The Role of Dominant Cause in Variation Reduction through Robust Parameter Design

by

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A thesis
presented to the University of Waterloo
in fulfillment of the
thesis requirement for the degree of
Master of Applied Science
in
Systems Design Engineering

Waterloo, Ontario, Canada, 2008

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Declaration

I hereby declare that I am the sole author of this thesis. This is a true copy of the thesis, including any required final revisions, as accepted by my examiners. I understand that my thesis may be made electronically available to the public.

Hossein Asilahijani

Abstract

Reducing variation in key product features is a very important goal in process improvement. Finding and trying to control the cause(s) of variation is one way to reduce variability, but is not cost effective or even possible in some situations. In such cases, Robust Parameter Design (RPD) is an alternative.

The goal in RPD is to reduce variation by reducing the sensitivity of the process to the sources of variation, rather than controlling these sources directly. That is, the goal is to find levels of the control inputs that minimize the output variation imposed on the process via the noise variables (causes). In the literature, a variety of experimental plans have been proposed for RPD, including Robustness, Desensitization and Taguchi's method. In this thesis, the efficiency of the alternative plans is compared in the situation where the most important source of variation, called the "Dominant Cause", is known. It is shown that desensitization is the most appropriate approach for applying the RPD method to an existing process.

Acknowledgments

I would like to extend my sincere gratitude to my supervisors, Professor Keith W. Hipel and Professor Stefan Steiner, for their guidance, patience, encouragement, and support throughout my study at the University of Waterloo, which led to this thesis. Without their support, both intellectual and financial, I would not have been able to accomplish this work and it is my great honor to work under their supervision and have my self-confidence strengthened through this experience. My thanks also go to my readers, Professor R. Jock MacKay and Professor G. J. Savage for their constructive comments in the final stages of the work. They have been abundantly helpful to me in numerous ways.

I cannot end without thanking my parents and my wife for their constant encouragement and love, on which I have relied throughout the period of this work.

Dedication

This is dedicated to Fahimeh, my beloved wife.

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Chapter 1

Motivation and Objectives

1.1 Motivation

Reducing variation in critical outputs is a very important goal in process improvement and, nowadays, a primary objective of engineering work in many firms is the continuous and systematic reduction of variability in key product features. Finding and trying to control the cause(s) of variation is one way to reduce variability, but in situations where it is not cost effective or even possible *Robust Parameter Design (RPD)* is an alternative.

The goal of RPD is to reduce variation by reducing sensitivity to the sources of variation rather than controlling these sources directly. Robust parameter design problems may arise in all three stages of the product development cycle: product design, process design and manufacturing. Designing a product that is robust against changes in environmental factors, product deterioration, and manufacturing imperfections illustrates the application of RPD in the product design stage. Identifying the settings of process variables so as to reduce variation in an output characteristic is an example of using RPD in the process design stage, when we talk about creating a new process, or manufacturing stage, when we are concerned about an existing process.

Despite Taguchi's suggestion that countermeasures against variation caused by environmental variables and product deterioration are best built into the product at the product design stage (Taguchi, 1987 and Kackar and Phadke, 1981), reviewing case studies given in ASI (1985 and 1986) reveals that Taguchi's method to RPD problems or a specific version of it, called "Robustness" in this thesis, are mostly used in the manufacturing stage. In the manufacturing stage, unlike product or process design stages, the main source(s) of variation can in many cases be identified by observing the existing process before trying to make the process robust. This is an important issue that can affect the efficiency of an

employed experiment and has not received much attention in the different approaches to RPD problem, including Taguchi's approach. Indeed, the vital role of dominant cause in RPD motivated us to explore the efficiency and effectiveness of robustness or Taguchi's approach to RPD when it is considered as a tool in process improvement.

In this thesis, "Desensitization" is presented as an alternative to the robustness/Taguchi method and as the most appropriate approach to deal with RPD problems at the manufacturing stage. The efficiency of desensitization is examined and compared with the robustness and Taguchi's approaches to the RPD in the situation where a dominant cause of output variation exists and can be found.

1.2 Objectives

The main objective of this thesis is to determine the role of dominant cause in variation reduction through robust parameter design. To do this, we identify desensitization as an appropriate approach to RPD at the manufacturing stage and compare its efficiency and effectiveness with the robustness and Taguchi's approaches in similar situations. The following are some specific goals:

- To review the RPD problem literature and present a precise explanation of dominant cause and a brief description of different approaches to this problem
- To compare the three RPD approaches qualitatively
- To present a quantitative comparison of desensitization and robustness by introducing and formulating a performance index
- To reassess and generalize the presented theoretical results by simulating different possible situations
- To apply and compare the performance measure of each discussed approach to a real-world RPD problem using a simulation study
- To present situations where desensitization is the recommended approach and the cases in which desensitization is not appropriate.

1.3 Overview of the Thesis

This chapter presents the motivation and the main objectives of the thesis. Chapter 2 presents a brief overview of RPD concepts and reviews some of the approaches to variation reduction and the robust parameter design problem. In addition, the definition of dominant cause in variation reduction literature is

discussed in detail. Next, Chapter 3 explains three different experimental plans for finding a robust solution and also describes their methods of analysis.

Chapter 4 focuses on the comparison of the aforementioned experimental plans and provides a qualitative and quantitative evaluation of them in terms of efficiency. Chapter 4 begins with a qualitative comparison of robustness/Taguchi and desensitization experiments. Next, it introduces a performance measure used for quantifying the efficiency of each method and for a quantitative comparison. Subsequently, the results of the conducted simulations, that reassess and generalize theoretical results, are presented.

To demonstrate the effectiveness of a desensitization experiment in comparison to a robustness/Taguchi experiment, a real-world RPD problem from automotive manufacturing industry is studied in Chapter 5. Chapter 6 then, describes the conditions needed for the desensitization method to be implemented successfully and also the conditions under which desensitization is not an appropriate approach. Finally, conclusions and some possible directions for future research are given in Chapter 7. Figure 1.1 summarizes the organization of the thesis.

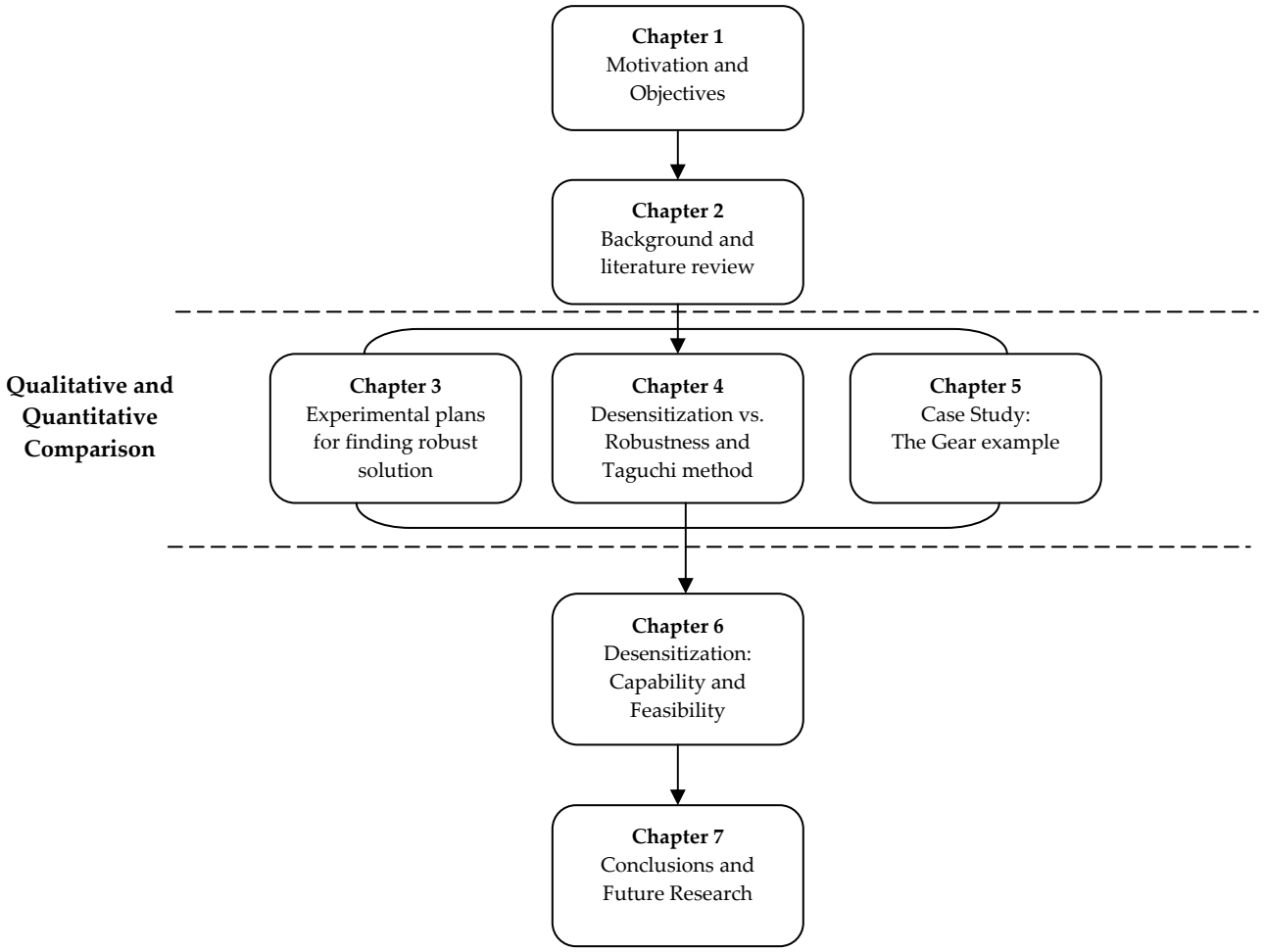

Figure 1.1: Contents of this Thesis

Chapter 2

Background and Literature Review of Robust Parameter Design (RPD)

2.1 A Brief Historical Perspective of RPD

Robust Parameter Design (RPD) problems are not new and RPD has a considerable history. Box (in Nair, 1992) points out that in the early part of last century Gossett, who studied the barley used by the Guinness brewery, emphasized that experiments had to be run in different areas of Ireland so as to

find barely varieties that were insensitive to particular local environments (Gossett, 1986). Later, in the 1930s, Sir R. A. Fisher introduced modern experimental design. Fisher's pioneering work and the notable contributions by F. Yates and D. J. Finney, motivated by problems in agriculture and biology, in addition to agricultural studies in the early 1940s where their goal was to develop agricultural products whose yield was robust to the weather and soil conditions, formed the foundation for RPD (Wu and Hamada, 2000).

Using statistical methods and experimental design to solve problems was not confined to agriculture and biology. Particularly, after World War II process industries, such as chemical or food industries, tried to take advantage of statistical techniques. The food industry, for instance, has for decades used design of experiments and robust parameter design to produce goods that are insensitive to deviations by the user from the instructions on the box.

Seeking the ability to make many parts with few defects placed emphasis on variation reduction in manufacturing and inspired new methods in experimental design throughout the last few decades. The continuous and systematic reduction of variability in key product features became a chief goal of quality and process improvement. In the 1950s Genichi Taguchi, a Japanese quality consultant, developed some novel concepts and techniques for the planning and

analysis of robust parameter design experiments and advocated the use of parameter design to make a system less sensitive to variation, which is hard to control during normal operation of a given system. He introduced and popularized his approach to RPD in the USA in the mid 1980s (Taguchi, 1987; Taguchi and Wu, 1980). Extensive interest among engineers and statisticians was generated by his new philosophy and during the 1980s his methodology was used at many large corporations in the USA (ASI, 1985 and 1986). His approach also generated controversy and debate in the statistical and engineering communities (see Nair, 1992 for a summary of some debates) and consequently a period of research and development on new approaches to the RPD started and is still ongoing.

2.2 RPD: Basic Concepts and Definitions

The concepts of RPD need to be clarified in the context of variation reduction since RPD is one particular approach to variation reduction. The International Organization for Standardization defines the word "quality" as "degree to which a set of inherent characteristic fulfills requirements". Considering this definition, variation reduction is embraced as a primary means of improving product

quality. Excessive variation in critical output characteristics affects product quality and leads to poor performance, low customer satisfaction, scrap, rework and eventually low production productivity. Reducing variation in critical outputs is a very important goal in process improvement. A primary goal of engineering efforts in many firms today is the continuous and systematic reduction of variability in key product features.

Reviewing many variation reduction algorithms including the Shainin System (Shainin, 1992, 1993), DMAIC or Six Sigma (Harry and Schroeder, 2000), Scholtes algorithm (1988) and Statistical Engineering (Steiner and MacKay, 2005), indicates *diagnostic* and *remedial journeys* (see Figure 2.1), described by Juran and Gryna (1980) and Juran (1988), as the common element of these algorithms. During the diagnostic phase, the problem of process is investigated by examining its symptoms in order to find the causes of the problem. In the second phase, the remedial journey, we search for a solution. The idea is that if we know the cause of the problem, we are more likely to find efficient and effective solutions.

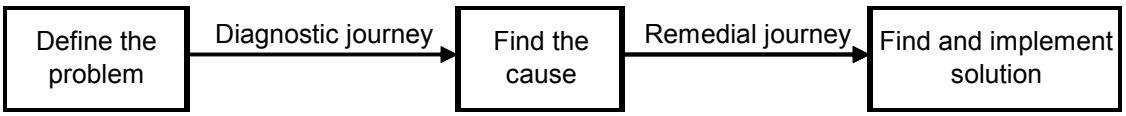

Figure 2.1: Common elements of well-known variation reduction algorithms

The inputs that operate on a system can be divided into two broad types (Wu and Hamada, 2000; Steiner and MacKay, 2005): varying inputs and fixed inputs. Varying inputs are process characteristics whose values change (unit to unit or time to time) in a process without deliberate intervention. Examples include: operators, pouring temperature, raw material characteristics and so forth. Fixed inputs, on the other hand, are a process inputs/characteristics whose values can be adjusted, but remain fixed once they are chosen. These are parameters/factors that can be easily controlled and manipulate in a system's normal production. For example, the product design and the target pouring temperature are fixed inputs.

A cause of variation in process output is a *varying* input with the property that if all other inputs were held constant, then the output changes when the input changes. Note that although changing the level of a *fixed* input can be a solution for excessive variation in the output, a fixed input can not be a cause of variation in a process output (Steiner and MacKay, 2005). The design of the product, for example, can not be a cause of variation since it is a fixed input; however, changing the design of the product (i.e. changing a fixed input) can be a solution to reduce output variation. In the process improvement literature, varying and

fixed inputs are also known as *noise* and *control* factors respectively (Wu and Hamada, 2000).

For any process there are a large number of causes, each with an effect. Applying the Pareto principle to the cause of variation, large effects can be attributable to only a few causes and these are called dominant causes (Steiner et al., 2007). A dominant cause(s) is varying input that has a large effect on the output with a relatively small change in its value. Juran and Gryna (1980, p. 105) define a dominant cause as "a major contributor to the existence of defects, and one which must be remedied before there can be an adequate solution". Consider the following simple model which describes relationship between a dominant cause (X) and an output (Y) (note that X and other varying inputs are assumed to be independent in this model). We can say the variable X is a dominant cause of output variation if standard deviation in the output due to X is large relative to the standard deviation due to rest of the causes (Steiner and MacKay, 2005).

$$Y = f(X) + \text{noise}$$

$$\text{sd}(Y) = \sqrt{\text{sd}(\text{due to } X)^2 + \text{sd}(\text{due to all other varying inputs})^2} \qquad (2.1)$$

In the Shainin System, the dominant cause is called the Red X™ and there is recognition that there may be a second or third large cause, called Pink X™ and Pale Pink X™ respectively (Steiner et al., 2007). Throughout this thesis we assume that a dominant cause(s) of variation in a process output exists. The emphasis on a dominant cause is justified because the effect of dominant cause on the overall output variation is magnified since the overall output variation is calculated as the square root of the sum of squares (Steiner et al., 2007). In other words, for any process output there are likely a large number of causes and if the effects of theses causes are independent and additive, the standard deviation of output that defines the variation can be decomposed as:

$$\text{stdev(output)} = \sqrt{(\text{stdev due to cause1})^2 + (\text{stdev due to cause2})^2 + \cdots}$$

From this equation we can conclude that by reducing the effect of a single cause the standard deviation of output (which defines the problem) can be substantially decreased only if that cause has a large effect (i.e. that cause is dominant cause). To simplify the language, we refer to a (single) dominant cause of variation, while recognizing that there may be more than one important cause. Finding a dominant cause of variation in an output characteristic and trying to control and reduce its variation is one way to reduce variation. In some instances, however, the dominant cause may be difficult, expensive or even

impossible to control in a system's normal production or usage condition (i.e. reducing variation in dominant cause is not a cost effective solution). In these cases, finding some fixed input and identifying new settings for them which will make the process output less sensitive to changes in the dominant cause is a possible solution (Steiner and MacKay, 2005). This idea is known as *Robust Parameter Design (RPD)* or simply *Parameter Design* which was popularized and introduced in the United States in the 1980s by the Japanese engineer, Genichi Taguchi, (Taguchi, 1987; Ross, 1988; Taguchi and Wu, 1980; Kackar, 1985). The term parameter design comes from an engineering tradition of referring to product characteristics as product parameters (Taguchi and Wu, 1980). Parameter design works by identifying appropriate settings of some fixed inputs to exploit interactions between the fixed inputs and the dominant cause to reduce the variation in the output without the necessity of reducing the variation in dominant cause. See Section 4.3 for a more detailed discussion of how this works. Parameter design can be used either to build quality into new products/processes (product/process design stage) or to improve the quality of existing ones (manufacturing stage) (Nair, 1992; Kackar, 1985). Our focus in this thesis is on the application of the robust parameter design in process improvement (manufacturing stage) and specifically in minimizing the output

variation around a target value in an existing process. As mentioned in last section, much attention has been given to the experimental design efforts and data analysis methods of the Taguchi approach in the 1980s and the 1990s. However, the effect of the knowledge of a dominant cause on the efficiency of the experiment for finding a robust solution has been not considered properly. A specific purpose of this thesis is to compare the efficiency of conducting an experiment to search for favorable interactions between control and noise factors in two situations: first when the dominant cause is known and second when the dominant cause is unknown. The former situation will be called *Desensitization* and the latter will be called *Robustness* in the thesis.

Comparing desensitization and robustness reveals how knowledge of a dominant cause can play an important role in process improvement.

2.3 Summary

In this chapter, some RPD concepts that are related to the discussion in the forthcoming chapters were reviewed. After a brief review of variation reduction context, we focused on the differences between varying and fixed inputs and their role in an output variation. Explicit definition of dominant cause was

discussed and the effect of a dominant cause on output variation was reviewed. In the next chapter three different experimental plans for finding a robust solution and their methods of analysis are discussed in detail.

Chapter 3

Experimental Plans for Finding a Robust Solution

3.1 Introduction

The goal in robust parameter design is to find new levels for fixed inputs that reduce the output variation. Since the value of a fixed input doesn't normally change in the process, an experiment needs to be conducted in which we assign

different levels to the selected fixed inputs and we examine the effect of those new settings on the output mean and variation. The goal of such an experiment is to find and exploit a favorable interaction between the selected fixed inputs (or *candidates)* and the dominant cause that makes process output less sensitive to uncontrollable changes in the dominant cause. In practice, process analysts have used at least three different types of experiments to find robust process settings. The first approach, called a desensitization experiment is useful within the Statistical Engineering algorithm as by Steiner and MacKay (2005). In the Statistical Engineering algorithm we first look for a dominant cause using observational studies and then run a desensitization experiment in which we also deliberately control the levels of the identified dominant cause. The second approach is to conduct a so called robustness experiment involving selected fixed inputs only. For the third option, an experiment is run with selected fixed inputs and a range of varying inputs that the experimenter believes are likely to be important causes. We call the third option a Taguchi experiment, although option #2 is also sometimes called a Taguchi experiment. Desensitization, robustness and Taguchi style experiments are described in the next sections as the three major experimental plans for finding a robust solution.

3.2 Robustness Experiment

3.2.1 Plan

Robustness is a variation reduction approach which tries to find new settings for the fixed inputs that make the output less sensitive to variation in the unknown dominant cause (Steiner and MacKay, 2005). In a robustness experiment a group of fixed inputs (called candidates) are selected based on engineering judgment and their effects on the output variation are examined. The experiment can be a full factorial or fractional factorial design. Once the candidates are identified, they will be systematically changed in the robustness experiment and a performance measure (usually the standard deviation of the output) will be recorded for each run. Whenever it is possible, randomization and replication should be used in robustness to improve precision of the experiment. Since knowledge of the dominant cause is not available, the length of experiment, the number of runs, the number of repeats in each run, and candidates are determined only based on engineering knowledge and the past experience of experimenters/analysts.

A famous positive example of an application of a robustness experiment is a case study reported by Quinlan (1985) on speedometer cables. Shrinkage in the plastic casing material can sometimes make speedometer cables noisy. So a project was initiated to reduce variation in postextrusion shrinkage of the casing for the speedometer cable. When the team's efforts to find the cause of the shrinkage variation failed, they chose 15 fixed inputs and selected one new level for each. They then ran a two-level (one level of each candidate was the existing level) experiment with 16 runs (i.e. a 2_{III}^{15-11} fractional design). For each run 3000 feet of plastic casing were produced. Four samples were haphazardly cut out from each run and the percentage shrinkage measured on each specimen. Then, a performance measure (standard deviation of percentage shrinkage) was calculated (for each run) using the four sample values. Finally, the best combination of levels to reduce the variation was found. The new levels were confirmed and the process was improved.

As illustrated by the Speedometer Cable example, the robustness approach can be successful; however, there are some substantial drawbacks. To limit interference with regular production the robustness experiment is usually run over a short time (ASI, 1985; ASI, 1986). As a consequence there is a risk of running a high-cost experiment with no return, since if the dominant cause dose

not acts with each run of the experiment and/or if the candidates (selected fixed inputs) do not include the one(s) that have interaction with dominant cause the robustness experiment will fail. We conclude that to have any hope of success in a robustness experiment the unknown dominant cause should act in the short-term family of variation (part-to-part for example). Otherwise the run lengths need to be very long to allow the dominant cause time to act during the experiment. If the dominant cause does not act within each run, it will not be possible to find a favorable cause/candidate interaction even if one exists. Moreover, in robustness experiments fixed inputs are selected based only on the engineering knowledge whereas in desensitization experiments the engineering judgment is supplemented by the knowledge of the dominant cause. The more you know about the cause of variation, the greater the chance you will select fixed inputs to change that will reduce variation in the output.

Considering these drawbacks and the fact that once a dominant cause is identified, in some instances, the remedy is obvious and no further investigations are needed, Steiner and Mackay (2005) recommend first finding the dominant cause of variation and then if the dominant cause can not be addressed directly, running a desensitization experiment.

3.2.2 Analysis

To illustrate the method of analysis in the robustness approach, an example of the application of robustness in process improvement (reported in Steiner and MacKay, 2005) is reconsidered.

In the painting department of an automotive manufacturing plant, excessive variation in film build (paint thickness) is observed from vehicle to vehicle in particular locations. As a consequence, to meet the minimum film build specification, the process center is kept well above the lower specification. However, running the process above target results in high paint usage and occasionally creates visual defects called “runs”. To solve the problem, the paint shop management decided to set up a project to reduce the standard deviation of film build from 0.67 thousandths of an inch, the baseline standard deviation, to 0.35 thousandths of an inch. The team’s efforts to find the dominant cause failed and they decided to adopt the process robustness approach. Based on process experience, candidates and their corresponding levels were chosen as follows:

Table 3.1: Selected fixed inputs and their levels in the Film Build experiment

Candidate	Low level	High level
Anode dimension	3.1	3.9
Conductivity of paint	Low	High
Temperature	30	50
Zone X voltage	450	475
Zone Z voltage	500	525

The team selected a fractional factorial resolution V experiment with the 16 runs given as Table 3.2. The order of runs was randomized.

Table 3.2: Experimental plan for the Film Build experiment

Treatment	Anode dimension	Conductivity	Temperature	X voltage	Z voltage
1	3.1	Low	30	450	500
2	3.9	Low	30	450	525
3	3.1	High	30	450	525
4	3.9	High	30	450	500
5	3.1	Low	50	450	525
6	3.9	Low	50	450	500
7	3.1	High	50	450	500
8	3.9	High	50	450	525
9	3.1	Low	30	475	525
10	3.9	Low	30	475	500
11	3.1	High	30	475	500
12	3.9	High	30	475	525
13	3.1	Low	50	475	500
14	3.9	Low	50	475	525
15	3.1	High	50	475	525
16	3.9	High	50	475	500

For each run, five panels were painted and film build was measured at five locations on each panel. The data are given in the following table (Table 3.3).

Table 3.3: Treatments and results for the Film Build experiment

Treatment	Order	Film build	Average	Log(s)
1	14	15.6, 15.3, 15.9, 15.2, 15.8	15.56	–0.51
2	5	16.0, 16.3, 17.3, 16.2, 16.6	16.47	–0.31
3	6	15.0, 14.8, 14.9, 15.3, 16.1	15.22	–0.28
4	2	16.1, 17.6, 17.2, 16.3, 16.1	16.69	–0.16
5	9	15.7, 15.6, 15.2, 15.2, 15.7	15.49	–0.57
6	12	17.3, 17.6, 16.8, 17.5, 17.3	17.28	–0.49
7	13	16.2, 14.4, 15.4, 14.5, 15.9	15.3	–0.09
8	4	17.3, 16.6, 16.6, 16.4, 17.8	16.94	–0.25
9	7	16.1, 14.7, 16.2, 14.7, 16.2	15.59	–0.09
10	16	17.2, 15.8, 16.4, 16.0, 15.8	16.23	–0.24
11	15	15.4, 15.2, 15.4, 15.3, 15.2	15.29	–1.06
12	1	16.6, 16.4, 16.4, 16.5, 16.4	16.48	–1.00
13	3	15.1, 15.4, 15.4, 15.0, 14.4	15.05	–0.41
14	10	16.8, 16.9, 17.0, 17.3, 16.3	16.89	–0.42
15	11	15.0, 15.1, 15.0, 14.9, 14.8	14.97	–0.86
16	8	16.6, 16.7, 16.3, 16.5, 16.3	16.48	–0.79

As a first step in the analysis, the performance measure (log within run standard deviation) is calculated across repeats for each run. Then, a full model and a

Pareto chart are used to analyze the performance measure looking for large main effects and important interaction effects.

The Pareto chart of the effects (Figure 3.1), shows there are large main effects due to conductivity and zone *X* voltage, and large interactions between conductivity and zone *X* voltage and between conductivity and temperature.

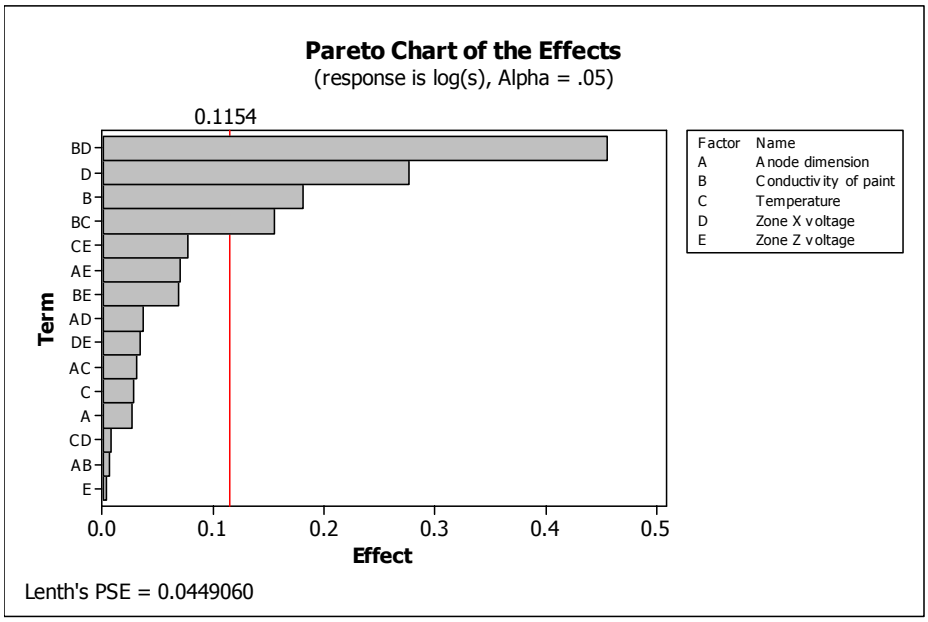

Figure 3.1: Pareto analysis of effects in the Film Build experiment

The main effect and interaction plots (Figures 3.2 and 3.3) are used to draw conclusions. A regression model can also be used to model log(s) as a function of the important effects and then the levels of candidates (fixed inputs) that minimize this function are suggested as the robust solution. Wu and Hamada (2000), call this method of analysis the dispersion model approach. We will use

this model building method in next chapter to analyze the results of different experiments and to compare their efficiency.

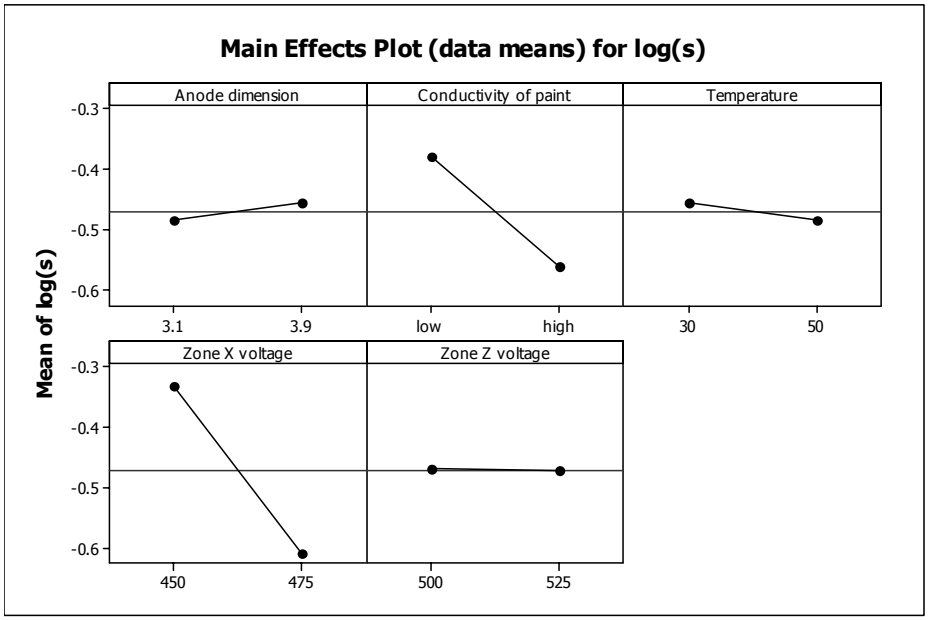

Figure 3.2: Main effects for the Film Build experiment

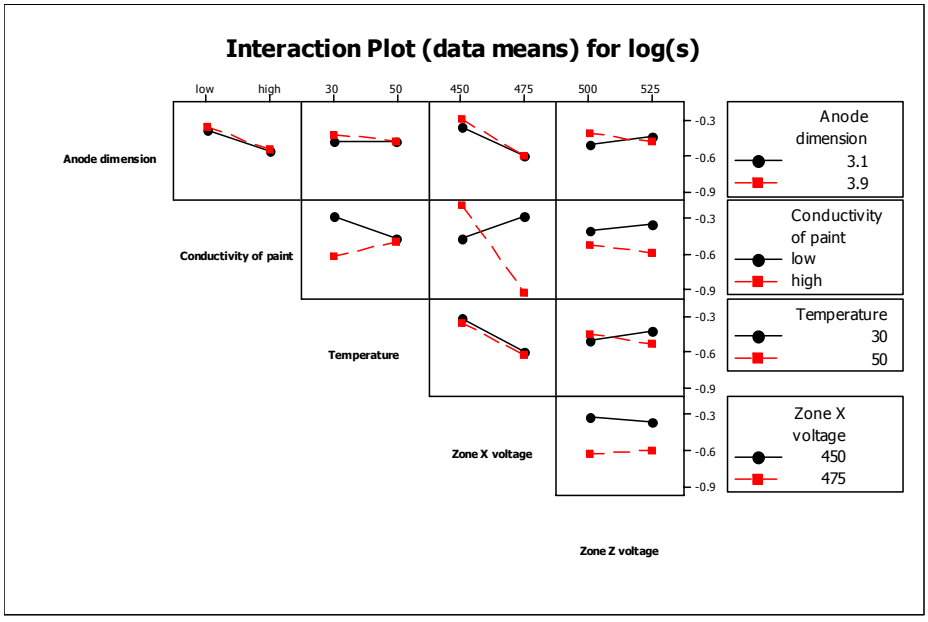

Figure 3.3: Interaction plot for the Film Build example

From the interaction plots, the combination of high zone *X* voltage, high conductivity, and low temperature is best. Note that smaller log(s) is better. The project was a success. Running the process with these new settings reduced the baseline standard deviation of film thickness from 0.67 to 0.37. This allowed the team to reduce the target film build and save a substantial amount of paint.

3.3 Taguchi Method Experiment

3.3.1 Plan

We now consider the second experimental approach, a Taguchi experiment. Fractional factorial designs or orthogonal arrays are often employed in conducting the experiment. Taguchi recommends a *crossed array* design for planning the experiment (Wu and Hamada, 2000). The *Inner-outer array* is a key concept in a crossed design or Taguchi's approach to robust parameter design. In this approach a two-part experimental design is recommended. The Outer array (noise array) sets the levels of varying inputs while the inner array (control array) defines the treatments in terms of the levels of fixed inputs (Nair, 1992). Usually a 2^k or 2^{k-p} experiment is used for the inner array and a full factorial experiment is

used for the outer array (Ross, 1988; Montgomery, 2001). Randomization, replication and blocking should also here be considered.

Each treatment combination in the control (inner) array is crossed with all level combinations in the noise (outer) array (Figure 3.4). Shoemaker et al. (1991) call this setup a product array since the outer array is run for every row in the control array.

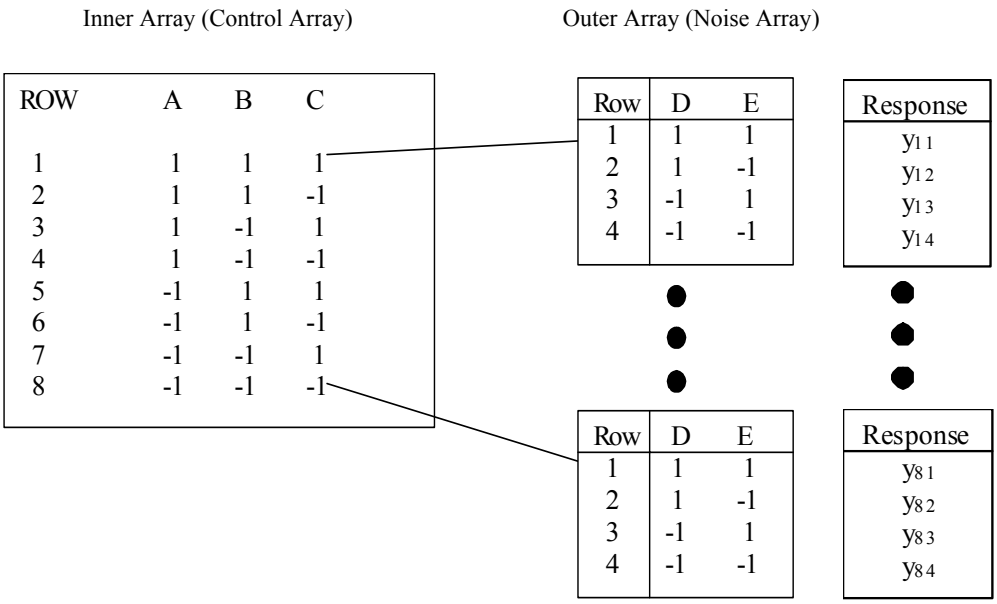

Figure 3.4: Product array in the Taguchi method for Robust Design

To define some notation, let y_{ij} be the observed response when the inner array is at its i^{th} treatment combination and the outer array is at its j^{th} treatment combination. Then, assuming there are "a" treatments in the inner array and "b"

treatments in the outer array the typical data for Taguchi experiment with a product array design will appear as in Table 3.4.

Table 3.4: General arrangement for a Taguchi experiment – product array

		treatment combinations of outer array			
		1	2	…	b
treatment combinations of inner array	1	y_{11}	y_{12}		y_{1b}
	2	y_{21}	y_{22}		y_{2b}
	⋮				
	a	y_{a1}	y_{a2}		y_{ab}

For each inner array treatment then, $\bar{y}_{i.}$ and s_i can be defined as:

$$\bar{y}_{i.} = \frac{\sum_{j=1}^{b} y_{ij}}{b} \qquad i = 1,2,...,a \qquad (3.1)$$

$$s_i = \sqrt{\frac{\sum_{j=1}^{b} (y_{ij} - \bar{y})^2}{b-1}} \qquad i = 1,2,...,a \qquad (3.2)$$

So, unlike robustness we now deliberately manipulate or control some noise factors. Since noise factors or varying inputs are usually hard to control in the normal process, running a Taguchi experiment may be difficult or impossible. This illustrates one of the disadvantages of a Taguchi experiment compared to a desensitization experiment, discussed in Section 3.4, in which noise factors are limited to one or two dominant causes. In this type of experiment once the noise factors (varying inputs) are selected, they should be systematically varied to reflect their variation in normal condition. So, the levels of noise factors are fixed during the experiment.

3.3.2 Analysis

Identifying optimal parameter settings in a Taguchi experiment requires specifying a criterion that is to be optimized. Taguchi suggests combining the mean and the variance, for each inner array treatment, into a single performance measure known as the signal-to-noise ratio (Kackar, 1985).

To derive conclusions, Taguchi recommends analyzing the mean response for each run in the inner array and also analyzing variation using an appropriate signal-to-noise ratio. Signal-to-noise ratios are derived from the quadratic loss function, and three of them are considered to be "standard" and widely

applicable (Montgomery, 2001; Wu et al., 2000). The goal of quality improvement can be stated as attempting to *maximize* the signal-to-noise (S/N) ratio. Considering Table 3.4 the signal to noise ratio is calculated for each i as: follows:

1. Nominal is best: i.e. you ideally want all output values to be equal to a target value

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

Where $\bar{y}$ and s^2 are defined by Equations (3.1) and (3.2) respectively. This signal-to-noise ratio is applicable whenever there is a target value and a two side specification. For example, the size of piston rings for an automobile engine must within the lower and upper limits and ideally close to a target to ensure product's high quality.

2. Larger the better: i.e. you want to maximize the output characteristics, e.g. breaking strength

$$S/N_L = -10\log\left(\frac{1}{b}\sum_{j=1}^{b}\frac{1}{y_{ij}^2}\right)$$

Where b is the number of observations at each treatment.

3. Smaller the better: i.e. you want to minimize the output characteristics, e.g. out of roundness

$$S/N_S = -10\log\left(\frac{1}{b}\sum_{j=1}^{b} y_{ij}^2\right)$$

Taguchi's methods of using the S/Ns in the analysis are detailed in Taguchi (1987) and Wu & Hamada (2001). To illustrate, a case study, originally reported by Miller et al. (1993), is considered. We will use this example later to compare Taguchi, desensitization and robustness approaches in a simulation study.

In automotive manufacturing, the drive pinion and gear "set" provides the transmission of power from the vehicle drive shaft to the rear axle. The parts are heat-treated to improve strength and wear characteristics. A quality problem arose from part distortion during heat-treatment, and a Taguchi style experiment was conducted in the attempt to find a way to improve the process. The five control factors (A-E) and three noise factors (F-H) are given in Table 3.5.

Table 3.5: The control and noise factors for the Gear experiment

Control Factors		Noise Factors	
A	carbon potential	F	furnace track
B	operating mode	G	tooth size
C	last zone temperature	H	part position
D	quench oil temperature		
E	quench oil agitation		

The design matrix and response data are given in Table 3.6. The response is the dishing of the gear. Two levels were considered for each of the factors. A 2^{5-1} fractional factorial design was used for the inner (control) array and a 2^3 full factorial design was used for the outer (noise) array. There are 16×8=128 runs in total. The purpose of experiment was to find a way to run the process that has less gear dishing variation around a target value.

Table 3.6: Design matrix and response data for the Gear experiment

						Outer Array									
					F	1	1	1	1	-1	-1	-1	-1		
					G	1	1	-1	-1	1	1	-1	-1		
					H	1	-1	1	-1	1	-1	1	-1		
	Inner Array														
Run	A	B	C	D	E									Y bar	S/N_T
1	1	1	1	1	1	7	12	6.5	14	3	14	4	16.5	9.625	5.4856
2	1	1	1	-1	-1	13.5	14.5	5.5	17	-7.5	15	-4.5	12	8.1875	-1.2167
3	1	1	-1	1	-1	3	11	5.5	18	3	19	1	21	10.188	1.9288
4	1	1	-1	-1	1	10.5	14.5	6.5	17.5	3	14.5	9	24	12.438	5.4641
5	1	-1	1	1	-1	10	23	3.5	23	4.5	25.5	10	21	15.063	4.4752
6	1	-1	1	-1	1	6.5	22	14.5	23	5.5	18.5	8	21.5	14.938	6.1476
7	1	-1	-1	1	1	5.5	28	7.5	28	4	27.5	10.5	30	17.625	3.5878
8	1	-1	-1	-1	-1	4	14	6.5	23	9	25.5	9	24.5	14.438	4.4127
9	-1	1	1	1	-1	-4	18.5	11.5	26	-0.5	13	0	16.5	10.125	-0.4057
10	-1	1	1	-1	1	9	19	17.5	21	0.5	20	6.5	18	13.938	5.2955
11	-1	1	-1	1	1	17.5	20	10	23	6.5	21.5	0	26	15.563	4.6716
12	-1	1	-1	-1	-1	7	23.5	1	20	7	22.5	4	22.5	13.438	2.9881
13	-1	-1	1	1	1	2.5	22	12	19.5	7	27.5	8.5	23.5	15.313	4.6048
14	-1	-1	1	-1	-1	24	26	14.5	27.5	7	22.5	13	22	19.563	8.6539
15	-1	-1	-1	1	-1	5.5	27	2.5	31	12.5	27	11.5	32.5	18.688	3.854
16	-1	-1	-1	-1	1	11	21.5	12	27	16.5	29.5	16	28.5	20.25	8.708

As the objective was to reduce the variation of response around a target value (nominal the best), S/N_T is used by experimenters. The last two columns of Table 3.6 contain $\bar{y}$ and S/N_T values for each of the 16 inner-array runs.

One approach to the analysis of this experiment is based on the "play the winners" rule. With this analysis we look for the treatment combination(s) that maximizes S/N_T. As can be seen in Table 3.6 the last treatment combination maximizes the signal-to-noise ratio and setting factors A, B, C, D to their low levels and E to its high level is the recommended solution based upon this rule. An alternative analysis involves using analysis of variance (Montgomery, 2001) or the half-normal and main effect plots (Wu and Hamada, 2000) to determine the main factors that influence the signal-to-noise ratio. For the Gear experiment, Table 3.7 and Figure 3.5 show that operating mode (B) and quench oil agitation (E) are marginally significant control factors.

Table 3.7: Estimated effects and coefficients for S/NT in the Gear experiment

Term	Effect	Coef	SE Coef	T	P
Constant		4.291	0.5230	8.20	0.000
A	-1.011	-0.505	0.5230	-0.97	0.357
B	-2.529	-1.265	0.5230	-2.42	0.036
C	-0.322	-0.161	0.5230	-0.31	0.765
D	-1.531	-0.766	0.5230	-1.46	0.174
E	2.409	1.205	0.5230	2.30	0.044

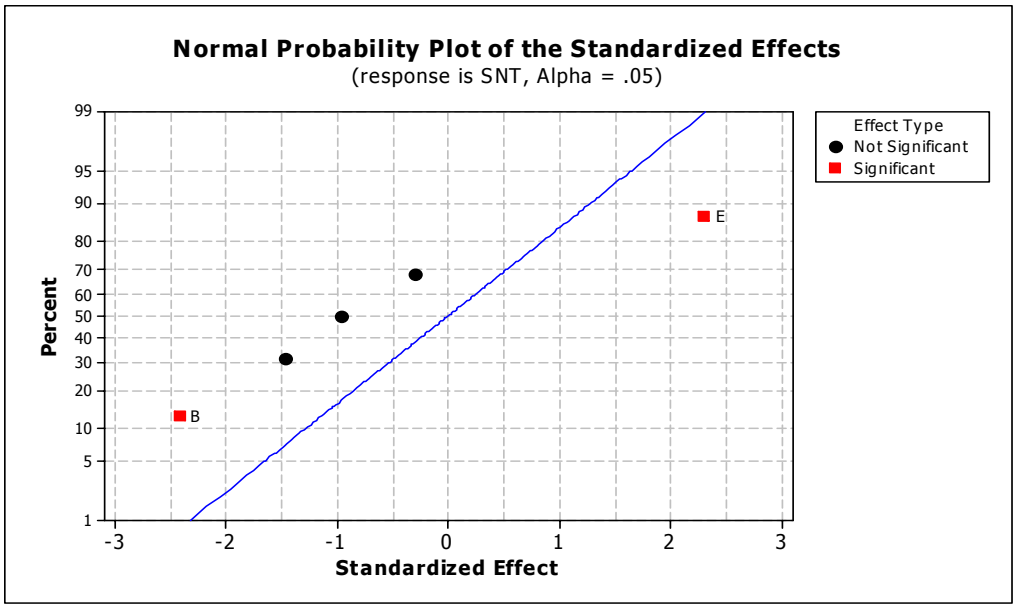

Figure 3.5: The normal probability plot of the effects for the Gear experiment

Once the significant control factors are determined, two different ways for deriving conclusions can be used. First, graphs of the main effects, called "marginal graphs" by Taguchi, are employed to find the robust solution. Figure 3.6 illustrates these graphs for the Gear example. The usual approach is to examine the graphs and "pick the winner" (Montgomery, 2001). In this case, factors B and E have larger effects than the others. As the objective is to

maximize S/N_T, the low level of factor B and the high level of factor E are recommended as a robust solution.

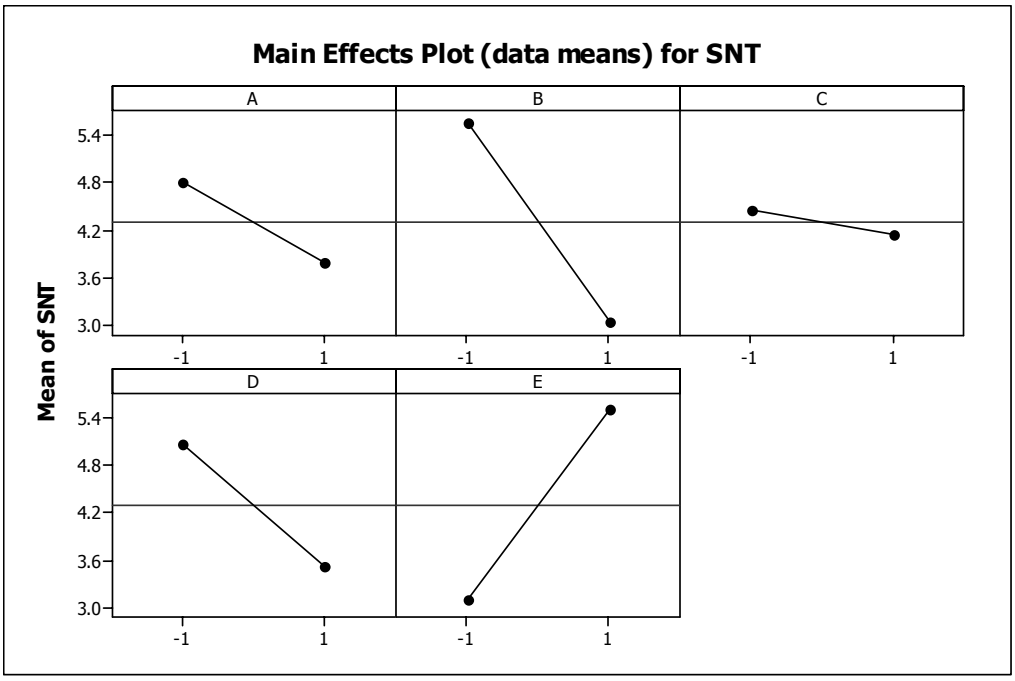

Figure 3.6: The main effect plot for the S/N_T in the Gear experiment

The graphical analysis can be supplemented with a regression model of the signal-to-noise ratio (Wu and Hamada, 2000). A regression model is used to model S/N_T in terms of the significant control factors and the robust solution is obtained by maximizing the function. Based on Table 3.7 the corresponding signal-to-noise ratio model for the Gear experiment is:

$$S/N_T = 4.29 - 1.265\, X_B + 1.205\, X_E$$

To maximize this function we would select the low level of factor B and the high level of factor E which is the same conclusion as in the graphical approach. This kind of model building analysis of Taguchi experiments is called "loss-model analysis" in the literature.

Taguchi advocates claim that the use of the S/N ratio generally eliminates the need for examining specific interactions between the control and noise factors (Montgomery, 2001). However, we believe that examining control-noise interactions by either including noise terms in the response model or exploring the corresponding interaction plots can improve the efficiency of experiment and has the advantages of yielding additional information about the specific noise-control interactions that may allow reduction of output variability induced by varying (noise) inputs. Shoemaker et al. (1991) point out this drawback of the loss-model approach, but the role of the dominant cause in improving the efficiency of the experiment and the advantages of knowing the dominant cause in the planning stage of the experiment have not been given much attention.

The Taguchi method suffers from many of the same drawbacks as the robustness experiment as briefly discussed in Section 3.2. Since we do not assume a known dominant cause(s), choosing fixed inputs and also selecting noise factors and determining noise factor extreme levels is difficult. Taguchi recommends using

engineering judgment to select noise factors and assumes that the choice includes all important noise factors. This coupled with the difficulty of choosing appropriate fixed inputs usually leads to a large experiment. In the Gear experiment, for instance, 128 tests were run to try to find a robust solution. We show in the Chapter 4 that only one of the three noise factors is a large cause and we could have gained this knowledge using inexpensive observational investigations before running the Taguchi experiment. Excluding two other noise factors from outer array (i.e. using desensitization experiment) can reduce the number of runs to 16×2=32 without reducing the efficiency of experiment. Moreover, had the experimenters not selected the dominant cause in their outer array, their experiment would have failed. Some critics of Taguchi (e.g. Shoemaker et al., 1991 and Miller et al., 1993), recommend using a combined array instead of crossed array to reduce the number of runs, but we believe that a more critical issue is finding the dominant cause before proceeding with an experiment. This not only reduces the number of runs (by removing ineffective factors from outer and inner array) but is also, as shown later, more efficient.

Another criticism of the Taguchi approach to parameter design, little discussed in the literature, is that running experiment with the outer array combinations is

challenging since these are normally varying inputs and it is usually hard and costly to keep them fixed for an experiment.

The desensitization approach, as discussed in the next section, can overcome these disadvantages and can provide a good framework to solve RPD problems.

3.4 Desensitization Experiment

3.4.1 Plan

In a desensitization experiment we choose a number of fixed inputs (candidates), based on knowledge of the dominant cause supplemented by engineering knowledge. We use an experimental plan to determine if these candidates and their new settings will make the process less sensitive to variation in the dominant cause.

Desensitization can be considered a version of the Taguchi method to RPD problem in which only the dominant cause is involved in outer array. Steiner and MacKay (2005) suggest using a full factorial design for the candidates, if there are

three or fewer, and using a fractional design with resolution[1] at least III otherwise. They also recommend selecting two levels for the dominant cause at the extremes of its normal range and using a crossed design where, for each treatment combination of candidates, there are runs for both levels of the dominant cause. Again, like any other experimental design, using the advantages of randomization, replication is advised to improve the precision of experiment.

Comparing desensitization and robust experiments, having knowledge of dominant cause reduces the size of the outer array and can lead better choices of candidates for the inner array.

Thus, desensitization experiments usually require fewer runs which reduces the cost and complexity of experiment. Also note that once a dominant cause is identified, in some instances, the remedy is obvious (dominant cause is controllable) and no further investigations are needed.

Statistical Engineering methodology (Steiner and Mackay, 2005) and some other variation reduction approaches like Shainin System and Six Sigma (Steiner et al., 2007) present a diagnostic journey for finding the dominant cause using progressive search and observational investigations. Generally observational studies are cheaper than experimental investigations because changing process

[1]For a brief explanation of design resolution and aliasing in a designed experiment see Appendix A.

settings and interrupting normal operations of the process are not needed. The knowledge of the dominant cause also assist us in selecting appropriate levels of dominant cause which makes our experiment more effective.

3.4.2 Analysis

Like the robustness and Taguchi method, analysis of a desensitization experiment can be carried out graphically or using a regression model.

Drawing a plot of the output by each treatment is first step in the graphical analysis to look for promising treatment combinations. Then, all cause by candidate interaction effects plots are drawn and finally the levels of candidates that make the output less sensitive to variation in the dominant cause are determined by examining these plots.

To analyze the results of desensitization experiment using a statistical model, a regression model, known as "response model", is employed to model the response (output) in terms of the control factors and the two term interactions of the control factors and the noise factor. A robust solution can be determined by minimizing the standard deviation of output based on the response model. To illustrate, an example, reported by Steiner and MacKay (2005), is reconsidered.

In a manufacturing plant, excessive failures in the accelerated life testing of electric motors were reported. A team was charged with reducing the unevenness in the commutator shaft (reducing the unevenness could solve the excessive failures) and they found the shaft profile as a dominant cause. As the dominant cause was uncontrollable they decided to use the desensitization approach to solve the problem. They conducted a fractional factorial experiment with eight treatments using four candidates. The selected candidates and their corresponding levels are given in Table 3.8.

Table 3.8: Selected fixed inputs and their levels in the Electric Motor experiment

Candidate	Low level	High level
Depth	Shallow	Deep
Grind time	Short	Long
Rotational Speed	1800	2400
Feed Rate	Slow	Fast

For each of the eight treatments there were two runs, one that used a shaft with a smooth or premachined profile (low level of the dominant cause), and a second that used a rough profile (high level of the dominant cause). The order of the runs was randomized and the surface unevenness (the response) was measured

on a scale of 1 (smooth) to 10 (rough). The experimental plan and data are given in Table 3.9.

Table 3.9: The experimental plan and data for the Electric Motor problem

Treatment	Depth	Grind Time	Rotational speed	Feed Rate	Profile (the dominant cause)	Smoothness (response)
1	Shallow	Short	1800	Slow	Smooth	2
2	Deep	Short	1800	Fast	Smooth	3
3	Shallow	Long	1800	Fast	Smooth	1
4	Deep	Long	1800	Slow	Smooth	2
5	Shallow	Short	2400	Fast	Smooth	3
6	Deep	Short	2400	Slow	Smooth	1
7	Shallow	Long	2400	Slow	Smooth	2
8	Deep	Long	2400	Fast	Smooth	3
1	Shallow	Short	1800	Slow	Rough	7
2	Deep	Short	1800	Fast	Rough	8
3	Shallow	Long	1800	Fast	Rough	9
4	Deep	Long	1800	Slow	Rough	8
5	Shallow	Short	2400	Fast	Rough	2
6	Deep	Short	2400	Slow	Rough	4
7	Shallow	Long	2400	Slow	Rough	3
8	Deep	Long	2400	Fast	Rough	5

As mentioned in the beginning of this section, drawing a plot of the output by each treatment to look for promising treatment combinations is the first step in the graphical analysis of a desensitization experiment. Figure 3.7 shows the plot of smoothness by each treatment. As you can see in the scatter plot, treatments 5 to 8, all with high rotational speed, look promising.

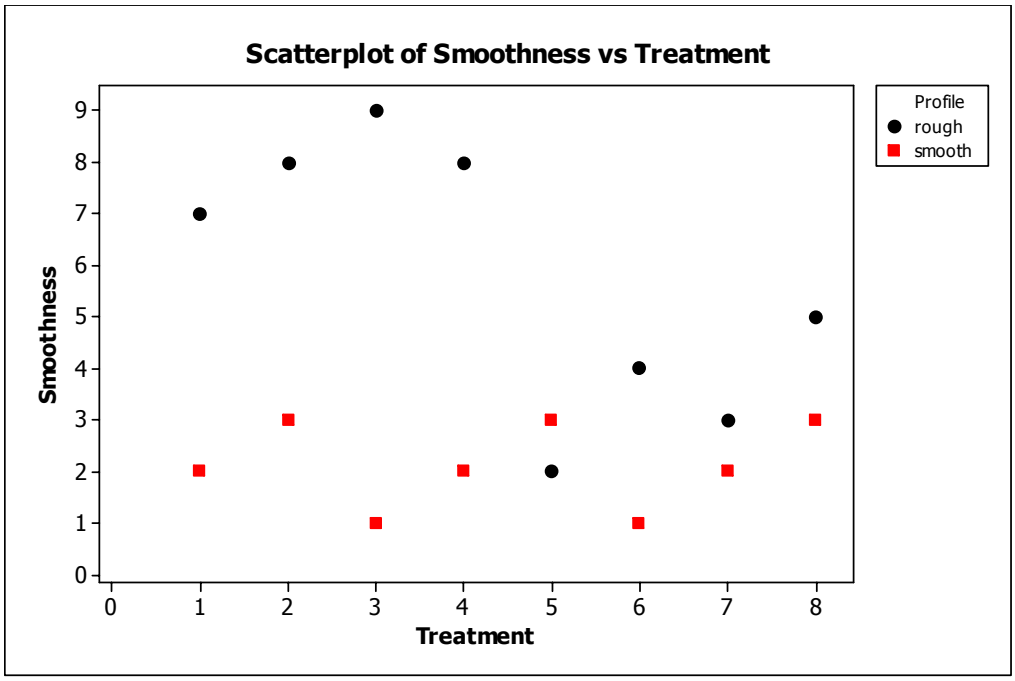

Figure 3.7: The plot of response versus treatments for the Electric Motor example

Next we can examine the interaction effects plot of the dominant cause by each candidate to see if any levels of the candidates make the output less sensitive to variation in the dominant cause. Figure 3.8 demonstrates the interaction plots for profile versus each of the four candidates. Only Rotational Speed flattens the relationship between smoothness and the initial shaft profile. In conclusion, setting the Rotational Speed to its high level (2400) is a solution and will desensitize the smoothness to changes in the shaft profile.

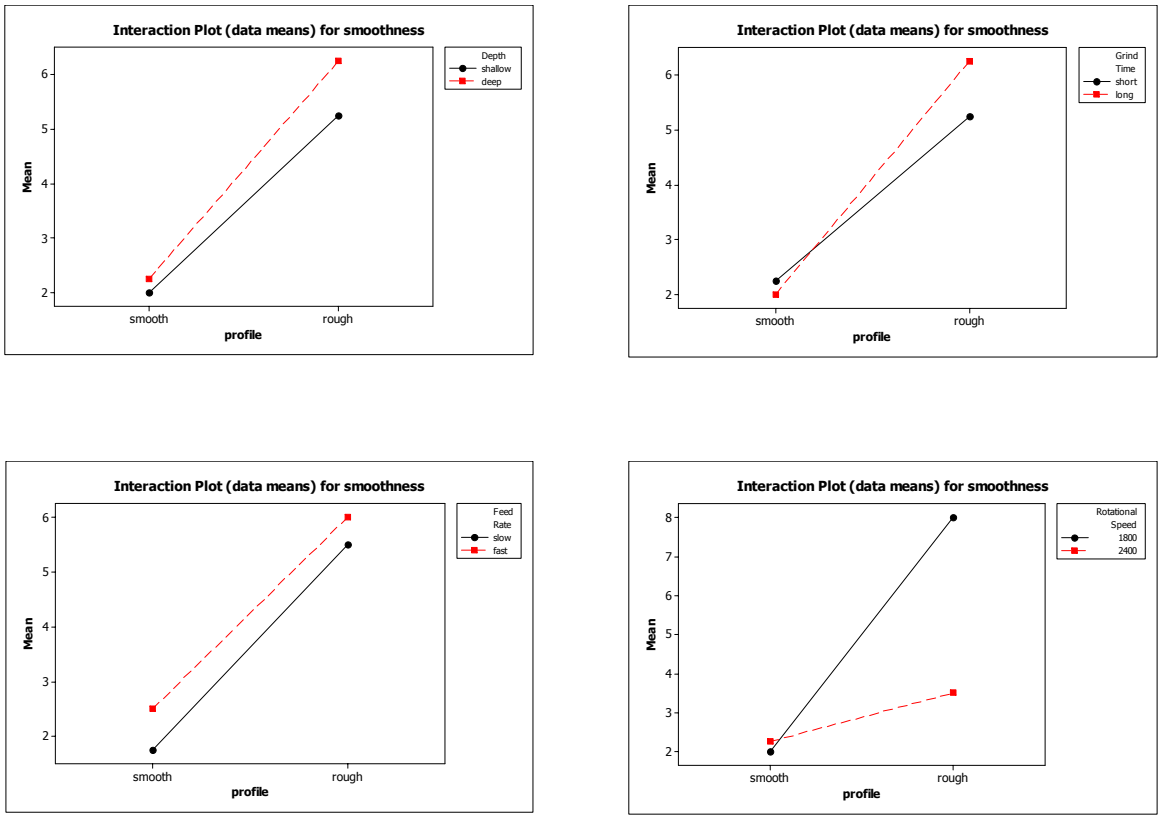

Figure 3.8: The interaction plots in the Electric Motor experiment

3.5 Summary

This chapter discussed in detail the three variation reduction approaches to robust parameter design (i.e. desensitization, robustness, and Taguchi method). The plan and analysis of each these types of experiments were discussed and illustrated using examples. Some drawbacks of the robustness and Taguchi experiments were mentioned and some advantages of the desensitization

approach over two other methods were briefly described. The next chapter focuses on the qualitative and quantitative comparison of the robustness, Taguchi and desensitization approaches.

Chapter 4

Desensitization versus Robustness and the Taguchi Method

4.1 Introduction

This chapter explores the advantages of desensitization over robustness and the Taguchi method. Following this introductory section, Section 4.2 presents a qualitative comparison of the three approaches. Subsequently, Section 4.3

presents a quantitative comparison of a desensitization experiment versus robustness experiment. Section 4.3 starts by considering and modeling the simplest case in which just one control factor and one dominant cause exist and we assume we have complete knowledge of the dominant cause. Desensitization and robustness are compared for this case using a performance measure. Subsequently, the results of this comparison are generalized to more than one control and one noise factor. We also compare desensitization and robustness in the more realistic case where we have uncertain knowledge of the dominant cause using a simulation study. As mentioned before, to simplify the language, we refer to a dominant cause of the variation here, recognizing that there may be more than one important cause.

4.2 Qualitative Comparison

Desensitization experiments have the following advantages over robustness and Taguchi style experiments:

1. As mentioned in Section 3.2, in robustness experiments fixed inputs (candidates) are selected based only on engineering knowledge whereas in desensitization experiments engineering judgment is supplemented by

knowledge of the dominant cause. Considering the dominant cause, the analyst tries to choose only fixed inputs that she/he feels are likely to have a favorable interaction with the dominant cause. This smart selection can improve the effectiveness of experiment. Generally, the more you know about the dominant cause of variation, the greater the chance you will select fixed inputs to change that will mitigate the variation in the dominant cause.

Knowing the dominant cause in desensitization can also help experimenters reduce the size of outer array. Including only the dominant cause decreases the total number of experimental runs when comparing desensitization to a Taguchi style experiment. Fewer runs leads to an easier, cheaper and shorter experiment.

2. Since noise factors or varying inputs are usually hard to control in the normal process operation, running a Taguchi experiment may be difficult, costly or sometimes impossible since you have a number of noise factors in the outer array and you need to fix the levels of these factors in each run of the experiment. This problem is mitigated somewhat in the

desensitization approach that recommends an outer array defined only using the dominant cause.

3. Having the dominant cause as a factor in the desensitization experiments, allows the analyst to model interactions between the dominant cause and the candidates directly whereas in the robustness experiments this interaction can not be assessed directly since dominant cause is not included as one of the experiment factors.

4. As mentioned before, the desensitization approach recommends first finding the dominant cause of variation and then if the dominant cause is not controllable, running a desensitization experiment. In some situations, once a dominant cause is identified, the remedy is obvious and no further investigations are needed. In these cases the dominant cause is controllable and variation in the output can be reduced by reducing the variability of the dominant cause.

5. Conducting baseline and observational investigations, as recommended by desensitization approach for finding the dominant cause, provides

useful information about how the process operates under current conditions. This information can be used to specify the problem goal by stating how the baseline should be changed. Although experimenters who follow the Taguchi or robustness method may also conduct these kind of investigations before proceeding to experimental investigations. However, conducting observational studies before any experimental investigations is not explicitly mentioned in Taguchi or robustness literature. In desensitization approach, however, conducting observational experiments for finding the dominant cause is a requirement. So, the likelihood of limited information about the current process is high in the Taguchi or robustness methods and this is another drawback of these methods. Recall the examples presented in Sections 3.2 and 3.3; if none of the runs represent the current setup of the process, how can experimenters be sure that the new setting, recommended by experiment, improves the process? The recommended robust solution may be much better than other settings used in the experiment, but still worse than the existing setting.

6. One of the most important requirements for a robustness experiment to be successful is that the unknown dominant cause acts in a short-term family

of variation (Steiner and MacKay, 2005). This is important because the length of each run in a robustness experiment must be long enough to be sure that the dominant cause will vary over (close to) its full range within each run. Otherwise assessing the interaction between a dominant cause and the candidates is not possible (even indirectly) and we will not be able to see if any candidate settings make the process robust to the variation in the unknown dominant cause. If experimenters do not have any information about the time nature of the dominant cause they do not have any idea about the desired length of the experiment runs. If they know the unknown dominant cause acts in a time-to-time family, it will likely not be feasible to conduct a robustness experiment since the runs would need to be too long. In the desensitization experiment, however, the length of runs is not an important issue because we include the dominant cause in the experiment and we select two levels for the dominant cause at the extremes of its normal range which can reflect the full extent of output variation and this allows the experimenter to reasonably evaluate the effect of different settings of control factors and their interaction with the dominant cause on the output variation.

7. As mentioned in the Section 3.3, Taguchi recommends using engineering judgment for selecting noise factors and assumes that the choice includes all the important noise factors. However, without substantial process knowledge and/or extensive preliminary investigations (as recommended in the desensitization approach) a poor choice of noise factors is possible. We will consider this issue in the next chapter where it is shown that the effectiveness of a Taguchi method experiment depends critically on the choice of noise factors.

8. In a desensitization experiment, the experimenter selects extreme levels of the dominant cause using information from preliminary investigations (conducted earlier when searching for the dominant cause). In Taguchi method, however, this information might not be available for experimenters since they are not required to conduct such preliminary investigations before conducting the experimental investigation; So, for Taguchi experiments we only on engineering judgment and past experience for selecting the levels of noise factors.

9. Regarding model based analysis, using the response model in the desensitization approach is an advantage in comparison to the robustness and Taguchi approaches in which constructing a loss-model is recommended for the analysis. In the loss-model approach focus is on modeling the optimization criterion, signal-to-noise ratio in Taguchi experiments and usually log(s) in robustness experiments, which is a nonlinear, many-to-one transformation of response and It is shown by Shoemaker et al. (1991) that modeling the optimization criterion may hide some of the relationship between individual control and noise factors and it is less likely that the optimization criterion can be a low-order linear model. Shoemaker et al. (1991) give an elaborated comparison between the loss-model approach over response model approach in data analysis.

Considering all these qualitative reasons, we conclude desensitization experiments are more effective than robustness and Taguchi method experiments. This is shown quantitatively in the next section.

4.3 Quantitative Comparison

4.3.1 Modeling

To start, we consider the simplest situation where we have just one fixed input and only one dominant cause. Then, the idea of desensitization and robustness can be demonstrated by considering the following regression model:

$$Y = \beta_0 + \beta_1 X + \beta_2 z + \beta_3 Xz + R \qquad (4.1)$$

where, Y represents a random variable that describes the possible values of output characteristic; X represents a random variable that describes the possible values of the dominant cause; z represents the levels of desensitizer (the fixed input that can desensitize the output to variation in the dominant cause) and R is a random variable that describes the effect of all other varying inputs on the response.

Equation (4.1) can be rewritten as:

$$Y = \beta_0 + (\beta_1 + \beta_3 z)X + \beta_2 z + R \qquad (4.2)$$

If z_0 is the value of z in the current process, $\beta_1 + \beta_3 z_0$ is the slope of the relationship between the dominant cause (X) and the output (Y) with the current process settings (see Figure 4.1).

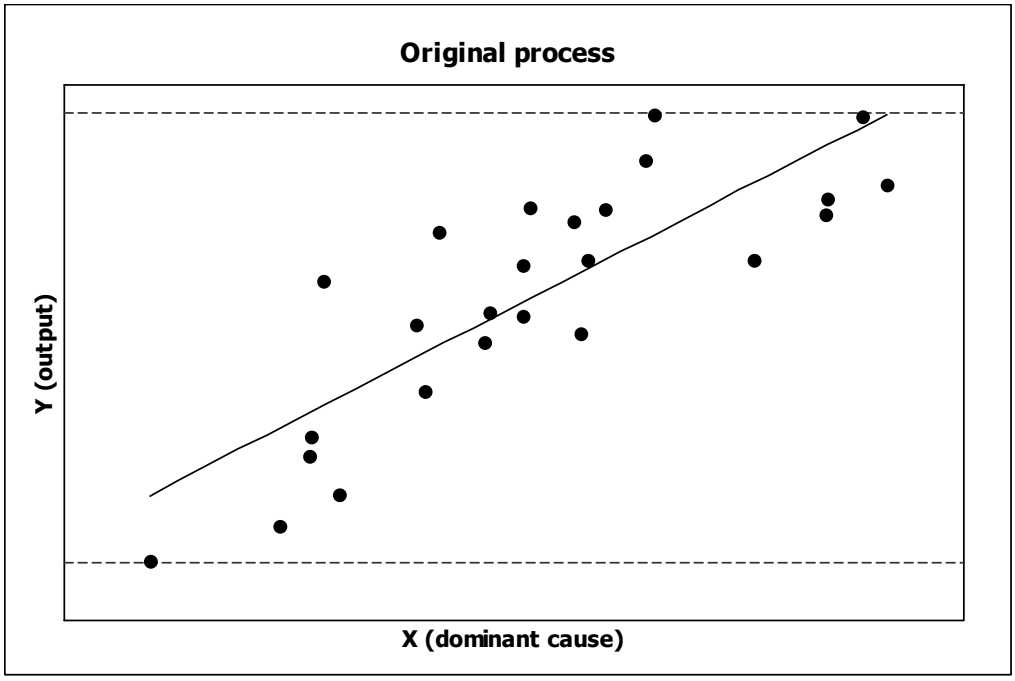

Figure 4.1: Range of output values in the current process ($z = z_0$)

Assuming the effect of all other causes, R, vary independently of the dominant cause, we can estimate the standard deviation of output using Equation (4.3).

$$sd(Y) = \sqrt{(\beta_1 + \beta_3 z)^2 \sigma_x^2 + \sigma_r^2} \qquad (4.3)$$

where, σ_x^2 and σ_r^2 are the variances of the dominant cause and residuals respectively. The purpose of desensitization and robustness experiments is to

find a new setting for z that flattens the relationship between output and dominant cause. This means we are looking for a new level of z, say z^*, where $\beta_1 + \beta_3 z^*$ is closer to zero than $\beta_1 + \beta_3 z_0$. With this change, while we continue to live with the variation of dominant cause (recall that we use these approaches when the dominant cause is hard to control or uncontrollable), we reduce the output variation (Figure 4.2) using the Xz interaction. We refer to this as a favorable interaction between X, a dominant cause, and z a (normally) fixed input.

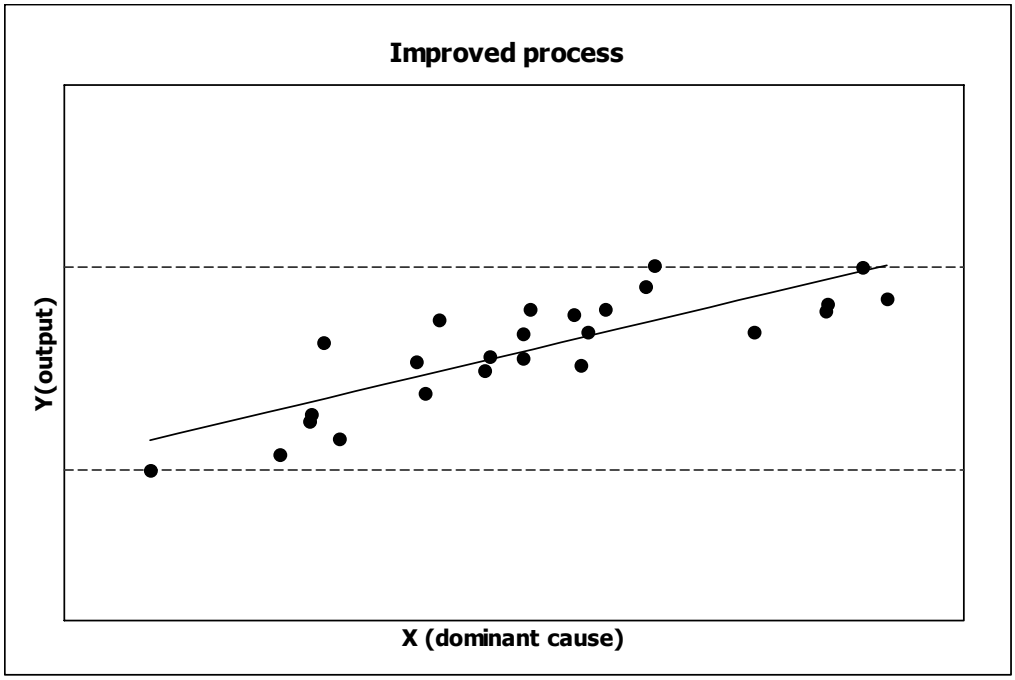

Figure 4.2: Range of output values with new setting ($z = z^*$)

The purpose of robustness and desensitization experiments is the same; however, in the robustness approach we assume that the dominant cause is not known and the experimenter tries to find the appropriate level of z without having the knowledge of a specific dominant cause

4.3.2 Performance Measure

To compare the efficiency of desensitization and robustness experiments we need a performance measure. The method that provides a better prediction of output variation will be better at determining the best choice of the levels of the candidates.

One way to define "*good*" prediction is to require the method have a reasonably consistent variance of the estimated response at points of interest (at specific levels of control factors used in the experiment). Consistent variance can be interpreted by smaller variation in estimated variance of output in either approach. So, we introduce *the standard deviation of estimated response variance* as a measure of efficiency or performance index, denoted by Std (P) in this thesis. Next, we formulate Std (P) for each method and then we compare each method using these formulated performance measures. The smaller the performance index the better.

In the case of desensitization, we first look for the dominant cause using observational investigations and a process of elimination (Shainin, 1993b; Steiner and MacKay, 2005), called the progressive search method. As such, to start we assume the standard deviation of dominant cause (σ_x), the slope of the relationship between the dominant cause and the output (i.e. $\beta_1 + \beta_3 z_0$), and the standard deviation of residuals (σ_r), are known from our prior investigations. In Section 4.3.4 we relax this assumption. The elimination method is detailed in Steiner and MacKay (2005) and we will describe it briefly later.

The model parameters are determined from our baseline investigation, an "input-output" investigation for verifying the dominant cause, and other preliminary enquiries for finding and verifying the dominant cause. Assuming σ_x, σ_r, and $\beta_1 + \beta_3 z_0$ are known and the current value of z (i.e. z_0) is equal to zero, the standard deviation of the output can be estimated with a desensitization experiment by estimating β_3 (denote the corresponding estimator as $\tilde{\beta}_3$). Thus, if we define P_{des} as

$$P_{des} = \mathrm{Var}(Y \mid z = z_1) = (\beta_1 + \tilde{\beta}_3 z_1)^2 \sigma_x^2 + \sigma_r^2 \qquad (4.4)$$

the performance index in the case of desensitization is the standard deviation of P_{des} (i.e. $\mathrm{Std}(P_{des})$).

In the robustness method, on the other hand, we estimate the standard deviation of output directly based on the experiment results. This means that P_{rob} is defined as:

$$P_{rob} = \mathrm{Var}(Y \mid z = z_1) = s^2 \qquad (4.5)$$

where s^2 is the sample variance of robustness experiment results when $z=z_1$.

Thus, the performance index in this case can be presented as the standard deviation of P_{rob} (i.e. $\mathrm{Std}(P_{rob})$).

Now, we derive $\mathrm{Std}(P_{des})$ and $\mathrm{Std}(P_{rob})$ as given by the desensitization and robustness plans discussed in Section 3.4 and Section 3.2 respectively. For the case that was modeled and describe early, we have z as the fixed input or control factor and X as the dominant cause in the desensitization experiment; each at two levels (say $\pm a$ for z, where "a" is a constant value, and $\mu_x \pm 2\sigma_x$ for x which are extreme levels of x). Using a crossed design, there are runs for both levels of the dominant cause for each treatment (each level of z). For the robustness experiment we have only a fixed input or z with the same levels in desensitization experiment (i.e. $\pm a$). To be fair we compare desensitization and robustness experiments with the same number of runs. This means that if we have k replicates in the desensitization experiment, the number of replicates will

be equal to 2k in the robustness experiment. The desensitization and robustness experiment plans for a simple case (k=2) are given in Tables 4.1 and 4.2 respectively.

Table 4.1: Design matrix for desensitization experiment with k replicates (k=2 here)

Treatment	Run	z	x	Y	
1	1	+a	$\mu_x + 2\sigma_x$	y_1	first replicate
2	2	+a	$\mu_x - 2\sigma_x$	y_2	
3	3	-a	$\mu_x + 2\sigma_x$	y_3	
4	4	-a	$\mu_x - 2\sigma_x$	y_4	
1	5	+a	$\mu_x + 2\sigma_x$	y_5	second replicate
2	6	+a	$\mu_x - 2\sigma_x$	y_6	
3	7	-a	$\mu_x + 2\sigma_x$	y_7	
4	8	-a	$\mu_x - 2\sigma_x$	y_8	

Table 4.2: Design matrix for robustness experiment with 2k replicates (k=2 here)

Treatment	Run	z	Y	
1	1	+a	y_1	1st replicate
2	2	-a	y_2	
1	3	+a	y_3	2nd replicate
2	4	-a	y_4	
1	5	+a	y_5	3rd replicate
2	6	-a	y_6	
1	7	+a	y_7	4th replicate
2	8	-a	y_8	

Note that the only random variable in Equation (4.4) is $\tilde{\beta}_3$ and before formulating the Std(P_{des}) we need to determine variance of $\tilde{\beta}_3$. This variance can be determined using a regression model that we fit based on the desensitization experiment's results. Note that with z_0 =0, knowing $\beta_1 + \beta_3 z_0$ we know β_1.

The regression model is presented as:

$$Y_i = \beta_0 + \beta_1 x_i + \beta_2 z_i + \beta_3 x_i z_i + R_i \qquad \text{i= 1, 2 ,…, 4k} \quad \text{and} \quad R_i \sim N(\mu_r, \sigma_r^2)$$

or

$$Y_i - \beta_1 x_i = \beta_0 + \beta_2 z_i + \beta_3 x_i z_i + R_i \ ;$$

This model may be written in matrix notation as:

$$\mathbf{Z} = X\tilde{\beta}$$

where

$$\mathbf{Z} = Y_i - \beta_1 x_i \qquad \tilde{\beta} = \begin{bmatrix} \beta_0 \\ \beta_2 \\ \beta_3 \end{bmatrix} \qquad \text{and} \qquad X = \begin{bmatrix} 1 & z_1 & x_1 z_1 \\ 1 & z_2 & x_2 z_2 \\ \vdots & \vdots & \vdots \\ 1 & z_{4k} & x_{4k} z_{4k} \end{bmatrix}$$

Using standard regression results (Montgomery, 2001) the variance of $\tilde{\beta}$ is expressed in covariance matrix:

$$\mathrm{COV}(\tilde{\beta}) = \sigma_r^2 (X^T X)^{-1}$$

a symmetric matrix whose diagonal entries give the variance of the individual regression coefficient $\tilde{\beta}$. Thus, $\mathrm{VAR}(\tilde{\beta}_3)$ is equal to $\sigma_r^2 (X^T X)^{-1}_{33}$ where $(X^T X)^{-1}_{33}$ is

the 3th main diagonal element of the matrix $(X^T X)^{-1}$). Considering the design matrix of desensitization experiment the $(X^T X)^{-1}$ matrix can be calculated[2]. By calculating $(X^T X)^{-1}$ we can see that its 3th diagonal element is $\frac{1}{16ka^2\sigma_x^2}$ where "k" is the number of replicates in the desensitization experiment and "a" is the absolute value of the levels of z. Accordingly $\mathrm{VAR}(\tilde{\beta}_3)$ is equal to $\frac{\sigma_r^2}{16ka^2\sigma_x^2}$ (i.e. $\tilde{\beta}_3 \sim N(\beta_3, \frac{\sigma_r^2}{16ka^2\sigma_x^2})$).

To find the Std (P_{des}), we denote $\beta_1 + \tilde{\beta}_3 z$ as "A" in Equation (4.4) and rewrite the equation as:

$$P_{des} = A^2\sigma_x^2 + \sigma_x^2 \quad , A \sim N(\mu_A, \sigma_A^2) \tag{4.6}$$

In above equation, "A" is a random variable and σ_x^2 & σ_r^2 are constants, so

$$\mathrm{VAR}(P_{des}) = \sigma_x^4\, \mathrm{VAR}(A^2) \tag{4.7}$$

where

$A = \beta_1 + \tilde{\beta}_3 z \;;\; E(A) = \mu_A = \beta_1 + \beta_3 z$ and

$$\mathrm{VAR}(A \mid z = a) = a^2\, \mathrm{VAR}(\tilde{\beta}_3) = a^2 \frac{\sigma_r^2}{16ka^2\sigma_x^2} = \frac{\sigma_r^2}{16k\sigma_x^2}$$

---

[2] See detailed calculation in Appendix B.1.

Based on the definition of noncentral chi-square distribution (Abramowitz and Stegun, 1972) we know that:

$$\left(\frac{A}{\sigma_A}\right)^2 \sim \chi_1^2(\lambda) \quad with \quad \lambda = \left(\frac{\mu_A}{\sigma_A}\right)^2$$

Thus, $A^2 \sim \sigma_A^2 \chi_1^2(\lambda)$ and

$$VAR(A^2) = \sigma_A^4 \; VAR(\chi_1^2(\lambda)) = \sigma_A^4 \; 2(1+2\lambda) = 2\sigma_A^2(\sigma_A^2 + 2\mu_A^2) \qquad (4.8)$$

Substituting Equation (4.8) into Equation (4.7), we get

$$VAR(P_{des}) = \frac{\sigma_r^4 + 2\sigma_r^2(16k\mu_A^2\sigma_x^2)}{8(4k)^2}$$

Thus, the performance index in the case of a desensitization experiment ($Std(P_{des})$) is the square root of above expression, namely:

$$Std(P_{des}) = \sqrt{\frac{\sigma_r^4 + 2\sigma_r^2(16k\mu_A^2\sigma_x^2)}{8(4k)^2}} \qquad (4.9)$$

Next, we need to find Std (P_{rob}) when P_{rob} is defined by Equation (4.5) (i.e. s^2).

The sampling distribution of the sample variance is a scaled chi-square (Abramowitz and Stegun, 1972):

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

where n (# of data points used in the calculation of s^2) is equal to 2k. So:

$$\mathrm{Var}(P_{rob}) = \frac{2\sigma_y^4}{2k-1}$$

Thus, the performance index in the case of robustness ($\mathrm{Std}(P_{rob})$) can be formulated as square root of above expression:

$$\mathrm{Std}(P_{rob}) = \sqrt{\frac{2\sigma_y^4}{2k-1}} \qquad (4.10)$$

The performance measures, Equation (4.9) and Equation (4.10), were also validated by a simulation. See related codes and result in Appendix C.1.

4.3.3 Comparing Performance Measures

As mentioned before, the smaller the performance index the higher the effectiveness. So, to quantitatively prove our claim that a desensitization experiment is more effective than a robustness experiment we should show that Equation (4.9) is always less than Equation (4.10) or

$$\frac{2\sigma_y^4}{2k-1} > \frac{\sigma_r^4 + 2\sigma_r^2(16k\mu_A^2\sigma_x^2)}{8(4k)^2} \text{ or}$$

$$256k^2\sigma_y^4 > (2k-1)(\sigma_r^4 + 32k\sigma_r^2\mu_A^2\sigma_x^2)$$

Substituting $\sigma_y^2 = (\mu_A^2\sigma_x^2 + \sigma_r^2)$ into above expression and rearranging we obtain

$$256k^2\mu_A^4\,\sigma_x^4 + 256k^2\sigma_r^4 + 512k^2\mu_A^2\sigma_x^2\sigma_r^2$$

$$> \qquad (4.11)$$

$$(2k-1)\sigma_r^4 + 64k^2\mu_A^2\sigma_x^2\sigma_r^2 - 32k\sigma_r^2\,\mu_A^2\,\sigma_x^2$$

Since $256k^2\mu_A^4\,\sigma_x^4$ is positive[3], $256k^2\sigma_r^4$ is greater than $(2k-1)\sigma_r^4$, and $512k^2\mu_A^2\sigma_x^2\sigma_r^2$ is also greater than $64k^2\mu_A^2\sigma_x^2\sigma_r^2$ we can conclude that inequality (4.11) is true and consequently conclude that $\mathrm{Std}(P_{des})$ is always less than $\mathrm{Std}(P_{rob})$. This conclusion indicates that the desensitization approach is always more efficient than the robustness approach (given our assumptions).

To generalize this conclusion we need to first consider cases in which there are more than one fixed input and one dominant cause and show that $\mathrm{Std}(P_{des})$ is also less than $\mathrm{Std}(P_{rob})$ in these situations. Second, we should challenge the assumption that we took in the desensitization case (i.e. σ_R^2, σ_X^2, $\beta_1+\beta_3 z_0$ are known) and think about situations where one or all of these components are not known and we need to estimate them using either the desensitization experiment results and/or preliminary investigations. The next section shows how we generalized the comparison result.

---

[3]Note that k, the number of replicates in experiment is positive

4.3.4 Generalizing the Result

In Appendix B.2, using a similar argument as given here, we show that the performance index of desensitization is less than the performance index of robustness even where we have "m" noise factors and "n" control factors.

As mentioned early, the desensitization approach recommends using the method of elimination to find the dominant cause(s). This method concentrates on ruling out possibilities rather than looking directly for the dominant cause (Steiner and MacKay, 2005). Using elimination, the set of all causes is divided into families and then an observational investigation is conducted to rule out all but one family. This exercise is repeated on the remaining family until a single dominant cause or a small number of suspects cause(s) remain. At this point, when the family of remaining suspects is small, an "input-output" relationship investigation is used to isolate the dominant cause. In an "input-output" investigation a time frame is selected based on the full extent of output variation and a sample of 30 or more parts, spread across the time frame, is chosen. Then, for each part, the interested output characteristic and all remain suspects are measured. By plotting the output versus each one of the suspects any strong linear relationship can be found and the dominant cause can be identified.

Steiner and MacKay (2005) not only recommend the method of elimination and a series of simple observational investigations to isolate a dominant cause but also recommend conducting a verification experiment to be sure that the suspected cause is dominant. Following these steps for finding the dominant cause before conducting the desensitization experiment it is reasonable to assume $\beta_1 + \beta_3 z_0$, σ_x and σ_r are already known (or well estimated) since these components can be estimated using the observational studies needed to find and verify the dominant cause. However, we shall also consider the situations where $\beta_1 + \beta_3 z_0$, σ_x and σ_r are not known and they are estimated using only the desensitization experiment results or using the desensitization results and a preliminary "input-output" investigation.

For this reason a simulation study was employed[4]. In the simulation study the model presented by Equation (4.2) is considered and without loss of generality we set:

$$\begin{aligned} \beta_0 &= 0 \qquad \sigma_r^2 = 1 \\ \beta_1 &= 1 \qquad \mu_r = 0 \\ \beta_2 &= 0 \qquad \mu_x = 0 \\ \beta_3 &= 1 \qquad z_0 = 0 \end{aligned}$$

[4] See related codes in Appendix C.2.

With this setup, the levels of z in the desensitization and robustness experiments are used to quantify the size of the dominant cause and the potential to reduce process sensitivity to variation in the dominant cause. For fixed z the variance of output is :

$$\mathrm{Var}(Y) = (\beta_1 + \beta_3 z)^2 \sigma_x^2 + \sigma_r^2 = (1 + 1z)^2 \sigma_x^2 + 1$$

Then X is a dominant cause if $(1+1z)^2 \sigma_x^2 > 1$. So in the current process where $z=z_0=0$, X is a dominant cause if $\sigma_x^2 > 1$. Note that with $z_0 = 0$ knowing $\beta_1 + \beta_3 z_0$ we know β_1.

Then, four possible situations are considered:

1. Assume the relationship between x and y (i.e. β_1), σ_x and σ_r are known and then estimate the standard deviation of y at high and low levels of z by estimating β_3 and using Equation (4.3)[5].
2. Assume the relationship between x and y (i.e. β_1) and the residual variation (i.e. σ_r) are not known, however σ_x is known. In this situation β_1 and σ_r are estimated using only the desensitization experiment results and then the standard deviation of y at high and low levels of z are estimated using Equation (4.3). This situation corresponds to a case where

[5] Note that this situation is the same as situation that we assumed earlier for the theoretical comparison in Section 4.3.3.

we know X is a dominant cause and know the distribution of X values (i.e. σ_x). If we know X is a dominant cause we would also have some knowledge of β_1. So this situation is not overly realistic but is included for the sake of comparison.

3. The same situation as option 2, but we use a preliminary input-output investigation (sample size =30) to help estimation of β_1 and σ_r.
4. Assume nothing is known, we only suspect that X is dominant cause and use the preliminary input-output investigation (with the same sample size as option 3) to estimate σ_x and use both input-output investigation and the desensitization experiment to estimate β_1 and σ_r.

As in the theoretical comparison, the levels of z for each run of the desensitization experiment are the same levels of z used in the corresponding robustness run and the level of X in desensitization runs are chosen to be extreme (i.e. $\mu_X \pm 2\sigma_X$). Simulation results are given by Figures 4.3 to 4.6.

In the figures we show contour plots of the performance ratio, which is $\mathrm{Std}(P_{rob})$ divided by $\mathrm{Std}(P_{des})$. Values greater than one suggests desensitization is more effective than robustness. The simulation estimates the standard deviation of the output using 1000 trials of each of the desensitization and robustness

experiments. Each of earlier listed four options is considered. Figures 4.3 to 4.6 show the performance ratio for option 1 through 4, respectively. These figures present the results for high levels of z, where X is a dominant cause. The number of replicates in all options is equal to 2 and the number of observations in the preliminary input-output investigations for option 3 and 4 is equal to 30.

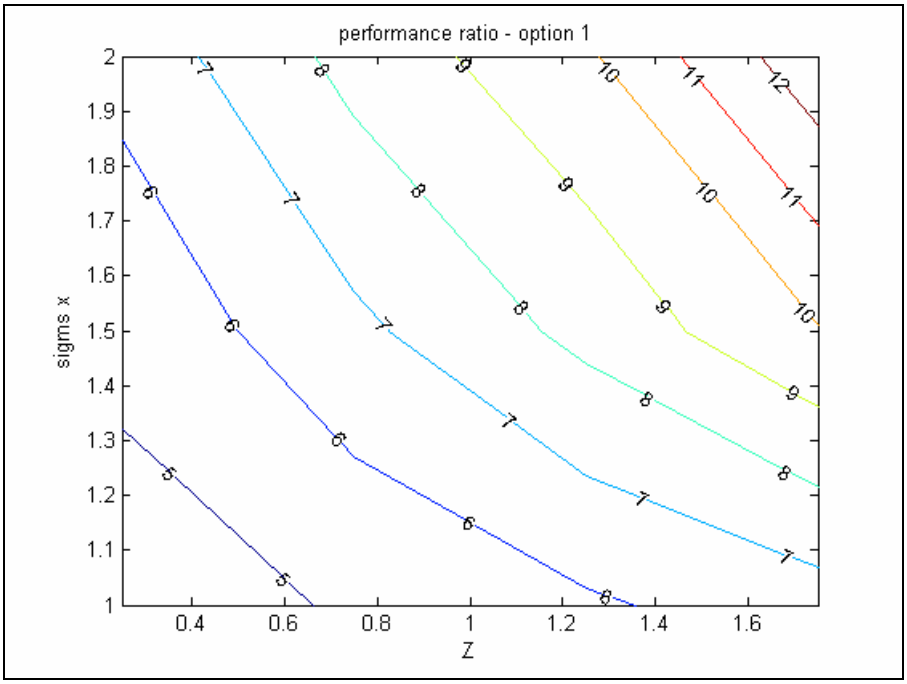

Figure 4.3: Performance ratio in situation 1

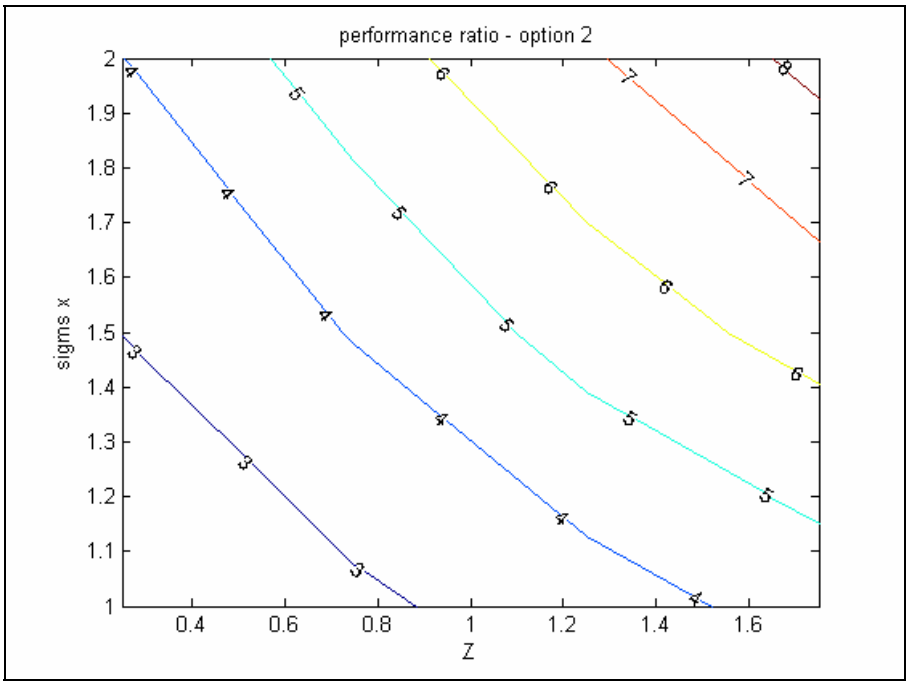

Figure 4.4: Performance ratio in situation 2

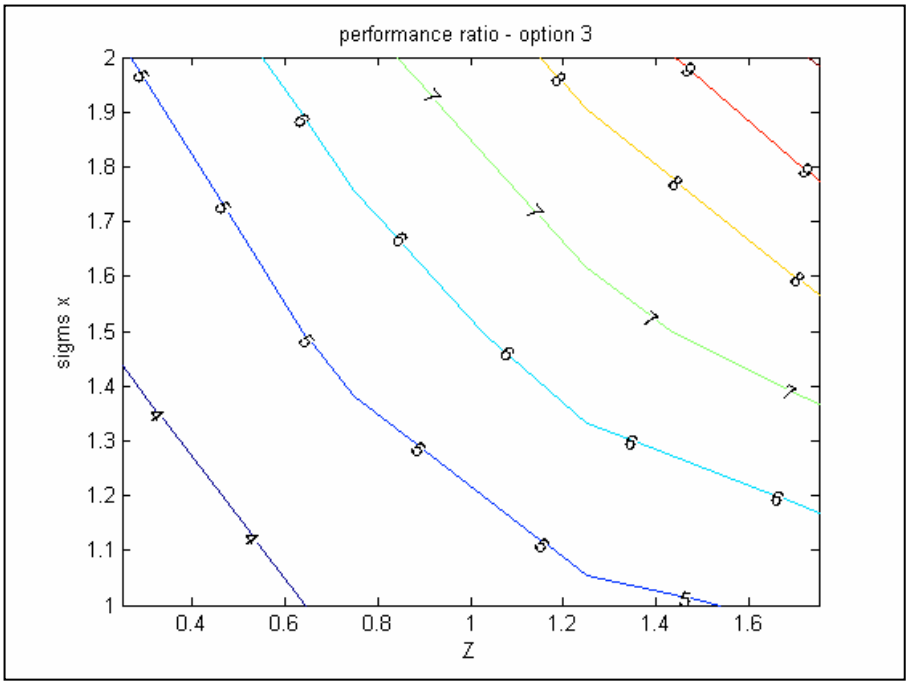

Figure 4.5: Performance ratio in situation 3

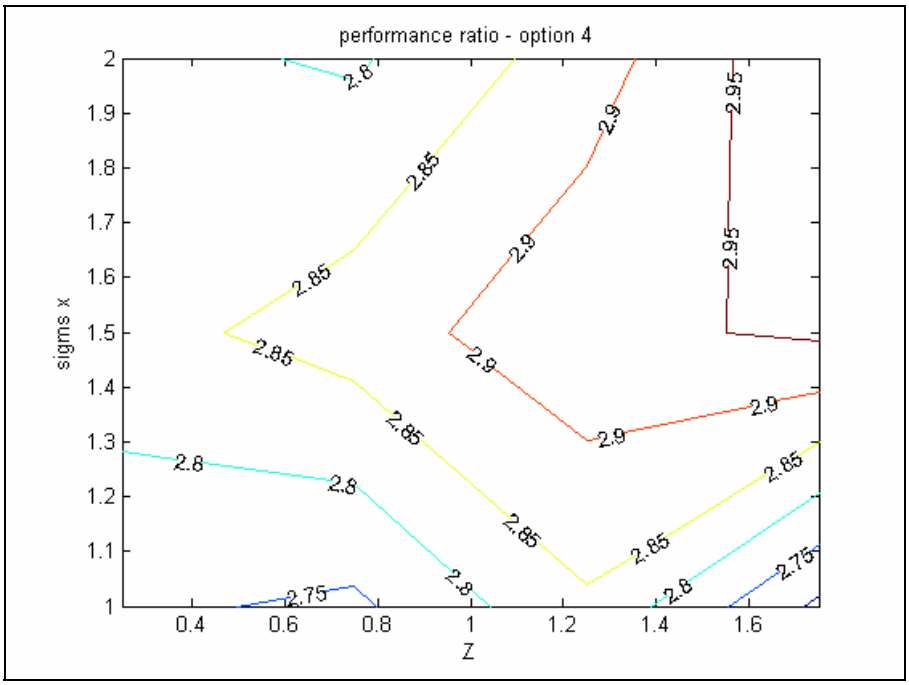

Figure 4.6: Performance ratio in situation 4

Figures 4.3 to 4.6 demonstrate that the performance ratio is bigger than 1 in all situations which validates and generalize, on some aspects, the theoretical results given earlier in Section 4.3.3.

The Figures also indicate that when the values of z and σ_x increase the performance ratio increases as well. The reason is that the standard deviation due to dominant cause (the value of $(\beta_1 + \beta_3 z)^2 \sigma_x^2$ in Equation (4.3)) grows when the value of z and/or σ_x increase. In other words, the effect of dominant cause in the output variation increases and we have a dominant cause that has higher

importance. Thus, the desensitization experiment is more effective when the the dominant cause has a greater effect.

4.4 Summary and Conclusions

This chapter began by presenting a qualitative comparison of the robustness, Taguchi and desensitization approaches. Then, the efficiency of desensitization and robustness was compared by introducing a performance measure and comparing the formulated performance indexes of each approach. It was shown that assuming a dominant cause and a known cause/output relationship that the desensitization experiment is always more effective than the robustness experiment for any number of fixed inputs and dominant cause(s). Next, a simulation study considered different likely situations in which the primary assumptions of theoretical comparison were relaxed. In these cases again desensitization was more effective than robustness.

In summary, both qualitative and quantitative comparisons suggest the desensitization method is a better approach to robust parameter design than the robustness or Taguchi approaches.

To further demonstrate the effectiveness of the desensitization approach, the Gear experiment, presented in Chapter 3, is reconsidered and studied in the next chapter.

Chapter 5

Case Study: Geometric Distortion of Drive Gears

5.1 Introduction

The Gear experiment, presented in Section 3.3, is reconsidered in this chapter. We use a simulation study to compare three different experiments (i.e. robustness, Taguchi style, and desensitization experiments) for solving the Gear

example problem. The simulation program calculates the performance measure of each approach and the method with the highest efficiency is determined by comparing these performance measures.

5.2 The Gear Example: Comparing Approaches Using a Simulation Study

In this section, all three approaches are applied to the experiment introduced earlier in Section 3.3. As described in the Gear example, there are five control factors and three noise variables. The main effect plot for dishing of the gear (Figure 5.1) suggests factor "H" as a dominant cause and scatter plots of the response versus noise factors (Figures 5.2, 5.3 and 5.4) confirm this suggestion.

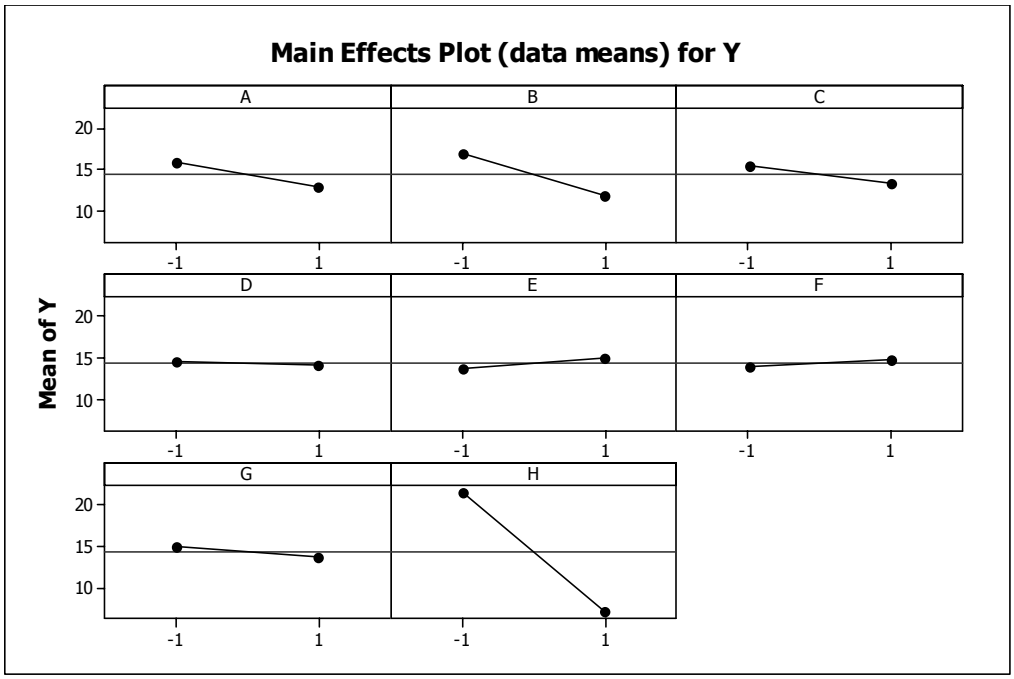

Figure 5.1: Main effects plot for the Gear experiment

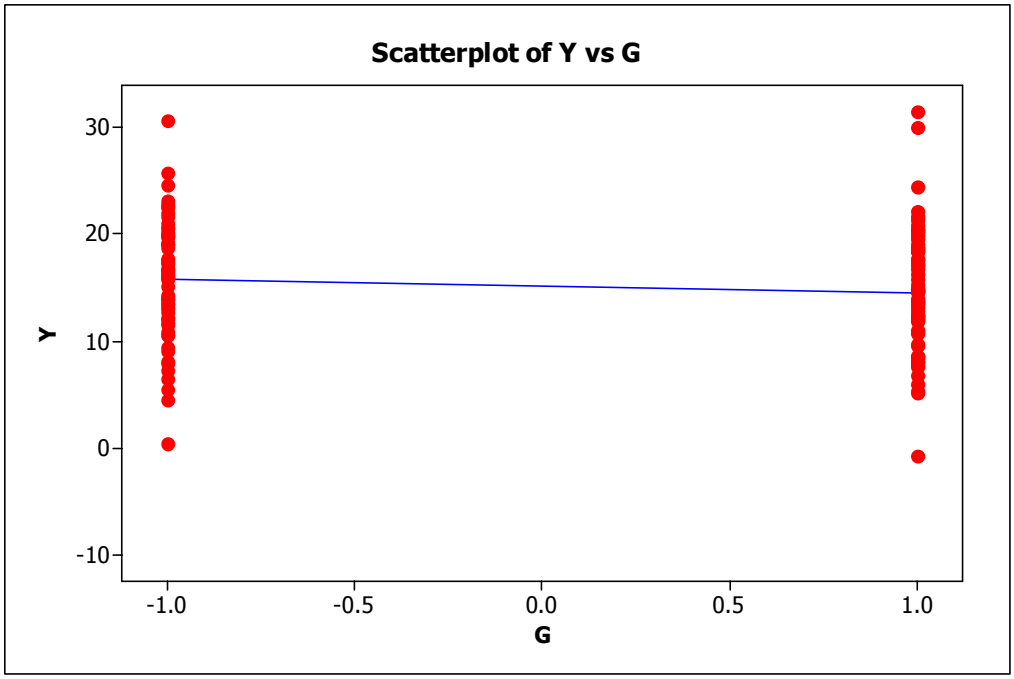

Figure 5.2: Scatter plot of the response versus factor G in the Gear experiment

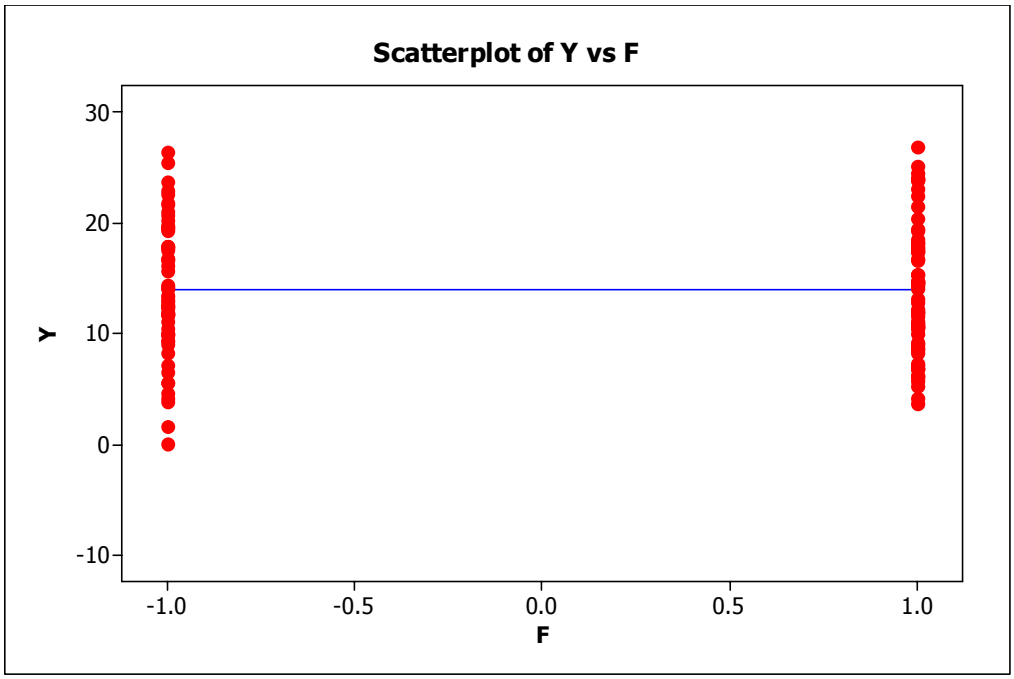

Figure 5.3: Scatter plot of the response versus factor F in the Gear experiment

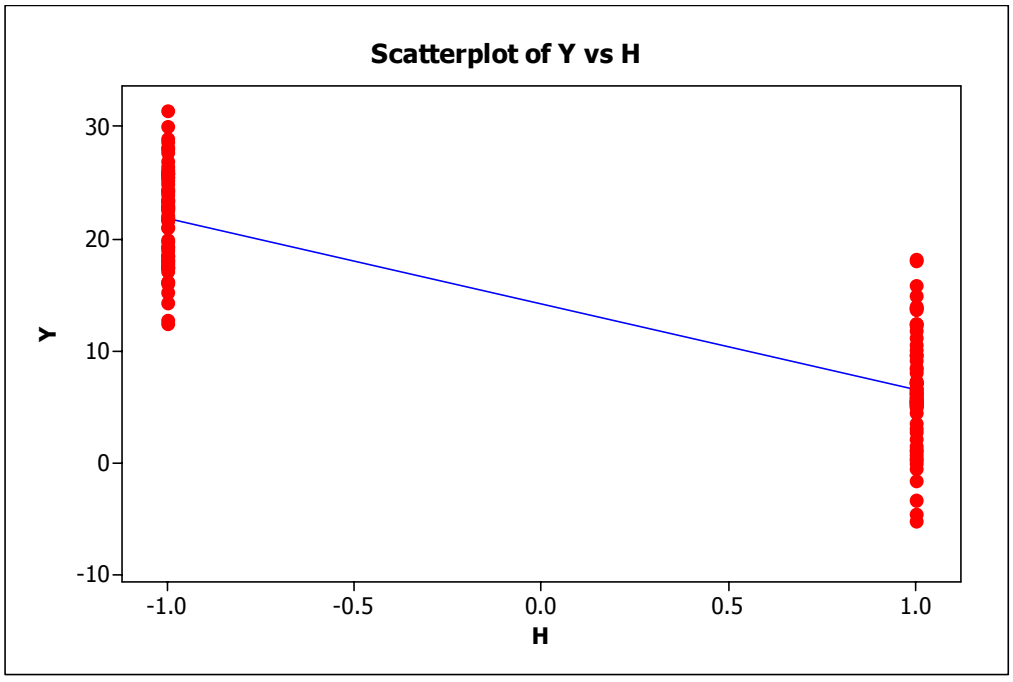

Figure 5.4: Scatter plot of the response versus factor H in the Gear experiment

Considering the normal probability plot (Figure 5.5) and Table 5.1, a reduced model is constructed as:

$$\begin{aligned} Y = {} & 14.336 - 1.523x_A - 2.648x_B - 0.992x_C - 0.312x_D + 0.625x_E \\ & + 0.422x_F - 0.695x_G - 7.195x_H + 1.297x_Cx_F + 0.922x_Bx_F + 0.859x_Fx_H \\ & - 0.844x_Dx_H - 0.93x_Cx_Dx_F + R \end{aligned} \tag{5.1}$$

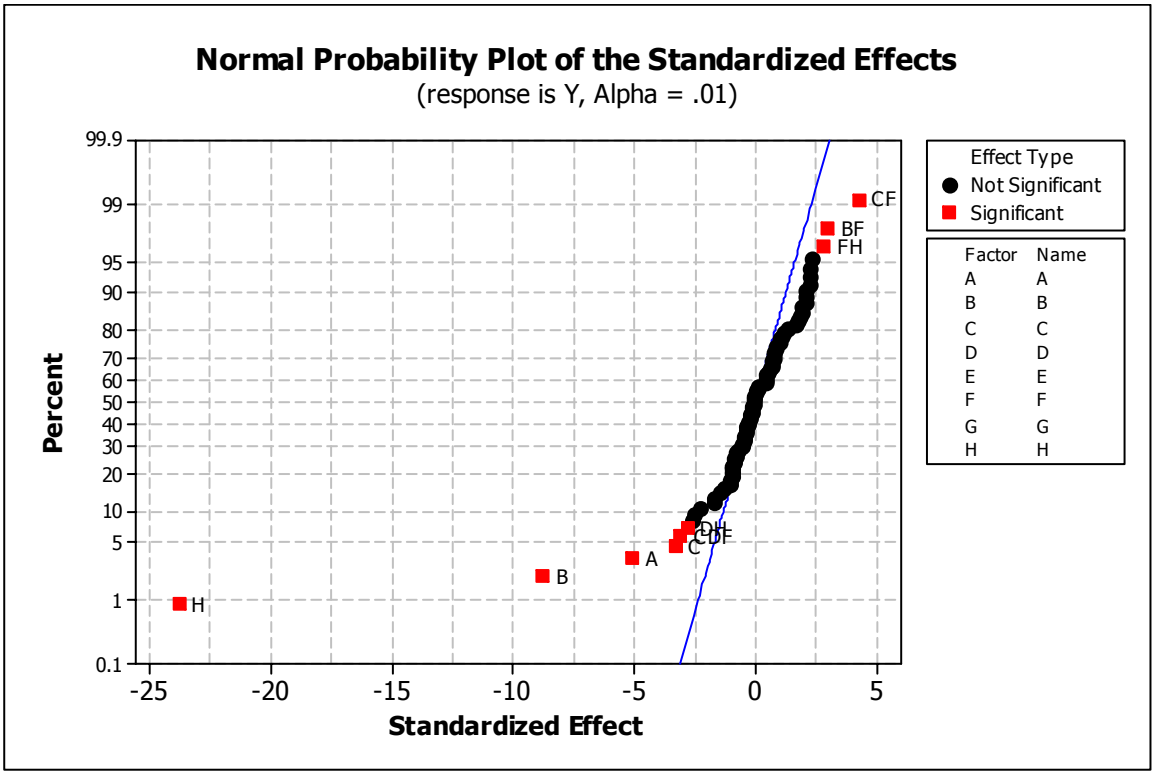

Figure 5.5: Normal probability plot in the Gear experiment (when response is Y)

Table 5.1: Estimated effects and coefficient for Y in the Gear example

Term	Effect	Coef	SE Coef	T	P
Constant		14.336	0.3026	47.38	0
A	-3.047	-1.523	0.3026	-5.03	0
B	-5.297	-2.648	0.3026	-8.75	0
C	-1.984	-0.992	0.3026	-3.28	0.002
D	-0.625	-0.312	0.3026	-1.03	0.307
E	1.25	0.625	0.3026	2.07	0.045
F	0.844	0.422	0.3026	1.39	0.17
G	-1.391	-0.695	0.3026	-2.3	0.026
H	-14.391	-7.195	0.3026	-23.78	0
A*B	-0.109	-0.055	0.3026	-0.18	0.857
A*C	0.266	0.133	0.3026	0.44	0.663
A*D	1.25	0.625	0.3026	2.07	0.045
A*E	0.437	0.219	0.3026	0.72	0.473
A*F	-0.313	-0.156	0.3026	-0.52	0.608
A*G	-0.297	-0.148	0.3026	-0.49	0.626
A*H	0.484	0.242	0.3026	0.8	0.428
B*C	-0.453	-0.227	0.3026	-0.75	0.458
B*D	0	0	0.3026	0	1
B*E	1.156	0.578	0.3026	1.91	0.062
B*F	1.844	0.922	0.3026	3.05	0.004
B*G	-0.047	-0.023	0.3026	-0.08	0.939
B*H	1.172	0.586	0.3026	1.94	0.059
C*D	-1	-0.5	0.3026	-1.65	0.105
C*E	-1.031	-0.516	0.3026	-1.7	0.095
C*F	2.594	1.297	0.3026	4.29	0
C*G	-0.234	-0.117	0.3026	-0.39	0.7
C*H	1.391	0.695	0.3026	2.3	0.026
D*E	-0.234	-0.117	0.3026	-0.39	0.7
D*F	-0.766	-0.383	0.3026	-1.27	0.212
D*G	-0.188	-0.094	0.3026	-0.31	0.758
D*H	-1.687	-0.844	0.3026	-2.79	0.008
E*F	-0.266	-0.133	0.3026	-0.44	0.663
E*G	-0.563	-0.281	0.3026	-0.93	0.358
E*H	1	0.5	0.3026	1.65	0.105
F*G	-0.062	-0.031	0.3026	-0.1	0.918
F*H	1.719	0.859	0.3026	2.84	0.007
G*H	0.453	0.227	0.3026	0.75	0.458
A*B*F	-0.531	-0.266	0.3026	-0.88	0.385
A*B*G	0.266	0.133	0.3026	0.44	0.663
A*B*H	1.141	0.57	0.3026	1.88	0.066
A*C*F	-0.094	-0.047	0.3026	-0.15	0.878
A*C*G	1.391	0.695	0.3026	2.3	0.026
A*C*H	-0.141	-0.07	0.3026	-0.23	0.817
A*D*F	-0.328	-0.164	0.3026	-0.54	0.59
A*D*G	0.625	0.312	0.3026	1.03	0.307
A*D*H	0.406	0.203	0.3026	0.67	0.505
A*E*F	0.359	0.18	0.3026	0.59	0.556
A*E*G	-0.563	-0.281	0.3026	-0.93	0.358
A*E*H	-0.469	-0.234	0.3026	-0.77	0.443
A*F*G	0.469	0.234	0.3026	0.77	0.443
A*F*H	0.531	0.266	0.3026	0.88	0.385
A*G*H	0.078	0.039	0.3026	0.13	0.898
B*C*F	-0.156	-0.078	0.3026	-0.26	0.797
B*C*G	-0.891	-0.445	0.3026	-1.47	0.148
B*C*H	-0.609	-0.305	0.3026	-1.01	0.319
B*D*F	0.266	0.133	0.3026	0.44	0.663
B*D*G	-0.563	-0.281	0.3026	-0.93	0.358
B*D*H	1.406	0.703	0.3026	2.32	0.025
B*E*F	-0.016	-0.008	0.3026	-0.03	0.98
B*E*G	0.281	0.141	0.3026	0.46	0.644
B*E*H	1.063	0.531	0.3026	1.76	0.086
B*F*G	0	0	0.3026	0	1
B*F*H	1.375	0.688	0.3026	2.27	0.028
B*G*H	0.641	0.32	0.3026	1.06	0.295
C*D*F	-1.859	-0.93	0.3026	-3.07	0.004
C*D*G	-0.125	-0.063	0.3026	-0.21	0.837
C*D*H	0.313	0.156	0.3026	0.52	0.608
C*E*F	-1.578	-0.789	0.3026	-2.61	0.012
C*E*G	-0.219	-0.109	0.3026	-0.36	0.719
C*E*H	-0.094	-0.047	0.3026	-0.15	0.878
C*F*G	-0.25	-0.125	0.3026	-0.41	0.681
C*F*H	0.406	0.203	0.3026	0.67	0.505
C*G*H	-1.516	-0.758	0.3026	-2.5	0.016
D*E*F	0.5	0.25	0.3026	0.83	0.413
D*E*G	1.266	0.633	0.3026	2.09	0.042
D*E*H	0.016	0.008	0.3026	0.03	0.98
D*F*G	-0.422	-0.211	0.3026	-0.7	0.489
D*F*H	-0.516	-0.258	0.3026	-0.85	0.399
D*G*H	0.031	0.016	0.3026	0.05	0.959
E*F*G	0.078	0.039	0.3026	0.13	0.898
E*F*H	0.672	0.336	0.3026	1.11	0.273
E*G*H	-0.594	-0.297	0.3026	-0.98	0.332
F*G*H	0.719	0.359	0.3026	1.19	0.241

For this simulation study,the model in Equation (5.1) is assumed the true model of the process and is used in simulation program to generate the data. Given the model we can generate a response surface model for the process variance:

$$\sigma_y^2 = (-7.195 - 0.844x_D)^2\sigma_H^2 + (0.422 + 1.297x_C + 0.922x_B - 0.93x_Cx_D)^2\sigma_F^2 + (-0.695)^2\sigma_G^2 + (0.859)^2\sigma_F^2\sigma_H^2 + \sigma_r^2 \quad (5.2)$$

Here, it is assumed that F, G, H are uncorrelated random variables and $\mu_F = \mu_G = \mu_H = 0$. (Note that Var(XY)= Var(X)×Var(Y) where X and Y are independent and E(X)=E(Y)=0 - see Appendix B.3 for the proof). The standard deviations of noise factors are also assumed to be all equal to 0.5 ($\sigma_F = \sigma_G = \sigma_H = 0.5$) and the value of $\sigma_r^2 = 11.72$ is taken from the ANOVA table (see MS of Residual Error in Table 5.2).

Table 5.2: ANOVA table in the Gear example

Analysis of Variance for Y

Source	DF	Seq SS	Adj SS	MS	F	P
Main Effects	8	8094.9	8094.9	1011.87	86.34	0.000
2-Way Interactions	28	877.7	877.7	31.35	2.67	0.002
3-Way Interactions	46	793.0	793.0	17.24	1.47	0.099
Residual Error	45	527.4	527.4	11.72		
Total	127	10293.1				

Considering the Equation (5.1) as the model that describes the real process, the simulator runs three different experiments (robustness, desensitization, and

Taguchi style) and then analyzes the resulting data to determine the optimum treatment combination recommended by each experiment. The experimental plans and data analyses in each approach are based on the corresponding sections in Chapter 3.

Following the desensitization approach an experiment is designed to include only the dominant cause (H) and five control factors (A, B, C, D, and E). The desensitization experiment includes a 2^{5-1} fractional factorial design for the control array and for each treatment combination of the candidates there are runs for both levels of the dominant cause. The dominant cause, factor H, is fixed at extreme levels (±1 i.e. $\pm 2\sigma_H$) and the total number of runs is determined based on the number of replicates. For example for just one replicate there will be 32 runs ($1\times2^{5-1}\times2$) and for two replicates we will have 64 runs. According to the Section 3.4, in the analysis a regression model is constructed based on the experiment results. The regression function models the response (output) in terms of the control factors and the interactions between the control factors and the dominant cause. This regression model is used to generate a response surface model for the process variance. For each simulation run, the solution is the setting that minimizes the process variance as predicted by the fitted response model.

The robustness experiment is a 2^{5-1} fractional factorial with only the five control factors. Each control factor is fixed at its low and high levels (±1) and the total numbers of runs are determined based on the number of replicates. To fairly compare the desensitization and the robustness experiment the same number of runs is considered for the two experiments. So, the number of replicates in the robustness experiment is two times of the number of replicates in the desensitization case. For two replicates in the desensitization case ($2\times2^{5-1}\times2 = 64$ runs), for instance, there would be four replicates in the robustness experiment ($4\times2^{5-1} = 64$ runs). Noise factors are varied during the experiment as three random variables.

The plan of the Taguchi experiment is the same as described for the Gear example in the Section 3.3. A 2^{5-1} fractional factorial design is used for the control array and a 2^3 full factorial design is used for the noise array. Using this plan the number of runs is 128 ($2^{5-1} \times 2^3 = 128$). So, given the described experimental plans, the number of runs in the simulated Taguchi experiment can not be less than 128, but for robustness experiment the number of runs can be the same as in the desensitization experiment.

Table 5.3 shows σ_y (i.e. square root of Equation (5.2)) for all 16 combinations of factors A to E in a 2^{5-1} fractional factorial design.

Table 5.3: Standard deviation of output for all candidates' combinations

Treatment	A	B	C	D	E	σ_y
1	-1	-1	-1	-1	1	4.8816
2	-1	-1	-1	1	-1	5.3133
3	-1	-1	1	-1	-1	4.7662
4	-1	-1	1	1	1	5.2960
5	-1	1	-1	-1	-1	4.7080
6	-1	1	-1	1	1	5.3181
7	-1	1	1	-1	1	5.0158
8	-1	1	1	1	-1	5.3642
9	1	-1	-1	-1	-1	4.8816
10	1	-1	-1	1	1	5.3133
11	1	-1	1	-1	1	4.7662
12	1	-1	1	1	-1	5.2960
13	1	1	-1	-1	1	4.7080
14	1	1	-1	1	-1	5.3181
15	1	1	1	-1	-1	5.0158
16	1	1	1	1	1	5.3642

As you can see in Table 5.3 and from Equation (5.2), the smallest output variation (4.7080) is obtained when we have either treatment 5 or 13 as the setting of fixed inputs. So optimum setting can be determined as:

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

Note that the most important control factor is D. We will use this optimum setting later to compare the suggested settings from the desensitization, robustness and Taguchi experiments.

The simulation program runs each test experiment 1000 times. For each simulation run, the proposed new process settings suggested by each experiment are evaluated using Equation (5.2) (i.e. using the true model). Then, the mean

and standard deviation of all 1000 σ_ys for each type of experiments are recorded[6]. Suggested settings are summarized in Tables 5.4 and 5.5. Looking at these tables we can say that the robustness experiment, for example, suggests factor A at its high level for 510 out of 1000 runs and at its low level for 490 times of simulation runs and suggests treatment #1 for 74 times of simulation runs. Table 5.4 also compares these recommendations with the optimum setting given by Table 5.3.

Table 5.4: Recommended settings by each method per 1000 runs of simulation

Method	Levels	A	B	C	D	E
Robustness	H	0.5100	0.4680	0.4820	0.3260	0.5020
	L	0.4900	0.5320	0.5180	0.6740	0.4980
	interpretation	high or low	high or low	high or low	high or low	high or low
Desensitization	H	0.4920	0.4820	0.5010	0.0090	0.5280
	L	0.5080	0.5180	0.4990	0.9910	0.4720
	interpretation	high or low	high or low	high or low	low	high or low
Taguchi	H	0.2880	0.1930	0.3960	0.4450	0.5360
	L	0.7120	0.8070	0.6040	0.5550	0.4640
	interpretation	low	low	high or low	high or low	high or low
Optimum setting		high or low	high	low	low	high or low

[6] See codes in Appendix C.3.

Table 5.5: Number of each treatment combination recommended by each experiment for 1000 runs of simulation

Treatment	A	B	C	D	E	σ_y	Desensitization	Robustness	Taguchi
1	-1	-1	-1	-1	1	4.8816	134	74	152
2	-1	-1	-1	1	-1	5.3133	0	39	149
3	-1	-1	1	-1	-1	4.7662	131	74	138
4	-1	-1	1	1	1	5.2960	2	47	112
5	-1	1	-1	-1	-1	4.7080	120	77	67
6	-1	1	-1	1	1	5.3181	1	54	50
7	-1	1	1	-1	1	5.0158	141	82	15
8	-1	1	1	1	-1	5.3642	0	49	11
9	1	-1	-1	-1	-1	4.8816	132	94	0
10	1	-1	-1	1	1	5.3133	0	50	85
11	1	-1	1	-1	1	4.7662	117	89	87
12	1	-1	1	1	-1	5.2960	0	48	31
13	1	1	-1	-1	1	4.7080	110	72	29
14	1	1	-1	1	-1	5.3181	2	36	7
15	1	1	1	-1	-1	5.0158	110	79	2
16	1	1	1	1	1	5.3642	0	36	1
Possible values for σ_y							4.7080, 4.7662, 4.8816, 5.0158	4.7080, 4.7662, 4.8816, 5.0158, 5.2960, 5.3133, 5.3181, 5.3642	4.7080, 4.7662, 5.0158, 5.2960, 5.3133, 5.3181

The mean and standard deviation of calculated σ_ys for each experiment are shown in Table 5.6.

Table 5.6: Calculated performance measures in each method

Number of Runs	Robustness		Desensitization		Taguchi method	
	Mean of σ_ys	Standard deviation of σ_ys	Mean of σ_ys	Standard deviation of σ_ys	Mean of σ_ys	Standard deviation of σ_ys
32	5.0590	0.2523	4.9140	0.1957		
64	4.9930	0.2456	4.8745	0.1627		
128	4.9603	0.2356	4.8458	0.1229	4.9047	0.1717

Table 5.4 indicates that the desensitization experiment suggests the level of the most important factor (i.e. factor D) correctly in 99 percent of simulation runs. The reason that D is the most important factor (for making the process insensitive to the variation in the dominant cause) is that factor D is the only fixed input that has interaction with the dominant cause (see Equation(5.1))

From Table 5.5, it we see that the desensitization experiment more likely leads to small values of σ_y compared with the robustness and Taguchi experiments. The largest possible value of σ_y using the desensitization experiment is 5.0158 while it is 5.3642 and 5.3181 in the robustness and Taguchi experiments respectively.

Table 5.6 summarizes the simulation results. Note that the best method will yield the lowest average and the least variation in σ_ys. The results in Table 5.6 shoe that the desensitization experiment has the lowest average of the σ_ys and thus the highest efficiency comparing with the robustness and Taguchi experiments regardless of the number of runs.

Moreover, if we compare the desensitization experiment in the case that has only 32 runs with the Taguchi experiment (with 128 runs); it is revealed that the desensitization experiment with 4 times fewer runs has almost the same efficiency of the Taguchi experiment. In other words, using the knowledge of

dominant cause, a desensitization experiment which is smaller, easier and consequently cheaper (in desensitization experiment you need to fix fewer noise factors than in a Taguchi experiment) can be conducted and the same efficiency and results of a much larger Taguchi experiment can be expected.

Equally important, the choice of noise factors in a Taguchi experiment is a critical issue. As mentioned in Chapter 4, Taguchi recommends using engineering judgment to select the noise factors and assumes that the choice includes all important noise factors. However, if the dominant cause is not known there is a risk of excluding the dominant cause from the outer array. This risk is one of the Taguchi method's main drawbacks. To assess this the consequences of risk we decided to exclude the dominant cause (e.g. H) from Taguchi experiment plan and then rerun the simulation and analyze the obtained data. Note that as we now have a 2^2 full factorial design for the outer array, we can also use a 64-run Taguchi experiment. Comparing the results in Table 5.7 with those in Table 5.6 shows that without the dominant cause in the noise array the Taguchi approach is the weakest approach.

Table 5.7: Performance measures of Taguchi method (the dominant cause H is excluded)

Number of Runs	Taguchi method	
	Mean of σ_ys	Standard deviation of σ_ys
64	5.0443	0.2647
128	5.0356	0.2556

So, a potential drawback of the Taguchi method experiments is that it depends critically on how well the noise factors are chosen. If the dominant cause is absent from the experimenter choice of noise factors, the experiment will likely fail. In the desensitization case, however, the dominant cause is known and we do not need to worry about the selection of noise factors.

5.4 Summary and Conclusions

In this chapter, a real-world problem with the goal of variation reduction of output characteristic using robust parameter design was modeled and considered. Using MATLAB, a simulation study was run to compare the efficiency of the desensitization experiment with two alternative methods in the context of the Gear example.

All together, it was shown that desensitization is not only the cheapest and the most convenient approach but also the most effective approach to robust parameter design. Using the example it was also shown that the efficiency of Taguchi experiment depends critically on which noise factors were chosen. If the dominant cause is not one of the selected noise factors the Taguchi experiment performs poorly. The next chapter discusses the situation in which the desensitization approach is not recommended and briefly talks about the conditions that should be met for the desensitization method to be implemented successfully.

Chapter 6

Desensitization Approach:

Capability and Feasibility

6.1 Introduction

Finding the dominant cause and identifying the new process settings that make the process less sensitive to variation in the dominant cause, are not the only requirements for the successful implementation of desensitization.

In fact, there are situations in which the desensitization approach is not an appropriate approach for finding a robust solution. In this chapter, we first discuss the situations where the desensitization method is not recommended and then we talk about assessing feasibility and validating of a robust solution suggested by the desensitization method.

6.2 Capability of the Desensitization Method

As mentioned before, a product's development cycle can be partitioned into three main stages: product design, process design, and manufacturing. At the product design stage engineers develop complete product design specifications including the specifications of materials, components, configuration and features. Next, process engineers design a manufacturing process to produce the product. The manufacturing department then uses the manufacturing process to produce many units of the product.

Thorough this thesis we advocated the desensitization method as the best approach to variation reduction in the manufacturing stage since we believe the most important source(s) of noise, i.e. the varying input(s) that have the largest effect on the overall output variation, can be found by investigating the existing

processes using observational studies. In product and process stages, on the other hand, the desensitization approach is not an appropriate approach because in these stages you can only predict and suspect some sources of noises (usually external noises such as temperature, humidity, dust, vibration, and human variations in operating the product) as the important noise factors and you are likely not able to exactly determine the dominant cause of variation in an output characteristic. You can include the suspected noise factors in the outer array to conduct a parameter design experiment. Thus, a Taguchi style experiment is the recommended approach in the product and process stages. In the product and process design stages, experimenters use their engineering knowledge and judgment to select noise factors of a Taguchi experiment.

We claim that the desensitization approach is the most effective approach in the manufacturing stage. This is true if you can find the dominant cause and you are able to fix the levels of dominant cause during the experiment. When the experimenters are not able to find the dominant cause of output variation despite all of their preliminary efforts, or when they find the dominant cause but it is impossible to fix the levels of dominant cause even for a short time during the experiment runs, a desensitization experiment would not be a feasible method. In this case, running a robustness experiment is probably the last hope. Steiner

and MacKay (2005) give an example where the problem was excessive variation in a crossbar dimension and experimenters were not able to hold the dominant cause fixed at its low or high level for a run of the desensitization experiment. Thereby, they resort to a robustness experiment as an alternative. The team found the barrel temperature as a dominant cause and raised the barrel temperature set point to solve the problem. But, with the new setting, the frequency of a mold defect, called burn, was increased. They suspected that the defect occurred when the mold cavity filed too fast. Since the dominant cause could not be controlled during a desensitization experiment they decided to conduct a robustness experiment. Then, they planned an experiment with four fixed inputs and they found a robust solution. An interesting point in the crossbar example is that although they were not able to proceed with a desensitization experiment they had a good choice of candidates based on the knowledge of dominant. The four fixed inputs (injection speed, injection pressure, back pressure, and screw speed) were selected based on their influence on fill time (i.e. dominant cause) and the robustness experiment was successful because of this correct choice. In other words, even if a desensitization approach is not potentially feasible in a specific case it is usually worth searching for a dominant cause of variation since the knowledge of dominant cause can be used

in the choice of candidates which improves the efficiency of either a desensitization experiment or a robustness experiment.

6.3 Assessing Feasibility and Validating the Desensitization Method

After finding the dominant cause, the feasibility of a desensitization experiment needs to be assessed. There are a few general rules to help us assess the feasibility of a variation reduction approach including the consideration of the costs and the likelihood of success.

In some cases, once a dominant cause has been identified, the remedy is obvious and the team does not need to conduct an experimental investigation. The definition of *obvious solution* depends on the process and the level of process knowledge. Analysts have an obvious fix if they are confident that it is feasible (Steiner and MacKay, 2005). In some other cases, once a dominant cause is determined, we can reduce output variation by reducing the variation in the dominant cause (i.e. the dominant cause is controllable).

Conducting a desensitization experiment is not recommended if the dominant cause is controllable or an obvious fix is available unless these ways of reducing

variation are not cost effective and finding a robust solution (i.e. making the process less sensitive to the variation of dominant cause) is preferable.

To assess the feasibility of desensitization, we consider the costs and likelihood of success. The costs of the desensitization approach include the cost of running an experiment to find the new levels of candidates that make the process less sensitive to variation in the dominant cause, the cost of a one-time change to the process settings (to implement the suggested new settings we need to change the current levels of candidates), and the cost of the ongoing operation of the process with the new settings. Although the desensitization approach is more effective than the robustness approach there is no information about whether either approaches will be feasible until the experimental investigation is complete. This is a common drawback of different approaches to robust parameter design.

Assuming that the process is well centered, to successfully implement desensitization, we must (Steiner and MacKay, 2005):

- Find the dominant cause and identify the fixed inputs (candidates) and their new levels that make the process less sensitive to variation in the dominant cause.
- Check for potential negative side effects of new settings.

- Estimate the costs of changing the settings and the new ongoing operating costs.
- Estimate the benefits of new settings and resultant variation reduction.

Desensitization is a viable option when all of these tasks are accomplished and the benefits prevail over the costs. The benefits can be assessed using the relationship between the dominant cause and the output. The maximum gain is given if the effect of the dominant cause could be totally eliminated.

Regardless of the chosen variation reduction approach it is advisable to validate the solution by checking the proposed solution to see whether the goal is met and also to check for any unexpected negative side effects due to the new settings of process. Moreover, we need to ensure that the implemented change is made permanent to have a lasting impact and to preserve the gains.

To validate a robust solution we need to first implement the solution and next, conduct a validation observational investigation to compare the new baseline with the initial baseline

6.4 Summary

In this chapter, we described the situations in which the desensitization approach is not feasible. The feasibility of desensitization was also discussed and the conditions that need to be met for the desensitization method to be implemented successfully were briefly presented. Finally, the importance of the validation stage of the Statistical Engineering algorithm was mentioned and the key tasks necessary to complete the implementation and validation of a robust solution were summarized.

Chapter 7

Conclusions and Future Research

7.1 Conclusions

Reducing variation in critical output characteristics of a product in the stage of manufacturing was considered in this thesis and the role of dominant cause in the variation reduction through Robust Parameter Design (RPD) was explored. A qualitative and quantitative comparison of the desensitization approach versus robustness and Taguchi approaches was presented and both kinds of

comparisons suggested the desensitization method is the cheapest, most convenient, and most effective approach to the RPD at the manufacturing stage of a product development life cycle. This result was reconfirmed by considering a real world problem and comparing the three different approaches in the context of that problem. To run a desensitization experiment we need knowledge of dominant cause(s) of output variation.

As a result, searching for the dominant cause of variation is highly recommended before proceeding to any experimental investigation to look for a robust solution. After finding the dominant cause, if an obvious solution is not evident and the dominant cause can be controlled temporarily, we suggest conducting a desensitization experiment to find a robust solution.

The robustness approach can be selected as a last hope when it is hard to fix the levels of the dominant cause during a desensitization experiment or when we can't find the dominant cause.

Finally, after conducting the desensitization experiment and finding the solution, the feasibility of the solution must be assessed and the validated to ensure that the desensitization is implemented successfully. In the validation stage, we need to first implement the solution and next conduct a validation observational

investigation to compare the new baseline with the initial baseline and also to monitor for unexpected negative side effects.

7.2 Suggestions for Future Research

The research contained in this thesis opens up a range of new avenues for future productive research. The following are some suggested area of research:

1. Expand the quantitative comparison of desensitization, robustness, and Taguchi experiment to processes with discrete output. The quantitative comparison presented in Chapter 4 only considered continues outputs. Since discrete outputs occur frequently in process variation reduction projects, it is useful to consider these situations and present a similar quantitative comparison.
2. Expand the quantitative comparison under dependence of the noise factors. In the presented quantitative comparison we assumed that all noise factors are independent. This assumption can be relaxed and the quantitative comparison can be developed for handling the situations for which there exists dependency among the noise factors.

3. Explore the diagnostic journeys of well known variation reduction methodologies and suggest the most efficient approach for finding the dominant cause.
4. Investigate the product and process stages of a product development cycle and suggest the most effective variation reduction approach to robust parameter design for each stage.

Appendix A

Resolution in Fractional Factorial Experiments

A brief description of the resolution in fractional factorial experiments is given in this appendix. We discuss this concept only for two-level fractional factorial designs.

The most intuitive approach to study and estimate the effects of a number of inputs simultaneously would be to vary the factors of interest in a full factorial

design, that is, to try all possible combinations of settings. This would work fine, except when the number of experimental runs is limited. Since each run may require time-consuming and costly settings and resetting of machinery, experiments with large numbers of run are often not feasible. In these cases, *fractional factorials* are used to reduce the number of runs by sacrificing some interaction effects so that main effects may still be computed correctly. A technical description of how fractional factorial designs are constructed is beyond the scope of this thesis and detailed accounts of 2^{k-p} fractional factorial experiments can be found, for example, in Box and Draper (1987), Box, Hunter, and Hunter (1978), Montgomery (2001), Deming and Morgan (1993), and in many other text books on this subject. In general, a fractional factorial design uses the high-order interactions to generate new factors. For example, consider the following design (Table A.1) that includes 9 factors but requires only 16 runs (instead of 2^9=512 required runs in a full factorial design). You may wonder how we found the column of signs for the factors in Table A.1. Note that the columns of signs for factor A, B, C, and D match a 2^4 full factorial design; but how about E, F, G, H, and J?

To find the column of signs of E, for instance, the corresponding columns for A, B, and C is multiplied. We use the convenient notation "$E = ABC$" and consider it

as one of this particular design's "*generators*". We also call "*I = EABC*" the "*defining relation*" of the design (by multiplying the column signs of E, A, B, and C a column of +1s, noted by I, is obtained). For the fractional factorial design, presented in Table A.1, the defining relation and design generators are as follow.

Design Generators:

E = ABC, F = BCD, G = ACD, H = ABD, J = ABCD

Defining Relation:

I = EABC = FBCD = GACD = HABD = JABCD = JED = JAF = JBH

The design given in Table A.1 is described as a 2^{9-5} fractional factorial design of resolution III. This means that you study overall 9 factors; however, 5 of those factors were generated from some interactions of a $2^{(9-5=4)}$ full factorial design. As a result, the design does not give full resolution; that is, there are certain interaction effects that are confounded or aliased with other effects. In general, the resolution of a fractional factorial design is determined by the length of the shortest "word" (i.e. combinations of letters representing factors) in the defining relation of the experimental design. The shortest word in the defining relation of presented design has three letters, so, the resolution of design is III or three.

Table A.1: 2^{9-5} fractional factorial design with resolution III

Run	A	B	C	D	E	F	G	H	J
1	-1	-1	-1	-1	-1	-1	-1	-1	1
2	-1	-1	-1	1	-1	1	1	1	-1
3	-1	-1	1	-1	1	1	1	-1	-1
4	-1	-1	1	1	1	-1	-1	1	1
5	-1	1	-1	-1	1	1	-1	1	-1
6	-1	1	-1	1	1	-1	1	-1	1
7	-1	1	1	-1	-1	-1	1	1	1
8	-1	1	1	1	-1	1	-1	-1	-1
9	1	-1	-1	-1	1	-1	1	1	-1
10	1	-1	-1	1	1	1	-1	-1	1
11	1	-1	1	-1	-1	1	-1	1	1
12	1	-1	1	1	-1	-1	1	-1	-1
13	1	1	-1	-1	-1	1	1	-1	1
14	1	1	-1	1	-1	-1	-1	1	-1
15	1	1	1	-1	1	-1	-1	-1	-1
16	1	1	1	1	1	1	1	1	1

As you can see in Table A.2, in resolution III design, main effects are aliased with two-way interactions but not with other main effects. In a resolution IV, design, however, main effects are confounded with three-way or higher-order interactions, and two-way interaction effects are confounded with other two-way interaction effects. And in a resolution V design, main and two-way interaction effects are confounded only with three-way or higher-order interactions and with such a design you are able to separately estimate all main effects and two-way interactions.

Table A.2: Alias structure of a 2^{9-5} fractional factorial design with resolution III

```
Alias Structure (up to order 4)

I + AFJ + BGJ + CHJ + DEJ + ABCE + ABDH + ABFG + ACDG + ACFH + ADEF +
 AEGH +
     BCDF + BCGH + BDEG + BEFH + CDEH + CEFG + DFGH

A + FJ + BCE + BDH + BFG + CDG + CFH + DEF + EGH + ABGJ + ACHJ + ADEJ +
 BCDJ +
     BEHJ + CEGJ + DGHJ
B + GJ + ACE + ADH + AFG + CDF + CGH + DEG + EFH + ABFJ + ACDJ + AEHJ +
 BCHJ +
     BDEJ + CEFJ + DFHJ
C + HJ + ABE + ADG + AFH + BDF + BGH + DEH + EFG + ABDJ + ACFJ + AEGJ +
 BCGJ +
     BEFJ + CDEJ + DFGJ
D + EJ + ABH + ACG + AEF + BCF + BEG + CEH + FGH + ABCJ + ADFJ + AGHJ +
 BDGJ +
     BFHJ + CDHJ + CFGJ
E + DJ + ABC + ADF + AGH + BDG + BFH + CDH + CFG + ABHJ + ACGJ + AEFJ +
 BCFJ +
     BEGJ + CEHJ + FGHJ
F + AJ + ABG + ACH + ADE + BCD + BEH + CEG + DGH + BCEJ + BDHJ + BFGJ +
 CDGJ +
     CFHJ + DEFJ + EGHJ
G + BJ + ABF + ACD + AEH + BCH + BDE + CEF + DFH + ACEJ + ADHJ + AFGJ +
 CDFJ +
     CGHJ + DEGJ + EFHJ
H + CJ + ABD + ACF + AEG + BCG + BEF + CDE + DFG + ABEJ + ADGJ + AFHJ +
 BDFJ +
     BGHJ + DEHJ + EFGJ
J + AF + BG + CH + DE + ABCD + ABEH + ACEG + ADGH + BCEF + BDFH + CDFG
 + EFGH
AB + CE + DH + FG + AGJ + BFJ + CDJ + EHJ + ACDF + ACGH + ADEG + AEFH +
 BCDG +
     BCFH + BDEF + BEGH
AC + BE + DG + FH + AHJ + BDJ + CFJ + EGJ + ABDF + ABGH + ADEH + AEFG +
 BCDH +
     BCFG + CDEF + CEGH
AD + BH + CG + EF + AEJ + BCJ + DFJ + GHJ + ABCF + ABEG + ACEH + AFGH +
 BCDE +
     BDFG + CDFH + DEGH
AE + BC + DF + GH + ADJ + BHJ + CGJ + EFJ + ABDG + ABFH + ACDH + ACFG +
 BDEH +
     BEFG + CDEG + CEFH
AG + BF + CD + EH + ABJ + CEJ + DHJ + FGJ + ABCH + ABDE + ACEF + ADFH +
 BCEG +
     BDGH + CFGH + DEFG
AH + BD + CF + EG + ACJ + BEJ + DGJ + FHJ + ABCG + ABEF + ACDE + ADFG +
 BCEH +
     BFGH + CDGH + DEFH
```

Appendix B

Calculations and Proofs

B.1 Performance Indexes (one noise factor and one control factor)

- **Performance index in desensitization case (Std(P_{des}))**

  $k : \#\ of\ replicates$

  $Model : Y_i = \beta_0 + \beta_1 x_i + \beta_2 z_i + \beta_3 z_i x_i + R_i$ i= 1, 2 ,&, 4k

  or

  $$Y_i - \beta_1 x_i = \beta_0 + \beta_2 z_i + \beta_3 z_i x_i + R_i$$

$x \sim N(0,\sigma_x^2) \quad R \sim N(0,\sigma_r^2)$

$$x_i = \begin{cases} +2\sigma_x & i = 1,\cdots,k \\ -2\sigma_x & i = k+1,\cdots,2k \\ +2\sigma_x & i = 2k+1,\cdots,3k \\ -2\sigma_x & i = 3k+1,\cdots,4k \end{cases} \qquad z_i = \begin{cases} +a & i = 1,\cdots,2k \\ -a & i = 2k+1,\cdots,4k \end{cases}$$

Model in matrix notation:

$$\mathbf{Z} = X\,\tilde{\beta}$$

where

$$\mathbf{Z} = Y_i - \beta_1 x_i \quad \text{and} \quad \tilde{\beta} = \begin{bmatrix} \beta_0 \\ \beta_2 \\ \beta_3 \end{bmatrix} \quad X = \begin{bmatrix} 1 & z_1 & x_1 z_1 \\ 1 & z_2 & x_2 z_2 \\ \vdots & \vdots & \vdots \\ 1 & z_i & x_i z_i \end{bmatrix}$$

$$COV(\tilde{\beta}) = \sigma_r^2 (X^T X)^{-1} \Rightarrow VAR(\tilde{\beta}_3) = \sigma_r^2 (X^T X)^{-1}_{33}$$

$$X^T X = \begin{bmatrix} 1 & 1 & \cdots & 1 \\ z_1 & z_2 & & z_i \\ \vdots & \vdots & & \vdots \\ x_1 z_1 & x_2 z_2 & & x_i z_i \end{bmatrix} \begin{bmatrix} 1 & z_1 & x_1 z_1 \\ 1 & z_2 & x_2 z_2 \\ \vdots & \vdots & \vdots \\ 1 & z_i & x_i z_i \end{bmatrix} = \begin{bmatrix} 4k & 0 & 0 \\ 0 & \sum z_i^2 & 0 \\ 0 & 0 & \sum x_i^2 z_i^2 \end{bmatrix}$$

$$\Rightarrow (X^T X)^{-1} = \begin{bmatrix} 1/4k & 0 & 0 \\ 0 & 1/\sum z_i^2 & 0 \\ 0 & 0 & 1/\sum x_i^2 z_i^2 \end{bmatrix}$$

and $VAR(\tilde{\beta}_3) = \sigma_r^2 (X^T X)^{-1}_{33} = \sigma_r^2 \left(\frac{1}{\sum x_i^2 z_i^2}\right) = \frac{\sigma_r^2}{16ka^2\sigma_x^2}$

$$P_{des} = (\underbrace{\beta_1 + \hat{\beta}_3 z}_{A})^2 \sigma_x^2 + \sigma_r^2 \quad and \quad VAR(P_{des}) = \sigma_x^4 VAR(A^2)$$

$$A = \beta_1 + \hat{\beta}_3 z$$

$$\mu_A = \beta_1 + \beta_3 z$$

$$\sigma_A^2 = VAR(\hat{\beta}_3) z^{\ 2}$$

$$\Rightarrow \sigma_A^2 = a^2 \frac{\sigma_r^2}{16ka^2\sigma_x^2} \Rightarrow \boxed{\sigma_A^2 = \frac{\sigma_r^2}{16k\,\sigma_x^2}}$$

$$A \sim N(\mu_A, \sigma_A^2) \Rightarrow \left(\frac{A}{\sigma_A}\right)^2 \sim \chi_1^2(\lambda)\ \&\ \lambda = \left(\frac{\mu_A}{\sigma_A}\right)^2$$

$$A^2 \sim \sigma_A^2 \chi_1^2(\lambda) \Rightarrow$$

$$VAR(A^2) = \sigma_A^4 VAR(\chi_1^2(\lambda)) = \sigma_A^4\, 2(1+2\lambda) = 2\sigma_A^4(\sigma_A^2 + 2\mu_A^2)$$

$$VAR(P_{des}) = \sigma_x^4 VAR(A^2) \Rightarrow VAR(P_{des}) = \frac{\sigma_r^4 + 2\sigma_r^2(16k\ \mu_A^2\ \sigma_x^2)}{8(4k)^2}$$

$$\therefore \quad Std(P_{des}) = \sqrt{\frac{\sigma_r^4 + 2\sigma_r^2(16k\ \mu_A^2\ \sigma_x^2)}{8(4k)^2}}$$

- **Performance index in robustness case(Std(P_{rob}))**

$$P = VAR(Y) = s^2$$

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

where n (# of data observations used in the calculation s^2) is equal to $2k$. So :

$$VAR(P) = \frac{2\sigma_y^4}{2k-1} \quad where \quad \sigma_y^2 = (\mu_A^2\sigma_x^2 + \sigma_r^2)$$

$$\therefore \quad Std(P_{rob}) = \sqrt{\frac{2\sigma_y^4}{2k-1}}$$

B.2 Performance Indexes ("m" noise factors and "n" control factors)

- **Performance index in desensitization case (Std(P_{des}))**

k : # of replicates

$i = 1, 2, ..., m$: # of noise factors

$j = 1, 2, ..., n$: # of control factors

$$Y = \beta_0 + \sum_{i=1}^{m} \beta_i x_i + \sum_{j=1}^{n} \beta_{m+1} z_j + \sum_{i=1}^{m} \sum_{j=1}^{n} \beta_{ij} x_i z_j + R$$

$$x_i \sim N(0, \sigma_{x_i}^2) \quad R \sim N(0, \sigma_r^2)$$

$$x_i = \begin{cases} +2\sigma_{x_i} \\ -2\sigma_{x_i} \end{cases} \quad z_j = \begin{cases} +a_j \\ -a_j \end{cases}$$

$$P_{des} = \sum_{i=1}^{m} (\underbrace{\beta_i + \sum_{j=1}^{n} \hat{\beta}_{ij} z_j}_{A_i})^2 \sigma_{x_i}^2 + \sigma_r^2$$

$$VAR(\hat{\beta}_{ij}) = \frac{\sigma_r^2}{(2\sigma_{x_i} a_j)^2 (2^n \times 2^m) k}$$

$$VAR(P_{des}) = \sum_{i=1}^{m} \sigma_{x_i}^4 VAR(A_i^2) \quad , A_i \sim N(\mu_{A_i}, \sigma_{A_i}^2)$$

$$A_i = \beta_i + \sum_{j=1}^{n} \hat{\beta}_{ij} z_j$$

$$\mu_{A_i} = \beta_i + \sum_{j=1}^{n} \beta_{ij} z_j$$

$$\sigma_{A_i}^2 = \sum_{j=1}^{n} VAR(\hat{\beta}_{ij}) z_j^2 = \frac{n\sigma_r^2}{(2\sigma_{x_i})^2 (2^n \times 2^m) k}$$

Ai are independent, normally distributed random variables with means μ_{A_i} and variances $\sigma_{A_i}^2$. Thus,

$$\sum_{i=1}^{m}\left(\frac{A_i}{\sigma_{A_i}}\right)^2 \sim \chi_m^2(\lambda) \text{ and } \lambda = \sum_{i=1}^{m}\left(\frac{\mu_{A_i}}{\sigma_{A_i}}\right)^2, VAR(\chi_m^2(\lambda)) = 2(m+2\lambda)$$

$$VAR(P_{des}) = \sum_{i=1}^{m}\sigma_{x_i}^4 VAR(A_i^2)$$

$$\sigma_{A_i}^2 = \frac{n\sigma_r^2}{(2\sigma_{x_i})^2(2^n \times 2^m)k}$$

Considering above euations and following similar procedure that we had in the case of one noise and one control factors, we can formulate the performance index in the desensitization case as

$$\mathrm{VAR}(\mathrm{P_{des}}) = \frac{\mathrm{n}^2\sigma_\mathrm{r}^4\ 2(\mathrm{m}+2\lambda)}{16\,(2^\mathrm{n}\times 2^\mathrm{m}\mathrm{k})^2}$$

$$\therefore \quad \mathrm{Std}(\mathrm{P_{des}}) = \sqrt{\frac{\mathrm{n}^2\sigma_\mathrm{r}^4\ 2(\mathrm{m}+2\lambda)}{16\,(2^\mathrm{n}\times 2^\mathrm{m}\mathrm{k})^2}}$$

- **Performance index in robustness case(Std(P_{rob}))**

$$P_{rob} = VAR(Y) = s^2$$

$$s^2 \sim \frac{\sigma_y^2}{n-1}\chi^2_{n-1} \Rightarrow VAR(s^2) = VAR(P_{rob}) = \left(\frac{\sigma_y^2}{n-1}\right)^2 2(n-1) = \frac{2\sigma_y^4}{n-1}$$

where n (# of observations used in *the calculation of* s^2*) for this case isequal to* $2^m \times k$*. So* :

$$VAR(P_{rob}) = \frac{2\sigma_y^4}{2^m k - 1} \quad where \quad \sigma_y^2 = (\sum_{i=1}^{m} \mu_{A_i}^2 \sigma_{x_i}^2) + \sigma_r^2$$

$$\therefore \quad Std(P_{rob}) = \sqrt{\frac{2\left[(\sum_{i=1}^{m} \mu_{A_i}^2 \sigma_{x_i}^2) + \sigma_r^2\right]^2}{2^m k - 1}}$$

To prove that performance index in the case of desensitization is less than performance index in the case of robustness (i.e. desensitization is more efficient than robustness), we need to show:

$$\frac{2\sigma_y^4}{2^m k - 1} \quad \rangle \quad \frac{n^2\sigma_r^4\ 2(m+2\lambda)}{16\,(2^n \times 2^m k)^2} \qquad \text{(B.2.1)}$$

$$\text{or} \quad \frac{2\left[(\sum_{i=1}^{m} \mu_{A_i}^2 \sigma_{x_i}^2) + \sigma_r^2\right]^2}{2^m k - 1} \quad \rangle \quad \frac{n^2\sigma_r^4\ 2(m+2\lambda)}{16(2^n \times 2^m k)^2}$$

and denoting $\sum_{i=1}^{m} \mu_{A_i}^2 (2\sigma_{x_i})^2$ by <u>d</u> we need to prove that

$$2\left[(d + 4\sigma_r^2)^2\right](2^n \times 2^m k)^2 \quad \rangle \quad 2n\sigma_r^2(mn\sigma_r^2 + 2(2^n \times 2^m)kd)(2^m k - 1)$$

or

$$2(2^n \times 2^m k)\left[\frac{1}{2}(2^n \times 2^m k)d^2 + 8(2^n \times 2^m k)\sigma_r^4 + 4(2^n \times 2^m)kd\sigma_r^2\right] \quad \rangle$$
$$n(2^m k - 1)(mn\sigma_r^4 + 4(2^n \times 2^m)kd\sigma_r^2)$$

As $2(2^n \times 2^m k)$ is greater than $n(2^m k - 1)$ for all natural numbers (in next page it has been proved using Mathematical Induction), we still have a true expression if we do not have $4(2^n \times 2^m)kd\sigma_r^2$ in the both sides brackets of above inequality:

$$2(2^n \times 2^m k)\left[\frac{1}{2}(2^n \times 2^m k)d^2 + 8(2^n \times 2^m k)\sigma_r^4\right] \quad \rangle$$
$$n(2^m k - 1)(mn\sigma_r^4) \qquad \text{(B.2.2)}$$

In this expression as it is mentioned before $2(2^n \times 2^m k)$ is greater than $n(2^m k - 1)$ and by Mathematical Induction we can also prove that $8(2^n \times 2^m k)\sigma_r^4 \rangle mn^2\sigma_r^4$ (or in the other word, $8(2^n \times 2^m) \rangle mn^2$) is true of all natural numbers (see next subsection). So, expression (B.2.2) and consequently expression (B.2.1) is true.

- **Mathematical Induction**

P(n,m): $8(2^n \times 2^m) \rangle mn^2$ or $2^{n+m+3} \rangle mn^2$

for all m and n natural numbers.

To prove above expression we prove $2^m \rangle m$ and also $2^{n+3} \rangle n^2$; then by multiplying these two expression we will get p(n,m).

a) $2^m \rangle m$

1. p(1) is true: $2^1 \rangle 1$

2. Assume that, for an arbitary n, p(m) is also true ; i.e. $2^n \rangle n$. We need to show that p(n+1) is also true; i.e. $2^{(n+1)} \rangle n+1$

$2^{n+1} \rangle n+1$ or $2^n+2^n \rangle n+1$; we assumed that $2^n \rangle n$, by cancelling out this from both side we have: $2^n \rangle 1$

which is a true expression. So, $2m \rangle m$ is true for all m natural numbers.

b) $2^{n+3}>n^2$

1. p(1) is true: $2^4 \rangle 1^2$

2. Assume that, for an arbitrary n, p(n) is also true ; i.e. $2^{n+3} \rangle n^2$. We need to show that p(n+1) is also true; i.e. $2^{(n+1)+3} \rangle (n+1)^2$

$2^{n+4} \rangle n^2+1+2n$ or $2^{n+3}+2^{n+3} \rangle n^2+1+2n$; we assumed that $2^{n+3} \rangle n^2$, by cancelling out this from both side we have: $2^{n+3} \rangle 1+2n$

Now we should prove that $2^{n+3} \rangle 1+2n$

1. p(1) is true: $2^4 \rangle 1+2$

2. Assume p(n) is also true: $2^{n+3} \rangle 1+2n$

3. Should prove that p(n+1) is also true:

$2^{(n+1)+3} \rangle 1+2(n+1)$

or $2^{n+3}+2^{n+3} \rangle 1+2n+2$

we assumed that $2^{n+3} \rangle 1+2n$, by cancelling out this from both side we have: $2^{n+3} \rangle 3$. which is a true statement.

B.3 Proof of a Theorem

Theorem:

Let X and Y two independent normal variables and E(X)=E(Y)=0. Then the Variance of XY is equal to Var (X) × Var(Y)

Proof:

Since
$$\begin{cases} E[(XY)^2] = E(X^2) \times E(Y^2) = [E(XY)]^2 + Var(XY) \\ E(X^2) = E(X)^2 + Var(X) \\ E(Y^2) = E(Y)^2 + Var(Y) \end{cases}$$

We have,

$E[(XY)^2] = [E(XY)]^2 + Var(XY) = E(X^2) \times E(Y^2) = [E(X)^2 + Var(X)] \times [E(Y)^2 + Var(Y)]$

But E(X)=E(Y)=0. So,

$[E(XY)]^2 + Var(XY) = Var(X) \times Var(Y)$ (B.3.1)

However, $[E(XY)]^2 = [E(X) \times E(Y)]^2 = E(X)^2 \times E(Y)^2 = 0$ (B.3.2)

Substituting (B.3.2) into. (B.3.1) gives,

$Var(XY) = Var(X) \times Var(Y)$

Appendix C

Simulation Codes[7]

C.1 Validating Performance Measures using Simulation

```
function [varrobust,vardesen,varprob,varpdes]=robustnessjun1(k);
%estimate the stdev of the output Y with desensitization and robustness
%use levels for the fixed input of z and -z
%model: Y = b0 + b1X + b2Z + b3X*Z + R
%X is the varying input X~N(0,sigx^2)
```

[7] Presented codes were written and run using MATLAB™

```
beta1=1; beta0=0;  beta2=0;   beta3=1;  %assumptions
sigR=1;  sigx=.3;
%4*k = number of runs
%generate the experimental results for robustness experiment using the above model
r = normrnd(0,1,4*k,1);    %R~N(0,1) - var(R)=1
x = normrnd(0,sigx,4*k,1);   %generate X randomly

%generate levels of z

zd=fracfact('a b ');
zd2=fracfact('a b');

if k==1
   zd=zd;
else
for i=2:k;

zd=[zd;zd2];

end; end;
zd1=[zd(:,1)];

z=zd1(:,1);

yrobust=beta0+beta1*x+beta2*z+beta3*z.*x+r;

%estimate of std of Y by robustness

y1=yrobust(1:2);
y2=yrobust(3:4);

if k==1

 varrobust=[var(y1);var(y2)];

else
for i=2:k;

   y1=[y1;yrobust((length(y1)*2)+1:length(y1)*2+2)];
   y2=[y2;yrobust((length(y2)*2)+3:length(y2)*2+4)];

end; end;

 varrobust=[var(y1);var(y2)];

%generate the experimental results for desensitization experiment using the above model
x=zd(:,2).*2*sigx;
%levels of fixed input z (need two levels to estimate B3)
r = normrnd(0,1,4*k,1);

ydens=beta0+beta1*x+beta2*z+beta3*z.*x+r;
```

```
%estimate of std OF y by desensitization, i.e. estimate B3

xdd=[ones(length(x),1) x z x.*z];
a=regress(ydens,xdd);
beta3hat=a(4,1);
%since stdev(Y) can be written as sqrt((b1+b3*z)^2*std(x)^2+std(R) we get

vardesen=(beta1+(beta3hat.*z)).^2.*(sigx^2)+sigR^2;
vardesen=[vardesen(1);vardesen(3)];

vary=(beta1+(beta3.*z)).^2.*(sigx^2)+sigR^2;
vary=[vary(1);vary(3)];

%since in desensetization we have p=(beta1+(beta3hat*z))^2*(sigx^2)+sigR^2 and
%var(p)=(sigR^2(sigR^2+2(16k)*sigx^2*mua^2))/8(4k)^2, we get
vara=((sigR^2)/(16*k*(sigx)^2));

mua=beta1+(beta3.*z);
landa=(((mua).^2)/vara);

varpdes=((sigR^4)*2*(1+2.*landa))/(16*(4*k)^2);

varpdes=[varpdes(1);varpdes(3)];

%since in robustness we have p= var(y) and var(p)=2*(sigR^4)/(2*k-1),
%we get
varprob=2.*(vary.^2)/((2*k)-1);

-------------------------------------------------
function [stdsrobv,stdsdesv]=comparejun1(k,t);
srobv=[]; sdesv=[];

 for nsim=1:t;
   [varrobust,vardesen,varprob,varpdes]=robustnessjun1(k);
   srobv=[srobv,varrobust];   sdesv=[sdesv,vardesen];

end;

stdsrobv=transpose(var(transpose(srobv)))
varprob

stdsdesv=transpose(var(transpose(sdesv)))
varpdes


clear all
```

```
-----------------------------------------------
>> comparejun1(5,1000)

Estimated performance measure in the case of robustness (for high and low level of z)

    0.2342
    0.4585

Calculated prformance measure in the case of robustness robustness (for high and low level of z)

    0.2222
    0.4110


Estimated performance measure in the case of desensitization (for high and low level of z)

    0.0003
    0.0185


Calculated prformance measure in the case of robustness robustness (for high and low level of z)
    0.0003
    0.0183
```

C.2 Simulation for Comparing Approaches

```
function [rmean,rstd,perf]=comparedes_robust(k,sigx,zlevel,ndom)
%compare desensitization and robustness approaches
%k gives the number of repeats for each of the combinations of the desen expt.
%ndom gives the number of observation in the prelim dominant cause investigation
%if ndom==0 skip analysis methods that require the prelim investigation
%robustness and desens expt. have a total number of runs = 4*k
%assume only one control and one noise factor (x is a dominant cause if sigx>1)
%z level should be between 0 and 1, choose 0.5 as default

%[rmean,rstd,perf]=comparedes_robust(10,2,0.5,30)

%Model: Y = beta0 + beta1*x + beta2*z + beta3*x*z + R
%wlog we assume beta0=0, beta1=1, beta2=0, beta3=1 and sigmar=1;
%So Model: Y = x+x*z+R, R~G(0,1)

nsim=1000;   %number of simulation runs
resultslow=[];   %at z=-zlevel
resultshigh=[];   %at z=zlevel

for ii=1:nsim,
   %Robustness expt.  with 2*k runs at z and -z
```

```
z=[ones(2*k,1)*zlevel;-ones(2*k,1)*zlevel];   %here order doesn't matter
x=normrnd(0,sigx,4*k,1);  %cause is not controlled, generate random values
r=normrnd(0,1,4*k,1);   %use same z and r for all expts.
yr=x+x.*z+r;  %output
%for each expt we can estimate the std of the output at z=zlevel and z=-zlevel
stdrob=[std(yr(1:2*k)),std(yr(2*k+1:end))];     %for robustness expt

%Desensitization expt.
%k runs at each of the 4 possible combinations
x=[ones(k,1)*2*sigx;-ones(k,1)*2*sigx;ones(k,1)*2*sigx;-ones(k,1)*2*sigx];          %cause set at extremes
%use the same values for z and r as in robustness expt.
yd=x+x.*z+r;  %output

if ndom~=0,  %skip if prelim investigation has 0 observations
   %run prelim investigation to determine dom. cause output relationship
   %these results only used for some analyses
   x0=normrnd(0,sigx,ndom,1);  %cause is not controlled
   meanxhat0=mean(x0); sigxhat0=std(x0);   %use these to set levels for last option only
   r0=normrnd(0,1,ndom,1);
   y0=x0+r0;    %note z=0 here
   [b0,bint,res]=regress(y0,[ones(ndom,1),x0]);    %estimate b0, b1 and sigmar
   b1hat0=b0(2);  sigmarhat0=sqrt(sum(res.^2)/(ndom-2));

end;

%do the analysis in three ways -
%first, assume the relationship between x and y is known and that the residual and dom. cause variation are known
%ignore prelim investigation
b=regress(yd-x,[ones(4*k,1),z,x.*z]);    %estimate b0, b2 and b3
%use estimate for interaction term b(3) to estimate std of y at z=zlevel and -zlevel
stddes=[sqrt((1+b(3)*zlevel)^2*sigx^2+1), sqrt((1-b(3)*zlevel)^2*sigx^2+1)];

%second, assume the relationship between x and y (i.e. beta1) and the residual
%variation are not known, however we ASSUME SIGX IS KNOWN
%we will estimate beta1 and sigmar only using the expt results
%ignore prelim investigation
[b,bint,res]=regress(yd,[ones(4*k,1),x,z,x.*z]);    %estimate b0, b1, b2 and b3
sigmarhat=sqrt(sum(res.^2)/(4*k-4));
%estimate std(y) at the 2 z levels
stddes2=[sqrt((b(2)+b(4)*zlevel)^2*sigx^2+sigmarhat^2)
sqrt((b(2)-b(4)*zlevel)^2*sigx^2+sigmarhat^2)];

if ndom~=0,   %skip options 3 and 4 if prelim investigation has 0 observations
   %third, assume sigx known, but use prelim inv to help estimate beta1 and sigmar
   [b,bint,res]=regress(yd,[ones(4*k,1),x,z,x.*z]);    %estimate b0, b1, b2 and b3
   beta3hat=b(4);
   sigmarhatdesens=sqrt(sum(res.^2)/(4*k-4));
   %a pooled estimate of sigmar using both prelim and desens expt results
   sigmarhatpooled=sqrt(((ndom-2)*sigmarhat0^2+(4*k-4)*sigmarhatdesens^2)/(ndom+4*k-6));
   %to get a pooled estimate of beta1 combine the data from the prelim (z=0) and desens expt together
```

```
      [bpooled]=regress([y0;yd],[ones(ndom+4*k,1),[x0;x],[zeros(ndom,1);z],[zeros(ndom,1);x.*z]]);
%estimate b0, b1, b2 and b3
      beta1hatpooled=bpooled(2);
      %estimate std(y) at the 2 z levels
      stddes3=[sqrt((beta1hatpooled+beta3hat*zlevel)^2*sigx^2+sigmarhatpooled^2),
sqrt((beta1hatpooled-beta3hat*zlevel)^2*sigx^2+sigmarhatpooled^2)];

      %4th option for desens expt. assume all the information about the x,y
      %relationship must be estimated from a prelim investigation
      %thus a desens expt with slightly different xlevels would have been run since sigx is not
known

      %new Desensitization expt.
      %k runs at each of the 4 possible combinations
      %xlevels would be mux +/- 2*sigx with both mux and sigx estimated
      xhigh=meanxhat0+2*sigxhat0;  xlow=meanxhat0-2*sigxhat0;
      x2=[ones(k,1)*xhigh;ones(k,1)*xlow;ones(k,1)*xhigh;ones(k,1)*xlow];          %cause set at
estimated extremes
      %use z and r generated before - new desens expt. just has new levels for x
      yd2=x2+x2.*z+r;  %output
      %analyze desens expt.
      [b,bint,res]=regress(yd2,[ones(4*k,1),x2,z,x2.*z]);    %estimate b0, b1, b2 and b3
      beta3hat=b(4);
      sigmarhatdesens2=sqrt(sum(res.^2)/(4*k-4));

      %for the analysis assume the relationship between x and y, the residual
      %variation and sigmax are not known
      %we will estimate the relationship using the prelim results
      sigmarhatpooled=sqrt(((ndom-2)*sigmarhat0^2+(4*k-4)*sigmarhatdesens2^2)/(ndom+4*k-
6));  %a pooled estimate of sigmar using both prelim and desens expt results
      %to get a pooled estimate of beta1 combine the data from the prelim (z=0) and desens expt
together

[bpooled]=regress([y0;yd2],[ones(ndom+4*k,1),[x0;x2],[zeros(ndom,1);z],[zeros(ndom,1);x2.*z]]);
%estimate b0, b1, b2 and b3
      beta1hatpooled=bpooled(2);
      %estimate std(y) at the 2 z levels
      stddes4=[sqrt((beta1hatpooled+beta3hat*zlevel)^2*sigxhat0^2+sigmarhatpooled^2),
sqrt((beta1hatpooled-beta3hat*zlevel)^2*sigxhat0^2+sigmarhatpooled^2)];
   end;

   %store all the results
   if ndom~=0,
      resultslow=[resultslow;stdrob(2),stddes(2),stddes2(2),stddes3(2),stddes4(2)];
      resultshigh=[resultshigh;stdrob(1),stddes(1),stddes2(1),stddes3(1),stddes4(1)];
   else
      resultslow=[resultslow;stdrob(2),stddes(2),stddes2(2)];
      resultshigh=[resultshigh;stdrob(1),stddes(1),stddes2(1)];
   end;
end;

%summarize the performance of the two types of experiments
rmean=mean(resultslow);
```

```
rstd=std(resultslow);

disp(['std(y) at z=zlevel is ',num2str(sqrt((1+zlevel)^2*sigx^2+1))])
disp(['means ',num2str(mean(resultshigh))])
disp(['stdev ',num2str(std(resultshigh))])
disp(['std(y) at z=-zlevel is ',num2str(sqrt((1-zlevel)^2*sigx^2+1))])
disp(['means ',num2str(rmean)])
disp(['stdev ',num2str(rstd)])


%performance ratio - std of the std estimate for the best z level (-zlevel)
disp(['std of performance measure for robustness over desensitization'])
disp([num2str(rstd(1)/rstd(2)),' estimate beta3 from desens expt. everything else is assumed known'])
disp([num2str(rstd(1)/rstd(3)),' assume stdx known, estimate beta1&3 and sigr from desens expt. only'])
if ndom~=0,
   disp([num2str(rstd(1)/rstd(4)),' assume stdx known, estimate beta1&3 and sigr from prelim inv. and desens expt.'])
   disp([num2str(rstd(1)/rstd(5)),' assume nothing known, use prelim inv. and desens expt.'])
end;

if ndom~=0, perf=[rstd(1)/rstd(2),rstd(1)/rstd(3),rstd(1)/rstd(4),rstd(1)/rstd(5)];
else, perf=[rstd(1)/rstd(2),rstd(1)/rstd(3)];  end;
```

C.3 Simulation for the Gear Problem

```
function [brob,bdes,btaguSN,sigYrob,sigYdes,sigYtaguSN]=millerreducedvar2(k);
%4*k=number of replicates
%reduced model is:
%Y = 14.336-1.523A-2.648B-0.992C-0.312D+0.625E+0.422F-0.695G-7.195H+1.297CF++0.922BF+0.859FH-0.844DH-0.93CDF+R

%F,G,H are the varying inputs; F~N(muF,sigF^2),G~N(muG,sigG^2),H~N(muH,sigH^2);
%H is dominant cause
%A,B,C,D,E are fixed inputs; extreme level= +1 and -1
%ALPHA=0.01

%assumptions and components of reduced model. MSE=11.72 and extreme levels of varying inputs
%are -1 and +1; so:
sigR=sqrt(11.72);
muF=0; sigF=0.5;
muG=0; sigG=0.5;
muH=0; sigH=0.5;
```

```
%generate the experimental results for %%robustness experiment%% using the reduced model
%k = number of runs
R= normrnd(0,sigR,4*k*16,1);     %R~N(0,sigR)
F= normrnd(muF,sigF,4*k*16,1);   %generate F randomly
G= normrnd(muG,sigG,4*k*16,1);   %generate G randomly
H= normrnd(muH,sigH,4*k*16,1);   %generate H randomly
%levels of fixed inputs in experiment: defining design matrix
%(fractional factorila design, 2^5-1, Resolution V)
% # of replicates=4*k!
matx1=fracfact('a b c d abcd');
matx=[matx1;matx1;matx1;matx1];
matx2=[matx1;matx1;matx1;matx1];

if k==0.5
   matx=[matx1;matx1];
end;

if k==1
   matx=matx;
else
for i=2:k;

matx=[matx;matx2];

end; end;


A=matx(:,1);
B=matx(:,2);
C=matx(:,3);
D=matx(:,4);
E=matx(:,5);

yrobust=14.336-1.523*A-2.648*B-0.992*C-0.312*D+0.625*E+0.422*F-0.695*G-
7.195*H+1.297*C.*F+0.922*B.*F+0.859*F.*H-0.844*D.*H-0.93*C.*D.*F+R;

for y=1:4*k;

for x=1:16;

yrobustmatx(x,y)=yrobust(x+(y-1)*16);

end;

end;


for i=1:16
p(i,1)=log(std(yrobustmatx(i,:)));
end;
stats=regstats(p,matx1,'linear','beta');
```

```
A=matx1(:,1);
B=matx1(:,2);
C=matx1(:,3);
D=matx1(:,4);
E=matx1(:,5);

logsrob=stats.beta(1)+stats.beta(2)*A+stats.beta(3)*B+stats.beta(4)*C+stats.beta(5)*D+stats.bet
a(6)*E;

[C,I] = min(logsrob);
brob=matx1(I,:);

%Calculate sigY with robustness recomandation

%Var(Y)=
%(-7.195-.844D)^2var(H)+(0.422+1.297C+0.922B-0.93CD)^2var(F)+(-0.695)^2var(
%G)+(0.859)^2var(F)+var(R);
A=brob(1);
B=brob(2);
C=brob(3);
D=brob(4);
E=brob(5);

sigYrob =sqrt(( -0.844*D-7.195).^2.*sigH^2+(0.422+1.297*C+0.922*B-0.93*C.*D).^2.*sigF^2+(-
0.695).^2*sigG^2+(0.859).^2.*sigF^2.*sigH^2+sigR^2);



%generate the experimental results for %%desensitization experiment%% using the reduced
model
%assigned two levels of H(-1, 1) for each run, 4*k:number of replicates

Rex = normrnd(0,sigR,4*k*16,1);
F= normrnd(muF,sigF,4*k*16,1);
G= normrnd(muG,sigG,4*k*16,1);
Hex=[ones(2*k*16,1);-1*ones(2*k*16,1)];
A=matx(:,1);
B=matx(:,2);
C=matx(:,3);
D=matx(:,4);
E=matx(:,5);

ydesen=14.336-1.523*A-2.648*B-0.992*C-0.312*D+0.625*E+0.422*F-0.695*G-
7.195*Hex+1.297*C.*F+0.922*B.*F+0.859*F.*Hex-0.844*D.*Hex-0.93*C.*D.*F+R;


xdd=[ones(length(Hex),1) A B C D E Hex A.*Hex B.*Hex C.*Hex D.*Hex E.*Hex];
ades=regress(ydesen,xdd);


A=matx1(:,1);
B=matx1(:,2);
```

```
C=matx1(:,3);
D=matx1(:,4);
E=matx1(:,5);


stdydesen=sqrt((ades(7)+ades(8).*A+ades(9).*B+ades(10).*C+ades(11).*D+ades(12).*E).^2.*sig
H^2+sigR^2);


[C,I] = min(stdydesen);

bdes=matx1(I,:);

%Calculate sigY with desensitization recomandation
%Var(Y) = (-7.195+0.242A+0.586B+0.695C-0.844D+0.5E)^2*Var(H)+Var(R)
A=bdes(1);
B=bdes(2);
C=bdes(3);
D=bdes(4);
E=bdes(5);

sigYdes   =sqrt((   -0.844*D-7.195).^2.*sigH^2+(0.422+1.297*C+0.922*B-0.93*C.*D).^2.*sigF^2+(-
0.695).^2*sigG^2+(0.859).^2.*sigF^2.*sigH^2+sigR^2);


%generate the experimental results for %%Taguchi experiment%% using the reduced model
%and pulling out the recomandation using Loss-model(%Signal/Noise ratio)

matx=-1.*fracfact('a b c d -abcd f g h');

A=matx(:,1);
B=matx(:,2);
C=matx(:,3);
D=matx(:,4);
E=matx(:,5);
F=matx(:,6);
G=matx(:,7);

H=matx(:,8);

H=normrnd(muH,sigH,128,1);

R= normrnd(0,sigR,128,1);

ytagu=14.336-1.523*A-2.648*B-0.992*C-0.312*D+0.625*E+0.422*F-0.695*G-
7.195*H+1.297*C.*F+0.922*B.*F+0.859*F.*H-0.844*D.*H-0.93*C.*D.*F+R;


for x=1:16;
for y=1:8;

ytagumatx(x,y)=ytagu((x-1)*8+y);
```

```
end;
end;

for i=1:16
p(i,1)=log(std(ytagumatx(i,:)));
Eta(i,1)=10*log10(Mean(ytagumatx(i,:))^2/std(ytagumatx(i,:))^2);
end;

matx1=fracfact('-a -b -c -d abcd');
A=matx1(:,1);
B=matx1(:,2);
C=matx1(:,3);
D=matx1(:,4);
E=matx1(:,5);



[C,I2] = max(Eta);
btaguSN=matx1(I2,:);

%Calculate sigY with first and second Taguchi recomandations
%Var(Y)          =          (-7.195+0.703BD+0.695C-0.844D)^2*Var(H)+(1.297C+0.922B-0.789CE-
0.93CD)^2*Var(F)+(0.695AC+0.633DE)^2*Var(G)+(0.859+0.688B)^2*Var(F)*Var(H)+(-
0.758C)^2*Var(G)*Var(H)+Var(R)



A=btaguSN(1);
B=btaguSN(2);
C=btaguSN(3);
D=btaguSN(4);
E=btaguSN(5);

sigYtaguSN=sqrt((-0.844*D-7.195).^2.*sigH^2+(0.422+1.297*C+0.922*B-
0.93*C.*D).^2.*sigF^2+(-0.695).^2*sigG^2+(0.859).^2.*sigF^2.*sigH^2+sigR^2);
-------------------------------------------
function [robt,dest,tagut]=millercompare2(k,t);

%4k is the number of replicates, t equals number of simulations

   brobmatx=[];
   bdesmatx=[];
   btaguSNmatx=[];
   sigYrobmatx=[];
   sigYdesmatx=[];
   sigYtaguSNmatx=[];

 for nsim=1:t;
       [brob,bdes,btaguSN,sigYrob,sigYdes,sigYtaguSN]=millerreducedvar2(k);
       brobmatx=[brobmatx;brob];
       bdesmatx=[bdesmatx;bdes];
       btaguSNmatx=[btaguSNmatx;btaguSN];
       sigYrobmatx=[sigYrobmatx,sigYrob];
```

```
        sigYdesmatx=[sigYdesmatx,sigYdes];
        sigYtaguSNmatx=[sigYtaguSNmatx,sigYtaguSN];
 end;


 for i=1:5
 if t-sum(brobmatx(:,i))==0
    robt(1,i)=(t/t)
    robt(2,i)=0
else
    robt(1,i)=(t-(t-sum(brobmatx(:,i)))/2)*(1/t);
    robt(2,i)=((t-sum(brobmatx(:,i)))/2)*(1/t);
end;end;


for i=1:5
 if t-sum(bdesmatx(:,i))==0
    dest(1,i)=(t/t);
    dest(2,i)=0;
else
    dest(1,i)=(t-(t-sum(bdesmatx(:,i)))/2)*(1/t);
    dest(2,i)=((t-sum(bdesmatx(:,i)))/2)*(1/t);
end;end;



for i=1:5
 if t-sum(btaguSNmatx(:,i))==0
    taguSNt(1,i)=(t/t)
    taguSNt(2,i)=0
else
    taguSNt(1,i)=(t-(t-sum(btaguSNmatx(:,i)))/2)*(1/t);
    taguSNt(2,i)=((t-sum(btaguSNmatx(:,i)))/2)*(1/t);
end;end;
disp('--------------Robustness-------------------------')
disp('       A         B         C         D         E')
disp('H')
disp(robt(1,:))
disp('L')
disp(robt(2,:))
disp('-----------------------------------------------')



disp('-----------------Desensitization----------------------')
disp('       A         B         C         D         E')
disp('H')
disp(dest(1,:))
disp('L')
disp(dest(2,:))
disp('-----------------------------------------------------')
```

```
disp('-------------------Taguchi S/N--------------------')

disp('      A       B       C       D       E')
disp('H')
disp(taguSNt(1,:))
disp('L')
disp(taguSNt(2,:))
disp('--------------------------------------------------')

%calculate optimum setting
%assumptions and components of reduced model. MSE=11.72 and extreme levels of varying
inputs
%are -1 and +1; so:
sigR=sqrt(11.72);
muF=0; sigF=0.5;
muG=0; sigG=0.5;
muH=0; sigH=0.5;

matx=fracfact('a b c d e');


A=matx(:,1);
B=matx(:,2);
C=matx(:,3);
D=matx(:,4);
E=matx(:,5);

%Calculate sigY and pick the winner (min)
%Var(Y) = (-7.195+0.242A+0.586B+0.695C-0.844D+0.5E)^2*Var(H)+Var(R)

sigY    =sqrt((    -0.844*D-7.195).^2.*sigH^2+(0.422+1.297*C+0.922*B-0.93*C.*D).^2.*sigF^2+(-
0.695).^2*sigG^2+(0.859).^2.*sigF^2.*sigH^2+sigR^2);



[C,I] = min(sigY);
I=matx(I,:);

disp('-------------------Optimum Setting--------------------')

disp('    A    B    C    D    E')
disp(I)
minsigy=min(sigY)
maxsigy=max(sigY)
disp('-------------Comparison Results--------------------')
disp('    mean  | std   | mean  | std   |  mean   |  std   ')
disp('  sigYrob | sigYrob | sigYdes | sigYdes | sigYtaguSN|sigYtaguSN')
I=[mean(sigYrobmatx),std(sigYrobmatx),mean(sigYdesmatx),std(sigYdesmatx),mean(sigYtagu
SNmatx),std(sigYtaguSNmatx)];
disp(I)
```

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