Variation Reduction and Robust Designs

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Abstract

In this paper we consider two strategies for variation reduction . One of them is the exploitation of interactions. We also discuss the role of experiments in discovering interactions and in particular the use of robust designs to obtain the interaction between control and noise factors. Then we attempt to reduce the variation in a measurement system using a robust design.

Key Words: Control factors, Designed experiments , Interaction, Measurement system, Noise factors, Product array, Quality improvement, Robust designs, Variation reduction.

1. Introduction

During the last several years North America has been going through a quality revolution. More people are aware of the importance of Quality in products and processes. Business leaders are discussing Quality Improvement issues and Statistical Thinking. In this context two important concepts came to the forefront: (i) Data Based Decisions, (ii) Variation Reduction (VR).

This paper deals with Variation Reduction. Variation is a fact of life and it is interpreted differently depending on contexts. There are two types of 'variation' often discussed. (i) Deviation from Target. (ii) Variation around a target or a mean. Often it is not clear what type of variation is being discussed. It is very important to distinguish between these two types of variation so that we can devise strategies to reduce variation.

Variation is caused. Some times the causes of variation are known and sometimes not. Known causes can be classified in terms of control. Process parameters such as tooling, material types and amounts, and operational practice are under the control of the process designer and supervisor. These causes and the associated factors are called **Control Factors**. Some times the causes are not controllable or they may be very expensive to control. Variation in incoming raw materials, tool wear, environmental factors such as humidity are examples. These are called **Noise Factors**. For instance once a product is shipped to a customer characteristics such as time to failure and performance are affected by factors outside the manufacture's control such as the method of use and the environment. These are also noise factors.

Quality Improvement efforts in many instances have been directed at reducing the variation (deviation) of a particular characteristic around a nominal design specification. For instance, in the manufacture of pistons the final diameter of the piston is a critical characteristic which has a strict specification. If the diameters vary then the pistons have to be carefully sized and marked so that they can be selectively fitted into bores which also would have been previously sorted. This adds complexity and cost to the process. Another area of Quality Improvement efforts is in the field performance of a product. A customer may expect that a product will work as promised under a variety of field conditions some of which may not have been envisioned by the product designer and the manufacturer. For instance, if frost free refrigerators which are designed and manufactured for use in a temperate climate, were sent to tropical areas where the climate and culture are different then the performance of the fridge may be affected. A high quality fridge is one which is **robust (insensitive)** to these changing conditions.
In the next section we briefly give two strategies for variation reduction. Section 3 discusses the role of experiments in variation reduction, section 4 describes an experiment for reducing variation in a measurement system and section 5 gives some concluding remarks.

2. Strategies for Variation Reduction

We consider two basic strategies to mitigate the effects of variation induced by a known noise factor. These are:
- Controlling the variation in the noise factor itself
- Exploitation of the interaction between the noise factor and an easily controllable factor

Controlling the Noise:

The first strategy is to have more strict control on the noise factor. For instance, consider Figure 2.1 in which the a noise factor (temperature) transmits large (unacceptable) variation to a quality characteristic. This is because the temperature variation around the set point is very high. If it is possible to put tighter controls on temperature as indicated in the figure so that the transmitted variation is acceptable, then that would be a strategy to reduce variation in the quality characteristic. (Note that Figure 2.1 is an idealization)

Exploitation of Interaction:

In this strategy the noise factor itself is ignored but some other easily controllable factor which interacts with the noise factor is changed. Thus the approach takes advantage of the interaction between noise and control factors as shown in Figure 2.2. In this case the effect on the response of changing the noise factor is very different at different settings of the control factors. And the interaction must of the special form, as shown in Figure 2.2, that flattens the response graph at particular settings of the control factors.

There could be problems with this approach. In some cases, the response graph can be flattened but the quality characteristic may not be on target so that it may be necessary to change another factor to move the response on to target. In some other cases it may be impossible to flatten the response as

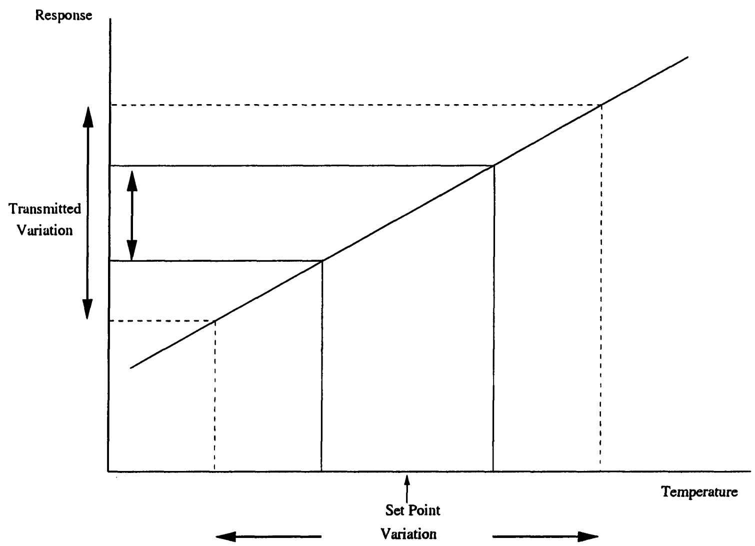

Figure 2.1 Controlling the Noise

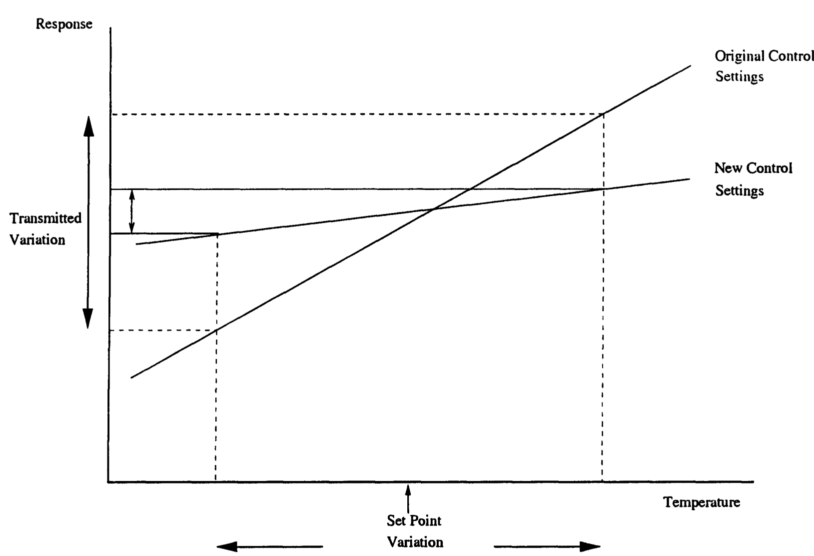

Figure 2.2 Exploitation of Interaction

required. It is also possible that changing control factors may change other quality characteristics in undesirable ways; in this case one problem is transformed into another one.

We discuss this strategy of exploitation of interactions more fully in the next section.

3. Role of Experiments

In the last section we advocated the use of interactions to counteract the effect on the response of variation transmitted from noise factors. An experiment is an intervention in to a process and this is the only way to discover interactions empirically. Thus to discover interactions and to use it appropriately we need to plan an experiment involving control and noise factors. This requires that
- the noise factors are known to transmit a significant amount of variation
- the noise factors can be temporarily controlled during the experiment
- the levels of the noise factors are far enough to capture variation in the factors during normal production or product use.

Experimental plan:

Taguchi (1987)(see also Taguchi and Wu(1979)) recommends using an experimental plan referred to as a **product array** in which the experimental plan for control factors (the **inner array**) and that for noise factors (the **outer array**) are crossed as shown in Table 4.1(for three control and two noise factors). Such a plan is referred to as a **robust design**. Some others such as Box(1988), and Shoemaker et al (1991) encourage a **combined array** in which there is only one plan for the control and noise factors taken together. Each approach has its own advantages and disadvantages (see Abraham and MacKay (1993)). For instance, a product array typically requires more runs than a combined array; on the other hand in a product array all noise by control interactions are estimable while the combined array cannot assure that all those can be estimated. Such matters become an issue only when fractional plans (subset of the possible factor level combinations) are considered. The actual conduct of the experiment depends also on other factors such as cost and convenience. We may use different types of blocking, split plotting etc. to increase the precision as well.

Analysis:

We consider only simple analysis such as calculations of effects, plotting of effects etc. There are different software available to do more sophisticated analysis. If we make an error in the analysis, it can be corrected in a subsequent analysis. This does not cost much. On the other hand if we make an error in the experimental plan or conduct, it is extremely costly to redo the experiment.

4. The Case of the Runaway Gauge

A foundry had been supplying a special part to a customer. The customer started to complain that a sizable proportion of parts supplied are out of specification for hardness, a critical measurement. This was surprising since every part supplied went through a 100 percent inspection system. They verified that the specifications used at the customer and supplier sites were the same. Then the foundry initiated a project to look in to the measurement system there.

Initial investigation:

The industry standard for measuring hardness was the Brinell Hardness (BH) test. In the foundry this was done by machining .100inch off the casting surface and indenting it with a 10mm steel ball under a 3000 kg load. The diameter of the resulting impression was measured through a microscope. This is called the Brinell Impression Diameter (B.I.D) which was a destructive measurement.

Since the B.I.D was a destructive measurement the foundry used an on-line gauge to get a proxy for the B.I.D. for each of the parts passing through the line. An operator would place a coil over the part which induces eddy currents in the casting. The gauge predicted a hardness measurement from the feedback provided by the coil.

The initial investigation consisted of studying the correlation between the two measurement systems (on-line gauge and off-line B.I.D measurements). Two hundred parts were measured by the on-line gauge first and then by the Brinell test. It was found that the correlation was nearly zero. Thus the on-line gauge was not doing what it was supposed to do. This initiated a further study to see whether or not the correlation can be improved.

Main investigation:

The main goal was to improve the correlation between the two measurement systems by changing the parameters in the on-line gauge. An experimental design was expected to help in obtaining the optimal settings. It was also expected that the resulting settings of the gauge would be robust to the variation from certain noise factors.

Response variables for the experiment were the hardness from the on-line gauge and the BH test.

Factors and Levels:

There were three machine parameters that the engineers could control; we suppress the actual names and label these as F, T and G because of confidentiality concerns. The levels of these factors were set with distinct machine settings or at opposite ends of normal operating specifications.

Historical studies had shown that the variation in chemistry and cleanliness of the casting had some effect on the gauge measurement . It was decided to select castings from two different days to simulate 2 levels of chemistry . For cleanliness the castings would be cleaned for 5min (level 1) and 20 min (level 2). The different factors and their levels are shown in Table 4.1.

Experimental Plan:

It was decided to perform a product array experiment with the inner array containing the three control factors in an 8 run full factorial array . The outer array with the two noise factors was also a full factorial with a 4 run array. The product array is shown in Table 4.2. This set up allows the replication of the control treatments over all four combinations of the noise factors. That means, castings from all the noise combinations, (day 1, 5min) (day 2, 5min) (day 1, 20min) (day 2, 20min), would be tested with each of the 8 set- ups

Table 4.1: Factors and Levels

Factors	**Levels**	
Control factors		
F	low (−)	high (+)
T	low (−)	high (+)
G	low (−)	high (+)
Noise factors		
Chemistry	Day1(1-16)	Day 2 (17-32)
Cleanliness	5 min	20 min

Table 4.2: Experimental Plan: Product Array

				Noise			
	Control	Factors		5	20	5	20
Run	F	T	G	Day 1	Day 1	Day 2	Day 2
1	−	−	−	.	.	.	.
2	−	−	+	.	.	.	.
3	−	+	−	.	.	.	.
4	−	+	+	.	.	.	.
5	+	−	−	.	.	.	.
6	+	−	+	.	.	.	.
7	+	+	−	.	.	.	.
8	+	+	+	.	.	.	.

Sixteen castings were to be selected for each of the chemistry levels (days 1 and 2). They were to be numbered from 1 to 32; 1 to 16 for day 1 and the rest for day 2. The 32 castings were to be cleaned for 5 min and measured using the online-gauge in the production area under the eight inner array conditions. Then they were to be subsequently cleaned for an additional 15 min and re-measured by the gauge. Then the B.I.D measurements were to be taken in the laboratory. Thus there were a total of 32 different conditions under which the hardness was to be measured.

Data Collection:

As per the plan, 16 castings were taken from day 1 and 16 others from day 2 , and they were numbered as required.

First all the 32 castings were cleaned for 5 min. Then castings 1 to 16 were measured by the on-line gauge using the 8 different (control) settings in a random order. Then they were cleaned for an additional 15 min (a total of 20 min) and measured again with the 8 settings in a random order. Finally the destructive B.I.D. measurements were taken from castings 1 to 16. The same procedure was followed for castings 17 to 32. Thus we have 32 experimental conditions each having 16 pairs of measurements with each pair containing a B.I.D. and a gauge measurement. The data for run numbers 1-4 are shown in Figure 4.1. Rest of the data are similar and not shown here to save space.

Data Analysis:

Initially the approach taken was to study the relationship between the value of the on-line gage measurement and the corresponding value for the Brinell test using scatter plots. There were 32 conditions and for each there were 16 castings measured by both the on-line and off-line gages. This produces 1 scatter plot per condition.

For the on-line gage to be a useful measurement system there should be a relationship between the on-line and off-line gage readings for at least one of the 8 runs in the inner array and this relationship should be consistent over different days (chemistries) and different levels of cleanliness. This consistency is essential because neither chemistry nor cleanliness can be controlled in production.

The scatter plots for runs 5-8 are in Figure 4.1. These indicate that the on-line gage readings and the Brinell test readings are not consistent from one chemistry to another and that different levels of cleaning have an inconsistent effect on the relationship. In addition, the correlation between the two sets of readings for all 32 conditions are negligible.

A more in-depth analysis can be done by fitting a line to the points in the scatter plot for each condition. Such an analysis would be useful if the scat-

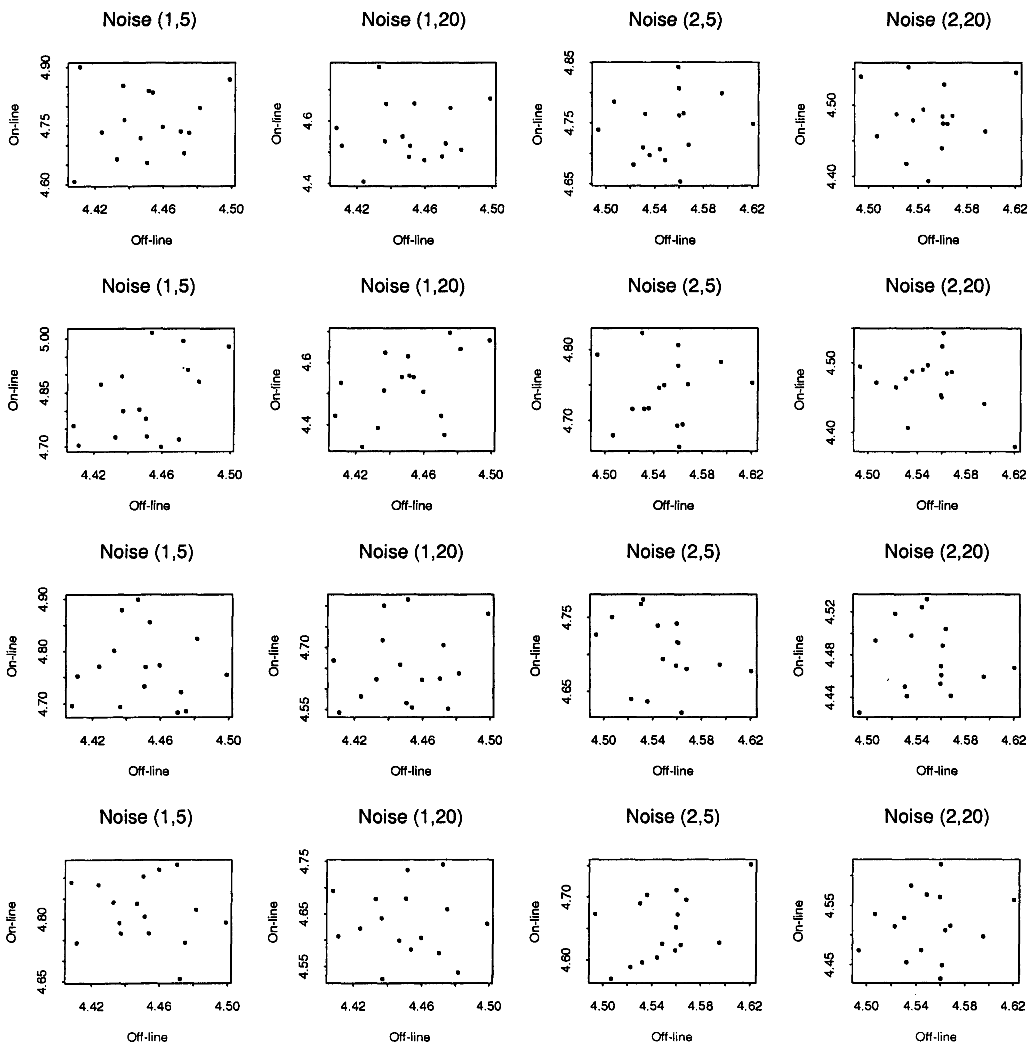

Figure 4.1 Data for Runs 1 to 4

ter plots had not given such obvious results. A brief analysis is given here to indicate the direction that one could take. We consider the model

$$y_{ijk} = \alpha_{ij} + \beta_{ij}x_{ijk} + \epsilon_{ij}, k = 1,2,...,16,\ j = 1,2,3,4,\ i = 1,2,...8,$$

where y_{ijk} and x_{ijk} are respectively the kth on-line and off-line reading for the ith run and jth noise condition; α_{ij} and β_{ij} are the corresponding intercepts and slopes respectively, and ϵ_{ijk} is an error assumed to be iid $N(0, \sigma_{ij}^2)$.

This analysis gives estimates for slopes and standard deviations (sd) for the variation of the points about the line. These are given in the Table 4.3.

Table 4.3: Slopes and Standard Deviations

Run #	**Slopes**				**Standard Deviations**			
	1	2	3	4	1	2	3	4
1	0.79	0.48	0.06	-0.09	0.08	0.07	0.04	0.04
2	0.78	0.55	0.09	-0.07	0.08	0.07	0.04	0.05
3	0.79	0.50	0.09	0.03	0.07	0.07	0.04	0.04
4	0.78	0.53	0.07	-0.05	0.08	0.07	0.04	0.04
5	0.89	0.61	0.02	-0.22	0.09	0.08	0.04	0.05
6	0.83	0.62	0.03	-0.21	0.09	0.08	0.05	0.05
7	0.84	0.68	0.02	-0.16	0.09	0.08	0.05	0.05
8	0.84	0.60	0.07	-0.19	0.09	0.08	0.05	0.05

The estimated slopes indicate that the relationship between on-line and off-line measurements is different for the two chemistries (sets of parts from days 1 and 2). For chemistry level 1(parts 1-16) the slopes range from .78 to .89 when the cleaning time is 5min and from .48 to .68 when the cleaning time is 20min. However, for chemistry level 2 (for parts 17-32) they range from .02 to .09 when the cleaning time is 5min and from -.22 to .03 when the cleaning time is 20min. Thus it indicates that for parts 17-32 the true slopes are zero and for parts 1-16 they are slightly larger but not significant. Hence the on-line measurements do not predict the offline measurements well.

The effects of the machine settings can be obtained for each combination of noise factors separately. These are given in Table 4.4 for all noise conditions and are negligible except possibly for factor F (parts 1-16). Even this is not large enough to be significant. We also obtained (but not shown here) the effects for log(sd) for all noise conditions. These were negligible as well.

Table 4.4: Effects for Slopes

Source	(Day 1, 5 min.)	(Day 1, 20 min.)	(Day 2, 5 min.)	(Day 2, 20 min.)
F	.06	.11	−0.04	−0.15
T	−.01	.01	0.01	0.06
FT	−.01	.01	0.01	−0.01
G	−.02	.01	0.02	−0.02
FG	−.01	−.04	0.01	0.01
TG	.02	−.03	−0.00	−0.03
FTG	.02	−.01	0.02	0.01

These confirm the earlier findings that the relationship between on-line and off-line measurements is clearly different for the two batches of chemistry conditions. It also indicates that the on-line measurements do not predict the off-line measurements ie. the machine was not performing as it was supposed to do and it cannot be optimized in the ranges considered for the purpose it was being used.

5. Concluding Remarks

We considered two strategies for variation reduction. One of them involves the use of interactions between control and noise factors. For implementing this strategy a robust design can be used. Such a design was considered for reducing the variation in a measurement system in a foundry. It was found that the measurement system could not be optimized to perform as it was supposed to. Such a finding was somewhat surprising since the machine was in operation in the foundry for a while. In any case, eventually the management decided to decommission the machines.

Acknowledgements

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