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Reducing Variation in an Existing Process with Robust Parameter Design

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ABSTRACT Reducing variation in key product features is an important goal in process improvement. Finding and controlling the cause(s) of variation is one way to reduce variability but may not be cost effective or even possible in some situations. Alternatively, we can reduce variation in a critical output by reducing the sensitivity of the process to the main sources of variation rather than controlling these sources directly. This approach is called *robust parameter design* and exploits interaction between the causes of output variation and control factors in the process. In the literature, a variety of experimental plans have been proposed to help implement robust parameter design. We compare two classes of plans that we call *desensitization* and *robustness* experiments. With a desensitization experiment, we need knowledge of a dominant cause and the ability to set its level in the experiment. With a robustness experiment, we use time or location (Shoemaker et al. 1991) to indirectly generate the effect of the dominant causes of output variation. In this article, we explore qualitatively and quantitatively the differences between robustness and desensitization experiments. We argue that for an existing process, desensitization is the preferred choice.

KEYWORDS desensitization, robustness, statistical engineering, Taguchi methods, variation reduction

INTRODUCTION

Excessive variation in critical process output characteristics can have many undesirable effects such as scrap/rework costs, customer dissatisfaction, impairment of function, etc. As such, variation reduction is an important goal of quality improvement.

The causes of output variation must be process inputs that vary as the process operates. One way to reduce output variation is to find and then remove or reduce the variation of a cause. When this is not feasible or cost effective, we consider the alternate strategy of robust parameter design. The goal is to reduce variation in the output by reducing the sensitivity of the process to the sources of variation rather than controlling these sources directly. There is an extensive literature on robust parameter design (Kacker 1985; Kackar and Phadke 1981; Ross 1988; Taguchi 1987a, 1987b; Taguchi and Wu 1980). See also the related discussion in Nair (1992) and Robinson

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et al. (2003) for an overview of the ideas and controversies. General background on experimental design is given in Box et al. (1978) or Montgomery (2005).

In the robust parameter design literature, a varying input (cause) is called a *noise factor*. Noise factors can be environmental, such as ambient temperature, or aging related, such as the number of usage cycles. These are called *external noise factors* by Taguchi (1987) and cannot be controlled. So-called internal noise factors are inputs that vary from unit to unit in the manufacturing process. Two examples are the pouring temperature of iron in a foundry that varies about a target and the dimension of a component characteristic in an assembly. Many of these internal noise factors can be controlled but at a cost. To implement robust parameter design, we change inputs to the process that are normally fixed; for example, temperature set point, aspects of the control plan, etc. Taguchi refers to these normally fixed inputs as *control factors*. The goal of robust parameter design is to find new values for the control factors that result in less output variation without changing the behavior of the noise factors.

To represent this idea mathematically, consider the following simple (unrealistic) model

$$Y = \beta_0 + \beta_1 Z + R \qquad [1]$$

where the random variable Y represents the output, the random variable Z describes a particular noise factor, and R represents the variation due to all other varying (noise) inputs. We also suppose for the sake of simplicity that Z and R vary independently, and so, denoting standard deviation as sd we have

$$sd(Y) = \sqrt{\beta_1^2 sd(Z)^2 + sd(R)^2} \qquad [2]$$

The first term under the square root is the contribution of the noise factor to the overall variation in the output. We can reduce this contribution by reducing or eliminating the variation in Z (i.e., reduce $sd(Z)$) or by reducing the magnitude of $|\beta_1|$. If we suppose that β_1 depends on the values of one or more control factors, we can carry out experiments to find these control factors and change their current levels to reduce $|\beta_1|$.

To illustrate how this might work, consider the simplest situation where we have just one control and one noise factor. Then, the basic idea behind robust parameter design can be demonstrated by considering the following simple extension to model [1] for the output Y:

$$\begin{aligned} Y &= \beta_0 + \beta_1 Z + \beta_2 x + \beta_3 xZ + R \\ &= \beta_0 + \beta_2 x + (\beta_1 + \beta_3 x)Z + R \end{aligned} \qquad [3]$$

where x represents the level of the control factor. If we denote the current value of x as x_0, then $\beta_1 + \beta_3 x_0$ is the slope of the linear relationship between the noise (Z) and the output (Y) with the current process settings. To implement robust parameter design, we need to find a new setting for x that flattens the relationship between output and noise. This means that we are looking for a new level of x, say x_1, where $|\beta_1 + \beta_3 x_1|$ is closer to zero than $|\beta_1 + \beta_3 x_0|$. If β_3 is large with respect to β_1, then we can reduce $|\beta_1 + \beta_3 x_0|$ with relatively small changes in x. With the process change, we continue to live with the variation in the dominant cause Z.

We show in Figure 1 how the process might behave before and after we have implemented this strategy. In statistical terminology, we have exploited the

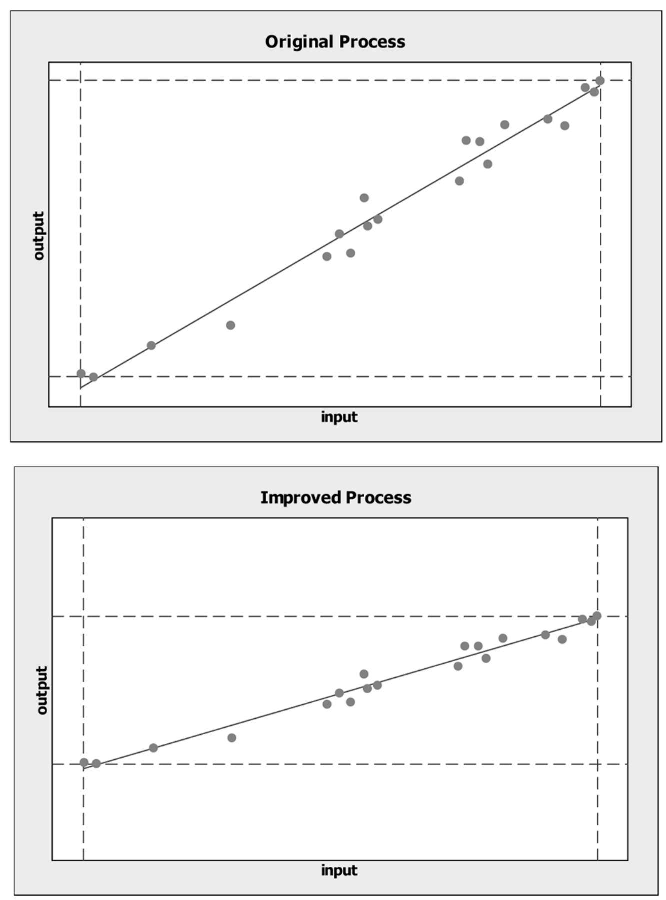

FIGURE 1 Illustration of process improvement using parameter design.

interaction between the noise factor and the control factor. Note that the variation in the output (as indicated by the dashed horizontal lines) is less in the right panel even though the variation in the noise factor (as indicated by the dashed vertical lines) is the same.

We can also use the model [2] to make the critical point that there is little value in reducing the contribution of Z to the variation in Y unless that contribution is large. Suppose $sd(\text{due to Z}) = |\beta_1| sd(Z)$ is equal to the contribution of all other causes $sd(\text{all other causes}) = sd(R)$. We can reduce the variation in the output by (only) about 30% if we completely eliminate the variation due to Z. If, on the other hand, $sd(\text{due to Z}) = sd(\text{due to all other causes})/2$, the possible reduction in output variation is only about 10%.

In this article we assume only one or a few dominant causes exist; that is, there are a few noise factors that are responsible for a substantial proportion of the variation in the output. This assumption corresponds to assuming the Pareto principle applies to the contribution of causes (Juran and Gyrna 1980; Steiner et al. 2008). We believe that it is difficult to substantially reduce output variation using robust parameter design or any other approach unless there are dominant causes.

We may also be able to apply robust parameter design if we can control the target value of an input but not the variation of the input about this target. Suppose the relationship between the output and input is nonlinear as shown in Figure 2. With the current target value, suppose we obtain the range of values for the input shown by the dashed vertical lines in the top panel of Figure 2. There is a corresponding large amount of variation in the output. If we reduce the target value of Z without controlling the variation, as shown in the bottom panel of Figure 2, we can reduce the output variation substantially. To take advantage of this solution, we likely also need to find a way to increase the average value of the output using some other control factor.

To apply robust parameter design, we need to plan and execute an experiment where we deliberately manipulate at least one control factor. The goal of the experiment is to find (directly or indirectly) an interaction between the important noise(s) and the selected control factors and then determine settings of these control factors that result in less output variation. We call an experiment with this goal a

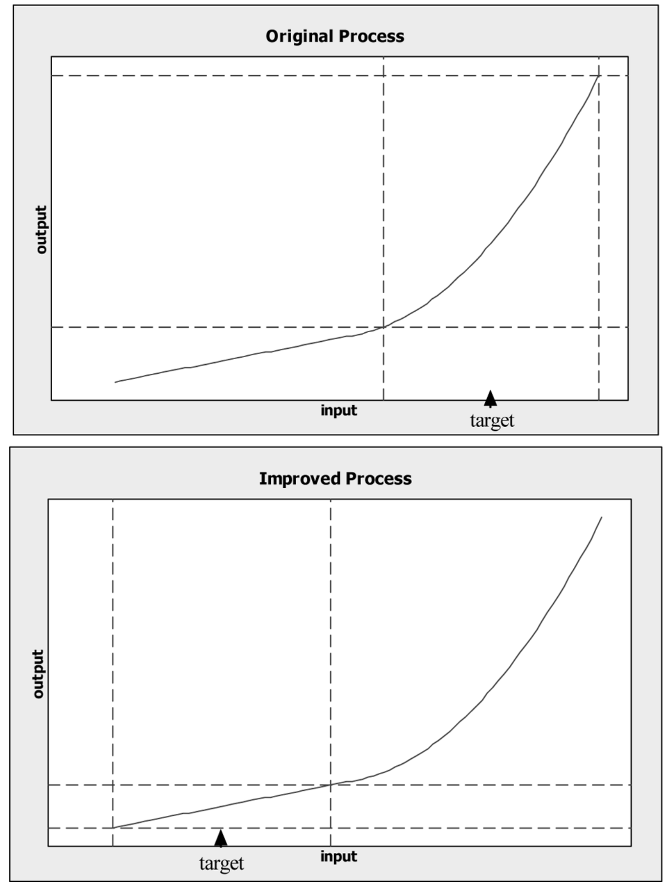

FIGURE 2 **Hypothetical nonlinear input–output relationship showing range of input and corresponding range of output values.**

Taguchi experiment. We may find that we have shifted the mean of the output with such a change. If we do not have a known way to adjust the average level of the output, we might also include other control factors in the experiment looking for a way to adjust the process mean without affecting the variation.

Taguchi proposed robust parameter design to address problems in process or product design. He suggested sensibly that the strategy is best applied as early as possible when design changes are easier and cheaper. However, this approach has also been widely applied in manufacturing to improve existing processes (see Miller et al. 1993; Quinlan 1985; Wu and Hamada 2000). In this article we consider only the setting where the goal is to improve an existing process. We discuss the implications of this context in the following section.

Consider the case study discussed by Miller et al. (1993). We will also use this example later to compare the different experimental plans. The process was the heat treatment of the pinion and ring gear set that provides for the transmission of power from

TABLE 1 The Control and Noise Factors for the Gear Experiment

Label	Control factor	Low level (−1)	High level (+1)
A	Carbon potential in furnace	1.15%	1.40%
B	Operating mode	Normal	Continuous
C	Last zone temperature	1,500°F	1,650°F
D	Quench oil temperature	300°F	360°F
E	Quench oil agitation	125 rpm-0 del	300 rpm-5 del
	Noise factor		
F	Furnace track	Left	Right
G	Initial tooth size deviation	Undersize	Oversize
H	Part orientation in basket	Up	Down

the drive shaft to the axle in a rear-wheel-drive vehicle. The sets are heat-treated to improve strength and wear characteristics. There was excessive variation in distortion during heat treatment as measured by the dishing of the gear. The production team conducted an experiment with five control factors (A–E) and three noise factors (F–H) hoping to be able to reduce the variation in dishing. The experimental factors and their corresponding low and high levels are given in Table 1. The factors and levels were selected through a brainstorming session.

The noise factors F and H could not be normally controlled because of the process design. To maintain production volume, the process used each of the parallel furnace tracks and the cone-shaped gears were placed in two orientations in the baskets that passed through the furnace. While noise factors F and H could be easily controlled in the experiment, controlling initial tooth size (factor G) required measuring and sorting parts before heat treatment that substantially increased the cost of the experiment. As we shall see, this effort was wasted because factor G is not an important noise factor.

The experiment consisted of a crossed control by noise array (Ross 1988) with a 2^{5-1} fractional-factorial design for the control factor array and a 2^3 full-factorial design for the noise array; see Table 2. Taguchi calls these the inner and outer arrays, respectively. For each combination of the control and noise factors, one part was measured after heat treatment. There were $16 \times 8 = 128$ parts in total. To conduct the experiment, the control factors were set and the process was allowed to stabilize. Baskets were prepared using gears with both levels of factors G and H. The remaining parts in the basket were scrap from an earlier part of the process because none of the parts produced during the experiment

TABLE 2 Design Matrix and Output Data (Gear Dishing) for the Gear Experiment

	Inner array					Outer array										
						F	1	1	1	1	−1	−1	−1	−1		
						G	1	1	−1	−1	1	1	−1	−1		
Treatment	A	B	C	D	E	H	1	−1	1	−1	1	−1	1	−1	Average	SD
1	1	1	1	1	1		7	12	6.5	14	3	14	4	16.5	9.63	5.12
2	1	1	1	−1	−1		13.5	14.5	5.5	17	−7.5	15	−4.5	12	8.19	9.42
3	1	1	−1	1	−1		3	11	5.5	18	3	19	1	21	10.19	8.16
4	1	1	−1	−1	1		10.5	14.5	6.5	17.5	3	14.5	9	24	12.44	6.63
5	1	−1	1	1	−1		10	23	3.5	23	4.5	25.5	10	21	15.06	9
6	1	−1	1	−1	1		6.5	22	14.5	23	5.5	18.5	8	21.5	14.94	7.36
7	1	−1	−1	1	1		5.5	28	7.5	28	4	27.5	10.5	30	17.63	11.66
8	1	−1	−1	−1	−1		4	14	6.5	23	9	25.5	9	24.5	14.44	8.69
9	−1	1	1	1	−1		−4	18.5	11.5	26	−0.5	13	0	16.5	10.13	10.61
10	−1	1	1	−1	1		9	19	17.5	21	0.5	20	6.5	18	13.94	7.58
11	−1	1	−1	1	1		17.5	20	10	23	6.5	21.5	0	26	15.56	9.09
12	−1	1	−1	−1	−1		7	23.5	1	20	7	22.5	4	22.5	13.44	9.53
13	−1	−1	1	1	1		2.5	22	12	19.5	7	27.5	8.5	23.5	15.31	9.01
14	−1	−1	1	−1	−1		24	26	14.5	27.5	7	22.5	13	22	19.56	7.22
15	−1	−1	−1	1	−1		5.5	27	2.5	31	12.5	27	11.5	32.5	18.69	11.99
16	−1	−1	−1	−1	1		11	21.5	12	27	16.5	29.5	16	28.5	20.25	7.43

could be shipped. Two baskets, one for each furnace track, were processed for each combination of the control factors in the control array. The treatment combinations defined by the inner array (control factors) were run in random order, whereas for each treatment all combinations of the outer array were conducted simultaneously. The experimental design and resulting data are shown in Table 2.

The rest of this article is organized as follows. The following section defines and qualitatively compares two types of Taguchi experiments we call desensitization and robustness experiments. Both types are used in practice to improve existing processes but little has been done to compare them. The next section quantitatively compares the two designs. Though a robustness experiment is easier to conduct, we show the performance superiority of desensitization. Finally, we discuss additional issues and draw conclusions.

EXPERIMENTAL PLANS FOR FINDING A ROBUST SOLUTION TO MANUFACTURING VARIATION

Here we outline some of the key steps in planning a Taguchi experiment. We assume that the goal is to reduce variation in a critical process output that relates to process/product quality. The output must closely match a high-priority management or customer concern. We focus on a single output, but often there are many.

Planning Issues

In our view, there are four critical steps in planning a Taguchi experiment in the context of reducing output variation in an existing manufacturing process. First, we need to identify the dominant noise factors in the process or at least understand how they act over time or location. In the second step, we need to decide the general class of experiment that we plan to carry out. Third, we must determine the control factors we will change in the experiment and their levels. Finally, we need to select a specific experimental design.

Identify Dominant Noise Factors

As discussed in the Introduction, we cannot reduce output variation substantially without reducing the effect of one or more dominant causes of the variation. This is especially true because we should not expect to be able to eliminate completely the effect of a noise factor. Adding noise factors with small effects to the experiment makes it larger and more complex without any possible benefit.

In the context of developing a new product or process design, the typical approach is to use brainstorming to identify the important noise factors. This is sensible in the design stage, because we have few alternatives. However, to improve an existing process, rarely should a Taguchi experiment be considered the first empirical investigation (Steiner and MacKay 2005). Rather, to improve an existing process, we should first look for dominant causes using observational studies that are cheaper and simpler than experimental investigations. Looking for large (root) causes of variation before attempting to find a solution is part of most process improvement systems such as the diagnostic and remedial journey discussed by Juran and Gyrna (1980), DMAIC in Six Sigma (define, measure, analyze, improve, and control; Breyfogle 1999), and statistical engineering (Steiner and MacKay 2005).

Recall that dominant causes are process inputs that naturally change as the process operates. If we are able to measure both the value of the dominant cause and the output on a number of parts, a scatterplot will reveal any strong relationship. Strategies for finding dominant causes using observational investigations are discussed in Steiner and MacKay (2005). Once we have identified a small number of suspected dominant causes, concerns about possible confounding can be put to rest through a simple targeted experiment. If a dominant cause is found, we may be able to pursue a different remedy, even if reducing variation in the dominant cause is not an option. For instance, we may employ feedforward control or use error-proofing with 100% inspection on the dominant cause.

Searching for a dominant cause can be difficult and time consuming and there is no guarantee of success. However, even if we do not find such a cause, we may discover other important process information that can help us plan a useful Taguchi experiment. For instance, as we shall discuss, knowing how the output varies over time can be critical. If we see that there is little variation in the output between consecutive parts but a great deal of variation across shifts, we will need to observe the process across many shifts to see the action of the dominant causes.

Select a Desensitization or Robustness Experiment

Once we have identified the dominant causes of variation or at least looked for them, we need to determine how to induce their effect in the experiment. This distinguishes the two types of Taguchi experiments we call desensitization and robustness experiments, following the terminology suggested by Steiner and MacKay (2005). For both types of experiments, the goal is to find new process (control factor) settings that make the process output less sensitive to variation in the dominant causes.

In a desensitization experiment, we control the dominant noise factors in the experiment. There are many examples of such experiments in the literature. See, for instance, the layer growth and leaf spring examples in Wu and Hamada (2000) and the engine block porosity, oil pan scrap, refrigerator frost buildup, and eddy current examples in Steiner and MacKay (2005). The gear example discussed in the Introduction is a desensitization experiment because the levels of the noise factors were deliberately set in the experiment.

We must choose the levels of the noise factors for a desensitization experiment. In the experiment, we want to change the levels of the noise factors to induce the variation seen in the regular process. Thus, the high and low levels of the noise factor should be set at the high and low ends of the range of values seen in regular production. This way, the high and low levels are expected to give extreme performance for the output from current process. If the levels are chosen too close together, we will not see the complete effect of the noise factor as it normally acts. If the levels are selected to be too extreme, the experimental results will not map well to the regular process. Selecting the appropriate levels requires that we know the range of values for the noise factors in regular production.

In a robustness experiment, we measure the output over time or location to generate the effect of the important noise factors. We do not need to first identify these factors. If we have failed in Step 1 to find the dominant causes of the process variation, we only have the option of a robustness experiment. Even in cases where we know the dominant causes, we may prefer a robustness experiment because controlling the levels of the noise factors in an experiment can be hard or impossible. If it were easy to control these noise factors, we would simply fix them to reduce the variation in the output. There is then no need to look for a way to make the process less sensitive to variation in these causes.

There are many examples of robustness experiments in the literature. See, for instance, the speedometer cable example in Quinlan (1986), the examples in Jiju et al. (2001, 2004), and the crossbar dimension, iron silicon concentration, and electroplating pinskip defect examples in Steiner and MacKay (2005). In the gear example discussed in the Introduction, a robustness experiment might have been planned as follows. For each treatment (defined by the combination of levels of the control factors), heat treat several baskets of parts. Then sample eight gears at random from each run of baskets. This is a robustness experiment with eight repeats for each treatment combination. Carrying out the robustness experiment would have been easier than the desensitization experiment because we would avoid sorting parts with respect to factor G, the initial gear size.

For the robustness experiment to have any hope of identifying process settings that will be more robust to variation in the dominant causes, the dominant causes must act among the repeats within each treatment. That is, we want the values from the repeats for each treatment to exhibit the same variation in the output we would see if we permanently change the process to the settings defined by the treatment. One major challenge with robustness experiments is that it is difficult to estimate output variation with a small number of observations per treatment. However, it is usually too expensive to do many repeats. In a desensitization experiment this problem is avoided because we set the noise factor(s) at extreme levels to generate the range of output values we expect to see if the process were permanently changed.

Choose Control Factors and Levels

In the third step, we need to select the control factors and their levels. The control factors are normally fixed inputs and thus we likely have little prior empirical knowledge to help us choose these factors. Instead, we rely on engineering or process knowledge. Knowing the dominant noise factors may help because the goal is to find control factors that interact with the noise factors. Brainstorming (Montgomery 2005) or similar exercises that try to capture process knowledge from a wide range of people can be useful. If we choose the control factors poorly, the experiment will fail to meet its goal.

In most applications, a preliminary investigation will give us a good estimate of the variation in the output of interest with the current process settings. We then have the freedom to select levels of the control factors different from the current settings. We can then compare this estimate against the predicted variation for various combinations of the control factors using the results of the experiment.

Select Final Design

One possible design for a desensitization experiment is a so-called crossed-array design. This consists of a full- or fractional-factorial design of the control factors, called the *inner array*, that is crossed with a full- or fractional-factorial design in the noise factors, called the *outer array* (Montgomery 2005; Ross 1988). Shoemaker et al. (1991) call this setup a *product array* because the outer array is run for every row in the inner array. See the gear example design in Table 2. The total number of runs in the crossed-array design can easily become large. In the gear example, with five control factors and three noise factors, even though we use a half fraction design for the inner array, there are a total of 128 runs. Some critics of Taguchi (e.g., Miller et al. 1993; Shoemaker et al. 1991) recommend using a combined array for a desensitization experiment instead of a crossed array to reduce the number of runs. See also Wu and Hamada (2000). The key is that this design must allow estimation of all noise by control interactions without aliasing with any other terms likely to be significant. Note that if we have spent the time and effort before planning the desensitization experiment to look for and find the dominant cause(s) we are much better off because then the outer array only has one or a small number of noise factors.

It is often convenient and beneficial to restrict the randomization of the order of the runs in a desensitization experiment. Restricted run order randomization leads to a split-plot experiment (Kowalski et al. 2007).

With split-plot experiments we obtain less information about the effect of the so-called whole-plot factors while obtaining more information about the effect of the subplots and subplot by whole plot interactions. Because the main goal is to identify large noise by control interactions, arranging the experiment as a split-plot design can be an advantage. This point was discussed for several scenarios by Box and Jones (1992) in the context of desensitization experiments. First, because noise factors are often hard to set and change, many desensitization experiments are split plots where noise factors are the whole plot factors. An example is the engine block porosity experiment in Steiner and MacKay (2005). On the other hand, we sometimes use the control factors to define the whole plot factors. Consider the refrigerator example in Steiner and MacKay (2005) where the noises were environmental conditions that were relatively easy to change through the use of a testing chamber. However, changes to the control factors were difficult because each treatment required a new prototype to be built. The gear experiment described in the Introduction is another example of a split-plot experiment where the control factors are the whole-plot factors.

The plan for a robustness experiment, on the other hand, is defined by the inner array design with a number of repeats for each treatment combination. To plan a good robustness experiment and decide on the number of repeats and how to select them, we need to know how the output variation (and thus dominant cause) acts over time. As discussed in stage 2, we want the repeats within each run to exhibit the long-term output variation for that combination of levels of the control factors. If we use consecutive parts to define the repeats, the dominant causes must act quickly; for example, part to part. Otherwise, the experimental run would need to be very long. For more guidance and examples of how to select the time span and the number of repeats for robustness experiments, see Steiner and MacKay (2005).

In a robustness experiment, we would like to completely randomize the order of the runs. However, often one or more of the selected factors is hard to change. For instance, changing a furnace temperature may be difficult because after a change we would need to wait for the temperature to stabilize. This suggests restricting the randomization so that runs with the same value of the hard-to-change factor(s) are conducted consecutively. With a robustness experiment the split-plot structure does not provide the benefit seen in the desensitization experiment because we are not estimating the control by noise factor interactions directly.

Analysis Issues

Many papers have addressed questions and choices in the analysis of Taguchi experiments; here we present only a short summary.

We discuss three analysis options. For two of these options, we start by calculating the average $\bar{y}$ and the standard deviation s (of the experimental results) for each of the combination of the control factors. The standard deviation captures the process variation generated by the changes in the noise factors. Taguchi (1987) suggested combining the average and the standard deviation for each inner array treatment into a single performance measure $\log(\bar{y}/s)$ known as the *signal-to-noise ratio* (S/N; Kackar 1985; Montgomery 2005; Wu and Hamada 2000). The form of the S/N ratio depends on the goal of the experiment. S/N ratios have been criticized as somewhat arbitrary (Nair 1992). Also, Wu and Hamada (2000, p. 468) stated "Although the S/N ratio . . . has a natural measure in some disciplines . . . it has little or no meaning for many other problems . . . . It lacks a strong physical justification in most practical problems (p. 468)."

An alternative analysis, called *location and dispersion modeling*, involves separately modeling the average $\bar{y}$ and dispersion s as functions of the control factors; see Nelder and Lee (1991) and Engel and Huele (1996). This adds flexibility and allows the user to compromise in optimizing the mean and variance as he or she sees fit. Note that when conducting the analysis based on either an S/N ratio or the dispersion s we are looking for a favorable interaction between the noise and control factors indirectly.

The third and probably best option is to use so-called response modeling (Wu and Hamada 2000). In response modeling we directly model the effect of both the control and noise factors on the output. With response modeling, we do not first summarize the results for each combination of the control factors but model the individual responses directly.

The first two analysis options are possible with both desensitization and robustness experiments. With robustness experiments, response modeling is not normally possible because noise factors are not manipulated in the experiment. Response modeling is possible for a robustness experiment if we measure the values of the noise factors during each run (Freeny and Nair 1992). By measuring the noise factors, we can also check whether each run has roughly the same variation in the dominant noise factors. Note that the variation seen in the noise factors in the robustness experiment would likely be smaller than the variation we deliberately create in a desensitization experiment. This lack of variation in the noise factors will make detecting noise by control interactions more difficult.

It can be argued that robustness experiments are more flexible than desensitization experiments because in the analysis we do not need to model the noise by control interaction explicitly. This allows us, in theory, to indirectly find higher order control by noise interactions.

To illustrate the response model approach, consider again the gear example we discussed in the Introduction. Here, following Miller et al. (1993), for simplicity we ignore the split-plot nature of the conducted experiment. Analyzing the experiment as a split plot will not change the estimated effects but may alter which effects are identified as significant. Figure 3 gives the normal probability plot of the effects when we fit a model with all terms up to the three-factor interactions.

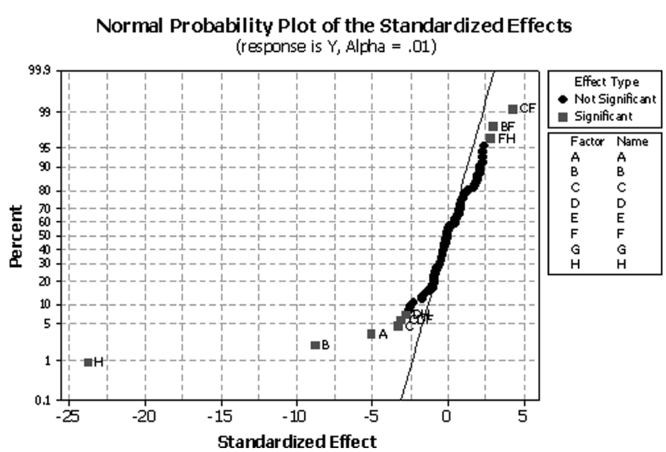

FIGURE 3 Normal probability plot in the gear experiment (when output is Y).

The results suggest a model with eight significant terms. Fitting the corresponding model and adding all main effects to preserve effect hierarchy from the two-way interactions, we get:

Estimated Effects and Coefficients for *y* (Coded Units)

Term	Effect	Coef	SE Coef	*T*	*P*
Constant		14.336	0.3288	43.60	0.000
A	−3.047	−1.523	0.3288	−4.63	0.000
B	−5.297	−2.648	0.3288	−8.05	0.000
C	−1.984	−0.992	0.3288	−3.02	0.003
D	−0.625	−0.312	0.3288	−0.95	0.344
E	1.250	0.625	0.3288	1.90	0.060
F	0.844	0.422	0.3288	1.28	0.202
G	−1.391	−0.695	0.3288	−2.11	0.037
H	−14.391	−7.195	0.3288	−21.88	0.000
B*F	1.844	0.922	0.3288	2.80	0.006
C*F	2.594	1.297	0.3288	3.94	0.000
D*H	−1.688	−0.844	0.3288	−2.57	0.012
F*H	1.719	0.859	0.3288	2.61	0.010
C*D*F	−1.859	−0.930	0.3288	−2.83	0.006

$S = 3.72021$; $R^2 = 84.67\%$; R^2 (adj) $= 82.92\%$.

Analysis of Variance for *y* (Coded Units)

Source	*DF*	Seq SS	Adj SS	Adj MS	*F*	*P*
Main effects	8	8,094.9	8,094.9	1,011.87	73.11	0.000
Two-way interactions	4	509.7	509.7	127.43	9.21	0.000
Three-way interactions	1	110.6	110.6	110.63	7.99	0.006
Residual error	114	1,577.8	1,577.8	13.84		
Total	127	10,293.1				

The largest effect comes from Factor H. This is seen both in the normal probability plot given in Figure 3 and the difference in the mean levels of the output for the low and high level of H in the individual value plot given in the top panel of Figure 4. Neither of the other two noise factors F or G appears to be important. This suggests that these noise factors were not well chosen. That is, varying F and G has not helped induce variation in distortion. Excluding F and G from the outer array would have simplified the experiment without substantially reducing the efficiency. Also, recall that setting the levels of factor G (initial gear size) was difficult and expensive because it required extensive sorting of parts. It would have been easy to discover that F and G were unimportant noise factors in a preliminary observational study.

Because H is the only important noise factor, the goal is to find large interactions between H and one (or more) of the control factors. The experimental results suggest that there is only one significant noise by control interaction (involving H), namely, DH. This suggests that we can reduce variation in the output by changing the level of the control factor D. However, it is disappointing that the effect of DH is small relative to the effect of H. We see this effect of the DH interaction indirectly in the (somewhat) smaller variation in the output for the low level of D compared with the high level of D in the bottom panel of Figure 4. Assuming that the fitted linear model is reasonable, we would need to move D much further than we have tried in the experiment to substantially reduce the variation in the output. Other constraints would not permit such a dramatic change.

With some additional information about the noise variation we can use the fitted model to estimate how much we could reduce the output variation by changing factor D. In the process, half of the gears

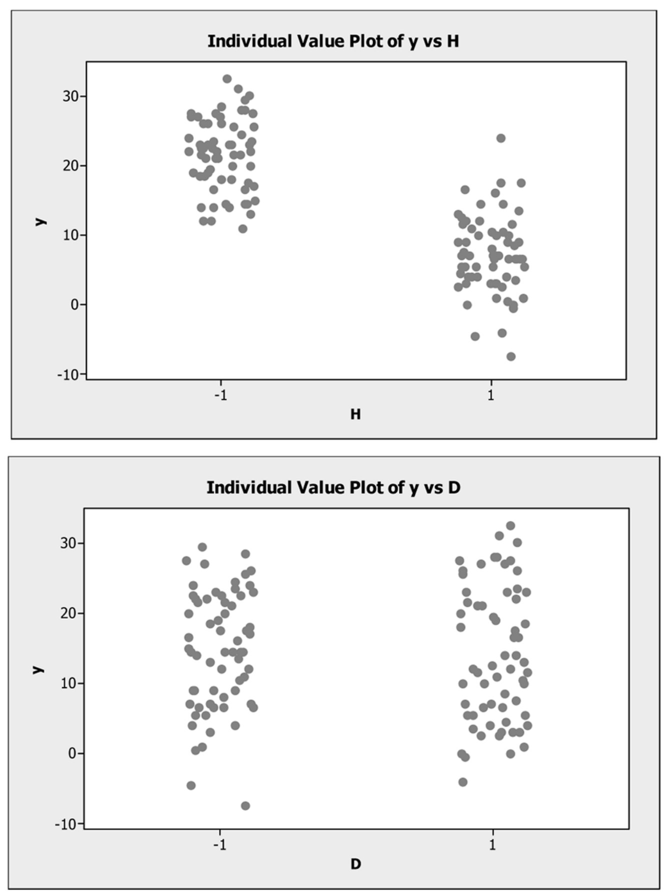

FIGURE 4 Individual value plots for gear example.

are place in the up position ($H = 1$) and the other half are down ($H = -1$). As a result, on the coded scale we have $sd(H)$ equal to 0.5. Then, using the estimated effects, if we move the control factor D to its low level, we would reduce the output standard deviation to 4.9 ($= \sqrt{(7.195 - .844)^2 0.5^2 + 3.72^2}$). Note that the residual variation is slightly too small because we have included the non-important noise factors F and G in the model removing their effect. The process standard deviation from preliminary studies is 5.1, so the predicted reduction in variation by changing D is small.

Summary of the Qualitative Comparison of Desensitization and Robustness Experiments

The goal of both desensitization and robustness experiments is to find settings of the control factors that make the process less sensitive to variation in the dominant noise factors. In the experiment we want the observed output values for each treatment combination (defined in terms of the control factors) to capture the variation we expect to see over the long term. Here, we summarize the qualitative advantages/disadvantages of robustness and desensitization experiments.

The major advantages of the desensitization experiments over robustness experiments are

- Knowing the dominant cause(s) may suggest that we can address the cause directly, thereby avoiding the need to look for a robust solution.
- Knowing the dominant cause(s) may make it easier to select control factors that interact with these causes.
- Desensitization experiments allow for a more efficient response model analysis.
- Because the noise factors are set in the experiment, we can guarantee their variation within each combination of the control factors.

The major advantages of the robustness studies over desensitization studies are

- Less process knowledge is required—we do not need to identify or even search for the dominant noise factors.
- Robustness experiments are easier to run because we do not need to hold noise factors fixed.
- The analysis of a robustness experiment is less model dependent than for a desensitization experiment.

QUANTITATIVE COMPARISON OF ROBUSTNESS AND DESENSITIZATION EXPERIMENTS

In this section we quantitatively compare robustness and desensitization experiments and attempt to quantify the benefit of knowing and controlling the dominant cause when conducting a Taguchi experiment.

The best way to summarize the performance of the two types of experiments is not immediately clear because the performance depends on the analysis method employed. Because our focus is primarily on comparing the robustness and desensitization plans, in each case we use the best available analysis. For the robustness experiment, the analysis is the usual one based on the standard deviation of the output for each treatment combination. For the desensitization experiment, we use the response modeling approach.

One way to compare the two types of experiments is to look at how well each can estimate the variability (standard deviation) of the output for any treatment combination defined by the levels of the control factors that were used in the experiment. We consider this comparison in the following subsection, where, in addition, to simplify the comparison, we use only a single control factor and a single noise factor. This matches the suggestion made earlier about including only dominant noise factors in desensitization experiments and the assumption that a dominant cause exists.

Ultimately, the usefulness of a desensitization or robustness experiment depends on the quality of the decisions made from each experiment about the best process settings for the control factors. In a later subsection we consider the comparison of the two types of experimental plans in the context of decision making and use the gear example, discussed in the Introduction, to illustrate the superiority of desensitization plans.

Comparison Based on Precision of the Estimate of the Output Variation

To quantify performance of the two types of plans, we adopt model [3] reproduced below

$$Y = \beta_0 + \beta_2 x + (\beta_1 + \beta_3 x)Z + R$$

and assume $Z \sim N(\mu_z, \sigma_z^2)$ and $R \sim N(\mu_r, \sigma_r^2)$. For the comparison, without loss of generality, because the mean output level given x will not matter in our comparison, we set $\beta_0 = \beta_2 = 0$, $\mu_r = \mu_z = 0$, and $x_0 = 0$. From model [3] the output variation is

$$sd(Y\,|\,x) = \sqrt{(\beta_1 + \beta_3)^2 \sigma_z^2 + \sigma_r^2} \qquad [4]$$

For the comparison we can simplify further by assuming $\beta_1 = \beta_3 = 1$ and $\sigma_r = 1$. These choices do not affect the comparison of the two plans but impact the relative importance of the dominant cause and the levels of the control factor. With these additional assumptions for the parameters, from [4], Z is a dominant cause if $(1+x)^2\sigma_z^2 > 1$. So in the current process where $x = x_0 = 0$, Z is a dominant cause if $\sigma_z^2 > 1$. The levels of x used in the desensitization and robustness experiments then reflect the potential to reduce process sensitivity to variation in the dominant cause. Note that in estimating the performance measure [4] we are estimating a standard deviation. We consider how well we can estimate [4] with comparable robustness and desensitization experiments.

In this comparison, the desensitization experiment consists of k replicates of the $2^2 = 4$ treatment experiment defined by the high and low levels of the noise and control factor. This is equivalent to a crossed array with full-factorial designs for both the inner and outer arrays. The levels of the control factor are $\pm x$ and the levels of the dominant cause z are $\pm 2\sigma_z$. The corresponding robustness experiment has only two treatments given by the high and low levels of the control factor. To make the experimental effort comparable, the robustness experiments use $2k$ repeats for each treatment. Both experiments produce a total of $4k$ observations.

For the robustness experiment, we estimate the output standard deviation directly based on the experimental results. This means that we estimate $sd(Y|x)$ using the sample standard deviation of robustness experiment results when the level of the control factor is x. We can theoretically determine the precision of the estimator for $sd(Y|x)$. The sampling distribution of the sample variance, denoted s^2, is a scaled chi-square (Abramowitz and Stegun 1972):

$$s^2 \sim \frac{\sigma_y^2}{n-1}\chi_{n-1}^2 \;\Rightarrow$$
$$Var(s^2) = \left(\frac{\sigma_y^2}{n-1}\right)^2 2\,(n-1) = \frac{2\,\sigma_y^4}{n-1},$$

where n is the number of observations used in the calculation of s^2. So, using a linear approximation to the square root evaluated at the expected value, the performance of the robustness experiment is approximately

$$P_{rob} \cong \frac{\sigma_y}{\sqrt{2(n-1)}} = \frac{\sqrt{(1+x)^2\sigma_z^2 + 1}}{\sqrt{2(2k-1)}}, \qquad [5]$$

because in the robustness experiment we use $2k$ observations (repeats) to estimate $sd(Y|x)$ for each level of x. From [5] we see that the performance of the robustness experiment depends on the size of the dominant cause, how far we move the control factor, and the number of repeats.

For the desensitization experiment, we start with model [3]. For a fixed x, the standard deviation of the output, $sd(Y|x)$, is given by Eq. [4]. To estimate $sd(Y|x)$, we must either know or estimate β_1, β_3, σ_r, and σ_z. The first three can be estimated from the desensitization experiment, whereas σ_z must be estimated from prior knowledge. We also need σ_z in the planning of the experiment to set the low and high levels of the dominant cause z.

For the desensitization experiment we consider two extreme cases.

1. Assume that β_1, σ_z, and σ_r are known; that is, assume that we have complete knowledge about the dominant cause and its relationship to the output from prior investigations. We need only to estimate β_3 from the desensitization experiment.
2. Assume that only σ_z is known so we can set the levels of Z in the experiment. We estimate β_3, β_1, and σ_r from the desensitization experiment.

In practice, the extent of our knowledge will usually be between these extreme cases. If we spend some resources looking for a dominant cause before conducting a Taguchi experiment, as we recommend, we will have an estimate of the relationship between the dominant cause and the output. As suggested by Steiner and MacKay (2005), we will likely have results from an observational study where the dominant cause and output are measured on a number of parts. We can estimate β_1 (really $\beta_1 + \beta_3 x_0$, but we assume $x_0 = 0$) and σ_r from these data using a simple linear regression model. In the overall analysis, we can combine these estimates with the results of the desensitization experiment.

For case 1 we can determine the performance of the desensitization experiment theoretically. Assuming that σ_z, σ_r, and β_1 are known and the current value of x (i.e., x_0) is equal to zero, the standard deviation of the output at some new x-level can be estimated with a desensitization experiment by estimating β_3 (we denote the corresponding estimate by $\hat{\beta}_3$). We have the estimate $Var(\widehat{Y}|x) = (\beta_1 + \hat{\beta}_3 x)^2 \sigma_z^2 + \sigma_r^2$ and because we use a regression model to estimate β_3 it can be shown (Asilahajani 2008) that the variance of the estimate is $\dfrac{\sigma_r^4 + 2\sigma_r^2(4n\mu_A^2\sigma_z^2)}{8n^2}$ where $A = 1 + \hat{\beta}_3 x$, $\mu_A = E(A)$, $\sigma_A^2 = Var(A)$, and n equals the total number of observations. Using linear approximation to square root at the expected value, and denoting P_{desens} as the standard deviation of the estimator of stdev $(Y|x)$, we get

$$P_{desens} \cong \sqrt{\frac{\sigma_r^4 + 2\sigma_r^2(16k\mu_A^2\sigma_z^2)}{128k^2}} \Bigg/ \left(2\sqrt{1 + \sigma_z^2(\mu_A^2 + \sigma_A^2)}\right).$$

For case 2, we use simulation with 10,000 trials for each combination over the range x between 0.2 and 2.0 in steps of 0.2 and σ_z between 1 and 2 in steps of 0.1. These conditions cover situations where Z is a large cause but not strictly dominant ($\sigma_z = 0.8$) to the case where Z is a clear dominant cause. For each run of the simulation, we fit the regression model [3] to the desensitization experiment results and estimate $sd(Y|x)$ using expression [4] with unknown parameters replaced by their estimated values.

In Figure 5, we compare the robustness and desensitization experiments using the ratio P_{rob}/P_{desens}. Because smaller performance values are better, we see that in both plots across the whole range of x and σ_z the desensitization experiment is much

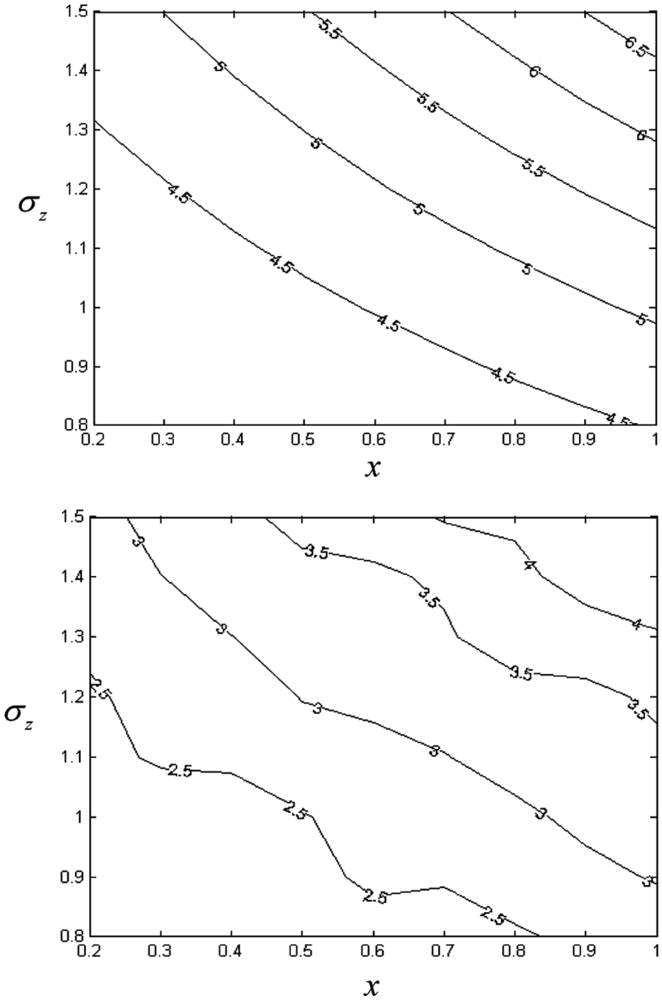

FIGURE 5 Contours of the performance ratio P_{rob}/P_{desens}. Case 1 on left, case 2 on right.

more efficient than the robustness experiment because the ratios are all much bigger than one. Desensitization becomes more beneficial as Z becomes a more dominant cause and when the potential for improvement increases (i.e., as x increases).

Comparison Based on Decisions from the Gear Example Model

Another way to compare desensitization and robustness experiments is based on the decisions made regarding the best combination of control factors from the experimental results. To simulate this in a more complex situation where there are multiple control factors, we use the gear example described in the Introduction. Suppose we assume that the fitted model given in the section on analysis issues represents reality. That is, we assume that we can generate data with the (true) model:

$$\begin{aligned} Y = 14.336 - 1.523x_A - 2.648x_B - 0.992x_C - 0.312x_D \\ + 0.625x_E + 0.422x_F - 0.695x_G - 7.195x_H \\ + 1.297x_Cx_F + 0.922x_Bx_F + 0.859x_Fx_H \\ - 0.844x_Dx_H - 0.93x_Cx_Dx_F + R, \end{aligned} \quad [6]$$

where $R \sim N(0, \sigma_r^2)$ and $\sigma_r = 3.7$ as given by the model fit in the section on analysis issues. Given model [6], and considering x_F, x_G, and x_H as noise factors with variances σ_F^2, σ_G^2, and σ_H^2, respectively, the process variance σ_y^2 is

$$\begin{aligned} \sigma_y^2 = (-7.195 - 0.844x_D)^2\sigma_H^2 + (0.422 + 1.297x_C \\ + 0.922x_B - 0.93x_Cx_D)^2\sigma_F^2 + (-0.695)^2\sigma_G^2 \\ + (0.859)^2\sigma_F^2\sigma_H^2 + \sigma_r^2. \end{aligned} \quad [7]$$

In the simulation study we further assume that the three noise factors x_F, x_G, and x_H are uncorrelated random variables that all have distribution $N(0, 0.5^2)$.

For the desensitization experiment, we include only the dominant cause (H) and the five control factors (A, B, C, D, and E). The experiment is a 2^{5-1} fractional-factorial design for the control array crossed with both levels of the noise factor H. The total number of observations is 32 ($2^{5-1} \times 2$) times the number of replicates.

Table 3 shows σ_y (i.e., square root of Eq. [7]) for all 16 combinations of factors A to E in the 2^{5-1} fractional-factorial design. The smallest output variation (4.9128) is obtained when we have either treatment 5 or 13. So the optimal settings are

A: high or low
B: high
C: low
D: low
E: high or low

Recall that the most important control factor is D because it is the only control factor that interacts with the dominant cause H in model [6].

The robustness experiment is a 2^{5-1} fractional-factorial with the five control factors. We use the same combinations, shown in Table 3, as in the desensitization experiment. The total number of observations is determined based on the number of repeats. To make a fair comparison, for the robustness experiment we use the number of repeats equal to twice the number of replicates used in the desensitization case. This way both experiments have the same total number of observations. For example, if there are two replicates in the desensitization case ($2 \times 2^{5-1} \times 2 = 64$ observations), there are four

TABLE 3 **Treatment Combination Recommended for 1,000 Runs of the Simulation**

							Desensitization			Robustness		
Treatment	A	B	C	D	E	σ_y	32 obs	64 obs	128 obs	32 obs	64 obs	128 obs
1	−1	−1	−1	−1	1	5.079	135	147	135	69	78	86
2	−1	−1	−1	1	−1	5.496	21	6	1	58	47	30
3	−1	−1	1	−1	−1	4.969	96	103	124	60	85	80
4	−1	−1	1	1	1	5.479	15	5	1	54	38	36
5	−1	1	−1	−1	−1	4.913	100	124	106	67	87	89
6	−1	1	−1	1	1	5.500	18	4	0	61	53	50
7	−1	1	1	−1	1	5.209	119	118	110	71	79	91
8	−1	1	1	1	−1	5.545	16	7	2	50	44	25
9	1	−1	−1	−1	−1	5.079	96	108	130	73	70	95
10	1	−1	−1	1	1	5.496	10	4	1	49	41	33
11	1	−1	1	−1	1	4.969	92	115	131	69	86	88
12	1	−1	1	1	−1	5.479	15	7	0	64	41	40
13	1	1	−1	−1	1	4.913	130	104	141	68	85	90
14	1	1	−1	1	−1	5.500	14	7	0	67	59	50
15	1	1	1	−1	−1	5.209	107	130	117	60	69	82
16	1	1	1	1	1	5.545	16	11	1	58	36	33

TABLE 4 **Proportion of the Recommended Settings by Each Method per 1,000 Runs of the Simulation**

		A			B			C			D			E		
Method	Levels	32 obs	64 obs	128 obs	32 obs	64 obs	128 obs	32 obs	64 obs	128 obs	32 obs	64 obs	128 obs	32 obs	64 obs	128 obs
Robustness	H	0.51	0.489	0.512	0.502	0.513	0.51	0.487	0.478	0.475	0.462	0.36	0.298	0.499	0.496	0.507
	L	0.49	0.511	0.488	0.498	0.487	0.49	0.513	0.522	0.525	0.538	0.64	0.702	0.501	0.504	0.493
	Interpretation	H/L	H/L	H/L	H/L	H/L	H/L	H/L	H/L	H/L	H/L	H/L	H/L	H/L	H/L	H/L
Desensitization	H	0.48	0.486	0.521	0.52	0.505	0.477	0.476	0.496	0.486	0.125	0.051	0.006	0.535	0.508	0.52
	L	0.528	0.514	0.479	0.48	0.495	0.523	0.524	0.504	0.514	0.875	0.949	0.994	0.465	0.492	0.48
	Interpretation	H/L	H/L	H/L	H/L	H/L	H/L	H/L	H/L	H/L	Low	Low	Low	H/L	H/L	H/L
Optimum setting			High or Low			High			Low			Low			High or Low	

repeats in the robustness experiment ($4 \times 2^{5-1} = 64$ observations).

For each simulated desensitization experiment, we fit a model relating the output (y) to the six main effects for the control and noise factors and all five two-way interactions between the control and noise factors. From this fitted model we determine which of the 16 treatment combinations used in the experiment is predicted to yield the lowest output standard deviation. For the robustness experiment we select the treatment combination with the lowest sample standard deviation for the output. Each experiment and analysis was simulated 1,000 times.

We summarize the results in Tables 3–5. Table 3 shows the number of times out of the 1000 simulated experiments that each treatment combination was selected for each of the six experiments given by desensitization and robustness experiments with 32, 64 or 128 observations. For instance, for the robustness experiment with 64 observations, Table 3 shows that the optimal treatment combinations #5 or #13 are selected only (87 + 85)=172 times out of 1000 simulation runs. From Table 3 comparing the rows with factor D at the low (optimal) level to rows with D at the high level, we see that the desensitization experiment is much more likely to yield settings that have small values of σ_y compared with a robustness experiment. With a desensitization experiment with 64 observations, by contrast, the low level of factor D is identified as preferred 94.9% of the time and the optimal treatment combinations 5 and 13 are recommended $124 + 104 = 228$ times out of 1000.

Table 4 shows the proportion of times each control factor is set at its low or high levels. For instance, for the robustness experiment with 64 observations, Table 4 shows that factor D is selected at its low level 64% of the time. We see that the desensitization experiment is much more likely than the robustness experiment to recommend the optimal low level for the most important control factor D. With factors B and C that have a much smaller effect that D, both desensitization and robustness experiments recommend low and high levels with roughly equal proportion. The row labelled "interpretation" summarizes whether we are likely to recommend the high or low level of the control factor. The label "H/L" suggests both levels are roughly equally common recommendations.

Table 5 summarizes the results from Table 3 numerically by calculating the average and standard deviation of the true σ_y values for the proposed

TABLE 5 **Summarizes of the Calculated Performance Measures**

	Robustness		Desensitization	
Number of observations	σ_y Mean	σ_y Standard deviation	σ_y Mean	σ_y Standard deviation
32	5.26	0.245	5.10	0.186
64	5.20	0.243	5.07	0.150
128	5.18	0.231	5.04	0.116

treatment combinations. Because there are only 16 treatments to choose from and only eight unique σ_y values, the distribution of the realized σ_y values is discrete. Note that the best method will yield the lowest average and the least variation in σ_y . The results in Table 5 show that the desensitization experiment has a lower σ_y average and thus higher efficiency than the robustness experiment regardless of the number of observations.

DISCUSSION AND CONCLUSIONS

We have shown qualitatively and quantified, using some examples, the superiority of the desensitization experiment over the robustness experiment when the goal is to reduce variation in an existing process. Because a desensitization experiment is only possible with a known (and controllable for the experiment) dominant cause, this conclusion provides further evidence that when applying robust parameter design to improve an existing process we should first try to find dominant causes of the output variation.

As discussed in Steiner and MacKay (2005), a dominant cause can be found using observational studies and the method of elimination. Using only engineering knowledge and tools such as cause-and-effect diagrams to determine which noise factors to include in the desensitization experiment is fraught with difficulty. Including any unimportant cause greatly increases the complexity of the Taguchi experiment because it will require more observations and holding an additional noise factor fixed during the experiment.

Inclusion of an unimportant cause will not increase the power of the experiment to find a better way to run the process. Even more critically, failing to include the dominant cause as one of the noise factors relegates the experiment to failure.

Using prior information to help plan the Taguchi experiment allows process improvement teams to connect stages of process improvement algorithms, such as DMAIC in Six Sigma (Breyfogle 1999) together. This makes sense because clearly when using DMAIC what we learn in the analysis stage should be useful in the improve stage.

Robustness experiments have some qualitative advantages over desensitization experiments as summarized previously. However, as we have suggested with our examples, robustness experiments are inefficient and typically require many repeats to find better process settings. We conclude that robustness experiments should be considered only when we are unable to find the dominant cause or when the dominant cause cannot be controlled during the experiment.

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