Design of Experiments

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INTRODUCTION

Experimental methods are widely used in research as well as in industrial settings, however, sometimes for very different purposes. The primary goal in scientific research is usually to show the statistical significance of an effect that a particular factor exerts on the dependent variable of interest.

In industrial settings, the primary goal is usually to extract the maximum amount of unbiased information regarding the factors affecting a production process from as few (costly) observations as possible. In industrial settings interaction effects are often regarded as a "nuisance" (they are often of no interest; they only complicate the process of identifying important factors).

A Design of Experiment (DoE) is a structured, organized method for determining the relationship between factors affecting a process and the output of that process. In other words, "Design of Experiments" (DoE) refers to experimental methods used to quantify indeterminate measurements of factors and interactions between factors statistically through observance of forced changes made methodically as directed by mathematically systematic tables.

The design of a particular type of experiment can be carried out in many ways. Thus, it is important to choose a particular experimental design, which is suited for the particular process.

It is a good idea to choose a design that requires somewhat fewer runs than the budget permits, so that center point runs can be added to check for curvature in a 2-level screening design and backup resources are available to redo runs that have processing mishaps.

REQUISITES OF A GOOD TEST

RANDOMIZATION

Randomization is the running of test parts in random order. It is the opposite of running tests systematically. The running of tests randomly prevents the confounding of effects that can happen when tests are run in a standard order. For example, if temperature is a controlled design variable, it would be best not to run all the temperatures at a given level at the same time. If all test points at a given temperature are run at the same time, the effects of time can be confounded (mixed up) with ten effects of temperature.

BLOCKING

Blocking is the deliberate screening out of the effects of variables thought to have an influence on the test results. For example, it may be thought that test instruments have an effect on the test results. If so then we may want to conduct the test using several test instruments to reduce and quantify the effects test instruments.

REPLICATION

Replication is the running of one or more test parts under the same conditions. That is to repeat some of the test parts in the test design. Repeat testing of test parts builds confidence in the test results and enables us to compute the statistical significance of test results.

RANGE OF TEST VARIABLES

The ranges of controlled design variables should be reasonable. These ranges should be in line with the test objectives. If the test objective is to improve the current product, then the test variable ranges should reflect this. Typically, the ranges would be clustered around the current product values. Whereas if the test objective is to develop a new product then the test variable ranges would encompass all possible achievable values, typically wider ranges then for improving a current product.

Increment between Variable Levels

The increment between levels of test variables should be realistic. Increments can be too wide or too narrow. If increments are too wide, we may miss finding information between the levels. If increments are too narrow, especially compared to our ability to hit the level, we may not get a good reading of the variable.

Experimental Design Objectives

Comparative objective

These are used when there are one or several factors under investigation, but the primary goal of the experiment is to make a conclusion about one a-priori important factor(in the presence of, and/or in spite of the existence of the other factors). If the question of interest is whether that factor is "significant" or not, (i.e., whether or not there is a significant change in the response for different levels of that factor), then a comparative design solution is required.

Screening objective

The primary purpose of the experiment is to select or screen out the few important main effects from the many less important ones. These screening designs are also termed main effects designs.

Response Surface (method) objective

The experiment is designed to allow us to estimate interaction and even quadratic effects, and therefore give us an idea of the (local) shape of the response surface we are investigating. For this reason, they are termed response surface method (RSM) designs. RSM designs are used to

- Find improved or optimal process settings
- Troubleshoot process problems and weak points
- Make a product or process more robust against external and non-controllable influences. "Robust" means relatively insensitive to these influences.

OPTIMIZING RESPONSES OBJECTIVE

If there are factors that are proportions of a mixture and it is required to know what the "best" proportions of the factors are so as to maximize (or minimize) a response, then a mixture design is used.

REGRESSION DESIGN

To model a response as a mathematical function (either known or empirical) of a few continuous factors and "good" model parameter estimates are desired (i.e., unbiased and minimum variance), then a regression design is used.

ADVANTAGES OF DOE

- DoE eliminates the 'confounding of effects' whereby the effects of design variables are mixed up.
- DoE helps us handle experimental error.
- DoE helps us determine the important variables that need to be controlled.
- DoE helps us find the unimportant variables that may not need to be controlled.
- DOE helps us measure interactions, which is very important.

COMPLETELY RANDOMIZED DESIGN

Completely randomized designs considered here have one primary factor. The experiment compares the values of a response variable based on the different levels of that primary factor.

In completely randomized designs, we randomly assign the levels of the primary factor to the experimental units. By randomization, we mean that the run sequence of the experimental units is determined randomly. For example, if there are 3 levels of the primary factor with each level to be run 2 times, then there are 6 factorial possible run sequences (or 6! ways to order the experimental trials). Because of the replication, the number of unique orderings is 90 (since $\frac{6!}{(2!\times 2!\times 2!)} = 90$). An example of an un-randomized design would be to always run 2 replications for the first level, then 2 for the second level, and finally 2 for the third level. To randomize the runs, one way would be to put 6 slips of paper in a box with 2 having level 1, 2 having level 2, and 2 having level 3. Before each run, one of the slips would be drawn blindly from the box and the level selected would be used for the next run of the experiment.

In practice, a computer program typically performs the randomization. However, the randomization can also be generated from random number tables or by some physical mechanism (e.g., drawing the slips of paper).

All completely randomized designs with are defined by 3 numbers

k = number of factors (= 1 for these designs)

L = number of levels

n = number of replications

Total sample size (number of runs) is N = k x L x n.

Balance dictates that the number of replications be the same at each level of the factor (this will maximize the sensitivity of subsequent statistical t (or F) tests).

EXAMPLE

A typical example of a completely randomized design is the following

k = 1 factor (X1)

L = 4 levels of that single factor (called "1", "2", "3", and "4")

n = 3 replications per level

N = 4 levels * 3 replications per level = 12 runs

The randomized sequence of trials might look like

X1	3	1	4	2	2	1	3	4	1	2	4	3

Note that in this example there are $\frac{12!}{(3!\times 3!\times 3!\times 3!)} = 369600$ ways to run the experiment, all equally likely to be picked by a randomization procedure.

MODEL

The model for the response is

$Y_{i,j} = \mu + T_i$ + random error

with

$Y_{i,j}$ being any observation for which X1 = i

μ is the general location parameter

T_i is the effect of having treatment level i

ESTIMATES AND STATISTICAL TESTS

Estimates for a Randomized Block Design

μ: $\hat{Y}$

$\hat{Y}$ = average of all the data

T_i: $\hat{Y}_i - \hat{Y}$

$\hat{Y}_i$ = average of all Y for which X1 = i.

RANDOMIZED BLOCK DESIGN

For randomized block designs, there is one factor or variable that is of primary interest. However, there are also several other nuisance factors.

Nuisance factors are those that may affect the measured result, but are not of primary interest. For example, in applying a treatment, nuisance factors might be the specific operator who prepared the treatment, the time of day the experiment was run, and the room temperature. All experiments have nuisance factors. The experimenter will typically need to spend some time deciding which nuisance factors are important enough to keep track of or control, if possible, during the experiment.

When we can control nuisance factors, an important technique known as blocking can be used to reduce or eliminate the contribution to experimental error contributed by nuisance factors. The basic concept is to create homogeneous blocks in which the nuisance factors are held constant and the factor of interest is allowed to vary. Within blocks, it is possible to assess the effect of different levels of the factor of interest without having to worry about variations due to changes of the block factors, which are accounted for in the analysis.

BLOCKING FACTORS

A nuisance factor is used as a blocking factor if every level of the primary factor occurs the same number of times with each level of the nuisance factor. The analysis of the experiment will focus on the effect of varying levels of the primary factor within each block of the experiment.

The general rule is

"Block what you can, randomize what you cannot."

Blocking is used to remove the effects of a few of the most important nuisance variables. Randomization is then used to reduce the contaminating effects of the remaining nuisance variables.

EXAMPLE OF A RANDOMIZED BLOCK DESIGN

Suppose engineers at a semiconductor manufacturing facility want to test whether different wafer implant material dosages have a significant effect on resistivity measurements after a diffusion process taking place in a furnace. They have four different dosages they want to try and enough experimental wafers from the same lot to run three wafers at each of the dosages.

The nuisance factor they are concerned with is "furnace run" since it is known that each furnace run differs from the last and impacts many process parameters.

An ideal way to run this experiment would be to run all the 4x3=12 wafers in the same furnace run. That would eliminate the nuisance furnace factor completely. However, regular production wafers have furnace priority, and only a few experimental wafers are allowed into any furnace run at the same time.

NON-BLOCKED METHOD

A non-blocked way to run this experiment would be to run each of the twelve experimental wafers, in random order, one per furnace run. That would increase the experimental error of each resistivity measurement by the run-to-run furnace variability and make it more difficult to study the effects of the different dosages.

BLOCKED METHOD

The blocked way to run this experiment, assuming you can put four experimental wafers in a furnace run, would be to put four wafers with different dosages in each of three furnace runs. The only randomization would be choosing which of the three wafers with dosage 1 would go into furnace run 1, and similarly for the wafers with dosages 2, 3 and 4.

DESCRIPTION OF THE EXPERIMENT

Let X1 be dosage "level" and X2 be the blocking factor furnace run. Then the experiment can be described as

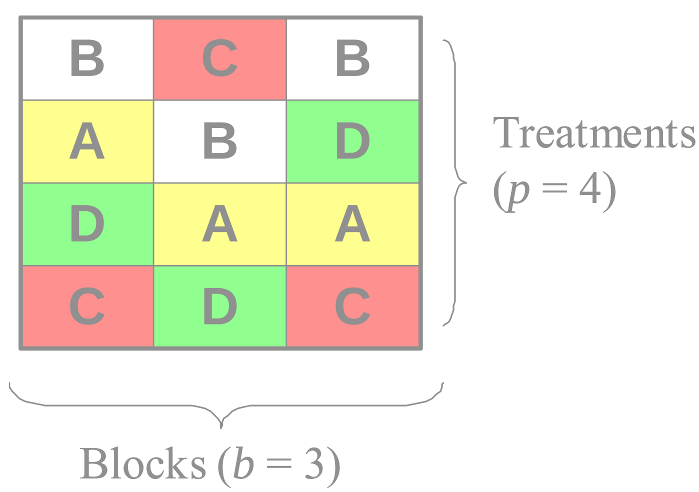
k = 2 factors (1 primary factor X1 and 1 blocking factor X2)
L1 = 4 levels of factor X1
L2 = 3 levels of factor X2
n = 1 replication per cell
N =L1 * L2 = 4 * 3 = 12 runs

Before randomization, the design trials look like

X1	1	1	1	2	2	2	3	3	3	4	4	4
X2	1	2	3	1	2	3	1	2	3	1	2	3

MATRIX REPRESENTATION

An alternate way of summarizing the design trials would be to use a 4x3 matrix whose 4 rows are the levels of the treatment X1 and whose columns are the 3 levels of the blocking variable X2. The cells in the matrix have indices that match the X1, X2 combinations above.



By extension, note that the trials for any K-factor randomized block design are simply the cell indices of a K dimensional matrix.

MODEL FOR A RANDOMIZED BLOCK DESIGN

The model for a randomized block design with one nuisance variable is

$Y_{i,j} = \mu + T_i + B_j$ + random error

where

$Y_{i,j}$ is any observation for which X1 = i and X2 = j

X1 is the primary factor

X2 is the blocking factor

μ is the general location parameter (i.e., the mean)

T_i is the effect for being in treatment i (of factor X1)

B_j is the effect for being in block j (of factor X2)

ESTIMATES FOR A RANDOMIZED BLOCK DESIGN

μ: $\hat{Y}$

$\hat{Y}$ = average of all the data

T_i: $\hat{Y}_i - \hat{Y}$

$\hat{Y}_i$ = average of all Y for which X1 = i.

B_j: $\hat{Y}j - \hat{Y}$

$\hat{Y}_j$ = average of all Y for which X2 = j.

LATIN SQUARES

A Latin square, as used in experimental design, is a balanced two-way classification scheme. Consider the following 3 x 3 arrangement.

a	b	c
b	c	a
c	a	b

In this arrangement, each letter occurs just once in each row and just once in each column. The following arrangement also exhibits this kind of balance.

b	c	a
a	b	c
c	a	b

The latter arrangement was obtained from first by interchanging the first and second rows.

Two Latin squares are orthogonal if, when they are combined, the same pair of symbols occurs no more than once in the composite square. For example, consider the following 3 x 3 Latin squares.

a_1	a_2	a_3	b_2	b_3	b_1	c_1	c_2	c_3
a_2	a_3	a_1	b_3	b_1	b_2	c_3	c_1	c_2
a_3	a_1	a_2	b_1	b_2	b_3	c_2	c_3	c_1

Combining squares (1) and (2) yields the composite square

a_1b_2	a_2b_3	a_3b_1
a_2b_3	a_3b_1	a_1b_2
a_3b_1	a_1b_2	a_2b_3

In this composite, the treatment combination a_1b_2 occurs more than once; hence squares (1) and (2) are not orthogonal.

Combining squares (1) and (3) yields the following composite

a_1c_1	a_2c_2	a_3c_3
a_2c_3	a_3c_1	a_1c_2
a_3c_2	a_1c_3	a_2c_1

In this composite, there is no repetition of treatment combination. Nine possible treatment combinations may be formed from three levels of factor A and three levels of factor C. each of these possibilities appears in the composite. Hence, squares (1) and (3) are orthogonal.

A Greco-Latin square is the composite square obtained by combining two orthogonal Latin squares. The 3 x 3 Greco-Latin square obtained by combining squares (1) and (3) above may be represented schematically as

11	22	33
23	31	12
32	13	21

This representation uses only the subscripts that appear with the *a*'s and *c*'s. Interchanging any two rows or any two columns of a Greco-Latin square will still yield a Greco-Latin square.

It is not always possible to find a Latin square orthogonal to a given Latin square. If the dimension of a Latin square is capable of being expressed in the form $(\text{prime number})^n$, where n is any integer, then orthogonal squares exist. In addition, there are cases in which orthogonal squares exist when the dimension of the square is not of the form $(\text{prime number})^n$ particularly when the dimension is divisible by 4.

A simple Latin square design has two nuisance factors, a Greco-Latin square has three nuisance factors and a Hyper Greco-Latin square has four nuisance factors. The nuisance factors are used as blocking variables. In a simple Latin square design, the two nuisance factors are divided into a tabular grid i.e. one nuisance factor is along the rows and the other factor is along the columns. Each row and each column receives a treatment exactly once.

ADVANTAGES OF LATIN SQUARE DESIGN

- They handle the case when we have several nuisance factors and we either cannot combine them into a single factor or wish to keep them separate.
- They allow experiments with a relatively small number of runs.

DISADVANTAGES OF LATIN SQUARE DESIGN

- The number of levels of each blocking variable must equal the number of levels of the treatment factor.
- The Latin square model assumes that there are no interactions between the blocking variables or between the treatment variable and the blocking variable.

EXAMPLE

To understand the application of Latin Square design in Design of Experiments, consider the following table. There are two blocking factors and one primary factor. There are three levels for each of the factors denoted by the numbers 1, 2 and 3.
The total number of runs required in this case is

$$\mathbf{N = L_1 \times L_2}$$

where L_1 = number of levels of first blocking factor,
L_2 = number of levels of second blocking factor

Therefore, in this case, the number of runs is 3 x 3 = 9.

Row blocking factor	Column blocking factor	Treatment factor
1	1	1
1	2	2
1	3	3
2	1	3
2	2	1
2	3	2
3	1	2
3	2	3
3	3	1

The elements of the Latin Square correspond to the arrangement of the treatment factor column above.

a	b	c
c	a	b
b	c	a

MODEL FOR LATIN SQUARE DESIGN

$$Y_{i,j,k} = \mu + R_i + C_j + T_k + \text{random error}$$

$Y_{i,j,k}$ is any observation for which X1 = i, X2 = j and X3=k

X3 is the primary factor

X1, X2 are the blocking factor

μ is the general location parameter (i.e., the mean)

T_k is the effect for treatment k

R_i is the effect for block i

C_j is the effect for block j

TAGUCHI METHOD

Taguchi has envisaged a new method of conducting the design of experiments which are based on well defined guidelines. This method uses a special set of arrays called orthogonal arrays. These standard arrays stipulate the way of conducting the minimal number of experiments which could give the full information of all the factors that affect the performance parameter. The crux of the orthogonal arrays method lies in choosing the level combinations of the input design variables for each experiment.

ORTHOGONAL ARRAY

While there are many standard orthogonal arrays available, each of the arrays is meant for a specific number of independent design variables and levels.

L_9 (3^4) Orthogonal array					
	Independent Variables				Performance Parameter Value
Experiment #	Variable 1	Variable 2	Variable 3	Variable 4	
1	1	1	1	1	p1
2	1	2	2	2	p2
3	1	3	3	3	p3
4	2	1	2	3	p4
5	2	2	3	1	p5
6	2	3	1	2	p6
7	3	1	3	2	p7
8	3	2	1	3	p8
9	3	3	2	1	p9

For example, if one wants to conduct an experiment to understand the influence of 4 different independent variables with each variable having 3 set values (level values) then an L9 orthogonal array might be the right choice. The L9 orthogonal array is meant for understanding the effect of 4 independent factors each having 3 factor level values. This array assumes that there is no interaction between any two factors. While in many cases, no interaction model assumption is valid, there are some cases where there is a clear evidence of interaction. A typical case of interaction would be the interaction between the material properties and temperature.

There are totally 9 experiments to be conducted and each experiment is based on the combination of level values as shown in the table. For example, the third experiment is conducted by keeping the independent design variable 1 at level 1, variable 2 at level 3, variable 3 at level 3, and variable 4 at level 3.

PROPERTIES OF AN ORTHOGONAL ARRAY

The orthogonal arrays have the following special properties that reduce the number of experiments to be conducted.

- The vertical column under each independent variables of the above table has a special combination of level settings. All the level settings appear an equal number of times. For L9 array under variable 4, level 1, level 2 and level 3 appears thrice. This is called the **balancing property** of orthogonal arrays.
- All the level values of independent variables are used for conducting the experiments.
- The sequence of level values for conducting the experiments shall not be changed. This means one can not conduct experiment 1 with variable 1, level 2 setup and experiment 4 with variable 1, level 1 setup. The reason for this is that the array of each factor columns is mutually orthogonal to any other column of level values. The inner product of vectors corresponding to weights is zero. If the above 3 levels are normalized between -1 and 1, then the weighing factors for level 1, level 2 , level 3 are -1 , 0 , 1 respectively. Hence the inner product of weighing factors of independent variable 1 and independent variable 3 would be

  $$(-1\times-1 + -1\times0 + -1\times1) + (0\times0 + 0\times1 + 0\times-1) + (1\times0 + 1\times1 + 1\times-1) = 0$$

MINIMUM NUMBER OF EXPERIMENTS

The design of experiments using the orthogonal array is, in most cases, efficient when compared to many other statistical designs. The minimum number of experiments that are required to conduct the Taguchi method can be calculated based on the degrees of freedom approach.

$$N_{Taguchi} = \sum_{i=1}^{n} (L_i - 1)$$

where n is the number of variables

L_i is the number of levels of i^{th} variable.

For example, in case of 8 independent variables study having 1 independent variable with 2 levels and remaining 7 independent variables with 3 levels (L18 orthogonal array), the minimum number of experiments required based on the above equation is 16. Because of the balancing property of the orthogonal arrays, the total number of experiments shall be multiple of 2 and 3. Hence the number of experiments for the above case is 18.

ASSUMPTIONS

The additive assumption implies that the individual or main effects of the independent variables on performance parameter are separable. Under this assumption, the effect of each factor can be linear, quadratic or of higher order, but the model assumes that there exists no cross product effects (interactions) among the individual factors. That means the effect of independent variable 1 on performance parameter does not depend on the different level settings of any other independent variables and vice versa. If at anytime, this assumption is violated, then the additivity of the main effects does not hold, and the variables interact.

ROBUST DESIGN

A main cause of poor yield in manufacturing processes is the manufacturing variation. These manufacturing variations include variation in temperature or humidity, variation in raw materials, and drift of process parameters. These sources of noise / variation are the variables that are impossible or expensive to control.

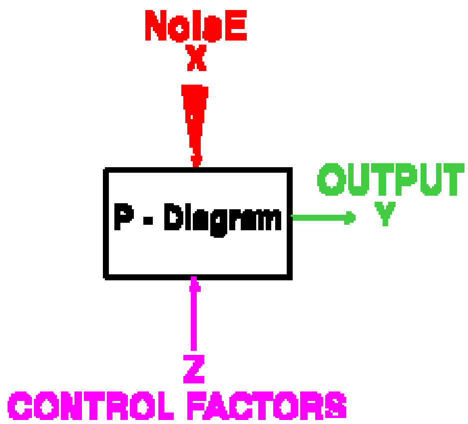
The objective of the robust design is to find the controllable process parameter settings for which noise or variation has a minimal effect on the product's or process's functional characteristics. It is to be noted that the aim is not to find the parameter settings for the uncontrollable noise variables, but the controllable design variables. To attain this objective, the control parameters, also known as inner array variables, are systematically varied as stipulated by the inner orthogonal array. For each experiment of the inner array, a series of new experiments are conducted by varying the level settings of the uncontrollable noise variables. The level combinations of noise variables are done using the outer orthogonal array.

The influence of noise on the performance characteristics can be found using the ratio here S is the standard deviation of the performance parameters for each inner array experiment and N is the total number of experiment in the outer orthogonal array. This ratio indicates the functional variation due to noise. Using this result, it is possible to predict which control parameter settings will make the process insensitive to noise.

STATIC PROBLEMS

Generally, a process to be optimized has several control factors which directly decide the target or desired value of the output. The optimization then involves determining the best control factor levels so that the output is at the target value. Such a problem is called as a Static Problem

This is best explained using a P-Diagram which is shown below ("P" stands for Process or Product). Noise is shown to be present in the process but should have no effect on the output. This is the primary aim of the Taguchi experiments - to minimize variations in output even though noise is present in the process. The process is then said to have become robust



S/N RATIO

There are 3 Signal-to-Noise ratios of common interest for optimization of Static Problems.

$$n = -10\, Log_{10} \left[\frac{\sum_{i=1}^{n} X_i^2}{n} \right]$$

This is usually the chosen S/N ratio for all undesirable characteristics like defects etc. for which the ideal value is zero. Also, when an ideal value is finite and its maximum or minimum value is defined then the difference between measured data and ideal value is expected to be as small as possible. The generic form of S/N ratio then becomes

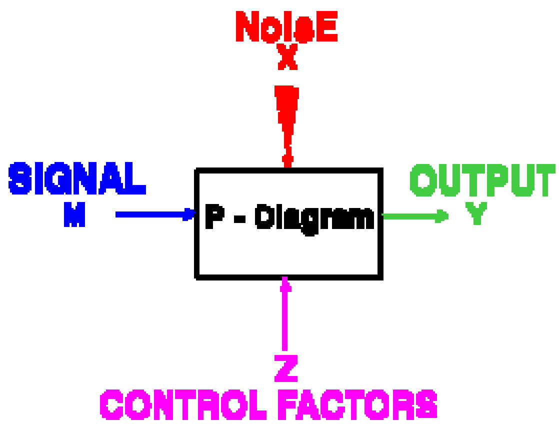
$$n = -10\, Log_{10} \left[\frac{\sum_{i=1}^{n} (X_{measured} - X_{ideal})^2}{n} \right]$$

When a specified value is most desired, and neither a smaller nor a larger value is desirable then S/N Ratio used is

$$n = -10\, Log_{10} \left[\frac{\mu^2}{\sigma} \right]$$

DYNAMIC PROBLEMS

If the product to be optimized has a signal input that directly decides the output, the optimization involves determining the best control factor levels so that the "input signal / output" ratio is closest to the desired relationship. Such a problem is called as a Dynamic Problem. This is best explained by a P-Diagram which is shown below. Again, the primary aim, to minimize variations in output even though noise is present in the process, is achieved by getting improved linearity in the input/output relationship.



In dynamic problems, we come across many applications where the output is supposed to follow input signal in a predetermined manner. Generally, a linear relationship between input and output is desirable.

SENSITIVITY

The slope of I/O characteristics should be at the specified value.

$$n = -10\,Log_{10}\left[slope^2\right]$$

LINEARITY

Most dynamic characteristics are required to have direct proportionality between the input and output. The straight line relationship between I/O must be truly linear i.e. with as little deviations from the straight line as possible. Variance in this case is the mean of the sum of squares of deviations of measured data points from the best-fit straight line (linear regression).

$$n = -10\,Log_{10}\left[\frac{slope^2}{\sigma}\right]$$

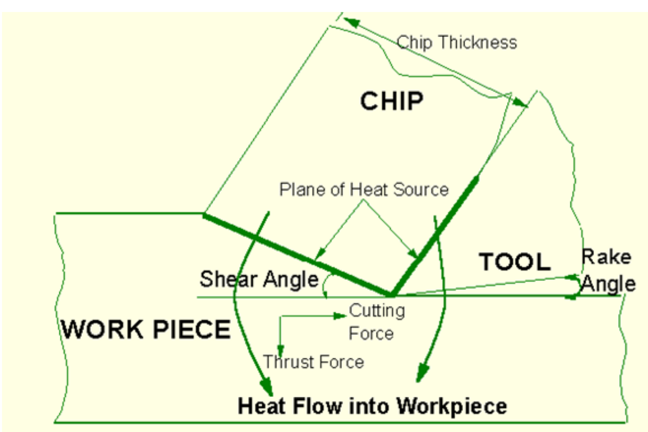
CASE STUDY

PROBLEM DESCRIPTION

The example considered here is the simulation of a machining process using the integrated approach of the Taguchi method and the finite element method. During the metal removing process, a large amount of heat is generated and some of the heat generated is dissipated to the surrounding and the remaining heat flows into the workpiece. The heat which flows into the workpiece results in thermal distortion of the workpiece. In order to machine the workpiece to a close tolerance, it is essential that the heat flow into the workpiece should be minimized. With this objective in mind, the following experiments are conducted to study the influence of various factors that minimizes the heat flow into the workpiece.

FINITE ELEMENT MODEL

The finite element model of the above machining process consists of 2-D thermal solid elements. This element type is used for conducting the steady state thermal analysis with conduction capability. In addition to this the heat generation within the body and convection along the edges can be simulated. From the heat flux calculations, it is possible to find out the total heat flow into the workpiece. Every time the experiment is conducted, the geometry automatically gets changed depending on the level settings of the design variables.



There are totally 85 assigned variables which are defined in the input file. Though only 13 design variables are identified for the above problem, the remaining variables are meant for node and element generation and other purposes. Hence one shall not choose the modeling variables as a design variable. From the set of possible design variables, the following seven variables are considered for experimental study: Depth of cut, Width of cut, Shear angle, Rake angle, Cutting Force, Tangential force and ambient temperature. For each design variable selected, the user inputs 3 different level values. This allows nonlinear effects of the design variables on the response to be represented. If all the level values of a particular design variable are same, then the variable is not considered for the design of experiment.

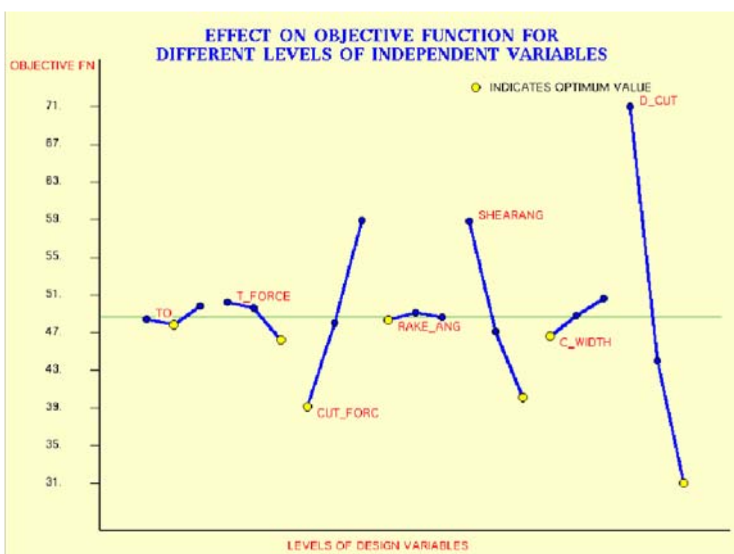
Once the design variables are selected, the program automatically selects the L18 orthogonal array. The L18 orthogonal array is meant for conducting the experiments with maximum of 7 independent design variables, each having 3 levels. After the selection of the orthogonal array, the ANSYS input file is modified with different level combinations of design variables. For each experiment, a new ANSYS parametric file is created. Since the geometry of the problem is changed in each experiment, ANSYS program calculates the stiffness matrix for every experiment and solves the problem. At the end of each experiment, the process parameter values are appended to a file. This is used for post processing process.

POST PROCESSING

Once the experiments are over, the program calculates the mean value of each level of all the variables, the sum of squares, the percent contribution and the near optimum level of each design variable. Based on the near optimum level of each variable, a new experiment is conducted with each design variable set to the near optimum level value. The program also calculates the F-value for each design variable.

Variable Name	**Level Settings**			**Optimum**	**Contribution (%)**	**ANOVA TEST**	
	1	2	3			F-Value	Significant
Tool Angle	25	30	35	2	0.17	0.001	No
Tool Force	30	35	40	3	0.72	0.005	No
Cutting Force	50	60	70	1	15.9	1.40	No
Rake Angle	0	2	4	1	0.03	0.0002	No
Shear Angle	20	30	40	3	14.43	1.26	No
Width	0.08	0.11	0.13	1	0.66	0.005	No
Depth of cut	0.001	0.002	0.003	3	67.13	15.3	Yes

It can be observed from the analysis results that the most significant factor which dominates the heat flow into the workpiece is the depth of cut. The other two factors viz. cutting force and shear angle contribute to a limited extend. Though the percent contribution of these two variables are significant, the ANOVA test conducted at 90% confidence level rejects the significance of these design variables.



The sensitivity analysis of the machining operation shows that most of the design variables vary linearly except ambient temperature. In addition to this, the results showed that depth of cut, cutting force, and shear angle influences more than any other variables. Both rake angle and ambient temperature does not have a significant impact on the objective function.

CONCLUSIONS

The experimental results obtained by using the integrated approach confirm the common expectation that the depth of cut, cutting force and shear angle are more predominant parameters compared to other factors. If the final aim of the experimental study is to optimize the performance parameter, then the designer can conduct further experiments by keeping the near optimum level values as the starting value for the new experiment. Moreover the ambient temperature, rake angle, tangential force and width of cut can be removed form the further study provided the operating range of the above factors do not change from the conducted experiment