11A Appendix: Incomplete Block Design Plans for 2^n Factorials

Table 11A.1 Number of factors and blocks, block sizes, defining contrasts, and generalized interactions to construct incomplete block designs with 2^n factorials

Factors n	*Blocks* 2^{n-q}	*Block Size* $k = 2^q$	*Defining Contrasts*	*Generalized Interactions*
4	2	8	*ABCD*	
	4	4	*ABC, ABD*	*CD*
5	2	16	*ABCDE*	
	4	8	*ABC, CDE*	*ABDE*
	8	4	*ABC, ACD, ADE*	*BD, CE, ABE, BCDE*
6	2	32	*ABCDEF*	
	4	16	*ABCD, CDEF*	*ABEF*
	8	8	*ACE, ABEF, ABCD*	*ADF, BCF, BED, CDEF*
	16	4	*ABF, ACF, CDF, DEF*	*AD, CE, BC, BE, AEF, BDF, ABCD, ABDE, ACDE, ABCEF, BCDEF*
7	2	64	*ABCDEFG*	
	4	32	*ABCDE, ABEFG*	*CDFG*
	8	16	*ABG, CDE, EFG*	*ABEF, CDFG, ABCDF, ABCDEG*
	16	8	*ABC, ADG, CDE, DEFG*	*AEF, BDF, BEG, CFG, ABDE, ABFG, ACDF, ACEG, BCDG, BCEF, ABCDEFG*
	32	4	*ABG, BCG, CDG, DEG, EFG*	*AC, BD, BF, DG, CE, ADG, AFG, BEG, CFG, ABCD, ABEF, ABFG, ACDF, ABDE, ACEG, ADEF, BCDE, BCEF, CDEF, ABCEG, ABDFG, ACDEG, ACEFG, BCDFG, BDEFG, ABCDEFG*

12 Fractional Factorial Designs

Discussions on the versatility of 2^n factorial treatment designs are continued in this chapter. Experiments using only a fractional replication of the factorial arrangement are proposed as a means of effectively obtaining information on factors in the early stages of experimentation. Methods for constructing the designs are described and the analysis to estimate and test significance of factorial effects is illustrated for large 2^n factorial experiments without complete replication.

12.1 Reduce Experiment Size with Fractional Treatment Designs

The 2^n factorial treatment designs are useful for conducting preliminary studies with many factors to identify the more important factors and factor interactions. However, the number of experimental units increases geometrically with the number of factors in the study.

Fractional factorial designs use only one-half, one-fourth, or even smaller fractions of the 2^n treatment combinations. They are used for one or more of several reasons, including

- the number of treatments required exceeds resources
- information is required only on main effects and low-order interaction
- screening studies are needed to check on many factors
- an assumption is made that only a few effects are important

Some industrial research and development studies can exceed the capacity of the research facility if all treatment combinations are included in the experiment for a 2^n factorial. For example, one complete replication of a 2^7 study requires 128 runs of a process. The observations from the 128 runs provide estimates of 7 main effects, 21 two-factor interactions, 35 three-factor interactions, and 64 interactions including four or more factors. The 128 treatment combinations provide a large amount of information, perhaps even more than is necessary for high-order interactions.

Reasons for Fractional Factorial Use

Design Redundancy in the Absence of High-Order Interactions

The experiences with studies involving many factors have led to the observation that at some point higher order interactions tend to become negligible and may be ignored in the overall scheme of preliminary investigations. In practice main effects generally tend to be larger than two-factor interactions, which in turn tend to be larger than three-factor interactions, and so forth.

The complete factorial has a degree of redundancy if the investigator can be reasonably assured that the high-order interactions are negligible. Under these circumstances estimates of main effects and low-order interactions can be obtained from a fraction of the full factorial treatment design.

The Factor Sparsity Hypothesis

The use of fractional factorial designs in industrial research (Diamond, 1989) or biotechnology (Haaland, 1989) rests to a large degree on a *factor sparsity hypothesis* (Box & Meyer, 1986). Factor sparsity assumes a small fraction of factor effects are large and of significance to a process, while the remaining effects are inert for all practical purposes. Thus, a large fraction of the variation is associated with only a few factors.

Screening Studies with Fractional Factorials

The fractional factorial also is useful in screening studies that include "major" factors and "minor" factors. The effects and interactions associated with major factors are the primary objective of the study. However, the study can include a number of minor factors that must be checked for their effects even though most or all are expected to be negligible. The features, construction, and analysis of fractional factorials are explored in this chapter beginning with the half fraction experiment in the next section.

12.2 The Half Fraction of the 2^n Factorial

The half fraction design is referred to as a 2^{n-1} fractional factorial design because $\frac{1}{2}2^n = 2^{n-1}$. The notation indicates the design includes n factors each at two levels that use only 2^{n-1} experimental units.

When one replication of 2^n factorial was placed into two incomplete blocks in Chapter 11 a defining contrast was used to separate the treatment combinations into two sets. Each of the two sets was placed into one of the incomplete blocks according to the + and − coefficients of the treatment combinations in the defining contrast. Each of the blocks was half of a complete replication of the treatments. Although the defining contrast was confounded with blocks, it was possible to estimate the remaining effects.

The same principle is used to construct fractional factorial designs. A half replicate of the design consists of all treatment combinations with the + coefficient of the defining contrast.

Example 12.1 Truck Leaf Spring Manufacture Revisited

Recall from Example 11.1 that an assembled truck leaf spring was passed through a high-temperature furnace. Afterward, it was put into a forming machine to induce curvature in the spring by holding the spring in a high-pressure press for a short length of time. The factors and levels for the 2^3 factorial were (A) furnace temperature at 1840° and 1880° F, (B) heating time at 23 and 25 seconds, and (C) transfer time at 10 and 12 seconds.

The eight treatment combinations, spring quality observations, and coefficients to estimate the factorial effects are shown in Table 12.1. Notice the eight treatments are divided into two groups of four treatments using the defining contrast based on the ABC, three-factor interaction. This particular division of treatments could have been used to construct an incomplete block design with the ABC interaction confounded with blocks.

Table 12.1 Treatment combinations required for a 2^3 factorial treatment design

	Factorial Effect								
Treatment	*I*	*A*	*B*	*C*	*AB*	*AC*	*BC*	*ABC*	*y*
a	+	+	−	−	−	−	+	+	35
b	+	−	+	−	−	+	−	+	28
c	+	−	−	+	+	−	−	+	48
abc	+	+	+	+	+	+	+	+	29
(1)	+	−	−	−	+	+	+	−	32
ab	+	+	+	−	+	−	−	−	31
ac	+	+	−	+	−	+	−	−	39
bc	+	−	+	+	−	−	+	−	28

If the engineers wanted to construct a half replicate fractional factorial design they would use the four treatments in the top half of Table 12.1 with a + coefficient for the ABC factorial effect.

Gains and Losses with the Fractional Factorial

Each Treatment Effect Has an Alias

The gain in reduced size of the experiment comes with a price—the loss of some information on the treatment effects. If a half replicate of the 2^3 factorial is used, we lose the ability to estimate the three-factor interaction and each main effect is confounded or aliased with a two-factor interaction. The contrasts for the main effects for A, B, and C are

$$l_A = a - b - c + abc = 35 - 28 - 48 + 29 = -12$$

$$l_B = -a + b - c + abc = -35 + 28 - 48 + 29 = -26$$

$$l_C = -a - b + c + abc = -35 - 28 + 48 + 29 = 14$$

Contrasts for the BC, AC, and AB interaction effects are

$$l_{BC} = a - b - c + abc = 35 - 28 - 48 + 29 = -12$$

$$l_{AC} = -a + b - c + abc = -35 + 28 - 48 + 29 = -26$$

$$l_{AB} = -a - b + c + abc = -35 - 28 + 48 + 29 = 14$$

Notice that $l_A = l_{BC}$ and the contrast $l = (a - b - c + abc)$ estimates the combined effect $A + BC$, making it impossible to differentiate between the main effect of temperature (A) and the interaction between heating time and transfer time (BC). Two effects estimated by the same contrast are known as *aliases*.

Similar relationships exist with the linear contrasts for the main effects of heating time (B) and transfer time (C), $l_B = l_{AC}$ and $l_C = l_{AB}$. The linear contrast l_B estimates $B + AC$ and l_C estimates $C + AB$. Therefore, B and AC are aliases and C and AB are aliases. The alias relationships are

$$A = BC$$
$$B = AC$$
$$C = AB$$

The ABC interaction used as the defining contrast is known as the *design generator*. The three-factor interaction ABC used as the defining contrast cannot be estimated from that half replication because it has the same coefficients (+) in

Table 12.1 as the estimate of the mean, or the identity column I. The identity relationship

$$I = ABC$$

is known as the *defining relation* for the design.

Generate the Design with Either Half of the Design Generator

Either half of the ABC contrast can be used for the design generator. If we use the treatment combinations with a − coefficient for the ABC interaction from the bottom half of Table 12.1 as the half replication, the defining relation for the design is $I = -ABC$. The contrast for the main effect of temperature (A) is

$$l_A = [-(1) + ab + ac - bc]$$

The contrast for the interaction between heating time and transfer time (BC) is

$$l_{BC} = [(1) - ab - ac + bc]$$

Thus, $l_A = -l_{BC}$; the temperature main effect is the negative of the heating time and transfer time interaction effect.

Similarly, the main effects of heating time (B) and transfer time (C) are the negative of the AC and AB interactions, respectively. The relationships between the main effects and two-factor interactions are $A = -BC$, $B = -AC$, and $C = -AB$. Regardless of which design half is used, the main effects are aliased with the two-factor interactions.

The alias for any treatment effect can be determined readily from its generalized interaction with the defining contrast ABC. (The determination of generalized interactions was explained in Chapter 11.) The aliases for the main effects are determined from their respective products with the defining contrast as

$$A \times ABC = A^2BC = BC$$
$$B \times ABC = AB^2C = AC$$
$$C \times ABC = ABC^2 = AB$$

Had the defining relation $I = -ABC$ been used, the aliases would be

$$A \times (-ABC) = -A^2BC = -BC$$
$$B \times (-ABC) = -AB^2C = -AC$$
$$C \times (-ABC) = -ABC^2 = -AB$$

Redundancy in 2^n Factorials

When there are no two-factor or three-factor interactions in the 2^3 factorial, the main effects for A, B, and C can be estimated from a half replicate of the design defined by either $I = ABC$ or $I = -ABC$. Thus, in the absence of interaction there is a redundancy in the complete design such that main effects can be estimated with two different contrasts, each from a different half of the design. The half fraction derived from $I = ABC$ is commonly known as the *principal fraction*, while the half fraction derived from $I = -ABC$ is the *complementary fraction*.

The two half fractions form a complete 2^3 design. If each is run on a separate occasion the resulting design is an incomplete block design with two blocks of four treatments each. The ABC interaction is confounded with blocks, but all main effects and two-factor interactions can be estimated.

How to Construct Half Replicate 2^{n-1} Designs

The half fraction design is constructed with the highest order interaction as the design generator. The treatment combinations are identified as follows:

- Write the + and − coefficients in standard order for the factors in a 2^{n-1} factorial.
- Identify the ± coefficients for the nth factor by equating them to the coefficients for the highest order interaction in the 2^{n-1} factorial.

The construction of a half replicate 2^{4-1} design is illustrated in Table 12.2. The − and + coefficients for the main effects of A, B, and C for the $2^{4-1} = 2^3$ factorial are written in standard order. The coefficients for the ABC interaction are taken as the level of the fourth factor, D, to be combined with the levels of the three other factors. The coefficients for the ABC interaction contrast are obtained as the product of the coefficients for the three main effects, as shown in the fourth column of Table 12.2.

Table 12.2 Construction of a 2^{4-1} fractional factorial design

A	B	C	$D = ABC$	*Treatment*
−	−	−	−	(1)
+	−	−	+	*ad*
−	+	−	+	*bd*
+	+	−	−	*ab*
−	−	+	+	*cd*
+	−	+	−	*ac*
−	+	+	−	*bc*
+	+	+	+	*abcd*

For example, the product of the coefficients for A, B, and C in the first row of coefficients is a − sign. Thus, the first row of coefficients is (− − − −) for the treatment combination (1). The second row of coefficients is (+ − − +) for the treatment combination ad.

Build the Half Replicate with the Evens–Odds Rule

The highest order interaction is the design generator on the basis of the Evens–Odds rule. The resulting treatment combinations required for the half replicate are shown in the right-hand column of Table 12.2. They are the treatment combinations that receive a + coefficient in the $ABCD$ interaction by the Evens–Odds rule since each has an even number of effect letters.

A table of design generators for fractional factorials is given in the Appendix. The generator for the 2^{4-1} design in Table 12.2 is listed in Appendix Table 12A.1 as $D = ABC$ to indicate how the levels for the fourth factor, D, are determined from the levels of the other factors.

The design in Table 12.2 is a complete factorial for A, B, and C if factor D is omitted from the design. Regardless of which factor is omitted from the half fraction design, the resulting design is a complete factorial in the remaining effects.

Choose the Highest Order Interaction for the Design Generator

The highest order interaction of least interest ordinarily is used to generate the half replicate because the defining contrast chosen for the design generator cannot be estimated. Suppose a half replicate of a 2^5 factorial is generated with the $ABCDE$ contrast. The design requires $2^{5-1} = 16$ experimental units. The complete set of aliases is shown in Table 12.3.

Table 12.3 Aliases for main effects and two factor interactions in a half fraction of the 2^5 factorial design

Main Effects	*Alias*	*Two-Factor Interactions*	*Alias*
A	$BCDE$	AB	CDE
B	$ACDE$	AC	BDE
C	$ABDE$	AD	BCE
D	$ABCE$	AE	BCD
E	$ABCD$	BC	ADE
		BD	ACE
		BE	ACD
		CD	ABE
		CE	ABD
		DE	ABC

Each main effect has a four-factor interaction as an alias, and each two-factor interaction has a three-factor interaction as an alias. For example, the alias of B is found as the generalized interaction $B \times ABCDE = AB^2CDE = ACDE$, and the alias of DE is $DE \times ABCDE = ABCD^2E^2 = ABC$.

Use the Fractional Factorial to Guide the Experimental Process

A sound experimental strategy in industrial research will utilize fractional factorial designs. For example, a half replicate of the 2^5 factorial requires 16 runs of an experimental process. Suppose the first 16 required runs were completed. The second fraction of 16 runs can always be run later in a second block if necessary to complete the factorial. There may be sufficient information on main effects and two-factor interactions from the first fraction that signals the need to change levels of some factor(s) or allows elimination of some inert factor(s). Thus, the investigator can move on to the next stage of experiments. The resources previously needed for the second set of 16 runs can be more wisely expended on the new experiments.

Randomization with fractional factorials is achieved by performing the actual runs of the treatment combinations in a random order if the treatments must be tested in sequence. If all treatments are tested on a set of physical experimental units they are randomly assigned to the units.

12.3 Design Resolution Related to Aliases

Fractional factorial designs are grouped into classes according to existing alias relationships in the design. The groups are identified by their **resolution**. The most common designs are those of Resolution III, IV, and V.

Resolution III: A design in which no main effect is confounded with any other main effect, but main effects are confounded with two-factor interactions and two-factor interactions are confounded with other two-factor interactions.

Resolution IV: A design in which no main effect is confounded with any other main effect or two-factor interaction, but two-factor interactions are confounded with one another.

Resolution V: A design in which no main effect or two-factor interaction is confounded with any other main effect or two-factor interaction, but two-factor interactions are confounded with three-factor interactions.

The resolution of a design is determined by the smallest number of characters appearing in the design generator. The 2^{3-1} design generated from the ABC contrast with three characters was a design of Resolution III, because main effects were not confounded with each other but were confounded with two-factor interactions. The 2^{5-1} design generated from the $ABCDE$ contrast with five characters was a design of Resolution V because there was no confounding among the main effects or two-factor interactions, but the two-factor interactions were confounded with three-factor interactions. The 2^{4-1} design generated from the $ABCD$ interaction with four characters is a design of Resolution IV.

In general, any fractional factorial design of Resolution R has no q-factor interaction confounded with any effect consisting of less than $R - q$ factors. The notation used to identify a design along with its resolution contains a Roman subscript indicating the design resolution. Thus, the notation 2_{V}^{5-1} identifies a 2^{5-1} fractional factorial with Resolution V.

12.4 Analysis of Half Replicate 2^{n-1} Designs

The analysis of replicated factorial experiments was discussed and illustrated in Chapters 6, 7, and 11. The replicated designs provided an estimate of experimental error variance from an analysis of variance. However, with fractional replication of the experiment no direct estimate of experimental error is available to evaluate the significance of factor effects and interactions. A strategy commonly used to estimate experimental error from fractional replications of 2^n experiments is illustrated with a 2_{V}^{5-1} fractional factorial experiment in Example 12.2.

Example 12.2 Polymer Coatings for Aluminum Cases

Research Objective: A company had received a contract to manufacture aluminum cases with a plastic polymer coating on the case for transceivers. A research team was given the responsibility to develop a process to adhere the polymer coating to the aluminum. The team identified five factors in the process that they thought had any potential to affect adhesion of the polymer to the aluminum. They were the type of aluminum alloy (A), the type of solvent used to clean the aluminum (S), the molecular structure of the coating polymer (M), the percent catalyst used in the adhesion process (C), and the curing temperature for the process (T).

Treatment Design: A 2^5 factorial design was chosen so they could take advantage of the factorial treatment structure to evaluate important interactions along with the main effects. They used two aluminum alloys, two types of solvents, two molecular structures for the polymer, 10% or 15% of the catalyst, and 150° or 175° as a curing temperature.

Experiment Design: With this preliminary experiment, the team wanted to identify any main effects and two-factor interaction effects that were important to the process. They also wanted to reduce the number of experimental units and time required for this first experiment.

They chose to use a 2_{V}^{5-1} fractional factorial or a half replicate design that required only 16 experimental units. The five-factor interaction $ASMCT$ was the defining contrast used to generate the design. Thus, they could run the second half of the design later if it became necessary to complete the factorial. Likewise, the half fraction might provide sufficient information to signal the need to alter the levels of some factors or eliminate some with no effect.

The treatment combinations, the coefficients for main effect and two-factor interaction contrasts, and the responses from the experimental runs are shown in Table 12.4. The force required to remove the plastic coating from the aluminum case was used as the response variable.

Table 12.4 Force required to remove plastic coating from an aluminum surface for treatments in a 2_{V}^{5-1} fractional factorial experiment

Design																
A	*S*	*M*	*C*	*T*	*AS*	*AM*	*AC*	*AT*	*SM*	*SC*	*ST*	*MC*	*MT*	*CT*	*Treatment*	*Force**
−	−	−	−	+	+	+	+	−	+	+	−	+	−	−	*t*	41.5
+	−	−	−	−	−	−	−	−	+	+	+	+	+	+	*a*	39.6
−	+	−	−	−	−	+	+	+	−	−	−	+	+	+	*s*	43.9
+	+	−	−	+	+	−	−	+	−	−	+	+	−	−	*ast*	38.8
−	−	+	−	−	+	−	+	+	−	+	+	−	−	+	*m*	48.7
+	−	+	−	+	−	+	−	+	−	+	−	−	+	−	*amt*	52.0
−	+	+	−	+	−	−	+	−	+	−	+	−	+	−	*smt*	55.8
+	+	+	−	−	+	+	−	−	+	−	−	−	−	+	*asm*	43.2
−	−	−	+	−	+	+	−	+	+	−	+	−	+	−	*c*	39.5
+	−	−	+	+	−	−	+	+	+	−	−	−	−	+	*act*	42.6
−	+	−	+	+	−	+	−	−	−	+	+	−	−	+	*sct*	44.0
+	+	−	+	−	+	−	+	−	−	+	−	−	+	−	*asc*	33.8
−	−	+	+	+	+	−	−	−	−	−	−	+	+	+	*mct*	53.6
+	−	+	+	−	−	+	+	−	−	−	+	+	−	−	*amc*	48.1
−	+	+	+	−	−	−	−	+	+	+	−	+	−	−	*smc*	51.3
+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	*asmct*	48.7

*Example computation for effect estimates [see Equation (12.1)]:
$A = 2(-41.5 + 39.6 - 43.9 + 38.8 - 48.7 + 52.0 - 55.8 + 43.2 - 39.5 + 42.6 - 44.0 + 33.8 - 53.6 + 48.1 - 51.3 + 48.7)/16 = 2(-31.5)/16 = -3.94$

The + and − coefficients for the first four factors (A, S, M, and C) of a $2^{5-1} = 2^4$ factorial are written in standard order in Table 12.4. Coefficients for the $ASMC$ interaction are used to identify the levels of the fifth factor (T) to be used in the treatment combinations. The 16 treatment combinations shown in Table 12.4 all have a + coefficient in the five-factor interaction, $ASMCT$, because they have an odd number of effect letters.

The 2_{V}^{5-1} fractional factorial design has no main effect or two-factor interaction aliased with any other main effect or two-factor interaction (see Table 12.3). The main effects are aliased with four-factor interactions (for example, $l_A = l_{SMCT}$), and two-factor interactions are aliased with three-factor interactions (for example, $l_{AS} = l_{MCT}$). Assuming the three-factor and four-factor interactions are negligible, estimates of the main effects and two-factor interactions can be used to identify the candidate factors for more in-depth investigation in follow-up studies if necessary.

The effect of a factor in a fractional factorial is estimated as

$$AB... = \frac{2(l_{AB...})}{N} \tag{12.1}$$

where $l_{AB...}$ is the contrast of the treatment combinations and N is the total number of observations in the experiment. The 1 degree of freedom sum of squares for an effect in a 2^{n-1} fractional factorial is

$$SS(AB...) = \frac{1}{2^{n-1}}(l_{AB...})^2 \tag{12.2}$$

The estimates of the main effects and two-factor interaction effects are listed in order of increasing value from −3.94 to +9.71 in Table 12.5. An example calculation for the effect estimate of factor A is given at the bottom of Table 12.4. The estimates of the other effects can be verified by using Equation (12.1).

Table 12.5 Estimates of main effects and two-factor interactions with their normal scores sum of squares from the 2^{5-1} fractional factorial data in Table 12.4

Effect	*Estimate*	*Normal Quantile*	*Sum of Squares*
A	−3.94	−1.74	62.02
AS	−3.69	−1.24	54.39
S	−0.76	−0.94	2.33
SC	−0.74	−0.71	2.18
AM	−0.41	−0.51	0.68
C	−0.24	−0.33	0.23
SM	−0.09	−0.16	0.03
AC	0.14	0.00	0.08
ST	0.16	0.16	0.11
CT	0.44	0.33	0.77
AT	0.74	0.61	2.18
MC	0.74	0.61	2.18
MT	1.09	0.94	4.73
T	3.61	1.24	52.20
M	9.71	1.74	377.33

Examine Effects with a Normal Probability Plot

The observations from the experiment are assumed to be normally distributed with a constant variance σ^2. The observations represent random normal variation about a fixed mean if the changes in the levels of the factors have no real effect on the response. Thus, the estimated factorial effects from these observations are normally distributed about a mean of 0 with variance σ^2 when the factors have no effect. The estimated effects will plot on a straight line in the normal probability plot (see Chapter 4). Estimated effects that do not fit on the straight line cannot be explained as random chance variation.

The corresponding normal quantile computed for each effect is shown in Table 12.5. Recall that the *normal quantile* is the expected value for the normally distributed variable with a mean of 0 and variance of 1 found in that position of the ordered effects (see Chapter 4).

The normal probability plot of main effects and two-factor interaction effects is shown in Figure 12.1. The main effects that do not plot on the straight line of the normal probability plot are the effects of the aluminum alloy (A), molecular structure of the polymer (M), and the curing temperature (T). The effect estimate for the interaction between aluminum alloy and type of solvent (AS) also deviates considerably from the straight line of the normal probability plot. All other effects appear to be representative of random experimental error.

Interpretations of the Largest Effects

Graphs of the effects are shown in Figure 12.2. The positive estimate for T, 3.61, in Figure 12.2a indicates a high level of curing temperature produced stronger adhesion. Likewise, the positive estimate for M, 9.71, in Figure 12.2b indicates the molecular structure represented by the "high" level produced stronger adhesion.

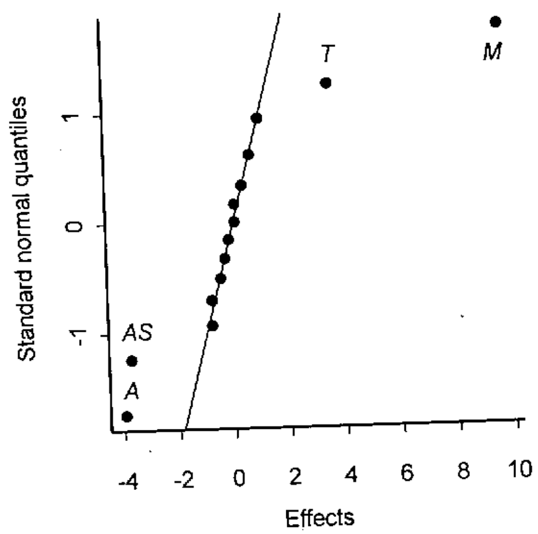

Figure 12.1 Normal probability plot of estimated factor effects and interactions from the 2^{5-1} fractional factorial in Example 12.2

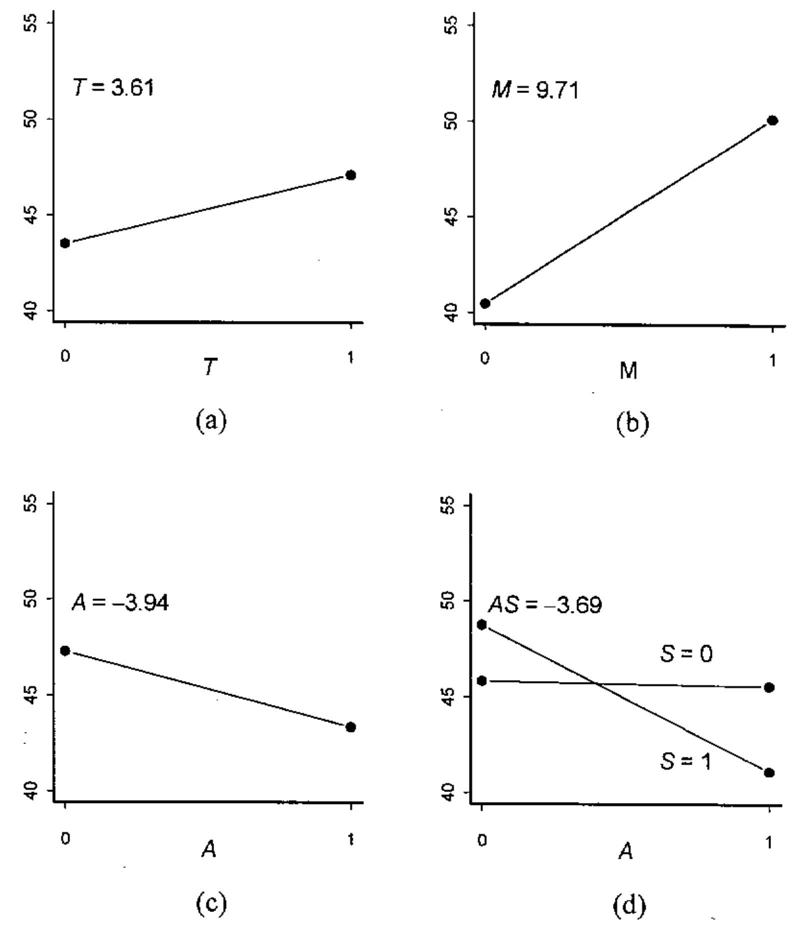

Figure 12.2 Graphs for the main effects of T, M, and A and the AS interaction

The negative estimate for A, –3.94, in Figure 12.2c suggests the "low" level of the aluminum alloy factor produced a stronger adhesion. However, the large estimate for AS interaction, –3.69, may alter the inference about the main effect of A, the aluminum alloy.

An investigation of the interaction between the aluminum alloy (A) and the solvent used to clean the aluminum (S) requires the averages of the four treatment combinations involving A and S. From Table 12.4 the values are

Treatments	*A*	*S*	*Total*	*Mean*
$t + m + c + mct$	0	0	183.3	45.8
$a + amt + act + amc$	1	0	182.3	45.6
$s + smt + sct + smc$	0	1	195.0	48.8
$ast + asm + asc + asmct$	1	1	164.5	41.1

The effect of A when $S = 0$ is $45.6 - 45.8 = -0.2$, whereas the effect of A when $S = 1$ is $41.1 - 48.8 = -7.7$. Therefore, the difference between the two alloys with the second solvent ($S = 1$) is much greater than that with the first solvent ($S = 0$), as shown in Figure 12.2d.

Some Cautions About the Assumptions

It is important to remember that because of aliasing the estimated effects judged important for the process are really $A + SMCT$, $M + ASCT$, $T + ASMC$, and $AS + MCT$. Thus, the assumption of negligible three-factor and higher interactions is crucial to the decisions regarding the importance of the three main effects and one two-factor interaction to the bonding process.

Daniel (1959) pointed out some possible misinterpretations with a normal plot of estimated effects in unreplicated factorials. A perceived smooth line of plotted effect estimates in one portion of the normal plot thought to indicate no evidence of factor effects could lead to an overestimate of error variance. An extremely irregular line could lead to an opposite error of judging effects real when in fact they only represent random experimental error. The plot is most effective when only a small proportion of the factorial effects are important.

One Method to Estimate Experimental Error Variance

A test for the significance of these effects requires an estimate of experimental error variance. One general strategy to obtain an estimate involves several steps. The first step selects those effects that appear to be negligible on the normal probability plot and pools their sums of squares to estimate experimental error. Based on Figure 12.1, only the effects for A, M, T, and AS appear to be significant and all other effects appear to be negligible and representative of random experimental variation. The analysis of variance for a model containing effects only for A, M, T, S, and AS is shown in Table 12.6. The main effect for S was included in the analysis because the interaction effect AS is defined on the main effects in the model (Chapter 6) and should properly be included in the model to obtain correct sums of squares

Table 12.6 Analysis of variance for the reduced model with A, M, T, and AS effects for the 2^{5-1} fractional factorial (Example 12.2)

Source of Variation	*Degrees of Freedom*	*Sum of Squares*	*Mean Square*	F	$Pr > F$
Total	15	561.41			
A	1	62.02	62.02	44.11	0.000
M	1	377.33	377.33	268.36	0.000
T	1	52.20	52.20	37.12	0.000
S	1	2.33	2.33	1.77	0.213
AS	1	54.39	54.39	38.68	0.000
Error	10	13.14	1.31		

values. Notice that S is not significant and could be pooled with the sum of squares for error following the general strategy. The pooled sum of squares for the effects that appear negligible is $SSE = SS(\text{Error}) + SSS = 13.14 + 2.33 = 15.47$ from the analysis of variance in Table 12.6 with 11 degrees of freedom. Thus, an estimate of experimental error variance is $s^2 = 15.47/11 = 1.41$.

No Substitute for Replication

A replicated experiment would have provided a legitimate estimate of σ^2. The validity of any significance test is questionable when the variance estimate is based on subjective judgments. The method just described to estimate experimental error variance from an unreplicated experiment is very subjective.

A number of more formal procedures have been developed to address the problem of variance estimation in unreplicated fractional factorials. However, each of them requires a degree of subjective judgment on the part of the investigator. None of them are discussed here but references are given for further reading.

Berk and Picard (1991) reviewed and evaluated some of the methods including one of their own. More computational intensive procedures were proposed by Box and Meyer (1986) and Zahn (1975a, 1975b). The Zahn methods are extensions of the original half normal plot methods of Daniel (1959). Some of the procedures (Lenth, 1989; Voss, 1988) proposed simpler methods based on standard analysis of variance procedures.

On the basis of simulation studies, Berk and Picard (1991) concluded most of the methods produced nearly identical error rates and most were prone to declare null effects to be real.

The dangers inherent in unreplicated fractional factorials cannot be ignored. Only replication can protect against selecting spurious effects as legitimate effects. Fractional factorials are legitimate studies if they are recognized as preliminary screening studies to prepare the way for more rigorous replicated experiments.

Standard Errors and Tests of Hypotheses About Effects

The variance of an effect estimate in a fractional factorial is

$$\text{variance} = \frac{4\sigma^2}{N} \tag{12.3}$$

where σ^2 is the experimental error variance and N is the number of observations in the experiment. The estimated experimental error variance is $s^2 = 1.41$ with 11 degrees of freedom from which a standard error estimate can be calculated for each of the effects in Table 12.5. The variance estimate for any effect is

$$\frac{4s^2}{N} = \frac{4(1.41)}{16} = 0.35$$

and the estimated standard error of the effect estimates is $\sqrt{0.35} = 0.59$. The significance of each effect can be determined with the Student t test as a ratio of the effect estimate to the standard error or with the F test as shown in Table 12.6. All of the suspected effects are significant according to the F tests in Table 12.6.

A Residual Plot to Evaluate the Model

The estimate for experimental error variance was found by assuming all effects other than those in the model were negligible. If effects other than those in the model are negligible the residual plots for the fitted model should provide evidence for correctness of model choice. The residuals were computed for the model including only the A, M, T, S, and AS effects. The plot of residuals versus predicted values and the normal probability plot of the residuals are shown in Figure 12.3. The residuals all appear to lie on a relatively straight line in the normal probability plot, and there does not appear to be any reason to suspect heterogeneous variances in the residuals versus the fitted values plot. Also, no major effects appear to be absent from the model since there are no evident outliers in the residual plots.

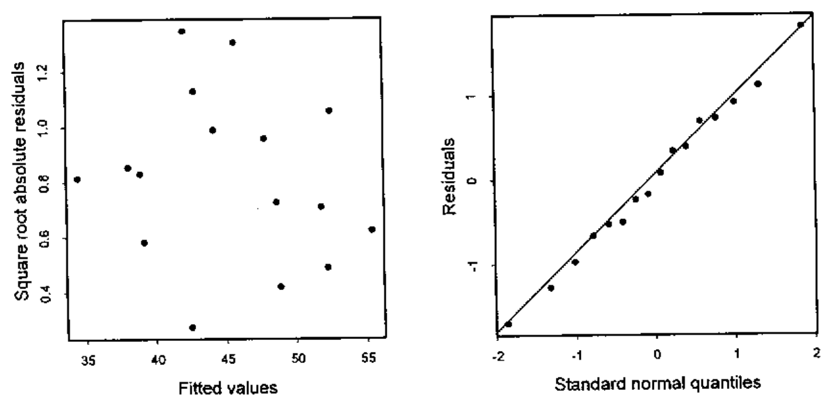

Figure 12.3 Residual plots for the model with A, M, T, S, and AS effects for Example 12.2

The catalyst, factor C, had no detectable effects and appears to be an inert factor. Thus, the design was effectively a single replicate of a complete 2^4 factorial in the other factors A, S, M, and T. The research team now can consider an in-depth study of the other factors and their interactions.

12.5 The Quarter Fractions of 2^n Factorials

Smaller fractions for initial runs of the 2^n factorial may provide sufficient information for critical decisions about the effectiveness of factors, especially when a moderately large number of factors are under consideration or individual runs are

quite expensive. The quarter replicate of the 2^n design is designated as a $\frac{1}{4}2^n = 2^{n-2}$ fractional factorial.

A quarter replicate of a 2^6 design is a 2^{6-2} fractional factorial that requires only 16 runs of the 64 runs required for the complete 2^6 design. The quarter replicate corresponds to one block of the incomplete block design for a 2^6 design in four blocks of 16 units per block (see Chapter 11). Any of the four blocks could be used for the 2^{6-2} design.

Use Two Defining Contrasts for Quarter Fraction Designs

Two defining contrasts are required to construct the four incomplete blocks, and they along with their generalized interaction are confounded with blocks. These same defining contrasts can be used to construct the 2^{6-2} design. Suppose $ABDE$ and $ABCF$ are chosen as the design generators. The generalized interaction of the design generator is

$$ABDE \times ABCF = A^2B^2CDEF = CDEF$$

The design generators along with their generalized interaction complete the defining relationship for the design as

$$I = ABDE = ABCF = CDEF$$

The treatment combinations required for the design can be identified in a manner analogous to that demonstrated for the half replicate design in Table 12.2. The $+$ and $-$ coefficients for the main effects of A, B, C, and D are written in standard order for a $2^{6-2} = 2^4$ complete factorial as shown in Table 12.7.

The aliases of E and F that contain any of the factor letters A through D are used to determine coefficients for E and F in the treatment combinations. Given the design generators $ABDE$, $ABCF$, and their generalized interaction $CDEF$, the aliases of E and F, respectively, are

$$E = ABD = CDF = ABCEF$$

and

$$F = ABC = CDE = ABDEF$$

The equivalencies $E = ABD$ and $F = ABC$ can be used to determine the coefficients for E and F. That is, the coefficients for ABD are taken as the coefficients for E, and the coefficients for ABC are taken as the coefficients for F. The treatments required for the design are shown in Table 12.7.

The defining contrasts for the quarter replicate design have four letters, so the resulting design is of Resolution IV, or a 2_{IV}^{6-2} fractional factorial. No main effect is aliased with any other main effect or two-factor interaction, but two-factor interactions are aliased with one or more other two-factor interactions. The complete set of aliases are shown in Table 12.8.

Table 12.7 Construction of a 2_{IV}^{6-2} fractional factorial design

Design						
A	*B*	*C*	*D*	*E* = *ABD*	*F* = *ABC*	*Treatment*
−	−	−	−	−	−	(1)
+	−	−	−	+	+	*aef*
−	+	−	−	+	+	*bef*
+	+	−	−	−	−	*ab*
−	−	+	−	−	+	*cf*
+	−	+	−	+	−	*ace*
−	+	+	−	+	−	*bce*
+	+	+	−	−	+	*abcf*
−	−	−	+	+	−	*de*
+	−	−	+	−	+	*adf*
−	+	−	+	−	+	*bdf*
+	+	−	+	+	−	*abde*
−	−	+	+	+	+	*cdef*
+	−	+	+	−	−	*acd*
−	+	+	+	−	−	*bcd*
+	+	+	+	+	+	*abcdef*

Table 12.8 Alias relationships for the 2_{IV}^{6-2} fractional factorial with defining relationship $I = ABDE = ABCF = CDEF$

	Alias from		
Effect	*ABDE*	*ABCF*	*CDEF*
A	*BDE*	*BCF*	*ACDEF*
B	*ADE*	*ACF*	*BCDEF*
C	*ABCDE*	*ABF*	*DEF*
D	*ABE*	*ABCDF*	*CEF*
E	*ABD*	*ABCEF*	*CDF*
F	*ABDEF*	*ABC*	*CDE*
AB	*DE*	*CF*	*ABCDEF*
AC	*BCDE*	*BF*	*ADEF*
AD	*BE*	*BCDF*	*ACEF*
AE	*BD*	*BCEF*	*ACDF*
BC	*ACDE*	*AF*	*BDEF*
CD	*ABCE*	*ABDF*	*EF*
CE	*ABCD*	*ABEF*	*DF*
ACD	*BCE*	*BDF*	*AEF*
ACE	*BCD*	*BEF*	*ADF*

A $1/2^p$ fraction of a 2^n design is designated as a 2^{n-p} fractional factorial. In general a $1/2^p$ fraction of the 2^n factorial will require p design generators with

$2^p - p - 1$ generalized interactions. Each effect will have $2^p - 1$ aliases. The $1/2^2$ or quarter fraction 2_{IV}^{6-2} design in Table 12.7 required $p = 2$ design generators resulting in $2^2 - 2 - 1 = 1$ generalized interaction. There were $2^2 - 1 = 3$ aliases for each effect.

Choose Design Generators to Avoid a Bad Alias Structure

Care must be exercised when selecting design generators to avoid aliasing those effects of interest in the study with one another. Thus, it is important to look at the alias structure prior to assigning actual factors to the factor letters when setting up the experiment.

Notice that the 2_{IV}^{6-2} design in Table 12.7 consists of a complete replication of the 2^4 design in A, B, C, and D. It is also a complete replication of a 2^4 design in any four factors that do not combine to form the defining relationships for the design. Any combination of factors other than $ABDE$, $ABCF$, and $CDEF$ will produce a complete 2^4 factorial. Upon close inspection the design in Table 12.7 is seen to consist of two replications of a 2^{4-1} design for experiments, consisting of the four factors used in any one of the defining relationships considered separately.

The 2^{n-p} design will contain a complete factorial for any $n - p$ factors that do not make up a defining relationship. A fractional factorial can be made up from some subset of $n - p$ factors. If it is suspected at the outset that some of the factors will have negligible effects, then the original 2^{n-p} fractional factorial can be set up so that a full factorial will exist for the factors of most interest or those expected to have real effects.

12.6 Construction of 2^{n-p} Designs with Resolution III and IV

Resolution III Designs with N Experimental Units for $N - 1$ Factors

A design of Resolution III can be constructed to investigate $n = N - 1$ factors using N experimental units, where N is a multiple of 4. For example, the half fraction of a 2^3 factorial in Table 12.1 is a 2_{III}^{3-1} fractional factorial that has $N - 1 = 3$ factors investigated with $N = 4$ experimental units. Designs of Resolution III have no main effects aliased with each other but do have main effects aliased with two-factor interactions.

A 2^{n-p} design of Resolution III requires p design generators. Suppose a design of Resolution III is desired for $N - 1 = 7$ factors in $N = 8$ runs. The eight runs would be a (1/16) fraction of the $2^7 = 128$ runs required for a full factorial. Thus, the design is a 2_{III}^{7-4} fractional factorial requiring $p = 4$ design generators.

The design construction begins by writing the + and − coefficients for $n - p$ main effects in standard order. The + and − coefficients for $n - p = 3$ main effects, A, B, and C, in standard order for a complete $2^{7-4} = 2^3$ factorial are shown in Table 12.9.

Table 12.9 A 2_{III}^{7-4} fractional factorial with design generators ABD, ACE, BCF, and $ABCG$

Unit	A	B	C	$D = AB$	$E = AC$	$F = BC$	$G = ABC$	*Treatment*
1	−	−	−	+	+	+	−	def
2	+	−	−	−	−	+	+	afg
3	−	+	−	−	+	−	+	beg
4	+	+	−	+	−	−	−	abd
5	−	−	+	+	−	−	+	cdg
6	+	−	+	−	+	−	−	ace
7	−	+	+	−	−	+	−	bcf
8	+	+	+	+	+	+	+	$abcdefg$

The coefficients required for the remaining factors, D, E, F, and G, are associated with the coefficients of all the interaction columns of A, B, and C. Thus, the columns that indicate the levels for factors D, E, F, and G are generated from the equivalence relationships

$$D = AB \qquad E = AC \qquad F = BC \qquad \text{and} \qquad G = ABC$$

The $p = 4$ design generators are then

$$D \times AB = ABD$$
$$E \times AC = ACE$$
$$F \times BC = BCF$$

and

$$G \times ABC = ABCG$$

The defining relationship is found by multiplying the four generators in all possible ways. The complete defining relationship is

$$I = ABD = ACE = AFG = BCF = BEG = CDG = DEF = ABCG$$
$$= ABEF = ACDF = ADEG = BCDE = BDFG = CEFG$$
$$= ABCDEFG$$

The alias structure of the design for the main effects is found by multiplying each of the main effects by the interactions included in the defining relationship. The two-factor interactions aliased with the main effects are

$$A = BD = CE = FG$$
$$B = AD = CF = EF$$
$$C = AE = BF = DG$$
$$D = AB = CG = EF$$
$$E = AC = BG = DF$$
$$F = AG = BC = DE$$
$$G = AF = BE = CD$$

The design in Table 12.9 has all available contrasts of the $2^{n-p} = 2^3$ factorial associated with the main effects along with their aliases, and the design is referred to as a *saturated design*. The design has 7 degrees of freedom available to estimate each of the seven main effects and no degrees of freedom to estimate experimental error. Designs of Resolution III can be used in screening experiments with many factors to identify clearly dominant factors without major expenses of time and other resources.

The Plackett–Burman Designs

Resolution III saturated designs with $n = N - 1$ factors requiring $N = 2^{n-p}$ experimental units can be generated from the basic 2^{n-p} factorial arrangement, as shown in the preceding paragraphs. The result is a design with the number of experimental units equal to the power of 2—experiments with 4, 8, $16, \ldots, 2^{n-p}$ units.

A class of Resolution III designs developed by Plackett and Burman (1946) requiring a number of experimental units equal to a multiple of 4 have been used extensively for screening experiments in industrial research. They provide designs for intermediate values of N that are not a power of 2. Designs were provided for $N \leq 100$, except for 92, by Plackett and Burman (1946). A sample of the most practical and easily constructed designs is given in Table 12.10 for $N = 12, 16, 20, 24$, and 32 experimental runs; the designs can be generated from rows of + and − signs.

Each row of generators in Table 12.10 has $N - 1$ coefficients. These are the + and − coefficients required for the first run of the $N - 1$ factors. The factor coefficients for the second run are generated from the first run by taking the first coefficient in the first run and placing it in the last position of the second run and shifting the coefficients of the first run one position to the left. Succeeding runs are generated in the same manner. Upon completing the cycle of coefficient replacements for $N - 1$ runs a final run is added with − coefficients for all $N - 1$ factors. A design for $N - 1 = 11$ factors in $N = 12$ runs is shown in Table 12.11.

Table 12.10 Generators for Plackett–Burman designs with $N = 12, 16, 20, 24$, and 32 runs

N	
12	+ + − + + + − − − + −
16	+ − − − + − − + + − + − + + +
20	+ + − − + + + + − + − + − − − − + + −
24	+ + + + + − + − + + − − + + − − + − + − − − −
32	− − − − + − + − + + + − + + − − − + + + + + − − + + − + − − +

Table 12.11 A Plackett–Burman design for $N - 1 = 11$ factors in $N = 12$ runs

	Factor										
Run	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*
1	+	+	−	+	+	+	−	−	−	+	−
2	+	−	+	+	+	−	−	−	+	−	+
3	−	+	+	+	−	−	−	+	−	+	+
4	+	+	+	−	−	−	+	−	+	+	−
5	+	+	−	−	−	+	−	+	+	−	+
6	+	−	−	−	+	−	+	+	−	+	+
7	−	−	−	+	−	+	+	−	+	+	+
8	−	−	+	−	+	+	−	+	+	+	−
9	−	+	−	+	+	−	+	+	+	−	−
10	+	−	+	+	−	+	+	+	−	−	−
11	−	+	+	−	+	+	+	−	−	−	+
12	−	−	−	−	−	−	−	−	−	−	−

Although this 12-run design could be used to screen 11 factors, it has been recommended that the designs be used for 6 less factors than there are runs (Mason, Gunst, & Hess, 1989). Upon deleting 6 factors from the design, 5 degrees of freedom remain for an estimate of experimental error variance after the main effects are estimated from the design. Of course, this practice still assumes the main effects are the dominant effects in the experiment. For example, a 12-run design for 6 factors is obtained by using only the first six columns of the design in Table 12.11. There are a total of 11 degrees of freedom in the design, of which 6 can be used to estimate the main effects and 5 used for an estimate of experimental error variance.

Resolution IV Designs Require $2N$ Experimental Units for N Factors

The 2^{n-p} designs of Resolution IV have no main effects aliased with other main effects or two-factor interactions, and main effects can be estimated free of confounding with two-factor interactions. Such was not the case with Resolution III designs. A 2_{IV}^{n-p} design with $N = n$ must have at least $2n$ experimental units or runs.

A design of Resolution IV with n factors may be generated from a design of Resolution III with $n - 1$ factors by a *fold-over* technique. A second fraction is added to a Resolution III design for $n - 1$ factors in n runs. All signs in the second fraction are reversed from the first fraction. The I column in the first fraction has all $+$ coefficients, whereas in the second fraction the I column has all $-$ coefficients. This column of coefficients is used for an added factor. The design now has n factors with $2n$ runs.

The procedure is illustrated in Table 12.12 with the generation of a 2_{IV}^{4-1} design from a 2_{III}^{3-1} design. The first fraction of the design is a 2^{3-1} design of Resolution III, in which the coefficients for factor C are derived from the coefficients for AB interaction. The second fraction is derived from the first with opposite signs for the coefficients. The column originally labeled I is used for the fourth factor, D, to complete the 2^{4-1} design. The design can be seen to be of Resolution IV since it is a half fraction of a 2^4 design requiring one defining contrast. It is a complete 2^3 factorial in A, B, and D with the coefficients of C derived from $C = ABD$, so that the defining contrast is $C \times ABD = ABCD$. The resolution of the design is equal to the number of letters in the defining contrast $ABCD$, or Resolution IV.

Table 12.12 Generation of a 2^{4-1} Resolution IV design by folding over a 2^{3-1} Resolution III design

$I = D$	A	B	$C = AB$	
+	−	−	+	First fraction is a 2_{III}^{3-1} design
+	+	−	−	
+	−	+	−	
+	+	+	+	
−	+	+	−	Second fraction with opposite signs
−	−	+	+	
−	+	−	+	
−	−	−	−	

12.7 Genichi Taguchi and Quality Improvement

Finally, we note one special application of fractional factorial designs because of recent developments in the application of statistics to manufacturing, part of which deals directly with the issue of statistical designs.

Fractional factorial designs are used extensively in off-line experiments for product quality improvement. Off-line investigations integrate engineering design and statistical design principles to improve the quality of products and increase productivity. In particular, the Taguchi methodology (Taguchi, 1986) has had a major impact on product and process improvement design in manufacturing.

Robust parameter design is the one part of the complete Taguchi methodology that involves factorial treatment designs. The design consists of both factors that are controllable in the manufacturing process and those that are not controllable. The controllable factors are called *parameters* in Taguchi's terminology; those not controllable are called *noise* factors, or variables. The noise variables or factors are those most likely to be sensitive to changes in environmental conditions during production and thus transmit variability to the responses of interest in the process.

Both types of factors, controllable and noise, can be controlled in off-line experimentation, and that is an important consideration in the Taguchi methodology. One objective is to determine which combination of controllable factors is least sensitive to changes in the noise variables. This is the concept from which the term *robust parameter design* is derived. The best choice of controllable factor levels leads to a manufacturing process that results in the desired product and is *robust* to any fluctuations in the uncontrollable noise factors.

The design concept is illustrated with a hypothetical off-line experiment with three controllable factors, A, B, and C, each at two levels, and one noise factor, D, with three levels. An orthogonal array for the control factors is crossed with an orthogonal array for the noise factors. For our example with four factors, the 2^3 factorial for factors A, B, and C would constitute the *inner array* in Taguchi terminology, and the 3^1 for factor D would constitute the *outer array*.

The three control factors are configured in Table 12.13 in a $\frac{1}{2}$ fraction of a 2^3 factorial with 4 runs, or a 2^{3-1}_{III} fractional factorial, in the inner array. Three levels of the noise factor, D, in the outer array are used for each run in the 2^{3-1}_{III} design, resulting in 12 runs for the experiment.

Table 12.13 Hypothetical Taguchi experiment

A	B	C	D_0	D_1	D_2	$\overline{y}$	s
−	−	+	54	56	52	54	2.0
+	−	−	69	70	71	70	1.0
−	+	−	58	55	49	54	4.6
+	+	+	58	65	69	64	5.6

Suppose the manufactured product has a desired response target of 54. The mean, $\overline{y}$, and standard deviation, s, of the observations from the runs on the three levels of the noise factor, D, for each of the four runs on the control factors are shown in Table 12.13.

Two of the treatments for the control factors, $(-,-,+)$ and $(-,+,-)$, meet the desired product response of 54. However, the $(-,-,+)$ treatment, with a standard deviation of 2.0, is less sensitive to changes in levels of the noise factor (D) than the $(-,+,-)$ treatment with a standard deviation of 4.6. Thus, the product from the $(-,-,+)$ treatment would be considered the *more robust* product since fluctuations on the noise factor (D) transmit less variability to the product's response.

The Taguchi analysis method concentrates on the maximization of a signal-to-noise ratio (SNR) specific to the process goals. The primary choices for goals are (1) minimize the response, (2) maximize the response, and (3) achieve a specified response target value other than minimum or maximum.

The method proceeds rather simply with an analysis of variance for the SNR that determines which control factors affect the SNR. Then the method continues with an analysis of variance for the $\overline{y}$ that ascertains which control factors impact the mean response and thus which control factors' levels can be set to achieve a process target response. Myers and Montgomery (1995) illustrate other details of the analysis for the interested reader.

Pignatiello and Ramberg (1991) discuss major advantages and disadvantages of the Taguchi approach. Myers and Montgomery (1995), Montgomery (1997), and Vining (1998) present more detailed descriptions, examples, and critiques of the Taguchi methodology. A major contribution of the Taguchi method was the validation of experimentation and statistical design as part of the quality improvement process. However, the Taguchi designs have evoked considerable controversy over the particular designs used and their implementation.

Although many standard 2^{n-p} fractional factorial designs are recommended in the Taguchi method, many are saturated or nearly saturated Plackett–Burman designs or fractional designs with three levels to detect response curvature. An assumption of no interaction among the control factors often is necessary for successful use of these designs. The crossing of orthogonal design for the control factors with that for the noise factors provides ample ability to estimate interactions between control and noise factors, but the ability to estimate important interactions among the control factors has been sacrificed to do so. Myers and Montgomery (1995) suggest several design strategies that could be more economical of experimental runs and yet provide the means to estimate interactions of interest to the investigator. Hunter (1985) presents a detailed discussion of the Taguchi three-level fractional designs based on Latin squares.

Better sequences of design strategies at the onset also have been suggested by Hunter (1989) and Pignatiello and Ramberg (1991). As one example, the sequence could start with 2^{n-p} fractional factorials to detect interactions with factor levels added to the center of the design to detect curvature. Additional suggested refinements as the process is characterized include the use of complete factorials and response surface designs (see Chapter 13).

As a final point, randomization is not advocated in the Taguchi approach; it can lead to important effects confounded with external variations being unaccounted for by the design.

12.8 Concluding Remarks

As we have seen in Chapters 11 and 12, factorials are versatile treatment designs that can be adapted to a variety of experimental conditions, including single replications, fractional replications, and experiments requiring incomplete block designs.

Only the rudimentary principles for the construction and analysis of these designs have been presented here. An extensive list of literature exists on fractional factorial designs for experimental work in industry. Extensive discussions on the design and analysis of fractional factorials in various fields of application may be found in Daniel (1976), Box, Hunter, and Hunter (1978), Diamond (1989), Haaland (1989), and Mason, Gunst, and Hess (1989). Some of the cited references contain tables of specific designs derived from the basic principles presented in this chapter. Diamond (1989) provides an extensive bibliography of published literature relating to fractional factorials.

An abundant number of commercial software products have been developed to produce designs and analyses for any experiments that require fractional factorial designs.

Fractional Designs for 3^n Factorials

Fractional replications may be constructed for 3^n factorials and other factorial series using the same principles as those for the 2^n series. One-third and one-ninth fractional factorials are possible with the 3^n series. Aliases for factorial effects are determined on the basis of symbolic products with the defining contrasts and squares of defining contrasts. An extensive selection of fractional factorial designs for the 3^n series is available in Conar and Zelen (1959). Details on the construction of fractional factorials in other than the 3^n series can be found in Kempthorne (1952) and John (1987). Examples of some designs can be found in Cochran and Cox (1957), Johnson and Leone (1977), and Montgomery (1997).

EXERCISES FOR CHAPTER 12

1. A list of designs is shown below.

i.	2^{5-1}	v.	2^{7-2}	ix.	2^{9-5}
ii.	2^{6-1}	vi.	2^{7-3}	x.	2^{10-6}
iii.	2^{6-2}	vii.	2^{7-4}	xi.	2^{11-6}
iv.	2^{6-3}	viii.	2^{8-4}	xii.	2^{15-11}

Indicate the following quantities for each design:
a. the fraction of the full design
b. the number of generators required
c. the number of generalized interactions
d. the number of aliases for each effect
e. the number of experimental units or runs required

2. Construct a 2^{5-1} fractional factorial design. Use $I = ABCDE$ as the defining relationship.
a. What is the resolution of the design?
b. Show the alias structure for main effects and two-factor interactions.

3. Construct a 2^{6-2} fractional factorial design. Use $ABCD$ and $CDEF$ as the design generators.
a. What is the generalized interaction?
b. What is the resolution of the design?
c. Show the alias structure for all main effects and two-factor interactions.

4. Construct a Resolution IV design that is a $\frac{1}{8}$ fraction of a 2^7 factorial.

5. If you fold over a 2^{5-2}_{III} fractional factorial, is the resulting design a 2^{6-2}_{IV} fractional factorial? Explain.

6. A 2^{5-2} fractional factorial is proposed with two possible generating relations:

i. $I = ABCD = BCE$

ii. $I = ABCDE = ABCD$

a. What fraction of a 2^5 design will it be?
b. Which defining relationship is preferred for the design? Explain.

7. A 2^{5-2}_{III} fractional factorial is designed with defining relationship $I = ACE = BCDE$. It is known that factors A, B, and C do not interact with one another and factors C, D, and E do not interact with one another.
a. Which effects can be estimated ignoring three-factor and larger interactions?
b. Is it possible to have a better 2^{5-2}_{III} design for this situation? Explain.

8. A researcher conducts an experiment with eight runs using a 2^{5-2} fractional factorial constructed with the equivalencies $D = AB$ and $E = ABC$. Then, the researcher conducts another eight runs with the same five factors but reverses the signs used in the original eight runs for each of the factors.
a. What is the resolution of the design with the original eight runs?
b. What is the resolution of the 16-run design?
c. Suppose it is known at the start that 16 runs are going to be used for the experiment. Can a better design of 16 runs be constructed? If so, construct the design and give its properties.

9. A 2^{6-3}_{III} fractional factorial design is generated with the equivalencies $D = AB$, $E = AC$, and $F = ABC$. What is the resulting design if the factors D and E are dropped from the design?

10. A process under study involved the heat treatment of leaf springs for trucks. The assembled leaf spring was put through a high-temperature furnace, transferred to a forming machine where the curvature of the spring was formed, and then submersed in an oil quench. The measurement of importance was the free height of the spring in the unloaded condition. The target value of the free height was 8 inches. Deviations from this value were considered undesirable. An experiment was conducted to evaluate the effect of four factors on the deviation of free height from the target value in the manufacturing process. The factors considered were (A) furnace temperature; (B) heating time; (C) transfer time, or the length of time to transfer the spring assembly from furnace to curvature former; and (D) hold-down time, or the time the curvature former is closed on the hot spring.

In addition, the engineers were interested in the two-factor interactions AB, AC, and BC as well as the main effects of each factor. A 2^{4-1} fractional factorial was used for the experiment with the defining relationship $I = ABCD$. The eight treatment combinations follow with the signal-to-noise ratio (Z) for six springs constructed with each treatment. The signal-to-noise ratio is a measure of the deviation of the springs from the target value.

Run	*A*	*B*	*C*	*D*	*Z*
1	+	−	−	+	29.46
2	−	−	+	+	28.11
3	−	−	−	−	28.00
4	+	−	+	−	30.59
5	+	+	+	+	35.31
6	+	+	−	−	38.68
7	−	+	+	−	31.55
8	−	+	−	+	47.70

Source: J. J. Pignatiello and J. S. Ramberg (1985), *Journal of Quality Technology* 17, 198–206. Discussion of article by R. N. Kackar (1985). Off-line quality control, parameter design, and the Taguchi method. *Journal of Quality Technology* 17, 176–188.

a. What is the resolution of this design?
b. Show how the + and − signs were determined for factor D.
c. Show the alias structure for this design.
d. What must be assumed to obtain estimates of the two-factor interactions AB, AC, and BC?
e. Estimate the main effects and two-factor interactions of interest and their standard errors.
f. Interpret the results.

11. The manufacturer of instant soup products wanted to produce dry soup mix packages with minimum weight variation among packages. Five factors were identified that might influence variation in the filling process. A 2^{5-1} fractional experiment with the defining relationship $I = -ABCDE$ was conducted to evaluate the effects of the factors and their interactions. The factors and levels were (A) the number of mixer ports through which the vegetable oil was added (1 and 3), (B) the temperature surrounding the mixer (− = cooled or + = ambient temperature), (C) the mixing time (60 and 80 sec), (D) the batch weight (1500 and 2000 lb), and (E) the days of delay between mixing and packaging (1 and 7). Between 125 and 150 packages of soup mix were sampled over an eight-hour production run for each treatment combination. The standard deviation for the weight of the packages was computed as a measure of variation in the filling process and used as the response variable y. The factor levels and the process standard deviation for each of the 16 treatment combinations follow.

Run	*A*	*B*	*C*	*D*	*E*	*y*
1	−1	−1	−1	1	1	0.78
2	1	−1	1	1	1	1.10
3	1	1	−1	−1	−1	1.70
4	1	−1	1	−1	−1	1.28
5	−1	1	−1	−1	1	0.97
6	−1	−1	1	−1	1	1.47
7	−1	1	−1	1	−1	1.85
8	1	1	1	1	−1	2.10
9	−1	1	1	1	1	0.76
10	1	1	−1	1	1	0.62
11	−1	−1	1	1	−1	1.09
12	−1	−1	−1	−1	−1	1.13
13	1	−1	−1	−1	1	1.25
14	1	1	1	−1	1	0.98
15	1	−1	−1	1	−1	1.36
16	−1	1	1	−1	−1	1.18

Source: L. B. Hare (1988), In the soup: A case study to identify contributors to filling variability. *Journal of Quality Technology* 20, 36–43.

a. What is the resolution of this design?
b. Show how the + and − signs for levels of factor E were determined for the design.
c. Show the alias structure for the design.
d. What assumptions must be made to estimate main effects and two-factor interactions free of any other effects?
e. Estimate the main effects and two-factor interactions of interest and their standard errors.
f. Interpret the results.
g. Construct a quarter fraction design from the present design.

12. A 2^{7-4} fractional factorial, or eight-run, Plackett–Burman design was used to study the effects of seven factors on oil consumption in diesel engines. Good oil control consumption is considered to be 0.195 grams/horsepower hour (g/hp hr). The seven engine factors each used at two levels were (A) top ring fit, (B) top ring twist, (C) intermediate ring face, (D) intermediate ring type, (E) piston crown clearance, (F) oil ring width, and (G) use of liner tabs. The oil consumption in grams/horsepower hour (y) is shown for each of the eight runs.

Run	*A*	*B*	*C*	*D*	*E*	*F*	*G*	*y*
1	−	−	−	+	+	−	+	0.204
2	−	−	+	+	−	+	−	0.662
3	−	+	−	−	+	+	−	1.075
4	−	+	+	−	−	−	+	0.404
5	+	−	−	−	−	+	+	0.445
6	+	−	+	−	+	−	−	1.297
7	+	+	−	+	−	−	−	0.386
8	+	+	+	+	+	+	+	1.157

Source: P. R. Shepler (1975), Fractional factorial plans for diesel engine oil control and for seals. *Proceedings of the 31st National Conference on Fluid Power*, 204–234.

a. What is the resolution of this design? What does the resolution of this design imply with regard to the estimation of factorial effects?
b. What fraction of a 2^7 factorial is this design?
c. Determine the design generators.
d. Show the alias structure for the main effects of the design.
e. Estimate the main effects.
f. Interpret the results.

The objective was to find the best configuration of the seven factors to reduce oil consumption. The engineers determined that the best test was obtained with run 1 (0.204 g/hp hr). They also considered a ratio of the average response of the − levels and + levels (largest average/smallest average) for each factor and called it the "favor ratio." If the largest average was from the − levels, the favor ratio was given a − sign; and if the largest average was from the + levels, the favor ratio was given a + sign. The favor ratio signs were the same as that for run 1 except for the sign of factor E, piston crown clearance. To check the results of their previous tests they conducted three additional runs. The first run, labeled "check," included factor levels the same as those for the favor ratio except for two factors A and G. The second run, labeled "proof," included all the same factor levels as those for the favor ratio. The third run was a repeat of run 1 with factor E at the + level. Their past experience indicated that large piston crown clearance (+ level) almost always had been better for oil control. The results of the three additional runs are shown along with the favor ratio.

Run	A	B	C	D	E	F	G	y
Check	+	−	−	+	−	−	−	0.435
Proof	−	−	+	+	−	−	+	0.662
Repeat 1	−	+	−	−	+	+	−	1.075
Favor ratio	−	−	−	+	−	−	−	

g. Were the engineers' conclusions based on the favor ratio consistent with the estimates of the effects from the test runs? Explain. Under what circumstances would the favor ratio be consistent with main effect estimates?
h. Were the engineers' suspicions about factor E correct? Explain.

12A Appendix: Fractional Factorial Design Plans

Table 12A.1 Selected 2^{n-p} fractional factorial designs

Number of Factors	*Experimental Units*	*Fraction*	*Design Resolution*	*Design Generator**
3	4	$\frac{1}{2}$	III	$C = AB$
4	8	$\frac{1}{2}$	IV	$D = ABC$
5	8	$\frac{1}{4}$	III	$D = AB$ $E = AC$
	16	$\frac{1}{2}$	V	$E = ABCD$
6	16	$\frac{1}{4}$	IV	$E = ABC$ $F = BCD$
	8	$\frac{1}{8}$	III	$D = AB$ $E = AC$ $F = BC$
7	32	$\frac{1}{4}$	IV	$F = ABCD$ $G = ABDE$
	16	$\frac{1}{8}$	IV	$E = ABC$ $F = BCD$ $G = ACD$
	8	$\frac{1}{16}$	III	$D = AB$ $E = AC$ $F = BC$ $G = ABC$
8	64	$\frac{1}{4}$	V	$G = ABCD$ $H = ABEF$
	32	$\frac{1}{8}$	IV	$F = ABC$ $G = ABD$ $H = BCDE$
	16	$\frac{1}{16}$	IV	$E = BCD$ $F = ACD$ $G = ABC$ $H = ABD$

Table 12A.1 *(continued)*

Number of Factors	*Experimental Units*	*Fraction*	*Design Resolution*	*Design Generator**
9	16	$\frac{1}{32}$	III	$E = ABC$
				$F = BCD$
				$G = ACD$
				$H = ABD$
				$J = ABCD$
	64	$\frac{1}{8}$	IV	$G = ABCD$
				$H = ACEF$
				$J = CDEF$
	32	$\frac{1}{16}$	IV	$F = BCDE$
				$G = ACDE$
				$H = ABDE$
				$J = ABCE$
10	16	$\frac{1}{64}$	III	$E = ABC$
				$F = BCD$
				$G = ACD$
				$H = ABD$
				$J = ABCD$
				$K = AB$
	32	$\frac{1}{32}$	IV	$F = ABCD$
				$G = ABCE$
				$H = ABDE$
				$J = ACDE$
				$K = BCDE$
	64	$\frac{1}{16}$	V	$G = BCDF$
				$H = AGDB$
				$J = ABDE$
				$K = ABCE$
	128	$\frac{1}{8}$	V	$H = ABCG$
				$J = BCDE$
				$K = ACDF$

* Either the positive or negative half of the design generators may be used to construct the fractional design.

13 Response Surface Designs

The central topic in this chapter is constructing designs for efficiently estimating response surfaces from factorial treatment designs with quantitative factors. The nature of linear and quadratic response surfaces is discussed, and designs developed specifically for response surface experiments are described. The discussions include estimating response surface equations and methods to explore the surfaces. Special designs are presented for experiments with factors that are ingredients of mixtures.

13.1 Describe Responses with Equations and Graphs

The objective of all experiments includes describing the response to treatment factors. Throughout this text when treatment factors had quantitative levels we have characterized the response (y) to the factor levels (x) with the polynomial regression equation. For example, in Chapter 3 a polynomial equation was used to estimate the relationship between seed production of plants, y, and density of plants in the plot, x. The estimated quadratic regression equation was graphed as a curve, and we were able to visualize the response of seed production to plant density throughout the range of plant densities included in the experiment. One of the main advantages of the response curve includes the ability to visualize the responses throughout the range of factor levels included in the experiment.

Response Surface Graphs for Two Treatment Factors

The response equation can be displayed as a surface when experiments investigate the effect of two quantitative factors such as the effect of temperature and pressure on the rate of a chemical reaction. In Chapter 6 a quadratic polynomial for two quantitative factors, salinity of media and number of days, was estimated to characterize plant response.