 0 represents the lowest reading that still results in a decodable matrix, while a value of 1 is the highest reading. A DMX Verifier was used to measure the UEC. The data from this experiment are shown in Table P6.5.

(a) Analyze the data from this experiment. Which factors significantly affect the UEC?

(b) Analyze the residuals from this experiment. Are there any indications of model inadequacy?

■ **TABLE P6.5**
The 2^4 Experiment for Problem 6.26

Standard Order	Run Order	Laser Power	Pulse Frequency	Cell Size	Writing Speed	UEC
8	1	1.00	1.00	1.00	−1.00	0.8
10	2	1.00	−1.00	−1.00	1.00	0.81
12	3	1.00	1.00	−1.00	1.00	0.79
9	4	−1.00	−1.00	−1.00	1.00	0.6
7	5	−1.00	1.00	1.00	−1.00	0.65
15	6	−1.00	1.00	1.00	1.00	0.55
2	7	1.00	−1.00	−1.00	−1.00	0.98
6	8	1.00	−1.00	1.00	−1.00	0.67
16	9	1.00	1.00	1.00	1.00	0.69
13	10	−1.00	−1.00	1.00	1.00	0.56
5	11	−1.00	−1.00	1.00	−1.00	0.63
14	12	1.00	−1.00	1.00	1.00	0.65
1	13	−1.00	−1.00	−1.00	−1.00	0.75
3	14	−1.00	1.00	−1.00	−1.00	0.72
4	15	1.00	1.00	−1.00	−1.00	0.98
11	16	−1.00	1.00	−1.00	1.00	0.63

6.27 Reconsider the experiment described in Problem 6.24. Suppose that four center points are available and that the UEC response at these four runs is 0.98, 0.95, 0.93, and 0.96, respectively. Reanalyze the experiment incorporating a test for curvature into the analysis. What conclusions can you draw? What recommendations would you make to the experimenters?

6.28 A company markets its products by direct mail. An experiment was conducted to study the effects of three factors on the customer response rate for a particular product. The three factors are A = type of mail used (3rd class, 1st class), B = type of descriptive brochure (color, black-and-white), and C = offered price ($19.95, $24.95). The mailings are made to two groups of 8000 randomly selected customers, with 1000 customers in each group receiving each treatment combination. Each group of customers is considered as a replicate. The response variable is the number of orders placed. The experimental data are shown in Table P6.6.

(a) Analyze the data from this experiment. Which factors significantly affect the customer response rate?

■ **TABLE P6.6**
The Direct Mail Experiment from Problem 6.28

	Coded Factors			Number of Orders		Factor Levels		
Run	A	B	C	Replicate 1	Replicate 2		Low (−1)	High (+1)
1	−	−	−	50	54	A (class)	3rd	1st
2	+	−	−	44	42	B (type)	BW	Color
3	−	+	−	46	48	C ($)	$19.95	$24.95
4	+	+	−	42	43			
5	−	−	+	49	46			
6	+	−	+	48	45			
7	−	+	+	47	48			
8	+	+	+	56	54			

(b) Analyze the residuals from this experiment. Are there any indications of model inadequacy?

(c) What would you recommend to the company?

6.29 Consider the single replicate of the 2^4 design in Example 6.2. Suppose that we had arbitrarily decided to analyze the data assuming that all three- and four-factor interactions were negligible. Conduct this analysis and compare your results with those obtained in the example. Do you think that it is a good idea to arbitrarily assume interactions to be negligible even if they are relatively high-order ones?

6.30 An experiment was run in a semiconductor fabrication plant in an effort to increase yield. Five factors, each at two levels, were studied. The factors (and levels) were A = aperture setting (small, large), B = exposure time (20% below nominal, 20% above nominal), C = development time (30 and 45 s), D = mask dimension (small, large), and E = etch time (14.5 and 15.5 min). The unreplicated 2^5 design shown below was run.

$(1) = 7$	$d = 8$	$e = 8$	$de = 6$
$a = 9$	$ad = 10$	$ae = 12$	$ade = 10$
$b = 34$	$bd = 32$	$be = 35$	$bde = 30$
$ab = 55$	$abd = 50$	$abe = 52$	$abde = 53$
$c = 16$	$cd = 18$	$ce = 15$	$cde = 15$
$ac = 20$	$acd = 21$	$ace = 22$	$acde = 20$
$bc = 40$	$bcd = 44$	$bce = 45$	$bcde = 41$
$abc = 60$	$abcd = 61$	$abce = 65$	$abcde = 63$

(a) Construct a normal probability plot of the effect estimates. Which effects appear to be large?

(b) Conduct an analysis of variance to confirm your findings for part (a).

(c) Write down the regression model relating yield to the significant process variables.

(d) Plot the residuals on normal probability paper. Is the plot satisfactory?

(e) Plot the residuals versus the predicted yields and versus each of the five factors. Comment on the plots.

(f) Interpret any significant interactions.

(g) What are your recommendations regarding process operating conditions?

(h) Project the 2^5 design in this problem into a 2^k design in the important factors. Sketch the design and show the average and range of yields at each run. Does this sketch aid in interpreting the results of this experiment?

6.31 ***Continuation of Problem 6.30***. Suppose that the experimenter had run four center points in addition to the 32 trials in the original experiment. The yields obtained at the center point runs were 68, 74, 76, and 70.

(a) Reanalyze the experiment, including a test for pure quadratic curvature.

(b) Discuss what your next step would be.

6.32 In a process development study on yield, four factors were studied, each at two levels: time (A), concentration (B), pressure (C), and temperature (D). A single replicate of a 2^4 design was run, and the resulting data are shown in Table P6.7.

(a) Construct a normal probability plot of the effect estimates. Which factors appear to have large effects?

(b) Conduct an analysis of variance using the normal probability plot in part (a) for guidance in forming an error term. What are your conclusions?

(c) Write down a regression model relating yield to the important process variables.

(d) Analyze the residuals from this experiment. Does your analysis indicate any potential problems?

(e) Can this design be collapsed into a 2^3 design with two replicates? If so, sketch the design with the average and range of yield shown at each point in the cube. Interpret the results.

6.33 ***Continuation of Problem 6.32***. Use the regression model in part (c) of Problem 6.32 to generate a response surface contour plot of yield. Discuss the practical value of this response surface plot.

6.34 ***The scrumptious brownie experiment***. The author is an engineer by training and a firm believer in learning by doing. I have taught experimental design for many years to a wide variety of audiences and have always assigned the planning, conduct, and analysis of an actual experiment to the class participants. The participants seem to enjoy this practical experience and always learn a great deal from it. This problem uses the results of an experiment performed by Gretchen Krueger at Arizona State University.

There are many different ways to bake brownies. The purpose of this experiment was to determine how the pan material, the brand of brownie mix, and the stirring method affect the scrumptiousness of brownies. The factor levels were as follows:

Factor	Low (−)	High (+)
A = pan material	Glass	Aluminum
B = stirring method	Spoon	Mixer
C = brand of mix	Expensive	Cheap

The response variable was scrumptiousness, a subjective measure derived from a questionnaire given to the subjects

■ **TABLE P6.7**
Process Development Experiment from Problem 6.32

Run Number	Actual Run Order	A	B	C	D	Yield (lbs)
1	5	−	−	−	−	12
2	9	+	−	−	−	18
3	8	−	+	−	−	13
4	13	+	+	−	−	16
5	3	−	−	+	−	17
6	7	+	−	+	−	15
7	14	−	+	+	−	20
8	1	+	+	+	−	15
9	6	−	−	−	+	10
10	11	+	−	−	+	25
11	2	−	+	−	+	13
12	15	+	+	−	+	24
13	4	−	−	+	+	19
14	16	+	−	+	+	21
15	10	−	+	+	+	17
16	12	+	+	+	+	23

	Factor Levels	
	Low (−)	High (+)
A (h)	2.5	3
B (%)	14	18
C (psi)	60	80
D (°C)	225	250

who sampled each batch of brownies. (The questionnaire dealt with issues such as taste, appearance, consistency, aroma.) An eight-person test panel sampled each batch and filled out the questionnaire. The design matrix and the response data are as follows.

(a) Analyze the data from this experiment as if there were eight replicates of a 2^3 design. Comment on the results.

(b) Is the analysis in part (a) the correct approach? There are only eight batches; do we really have eight replicates of a 2^3 factorial design?

(c) Analyze the average and standard deviation of the scrumptiousness ratings. Comment on the results. Is this analysis more appropriate than the one in part (a)? Why or why not?

Brownie				Test Panel Results							
Batch	A	B	C	1	2	3	4	5	6	7	8
1	−	−	−	11	9	10	10	11	10	8	9
2	+	−	−	15	10	16	14	12	9	6	15
3	−	+	−	9	12	11	11	11	11	11	12
4	+	+	−	16	17	15	12	13	13	11	11
5	−	−	+	10	11	15	8	6	8	9	14
6	+	−	+	12	13	14	13	9	13	14	9
7	−	+	+	10	12	13	10	7	7	17	13
9	+	+	+	15	12	15	13	12	12	9	14

6.35 An experiment was conducted on a chemical process that produces a polymer. The four factors studied were temperature (A), catalyst concentration (B), time (C), and pressure (D). Two responses, molecular weight and viscosity, were observed. The design matrix and response data are shown in Table P6.8.

(a) Consider only the molecular weight response. Plot the effect estimates on a normal probability scale. What effects appear important?

(b) Use an analysis of variance to confirm the results from part (a). Is there indication of curvature?

(c) Write down a regression model to predict molecular weight as a function of the important variables.

(d) Analyze the residuals and comment on model adequacy.

(e) Repeat parts (a)–(d) using the viscosity response.

6.36 ***Continuation of Problem 6.35***. Use the regression models for molecular weight and viscosity to answer the following questions.

(a) Construct a response surface contour plot for molecular weight. In what direction would you adjust the process variables to increase molecular weight?

(b) Construct a response surface contour plot for viscosity. In what direction would you adjust the process variables to decrease viscosity?

■ **TABLE P6.8**
The 2^4 Experiment for Problem 6.35

Run Number	Actual Run Order	A	B	C	D	Molecular Weight	Viscosity	Factor Levels	Low (−)	High (+)
1	18	−	−	−	−	2400	1400	A (°C)	100	120
2	9	+	−	−	−	2410	1500	B (%)	4	8
3	13	−	+	−	−	2315	1520	C (min)	20	30
4	8	+	+	−	−	2510	1630	D (psi)	60	75
5	3	−	−	+	−	2615	1380			
6	11	+	−	+	−	2625	1525			
7	14	−	+	+	−	2400	1500			
8	17	+	+	+	−	2750	1620			
9	6	−	−	−	+	2400	1400			
10	7	+	−	−	+	2390	1525			
11	2	−	+	−	+	2300	1500			
12	10	+	+	−	+	2520	1500			
13	4	−	−	+	+	2625	1420			
14	19	+	−	+	+	2630	1490			
15	15	−	+	+	+	2500	1500			
16	20	+	+	+	+	2710	1600			
17	1	0	0	0	0	2515	1500			
18	5	0	0	0	0	2500	1460			
19	16	0	0	0	0	2400	1525			
20	12	0	0	0	0	2475	1500			

(c) What operating conditions would you recommend if it was necessary to produce a product with molecular weight between 2400 and 2500 and the lowest possible viscosity?

6.37 Consider the single replicate of the 2^4 design in Example 6.2. Suppose that we ran five points at the center (0, 0, 0, 0) and observed the responses 93, 95, 91, 89, and 96. Test for curvature in this experiment. Interpret the results.

6.38 ***A missing value in a 2^k* factorial**. It is not unusual to find that one of the observations in a 2^k design is missing due to faulty measuring equipment, a spoiled test, or some other reason. If the design is replicated n times ($n > 1$), some of the techniques discussed in Chapter 5 can be employed. However, for an unreplicated factorial ($n = 1$) some other method must be used. One logical approach is to estimate the missing value with a number that makes the highest order interaction contrast zero. Apply this technique to the experiment in Example 6.2 assuming that run ab is missing. Compare the results with the results of Example 6.2.

6.39 An engineer has performed an experiment to study the effect of four factors on the surface roughness of a machined part. The factors (and their levels) are A = tool angle (12, 15°), B = cutting fluid viscosity (300, 400), C = feed rate (10 and 15 in./min), and D = cutting fluid cooler used (no, yes). The data from this experiment (with the factors coded to the usual −1, +1 levels) are shown in Table P6.9.

(a) Estimate the factor effects. Plot the effect estimates on a normal probability plot and select a tentative model.

(b) Fit the model identified in part (a) and analyze the residuals. Is there any indication of model inadequacy?

(c) Repeat the analysis from parts (a) and (b) using $1/y$ as the response variable. Is there an indication that the transformation has been useful?

(d) Fit a model in terms of the coded variables that can be used to predict the surface roughness. Convert this prediction equation into a model in the natural variables.

■ **TABLE P6.9**
The Surface Roughness Experiment from Problem 6.39

Run	*A*	*B*	*C*	*D*	Surface Roughness
1	−	−	−	−	0.00340
2	+	−	−	−	0.00362
3	−	+	−	−	0.00301
4	+	+	−	−	0.00182
5	−	−	+	−	0.00280
6	+	−	+	−	0.00290
7	−	+	+	−	0.00252
8	+	+	+	−	0.00160
9	−	−	−	+	0.00336
10	+	−	−	+	0.00344
11	−	+	−	+	0.00308
12	+	+	−	+	0.00184
13	−	−	+	+	0.00269
14	+	−	+	+	0.00284
15	−	+	+	+	0.00253
16	+	+	+	+	0.00163

6.40 Resistivity on a silicon wafer is influenced by several factors. The results of a 2^4 factorial experiment performed during a critical processing step are shown in Table P6.10.

■ **TABLE P6.10**
The Resistivity Experiment from Problem 6.40

Run	*A*	*B*	*C*	*D*	Resistivity
1	−	−	−	−	1.92
2	+	−	−	−	11.28
3	−	+	−	−	1.09
4	+	+	−	−	5.75
5	−	−	+	−	2.13
6	+	−	+	−	9.53
7	−	+	+	−	1.03
8	+	+	+	−	5.35
9	−	−	−	+	1.60
10	+	−	−	+	11.73
11	−	+	−	+	1.16
12	+	+	−	+	4.68
13	−	−	+	+	2.16
14	+	−	+	+	9.11
15	−	+	+	+	1.07
16	+	+	+	+	5.30

(a) Estimate the factor effects. Plot the effect estimates on a normal probability plot and select a tentative model.

(b) Fit the model identified in part (a) and analyze the residuals. Is there any indication of model inadequacy?

(c) Repeat the analysis from parts (a) and (b) using $\ln(y)$ as the response variable. Is there an indication that the transformation has been useful?

(d) Fit a model in terms of the coded variables that can be used to predict the resistivity.

6.41 ***Continuation of Problem 6.40***. Suppose that the experimenter had also run four center points along with the 16 runs in Problem 6.40. The resistivity measurements at the center points are 8.15, 7.63, 8.95, and 6.48. Analyze the experiment again incorporating the center points. What conclusions can you draw now?

6.42 The book by Davies (*Design and Analysis of Industrial Experiments*) describes an experiment to study the yield of isatin. The factors studied and their levels are as follows:

Factor	Low (−)	High (+)
A: Acid strength (%)	87	93
B: Reaction time (min)	15	30
C: Amount of acid (mL)	35	45
D: Reaction temperature (°C)	60	70

The data from the 2^4 factorial are shown in Table P6.11.

(a) Fit a main-effects-only model to the data from this experiment. Are any of the main effects significant?

(b) Analyze the residuals. Are there any indications of model inadequacy or violation of the assumptions?

(c) Find an equation for predicting the yield of isatin over the design space. Express the equation in both coded and engineering units.

(d) Is there any indication that adding interactions to the model would improve the results that you have obtained?

■ **TABLE P6.11**

The 2^4 Factorial Experiment in Problem 6.42

A	B	C	D	Yield
−1	−1	−1	−1	6.08
1	−1	−1	−1	6.04
−1	1	−1	−1	6.53
1	1	−1	−1	6.43
−1	−1	1	−1	6.31
1	−1	1	−1	6.09
−1	1	1	−1	6.12
1	1	1	−1	6.36
−1	−1	−1	1	6.79
1	−1	−1	1	6.68
−1	1	−1	1	6.73
1	1	−1	1	6.08
−1	−1	1	1	6.77
1	−1	1	1	6.38
−1	1	1	1	6.49
1	1	1	1	6.23

6.43 An article in *Quality and Reliability Engineering International* (2010, Vol. 26, pp. 223–233) presents a 2^5 factorial design. The experiment is shown in Table P6.12.

(a) Analyze the data from this experiment. Identify the significant factors and interactions.

(b) Analyze the residuals from this experiment. Are there any indications of model inadequacy or violations of the assumptions?

(c) One of the factors from this experiment does not seem to be important. If you drop this factor, what type of design remains? Analyze the data using the full factorial model for only the four active factors. Compare your results with those obtained in part (a).

(d) Find settings of the active factors that maximize the predicted response.

■ **TABLE P6.12**

The 2^5 Design in Problem 6.43

A	B	C	D	E	y
−1.00	−1.00	−1.00	−1.00	−1.00	8.11
1.00	−1.00	−1.00	−1.00	−1.00	5.56
−1.00	1.00	−1.00	−1.00	−1.00	5.77
1.00	1.00	−1.00	−1.00	−1.00	5.82
−1.00	−1.00	1.00	−1.00	−1.00	9.17
1.00	−1.00	1.00	−1.00	−1.00	7.8
−1.00	1.00	1.00	−1.00	−1.00	3.23
1.00	1.00	1.00	−1.00	−1.00	5.69
−1.00	−1.00	−1.00	1.00	−1.00	8.82
1.00	−1.00	−1.00	1.00	−1.00	14.23
−1.00	1.00	−1.00	1.00	−1.00	9.2
1.00	1.00	−1.00	1.00	−1.00	8.94
−1.00	−1.00	1.00	1.00	−1.00	8.68
1.00	−1.00	1.00	1.00	−1.00	11.49
−1.00	1.00	1.00	1.00	−1.00	6.25
1.00	1.00	1.00	1.00	−1.00	9.12
−1.00	−1.00	−1.00	−1.00	1.00	7.93
1.00	−1.00	−1.00	−1.00	1.00	5
−1.00	1.00	−1.00	−1.00	1.00	7.47
1.00	1.00	−1.00	−1.00	1.00	12
−1.00	−1.00	1.00	−1.00	1.00	9.86
1.00	−1.00	1.00	−1.00	1.00	3.65
−1.00	1.00	1.00	−1.00	1.00	6.4
1.00	1.00	1.00	−1.00	1.00	11.61
−1.00	−1.00	−1.00	1.00	1.00	12.43
1.00	−1.00	−1.00	1.00	1.00	17.55
−1.00	1.00	−1.00	1.00	1.00	8.87
1.00	1.00	−1.00	1.00	1.00	25.38
−1.00	−1.00	1.00	1.00	1.00	13.06
1.00	−1.00	1.00	1.00	1.00	18.85
−1.00	1.00	1.00	1.00	1.00	11.78
1.00	1.00	1.00	1.00	1.00	26.05

6.44 A paper in the *Journal of Chemical Technology and Biotechnology* ("Response Surface Optimization of the Critical Media Components for the Production of Surfactin," 1997, Vol. 68, pp. 263–270) describes the use of a designed experiment to maximize surfactin production. A portion of the data from this experiment is shown in Table P6.13. Surfactin was assayed by an indirect method, which involves measurement of surface tensions of the diluted broth samples. Relative surfactin concentrations were determined by serially diluting the broth until the critical micelle concentration (CMC) was reached. The dilution at which the surface tension starts rising abruptly was denoted by CMC^{-1} and was considered proportional to the amount of surfactant present in the original sample.

(a) Analyze the data from this experiment. Identify the significant factors and interactions.

(b) Analyze the residuals from this experiment. Are there any indications of model inadequacy or violations of the assumptions?

(c) What conditions would optimize the surfactin production?

■ **TABLE P6.13**
The Factorial Experiment in Problem 6.44

Run	Glucose ($g\ dm^{-3}$)	NH_4NO_3 ($g\ dm^{-3}$)	$FeSO_4$ ($g\ dm^{-3} \times 10^{-4}$)	$MnSO_4$ ($g\ dm^{-3} \times 10^{-2}$)	y $(CMC)^{-1}$
1	20.00	2.00	6.00	4.00	23
2	60.00	2.00	6.00	4.00	15
3	20.00	6.00	6.00	4.00	16
4	60.00	6.00	6.00	4.00	18
5	20.00	2.00	30.00	4.00	25
6	60.00	2.00	30.00	4.00	16
7	20.00	6.00	30.00	4.00	17
8	60.00	6.00	30.00	4.00	26
9	20.00	2.00	6.00	20.00	28
10	60.00	2.00	6.00	20.00	16
11	20.00	6.00	6.00	20.00	18
12	60.00	6.00	6.00	20.00	21
13	20.00	2.00	30.00	20.00	36
14	60.00	2.00	30.00	20.00	24
15	20.00	6.00	30.00	20.00	33
16	60.00	6.00	30.00	20.00	34

6.45 ***Continuation of Problem 6.44***. The experiment in Problem 6.44 actually included six center points. The responses at these conditions were 35, 35, 35, 36, 36, and 34. Is there any indication of curvature in the response function? Are additional experiments necessary? What would you recommend doing now?

6.46 An article in the *Journal of Hazardous Materials* ("Feasibility of Using Natural Fishbone Apatite as a Substitute for Hydroxyapatite in Remediating Aqueous Heavy Metals," Vol. 69, Issue 2, 1999, pp. 187–196) describes an experiment to study the suitability of fishbone, a natural, apatite, rich substance, as a substitute for hydroxyapatite in the sequestering of aqueous divalent heavy metal ions. Direct comparison of hydroxyapatite and fishbone apatite was performed using a three-factor two-level full factorial design. Apatite (30 or 60 mg) was added to 100 mL deionized water and gently agitated overnight in a shaker. The pH was then adjusted to 5 or 7 using nitric acid. Sufficient concentration of lead nitrate solution was added to each flask to result in a final volume of 200 mL and a lead concentration of 0.483 or 2.41 mM, respectively. The experiment was a 2^3 replicated twice and it was performed for both fishbone and synthetic apatite. Results are shown in Table P6.14.

(a) Analyze the lead response for fishbone apatite. What factors are important?

(b) Analyze the residuals from this response and comment on model adequacy.

(c) Analyze the pH response for fishbone apatite. What factors are important?

(d) Analyze the residuals from this response and comment on model adequacy.

(e) Analyze the lead response for hydroxyapatite apatite. What factors are important?

(f) Analyze the residuals from this response and comment on model adequacy.

(g) Analyze the pH response for hydroxyapatite apatite. What factors are important?

(h) Analyze the residuals from this response and comment on model adequacy.

(i) What differences do you see between fishbone and hydroxyapatite apatite? The authors of this paper concluded that that fishbone apatite was comparable to hydroxyapatite apatite. Because the fishbone apatite is cheaper, it was recommended for adoption. Do you agree with these conclusions?

■ **TABLE P6.14**
The Experiment for Problem 6.46. For apatite, + is 60 mg and − is 30 mg per 200 mL metal solution. For initial pH, + is 7 and − is 4. For Pb + is 2.41 mM (500 ppm) and − is 0.483 mM (100 ppm)

			Fishbone		Hydroxyapatite	
Apatite	pH	Pb	Pb, mM	pH	Pb, mM	pH
+	+	+	1.82	5.22	0.11	3.49
+	+	+	1.81	5.12	0.12	3.46
+	+	−	0.01	6.84	0.00	5.84
+	+	−	0.00	6.61	0.00	5.90
+	−	+	1.11	3.35	0.80	2.70
+	−	+	1.04	3.34	0.76	2.74
+	−	−	0.00	5.77	0.03	3.36
+	−	−	0.01	6.25	0.05	3.24
−	+	+	2.11	5.29	1.03	3.22
−	+	+	2.18	5.06	1.05	3.22
−	+	−	0.03	5.93	0.00	5.53
−	+	−	0.05	6.02	0.00	5.43
−	−	+	1.70	3.39	1.34	2.82
−	−	+	1.69	3.34	1.26	2.79
−	−	−	0.05	4.50	0.06	3.28
−	−	−	0.05	4.74	0.07	3.28

6.47 Often the fitted regression model from a 2^k factorial design is used to make predictions at points of interest in the design space. Assume that the model contains all main effects and two-factor interactions.

(a) Find the variance of the predicted response $\hat{y}$ at a point $x_1, x_2, \ldots, x_k$ in the design space. *Hint:* Remember that the x's are coded variables and assume a 2^k design with an equal number of replicates n at each design point so that the variance of a regression coefficient $\hat{\beta}$ is $\sigma^2/(n2^k)$ and that the covariance between any pair of regression coefficients is zero.

(b) Use the result in part (a) to find an equation for a $100(1-\alpha)$ percent confidence interval on the true mean response at the point $x_1, x_2, \ldots, x_k$ in design space.

6.48 ***Hierarchical models***. Several times we have used the hierarchy principle in selecting a model; that is, we have included nonsignificant lower order terms in a model because they were factors involved in significant higher order terms. Hierarchy is certainly not an absolute principle that must be followed in all cases. To illustrate, consider the model resulting from Problem 6.5, which required that a nonsignificant main effect be included to achieve hierarchy. Using the data from Problem 6.5.

(a) Fit both the hierarchical and the nonhierarchical models.

(b) Calculate the PRESS statistic, the adjusted R^2, and the mean square error for both models.

(c) Find a 95 percent confidence interval on the estimate of the mean response at a cube corner ($x_1 = x_2 = x_3 = \pm 1$). *Hint:* Use the results of Problem 6.40.

(d) Based on the analyses you have conducted, which model do you prefer?

6.49 Suppose that you want to run a 2^3 factorial design. The variance of an individual observation is expected to be about 4. Suppose that you want the length of a 95 percent confidence interval on any effect to be less than or equal to 1.5. How many replicates of the design do you need to run?

6.50 Suppose that a full 2^4 factorial uses the following factor levels:

Factor	Low (−)	High (+)
A: Acid strength (%)	85	95
B: Reaction time (min)	15	35
C: Amount of acid (mL)	35	45
D: Reaction temperature (°C)	60	80

The fitted model from this experiment is $\hat{y} = 24 + 16x_1 - 34x_2 + 12x_3 + 6x_4 - 10x_1x_2 + 16x_1x_3$. Predict the response at the following points:

(a) $A = 89, B = 20, C = 38, D = 66$

(b) $A = 90, B = 16, C = 40, D = 70$

(c) $A = 87, B = 28, C = 42, D = 61$

(d) $A = 90, B = 27, C = 37, D = 69$

6.51 An article in *Quality and Reliability Engineering International* (2010, Vol. 26, pp. 223–233) presents a 2^5 factorial design. The experiment is shown in Table P6.15.

■ **TABLE P6.15**
The Experiment for Problem 6.51

A	B	C	D	E	y
−1	−1	−1	−1	−1	8.11
1	−1	−1	−1	−1	5.56
−1	1	−1	−1	−1	5.77
1	1	−1	−1	−1	5.82
−1	−1	1	−1	−1	9.17
1	−1	1	−1	−1	7.8
−1	1	1	−1	−1	3.23
1	1	1	−1	−1	5.69
−1	−1	−1	1	−1	8.82
1	−1	−1	1	−1	14.23
−1	1	−1	1	−1	9.2
1	1	−1	1	−1	8.94
−1	−1	1	1	−1	8.68
1	−1	1	1	−1	11.49
−1	1	1	1	−1	6.25
1	1	1	1	−1	9.12
−1	−1	−1	−1	1	7.93
1	−1	−1	−1	1	5
−1	1	−1	−1	1	7.47
1	1	−1	−1	1	12
−1	−1	1	−1	1	9.86
1	−1	1	−1	1	3.65
−1	1	1	−1	1	6.4
1	1	1	−1	1	11.61
−1	−1	−1	1	1	12.43
1	−1	−1	1	1	17.55
−1	1	−1	1	1	8.87
1	1	−1	1	1	25.38
−1	−1	1	1	1	13.06
1	−1	1	1	1	18.85
−1	1	1	1	1	11.78
1	1	1	1	1	26.05

(a) Analyze the data from this experiment. Identify the significant factors and interactions.

(b) Analyze the residuals from this experiment. Are there any indications of model inadequacy or violations of the assumptions?

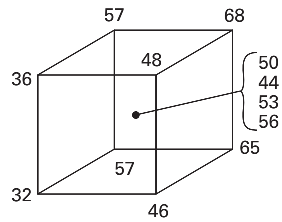
(c) One of the factors from this experiment does not seem to be important. If you drop this factor, what type of design remains? Analyze the data using the full factorial model for only the four active factors. Compare your results with those obtained in part (a).

(d) Find settings of the active factors that maximize the predicted response.

6.52 Consider the 2^3 shown below:

	Process Variables			Coded Variables			
Run	**Temp (°C)**	**Pressure (psig)**	**Conc (g/l)**	x_1	x_2	x_3	**Yield, y**
1	120	40	15	−1	−1	−1	32
2	160	40	15	1	−1	−1	46
3	120	80	15	−1	1	−1	57
4	160	80	15	1	1	−1	65
5	120	40	30	−1	−1	1	36
6	160	40	30	1	−1	1	48
7	120	80	30	−1	1	1	57
8	160	80	30	1	1	1	68
9	140	60	22.5	0	0	0	50
10	140	60	22.5	0	0	0	44
11	140	60	22.5	0	0	0	53
12	140	60	22.5	0	0	0	56

$$x_1 = \frac{\text{Temp} - 140}{20},\quad x_2 = \frac{\text{Pressure} - 60}{20},\quad x_3 = \frac{\text{Conc} - 22.5}{7.5}$$



When running a designed experiment, it is sometimes difficult to reach and hold the precise factor levels required by the design. Small discrepancies are not important, but large ones are potentially of more concern. To illustrate, the experiment presented in Table P6.16 shows a variation of the 2^3 design above, where many of the test combinations are not exactly the ones specified in the design. Most of the difficulty seems to have occurred with the temperature variable.

Fit a first-order model to both the original data and the data in Table P6.16. Compare the inference from the two models. What conclusions can you draw from this simple example?

6.53 In two-level design, the expected value of a nonsignificant factor effect is zero.

(a) True

(b) False

6.54 A half-normal plot of factor effects plots the expected normal percentile versus the effect estimate.

(a) True

(b) False

■ **TABLE P6.16**
Revised Experimental Data

	Process Variables			Coded Variables			
Run	**Temp (°C)**	**Pressure (psig)**	**Conc (g/l)**	x_1	x_2	x_3	**Yield, y**
1	125	41	14	−0.75	−0.95	−1.133	32
2	158	40	15	0.90	−1	−1	46
3	121	82	15	−0.95	1.1	−1	57
4	160	80	15	1	1	−1	65
5	118	39	33	−1.10	−1.05	1.14	36
6	163	40	30	1.15	−1	1	48
7	122	80	30	−0.90	1	1	57
8	165	83	30	1.25	1.15	1	68
9	140	60	22.5	0	0	0	50
10	140	60	22.5	0	0	0	44
11	140	60	22.5	0	0	0	53
12	140	60	22.5	0	0	0	56

6.55 In an unreplicated design, the degrees of freedom associated with the "pure error" component of error are zero.

(a) True

(b) False

6.56 In a replicated 2^3 design (16 runs), the estimate of the model intercept is equal to one-half of the total of all 16 runs.

(a) True

(b) False

6.57 Adding center runs to a 2^k design affects the estimate of the intercept term but not the estimates of any other factor effects.

(a) True

(b) False

6.58 The mean square for pure error in a replicated factorial design can get smaller if nonsignificant terms are added to a model.

(a) True

(b) False

6.59 A 2^k factorial design is a *D*-optimal design for fitting a first-order model.

(a) True

(b) False

6.60 If a *D*-optimal design algorithm is used to create a 12-run design for fitting a first-order model in three variables with all three two-factor interactions, the algorithm will construct a 2^3 factorial with four center runs.

(a) True

(b) False

6.61 Suppose that you want to replicate 2 of the 8 runs in a 2^3 factorial design. How many ways are there to choose the 2 runs to replicate? Suppose that you decide to replicate the run with all three factors at the high level and the run with all three factors at the low level.

(a) Is the resulting design orthogonal?

(b) What are the relative variances of the model coefficients if the main effects plus two-factor interaction model are fit to the data from this design?

(c) What is the power for detecting effects of two standard deviations in magnitude?

6.62 The display below summarizes the results of analyzing a 2^4 factorial design.

Term Intercept	**Effect Estimate**	**Sum of Squares**	**% Contribution**
A		6.25	3.25945
B	5.25	110.25	57.4967
C	3.5	49	25.5541
D	0.75		1.1734
AB	0.75	2.25	1.1734
AC	–0.5	1	0.521512
AD	0.75	2.25	1.1734
BC	1.5	9	
BD	0.25	0.25	0.130378
CD	0.5	1	0.521512
ABC	–1	4	2.08605
ABD		2.25	1.1734
ACD	–0.5		0.521512
BCD	0	0	0
ABCD	–0.5	1	

(a) Fill in the missing information in this table.

(b) Construct a normal probability plot of the effects. Which factors seem to be active?

CHAPTER 7

Blocking and Confounding in the 2^k Factorial Design

CHAPTER OUTLINE

The supplemental material is on the textbook website www.wiley.com/college/montgomery.

CHAPTER LEARNING OBJECTIVES

1. Learn about how the blocking technique can be used with 2^k factorial designs.
2. Learn about how blocking can be used with unreplicated 2^k factorial designs, and how this leads to confounding of effects.
3. Know how to construct the 2^k factorial designs in 2^p blocks.
4. Understand how to construct designs that confound different effects in different replicates.

7.1 Introduction

In many situations it is impossible to perform all of the runs in a 2^k factorial experiment under homogeneous conditions. For example, a single batch of raw material might not be large enough to make all of the required runs. In other cases, it might be desirable to deliberately vary the experimental conditions to ensure that the treatments are equally effective (i.e., robust) across many situations that are likely to be encountered in practice. For example, a chemical engineer may run a pilot plant experiment with several batches of raw material because he knows that different raw material batches of different quality grades are likely to be used in the actual full-scale process.

The design technique used in these situations is **blocking**. Chapter 4 was an introduction to the blocking principle, and you may find it helpful to read the introductory material in that chapter again. We also discussed blocking general factorial experiments in Chapter 5. In this chapter, we will build on the concepts introduced in Chapter 4, focusing on some special techniques for blocking in the 2^k factorial design.

7.2 Blocking a Replicated 2^k Factorial Design

Suppose that the 2^k factorial design has been replicated n times. This is identical to the situation discussed in Chapter 5, where we showed how to run a general factorial design in blocks. If there are n replicates, then each set of nonhomogeneous conditions defines a block, and each replicate is run in one of the blocks. The runs in each block (or replicate) would be made in random order. The analysis of the design is similar to that of any blocked factorial experiment; for example, see the discussion in Section 5.6.

EXAMPLE 7.1

Consider the chemical process experiment first described in Section 6.2. Suppose that only four experimental trials can be made from a single batch of raw material. Therefore, three batches of raw material will be required to run all three replicates of this design. Table 7.1 shows the design, where each batch of raw material corresponds to a block.

The ANOVA for this blocked design is shown in Table 7.2. All of the sums of squares are calculated exactly as in a standard, unblocked 2^k design. The sum of squares for blocks is calculated from the block totals. Let B_1, B_2, and B_3 represent the block totals (see Table 7.1). Then

$$
\begin{aligned}
SS_{\text{Blocks}} &= \sum_{i=1}^{3} \frac{B_i^2}{4} - \frac{y_{\ldots}^2}{12} \\
&= \frac{(113)^2 + (106)^2 + (111)^2}{4} - \frac{(330)^2}{12} \\
&= 6.50
\end{aligned}
$$

There are two degrees of freedom among the three blocks. Table 7.2 indicates that the conclusions from this analysis, had the design been run in blocks, are identical to those in Section 6.2 and that the block effect is relatively small. The F-Statistic for blocks is $F_0 = (6.50/2)/4.14 = 0.79$, which is not significant.

■ **TABLE 7.1**
Chemical Process Experiment in Three Blocks

	Block 1	Block 2	Block 3
	$(1) = 28$	$(1) = 25$	$(1) = 27$
	$a = 36$	$a = 32$	$a = 32$
	$b = 18$	$b = 19$	$b = 23$
	$ab = 31$	$ab = 30$	$ab = 29$
Block totals:	$B_1 = 113$	$B_2 = 106$	$B_3 = 111$

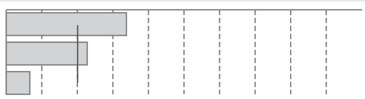
■ **TABLE 7.2**
Analysis of Variance for the Chemical Process Experiment in Three Blocks

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	*P*-Value
Blocks	6.50	2	3.25		
A (concentration)	208.33	1	208.33	50.32	0.0004
B (catalyst)	75.00	1	75.00	18.12	0.0053
AB	8.33	1	8.33	2.01	0.2060
Error	24.84	6	4.14		
Total	323.00	11			

The analysis shown in Example 7.1 assumes that blocks are a fixed effect. It is probably more realistic to think of the batches of raw material used in the experiment as random. The display below shows the analysis from JMP employing the REML method to treat blocks as a random effect. The estimate of the block variance component is actually very small and negative. This is consistent with the conclusions from the previous analysis where the block effect wasn't significant. The JMP output reports the log worth statistic in addition to the usual *P*-value. Log worth is calculated as log worth – $\log_{10}$(*P*-value). Values of log worth that are 2 or greater are usually taken as an indication that the factor is significant.

Response Y
Effect Summary

Source	LogWorth	P-Value
Concentration	3.405	0.00039
Catalyst	2.272	0.00534
Concentration*Catalyst	0.687	0.20571



Summary of Fit

RSquare	0.89048
RSquare Adj	0.849409
Root Mean Square Error	2.034426
Mean of Response	27.5
Observations (or Sum Wgts)	12

Parameter Estimates

Term	Estimate	Std Error	DFDen	t Ratio	Prob > |t|
Intercept	27.5	0.520416	2	52.84	0.0004*
Concentration	4.1666667	0.587288	6	7.09	0.0004*
Catalyst	2.5	0.587288	6	4.26	0.0053*
Concentration*Catalyst	0.8333333	0.587288	6	1.42	0.2057

REML Variance Component Estimates

Random Effect	Var Ratio	Var Component	Std Error	95% Lower	95% Upper	Pct of Total
Blocks	−0.053691	−0.222222	1.0084838	−2.198814	1.7543697	0.000
Residual		4.1388889	2.3895886	1.7186441	20.069866	100.000
Total		4.1388889	2.3895886	1.7186441	20.069866	100.000

−2 LogLikelihood = 43.522517328
Note: Total is the sum of the positive variance components.
Total including negative estimates = 3.9166667

Fixed Effect Tests

Source	Nparm	DF	DFDen	F Ratio	Prob > F
Concentration	1	1	6	50.3356	0.0004*
Catalyst	1	1	6	18.1208	0.0053*
Concentration*Catalyst	1	1	6	2.0134	0.2057

7.3 Confounding in the 2^k Factorial Design

In many problems it is impossible to perform a complete replicate of a factorial design in one block. **Confounding** is a design technique for arranging a complete factorial experiment in blocks, where the block size is smaller than the number of treatment combinations in one replicate. The technique causes information about certain treatment effects (usually high-order interactions) to be **indistinguishable from**, or **confounded with**, **blocks**. In this chapter we concentrate on confounding systems for the 2^k factorial design. Note that even though the designs presented are **incomplete block designs** because each block does not contain all the treatments or treatment combinations, the special structure of the 2^k factorial system allows a simplified method of analysis.

We consider the construction and analysis of the 2^k factorial design in 2^p incomplete blocks, where $p < k$. Consequently, these designs can be run in two blocks ($p = 1$), four blocks ($p = 2$), eight blocks ($p = 3$), and so on.

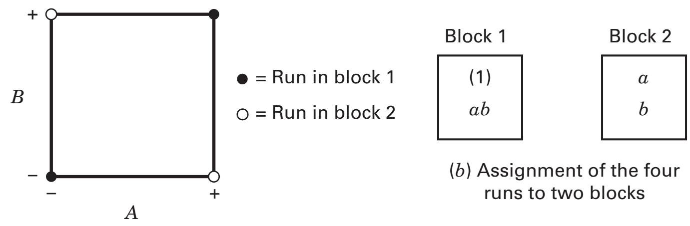
7.4 Confounding the 2^k Factorial Design in Two Blocks

Suppose that we wish to run a single replicate of the 2^2 design. Each of the $2^2 = 4$ treatment combinations requires a quantity of raw material, for example, and each batch of raw material is only large enough for two treatment combinations to be tested. Thus, two batches of raw material are required. If batches of raw material are considered as blocks, then we must assign two of the four treatment combinations to each block.

Figure 7.1 shows one possible design for this problem. The geometric view, Figure 7.1*a*, indicates that treatment combinations on opposing diagonals are assigned to different blocks. Notice from Figure 7.1*b* that block 1 contains the treatment combinations (1) and *ab* and that block 2 contains *a* and *b*. Of course, the *order* in which the treatment combinations are run within a block is randomly determined. We would also randomly decide which block to run first. Suppose we estimate the main effects of *A* and *B* just as if no blocking had occurred. From Equations 6.1 and 6.2, we obtain

$$A = \frac{1}{2}[ab + a - b - (1)]$$
$$B = \frac{1}{2}[ab + b - a - (1)]$$

■ **FIGURE 7.1** **A 2^2 design in two blocks**



(*a*) Geometric view

(*b*) Assignment of the four runs to two blocks

Note that both A and B are unaffected by blocking because in each estimate there is one plus and one minus treatment combination from each block. That is, any difference between block 1 and block 2 will cancel out.

Now consider the AB interaction

$$AB = \tfrac{1}{2}[ab + (1) - a - b]$$

Because the two treatment combinations with the plus sign [ab and (1)] are in block 1 and the two with the minus sign (a and b) are in block 2, the block effect and the AB interaction are identical. That is, AB is **confounded** with blocks.

The reason for this is apparent from the table of plus and minus signs for the 2^2 design. This was originally given as Table 6.2, but for convenience it is reproduced as Table 7.3 here. From this table, we see that all treatment combinations that have a plus sign on AB are assigned to block 1, whereas all treatment combinations that have a minus sign on AB are assigned to block 2. This approach can be used to confound any effect (A, B, or AB) with blocks. For example, if (1) and b had been assigned to block 1 and a and ab to block 2, the main effect A would have been confounded with blocks. The usual practice is to confound the highest order interaction with blocks.

This scheme can be used to confound any 2^k design in two blocks. As a second example, consider a 2^3 design run in two blocks. Suppose we wish to confound the three-factor interaction ABC with blocks. From the table of plus and minus signs shown in Table 7.4, we assign the treatment combinations that are minus on ABC to block 1 and those that are plus on ABC to block 2. The resulting design is shown in Figure 7.2. Once again, we emphasize that the treatment combinations *within* a block are run in random order.

Other Methods for Constructing the Blocks. There is another method for constructing these designs. The method uses the linear combination

$$L = \alpha_1 x_1 + \alpha_2 x_2 + \cdots + a_k x_k \tag{7.1}$$

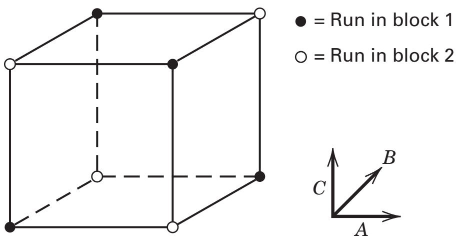
where x_i is the level of the ith factor appearing in a particular treatment combination and α_i is the exponent appearing on the ith factor in the effect to be confounded. For the 2^k system, we have $\alpha_i = 0$ or 1 and $x_i = 0$ (low level) or $x_i = 1$ (high level). Equation 7.1 is called a **defining contrast**. Treatment combinations that produce the same value of L (mod 2) will be placed in the same block. Because the only possible values of L (mod 2) are 0 and 1, this will assign the 2^k treatment combinations to exactly two blocks.

■ **TABLE 7.3**
Table of Plus and Minus Signs for the 2^2 Design

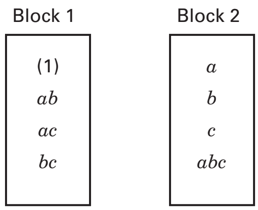
Treatment Combination	Factorial Effect				
	I	***A***	***B***	***AB***	**Block**
(1)	+	−	−	+	1
a	+	+	−	−	2
b	+	−	+	−	2
ab	+	+	+	+	1

■ **TABLE 7.4**
Table of Plus and Minus Signs for the 2^3 Design

Treatment Combination	Factorial Effect								Block
	I	A	B	AB	C	AC	BC	ABC	
(1)	+	−	−	+	−	+	+	−	1
a	+	+	−	−	−	−	+	+	2
b	+	−	+	−	−	+	−	+	2
ab	+	+	+	+	−	−	−	−	1
c	+	−	−	+	+	−	−	+	2
ac	+	+	−	−	+	+	−	−	1
bc	+	−	+	−	+	−	+	−	1
abc	+	+	+	+	+	+	+	+	2



(a) Geometric view



(b) Assignment of the eight runs to two blocks

■ **FIGURE 7.2** **The 2^3 design in two blocks with ABC confounded**

To illustrate the approach, consider a 2^3 design with ABC confounded with blocks. Here x_1 corresponds to A, x_2 to B, x_3 to C, and $\alpha_1 = \alpha_2 = \alpha_3 = 1$. Thus, the defining contrast corresponding to ABC is

$$L = x_1 + x_2 + x_3$$

The treatment combination (1) is written 000 in the (0, 1) notation; therefore,

$$L = 1(0) + 1(0) + 1(0) = 0 = 0 \text{ (mod 2)}$$

Similarly, the treatment combination a is 100, yielding

$$L = 1(1) + 1(0) + 1(0) = 1 = 1 \text{ (mod 2)}$$

Thus, (1) and a would be run in different blocks. For the remaining treatment combinations, we have

$$\begin{aligned}
b&: L = 1(0) + 1(1) + 1(0) = 1 = 1 \text{ (mod 2)}\\
ab&: L = 1(1) + 1(1) + 1(0) = 2 = 0 \text{ (mod 2)}\\
c&: L = 1(0) + 1(0) + 1(1) = 1 = 1 \text{ (mod 2)}\\
ac&: L = 1(1) + 1(0) + 1(1) = 2 = 0 \text{ (mod 2)}\\
bc&: L = 1(0) + 1(1) + 1(1) = 2 = 0 \text{ (mod 2)}\\
abc&: L = 1(1) + 1(1) + 1(1) = 3 = 1 \text{ (mod 2)}
\end{aligned}$$

Thus, (1), *ab*, *ac*, and *bc* are run in block 1 and *a*, *b*, *c*, and *abc* are run in block 2. This is the same design shown in Figure 7.2, which was generated from the table of plus and minus signs.

Another method may be used to construct these designs. The block containing the treatment combination (1) is called the **principal block**. The treatment combinations in this block have a useful group-theoretic property; namely, they form a group with respect to multiplication modulus 2. This implies that any element [except (1)] in the principal block may be generated by multiplying two other elements in the principal block modulus 2. For example, consider the principal block of the 2^3 design with *ABC* confounded, as shown in Figure 7.2.

Note that

$$ab \cdot ac = a^2bc = bc$$
$$ab \cdot bc = ab^2c = ac$$
$$ac \cdot bc = abc^2 = ab$$

Treatment combinations in the other block (or blocks) may be generated by multiplying one element in the new block by each element in the principal block modulus 2. For the 2^3 with *ABC* confounded, because the principal block is (1), *ab*, *ac*, and *bc*, we know that *b* is in the other block. Thus, the elements of this second block are

$$\begin{aligned} b \cdot (1) \quad\quad &= b \\ b \cdot ab = ab^2 &= a \\ b \cdot ac \quad\quad &= abc \\ b \cdot bc = b^2c &= c \end{aligned}$$

This agrees with the results obtained previously.

Estimation of Error. When the number of variables is small, say $k = 2$ or 3, it is usually necessary to replicate the experiment to obtain an estimate of error. For example, suppose that a 2^3 factorial must be run in two blocks with *ABC* confounded, and the experimenter decides to replicate the design four times. The resulting design might appear as in Figure 7.3. Note that *ABC* is confounded in each replicate.

The analysis of variance for this design is shown in Table 7.5. There are 32 observations and 31 total degrees of freedom. Furthermore, because there are eight blocks, seven degrees of freedom must be associated with these blocks. One breakdown of those seven degrees of freedom is shown in Table 7.5. The error sum of squares actually consists of the interactions between replicates and each of the effects (A, B, C, AB, AC, BC). It is usually safe to consider these interactions to be zero and to treat the resulting mean square as an estimate of error. Main effects and two-factor interactions are tested against the mean square error. Cochran and Cox (1957) observe that the block or *ABC* mean square could be compared to the error for the *ABC* mean square, which is really replicates × blocks. This test is usually very insensitive.

If resources are sufficient to allow the replication of confounded designs, it is generally better to use a slightly different method of designing the blocks in each replicate. This approach consists of confounding a different effect in each replicate so that some information on all effects is obtained. Such a procedure is called **partial confounding** and is discussed in Section 7.8.

■ FIGURE 7.3 Four replicates of the 2^3 design with *ABC* confounded

Replicate I		Replicate II		Replicate III		Replicate IV	
Block 1	Block 2	Block 1	Block 2	Block 1	Block 2	Block 1	Block 2
(1)	*abc*	(1)	*abc*	(1)	*abc*	(1)	*abc*
ac	*a*	*ac*	*a*	*ac*	*a*	*ac*	*a*
ab	*b*	*ab*	*b*	*ab*	*b*	*ab*	*b*
bc	*c*	*bc*	*c*	*bc*	*c*	*bc*	*c*

■ **TABLE 7.5**
Analysis of Variance for Four Replicates of a 2^3 Design with ABC Confounded

Source of Variation	Degrees of Freedom
Replicates	3
Blocks (ABC)	1
Error for ABC (replicates $\times$ blocks)	3
A	1
B	1
C	1
AB	1
AC	1
BC	1
Error (or replicates $\times$ effects)	18
Total	31

If k is moderately large, say $k \geq 4$, we can frequently afford only a single replicate. The experimenter usually assumes higher order interactions to be negligible and combines their sums of squares as error. The normal probability plot of factor effects can be very helpful in this regard.

EXAMPLE 7.2

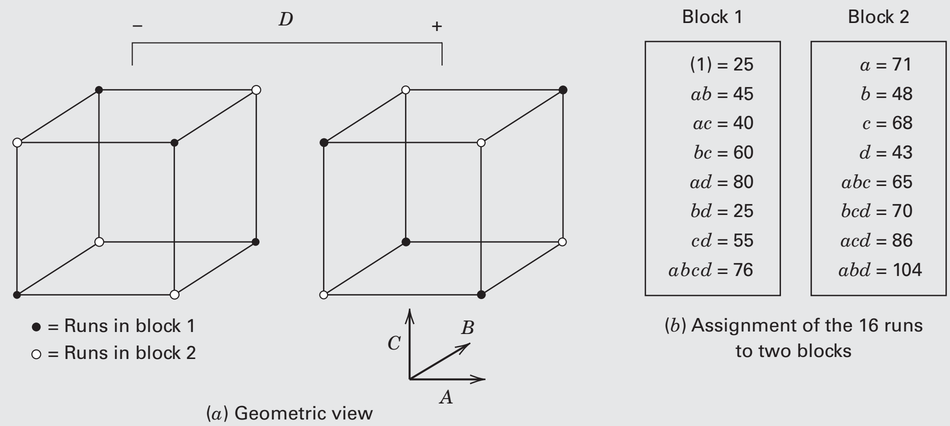
Consider the situation described in Example 6.2. Recall that four factors—temperature (A), pressure (B), concentration of formaldehyde (C), and stirring rate (D)—are studied in a pilot plant to determine their effect on product filtration rate. We will use this experiment to illustrate the ideas of blocking and confounding in an unreplicated design. We will make two modifications to the original experiment. First, suppose that the $2^4 = 16$ treatment combinations cannot all be run using one batch of raw material. The experimenter can run eight treatment combinations from a single batch of material, so a 2^4 design confounded in two blocks seems appropriate. It is logical to confound the highest order interaction $ABCD$ with blocks. The defining contrast is

$$L = x_1 + x_2 + x_3 + x_4$$

and it is easy to verify that the design is as shown in Figure 7.4. Alternatively, one may examine Table 6.11 and observe that the treatment combinations that are + in the $ABCD$ column are assigned to block 1 and those that are $-$ in $ABCD$ column are in block 2.

The second modification that we will make is to introduce a **block effect** so that the utility of blocking can be demonstrated. Suppose that when we select the two batches of raw material required to run the experiment, one of them is of much poorer quality and, as a result, all responses will be 20 units lower in this material batch than in the other. The poor quality batch becomes block 1 and the good quality batch becomes block 2 (it doesn't matter which batch is called block 1 or which batch is called block 2). Now all the tests in block 1 are performed first (the eight runs in the block are, of course, performed in random order), but the responses are 20 units lower than they would have been if good quality material had been used. Figure 7.4*b* shows the resulting responses—note that these have been found by subtracting the block effect from the original observations given in Example 6.2. That is, the original response for treatment combination (1) was 45, and in Figure 7.4*b* it is reported as (1) = 25 (= 45 − 20). The other responses in this block are obtained similarly. After the tests in block 1 are performed, the eight tests in block 2 follow. There is no problem with the raw material in this batch, so the responses are exactly as they were originally in Example 6.2.

The effect estimates for this "modified" version of Example 6.2 are shown in Table 7.6. Note that the estimates of the four main effects, the six two-factor interactions, and the four three-factor interactions are identical to the effect estimates obtained in Example 6.2 where there



■ **FIGURE 7.4** **The 2^4 design in two blocks for Example 7.2**

■ **TABLE 7.6**
Effect Estimates for the Blocked 2^4 Design in Example 7.2

Model Term	Regression Coefficient	Effect Estimate	Sum of Squares	Percent Contribution
A	10.81	21.625	1870.5625	26.30
B	1.56	3.125	39.0625	0.55
C	4.94	9.875	390.0625	5.49
D	7.31	14.625	855.5625	12.03
AB	0.062	0.125	0.0625	<0.01
AC	−9.06	−18.125	1314.0625	18.48
AD	8.31	16.625	1105.5625	15.55
BC	1.19	2.375	22.5625	0.32
BD	−0.19	−0.375	0.5625	<0.01
CD	−0.56	−1.125	5.0625	0.07
ABC	0.94	1.875	14.0625	0.20
ABD	2.06	4.125	68.0625	0.96
ACD	−0.81	−1.625	10.5625	0.15
BCD	−1.31	−2.625	27.5625	0.39
Block ($ABCD$)		−18.625	1387.5625	19.51

was *no block effect*. When a normal probability of these effect estimates is constructed, factors A, C, D, and the AC and AD interactions emerge as the important effects, just as in the original experiment. (The reader should verify this.)

What about the $ABCD$ interaction effect? The estimate of this effect in the original experiment (Example 6.2) was $ABCD = 1.375$. In this example, the estimate of the $ABCD$ interaction effect is $ABCD = -18.625$. Because $ABCD$ is confounded with blocks, the $ABCD$ interaction estimates the *original interaction effect* (1.375) plus the *block effect* (−20), so $ABCD = 1.375 + (-20) = -18.625$. (Do you see why the block effect is −20?) The block effect may also be calculated directly as the difference in average response between the two blocks, or

$$\begin{aligned}\text{Block effect} &= \bar{y}_{\text{Block 1}} - \bar{y}_{\text{Block 2}}\\ &= \frac{406}{8} - \frac{555}{8}\\ &= \frac{-149}{8}\\ &= -18.625\end{aligned}$$

Of course, this effect really estimates Blocks + $ABCD$.

Table 7.7 summarizes the ANOVA for this experiment. The effects with large estimates are included in the model, and the block sum of squares is

$$SS_{\text{Blocks}} = \frac{(406)^2 + (555)^2}{8} - \frac{(961)^2}{16} = 1387.5625$$

The conclusions from this experiment exactly match those from Example 6.2, where no block effect was present. Notice that if the experiment had not been run in blocks, and if an effect of magnitude −20 had affected the first 8 trials (which would have been selected in a random fashion, because the 16 trials would be run in random order in an unblocked design), the results could have been very different.

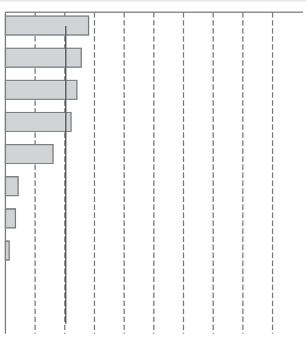
■ **TABLE 7.7**
Analysis of Variance for Example 7.2

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	*P*-Value
Blocks (*ABCD*)	1387.5625	1			
A	1870.5625	1	1870.5625	89.76	< 0.0001
C	390.0625	1	390.0625	18.72	0.0019
D	855.5625	1	855.5625	41.05	0.0001
AC	1314.0625	1	1314.0625	63.05	< 0.0001
AD	1105.5625	1	1105.5625	53.05	< 0.0001
Error	187.5625	9	20.8403		
Total	7110.9375	15			

The display below shows the output from JMP assuming that blocks are random and using REML for the analysis. The analysis only considers the main effects and the two-factor interactions, but it essentially agrees with the one presented in Example 7.2, identifying factors X1, X3, X4 and the two interactions X1X3 and X1X4 as significant. The confidence interval on the variance component for blocks is extremely wide and includes zero. This is probably an artifact of having only two blocks and only one degree of freedom to estimate the variance component associated with blocks.

Response Y
Effect Summary

Source	LogWorth	P-Value
X1	2.855	0.00140
X1*X3	2.567	0.00271
X1*X4	2.428	0.00373
X4	2.226	0.00595
X3	1.644	0.02272
X2	0.498	0.31795
X2*X3	0.361	0.43518
X3*X4	0.153	0.70257
X2*X4	0.047	0.89781
X1*X2	0.015	0.96582



Summary of Fit

RSquare	0.982998
RSquare Adj	0.948994
Root Mean Square Error	5.482928
Mean of Response	60.0625
Observations (or Sum Wgts)	16

Parameter Estimates

Term	Estimate	Std Error	DFDen	t Ratio	Prob > |t|
Intercept	60.0625	9.3125	1	6.45	0.0979
X1	10.8125	1.370732	4	7.89	0.0014*
X2	1.5625	1.370732	4	1.14	0.3180
X3	4.9375	1.370732	4	3.60	0.0227*
X4	7.3125	1.370732	4	5.33	0.0059*
X1*X2	0.0625	1.370732	4	0.05	0.9658
X1*X3	9.0625	1.370732	4	6.61	0.0027*
X1*X4	8.3125	1.370732	4	6.06	0.0037*
X2*X3	1.1875	1.370732	4	0.87	0.4352
X2*X4	0.1875	1.370732	4	0.14	0.8978
X3*X4	0.5625	1.370732	4	0.41	0.7026

REML Variance Component Estimates

Random Effect	Var Ratio	Var Component	Std Error	95% Lower	95% Upper	Pct of Total
Block	5.6444906	169.6875	245.30311	311.0978	650.47275	84.950
Residual		30.0625	21.257398	10.791251	248.23574	15.050
Total		199.75	245.99293	45.07048	41373.205	100.000

2 Log Likelihood = 65.536279358
Note: Total is the sum of the positive variance components.
Total including negative estimates = 199.75

Fixed Effect Tests

Source	Nparm	DF	DFDen	*F* Ratio	Prob > *F*
X1	1	1	4	62.2225	0.0014*
X2	1	1	4	1.2994	0.3180
X3	1	1	4	12.9751	0.0227*
X4	1	1	4	28.4595	0.0059*
X1*X2	1	1	4	0.0021	0.9658
X1*X3	1	1	4	43.7110	0.0027*
X1*X4	1	1	4	36.7755	0.0037*
X2*X3	1	1	4	0.7505	0.4352
X2*X4	1	1	4	0.0187	0.8978
X3*X4	1	1	4	0.1684	0.7026

7.5 Another Illustration of Why Blocking Is Important

Blocking is a very useful and important design technique. In Chapter 4 we pointed out that blocking has such dramatic potential to reduce the noise in an experiment that an experimenter should always consider the potential impact of nuisance factors, and when in doubt, block.

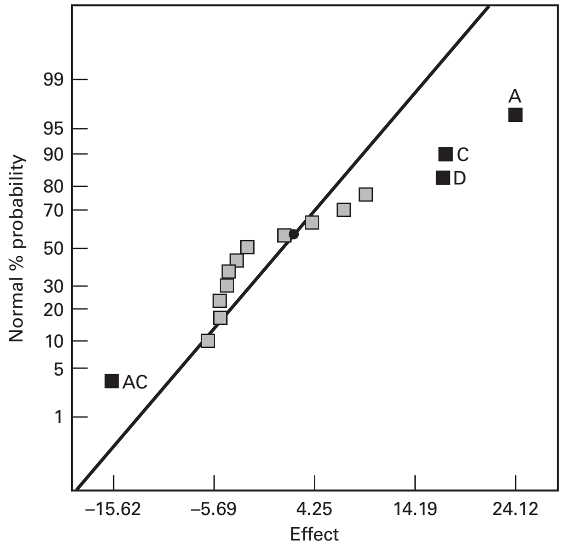
To illustrate what can happen if an experimenter doesn't block when he or she should have, consider a variation of Example 7.2 from the previous section. In this example we utilized a 2^4 unreplicated factorial experiment originally presented as Example 6.2. We constructed the design in two blocks of eight runs each, and we inserted a "block effect" or nuisance factor effect of magnitude -20 that affects all of the observations in block 1 (refer to Figure 7.4). Now suppose that we had not run this design in blocks and that the -20 nuisance factor effect impacted the first eight observations that were taken (in random or run order). The modified data are shown in Table 7.8.

Figure 7.5 is a normal probability plot of the factor effects from this modified version of the experiment. Notice that although the appearance of this plot is not too dissimilar from the one given with the original analysis of the experiment in Chapter 6 (refer to Figure 6.11), one of the important interactions, *AD*, is not identified. Consequently, we will not discover this important effect that turns out to be one of the keys to solving the original problem. We remarked in Chapter 4 that blocking is a **noise reduction technique**. If we don't block, then the added variability from the nuisance variable effect ends up getting distributed across the other design factors.

Some of the nuisance variability also ends up in the error estimate. The residual mean square for the model based on the data in Table 7.8 is about 109, which is several times larger than the residual mean square based on the original data (see Table 6.13).

■ **TABLE 7.8**
The Modified Data from Example 7.2

Run Order	Std. Order	Factor *A*: Temperature	Factor *B*: Pressure	Factor *C*: Concentration	Factor *D*: Stirring Rate	Response Filtration Rate
8	1	−1	−1	−1	−1	25
11	2	1	−1	−1	−1	71
1	3	−1	1	−1	−1	28
3	4	1	1	−1	−1	45
9	5	−1	−1	1	−1	68
12	6	1	−1	1	−1	60
2	7	−1	1	1	−1	60
13	8	1	1	1	−1	65
7	9	−1	−1	−1	1	23
6	10	1	−1	−1	1	80
16	11	−1	1	−1	1	45
5	12	1	1	−1	1	84
14	13	−1	−1	1	1	75
15	14	1	−1	1	1	86
10	15	−1	1	1	1	70
4	16	1	1	1	1	76



■ **FIGURE 7.5** **Normal probability plot for the data in Table 7.8**

7.6 Confounding the 2^k Factorial Design in Four Blocks

It is possible to construct 2^k factorial designs confounded in four blocks of 2^{k-2} observations each. These designs are particularly useful in situations where the number of factors is moderately large, say $k \geq 4$, and block sizes are relatively small.

As an example, consider the 2^5 design. If each block will hold only eight runs, then four blocks must be used. The construction of this design is relatively straightforward. Select *two* effects to be confounded with blocks, say *ADE* and *BCE*. These effects have the two defining contrasts

$$L_1 = x_1 + x_4 + x_5$$
$$L_2 = x_2 + x_3 + x_5$$

associated with them. Now every treatment combination will yield a particular pair of values of L_1 (mod 2) and L_2 (mod 2), that is, either $(L_1, L_2) = (0, 0), (0, 1), (1, 0)$, or (1, 1). Treatment combinations yielding the same values of (L_1, L_2) are assigned to the same block. In our example we find

$$\begin{array}{lll} L_1 = 0, L_2 = 0 & \text{for} & (1), ad, bc, abcd, abe, ace, cde, bde \\ L_1 = 1, L_2 = 0 & \text{for} & a, d, abc, bcd, be, abde, ce, acde \\ L_1 = 0, L_2 = 1 & \text{for} & b, abd, c, acd, ae, de\ abce, bcde \\ L_1 = 1, L_2 = 1 & \text{for} & e, ade, bce, abcde, ab, bd, ac, cd \end{array}$$

These treatment combinations would be assigned to different blocks. The complete design is as shown in Figure 7.6.

With a little reflection we realize that another effect in addition to *ADE* and *BCE* must be confounded with blocks. Because there are four blocks with three degrees of freedom between them, and because *ADE* and *BCE* have only one degree of freedom each, clearly an additional effect with one degree of freedom must be confounded. This effect is the **generalized interaction** of *ADE* and *BCE*, which is defined as the product of *ADE* and *BCE* modulus 2. Thus, in our example the generalized interaction $(ADE)(BCE) = ABCDE^2 = ABCD$ is also confounded with blocks. It is easy to verify this by referring to a table of plus and minus signs for the 2^5 design, such as

Block 1 $L_1 = 0$ $L_2 = 0$	Block 2 $L_1 = 1$ $L_2 = 0$	Block 3 $L_1 = 0$ $L_2 = 1$	Block 4 $L_1 = 1$ $L_2 = 1$
(1) *abe*	*a* *be*	*b* *abce*	*e* *abcde*
ad *ace*	*d* *abde*	*abd* *ae*	*ade* *bd*
bc *cde*	*abc* *ce*	*c* *bcde*	*bce* *ac*
abcd *bde*	*bcd* *acde*	*acd* *de*	*ab* *cd*

■ **FIGURE 7.6 The 2^5 design in four blocks with *ADE*, *BCE*, and *ABCD* confounded**

in Davies (1956). Inspection of such a table reveals that the treatment combinations are assigned to the blocks as follows:

Treatment Combinations in	Sign on *ADE*	Sign on *BCE*	Sign on *ABCD*
Block 1	−	−	+
Block 2	+	−	−
Block 3	−	+	−
Block 4	+	+	+

Notice that the product of signs of any two effects for a particular block (e.g., *ADE* and *BCE*) yields the sign of the other effect for that block (in this case, *ABCD*). Thus, *ADE*, *BCE*, and *ABCD* are all confounded with blocks.

The group-theoretic properties of the principal block mentioned in Section 7.4 still hold. For example, we see that the product of two treatment combinations in the principal block yields another element of the principal block. That is,

$$ad \cdot bc = abcd \quad \text{and} \quad abe \cdot bde = ab^2de^2 = ad$$

and so forth. To construct another block, select a treatment combination that is not in the principal block (e.g., *b*) and multiply *b* by all the treatment combinations in the principal block. This yields

$$b \cdot (1) = b \quad b \cdot ad = abd \qquad b \cdot bc = b^2c = c \quad b \cdot abcd = ab^2cd = acd$$

and so forth, which will produce the eight treatment combinations in block 3. In practice, the principal block can be obtained from the defining contrasts and the group-theoretic property, and the remaining blocks can be determined from these treatment combinations by the method shown above.

The general procedure for constructing a 2^k design confounded in four blocks is to choose two effects to generate the blocks, automatically confounding a third effect that is the generalized interaction of the first two. Then, the design is constructed by using the two defining contrasts (L_1, L_2) and the group-theoretic properties of the principal block. In selecting effects to be confounded with blocks, care must be exercised to obtain a design that does not confound effects that may be of interest. For example, in a 2^5 design we might choose to confound *ABCDE* and *ABD*, which automatically confounds *CE*, an effect that is probably of interest. A better choice is to confound *ADE* and *BCE*, which automatically confounds *ABCD*. It is preferable to sacrifice information on the three-factor interactions *ADE* and *BCE* instead of the two-factor interaction *CE*.

7.7 Confounding the 2^k Factorial Design in 2^p Blocks

The methods described above may be extended to the construction of a 2^k factorial design confounded in 2^p blocks ($p < k$), where each block contains exactly 2^{k-p} runs. We select p independent effects to be confounded, where by "independent" we mean that no effect chosen is the generalized interaction of the others. The blocks may be generated by use of the p defining contrasts $L_1, L_2, \ldots, L_p$ associated with these effects. In addition, exactly $2^p - p - 1$ other effects will be confounded with blocks, these being the generalized interactions of those p independent effects initially chosen. Care should be exercised in selecting effects to be confounded so that information on effects that may be of potential interest is not sacrificed.

The statistical analysis of these designs is straightforward. Sums of squares for all the effects are computed as if no blocking had occurred. Then, the block sum of squares is found by adding the sums of squares for all the effects confounded with blocks.

Obviously, the choice of the p effects used to generate the block is critical because the confounding structure of the design directly depends on them. Table 7.9 presents a list of useful designs. To illustrate the use of this

■ **TABLE 7.9**
Suggested Blocking Arrangements for the 2^k Factorial Design

Number of Factors, k	Number of Blocks, 2^p	Block Size, 2^{k-p}	Effects Chosen to Generate the Blocks	Interactions Confounded with Blocks
3	2	4	ABC	ABC
	4	2	AB, AC	AB, AC, BC
4	2	8	$ABCD$	$ABCD$
	4	4	ABC, ACD	ABC, ACD, BD
	8	2	AB, BC, CD	$AB, BC, CD, AC, BD, AD, ABCD$
5	2	16	$ABCDE$	$ABCDE$
	4	8	ABC, CDE	$ABC, CDE, ABDE$
	8	4	ABE, BCE, CDE	$ABE, BCE, CDE, AC, ABCD, BD, ADE$
	16	2	AB, AC, CD, DE	All two- and four-factor interactions (15 effects)
6	2	32	$ABCDEF$	$ABCDEF$
	4	16	$ABCF, CDEF$	$ABCF, CDEF, ABDE$
	8	8	$ABEF, ABCD, ACE$	$ABEF, ABCD, ACE, BCF, BDE, CDEF, ADF$
	16	4	ABF, ACF, BDF, DEF	$ABF, ACF, BDF, DEF, BC, ABCD, ABDE, AD, ACDE, CE, CDF, BCDEF, ABCEF, AEF, BE$
	32	2	AB, BC, CD, DE, EF	All two-, four-, and six-factor interactions (31 effects)
7	2	64	$ABCDEFG$	$ABCDEFG$
	4	32	$ABCFG, CDEFG$	$ABCFG, CDEFG, ABDE$
	8	16	$ABCD, CDEF, ADFG$	$ABC, DEF, AFG, ABCDEF, BCFG, ADEG, BCDEG$
	16	8	$ABCD, EFG, CDE, ADG$	$ABCD, EFG, CDE, ADG, ABCDEFG, ABE, BCG, CDFG, ADEF, ACEG, ABFG, BCEF, BDEG, ACF, BDF$
	32	4	ABG, BCG, CDG, DEG, EFG	$ABG, BCG, CDG, DEG, EFG, AC, BD, CE, DF, AE, BF, ABCD, ABDE, ABEF, BCDE, BCEF, CDEF, ABCDEFG, ADG, ACDEG, ACEFG, ABDFG, ABCEG, BEG, BDEFG, CFG, ADEF, ACDF, ABCF, AFG, BCDFG$
	64	2	AB, BC, CD, DE, EF, FG	All two-, four-, and six-factor interactions (63 effects)

table, suppose we wish to construct a 2^6 design confounded in $2^3 = 8$ blocks of $2^3 = 8$ runs each. Table 7.9 indicates that we would choose *ABEF*, *ABCD*, and *ACE* as the $p = 3$ independent effects to generate the blocks. The remaining $2^p - p - 1 = 2^3 - 3 - 1 = 4$ effects that are confounded are the generalized interactions of these three; that is,

$$(ABEF)(ABCD) = A^2B^2CDEF = CDEF$$
$$(ABEF)(ACE) = A^2BCE^2F = BCF$$
$$(ABCD)(ACE) = A^2BC^2ED = BDE$$
$$(ABEF)(ABCD)(ACE) = A^3B^2C^2DE^2F = ADF$$

The reader is asked to generate the eight blocks for this design in Problem 7.11.

7.8 Partial Confounding

We remarked in Section 7.4 that, unless experimenters have a prior estimate of error or are willing to assume certain interactions to be negligible, they must replicate the design to obtain an estimate of error. Figure 7.3 shows a 2^3 factorial in two blocks with *ABC* confounded, replicated four times. From the analysis of variance for this design, shown in Table 7.5, we note that information on the *ABC* interaction cannot be retrieved because *ABC* is confounded with blocks *in each replicate*. This design is said to be **completely confounded**.

Consider the alternative shown in Figure 7.7. Once again, there are four replicates of the 2^3 design, but a *different* interaction has been confounded in each replicate. That is, *ABC* is confounded in replicate I, *AB* is confounded in replicate II, *BC* is confounded in replicate III, and *AC* is confounded in replicate IV. As a result, information on *ABC* can be obtained from the data in replicates II, III, and IV; information on *AB* can be obtained from replicates I, III, and IV; information on *AC* can be obtained from replicates I, II, and III; and information on *BC* can be obtained from replicates I, II, and IV. We say that three-quarters information can be obtained on the interactions because they are unconfounded in only three replicates. Yates (1937) calls the ratio 3/4 the **relative information for the confounded effects**. This design is said to be **partially confounded**.

The analysis of variance for this design is shown in Table 7.10. In calculating the interaction sums of squares, only data from the replicates in which an interaction is unconfounded are used. The error sum of squares consists of replicates × main effect sums of squares plus replicates × interaction sums of squares for each replicate in which that interaction is unconfounded (e.g., replicates × *ABC* for replicates II, III, and IV). Furthermore, there are seven degrees of freedom among the eight blocks. This is usually partitioned into three degrees of freedom for replicates and four degrees of freedom for blocks within replicates. The composition of the sum of squares for blocks is shown in Table 7.10 and follows directly from the choice of the effect confounded in each replicate.

Replicate I *ABC* Confounded		Replicate II *AB* Confounded		Replicate III *BC* Confounded		Replicate IV *AC* Confounded	
(1)	*a*	(1)	*a*	(1)	*b*	(1)	*a*
ab	*b*	*c*	*b*	*a*	*c*	*b*	*c*
ac	*c*	*ab*	*ac*	*bc*	*ab*	*ac*	*ab*
bc	*abc*	*abc*	*bc*	*abc*	*ac*	*abc*	*bc*

■ **FIGURE 7.7** **Partial confounding in the 2^3 design**

■ **TABLE 7.10**
Analysis of Variance for a Partially Confounded 2^3 Design

Source of Variation	Degrees of Freedom
Replicates	3
Blocks within replicates [or ABC (rep. I) + AB (rep. II) + BC (rep. III) + AC (rep. IV)]	4
A	1
B	1
C	1
AB (from replicates I, III, and IV)	1
AC (from replicates I, II, and III)	1
BC (from replicates I, II, and IV)	1
ABC (from replicates II, III, and IV)	1
Error	17
Total	31

EXAMPLE 7.3 A 2^3 Design with Partial Confounding

Consider Example 6.1, in which an experiment was conducted to develop a plasma etching process. There were three factors, A = gap, B = gas flow, and C = RF power, and the response variable was the etch rate. Suppose that only four treatment combinations can be tested during a shift, and because there could be shift-to-shift differences in etching tool performance, the experimenters decide to use shifts as a blocking factor. Thus, each replicate of the 2^3 design must be run in two blocks. Two replicates are run, with ABC confounded in replicate I and AB confounded in replicate II. The data are as follows:

Replicate I, ABC Confounded: Block 1	Replicate I, ABC Confounded: Block 2	Replicate II, AB Confounded: Block 1	Replicate II, AB Confounded: Block 2
(1) = 550	a = 669	(1) = 604	a = 650
ab = 642	b = 633	c = 1052	b = 601
ac = 749	c = 1037	ab = 635	ac = 868
bc = 1075	abc = 729	abc = 860	bc = 1063

The sums of squares for A, B, C, AC, and BC may be calculated in the usual manner, using all 16 observations. However, we must find SS_{ABC} using only the data in replicate II and SS_{AB} using only the data in replicate I as follows:

$$SS_{ABC} = \frac{[a + b + c + abc - ab - ac - bc - (1)]^2}{n2^k}$$
$$= \frac{[650 + 601 + 1052 + 860 - 635 - 868 - 1063 - 604]^2}{(1)(8)} = 6.1250$$
$$SS_{AB} = \frac{[(1) + abc - ac + c - a - b + ab - bc]^2}{n2^k}$$
$$= \frac{[550 + 729 - 749 + 1037 - 669 - 633 + 642 - 1075]^2}{(1)(8)} = 3528.0$$

The sum of squares for the replicates is, in general,

$$SS_{\text{Rep}} = \sum_{h=1}^{n} \frac{R_h^2}{2^k} - \frac{y_{...}^2}{N}$$
$$= \frac{(6084)^2 + (6333)^2}{8} - \frac{(12{,}417)^2}{16} = 3875.0625$$

where R_h is the total of the observations in the hth replicate. The block sum of squares is the sum of SS_{ABC} from replicate I and SS_{AB} from replicate II, or $SS_{\text{Blocks}} = 458.1250$.

The analysis of variance is summarized in Table 7.11. The main effects of A and C and the AC interaction are important.

■ **TABLE 7.11**
Analysis of Variance for Example 7.3

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	P-Value
Replicates	3875.0625	1	3875.0625	—	
Blocks within replicates	458.1250	2	229.0625	—	
A	41,310.5625	1	41,310.5625	16.20	0.01
B	217.5625	1	217.5625	0.08	0.78
C	374,850.5625	1	374,850.5625	146.97	<0.001
AB (rep. I only)	3528.0000	1	3528.0000	1.38	0.29
AC	94,404.5625	1	94,404.5625	37.01	<0.001
BC	18.0625	1	18.0625	0.007	0.94
ABC (rep. II only)	6.1250	1	6.1250	0.002	0.96
Error	12,752.3125	5	2550.4625		
Total	531,420.9375	15			

7.9 Problems

7.1 Consider the experiment described in Problem 6.5. Analyze this experiment assuming that each replicate represents a block of a single production shift.

7.2 Consider the experiment described in Problem 6.9. Analyze this experiment assuming that each one of the four replicates represents a block.

7.3 Consider the alloy cracking experiment described in Problem 6.19. Suppose that only 16 runs could be made on a single day, so each replicate was treated as a block. Analyze the experiment and draw conclusions.

7.4 Consider the data from the first replicate of Problem 6.5. Suppose that these observations could not all be run using the same bar stock. Set up a design to run these observations in two blocks of four observations each with ABC confounded. Analyze the data.

7.5 Consider the data from the first replicate of Problem 6.11. Construct a design with two blocks of eight observations each with $ABCD$ confounded. Analyze the data.

7.6 Repeat Problem 7.5 assuming that four blocks are required. Confound ABD and ABC (and consequently CD) with blocks.

7.7 Using the data from the 2^5 design in Problem 6.30, construct and analyze a design in two blocks with $ABCDE$ confounded with blocks.

7.8 Repeat Problem 7.7 assuming that four blocks are necessary. Suggest a reasonable confounding scheme.

7.9 Consider the data from the 2^5 design in Problem 6.30. Suppose that it was necessary to run this design in four blocks with $ACDE$ and BCD (and consequently ABE) confounded. Analyze the data from this design.

7.10 Consider the fill height deviation experiment in Problem 6.24. Suppose that each replicate was run on a separate day. Analyze the data assuming that days are blocks.

7.11 Consider the fill height deviation experiment in Problem 6.24. Suppose that only four runs could be made on each shift. Set up a design with *ABC* confounded in replicate I and *AC* confounded in replicate II. Analyze the data and comment on your findings.

7.12 Consider the putting experiment in Problem 6.25. Analyze the data considering each replicate as a block.

7.13 Using the data from the 2^4 design in Problem 6.26, construct and analyze a design in two blocks with *ABCD* confounded with blocks.

7.14 Consider the direct mail experiment in Problem 6.28. Suppose that each group of customers is in a different part of the country. Suggest an appropriate analysis for the experiment.

7.15 Consider the isatin yield experiment in Problem 6.42. Set up the 2^4 experiment in this problem in two blocks with ABCD confounded. Analyze the data from this design. Is the block effect large?

7.16 The experiment in Problem 6.43 is a 2^5 factorial. Suppose that this design had been run in four blocks of eight runs each.

(a) Recommend a blocking scheme and set up the design.

(b) Analyze the data from this blocked design. Is blocking important?

7.17 Repeat Problem 7.16 using a design in two blocks.

7.18 The design in Problem 6.44 is a 2^4 factorial. Set up this experiment in two blocks with ABCD confounded. Analyze the data from this design. Is the block effect large?

7.19 The design in Problem 6.46 is a 2^3 factorial replicated twice. Suppose that each replicate was a block. Analyze all of the responses from this blocked design. Are the results comparable to those from Problem 6.46? Is the block effect large?

7.20 Design an experiment for confounding a 2^6 factorial in four blocks. Suggest an appropriate confounding scheme, different from the one shown in Table 7.9.

7.21 Consider the 2^6 design in eight blocks of eight runs each with *ABCD*, *ACE*, and *ABEF* as the independent effects chosen to be confounded with blocks. Generate the design. Find the other effects confounded with blocks.

7.22 Consider the 2^2 design in two blocks with *AB* confounded. Prove algebraically that $SS_{AB} = SS_{\text{Blocks}}$.

7.23 Consider the data in Example 7.2. Suppose that all the observations in block 2 are increased by 20. Analyze the data that would result. Estimate the block effect. Can you explain its magnitude? Do blocks now appear to be an important factor? Are any other effect estimates impacted by the change you made to the data?

7.24 Suppose that in Problem 6.5 we had confounded *ABC* in replicate I, *AB* in replicate II, and *BC* in replicate III. Calculate the factor effect estimates. Construct the analysis of variance table.

7.25 Repeat the analysis of Problem 6.5 assuming that *ABC* was confounded with blocks in each replicate.

7.26 Suppose that in Problem 6.11 *ABCD* was confounded in replicate I and *ABC* was confounded in replicate II. Perform the statistical analysis of this design.

7.27 Construct a 2^3 design with *ABC* confounded in the first two replicates and *BC* confounded in the third. Outline the analysis of variance and comment on the information obtained.

7.28 Suppose that a 2^2 design has been conducted. There are four replicates and the experiment has been conducted in four blocks. The error sum of squares is 500 and the block sum of squares is 250. If the experiment had been conducted as a completely randomized design, the estimate of the error variance σ^2 would be

(a) 25.0 **(b)** 25.5 **(c)** 35.0

(d) 38.5 **(e)** none of the above

7.29 The block effect in a two-level design with two blocks can be calculated directly as the difference in the average response between the two blocks.

(a) True

(b) False

7.30 When constructing the 2^7 design confounded in eight blocks, three independent effects are chosen to generate the blocks, and there are a total of eight interactions confounded with blocks.

(a) True

(b) False

7.31 Consider the 2^5 factorial design in two blocks. If *ABCDE* is confounded with blocks, then which of the following runs is in the same block as run *acde*?

(a) *a* **(b)** *acd* **(c)** *bcd*

(d) *be* **(e)** *abe* **(f)** None of the above

7.32 The information on the interaction confounded with the block can always be separated from the block effect.

(a) True

(b) False

7.33 Consider the full 2^5 factorial design in Problem 6.51. Suppose that this experiment had been run in two blocks with *ABCDE* confounded with the blocks. Set up the blocked design and perform the analysis. Compare your results with the results obtained for the completely randomized design in Problem 6.51.

7.34 Suppose that you are designing an experiment for four factors and that due to material properties it is necessary to conduct the experiment in blocks. Material availability restricts you to the use of two blocks; however, each batch of material is only sufficient for six runs. So the standard 2^4 factorial in two blocks of eight runs each with *ABCD* confounded will not work. Recommend a design. Suggestion: this is a reasonable application for a *D*-optimal design. What type of design do you find in each block?

7.35 Suppose that you are designing an experiment for four factors and that due to material properties it is necessary to conduct the experiment in blocks. Material availability restricts you to the use of two blocks but each batch of material is large enough for up to 10 runs. You can afford to make four additional runs beyond the 16 required by the full 2^4. What runs would you choose to make? How would you allocate these additional four runs to the two blocks?

CHAPTER 8

Two-Level Fractional Factorial Designs

CHAPTER OUTLINE

The supplemental material is on the textbook website www.wiley.com/college/montgomery.

CHAPTER LEARNING OBJECTIVES

1. Know how to construct and analyze 2^{k-p} fractional factorial designs.
2. Know how to construct fractional factorials in blocks.
3. Understand how to determine the alias structure of a fractional factorial design.
4. Understand the concepts of design resolution and minimum aberration.
5. Know how to use fold over to augment a fractional factorial to simplify the alias relationships.
6. Know how to use other design augmentation strategies, such as optimal augmentation and partial fold over.
7. Know how to construct and analyze supersaturated designs.

8.1 Introduction

As the number of factors in a 2^k factorial design increases, the number of runs required for a complete replicate of the design rapidly outgrows the resources of most experimenters. For example, a complete replicate of the 2^6 design requires 64 runs. In this design, only 6 of the 63 degrees of freedom correspond to main effects, and only 15 degrees of freedom correspond to two-factor interactions. There are only 21 degrees of freedom associated with effects that are likely to be of major interest. The remaining 42 degrees of freedom are associated with three-factor and higher interactions.

If the experimenter can reasonably assume that certain high-order interactions are negligible, information on the main effects and low-order interactions may be obtained by running only a fraction of the complete factorial experiment. These **fractional factorial designs** are among the most widely used types of designs for product and process design, process improvement, and industrial/business experimentation.

A major use of fractional factorials is in **screening experiments**—experiments in which many factors are considered and the objective is to identify those factors (if any) that have large effects. Screening experiments are usually performed in the early stages of a project when many of the factors initially considered likely have little or no effect on the response. The factors identified as important are then investigated more thoroughly in subsequent experiments.

The successful use of fractional factorial designs is based on three key ideas:

1. ***The sparsity of effects principle.*** When there are several variables, the system or process is likely to be driven primarily by some of the main effects and low-order interactions. Sparsity of effects usually implies that no more than about half the number of effects will be active. For example, if there are 4 factors, then there are 15 effects, and effect sparsity suggests that no more than 6 or 7 of these will be active.
2. ***The projection property.*** Fractional factorial designs can be projected into stronger (larger) designs in the subset of significant factors.
3. ***Sequential experimentation.*** It is possible to combine the runs of two (or more) fractional factorials to construct sequentially a larger design to estimate the factor effects and interactions of interest.

We will focus on these principles in this chapter and illustrate them with several examples.

8.2 The One-Half Fraction of the 2^k Design

8.2.1 Definitions and Basic Principles

Consider a situation in which three factors, each at two levels, are of interest, but the experimenters cannot afford to run all $2^3 = 8$ treatment combinations. They can, however, afford four runs. This suggests a **one-half fraction** of a 2^3 design. Because the design contains $2^{3-1} = 4$ treatment combinations, a one-half fraction of the 2^3 design is often called a $\mathbf{2^{3-1}}$ **design**.

The table of plus and minus signs for the 2^3 design is shown in Table 8.1. Suppose we select the four treatment combinations *a, b, c*, and *abc* as our one-half fraction. These runs are shown in the top half of Table 8.1 and in Figure 8.1*a*.

Notice that the 2^{3-1} design is formed by selecting only those treatment combinations that have a plus in the *ABC* column. Thus, *ABC* is called the **generator** of this particular fraction. Usually we will refer to a generator such as *ABC* as a **word**. Furthermore, the identity column *I* is also always plus, so we call

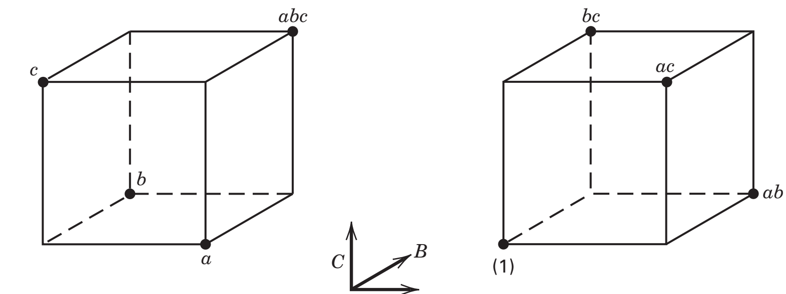
$$I = ABC$$

the **defining relation** for our design. In general, the defining relation for a fractional factorial will always be the set of all columns that are equal to the identity column *I*.

■ **TABLE 8.1**
Plus and Minus Signs for the 2^3 Factorial Design

Treatment Combination	Factorial Effect							
	I	*A*	*B*	*C*	*AB*	*AC*	*BC*	*ABC*
a	+	+	−	−	−	−	+	+
b	+	−	+	−	−	+	−	+
c	+	−	−	+	+	−	−	+
abc	+	+	+	+	+	+	+	+
ab	+	+	+	−	+	−	−	−
ac	+	+	−	+	−	+	−	−
bc	+	−	+	+	−	−	+	−
(1)	+	−	−	−	+	+	+	−

■ **FIGURE 8.1 The two one-half fractions of the 2^3 design**



(*a*) The principal fraction, $I = +ABC$ (*b*) The alternate fraction, $I = -ABC$

The treatment combinations in the 2^{3-1} design yield three degrees of freedom that we may use to estimate the main effects. Referring to Table 8.1, we note that the linear combinations of the observations used to estimate the main effects of *A*, *B*, and *C* are

$$[A] = \tfrac{1}{2}(a - b - c + abc)$$
$$[B] = \tfrac{1}{2}(-a + b - c + abc)$$
$$[C] = \tfrac{1}{2}(-a - b + c + abc)$$

where the notation [*A*], [*B*], and [*C*] is used to indicate the linear combinations associated with the main effects. It is also easy to verify that the linear combinations of the observations used to estimate the two-factor interactions are

$$[BC] = \tfrac{1}{2}(a - b - c + abc)$$
$$[AC] = \tfrac{1}{2}(-a + b - c + abc)$$
$$[AB] = \tfrac{1}{2}(-a - b + c + abc)$$

Thus, $[A] = [BC]$, $[B] = [AC]$, and $[C] = [AB]$; consequently, it is impossible to differentiate between *A* and *BC*, *B* and *AC*, and *C* and *AB*. In fact, when we estimate *A*, *B*, and *C* we are *really* estimating $A + BC$, $B + AC$, and $C + AB$. Two or more effects that have this property are called **aliases**. In our example, *A* and *BC* are aliases, *B* and *AC* are aliases, and *C* and *AB* are aliases. We indicate this by the notation $[A] \rightarrow A + BC$, $[B] \rightarrow B + AC$, and $[C] \rightarrow C + AB$.

The alias structure for this design may be easily determined by using the defining relation $I = ABC$. Multiplying any column (or effect) by the defining relation yields the aliases for that column (or effect). In our example, this yields as the alias of *A*

$$A \cdot I = A \cdot ABC = A^2BC$$

or, because the square of any column is just the identity I,

$$A = BC$$

Similarly, we find the aliases of B and C as

$$B \cdot I = B \cdot ABC$$
$$B = AB^2C = AC$$

and

$$C \cdot I = C \cdot ABC$$
$$C = ABC^2 = AB$$

This one-half fraction, with $I = +ABC$, is usually called the **principal fraction**.

Now suppose that we had chosen the *other* one-half fraction, that is, the treatment combinations in Table 8.1 associated with minus in the ABC column. This **alternate**, or **complementary**, one-half fraction (consisting of the runs (1), *ab, ac*, and *bc*) is shown in Figure 8.1*b*. The defining relation for this design is

$$I = -ABC$$

The linear combination of the observations, say $[A]'$, $[B]'$, and $[C]'$, from the alternate fraction gives us

$$[A]' \rightarrow A - BC$$
$$[B]' \rightarrow B - AC$$
$$[C]' \rightarrow C - AB$$

Thus, when we estimate A, B, and C with this particular fraction, we are really estimating $A - BC, B - AC$, and $C - AB$.

In practice, it does not matter which fraction is actually used. Both fractions belong to the same **family**; that is, the two one-half fractions form a complete 2^3 design. This is easily seen by reference to parts *a* and *b* of Figure 8.1.

Suppose that after running one of the one-half fractions of the 2^3 design, the other fraction was also run. Thus, all eight runs associated with the full 2^3 are now available. We may now obtain de-aliased estimates of all the effects by analyzing the eight runs as a full 2^3 design in two blocks of four runs each. This could also be done by adding and subtracting the linear combination of effects from the two individual fractions. For example, consider $[A] \rightarrow A + BC$ and $[A]' \rightarrow A - BC$. This implies that

$$\tfrac{1}{2}([A] + [A]') = \tfrac{1}{2}(A + BC + A - BC) \rightarrow A$$

and that

$$\tfrac{1}{2}([A] - [A]') = \tfrac{1}{2}(A + BC - A + BC) \rightarrow BC$$

Thus, for all three pairs of linear combinations, we would obtain the following:

i	From $\frac{1}{2}([i] + [i]')$	From $\frac{1}{2}([i] - [i]')$
A	A	BC
B	B	AC
C	C	AB

Furthermore, by assembling the full 2^3 in this fashion with $I = +ABC$ in the first group of runs and $I = -ABC$ in the second, the 2^3 confounds ABC with blocks.

More About Effect Sparsity. As noted earlier, effect sparsity is one of the reasons that fractional factorial designs are so successful. This phenomenon has been observed empirically by experimenters in many fields for decades. However, a recent paper by Li, Sudarsanam, and Frey(2006) provides more objective evidence of effect sparsity.

Li, Sudarsanam, and Frey (2006) re-examined 133 response variables from published full factorial experiments with from 3 to 7 factors. They re-analyzed all of the responses. They found that in the experiments that they studied 41% of the main effects were active. Generally, the size of an active main effect was twice the size of an active two-factor interaction. The percent of active two-factor interactions overall was 11%. Interactions beyond order two were extremely rare. They also reported some "conditional" percentages regarding active two-factor interactions:

- A two-factor interaction was active and both main effects involved in that interaction were active occurred 33% of the time.
- A two-factor interaction was active but only one of the main effects involved in that interaction was active occurred 4.5% of the time.
- A two-factor interaction was active and neither of the main effects involved in that interaction was active occurred only 0.5% of the time.

These results strongly support the sparsity of effects assumption. They also support the usual assumptions of model hierarchy and effect heredity. However, the results are strongly dependent on the types of experiments analyzed. If more experiments involving chemical processes and systems and biological systems were included, two-factor interactions would probably be more likely to occur. Three-factor interactions can be encountered in some of these systems. For example, consider a three-factor chemical process experiment involving two continuous factor, time and temperature, and a categorical factor, catalyst type. If the two-factor interaction involving time and temperature is different for each catalyst type, then there is a three-factor interaction.

8.2.2 Design Resolution

The preceding 2^{3-1} design is called a **resolution III design**. In such a design, main effects are aliased with two-factor interactions. A design is of resolution R if no p-factor effect is aliased with another effect containing less than $R - p$ factors. We usually employ a Roman numeral subscript to denote design resolution; thus, the one-half fraction of the 2^3 design with the defining relation $I = ABC$ (or $I = -ABC$) is a 2^{3-1}_{III} design.

Designs of resolution III, IV, and V are particularly important. The definitions of these designs and an example of each follow:

1. ***Resolution III designs.*** These are designs in which no main effects are aliased with any other main effect, but main effects are aliased with two-factor interactions and some two-factor interactions may be aliased with each other. The 2^{3-1} design in Table 8.1 is of resolution III (2^{3-1}_{III}).
2. ***Resolution IV designs.*** These are designs in which no main effect is aliased with any other main effect *or* with any two-factor interaction, but two-factor interactions are aliased with each other. A 2^{4-1} design with $I = ABCD$ is a resolution IV design (2^{4-1}_{IV}).
3. ***Resolution V designs.*** These are designs in which no main effect or two-factor interaction is aliased with any other main effect or two-factor interaction, but two-factor interactions are aliased with three-factor interactions. A 2^{5-1} design with $I = ABCDE$ is a resolution V design (2^{5-1}_{V}).

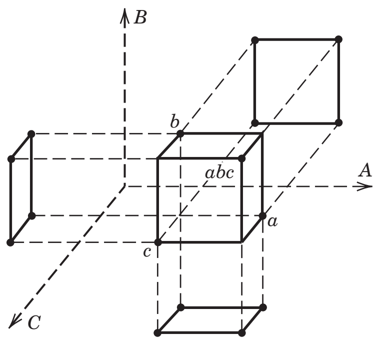
In general, the resolution of a two-level fractional factorial design is equal to the number of letters in the shortest word in the defining relation. Consequently, we could call the preceding design types three-, four-, and five-letter designs, respectively. We usually like to employ fractional designs that have the highest possible resolution consistent with the degree of fractionation required. The higher the resolution, the less restrictive the assumptions that are required regarding which interactions are negligible to obtain a unique interpretation of the results.

8.2.3 Construction and Analysis of the One-Half Fraction

A one-half fraction of the 2^k design of the highest resolution may be constructed by writing down a **basic design** consisting of the runs for a *full* 2^{k-1} factorial and then adding the kth factor by identifying its plus and minus levels with the plus and minus signs of the highest order interaction $ABC \cdots (K - 1)$. Therefore, the 2^{3-1}_{III} fractional factorial is obtained by writing down the full 2^2 factorial as the basic design and then equating factor C to the AB interaction. The alternate fraction would be obtained by equating factor C to the $-AB$ interaction. This approach is illustrated in Table 8.2. Notice that the basic design always has the right number of runs (rows), but it is missing one column.

■ **TABLE 8.2**
The Two One-Half Fractions of the 2^3 Design

Run	Full 2^2 Factorial (Basic Design) A	B	$2^{3-1}_{\text{III}}, I = ABC$ A	B	$C = AB$	$2^{3-1}_{\text{III}}, I = -ABC$ A	B	$C = -AB$
1	−	−	−	−	+	−	−	−
2	+	−	+	−	−	+	−	+
3	−	+	−	+	−	−	+	+
4	+	+	+	+	+	+	+	−



■ **FIGURE 8.2** Projection of a 2^{3-1}_{III} design into three 2^2 designs

The generator $I = ABC \cdots K$ is then solved for the missing column (K) so that $K = ABC \cdots (K-1)$ defines the product of plus and minus signs to use in each row to produce the levels for the kth factor.

Note that *any* interaction effect could be used to generate the column for the kth factor. However, using any effect other than $ABC \cdots (K-1)$ will not produce a design of the highest possible resolution.

Another way to view the construction of a one-half fraction is to partition the runs into two blocks with the highest order interaction $ABC \cdots K$ confounded. Each block is a 2^{k-1} fractional factorial design of the highest resolution.

Projection of Fractions into Factorials. Any fractional factorial design of resolution R contains complete factorial designs (possibly replicated factorials) in any subset of $R - 1$ factors. This is an important and useful concept. For example, if an experimenter has several factors of potential interest but believes that only $R - 1$ of them have important effects, then a fractional factorial design of resolution R is the appropriate choice of design. If the experimenter is correct, the fractional factorial design of resolution R will project into a full factorial in the $R - 1$ significant factors. This property is illustrated in Figure 8.2 for the 2^{3-1}_{III} design, which projects into a 2^2 design in every subset of two factors.

Because the maximum possible resolution of a one-half fraction of the 2^k design is $R = k$, every 2^{k-1} design will project into a full factorial in any $(k-1)$ of the original k factors. Furthermore, a 2^{k-1} design may be projected into two replicates of a full factorial in any subset of $k - 2$ factors, four replicates of a full factorial in any subset of $k - 3$ factors, and so on.

EXAMPLE 8.1

Consider the filtration rate experiment in Example 6.2. The original design, shown in Table 6.10, is a single replicate of the 2^4 design. In that example, we found that the main effects A, C, and D and the interactions AC and AD were different from zero. We will now return to this experiment and simulate what would have happened if a half-fraction of the 2^4 design had been run instead of the full factorial.

We will use the 2^{4-1} design with $I = ABCD$, because this choice of generator will result in a design of the highest possible resolution (IV). To construct the design, we first write down the basic design, which is a 2^3 design, as shown in

■ **TABLE 8.3**
The 2^{4-1}_{IV} Design with the Defining Relation $I = ABCD$

	Basic Design					
Run	A	B	C	$D = ABC$	Treatment Combination	Filtration Rate
1	−	−	−	−	(1)	45
2	+	−	−	+	*ad*	100
3	−	+	−	+	*bd*	45
4	+	+	−	−	*ab*	65
5	−	−	+	+	*cd*	75
6	+	−	+	−	*ac*	60
7	−	+	+	−	*bc*	80
8	+	+	+	+	*abcd*	96

the first three columns of Table 8.3. This basic design has the necessary number of runs (eight) but only three columns (factors). To find the fourth factor levels, solve $I = ABCD$ for D, or $D = ABC$. Thus, the level of D in each run is the product of the plus and minus signs in columns A, B, and C. The process is illustrated in Table 8.3. Because the generator $ABCD$ is positive, this 2^{4-1}_{IV} design is the principal fraction. The design is shown graphically in Figure 8.3.

Using the defining relation, we note that each main effect is aliased with a three-factor interaction; that is, $A = A^2BCD = BCD, B = AB^2CD = ACD, C = ABC^2D = ABD$, and $D = ABCD^2 = ABC$. Furthermore, every two-factor interaction is aliased with another two-factor interaction. These alias relationships are $AB = CD$, $AC = BD$, and $BC = AD$. The four main effects plus the three two-factor interaction alias pairs account for the seven degrees of freedom for the design.

At this point, we would normally randomize the eight runs and perform the experiment. Because we have already run the full 2^4 design, we will simply select the eight observed filtration rates from Example 6.2 that correspond to the runs in the 2^{4-1}_{IV} design. These observations are shown in the last column of Table 8.3 as well as in Figure 8.3.

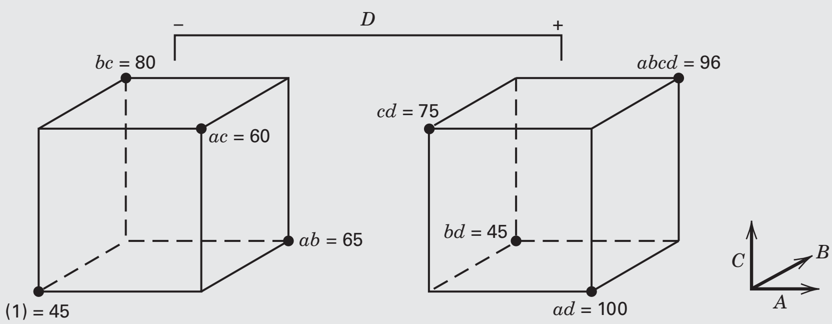
The estimates of the effects obtained from this 2^{4-1}_{IV} design are shown in Table 8.4. To illustrate the calculations, the linear combination of observations associated with the A effect is

$$[A] = \tfrac{1}{4}(-45 + 100 - 45 + 65 - 75 + 60 - 80 + 96) = 19.00 \rightarrow A + BCD$$

whereas for the AB effect, we would obtain

$$[AB] = \tfrac{1}{4}(45 - 100 - 45 + 65 + 75 - 60 - 80 + 96) = -1.00 \rightarrow AB + CD$$

From inspection of the information in Table 8.4, it is not unreasonable to conclude that the main effects A, C, and D are large. The $AB + CD$ alias chain has a small estimate, so the simplest interpretation is that both the AB and CD



■ **FIGURE 8.3 The 2^{4-1}_{IV} design for the filtration rate experiment of Example 8.1**

■ **TABLE 8.4**
Estimates of Effects and Aliases from Example 8.1[a]

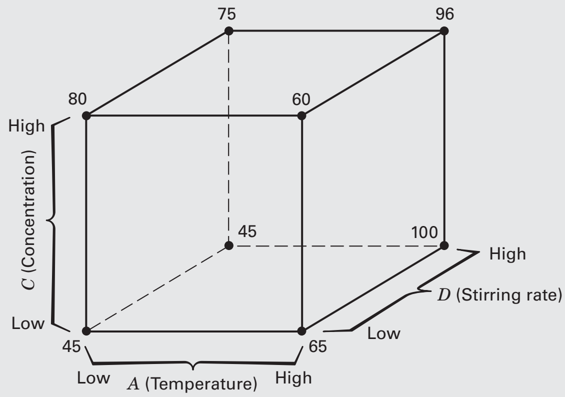
Estimate	Alias Structure
$[A] = 19.00$	$[A] \rightarrow A + BCD$
$[B] = 1.50$	$[B] \rightarrow B + ACD$
$[C] = 14.00$	$[C] \rightarrow C + ABD$
$[D] = 16.50$	$[D] \rightarrow D + ABC$
$[AB] = -1.00$	$[AB] \rightarrow AB + CD$
$[AC] = -18.50$	$[AC] \rightarrow AC + BD$
$[AD] = 19.00$	$[AD] \rightarrow AD + BC$

[a] Significant effects are shown in boldface type.

interactions are negligible (otherwise, both *AB* and *CD* are large, but they have nearly identical magnitudes and opposite signs—this is fairly unlikely). Furthermore, if *A*, *C*, and *D* are the important main effects, then it is logical to conclude that the two interaction alias chains $AC + BD$ and $AD + BC$ have large effects because the *AC* and *AD* interactions are also significant. In other words, if *A*, *C*, and *D* are significant, then the significant interactions are most likely *AC* and *AD*. This is an application of **Ockham's razor** (after William of Ockham), a scientific principle that when one is confronted with several different possible interpretations of a phenomena, the simplest interpretation is usually the correct one. Note that this interpretation agrees with the conclusions from the analysis of the complete 2^4 design in Example 6.2.

Another way to view this interpretation is from the standpoint of **effect heredity**. Suppose that *AB* is significant and that both main effects *A* and *B* are significant. This is called **strong heredity**, and it is the usual situation (if an interaction is significant and only one of the main effects is significant this is called **weak heredity**; and this is relatively less common). So in this example, with *A* significant and *B* not significant this support the assumption that *AB* is not significant.

Because factor *B* is not significant, we may drop it from consideration. Consequently, we may project this 2^{4-1}_{IV} design into a single replicate of the 2^3 design in factors *A*, *C*, and *D*, as shown in Figure 8.4. Visual examination of this cube plot makes us more comfortable with the conclusions reached above. Notice that if the temperature (*A*) is at the low level, the concentration (*C*) has a large positive effect, whereas if the temperature is at the high level, the concentration has a very small effect. This is probably due to an *AC* interaction. Furthermore, if the temperature is at the low level, the effect of the stirring rate (*D*) is negligible, whereas if the temperature is at the high level, the stirring rate has a large positive effect. This is probably due to the *AD* interaction tentatively identified previously.



■ **FIGURE 8.4** **Projection of the 2^{4-1}_{IV} design into a 2^3 design in *A*, *C*, and *D* for Example 8.1**

Based on the above analysis, we can now obtain a model to predict filtration rate over the experimental region. This model is

$$\hat{y} = \hat{\beta}_0 + \hat{\beta}_1 x_1 + \hat{\beta}_3 x_3 + \hat{\beta}_4 x_4 + \hat{\beta}_{13} x_1 x_3 + \hat{\beta}_{14} x_1 x_4$$

where x_1, x_3, and x_4 are coded variables $(-1 \leq x_i \leq +1)$ that represent *A*, *C*, and *D*, and the $\hat{\beta}$'s are regression coefficients that can be obtained from the effect estimates as we did previously. Therefore, the prediction equation is

$$\hat{y} = 70.75 + \left(\frac{19.00}{2}\right) x_1 + \left(\frac{14.00}{2}\right) x_3 + \left(\frac{16.50}{2}\right) x_4 + \left(\frac{-18.50}{2}\right) x_1 x_3 + \left(\frac{19.00}{2}\right) x_1 x_4$$

Remember that the intercept $\hat{\beta}_0$ is the average of all responses at the eight runs in the design. This model is very similar to the one that resulted from the full 2^k factorial design in Example 6.2.

The JMP screening analysis for Example 8.1 is shown in the boxed display below. Because there are only eight runs and seven degrees of freedom, we only included the intercept, the four main effects, and three of the six two-factor interactions (and their aliases) in the model. All of the *P*-values from Lenth's procedure are large. Eight runs with five active effects are not adequate to produce a reliable error estimate from Lenth's method. Also, notice that the R^2 statistic is 1, and no values are reported for the adjusted R^2 and the square root of the mean square error because the model is saturated. However, the largest effects are the three main effects and the two two-factor interactions identified previously in Example 8.1. The prediction profiler portion of the output has been set to the levels of the active factors that maximize the filtration rate.

Response Y

Summary of Fit

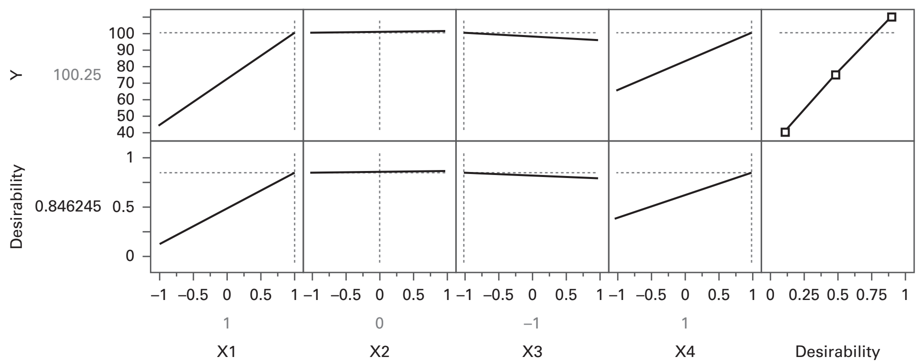
RSquare	1
RSquare Adj	.
Root Mean Square Error	.
Mean of Response	70.75
Observations (or Sum Wgts)	8

Sorted Parameter Estimates

Term	Estimate	Relative Std Error	Pseudo t-Ratio	Pseudo p-Value
X1	9.5	0.353553	0.77	0.5128
X1*X4	9.5	0.353553	0.77	0.5128
X1*X3	–9.25	0.353553	–0.75	0.5228
X4	8.25	0.353553	0.67	0.5649
X3	7	0.353553	0.57	0.6213
X2	0.75	0.353553	0.06	0.9565
X1*X2	–0.5	0.353553	–0.04	0.9710

No error degrees of freedom, so ordinary tests uncomputable. Relative Std Error corresponds to residual standard error of 1. Pseudo t-Ratio and p-Value calculated using Lenth PSE = 12.375 and DFE = 2.3333

Prediction Profiler



Effect Screening

The parameter estimates have equal variances.
The parameter estimates are not correlated.

Lenth PSE
12.375

Parameter Estimate Population

Term	Estimate	Pseudo t-Ratio	Pseudo p-Value
Intercept	70.7500	5.7172	0.0203*
X1	9.5000	0.7677	0.5128
X2	0.7500	0.0606	0.9565
X3	7.0000	0.5657	0.6213
X4	8.2500	0.6667	0.5649
X1*X2	–0.5000	–0.0404	0.9710
X1*X3	–9.2500	–0.7475	0.5228
X1*X4	9.5000	0.7677	0.5128

Orthog t-Test used Pseudo Standard Error

EXAMPLE 8.2 A 2^{5-1} Design Used for Process Improvement

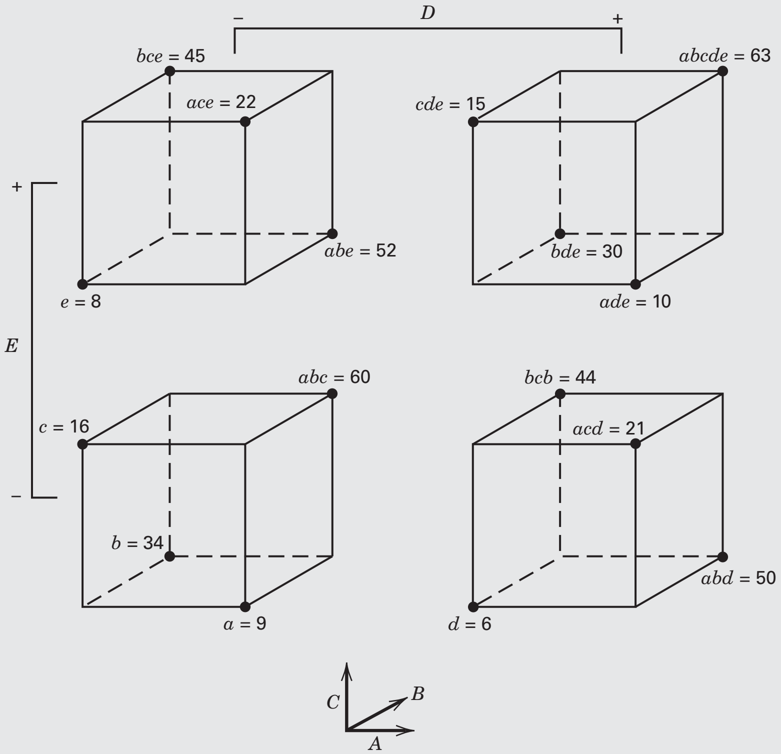
Five factors in a manufacturing process for an integrated circuit were investigated in a 2^{5-1} design with the objective of improving the process yield. The five factors were A = aperture setting (small, large), B = exposure time (20 percent below nominal, 20 percent above nominal), C = develop time (30 and 45 sec), D = mask dimension (small, large), and E = etch time (14.5 and 15.5 min). The construction of the 2^{5-1} design is shown in Table 8.5. Notice that the design was constructed by writing down the basic design having 16 runs (a 2^4 design in *A, B, C*, and *D*), selecting *ABCDE* as the generator, and then setting the levels of the fifth factor $E = ABCD$. Figure 8.5 gives a pictorial representation of the design.

The defining relation for the design is $I = ABCDE$. Consequently, every main effect is aliased with a four-factor interaction (for example, $[A] \rightarrow A + BCDE$), and every two-factor interaction is aliased with a three-factor interaction (e.g., $[AB] \rightarrow AB + CDE$). Thus, the design is of resolution V. We would expect this 2^{5-1} design to provide excellent information concerning the main effects and two-factor interactions.

Table 8.6 contains the effect estimates, sums of squares, and model regression coefficients for the 15 effects from this experiment. Figure 8.6 presents a normal probability plot of the effect estimates from this experiment. The main effects of *A*, *B*, and *C* and the *AB* interaction are large. Remember that, because of aliasing, these effects are really $A + BCDE$, $B + ACDE$, $C + ABDE$, and $AB + CDE$. However, because it seems plausible that three-factor and higher interactions are negligible, we feel safe in concluding that only *A, B, C*, and *AB* are important effects.

■ **TABLE 8.5**
A 2^{5-1} Design for Example 8.2

	Basic Design						
Run	***A***	***B***	***C***	***D***	***E = ABCD***	**Treatment Combination**	**Yield**
1	−	−	−	−	+	*e*	8
2	+	−	−	−	−	*a*	9
3	−	+	−	−	−	*b*	34
4	+	+	−	−	+	*abe*	52
5	−	−	+	−	−	*c*	16
6	+	−	+	−	+	*ace*	22
7	−	+	+	−	+	*bce*	45
8	+	+	+	−	−	*abc*	60
9	−	−	−	+	−	*d*	6
10	+	−	−	+	+	*ade*	10
11	−	+	−	+	+	*bde*	30
12	+	+	−	+	−	*abd*	50
13	−	−	+	+	+	*cde*	15
14	+	−	+	+	−	*acd*	21
15	−	+	+	+	−	*bcd*	44
16	+	+	+	+	+	*abcde*	63



■ **FIGURE 8.5** The 2_{V}^{5-1} design for Example 8.2

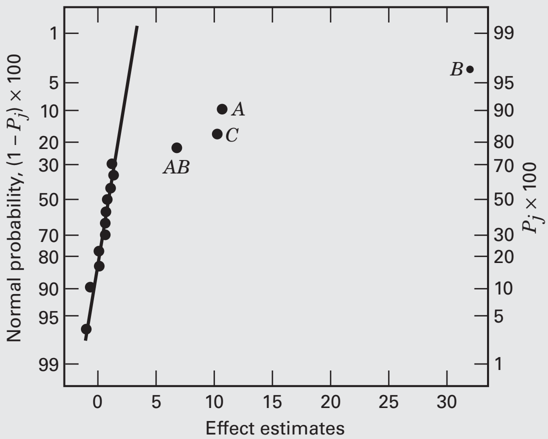
■ **TABLE 8.6**
Effects, Regression Coefficients, and Sums of Squares for Example 8.2

Variable	Name	−1 Level	+1 Level
A	Aperture	Small	Large
B	Exposure time	−20%	+20%
C	Develop time	30 sec	40 sec
D	Mask dimension	Small	Large
E	Etch time	14.5 min	15.5 min

Variable	Regression Coefficient	Estimated Effect	Sum of Squares
Overall Average	30.3125		
A	5.5625	11.1250	495.062
B	16.9375	33.8750	4590.062
C	5.4375	10.8750	473.062
D	−0.4375	−0.8750	3.063
E	0.3125	0.6250	1.563
AB	3.4375	6.8750	189.063

■ **TABLE 8.6** (*Continued*)

Variable	Regression Coefficient	Estimated Effect	Sum of Squares
AC	0.1875	0.3750	0.563
AD	0.5625	1.1250	5.063
AE	0.5625	1.1250	5.063
BC	0.3125	0.6250	1.563
BD	−0.0625	−0.1250	0.063
BE	−0.0625	−0.1250	0.063
CD	0.4375	0.8750	3.063
CE	0.1875	0.3750	0.563
DE	−0.6875	−1.3750	7.563



■ **FIGURE 8.6 Normal probability plot of effects for Example 8.2**

Table 8.7 summarizes the analysis of variance for this experiment. The model sum of squares is $SS_{\text{Model}} = SS_A + SS_B + SS_C + SS_{AB} = 5747.25$, and this accounts for over 99 percent of the total variability in yield. Figure 8.7 presents a normal probability plot of the residuals, and Figure 8.8 is a plot of the residuals versus the predicted values. Both plots are satisfactory.

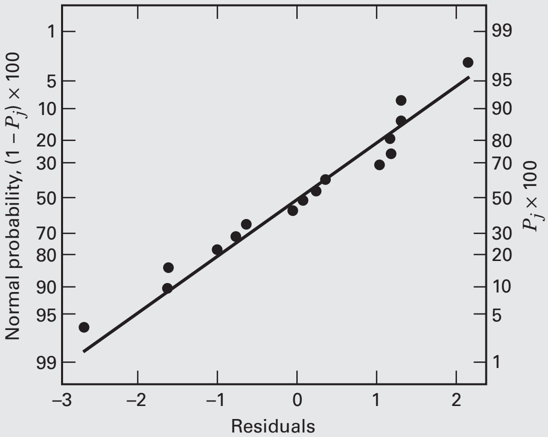
The three factors *A*, *B*, and *C* have large positive effects. The *AB* or aperture–exposure time interaction is plotted in Figure 8.9. This plot confirms that the yields are higher when both *A* and *B* are at the high level.

The 2^{5-1} design will collapse into two replicates of a 2^3 design in any three of the original five factors. (Looking at Figure 8.5 will help you visualize this.) Figure 8.10 is a cube plot in the factors *A*, *B*, and *C* with the average yields superimposed on the eight corners. It is clear from inspection of the cube plot that highest yields are achieved with *A*, *B*,

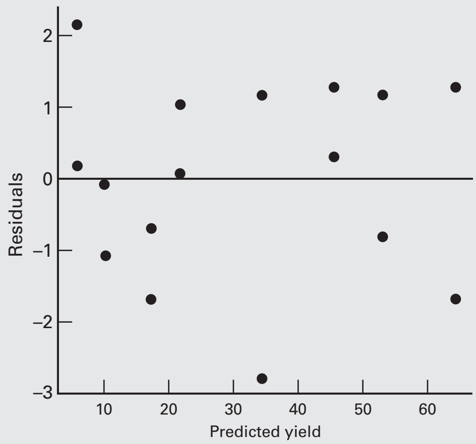
■ **TABLE 8.7**
Analysis of Variance for Example 8.2

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	*P*-Value
A (Aperture)	495.0625	1	495.0625	193.20	<0.0001
B (Exposure time)	4590.0625	1	4590.0625	1791.24	<0.0001
C (Develop time)	473.0625	1	473.0625	184.61	<0.0001
AB	189.0625	1	189.0625	73.78	<0.0001
Error	28.1875	11	2.5625		
Total	5775.4375	15			

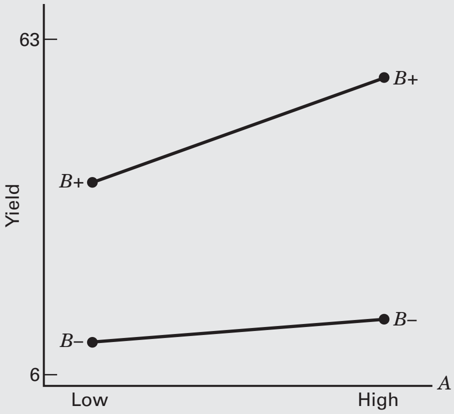
and C all at the high level. Factors D and E have little effect on average process yield and may be set to values that optimize other objectives (such as cost).



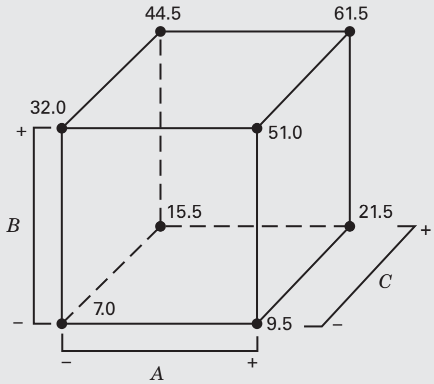
■ **FIGURE 8.7 Normal probability plot of the residuals for Example 8.2**



■ **FIGURE 8.8 Plot of residuals versus predicted yield for Example 8.2**



■ **FIGURE 8.9 Aperture–exposure time interaction for Example 8.2**



■ **FIGURE 8.10 Projection of the 2_{V}^{5-1} design in Example 8.2 into two replicates of a 2^3 design in the factors A, B, and C**

The output from the JMP screening analysis is shown in the following display. The JMP screening platform uses Lenth's method to determine the active effects. The results agree with the normal probability plot of effects method used in Example 8.2. Because the design is saturated when all main effects and two-factor interactions are included in the model, there are no degrees of freedom available to estimate error. Consequently, $R^2 = 1$, and the adjusted R^2 and square root of mean square error cannot be computed.

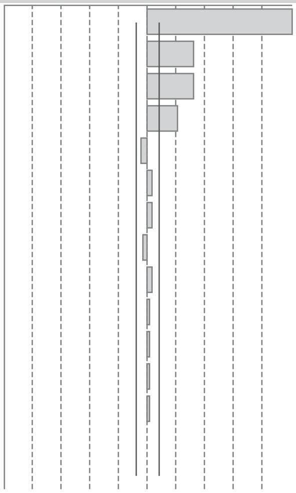
Response Y

Summary of Fit

RSquare	1
RSquare Adj	.
Root Mean Square Error	.
Mean of Response	30.3125
Observations (or Sum Wgts)	16

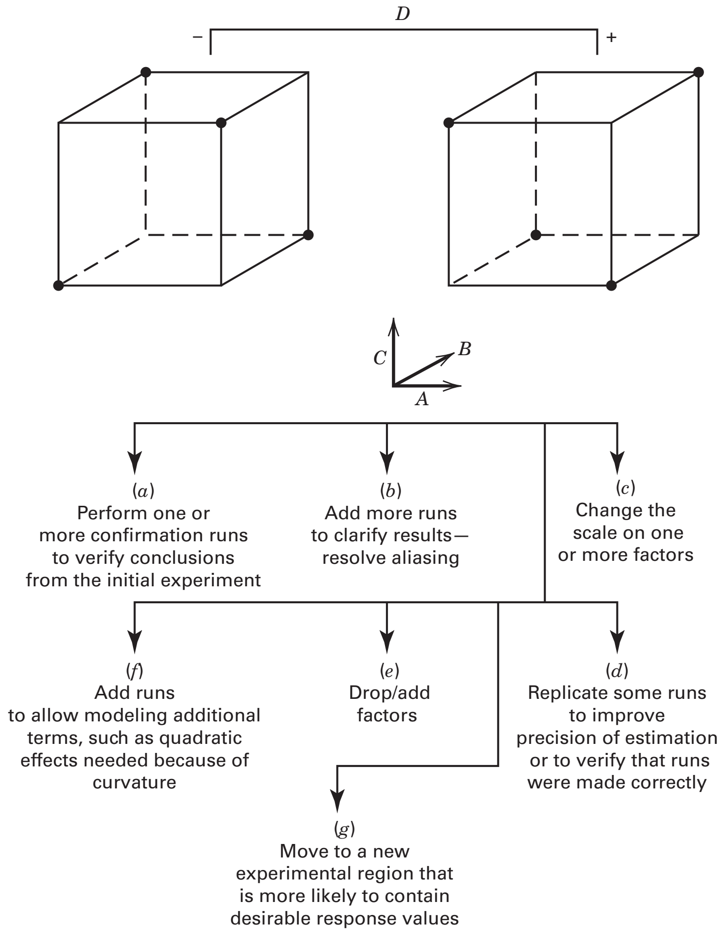
Sorted Parameter Estimates

Term	Estimate	Relative Std Error	Pseudo t-Ratio	Pseudo p-Value
X2	16.9375	0.25	36.13	<.0001*
X1	5.5625	0.25	11.87	<.0001*
X3	5.4375	0.25	11.60	<.0001*
X1*X2	3.4375	0.25	7.33	0.0007*
X4*X5	–0.6875	0.25	–1.47	0.2024
X1*X4	0.5625	0.25	1.20	0.2839
X1*X5	0.5625	0.25	1.20	0.2839
X4	–0.4375	0.25	–0.93	0.3935
X3*X4	0.4375	0.25	0.93	0.3935
X5	0.3125	0.25	0.67	0.5345
X2*X3	0.3125	0.25	0.67	0.5345
X1*X3	0.1875	0.25	0.40	0.7057
X3*X5	0.1875	0.25	0.40	0.7057
X2*X4	–0.0625	0.25	–0.13	0.8991
X2*X5	–0.0625	0.25	–0.13	0.8991



No error degrees of freedom, so ordinary tests uncomputable. Relative Std Error corresponds to residual standard error of 1. Pseudo t-Ratio and p-Value calculated using Lenth PSE = 0.46875 and DFE = 5

Sequences of Fractional Factorials. Using fractional factorial designs often leads to great economy and efficiency in experimentation, particularly if the runs can be made **sequentially**. For example, suppose that we are investigating $k = 4$ factors ($2^4 = 16$ runs). It is almost always preferable to run a 2_{IV}^{4-1} fractional design (eight runs), analyze the results, and then decide on the best set of runs to perform next. If it is necessary to resolve ambiguities, we can always run the alternate fraction and complete the 2^4 design. When this method is used to complete the design, both one-half fractions represent **blocks** of the complete design with the highest order interaction confounded with blocks (here *ABCD* would be confounded). Thus, sequential experimentation has the result of losing information only on the highest order interaction. Its advantage is that in many cases we learn enough from the one-half fraction to proceed to the next stage of experimentation, which might involve adding or removing factors, changing responses, or varying some of the factors over new ranges. Some of these possibilities are illustrated graphically in Figure 8.11.



■ **FIGURE 8.11 Possibilities for follow-up experimentation after an initial fractional factorial experiment**

EXAMPLE 8.3

Reconsider the experiment in Example 8.1. We have used a 2^{4-1}_{IV} design and tentatively identified three large main effects—A, C, and D. There are two large effects associated with two-factor interactions, $AC + BD$ and $AD + BC$. In Example 8.2, we used the fact that the main effect of B was negligible to tentatively conclude that the important interactions were AC and AD. Sometimes the experimenter will have process knowledge that can assist in discriminating between interactions likely to be important. However, we can always isolate the significant interaction by running the alternate fraction, given by $I = -ABCD$. It is straightforward to show that the design and the responses are as follows:

Run	Basic Design *A*	*B*	*C*	$D = -ABC$	Treatment Combination	Filtration Rate
1	−	−	−	+	*d*	43
2	+	−	−	−	*a*	71

Run	Basic Design A	B	C	$D = -ABC$	Treatment Combination	Filtration Rate
3	−	+	−	−	b	48
4	+	+	−	+	abd	104
5	−	−	+	−	c	68
6	+	−	+	+	acd	86
7	−	+	+	+	bcd	70
8	+	+	+	−	abc	65

The effect estimates (and their aliases) obtained from this alternate fraction are

$$
\begin{aligned}
[A]' &= 24.25 \rightarrow A - BCD \\
[B]' &= 4.75 \rightarrow B - ACD \\
[C]' &= 5.75 \rightarrow C - ABD \\
[D]' &= 12.75 \rightarrow D - ABC \\
[AB]' &= 1.25 \rightarrow AB - CD \\
[AC]' &= -17.75 \rightarrow AC - BD \\
[AD]' &= 14.25 \rightarrow AD - BC
\end{aligned}
$$

These estimates may be combined with those obtained from the original one-half fraction to yield the following estimates of the effects:

i	From $\frac{1}{2}([i] + [i]')$	From $\frac{1}{2}([i] - [i]')$
A	$21.63 \rightarrow A$	$-2.63 \rightarrow BCD$
B	$3.13 \rightarrow B$	$-1.63 \rightarrow ACD$
C	$9.88 \rightarrow C$	$4.13 \rightarrow ABD$
D	$14.63 \rightarrow D$	$1.88 \rightarrow ABC$
AB	$10.13 \rightarrow AB$	$-1.13 \rightarrow CD$
AC	$-18.13 \rightarrow AC$	$-0.38 \rightarrow BD$
AD	$16.63 \rightarrow AD$	$2.38 \rightarrow BC$

These estimates agree exactly with those from the original analysis of the data as a single replicate of a 2^4 factorial design, as reported in Example 6.2. Clearly, it is the AC and AD interactions that are large.

Confirmation Experiments. Adding the alternate fraction to the principal fraction may be thought of as a type of **confirmation experiment** in that it provides information that will allow us to strengthen our initial conclusions about the two-factor interaction effects. We will investigate some other aspects of combining fractional factorials to isolate interactions in Sections 8.5 and 8.6.

A confirmation experiment need not be this elaborate. A very simple confirmation experiment is to use the model equation to predict the response at a point of interest in the design space (this should not be one of the runs in the current design) and then actually run that treatment combination (perhaps several times), comparing the predicted and observed responses. Reasonably close agreement indicates that the interpretation of the fractional factorial was correct, whereas serious discrepancies mean that the interpretation was problematic. This would be an indication that additional experimentation is required to resolve ambiguities.

To illustrate, consider the 2^{4-1} fractional factorial in Example 8.1. The experimenters are interested in finding a set of conditions where the response variable filtration rate is high, but low concentrations of formaldehyde (factor C) are desirable. This would suggest that factors A and D should be at the high level and factor C should be at the low level. Examining Figure 8.3, we note that when B is at the low level, this treatment combination was run in the fractional factorial, producing an observed response of 100. The treatment combination with B at the high level was not in the original fraction, so this would be a reasonable confirmation run. With A, B, and D at the high level and C at the low level, we use the model equation from Example 8.1 to calculate the predicted response as follows:

$$\begin{aligned}
\hat{y} &= 70.75 + \left(\frac{19.00}{2}\right)x_1 + \left(\frac{14.00}{2}\right)x_3 + \left(\frac{16.50}{2}\right)x_4 + \left(\frac{-18.50}{2}\right)x_1x_3 + \left(\frac{19.00}{2}\right)x_1x_4 \\
&= 70.75 + \left(\frac{19.00}{2}\right)(1) + \left(\frac{14.00}{2}\right)(-1) + \left(\frac{16.50}{2}\right)(1) + \left(\frac{-18.50}{2}\right)(1)(-1) \\
&\quad + \left(\frac{19.00}{2}\right)(1)(1) \\
&= 100.25
\end{aligned}$$

The observed response at this treatment combination is 104 (refer to Figure 6.10 where the response data for the complete 2^4 factorial design are presented). Since the observed and predicted values of filtration rate are very similar, we have a successful confirmation run. This is additional evidence that our interpretation of the fractional factorial was correct.

There will be situations where the predicted and observed values in a confirmation experiment will not be this close together, and it will be necessary to answer the question of whether the two values are sufficiently close to reasonably conclude that the interpretation of the fractional design was correct. One way to answer this question is to construct a **prediction interval** on the future observation for the confirmation run and then see if the actual observation falls inside the prediction interval. We show how to do this using this example in Section 10.6, where prediction intervals for a regression model are introduced.

8.3 The One-Quarter Fraction of the 2^k Design

For a moderately large number of factors, smaller fractions of the 2^k design are frequently useful. Consider a one-quarter fraction of the 2^k design. This design contains 2^{k-2} runs and is usually called a **2^{k-2} fractional factorial**.

The 2^{k-2} design may be constructed by first writing down a **basic design** consisting of the runs associated with a full factorial in $k-2$ factors and then associating the two additional columns with appropriately chosen interactions involving the first $k-2$ factors. Thus, a one-quarter fraction of the 2^k design has two generators. If P and Q represent the generators chosen, then $I = P$ and $I = Q$ are called the **generating relations** for the design. The signs of P and Q (either + or −) determine which one of the one-quarter fractions is produced. All four fractions associated with the choice of **generators** $\pm P$ and $\pm Q$ are members of the same **family**. The fraction for which both P and Q are positive is the principal fraction.

The **complete defining relation** for the design consists of all the columns that are equal to the identity column I. These will consist of P, Q, and their **generalized interaction** PQ; that is, the defining relation is $I = P = Q = PQ$. We call the elements P, Q, and PQ in the defining relation **words**. The aliases of any effect are produced by the multiplication of the column for that effect by each word in the defining relation. Clearly, each effect has *three* aliases. The experimenter should be careful in choosing the generators so that potentially important effects are not aliased with each other.

As an example, consider the 2^{6-2} design. Suppose we choose $I = ABCE$ and $I = BCDF$ as the design generators. Now the generalized interaction of the generators $ABCE$ and $BCDF$ is $ADEF$; therefore, the complete defining relation for this design is

$$I = ABCE = BCDF = ADEF$$

■ **TABLE 8.8**
Alias Structure for the 2_{IV}^{6-2} Design with $I = ABCE = BCDF = ADEF$

$A = BCE = DEF = ABCDF$	$AB = CE = ACDF = BDEF$
$B = ACE = CDF = ABDEF$	$AC = BE = ABDF = CDEF$
$C = ABE = BDF = ACDEF$	$AD = EF = BCDE = ABCF$
$D = BCF = AEF = ABCDE$	$AE = BC = DF = ABCDEF$
$E = ABC = ADF = BCDEF$	$AF = DE = BCEF = ABCD$
$F = BCD = ADE = ABCEF$	$BD = CF = ACDE = ABEF$
	$BF = CD = ACEF = ABDE$
$ABD = CDE = ACF = BEF$	
$ACD = BDE = ABF = CEF$	

Consequently, this is a resolution IV design. To find the aliases of any effect (e.g., A), multiply that effect by each word in the defining relation. For A, this produces

$$A = BCE = ABCDF = DEF$$

It is easy to verify that every main effect is aliased by three- and five-factor interactions, whereas two-factor interactions are aliased with each other and with higher order interactions. Thus, when we estimate A, for example, we are really estimating $A + BCE + DEF + ABCDF$. The complete alias structure of this design is shown in Table 8.8. If three-factor and higher interactions are negligible, this design gives clear estimates of the main effects.

To construct the design, first write down the **basic design**, which consists of the 16 runs for a full $2^{6-2} = 2^4$ design in A, B, C, and D. Then the two factors E and F are added by associating their plus and minus levels with the plus and minus signs of the interactions ABC and BCD, respectively. This procedure is shown in Table 8.9.

Another way to construct this design is to derive the four blocks of the 2^6 design with $ABCE$ and $BCDF$ confounded and then choose the block with treatment combinations that are positive on $ABCE$ and $BCDF$. This would be a 2^{6-2} fractional factorial with generating relations $I = ABCE$ and $I = BCDF$, and because both generators $ABCE$ and $BCDF$ are positive, this is the principal fraction.

There are, of course, three **alternate fractions** of this particular 2_{IV}^{6-2} design. They are the fractions with generating relationships $I = ABCE$ and $I = -BCDF$; $I = -ABCE$ and $I = BCDF$; and $I = -ABCE$ and $I = -BCDF$. These fractions may be easily constructed by the method shown in Table 8.9. For example, if we wish to find the fraction for which $I = ABCE$ and $I = -BCDF$, then in the last column of Table 8.9 we set $F = -BCD$, and the column of levels for factor F becomes

$$+ + - - - - + + - - + + + + - -$$

The complete defining relation for this alternate fraction is $I = ABCE = -BCDF = -ADEF$. Certain signs in the alias structure in Table 8.9 are now changed; for instance, the aliases of A are $A = BCE = -DEF = -ABCDF$. Thus, the linear combination of the observations $[A]$ actually estimates $A + BCE - DEF - ABCDF$.

Finally, note that the 2_{IV}^{6-2} fractional factorial will project into a single replicate of a 2^4 design in any subset of four factors that is not a word in the defining relation. It also collapses to a replicated one-half fraction of a 2^4 in any subset of four factors that is a word in the defining relation. Thus, the design in Table 8.9 becomes two replicates of a 2^{4-1} in the factors $ABCE$, $BCDF$, and $ADEF$, because these are the words in the defining relation. There are 12 other

■ **TABLE 8.9**
Construction of the 2_{IV}^{6-2} Design with the Generators $I = ABCE$ and $I = BCDF$

	Basic Design					
Run	***A***	***B***	***C***	***D***	***E = ABC***	***F = BCD***
1	−	−	−	−	−	−
2	+	−	−	−	+	−
3	−	+	−	−	+	+
4	+	+	−	−	−	+
5	−	−	+	−	+	+
6	+	−	+	−	−	+
7	−	+	+	−	−	−
8	+	+	+	−	+	−
9	−	−	−	+	−	+
10	+	−	−	+	+	+
11	−	+	−	+	+	−
12	+	+	−	+	−	−
13	−	−	+	+	+	−
14	+	−	+	+	−	−
15	−	+	+	+	−	+
16	+	+	+	+	+	+

combinations of the six factors, such as *ABCD, ABCF*, for which the design projects to a single replicate of the 2^4. This design also collapses to two replicates of a 2^3 in *any* subset of three of the six factors or four replicates of a 2^2 in any subset of two factors.

In general, any 2^{k-2} fractional factorial design can be collapsed into either a full factorial or a fractional factorial in some subset of $r \leq k - 2$ of the original factors. Those subsets of variables that form full factorials are not words in the complete defining relation.

EXAMPLE 8.4

Parts manufactured in an injection molding process are showing excessive shrinkage. This is causing problems in assembly operations downstream from the injection molding area. A quality improvement team has decided to use a designed experiment to study the injection molding process so that shrinkage can be reduced. The team decides to investigate six factors—mold temperature (A), screw speed (B), holding time (C), cycle time (D), gate size (E), and holding pressure (F)—each at two levels, with the objective of learning how each factor affects shrinkage and also something about how the factors interact.

The team decides to use the 16-run two-level fractional factorial design in Table 8.9. The design is shown again in Table 8.10, along with the observed shrinkage (×10) for the test part produced at each of the 16 runs in the design. Table 8.11 shows the effect estimates, sums of squares, and the regression coefficients for this experiment.

■ **TABLE 8.10**

A 2^{6-2}_{IV} Design for the Injection Molding Experiment in Example 8.4

	Basic Design							
Run	***A***	***B***	***C***	***D***	***E = ABC***	***F = BCD***	**Observed Shrinkage (× 10)**	
1	−	−	−	−	−	−	6	(1)
2	+	−	−	−	+	−	10	*ae*
3	−	+	−	−	+	+	32	*bef*
4	+	+	−	−	−	+	60	*abf*
5	−	−	+	−	+	+	4	*cef*
6	+	−	+	−	−	+	15	*acf*
7	−	+	+	−	−	−	26	*bc*
8	+	+	+	−	+	−	60	*abce*
9	−	−	−	+	−	+	8	*df*
10	+	−	−	+	+	+	12	*adef*
11	−	+	−	+	+	−	34	*bde*
12	+	+	−	+	−	−	60	*abd*
13	−	−	+	+	+	−	16	*cde*
14	+	−	+	+	−	−	5	*acd*
15	−	+	+	+	−	+	37	*bcdf*
16	+	+	+	+	+	+	52	*abcdef*

■ **TABLE 8.11**

Effects, Sums of Squares, and Regression Coefficients for Example 8.4

Variable	**Name**	**−1 Level**	**+1 Level**
A	Mold temperature	−1.000	1.000
B	Screw speed	−1.000	1.000
C	Holding time	−1.000	1.000
D	Cycle time	−1.000	1.000
E	Gate size	−1.000	1.000
F	Hold pressure	−1.000	1.000

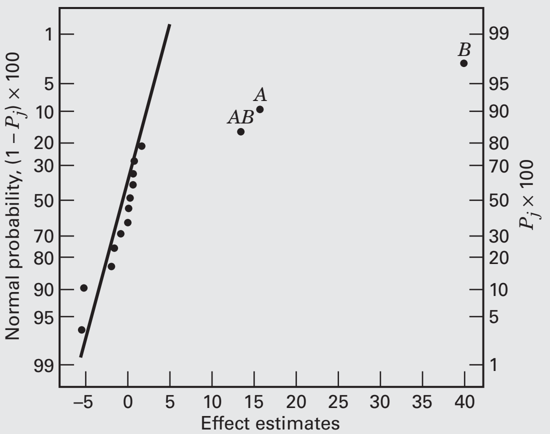
Variable[a]	**Regression Coefficient**	**Estimated Effect**	**Sum of Squares**
Overall average	27.3125		
A	6.9375	13.8750	770.062
B	17.8125	35.6250	5076.562
C	−0.4375	−0.8750	3.063
D	0.6875	1.3750	7.563
E	0.1875	0.3750	0.563
F	0.1875	0.3750	0.563

■ **TABLE 8.11** (*Continued*)

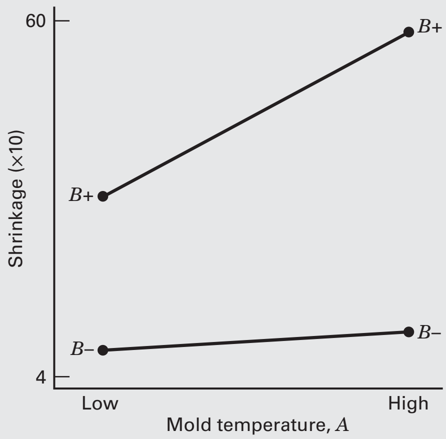
Variable[a]	Regression Coefficient	Estimated Effect	Sum of Squares
$AB + CE$	5.9375	11.8750	564.063
$AC + BE$	−0.8125	−1.6250	10.562
$AD + EF$	−2.6875	−5.3750	115.562
$AE + BC + DF$	−0.9375	−1.8750	14.063
$AF + DE$	0.3125	0.6250	1.563
$BD + CF$	−0.0625	−0.1250	0.063
$BF + CD$	−0.0625	−0.1250	0.063
ABD	0.0625	0.1250	0.063
ABF	−2.4375	−4.8750	95.063

[a] Only main effects and two-factor interactions.

A normal probability plot of the effect estimates from this experiment is shown in Figure 8.12. The only large effects are *A* (mold temperature), *B* (screw speed), and the *AB* interaction. In light of the alias relationships in Table 8.8, it seems reasonable to adopt these conclusions tentatively. The plot of the *AB* interaction in Figure 8.13 shows that the process is very insensitive to temperature if the screw speed is at the low level but very sensitive to temperature if the screw speed is at the high level. With the screw speed at the low level, the process should produce an average shrinkage of around 10 percent regardless of the temperature level chosen.

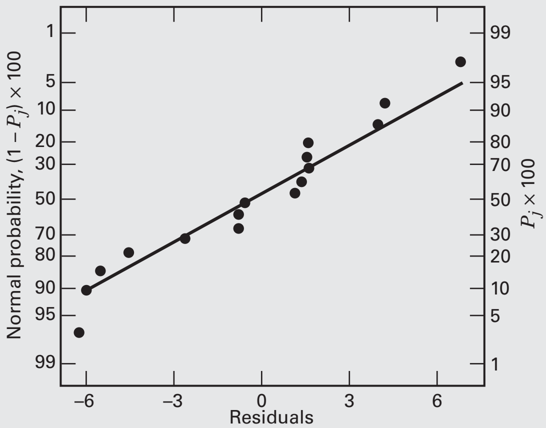


■ **FIGURE 8.12 Normal probability plot of effects for Example 8.4**

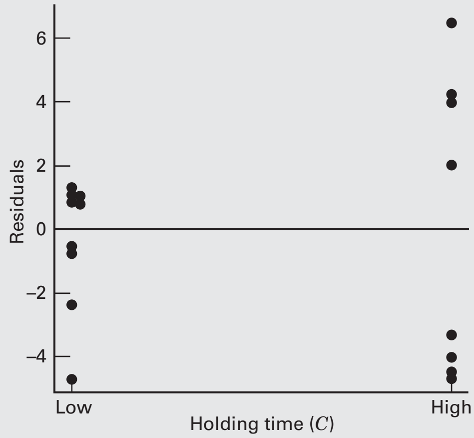


■ **FIGURE 8.13 Plot of *AB* (mold temperature-screw speed) interaction for Example 8.4**

Based on this initial analysis, the team decides to set both the mold temperature and the screw speed at the low level. This set of conditions will reduce the *mean* shrinkage of parts to around 10 percent. However, the variability in shrinkage from part to part is still a potential problem. In effect, the mean shrinkage can be adequately reduced by the above modifications; however, the part-to-part variability in shrinkage over a production run could still cause problems in assembly. One way to address this issue is to see if any of the process factors affect the *variability* in parts shrinkage.



■ **FIGURE 8.14 Normal probability plot of residuals for Example 8.4**



■ **FIGURE 8.15 Residuals versus holding time (*C*) for Example 8.4**

Figure 8.14 presents the normal probability plot of the residuals. This plot appears satisfactory. The plots of residuals versus each factor were then constructed. One of these plots, that for residuals versus factor *C* (holding time), is shown in Figure 8.15. The plot reveals that there is much less scatter in the residuals at the low holding time than at the high holding time. These residuals were obtained in the usual way from a model for predicted shrinkage:

$$\begin{aligned}\hat{y} &= \hat{\beta}_0 + \hat{\beta}_1 x_1 + \hat{\beta}_2 x_2 + \hat{\beta}_{12} x_1 x_2 \\ &= 27.3125 + 6.9375 x_1 + 17.8125 x_2 + 5.9375 x_1 x_2\end{aligned}$$

where x_1, x_2, and x_1x_2 are coded variables that correspond to the factors *A* and *B* and the *AB* interaction. The residuals are then

$$e = y - \hat{y}$$

The regression model used to produce the residuals essentially removes the **location effects** of *A*, *B*, and *AB* from the data; the residuals therefore contain information about unexplained variability. Figure 8.15 indicates that there is a *pattern* in the variability and that the variability in the shrinkage of parts may be smaller when the holding time is at the low level. (Please recall that we observed in Chapter 6 that residuals only convey information about dispersion effects when the location or mean model is correct.)

This is further amplified by the analysis of residuals shown in Table 8.12. In this table, the residuals are arranged at the low (−) and high (+) levels of each factor, and the standard deviations of the residuals at the low and high levels of each factor have been calculated. Note that the standard deviation of the residuals with *C* at the low level $[S(C^-) = 1.63]$ is considerably smaller than the standard deviation of the residuals with *C* at the high level $[S(C^+) = 5.70]$.

The bottom line of Table 8.12 presents the statistic

$$F_i^* = \ln \frac{S^2(i^+)}{S^2(i^-)}$$

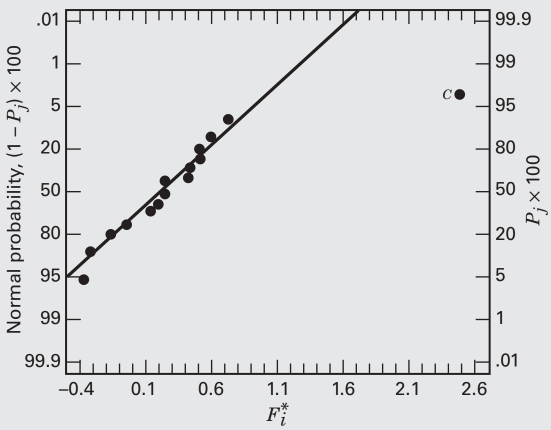
Recall that if the variances of the residuals at the high (+) and low (−) levels of factor *i* are equal, then this ratio is approximately normally distributed with mean zero, and it can be used to judge the difference in the response variability at the two levels of factor *i*. Because the ratio F_C^* is relatively large, we would conclude that the apparent **dispersion** or **variability effect** observed in Figure 8.15 is real. Thus, setting the holding time at its low level would contribute to reducing the variability in shrinkage from part to part during a production run. Figure 8.16 presents a normal probability plot of the F_i^* values in Table 8.12; this also indicates that factor *C* has a large dispersion effect.

Figure 8.17 shows the data from this experiment projected onto a cube in the factors *A*, *B*, and *C*. The average observed shrinkage and the range of observed shrinkage are shown at each corner of the cube. From inspection of this figure, we see that running the process with the screw speed (*B*) at the low level is the key to reducing average parts shrinkage. If *B* is low, virtually any combination of temperature (*A*) and holding time (*C*) will result in low values of average parts shrinkage. However, from examining the ranges of the shrinkage values at each corner of the cube, it is immediately clear that setting the holding time (*C*) at the low level is the only reasonable choice if we wish to keep the part-to-part variability in shrinkage low during a production run.

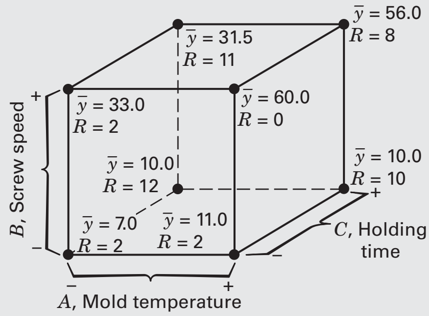
■ TABLE 8.12

Calculation of Dispersion Effects for Example 8.4

Run	A	B	$AB = CE$	C	$AC = BE$	$AE = BC = DF$	E	D	$AD = EF$	$BD = CE$	ABD	$BF = CD$	ACD	F	$AF = DE$	Residual
1	−	−	+	−	+	+	−	−	+	+	−	+	−	−	+	−2.50
2	+	−	−	−	−	+	+	−	−	+	+	+	+	−	−	−0.50
3	−	+	−	−	+	−	+	−	+	−	+	+	−	+	−	−0.25
4	+	+	+	−	−	−	−	−	−	−	−	+	+	+	+	2.00
5	−	−	+	+	−	−	+	−	+	+	−	−	+	+	−	−4.50
6	+	−	−	+	+	−	−	−	−	+	+	−	−	+	+	4.50
7	−	+	−	+	−	+	−	−	+	−	+	−	+	−	+	−6.25
8	+	+	+	+	+	+	+	−	−	−	−	−	−	−	−	2.00
9	−	−	+	−	+	+	−	+	−	−	+	−	+	+	−	−0.50
10	+	−	−	−	−	+	+	+	+	−	−	−	−	+	+	1.50
11	−	+	−	−	+	−	+	+	−	+	−	−	+	−	+	1.75
12	+	+	+	−	−	−	−	+	+	+	+	−	−	−	−	2.00
13	−	−	+	+	−	−	+	+	−	−	+	+	−	−	+	7.50
14	+	−	−	+	+	−	−	+	+	−	−	+	+	−	−	−5.50
15	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−	4.75
16	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	−6.00
$S(i^+)$	3.80	4.01	4.33	5.70	3.68	3.85	4.17	4.64	3.39	4.01	4.72	4.71	3.50	3.88	4.87	
$S(i^-)$	4.60	4.41	4.10	1.63	4.53	4.33	4.25	3.59	2.75	4.41	3.64	3.65	3.12	4.52	3.40	
F_i^*	−0.38	−0.19	0.11	2.50	−0.42	−0.23	−0.04	0.51	0.42	−0.19	0.52	0.51	0.23	−0.31	0.72	



■ **FIGURE 8.16 Normal probability plot of the dispersion effects F_i^* for Example 8.4**



■ **FIGURE 8.17 Average shrinkage and range of shrinkage in factors *A*, *B*, and *C* for Example 8.4**

8.4 The General 2^{k-p} Fractional Factorial Design

8.4.1 Choosing a Design

A 2^k fractional factorial design containing 2^{k-p} runs is called a $1/2^p$ fraction of the 2^k design or, more simply, a **2^{k-p} fractional factorial design**. These designs require the selection of p independent generators. The defining relation for the design consists of the p generators initially chosen and their $2^p - p - 1$ generalized interactions. In this section, we discuss the construction and analysis of these designs.

The alias structure may be found by multiplying each effect column by the defining relation. Care should be exercised in choosing the generators so that effects of potential interest are not aliased with each other. Each effect has $2^p - 1$ aliases. For moderately large values of k, we usually assume higher order interactions (say, third- or fourth-order and higher) to be negligible, and this greatly simplifies the alias structure.

It is important to select the p generators for a 2^{k-p} fractional factorial design in such a way that we obtain the best possible alias relationships. A reasonable criterion is to select the generators such that the resulting 2^{k-p} design has the **highest possible resolution**. To illustrate, consider the 2^{6-2}_{IV} design in Table 8.9, where we used the generators $E = ABC$ and $F = BCD$, thereby producing a design of resolution IV. This is the maximum resolution design. If we had selected $E = ABC$ and $F = ABCD$, the complete defining relation would have been $I = ABCE = ABCDF = DEF$, and the design would be of resolution III. Clearly, this is an inferior choice because it needlessly sacrifices information about interactions.

Sometimes resolution alone is insufficient to distinguish between designs. For example, consider the three 2^{7-2}_{IV} designs in Table 8.13. All of these designs are of resolution IV, but they have rather different alias structures (we have assumed that three-factor and higher interactions are negligible) with respect to the two-factor interactions. Clearly, design A has more extensive aliasing and design C the least, so design C would be the best choice for a 2^{7-2}_{IV}.

The three word lengths in design A are all 4; that is, the word length **pattern** is {4, 4, 4}. For design B it is {4, 4, 6}, and for design C it is {4, 5, 5}. Notice that the defining relation for design C has only one four-letter word, whereas the other designs have two or three. Thus, design C minimizes the number of words in the defining relation that are of minimum length. We call such a design a **minimum aberration design**. Minimizing aberration in a design of resolution R ensures that the design has the minimum number of main effects aliased with interactions of order

■ **TABLE 8.13**
Three Choices of Generators for the 2_{IV}^{7-2} Design

Design *A* Generators: $F = ABC$, $G = BCD$ $I = ABCF = BCDG = ADFG$	Design *B* Generators: $F = ABC$, $G = ADE$ $I = ABCF = ADEG = BCDEFG$	Design *C* Generators: $F = ABCD$, $G = ABDE$ $I = ABCDF = ABDEG = CEFG$
Aliases (two-factor interactions)	Aliases (two-factor interactions)	Aliases (two-factor interactions)
$AB = CF$	$AB = CF$	$CE = FG$
$AC = BF$	$AC = BF$	$CF = EG$
$AD = FG$	$AD = EG$	$CG = EF$
$AG = DF$	$AE = DG$	
$BD = CG$	$AF = BC$	
$BG = CD$	$AG = DE$	
$AF = BC = DG$		

$R - 1$, the minimum number of two-factor interactions aliased with interactions of order $R - 2$, and so forth. Refer to Fries and Hunter (1980) for more details.

Table 8.14 presents a selection of 2^{k-p} fractional factorial designs for $k \leq 15$ factors and up to $n \leq 128$ runs. The suggested generators in this table will result in a design of the highest possible resolution. These are also the minimum aberration designs.

The alias relationships for all of the designs in Table 8.14 for which $n \leq 64$ are given in Appendix Table VIII(a–w). The alias relationships presented in this table focus on main effects and two- and three-factor interactions. The complete defining relation is given for each design. This appendix table makes it very easy to select a design of sufficient resolution to ensure that any interactions of potential interest can be estimated.

EXAMPLE 8.5

To illustrate the use of Table 8.14, suppose that we have seven factors and that we are interested in estimating the seven main effects and getting some insight regarding the two-factor interactions. We are willing to assume that three-factor and higher interactions are negligible. This information suggests that a resolution IV design would be appropriate.

Table 8.14 shows that there are two resolution IV fractions available: the 2_{IV}^{7-2} with 32 runs and the 2_{IV}^{7-3} with 16 runs. Appendix Table VIII contains the complete alias relationships for these two designs. The aliases for the 2_{IV}^{7-3} 16-run design are in Appendix Table VIII(i). Notice that all seven main effects are aliased with three-factor interactions. The two-factor interactions are all aliased in groups of three. Therefore, this design will satisfy our objectives; that is, it will allow the estimation of the main effects, and it will give some insight regarding two-factor interactions. It is not necessary to run the 2_{IV}^{7-2} design, which would require 32 runs. Appendix Table VIII(j) shows that this design would allow the estimation of all seven main effects and that 15 of the 21 two-factor interactions could also be uniquely estimated. (Recall that three-factor and higher interactions are negligible.) This is probably more information about interactions than is necessary. The complete 2_{IV}^{7-3} design is shown in Table 8.15. Notice that it was constructed by starting with the 16-run 2^4 design in A, B, C, and D as the basic design and then adding the three columns $E = ABC$, $F = BCD$, and $G = ACD$. The generators are $I = ABCE, I = BCDF$, and $I = ACDG$ (Table 8.14). The complete defining relation is $I = ABCE = BCDF = ADEF = ACDG = BDEG = CEFG = ABFG$.

(*Continued on* p. 354)

■ TABLE 8.14
Selected 2^{k-p} Fractional Factorial Designs

Number of Factors, k	Fraction	Number of Runs	Design Generators	Number of Factors, k	Fraction	Number of Runs	Design Generators	Number of Factors, k	Fraction	Number of Runs	Design Generators
3	2^{3-1}_{III}	4	$C = \pm AB$		2^{9-5}_{III}	16	$E = \pm ABC$				$L = \pm AC$
4	2^{4-1}_{IV}	8	$D = \pm ABC$				$F = \pm BCD$	12	2^{12-8}_{III}	16	$E = \pm ABC$
5	2^{5-1}_{V}	16	$E = \pm ABCD$				$G = \pm ACD$				$F = \pm ABD$
	2^{5-2}_{III}	8	$D = \pm AB$				$H = \pm ABD$				$G = \pm ACD$
			$E = \pm AC$				$J = \pm ABCD$				$H = \pm BCD$
6	2^{6-1}_{VI}	32	$F = \pm ABCDE$	10	2^{10-3}_{V}	128	$H = \pm ABCG$				$J = \pm ABCD$
	2^{6-2}_{IV}	16	$E = \pm ABC$				$J = \pm BCDE$				$K = \pm AB$
			$F = \pm BCD$				$K = \pm ACDF$				$L = \pm AC$
	2^{6-3}_{III}	8	$D = \pm AB$		2^{10-4}_{IV}	64	$G = \pm BCDF$				$M = \pm AD$
			$E = \pm AC$				$H = \pm ACDF$	13	2^{13-9}_{III}	16	$E = \pm ABC$
			$F = \pm BC$				$J = \pm ABDE$				$F = \pm ABD$
7	2^{7-1}_{VII}	64	$G = \pm ABCDEF$				$K = \pm ABCE$				$G = \pm ACD$
	2^{7-2}_{IV}	32	$F = \pm ABCD$		2^{10-5}_{IV}	32	$F = \pm ABCD$				$H = \pm BCD$
			$G = \pm ABDE$				$G = \pm ABCE$				$J = \pm ABCD$
	2^{7-3}_{IV}	16	$E = \pm ABC$				$H = \pm ABDE$				$K = \pm AB$
			$F = \pm BCD$				$J = \pm ACDE$				$L = \pm AC$
			$G = \pm ACD$				$K = \pm BCDE$				$M = \pm AD$
	2^{7-4}_{III}	8	$D = \pm AB$		2^{10-6}_{III}	16	$E = \pm ABC$				$N = \pm BC$
			$E = \pm AC$				$F = \pm BCD$	14	2^{14-10}_{III}	16	$E = \pm ABC$
			$F = \pm BC$				$G = \pm ACD$				$F = \pm ABD$
			$G = \pm ABC$				$H = \pm ABD$				$G = \pm ACD$
8	2^{8-2}_{V}	64	$G = \pm ABCD$				$J = \pm ABCD$				$H = \pm BCD$
			$H = \pm ABEF$				$K = \pm AB$				$J = \pm ABCD$
	2^{8-3}_{IV}	32	$F = \pm ABC$	11	2^{11-5}_{IV}	64	$G = \pm CDE$				$K = \pm AB$
			$G = \pm ABD$				$H = \pm ABCD$				$L = \pm AC$
			$H = \pm BCDE$				$J = \pm ABF$				$M = \pm AD$
	2^{8-4}_{IV}	16	$E = \pm BCD$				$K = \pm BDEF$				$N = \pm BC$
			$F = \pm ACD$				$L = \pm ADEF$				$O = \pm BD$
			$G = \pm ABC$		2^{11-6}_{IV}	32	$F = \pm ABC$	15	2^{15-11}_{III}	16	$E = \pm ABC$
			$H = \pm ABD$				$G = \pm BCD$				$F = \pm ABD$
9	2^{9-2}_{VI}	128	$H = \pm ACDFG$				$H = \pm CDE$				$G = \pm ACD$
			$J = \pm BCEFG$				$J = \pm ACD$				$H = \pm BCD$
	2^{9-3}_{IV}	64	$G = \pm ABCD$				$K = \pm ADE$				$J = \pm ABCD$
			$H = \pm ACEF$				$L = \pm BDE$				$K = \pm AB$
			$J = \pm CDEF$		2^{11-7}_{III}	16	$E = \pm ABC$				$L = \pm AC$
	2^{9-4}_{IV}	32	$F = \pm BCDE$				$F = \pm BCD$				$M = \pm AD$
			$G = \pm ACDE$				$G = \pm ACD$				$N = \pm BC$
			$H = \pm ABDE$				$H = \pm ABD$				$O = \pm BD$
			$J = \pm ABCE$				$J = \pm ABCD$				$P = \pm CD$
							$K = \pm AB$				

■ **TABLE 8.15**
A 2_{IV}^{7-3} Fractional Factorial Design

	Basic Design						
Run	A	B	C	D	$E = ABC$	$F = BCD$	$G = ACD$
1	−	−	−	−	−	−	−
2	+	−	−	−	+	−	+
3	−	+	−	−	+	+	−
4	+	+	−	−	−	+	+
5	−	−	+	−	+	+	+
6	+	−	+	−	−	+	−
7	−	+	+	−	−	−	+
8	+	+	+	−	+	−	−
9	−	−	−	+	−	+	+
10	+	−	−	+	+	+	−
11	−	+	−	+	+	−	+
12	+	+	−	+	−	−	−
13	−	−	+	+	+	−	−
14	+	−	+	+	−	−	+
15	−	+	+	+	−	+	−
16	+	+	+	+	+	+	+

8.4.2 Analysis of 2^{k-p} Fractional Factorials

There are many computer programs that can be used to analyze the 2^{k-p} fractional factorial design. For example, Design-Expert, JMP, and Minitab all have this capability.

The design may also be analyzed by resorting to first principles; the ith effect is estimated by

$$\text{Effect}_i = \frac{2(\text{Contrast}_i)}{N} = \frac{\text{Contrast}_i}{(N/2)}$$

where the Contrast_i is found using the plus and minus signs in column i and $N = 2^{k-p}$ is the total number of observations. The 2^{k-p} design allows only $2^{k-p} - 1$ effects (and their aliases) to be estimated. Normal probability plots of the effect estimates and Lenth's method are very useful analysis tools.

Projection of the 2^{k-p} Fractional Factorial. The 2^{k-p} design collapses into either a full factorial or a fractional factorial in any subset of $r \le k - p$ of the original factors. Those subsets of factors providing fractional factorials are subsets appearing as words in the complete defining relation. This is particularly useful in screening experiments when we suspect at the outset of the experiment that most of the original factors will have small effects. The original 2^{k-p} fractional factorial can then be projected into a full factorial, say, in the most interesting factors. Conclusions drawn from designs of this type should be considered tentative and subject to further analysis. It is usually possible to find alternative explanations of the data involving higher order interactions.

As an example, consider the 2_{IV}^{7-3} design from Example 8.5. This is a 16-run design involving seven factors. It will project into a full factorial in any four of the original seven factors that is not a word in the defining relation. There are 35 subsets of four factors, seven of which appear in the complete defining relation (see Table 8.15). Thus, there are 28 subsets of four factors that would form 2^4 designs. One combination that is obvious upon inspecting Table 8.15 is A, B, C, and D.

To illustrate the usefulness of this projection properly, suppose that we are conducting an experiment to improve the efficiency of a ball mill and the seven factors are as follows:

1. Motor speed
2. Gain
3. Feed mode
4. Feed sizing
5. Material type
6. Screen angle
7. Screen vibration level

We are fairly certain that motor speed, feed mode, feed sizing, and material type will affect efficiency and that these factors may interact. The role of the other three factors is less well known, but it is likely that they are negligible. A reasonable strategy would be to assign motor speed, feed mode, feed sizing, and material type to columns *A, B, C*, and *D*, respectively, in Table 8.15. Gain, screen angle, and screen vibration level would be assigned to columns *E, F*, and *G*, respectively. If we are correct and the "minor variables" *E, F*, and *G* are negligible, we will be left with a full 2^4 design in the key process variables.

8.4.3 Blocking Fractional Factorials

Occasionally, a fractional factorial design requires so many runs that all of them cannot be made under homogeneous conditions. In these situations, fractional factorials may be confounded in blocks. Appendix Table VIII contains recommended blocking arrangements for many of the fractional factorial designs in Table 8.14. The minimum block size for these designs is eight runs.

To illustrate the general procedure, consider the 2_{IV}^{6-2} fractional factorial design with the defining relation $I = ABCE = BCDF = ADEF$ shown in Table 8.10. This fractional design contains 16 treatment combinations. Suppose we wish to run the design in two blocks of eight treatment combinations each. In selecting an interaction to confound with blocks, we note from examining the alias structure in Appendix Table VIII(f) that there are two alias sets involving only three-factor interactions. The table suggests selecting *ABD* (and its aliases) to be confounded with blocks. This would give the two blocks shown in Figure 8.18. Notice that the principal block contains those treatment combinations that have an even number of letters in common with *ABD*. These are also the treatment combinations for which $L = x_1 + x_2 + x_4 = 0 \pmod 2$.

Block 1	Block 2
(1)	*ae*
abf	*acf*
cef	*bef*
abce	*bc*
adef	*df*
bde	*abd*
acd	*cde*
bcdf	*abcdef*

■ **FIGURE 8.18** **The 2_{IV}^{6-2} design in two blocks with *ABD* confounded**

EXAMPLE 8.6

A five-axis CNC machine is used to produce an impeller for a jet turbine engine. The blade profiles are an important quality characteristic. Specifically, the deviation of the blade profile from the profile specified on the engineering drawing is of interest. An experiment is run to determine which machine parameters affect profile deviation. The eight factors selected for the design are as follows:

Factor	Low Level (−)	High Level (+)
$A = x$-Axis shift (0.001 in.)	0	15
$B = y$-Axis shift (0.001 in.)	0	15
$C = z$-Axis shift (0.001 in.)	0	15
$D =$ Tool supplier	1	2
$E = a$-Axis shift (0.001 deg)	0	30
$F =$ Spindle speed (%)	90	110
$G =$ Fixture height (0.001 in.)	0	15
$H =$ Feed rate (%)	90	110

One test blade on each part is selected for inspection. The profile deviation is measured using a coordinate measuring machine, and the standard deviation of the difference between the actual profile and the specified profile is used as the response variable.

The machine has four spindles. Because there may be differences in the spindles, the process engineers feel that the spindles should be treated as blocks.

The engineers feel confident that three-factor and higher interactions are not too important, but they are reluctant to ignore the two-factor interactions. From Table 8.14, two designs initially appear appropriate: the 2^{8-4}_{IV} design with 16 runs and the 2^{8-3}_{IV} design with 32 runs. Appendix Table VIII(l) indicates that if the 16-run design is used, there will be fairly extensive aliasing of two-factor interactions. Furthermore, this design cannot be run in four blocks without confounding four two-factor interactions with blocks. Therefore, the experimenters decide to use the 2^{8-3}_{IV} design in four blocks. This confounds one three-factor interaction alias chain and one two-factor interaction (*EH*) and its three-factor interaction aliases with blocks. The *EH* interaction is the interaction between the *a*-axis shift and the feed rate, and the engineers consider an interaction between these two variables to be fairly unlikely.

Table 8.16 contains the design and the resulting responses as standard deviation $\times 10^3$ in.. Because the response variable is a standard deviation, it is often best to perform the analysis following a log transformation. The effect estimates are shown in Table 8.17. Figure 8.19 is a normal probability plot of the effect estimates, using ln (standard deviation $\times 10^3$) as the response variable. The only large effects are $A = x$-axis shift, $B = y$-axis shift, and the alias chain involving $AD + BG$. Now AD is the x-axis shift-tool supplier interaction, and BG is the y-axis shift-fixture height interaction, and since these two interactions are aliased it is impossible to separate them based on the data from the current experiment. Since both interactions involve one large main effect it is also difficult to apply any "obvious" simplifying logic such as effect heredity to the situation either. If there is some engineering knowledge or process knowledge available that sheds light on the situation, then perhaps a choice could be made between the two interactions; otherwise, more data will be required to separate these two effects. (The problem of adding runs to a fractional factorial to de-alias interactions is discussed in Sections 8.6 and 8.7.)

Suppose that process knowledge suggests that the appropriate interaction is likely to be *AD*. Table 8.18 is the resulting analysis of variance for the model with factors *A, B, D*, and *AD* (factor *D* was included to preserve the hierarchy principle). Notice that the block effect is small, suggesting that the machine spindles are not very different.

Figure 8.20 is a normal probability plot of the residuals from this experiment. This plot is suggestive of slightly heavier than normal tails, so possibly other transformations should be considered. The *AD* interaction plot is in Figure 8.21. Notice that tool supplier (*D*) and the magnitude of the *x*-axis shift (*A*) have a profound impact on the variability of the blade profile from design specifications. Running *A* at the low level (0 offset) and buying tools from supplier 1 gives the best results. Figure 8.22 shows the projection of this 2^{8-3}_{IV} design into four replicates of a 2^3 design in factors *A*, *B*, and *D*. The best combination of operating conditions is *A* at the low level (0 offset), *B* at the high level (0.015 in offset), and *D* at the low level (tool supplier 1).

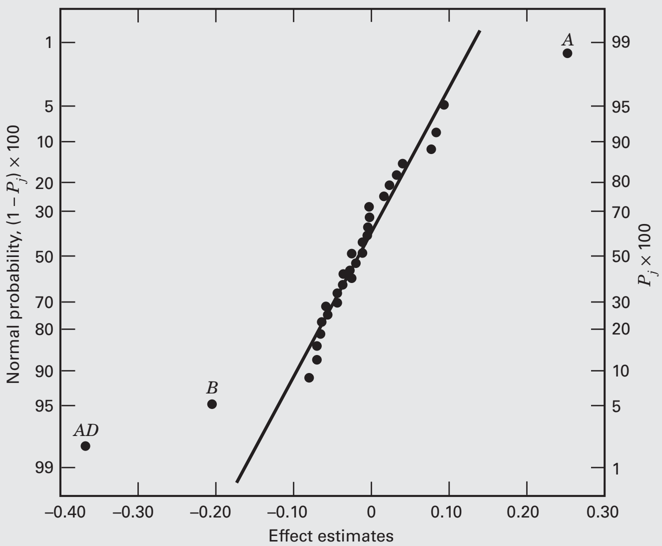
■ **TABLE 8.16**
The 2^{8-3} Design in Four Blocks for Example 8.6

Run	Basic Design A	B	C	D	E	F = ABC	G = ABD	H = BCDE	Block	Actual Run Order	Standard Deviation (× 10³ in.)
1	−	−	−	−	−	−	−	+	3	18	2.76
2	+	−	−	−	−	+	+	+	2	16	6.18
3	−	+	−	−	−	+	+	−	4	29	2.43
4	+	+	−	−	−	−	−	−	1	4	4.01
5	−	−	+	−	−	+	−	−	1	6	2.48
6	+	−	+	−	−	−	+	−	4	26	5.91
7	−	+	+	−	−	−	+	+	2	14	2.39
8	+	+	+	−	−	+	−	+	3	22	3.35
9	−	−	−	+	−	−	+	−	1	8	4.40
10	+	−	−	+	−	+	−	−	4	32	4.10
11	−	+	−	+	−	+	−	+	2	15	3.22
12	+	+	−	+	−	−	+	+	3	19	3.78
13	−	−	+	+	−	+	+	+	3	24	5.32
14	+	−	+	+	−	−	−	+	2	11	3.87
15	−	+	+	+	−	−	−	−	4	27	3.03
16	+	+	+	+	−	+	+	−	1	3	2.95
17	−	−	−	−	+	−	−	−	2	10	2.64
18	+	−	−	−	+	+	+	−	3	21	5.50
19	−	+	−	−	+	+	+	+	1	7	2.24
20	+	+	−	−	+	−	−	+	4	28	4.28
21	−	−	+	−	+	+	−	+	4	30	2.57
22	+	−	+	−	+	−	+	+	1	2	5.37
23	−	+	+	−	+	−	+	−	3	17	2.11
24	+	+	+	−	+	+	−	−	2	13	4.18
25	−	−	−	+	+	−	+	+	4	25	3.96
26	+	−	−	+	+	+	−	+	1	1	3.27
27	−	+	−	+	+	+	−	−	3	23	3.41
28	+	+	−	+	+	−	+	−	2	12	4.30
29	−	−	+	+	+	+	+	−	2	9	4.44
30	+	−	+	+	+	−	−	−	3	20	3.65
31	−	+	+	+	+	−	−	+	1	5	4.41
32	+	+	+	+	+	+	+	+	4	31	3.40

■ **TABLE 8.17**
Effect Estimates, Regression Coefficients, and Sums of Squares for Example 8.6

Variable	Name	−1 Level	+1 Level
A	*x*-Axis shift	0	15
B	*y*-Axis shift	0	15
C	*z*-Axis shift	0	15
D	Tool supplier	1	2
E	*a*-Axis shift	0	30
F	Spindle speed	90	110
G	Fixture height	0	15
H	Feed rate	90	110

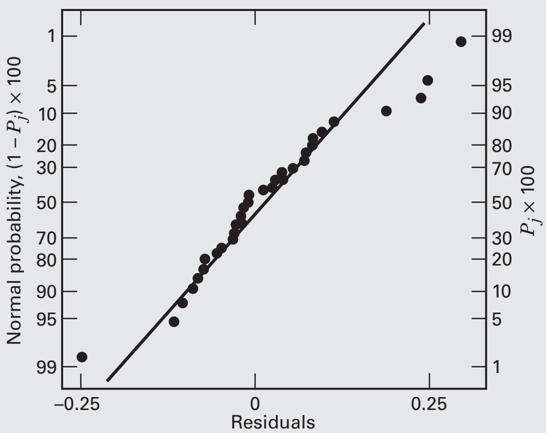
Variable	Regression Coefficient	Estimated Effect	Sum of Squares
Overall average	1.28007		
A	0.14513	0.29026	0.674020
B	−0.10027	−0.20054	0.321729
C	−0.01288	−0.02576	0.005310
D	0.05407	0.10813	0.093540
E	−2.531E-04	−5.063E-04	2.050E-06
F	−0.01936	−0.03871	0.011988
G	0.05804	0.11608	0.107799
H	0.00708	0.01417	0.001606
$AB + CF + DG$	−0.00294	−0.00588	2.767E-04
$AC + BF$	−0.03103	−0.06206	0.030815
$AD + BG$	−0.18706	−0.37412	1.119705
AE	0.00402	0.00804	5.170E-04
$AF + BC$	−0.02251	−0.04502	0.016214
$AG + BD$	0.02644	0.05288	0.022370
AH	−0.02521	−0.05042	0.020339
BE	0.04925	0.09851	0.077627
BH	0.00654	0.01309	0.001371
$CD + FG$	0.01726	0.03452	0.009535
CE	0.01991	0.03982	0.012685
$CG + DF$	−0.00733	−0.01467	0.001721
CH	0.03040	0.06080	0.029568
DE	0.00854	0.01708	0.002334
DH	0.00784	0.01569	0.001969
EF	−0.00904	−0.01808	0.002616
EG	−0.02685	−0.05371	0.023078
EH	−0.01767	−0.03534	0.009993
FH	−0.01404	−0.02808	0.006308
GH	0.00245	0.00489	1.914E-04
ABE	0.01665	0.03331	0.008874
ABH	−0.00631	−0.01261	0.001273
ACD	−0.02717	−0.05433	0.023617



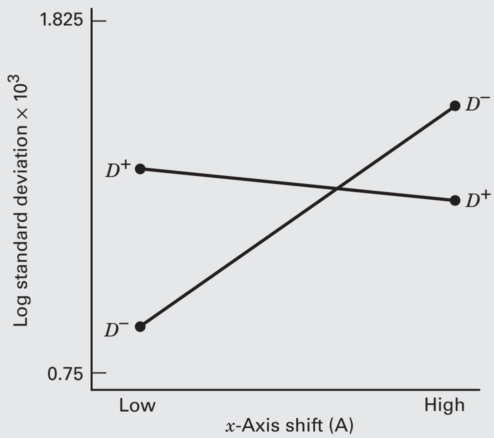
■ **FIGURE 8.19** Normal probability plot of the effect estimates for Example 8.6

■ **TABLE 8.18**
Analysis of Variance for Example 8.6

Source of Variation	Sum of Squares	Degrees of Freedom	Mean Square	F_0	*P*-Value
A	0.6740	1	0.6740	39.42	<0.0001
B	0.3217	1	0.3217	18.81	0.0002
D	0.0935	1	0.0935	5.47	0.0280
AD	1.1197	1	1.1197	65.48	<0.0001
Blocks	0.0201	3	0.0067		
Error	0.4099	24	0.0171		
Total	2.6389	31			

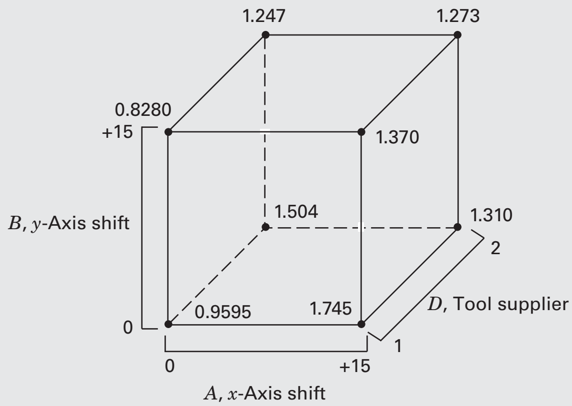


■ **FIGURE 8.20** Normal probability plot of the residuals for Example 8.6



■ **FIGURE 8.21** Plot of *AD* interaction for Example 8.6

■ **FIGURE 8.22** **The 2_{IV}^{8-3} design in Example 8.6 projected into four replicates of a 2^3 design in factors A, B, and D**



8.5 Alias Structures in Fractional Factorials and Other Designs

In this chapter, we show how to find the alias relationships in a 2^{k-p} fractional factorial design by use of the complete defining relation. This method works well in simple designs, such as the regular fractions we use most frequently, but it does not work as well in more complex settings, such as some of the nonregular fractions and partial fold-over designs that we will discuss subsequently. Furthermore, there are some fractional factorials that do not have defining relations, such as the Plackett–Burman designs in Section 8.6.3, so the defining relation method will not work for these types of designs at all.

Fortunately, there is a general method available that works satisfactorily in many situations. The method uses the polynomial or regression model representation of the model, say

$$\mathbf{y} = \mathbf{X}_1\boldsymbol{\beta}_1 + \boldsymbol{\epsilon}$$

where $\mathbf{y}$ is an $n \times 1$ vector of the responses, $\mathbf{X}_1$ is an $n \times p_1$ matrix containing the design matrix expanded to the form of the model that the experimenter is fitting, $\boldsymbol{\beta}_1$ is a $p_1 \times 1$ vector of the model parameters, and ϵ is an $n \times 1$ vector of errors. The least squares estimate of $\boldsymbol{\beta}_1$ is

$$\hat{\boldsymbol{\beta}}_1 = (\mathbf{X}_1'\mathbf{X}_1)^{-1}\mathbf{X}_1'\mathbf{y}$$

Suppose that the **true** model is

$$\mathbf{y} = \mathbf{X}_1\boldsymbol{\beta}_1 + \mathbf{X}_2\boldsymbol{\beta}_2 + \epsilon$$

where $\mathbf{X}_2$ is an $n \times p_2$ matrix containing additional variables that are not in the fitted model and $\boldsymbol{\beta}_2$ is a $p_2 \times 1$ vector of the parameters associated with these variables. It can be shown that

$$\begin{aligned} E(\hat{\boldsymbol{\beta}}_1) &= \boldsymbol{\beta}_1 + (\mathbf{X}_1'\mathbf{X}_1)^{-1}\mathbf{X}_1'\mathbf{X}_2\boldsymbol{\beta}_2 \\ &= \boldsymbol{\beta}_1 + \mathbf{A}\boldsymbol{\beta}_2 \end{aligned} \tag{8.1}$$

The matrix $\mathbf{A} = (\mathbf{X}_1'\mathbf{X}_1)^{-1}\mathbf{X}_1'\mathbf{X}_2$ is called the **alias matrix**. The elements of this matrix operating on $\boldsymbol{\beta}_2$ identify the alias relationships for the parameters in the vector $\boldsymbol{\beta}_1$.

We illustrate the application of this procedure with a familiar example. Suppose that we have conducted a 2^{3-1} design with defining relation $I = ABC$ or $I = x_1x_2x_3$. The model that the experimenter plans to fit is the main-effects-only model

$$y = \beta_0 + \beta_1x_1 + \beta_2x_2 + \beta_3x_3 + \epsilon$$

In the notation defined above

$$\boldsymbol{\beta}_1 = \begin{bmatrix} \beta_0 \\ \beta_1 \\ \beta_2 \\ \beta_3 \end{bmatrix} \quad \text{and} \quad \mathbf{X}_1 = \begin{bmatrix} 1 & -1 & -1 & 1 \\ 1 & 1 & -1 & -1 \\ 1 & -1 & 1 & -1 \\ 1 & 1 & 1 & 1 \end{bmatrix}$$

Suppose that the true model contains all the two-factor interactions, so that

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_3 x_3 + \beta_{12} x_1 x_2 + \beta_{13} x_1 x_3 + \beta_{23} x_2 x_3 + \epsilon$$

and

$$\boldsymbol{\beta}_2 = \begin{bmatrix} \beta_{12} \\ \beta_{13} \\ \beta_{23} \end{bmatrix}, \quad \text{and} \quad \mathbf{X}_2 = \begin{bmatrix} 1 & -1 & -1 \\ -1 & -1 & 1 \\ -1 & 1 & -1 \\ 1 & 1 & 1 \end{bmatrix}$$

Now

$$\mathbf{X}_1'\mathbf{X}_1 = 4\,\mathbf{I}_4 \quad \text{and} \quad \mathbf{X}_1'\mathbf{X}_2 = \begin{bmatrix} 0 & 0 & 0 \\ 0 & 0 & 4 \\ 0 & 4 & 0 \\ 4 & 0 & 0 \end{bmatrix}$$

Therefore,

$$(\mathbf{X}_1'\mathbf{X}_1)^{-1} = \frac{1}{4}\mathbf{I}_4$$

and

$$E(\hat{\boldsymbol{\beta}}_1) = \boldsymbol{\beta}_1 + \mathbf{A}\boldsymbol{\beta}_2$$

$$E\begin{bmatrix} \hat{\beta}_0 \\ \hat{\beta}_1 \\ \hat{\beta}_2 \\ \hat{\beta}_3 \end{bmatrix} = \begin{bmatrix} \beta_0 \\ \beta_1 \\ \beta_2 \\ \beta_3 \end{bmatrix} + \frac{1}{4}\mathbf{I}_4 \begin{bmatrix} 0 & 0 & 0 \\ 0 & 0 & 4 \\ 0 & 4 & 0 \\ 4 & 0 & 0 \end{bmatrix} \begin{bmatrix} \beta_{12} \\ \beta_{13} \\ \beta_{23} \end{bmatrix}$$

$$= \begin{bmatrix} \beta_0 \\ \beta_1 \\ \beta_2 \\ \beta_3 \end{bmatrix} + \begin{bmatrix} 0 & 0 & 0 \\ 0 & 0 & 1 \\ 0 & 1 & 0 \\ 1 & 0 & 0 \end{bmatrix} \begin{bmatrix} \beta_{12} \\ \beta_{13} \\ \beta_{23} \end{bmatrix}$$

$$= \begin{bmatrix} \beta_0 \\ \beta_1 \\ \beta_2 \\ \beta_3 \end{bmatrix} + \begin{bmatrix} 0 \\ \beta_{23} \\ \beta_{13} \\ \beta_{12} \end{bmatrix}$$

$$= \begin{bmatrix} \beta_0 \\ \beta_1 + \beta_{23} \\ \beta_2 + \beta_{13} \\ \beta_3 + \beta_{12} \end{bmatrix}$$

The interpretation of this, of course, is that each of the main effects is aliased with one of the two-factor interactions, which we know to be the case for this design. Notice that every row of the alias matrix represents one of the factors in $\boldsymbol{\beta}_1$ and every column represents one of the factors in $\boldsymbol{\beta}_2$. While this is a very simple example, the method is very general and can be applied to much more complex designs.

8.6 Resolution III Designs

8.6.1 Constructing Resolution III Designs

As indicated earlier, the sequential use of fractional factorial designs is very useful, often leading to great economy and efficiency in experimentation. This application of fractional factorials occurs frequently in situations of pure factor screening; that is, there are relatively many factors but only a few of them are expected to be important. Resolution III designs can be very useful in these situations.

It is possible to construct resolution III designs for investigating up to $k = N - 1$ factors in only N runs, where N is a multiple of 4. These designs are frequently useful in industrial experimentation. Designs in which N is a power of 2 can be constructed by the methods presented earlier in this chapter, and these are presented first. Of particular importance are designs requiring 4 runs for up to 3 factors, 8 runs for up to 7 factors, and 16 runs for up to 15 factors. If $k = N - 1$, the fractional factorial design is said to be **saturated**.

A design for analyzing up to three factors in four runs is the 2_{III}^{3-1} design, presented in Section 8.2. Another very useful saturated fractional factorial is a design for studying seven factors in eight runs, that is, the 2_{III}^{7-4} design. This design is a one-sixteenth fraction of the 2^7. It may be constructed by first writing down as the basic design the plus and minus levels for a full 2^3 design in A, B, and C and then associating the levels of four additional factors with the interactions of the original three as follows: $D = AB, E = AC, F = BC$, and $G = ABC$. Thus, the generators for this design are $I = ABD, I = ACE, I = BCF$, and $I = ABCG$. The design is shown in Table 8.19.

The complete defining relation for this design is obtained by multiplying the four generators ABD, ACE, BCF, and $ABCG$ together two at a time, three at a time, and four at a time, yielding

$$\begin{aligned} I &= ABD = ACE = BCF = ABCG = BCDE = ACDF = CDG \\ &= ABEF = BEG = AFG = DEF = ADEG = CEFG = BDFG = ABCDEFG \end{aligned}$$

To find the aliases of any effect, simply multiply the effect by each word in the defining relation. For example, the aliases of B are

$$\begin{aligned} B &= AD = ABCE = CF = ACG = CDE = ABCDF = BCDG = AEF = EG \\ &= ABFG = BDEF = ABDEG = BCEFG = DFG = ACDEFG \end{aligned}$$

This design is a one-sixteenth fraction, and because the signs chosen for the generators are positive, this is the principal fraction. It is also a resolution III design because the smallest number of letters in any word of the defining contrast is three. Any one of the 16 different 2_{III}^{7-4} designs in this family could be constructed by using the generators with one of the 16 possible arrangements of signs in $I = \pm ABD, I = \pm ACE, I = \pm BCF, I = \pm ABCG$.

■ **TABLE 8.19**
The 2_{III}^{7-4} Design with the Generators $I = ABD, I = ACE, I = BCF$, and $I = ABCG$

	Basic Design							
Run	A	B	C	$D = AB$	$E = AC$	$F = BC$	$G = ABC$	
1	−	−	−	+	+	+	−	*def*
2	+	−	−	−	−	+	+	*afg*
3	−	+	−	−	+	−	+	*beg*
4	+	+	−	+	−	−	−	*abd*
5	−	−	+	+	−	−	+	*cdg*
6	+	−	+	−	+	−	−	*ace*
7	−	+	+	−	−	+	−	*bcf*
8	+	+	+	+	+	+	+	*abcdefg*

The seven degrees of freedom in this design may be used to estimate the seven main effects. Each of these effects has 15 aliases; however, if we assume that three-factor and higher interactions are negligible, then considerable simplification in the alias structure results. Making this assumption, each of the linear combinations associated with the seven main effects in this design actually estimates the main effect and three two-factor interactions:

$$\begin{aligned}
[A] &\to A + BD + CE + FG \\
[B] &\to B + AD + CF + EG \\
[C] &\to C + AE + BF + DG \\
[D] &\to D + AB + CG + EF \\
[E] &\to E + AC + BG + DF \\
[F] &\to F + BC + AG + DE \\
[G] &\to G + CD + BE + AF
\end{aligned} \tag{8.2}$$

These aliases are found in Appendix Table VIII(h), ignoring three-factor and higher interactions.

The saturated 2_{III}^{7-4} design in Table 8.19 can be used to obtain resolution III designs for studying fewer than seven factors in eight runs. For example, to generate a design for six factors in eight runs, simply drop any one column in Table 8.19, for example, column G. This produces the design shown in Table 8.20.

It is easy to verify that this design is also of resolution III; in fact, it is a 2_{III}^{6-3}, or a one-eighth fraction, of the 2^6 design. The defining relation for the 2_{III}^{6-3} design is equal to the defining relation for the original 2_{III}^{7-4} design with any words containing the letter G deleted. Thus, the defining relation for our new design is

$$I = ABD = ACE = BCF = BCDE = ACDF = ABEF = DEF$$

In general, when d factors are dropped to produce a new design, the new defining relation is obtained as those words in the original defining relation that do not contain any dropped letters. When constructing designs by this method, care should be exercised to obtain the best arrangement possible. If we drop columns B, D, F, and G from Table 8.19, we obtain a design for three factors in eight runs, yet the treatment combinations correspond to two replicates of a 2^{3-1} design. The experimenter would probably prefer to run a full 2^3 design in A, C, and E.

It is also possible to obtain a resolution III design for studying up to 15 factors in 16 runs. This saturated 2_{III}^{15-11} design can be generated by first writing down the 16 treatment combinations associated with a 2^4 design in A, B, C, and D and then equating 11 new factors with the two-, three-, and four-factor interactions of the original four. In this

■ **TABLE 8.20**
A 2_{III}^{6-3} Design with the Generators $I = ABD$, $I = ACE$, and $I = BCF$

	Basic Design						
Run	***A***	***B***	***C***	***D* = *AB***	***E* = *AC***	***F* = *BC***	
1	−	−	−	+	+	+	*def*
2	+	−	−	−	−	+	*af*
3	−	+	−	−	+	−	*be*
4	+	+	−	+	−	−	*abd*
5	−	−	+	+	−	−	*cd*
6	+	−	+	−	+	−	*ace*
7	−	+	+	−	−	+	*bcf*
8	+	+	+	+	+	+	*abcdef*

design, each of the 15 main effects is aliased with seven two-factor interactions. A similar procedure can be used for the 2_{III}^{31-26} design, which allows up to 31 factors to be studied in 32 runs.

8.6.2 Fold Over of Resolution III Fractions to Separate Aliased Effects

By combining fractional factorial designs in which certain signs are switched, we can systematically isolate effects of potential interest. This type of sequential experiment is called a **fold over** of the original design. The alias structure for any fraction with the signs for one or more factors reversed is obtained by making changes of sign on the appropriate factors in the alias structure of the original fraction.

Consider the 2_{III}^{7-4} design in Table 8.19. Suppose that along with this principal fraction a second fractional design with the signs reversed in the column for factor D is also run. That is, the column for D in the second fraction is

$$- + + - - + + -$$

The effects that may be estimated from the first fraction are shown in Equation 8.2, and from the second fraction we obtain

$$\begin{aligned}
[A]' &\rightarrow A - BD + CE + FG \\
[B]' &\rightarrow B - AD + CF + EG \\
[C]' &\rightarrow C + AE + BF - DG \\
[D]' &\rightarrow D - AB - CG - EF \\
[-D]' &\rightarrow -D + AB + CG + EF \\
[E]' &\rightarrow E + AC + BG - DF \\
[F]' &\rightarrow F + BC + AG - DE \\
[G]' &\rightarrow G - CD + BE + AF
\end{aligned} \tag{8.3}$$

assuming that three-factor and higher interactions are insignificant. Now from the two linear combinations of effects $\frac{1}{2}([i] + [i]')$ and $\frac{1}{2}([i] - [i]')$ we obtain

i	From $\frac{1}{2}([i] + [i]')$	From $\frac{1}{2}([i] - [i]')$
A	$A + CE + FG$	BD
B	$B + CF + EG$	AD
C	$C + AE + BF$	DG
D	D	$AB + CG + EF$
E	$E + AC + BG$	DF
F	$F + BC + AG$	DE
G	$G + BE + AF$	CD

Thus, we have isolated the main effect of D and all of its two-factor interactions. In general, if we add to a fractional design of resolution III or higher a further fraction with the signs of a *single factor* reversed, then the combined design will provide estimates of the main effect of that factor and its two-factor interactions. This is sometimes called a **single-factor fold over**.

Now suppose we add to a resolution III fractional a second fraction in which the *signs for all the factors are reversed*. This type of fold over (sometimes called a **full fold over** or a reflection) breaks the alias links between all main effects and their two-factor interactions. That is, we may use the **combined design** to estimate all of the main effects clear of any two-factor interactions. The following example illustrates the full fold-over technique.

EXAMPLE 8.7

A human performance analyst is conducting an experiment to study eye focus time and has built an apparatus in which several factors can be controlled during the test. The factors he initially regards as important are acuity or sharpness of vision (*A*), distance from target to eye (*B*), target shape (*C*), illumination level (*D*), target size (*E*), target density (*F*), and subject (*G*). Two levels of each factor are considered. He suspects that only a few of these seven factors are of major importance and that high-order interactions between the factors can be neglected. On the basis of this assumption, the analyst decides to run a screening experiment to identify the most important factors and then to concentrate further study on those. To screen these seven factors, he runs the treatment combinations from the 2_{III}^{7-4} design in Table 8.19 in random order, obtaining the focus times in milliseconds, as shown in Table 8.21.

Seven main effects and their aliases may be estimated from these data. From Equation 8.2, we see that the effects and their aliases are

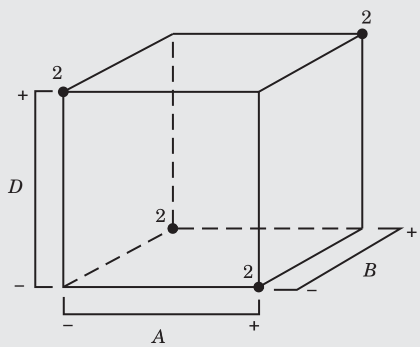
$$
\begin{aligned}
[A] &= 20.63 \rightarrow A + BD + CE + FG \\
[B] &= 38.38 \rightarrow B + AD + CF + EG \\
[C] &= -0.28 \rightarrow C + AE + BF + DG \\
[D] &= 28.88 \rightarrow D + AB + CG + EF \\
[E] &= -0.28 \rightarrow E + AC + BG + DF \\
[F] &= -0.63 \rightarrow F + BC + AG + DE \\
[G] &= -2.43 \rightarrow G + CD + BE + AF
\end{aligned}
$$

For example, the estimate of the main effect of *A* and its aliases is

$$
\begin{aligned}
[A] = \tfrac{1}{4}(&-85.5 + 75.1 - 93.2 + 145.4 - 83.7 \\
&+ 77.6 - 95.0 + 141.8) = 20.63
\end{aligned}
$$

The three largest effects are [*A*], [*B*], and [*D*]. The simplest interpretation of the results of this experiment is that the main effects of *A*, *B*, and *D* are all significant. However, this interpretation is not unique, because one could also logically conclude that *A*, *B*, and the *AB* interaction, or perhaps *B*, *D*, and the *BD* interaction, or perhaps *A*, *D*, and the *AD* interaction are the true effects.

Notice that *ABD* is a word in the defining relation for this design. Therefore, this 2_{III}^{7-4} design does not project into a full 2^3 factorial in *ABD*; instead, it projects into two replicates of a 2^{3-1} design, as shown in Figure 8.23. Because the 2^{3-1} design is a resolution III design, *A* will be aliased with *BD*, *B* will be aliased with *AD*, and *D* will be aliased with *AB*, so the interactions cannot be separated from the main effects. The experimenter here may have been unlucky. If he had assigned the factor illumination level to *C* instead of *D*,



■ **FIGURE 8.23** **The 2_{III}^{7-4} design projected into two replicates of a 2_{III}^{3-1} design in *A*, *B*, and *D***

■ **TABLE 8.21**
A 2_{III}^{7-4} Design for the Eye Focus Time Experiment

	Basic Design								
Run	*A*	*B*	*C*	*D* = *AB*	*E* = *AC*	*F* = *BC*	*G* = *ABC*		Time
1	−	−	−	+	+	+	−	*def*	85.5
2	+	−	−	−	−	+	+	*afg*	75.1
3	−	+	−	−	+	−	+	*beg*	93.2
4	+	+	−	+	−	−	−	*abd*	145.4
5	−	−	+	+	−	−	+	*cdg*	83.7
6	+	−	+	−	+	−	−	*ace*	77.6
7	−	+	+	−	−	+	−	*bcf*	95.0
8	+	+	+	+	+	+	+	*abcdefg*	141.8

the design would have projected into a full 2^3 design, and the interpretation could have been simpler.

To separate the main effects and the two-factor interactions, the full fold-over technique is used, and a second fraction is run with all the signs reversed. This fold-over design is shown in Table 8.22 along with the observed responses. Notice that when we construct a full fold over of a resolution III design, we (in effect) change the signs on the generators that have an odd number of letters. The effects estimated by this fraction are

$$\begin{aligned}
[A]' &= -17.68 \rightarrow A - BD - CE - FG \\
[B]' &= 37.73 \rightarrow B - AD - CF - EG \\
[C]' &= -3.33 \rightarrow C - AE - BF - DG \\
[D]' &= 29.88 \rightarrow D - AB - CG - EF \\
[E]' &= 0.53 \rightarrow E - AC - BG - DF \\
[F]' &= 1.63 \rightarrow F - BC - AG - DE \\
[G]' &= 2.68 \rightarrow G - CD - BE - AF
\end{aligned}$$

By combining this second fraction with the original one, we obtain the following estimates of the effects:

i	From $\frac{1}{2}([i] + [i]')$	From $\frac{1}{2}([i] - [i]')$
A	$A = 1.48$	$\mathbf{BD} + CE + FG = 19.15$
B	$\mathbf{B} = 38.05$	$AD + CE + FG = 19.15$
C	$C = -1.80$	$BD + CE + FG = 19.15$
D	$\mathbf{D} = 29.38$	$AB + CG + EF = -0.50$
E	$E = 0.13$	$AC + BG + DF = -0.40$
F	$F = 0.50$	$BC + AG + DE = -1.13$
G	$G = 0.13$	$CD + BE + AF = -2.55$

The two largest effects are B and D. Furthermore, the third largest effect is $BD + CE + FG$, so it seems reasonable to attribute this to the BD interaction. The experimenter used the two factors distance (B) and illumination level (D) in subsequent experiments with the other factors A, C, E, and F at standard settings and verified the results obtained here. He decided to use subjects as blocks in these new experiments rather than ignore a potential subject effect because several different subjects had to be used to complete the experiment.

■ **TABLE 8.22**
A Fold-Over 2^{7-4}_{III} Design for the Eye Focus Experiment

	Basic Design								
Run	**A**	**B**	**C**	**$D = -AB$**	**$E = -AC$**	**$F = -BC$**	**$G = ABC$**		**Time**
1	+	+	+	−	−	−	+	*abcg*	91.3
2	−	+	+	+	+	−	−	*bcde*	136.7
3	+	−	+	+	−	+	−	*acdf*	82.4
4	−	−	+	−	+	+	+	*cefg*	73.4
5	+	+	−	−	+	+	−	*abef*	94.1
6	−	+	−	+	−	+	+	*bdfg*	143.8
7	+	−	−	+	+	−	+	*adeg*	87.3
8	−	−	−	−	−	−	−	(1)	71.9

The Defining Relation for a Fold-Over Design. Combining fractional factorial designs via fold over as demonstrated in Example 8.7 is a very useful technique. It is often of interest to know the defining relation for the combined design. It can be easily determined. Each separate fraction will have $L + U$ words used as generators: L words of like sign and U words of unlike sign. The combined design will have $L + U - 1$ words used as generators. These will be the L words of like sign and the $U - 1$ words consisting of independent even products of the words of unlike sign. (**Even products** are words taken two at a time, four at a time, and so forth.)

To illustrate this procedure, consider the design in Example 8.7. For the first fraction, the generators are

$$I = ABD, \quad I = ACE, \quad I = BCF, \quad \text{and} \quad I = ABCG$$

and for the second fraction, they are

$$I = -ABD, \quad I = -ACE, \quad I = -BCF, \quad \text{and} \quad I = ABCG$$

Notice that in the second fraction we have switched the signs on the generators with an odd number of letters. Also, notice that $L + U = 1 + 3 = 4$. The combined design will have $I = ABCG$ (the like sign word) as a generator and two words that are independent even products of the words of unlike sign. For example, take $I = ABD$ and $I = ACE$; then $I = (ABD)(ACE) = BCDE$ is a generator of the combined design. Also, take $I = ABD$ and $I = BCF$; then $I = (ABD)(BCF) = ACDF$ is a generator of the combined design. The complete defining relation for the combined design is

$$I = ABCG = BCDE = ACDF = ADEG = BDFG = ABEF = CEFG$$

Blocking in a Fold-Over Design. Usually a fold-over design is conducted in two distinct time periods. Following the initial fraction, some time usually elapses while the data are analyzed and the fold-over runs are planned. Then the second set of runs is made, often on a different day, or different shift, or using different operating personnel, or perhaps material from a different source. This leads to a situation where **blocking** to eliminate potential nuisance effects between the two time periods is of interest. Fortunately, blocking in the combined experiment is easily accomplished.

To illustrate, consider the fold-over experiment in Example 8.7. In the initial group of eight runs shown in Table 8.21, the generators are $D = AB, E = AC, F = BC$, and $G = ABC$. In the fold-over set of runs, Table 8.22, the signs are changed on three of the generators so that $D = -AB, E = -AC$, and $F = -BC$. Thus, in the first group of eight runs the signs on the effects *ABD, ACE*, and *BCF* are positive, and in the second group of eight runs the signs on *ABD, ACE*, and *BCF* are negative; therefore, these effects are confounded with blocks. Actually, there is a single-degree-of-freedom alias chain confounded with blocks (remember that there are two blocks, so there must be one degree of freedom for blocks), and the effects in this alias chain may be found by multiplying any one of the effects *ABD, ACE*, and *BCF* through the defining relation for the design. This yields

$$ABD = CDG = ACE = BCF = BEG = AFG = DEF = ABCDEFG$$

as the complete set of effects that are confounded with blocks. In general, a completed fold-over experiment will always form two blocks with the effects whose signs are positive in one block and negative in the other (and their aliases) confounded with blocks. These effects can always be determined from the generators whose signs have been switched to form the fold over.

8.6.3 Plackett–Burman Designs

These are two-level fractional factorial designs developed by Plackett and Burman (1946) for studying up to $k = N - 1$ variables in N runs, where N is a multiple of 4. If N is a power of 2, these designs are identical to those presented earlier in this section. However, for $N = 12$, 20, 24, 28, and 36, the Plackett–Burman designs are sometimes of interest. Because these designs cannot be represented as cubes, they are sometimes called **nongeometric designs**.

The upper half of Table 8.23 presents rows of plus and minus signs that are used to construct the Plackett–Burman designs for $N = 12, 20, 24$, and 36, whereas the lower half of the table presents blocks of plus and minus signs for constructing the design for $N = 28$. The designs for $N = 12, 20, 24$, and 36 are obtained by writing the appropriate row in Table 8.23 as a column (or row). A second column (or row) is then generated from this first one by moving the elements of the column (or row) down (or to the right) one position and placing the last element in the first position. A third column (or row) is produced from the second similarly, and the process is continued until column (or row) k is generated. A row of minus signs is then added, completing the design. For $N = 28$, the three blocks *X, Y*, and *Z* are written down in the order

$$\begin{matrix} X & Y & Z \\ Z & X & Y \\ Y & Z & X \end{matrix}$$

■ **TABLE 8.23**
Plus and Minus Signs for the Plackett–Burman Designs

$k = 11,\ N = 12$ $+ + - + + + - - - + -$

$k = 19,\ N = 20$ $+ + - - + + + + - + - + - - - - + + -$

$k = 23,\ N = 24$ $+ + + + + - + - + + - - + + - - + - + - - - -$

$k = 35,\ N = 36$ $- + - + + + - - - + + + + + - + + + - - + - - - - + - + - + + - - + -$

$k = 27,\ N = 28$

+ − + + + + − − −	− + − − − + − − +	+ + − + − + + − +
+ + − + + + − − −	− − + + − − + − −	− + + + + − + + −
− + + + + + − − −	+ − − − + − − + −	+ − + − + + − + +
− − − + − + + + +	− − + − + − − − +	+ − + + + − + − +
− − − + + − + + +	+ − − − − + + − −	+ + − − + + + + −
− − − − + + + + +	− + − + − − − + −	− + + + − + − + +
+ + + − − − + − +	− − + − − + − + −	+ − + + − + + + −
+ + + − − − + + −	+ − − + − − − − +	+ + − + + − − + +
+ + + − − − − + +	− + − − + − + − −	− + + − + + + − +

and a row of minus signs is added to these 27 rows. The design for $N = 12$ runs and $k = 11$ factors is shown in Table 8.24.

The nongeometric Plackett–Burman designs for $N = 12, 20, 24, 28$, and 36 have complex alias structures. For example, in the 12-run design every main effect is **partially aliased** with every two-factor interaction not involving itself. For example, the AB interaction is aliased with the nine main effects $C, D, \ldots, K$ and the AC interaction is aliased with the nine main effects $B, D, \ldots, K$. Furthermore, each main effect is **partially aliased** with 45 two-factor interactions. As an example, consider the aliases of the main effect of factor A:

$$[A] = A - \frac{1}{3}BC - \frac{1}{3}BD - \frac{1}{3}BE + \frac{1}{3}BF + \ldots - \frac{1}{3}KL$$

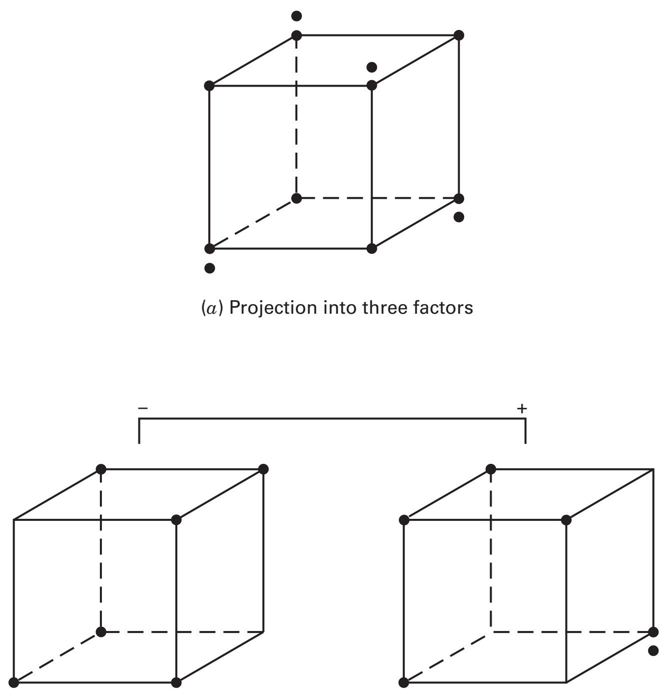
■ **TABLE 8.24**
Plackett–Burman Design for $N = 12, k = 11$

Run	A	B	C	D	E	F	G	H	I	J	K
1	+	−	+	−	−	−	+	+	+	−	+
2	+	+	−	+	−	−	−	+	+	+	−
3	−	+	+	−	+	−	−	−	+	+	+
4	+	−	+	+	−	+	−	−	−	+	+
5	+	+	−	+	+	−	+	−	−	−	+
6	+	+	+	−	+	+	−	+	−	−	−
7	−	+	+	+	−	+	+	−	+	−	−
8	−	−	+	+	+	−	+	+	−	+	−
9	−	−	−	+	+	+	−	+	+	−	+
10	+	−	−	−	+	+	+	−	+	+	−
11	−	+	−	−	−	+	+	+	−	+	+
12	−	−	−	−	−	−	−	−	−	−	−

Each one of the 45 two-factor interactions in the alias chain in weighed by the constant $\pm\frac{1}{3}$. This weighting of the two-factor interactions occurs throughout the Plackett–Burman series of nongeometric designs. In other Plackett–Burman designs, the constant will be different than $\pm\frac{1}{3}$.

Plackett–Burman designs are examples of **nonregular designs**. This term appears frequently in the experimental design literature. Basically, a **regular** design is one in which all effects can be estimated independently of the other effects and in the case of a fractional factorial, the effects that cannot be estimated are completely aliased with the other effects. Obviously, a full factorial such as the 2^k is a regular design, and so are the 2^{k-p} fractional factorials because while all of the effects cannot be estimated the "constants" in the alias chains for these designs are always either zero or plus or minus unity. That is, the effects that are not estimable because of the fractionation are completely aliased (some say completely confounded) with the effects that can be estimated. In nonregular designs, because some of the nonzero constants in the alias chains are not equal to ± 1, there is always at least a chance that some information on the aliased effects may be available.

The projection properties of the nongeometric Plackett–Burman designs are interesting, and in many cases, useful. For example, consider the 12-run design in Table 8.24. This design will project into three replicates of a full 2^2 design in any two of the original 11 factors. In three factors, the projected design is a full 2^3 factorial plus a 2_{III}^{3-1} fractional factorial (see Figure 8.24*a*). All Plackett–Burman designs will project into a full factorial plus some additional runs in any three factors. Thus, the resolution III Plackett–Burman design has **projectivity** 3, meaning it will collapse into a full factorial in any subset of three factors (actually, some of the larger Plackett–Burman designs, such as those with 68, 72, 80, and 84 runs, have projectivity 4). In contrast, the 2_{III}^{k-p} design only has projectivity 2. The four-dimensional projections of the 12-run design are shown in Figure 8.24*b*. Notice that there are 11 distinct runs. This design can fit all four of the main effects and all 6 two-factor interactions, assuming that all other main effects and interactions are negligible. The design in Figure 8.24*b* needs 5 additional runs to form a complete 2^4 (with one additional run) and only a single run to form a 2^{4-1} (with 5 additional runs). Regression methods can be used to fit models involving main effects and interactions using those projected designs.



(*a*) Projection into three factors

(*b*) Projection into four factors

■ **FIGURE 8.24** **Projection of the 12-run Plackett–design into three- and four-factor designs**

EXAMPLE 8.8

We will illustrate the analysis of a Plackett–Burman design with an example involving 12 factors. The smallest regular fractional factorial for 12 factors is a 16-run 2^{12-8} fractional factorial design. In this design, all 12 main effects are aliased with four two-factor interactions and three chains of two-factor interactions each containing six two-factor interactions (refer to Appendix VIII, design w). If there are significant two-factor interactions along with the main effects it is very possible that additional runs will be required to de-alias some of these effects.

Suppose that we decide to use a 20-run Plackett–Burman design for this problem. Now this has more runs that the smallest regular fraction, but it contains fewer runs than would be required by either a full fold over or a partial fold-over of the 16-run regular fraction. This design was created in JMP and is shown in Table 8.25, along with the observed response data obtained when the experiment was conducted. The alias matrix for this design, also produced from JMP, is in Table 8.26. Note that the coefficients of the aliased two-factor interactions are not either 0, −1, or +1 because this is a nonregular design). Hopefully this will provide some flexibility with which to estimate interactions if necessary.

Table 8.27 shows the JMP analysis of this design, using a forward-stepwise regression procedure to fit the model. In forward-stepwise regression, variables are entered into the model one at a time, beginning with those that appear most important, until no variables remain that are reasonable candidates for entry. In this analysis, we consider all main effects and two-factor interactions as possible variables of interest for the model.

Considering the *P*-values for the variables in Table 8.27, the most important factor is x_2, so this factor is entered into the model first. JMP then recalculates the *P*-values and the next variable entered would be x_4. Then the x_1x_4 interaction is entered along with the main effect of x_1 to preserve the hierarchy of the model. This is followed by the x_1x_4 interactions. The JMP output for these steps is not shown but is summarized at the bottom of Table 8.28. Finally, the last variable entered is x_5. Table 8.28 summarizes the final model.

■ **TABLE 8.25**
Plackett–Burman Design for Example 8.8

Run	X1	X2	X3	X4	X5	X6	X7	X8	X9	X10	X11	X12	y
1	1	1	1	1	1	1	1	1	1	1	1	1	221.5032
2	−1	1	−1	−1	1	1	1	1	−1	1	−1	1	213.8037
3	−1	−1	1	−1	−1	1	1	1	1	−1	1	−1	167.5424
4	1	−1	−1	1	−1	−1	1	1	1	1	−1	1	232.2071
5	1	1	−1	−1	1	−1	−1	1	1	1	1	−1	186.3883
6	−1	1	1	−1	−1	1	−1	−1	1	1	1	1	210.6819
7	−1	−1	1	1	−1	−1	1	−1	−1	1	1	1	168.4163
8	−1	−1	−1	1	1	−1	−1	1	−1	−1	1	1	180.9365
9	−1	−1	−1	−1	1	1	−1	−1	1	−1	−1	1	172.5698
10	1	−1	−1	−1	−1	1	1	−1	−1	1	−1	−1	181.8605
11	−1	1	−1	−1	−1	−1	1	1	−1	−1	1	−1	202.4022
12	1	−1	1	−1	−1	−1	−1	1	1	−1	−1	1	186.0079
13	−1	1	−1	1	−1	−1	−1	−1	1	1	−1	−1	216.4375
14	1	−1	1	−1	1	−1	−1	−1	−1	1	1	−1	192.4121
15	1	1	−1	1	−1	1	−1	−1	−1	−1	1	1	224.4362
16	1	1	1	−1	1	−1	1	−1	−1	−1	−1	1	190.3312
17	1	1	1	1	−1	1	−1	1	−1	−1	−1	−1	228.3411
18	−1	1	1	1	1	−1	1	−1	1	−1	−1	−1	223.6747
19	−1	−1	1	1	1	1	−1	1	−1	1	−1	−1	163.5351
20	1	−1	−1	1	1	1	1	−1	1	−1	1	−1	236.5124

■ **TABLE 8.26**
The Alias Matrix

Effect	1 2	1 3	1 4	1 5	1 6	1 7	1 8	1 9	1 10	1 11	1 12	2 3	2 4	2 5	2 6	2 7	2 8	2 9	2 10	2 11	2 12	3 4	3 5	3 6	3 7	3 8	3 9	3 10	3 11	3 12	4 5	4 6	4 7	4 8
Intercept	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
X1	0	0	0	0	0	0	0	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	−0.2	0.2	−0.2	0.6	0.2	0.2
X2	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	0.2	0	0	0	0	0	0	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	0.2	−0.2	−0.2
X3	0.2	0	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	−0.2	0.2	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	0	0	0	0	0	0	0	0	0	0.2	0.2	0.2	0.2
X4	0.2	−0.2	0	−0.2	0.6	0.2	0.2	0.2	−0.2	0.2	0.2	0.2	0	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	−0.2	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0	0	0	0
X5	0.2	0.2	−0.2	0	−0.2	0.2	−0.2	0.2	0.2	0.6	−0.2	0.2	−0.2	0	−0.2	0.6	0.2	0.2	0.2	−0.2	0.2	0	0	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	0	0.2	0.2	0.2
X6	0.2	−0.2	0.6	−0.2	0	0.2	−0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	−0.2	0	−0.2	0.2	−0.2	0.2	0.2	0.6	0.2	−0.2	0	−0.2	0.6	0.2	0.2	0.2	−0.2	0.2	0	−0.2	0.2
X7	−0.2	−0.2	0.2	0.2	0.2	0	−0.2	0.2	0.2	−0.2	0.2	0.2	−0.2	0.6	−0.2	0	0.2	−0.2	−0.2	−0.2	0.2	0.2	0.2	−0.2	0	−0.2	0.2	−0.2	0.2	0.2	0.2	−0.2	0	−0.2
X8	0.2	0.2	0.2	−0.2	−0.2	−0.2	0	0.6	0.2	−0.2	0.2	−0.2	−0.2	0.2	0.2	0.2	0	−0.2	0.2	0.2	−0.2	0.2	−0.2	0.6	−0.2	0	0.2	−0.2	−0.2	−0.2	0.2	0.2	−0.2	0
X9	−0.2	−0.2	0.2	0.2	−0.2	0.2	0.6	0	0.2	0.2	0.2	0.2	0.2	0.2	−0.2	−0.2	−0.2	0	0.6	0.2	−0.2	−0.2	−0.2	0.2	0.2	0.2	0	−0.2	0.2	0.2	0.2	−0.2	0.6	−0.2
X10	−0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	0.2	0	0.2	−0.2	−0.2	−0.2	0.2	0.2	−0.2	0.2	0.6	0	0.2	0.2	0.2	0.2	0.2	−0.2	−0.2	−0.2	0	0.6	0.2	−0.2	−0.2	0.2	0.2
X11	0.2	−0.2	0.2	0.6	0.2	−0.2	−0.2	0.2	0.2	0	−0.2	−0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	0.2	0	0.2	−0.2	−0.2	0.2	0.2	−0.2	0.2	0.6	0	0.2	0.2	0.2	0.2	−0.2
X12	0.2	0.2	0.2	−0.2	−0.2	0.2	0.2	0.2	−0.2	−0.2	0	0.2	−0.2	0.2	0.6	0.2	−0.2	−0.2	0.2	0.2	0	−0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	0.2	0	−0.2	−0.2	0.2	0.2

Effect	4 9	4 10	4 11	4 12	5 6	5 7	5 8	5 9	5 10	5 11	5 12	6 7	6 8	6 9	6 10	6 11	6 12	7 8	7 9	7 10	7 11	7 12	8 9	8 10	8 11	8 12	9 10	9 11	9 12	10 11	10 12	11 12
Intercept	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
X1	0.2	−0.2	0.2	0.2	−0.2	0.2	−0.2	0.2	0.2	0.6	−0.2	0.2	−0.2	−0.2	−0.2	0.2	−0.2	−0.2	0.2	0.2	−0.2	0.2	0.6	0.2	−0.2	0.2	0.2	0.2	0.2	0.2	−0.2	−0.2
X2	0.2	−0.2	−0.2	−0.2	−0.2	0.6	0.2	0.2	0.2	−0.2	0.2	−0.2	0.2	−0.2	0.2	0.2	0.6	0.2	−0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	−0.2	0.6	0.2	−0.2	0.2	0.2	0.2
X3	−0.2	0.2	−0.2	−0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	−0.2	0.6	0.2	0.2	0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	0.2	−0.2	−0.2	−0.2	−0.2	0.2	0.2	0.6	0.2	0.2
X4	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	0.6	0.2	0.2	0.2	−0.2	0.2	−0.2	0.2	0.2	−0.2	−0.2	−0.2	0.2	0.6
X5	0.2	−0.2	0.2	−0.2	0	0	0	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	0.6	0.2	0.2	−0.2	0.2	−0.2	0.2	−0.2	−0.2
X6	−0.2	−0.2	0.2	−0.2	0	0.2	0.2	0.2	0.2	−0.2	0.2	0	0	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	−0.2	0.6	0.2	−0.2	0.2	0.2
X7	0.6	0.2	0.2	0.2	0.2	0	−0.2	0.2	−0.2	−0.2	0.2	0	0.2	0.2	0.2	0.2	−0.2	0	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.6	−0.2
X8	−0.2	0.2	−0.2	0.2	0.2	−0.2	0	−0.2	0.6	0.2	0.2	0.2	0	−0.2	0.2	−0.2	−0.2	0	0.2	0.2	0.2	0.2	0	0	0	0	0.2	0.2	0.2	−0.2	0.2	−0.2
X9	0	0.2	−0.2	−0.2	0.2	0.2	−0.2	0	−0.2	0.2	−0.2	0.2	−0.2	0	−0.2	0.6	0.2	0.2	0	−0.2	0.2	−0.2	0	0.2	0.2	0.2	0	0	0	0.2	0.2	−0.2
X10	0.2	0	−0.2	0.2	0.2	−0.2	0.6	−0.2	0	0.2	−0.2	0.2	0.2	−0.2	0	−0.2	0.2	0.2	−0.2	0	−0.2	0.6	0.2	0	−0.2	0.2	0	0.2	0.2	0	0	0.2
X11	−0.2	−0.2	0	0.6	−0.2	−0.2	0.2	0.2	0.2	0	−0.2	0.2	−0.2	0.6	−0.2	0	0.2	0.2	0.2	−0.2	0	−0.2	0.2	−0.2	0	−0.2	0.2	0	−0.2	0	0.2	0
X12	−0.2	0.2	0.6	0	0.2	0.2	0.2	−0.2	−0.2	−0.2	0	−0.2	−0.2	0.2	0.2	0.2	0	0.2	−0.2	0.6	−0.2	0	0.2	0.2	−0.2	0	0.2	−0.2	0	0.2	0	0

■ **TABLE 8.27**
JMP Stepwise Regression Analysis of Example 8.8, Initial Solution

Stepwise Fit
Response:
Y
Stepwise Regression Control
Prob to Enter 0.250
Prob to Leave 0.100
Current Estimates

SSE	DFE	MSE	RSquare	RSquare Adj	Cp
10732	19	564.84211	0.0000	0.0000	12

Parameter	Estimate	nDF	SS	"F Ratio"	"Prob>F"
Intercept	200	1	0	0.000	1.0000
X1	0	1	1280	2.438	0.1359
X2	0	1	2784.8	6.307	0.0218
X3	0	1	452.279	0.792	0.3853
X4	0	1	1843.2	3.733	0.0693
X5	0	1	67.21943	0.113	0.7401
X6	0	1	86.41367	0.146	0.7068
X7	0	1	292.6697	0.505	0.4866
X8	0	1	60.08353	0.101	0.7539
X9	0	1	572.9881	1.015	0.3270
X10	0	1	32.53443	0.055	0.8177
X11	0	1	15.37763	0.026	0.8741
X12	0	1	0.159759	0.000	0.9871
X1*X2	0	3	5908	6.532	0.0043
X1*X3	0	3	1736.782	1.030	0.4058
X1*X4	0	3	5543.2	5.698	0.0075
X1*X5	0	3	1358.09	0.773	0.5261
X1*X6	0	3	2795.154	1.878	0.1740
X1*X7	0	3	1581.316	0.922	0.4528
X1*X8	0	3	1767.483	1.052	0.3970
X1*X9	0	3	1866.724	1.123	0.3692
X1*X10	0	3	1609.033	0.941	0.4441
X1*X11	0	3	1821.162	1.090	0.3818
X1*X12	0	3	1437.829	0.825	0.4991
X2*X3	0	3	4473.249	3.812	0.0309
X2*X4	0	3	4671.721	4.111	0.0243
X2*X5	0	3	3011.798	2.081	0.1431
X2*X6	0	3	3561.431	2.649	0.0842
X2*X7	0	3	3635.536	2.732	0.0781
X2*X8	0	3	2848.428	1.927	0.1659
X2*X9	0	3	3944.319	3.099	0.0564
X2*X10	0	3	2828.937	1.909	0.1688
X2*X11	0	3	2867.948	1.945	0.1631
X2*X12	0	3	2786.331	1.870	0.1753

■ **TABLE 8.27** *(Continued)*

Parameter	Estimate	nDF	SS	"F Ratio"	"Prob>F"
X3*X4	0	3	2576.807	1.685	0.2102
X3*X5	0	3	995.7837	0.545	0.6582
X3*X6	0	3	558.5936	0.293	0.8300
X3*X7	0	3	1201.228	0.672	0.5815
X3*X8	0	3	512.677	0.268	0.8478
X3*X9	0	3	1058.287	0.583	0.6344
X3*X10	0	3	626.2659	0.331	0.8034
X3*X11	0	3	569.497	0.299	0.8257
X3*X12	0	3	452.4973	0.235	0.8708
X4*X5	0	3	2038.876	1.251	0.3244
X4*X6	0	3	2132.749	1.323	0.3017
X4*X7	0	3	2320.382	1.471	0.2599
X4*X8	0	3	2034.576	1.248	0.3255
X4*X9	0	3	4886.816	4.459	0.0185
X4*X10	0	3	3125.433	2.191	0.1288
X4*X11	0	3	1970.181	1.199	0.3418
X4*X12	0	3	2194.402	1.371	0.2875
X5*X6	0	3	189.5188	0.096	0.9612
X5*X7	0	3	4964.273	4.590	0.0168
X5*X8	0	3	332.1148	0.170	0.9149
X5*X9	0	3	1065.334	0.588	0.6318
X5*X10	0	3	136.8974	0.069	0.9757
X5*X11	0	3	866.5116	0.468	0.7084
X5*X12	0	3	185.205	0.094	0.9625
X6*X7	0	3	434.1661	0.225	0.8777
X6*X8	0	3	185.7122	0.094	0.9623
X6*X9	0	3	1302.2	0.737	0.5455
X6*X10	0	3	246.5934	0.125	0.9437
X6*X11	0	3	2492.598	1.613	0.2256
X6*X12	0	3	913.7187	0.496	0.6900
X7*X8	0	3	935.8699	0.510	0.6813
X7*X9	0	3	1876.723	1.130	0.3665
X7*X10	0	3	345.5343	0.177	0.9101
X7*X11	0	3	577.8999	0.304	0.8224
X7*X12	0	3	328.611	0.168	0.9161
X8*X9	0	3	1111.212	0.616	0.6146
X8*X10	0	3	936.6248	0.510	0.6811
X8*X11	0	3	710.6107	0.378	0.7700
X8*X12	0	3	1517.358	0.878	0.4731
X9*X10	0	3	2360.154	1.504	0.2517
X9*X11	0	3	588.4157	0.309	0.8183
X9*X12	0	3	587.527	0.309	0.8186
X10*X11	0	3	125.3218	0.063	0.9786
X10*X12	0	3	2241.266	1.408	0.2770
X11*X12	0	3	94.12651	0.047	0.9859

■ **TABLE 8.28**
JMP Final Stepwise Regression Solution, Example 8.8

Stepwise Fit
Response:
Y

Stepwise Regression Control

Prob to Enter	0.250
Prob to Leave	0.100
Direction:	
Rules:	

Current Estimates

SSE	DFE	MSE	RSquare	RSquare Adj	Cp
381.79001	13	29.368462	0.9644	0.9480	72

Parameter	Estimate	nDF	SS	"F Ratio"	"Prob>F"
Intercept	200	1	0	0.000	1.0000
X1	8	3	5654.991	64.184	0.0000
X2	9.89242251	2	4804.208	81.792	0.0000
X3	0	1	2.547056	0.081	0.7813
X4	12.1075775	2	4442.053	75.626	0.0000
X5	2.581897	1	122.21	4.161	0.0622
X6	0	1	44.86956	1.598	0.2302
X7	0	1	7.652516	0.245	0.6292
X8	0	1	28.02042	0.950	0.3488
X9	0	1	19.33012	0.640	0.4393
X10	0	1	76.73973	3.019	0.1079
X11	0	1	1.672382	0.053	0.8221
X12	0	1	10.36884	0.335	0.5734
X1*X2	−12.537887	1	2886.987	98.302	0.0000
X1*X3	0	2	6.20474	0.091	0.9138
X1*X4	9.53788744	1	1670.708	56.888	0.0000
X1*X5	0	1	1.889388	0.060	0.8111
X1*X6	0	2	45.6286	0.747	0.4966
X1*X7	0	2	10.10477	0.150	0.8628
X1*X8	0	2	41.24821	0.666	0.5332
X1*X9	0	2	90.27392	1.703	0.2268
X1*X10	0	2	76.84386	1.386	0.2905
X1*X11	0	2	27.15307	0.421	0.6665
X1*X12	0	2	37.51692	0.599	0.5662
X2*X3	0	2	54.47309	0.915	0.4288
X2*X4	0	1	3.403658	0.108	0.7482
X2*X5	0	1	0.216992	0.007	0.9355
X2*X6	0	2	46.47256	0.762	0.4897
X2*X7	0	2	37.44377	0.598	0.5668
X2*X8	0	2	65.97489	1.149	0.3522
X2*X9	0	2	69.32501	1.220	0.3322
X2*X10	0	2	98.35266	1.908	0.1943

■ **TABLE 8.28** *(Continued)*

Parameter	Estimate	nDF	SS	"F Ratio"	"Prob>F"
X2*X11	0	2	141.1503	3.226	0.0790
X2*X12	0	2	52.05325	0.868	0.4466
X3*X4	0	2	111.3687	2.265	0.1500
X3*X5	0	2	80.40096	1.467	0.2724
X3*X6	0	3	67.40344	0.715	0.5653
X3*X7	0	3	99.64513	1.177	0.3667
X3*X8	0	3	66.19013	0.699	0.5737
X3*X9	0	3	29.41242	0.278	0.8399
X3*X10	0	3	120.8801	1.544	0.2632
X3*X11	0	3	4.678496	0.041	0.9881
X3*X12	0	3	56.41798	0.578	0.6426
X4*X5	0	1	49.01055	1.767	0.2084
X4*X6	0	2	148.7678	3.511	0.0662
X4*X7	0	2	10.61344	0.157	0.8564
X4*X8	0	2	29.55318	0.461	0.6420
X4*X9	0	2	25.40367	0.392	0.6847
X4*X10	0	2	112.0974	2.286	0.1478
X4*X11	0	2	1.673771	0.024	0.9761
X4*X12	0	2	24.16136	0.372	0.6980
X5*X6	0	2	169.9083	4.410	0.0392
X5*X7	0	2	31.18914	0.489	0.6258
X5*X8	0	2	90.33176	1.705	0.2265
X5*X9	0	2	34.4118	0.545	0.5948
X5*X10	0	2	154.654	3.745	0.0575
X5*X11	0	2	10.09686	0.149	0.8629
X5*X12	0	2	12.34385	0.184	0.8346
X6*X7	0	3	59.7591	0.619	0.6187
X6*X8	0	3	94.11651	1.091	0.3974
X6*X9	0	3	57.73503	0.594	0.6331
X6*X10	0	3	165.7402	2.557	0.1139
X6*X11	0	3	77.11154	0.844	0.5007
X6*X12	0	3	58.58914	0.604	0.6270
X7*X8	0	3	44.58254	0.441	0.7290
X7*X9	0	3	29.92824	0.284	0.8362
X7*X10	0	3	86.08846	0.970	0.4445
X7*X11	0	3	63.54514	0.666	0.5920
X7*X12	0	3	31.78299	0.303	0.8229
X8*X9	0	3	60.30138	0.625	0.6148
X8*X10	0	3	104.4506	1.255	0.3414
X8*X11	0	3	33.70238	0.323	0.8089
X8*X12	0	3	51.03759	0.514	0.6816
X9*X10	0	3	110.8786	1.364	0.3092
X9*X11	0	3	50.35583	0.506	0.6865
X9*X12	0	3	119.2043	1.513	0.2706
X10*X11	0	3	93.00237	1.073	0.4037

■ **TABLE 8.28** (*Continued*)

Parameter	Estimate	nDF	SS	"F Ratio"	"Prob>F"	
X10*X12	0	3	94.6634	1.099	0.3943	
X11*X12	0	3	38.30184	0.372	0.7753	
Step History						
Step	**Parameter**	**Action**	**"Sig Prob"**	**Seq SS**	**RSquare**	**Cp**
1	X2	Entered	0.0218	2784.8	0.2595	.
2	X4	Entered	0.0368	1843.2	0.4312	.
3	X1*X2	Entered	0.0003	4044.8	0.8081	.
4	X1*X4	Entered	0.0000	1555.2	0.9530	.
5	X5	Entered	0.0622	122.21	0.9644	.

The final model for this experiment contains the main effects of factors x_1, x_2, x_4, and x_5, plus the two-factor interactions x_1x_2 and x_1x_4. Now, it turns out that the data for this experiment were simulated from a model. The model used was

$$y = 200 + 8x_1 + 10x_2 + 12x_4 - 12x_1x_2 + 9x_1x_4 + \epsilon$$

where the random error term was normal with mean zero and standard deviation 5. The Plackett–Burman design was able to correctly identify all of the significant main effects and the two significant two-factor interactions. From Table 8.28 we observe that the model parameter estimates are actually very close to the values chosen for the model.

The partial aliasing structure of the Plackett–Burman design has been very helpful in identifying the significant interactions. Another approach to the analysis would be to realize that this design could be used to fit the main effects in any four factors and all of their two factor interactions, then use a normal probability plot to identify the four largest main effects, and finally fit the four factorial model in those four factors.

Notice that there is the main effect x_5 is identified as significant that was not in the simulation model used to generate the data. A type I error has been committed with respect to this factor. In screening experiments type I errors are not as serious as type II errors. A type I error results in a non-significant factor being identified as important and retained for subsequent experimentation and analysis. Eventually, we will likely discover that this factor really isn't important. However, a type II error means that an important factor has not been discovered. This variable will be dropped from subsequent studies and if it really turns out to be a critical factor, product or process performance can be negatively impacted. It is highly likely that the effect of this factor will never be discovered because it was discarded early in the research. In our example, all important factors were discovered, including the interactions, and that is the key point.

8.7 Resolution IV and V Designs

8.7.1 Resolution IV Designs

A 2^{k-p} fractional factorial design is of resolution IV if the main effects are clear of two-factor interactions and some two-factor interactions are aliased with each other. Thus, if three-factor and higher interactions are suppressed, the main effects may be estimated directly in a 2_{IV}^{k-p} design. An example is the 2_{IV}^{6-2} design in Table 8.10. Furthermore, the two combined fractions of the 2_{III}^{7-4} design in Example 8.7 yield a 2_{IV}^{7-3} design. Resolution IV designs are used extensively as screening experiments. The 2^{4-1} with eight runs and the 16-run fractions with 6, 7, and 8 factors are very popular.

Any 2_{IV}^{k-p} design must contain at least $2k$ runs. Resolution IV designs that contain exactly $2k$ runs are called **minimal designs**. Resolution IV designs may be obtained from resolution III designs by the process of **fold over**. Recall that to fold over a 2_{III}^{k-p} design, simply add to the original fraction a second fraction with all the signs reversed. Then the plus signs in the identity column I in the first fraction could be switched in the second fraction, and a $(k + 1)$st factor

■ **TABLE 8.29**
A 2_{IV}^{4-1} Design Obtained by Fold Over

D I	A	B	C
Original $2_{III}^{3-1} I = ABC$			
+	−	−	+
+	+	−	−
+	−	+	−
+	+	+	+
Second 2_{III}^{3-1} with Signs Switched			
−	+	+	−
−	−	+	+
−	+	−	+
−	−	−	−

could be associated with this column. The result is a 2_{IV}^{k+1-p} fractional factorial design. The process is demonstrated in Table 8.29 for the 2_{III}^{3-1} design. It is easy to verify that the resulting design is a 2_{IV}^{4-1} design with defining relation $I = ABCD$.

Table 8.30 provides a convenient summary of 2^{k-p} fractional factorial designs with $N = 4, 8, 16$, and 32 runs. Notice that although 16-run resolution IV designs are available for $6 \leq k \leq 8$ factors, if there are nine or more factors the smallest resolution IV design in the 2^{9-p} family is the 2^{9-4}, which requires 32 runs. Since this is a rather large number of runs, many experimenters are interested in smaller designs. Recall that a resolution IV design must contain at least $2k$ runs, so for example, a nine-factor resolution IV design must have at least 18 runs. A design with exactly $N = 18$ runs can be created by using an algorithm for constructing "optimal" designs. This design is a nonregular design, and it will be illustrated in Chapter 9 as part of a broader discussion of nonregular designs.

8.7.2 Sequential Experimentation with Resolution IV Designs

Because resolution IV designs are used as screening experiments, it is not unusual to find that upon conducting and analyzing the original experiment, additional experimentation is necessary to completely resolve all of the effects. We discussed this in Section 8.6.2 for the case of resolution III designs and introduced **fold over** as a sequential

■ **TABLE 8.30**
Useful Factorial and Fractional Factorial Designs from the 2^{k-p} System. The Numbers in the Cells Are the Numbers of Factors in the Experiment

	Number of Runs			
Design Type	**4**	**8**	**16**	**32**
Full factorial	2	3	4	5
Half-fraction	3	4	5	6
Resolution IV fraction	—	4	6–8	7–16
Resolution III fraction	3	5–7	9–15	17–31

experimentation strategy. In the resolution III situation, main effects are aliased with two-factor interaction, so the purpose of the fold over is to separate the main effects from the two-factor interactions. It is also possible to fold over resolution IV designs to separate two-factor interactions that are aliased with each other.

Montgomery and Runger (1996) observe that an experimenter may have several objectives in folding over a resolution IV design, such as

1. breaking as many two-factor interaction alias chains as possible;
2. breaking the two-factor interactions on a specific alias chain; or
3. breaking the two-factor interaction aliases involving a specific factor.

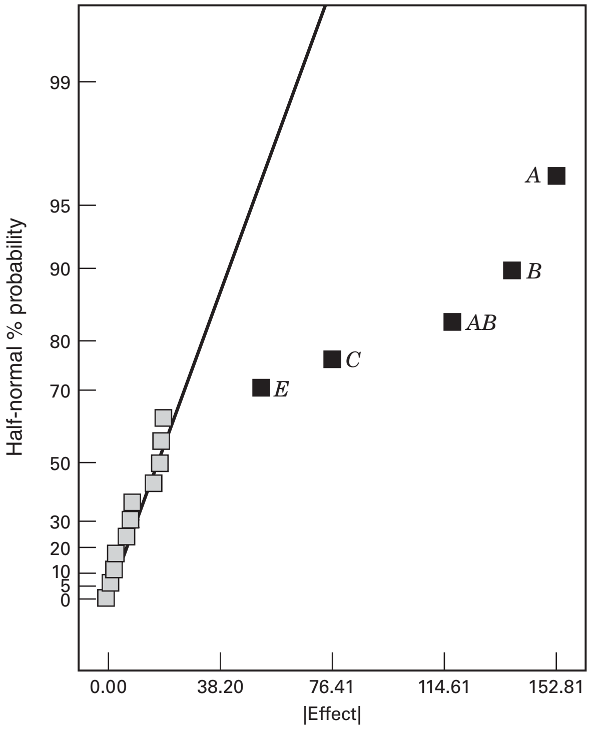
However, one has to be careful in folding over a resolution IV design. The full fold-over rule that we used for resolution III designs, simply run another fraction with all of the signs reversed, will not work for the resolution IV case. If this rule is applied to a resolution IV design, the result will be to produce exactly the same design with the runs in a different order. Try it! Use the 2_{IV}^{6-2} in Table 8.9 and see what happens when you reverse all of the signs in the test matrix.

The simplest way to fold over a resolution IV design is to switch the signs on a single variable of the original design matrix. This single-factor fold over allows all the two-factor interactions involving the factor whose signs are switched to be separated and accomplishes the third objective listed above.

To illustrate how a single-factor fold over is accomplished for a resolution IV design, consider the 2_{IV}^{6-2} design in Table 8.31 (the runs are in standard order, not run order). This experiment was conducted to study the effects of six factors on the thickness of photoresist coating applied to a silicon wafer. The design factors are A = spin speed,

■ **TABLE 8.31**
The Initial 2_{IV}^{6-2} Design for the Spin Coater Experiment

A	*B*	*C*	*D*	*E*	*F*	
Speed (RPM)	**Acceleration**	**Vol (cc)**	**Time (sec)**	**Resist Viscosity**	**Exhaust Rate**	**Thickness (mil)**
−	−	−	−	−	−	4524
+	−	−	−	+	−	4657
−	+	−	−	+	+	4293
+	+	−	−	−	+	4516
−	−	+	−	+	+	4508
+	−	+	−	−	+	4432
−	+	+	−	−	−	4197
+	+	+	−	+	−	4515
−	−	−	+	−	+	4521
+	−	−	+	+	+	4610
−	+	−	+	+	−	4295
+	+	−	+	−	−	4560
−	−	+	+	+	−	4487
+	−	+	+	−	−	4485
−	+	+	+	−	+	4195
+	+	+	+	+	+	4510



■ **FIGURE 8.25 Half-normal plot of effects for the initial spin coater experiment in Table 8.31**

B = acceleration, C = volume of resist applied, D = spin time, E = resist viscosity, and F = exhaust rate. The alias relationships for this design are given in Table 8.8. The half-normal probability plot of the effects is shown in Figure 8.25. Notice that the largest main effects are A, B, C, and E, and since these effects are aliased with three-factor or higher interactions, it is logical to assume that these are real effects. However, the effect estimate for the $AB + CE$ alias chain is also large. Unless other process knowledge or engineering information is available, we do not know whether this is AB, CE, or both of the interaction effects.

The fold-over design is constructed by setting up a new 2_{IV}^{6-2} fractional factorial design and changing the signs on factor A. The complete design following the addition of the fold-over runs is shown (in standard order) in Table 8.32. Notice that the runs have been assigned to two blocks; the runs from the initial 2_{IV}^{6-2} design in Table 8.32 are in block 1, and the fold-over runs are in block 2. The effects that are estimated from the combined set of runs are (ignoring interactions involving three or more factors)

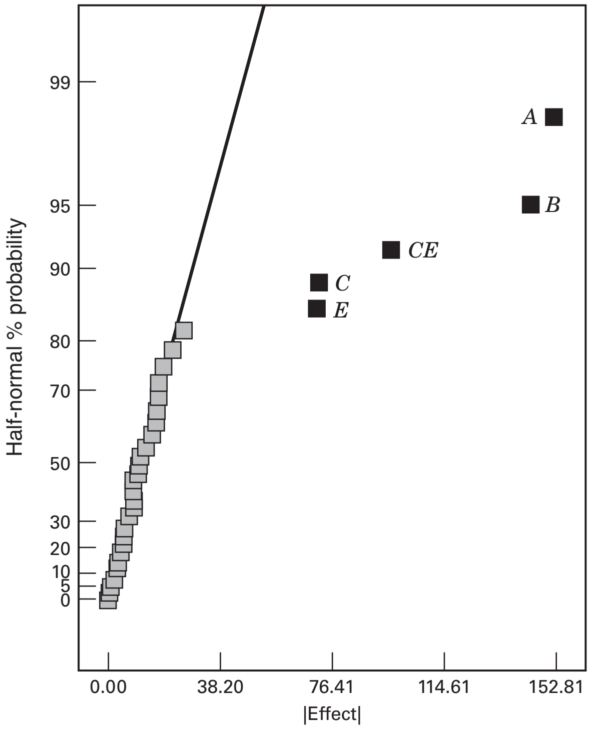
$$
\begin{array}{ll}
[A] = A & [AE] = AE \\
[B] = B & [AF] = AF \\
[C] = C & [BC] = BC + DF \\
[D] = D & [BD] = BD + CF \\
[E] = E & [BE] = BE \\
[F] = F & [BF] = BF + CD \\
[AB] = AB & [CE] = CE \\
[AC] = AC & [DE] = DE \\
[AD] = AD & [EF] = EF
\end{array}
$$

■ **TABLE 8.32**
The Completed Fold Over for the Spin Coater Experiment

		A	*B*	*C*	*D*	*E*	*F*	
Std. Order	**Block**	**Speed (RPM)**	**Acceleration**	**Vol (cc)**	**Time (sec)**	**Resist Viscosity**	**Exhaust Rate**	**Thickness (mil)**
1	1	−	−	−	−	−	−	4524
2	1	+	−	−	−	+	−	4657
3	1	−	+	−	−	+	+	4293
4	1	+	+	−	−	−	+	4516
5	1	−	−	+	−	+	+	4508
6	1	+	−	+	−	−	+	4432
7	1	−	+	+	−	−	−	4197
8	1	+	+	+	−	+	−	4515
9	1	−	−	−	+	−	+	4521
10	1	+	−	−	+	+	+	4610
11	1	−	+	−	+	+	−	4295
12	1	+	+	−	+	−	−	4560
13	1	−	−	+	+	+	−	4487
14	1	+	−	+	+	−	−	4485
15	1	−	+	+	+	−	+	4195
16	1	+	+	+	+	+	+	4510
17	2	+	−	−	−	−	−	4615
18	2	−	−	−	−	+	−	4445
19	2	+	+	−	−	+	+	4475
20	2	−	+	−	−	−	+	4285
21	2	+	−	+	−	+	+	4610
22	2	−	−	+	−	−	+	4325
23	2	+	+	+	−	−	−	4330
24	2	−	+	+	−	+	−	4425
25	2	+	−	−	+	−	+	4655
26	2	−	−	−	+	+	+	4525
27	2	+	+	−	+	+	−	4485
28	2	−	+	−	+	−	−	4310
29	2	+	−	+	+	+	−	4620
30	2	−	−	+	+	−	−	4335
31	2	+	+	+	+	−	+	4345
32	2	−	+	+	+	+	+	4305

Notice that all of the two-factor interactions involving factor *A* are now clear of other two-factor interactions. Also, *AB* is no longer aliased with *CE*. The half-normal probability plot of the effects from the combined design is shown in Figure 8.26. Clearly it is the *CE* interaction that is significant.

It is easy to show that the completed fold-over design in Table 8.32 allows estimation of the 6 main effects and 12 two-factor interaction alias chains shown previously, along with estimation of 12 other alias chains involving



■ **FIGURE 8.26 Half-normal plot of effects for the spin coater experiment in Table 8.32**

higher order interactions and the block effect. The generators for the original fractions are $E = ABC$ and $F = BCD$, and because we changed the signs in column A to create the fold over, the generators for the second group of 16 runs are $E = -ABC$ and $F = BCD$. Since there is only one word of like sign ($L = 1, U = 1$) and the combined design has only one generator (it is a one-half fraction), the generator for the combined design is $F = BCD$. Furthermore, since $ABCE$ is positive in block 1 and $ABCE$ is negative in block 2, $ABCE$ plus its alias $ADEF$ are confounded with blocks.

Examination of the alias chains involving the two-factor interactions for the original 16-run design and the completed fold over reveals some troubling information. In the original resolution IV fraction, every two-factor interaction was aliased with another two-factor interaction in six alias chains, and in one alias chain there were three two-factor interactions (refer to Table 8.8). Thus, seven degrees of freedom were available to estimate two-factor interactions. In the completed fold over, there are nine two-factor interactions that are estimated free of other two-factor interactions and three alias chains involving two two-factor interactions, resulting in 12 degrees of freedom for estimating two-factor interactions. Put another way, we used 16 additional runs but only gained five additional degrees of freedom for estimating two-factor interactions. This is not a terribly efficient use of experimental resources.

Fortunately, there is another alternative to using a complete fold over. In a **partial fold over** (or **semifold**) we make only half of the runs required for a complete fold over, which for the spin coater experiment would be eight runs. The following steps will produce a partial fold-over design:

1. Construct a single-factor fold over from the original design in the usual way by changing the signs on a factor that is involved in a two-factor interaction of interest.
2. Select only half of the fold-over runs by choosing those runs where the chosen factor is either at its high or low level. Selecting the level that you believe will generate the most desirable response is usually a good idea.

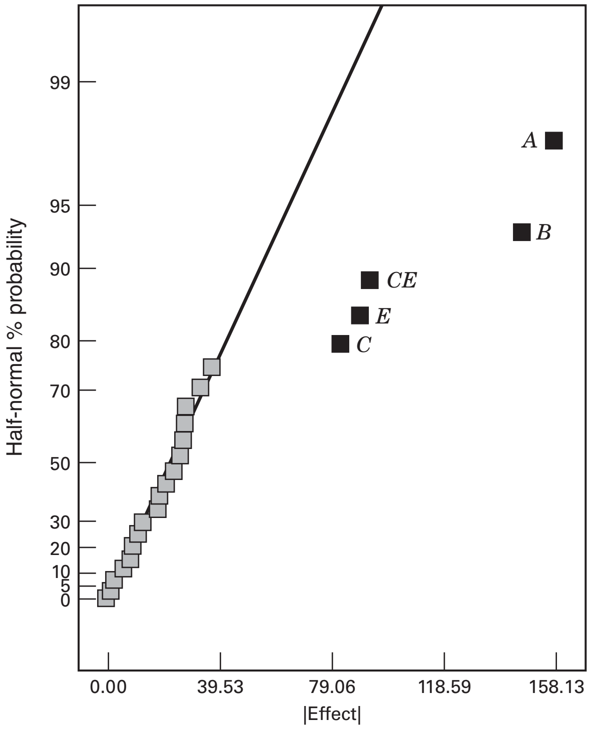
■ **TABLE 8.33**
The Partial Fold Over for the Spin Coater Experiment

		A	*B*	*C*	*D*	*E*	*F*	
Std. Order	**Block**	**Speed (RPM)**	**Acceleration**	**Vol (cc)**	**Time (sec)**	**Resist Viscosity**	**Exhaust Rate**	**Thickness (mil)**
1	1	−	−	−	−	−	−	4524
2	1	+	−	−	−	+	−	4657
3	1	−	+	−	−	+	+	4293
4	1	+	+	−	−	−	+	4516
5	1	−	−	+	−	+	+	4508
6	1	+	−	+	−	−	+	4432
7	1	−	+	+	−	−	−	4197
8	1	+	+	+	−	+	−	4515
9	1	−	−	−	+	−	+	4521
10	1	+	−	−	+	+	+	4610
11	1	−	+	−	+	+	−	4295
12	1	+	+	−	+	−	−	4560
13	1	−	−	+	+	+	−	4487
14	1	+	−	+	+	−	−	4485
15	1	−	+	+	+	−	+	4195
16	1	+	+	+	+	+	+	4510
17	2	−	−	−	−	+	−	4445
18	2	−	+	−	−	−	+	4285
19	2	−	−	+	−	−	+	4325
20	2	−	+	+	−	+	−	4425
21	2	−	−	−	+	+	+	4525
22	2	−	+	−	+	−	−	4310
23	2	−	−	+	+	−	−	4335
24	2	−	+	+	+	+	+	4305

Table 8.33 is the partial fold-over design for the spin coater experiment. Notice that we selected the runs where *A* is at its low level because in the original set of 16 runs (Table 8.31), thinner coatings of photoresist (which are desirable in this case) were obtained with *A* at the low level. (The estimate of the *A* effect is positive in the analysis of the original 16 runs, also suggesting that *A* at the low level produces the desired results.)

The alias relations from the partial fold over (ignoring interactions involving three or more factors) are

$$
\begin{aligned}
[A] &= A & [AE] &= AE \\
[B] &= B & [AF] &= AF \\
[C] &= C & [BC] &= BC + DF \\
[D] &= D & [BD] &= BD + CF \\
[E] &= E & [BE] &= BE \\
[F] &= F & [BF] &= BF + CD \\
[AB] &= AB & [CE] &= CE \\
[AC] &= AC & [DE] &= DE \\
[AD] &= AD & [EF] &= EF
\end{aligned}
$$



■ **FIGURE 8.27 Half-normal plot of effects from the partial fold over of the spin coater experiment in Table 8.33**

Notice that there are 12 degrees of freedom available to estimate two-factor interactions, exactly as in the complete fold over. Furthermore, *AB* is no longer aliased with *CE*. The half-normal plot of the effects from the partial fold over is shown in Figure 8.27. As in the complete fold over, *CE* is identified as the significant two-factor interaction.

The partial fold-over technique is very useful with resolution IV designs and usually leads to an efficient use of experimental resources. Resolution IV designs always provide good estimates of main effects (assuming that three-factor interactions are negligible), and usually the number of possible two-factor interaction that need to be de-aliased is not large. A partial fold over of a resolution IV design will usually support estimation of as many two-factor interactions as a full fold over. One disadvantage of the partial fold over is that it is not orthogonal. This causes parameter estimates to be correlated and leads to inflation in the standard errors of the effects or regression model coefficients. For example, in the partial fold over of the spin coater experiment, the standard errors of the regression model coefficients range from 0.20σ to 0.25σ, while in the complete fold over, which is orthogonal, the standard errors of the model coefficients are 0.18σ. For more information on partial fold overs, see Mee and Peralta (2000) and the supplemental material for this chapter.

8.7.3 Resolution V Designs

Resolution V designs are fractional factorials in which the main effects and the two-factor interactions do not have other main effects and two-factor interactions as their aliases. Consequently, these are very powerful designs, allowing unique estimation of all main effects and two-factor interactions, provided of course that all interactions involving three or more factors are negligible. The shortest word in the defining relation of a resolution V design must have five letters. The 2^{5-1} design with $I = ABCDE$ is perhaps the most widely used resolution V design, permitting study of five factors and estimation of all five main effects and all 10 two-factor interactions in only 16 runs. We illustrated the use of this design in Example 8.2.

The smallest design of resolution at least V for $k = 6$ factors is the 2_{VI}^{6-1} design with 32 runs, which is of resolution VI. For $k = 7$ factors, it is the 64-run 2_{VII}^{7-1} which is of resolution VII, and for $k = 8$ factors, it is the 64 run 2_{V}^{8-2} design. For $k \geq 9$ or more factors, all these designs require at least 128 runs. These are very large designs, so statisticians have long been interested in smaller alternatives that maintain the desired resolution. Mee (2004) gives a survey of this topic. **Nonregular fractions** can be very useful. This will be discussed further in Chapter 9.

8.8 Supersaturated Designs

A saturated design is defined as a fractional factorial in which the number of factors or design variables $k = N - 1$, where N is the number of runs. In recent years, considerable interest has been shown in developing and using **supersaturated designs** for factor screening experiments. In a supersaturated design, the number of variables $k > N - 1$, and usually these designs contain quite a few more variables than runs. The idea of using supersaturated designs was first proposed by Satterthwaite (1959). He proposed generating these designs at random. In an extensive discussion of this paper, some of the leading authorities in experimental design of the day, including Jack Youden, George Box, J. Stuart Hunter, William Cochran, John Tukey, Oscar Kempthorne, and Frank Anscombe, criticized random balanced designs. As a result, supersaturated designs received little attention for the next 30 years. A notable exception is the systematic supersaturated design developed by Booth and Cox (1962). Their designs were not randomly generated, which was a significant departure from Satterthwaite's proposal. They generated their designs with elementary computer search methods. They also developed the basic criteria by which supersaturated designs are judged.

Lin (1993) revisited the supersaturated design concept and stimulated much additional research on the topic. Many authors have proposed methods to construct supersaturated designs. A good survey is in Lin (2000). Most design construction techniques are limited computer search procedures based on simple heuristics [see Lin (1995), Li and Wu (1997), and Holcomb and Carlyle (2002), for example]. Others have proposed methods based on optimal design construction techniques.

Another construction method for supersaturated designs is based on the structure of existing orthogonal designs. These include using the half-fraction of Hadamard matrices [Lin (1993)] and enumerating the two-factor interactions of certain Hadamard matrices. A Hadamard matrix is a square orthogonal matrix whose elements are either -1 or $+1$. When the number of factors in the experiment exceeds the number of runs, the design matrix cannot be orthogonal. Consequently, the factor effect estimates are not independent. An experiment with one dominant factor may contaminate and obscure the contribution of another factor. Supersaturated designs are created to minimize this amount of nonorthogonality between factors. Supersaturated designs can also be constructed using the optimal design approach. The custom designer in JMP uses this approach to constructing supersaturated designs.

The supersaturated designs that are based on the half fraction of a Hadamard matrix are very easy to construct. Table 8.34 is the Plackett–Burman design for $N = 12$ runs and $k = 11$ factors. It is also a Hadamard matrix design. In the table, the design has been sorted by the signs in the last column (Factor 11 or L). This is sometimes called the **branching column**. Now retain only the runs that are positive (say) in column L from the design and delete column L from this group of runs. The resulting design is a supersaturated design for $k = 10$ factors in $N = 6$ runs. We could have used the runs that are negative in column L equally well. This procedure will always produce a supersaturated design for $k = N - 2$ factors in $N/2$ runs. If there are fewer than $N - 2$ factors of interest, additional columns can be removed from the complete design.

■ **TABLE 8.34**
A Supersaturated Design Derived from a 12-Run Hadamard Matrix (Plackett-Burman) Design

Run	*I*	Factor 1 (*A*)	Factor 2 (*B*)	Factor 3 (*C*)	Factor 4 (*D*)	Factor 5 (*E*)	Factor 6 (*F*)	Factor 7 (*G*)	Factor 8 (*H*)	Factor 9 (*J*)	Factor 10 (*K*)	Factor 11 (*L*)
1	+	−	+	+	+	−	+	+	−	+	−	−
2	+	−	−	−	−	−	−	−	−	−	−	−
3	+	+	−	−	−	+	+	+	−	+	+	−
4	+	−	−	+	+	+	−	+	+	−	+	−
5	+	+	+	+	−	+	+	−	+	−	−	−
6	+	+	+	−	+	−	−	−	+	+	+	−
7	+	−	+	+	−	+	−	−	−	+	+	+
8	+	+	+	−	+	+	−	+	−	−	−	+
9	+	+	−	+	−	−	−	+	+	+	−	+
10	+	+	−	+	+	−	+	−	−	−	+	+
11	+	−	−	−	+	+	+	−	+	+	−	+
12	+	−	+	−	−	−	+	+	+	−	+	+

Supersaturated designs are typically analyzed by regression model-fitting methods, such as the forward selection method we have illustrated previously. In this procedure, variables are selected one at a time for inclusion in the model until no other variables appear useful in explaining the response. Abraham, Chipman, and Vijayan (1999) and Holcomb, Montgomery, and Carlyle (2003) have studied analysis methods for supersaturated designs. Generally, these designs can experience large type I and type II errors, but some analysis methods can be tuned to emphasize type I errors so that the type II error rate will be moderate. In a factor screening situation, it is usually more important not to exclude an active factor than it is to conclude that inactive factors are important, so type I errors are less critical than type II errors. However, because both error rates can be large, the philosophy in using a supersaturated design should be to eliminate a large portion of the inactive factors, and not to clearly identify the few important or active factors. Holcomb, Montgomery, and Carlyle (2003) found that some types of supersaturated designs perform better than others with respect to type I and type II errors. Generally, the designs produced by search algorithms were outperformed by designs constructed from standard orthogonal designs. Supersaturated designs created using the *D*-optimality criterion also usually work well.

Supersaturated designs have not had widespread use. However, they are an interesting and potentially useful method for experimentation with systems where there are many variables and only a very few of these are expected to produce large effects.

8.9 Summary

This chapter has introduced the 2^{k-p} fractional factorial design. We have emphasized the use of these designs in screening experiments to quickly and efficiently identify the subset of factors that are active and to provide some information on interaction. The projective property of these designs makes it possible in many cases to examine the active factors in more detail. Sequential assembly of these designs via fold over is a very effective way to gain additional information about interactions that an initial experiment may identify as possibly important.

In practice, 2^{k-p} fractional factorial designs with $N = 4, 8, 16$, and 32 runs are highly useful. Table 8.28 summarizes these designs, identifying how many factors can be used with each design to obtain various types of screening experiments. For example, the 16-run design is a full factorial for 4 factors, a one-half fraction for 5 factors, a resolution IV fraction for 6 to 8 factors, and a resolution III fraction for 9 to 15 factors. All of these designs may be constructed using the methods discussed in this chapter, and many of their alias structures are shown in Appendix Table VIII.

8.10 Problems

8.1 Suppose that in the chemical process development experiment described in Problem 6.11, it was only possible to run a one-half fraction of the 2^4 design. Construct the design and perform the statistical analysis, using the data from replicate I.

8.2 Suppose that in Problem 6.19, only a one-half fraction of the 2^4 design could be run. Construct the design and perform the analysis, using the data from replicate I.

8.3 Consider the plasma etch experiment described in Example 6.1. Suppose that only a one-half fraction of the design could be run. Set up the design and analyze the data.

8.4 Problem 6.30 describes a process improvement study in the manufacturing process of an integrated circuit. Suppose that only eight runs could be made in this process. Set up an appropriate 2^{5-2} design and find the alias structure. Use the appropriate observations from Problem 6.28 as the observations in this design and estimate the factor effects. What conclusions can you draw?

8.5 ***Continuation of Problem 8.4.*** Suppose you have made the eight runs in the 2^{5-2} design in Problem 8.4. What additional runs would be required to identify the factor effects that are of interest? What are the alias relationships in the combined design?

8.6 In Example 6.10, a 2^4 factorial design was used to improve the response rate to a credit card mail marketing offer. Suppose that the researchers had used the 2^{4-1} fractional factorial design with $I = ABCD$ instead. Set up the design and select the responses for the runs from the full factorial data in Example 6.6. Analyze the data and draw conclusions. Compare your findings with those from the full factorial in Example 6.6.

8.7 ***Continuation of Problem 8.6.*** In Problem 6.6, we found that all four main effects and the two-factor AB interaction were significant. Show that if the alternate fraction ($I = -ABCD$) is added to the 2^{4-1} design in Problem 8.6 that the analysis of the results from the combined design produce results identical to those found in Problem 6.6.

8.8 ***Continuation of Problem 8.6.*** Reconsider the 2^{4-1} fractional factorial design with $I = ABCD$ from Problem 8.6. Set a partial fold over of this fraction to isolate the AB interaction. Select the appropriate set of responses from the full factorial data in Example 6.6 and analyze the resulting data.

8.9 R. D. Snee ("Experimenting with a Large Number of Variables," in *Experiments in Industry: Design, Analysis and Interpretation of Results*, by R. D. Snee, L. B. Hare, and J. B. Trout, Editors, ASQC, 1985) describes an experiment in which a 2^{5-1} design with $I = ABCDE$ was used to investigate the effects of five factors on the color of a chemical product. The factors are A = solvent/reactant, B = catalyst/reactant, C = temperature, D = reactant purity, and E = reactant pH. The responses obtained are as follows:

$e =$	-0.63	$d =$	6.79
$a =$	2.51	$ade =$	5.47
$b =$	-2.68	$bde =$	3.45
$abe =$	1.66	$abd =$	5.68
$c =$	2.06	$cde =$	5.22
$ace =$	1.22	$acd =$	4.38
$bce =$	-2.09	$bcd =$	4.30
$abc =$	1.93	$abcde =$	4.05

(a) Prepare a normal probability plot of the effects. Which effects seem active?

(b) Calculate the residuals. Construct a normal probability plot of the residuals and plot the residuals versus the fitted values. Comment on the plots.

(c) If any factors are negligible, collapse the 2^{5-1} design into a full factorial in the active factors. Comment on the resulting design, and interpret the results.

8.10 An article by J. J. Pignatiello Jr. and J. S. Ramberg in the *Journal of Quality Technology* (Vol. 17, 1985, pp. 198–206) describes the use of a replicated fractional factorial to investigate the effect of five factors on the free height of leaf springs used in an automotive application. The factors are A = furnace temperature, B = heating time, C = transfer time, D = hold down time, and E = quench oil temperature. The data are shown in Table P8.1.

■ **TABLE P8.1**
Leaf Spring Experiment

A	B	C	D	E	Free Height		
−	−	−	−	−	7.78	7.78	7.81
+	−	−	+	−	8.15	8.18	7.88
−	+	−	+	−	7.50	7.56	7.50
+	+	−	−	−	7.59	7.56	7.75
−	−	+	+	−	7.54	8.00	7.88
+	−	+	−	−	7.69	8.09	8.06
−	+	+	−	−	7.56	7.52	7.44
+	+	+	+	−	7.56	7.81	7.69
−	−	−	−	+	7.50	7.25	7.12
+	−	−	+	+	7.88	7.88	7.44
−	+	−	+	+	7.50	7.56	7.50
+	+	−	−	+	7.63	7.75	7.56
−	−	+	+	+	7.32	7.44	7.44
+	−	+	−	+	7.56	7.69	7.62
−	+	+	−	+	7.18	7.18	7.25
+	+	+	+	+	7.81	7.50	7.59

(a) Write out the alias structure for this design. What is the resolution of this design?

(b) Analyze the data. What factors influence the mean free height?

(c) Calculate the range and standard deviation of the free height for each run. Is there any indication that any of these factors affects variability in the free height?

(d) Analyze the residuals from this experiment, and comment on your findings.

(e) Is this the best possible design for five factors in 16 runs? Specifically, can you find a fractional design for five factors in 16 runs with a higher resolution than this one?

8.11 An article in *Industrial and Engineering Chemistry* ("More on Planning Experiments to Increase Research Efficiency," 1970, pp. 60–65) uses a 2^{5-2} design to investigate the effect of A = condensation temperature, B = amount of material 1, C = solvent volume, D = condensation time, and E = amount of material 2 on yield. The results obtained are as follows:

$$e = 23.2 \quad ad = 16.9 \quad cd = 23.8 \quad bde = 16.8$$
$$ab = 15.5 \quad bc = 16.2 \quad ace = 23.4 \quad abcde = 18.1$$

(a) Verify that the design generators used were $I = ACE$ and $I = BDE$.

(b) Write down the complete defining relation and the aliases for this design.

(c) Estimate the main effects.

(d) Prepare an analysis of variance table. Verify that the AB and AD interactions are available to use as error.

(e) Plot the residuals versus the fitted values. Also construct a normal probability plot of the residuals. Comment on the results.

8.12 Consider the leaf spring experiment in Problem 8.10. Suppose that factor E (quench oil temperature) is very difficult to control during manufacturing. Where would you set factors A, B, C, and D to reduce variability in the free height as much as possible regardless of the quench oil temperature used?

8.13 Construct a 2^{7-2} design by choosing two four-factor interactions as the independent generators. Write down the complete alias structure for this design. Outline the analysis of variance table. What is the resolution of this design?

8.14 Consider the 2^5 design in Problem 6.30. Suppose that only a one-half fraction could be run. Furthermore, two days were required to take the 16 observations, and it was necessary to confound the 2^{5-1} design in two blocks. Construct the design and analyze the data.

8.15 Analyze the data in Problem 6.32 as if it came from a 2^{4-1}_{IV} design with $I = ABCD$. Project the design into a full factorial in the subset of the original four factors that appear to be significant.

8.16 Repeat Problem 8.15 using $I = -ABCD$. Does the use of the alternate fraction change your interpretation of the data?

8.17 Project the 2^{4-1}_{IV} design in Example 8.1 into two replicates of a 2^2 design in the factors A and B. Analyze the data and draw conclusions.

8.18 Construct a 2^{5-2}_{III} design. Determine the effects that may be estimated if a full fold over of this design is performed.

8.19 Construct a 2^{6-3}_{III} design. Determine the effects that may be estimated if a full fold over of this design is performed.

8.20 Consider the 2^{6-3}_{III} design in Problem 8.19. Determine the effects that may be estimated if a single factor fold over of this design is run with the signs for factor A reversed.

8.21 Fold over the 2^{7-4}_{III} design in Table 8.19 to produce an eight-factor design. Verify that the resulting design is a 2^{8-4}_{IV} design. Is this a minimal design?

8.22 Fold over a 2^{5-2}_{III} design to produce a six-factor design. Verify that the resulting design is a 2^{6-2}_{IV} design. Compare this design to the 2^{6-2}_{IV} design in Table 8.10.

8.23 An industrial engineer is conducting an experiment using a Monte Carlo simulation model of an inventory system. The independent variables in her model are the order quantity (A), the reorder point (B), the setup cost (C), the backorder cost (D), and the carrying cost rate (E). The response variable is average annual cost. To conserve computer time, she decides to investigate these factors using a 2^{5-2}_{III} design with $I = ABD$ and $I = BCE$. The results she obtains are $de = 95$,

$ae = 134, b = 158, abd = 190, cd = 92, ac = 187, bce = 155,$ and $abcde = 185$.

(a) Verify that the treatment combinations given are correct. Estimate the effects, assuming three-factor and higher interactions are negligible.

(b) Suppose that a second fraction is added to the first, for example, $ade = 136, e = 93, ab = 187, bd = 153, acd = 139, c = 99, abce = 191,$ and $bcde = 150$. How was this second fraction obtained? Add this data to the original fraction, and estimate the effects.

(c) Suppose that the fraction $abc = 189, ce = 96, bcd = 154, acde = 135, abe = 193, bde = 152, ad = 137,$ and $(1) = 98$ was run. How was this fraction obtained? Add this data to the original fraction and estimate the effects.

8.24 Construct a 2^{5-1} design. Show how the design may be run in two blocks of eight observations each. Are any main effects or two-factor interactions confounded with blocks?

8.25 Construct a 2^{7-2} design. Show how the design may be run in four blocks of eight observations each. Are any main effects or two-factor interactions confounded with blocks?

8.26 ***Nonregular fractions of the 2^k [John (1971)].*** Consider a 2^4 design. We must estimate the four main effects and the six two-factor interactions, but the full 2^4 factorial cannot be run. The largest possible block size contains 12 runs. These 12 runs can be obtained from the four one-quarter replicates defined by $I = \pm AB = \pm ACD = \pm BCD$ by omitting the principal fraction. Show how the remaining three 2^{4-2} fractions can be combined to estimate the required effects, assuming three-factor and higher interactions are negligible. This design could be thought of as a three-quarter fraction.

8.27 Carbon anodes used in a smelting process are baked in a ring furnace. An experiment is run in the furnace to determine which factors influence the weight of packing material that is stuck to the anodes after baking. Six variables are of interest, each at two levels: A = pitch/fines ratio (0.45, 0.55), B = packing material type (1, 2), C = packing material temperature (ambient, 325°C), D = flue location (inside, outside), E = pit temperature(ambient, 195°C), and F = delay time before packing (zero, 24 hours). A 2^{6-3} design is run, and three replicates are obtained at each of the design points. The weight of packing material stuck to the anodes is measured in grams. The data in run order are as follows: abd = (984, 826, 936); $abcdef$ = (1275, 976, 1457); be = (1217, 1201, 890); af = (1474, 1164, 1541); def = (1320, 1156, 913); cd = (765, 705, 821); ace= (1338, 1254, 1294); and bcf = (1325, 1299, 1253). We wish to minimize the amount of stuck packing material.

(a) Verify that the eight runs correspond to a 2^{6-3}_{III} design. What is the alias structure?

(b) Use the average weight as a response. What factors appear to be influential?

(c) Use the range of the weights as a response. What factors appear to be influential?

(d) What recommendations would you make to the process engineers?

8.28 A 16-run experiment was performed in a semiconductor manufacturing plant to study the effects of six factors on the curvature or camber of the substrate devices produced. The six variables and their levels are shown in Table P8.2.

(a) What type of design did the experimenters use?

(b) What are the alias relationships in this design?

(c) Do any of the process variables affect average camber?

(d) Do any of the process variables affect the variability in camber measurements?

(e) If it is important to reduce camber as much as possible, what recommendations would you make?

8.29 A spin coater is used to apply photoresist to a bare silicon wafer. This operation usually occurs early in the semiconductor manufacturing process, and the average coating thickness and the variability in the coating thickness have an important impact on downstream manufacturing steps. Six variables are used in the experiment. The variables and their high and low levels are as follows:

Factor	Low Level	High Level
Final spin speed	7350 rpm	6650 rpm
Acceleration rate	5	20
Volume of resist applied	3 cc	5 cc
Time of spin	14 sec	6 sec
Resist batch variation	Batch 1	Batch 2
Exhaust pressure	Cover off	Cover on

The experimenter decides to use a 2^{6-1} design and to make three readings on resist thickness on each test wafer. The data are shown in Table P8.4.

(a) Verify that this is a 2^{6-1} design. Discuss the alias relationships in this design.

(b) What factors appear to affect average resist thickness?

(c) Because the volume of resist applied has little effect on average thickness, does this have any important practical implications for the process engineers?

■ TABLE P8.2
Factor Levels for the Experiment in Problem 8.28

Run	Lamination Temperature (°C)	Lamination Time (sec)	Lamination Pressure (tn)	Firing Temperature (°C)	Firing Cycle Time (h)	Firing Dew Point (°C)
1	55	10	5	1580	17.5	20
2	75	10	5	1580	29	26
3	55	25	5	1580	29	20
4	75	25	5	1580	17.5	26
5	55	10	10	1580	29	26
6	75	10	10	1580	17.5	20
7	55	25	10	1580	17.5	26
8	75	25	10	1580	29	20
9	55	10	5	1620	17.5	26
10	75	10	5	1620	29	20
11	55	25	5	1620	29	26
12	75	25	5	1620	17.5	20
13	55	10	10	1620	29	20
14	75	10	10	1620	17.5	26
15	55	25	10	1620	17.5	20
16	75	25	10	1620	29	26

■ TABLE P8.3
Data from the Experiment in Problem 8.28

Run	Camber for Replicate (in./in.) 1	2	3	4	Total (10^{-4}in./in.)	Mean (10^{-4}in./in.)	Standard Deviation
1	0.0167	0.0128	0.0149	0.0185	629	157.25	24.418
2	0.0062	0.0066	0.0044	0.0020	192	48.00	20.976
3	0.0041	0.0043	0.0042	0.0050	176	44.00	4.083
4	0.0073	0.0081	0.0039	0.0030	223	55.75	25.025
5	0.0047	0.0047	0.0040	0.0089	223	55.75	22.410
6	0.0219	0.0258	0.0147	0.0296	920	230.00	63.639
7	0.0121	0.0090	0.0092	0.0086	389	97.25	16.029
8	0.0255	0.0250	0.0226	0.0169	900	225.00	39.42
9	0.0032	0.0023	0.0077	0.0069	201	50.25	26.725
10	0.0078	0.0158	0.0060	0.0045	341	85.25	50.341
11	0.0043	0.0027	0.0028	0.0028	126	31.50	7.681
12	0.0186	0.0137	0.0158	0.0159	640	160.00	20.083
13	0.0110	0.0086	0.0101	0.0158	455	113.75	31.12
14	0.0065	0.0109	0.0126	0.0071	371	92.75	29.51
15	0.0155	0.0158	0.0145	0.0145	603	150.75	6.75
16	0.0093	0.0124	0.0110	0.0133	460	115.00	17.45

Each run was replicated four times, and a camber measurement was taken on the substrate. The data are shown in Table P8.3.

■ **TABLE P8.4**
Data for Problem 8.29

	A	*B*	*C*	*D*	*E*	*F*	Resist Thickness				
Run	Volume	Batch	Time (sec)	Speed	Acc.	Cover	Left	Center	Right	Avg.	Range
1	5	Batch 2	14	7350	5	Off	4531	4531	4515	4525.7	16
2	5	Batch 1	6	7350	5	Off	4446	4464	4428	4446	36
3	3	Batch 1	6	6650	5	Off	4452	4490	4452	4464.7	38
4	3	Batch 2	14	7350	20	Off	4316	4328	4308	4317.3	20
5	3	Batch 1	14	7350	5	Off	4307	4295	4289	4297	18
6	5	Batch 1	6	6650	20	Off	4470	4492	4495	4485.7	25
7	3	Batch 1	6	7350	5	On	4496	4502	4482	4493.3	20
8	5	Batch 2	14	6650	20	Off	4542	4547	4538	4542.3	9
9	5	Batch 1	14	6650	5	Off	4621	4643	4613	4625.7	30
10	3	Batch 1	14	6650	5	On	4653	4670	4645	4656	25
11	3	Batch 2	14	6650	20	On	4480	4486	4470	4478.7	16
12	3	Batch 1	6	7350	20	Off	4221	4233	4217	4223.7	16
13	5	Batch 1	6	6650	5	On	4620	4641	4619	4626.7	22
14	3	Batch 1	6	6650	20	On	4455	4480	4466	4467	25
15	5	Batch 2	14	7350	20	On	4255	4288	4243	4262	45
16	5	Batch 2	6	7350	5	On	4490	4534	4523	4515.7	44
17	3	Batch 2	14	7350	5	On	4514	4551	4540	4535	37
18	3	Batch 1	14	6650	20	Off	4494	4503	4496	4497.7	9
19	5	Batch 2	6	7350	20	Off	4293	4306	4302	4300.3	13
20	3	Batch 2	6	7350	5	Off	4534	4545	4512	4530.3	33
21	5	Batch 1	14	6650	20	On	4460	4457	4436	4451	24
22	3	Batch 2	6	6650	5	On	4650	4688	4656	4664.7	38
23	5	Batch 1	14	7350	20	Off	4231	4244	4230	4235	14
24	3	Batch 2	6	7350	20	On	4225	4228	4208	4220.3	20
25	5	Batch 1	14	7350	5	On	4381	4391	4376	4382.7	15
26	3	Batch 2	6	6650	20	Off	4533	4521	4511	4521.7	22
27	3	Batch 1	14	7350	20	On	4194	4230	4172	4198.7	58
28	5	Batch 2	6	6650	5	Off	4666	4695	4672	4677.7	29
29	5	Batch 1	6	7350	20	On	4180	4213	4197	4196.7	33
30	5	Batch 2	6	6650	20	On	4465	4496	4463	4474.7	33
31	5	Batch 2	14	6650	5	On	4653	4685	4665	4667.7	32
32	3	Batch 2	14	6650	5	Off	4683	4712	4677	4690.7	35

(d) Project this design into a smaller design involving only the significant factors. Graphically display the results. Does this aid in interpretation?

(e) Use the range of resist thickness as a response variable. Is there any indication that any of these factors affect the variability in resist thickness?

(f) Where would you recommend that the engineers run the process?

8.30 Harry Peterson-Nedry (a friend of the author) owns a vineyard and winery in Newberg, Oregon. He grows several varieties of grapes and produces wine. Harry has used factorial designs for process and product development in the winemaking segment of the business. This problem describes the experiment conducted for the 1985 Pinot Noir. Eight variables, shown in Table P8.5, were originally studied in this experiment:

■ **TABLE P8.5**
Factors and Levels for the Winemaking Experiment

Variable	Low Level (−)	High Level (+)
A = Pinot Noir clone	Pommard	Wadenswil
B = Oak type	Allier	Troncais
C = Age of barrel	Old	New
D = Yeast/skin contact	Champagne	Montrachet
E = Stems	None	All
F = Barrel toast	Light	Medium
G = Whole cluster	None	10%
H = Fermentation temperature	Low (75°F max)	High (92°F max)

Harry decided to use a 2_{IV}^{8-4} design with 16 runs. The wine was taste-tested by a panel of experts on March 8, 1986. Each expert ranked the 16 samples of wine tasted, with rank 1 being the best. The design and the taste-test panel results are shown in Table P8.6.

(a) What are the alias relationships in the design selected by Harry?

(b) Use the average ranks ($\bar{y}$) as a response variable. Analyze the data and draw conclusions. You will find it helpful to examine a normal probability plot of the effect estimates.

(c) Use the standard deviation of the ranks (or some appropriate transformation such as log s) as a response variable. What conclusions can you draw about the effects of the eight variables on variability in wine quality?

(d) After looking at the results, Harry decides that one of the panel members (DCM) knows more about beer than he does about wine, so they decide to delete his ranking.

■ **TABLE P8.6**
Design and Results for Wine Tasting Experiment

	Variable								Panel Rankings					Summary	
Run	A	B	C	D	E	F	G	H	HPN	JPN	CAL	DCM	RGB	$\bar{y}$	s
1	−	−	−	−	−	−	−	−	12	6	13	10	7	9.6	3.05
2	+	−	−	−	−	+	+	+	10	7	14	14	9	10.8	3.11
3	−	+	−	−	+	−	+	+	14	13	10	11	15	12.6	2.07
4	+	+	−	−	+	+	−	−	9	9	7	9	12	9.2	1.79
5	−	−	+	−	+	+	+	−	8	8	11	8	10	9.0	1.41
6	+	−	+	−	+	−	−	+	16	12	15	16	16	15.0	1.73
7	−	+	+	−	−	+	−	+	6	5	6	5	3	5.0	1.22
8	+	+	+	−	−	−	+	−	15	16	16	15	14	15.2	0.84
9	−	−	−	+	+	+	−	+	1	2	3	3	2	2.2	0.84
10	+	−	−	+	+	−	+	−	7	11	4	7	6	7.0	2.55
11	−	+	−	+	−	+	+	−	13	3	8	12	8	8.8	3.96
12	+	+	−	+	−	−	−	+	3	1	5	1	4	2.8	1.79
13	−	−	+	+	−	−	+	+	2	10	2	4	5	4.6	3.29
14	+	−	+	+	−	+	−	−	4	4	1	2	1	2.4	1.52
15	−	+	+	+	+	−	−	−	5	15	9	6	11	9.2	4.02
16	+	+	+	+	+	+	+	+	11	14	12	13	13	12.6	1.14

What effect would this have on the results and conclusions from parts (b) and (c)?

(e) Suppose that just before the start of the experiment, Harry and Judy discovered that the eight new barrels they ordered from France for use in the experiment would not arrive in time, and all 16 runs would have to be made with old barrels. If Harry just drops column *C* from their design, what does this do to the alias relationships? Does he need to start over and construct a new design?

(f) Harry knows from experience that some treatment combinations are unlikely to produce good results. For example, the run with all eight variables at the high level generally results in a poorly rated wine. This was confirmed in the March 8, 1986 taste test. He wants to set up a new design for their 1986 Pinot Noir using these same eight variables, but he does not want to make the run with all eight factors at the high level. What design would you suggest?

8.31 Consider the isatin yield data from the experiment described in Problem 6.42. The original experiment was a 2^4 full factorial. Suppose that the original experimenters could only afford eight runs. Set up the 2^{4-1} fractional factorial design with $I = ABCD$ and select the responses for the runs from the full factorial data in Problem 6.42. Analyze the data and draw conclusions. Compare your findings with those from the full factorial in Problem 6.42.

8.32 Consider the 2^5 factorial in Problem 6.43. Suppose that the experimenters could only afford 16 runs. Set up the 2^{5-1} fractional factorial design with $I = ABCDE$ and select the responses for the runs from the full factorial data in Problem 6.43.

(a) Analyze the data and draw conclusions.

(b) Compare your findings with those from the full factorial in Problem 6.43.

(c) Are there any potential interactions that need further study? What additional runs do you recommend? Select these runs from the full factorial design in Problem 6.43 and analyze the new design. Discuss your conclusions.

8.33 Consider the 2^4 factorial experiment for surfactin production in Problem 6.44. Suppose that the experimenters could only afford eight runs. Set up the 2^{4-1} fractional factorial design with $I = ABCD$ and select the responses for the runs from the full factorial data in Problem 6.44.

(a) Analyze the data and draw conclusions.

(b) Compare your findings with those from the full factorial in Problem 6.44.

8.34 Consider the 2^4 factorial experiment in Problem 6.46. Suppose that the experimenters could only afford eight runs. Set up the 2^{4-1} fractional factorial design with $I = ABCD$ and select the responses for the runs from the full factorial data in Problem 6.46.

(a) Analyze the data for all of the responses and draw conclusions.

(b) Compare your findings with those from the full factorial in Problem 6.46.

8.35 An article in the *Journal of Chromatography A* ("Simultaneous Supercritical Fluid Derivatization and Extraction of Formaldehyde by the Hantzsch Reaction," 2000, Vol. 896, pp. 51–59) describes an experiment where the Hantzsch reaction is used to produce the chemical derivatization of formaldehyde in a supercritical medium. Pressure, temperature, and other parameters such as static and dynamic extraction time must be optimized to increase the yield of this kinetically controlled reaction. A 2^{5-1} fractional factorial design with one center run was used to study the significant parameters affecting the supercritical process in terms of resolution and sensitivity. Ultraviolet–visible spectrophotometry was used as the detection technique. The experimental design and the responses are shown in Table P8.7.

(a) Analyze the data from this experiment and draw conclusions.

(b) Analyze the residuals. Are there any concerns about model adequacy or violations of assumptions?

(c) Does the single center point cause any concerns about curvature or the possible need for second-order terms?

(d) Do you think that running one center point was a good choice in this design?

8.36 An article in *Thin Solid Films* (504, "A Study of Si/SiGe Selective Epitaxial Growth by Experimental Design Approach," 2006, Vol. 504, pp. 95–100) describes the use of a fractional factorial design to investigate the sensitivity of low-temperature (740–760°C) Si/SiGe selective epitaxial growth to changes in five factors and their two-factor interactions. The five factors are SiH_2Cl_2, GeH_4, HCl, B_2H_6 and temperature. The factor levels studied are as follows:

	Levels	
Factors	**(−)**	**(+)**
SiH_2Cl_2 (sccm)	8	12
GeH_4 (sccm)	7.2	10.8
HCl (sccm)	3.2	4.8
B_2H_6 (sccm)	4.4	6.6
Temperature (°C)	740	760

Table P8.8 contains the design matrix and the three measured responses. Bede RADS Mercury software based on the

■ **TABLE P8.7**

The 2^{5-1} Fractional Factorial Design for Problem 8.35

Experiment	P (MPa)	T (°C)	s (min)	d (min)	c (μl)	Resolution	Sensitivity
1	13.8	50	2	2	100	0.00025	0.057
2	55.1	50	2	2	10	0.33333	0.094
3	13.8	120	2	2	10	0.02857	0.017
4	55.1	120	2	2	100	0.20362	1.561
5	13.8	50	15	2	10	0.00027	0.010
6	55.1	50	15	2	100	0 52632	0.673
7	13.8	120	15	2	100	0.00026	0.028
8	55.1	120	15	2	10	0.52632	1.144
9	13.8	50	2	15	10	0 42568	0.142
10	55.1	50	2	15	100	0.60150	0.399
11	13.8	120	2	15	100	0.06098	0.767
12	55.1	120	2	15	10	0.74165	1.086
13	13.8	50	15	15	100	0.08780	0.252
14	55.1	50	15	15	10	0.40000	0.379
15	13.8	120	15	15	10	0.00026	0.028
16	55.1	120	15	15	100	0.28091	3.105
Central	34.5	85	8.5	8.5	55	0.75000	1.836

■ **TABLE P8.8**

The Epitaxial Growth Experiment in Problem 8.36

Run Order	Factors A	B	C	D	E	Si Cap Thickness (Å)	SiGe Thickness (Å)	Ge Concentration (at.%)
7	−	−	−	−	+	371.18	475.05	8.53
17	−	−	−	+	−	152.36	325.21	9.74
6	−	−	+	−	−	91.69	258.60	9.78
10	−	−	+	+	+	234.48	392.27	9.14
16	−	+	−	−	−	151.36	440.37	12.13
2	−	+	−	+	+	324.49	623.60	10.68
15	−	+	+	−	+	215.91	518.50	11.42
4	−	+	+	+	−	97.91	356.67	12.96
9	+	−	−	−	−	186.07	320.95	7.87
13	+	−	−	+	+	388.69	487.16	7.14
18	+	−	+	−	+	277.39	422.35	6.40
5	+	−	+	+	−	131.25	241.51	8.54
14	+	+	−	−	+	378.41	630.90	9.17
3	+	+	−	+	−	192.65	437.53	10.35
1	+	+	+	−	−	128.99	346.22	10.95
12	+	+	+	+	+	298.40	526.69	9.73
8	0	0	0	0	0	215.70	416.44	9.78
11	0	0	0	0	0	212.21	419.24	9.80

Takagi–Taupin dynamical scattering theory was used to extract the Si cap thickness, SiGe thickness, and Ge concentration of each sample.

(a) What design did the experimenters use? What is the defining relation?

(b) Will the experimenters be able to estimate all main effects and two-factor interactions with this experimental design?

(c) Analyze all three responses and draw conclusions.

(d) Is there any indication of curvature in the responses?

(e) Analyze the residuals and comment on model adequacy.

8.37 An article in *Soldering & Surface Mount Technology* ("Characterization of a Solder Paste Printing Process and Its Optimization," 1999, Vol. 11, No. 3, pp. 23–26) describes the use of a 2^{8-3} fractional factorial experiment to study the effect of eight factors on two responses; percentage volume matching (PVM) – the ratio of the actual printed solder paste volume to the designed volume; and nonconformities per unit (NPU)—the number of solder paste printing defects determined by visual inspection ($20'$ magnification) after printing according to an industry workmanship standard. The factor levels are shown below and the test matrix and response data are shown in Table P8.9.

■ **TABLE P8.9**
The Solder Paste Experiment

Run	Parameters									NPU
Order	*A*	*B*	*C*	*D*	*E*	*F*	*G*	*H*	PVM	(%)
4	−	−	−	−	−	−	−	+	1.00	5
13	+	−	−	−	−	+	+	+	1.04	13
6	−	+	−	−	−	+	+	−	1.02	16
3	+	+	−	−	−	−	−	−	0.99	12
19	−	−	+	−	−	+	−	−	1.02	15
25	+	−	+	−	−	−	+	−	1.01	9
21	−	+	+	−	−	−	+	+	1.01	12
14	+	+	+	−	−	+	−	+	1.03	17
10	−	−	−	+	−	−	+	−	1.04	21
22	+	−	−	+	−	+	−	−	1.14	20
1	−	+	−	+	−	+	−	+	1.20	25
2	+	+	−	+	−	−	+	+	1.13	21
30	−	−	+	+	−	+	+	+	1.14	25
8	+	−	+	+	−	−	−	+	1.07	13
9	−	+	+	+	−	−	−	−	1.06	20
20	+	+	+	+	−	+	+	−	1.13	26
17	−	−	−	−	+	−	−	−	1.02	10
18	+	−	−	−	+	+	+	−	1.10	13
5	−	+	−	−	+	+	+	+	1.09	17
26	+	+	−	−	+	−	−	+	0.96	13
31	−	−	+	−	+	+	−	+	1.02	14
11	+	−	+	−	+	−	+	+	1.07	11
29	−	+	+	−	+	−	+	−	0.98	10
23	+	+	+	−	+	+	−	−	0.95	14
32	−	−	−	+	+	−	+	+	1.10	28
7	+	−	−	+	+	+	−	+	1.12	24
15	−	+	−	+	+	+	−	−	1.19	22
27	+	+	−	+	+	−	+	−	1.13	15
12	−	−	+	+	+	+	+	−	1.20	21
28	+	−	+	+	+	−	−	−	1.07	19
24	−	+	+	+	+	−	−	+	1.12	21
16	+	+	+	+	+	+	+	+	1.21	27

Parameters	Levels Low (−)	High (+)
A. Squeegee pressure, *MPa*	0.1	0.3
B. Printing speed, *mm/s*	24	32
C. Squeegee angle, *deg*	45	65
D. Temperature, °*C*	20	28
E. Viscosity, *kCps*	1,100-1,150	1,250-1,300
F. Cleaning interval, *stroke*	8	15
G. Separation speed, *mm/s*	0.4	0.8
H. Relative humidity, %	30	70

(a) Verify that the generators are $I = ABCF$, $I = ABDG$, and $I = BCDEH$ for this design.

(b) What are the aliases for the main effects and two-factor interactions? You can ignore all interactions of order three and higher.

(c) Analyze both PVM and NPU responses.

(d) Analyze the residual for both responses. Are there any problems with model adequacy?

(e) The ideal value of PVM is unity and the NPU response should be as small as possible. Recommend suitable operating conditions for the process based on the experimental results.

8.38 An article in the *International Journal of Research in Marketing* ("Experimental design on the front lines of marketing: Testing new ideas to increase direct mail sales," 2006, Vol. 23, pp. 309–319) describes the use of a 20-run Plackett–Burman design to investigate the effects of 19 factors to improve the response rate to a direct mail sales campaign to attract new customers to a credit card. The 19 factors are as follows:

Factor	(−) Control	(+) New Idea
A: Envelope teaser	General offer	Product-specific offer
B: Return address	Blind	Add company name
C: "Official" ink-stamp on envelope	Yes	No
D: Postage	Pre-printed	Stamp
E: Additional graphic on envelope	Yes	No
F: Price graphic on letter	Small	Large
G: Sticker	Yes	No
H: Personalize letter copy	No	Yes
I: Copy message	Targeted	Generic
J: Letter headline	Headline 1	Headline 2
K: List of benefits	Standard layout	Creative layout
L: Postscript on letter	Control version	New P.S.
M: Signature	Manager	Senior executive
N: Product selection	Many	Few
O: Value of free gift	High	Low
P: Reply envelope	Control	New style
Q: Information on buckslip	Product info	Free gift info
R: 2nd buckslip	No	Yes
S: Interest rate	Low	High

The 20-run Plackett–Burman design is shown in Table P8.10. Each test combination in Table P8.17 was mailed to 5,000 potential customers, and the response rate is the percentage of customers who responded positively to the offer.

(a) Verify that in this design each main effect is aliased with all two-factor interactions except those that involve that main effect. That is, in the 19-factor design, the main effect for each factor is aliased with all two-factor interactions involving the other 18 factors, or 153 two-factor interactions (18!/2!16!).

(b) Show that for the 20-run Plackett–Burman design in Table P8.17, the weights (or correlations) that multiple the two-factor interactions in each alias chain are either −0.2, +0.2, or −0.6. Of the 153 interactions that are aliased with each main effect, 144 have weights of −0.2 or +0.2, while 9 interactions have weights of −0.6.

(c) Verify that the five largest main effects are *S, G, R, I*, and *J*.

(d) Factors *S* (interest rate) and *G* (presence of a sticker) are by far the largest main effects. The correlation between the main effect of *R* (2nd buckslip) and the *SG* interaction is −0.6. This means that a significant *SG* interaction would bias the estimate of the main effect of *R* by −0.6 times the value of the interaction. This suggests that it may not be the main effect of factor *R* that is important, but the two-factor interaction between *S* and *G*.

(e) Since this design projects into a full factorial in any three factors, obtain the projection in factors *S*, *G*, and *R* and verify that it is a full factorial with some runs replicated. Fit a full factorial model involving all three of these factors and the interactions (you will need to use a regression program to do this). Show that *S*, *G*, and the *SG* interaction are significant.

■ **TABLE P8.10**

The Plackett-Burman Design for the Direct Mail Experiment in Problem 8.38

Test Cell	Envelope Teaser *A*	Return Address *B*	"Official" Ink-stamp on Envelope *C*	Postage *D*	Additional Graphic on Envelope *E*	Price Graphic on Letter *F*	Sticker *G*	Personalize Letter Copy *H*	Copy Message *I*	Letter Headline *J*
1	+	+	−	−	+	+	+	+	−	+
2	−	+	+	−	−	+	+	+	+	−
3	+	−	+	+	−	−	+	+	+	+
4	+	+	−	+	+	−	−	+	+	+
5	−	+	+	−	+	+	−	−	+	+
6	−	−	+	+	−	+	+	−	−	+
7	−	−	−	+	+	−	+	+	−	−
8	−	−	−	−	+	+	−	+	+	−
9	+	−	−	−	−	+	+	−	+	+
10	−	+	−	−	−	−	+	+	−	+
11	+	−	+	−	−	−	−	+	+	−
12	−	+	−	+	−	−	−	−	+	+
13	+	−	+	−	+	−	−	−	−	+
14	+	+	−	+	−	+	−	−	−	−
15	+	+	+	−	+	−	+	−	−	−
16	+	+	+	+	−	+	−	+	−	−
17	−	+	+	+	+	−	+	−	+	−
18	−	−	+	+	+	+	−	+	−	+
19	+	−	−	+	+	+	+	−	+	−
20	−	−	−	−	−	−	−	−	−	−

List of Benefits *K*	Postscript on Letter *L*	Signature *M*	Product Selection *N*	Value of Free gift *O*	Reply Envelope *P*	Information on Buckslip *Q*	2nd Buckslip *R*	Interest Rate *S*	Orders	Response Rate
−	+	−	−	−	−	+	+	−	52	1.04%
+	−	+	−	−	−	−	+	+	38	0.76%
−	+	−	+	−	−	−	−	+	42	0.84%
+	−	+	−	+	−	−	−	−	134	2.68%
+	+	−	+	−	+	−	−	−	104	2.08%
+	+	+	−	+	−	+	−	−	60	1.20%
+	+	+	+	−	+	−	+	−	61	1.22%
−	+	+	+	+	−	+	−	+	68	1.36%
−	−	+	+	+	+	−	+	−	57	1.14%
+	−	−	+	+	+	+	−	+	30	0.60%
+	+	−	−	+	+	+	+	−	108	2.16%
−	+	+	−	−	+	+	+	+	39	0.78%
+	−	+	+	−	−	+	+	+	40	0.80%
+	+	−	+	+	−	−	+	+	49	0.98%
−	+	+	−	+	+	−	−	+	37	0.74%
−	−	+	+	−	+	+	−	−	99	1.98%
−	−	−	+	+	−	+	+	−	86	1.72%
−	−	−	−	+	+	−	+	+	43	0.86%
+	−	−	−	−	+	+	−	+	47	0.94%
−	−	−	−	−	−	−	−	−	104	2.08%

8.39 Consider the following experiment:

Run	Treatment Combination
1	*d*
2	*ae*
3	*b*
4	*abde*
5	*cde*
6	*ac*
7	*bce*
8	abcd

Answer the following questions about this experiment:

(a) How many factors did this experiment investigate?

(b) How many factors are in the basic design?

(c) Assume that the factors in the experiment are represented by the initial letters of the alphabet (i.e., *A*, *B*, etc.), what are the design generators for the factors beyond the basic design?

(d) Is this design a principal fraction?

(e) What is the complete defining relation?

(f) What is the resolution of this design?

8.40 Consider the following experiment:

Run	Treatment Combination	y
1	(1)	8
2	*ad*	10
3	*bd*	12
4	*ab*	7
5	*cd*	13
6	*ac*	6
7	*bc*	5
8	*abcd*	11

Answer the following questions about this experiment:

(a) How many factors did this experiment investigate?

(b) What is the resolution of this design?

(c) Calculate the estimates of the main effects.

(d) What is the complete defining relation for this design?

8.41 An unreplicated 2^{5-1} fractional factorial experiment with four center points has been run in a chemical process. The response variable is molecular weight. The experimenter has used the following factors:

Factor	Natural Levels	Coded Levels (x's)
***A* - time**	20, 40 (minutes)	−1, 1
***B* - temperature**	160, 180 (deg C)	−1, 1
***C* - concentration**	30, 60 (percent)	−1, 1
***D* - stirring rate**	100, 150 (RPM)	−1, 1
***E* - catalyst type**	1, 2 (Type)	−1. 1

Suppose that the prediction equation that results from this experiment is $\hat{y} = 10 + 3x_1 + 2x_2 - 1x_1x_2$. What is the predicted response at $A = 30$, $B = 165$, $C = 50$, $D = 135$, and $E = 1$?

8.42 An unreplicated 2^{4-1} fractional factorial experiment with four center points has been run. The experimenter has used the following factors:

Factor	Natural Levels	Coded Levels (x's)
***A* - time**	10, 50 (minutes)	−1, 1
***B* - temperature**	200, 300 (deg C)	−1, 1
***C* - concentration**	70, 90 (percent)	−1, 1
***D* - pressure**	260, 300 (psi)	−1, 1

(a) Suppose that the average of the 16 factorial design points is 100 and the average of the center points is 120, what is the sum of squares for pure quadratic curvature?

(b) Suppose that the prediction equation that results from this experiment is $\hat{y} = 50 + 5x_1 + 2x_2 - 2x_1x_2$. Find the predicted response at $A = 20$, $B = 250$, $C = 80$, and $D = 275$.

8.43 An unreplicated 2^{4-1} fractional factorial experiment has been run. The experimenter has used the following factors:

Factor	Natural Levels	Coded Levels (x's)
A	20, 50	−1, 1
B	200, 280	−1, 1
C	50, 100	−1, 1
D	150, 200	−1, 1

(a) Suppose that this design has four center runs that average 100. The average of the 16 factorial design points is 95. What is the sum of squares for pure quadratic curvature?

(b) Suppose that the prediction equation that results from this experiment is $\hat{y} = 100 + -2x_1 + 10x_2 - 4x_1x_2$. What is the predicted response at $A = 41$, $B = 280$, $C = 60$, and $D = 185$?

8.44 A 2^{6-2} factorial experiment with three replicates has been run in a pharmaceutical drug manufacturing process. The experimenter has used the following factors:

Factor	Natural Levels	Coded Levels (x's)
A	50, 100	−1, 1
B	20, 60	−1, 1
C	10, 30	−1, 1
D	12, 18	−1, 1
E	15, 30	−1, 1
F	60, 100	−1, 1

(a) If two main effects and one two-factor interaction are included in the final model, how many degrees of freedom for error will be available?

(b) Suppose that the significant factors are *A*, *C*, *AB*, and *AC*. What other effects need to be included to obtain a hierarchical model?

8.45 Consider the following design:

Run	*A*	*B*	*C*	*D*	*E*	*y*
1	−1	−1	−1	−1	−1	63
2	1	−1	−1	−1	1	21
3	−1	1	−1	−1	1	36
4	1	1	−1	−1	−1	99
5	−1	−1	1	−1	1	24
6	1	−1	1	−1	−1	66
7	−1	1	1	−1	−1	71
8	1	1	1	−1	1	54
9	−1	−1	−1	1	−1	23
10	1	−1	−1	1	1	74
11	−1	1	−1	1	1	80
12	1	1	−1	1	−1	33
13	−1	−1	1	1	1	63
14	1	−1	1	1	−1	21
15	−1	1	1	1	−1	44
16	1	1	1	1	1	96

(a) What is the generator for column *E*?

(b) If *ABC* is confounded with blocks, run 1 above goes in the ______ block. Answer either + or −.

(c) What is the resolution of this design?

(d) (8 pts) Find the estimates of the main effects and their aliases.

8.46 Consider the following design:

Run	*A*	*B*	*C*	*D*	*E*	*y*
1	−1	−1	−1	−1	−1	65
2	1	−1	−1	−1	1	25
3	−1	1	−1	−1	1	30
4	1	1	−1	−1	−1	89
5	−1	−1	1	−1	1	25
6	1	−1	1	−1	−1	60
7	−1	1	1	−1	−1	70
8	1	1	1	−1	1	50
9	−1	−1	−1	1	1	20
10	1	−1	−1	1	−1	70
11	−1	1	−1	1	−1	80
12	1	1	−1	1	1	30
13	−1	−1	1	1	−1	60
14	1	−1	1	1	1	20
15	−1	1	1	1	1	40
16	1	1	1	1	−1	90

(a) What is the generator for column *E*?

(b) If *ABE* is confounded with blocks, run 16 goes in the ______ block. Answer either − or +.

(c) The resolution of this design is ______.

(d) Find the estimates of the main effects and their aliases.

8.47 Consider the following design:

Run	*A*	*B*	*C*	*D*	*E*	*y*
1	−1	−1	−1	1	−1	50
2	1	−1	−1	−1	−1	20
3	−1	1	−1	−1	1	40
4	1	1	−1	1	1	25
5	−1	−1	1	−1	1	45
6	1	−1	1	1	1	30
7	−1	1	1	1	−1	40
8	1	1	1	−1	−1	30

(a) What is the generator for column *D*?

(b) What is the generator for column *E*?

(c) If this design were run in two blocks with the AB interaction confounded with blocks, the run d would be in the block where the sign on AB is ______. Answer either $-$ or $+$.

8.48 Consider the following design:

Std	A	B	C	D	E	y
1	−1	−1	−1	1	1	40
2	1	−1	−1	−1	1	10
3	−1	1	−1	−1	−1	30
4	1	1	−1	1	−1	20
5	−1	−1	1	−1	−1	40
6	1	−1	1	1	−1	30
7	−1	1	1	1	1	20
8	1	1	1	−1	1	30

(a) What is the generator for column D?

(b) What is the generator for column E?

(c) If this design were folded over, what is the resolution of the combined design?

8.49 In an article in *Quality Engineering* ("An Application of Fractional Factorial Experimental Designs," 1988, Vol. 1, pp. 19–23), M. B. Kilgo describes an experiment to determine the effect of CO_2 pressure (A), CO_2 temperature (B), peanut moisture (C), CO_2 flow rate (D), and peanut particle size (E) on the total yield of oil per batch of peanuts (y). The levels that she used for these factors are shown in Table P8.11. She conducted the 16-run fractional factorial experiment shown in Table P8.12.

■ TABLE P8.11
Factor Levels for the Experiment in Problem 8.49

Coded Level	A, Pressure (bar)	B, Temp, (°C)	C, Moisture (% by weight)	D, Flow (liters/min)	E, Part. Size (mm)
−1	415	25	5	40	1.28
1	550	95	15	60	4.05

(a) What type of design has been used? Identify the defining relation and the alias relationships.

(b) Estimate the factor effects and use a normal probability plot to tentatively identify the important factors.

(c) Perform an appropriate statistical analysis to test the hypotheses that the factors identified in part (b) have a significant effect on the yield of peanut oil.

■ TABLE P8.12
The Peanut Oil Experiment

A	B	C	D	E	y
415	25	5	40	1.28	63
550	25	5	40	4.05	21
415	95	5	40	4.05	36
550	95	5	40	1.28	99
415	25	15	40	4.05	24
550	25	15	40	1.28	66
415	95	15	40	1.28	71
550	95	15	40	4.05	54
415	25	5	60	4.05	23
550	25	5	60	1.28	74
415	95	5	60	1.28	80
550	95	5	60	4.05	33
415	25	15	60	1.28	63
550	25	15	60	4.05	21
415	95	15	60	4.05	44
550	95	15	60	1.28	96

(d) Fit a model that could be used to predict peanut oil yield in terms of the factors that you have identified as important.

(e) Analyze the residuals from this experiment and comment on model adequacy.

8.50 A 16-run fractional factorial experiment in 10 factors on sand-casting of engine manifolds was conducted by engineers at the Essex Aluminum Plant of the Ford Motor Company and described in the article "Evaporative Cast Process 3.0 Liter Intake Manifold Poor Sandfill Study," by D. Becknell (*Fourth Symposium on Taguchi Methods*, American Supplier Institute, Dearborn, MI, 1986, pp. 120–130). The purpose was to determine which of 10 factors has an effect on the proportion of defective castings. The design and the resulting proportion of nondefective castings $\hat{p}$ observed on each run are shown in Table P8.13. This is a resolution III fraction with generators $E = CD$, $F = BD$, $G = BC$, $H = AC$, $J = AB$, and $K = ABC$. Assume that the number of castings made at each run in the design is 1000.

(a) Find the defining relation and the alias relationships in this design.

(b) Estimate the factor effects and use a normal probability plot to tentatively identify the important factors.

(c) Fit an appropriate model using the factors identified in part (b).

■ **TABLE P8.13**
The Sand-Casting Experiment

Run	A	B	C	D	E	F	G	H	J	K	$\hat{p}$	Arcsin $\sqrt{\hat{p}}$	F&T's Modification
1	−	−	−	−	+	+	+	+	+	−	0.958	1.364	1.363
2	+	−	−	−	+	+	+	−	−	+	1.000	1.571	1.555
3	−	+	−	−	+	−	−	+	−	+	0.977	1.419	1.417
4	+	+	−	−	+	−	−	−	+	−	0.775	1.077	1.076
5	−	−	+	−	−	+	−	−	+	+	0.958	1.364	1.363
6	+	−	+	−	−	+	−	+	−	−	0.958	1.364	1.363
7	−	+	+	−	−	−	+	−	−	−	0.813	1.124	1.123
8	+	+	+	−	−	−	+	+	+	+	0.906	1.259	1.259
9	−	−	−	+	−	−	+	+	+	−	0.679	0.969	0.968
10	+	−	−	+	−	−	+	−	−	+	0.781	1.081	1.083
11	−	+	−	+	−	+	−	+	−	+	1.000	1.571	1.556
12	+	+	−	+	−	+	−	−	+	−	0.896	1.241	1.242
13	−	−	+	+	+	−	−	−	+	+	0.958	1.364	1.363
14	+	−	+	+	+	−	−	+	−	−	0.818	1.130	1.130
15	−	+	+	+	+	+	+	−	−	−	0.841	1.161	1.160
16	+	+	+	+	+	+	+	+	+	+	0.955	1.357	1.356

(d) Plot the residuals from this model versus the predicted proportion of nondefective castings. Also prepare a normal probability plot of the residuals. Comment on the adequacy of these plots.

(e) In part (d) you should have noticed an indication that the variance of the response is not constant. (Considering that the response is a proportion, you should have expected this.) The previous table also shows a transformation on $\hat{p}$, the arcsin square root, that is a widely used *variance stabilizing transformation* for proportion data (refer to the discussion of variance stabilizing transformations in Chapter 3). Repeat parts (a) through (d) using the transformed response and comment on your results. Specifically, are the residual plots improved?

(f) There is a modification to the arcsin square root transformation, proposed by Freeman and Tukey ("Transformations Related to the Angular and the Square Root," *Annals of Mathematical Statistics*, Vol. 21, 1950, pp. 607–611), that improves its performance in the tails. F&T's modification is

$$[\arcsin\sqrt{n\hat{p}/(n+1)} + \arcsin\sqrt{(n\hat{p}+1)/(n+1)}]/2$$

Rework parts (a) through (d) using this transformation and comment on the results. (For an interesting discussion and analysis of this experiment, refer to "Analysis of Factorial Experiments with Defects or Defectives as the Response," by S. Bisgaard and H. T. Fuller, *Quality Engineering*, Vol. 7, 1994–95, pp. 429–443.)

8.51 A 16-run fractional factorial experiment in nine factors was conducted by Chrysler Motors Engineering and described in the article "Sheet Molded Compound Process Improvement," by P. I. Hsieh and D. E. Goodwin (*Fourth Symposium on Taguchi Methods*, American Supplier Institute, Dearborn, MI, 1986, pp. 13–21). The purpose was to reduce the number of defects in the finish of sheet-molded grill opening panels. The design, and the resulting number of defects, c, observed on each run, is shown in Table P8.14. This is a resolution III fraction with generators $E = BD$, $F = BCD$, $G = AC$, $H = ACD$, and $J = AB$.

(a) Find the defining relation and the alias relationships in this design.

(b) Estimate the factor effects and use a normal probability plot to tentatively identify the important factors.

(c) Fit an appropriate model using the factors identified in part (b).

(d) Plot the residuals from this model versus the predicted number of defects. Also, prepare a normal probability plot of the residuals. Comment on the adequacy of these plots.

■ **TABLE P8.14**
The Grill Defects Experiment

Run	A	B	C	D	E	F	G	H	J	c	$\sqrt{c}$	F&T's Modification
1	−	−	−	−	+	−	+	−	+	56	7.48	7.52
2	+	−	−	−	+	−	−	+	−	17	4.12	4.18
3	−	+	−	−	−	+	+	−	−	2	1.41	1.57
4	+	+	−	−	−	+	−	+	+	4	2.00	2.12
5	−	−	+	−	+	+	−	+	+	3	1.73	1.87
6	+	−	+	−	+	+	+	−	−	4	2.00	2.12
7	−	+	+	−	−	−	−	+	−	50	7.07	7.12
8	+	+	+	−	−	−	+	−	+	2	1.41	1.57
9	−	−	−	+	−	+	+	+	+	1	1.00	1.21
10	+	−	−	+	−	+	−	−	−	0	0.00	0.50
11	−	+	−	+	+	−	+	+	−	3	1.73	1.87
12	+	+	−	+	+	−	−	−	+	12	3.46	3.54
13	−	−	+	+	−	−	−	−	+	3	1.73	1.87
14	+	−	+	+	−	−	+	+	−	4	2.00	2.12
15	−	+	+	+	+	+	−	−	−	0	0.00	0.50
16	+	+	+	+	+	+	+	+	+	0	0.00	0.50

(e) In part (d) you should have noticed an indication that the variance of the response is not constant. (Considering that the response is a count, you should have expected this.) The previous table also shows a transformation on c, the square root, that is a widely used *variance stabilizing transformation* for count data. (Refer to the discussion of variance stabilizing transformations in Chapter 3.) Repeat parts (a) through (d) using the transformed response and comment on your results. Specifically, are the residual plots improved?

(f) There is a modification to the square root transformation, proposed by Freeman and Tukey ("Transformations Related to the Angular and the Square Root," *Annals of Mathematical Statistics*, Vol. 21, 1950, pp. 607–611) that improves its performance. F&T's modification to the square root transformation is

$$[\sqrt{c} + \sqrt{(c+1)}]/2$$

Rework parts (a) through (d) using this transformation and comment on the results. (For an interesting discussion and analysis of this experiment, refer to "Analysis of Factorial Experiments with Defects or Defectives as the Response," by S. Bisgaard and H. T. Fuller, *Quality Engineering*, Vol. 7, 1994–95, pp. 429–443.)

8.52 An experiment is run in a semiconductor factory to investigate the effect of six factors on transistor gain. The design selected is the 2^{6-2}_{IV} shown in Table P8.15.

■ **TABLE P8.15**
The Transistor Gain Experiment

Standard Order	Run Order	A	B	C	D	E	F	Gain
1	2	−	−	−	−	−	−	1455
2	8	+	−	−	−	+	−	1511
3	5	−	+	−	−	+	+	1487
4	9	+	+	−	−	−	+	1596
5	3	−	−	+	−	+	+	1430
6	14	+	−	+	−	−	+	1481
7	11	−	+	+	−	−	−	1458
8	10	+	+	+	−	+	−	1549
9	15	−	−	−	+	−	+	1454
10	13	+	−	−	+	+	+	1517
11	1	−	+	−	+	+	−	1487
12	6	+	+	−	+	−	−	1596
13	12	−	−	+	+	+	−	1446
14	4	+	−	+	+	−	−	1473
15	7	−	+	+	+	−	+	1461
16	16	+	+	+	+	+	+	1563

(a) Use a normal plot of the effects to identify the significant factors.

(b) Conduct appropriate statistical tests for the model identified in part (a).

(c) Analyze the residuals and comment on your findings.

(d) Can you find a set of operating conditions that produce gain of 1500 ± 25?

8.53 Heat treating is often used to carbonize metal parts, such as gears. The thickness of the carbonized layer is a critical output variable from this process, and it is usually measured by performing a carbon analysis on the gear pitch (the top of the gear tooth). Six factors were studied in a 2_{IV}^{6-2} design: A = furnace temperature, B = cycle time, C = carbon concentration, D = duration of the carbonizing cycle, E = carbon concentration of the diffuse cycle, and F = duration of the diffuse cycle. The experiment is shown in Table P8.16.

■ **TABLE P8.16**
The Heat Treating Experiment

Standard Order	Run Order	*A*	*B*	*C*	*D*	*E*	*F*	Pitch
1	5	−	−	−	−	−	−	74
2	7	+	−	−	−	+	−	190
3	8	−	+	−	−	+	+	133
4	2	+	+	−	−	−	+	127
5	10	−	−	+	−	+	+	115
6	12	+	−	+	−	−	+	101
7	16	−	+	+	−	−	−	54
8	1	+	+	+	−	+	−	144
9	6	−	−	−	+	−	+	121
10	9	+	−	−	+	+	+	188
11	14	−	+	−	+	+	−	135
12	13	+	+	−	+	−	−	170
13	11	−	−	+	+	+	−	126
14	3	+	−	+	+	−	−	175
15	15	−	+	+	+	−	+	126
16	4	+	+	+	+	+	+	193

(a) Estimate the factor effects and plot them on a normal probability plot. Select a tentative model.

(b) Perform appropriate statistical tests on the model.

(c) Analyze the residuals and comment on model adequacy.

(d) Interpret the results of this experiment. Assume that a layer thickness of between 140 and 160 is desirable.

8.54 An article by L. B. Hare ("In the Soup: A Case Study to Identify Contributors to Filling Variability," *Journal of Quality Technology*, Vol. 20, pp. 36–43) describes a factorial experiment used to study the filling variability of dry soup mix packages. The factors are A = number of mixing ports through which the vegetable oil was added (1,2), B = temperature surrounding the mixer (cooled, ambient), C = mixing time (60, 80 sec), D = batch weight (1500, 2000 lb), and E = number of days of delay between mixing and packaging (1, 7). Between 125 and 150 packages of soup were sampled over an 8-hour period for each run in the design, and the standard deviation of package weight was used as the response variable. The design and resulting data are shown in Table P8.17.

■ **TABLE P8.17**
The Soup Experiment

Std. Order	*A* Mixer Ports	*B* Temp.	*C* Time	*D* Batch Weight	*E* Delay	*y* Std. Dev
1	−	−	−	−	−	1.13
2	+	−	−	−	+	1.25
3	−	+	−	−	+	0.97
4	+	+	−	−	−	1.7
5	−	−	+	−	+	1.47
6	+	−	+	−	−	1.28
7	−	+	+	−	−	1.18
8	+	+	+	−	+	0.98
9	−	−	−	+	+	0.78
10	+	−	−	+	−	1.36
11	−	+	−	+	−	1.85
12	+	+	−	+	+	0.62
13	−	−	+	+	−	1.09
14	+	−	+	+	+	1.1
15	−	+	+	+	+	0.76
16	+	+	+	+	−	2.1

(a) What is the generator for this design?

(b) What is the resolution of this design?

(c) Estimate the factor effects. Which effects are large?

(d) Does a residual analysis indicate any problems with the underlying assumptions?

(e) Draw conclusions about this filling process.

8.55 Consider the 2_{IV}^{6-2} design.

(a) Suppose that the design had been folded over by changing the signs in column B instead of column A. What changes would have resulted in the effects that can be estimated from the combined design?

(b) Suppose that the design had been folded over by changing the signs in column E instead of column A. What changes would have resulted in the effects that can be estimated from the combined design?

8.56 Consider the 2_{IV}^{7-3} design. Suppose that a fold over of this design is run by changing the signs in column A. Determine the alias relationships in the combined design.

8.57 Reconsider the 2_{IV}^{7-3} design in Problem 8.56.

(a) Suppose that a fold over of this design is run by changing the signs in column B. Determine the alias relationships in the combined design.

(b) Compare the aliases from this combined design to those from the combined design from Problem 8.35. What differences resulted by changing the signs in a different column?

8.58 Consider the 2_{IV}^{7-3} design.

(a) Suppose that a partial fold over of this design is run using column A (+ signs only). Determine the alias relationships in the combined design.

(b) Rework part (a) using the negative signs to define the partial fold over. Does it make any difference which signs are used to define the partial fold over?

8.59 Consider a partial fold over for the 2_{IV}^{6-2} design. Suppose that the signs are reversed in column A, but the eight runs that are retained are the runs that have positive signs in column C. Determine the alias relationships in the combined design.

8.60 Consider a partial fold over for the 2_{III}^{7-4} design. Suppose that the partial fold over of this design is constructed using column A (+ signs only). Determine the alias relationships in the combined design.

8.61 Consider a partial fold over for the 2_{III}^{5-2} design. Suppose that the partial fold over of this design is constructed using column A (+ signs only). Determine the alias relationships in the combined design.

8.62 Reconsider the 2^{4-1} design in Example 8.1. The significant factors are A, C, D, $AC + BD$, and $AD + BC$. Find a partial fold-over design that will allow the AC, BD, AD, and BC interactions to be estimated.

8.63 Construct a supersaturated design for $k = 8$ factors in $P = 6$ runs.

8.64 Consider the 2^{8-3} design in Problem 8.37. Suppose that the alias chain involving the AB interaction was large. Recommend a partial fold-over design to resolve the ambiguity about this interaction.

8.65 Construct a supersaturated design for $h = 12$ factors in $N = 10$ runs.

8.66 How could an "optimal design" approach be used to augment a fractional factorial design to de-alias effects of potential interest?

8.67 Consider the full 2^5 factorial design in Problem 6.51. Suppose that this experiment had been run as a 2^{5-1} fractional factorial. Set up the fractional design using the principal fraction. Using the 16 runs associated with this design from the original experiment, analyze the data and compare your results with those obtained from the analysis of the original full factorial.

8.68 Consider the full 2^5 factorial design in Problem 6.51. Suppose that this experiment had been run as a 2^{5-1} fractional factorial. Set up the fractional design using the alternate fraction. Using the 16 runs associated with this design from the original experiment, analyze the data and compare your results with those obtained from the analysis of the principal fraction in Problem 8.67.

8.69 Consider the full 2^5 factorial design in Problem 6.51. Suppose that this experiment had been run as a 2^{5-2} fractional factorial. Set up the fractional design using the principal fraction. Using the eight runs associated with this design from the original experiment, analyze the data and compare your results with those obtained from the analysis of the original full factorial. Then construct the fold-over design and analyze the data from the combined design.

8.70 Consider the fold over of the 2^{5-2} fractional factorial constructed in Problem 8.69. Compare this design with the two one-half fractions of the 2^{5-1}. Is the fold-over design the same as either of the one-half fractions?

8.71 The resolution of a two-level fractional factorial design is the number of words in the defining relation.

(a) True

(b) False

8.72 For a half fraction of a two-level factorial design the maximum resolution possible is equal to the number of factors.

(a) True

(b) False

8.73 The 12-run Plackett-Burman for up to 11 factors design is a regular fraction.

(a) True

(b) False

8.74 The design points of the 2^{k-p} family are at the corners of a cube in a k-dimensional space and they project into a full factorial in any subset of the original k factors.

(a) True

(b) False

8.75 It is good practice to keep the number of factor levels low and region of interest small in a screening experiment.

(a) True

(b) False

8.76 Consider a 2^{4-1} fractional factorial design. If the principal fraction is run first—first block (I = ABCD)—and then later augmented with the alternate fraction—second block—the four-factor interaction effect is confounded with blocks.

(a) True

(b) False

8.77 In a 2^{k-2} design every effect has four aliases.

(a) True

(b) False

8.78 In a 2^{k-3} design, the complete defining relation has 15 words.

(a) True

(b) False

8.79 The aberration of a fractional factorial design is related to the length of the longest word in the defining relation.

(a) True

(b) False