Chapter 11. Supplemental Text Material

11-1. The Method of Steepest Ascent

The method of steepest ascent can be derived as follows. Suppose that we have fit a first-order model

$$\hat{y} = \hat{\beta}_0 + \sum_{i=1}^{k} \hat{\beta}_i x_i$$

and we wish to use this model to determine a path leading from the center of the design region **x** = **0** that increases the predicted response most quickly. Since the first–order model is an unbounded function, we cannot just find the values of the *x*'s that maximize the predicted response. Suppose that instead we find the *x*'s that maximize the predicted response at a point on a hypersphere of radius *r*. That is

$$\text{Max } \hat{y} = \beta_0 + \sum_{i=1}^{k} \hat{\beta}_i x_i$$

subject to

$$\sum_{i=1}^{k} x_i^2 = r^2$$

The can be formulated as

$$\text{Max } G = \beta_0 + \sum_{i=1}^{k} \hat{\beta}_i x_i - \lambda \left[\sum_{i=1}^{k} x_i^2 - r^2 \right]$$

where λ is a LaGrange multiplier. Taking the derivatives of *G* yields

$$\frac{\partial G}{\partial x_i} = \hat{\beta}_i - 2\lambda x_i \quad i - 1,2,\cdots,k$$

$$\frac{\partial G}{\partial \lambda} = -\left[\sum_{i=1}^{k} x_i^2 - r^2 \right]$$

Equating these derivatives to zero results in

$$x_i = \frac{\hat{\beta}_i}{2\lambda} \quad i = 1,2,\cdots,k$$

$$\sum_{i=1}^{k} x_i^2 = r^2$$

Now the first of these equations shows that the coordinates of the point on the hypersphere are proportional to the signs and magnitudes of the regression coefficients (the quantity 2λ is a constant that just fixes the radius of the hypersphere). The second equation just states that the point satisfies the constraint. Therefore, the heuristic description of the method of steepest ascent can be justified from a more formal perspective.

11-2. The Canonical Form of the Second-Order Response Surface Model

Equation (11-9) presents a very useful result, the **canonical form** of the second-order response surface model. We state that this form of the model is produced as a result of a translation of the original coded variable axes followed by rotation of these axes. It is easy to demonstrate that this is true.

Write the second-order model as

$$\hat{y} = \hat{\beta}_0 + \mathbf{x}'\hat{\beta} + \mathbf{x}'\mathbf{B}\mathbf{x}$$

Now translate the coded design variable axes **x** to a new center, the stationary point, by making the substitution $\mathbf{z} = \mathbf{x} - \mathbf{x}_s$. This translation produces

$$\begin{aligned} \hat{y} &= \hat{\beta}_0 + (\mathbf{z}+\mathbf{x}_s)'\hat{\beta} + (\mathbf{z}+\mathbf{x}_s)'\mathbf{B}(\mathbf{z}+\mathbf{x}_s) \\ &= \left[\hat{\beta}_0 + \mathbf{x}_s'\hat{\beta} + \mathbf{x}_s'\mathbf{B}\mathbf{x}_s\right] + \mathbf{z}'\hat{\beta} + \mathbf{z}'\mathbf{B}\mathbf{z} + 2\mathbf{x}_s'\mathbf{B}\mathbf{z} \\ &= \hat{y}_s + \mathbf{z}'\mathbf{B}\mathbf{z} \end{aligned}$$

because from Equation (11-7) we have $2\mathbf{x}_s'\mathbf{B}\mathbf{z} = -\mathbf{z}'\hat{\beta}$. Now rotate these new axes (**z**) so that they are parallel to the principal axes of the contour system. The new variables are $\mathbf{w} = \mathbf{M}'\mathbf{z}$, where

$$\mathbf{M}'\mathbf{B}\mathbf{M} = \Lambda$$

The diagonal matrix Λ has the eigenvalues of **B**, $\lambda_1, \lambda_2, \cdots, \lambda_k$ on the main diagonal and **M** is a matrix of normalized eigenvectors. Therefore,

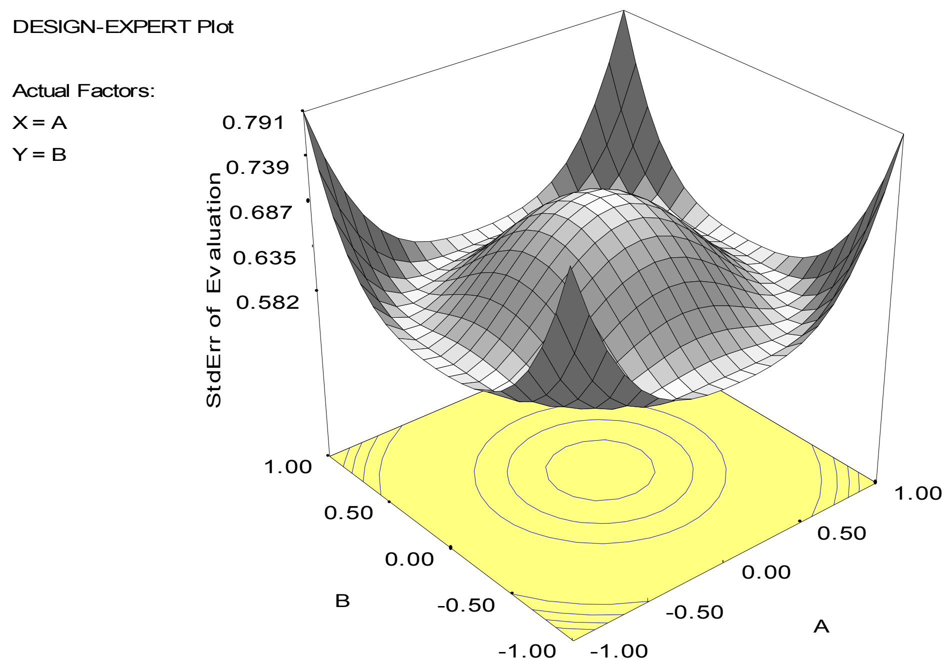
$$\begin{aligned} \hat{y} &= \hat{y}_s + \mathbf{z}'\mathbf{B}\mathbf{z} \\ &= \hat{y}_s + \mathbf{w}'\mathbf{M}'\mathbf{B}\mathbf{M}\mathbf{z} \\ &= \hat{y}_s + \mathbf{w}'\Lambda\mathbf{w} \\ &= \hat{y}_s + \sum_{i=1}^{k} \lambda_i w_i^2 \end{aligned}$$

which is Equation (11-9).

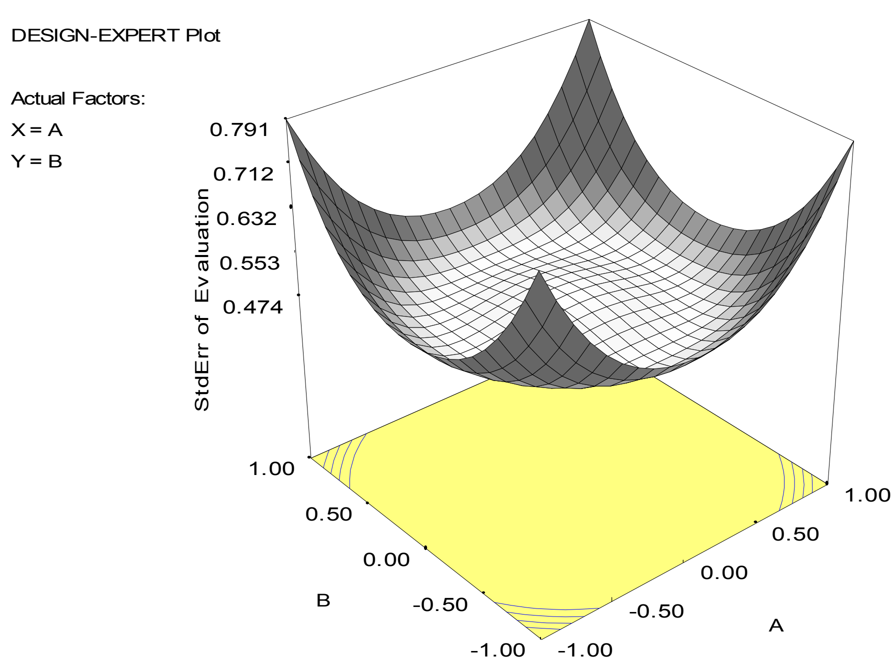
11-3. Center Points in the Central Composite Design

In section 11-4,2 we discuss designs for fitting the second-order model. The CCD is a very important second-order design. We have given some recommendations regarding the number of center runs for the CCD; namely, $3 \le n_c \le 5$ generally gives good results.

The center runs serves to stabilize the prediction variance, making it nearly constant over a broad region near the center of the design space. To illustrate, suppose that we are considering a CCD in $k = 2$ variables but we only plan to run $n_c = 2$ center runs. The following graph of the standardized standard deviation of the predicted response was obtained from Design-Expert:



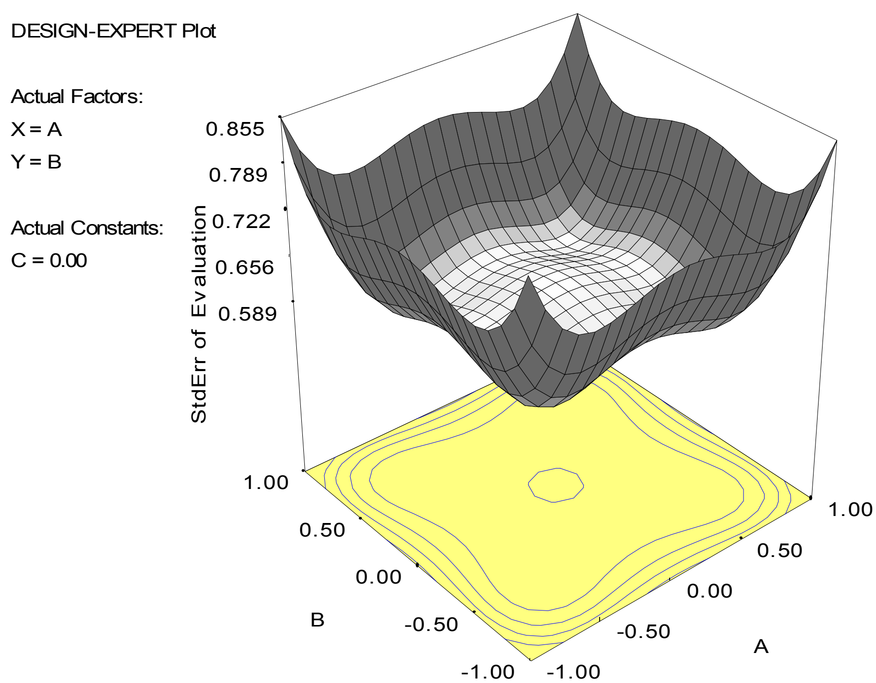
Notice that the plot of the prediction standard deviation has a large "bump" in the center. This indicates that the design will lead to a model that does not predict accurately near the center of the region of exploration, a region likely to be of interest to the experimenter. This is the result of using an insufficient number of center runs. Suppose that the number of center runs is increased to $n_c = 4$. The prediction standard deviation plot now looks like this:



Notice that the addition of two more center runs has resulted in a much flatter (and hence more stable) standard deviation of predicted response over the region of interest.
The CCD is a spherical design. Generally, every design on a sphere must have at least one center point or the $\mathbf{X'X}$ matrix will be singular. However, the number of center points can often influence other properties of the design, such as prediction variance.

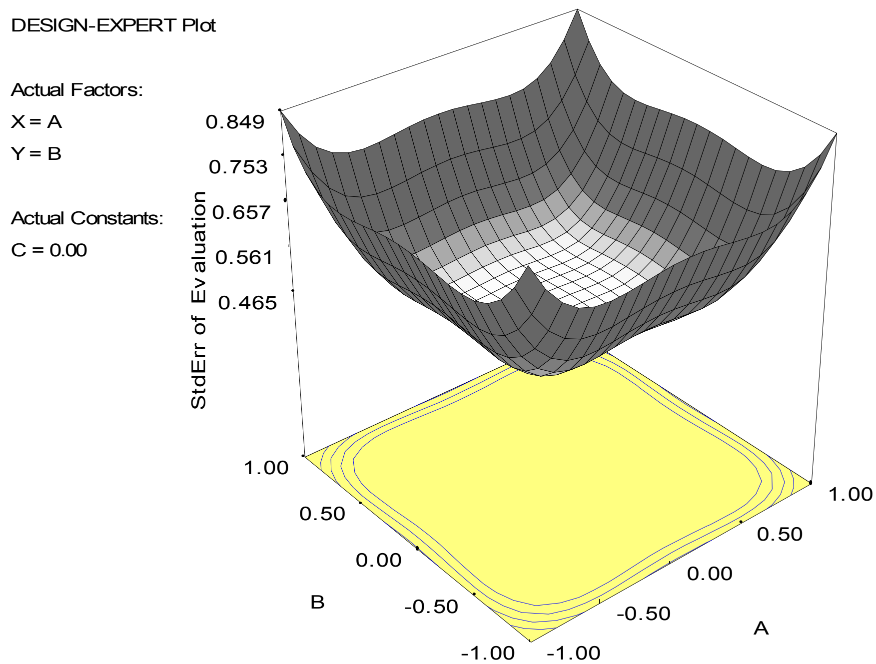
11-4. Center Runs in the Face-Centered Cube

The face-centered cube is a CCD with $\alpha = 1$; consequently, it is a design on a cube, it is not a spherical design. This design can be run with as few as $n_c = 0$ center points. The prediction standard deviation for the case $k = 3$ is shown below:



Notice that despite the absence of center points, the prediction standard deviation is relatively constant in the center of the region of exploration. Note also that the contours of constant prediction standard deviation are not concentric circles, because this is not a rotatable design.

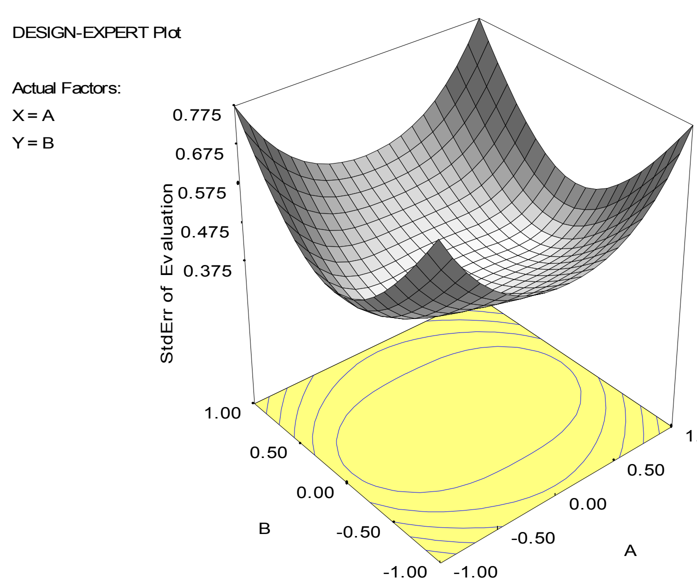
While this design will certainly work with no center points, this is usually not a good choice. Two or three center points generally gives good results. Below is a plot of the prediction standard deviation for a face-centered cube with two center points. This choice work very well.



11-5. A Note on Rotatability

Rotatability is a property of the prediction variance in a response surface design. If a design is rotatable, the prediction variance is constant at all points that are equidistant from the center of the design.

What is not widely known is that rotatability depends on both the design and the model. For example, if we have run a rotatable CCD and fit a **reduced** second-order model, the variance contours are no longer spherical. To illustrate, below we show the standardized standard deviation of prediction for a rotatable CCD with $k = 2$, but we have fit a reduced quadratic (one of the pure quadratic terms is missing).



Notice that the contours of prediction standard deviation are not circular, even though a rotatable design was used.

11-6. The Taguchi Approach to Robust Parameter Design

Throughout this book, we have emphasized the importance of using designed experiments for product and process improvement. Today, many engineers and scientists are exposed to the principles of statistically designed experiments as part of their formal technical education. However, during the 1960-1980 time period, the principles of experimental design (and statistical methods, in general) were not as widely used as they are today

In the early 1980s, Genichi Taguchi, a Japanese engineer, introduced his approach to using experimental design for

1. Designing products or processes so that they are robust to environmental conditions.

2. Designing/developing products so that they are robust to component variation.

3. Minimizing variation around a target value.

Note that these are essentially the same objectives we discussed in Section 11-7.1.

Taguchi has certainly defined meaningful engineering problems and the philosophy that recommends is sound. However, as noted in the textbook, he advocated some novel methods of statistical data analysis and some approaches to the design of experiments that the process of peer review revealed were unnecessarily complicated, inefficient, and sometimes ineffective. In this section, we will briefly overview Taguchi's philosophy regarding quality engineering and experimental design. We will present some examples of his approach to parameter design, and we will use these examples to highlight the problems with his technical methods. As we saw in the Section 11-7.2 of the textbook, it is possible to combine his sound engineering concepts with more efficient and effective experimental design and analysis based on response surface methods.

11-6.1 The Taguchi Philosophy

Taguchi advocates a philosophy of quality engineering that is broadly applicable. He considers three stages in product (or process) development: system design, parameter design, and tolerance design. In **system design,** the engineer uses scientific and engineering principles to determine the basic system configuration. For example, if we wish to measure an unknown resistance, we may use our knowledge of electrical circuits to determine that the basic system should be configured as a Wheatstone bridge. If we are designing a process to assemble printed circuit boards, we will determine the need for specific types of axial insertion machines, surface-mount placement machines, flow solder machines, and so forth.

In the **parameter design** stage, the specific values for the system parameters are determined. This would involve choosing the nominal resistor and power supply values for the Wheatstone bridge, the number and type of component placement machines for the printed circuit board assembly process, and so forth. Usually, the objective is to specify these nominal parameter values such that the variability transmitted from uncontrollable or noise variables is minimized.

Tolerance design is used to determine the best tolerances for the parameters. For example, in the Wheatstone bridge, tolerance design methods would reveal which components in the design were most sensitive and where the tolerances should be set. If a component does not have much effect on the performance of the circuit, it can be specified with a wide tolerance.

Taguchi recommends that statistical experimental design methods be employed to assist in this process, particularly during parameter design and tolerance design. We will focus on parameter design. Experimental design methods can be used to find a best product or process design, where by "best" we mean a product or process that is robust or insensitive to uncontrollable factors **that will** influence the product or process once it is in routine operation.

The notion of **robust design** is not new. Engineers have always tried to design products so that they will work well under uncontrollable conditions. For example, commercial transport aircraft fly about as well in a thunderstorm as they do in clear air. Taguchi deserves recognition for realizing that experimental design can be used as a formal part of the **engineering design process** to help accomplish this objective.

A key component of Taguchi's philosophy is the **reduction of variability.** Generally, each product or process performance characteristic will have a target or **nominal** value. The objective is to reduce the variability around this target value. Taguchi models the departures that may occur from this target value with a **loss function.** The loss refers to the cost that is incurred by *society* when the consumer uses a product whose quality characteristics differ from the nominal. The concept of societal loss is a departure from traditional thinking. Taguchi imposes a quadratic loss function of the form

$$L(y) = k(y- T)^2$$

shown in Figure 1 below. Clearly this type of function will penalize even small departures of y from the target T. Again, this is a departure from traditional thinking, which usually attaches penalties only to cases where y is outside of the upper and lower specifications (say $y > USL$ or $y < LSL$ in Figure 1. However, the Taguchi philosophy regarding reduction of variability and the emphasis on minimizing costs is entirely consistent with the continuous improvement philosophy of Deming and Juran.

In summary, Taguchi's philosophy involves three central ideas:

1. Products and processes should be designed so that they are robust to external sources of variability.
2. Experimental design methods are an engineering tool to help accomplish this objective.
3. Operation on-target is more important than conformance to specifications.

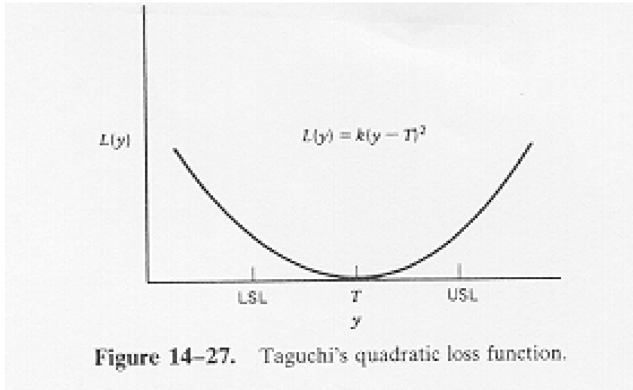


Figure 1. Taguchi's Quadratic Loss Function

These are sound concepts, and their value should be readily apparent. Furthermore, as we have seen in the textbook, experimental design methods can play a major role in translating these ideas into practice.

We now turn to a discussion of the specific methods that Professor Taguchi recommends for applying his concepts in practice. As we will see, his approach to experimental design and data analysis can be improved.

11-6.2 Taguchi's Technical Methods

<u>An Example</u>

We will use the connector pull-off force example described in the textbook to illustrate Taguchi'a technical methods. For more information about the problem, refer to the text and to the original article in *Quality Progress* in December 1987 (see "The Taguchi Approach to Parameter Design," by D. M. Byrne and S. Taguchi, *Quality Progress,* December 1987, pp. 19-26). Recall that the experiment involves finding a method to assemble an elastomeric connector to a nylon tube that would deliver the required pull-off performance to be suitable for use in an automotive engine application. The specific objective of the experiment is to maximize the pull-off force. Four controllable and three uncontrollable noise factors were identified. These factors are defined in the textbook, and repeated for convenience in Table 1 below. We want to find the levels of the controllable factors that are the least influenced by the noise factors and that provides the maximum pull-off force. Notice that although the noise factors are not controllable during routine operations, they can be controlled for the purposes of a test. Each controllable factor is tested at three levels, and each noise factor is tested at two levels.

Recall from the discussion in the textbook that in the Taguchi parameter design methodology, one experimental design is selected for the controllable factors and another experimental design is selected for the noise factors. These designs are shown in Table 2. Taguchi refers to these designs as **orthogonal arrays**, and represents the factor levels with integers 1, 2, and 3. In this case the designs selected are just a standard 2^3 and a 3^{4-2} fractional factorial. Taguchi calls these the L_8 and L_9 orthogonal arrays, respectively.

Table 1. Factors and Levels for the Taguchi Parameter Design Example

Controllable Factors			Levels	
A =	Interference	Low	Medium	High
B =	Connector wall thickness	Thin	Medium	Thick
C =	Insertion,depth	Shallow	Medium	Deep
D =	Percent adhesive in connector pre-dip	Low	Medium	High

Uncontrollable Factors	Levels	
E = Conditioning time	24 h	120 h
F = Conditioning temperature	72°F	150°F
G = Conditioning relative humidity	25%	75%

Table 2. Designs for the Controllable and Uncontrollable Factors

(a) L_9 Orthogonal Array for the Controllable Factors — (b) L_8 Orthogonal Array for the Uncontrollable Factors

Variable						Variable					
Run	A	*B*	*C*	*D*	Run	*E*	*F*	*E X F*	*G*	*Ex G*	
Fx G	*e*										
11	1	1	1	1	1	1	1	1	1	1	1
21	2	2	2	2	1	1	1	2	2	2	2
31	3	3	3	3	1	2	2	1	1	2	2
42	1	2	3	4	1	2	2	2	2	1	1
52	2	3	1	5	2	1	2	1	2	1	2
62	3	1	2	6	2	1	2	2	1	2	1
73	1	3	2	7	2	2	1	1	2	2	1
83	2	1	3	8	2	2	1	2	1	1	2
93	3	2	1								

The two designs are combined as shown in Table 11-22 in the textbook, repeated for convenience as Table 3 below. Recall that this is called a **crossed** or **product array design**, composed of the **inner array** containing the controllable factors**,** and the **outer array** containing the noise factors**.** Literally, each of the 9 runs from the inner array is tested across the 8 runs from the outer array, for a total sample size of 72 runs. The observed pull-off force is reported in Table 3.

Data Analysis and Conclusions

The data from this experiment may now be analyzed. Recall from the discussion in Chapter 11 that Taguchi recommends analyzing the mean response for each run in the inner array (see Table 3), and he also suggests analyzing variation using an appropriately chosen **signal-to-noise ratio (SN).** These signal-to-noise ratios are derived from the quadratic loss function, and three of them are considered to be "standard" and widely applicable. They are defined as follows:

1. Nominal the best:

$$SN_T = 10\log\left(\frac{\bar{y}^2}{S^2}\right)$$

2. Larger the better:

$$SN_L = -10\log\left(\frac{1}{n}\sum_{i=1}^{n}\frac{1}{y_i^2}\right)$$

Table 3. Parameter Design with Both Inner and Outer Arrays

	Outer Array (L_8)						
E	1	1	1	2	2	2	2
F	1	2	2	1	1	2	2
G	2	1	2	1	2	1	2

	Inner Array (L_9)												Responses	
Run	A	B	C	D									$\bar{y}$	SN_L
1	1	1	1	1	15.6	9.5	16.9	19.9	19.6	19.6	20.0	19.1	17.525	24.025
2	1	2	2	2	15.0	16.2	19.4	19.2	19.7	19.8	24.2	21.9	19.475	25.522
3	1	3	3	3	16.3	16.7	19.1	15.6	22.6	18.2	23.3	20.4	19.025	25.335
4	2	1	2	3	18.3	17.4	18.9	18.6	21.0	18.9	23.2	24.7	20.125	25.904
5	2	2	3	1	19.7	18.6	19.4	25.1	25.6	21.4	27.5	25.3	22.825	26.908
6	2	3	1	2	16.2	16.3	20.0	19.8	14.7	19.6	22.5	24.7	19.225	25.326
7	3	1	3	2	16.4	19.1	18.4	23.6	16.8	18.6	24.3	21.6	19.8	25.711
8	3	2	t	3	14.2	15.6	15.1	16.8	17.8	19.6	23.2	24.2	18.338	24.852
9	3	3	2	1	16.1	19.9	19.3	17.3	23.1	22.7	22.6	28.6	21.200	26.152

3. Smaller the better:

$$SN_L = -10\log\left(\frac{1}{n}\sum_{i=1}^{n} y_i^2\right)$$

Notice that these *SN* ratios are expressed on a decibel scale. We would use SN_T if the objective is to reduce variability around a specific target, SN_L if the system is optimized when the response is as large as possible, and SN_S if the system is optimized when the response is as small as possible. Factor levels that maximize the appropriate *SN* ratio are optimal.

In this problem, we would use SN_L because the objective is to maximize the pull-off force. The last *two* columns of Table 3 contain $\bar{y}$ and SN_L values for each of the nine inner-array runs. Taguchi-oriented practitioners often use the analysis of variance to determine the factors that influence $\bar{y}$ and the factors that influence the signal-to-noise ratio. They also employ graphs of the "marginal means" of each factor, such as the ones shown in Figures 2 and 3. The usual approach is to examine the graphs and "pick the winner." In this case, factors A and C have larger effects than do B and D. In terms of maximizing SN_L we would select A_{Medium}, C_{Deep}, B_{Medium}, and D_{Low}. In terms of maximizing the average pull-off force $\bar{y}$, we would choose A_{Medium}, C_{Medium}, B_{Medium} and $D_{Low.}$ Notice that there is almost no difference between C_{Medium} and C_{Deep}. The implication is that this choice of levels will maximize the mean pull-off force and reduce variability in the pull-off force.

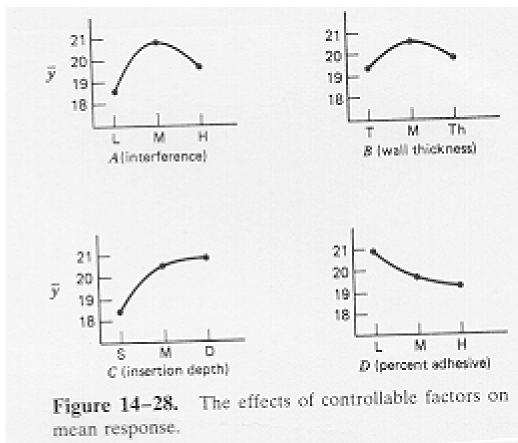


Figure 2. The Effects of Controllable Factors on Each Response

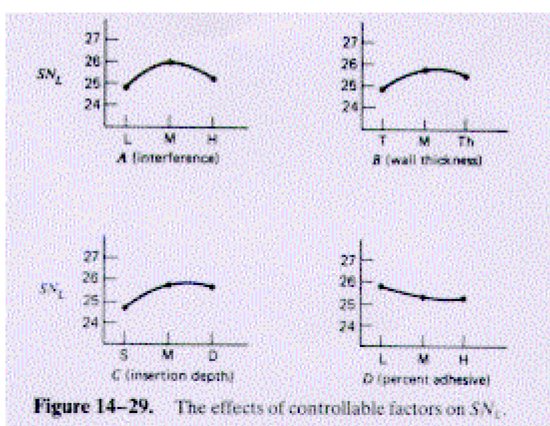


Figure 3. The Effects of Controllable Factors on the Signal to Noise Ratio

Taguchi advocates claim that the use of the *SN* ratio generally eliminates the need for examining specific interactions between the controllable and noise factors, although sometimes looking at these interactions improves process understanding. The authors of this study found that the *AG* and *DE* interactions were large. Analysis of these interactions, shown in Figure 4, suggests that A_{Medium} is best. (It gives the highest pull-off force and a slope close to zero, indicating that if we choose A_{Medium} the effect of relative humidity is minimized.) The analysis also suggests that D_{Low} gives the highest pull-off force regardless of the conditioning time.

When cost and other factors were taken into account, the experimenters in this example finally decided to use A_{Medium}, B_{Thin}, C_{Medium}**,** and $D_{low.}$ (B_{Thin} was much less expensive than B_{Medium}, and C_{Medium} was felt to give slightly less variability than C_{Deep}.) Since this combination was not a run in the original nine inner array trials, five additional tests were made at this set of conditions as a confirmation experiment**.** For this confirmation experiment, the levels used on the noise variables were E_{Low}, F_{Low}, and G_{Low}. The authors report that good results were obtained from the confirmation test.

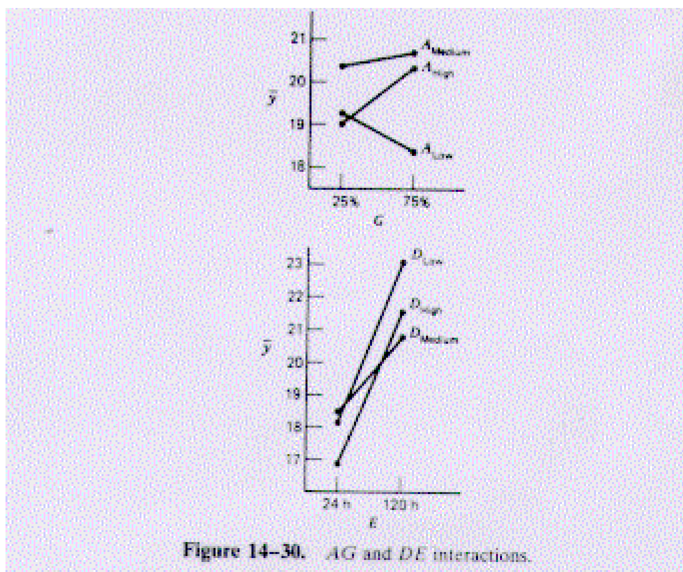


Figure 4. The *AG* and *DE* Interactions

Critique of Taguchi's Experimental Strategy and Designs

The advocates of Taguchi's approach to parameter design utilize the orthogonal array designs, two of which (the L_8 and the L_9) were presented in the foregoing example. There are other orthogonal arrays: the L_4, L_{12}, L_{16}, L_{18}, and L_{27}. These designs were not developed by Taguchi; for example, the L_8 is a 2_{III}^{7-4} fractional factorial, the L_9 is a 3_{III}^{4-2} fractional factorial, the L_{12} is a Plackett-Burman design, the L_{16} is a 2_{III}^{15-11} fractional factorial, and so on. Box, Bisgaard, and Fung (1988) trace the origin of these designs. As we know from Chapters 8 and 9 of the textbook, some of these designs have very complex alias structures. In particular, the L_{12} and all of the designs that use three-level factors will involve **partial aliasing** of two-factor interactions with main effects. If any two-factor interactions are large, this may lead to a situation in which the experimenter does not get the correct answer.

Taguchi argues that we do not need to consider two-factor interactions explicitly. He claims that it is possible to eliminate these interactions either by correctly specifying the response and design factors or by using a **sliding setting approach** to choose factor levels. As an example of the latter approach, consider the two factors pressure and temperature. Varying these factors independently will probably produce an interaction. However, if temperature levels are chosen contingent on the pressure levels, then the interaction effect can be minimized. In practice, these two approaches are usually difficult to implement unless we have an unusually high level of process knowledge. The lack of provision for adequately dealing with potential interactions between the controllable process factors is a major weakness of the Taguchi approach to parameter design.

Instead of designing the experiment to investigate potential interactions, Taguchi prefers to use three-level factors to estimate curvature. For example, in the inner and outer array design used by Byrne and Taguchi, all four controllable factors were run at three levels. Let x_1, x_2, x_3 and x_4 represent the controllable factors and let z_1, z_2, and z_3 represent the three noise factors. Recall that the noise factors were run at two levels in a complete factorial design. The design they used allows us to fit the following model:

$$y = \beta_0 + \sum_{j=1}^{4} \beta_j x_j + \sum_{j=1}^{4} \beta_{jj} x^2{}_j + \sum_{j=1}^{3} \gamma_j z_j + \sum_{i<j} \sum_{j=2}^{3} \gamma_{ij} z_i z_j + \sum_{i=1}^{3} \sum_{j=1}^{4} \delta_{ij} z_i x_j + \varepsilon$$

Notice that we can fit the linear and quadratic effects of the controllable factors but not their two-factor interactions (which are aliased with the main effects). We can also fit the linear effects of the noise factors and all the two-factor interactions involving the noise factors. Finally, we can fit the two-factor interactions involving the controllable factors and the noise factors. It may be unwise to ignore potential interactions in the controllable factors.

This is a rather odd strategy, since **interaction is a form of curvature**. A much safer strategy is to identify potential effects and interactions that may be important and then consider curvature only in the important variables if there is evidence that the curvature is important. This will usually lead to fewer experiments, simpler interpretation of the data, and better overall process understanding.

Another criticism of the Taguchi approach to parameter design is that the crossed array structure usually leads to a very large experiment. For example, in the foregoing application, the authors used 72 tests to investigate only seven factors, and they still could not estimate any of the two-factor interactions among the four controllable factors.

There are several alternative experimental designs that would be superior to the inner and outer method used in this example. Suppose that we run all seven factors at two levels in the combined **array design** approach discussed on the textbook. Consider the 2^{7-2}_{IV} fractional factorial design. The alias relationships for this design are shown in the top half of Table 4. Notice that this design requires only 32 runs (as compared to 72). In the bottom half of Table 4, two different possible schemes for assigning process controllable variables and noise variables to the letters A through G are given. The first assignment scheme allows all the interactions between controllable factors and noise factors to be estimated, and it allows main effect estimates to be made that are clear of two-factor interactions. The second assignment scheme allows all the controllable factor main effects and their two-factor interactions to be estimated; it allows all noise factor main effects to be estimated clear of two-factor interactions; and it aliases only three interactions between controllable factors and noise factors with a two-factor interaction between two noise factors. Both of these arrangements present much cleaner alias relationships than are obtained from the inner and outer array parameter design, which also required over twice as many runs.

In general, the crossed array approach is often unnecessary. A better strategy is to use the **combined array design discussed in the textbook.** This approach will almost always lead to a dramatic reduction in the size of the experiment, and at the same time, it will produce information that is more likely to improve process understanding. For more discussion of this approach, see Myers and Montgomery (1995) and Example 11-6 in the textbook. We can also use a combined array design that allows the experimenter to directly model the noise factors as a complete quadratic and to fit all interactions between the controllable factors and the noise factors, as demonstrated in the textbook in Example 11-7.

Table 4. An Alternative Parameter Design

A one-quarter fraction of 7 factors in 32 runs. Resolution IV.
I = ABCDF = ABDEG = CEFG.

Aliases:

A	*AF = BCD*	*CG = EF*
B	*AG = BDE*	*DE = ABG*
C = EFG	*BC = ADF*	*DF = ABC*
D	*BD = ACF = AEG*	*DG = ABE*
E = CFG	*BE = ADG*	*ACE = AFG*
F = CEG	*BF = ACD*	*ACG = AEF*
G = CEF	*BG = ADE*	*BCE = BFG*
AB = CDF = DEG	*CD = ABF*	*BCG = BEF*
AC = BDF	*CE = FG*	*CDE = DFG*
AD = BCF = BEG	*CF = ABD = EG*	*CDG = DEF*
AF = BDG		

Factor Assignment Schemes:

1. Controllable factors are assigned to the letters *C, E, F,* and G. Noise factors are assigned to the letters *A, B,* and D. All interactions between controllable factors and noise factors can be estimated. and all controllable factor main effects can be estimated clear of two-factor interactions.
2. Controllable factors are assigned to the letters *A, B, C, and* D. Noise factors are assigned to the letters *E, F.* and G. All controllable factor main effects and two-factor interactions can be estimated; only the *CE, CF,* and CG interactions are aliased with interactions of the noise factors.

Another possible issue with the Taguchi inner and outer array design relates to the order in which the runs are performed. Now we know that for experimental validity, the runs in a designed experiment should be conducted in **random order**. However, in many crossed array experiments, it is possible that the run order wasn't randomized. In some cases it would be more convenient to fix each row in the inner array (that is, set the levels of the controllable factors) and run all outer-array trials. In other cases, it might be more convenient to fix the each column in the outer array and the run each on the inner array trials at that combination of noise factors. Exactly which strategy is pursued probably depends on which group of factors is easiest to change, the controllable factors or the noise factors. If the tests are run in either manner described above, then a **split-plot structure** has been introduced into the experiment. If this is not accounted for in the analysis, then the results and conclusions can be misleading. There is no evidence that Taguchi advocates used split-plot analysis methods. Furthermore, since Taguchi frequently downplayed the importance of randomization, it is highly likely that many actual inner and outer array experiments were inadvertently conducted as split-plots, and perhaps incorrectly analyzed. We introduce the split-plot design in Chapter in Chapter 13. A good reference on split-plots in robust design problems is Box and Jones (1992).

A final aspect of Taguchi's parameter design is the use of **linear graphs** to assign factors to the columns of the orthogonal array. A set of linear graphs for the L_8 design is shown in Figure 5. In these graphs, each number represents a column in the design. A line segment on the graph corresponds to an interaction between the nodes it connects. To assign variables to columns in an orthogonal array, assign the variables to nodes first; then when the nodes are used up, assign the variables to the line segments. When you assign variables to the nodes, strike out any line segments that correspond to interactions that might be important. The linear graphs in Figure 5 imply that column 3 in the L_8 design contains the interaction between columns 1 and 2, column 5 contains the interaction between columns 1 and 4, and so forth. If we had four factors, we would assign them to columns 1, 2, 4, and 7. This would ensure that each main effect is clear of two-factor interactions. What is *not* clear is the two-factor interaction aliasing. If the main effects are in columns 1, 2, 4, and 7, then column 3 contains the 1-2 *and* the 4-7 interaction, column 5 contains the 1-4 *and* the 2-7 interaction, and column 6 contains the 1-7 *and* the 2-4 interaction. This is clearly the case because four variables in eight runs is a resolution IV plan with all pairs of two-factor interactions aliased. In order to understand fully the two-factor interaction aliasing, Taguchi would refer the experiment designer to a supplementary interaction table.

Taguchi (1986) gives a collection of linear graphs for each of his recommended orthogonal array designs. These linear graphs seem -to have been developed heuristically. Unfortunately, their use can lead to inefficient designs. For examples, see his car engine experiment [Taguchi and Wu (1980)] and his cutting tool experiment [Taguchi (1986)]. Both of these are 16-run designs that he sets up as resolution III designs in which main effects are aliased with two-factor interactions. Conventional methods for constructing these designs would have resulted in resolution IV plans in which the main effects are clear of the two-factor interactions. For the experimenter who simply wants to generate a good design, the linear graph approach may not produce the best result. A better approach is to use a simple table that presents the design and its full alias structure such as in Appendix Table XII. These tables are easy to construct and are routinely displayed by several widely available and inexpensive computer programs.

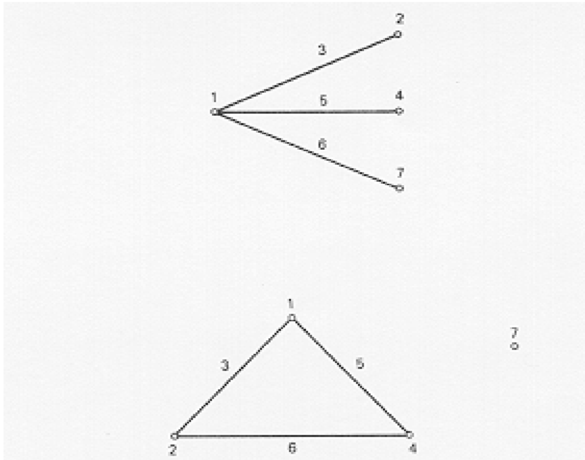


Figure 14–32. Linear graphs for the L_8 design.

Figure 5. Linear Graphs for the L_8 Design

Critique of Taguchi's Data Analysis Methods

Several of Taguchi's data analysis methods are questionable. For example, he recommends some variations of the analysis of variance that are known to produce spurious results, and he also proposes some unique methods for the analysis of attribute and life testing data. For a discussion and critique of these methods, refer to Box, Bisgaard, and Fung (1988), Myers and Montgomery (1995), and the references contained therein. In this section we focus on three aspects of his recommendations concerning data analysis: the use of "marginal means" plots to optimize factor settings, the use of signal-to-noise ratios, and some of his uses of the analysis of variance.

Consider the use of "marginal means" plots and the associated "pick the winner" optimization that was demonstrated previously in the pull-off force problem. To keep the situation simple, suppose that we have two factors *A* and *B*, each at three levels, as shown in Table 5. The "marginal means" plots are shown in Figure 6. From looking at these graphs, we would select A_3 and B_1, as the optimum combination, assuming that we wish to maximize y. However, this is the wrong answer. Direct inspection of Table 5 or the *AB* interaction plot in Figure 7 shows that the combination of A_3 and B_2 produces the maximum value of y. In general, playing "pick the winner" with marginal averages can never be guaranteed to produce the optimum. The Taguchi advocates recommend that a confirmation experiment be run, although this offers no guarantees either. We might be confirming a response that differs dramatically from the optimum. The best way to find a set of optimum conditions is with the use of response surface methods, as discussed and illustrated in Chapter 11 of the textbook.

Taguchi's signal-to-noise ratios are his recommended performance measures in a wide variety of situations. By maximizing the appropriate *SN* ratio, he claims that variability is minimized.

Table 5. Data for the "Marginal Means" Plots in Figure 6

		Factor A			
Factor B		1	2	3	B Averages
	1	10	10	13	11.00
	2	8	10	14	9.67
	3	6	9	10	8.33
	A Averages	8.00	9.67	11.67	

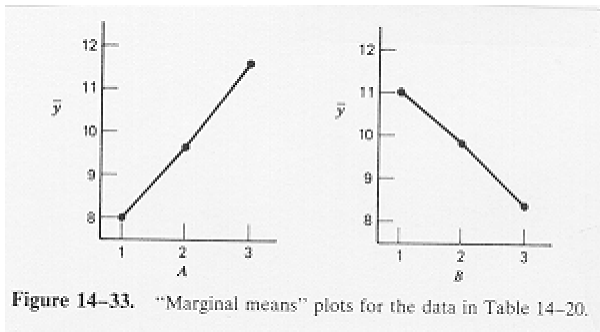


Figure 6. Marginal Means Plots for the Data in Table 5

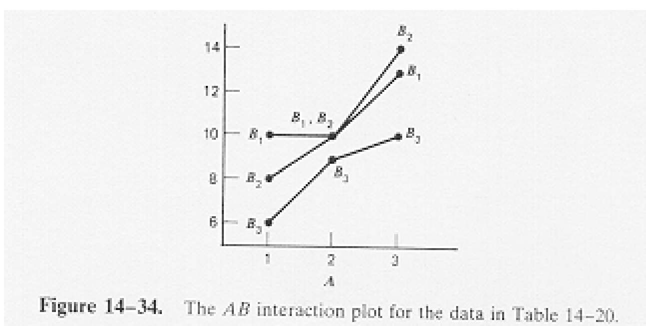


Figure 7. The *AB* Interaction Plot for the Data in Table 5.

Consider first the signal to noise ratio for the target is best case

$$SN_T = 10\log\left(\frac{\bar{y}^2}{S^2}\right)$$

This ratio would be used if we wish to minimize variability around a fixed target value. It has been suggested by Taguchi that it is preferable to work with SN_T instead of the standard deviation because in many cases the process mean and standard deviation are related. (As μ gets larger, σ gets larger, for example.) In such cases, he argues that we cannot directly minimize the standard deviation and then bring the mean on target.

Taguchi claims he found empirically that the use of the SN_T ratio coupled with a two-stage optimization procedure would lead to a combination of factor levels where the standard deviation is minimized and the mean is on target. The optimization procedure consists of (1) finding the **set** of controllable factors that affect SN_T, called the **control factors,** and setting them to levels that maximize SN_T and then (2) finding the set of factors that have significant effects on the mean but do not influence the SN_T ratio, called the **signal factors,** and using these factors to bring the mean on target.

Given that this partitioning of factors is possible, SN_T is an example of a **performance measure independent of adjustment** (PERMIA) [see Leon et al. (1987)]. The signal factors would be the **adjustment factors**. The motivation behind the signal-to-noise ratio is to uncouple location and dispersion effects. It can be shown that the use of SN_T is equivalent to an analysis of the standard deviation of the logarithm of the original data. Thus, using SN_T implies that a log transformation will *always* uncouple location and dispersion effects. There is no assurance that this will happen. A much safer approach is to investigate what type of transformation is appropriate.

Note that we can write the SN_T ratio as

$$SN_T = 10\log\left(\frac{\bar{y}^2}{S^2}\right)$$

$$= 10\log(\bar{y}^2) - 10\log(S^2)$$

If the mean is fixed at a target value (estimated by $\bar{y}$), then maximizing the SN_T ratio is equivalent to minimizing log (S^2). Using log (S^2) would require fewer calculations, is more intuitively appealing, and would provide a clearer understanding of the factor relationships that influence process variability - in other words, it would provide better process understanding. Furthermore, if we minimize log (S^2) directly, we eliminate the risk of obtaining wrong answers from the maximization of SN_T if some of the manipulated factors drive the mean $\bar{y}$ upward instead of driving S^2 downward. In general, if the response variable can be expressed in terms of the model

$$y = \mu(x_d, x_a)\varepsilon(x_d)$$

where x_d is the subset of factors that drive the dispersion effects and x_a is the subset of adjustment factors that do not affect variability, then maximizing SN_T will be equivalent to minimizing the standard deviation. Considering the other potential problems surrounding SN_T , it is likely to be safer to work directly with the standard deviation (or its logarithm) as a response variable, as suggested in the textbook. For more discussion, refer to Myers and Montgomery (1995).

The ratios SN_L and SN_S are even more troublesome. These quantities may be completely ineffective in identifying dispersion effects, although they may serve to identify **location effects,** that is, factors that drive the mean. The reason for this is relatively easy to see. Consider the SN_S (smaller-the-better) ratio:

$$SNS = -10\log\left(\frac{1}{n}\sum_{i=1}^{n} y_i^{\,2}\right)$$

The ratio is motivated by the assumption of a quadratic loss function with *y* nonnegative. The loss function for such a case would be

$$L = C\frac{1}{n}\sum_{i=1}^{n} y_i^{\,2}$$

where *C* is a constant. Now

$$\log L = \log C + \log\left(\frac{1}{n}\sum_{i=1}^{n} y_i^{\,2}\right)$$

and

$$SN_S = 10 \log C - 10 \log L$$

so maximizing *SNs* will minimize *L*. However, it is easy to show that

$$\frac{1}{n}\sum_{i=1}^{n} y_i^{\,2} = \bar{y}^2 + \frac{1}{n}\left(\sum_{i=1}^{n} y_i^{\,2} - n\bar{y}^2\right)$$

$$= \bar{y}^2 + \left(\frac{n-1}{n}\right)S^2$$

Therefore, the use of SN_S as a response variable confounds location and dispersion effects.

The confounding of location and dispersion effects was observed in the analysis of the SN_L ratio in the pull-off force example. In Figures 3 and 3 notice that the plots of $\bar{y}$ and SN_L versus each factor have approximately the same shape, implying that both responses measure location. Furthermore, since the SN_S and SN_L ratios involve y^2 and $1/y^2$, they will be very sensitive to outliers or values near zero, and they are not invariant to linear transformation of the original response. We strongly recommend that these signal-to-noise ratios not be used.

A better approach for isolating location and dispersion effects is to develop separate response surface models for $\overline{y}$ and $\log(S^2)$. If no replication is available to estimate variability at each run in the design, methods for analyzing residuals can be used. Another very effective approach is based on the use of the **response model**, as demonstrated in the textbook and in Myers and Montgomery (1995). Recall that this allows both a response surface for the variance and a response surface for the mean to be obtained for a single model containing both the controllable design factors and the noise variables. Then standard response surface methods can be used to optimize the mean and variance.

Finally, we turn to some of the applications of the analysis of variance recommended by Taguchi. As an example for discussion, consider the experiment reported by Quinlan (1985) at a symposium on Taguchi methods sponsored by the American Supplier Institute. The experiment concerned the quality improvement of speedometer cables. Specifically, the objective was to reduce the shrinkage in the plastic casing material. (Excessive shrinkage causes the cables to be noisy.) The experiment used an L_{16} orthogonal array (the 2_{III}^{15-11} design). The shrinkage values for four samples taken from 3000-foot lengths of the product manufactured at each set of test conditions were measured and the responses $\overline{y}$ and SN_{Sheila} computed.

Quinlan, following the Taguchi approach to data analysis, used SN_S as the response variable in an analysis of variance. The error mean square was formed by pooling the mean squares associated with the seven effects that had the smallest absolute magnitude. This resulted in all eight remaining factors having significant effects (in order of magnitude: *E, G, K, A, C, F, D, H).* The author did note that *E* and *G* were the most important.

Pooling of mean squares as in this example is a procedure that has long been known to produce considerable bias in the ANOVA test results, To illustrate the problem, consider the 15 NID(0, 1) random numbers shown in column 1 of Table 6. The square of each of these numbers, shown in column 2 of the table, is a single-degree-of-freedom mean square corresponding to the observed random number. The seven smallest random numbers are marked with an asterisk in column 1 of Table 6. The corresponding mean squares are pooled to form a mean square for error with seven degrees of freedom. This quantity is

$$MS_E = \frac{0.5088}{7} = 0.0727$$

Finally, column 3 of Table 6 presents the *F* ratio formed by dividing each of the eight remaining mean squares by MS_E. Now $F_{0.05,1,7} = 5.59$, and this implies that five of the eight effects would be judged significant at the 0.05 level. Recall that since the original data came from a normal distribution with mean zero, *none* of the effects is different from zero.

Analysis methods such as this virtually guarantee erroneous conclusions. The normal probability plotting of effects avoids this invalid pooling of mean squares and provides a simple, easy to interpret method of analysis. Box (1988) provides an alternate analysis of

Table 6. Pooling of Mean Squares

NID(0,1) Random Numbers	Mean Squares with One Degree of Freedom	F_0
-08607	0.7408	10.19
-0.8820	0.7779	10.70
0.3608*	0.1302	
0.0227*	0.0005	
0.1903*	0.0362	
-0.3071*	0.0943	
1.2075	1.4581	20.06
0.5641	0.3182	4038
-0.3936*	0.1549	
-0.6940	0.4816	6.63
-0.3028*	0.0917	
0.5832	0.3401	4.68
0.0324*	0.0010	
1.0202	1.0408	14.32
-0.6347	0.4028	5.54

the Quinlan data that correctly reveals *E* and *G* to be important along with other interesting results not apparent in the original analysis.

It is important to note that the Taguchi analysis identified negligible factors as significant. This can have profound impact on our use of experimental design to enhance process knowledge. Experimental design methods should make gaining process knowledge easier, not harder.

Some Final Remarks

In this section we have directed some major criticisms toward the specific methods of experimental design and data analysis used in the Taguchi approach to parameter design. Remember that these comments have focused on technical issues, and that the broad **philosophy** recommended by Taguchi is inherently sound.

On the other hand, while the "Taguchi controversy" was in full bloom, many companies reported success with the use of Taguchi's parameter design methods. If the methods are flawed, why do they produce successful results? Taguchi advocates often refute criticism with the remark that "they work." We must remember that the "best guess" and "one-factor-at-a-time" methods will also work-and occasionally they produce good results. This is no reason to claim that they are good methods. Most of the successful applications of Taguchi's technical methods have been in industries where there was no history of good experimental design practice. Designers and developers were using the **best guess** and **one-factor-at-a-time methods** (or other unstructured approaches), and since the Taguchi approach is based on the factorial design concept, it often produced better results than the methods it replaced. In other words, the factorial design is so powerful that, even when it is used inefficiently, it will often work well.

As pointed out earlier, the Taguchi approach to parameter design often leads to **large, comprehensive experiments**, often having 70 or more runs. Many of the successful applications of this approach were in industries characterized by a high-volume, low-cost manufacturing environment. In such situations, large designs may not be a real problem, if it is really no more difficult to make 72 runs than to make 16 or 32 runs. On the other hand, in industries characterized by low-volume and/or high-cost manufacturing (such as the aerospace industry, chemical and process industries, electronics and semiconductor manufacturing, and so forth), these methodological inefficiencies can be significant.

A final point concerns the learning process. If the Taguchi approach to parameter design works and yields good results, we may still not know what has caused the result because of the aliasing of critical interactions. In other words, we may have solved a problem (a short-term success), but we may not have gained **process knowledge,** which could be invaluable in future problems.

In summary, we should support Taguchi's **philosophy** of quality engineering. However, we must rely on simpler, more efficient methods that are easier to learn and apply to carry this philosophy into practice. The response surface modeling framework that we present in the textbook is an ideal approach to process optimization and as we have demonstrated, it is fully adaptable to the robust parameter design problem.

Supplemental References

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