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Report 3

A Guide to the use of Design of Experiment Methods

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ABSTRACT

Design of experiments, DOE, is a rational scientific approach which allows an understanding of how the inputs interact thus affecting the process and allowing the refinement such that the process output can be optimised according to requirements. This review introduces the use of the design of experiments methodology to optimise processes and outlines the advantages and disadvantages of some of the design types available. This guide is to aid the reader when using their preferred software package in understanding the underlying principles of DOE, and investigating possible alternative designs for analysis of their data which may be more suited for their application. A worked example of a manufacturing process is presented. In this case study the mathematical processes which are used in a design of experiments process are presented to allow the reader to follow the analysis process. A summary of some of the available commercial software is presented, indicating the experimental designs available within the package.

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1. INTRODUCTION;WHAT IS EXPERIMENTAL DESIGN?

Experimental design consists of refined changes of the inputs to a process or an activity in order to observe the corresponding outputs or responses. The process can be defined from a series of interacting parameters, for example, materials, environment, measurement, human interaction etc. which combine in a defined way to provide a service, produce a product or complete some aspect of a task. Design of experiments is an rational scientific approach which allows an understanding of how the inputs interact thus affecting the process and allowing the refining of the process to optimise the output according to specific requirements.

In the previous programme, a series of case studies using adhesive bonding were investigated using design of experiment techniques [1, 2, 3, 4, 5, 6]. These case studies combined laboratory measurements and process control to optimise manufacturing processes in the packaging and engineering sectors and included a laboratory based investigation of the factors which affect adhesive bonding in a large scale construction project.

2. WHY USE EXPERIMENTAL DESIGN?

The task of scientists and engineers regardless of where they are based research, development, design, quality, testing, manufacturing etc., is to obtain, report and transfer product and/or process knowledge. This understanding is a step away from hunches or intuition and is based upon facts and data. In the modern environment the collection of these data and facts is required at an ever increasing pace in order to keep up with the demands of a competitive marketplace. The methods by which data or processes are controlled have been collected historically and are being pushed to their limits. These are frequently based upon an experienced hand who has 'seen it all before'. Correctly applied experimental design will assist the management of a process by the gaining the knowledge to:

1. improve performance characteristics;
2. reduce costs
3. decrease product development and production time

For the engineer or scientist experimental designs are used as:

1) an effective method for identification of the key input factors;
2) the most efficient method to understand the relationship between the input factors and the response;
3) a means of building a mathematical model relating the response to the input factor;
4) a means to set the input parameters to optimise the response according to requirements
5) a scientific means for setting the process/experiment tolerances.

3. THE STAGES IN EXPERIMENTAL DESIGN

The design of experiments approach is structured as follows;

Phase One: Planning

1 Define the problem and establish objectives for the experiment.
2 Determine the measurable output, (product/process quality), and how to measure it.
3 Define the process. Determine experimental objective.
4 Establish potential causal factors, brain-storm sessions with all personnel involved with the process.
5 Prioritise factors and agree those to be included. Set levels.

Phase Two:Selection of an orthogonal array

6 Design the experiment, select orthogonal array and analysis strategy
7 Obtain all materials.
8 Plan the experiment, recording of data etc.

Phase Three:Experimentation

9 Perform the experiment.

Phase Four:Analysis

10 Analyse results.
11 Predict best condition and confidence.

Phase Five:Confirmation, implementation.

12 Run confirmation experiment/s using predicted best conditions. Apply to process. Monitor, refine and improve process as required.

The procedure in selecting a suitable design of experiments array is shown in flow chart form in Figure 1.

4. DEFINITION OF THE PROBLEM AND OBJECTIVES

These definitions should take the form of succinct statements that identify the problem or opportunity that exists, and the objectives which need to be achieved for completing the work. Due to the number of people involved in certain steps of the methodology it is important that the problem/opportunity and objectives are clearly stated so that there is no ambiguity and everybody is focused on a common goal. The objectives should state product requirements in terms of measurable response(s) that will be used to gauge the "robustness" of the process.

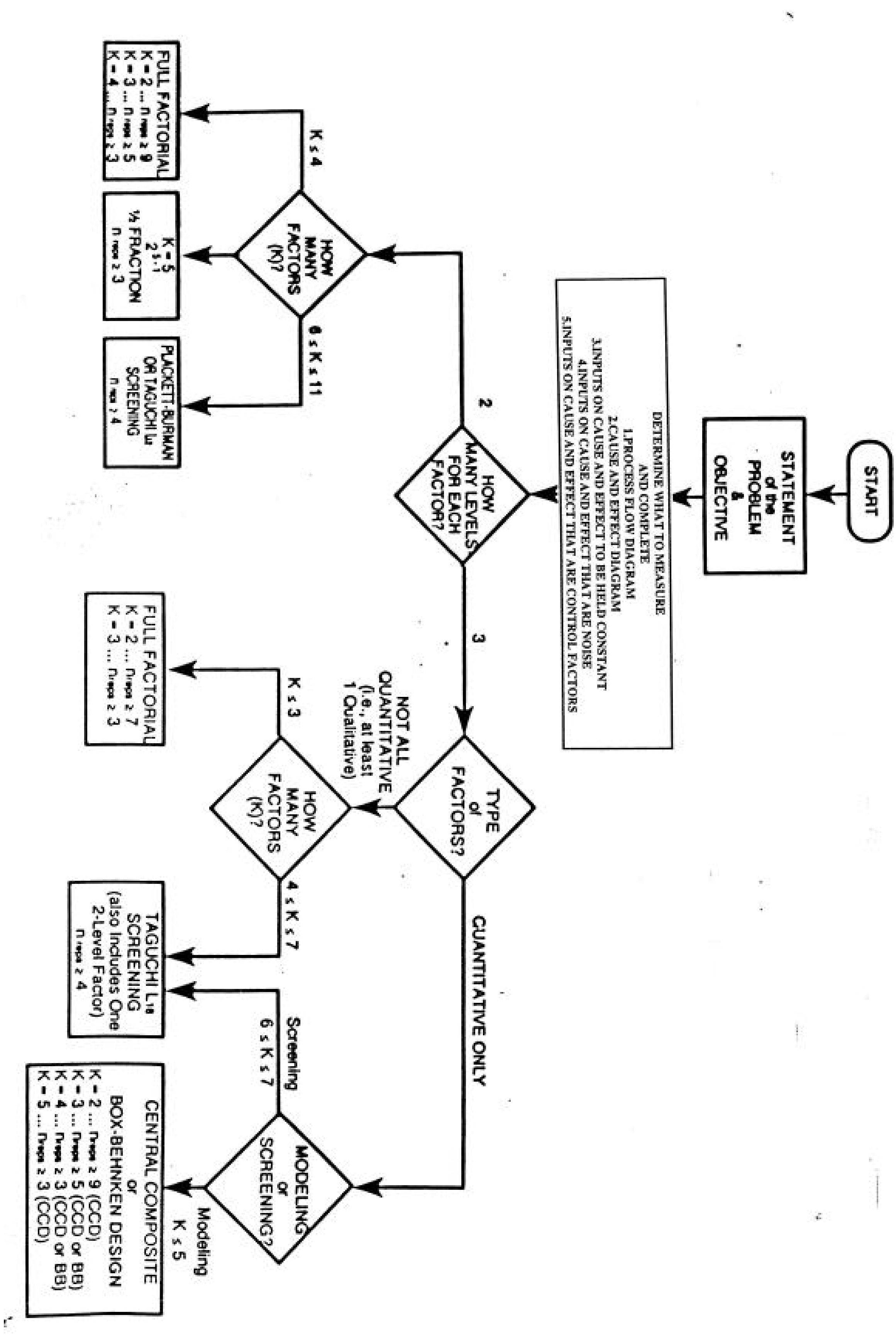

Figure 1:Guidelines For Choosing an Experimental Design, (Schmidt and Launsby[7])

For example, the important measurement stages in a construction sector DOE experiment, steel plate bonding [6] were found to be:

A	Preparation of the steel plate reinforcement
B	Mixing and application of the adhesive
C	Preparation of the bridge soffit, (the bottom surface of the bridge deck).

A flow chart highlighting the interaction of these stages is shown in Figure 2.

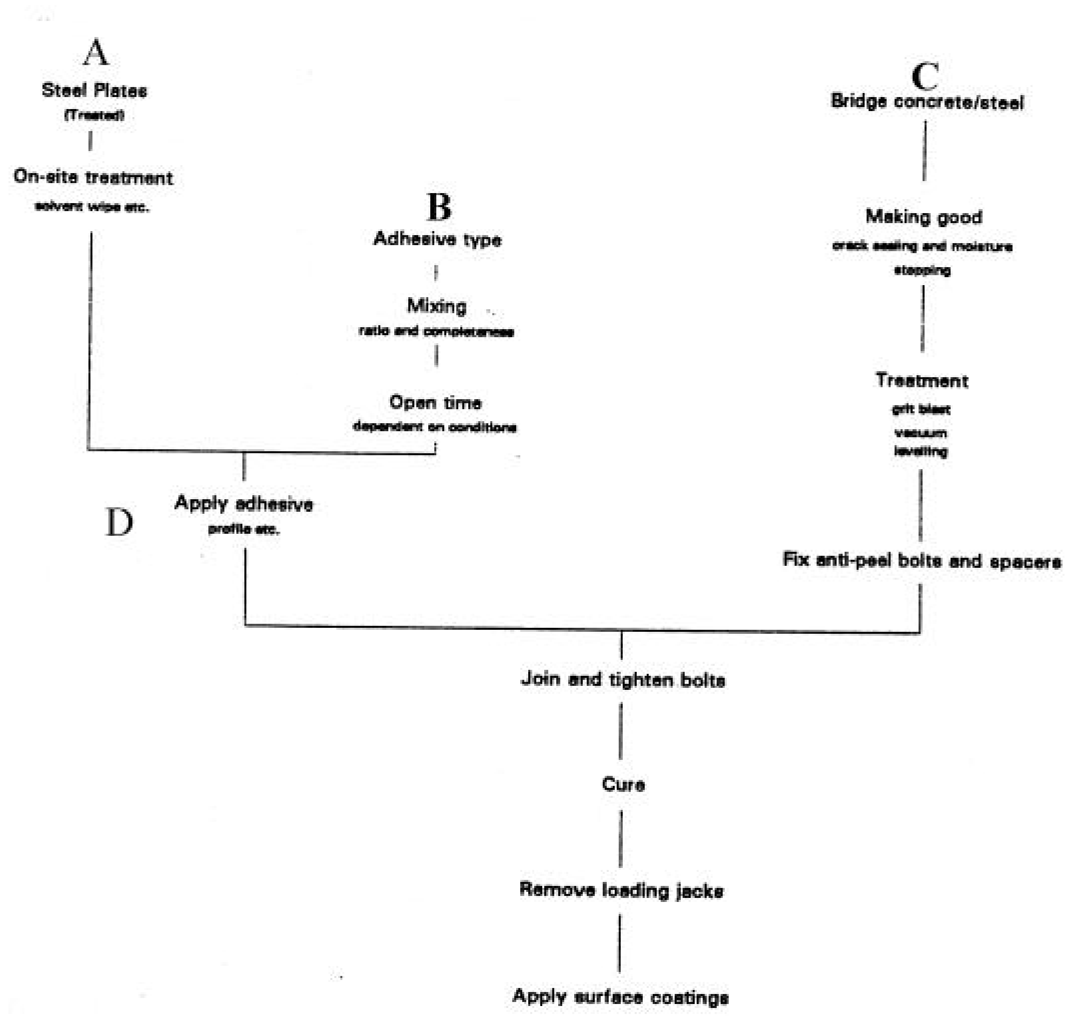

Figure 2:Flow Chart of Steel Plate Bonding Process

5. MEASUREMENT OF PRODUCT QUALITY

The type and quality of the measurement system used to gauge the performance of the measurement system should have good gauge capability (repeatability and reproducibility) so that ambiguity in the results is minimised.

6. DEFINITION OF THE PROCESS AND DETERMINATION OF THE EXPERIMENTAL OBJECTIVE

With the variety and number of people involved in process optimisation activity there is often some confusion between the perceived process (as documented and rumoured) and the actual process (as performed with informal "tweaks" and changes). To obtain a clear understanding of the process it is recommended that all the participants in the process optimisation visits and studies the process and asks all the questions necessary to remove any lingering doubts or confusion about materials, settings, testing, etc. After the process visit it is often useful to produce a process flow chart that acts as a point of reference in future steps. It should also be decided what design of experiments objectives are to be targeted. This aids the selection at a later stage of the methodology to be used in selecting an experimental design. The DOE objectives that can be investigated for a process/experiment are:

1 Troubleshooting: a troubleshooting experiment for example can be represented by four factors at two levels which determine the most important factors affecting the measured response

2 Screening: in which the dominant factors of the experiment/process are separated from those which have little or no effect.

3 Modelling: a process/experiment can be modelled by selecting three factors at three levels and using Central composite or Box-Behnken design matrix

4 Robust Design: a robust design can be demonstrated by determining the settings for the control factors so that the measured response is insensitive to the noise, i.e. the uncontrollable factors, for example, machine wear, the weather, etc.

7. IDENTIFICATION OF THE CAUSAL FACTORS

At this stage all the potential factors that could possibly cause variation in the quality of the finished product need to be identified. This is probably the most critical stage in the methodology as the experimentation to determine the optimum process will depend on the correct identification of the key causal factors. Two useful techniques to help run this stage efficiently are brainstorming and, cause and effect analysis. The cause and effect analysis breaks the process down into five sections to focus the mind, as follows; people, machines, environment, methods and materials. When using the two techniques it is often necessary to have the sessions independently facilitated so that the views and ideas of everyone are identified rather than those of the strong willed, or senior staff.

8. ESTABLISHING THE CONTROL FACTORS

This stage identifies the key causal factors (now referred to as control factors) that will be taken forward for experimentation. It should be noted that the causal factors, (identified in Section 7), can be placed in one of the following two categories, namely, control factors which can be placed under control readily, and noise factors which are hard, impossible or costly to control.

It is important to limit the number of control factors so that the experimental stage is not cumbersome and costly, large number of experiments required. This stage may be difficult, typically 10 or less factors would be recommended. Again, an independent facilitator can be very useful in getting consensus from the brainstorming participants about identifying the control factors that will be investigated in the experimentation and a voting system is often accepted as a fair method for ranking the importance of the factors.

For example, the steel plate bonding process selected ten process variables, shown in Table 1.

Process stage	Variable
A	Degreasing of steel substrate
B	Adhesive type
B	Resin to Hardener Mix ratio
B	Humidity at mixing
B	Open time to joint closure
C	Concrete/Steel surface condition
C	Vacuum cleaning of concrete/steel substrate
D	Adhesive Application
D	Cure Temperature
D	Curing period before testing

Table 1:Process variables for steel plate bonding case study [6]

9. DETERMINATION OF THE LEVELS FOR THE CONTROL FACTORS

The levels, or settings, for the control factors in the experiment are fixed to test the sensitivity of the process to each parameter. For the first experiment it is recommended that only two levels per control factor are used unless there is an overriding belief that the response between the two levels is non-linear and more levels would be required to determine the response function. It is appropriate to select levels that are as reasonably widely spread in order to realistically stress the process but not to a point of abnormal response or catastrophic failure.

From this data a design matrix showing the levels of each factor can be produced.

Table 2 shows the design matrix from the construction case study [(6)].

Variable	Setting 1	Setting 2
Degreasing of steel substrate	No solvent cleaning	Solvent cleaning
Adhesive type	Normal Grade	Low Temperature Grade
Resin to Hardener Mix ratio	Specified ratio	Minus 10% hardener
Humidity at mixing	10% Humidity	70% Humidity
Open time to joint closure	Minimum possible	Recommended maximum + 10%
Concrete/Steel surface condition	Wet	Dry
Vacuum cleaning of concrete/steel substrate	No Vacuum cleaning	Vacuum cleaning
Adhesive Application	Levelled over surface	Applied as discrete blobs
Cure Temperature	$23^{o}C$	Recommended minimum $+5^{o}C$
Curing period before testing	1 day	2 days

Table 2:Design matrix for steel plate bonding case study [(6)].

10. DESIGNING THE EXPERIMENT

Once the number of factors and their associated levels are agreed, a design matrix can be produced. This will display the levels of each factor for all the run combinations in the experiment. The number of experimental runs for a full factorial experiment, (all possible permutations of factors examined), can be determined from the relationship;

$$n = L^{f} \qquad \text{Equation 1}$$

where n = number of experimental runs
L = number of levels
and
f = the number of factors

Therefore for an experiment with 3 factors at 2 levels, a design matrix of all possible combination of runs, the full factorial, would have 8 experimental runs. It is obvious that the number of experimental runs rapidly increases with an increase in the number of factors and or levels; for example, the corresponding full factorial design matrix for the steel plate bonding example [(6)], would consist of 2^{10}, 1024 experiments.

The actual level settings are now coded as high and low or commonly as +1 and -1.

For example, Table 3 shows a simple 2 factor 2 level design

Run	Factor 1	Factor 2
1	+1	+1
2	+1	-1
3	-1	+1
4	-1	-1

Table 3:A 2 factor 2 level design matrix

11. DERIVATION OF EXPERIMENTAL DESIGN MATRICES - ORTHOGONALITY, ALIASING AND CONFOUNDING OF DESIGN MATRICES

Each column of factor values is now regarded as a vector in the design matrix. The concept of orthogonality is used in mathematics to describe the independence among vectors, factors, functions etc. For example, in design of experiments, (DOE), it is used to describe the effect of each of the factors on the response, and on the variation of the response, in an experimental matrix. The matrix shown in Table 3 is an orthogonal matrix, that is to say that it is vertically and horizontally balanced. Mathematically, vertical balancing occurs if the sum of each column is zero. Horizontal balancing occurs when the sum of the products of the corresponding rows in two columns is zero. A design is horizontally balanced if each two column combination sums to zero.

When a matrix is balanced vertically **and** horizontally the matrix is said to be orthogonal. These orthogonal matrices allow factors to be evaluated independently and are an important feature in the Taguchi methodology of design of experiments. Features of non orthogonal matrices are aliasing and confounding. In a design, matrix columns which are identical are said to be aliased.

A non-orthogonal matrix with aliased columns is shown in Table 4.

Run	A Factor 1	B Factor 2	C Factor 3
1	+1	+1	+1
2	+1	-1	-1
3	-1	+1	+1
4	-1	-1	-1

Table 4: A non-orthogonal matrix with aliased columns B and C

It can be seen from Table 4 that the vector dot products for A.B and A.C are 0, whereas for B.C is 4. This **aliasing** prevents individual assessment of the factors B and C any analysis of the resultant experimental runs for the factor B would include an undetermined dependence upon factor C and vice-versa.

The alternative, a vertically balanced non-orthogonal matrix is shown in Table 5.

Factors				
Run	A	B	C	D
1	+1	-1	-1	-1
2	-1	+1	-1	-1
3	-1	-1	+1	-1
4	-1	-1	-1	+1
5	+1	+1	+1	+1
6	-1	+1	+1	+1
7	+1	-1	+1	+1
8	+1	+1	-1	+1
9	+1	+1	+1	-1
10	-1	-1	-1	-1

Table 5:Non-orthogonal matrix without aliased columns.

It can be determined from this example that each possible matrix dot product is 0 and that the factors are not aliased, (identical columns). In consequence the factors are said to be confounded, partially aliased with each other. Aliasing plays an important part in the design of fraction factorial matrices for experimental design. As was seen from equation 1, it does not take a large number of factors or levels before the number of experimental runs for a full factorial design to become unwieldy and uneconomical, also it is often the case that the higher level interactions , 3-way and higher are often insignificant. This is where an orthogonal design, the fractional factorial, can be utilised.

The number of experimental runs in a fractional factorial array for a designated number of factors can be calculated by use of equation 2

$$n = L^{f-q} \qquad \text{Equation 2}$$

Where N = the number of experimental runs
L = the number of levels
f is the number of factors
and
q is a positive integer which designates the fraction of the full factorial matrix,

i.e. q = 1 equals a 1/2 design fraction, q =2 equals a 1/4 design fraction, q = 3 equals an 1/8 design fraction ...q= 6 equals a 1/64 design fraction etc.

In fractional factorial designs, selected higher order interactions, AxBxC etc. which are deemed to be insignificant are aliased with other single factors.

This can be seen in the following example. For a 2-level experiment with 4 factors, A, B, C and D, n = 16, (from equation 1), if we assume that the high level interaction ABC is insignificant then q = 1 and then, according to equation 2, the number of runs, n, equals 2^{4-1} or 8 runs a half fraction. This 2^{4-1} fractional factorial design matrix is shown in Table 6.

Factors							
Run	A	B	C	AB	AC	BC	D=ABC
1	-1	-1	-1	+1	+1	+1	-1
2	-1	-1	+1	+1	-1	-1	+1
3	-1	+1	-1	-1	+1	-1	+1
4	-1	+1	+1	-1	-1	+1	-1
5	+1	-1	-1	-1	-1	+1	+1
6	+1	-1	+1	-1	+1	-1	-1
7	+1	+1	-1	+1	-1	-1	-1
8	+1	+1	+1	+1	+1	+1	+1

Table 6:A 2^{4-1} fractional factorial design matrix

The column designated D=ABC does not imply that the effects of factor D and the interactions of factors A, B and C are equivalent, it shows that in resolving the data the effects due to the two separate factors cannot be separated. Thus if this column effect is significant it is due to D, ABC or a combination of D and ABC. The aliasing structure can be obtained from a DOE software packages.

We refer to the steel plate bonding case study [6], as shown in Table 2 with ten factors at 2 levels. This would require 1024 experimental runs. A design factor of 6 was selected, i.e. q = 6 and therefore the design matrix was reduced to 16 runs , a 1/64 design fraction, $2^{(10-6)}$ fractional factorial design matrix. The resulting aliasing structure from use of a L_{12} fractional array up to and including the second order interactions is shown in Table 7.

There exists 2^q different fractions of a 2^{f-q} design. For the steel plate bonding example, as q = 6 there are 2^6, 64 different fractional designs represented by +1 or -1 in the factors columns.

EFFECT	ALIASES
A	BH CJ BCF BDG CDI EGJ EHI FGI FHJ
B	AH EI FJ ACF ADG CGI CHJ DEJ DFI
C	AJ EG FH ABF ADI BGI BHJ DEH DFG
D	EF GH IJ ABG ACI BEJ BFI CEH CFG
E	BI CG DF AGJ AHI BDJ CDH FGH FIJ
F	BJ CH DE ABC AGI AHJ BDI CDG EGH EIJ
G	CE DH ABD AEJ AFI BCI CDF EFH HIJ
H	AB CF DG AEI AFJ BCJ CDE EFG GIJ
I	BE DJ ACD AEH AFG BCG BDF EFJ GHJ
J	AC BF DI AEG AFH BCH BDE EFI GHI
AB	H CF DG AEI AFJ BCJ CDE EFG GIJ
AC	J BF DI AEG AFH BCH BDE EFI GHI
AD	BG CI AEF AGH AIJ BCE BDH CDJ EGI EHJ FGJ FHI
AE	GJ HI ABI ACG ADF BCD BEH BFG CEJ CFI DGI DHJ
AF	BC GI HJ ABJ ACH ADE BEG BFH CEI CFJ DGJ DHI
AG	BD EJ FI ACE ADH BEF BGH BIJ CGJ CHI DEI DFJ
AH	B EI FJ ACF ADG CGI CHJ DEJ DFI
AI	CD EH FG ABE ADJ BGJ BHI CEF CGH CIJ DEG DFH
AJ	C EG FH ABF ADI BGI BHJ DEH DFG
BC	AF GI HJ ABJ ACH ADE BEG BFH CEI CFJ DGJ DHI
BD	AG EJ FI ACE ADH BEF BGH BIJ CGJ CHI DEI DFJ
BE	I DJ ACD AEH AFG BCG BDF EFJ GHJ
BF	J AC DI AEG AFH BCH BDE EFI GHI
BG	AD CI AEF AGH AIJ BCE BDH CDJ EGI EHJ FGJ FHI
BH	A CJ BCF BDG CDI EGJ EHI FGI FHJ
BI	E CG DF AGJ AHI BDJ CDH FGH FIJ
BJ	F CH DE ABC AGI AHJ BDI CDG EGH EIJ
CD	AI EH FG ABE ADJ BGJ BHI CEF CGH CIJ DEG DFH
CE	G DH ABD AEJ AFI BCI CDF EFH HIJ
CF	H AB DG AEI AFJ BCJ CDE EFG GIJ
CG	E BI DF AGJ AHI BDJ CDH FGH FIJ
CH	F BJ DE ABC AGI AHJ BDI CDG EGH EIJ
CI	AD BG AEF AGH AIJ BCE BDH CDJ EGI EHJ FGJ FHI
CJ	A BH BCF BDG CDI EGJ EHI FGI FHJ
DE	F BJ CH ABC AGI AHJ BDI CDG EGH EIJ
DF	E BI CG AGJ AHI BDJ CDH FGH FIJ
DG	H AB CF AEI AFJ BCJ CDE EFG GIJ
DH	G CE ABD AEJ AFI BCI CDF EFH HIJ
DI	J AC BF AEG AFH BCH BDE EFI GHI
DJ	I BE ACD AEH AFG BCG BDF EFJ GHJ
EF	D GH IJ ABG ACI BEJ BFI CEH CFG
EG	C AJ FH ABF ADI BGI BHJ DEH DFG
EH	AI CD FG ABE ADJ BGJ BHI CEF CGH CIJ DEG DFH
EI	B AH FJ ACF ADG CGI CHJ DEJ DFI
EJ	AG BD FI ACE ADH BEF BGH BIJ CGJ CHI DEI DFJ
FG	AI CD EH ABE ADJ BGJ BHI CEF CGH CIJ DEG DFH
FH	C AJ EG ABF ADI BGI BHJ DEH DFG
FI	AG BD EJ ACE ADH BEF BGH BIJ CGJ CHI DEI DFJ
FJ	B AH EI ACF ADG CGI CHJ DEJ DFI
GH	D EF IJ ABG ACI BEJ BFI CEH CFG
GI	AF BC HJ ABJ ACH ADE BEG BFH CEI CFJ DGJ DHI
GJ	AE HI ABI ACG ADF BCD BEH BFG CEJ CFI DGI DHJ
HI	AE GJ ABI ACG ADF BCD BEH BFG CEJ CFI DGI DHJ
HJ	AF BC GI ABJ ACH ADE BEG BFH CEI CFJ DGJ DHI
IJ	D EF GH ABG ACI BEJ BFI CEH CFG

Table 7: Aliasing structure for the steel plate bonding case study [6]. A 1/64 design fraction, $2^{(10-6)}$ fractional factorial design matrix of resolution, R_{III}, the main effects are not aliased with each other, but main effects are aliased with two-way interactions.

12. RESOLUTION OF A FRACTIONAL FACTORIAL DESIGN

The term used to describe the effect of the degree of aliasing in a design is **resolution**, R. The definitions of the various. orders of resolution are as follows

R_{II} - Resolution 2	A design in which the main effects are aliased with other main effects. This is obviously a design to be avoided.
R_{III}- Resolution 3	A design in which the main effects are not aliased with each other, but main effects are aliased with two-way interactions . Used in screening a large number of factors to find the most important.

R_{IV}- Resolution 4 — A design where the main effects are not aliased with each other or with two-way interactions. However the two way interactions are aliased with each other. Designs of this type are typically used in the experimental modelling. Some knowledge of the two-way interactions is required to determine which aliased two-way interactions are used in the prediction equation.

R_V- Resolution 5 — A design in which the main factors and the two-way interactions are not aliased **and** the two way interactions are also not aliased with each other. Used in designing prediction equations that will not have concerns due to interactions.

R_N — Required if higher interactions are of interest. High resolution designs eliminate aliasing of higher order interactions.

From these definitions, it can be seen that the steel plate bonding fractional factorial design matrix had a resolution of R_{III}, the main effects are not aliased with each other, but main effects are aliased with two-way interactions, Table 7.

13. EXPERIMENTAL DESIGN TYPES

There are a number of experiment designs that have been used. These are summarised below by the desired experimental objective.

Levels of Factors	OBJECTIVE			
	Troubleshooting	Screening	Modelling	Robust design
2	Hadamard D-Optimal Taguchi	Hadamard/Foldover D-Optimal Plackett-Burman Taguchi	D-Optimal	D-Optimal Plackett-Burman Taguchi
3	D-Optimal Taguchi	D-Optimal Taguchi	Box-Behnken Central Composite D-Optimal	Fractional factorial Box-Behnken D-Optimal Taguchi
More than 3 levels	D-Optimal Taguchi	D-Optimal Taguchi	Central Composite D-Optimal	Central Composite D-Optimal Taguchi

Table 8: Experiment design types with regards to experimental objectives.

13.1. 2 LEVEL DESIGNS

13.2. FOLDOVER DESIGNS

A Foldover design is a type of fractional factorial in which the basic design is supplemented by a the complement of the basic design. For example, if we examine a 2^{3-1} fractional factorial design as shown in Table 9.

Run	Factors A	B	C=AxB interaction
1	-1	-1	-1
2	-1	+1	+1
3	+1	-1	+1
4	+1	+1	-1

Table 9: 2^{3-1} fractional factorial design, Resolution R_{III}.

Folding over this design obtains the design shown in Table 10.

Run	Factors A	B	C=AxB, interaction
1	-1	-1	-1
2	-1	+1	+1
3	+1	-1	+1
4	+1	+1	-1
5	-1	-1	+1
6	-1	+1	-1
7	+1	-1	-1
8	+1	+1	+1

Table 10:Foldover 2^{3-1} fractional factorial design, Resolution, R_{IV}.

As the fraction design integer, q, in this instance was 1 the basic design was a 1/2 factorial design. Folding over created the alternative half of the fractional design. Single folding over increases the resolution of the design, for the example shown in Tables 9 and 10 this increases the resolution from R_{III} to R_{IV}. Multiple folding over creates replicate runs.

13.3. PLACKETT-BURMAN DESIGNS

These designs are R_{III} designs used for screening a large number of factors. Plackett-Burman designs are based on Hadamard matrices which are generated in multiples of 4, not 2 as in fractional factorial designs, i.e. the number of runs is 4, 8, 12, 16, 20, 24.....etc. When a Plackett-Burman design has the same number of runs as a equivalent fractional factorial design, (where 2^{f-q}), the design is considered geometric and is the same as the fractional factorial matrix, albeit with the columns and rows reordered. For geometric R_{III} designs each 2 way interaction is positively or negatively aliased with a main effect. For the non-geometric designs, the main effects are only partially confounded with 2 way interactions. The advantage of these designs is that a limited number of runs is required to evaluate a large number of factors, however this assumes that the factor interactions are negligible. Taguchi design of experiments systems use these types of matrices.

13.4. NON-ORTHOGONAL 2 LEVEL DESIGNS

There are number of non-orthogonal 2 level design types these include: random balanced designs and D-optimal designs.

13.5. RANDOM BALANCED DESIGNS

This design is produced by randomly generating the first n/2 runs for f factors. This design is then folded to generate a the final balanced design. For this type of design the number of runs, n, is determined independently of the number of factors. The main disadvantage is that the confounding/aliasing structure is totally random. The correlation between the factors and interactions is required to be calculated for all possible pairs of effects. This type of design is generally avoided as resolution of the matrix is closely tied to the analysis of the data, thus once the correlation structure is determined the matrix can then be modified either to avoid correlation or to reduce them to satisfactory levels.

13.6. D-OPTIMAL DESIGNS

Due to the ready availability of low cost computing, a new class of experimental design has been developed, (ca. 1985), using near orthogonal matrices, called D-optimal designs. These designs are based on the optimisation of the determinant, D, of a matrix and its transpose, i.e. $D = |M'M|$. Optimal values of D vary with the number of runs, n.

Some of the advantages of these designs are:

1. They are able to be used for all for experimental objectives, screening, troubleshooting, modelling and robustness

2. Able to be used for any number of factors and levels

3. Can use qualitative, (i.e. method, material type, operator), and quantitative measurable, (time, pressure, concentration), factors
4. Designs accept any arrangement of interactions.
5. In the main they require fewer runs than an orthogonal design for the same situation

However there are some disadvantages these include:

1. There are no simple tables of available designs
2. D-Optimal designs are computer generated and require specialist software
3. Regression is required to resolve confounding in the designs
4. Designs are not generally orthogonal. The confounding due to lack of orthogonality further complicates the analysis

If a software program such as DOE Wisdom is available, (see Appendix 1), D-Optimal designs can be readily generated and analysed.

13.7. 3 LEVEL DESIGNS

Three level fractional factorial designs can be defined by Equation 3:

$$n = 3^{f-q} \qquad \text{Equation 3}$$

where

n= number of runs
f= number of factors
and q is an integer defining the fractional factorial.

Fractional factorials with three levels can be represented as three dimensional rectangular structures with mid-face and centre points, Figure 3. Their structure precludes the use of corner points and axial points, (see central composite and Box-Behnken designs, Sections 13.3 and 13.4).

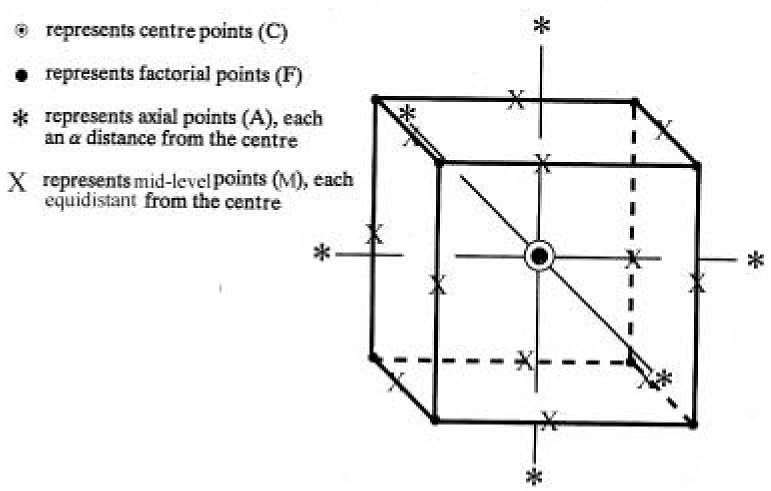

Figure 3:Graphical representation of a 3 level design

The variables are coded as -1, 0 and +1. Due to this combination of data, these designs can be used to test linear effects, quadratic, (second order effects) and linear -second order interactions. The deficiencies of these designs are the number of experimental trials required to estimate all the desired effects, the limited number of interactions that can be evaluated and the complex nature of the aliasing /confounding patterns. There level fractional factorial designs are very useful for screening qualitative factors which have few, if any, interactions

Table 11 summarises the main three level designs.

Design	Objective	Capabilities	Disadvantages
Fractional factorial 3^{f-q}	Modelling	Used for qualitative or quantitative factors	Large number of runs required. Limited number of interactions can be evaluated. Complex confounding/aliasing patterns
Box-Behnken	Modelling	Used for quantitative factors. Estimates **all** linear, quadratic and 2-way linear interactions	The required number of runs estimates all linear, quadratic and 2-way linear interactions, whether required or not
Box-Wilson or Central Composite design	Modelling Robust design	Estimates all linear , quadratic effects. Estimates all some or no interactions	Best suited to quantitative factors
D-Optimal	Screening Troubleshooting Modelling Robust design	Able to be used for any number of factors and levels. Can use qualitative and quantitative factors. Designs accept any arrangement of interactions. Require fewer runs than an orthogonal design for the same situation.	No simple tables of available designs. Specialist computer software is required to generate designs. Regression is required to resolve confounding in the designs. Designs are not generally orthogonal. The resultant confounding structure further complicates the subsequent analysis.

Table 11:The Main Three Level Designs

13.8. BOX-BEHNKEN DESIGNS.

For the modelling of three level quantitative factors, time, pressure, temperature, concentration, flow, etc., Box-Behnken designs are frequently used. These designs effectively use a series of embedded 2^2 designs and maintain specific factors at their mid or centre point. Table 12 shows a Box-Behnken design for four 3 level factors.

These Box-Behnken designs do not contain any corner points in their structure. The designs are near orthogonal, resolution 5, R_V, systems and the number of experimental trials is always sufficient to estimate all the linear and quadratic effects and all linear 2-way interactions whether they are required or not. If the number of factors to be examined is greater than 4, then the equivalent central composite design, (Section 13.4), will require fewer experimental trials. The non-orthogonality does not cause any problems if the analysis is carried out using linear least squares techniques.

FACTORS				
Run	A	B	C	D
1	-1	-1	0	0
2	-1	+1	0	0
3	-1	-1	0	0
4	+1	+1	0	0
5	0	0	-1	-1
6	0	0	-1	+1
7	0	0	+1	-1
8	0	0	+1	+1
9	0	0	0	0
10	-1	0	0	-1
11	-1	0	0	+1
12	+1	0	0	-1
13	+1	0	0	+1
14	0	-1	-1	0
15	0	-1	+1	0
16	0	+1	-1	0
17	0	+1	+1	0
18	0	0	0	0
19	-1	0	-1	0
20	-1	0	+1	0
21	+1	0	-1	0
22	+1	0	+1	0
23	0	-1	0	-1
24	0	-1	0	+1
25	0	+1	0	-1
26	0	+1	0	+1
27	0	0	0	0

indicates 2^2 embedded designs. The 3 orthogonal blocks are separated by ===
Table 12:Complete series of experimental trials for a four factor Box-Behnken design

Box and Behnken[8] have produced tables for up to 16 factors, (F = 8 excluded).

13.9. BOX-WILSON OR CENTRAL COMPOSITE DESIGNS.

Box-Wilson or more commonly known central composite designs are used to model 3-level quantitative factors. A typical design is formulated from three distinct blocks:

1. F. a factorial block which can be of resolution R_{III}, R_{IV} or R_{IV} depending on the application and level of interactions required.

2. C. a centre point block which gives an estimate of pure experimental error and maintains the orthogonality of the design. The number of centre points, n_c, can be calculated from the formula :

$$n_c = \sqrt[4]{n_f + 1} - 2f \qquad \text{Equation 4}$$

where:

n_c = number of centre points
n_f = number of experimental trials in the factorial (2-level) block
and f = number of factors

3. A. an axial block of 2*f, experimental trials. The value of α is commonly chosen to be $(n_f)^4$ where n_f is the number of experimental trials in the factorial (2-level) block. These values are selected to allow the predicted response to be estimated with equal variance from any direction from the centre of the design. This property is called **rotatability**. Whilst rotatability is a desired design property it is not a compulsory feature for a successful design.

To satisfy the requirements for a rotatable design the value of α can assume values greater than 1, this means that ±1 no longer represents the factor maximum and minimum. To resolve the transformation to or from the standard design notation of 0, ±1 and ±α the following mathematical transformation is applied:

If we assume for this example that:

α = 1.5
factor +1 = 100
and factor -1 = 10
therefore factor 0 = (100+10)/2 = 55

as ±1 no longer represent the factor maximum and minimum this is now assumed by ±α, therefore the real value of ±1 is now the midpoint, 0, ± a fraction, Δ, which is dependent on the value of α.

design value	-a	**-1**	**0**	**+1**	+a
real value	10	55-Δ	55	55+Δ	100

As for our example as we assumed $\alpha = 1.5$

$$\Delta/2 = (100\text{-}10)/1.5 = 60$$

and

$$\Delta = 30$$

Therefore the completed transformation table is:

design value	**-**a	**-1**	**0**	**+1**	**+**a
real value	10	25	55	85	100

This shows the 5 factor values to be set for the experimental trials to complete a central composite design.

If it is difficult to run 5 levels of a factor, by setting α equal to 1, i.e. α lies within a face, a central composite face design, (CCF), is generated. This design has no rotatability and has a reduced orthogonality, especially for quadratic terms.

The advantages of the central composite design include:

i) It is the most effective and efficient design for modelling second order quantitative factors.

ii) The factorial block can be of any resolution

iii) The aliasing and confounding structure is developed as for 2-level fractional factorials.

iv) The ability exists to run the designs sequentially thus saving resources. In this case the factorial and centre blocks are performed first. The linear model is then developed from the 2-level block and the centre point results are predicted. If this linear model is not validated at the centre points then additional runs are added from the axial block to complete a quadratic model.

The main disadvantage is that these designs are primarily suited to quantitative factors. A central composite design can be adapted to cater for a single qualitative factor, but if several exist in your process then a fractional factorial or a D-optimal design is best suited.

Table 13 shows a central composite design developed for three 3-level factors. Table 13a shows a full factorial block, whilst Table 13b develops a fractional factorial block.

f = 3 using a 2^3 full factorial FACTOR				
Run	A	B	C	
1	-1	-1	-1	Factorial Block
2	-1	-1	+1	
3	-1	+1	-1	
4	-1	+1	+1	
5	+1	-1	-1	
6	+1	-1	+1	
7	+1	+1	-1	
8	+1	+1	+1	
9	0	0	0	Centre point Block
10	0	0	0	
11	0	0	0	
12	0	0	0	
13	0	0	0	
14	0	0	0	
15	+α	0	0	Axial Block
16	-α	0	0	
17	0	+α	0	
18	0	-α	0	
19	0	0	+α	
20	0	0	-α	

Table 13a: Central composite designs for three 3-level factors using a 2^3 full factorial block

f = 3 using a 2^{3-1} 1/2 fractional factorial FACTOR			
A	B	C	
-1	-1	-1	Factorial Block
-1	+1	+1	
+1	-1	+1	
+1	+1	-1	
0	0	0	Centre point Block
0	0	0	
0	0	0	
+α	0	0	Axial Block
-α	0	0	
0	+α	0	
0	-α	0	
0	0	+α	
0	0	-α	

Table 13b:Central composite design for three 3-level factors using 2^{3-1} a 1/2 fractional factorial

Table 13:Central composite design developed for three 3-level factors

13.10. NESTED DESIGNS.

A special design type is required when the settings of one factor are dependent upon the settings of another. For example, if two different adhesives, (1, 2) are used in a process, (Factor A), with different curing temperature ranges, i.e., adhesive 1 cures between 70 to 90°C and adhesive 2 between 100 to 120°C. If additional factors such as cure time, (Factor B), and application method, (Factor C), are introduced, the process can be depicted as shown in Figure 4. It can be seen from Figure 4 that the temperature, factor B is not a 2-level factor as its levels are different for each level of Factor A, the adhesive. For cases such as these Factor B is said to be “nested” in Factor A and a standard 2-level design cannot be applied.

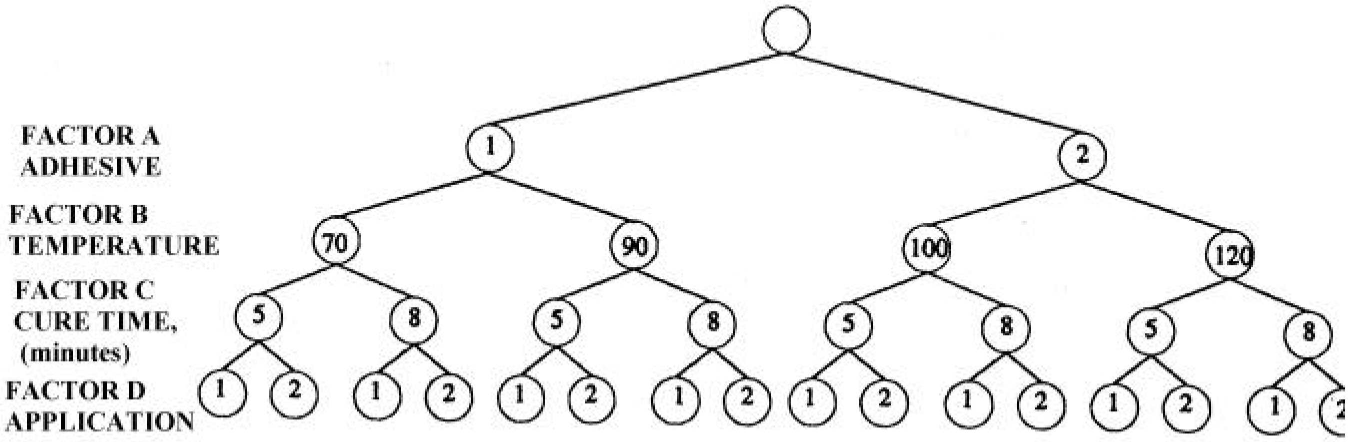

Figure 4:Design tree showing the combinations of 4 factors with factor B nested in A

This type of process is relatively common [2, 4]. In some cases these types of nested processes can be treated relatively simply. The process shown in Figure 4 can be easily resolved into two sets of experiments, Figure 5.

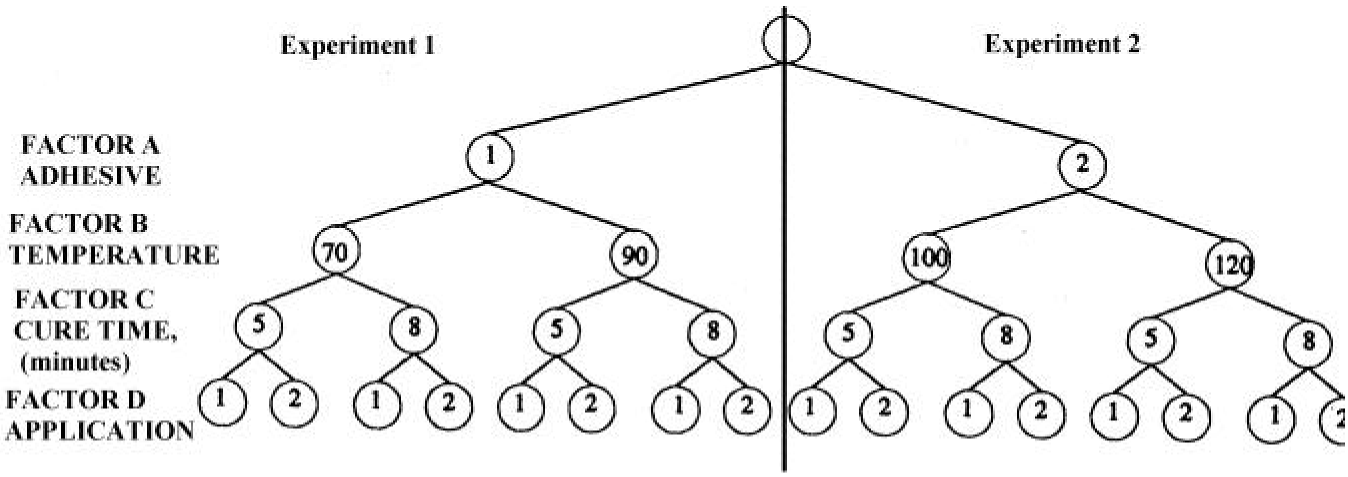

Figure 5:Design tree showing the combinations of 4 factors with factor B nested in A resolved into two experiments

Effectively splitting the process into 2, reduces the design to a simple 2-level system. More complex nested designs that cannot be resolved into an easier design can be developed using specialist software, such as CARD Design of Experiments Software for Windows by S-Matrix Corp. and DOE KISS for Excel by Air Academy Associates, (see Appendix 1).

13.11. MIXTURE DESIGNS.

These are use when you have a process where the sum of the actual factor settings must equal the same fixed total for every run. These types of systems occur frequently in a chemical processing environment where for example a chemical compound is being manufactured. These designs can get quite complex and once again specialist software as detailed in Appendix 1 should be used.

14. TAGUCHI DESIGN

Dr Genichi Taguchi developed a standardised approach to the use of the design of experiments methodology. The basis of the techniques he proposed arose out of three fundamental concepts:

1. Quality should be designed into a product and not by rejection after inspection.

2. Quality is achieved by minimisation of the deviation from a target.

3. The cost of quality should be measured as a function of deviation from the standard and be measured over the complete process.

A Taguchi design of experiments strategy is characterised by three elements

1. The use of special tabulated orthogonal arrays based on fractional factorials, Plackett-Burman or Latin Squares designs. The use of the ‘Taguchi” matrices forces a consistent approach to the design of experiments methodology.

2. Design modification to allow an outer array which contains noise factors, i.e. those factors which are impossible to control , for example the weather, machinery wear etc. The inner array holds the control factors. This use of an outer array take the place of randomisation when performing experimental runs, which has the effect of spreading the noise factors over the experiment. Taguchi assumes that noise factors are measurable and can be included in the design.

3. Analysis of a function called the signal to noise ratio which will determine the optimum conditions.

This S/N function is evaluated at one of three levels

A the bigger the better
B the smaller the better
C the nominal the better

The Taguchi method stresses simplicity in its approach and avoids statistical rigor in its approach to the analysis of the results. The method thus has to face a deal of criticism from traditional statisticians. However due to its appeal to engineers, its use is increasing and provides a good starting point for the use of the design of experiments methodology.

The main use of the Taguchi approach is in robust designs. Disadvantages are that the signal to noise, S/N ratio is not the best measure for determining the optimal condition. The Taguchi approach is considered to be 60-80% effective in obtaining the optimal, whereas the classical methods are of the order of 80-100% effective. Cost effectiveness will commonly drive the choice of methodology.

14.1. COMPARISON OF CLASSICAL AND TAGUCHI APPROACHES

	Classical		Taguchi
1.	Brainstorming. Select responses based on customer needs	1	Brainstorming. Try to select responses that do not have interactive inputs
2	Effects to be tested depend on screening or modelling strategy. Screening: estimates only linear effects. Modelling linear 2-way interactions and non-linear (or quadratic) effects are included.	2	Effects to be tested are usually the linear and quadratic effects. 2-way interactions can be included, but emphasis is given to the other effects for simplicity
3	Statistical determination of sample size	3	Sample size is based upon the number of effects to be tested and the levels of each factor.
4	Build an orthogonal design	4	Tables of selected orthogonal designs are used
5	Randomise the order of runs to spread unmeasured noise factors across all effects	5	Experiments are run according to the difficulty level of resetting the specific factor.
6	Run experiment under rigorous conditions	6	Run experiment under rigorous conditions
7	Normal probability charts used to estimate errors in desired effects	7	S/N ratio use to examine effects
8	Construct a simplified mathematical model to i) predict the output for any setting of the inputs ii) estimate prediction intervals iii) estimate the optimal response and iv) model the sensitivity and tolerance of the model	8	Important effects are determined graphically, Box-Whisker plots etc. based on the largest sound to noise ratio
9	If the optimal response is outside the sample region then additional experiments are run in the direction of the optimal	9	No iterative experimentation
10	Find optimal response through an iterative experimental procedure	10	"Optimal response" is based on the 'best' response from the total experiment
11	Confirmatory runs based on optimal process	11	Confirmatory runs based on optimal process

15. COLLECTION OF THE EXPERIMENTAL MATERIAL

To ensure that the experiment is performed in an efficient, timely and consistent manner it is important to assemble all parts, materials and consumables before any practical work commences. This will allow the experiment to be run in the required randomised manner and minimise the opportunity for extraneous noise factors to be introduced. In the same vein sufficient materials should be collected initially to support both the matrix experiment and the final confirmation trial.

16. PLANNING THE EXPERIMENT

To maintain consistency and control through the experiment (particularly with larger experimental arrays) it is critical to have a plan that ensures equipment is available and the people know what to do and when to do it. The array can often appear daunting to see all the changes in levels for each control factor so it can be helpful to use separate work sheets for each run.

17. PERFORMING THE EXPERIMENT

In performing the experiment it is important to have a disciplined approach in line with the plan so that the randomisation is maintained and the runs are performed in a consistent manner. There should be supervision available and communication from the operator should be encouraged so that no confusion is introduced into the experiment or results. The operator should be encouraged to record any observations (e.g. unusual features, difficulties, etc.) made during the experiment. Such observations are often useful in explaining unexpected results or analysis output.

18. ANALYSIS OF THE RESULTS AND DETERMINATION OF THE ‘OPTIMUM’ PROCESS CONDITIONS

Before commencing with a detailed analysis of the results from the experiment, it is useful to perform a preview assessment with two analysis tools that provide a simple pictorial view of the whole experiment.

Firstly box whisker plots are used on repetition results for each run. This exploratory data analysis tool acts as a "sanity" check on the data consistency in the order in which the experiment was run and, also can be used to assess whether some transformation of the data is necessary for further analysis. Secondly response plots are used to visualise the effect of the control factors at each level.

To demonstrate the analysis of a designed experiment a case study for precision mechanical assembly in the computer industry will be examined in depth[1].

18.1. DESIGN OF EXPERIMENTS:EXAMPLE - RAIL BONDING ON A COMPUTER HARD DISC ASSEMBLY

A smooth zirconia ceramic rail is bonded to a nickel coated mild steel core using an anaerobic curing adhesive. This provides the guide rail for the movement of the read/write head to traverse the magnetic hard disc unit

Problem: Variable bond strength frequently falling below quality targets. High inspection levels and high rejection rates were suffered, ("inspect in quality").

Objective: Bond strength better than 2kN.

Experiment: 7 process variables were identified and the associated levels determined. The process variables are shown in Table 14.

Label	Factor	Level 1	Level 2
A	Parts temperature	20°C	50°C
B	Time in fixture before cure	5 min	15 min
C	Time to cure	30 min	120 min
D	Time in solvent wash (cleans excess adhesive)	5 min	20 min
E	Time in oven (final cure)	30 min	60 min
F	Oven temperature	20°C	100°C
G	Time post oven	4 hours	12 hours

Table 14:Process variables and associated levels for rail bonding on a computer hard disc assembly.

A flow diagram of the process is shown in Figure 5.

A L_{12} orthogonal array was selected for the experimental design with columns 2- 8 used to hold the experimental factors, Table 15.

Trial.	Column Number										
	1	2	3	4	5	6	7	8	9	10	11
1	1	1	1	1	1	1	1	1	1	1	1
2	1	1	1	1	1	2	2	2	2	2	2
3	1	1	2	2	2	1	1	1	2	2	2
4	1	2	1	2	2	1	2	2	1	1	2
5	1	2	2	1	2	2	1	2	1	2	1
6	1	2	2	2	1	2	2.	1	2	1	1
7	2	1	2	2	1	1	2	2	1	2	1
8	2	1	2	1	2	2	2	1	1	1	2
9	2	1	1	2	2	2	1	2	2	1	1
10	2	2	2	1	1	1	1	2	2	1	2
11	2	2	1	2	1	2	1	1	1	2	2
12	2	2	1	1	2	1	2	1	2	2	1

Columns selected from the L_{12} array to represent the 7 process variables

Table 15 Experimental L_{12} array selected for Precision mechanical assembly

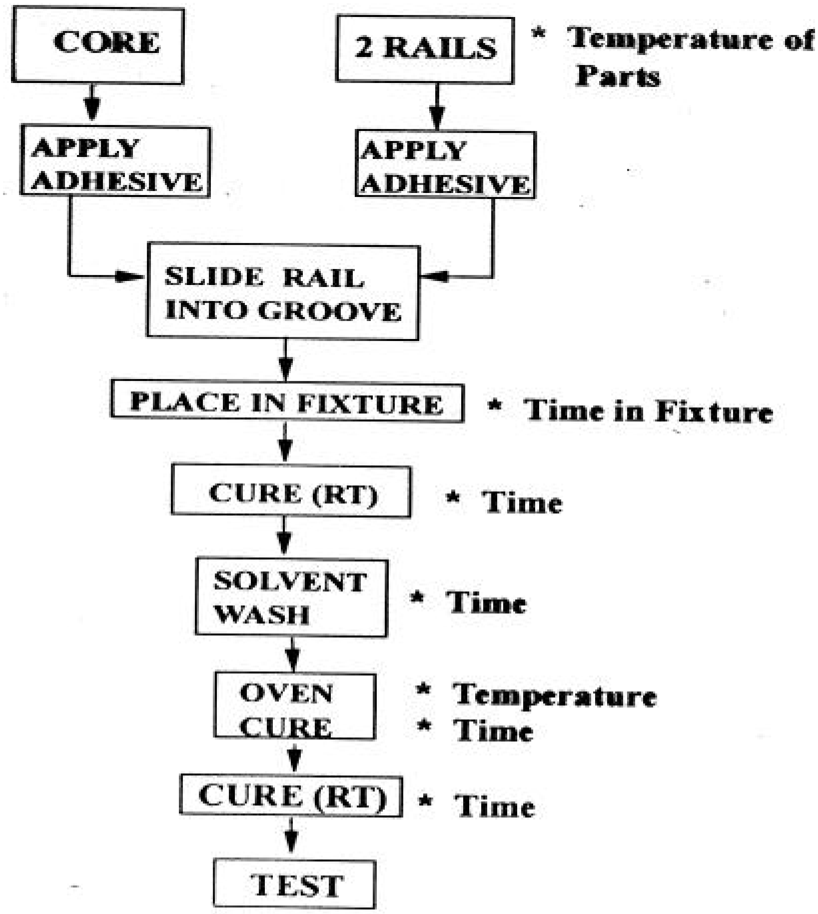

Figure 5:Flow chart of Core/Rail bonding process and control factors*

With column 2 used for factor F the results matrix is as shown in Table 16.

Trial	Variable Factors											Results, (kN)* (R)				
	1	F	A	B	C	D	E	G	9	10	11	Average Bond Strength	Run 1	Run 2	Run 3	Run 4
1	X	1	1	1	1	1	1	1	X	X	X	2.61	3.98	5.23	0.58	0.64
2	X	1	1	1	1	2	2	2	X	X	X	6.33	7.12	2.82	8.99	6.38
3	X	1	2	2	2	1	1	1	X	X	X	4.97	3.92	3.92	6.16	5.86
4	X	2	1	2	2	1	2	2	X	X	X	5.78	8.45	3.47	8.66	2.53
5	X	2	2	1	2	2	1	2	X	X	X	10.00	10.79	10.07	10.08	9.06
6	X	2	2	2	1	2	2.	1	X	X	X	0.92	2.73	0.00	0.96	0.00
7	X	1	2	2	1	1	2	2	X	X	X	2.58	6.97	0.00	3.34	0.00
8	X	1	2	1	2	2	2	1	X	X	X	4.24	3.88	3.56	6.69	2.81
9	X	1	1	2	2	2	1	2	X	X	X	11.50	11.39	10.25	12.15	12.19
10	X	2	2	1	1	1	1	2	X	X	X	6.56	8.87	0.42	9.74	7.19
11	X	2	1	2	1	2	1	1	X	X	X	0.49	0.66	0.00	1.06	0.23
12	X	2	1	1	2	1	2	1	X	X	X	1.14	1.90	0.00	1.90	0.76
Total												**57.09**	**70.66**	**39.74**	**70.31**	**47.65**
Grand Average Performance, G.A.P., (Trials/number of runs)												**4.758**				
Randomising sequence: Replicate 1 trial numbers 6, 5, 9, 4, 12, 7, 8, 2, 11, 1, 10, 3 Replicate 2 trial numbers 7, 6, 5, 9, 4, 2, 10, 1, 11, 3, 12, 8																

Table 16 Results L_{12} array for Precision mechanical assembly

The objective of the subsequent analysis is to seek answers to the following:

1. What is the optimum condition?
2. Which factors contribute to the results and by how much?
3. What will be the expected result at the optimum condition?

18.2. CALCULATION OF THE AVERAGE PERFORMANCE OF FACTORS, THE OPTIMUM CONDITION

The average performance of the factors at levels 1 and 2 can be calculated by adding the results which contain the factor for example A_1, (Factor A at level 1), and dividing by the number of trials which contain that factor.

The average performance of the factors shown in Table 13 is calculated as follows:

$\overline{A}_1 = (R_1 + R_2 + R_4 + R_9 + R_{11} + R_{12})/6 = (2.61 + 6.33 + 5.78 + 11.50 + 0.49 + 1.14/6 = 4.64$
$\overline{A}_2 = (R_3 + R_5 + R_6 + R_7 + R_8 + R_{10})/6 = (4.97 + 10.0 + 0.92 + 2.58 + 4.24 + 6.56)/6 = 4.88$
$\overline{B}_1 = (R_1 + R_2 + R_5 + R_8 + R_{10} + R_{12})/6 = (2.61 + 6.33 + 10.0 + 4.24 + 6.56 + 1.14)/6 = 5.15$
$\overline{B}_2 = (R_3 + R_4 + R_6 + R_7 + R_9 + R_{11})/6 = (\qquad)/6 = 4.37$
$\overline{C}_1 = (R_1 + R_2 + R_6 + R_7 + R_{10} + R_{11})/6 = (\qquad)/6 = 3.25$
$\overline{C}_2 = (R_3 + R_4 + R_x + R_x + R_x + R_x).6 = (\qquad)/6 = 6.27$
$\overline{D}_1 = (R_x + R_x + R_x + R_x + R_x + R_x)/6 = (\qquad)/6 = 3.94$
$\overline{D}_2 = (R_x + R_x + R_x + R_x + R_x + R_x)/6 = (\qquad)/6 =$
$\overline{E}_1 = (R_x + R_x + R_x + R_x + R_x + R_x)/6 = (\qquad)/6 =$
$\overline{E}_2 = (R_x + R_x + R_x + R_x + R_x + R_x)/6 = (\qquad)/6 =$
$\overline{F}_1 = (R_x + R_x + R_x + R_x + R_x + R_x)/6 = (\qquad)/6 =$
$\overline{F}_2 = (R_x + R_x + R_x + R_x + R_x + R_x)/6 = (\qquad)/6 =$
$\overline{G}_1 = (R_x + R_x + R_x + R_x + R_x + R_x)/6 = (\qquad)/6 =$
$\overline{G}_2 = (R_x + R_x + R_x + R_x + R_x + R_x)/6 = (\qquad)/6 =$

The relationships above have been left incomplete so that the reader can satisfy themselves as to the method of calculation. The average factor effect is shown in Table 17 and as a response graph in Figure 6.

Level	A	B	C	D	E	F	G
1	4.64	5.15	3.25	3.94	6.02	5.37	2.40
2	4.88	4.37	6.27	5.58	3.50	4.15	7.13
Difference	0.24	0.77	3.02	1.64	2.52	1.22	4.73

Table 17: The average factor effect for Precision mechanical assembly experiment

The average effects are shown in a response plot, Figure 6:

As experimental runs were replicated a signal to noise analysis could also be undertaken, (S/N) ratio.

This S/N function is evaluated at one of three levels:

- A the smaller the better
- B the nominal the better
- C the bigger the better

These criteria represent all the possible variations of a quality measure.

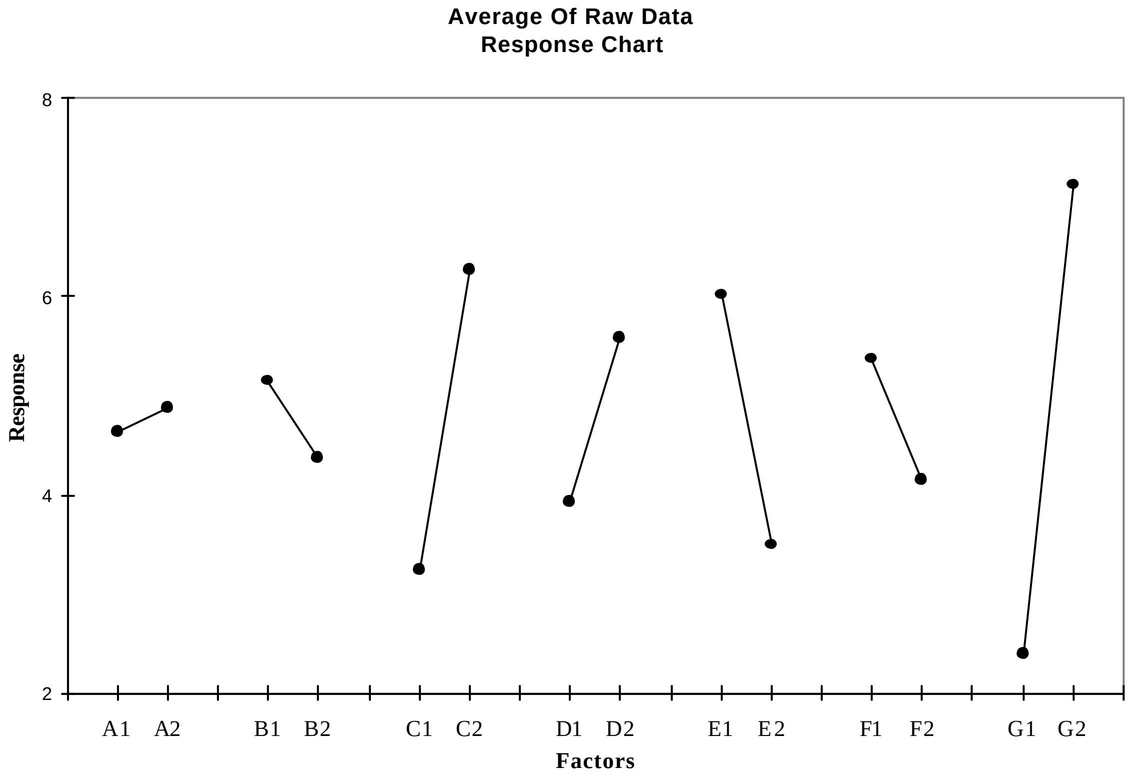

Figure 6:Response plot of average data for the factors for precision mechanical assembly experiment

It is represented mathematically as:

$$S/N = -10 \ \mathrm{Log}_{10} \ (MSD)$$

where MSD = the mean squared deviation from the target value of the quality characteristic.

The value of S/N is intended to be large therefore MSD should be small. It follows therefore that MSD is defined differently for each of the three quality characteristics, A to C.

A: Smaller is Better

$$MSD = (R_1^2 + R_2^2 + R_3^2 + R_4^2 + ...)/n$$

$$MSD = 1/n \sum R_i^2$$

B: Nominal is better

$$MSD = ((R_1 - m)^2 + (R_2 - m)^2 + ...)/n$$

$$MSD = 1/n \sum (R_i - m)^2$$

and

C: Bigger is better

$$MSD = (1/R_1^2 + 1/R_2^2 + 1/R_3^2 + ...)/n$$

$$MSD = 1/n \Sigma 1/R_i^2$$

Where

R_1, R_2, R_3, etc. = Results of experiments
m = Target value of experimental results
and n = number of repetitions, R_i

For this process the criterion, C, bigger is better, i.e. the higher the bond strength the better. Using the raw data for the for replicate runs presented in Table 16, the S/N ratio for the experimental trials can be calculated, Table 18.

Trial.	Variable Factors											Results, (kN)*				
	1	F	A	B	C	D	E	G	9	10	11	S/N ratio	Run 1	Run 2	Run 3	Run 4
1	X	1	1	1	1	1	1	1	X	X	X	-1.39	3.98	5.23	0.58	0.64
2	X	1	1	1	1	2	2	2	X	X	X	13.41	7.12	2.82	8.99	6.38
3	X	1	2	2	2	1	1	1	X	X	X	13.33	3.92	3.92	6.16	5.86
4	X	2	1	2	2	1	2	2	X	X	X	11.76	8..45	3.47	8.66	2.53
5	X	2	2	1	2	2	1	2	X	X	X	19.95	10.79	10.07	10.08	9.06
6	X	2	2	2	1	2	2.	1	X	X	X	-56.99	2.73	0.00	0.96	0.00
7	X	1	2	2	1	1	2	2	X	X	X	-56.99	6.97	0.00	3.34	0.00
8	X	1	2	1	2	2	2	1	X	X	X	11.33	3.88	3.56	6.69	2.81
9	X	1	1	2	2	2	1	2	X	X	X	21.14	11.39	10.25	12.15	12.19
10	X	2	2	1	1	1	1	2	X	X	X	-1.55	8.87	0.42	9.74	7.19
11	X	2	1	2	1	2	1	1	X	X	X	-53.98	0.66	0.00	1.06	0.23
12	X	2	1	1	2	1	2	1	X	X	X	-53.98	1.90	0.00	1.90	0.76
(Randomising sequence:as Table 16)																

Table 18:S/N ratio for Precision mechanical assembly experiment

The S/N performance of the factors is calculated in a similar manner as the data shown in Table 13. The S/N performance of the factors is shown in Table 18.

Level	A	B	C	D	E	F	G
1	-10.51	-2.04	-26.25	-14.80	-0.42	0.14	-23.61
2	-11.82	-20.04	3.92	-7.55	-21.91	-22.46	1.29
Difference	1.31	18.00	30.17	7.30	21.49	22.60	24.90

Table 18: The S/N performance of the factors for the precision mechanical assembly experiment

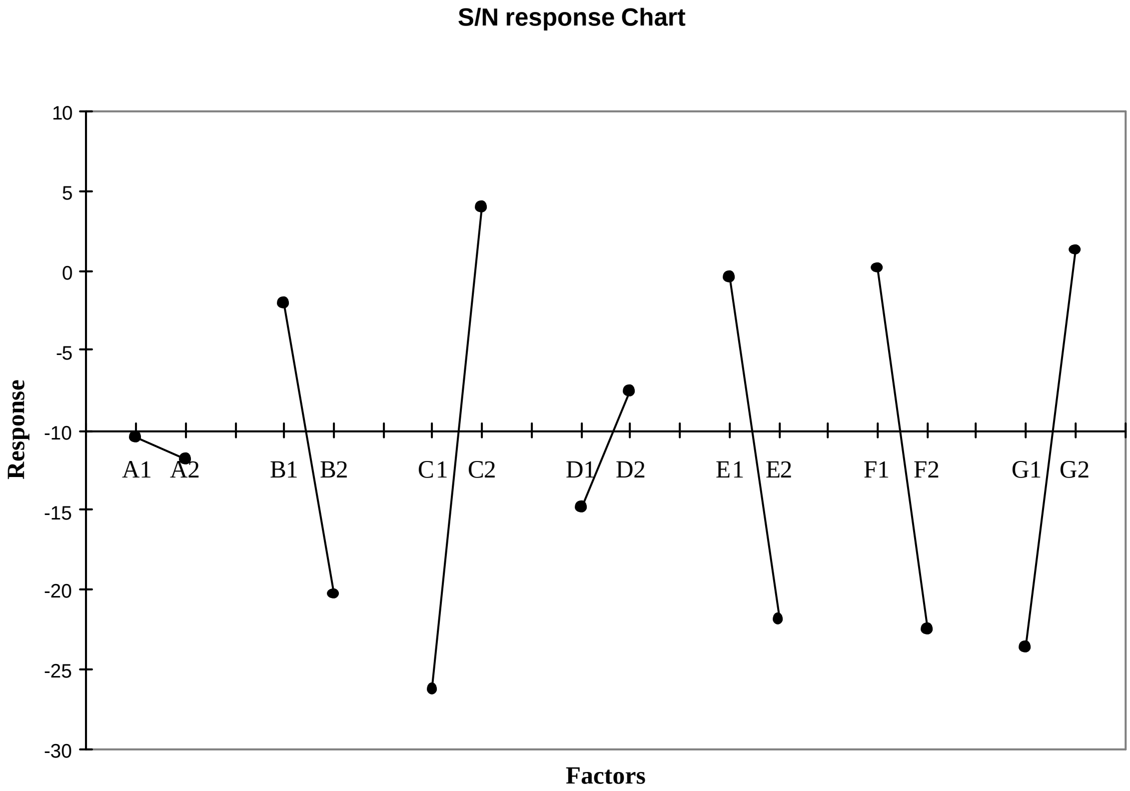

Figure 7: S/N response plot of the factors in the precision mechanical assembly experiment

18.3. CALCULATION OF THE RELATIVE CONTRIBUTION OF FACTORS, THE SIGNIFICANCE OF FACTORS

ANalysis **O**f **VA**riance (ANOVA) can be applied to determine the relative contributions of the factors to the optimum condition.

The analysis of variance calculates quantities known as ; degrees of freedom, sums of squares, mean squares , variance ratio etc. and presents them in a standard tabular format. These quantities and their interrelationships are defined as follows;

Degrees of Freedom, f.

The number of degrees of freedom, f, for a factor or a column is equivalent to one less than the number of levels. Therefore for a 2 level factor in a 2 level column as in the precision assembly the degree of freedom, f, for any factor is **one**. If we consider a factor, A, with 4 levels then f = 3. The degree of freedom is a measure of the amount of information that can be uniquely determined from a given set of data, thus for our four level factor A, A_1 data can be compared with A_2, A_3 and A_4 data but not with itself.

The degrees of freedom concept can be extended to an experiment where the total degrees of freedom, f_T, is given by

$$f_T, = n \times r - 1$$

where:

n is the number of experimental runs

and r is the number of **repetitions** of each experimental run

Therefore for our precision assembly example:

Each factor, A to F has $f = 1$

and

$f_T = 12$, the number of experimental runs x 4, the number of repetitions - 1 = 47

Additionally four columns in the original L_{12} array were not used, Table 15, there are also single degrees of freedom associated with these columns.

Sum of squares, SS

The sum of squares , total variance, of each factor is calculated from the square of each experimental trial result which includes the factor and subtracting the correction factor, C.F.

The correction factor, C.F. is given by the relationship: T^2/N

where

T= Sum total of all experimental results

and N is the **total** number of experiments, i.e. n x the number of repetitions,

For the precision assembly experimental design , T = 228.36 kN and: N= 48

therefore

$$C.F. = (228.36)^2/48 = 1086.42$$

and $S_A = A_1^2/N_{A1} + A_2^2/N_{A2} - C.F.$

where:

A_1^2 is (factor A , level 1)^2 etc.

and

N_{A1} is the number of experimental runs containing factor A at level 1, etc.

$$S_A = (111.342)^2/24 + (117.024)^2/24 = 516.543+570.609 - C.F.$$

$$= 1087.0152 - 1086.48 = 0.673$$

Table 19 shows the sum of squares for each factor.

Factor	A	B	C	D	E	F	G	*Other
Total Sum of Squares	0.673	7.173	109.614	32.308	76.296	17.883	268.371	28.969

*Other refers to the unused columns in the design, columns 1, 9, 10 and 11
Table 19:The sum of squares for each factor.

The total variation, S_T of the experiment is calculated from the sum of the squares of **all** experimental results minus the correction factor, C.F.

$$S_T = \sum R_1^2 + R_2^2 + R_3^2 + R_4^2 + \ldots\ldots + R_n^2 - \text{C.F.}$$

$$S_T = 1807.76 - 1086.42 = 721.36$$

The total error variation is given by the sum of the sum of squares for all factors:

$$S_e = S_A + S_B + S_C + S_D + S_E + S_F + S_{other}$$
$$S_e = 0.673+7.173 +109.614 + 32.308 + 76.296 + 17.883 + 268.371 +28.969$$
$$S_e = 180.053$$

<u>Variance, mean squares, V:</u>

The variance of each factor is calculated from the sum of the square of each experimental trial result which includes the factor, S_n, divided by the degrees of freedom of the factor, f_n.
Thus:

V_A, the variance for factor A = S_A/f_A, etc.
and
V_e, the variance for error terms = S_e/f_e

The variance of the factors is shown in Table 20.

Factor	A	B	C	D	E	F	G	Other
Variance	0.673	7.173	109.614	32.308	76.296	17.883	268.371	7.242

*Other, the unused columns in the design, columns 1, 9, 10 and 11 have a total of 4 degrees of freedom
Table 20:The variance for the factors in the precision assembly case study

The degrees of freedom of the experiment error = 36, i.e. the total degrees of freedom of the experiment, 47 minus the degrees of freedom of the factors, 11. Therefore the variance of the error is:

$$S_e/f_e = 180.05/36 = 5.001$$

Variance ratio, F:

The variance ratio is the variance of the factor, V_A, divided by the error variance, V_e. e.g.

$F_A = V_A/V_e$ for factor A

Fe = Ve/Ve =1 for error terms

The F ratios for the precision assembly case study are shown in Table 21.

Factor	A	B	C	D	E	F	G	Other
F Ratio	0.134	1.434	21.916	6.460	15.255	3.576	53.658	1.448

Table 21:The F ratios for the factors in the precision assembly case study

If the F ratio is > 6, this indicates that there is a possible shift in the response at the different run settings. An F Distribution Table provides a means for the determination of the significance of a factor to a specified level of confidence by comparing calculated F ratios to those from the F Distribution Table. If the F ratio is greater than the table value , then there is a significant effect.

P, the percent contribution:

The percent contribution of each factor is the ratio of the factor sum, S to the sum total, $S_{T,}$ Table 22.

$P_A = S_A \times 100/S_T$, etc., $P_e = S_e \times 100/S_T$

Factor	A	B	C	D	E	F	G	Other
P, % contribution	0.093	0.994	15.20	4.479	10.577	2.479	37.205	4.016

Table 22:P, the % contribution for the factors in the precision assembly case study

$P_e = S_e \times 100/S_T$ = 180.053/721.34 x 100 = 24.961%

Table 23 summarises the ANOVA table(Raw data), for the case study.

Factor	Degrees of freedom f	Sum of Squares SS	Variance Mean Squares	F Ratio	Significance (from F tables)	P % contribution
A	1	0.673	0.673	0.134	0.284	0.093
B	1	7.173	7.173	1.434	0.761	0.994
C	1	109.614	109.614	21.916	0.999*	15.196
D	1	32.308	32.308	6.460	0.985*	4.479
E	1	76.296	76.296	15.255	0.999*	10.576
F	1	17.883	17.883	3.576	0.933*	2.479
G	1	268.371	268.371	53.658	0.999*	37.205
Other	4	28.969	7.242	1.448	0.762	4.016
Error	36	180.053	5.001	1.000		24.961
Total	47	721.34				

*significant factors

Table 23:ANOVA table for the case study

Further output from the ANOVA can be used to calculate the Coefficient of Determination which is a value for the coverage of the experiment by gauging the degree to which the factors tested in the experiment explain the spread of observed results. A low value for the Coefficient of Determination (less than 50%) is an indication that a significant factor (or factors) is missing from the experiment. In this case it is recommended that there is a return to Section 3.4 and repeat with a revised set of control factors.

If the Coefficient of Determination is acceptable then the "best settings" process can be readily defined by listing the control factor levels that have a positive significant effect on the output response. Control factors that are insignificant in terms of output response can be set at whichever level is most efficient and cost effective, it should be noted that this knowledge forms a crucial output from the overall methodology.

18.4. PREDICTION OF THE PERFORMANCE FROM THE 'OPTIMUM SETTINGS' PROCESS

From the significant effects identified from the ANOVA it is possible to generate a response equation. Feeding the results from the "best" settings, or levels, from these significant control factors into the response equation will provide a prediction of the output performance of the process together with a confidence interval, or tolerance. This prediction can be used as a comparison of the expected "best" process output against the original objectives, (Section 5), and as a comparison of the results and analysis from the experimental array against those from the confirmation trial.

From the ANOVA table and from the response plots the probable optimum bonding process can be defined by setting the control factors at the level which produces the highest level average.

For this process the optimum condition is shown in Table 24.

Label	Factor	Level	Setting	Average Bond Strength, kN
C	Time to cure	2	120 min	6.27
D	Time in solvent wash (cleans excess adhesive)	2	20 min	5.58
E	Time in oven (final cure)	1	30 min	6.02
F	Oven temperature	1	20°C	5.37
G	Time post oven	2	12 hours	7.12

Table 24. Optimum condition for precision mechanical assembly experiment

The Grand Average Performance, (GAP), of the experiment was 4.758kN, (Table 16).

As the factors C, D, E, F and G were considered significant the optimum bond strength can be predicted using the following prediction equation:

Predicted Bond Strength =

$$= GAP + (C_2 - GAP) + (D_2 - GAP) + (E_1 - GAP) + (F_1 - GAP) + (G_2 - GAP)$$

$$= 4.758 + (6.27 - 4.758) + (5.58 - 4.758) + (6.02 - 4.758) + (5.37 - 4.758) + (7.12 - 4.758)$$

Predicted Bond Strength = 11.33kN

18.5. CONFIDENCE INTERVAL OF THE PREDICTED PERFORMANCE FROM THE 'OPTIMUM SETTINGS' PROCESS

The formula to calculate the confidence interval for the predicted performance at the optimum condition is:

$$\text{C.I.} = \pm \sqrt{(F(1, f_e) \times V_e / N_e}$$

where

F(1, f_e) = the F value from the F Distribution Table at the required confidence level at degree of freedom 1 and f_e, the degrees of freedom of the error term.

V_e = the Variance of the error term (from ANOVA table, Table 23)

and

N_e = Effective number of replications

$$= \frac{\text{Total number of results (or number of S/N ratios)}}{\text{Degree of freedom of Grand Average, GAP, (1), + degree of freedom of all factors included in the prediction of the optimum condition}}$$

Five factors, C, D, E, F and G were used to estimated the performance at the optimum condition.

Therefore: $N_e = \dfrac{48}{(1+5)} = 6$

For our case study, $F(1, f_e) = F(1, 36) = 2.8503$ from the F Distribution table at 90% confidence level and Ve = 5.001,(see ANOVA table)

and C.I. = $\pm\sqrt{(F(1,\ {}_e) \times V_e / N_e} = \pm\sqrt{2.8503 \times (48 / 6)} = \pm\ 1.541$kN

Predicted Bond Strength = 11.33kN ± 1.541kN

19. RUNNING THE CONFIRMATION TRIAL

It is always essential to confirm the results and analysis from the experimental array with additional confirmation trials using the "best settings", or optimum, process. The confirmation trial also assures the reproducibility of process performance in a different time frame compared with the original experiment. If the performance of the "best settings" process in the confirmation trial is significantly lower than the predicted range then it is probable that extra causal factors exist. For this occurrence, it is recommended to redefine and/or select additional casual factors as detailed in Section 8 and then repeat the methodology with the revised set of control factors.

For the confirmation trial for the precision mechanical assembly case study, twelve core/rail bonds were prepared using the optimum process as shown in Table 24.

Since factors A and B show no significance there is freedom to choose whichever level suits the production process.

Therefore:

factor A, Parts temperature, was set at ambient temperature, Level 1, 20^{o}C and
factor B, Time in fixture before cure, was set for the shortest time, Level 1, 5 minutes.

This process resulted in a mean bond strength of 11.41kN with the lowest value being 8.90kN which is very close to the prediction.

20. RUNNING FURTHER EXPERIMENTS IN THE 'OPTIMUM SETTINGS' ZONE, CONFIRMATORY EXPERIMENTS

If there are any concerns about the linearity of response between the levels tested for one or more, control factor(s), or just a desire for more detail on the response characteristics of a few "key" control factors, then it is recommended that a further small experiment is performed. This is preferable to trying to obtain all the information from the original experiment and making it too large and difficult to manage and control.

21. CONCLUSIONS

The structured approach in the design of experiments technique is the backbone of the success of the method. This approach ensures that no items are overlooked in the analysis of the process. In the first instance, a clear definition of the problem, objectives and measurement system are important to ensure the correct focus to the work. The brainstorming session is the most crucial step because no amount of statistical theory can compensate for a critical factor that has been missed and the views of ALL the people involved in the process must be considered in the selection of the control factors for the experiment. An open airing of all the views of the personnel involved with the process can lead to a more efficient analysis of the current operating conditions and aid its modification. The list of factors which are developed at this session will be useful for fine-tuning the process after the initial confirmation trials. Any important controlling factor missed in the initial series of experiments can usually be picked up by re-examining the brainstorming session. Once the list of factors has been selected the experimental design can be selected

Once the design has been selected and the experimental trials completed, the objective of the subsequent analysis is to seek answers to the following:

1. What is the optimum condition?
2. Which factors contribute to the results and by how much?
3. What will be the expected result at the optimum condition?

The use of graphical and statistical, (ANOVA), methods to determine the consistency, repeatability and robustness of the process all play a role in providing the solution to these questions.

The Coefficient of Determination, (which can be calculated from ANOVA), is used to gauge the degree to which the factors tested in the experiment explain the spread of observed results. A value for the Coefficient of Determination of less than 50%, is an indication that a significant factor/s has been missed from the experiment. If this is the case, then the methodology should be repeated and additional factor/s added displacing those factor/s which have a minor role in the process.

The % contribution term, P, in the ANOVA table also acts as an indicator as to the validity of the selected factors in the process. If P, of the error terms is greater than 50%, it is again an indication that a significant factor/s has been missed from the experiment.

Finally, the confirmation trial is essential, to ratify the selection of variables to be investigated and the experimental analysis, before the process is installed in production. A committed team approach is needed to ensure that the discipline of the methodology is maintained to a successful completion, short cuts will normally lead to failure.

22. REFERENCES

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7. Schmidt S., R., Launsby R., G., Understanding Industrial Design Experiments, 4th Edition. Colorado Springs, CO: Air Academy Press 1994.
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23. LIST OF FIGURES

APPENDIX 1 SOFTWARE FOR DESIGN OF EXPERIMENTS

Package	Supplier	Operating system	Designs
DOE Wisdom	Launsby Consulting USA	Windows 3.1 and Windows 95	D-Optimal Full and Fractional Designs Plackett-Burman Designs Taguchi's common orthogonal arrays Box-Behnken Designs Central Composite Designs
Q-EDGE	Air Academy Associates USA	Windows 3.1	Full and Fractional Designs Taguchi's common orthogonal arrays Box-Behnken Designs Central Composite Designs Custom designs
DOE KISS for Excel	Air Academy Associates USA	Windows 3.1 and Microsoft Excel	Full and Fractional Designs Plackett-Burman Designs Taguchi's common orthogonal arrays Box-Behnken Designs Central Composite Designs Nested Designs Custom designs
DOE-PC IV	Quality America Inc. USA	Windows 3.1	Full and Fractional Designs Plackett-Burman Designs John's 3/4 Designs***** Box-Behnken Designs Central Composite Designs D-Optimal
MODDE for Windows	Umetri AB Sweden	Windows 3.1	Full and Fractional Designs Plackett-Burman Designs Box-Behnken Designs Central Composite Designs D-Optimal
CARD Design of Experiments Software for Windows	S-Matrix Corp. USA	Windows 3.1 and Microsoft Excel V5	Full and Fractional Designs Box-Behnken Designs Central Composite Designs Nested and Mixture Designs S-Matrix custom Designs
Qualitek -4	Nutek Inc USA	Windows 3.1	Taguchi Designs
SAS/QC	SAS Institute USA	Windows 3.1 Windows NT UNIX	Full and Fractional Designs Taguchi Designs Plackett-Burman Designs Box-Behnken Designs Central Composite Designs D-Optimal
PEXLAB **P**lanning **EX**periments **LAB**oratory	Leeds University UK	UNIX	Design of symmetrical and asymmetrical balanced factorial experiments

*John's 3/4 Designs are 3/4 fractions of the regular fractional factorials with rows and columns selected to maximise the number of available interactions.

APPENDIX 2. FURTHER READING

1. Roy R., A Primer on the Taguchi Method, Society of Manufacturing Engineers 1990
2. Peace G., S., Taguchi Methods, A Hands on Approach, Addison-Wesley
3. Launsby R., G., Weese D., L., Straight Talk On Designing Experiments, 2nd Edition. Colorado Springs, CO: Launsby Consulting 1995

GLOSSARY

ANOVA, **AN**alysis **O**f **VA**riance can be applied to determine the relative contributions of the factors to the optimum condition.

The analysis of variance calculates quantities known as ; degrees of freedom, sums of squares, mean squares , variance ratio etc. and presents them in a standard tabular format

Degrees of Freedom, f.

The number of degrees of freedom, f, for a factor or a column is equivalent to one less than the number of levels.

Variance, mean squares, V:

The variance of each factor is calculated from the sum of the square of each experiment result including the factor, S, divided by the degrees of freedom of the factor, f_n.

Variance ratio, F:

The variance ratio is the variance of the factor, V_A, divided by the error variance, V_e. If F is > 6, this indicates that there is a likely shift in the response at the different run settings. An F table provides a means for the determination of the significance of a factor to a specified level of confidence by comparing calculated F ratios to those from the F distribution. If the F ration is greater than the table value , then there is a significant effect.

P, the percent contribution:

The percent contribution of each factor is the ratio of the factor sum, S to the sum total, S_T

Pure sum of squares, S¢:

The pure sum of squares is the sum minus the product of the degrees of freedom and the error variance.

S_T the total variation, = the sum of the squares of all experimental results minus the correction factor, C.F.

Correction factor, C.F. = T^2/N

where

T= Total of all results and N is the total number of experiment al trials

Alaising: In a design matrix columns which are identical are said to be aliased. This **aliasing** prevents individual assessment of the factors. See also **confounding**.

Box-Behnken: A 3 level design used for modelling quantitative factors. Estimates **all** linear , quadratic and 2-way linear interactions

Box-Wilson or Central composite design: A 3 level design used for modelling and robust design. Estimates all linear , quadratic effects. Estimates all, some or no interactions.

A typical design is formulated from three distinct blocks:

1. F. a factorial block which can be of resolution R_{III}, R_{IV} or R_{IV} depending on the application and level of interactions required.

2. C. a centre point block which gives an estimate of pure experimental error and maintains the orthogonality of the design. The number of centre points, n_c, can be calculated from the formula :

$$n_c = \sqrt[4]{n_f + 1} - 2f \qquad \text{Equation 4}$$

where:

n_c = number of centre points
n_f = number of experimental trials in the factorial (2-level) block
and f = number of factors

and

3. A. an axial block of 2*f, experimental trials. The value of α is commonly chosen to be $(n_f)^4$ where n_f is the number of experimental trials in the factorial (2-level) block. These values are selected to allow the predicted response to be estimated with equal variance from any direction from the centre of the design. This property is called **rotatability**. Whilst rotatability is a desired design property it is not a compulsory feature for a successful design.

Central composite face design: A central composite design, (Box-Wilson), with no rotatability, i.e. $\alpha = 1$.

Confidence interval, C.I.:

The expression to calculate C.I**.** for the predicted performance at the optimum condition is:

$$\text{C.I.} = \pm \sqrt{(F(1, {}_e) \times V_e / N_e}$$

where

F(1, f_e) = the F value from the F Distribution Table at the required confidence level at degree of freedom 1 and f_e, the degrees of freedom of the error term.

V_e = the Variance of the error term (from ANOVA table)

and

N_e = Effective number of replications

$$= \frac{\text{Total number of results (or number of S/N ratios)}}{\text{Degree of freedom of Grand Average, GAP, (1), + degree of freedom of all factors included in the prediction of the optimum condition}}$$

To calculate C.I**.** for a factor, N_e, becomes:

$$N_e = \frac{\text{Total number of results (or number of S/N ratios)}}{\text{Degree of freedom of Grand Average, GAP, (1), + degree of freedom of all factors included in estimate of the mean}}$$

for example for a single factor the denominator is (1+1), for a two way interaction, i.e. AxB, the denominator is (1+2).

Confounding:, partial aliasing of factors and interaction with each other.

D-optimal designs: Due to the ready availability of low cost computing, a new class of experimental design has been developed, (ca. 1985), using near orthogonal matrices. These designs are based on the optimisation of the determinant, D, of a matrix and its transpose, i.e. $D = |M' M|$. Optimal values of D vary with the number of runs, n.

Foldover design are types of 2 level fractional factorial in which the basic design is supplemented by the complement of the basic design.

Full factorial: a design matrix of all possible combination of runs. The number of experimental runs for a full factorial experiment, all possible permutations of factors examined, can be determined from the relationship;

$$n = L^f$$

n = number of experimental runs

L = number of levels
and
f = the number of factors

Fractional factorial: a design matrix which is a fraction of the full factorial matrix. The formula which calculates the number of experimental runs in a fractional factorial array for a designated number of factors is:

$$n = L^{f-q}$$

Where n = the number of experimental runs
L = the number of levels
f is the number of factors
and
q is a positive integer which designates the fraction of the full factorial matrix,

i.e. q = 1 equals a 1/2 design fraction, q =2 equals a 1/4 design fraction, q = 3 equals an 1/8 design fraction ...q= 6 equals a 1/64 design fraction etc.

Levels, or settings, for the control factors in the experiment are fixed to test the "robustness" of the process to each parameter.
Mixture designs: These are used when you have a process where the sum of the actual factor settings must equal the same fixed total for every run

Modelling: A process/experiment can be modelled by selecting three factors at three levels and using **Central composite** or **Box-Behnken** design matrix.

Orthogonal: When a matrix is balanced vertically **and** horizontally the matrix is said to be orthogonal. In mathematical terms, vertical balancing occurs if the sum of each column is zero. Horizontal balancing occurs when the sum of the products of the corresponding rows in two columns is zero. A design is horizontally balanced if each two column combination sums to zero. These orthogonal matrices allow factors to be evaluated independently.

Nested designs: A special design required when the settings of one factor are dependent upon the settings of another.

Plackett-Burman designs are R_{III} designs used for screening a large number of factors. These designs are based on Hadamard matrices which are generated in multiples of 4, not 2 as in fractional factorial designs, i.e. the number of runs is 4, 8, 12, 16, 20, 24.....etc.

Random balanced designs: These designs are produced by randomly generating the first n/2 runs for f factors. The design is then folded to generate a the final balanced design. For this type of design the number of runs, n, is determined independently of the number of factors.

Randomisation: This is the mixing or shuffling of the order in which events occur so that each event has an equal chance of being selected or happening next. Within the context of the experiment, randomisation can refer to the sequence in which the experimental runs are carried out, sample are collected or measurements are taken. Randomisation serves two purposes:

1. prevent systematic bias that could be caused by creating or collecting data

and

2. to reduce the effect of the non-relevant factors and other effects not measured under the scope of the experimental plan.

Repetition: A type of randomisation in which groups of data are mixed so that each group of data has an equal chance of being selected. When a group is selected, each unit or data point within that group is automatically selected as part of that group.

Resolution, **R.** The term used to describe the effect of the degree of aliasing in a design. The definitions of the various. orders of resolution are as follows:

R_{II} - Resolution 2	A design in which the main effects are aliased with other main effects. This is obviously a design to be avoided.
R_{III}- Resolution 3	A design in which the main effects are not aliased with each other, but main effects are aliased with two-way interactions . Used in screening a large number of factors to find the most important.
R_{IV}- Resolution 4	A design where the main effects are not aliased with each other or with two-way interactions. However the two way interactions are aliased with each other. Designs of this type are typically used in the experimental modelling. Some knowledge of the two-way interactions is required to determine which aliased two-way interactions are used in the prediction equation.
R_V- Resolution 5	A design in which the main factors and the two-way interactions are not aliased **and** the two way interactions are also not aliased with each other. Used in designing prediction equations that will not have concerns due to interactions.
R_N	Required if higher interactions are of interest. High resolution designs eliminate aliasing of higher order interactions.

Response graph: A method of depicting factor effects.

Robust Design

A robust design can be demonstrated by finding settings for the control factors so that the measured response is insensitive to the noise, variation each input factor.

Rotatability

The predicted response is able to be estimated with equal variance from any direction from the centre of the design, (see Box-Wilson, central composite design , Section 13.4). Whilst rotatability is a desired design property it is not a compulsory feature for a successful design.

Screening:

The important few factors of the experiment/process are separated from those which have little or no effect.

Signal to noise ratio, (S/N) ratio.

This S/N function is evaluated at one of three levels

A the smaller the better
B the nominal the better
C the bigger the better

It is represented as $S/N = -10 \ Log_{10} \ (MSD)$

where MSD = the mean squared deviation from the target value of the quality characteristic. The MSD is defined differently for each of the three quality characteristics, A-C.

A: Smaller is Better

$$MSD = (R_1^2 + R_2^2 + R_3^2 + R_4^2 + ...)/n$$

B: Nominal is better

$$MSD = ((R_1 - m)^2 + (R_2 - m)^2 + ...)/n$$

and

C: Bigger is better

$$MSD = (1/R_1^2 + 1/R_2^2 + 1/R_3^2 + ...)/n$$

Where

R_1, R_2, R_3, etc. = Results of experiments
m = Target value of experimental results
and n = number of repetitions, R_i

Taguchi design of experiments

Characterised by three elements:

1. The use of special tabulated orthogonal arrays based on fractional factorials, Plackett-Burman or Latin Squares designs. The use of the ‘Taguchi” matrices forces a consistent approach to the design of experiments methodology.

2. Design modification to allow an outer array which contains noise factors, i.e. those factors which are uneconomical/difficult to control , for example the weather, machinery wear etc. The inner array holds the control factors. This use of an outer array take the place of randomisation when performing experimental runs, which has the effect of spreading the noise factors over the experiment. Taguchi assumes that noise factors are measurable and can be included in the design.

3. Analysis of a function called the **signal to noise ratio**,(S/N ratio), which determines the optimum conditions.

Three level fractional factorial designs:include a mixture of corner points and mid-level points, i.e. factors are coded as -1, 0 and +1. Due to this combination of experimental data these designs can be used to test linear effects, quadratic, (second order effects) and linear -second order interactions.

Troubleshooting

A troubleshooting experiment can be represented by for example four factors at two levels which determines the most important factors affecting the measured response