CHAPTER 8

Robust Designs

We begin at the outset by pointing out that model-robust designs are not discussed in this chapter, as that is a type of robustness that is different from the type with which we are concerned in the following sections. Model-robust designs are robust under various models that contain all the available main effects and some of the interactions. They are discussed in Section 13.19. Similarly, we will not be particularly concerned with designs that are robust to errors in the settings of factors, although this is certainly not unimportant. Box and Draper (1987) stated that "good behavior when errors in settings of input variables occur" is a desirable property of a response surface design and is obviously a desirable property of any type of design. (Some of the other desirable properties of designs were discussed in Section 1.2.)

Box (1994) made the important point that for a robust design (as discussed in the next paragraph) to be effective, the functional relationship between the response variable and the factors needs to be known, at least approximately, as well as "the standard deviations of all of the error variables and design variables, and the *dependence of these standard deviations on the nominal levels of the variables*." This is certainly important, just as good statistical models are always important, in general, but the focus in this chapter is on robust designs as have been given in the literature.

Here is what we *will* discuss. In the 1980s, the concept of a robust design was popularized by Genichi Taguchi, and there have been many refinements since then. If a designed experiment is used to select optimal settings of the factors under consideration so as to optimize (maximize or minimize) the response, it is certainly desirable that when these settings are used in production, the response values are not greatly affected by particular levels of *noise factors*. The latter are variables, such as environmental variables, which can be fixed in the controlled setting used for the experiment, but cannot otherwise be set at any specific values. Companies also want the performance of their products to be relatively insensitive to variations in intended customer usage, such as toys that might be banged around excessively by children.

We will use the term *control factor* to indicate a factor that is of interest, with a robust design consisting of noise factors and control factors. Of course when noise factors are used in a design, a decision must be made as to the levels of the noise factors to use, just as this decision must be made for control factors (recall the discussion of this in Section 1.6.2.2). Steinberg and Bursztyn (1998) recommended that the level of each noise factor be set at $\pm 1.5\sigma_{\mathrm{N}}$, with σ_{N} denoting the standard deviation of the noise factor. Of course this requires an estimate of σ_{N}.

Another one of Taguchi's major contributions during the 1980s and thereafter was to motivate statisticians and experimenters to separate factors that affect the average value of the response (i.e., location effects) from factors that have dispersion effects (i.e., affect the variability of the response variable). This is easier said than done, however, as location and dispersion effects will generally be confounded in fractional factorial experiments and very often a complete separation will not exist even when there are enough observations to allow the effects to be estimated independently.

Consequently, an experimenter will frequently have to choose between a desirable combination of levels from a location standpoint (i.e., maximizing or minimizing the response), and minimum dispersion. For example, in analyzing Taguchi's arc welding experiment, Bisgaard and Pinho (2003–2004) pointed out that if high tensile strength was to be achieved, the material that exhibited the largest dispersion had to be used, whereas the material that produced the least amount of dispersion seemed to have inferior breaking strength. Occasionally (and perhaps, rarely), it will be possible to accomplish both objectives. For example, Ankenman and Dean (2003, p. 299) described an experiment, concluding "…and so the response variability and the average response be minimized simultaneously in this experiment."

8.1 "TAGUCHI DESIGNS?"

There has been a vast amount of misinformation regarding the field of experimental design and Taguchi's contribution to it. Some sources have even given Taguchi credit for inventing design of experiments. (See, for example, Sharma (2003) who stated "…DOE was invented in the 1960s and developed by people like Taguchi and the military.") Similarly, the promotional material for *Taguchi's Quality Engineering Handbook* by Taguchi, Chowdhury, and Wu (2005) reads "Design of Experiments (known as the Taguchi method)." Gell, Xie, Ma, Jordan, and Padture (2004) stated, "In this study, a Taguchi design of experiments was employed…." Later in this article the authors stated "The selected Taguchi orthogonal array is $L_{27}(3^{13})$." This is another misconception as only a few of the orthogonal arrays that have been attributed to Taguchi were actually invented by Taguchi. (The notation $L_{27}(3^{13})$, as used in Chapter 6, means that 13 three-level factors are examined in 27 runs. Addelman (1962, p. 38) presented a 3^{13-10} design that differs only slightly from the L_{27} array.) Note that full and fractional factorials are orthogonal arrays, but not every fractional factorial is an orthogonal array, as stated in earlier chapters (see Raktoe, Hedayat, and Federer (1981) for details).

Taguchi's approach to experimental design was laid out in Taguchi (1987), a two-volume set. Volume 1 was reviewed by Senturia (1989) and Volume 2 was reviewed by Bisgaard (1989a). The reader may wish to read these reviews, especially Bisgaard (1989a), which is an appropriately critical review and which also draws a conclusion about Volume 1, as well as Box, Bisgaard, and Fung (1988) and Bisgaard (1989b), which is a nontechnical article. Bisgaard (1989a) stated we would be regressing to a level that even the books on experimental design in the 1940s would exceed if Volume 1 were to become "a standard course on the design of experiments" and pointed out that one often encounters methods in Volume 2 that are "... overly complicated, inefficient, and sometimes simply wrong."

The orthogonal arrays that Taguchi has advocated have received some criticism. Ryan (1988) was apparently the first to point out in print that some of these orthogonal arrays are equivalent to suboptimal fraction factorials, suboptimal in the sense that the resolution of the design is not maximized. This is also discussed in Ryan (2000, p. 442).

When these arrays are generated with software, the low level is denoted by "1" and the high level by "2." For example, given below is the $L_8(2^7)$ produced by the Design-Expert software, with the labeling of the columns being that given by Taguchi and Wu (1979), for example, under the assumption that there are four factors and two of the interactions are of interest.

Std. Order	Run Order	B	C	BC	D	BD	A	e
1	5	1	1	1	1	1	1	1
2	8	1	1	1	2	2	2	2
3	4	1	2	2	1	1	2	2
4	2	1	2	2	2	2	1	1
5	6	2	1	2	1	2	1	2
6	7	2	1	2	2	1	2	1
7	3	2	2	1	1	2	2	1
8	1	2	2	1	2	1	1	2

It is not obvious that the column labeled BC is the product of the columns labeled B and C, but this does become apparent if the (1, 2) level designation is replaced by (0, 1), so as to produce the following configuration:

Std. Order	Run Order	B	C	BC	D	BD	A	e
1	5	0	0	0	0	0	0	0
2	8	0	0	0	1	1	1	1
3	4	0	1	1	0	0	1	1
4	2	0	1	1	1	1	0	0
5	6	1	0	1	0	1	0	1
6	7	1	0	1	1	0	1	0
7	3	1	1	0	0	1	1	0
8	1	1	1	0	1	0	0	1

Then the BC interaction levels are the sum of the B and C levels, mod 2. (Recall this use of modular arithmetic in Section 6.1.1, for example.) With this change, the levels of both of the interaction columns are easy to verify, using modular arithmetic.

With this designation we can easily see, for example, that $A = CD$, and also that $C = AD$ and $D = AC$. Thus, the design could not be resolution IV, whereas a 2_{IV}^{4-1} design could be constructed. The L_8 when used with four factors *must* be equivalent to "some" 2^{4-1} design and from the alias structure we know that this is the half fraction with the defining relation $I = ACD$, whereas the defining relation of the 2_{IV}^{4-1} design is of course $I = ABCD$.

8.2 IDENTIFICATION OF DISPERSION EFFECTS

Methods for checking on homogeneity of variance in linear models can be adapted to the detection of dispersion effects. As discussed by, for example, Bisgaard and Pinho (2003–2004), this can be accomplished by computing the residuals from an appropriate model for location. If location were the sole interest, the residuals would be used to test the model assumptions and to check for bad data. Certainly this should still be done, but in searching for dispersion effects the residuals would be additionally used as the response and a normal probability plot analysis performed. It is necessary for the important effects to be used in the first stage in the model that produces the residuals, however, because if this isn't done, important location effects will be spotlighted by the normal probability plot because this is what happens when necessary terms are left out of the model.

Even if all important terms have been included, however, the plot can only be suggestive of possible dispersion effects since replication is necessary to identify such effects, an important but often overlooked point that was originally made by Box and Meyer (1986). It is becoming better known that certain tests for dispersion effects, such as F-tests, are undermined by the failure to identify significant location effects. In particular, Schoen (2004) stated, "Recent literature shows a severe sensitivity of the dispersion F-test to unidentified location effects, to the link function for the variance, and to the presence of other dispersion effects."

The following examples illustrate these ideas.

Example 8.1

Steinberg and Bursztyn (1994) showed with two examples how the failure to model noise factors can cause errors in the identification of dispersion effects. The first example utilized data from Engel (1992), who described an experiment designed to improve an injection molding process. The goal was to determine factor settings for which the amount of percent shrinkage (the response variable) would be close to the target value. The latter was not specified, however.

Seven control factors and three noise factors were used in the experiment. A 2^{7-4} design was used for the control factors, which is referred to as the *inner array*. This design was crossed with a 2^{3-1} design that was used for the noise factors, that being the *outer array*. Thus, there were 32 design points and the use of an inner array and an outer array means that a *product array* was used. In a *combined array*, there is only one array, which contains the columns for both the controllable and noise variables.

TABLE 8.1 Data from Injection Molding Experiment

Control Factors							Noise Factors (M, N, O)			
A	B	C	D	E	F	G	(−1, −1, −1)	(−1, 1, 1)	(1, −1, 1)	(1, 1, −1)
−1	−1	−1	−1	−1	−1	−1	2.2	2.1	2.3	2.3
−1	−1	−1	1	1	1	1	0.3	2.5	2.7	0.3
−1	1	1	−1	−1	1	1	0.5	3.1	0.4	2.8
−1	1	1	1	1	−1	−1	2.0	1.9	1.8	2.0
1	−1	1	−1	1	−1	1	3.0	3.1	3.0	3.0
1	−1	1	1	−1	1	−1	2.1	4.2	1.0	3.1
1	1	−1	−1	1	1	−1	4.0	1.9	4.6	2.2
1	1	−1	1	−1	−1	1	2.0	1.9	1.9	1.8

(Early work on constructing outer array points was given by Wang, Lin, and Fang, 1995.)

The design(s) and the observed percentage shrinkages as given in Steinberg and Bursztyn (1994) were as follows, with the triplets in parentheses denoting the levels of the three noise factors (Table 8.1).

We can see by the way that the design in the control factors is listed that although this is equivalent to a 2^{7-4} design, it was probably not constructed as a fractional factorial, but rather a Taguchi orthogonal array was used, specifically the L_8. This seems apparent because the first three columns do not constitute a full factorial design. Columns A, B, and D do constitute a 2^3 design, however, with $C = -AB$, $E = -AD$, $F = -BD$, and $G = ABD$. Similarly, the design in the noise factors has $O = -MN$.

Engel (1992) analyzed the data by computing an average at each design point, averaging over the noise factors, and obtained a model for the mean with terms that were the main effects of factors A, D, and E. Thus, the noise factors were not analyzed and, in particular, no control × noise interactions could be computed. Steinberg and Bursztyn (1994) analyzed the data as having come from a 2^{10-5} design, so that the noise factors are analyzed as factors and all 32 observations are used. Although this can certainly be done and is clearly preferred over the analysis method of Engel (1992), the "product design" so obtained is not necessarily a good design. The product of the two designs cannot have a higher resolution than the smaller resolution of the two designs that are used to form the product (see, e.g., Ryan, 2000, p. 449). Therefore, the product design cannot be greater than resolution III, which is also obvious from the fact that $C = -AB$, for example. A 2_{IV}^{10-5} design can be constructed, so if it were known in advance that all 10 factors were to be analyzed, then a 2_{IV}^{10-5} could have been used.

In this case Steinberg and Bursztyn (1994) were simply trying to salvage what they could from the experiment. As they pointed out, however, the product design does permit the estimation of all of the control factor × noise factor interactions, but this comes at the cost of having the main effects of the control factors confounded with two-factor interactions of the control factors. The advantages and disadvantages of a product array are discussed in detail in Section 8.3. With 32 observations, 31 effects could be estimated; the 10 main effects and $7 \times 3 = 21$ two-factor interactions of the control factors and noise factors use all of those degrees of freedom.

One important finding of the experiment, which should be apparent from Table 8.1, is that the variability in the response is much less when factor F is at the low level than when the factor is at the high level. This would suggest using the low level of the factor since the average response for the two levels does not differ greatly. This conclusion, however, would be based on the standard deviations at each design point, which are computed from only four numbers. Furthermore, there are two suspicious data points that have a considerable effect on the conclusions, as noted by Steinberg and Bursztyn (1994). The reader is asked to pursue this line of analysis in Exercise 8.3.

The variability might have been modeled, such as by using $\log(s)$ as the response variable, with s being the standard deviation. Only main effects of the control factors could be estimated, however, as that would use up all of the degrees of freedom, but the control factors that affect the variability can be seen by looking at Table 8.1.

8.3 DESIGNS WITH NOISE FACTORS

There are two major ways in which noise factors can be used in an experimental design: (1) by constructing a separate design for the noise factors and using the resultant number of design points at *each* treatment combination for the main design of the factors of interest, or (2) by constructing a single design and simply having the noise factors be part of the design. The former is called a product array, which will usually be too expensive to run, and the latter is a combined array, as indicated previously.

We hope that noise factors are not significant because since they are uncontrollable in practice, we cannot set a noise factor at what might seem to be the most desirable level. Similarly, noise $\times$ noise interactions that are significant are also of no value to us, but control factor $\times$ noise factor interactions are important and provide useful information. Accordingly, some work has been directed at identifying these interactions, including Russell, Lewis, and Dean (2004). See also Vine, Lewis, and Dean (2005) who proposed a two-stage group screening procedure that was based upon subjective probabilities of the various effects, including control $\times$ noise interactions, being real effects. Bingham and Li (2002) stated that the models of primary interest are those that contain at least one control factor $\times$ noise factor interaction and introduced the model-ordering principle with models being ranked by their order of importance. Bingham and Sitter (2003) considered the use of fractional factorial split-plot designs in robust parameter design work. Other work includes Kuhn (2003), Kuhn, Carter, and Myers (2000), and Bisgaard and Ankenman (1995), with the latter formulating the parameter design problem as a constrained optimization problem.

As a simple illustration of this, assume that we have only a single control factor and a single noise factor and their interaction graph that results from a particular experiment is given in Figure 8.1.

If the objective is to minimize the variability of the response, the high (+1) level of the control factor should be used since the response is constant over the two noise levels for that level of the control factor. Interaction plots such as the idealized, in terms

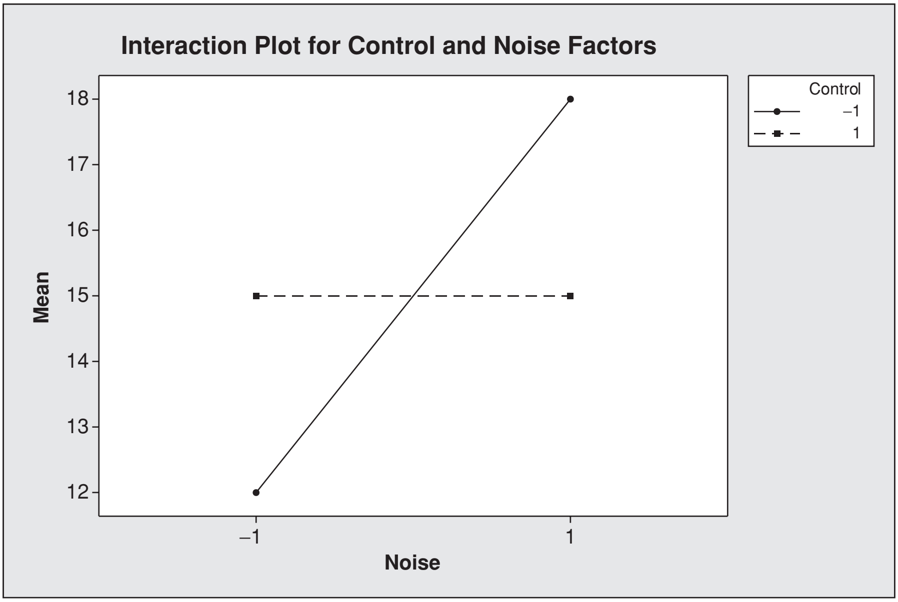

Figure 8.1 Control × noise interaction.

of robustness, plot in Figure 8.1 are potentially very helpful and should be routinely constructed, just as we construct interaction plots when factorial designs are used. We have to look at the plots and not just go by the magnitude of the interaction; the reason for this is explained in Section 8.4.

Certainly these plots are more useful than the results that are obtained by modeling location and dispersion separately, which is the preferred alternative to the use of signal-to-noise (S/N) ratios that were debunked many years ago (see, e.g., Box, 1988; Nair, 1992). (We do not discuss S/N ratios in this chapter, as the emphasis is on experimental designs. Readers interested in the subject can find these ratios discussed in books and articles on the Taguchi methods.)

What should be done if the objective is to maximize the average response? In the absence of any information regarding which noise level should predominate in practice, nothing should be done.

Of course, ideally, we would like to be able to identify control factors that affect dispersion but do not affect location, and then use a set of factors that affect location, but not dispersion, to arrive at the factor settings that maximize or minimize the average response.

It seems unlikely that such a scenario will be encountered very often, however, when we view the problem in terms of conditional effects. The main effect estimate for the control factor shown in Figure 8.1 is zero because the average response is the same at each level of the control factor. The conditional effects are 3 and −3, however, and this is 20 percent of the average response. Such a difference may or may not be deemed significant, but clearly we should not say that the factor has no effect. (Note that if the line for the low level of the control factor were twisted so that the slope increased, the absolute value of the conditional effects would increase but the main effect estimate would remain at zero. Thus, it would be easy to envision a scenario for which the conditional effects would undoubtedly be significant.)

Stated generally, if a control factor has a dispersion effect, then the lines in the interaction graph cannot be parallel, nor can one slope of a line be the negative of the slope of the other line. In the latter case the main effect estimate would be zero and we might erroneously conclude that a factor has neither a location effect nor a dispersion effect even though the conditional effects could be quite large.

Thus, we must consider conditional effects when robust designs are used, just as we must do so when factorial designs are used without noise factors.

8.4 PRODUCT ARRAY, COMBINED ARRAY, OR COMPOUND ARRAY?

An experimenter who decides to use a robust design can select a design from certain types of robust designs, the best known of which are product arrays and combined arrays.

With the product array approach, a separate factorial or fractional factorial design or orthogonal array is constructed for the control factors and the noise factors (termed the inner array and outer array, respectively, as stated in Example 8.1). The product array is simply the product of the inner array and the outer array, as indicated previously.

As a simple example, assume that there are two control factors and a 2^2 design is constructed. Similarly, a 2^2 design is also constructed for the noise factors. When we take the product of the two designs, we will have a design with 16 points because the outer array with the noise factors is at each of the four design points of the inner array. So the design has $4 \times 4 = 16$ points. What is the nature of this particular product array? That is, how can the resultant array (design) be described? As the reader is asked to show in Exercise 8.1, the result is a 2^4 design.

Shoemaker, Tsui, and Wu (1991) proved that the effects that are estimable in the product array are the effects that are estimable in the inner and outer arrays, respectively, plus all generalized interactions of those estimable effects. For the present example, let A and B denote the control factors and let C and D denote the noise factors. The effects that are estimable in the inner array are thus A, B, and AB, with C, D, and CD estimable from the outer array. The combination of these estimable effects with all of their cross products obviously constitutes the effects that are estimable with a 2^4 design.

This is clearly not a large design, but if either the number of control factors or the number of noise factors is larger than 2 (or both), the number of design points could be too large if a full factorial were used for each array. For example, 128 runs would be required if the inner array had four factors at two levels and the outer array had three factors at two levels. Consequently, a fractional factorial would often have to be used, but another disadvantage of the product array approach is the inflexibility regarding the effects that can be estimated. As stated previously, the effects that are estimable beyond those that are estimable in each array are the generalized interactions of those estimable effects. Those generalized interactions will often include effects that the experimenter would not want to estimate, however. With the example given previously, an experimenter would generally not want to estimate the ABCD interaction as four-factor interactions rarely exist.

A combined array has the advantage of economy, but care should be exercised to ensure that certain categories of effects are not confounded. In particular, we don't want to confound control $\times$ noise interactions with anything that is likely to be significant, as these interactions are the key to robust design. Therefore, we would want to confound those interactions with noise $\times$ noise interactions or with high-order interactions among the control factors.

A compromise between a product array and a combined array is a *compound array*, as introduced by Rosenbaum (1994, 1996). Such a design has a specified number of treatment combinations of noise factors for each treatment combination of control factors, with the number being less than all possible combinations (as would be used in a product array). Rosenbaum (1996) supports the use of a (4, 3, 4) design, with $(\gamma, \lambda, \alpha)$ denoting a design that is of resolution α for the control and noise factors combined, of resolution γ for the control factors only, and of resolution λ for the noise factors only. The advantage of a compound array for the designs considered by Rosenbaum (1996) is that by using a combined array rather than a product array, α will be 4 instead of 3. This is desirable because, as stated previously, it is important to be able to estimate the control $\times$ noise interactions. Of course with a resolution IV design the two-factor interactions will be confounded among themselves, so having $\alpha = 4$ is helpful only if the control $\times$ noise interactions are confounded with noise $\times$ noise interactions, *and* the noise $\times$ noise interactions can be assumed to be quite small. That is, each control $\times$ noise interaction must not be confounded with a main effect of a control factor nor confounded with another control $\times$ noise interaction.

Certainly that is necessary for Figure 8.1 to point the direction toward desired results, as we want to be able to select the value of the control factor so as to reduce variability, but we won't necessarily be able to do that if that particular interaction is confounded with another two-factor interaction that is significant, or with a significant main effect.

It is important to look at graphs such as Figure 8.1 and not simply go by the magnitude of a control $\times$ noise interaction, as it is the *shape* of the interaction profile that is important. More specifically, a control $\times$ interaction that graphs as an "X" will have a larger sum of squares than the interaction depicted in Figure 8.1, but an interaction that graphs as an "X" is of no value in selecting control factor settings so as to minimize dispersion since with such a configuration the two levels of the control factor (assuming there are two) result in equal variability. So we can't just go by sums of squares and F-statistics. Shoemaker et al. (1991) were apparently the first to point this out.

This begs for an analysis using graphs rather than a strictly numerical approach (ANOVA tables, etc.) for identifying important control $\times$ noise interactions. Indeed, Ryan (2000, p. 453) showed that for a 2^2 design, the A $\times$ B interaction must be almost to the point of being of practically no value from a robustification standpoint before it is declared significant using a method for analyzing unreplicated factorials such as the one given by Lenth (1989).

The important control $\times$ noise interactions are those for which the absolute values of the slopes of the lines that connect the points in the graph differ more than slightly. Consequently, software for robust design and analysis might be constructed so as to rank order the interactions by this criterion, but I am not aware of any software that does so. Of course with some software packages this could be easily done as the effect estimates and the corresponding names could be stored and then sorted into descending order.

Example 8.2

Brennerman and Myers (2003) described a robust parameter designed experiment that utilized a combined array. Engineers in the packaging development department at Procter and Gamble wanted to develop optimum and robust settings for the control variables for a new sealing process so as to hit a target value for the response variable, which was maximum peel strength. This is the maximum amount of strength, measured in pounds of force, which is required to open a package. There were three control factors: temperature, pressure, and speed. The packaging materials were furnished by different suppliers and the engineers did not want to have a single set of manufacturing conditions for each packaging material, so it was important to have settings for the control factors that would be robust to supplier-to-supplier variation. Supplier was the categorical noise variable and there were three suppliers. A 37-run D-efficient design was used with 37 being the maximum number of runs that could be made because of constraints on resources and time. A D-efficient design is (as the name implies) a design that is efficient relative to a D-optimal design. A formal definition of D-efficiency is given on, for example, page 9 of Waterhouse (2005).

The authors did not give the design, nor did they give the number of levels used for each of the control factors. Since 37 is a prime number, it is obvious that the design had to be lacking in balance. That is, for whatever number of levels used (less than 37, presumably), the levels could not occur an equal number of times. The design may have had good properties, however, as it is possible, for example, to construct a design with 37 runs that has two levels for each of the control factors and three levels for the noise factor with very small pairwise correlations, so that the design is near-orthogonal. No other properties of the design were stated, however, but one obvious weakness of the design was that the standard error of the point estimate of the parameters for the terms in the fitted model varied greatly, ranging from 0.021 to 1.344. One problem with such a design is that it is difficult to apply any rules of thumb regarding the magnitude of interactions relative to the magnitude of main effects (as discussed, for example, in Section 4.2), when the standard errors differ by orders of magnitude, as obviously we would strongly prefer that the standard errors be the same in applying any such rules.

8.5 SOFTWARE

Although software that has the capability for Taguchi's orthogonal arrays is plentiful, the designs should be used with caution, if at all. At the very least, the user should know the properties of a chosen design before using it in an experiment.

Unfortunately, Reece's (2003) very comprehensive study of software with experimental design capabilities did not include Taguchi designs, so some guidance will be given in this section.

Release 14 of MINITAB generates Taguchi (orthogonal array) designs, as this is one of the pull-down menu options. Many different designs can be generated: two-level designs for up to 31 factors, three-level designs for up to 13 factors, four-level designs for up to 5 factors, five-level designs for up to 6 factors, plus mixed-level designs. The available mixed-level designs are 2-3, 2-4, 2-8, and 3-6, with, for example, "2-4" referring to a mixture of two-level factors and four-level factors. (Only one 2-8 and one 3-6 are available.) These arrays have 8, 16, 18, 32, 36, and 54 design points.

Design-Expert 7.0 has capabilities for more designs than does MINITAB, with the number of design points for Taguchi orthogonal array designs being 4, 8, 9, 12, 16, 18, 25, 27, 32, 36, 50, 54, and 64. These are for factors with 2, 3, 4, or 5 levels, with seven of the listed designs being mixed-level designs.

Of course, since these are orthogonal arrays, each array could be used with fewer than the maximum number of factors. Thus, there is a moderately large number of possible designs.

The screen display in Design-Expert 7.0 that lists the available designs also states the following.

> Use these designs with caution. Always use the design evaluation to examine aliasing before running any experiments and again when analyzing and interpreting the results.

JMP 5.1 also has the capability for Taguchi designs but unlike MINITAB and Design-Expert there is no list of available designs. Rather, the user simply specifies each factor as a two-level or three-level control factor, or as a noise factor. If a moderate number of control factors are specified, design options are listed. For example, the L_{16}, L_{20}, L_{24}, L_{32}, L_{64}, and L_{128} arrays are listed as available designs for the inner array when 15 two-level control factors and two noise factors are indicated (the noise factors must have two levels), with an L_4 array listed as the only available design for the noise factors in the outer array. If too many control factors are specified, however, such as 38 two-level control factors and two noise factors, the program crashes and an "unknown error" message is displayed.

When three-level control factors are specified, different arrays are listed as possible designs, such as the L_{36} when there are 11 three-level control factors and 2 noise factors and the L_{27} when there are 7 three-level factors and 2 noise factors. For a small number of factors, the options could include a full factorial for the inner array, such as when only 3 three-level control factors are indicated.

Although the options (and there might be only one option) are indicated, once the control factors and noise factors are specified, it would be better to know what is available before the latter is done.

Because of the popularity of Taguchi methods/designs in certain quarters, there are many other software packages that will generate these designs. Among the other statistical software packages studied by Reece (2003), D. o. E. Fusion (previously known as CARD) does not have such capability, however, and allows a maximum of only 10 factors. Thus, the software could not be used for screening with a large number of candidate variables, such as is sometimes the case when Taguchi designs are used.

8.6 FURTHER READING

There is much useful information in the statistical literature on critiques of Taguchi's design methods and superior alternatives, especially in Steinberg (1996), Steinberg and Bursztyn (1994, 1998), and Tsui (1994, 1996a,b, 1998). A review of parameter design was given by Robinson, Borror, and Myers (2003) and application articles and case studies were given by Chen, Allen, Tsui, and Mistree (1996), Czitrom, Mohammadi, Flemming, and Dyas (1998), and Muzammil, Singh, and Talib (2003). An interesting feature of the experiment described by Czitrom et al. (1998) is that control charts were used *after* the experiment but there is no mention of them being used during the experiment. Runs at the standard operating conditions were made at the beginning and the end of the experiment, however, which serves a similar purpose and has been recommended as a control procedure, as discussed in Section 1.7. Control charts were used after the experiment to show the reduction in variability that was achieved.

Also listed in the references are articles on various related topics, such as robust designs with cost considerations (Morehead and Wu, 1998), analysis of certain types of robust design experiments (McCaskey and Tsui, 1999; Miller, 2002), and combined arrays with a minimum number of runs (Evangelaras and Koukouvinos, 2004), and an improved dual response method given by Miro-Quesada and Del Castillo (2004). There have also been standard techniques such as split-plot experiments presented as applicable to robust design and discussed by Kowalski (2002), as was the possible use of generalized linear modeling methods in robust design, as discussed by Engel and Huele (1996) and Lesperance and Park (2003). See also Lawson and Helps (1996), Tsui (1999) and Joseph (2003).

Section 14.9 of Ryan (2000) gives certain detailed information regarding Taguchi methods of design that are covered only generally in this chapter, and also contains a very detailed analysis of a designed experiment that was presented by Lewis, Hutchens, and Smith (1997), which is mentioned in Exercise 8.4.

8.7 SUMMARY

The emphasis in this chapter was on certain important aspects of experimental design as related to making products that are robust to variations in manufacturing conditions (noise factors). Readers interested in S/N ratios and other statistical methods used by G. Taguchi will find information about them in many sources, including Fowlkes and Creveling (1995), but those methods are not advocated here, just as Taguchi's design methods were not advocated in sources such as Bisgaard (1989a). Similarly, Anderson and Kraber (2003) gave an example that illustrated the superiority of two-level fractional factorials over Taguchi designs. See also the critique of Taguchi methods in Ramberg, Pignatiello, and Sanchez (1992).

Taguchi's engineering ideas are quite important, however, and have led researchers and practitioners to focus their attention on noise factors and control × noise interactions.

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EXERCISES

8.1 Show that a product array that results from a 2^2 inner array and a 2^2 outer array is a 2^4 design.

8.2 What are the advantages and disadvantages of a product array relative to a combined array?

8.3 Consider Example 8.1. Steinberg and Bursztyn (1994) discovered that there are two suspicious data points and that the predicted value for one data point is equal to the observed value of the other data point, suggesting that the points may have been accidentally switched. This illustrates the need for a careful analysis, which of course is true for any dataset. Identify the two points, switch them, and reanalyze the data. Does this have a material effect on the results of the analysis? Explain. Assuming the two data points indeed should be switched, what do you conclude and what would you recommend?

8.4 Lewis et al. (1997, references) described an experiment performed by Electro-Scientific-Industries (ESI) that was motivated by the dissatisfaction expressed by a company's customer in Japan. The customer was dissatisfied with the mean-time-between failures (MTBF) performance of one of ESI's products. The company had six months to find a solution to the problem; otherwise, the Japanese company would switch to one of ESI's competitors.

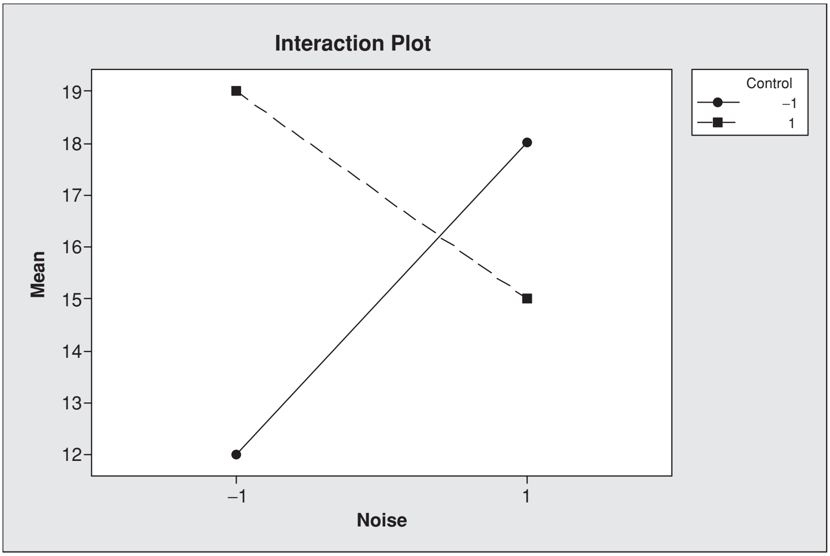
Eight control factors and three noise factors were studied with the inner array for the control factors being a 2^{8-4}_{IV} design and the outer array being a 2^{3-1}_{III} design.

(a) Critique the design that was used. In particular, what was the design in terms of the (γ, λ, α) notation that was used in Section 8.4? Would you have recommended that a different design be used? If so, which design? If not, explain why you believe that the product array used was a good design.

(b) The dataset is rather large since there are 64 design points and 11 factors, so it will not be given here. In addition to Lewis et al. (1997), the dataset can also be found in Ryan (2000). Obtain the data and perform an appropriate analysis. Do you believe that a follow-up experiment is needed to resolve any ambiguities? Why, or why not? What do you conclude from your analysis?

(c) Compare your analysis to that given by Ryan (2000, pp. 455–462) and comment.

8.5 Consider the following control $\times$ noise interaction plot, which is a variation of Figure 8.1. Is this configuration of points helpful in terms of selecting a level of the control factor? Why, or why not?



8.6 Which would we rather see, a control $\times$ noise interaction profile in which both slopes (assuming two levels of the control factor) are close to zero, or a profile in which one slope is close to $+1$ and the other slope is close to -1?

8.7 Critique the following statement: “Why should I include noise factors in an experimental design since they can’t be controlled during production?”

8.8 Consider the caution given by the Design-Expert software that was quoted in Section 8.5 relative to the following orthogonal array, which was produced by Design-Expert. Under what conditions, if any, would you recommend that this design be used? In particular, could this design be used as a combined array to investigate control $\times$ noise interactions. Explain.

1	1	1	1	1	1	1	1	1	1	1
1	1	1	1	1	2	2	2	2	2	2
1	1	2	2	2	1	1	1	2	2	2
1	2	1	2	2	1	2	2	1	1	2
1	2	2	1	2	2	1	2	1	2	1
1	2	2	2	1	2	2	1	2	1	1
2	1	2	2	1	1	2	2	1	2	1
2	1	2	1	2	2	2	1	1	1	2
2	1	1	2	2	2	1	2	2	1	1
2	2	2	1	1	1	1	2	2	1	2
2	2	1	2	1	2	1	1	1	2	2
2	2	1	1	2	1	2	1	2	2	1

8.9 It was shown in Section 8.1 that the L_8 design given in that section is equivalent to a suboptimal fractional factorial. Does the design in Exercise 8.8 have the same weakness? Why, or why not?

8.10 Case 35 in Taguchi et al. (2005, references) is a typical application of an orthogonal array design in the Taguchi literature in which there is no discussion of the properties of the design that is used, which is the $L_{18}(2^2 \times 3^6)$ that can be generated by Design-Expert, for example. (Recall that the notation specifies that the design has 2 two-level factors and 6 three-level factors.) The alias structure for this design is very complex but is produced automatically by Design-Expert. Bearing in mind that we use three-level factors for the purpose of investigating quadrature, use Design-Expert or other software and comment on the alias structure. Specifically, under what conditions would you recommend use of the design?

1	1	1	1	1	1	1	1
1	1	2	2	1	2	2	2
1	1	3	3	2	3	3	3
1	2	1	2	2	2	3	1
1	2	2	3	1	3	1	2
1	2	3	1	3	1	2	3
1	2	1	3	3	2	1	3
1	2	2	1	2	3	2	1
1	2	3	2	1	1	3	2
2	1	1	3	2	1	2	2
2	1	2	1	1	2	3	3
2	1	3	2	3	3	1	1

2	2	1	1	3	3	3	2
2	2	2	2	2	1	1	3
2	2	3	3	1	2	2	1
2	2	1	2	1	3	2	3
2	2	2	3	3	1	3	1
2	2	3	1	2	2	1	2

8.11 One of the problems regarding case studies of Taguchi's methods is that the raw data are generally not given. If the data were given, then readers could perform alternative analyses, if desired, and compare those analyses with the analysis given in the case study. For example, Rowlands, Antony, and Knowles (2000, references) described an application but did not give the raw data, although they did give the ANOVA table for the raw data. This article is available at http://www.emeraldinsight.com/Insight/ViewContentServlet?Filename=Published/EmeraldFullTextArticle/Articles/1060120201.html and at http://www.caledonian.ac.uk/crisspi/downloads/publication6.pdf. Read the article and critique the analysis. Do you believe that the conclusions are justified based on the content of the article?