Design of Experiments: New Methods and How to Use Them

Douglas C. Montgomery
Arizona State University
Bradley Jones
SAS Institute

DOE Course – Module 1

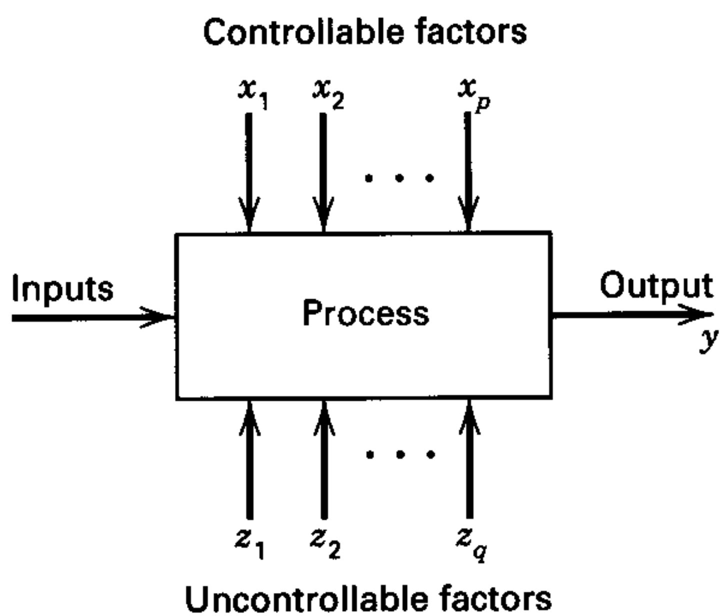
Introduction, Definitions and an Example

Goals

1. Introduce fundamental concepts
2. Design & analyze an experiment
3. Introduce linear statistical models
4. Explain factor coding conventions
5. Show the relationship of a model to a design
6. Introduce criteria for evaluating the goodness of a design

What Is a Designed Experiment?

a structured set of tests of a system or process



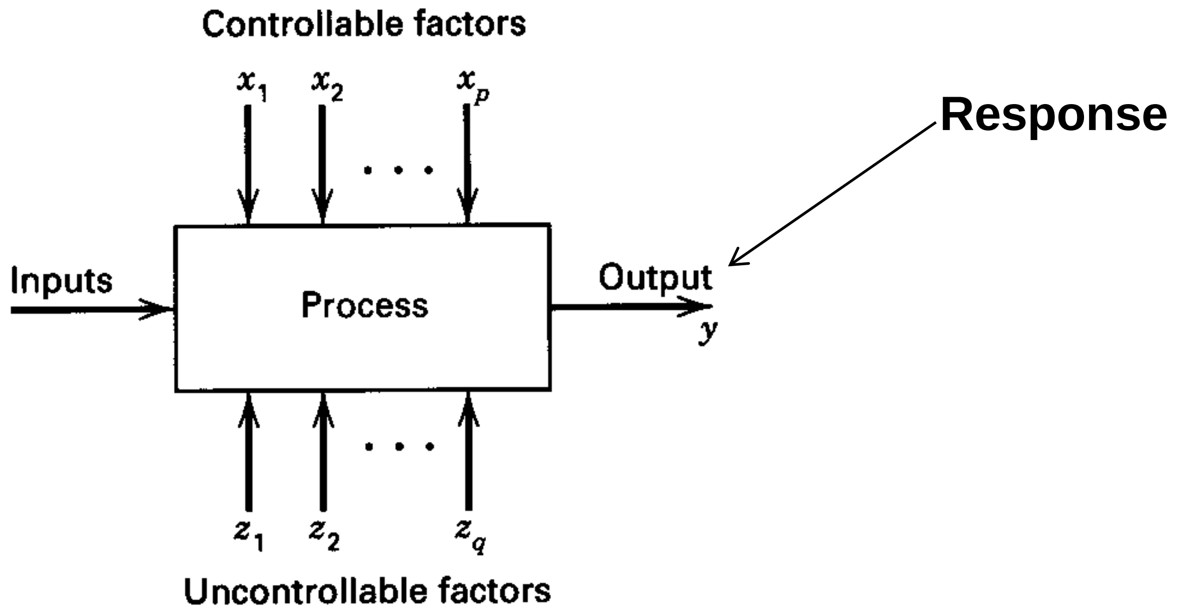
Integral to a designed experiment are…

1. Response(s)
2. Factor(s)
3. Model

What Is a Response?

A *response* is a **measurable** result.

- yield of a chemical reaction (chemical process)
- deposition rate (semiconductor)
- gas mileage (automotive)



What Is a Factor?

A *factor* is any **variable** that you think may affect a response of interest. We begin by considering two types of factors – continuous and categorical

continuous factors take any value on an interval

e.g. octane rating [89 93]

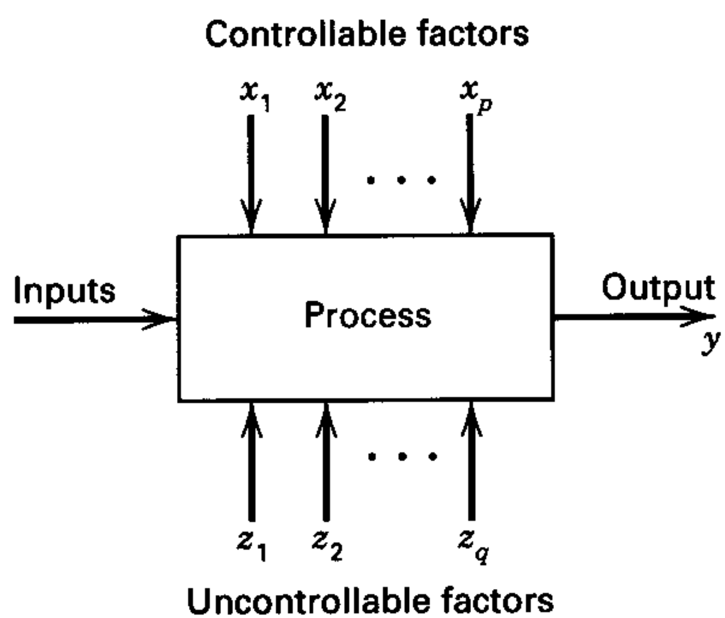
categorical factors have a discrete number of levels

e.g. brand [BP, Shell, Exxon]

What is a model?

a simplified mathematical surrogate for the process

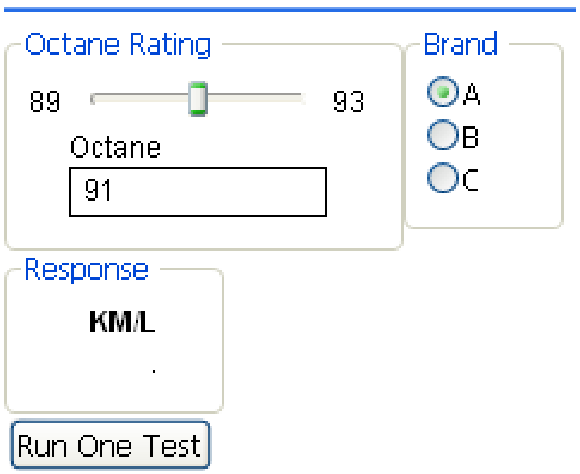
Factor(s) $\longrightarrow$ Model $\longrightarrow$ Response(s)



Example Experiment #1

You want to know 2 things:

1. Does higher octane rating improve gas mileage?
2. Which brand (BP, Shell or Exxon) is best for gas mileage?



Important Points from the Fathers of DOE

DOE – Problem solving methodology for efficiently identifying cause-and-effect relationships.

Fisher's Four Fundamentals of DOE

1. Factorial principle
2. Randomization
3. Blocking
4. Replication

R.A. Fisher

"To discover what happens to a process when a factor is changed, you must actually change it!"

George Box

Examples of Models

Comparing three brands of gasoline using an ANOVA model:

$$Y_{ij} = \mu + \alpha_i + \epsilon_{ij}$$

Finding the effect of octane rating using a regression model:

$$Y_i = \beta_0 + \beta_1 X_1 + \epsilon_i$$

Y (the response) is the mileage of a car in miles per gallon.

ANOVA Model for Mileage Study

$$Mileage_i = \begin{cases} \mu + \alpha_1 + \epsilon_i & \text{if } Brand = BP; \\ \mu + \alpha_2 + \epsilon_i & \text{if } Brand = Shell; \\ \mu + \alpha_3 + \epsilon_i & \text{if } Brand = Exxon. \end{cases}$$

Note that we have 4 unknown parameters and only 3 brands of gasoline.
Our model is overspecified –

if we know any three parameters, we can compute the 4th.
We say there are 2 degrees of freedom (df) for alpha.

Categorical Factor Coding – 3 levels

Names | Numeric Label | Orthogonal Coding **or** Effects Coding

$$\begin{bmatrix} BP \\ Shell \\ Exxon \end{bmatrix} \Rightarrow \begin{bmatrix} 1 \\ 2 \\ 3 \end{bmatrix} \Rightarrow \begin{array}{c} \begin{array}{cc} X_1 & \qquad X_2 \end{array} \\ \begin{bmatrix} \sqrt{\frac{3}{2}} & -\sqrt{\frac{1}{2}} \\ 0 & \sqrt{2} \\ -\sqrt{\frac{3}{2}} & -\sqrt{\frac{1}{2}} \end{bmatrix} \end{array} \begin{array}{c} \begin{array}{cc} X_1 & \quad X_2 \end{array} \\ \begin{bmatrix} 1 & 0 \\ 0 & 1 \\ -1 & -1 \end{bmatrix} \end{array}$$

Orthogonal Coding:

There are two “dummy” columns – 2 degrees of freedom

The sum of squares of both columns is 3.

The sum of the element wise products is zero

(i.e. the dot product is zero)

Orthogonal Coding and Orthogonal Design

1. Dummy columns for categorical factors are orthogonally coded if their dot product is zero.
   a) The column means are zero.
   b) The pairwise column correlations are zero.
2. For the purpose of this course we say that a design is orthogonal if:
   a) The means of the columns of the design matrix are all zero.
   b) The pairwise correlation for all column pairs of the design matrix are zero.
   c) So, whether a design is orthogonal can depend on the model you fit.

ANOVA and regression models are equivalent…

$$Mileage_i = \begin{cases} \mu + \alpha_1 + \epsilon_i & \text{if } Brand = BP; \\ \mu + \alpha_2 + \epsilon_i & \text{if } Brand = Shell; \\ \mu + \alpha_3 + \epsilon_i & \text{if } Brand = Exxon. \end{cases}$$

Replace μ with β_0 and α_1 and α_2 with β_1 and β_2.

$$Mileage_i = \begin{cases} \beta_0 + \beta_1 1 + \beta_2 0 + \epsilon_i & \text{if } Brand = BP; \\ \beta_0 + \beta_1 0 + \beta_2 1 + \epsilon_i & \text{if } Brand = Shell; \\ \beta_0 + \beta_1(-1) + \beta_2(-1) + \epsilon_i & \text{if } Brand = Exxon. \end{cases}$$

$$Mileage_i = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \epsilon_i$$

ANOVA/Regression Model – Matrix Notation

$$\begin{bmatrix} y_1 \\ y_2 \\ y_3 \end{bmatrix} = \begin{bmatrix} 1 & 1 & 0 \\ 1 & 0 & 1 \\ 1 & -1 & -1 \end{bmatrix} \begin{bmatrix} \beta_0 \\ \beta_1 \\ \beta_2 \end{bmatrix} + \begin{bmatrix} \epsilon_1 \\ \epsilon_2 \\ \epsilon_3 \end{bmatrix}$$

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon}$$

$$\mathrm{Var}(\boldsymbol{\varepsilon}) = \sigma^2 \mathbf{I}$$

Categorical Factor Coding – 4 levels

$$\begin{bmatrix} 1 \\ 2 \\ 3 \\ 4 \end{bmatrix} \Rightarrow \begin{bmatrix} \sqrt{2} & -\sqrt{\frac{2}{3}} & -\sqrt{\frac{1}{3}} \\ 0 & \sqrt{\frac{8}{3}} & -\sqrt{\frac{1}{3}} \\ 0 & 0 & \sqrt{3} \\ -\sqrt{2} & -\sqrt{\frac{2}{3}} & -\sqrt{\frac{1}{3}} \end{bmatrix}$$

There are three columns – 3 degrees of freedom
The sum of squares of all columns is 4.
The sum of the element wise products are zero
(i.e. all dot products of column pairs are zero)

Categorical Factor Coding – 2 levels

Orthogonal
&
Effects
Coding

$$\begin{bmatrix} 1 \\ 2 \end{bmatrix} \Rightarrow \begin{bmatrix} -1 \\ 1 \end{bmatrix}$$

JMP Scripting Language (JSL) Function for Orthogonal Dummy Variable Coding

```
level2dummy = function({nl,val},
   dummy = j(1,nl-1,0);
   c1 = sqrt(nl*val/(val+1));
   for (i=1,i<nl,i++,
      c2 = sqrt(nl/(i*(i+1)));
      if (val==i,l1=1,l1=0);
      if ((i>val)|(val==nl),l2=1,l2=0);
      dummy[1,i]=c1*l1-c2*l2;
   );
   dummy;
);
```

nl is the number of levels
val is the numeric label for the level you want to code

Continuous Factor Coding

$$X_{scaled} = \frac{X - MR}{HR}$$

$$MR = \frac{Hi + Lo}{2}$$

$$HR = \frac{Hi - Lo}{2}$$

MR – midrange
HR – half range
Hi – high value
Lo – low value

Continuous Factor Coding Example

$$X_{scaled} = \frac{X - MR}{HR}$$

$$MR = \frac{Hi + Lo}{2}$$

$$HR = \frac{Hi - Lo}{2}$$

If Hi is 93
& Lo is 89,
then MR is 91
& HR is 2

Suppose X is 92, what is the scaled value of X?

$$\frac{92 - 91}{2} = \frac{1}{2}$$

If X is 93, then the scaled value is 1.
If X is 89, then the scaled value is -1.
If X is 91, then the scale value is 0.

The Model/Design Relationship – Parameter Estimates

$$\mathbf{y} = \mathbf{X}\boldsymbol{\beta} + \boldsymbol{\varepsilon} \qquad \mathrm{Var}(\boldsymbol{\varepsilon}) = \sigma^2\mathbf{I}$$

The matrix, **X**, is called the design matrix. The least-squares estimator of β is:

$$\hat{\boldsymbol{\beta}} = (\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{y}$$

The variance of the least-squares estimator of β is:

$$\mathrm{Var}(\hat{\boldsymbol{\beta}}) = \sigma^2(\mathbf{X}'\mathbf{X})^{-1}$$

σ is inherent to the system but we choose the design matrix, **X**.

The Model/Design Relationship – Predicted Responses

The predicted values of the response are contained in the vector:

$$\hat{\mathbf{y}} = \mathbf{X}\boldsymbol{\beta} = \mathbf{X}(\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'\mathbf{y} = \mathbf{H}\mathbf{y}$$

Where the so-called, "hat" matrix, H, is: $\mathbf{H} = \mathbf{X}(\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'$

The variance matrix of the predicted responses is:

$$Var(\hat{\mathbf{y}}) = \sigma^2\mathbf{X}(\mathbf{X}'\mathbf{X})^{-1}\mathbf{X}'$$

Again, σ is inherent to the system, but we choose the design, **X**.

The Model/Design Relationship – Aliasing

Suppose the best polynomial approximating model is:

$$\mathbf{y} = \mathbf{X}_1\boldsymbol{\beta}_1 + \mathbf{X}_2\boldsymbol{\beta}_2 + \boldsymbol{\varepsilon}$$

But we estimate only β_1 using least-squares:

$$\hat{\boldsymbol{\beta}}_1 = (\mathbf{X}_1'\mathbf{X}_1)^{-1}\mathbf{X}_1'\mathbf{y}$$

Now the elements of the least-squares estimates of β_1 are biased by β_2, that is:

$$\mathrm{E}(\hat{\boldsymbol{\beta}}_1) = \boldsymbol{\beta}_1 + \mathbf{A}\boldsymbol{\beta}_2$$

where the alias matrix, A, is:

$$(\mathbf{X}_1'\mathbf{X}_1)^{-1}\mathbf{X}_1'\mathbf{X}_2$$

What makes a design good?

1. Low variance of the coefficients.
2. Low variance of predicted responses.
3. Minimal aliasing of terms in the model from likely effects that are not in the model (0.5 or less).
4. Correlations between likely effects that are not in the model are small (0.5 or less).

The first two deal with variance – the last two with bias.
Reducing variance and bias are fundamental goals.

Design Optimality Criteria

D-optimality $$\max_{d} |\mathbf{X_d}'\mathbf{X_d}|$$

I-optimality $$\min_{d} \frac{\int_{\chi} \mathbf{f}'(\mathbf{x})(\mathbf{X_d}'\mathbf{X_d})^{-1}\mathbf{f}(\mathbf{x})d\mathbf{x}}{\int_{\chi} d\mathbf{x}}$$

Alias optimality $$\min_{d} \mathrm{Tr}[\mathbf{A}(d)'\mathbf{A}(d)], \text{ subject to } D_e(d) \geq l_D$$

Why isn't orthogonality a design criterion?

1. Not all orthogonal designs are good.
   a) It is inappropriate to change the requirements of a problem to use an orthogonal design as a "solution".
   b) As we will see, in many practical situations no orthogonal design exists.
2. Not all good designs are orthogonal.

   Sometimes it may be useful to sacrifice orthogonality for some other desirable design feature.
3. In standard two-level screening design orthogonal designs minimize the variance of the coefficient estimates, so focusing on variance results in orthogonal designs, if they are possible.

Example Experiment #2

Design

Run	X1	X2	X3
1	1	-1	1
2	1	1	-1
3	-1	-1	-1
4	-1	1	1

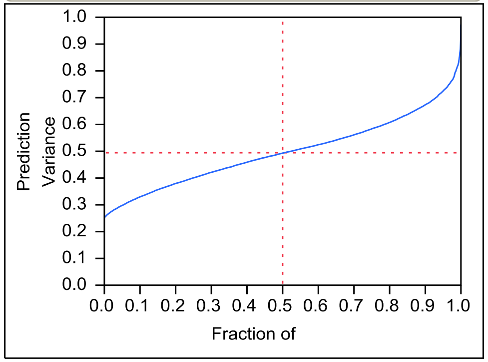
Relative Variance of Coefficients

Significance Level 0.05

Signal to Noise Ratio 1

Effect	Variance	Power
Intercept	0.25	0.126
X1	0.25	0.126
X2	0.25	0.126
X3	0.25	0.126

Fraction of Design Space Plot

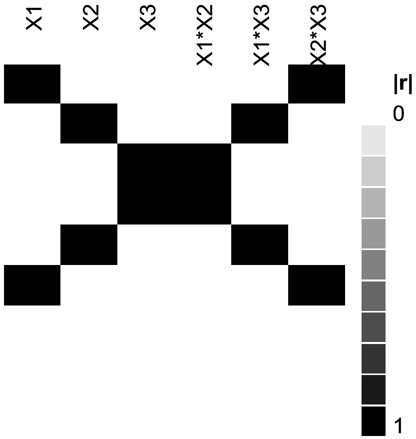


Simple experiment for three factors and four runs to illustrate design diagnostics.

Alias Matrix

Effect	X1*X2	X1*X3	X2*X3
Intercept	0	0	0
X1	0	0	-1
X2	0	-1	0
X3	-1	0	0

Color Map On Correlations



Module 1 – Conclusions

1. Remember Fisher's Four Principles
    1. Factorial Principle
    2. Randomization
    3. Blocking
    4. Replication
2. ANOVA models can be converted to regression models.
3. Factor coding for continuous and categorical factors is a technical detail important for this conversion.
4. Variance and bias are fundamental criteria for evaluating designs.

DOE Course – Module 2

Standard designs using an optimal design tool.

Goals

1. Give many examples of familiar designs created using an optimal design algorithm

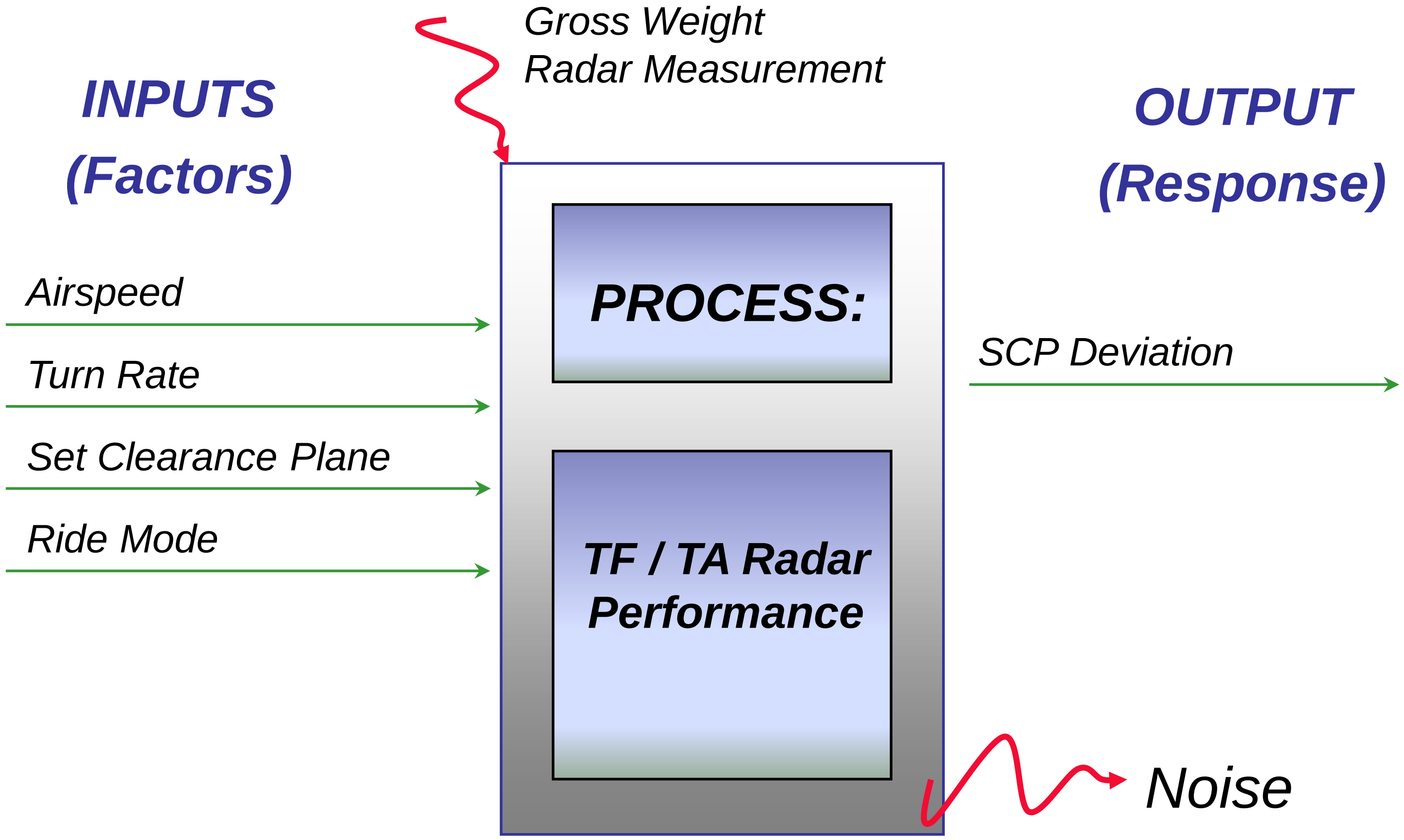
Optimal <> Full Factorial

Full Factorial designs are D-optimal for the models they support.

Example:

- 2^k designs are optimal for main effects plus interactions up to any order less than or equal to k.

Flight Test


Gross Weight
Radar Measurement
INPUTS
(Factors)
OUTPUT
(Response)
Airspeed
Turn Rate
Set Clearance Plane
Ride Mode
PROCESS:
TF / TA Radar Performance
SCP Deviation
Noise

Properties of this design

- Orthogonal
- Makes interpretation easy
- Minimizes the variance of the model coefficients
- Minimizes the average prediction variance
- Minimizes the maximum prediction variance
- You can’t do any better than this (for three factors in eight runs)!

Optimal <> Fractional Factorial

Fractional Factorial designs are D-optimal for the models they support.

Example:

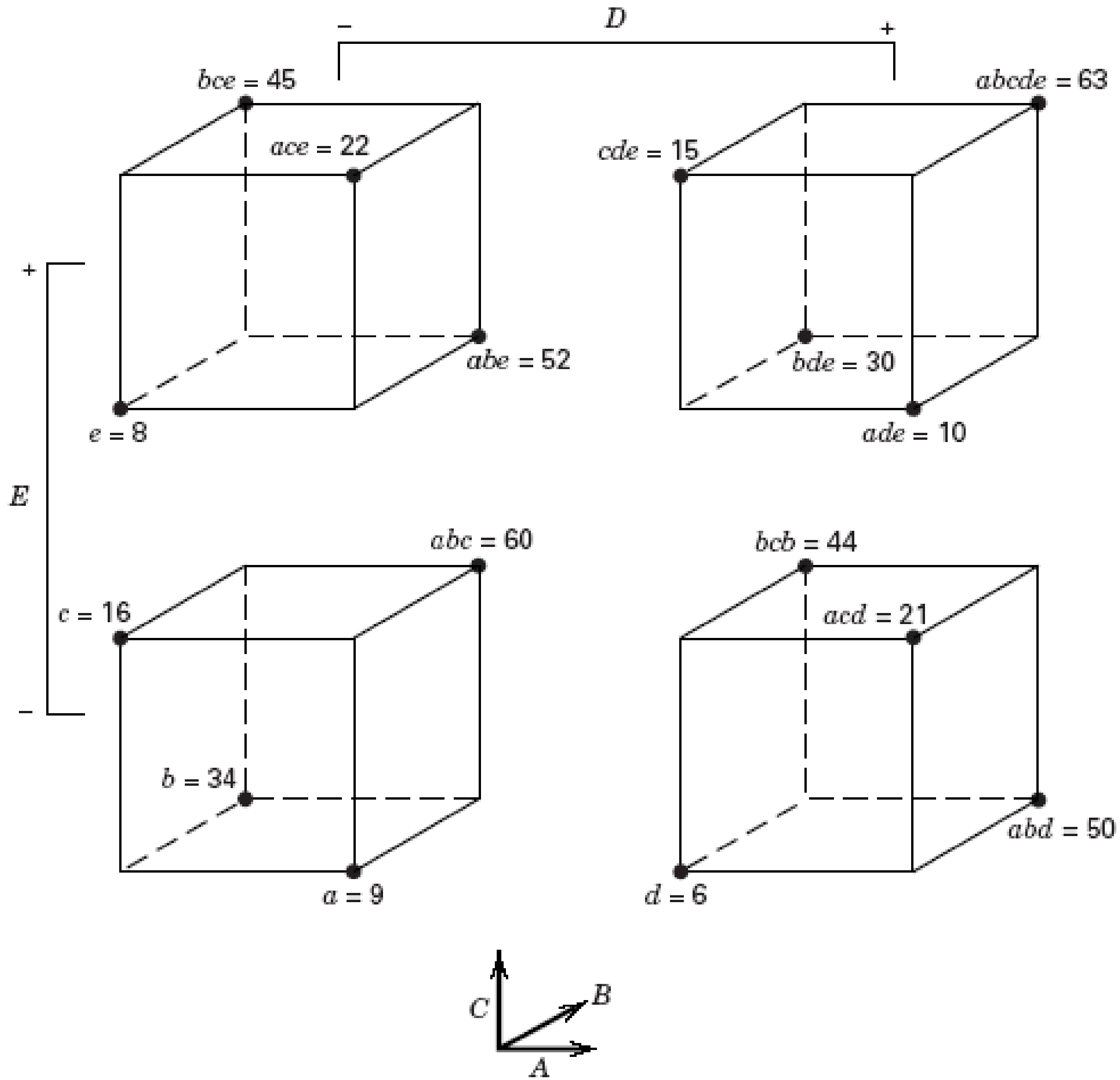
2^{k-p} designs are optimal for main effects plus interactions an order dependent on the resolution of the design.

Resolution V

- Models for main effects + all two-factor interactions
- Consider five factors in a photolithography process
  - A = aperture setting
  - B = exposure time
  - C = develop time
  - D = mask dimension
  - E = etch time

Consider the 2^{5-1} – Again Created Using an Optimal Design Tool

	Basic Design						
Run	A	B	C	D	$E = ABCD$	Treatment Combination	Yield
1	−	−	−	−	+	*e*	8
2	+	−	−	−	−	*a*	9
3	−	+	−	−	−	*b*	34
4	+	+	−	−	+	*abe*	52
5	−	−	+	−	−	*c*	16
6	+	−	+	−	+	*ace*	22
7	−	+	+	−	+	*bce*	45
8	+	+	+	−	−	*abc*	60
9	−	−	−	+	−	*d*	6
10	+	−	−	+	+	*ade*	10
11	−	+	−	+	+	*bde*	30
12	+	+	−	+	−	*abd*	50
13	−	−	+	+	+	*cde*	15
14	+	−	+	+	−	*acd*	21
15	−	+	+	+	−	*bcd*	44
16	+	+	+	+	+	*abcde*	63



D
−
+
bce = 45
ace = 22
abe = 52
e = 8
abcde = 63
cde = 15
bde = 30
ade = 10
E
+
−
abc = 60
c = 16
b = 34
a = 9
bcb = 44
acd = 21
abd = 50
d = 6
C
B
A

The 2^{5-1}

- Aliases:
  - All main effects are clear of the two-factor interactions
  - All two-factor interactions are clear of each other
- Orthogonal
- Makes interpretation easy
- Minimizes the variance of the model coefficients
- Minimizes the average prediction variance
- Minimizes the maximum prediction variance
- Once again, you can’t do any better than this!

JMP Demo

Relative Variance of Coefficients

Significance Level	0.050
Signal to Noise Ratio	1.000

Effect	Variance	Power
Intercept	0.063	0.246
A	0.063	0.246
B	0.063	0.246
C	0.063	0.246
D	0.063	0.246
E	0.063	0.246
A*B	0.063	0.246
A*C	0.063	0.246
A*D	0.063	0.246
A*E	0.063	0.246
B*C	0.063	0.246
B*D	0.063	0.246
B*E	0.063	0.246
C*D	0.063	0.246
C*E	0.063	0.246
D*E	0.063	0.246

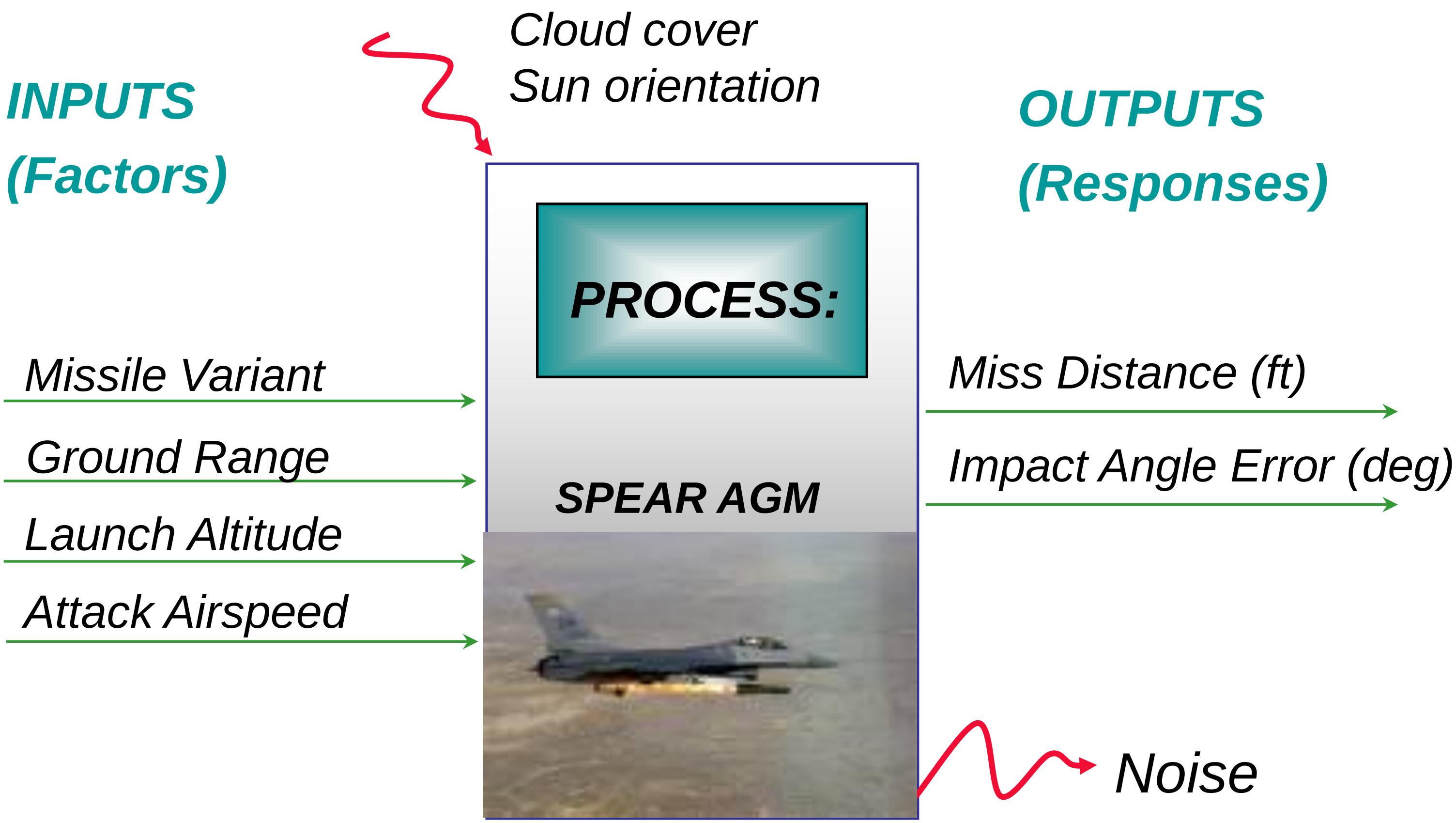
Resolution IV

Designs are optimal for main effects models

Certain two-factor interactions are also estimable with full precision but may be fully aliased with other two-factor interactions.

Example: $2^{(4-1)}$

SPEAR AGM Tests



The 2^{4-1}

- Aliases:
  - All main effects are clear of the two-factor interactions
  - two-factor interactions may be confounded with each other
- Orthogonal
- Makes interpretation easy – if there are no active interactions
- Minimizes the variance of the model coefficients
- Minimizes the average prediction variance
- Minimizes the maximum prediction variance
- Once again, you can't do any better than this!

Suppose that we want to focus on main effects.

- Six factors [eye focus time experiment from Montgomery (2009)]
  - A = visual acuity
  - B = distance to target
  - C = target shape
  - D = illumination level
  - E = target size
  - F = target density
- What are reasonable design choices?

	Basic Design						
Run	*A*	*B*	*C*	$D = AB$	$E = AC$	$F = BC$	
1	−	−	−	+	+	+	*def*
2	+	−	−	−	−	+	*af*
3	−	+	−	−	+	−	*be*
4	+	+	−	+	−	−	*abd*
5	−	−	+	+	−	−	*cd*
6	+	−	+	−	+	−	*ace*
7	−	+	+	−	−	+	*bcf*
8	+	+	+	+	+	+	*abcdef*

This is a 2^{6-3} fractional factorial, resolution III The defining relation is

$$I = ABD = ACE = BCF = BCDE = ACDF = ABEF = DEF$$

Let’s work out the aliases

We can create this design using an optimal design tool. It is useful to see the alias structure.

JMP Demo

Alias Matrix

Effect	1 2	1 3	1 4	1 5	1 6	2 3	2 4	2 5	2 6	3 4	3 5	3 6	4 5	4 6	5 6
Intercept	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
A	0	0	0	0	0	0	1	0	0	0	1	0	0	0	0
B	0	0	1	0	0	0	0	0	0	0	0	0	0	0	1
C	0	0	0	1	0	0	0	0	0	0	0	0	0	1	0
D	1	0	0	0	0	0	0	0	0	0	0	1	0	0	0
E	0	1	0	0	0	0	0	0	1	0	0	0	0	0	0
F	0	0	0	0	0	0	0	1	0	1	0	0	0	0	0
A*F	0	0	0	0	1	1	0	0	0	0	0	0	1	0	0

The 2^{6-3}

- Aliases:
  - All main effects are confounded with two-factor interactions
- Orthogonal
- Makes interpretation easy – if there are no active interactions
- Minimizes the variance of the model coefficients
- Minimizes the average prediction variance
- Minimizes the maximum prediction variance

Module 2 - Summary

1. Main message is that standard designs are optimal designs.
2. Optimal design generators can reproduce standard designs for routine problems.

Module 3 – Modern Screening Methods

There is substantial new research in both design and analysis of screening experiments in the last 15 years.

Much of this new research calls into question the conventional strategy of the standard use of regular fractional factorial designs for screening.

We will introduce some of these new methods in this section.

Many of the new designs are orthogonal but have more desirable aliasing properties than the regular fractional factorial designs previously shown.

Regular Designs may not Always be the Best Choice for Screening

- In regular designs the alias matrix consists of either 0, +1 or -1 entries
- That means that effects are completely confounded
- Unless the experimenter has some “process knowledge”, effects cannot be separated without conducting additional experiments
  – Fold-over
  – Partial fold-over
  – Optimal augmentation

Number of Orthogonal Designs versus Number of Factors

Number of Factors	Number of Nonisomorphic Designs
6	27
7	55
8	80
9	87
10	78
11	58
12	36
13	18
14	10
15	5

Define Nonisomorphic

Two designs are nonisomorphic if you cannot get one from the other by:

- Permuting rows
- Permuting columns
- Relabeling the level names

A Six-Factor Example

- Based on Example 8.4, DCM (2009)
- A = mold temperature, B = screw speed, C = holding time, D =cycle time, E = gate size, F = holding pressure
- Response = shrinkage
- The regular design is a 2^{6-2} fraction – this design is the maximum resolution (IV) and minimum aberration fraction

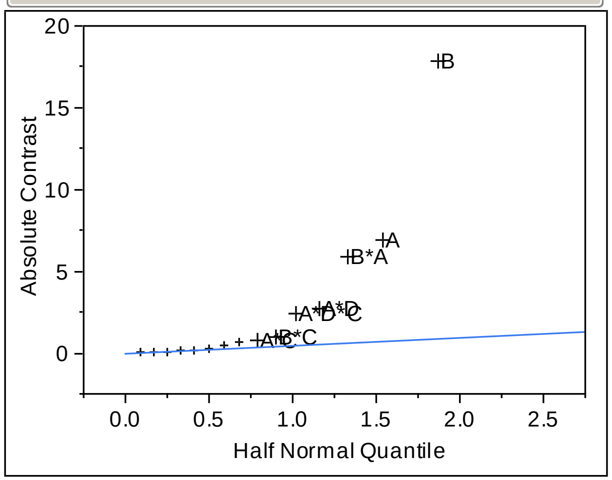
	Pattern	A	B	C	D	E	F	Shrinkage
1	−−−−−−	-1	-1	-1	-1	-1	-1	6
2	+−−−+−	1	-1	-1	-1	1	-1	10
3	−+−−++	-1	1	-1	-1	1	1	32
4	++−−−+	1	1	-1	-1	-1	1	60
5	−−+−++	-1	-1	1	-1	1	1	4
6	+−+−−+	1	-1	1	-1	-1	1	15
7	−++−−−	-1	1	1	-1	-1	-1	26
8	+++−+−	1	1	1	-1	1	-1	60
9	−−−+−+	-1	-1	-1	1	-1	1	8
10	+−−+++	1	-1	-1	1	1	1	12
11	−+−++−	-1	1	-1	1	1	-1	34
12	++−+−−	1	1	-1	1	-1	-1	60
13	−−+++−	-1	-1	1	1	1	-1	16
14	+−++−−	1	-1	1	1	-1	-1	5
15	−+++−+	-1	1	1	1	-1	1	37
16	++++++	1	1	1	1	1	1	52

Screening for Shrinkage

Contrasts

Term	Contrast	Lenth t-Ratio	Individual p-Value	Simultaneous p-Value	Aliases
B	17.8125	38.00	<.0001*	<.0001*	A*C*E, D*C*F
A	6.9375	14.80	<.0001*	0.0002*	B*C*E, D*E*F
D	0.6875	1.47	0.1475	0.8300	B*C*F, A*E*F
C	-0.4375	-0.93	0.3217	0.9974	B*A*E, B*D*F
E	0.1875	0.40	0.7173	1.0000	B*A*C, A*D*F
F	0.1875	0.40	0.7173	1.0000	B*D*C, A*D*E
B*A	5.9375	12.67	<.0001*	0.0003*	C*E
B*D	-0.0625	-0.13	0.9007	1.0000	C*F
A*D	-2.6875	-5.73	0.0022*	0.0150*	E*F
B*C	-0.9375	-2.00	0.0655	0.4886	A*E, D*F
A*C	-0.8125	-1.73	0.0977	0.6557	B*E
D*C	-0.0625	-0.13	0.9007	1.0000	B*F
D*E	0.3125	0.67	0.5315	1.0000	A*F
B*A*D	0.0625	0.13	0.9007	1.0000	D*C*E, A*C*F, B*E*F
A*D*C	-2.4375	-5.20	0.0030*	0.0220*	B*D*E, B*A*F, C*E*F

Half Normal Plot



Lenth PSE=0.46875

P-Values derived from a simulation of 10000 Lenth t ratios.

Main effects of A and B are important

The AB + CE interaction is important

The AD + EF interaction is important

How do we separate these interactions?

Unless there is outside information available, we'll need more data

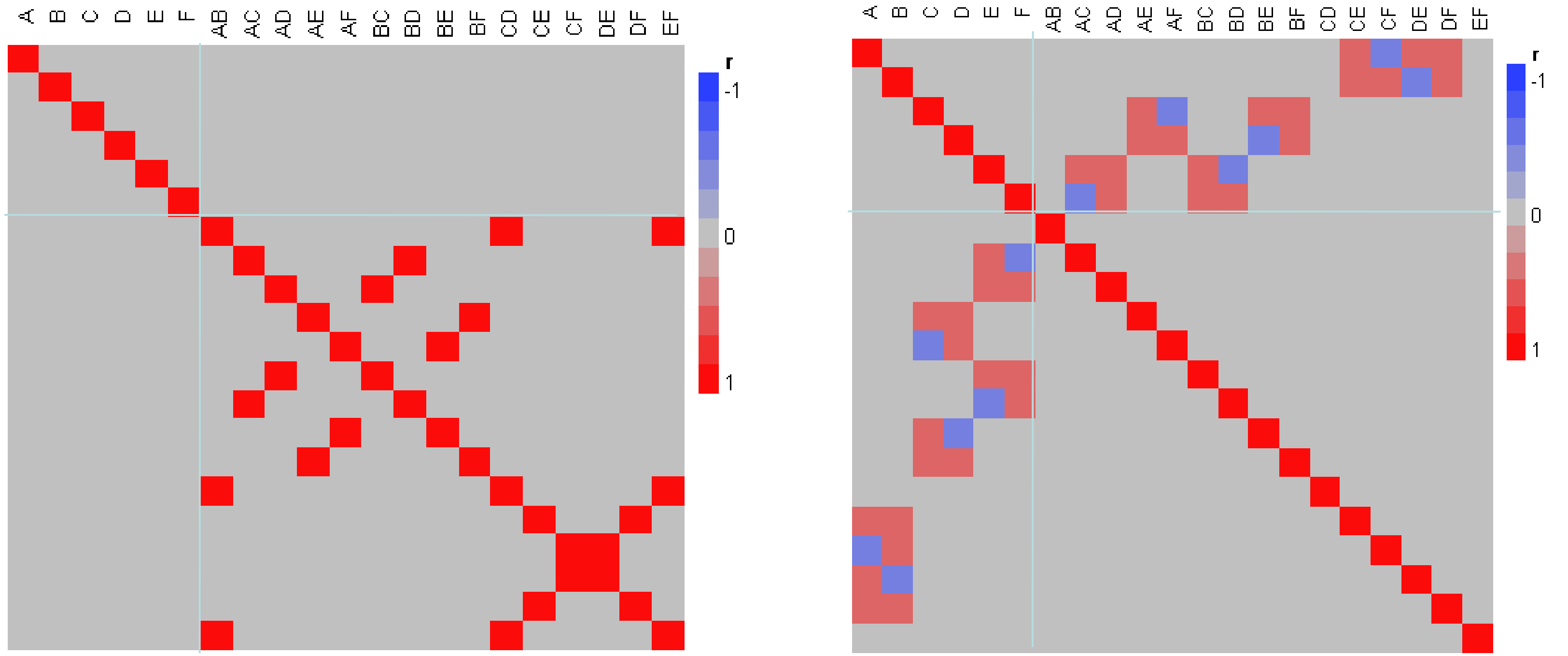
The No-Confounding Design

	A	B	C	D	E	F	Shrinkage
1	1	1	1	1	1	1	52.6802341
2	1	1	-1	-1	-1	-1	64.6413187
3	-1	-1	1	1	-1	-1	7.96901017
4	-1	-1	-1	-1	1	1	2.42780873
5	1	1	1	-1	1	-1	57.7850438
6	1	1	-1	1	-1	1	56.8934034
7	-1	-1	1	-1	-1	1	6.87241076
8	-1	-1	-1	1	1	-1	16.7307703
9	1	-1	1	1	1	-1	9.37286885
10	1	-1	-1	-1	-1	1	11.5379273
11	-1	1	1	1	-1	1	35.51462
12	-1	1	-1	-1	1	-1	26.9296784
13	1	-1	1	-1	-1	-1	13.7857102
14	1	-1	-1	1	1	1	7.30349366
15	-1	1	1	-1	1	1	34.5201021
16	-1	1	-1	1	-1	-1	32.0355995

Where Did the Data in this Experiment Come From?

- Simulated data
- We chose the significant main effects A and B, along with the two interactions AB and AD.
- We selected the random component to have the same standard deviation as the original data
- The result is data that represents closely the original experiment if the no-confounding design had been run

Color Plot for the No-Confounding Design



The design is orthogonal

No two-factor interactions are aliased with each other

There is no complete confounding

Stepwise Fit

Response:Shrinkage

Stepwise Regression Control

Prob to Enter 0.250
Prob to Leave 0.100
Direction Forward
Rules: Combine

Current Estimates

SSE	DFE	MSE	RSquare	RSquare Adj	Cp	AIC
6659.4375	15	443.9625	0.0000	0.0000	.	98.49923

Lock	Entered	Parameter	Estimate	nDF	SS	"F Ratio"	"Prob>F"
☑	☑	Intercept	27.3125	1	0	0.000	1.0000
☐	☐	A	0	1	770.0625	1.831	0.1975
☐	☐	B	0	1	5076.563	44.900	0.0000
☐	☐	C	0	1	3.16e-30	0.000	1.0000
☐	☐	D	0	1	3.16e-30	0.000	1.0000
☐	☐	E	0	1	28.89063	0.061	0.8085
☐	☐	F	0	1	28.89063	0.061	0.8085
☐	☐	A*B	0	3	6410.688	103.086	0.0000
☐	☐	A*C	0	3	781.4608	0.532	0.6691
☐	☐	A*D	0	3	885.625	0.614	0.6192
☐	☐	A*E	0	3	819.0536	0.561	0.6509
☐	☐	A*F	0	3	809.2569	0.553	0.6556
☐	☐	B*C	0	3	5076.563	12.829	0.0005
☐	☐	B*D	0	3	5087.961	12.951	0.0005
☐	☐	B*E	0	3	5115.757	13.256	0.0004
☐	☐	B*F	0	3	5125.554	13.366	0.0004
☐	☐	C*D	0	3	13.78835	0.008	0.9989
☐	☐	C*E	0	3	2577.449	2.526	0.1068
☐	☐	C*F	0	3	690.0164	0.462	0.7138
☐	☐	D*E	0	3	345.2905	0.219	0.8815
☐	☐	D*F	0	3	2382.766	2.229	0.1374
☐	☐	E*F	0	3	60.24101	0.037	0.9902

Step History

Step	Parameter	Action	"Sig Prob"	Seq SS	RSquare	Cp	p

We can use stepwise regression model fitting

All main effects and two-factor interactions are candidate variables for the model

Because there is no complete confounding, all interactions are potential candidates

Stepwise Fit

Response:Shrinkage

Stepwise Regression Control

Prob to Enter 0.250
Prob to Leave 0.100
Direction Forward
Rules: Combine

Current Estimates

SSE	DFE	MSE	RSquare	RSquare Adj	Cp	AIC
133.18753	10	13.318753	0.9800	0.9700	.	45.90671

Lock	Entered	Parameter	Estimate	nDF	SS	"F Ratio"	"Prob>F"
☑	☑	Intercept	27.3125	1	0	0.000	1.0000
☐	☑	A	6.9375	3	1449.688	36.282	0.0000
☐	☑	B	17.8125	2	5640.625	211.755	0.0000
☐	☐	C	0	1	3.16e-30	0.000	1.0000
☐	☑	D	-4.441e-16	2	115.5625	4.338	0.0440
☐	☐	E	0	1	4.21e-30	0.000	1.0000
☐	☐	F	0	1	4.21e-30	0.000	1.0000
☐	☑	A*B	5.9375	1	564.0625	42.351	0.0001
☐	☐	A*C	0	2	11.39834	0.374	0.6992
☐	☑	A*D	-2.6875	1	115.5625	8.677	0.0146
☐	☐	A*E	0	2	26.80068	1.008	0.4071
☐	☐	A*F	0	2	13.7383	0.460	0.6470
☐	☐	B*C	0	2	1.58e-29	0.000	1.0000
☐	☐	B*D	0	1	11.39834	0.842	0.3827
☐	☐	B*E	0	2	13.7383	0.460	0.6470
☐	☐	B*F	0	2	26.80068	1.008	0.4071
☐	☐	C*D	0	2	13.78835	0.462	0.6459
☐	☐	C*E	0	3	1.933557	0.034	0.9907
☐	☐	C*F	0	3	31.4007	0.720	0.5711
☐	☐	D*E	0	2	31.4007	1.234	0.3411
☐	☐	D*F	0	2	1.933557	0.059	0.9432
☐	☐	E*F	0	3	2.459758	0.044	0.9867

Step History

Step	Parameter	Action	"Sig Prob"	Seq SS	RSquare	Cp	p
1	A*B	Entered	0.0000	6410.688	0.9626	.	4
2	A*D	Entered	0.0440	115.5625	0.9800	.	6

Stepwise regression selects the main effects of A and B, along with the AB and AD interactions

The main effect of D is added to preserve the hierarchy in the model

The no-confounding design correctly identifies the model without any ambiguity and no need for additional runs

No-Confounding Designs

- The 16-run minimum aberration resolution IV designs (6, 7, and 8 factors) are among the most widely used designs in practice
- It is possible to find no-confounding designs that are superior to the standard minimum aberration resolution IV designs in the sense that they offer a better chance of detecting significant two-factor interactions
- These designs are constructed from the Hall matrices

Hall I 15 Factor Design

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

Hall II 15 Factor Design

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

Hall III 15 Factor Design

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

Hall IV 15 Factor Design

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

Hall V 15 Factor Design

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

Constructing the Recommended 6 Factor Design

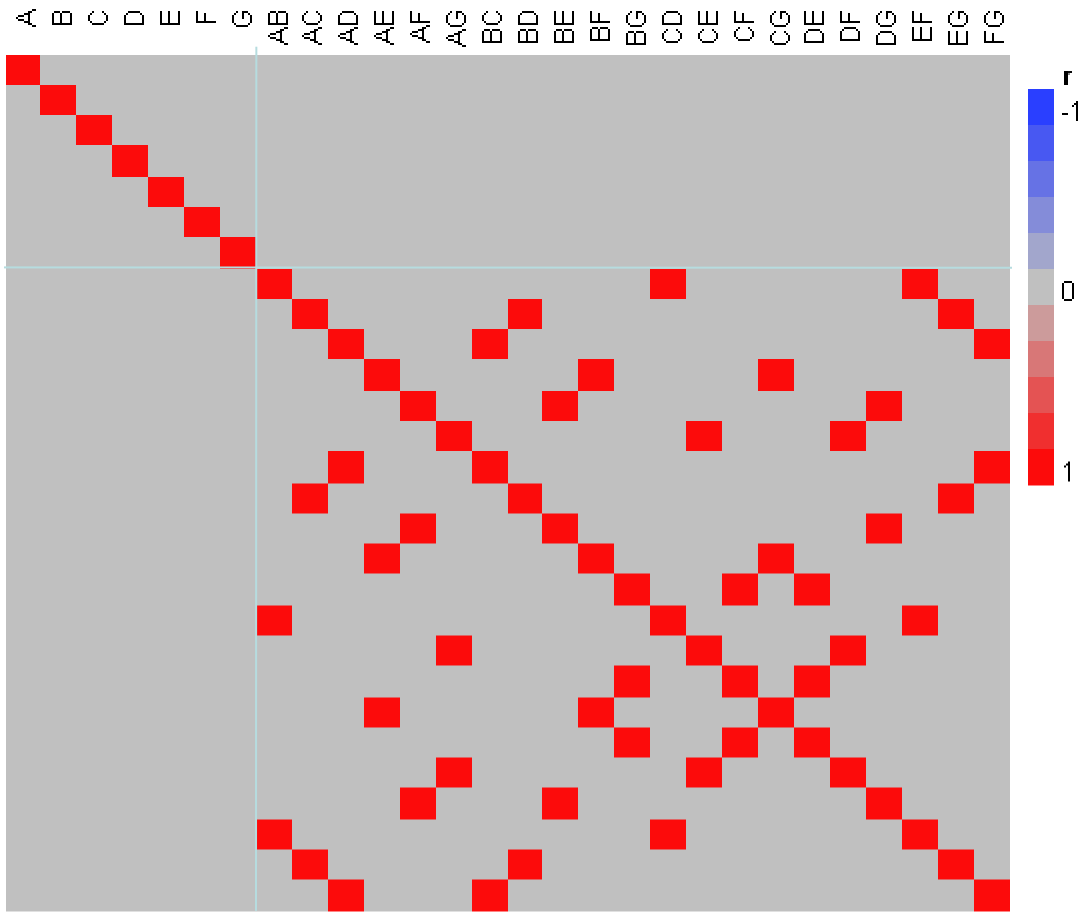
Run	A	B	C	D	E	F	G	H	J	K	L	M	N	P	Q
1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
2	1	1	1	1	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1
3	1	1	1	-1	-1	-1	-1	1	1	1	1	-1	-1	-1	-1
4	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	1
5	1	-1	-1	1	1	-1	-1	1	1	-1	-1	1	1	-1	-1
6	1	-1	-1	1	1	-1	-1	-1	-1	1	1	-1	-1	1	1
7	1	-1	-1	-1	-1	1	1	1	1	-1	-1	-1	-1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

Hall II – Columns D, E, H, K, M, Q

Recommended Nonregular 6 Factor Design

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

Color Plot for the Standard Minimum Aberration Resolution IV 7-Factor Design



Constructing the Recommended 7 Factor Design

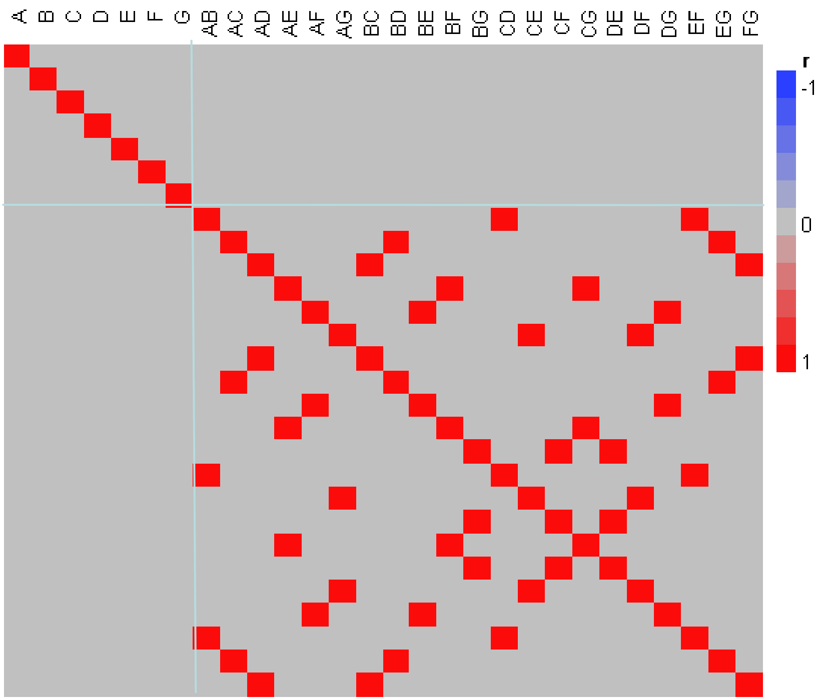
Run	A	B	C	D	E	F	G	H	J	K	L	M	N	P	Q
1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
2	1	1	1	1	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1
3	1	1	1	-1	-1	-1	-1	1	1	1	1	-1	-1	-1	-1
4	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	1
5	1	-1	-1	1	1	-1	-1	1	1	-1	-1	1	1	-1	-1
6	1	-1	-1	1	1	-1	-1	-1	-1	1	1	-1	-1	1	1
7	1	-1	-1	-1	-1	1	1	1	1	-1	-1	-1	-1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

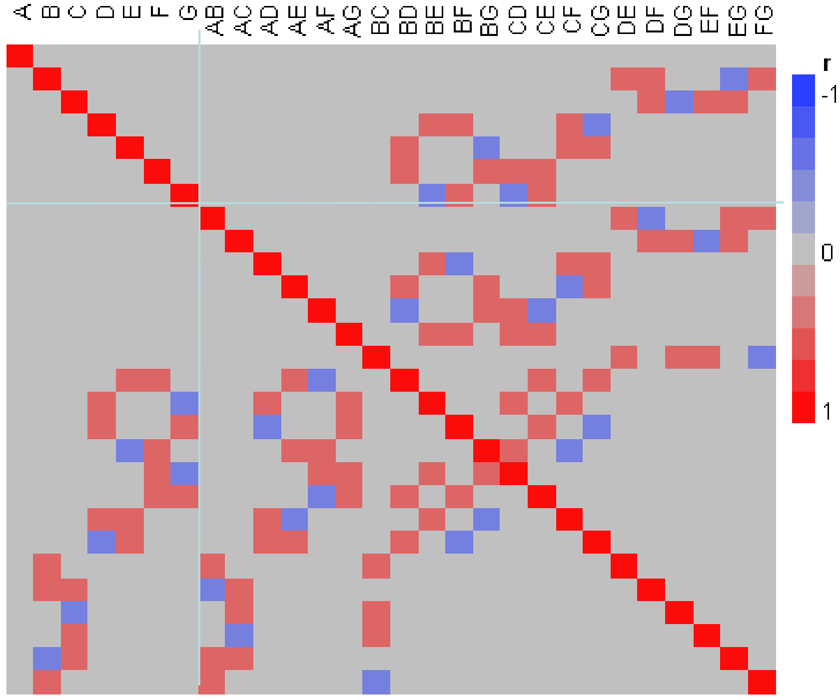
Hall III – Columns A, B, D, H, J, M, Q

Recommended Nonregular 7 Factor Design

Run	A	B	C	D	E	F	G
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
9	-1	1	1	1	1	1	-1
10	-1	1	1	-1	-1	-1	1
11	-1	1	-1	1	-1	1	1
12	-1	1	-1	-1	1	-1	-1
13	-1	-1	1	1	-1	-1	-1
14	-1	-1	1	-1	1	1	1
15	-1	-1	-1	1	1	-1	1
16	-1	-1	-1	-1	-1	1	-1

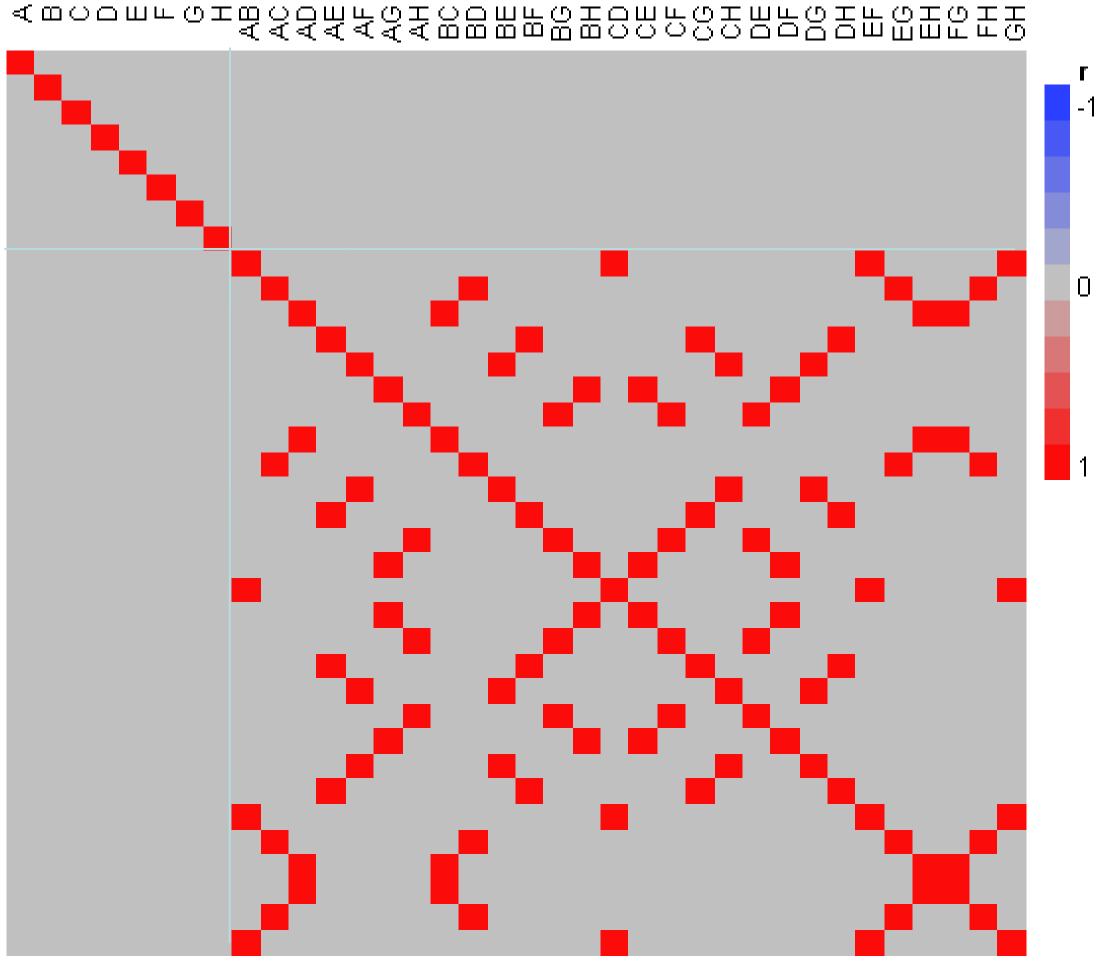
Comparison of Color Plots for the Standard and No-confounding Designs





The recommended design is orthogonal and does not have any complete confounding of effects

Color Plot for the Standard Minimum Aberration Resolution IV 8-Factor Design



Constructing the Recommended 8 Factor Design

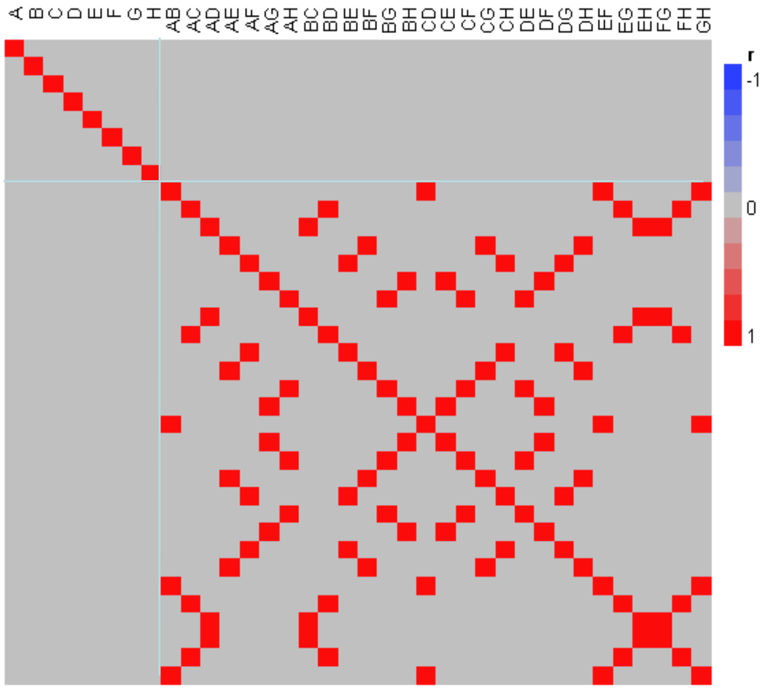
Run	A	B	C	D	E	F	G	H	J	K	L	M	N	P	Q
1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
2	1	1	1	1	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1
3	1	1	1	-1	-1	-1	-1	1	1	1	1	-1	-1	-1	-1
4	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	1
5	1	-1	-1	1	1	-1	-1	1	1	-1	-1	1	1	-1	-1
6	1	-1	-1	1	1	-1	-1	-1	-1	1	1	-1	-1	1	1
7	1	-1	-1	-1	-1	1	1	1	1	-1	-1	-1	-1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

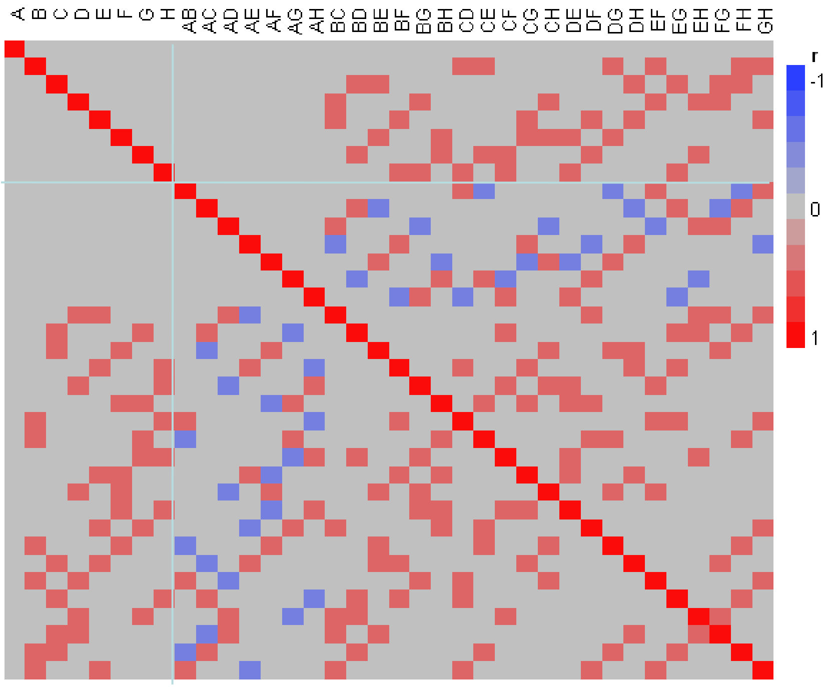
Hall IV – Columns A, B, D, F, H, J, M, P

Recommended Nonregular 8 Factor Design

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

Comparison of Color Plots for the Standard and No-confounding Designs





The recommended design is orthogonal and does not have any complete confounding of effects

Alternatives to Resolution III Designs

The regular resolution III designs with from 9 to 15 factors in 16 runs are used frequently in practice

These designs completely confound some interactions with main effects

For example, in the minimum aberration nine factor case, 12 two-factor interactions are aliased with main effects and 24 two-factor interactions are confounded in groups with other two-factor interactions

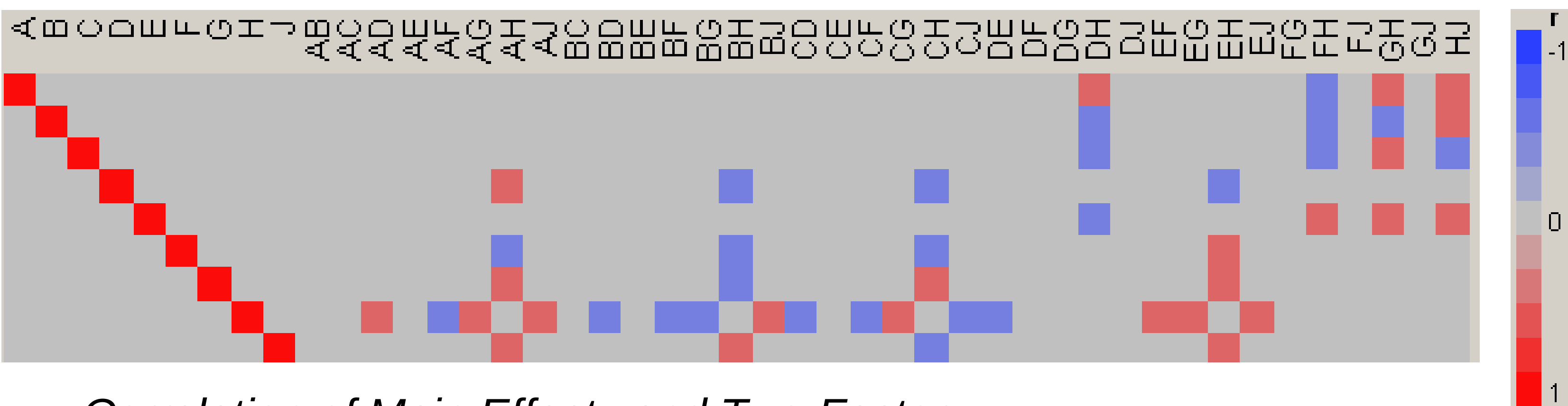
Follow-up experiments are often necessary, and the best augmentation approach may not be obvious.

Nonregular designs with no pure confounding of main effects and two-factor interactions are useful alternatives.

We provide a collection of these designs.

Recommended 9 Factor Design

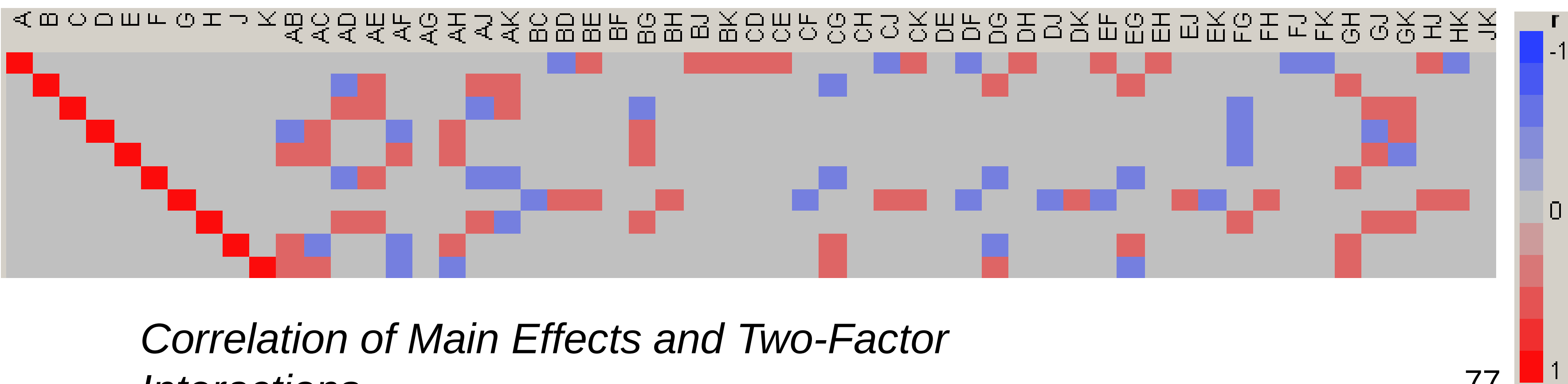
Run	A	B	C	D	E	F	G	H	J
1	-1	-1	-1	-1	-1	-1	1	-1	1
2	-1	-1	-1	1	-1	1	-1	1	-1
3	-1	-1	1	-1	1	1	1	1	-1
4	-1	-1	1	1	1	-1	-1	-1	1
5	-1	1	-1	-1	1	1	-1	1	1
6	-1	1	-1	1	1	-1	1	-1	-1
7	-1	1	1	-1	-1	-1	-1	1	-1
8	-1	1	1	1	-1	1	1	-1	1
9	1	-1	-1	-1	1	-1	-1	-1	-1
10	1	-1	-1	1	1	1	1	1	1
11	1	-1	1	-1	-1	1	-1	-1	1
12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



Correlation of Main Effects and Two-Factor Interactions

Recommended 10 Factor Design

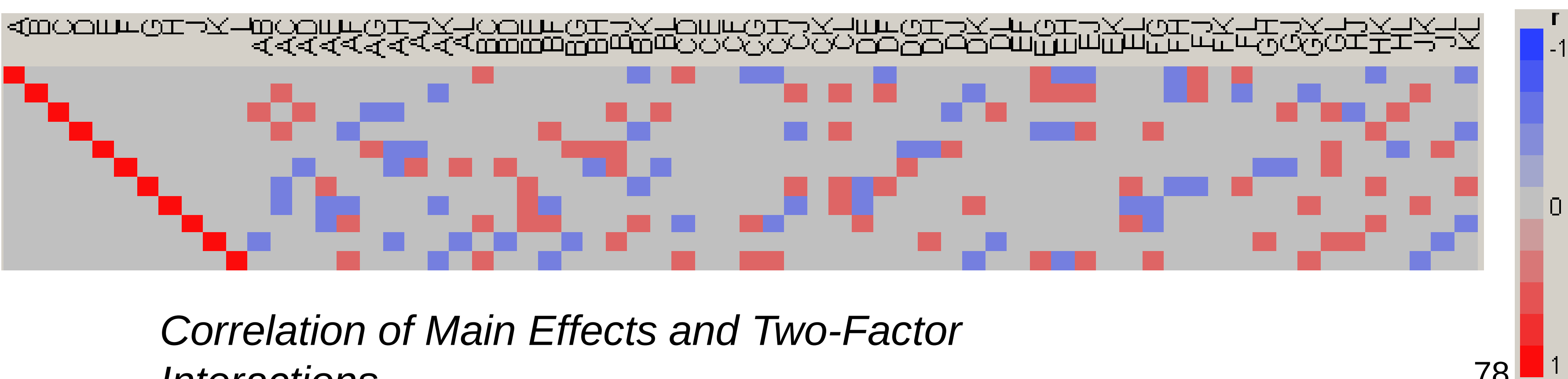
Run	A	B	C	D	E	F	G	H	J	K
1	-1	-1	-1	-1	1	-1	-1	1	-1	1
2	-1	-1	-1	1	1	1	-1	-1	1	1
3	-1	-1	1	-1	-1	1	1	1	1	1
4	-1	-1	1	-1	1	-1	1	-1	1	-1
5	-1	1	-1	1	-1	1	1	1	-1	1
6	-1	1	-1	1	1	-1	1	-1	-1	-1
7	-1	1	1	-1	-1	-1	-1	1	-1	-1
8	-1	1	1	1	-1	1	-1	-1	1	-1
9	1	-1	-1	-1	-1	1	1	-1	-1	-1
10	1	-1	-1	1	-1	-1	-1	1	1	-1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



Correlation of Main Effects and Two-Factor Interactions

Recommended 11 Factor Design

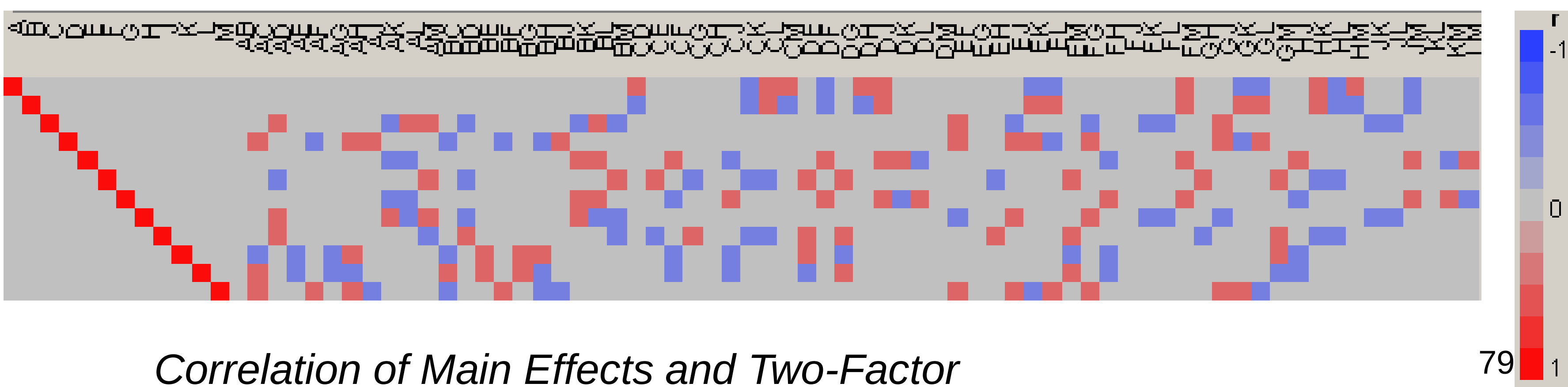
Run	A	B	C	D	E	F	G	H	J	K	L
1	-1	-1	-1	1	1	-1	-1	-1	1	-1	1
2	-1	-1	1	-1	-1	-1	1	-1	1	-1	-1
3	-1	-1	1	-1	1	1	-1	1	-1	-1	-1
4	-1	-1	1	1	-1	1	1	1	-1	1	1
5	-1	1	-1	-1	-1	-1	-1	-1	-1	1	1
6	-1	1	-1	1	-1	1	1	-1	-1	-1	-1
7	-1	1	-1	1	1	1	-1	1	1	1	-1
8	-1	1	1	-1	1	-1	1	1	1	1	1
9	1	-1	-1	-1	-1	1	-1	1	1	-1	1
10	1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



Correlation of Main Effects and Two-Factor Interactions

Recommended 12 Factor Design

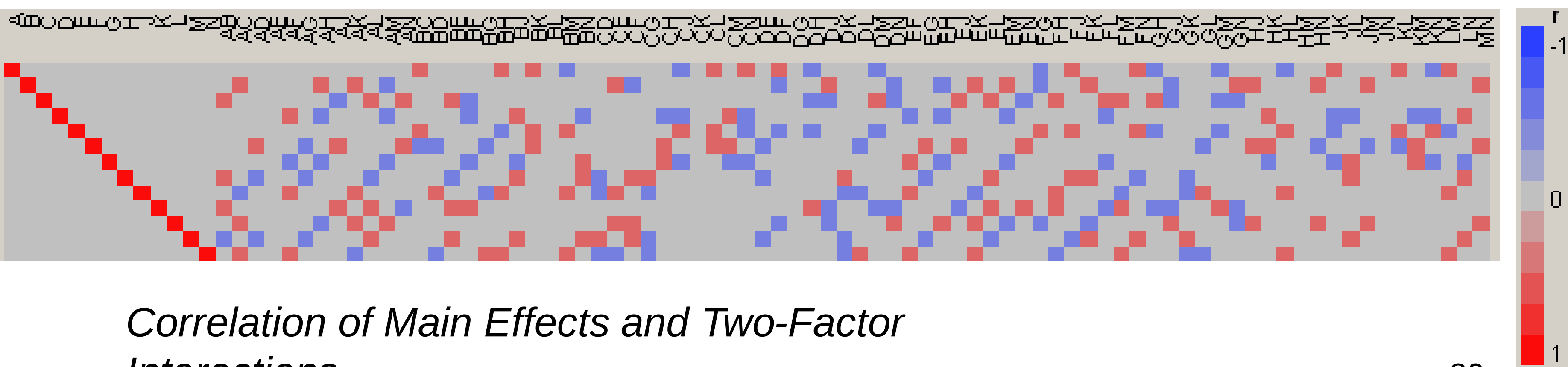
Run	A	B	C	D	E	F	G	H	J	K	L	M
1	-1	-1	-1	-1	1	-1	-1	1	1	-1	1	1
2	-1	-1	-1	1	-1	1	1	1	-1	-1	1	-1
3	-1	-1	1	-1	-1	-1	1	-1	1	1	-1	1
4	-1	-1	1	1	1	1	-1	-1	-1	1	-1	-1
5	-1	1	-1	1	-1	-1	-1	-1	-1	-1	-1	1
6	-1	1	-1	1	1	1	1	-1	1	1	1	1
7	-1	1	1	-1	-1	1	-1	1	1	-1	-1	-1
8	-1	1	1	-1	1	-1	1	1	-1	1	1	-1
9	1	-1	-1	-1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



Correlation of Main Effects and Two-Factor Interactions

Recommended 13 Factor Design

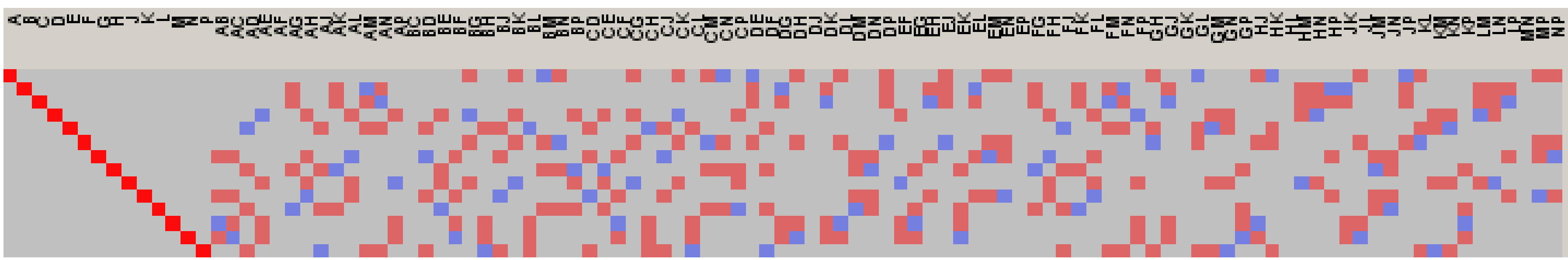
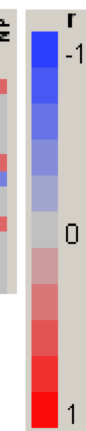
Run	A	B	C	D	E	F	G	H	J	K	L	M	N
1	-1	-1	-1	1	1	-1	-1	1	-1	1	1	-1	1
2	-1	-1	1	-1	-1	-1	-1	-1	1	1	-1	-1	1
3	-1	-1	1	-1	1	1	1	1	1	-1	1	-1	-1
4	-1	-1	1	1	-1	1	1	1	-1	1	-1	1	-1
5	-1	1	-1	-1	-1	1	-1	-1	-1	-1	1	-1	-1
6	-1	1	-1	-1	1	1	1	-1	-1	1	-1	1	1
7	-1	1	-1	1	-1	-1	1	1	1	-1	1	1	1
8	-1	1	1	1	1	-1	-1	-1	1	-1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



Correlation of Main Effects and Two-Factor Interactions

Recommended 14 Factor Design

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

Correlation of Main Effects and Two-Factor Interactions

Nine Factor Example from a Consumer Products Company

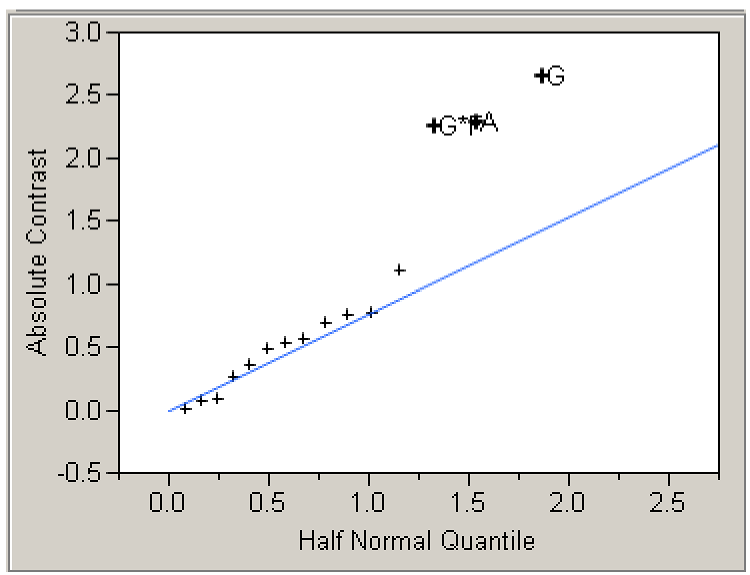
Factor names and levels have been changed to protect confidentiality.

Pattern	A	B	C	D	E	F	G	H	J	Y
−−−−+++−+	-1	-1	-1	-1	1	1	1	-1	1	26.18
+−−−++−+−	1	-1	-1	-1	1	1	-1	1	-1	21.14
−+−−+−++−	-1	1	-1	-1	1	-1	1	1	-1	33.16
++−−+−−−+	1	1	-1	-1	1	-1	-1	-1	1	20.3
−−+−−+++−	-1	-1	1	-1	-1	1	1	1	-1	30.4
+−+−−+−−+	1	-1	1	-1	-1	1	-1	-1	1	22.46
−++−−−+−+	-1	1	1	-1	-1	-1	1	-1	1	33.55
+++−−−−+−	1	1	1	-1	-1	-1	-1	1	-1	19.9
−−−+−−−−−	-1	-1	-1	1	-1	-1	-1	-1	-1	22.93
+−−+−−+++	1	-1	-1	1	-1	-1	1	1	1	30.54
−+−+−+−++	-1	1	-1	1	-1	1	-1	1	1	29.66
++−+−++−−	1	1	-1	1	-1	1	1	-1	-1	21.05
−−+++−−++	-1	-1	1	1	1	-1	-1	1	1	23.8
+−+++−+−−	1	-1	1	1	1	-1	1	-1	-1	28.94
−+++++−−−	-1	1	1	1	1	1	-1	-1	-1	22.84
+++++++++	1	1	1	1	1	1	1	1	1	21.64

Screening Results

Term	Contrast	Lenth t-Ratio	Individual p-Value	Simultaneous p-Value	Aliases
G	2.65188	3.45	0.0127*	0.1084	A*D
A	-2.28438	-2.97	0.0200*	0.1806	G*D
F	-1.10937	-1.44	0.1457	0.8422	D*B
E	-0.78062	-1.02	0.2774	0.9923	D*C
H	0.74937	0.97	0.2956	0.9959	J*D
J	0.48562	0.63	0.5638	1.0000	H*D
D	-0.35563	-0.46	0.6728	1.0000	G*A, H*J, F*B, E*C
B	-0.26813	-0.35	0.7459	1.0000	F*D
C	-0.08937	-0.12	0.9056	1.0000	E*D
G*F	-2.25563	-2.93	0.0208*	0.1874	E*J, A*B, H*C
A*F	-0.56438	-0.73	0.4325	1.0000	E*H, G*B, J*C
G*E	0.07813	0.10	0.9173	1.0000	F*J, H*B, A*C
A*E	0.53938	0.70	0.4548	1.0000	F*H, J*B, G*C
F*E	-0.69063	-0.90	0.3363	0.9993	A*H, G*J, B*C
G*H	0.00312	0.00	0.9967	1.0000	A*J, E*B, F*C

Note that both main effects and two-factor interactions are confounded. Many models are confounded leading to ambiguity and the need for follow-up experimentation.



Nonregular Alternative

Data was constructed similarly to the earlier six factor example.

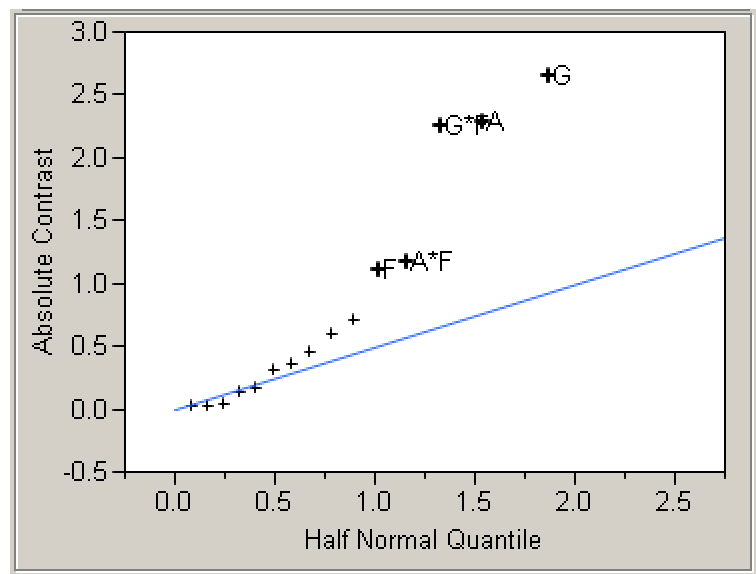
A	B	C	D	E	F	G	H	J	y
-1	-1	-1	-1	-1	-1	1	-1	1	32.0926491
-1	-1	-1	1	-1	1	-1	1	-1	28.0676578
-1	-1	1	-1	1	1	1	1	-1	27.6615287
-1	-1	1	1	1	-1	-1	-1	1	22.6731614
-1	1	-1	-1	1	1	-1	1	1	27.3251377
-1	1	-1	1	1	-1	1	-1	-1	31.3646236
-1	1	1	-1	-1	-1	-1	1	-1	24.5011871
-1	1	1	1	-1	1	1	-1	1	28.8340547
1	-1	-1	-1	1	-1	-1	-1	-1	21.4708721
1	-1	-1	1	1	1	1	1	1	21.3770478
1	-1	1	-1	-1	1	-1	-1	1	19.0010485
1	-1	1	1	-1	-1	1	1	-1	31.7377378
1	1	-1	-1	-1	1	1	-1	-1	21.3973688
1	1	-1	1	-1	-1	-1	1	1	18.2847795
1	1	1	-1	1	-1	1	1	1	30.9949895
1	1	1	1	1	1	-1	-1	-1	21.7061561

Screening Analysis

Term	Contrast		Lenth t-Ratio	Individual p-Value	Simultaneous p-Value	Aliases
G	2.65188		5.34	0.0026*	0.0209*	
A	-2.28438		-4.60	0.0045*	0.0376*	
F	-1.10938		-2.24	0.0448*	0.3502	
H	0.71313		1.44	0.1497	0.8424	
J	-0.45777		-0.92	0.3288	0.9983	
C	0.35811		0.72	0.4463	1.0000	
E	0.04106		0.08	0.9380	1.0000	
D	-0.02497		-0.05	0.9616	1.0000	
B	0.02041		0.04	0.9686	1.0000	
G*A	0.14098	*	0.28	0.7974	1.0000	
G*F	-2.25563		-4.55	0.0046*	0.0389*	A*C, J*D, E*B
A*F	-1.16424	*	-2.35	0.0381*	0.3020	
G*J	0.59995		1.21	0.2126	0.9557	A*E, F*D, C*B
A*J	-0.30347	*	-0.61	0.5785	1.0000	
F*J	0.17084		0.34	0.7545	1.0000	C*E, G*D, A*B

Main effects are not aliased.

One two-factor interaction is confounded with others. Much less ambiguity and an easy prospect for augmentation.



Plackett-Burman Designs

- These are a relatively familiar class of resolution III design
- The number of runs, *N*, need only be a multiple of four
- *N* = 4, 8, 12, 16, 20, 24, 28, 32, 36, 40, …
- The designs where *N* = 12, 20, 24, etc. are called **nongeometric** PB designs
- The nongeometric deigns are nonregular designs

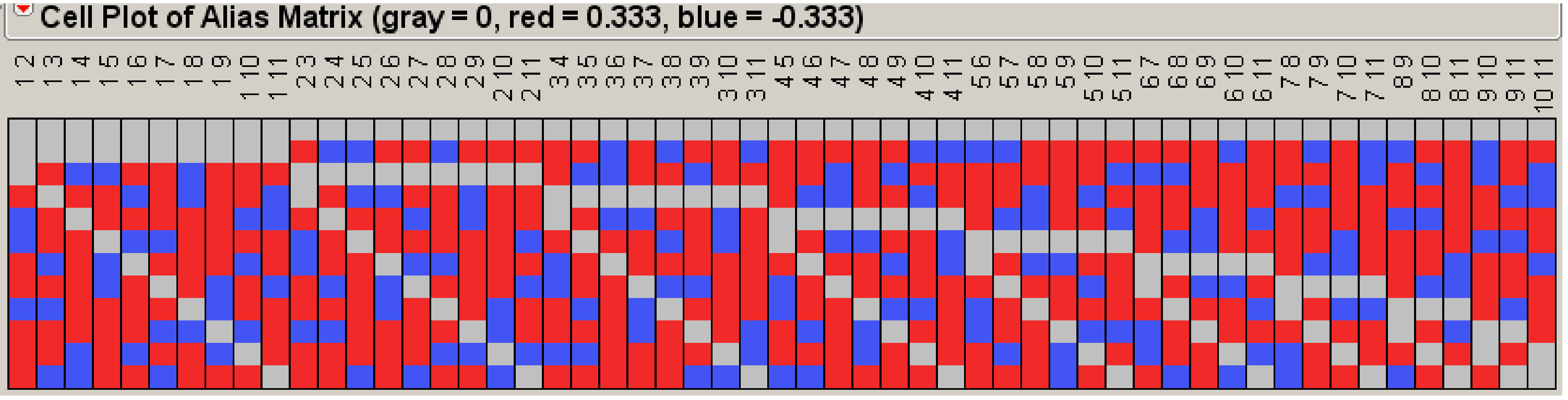
Plackett-Burman Designs

■ **TABLE 8.24**

Plackett–Burman Design for $N = 12$, $k = 11$

Run	A	B	C	D	E	F	G	H	I	J	K
1	+	−	+	−	−	−	+	+	+	−	+
2	+	+	−	+	−	−	−	+	+	+	−
3	−	+	+	−	+	−	−	−	+	+	+
4	+	−	+	+	−	+	−	−	−	+	+
5	+	+	−	+	+	−	+	−	−	−	+
6	+	+	+	−	+	+	−	+	−	−	−
7	−	+	+	+	−	+	+	−	+	−	−
8	−	−	+	+	+	−	+	+	−	+	−
9	−	−	−	+	+	+	−	+	+	−	+
10	+	−	−	−	+	+	+	−	+	+	−
11	−	+	−	−	−	+	+	+	−	+	+
12	−	−	−	−	−	−	−	−	−	−	−

The Alias Matrix for the 12-run Plackett-Burman Design



A 12-Factor Example

1	1	1	1	1	1	1	1	1	1	1	1	221.5032
-1	1	-1	-1	1	1	1	1	-1	1	-1	1	213.8037
-1	-1	1	-1	-1	1	1	1	1	-1	1	-1	167.5424
1	-1	-1	1	-1	-1	1	1	1	1	-1	1	232.2071
1	1	-1	-1	1	-1	-1	1	1	1	1	-1	186.3883
-1	1	1	-1	-1	1	-1	-1	1	1	1	1	210.6819
-1	-1	1	1	-1	-1	1	-1	-1	1	1	1	168.4163
-1	-1	-1	1	1	-1	-1	1	-1	-1	1	1	180.9365
-1	-1	-1	-1	1	1	-1	-1	1	-1	-1	1	172.5698
1	-1	-1	-1	-1	1	1	-1	-1	1	-1	-1	181.8605
-1	1	-1	-1	-1	-1	1	1	-1	-1	1	-1	202.4022
1	-1	1	-1	-1	-1	-1	1	1	-1	-1	1	186.0079
-1	1	-1	1	-1	-1	-1	-1	1	1	-1	-1	216.4375
1	-1	1	-1	1	-1	-1	-1	-1	1	1	-1	192.4121
1	1	-1	1	-1	1	-1	-1	-1	-1	1	1	224.4362
1	1	1	-1	1	-1	1	-1	-1	-1	-1	1	190.3312
1	1	1	1	-1	1	-1	1	-1	-1	-1	-1	228.3411
-1	1	1	1	1	-1	1	-1	1	-1	-1	-1	223.6747
-1	-1	1	1	1	1	-1	1	-1	1	-1	-1	163.5351
1	-1	-1	1	1	1	1	-1	1	-1	1	-1	236.5124

This is a 20-run Plackett-Burman design.

It is a nonregular design

■ **TABLE 8.26**

The Alias Matrix

Effect	1 2	1 3	1 4	1 5	1 6	1 7	1 8	1 9	1 10	1 11	1 12	2 3	2 4	2 5	2 6	2 7	2 8	2 9	2 10	2 11	2 12	3 4	3 5	3 6	3 7	3 8	3 9	3 10	3 11	3 12	4 5	4 6	4 7	4 8
Intercept	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
X1	0	0	0	0	0	0	0	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	−0.2	0.2	−0.2	0.6	0.2	0.2
X2	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	0.2	0	0	0	0	0	0	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	0.2	−0.2	−0.2
X3	0.2	0	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	−0.2	0.2	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	0	0	0	0	0	0	0	0	0	0.2	0.2	0.2	0.2
X4	0.2	−0.2	0	−0.2	0.6	0.2	0.2	0.2	−0.2	0.2	0.2	0.2	0	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	−0.2	0	0.2	0.2	0.2	0.2	−0.2	0.2	− 0.2	−0.2	0	0	0	0
X5	0.2	0.2	−0.2	0	−0.2	0.2	−0.2	0.2	0.2	0.6	−0.2	0.2	−0.2	0	−0.2	0.6	0.2	0.2	0.2	−0.2	0.2	0	0	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	0	0.2	0.2	0.2
X6	0.2	−0.2	0.6	−0.2	0	0.2	−0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	−0.2	0	−0.2	0.2	−0.2	0.2	0.2	0.6	0.2	−0.2	0	−0.2	0.6	0.2	0.2	0.2	−0.2	0.2	0	−0.2	0.2
X7	−0.2	−0.2	0.2	0.2	0.2	0	−0.2	0.2	0.2	−0.2	0.2	0.2	−0.2	0.6	−0.2	0	0.2	−0.2	−0.2	− 0.2	0.2	0.2	0.2	−0.2	0	−0.2	0.2	−0.2	0.2	0.2	0.2	−0.2	0	−0.2
X8	0.2	0.2	0.2	−0.2	−0.2	−0.2	0	0.6	0.2	−0.2	0.2	−0.2	−0.2	0.2	0.2	0.2	0	−0.2	0.2	0.2	−0.2	0.2	−0.2	0.6	−0.2	0	0.2	−0.2	−0.2	−0.2	0.2	0.2	−0.2	0
X9	−0.2	−0.2	0.2	0.2	−0.2	0.2	0.6	0	0.2	0.2	0.2	0.2	0.2	0.2	−0.2	−0.2	−0.2	0	0.6	0.2	−0.2	−0.2	−0.2	0.2	0.2	0.2	0	−0.2	0.2	0.2	0.2	−0.2	0.6	−0.2
X10	−0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	0.2	0	0.2	−0.2	−0.2	−0.2	0.2	0.2	−0.2	0.2	0.6	0	0.2	0.2	0.2	0.2	0.2	−0.2	−0.2	−0.2	0	0.6	0.2	−0.2	−0.2	0.2	0.2
X11	0.2	−0.2	0.2	0.6	0.2	−0.2	−0.2	0.2	0.2	0	−0.2	−0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	0.2	0	0.2	−0.2	−0.2	0.2	0.2	−0.2	0.2	0.6	0	0.2	0.2	0.2	0.2	−0.2
X12	0.2	0.2	0.2	−0.2	−0.2	0.2	0.2	0.2	−0.2	−0.2	0	0.2	−0.2	0.2	0.6	0.2	−0.2	−0.2	0.2	0.2	0	−0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	0.2	0	−0.2	−0.2	0.2	0.2

Effect	4 9	4 10	4 11	4 12	5 6	5 7	5 8	5 9	5 10	5 11	5 12	6 7	6 8	6 9	6 10	6 11	6 12	7 8	7 9	7 10	7 11	7 12	8 9	8 10	8 11	8 12	9 10	9 11	9 12	10 11	10 12	11 12
Intercept	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
X1	0.2	−0.2	0.2	0.2	−0.2	0.2	−0.2	0.2	0.2	0.6	−0.2	0.2	−0.2	−0.2	−0.2	0.2	−0.2	−0.2	0.2	0.2	−0.2	0.2	0.6	0.2	−0.2	0.2	0.2	0.2	0.2	0.2	−0.2	−0.2
X2	0.2	−0.2	−0.2	−0.2	−0.2	0.6	0.2	0.2	0.2	−0.2	0.2	−0.2	0.2	−0.2	0.2	0.2	0.6	0.2	−0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	−0.2	0.6	0.2	−0.2	0.2	0.2	0.2
X3	−0.2	0.2	−0.2	−0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	−0.2	0.6	0.2	0.2	0.2	−0.2	−0.2	0.2	−0.2	0.2	0.2	0.2	−0.2	−0.2	−0.2	−0.2	0.2	0.2	0.6	0.2	0.2
X4	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	0.6	0.2	0.2	0.2	−0.2	0.2	−0.2	0.2	0.2	−0.2	−0.2	−0.2	0.2	0.6
X5	0.2	−0.2	0.2	−0.2	0	0	0	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	0.2	−0.2	−0.2	0.2	−0.2	0.6	0.2	0.2	−0.2	0.2	−0.2	0.2	−0.2	−0.2
X6	−0.2	−0.2	0.2	−0.2	0	0.2	0.2	0.2	0.2	−0.2	0.2	0	0	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	−0.2	0.2	−0.2	−0.2	−0.2	0.6	0.2	−0.2	0.2	0.2
X7	0.6	0.2	0.2	0.2	0.2	0	−0.2	0.2	−0.2	−0.2	0.2	0	0.2	0.2	0.2	0.2	−0.2	0	0	0	0	0	0.2	0.2	0.2	0.2	−0.2	0.2	−0.2	−0.2	0.6	−0.2
X8	−0.2	0.2	−0.2	0.2	0.2	−0.2	0	−0.2	0.6	0.2	0.2	0.2	0	−0.2	0.2	−0.2	−0.2	0	0.2	0.2	0.2	0.2	0	0	0	0	0.2	0.2	0.2	−0.2	0.2	−0.2
X9	0	0.2	−0.2	−0.2	0.2	0.2	−0.2	0	−0.2	0.2	−0.2	0.2	−0.2	0	−0.2	0.6	0.2	0.2	0	−0.2	0.2	−0.2	0	0.2	0.2	0.2	0	0	0	0.2	0.2	−0.2
X10	0.2	0	−0.2	0.2	0.2	−0.2	0.6	−0.2	0	0.2	−0.2	0.2	0.2	−0.2	0	−0.2	0.2	0.2	−0.2	0	−0.2	0.6	0.2	0	−0.2	0.2	0	0.2	0.2	0	0	0.2
X11	−0.2	−0.2	0	0.6	−0.2	−0.2	0.2	0.2	0.2	0	−0.2	0.2	−0.2	0.6	−0.2	0	0.2	0.2	0.2	−0.2	0	−0.2	0.2	−0.2	0	−0.2	0.2	0	−0.2	0	0.2	0
X12	−0.2	0.2	0.6	0	0.2	0.2	0.2	−0.2	−0.2	−0.2	0	−0.2	−0.2	0.2	0.2	0.2	0	0.2	−0.2	0.6	−0.2	0	0.2	0.2	−0.2	0	0.2	−0.2	0	0.2	0	0

Stepwise Regression Analysis

Stepwise Fit

Response: Y

Stepwise Regression Control

Prob to Enter 0.250

Prob to Leave 0.100

Direction: Forward

Rules: Combine

Enter All

Remove All

Go | Stop | Step | Make Model

Current Estimates

SSE	DFE	MSE	RSquare	RSquare Adj	Cp	AICc
10731.993	19	564.84173	0.0000	0.0000	.	187.1685

Lock	Entered	Parameter	Estimate	nDF	SS	"F Ratio"	"Prob>F"
✓	✓	Intercept	200.000005	1	0	0.000	1
☐	☐	X1	0	1	1279.998	2.438	0.13587
☐	☐	X2	0	1	2784.798	6.307	0.02178
☐	☐	X3	0	1	452.2794	0.792	0.38525
☐	☐	X4	0	1	1843.202	3.733	0.06926
☐	☐	X5	0	1	67.22014	0.113	0.74014
☐	☐	X6	0	1	86.41364	0.146	0.70675
☐	☐	X7	0	1	292.6683	0.505	0.48657
☐	☐	X8	0	1	60.08346	0.101	0.75389
☐	☐	X9	0	1	572.9883	1.015	0.32701
☐	☐	X10	0	1	32.53469	0.055	0.81766
☐	☐	X11	0	1	15.37749	0.026	0.87411
☐	☐	X12	0	1	0.159758	0.000	0.98712
☐	☐	X1*X2	0	3	5907.994	6.532	0.00431
☐	☐	X1*X3	0	3	1736.781	1.030	0.40582

The data for the example came from a simulation model:

$$y = 200 + 8x_1 + 10x_2 + 12x_4 - 12x_1x_2 + 9x_1x_4 + \varepsilon$$

$$\varepsilon \sim N(0,25)$$

Current Estimates

SSE	DFE	MSE	RSquare	RSquare Adj	Cp	AIC
211.8823	11	19.262027	0.9803	0.9659	.	148.408

Lock	Entered	Parameter	Estimate	nDF	SS	"F Ratio"	"Prob>F"
☑	☑	Intercept	200.000005	1	0	0.000	1
☐	☑	X1	7.41406665	3	5669.637	98.114	3.2e-8
☐	☑	X2	8.8209798	2	4202.343	109.084	5.58e-8
☐	☐	X3	0	1	3.198243	0.153	0.70365
☐	☑	X4	12.7575105	2	4043.746	104.967	6.83e-8
☐	☑	X5	3.13135896	2	263.0099	6.827	0.01181
☐	☑	X6	-2.5290584	2	169.9072	4.410	0.03922
☐	☐	X7	0	1	6.902396	0.337	0.57456
☐	☐	X8	0	1	22.63211	1.196	0.29979
☐	☐	X9	0	1	21.37415	1.122	0.31441
☐	☐	X10	0	1	18.95875	0.983	0.34491
☐	☐	X11	0	1	41.50512	2.436	0.14963
☐	☐	X12	0	1	17.5764	0.905	0.36398
☐	☑	X1*X2	-12.857886	1	2691.884	139.751	1.36e-7
☐	☐	X1*X3	0	2	18.25134	0.424	0.66675
☐	☑	X1*X4	11.9654342	1	1415.354	73.479	3.37e-6

All model terms were correctly identified.

Estimated parameters are very close to the actual values

One non-significant factor (X_5) was identified – a type I error

Type I errors in screening experiments are less of a problem than Type II errors.

Alias Optimal Design

One criticism of variance optimal designs (*D*, *I* and *G*) is that they focus all the effort on precise estimation of only one model.

In particular, there is no attention to possible aliasing of terms in this model by likely higher order terms.

Example:

> In screening designs we want to get good estimates of main effects but we do not want these estimates biased by two-factor interactions.

Embarrassing Problem Case

Suppose we have 4 factors and want to generate an 8 run experiment.

The classical design everyone would use is the resolution IV design that confounds factor 4 with the 123 interaction.

Yet, any orthogonal 2-level design is optimal for the main effect model.

Demonstration in JMP

A New Optimality Criterion

Recently Jones and Nachtsheim proposed a new criterion that addresses this concern.

Their designs minimize the squared norm of the alias matrix subject to a lower bound constraint on the *D*-efficiency of the design.

Their results are impressive and provide a safer approach to screening design for novice investigators.

Reactor Case Study

Box, Hunter and Hunter (2005) p. 260 present the results of a reactor study that was a full factorial design with 5 factors at 2 levels each.

Suppose that they had run a 12 run screening experiment instead.

The D-optimal design is the orthogonal 12 run design that is isomorphic to the Plackett-Burman design.

We will compare the performance of this design with the alias optimal design.

Robust Screening Designs

Engineers often prefer designs for quantitative factors to have three levels. Yet the most familiar screening designs are two-level designs.

Robust screening designs are three-level designs for quantitative factors with some very nice properties.

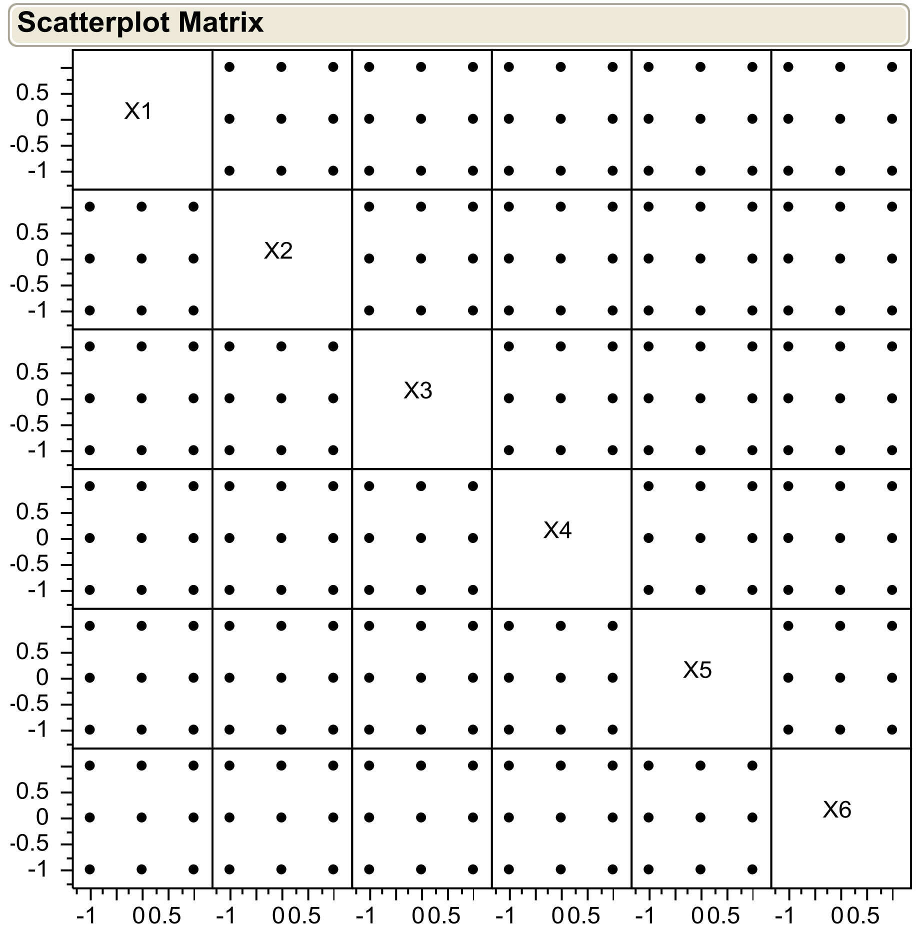
Robust Screening Design Properties

1. The number of required runs is only one more than twice the number of factors.
2. Unlike resolution III designs, main effects are completely independent of two-factor interactions. As a result, estimates of main effects are not biased by the presence of active two-factor interactions, regardless of whether the interactions are included in the model.
3. Unlike resolution IV designs, two-factor interactions are not completely confounded with other two-factor interactions, although they may be correlated.
4. Unlike resolution III, IV and V designs with added center points, all quadratic effects are estimable in models comprised of any number of linear and quadratic main effects terms.
5. Quadratic effects are orthogonal to main effects and not completely confounded (though correlated) with interaction effects.
6. With six or more factors, the designs are capable of estimating all possible full quadratic models involving three or fewer factors.

Robust Screening Design Structure

Foldover Pair	Run (i)	Factor Levels $x_{i,1}$	$x_{i,2}$	$x_{i,3}$	$\cdots$	$x_{i,m}$
1	1	0	± 1	± 1	$\cdots$	± 1
	2	0	∓ 1	∓ 1	$\cdots$	∓ 1
2	3	± 1	0	± 1	$\cdots$	± 1
	4	∓ 1	0	∓ 1	$\cdots$	∓ 1
3	5	± 1	± 1	0	$\cdots$	± 1
	6	∓ 1	∓ 1	0	$\cdots$	∓ 1
$\vdots$	$\vdots$	$\vdots$	$\vdots$	$\vdots$	$\ddots$	$\vdots$
m	$2m-1$	± 1	± 1	± 1	$\cdots$	0
	$2m$	∓ 1	∓ 1	∓ 1	$\cdots$	0
Centerpoint	$m+1$	0	0	0	$\cdots$	0

Robust Screening Design JMP Example



The Case for Non-orthogonal Designs

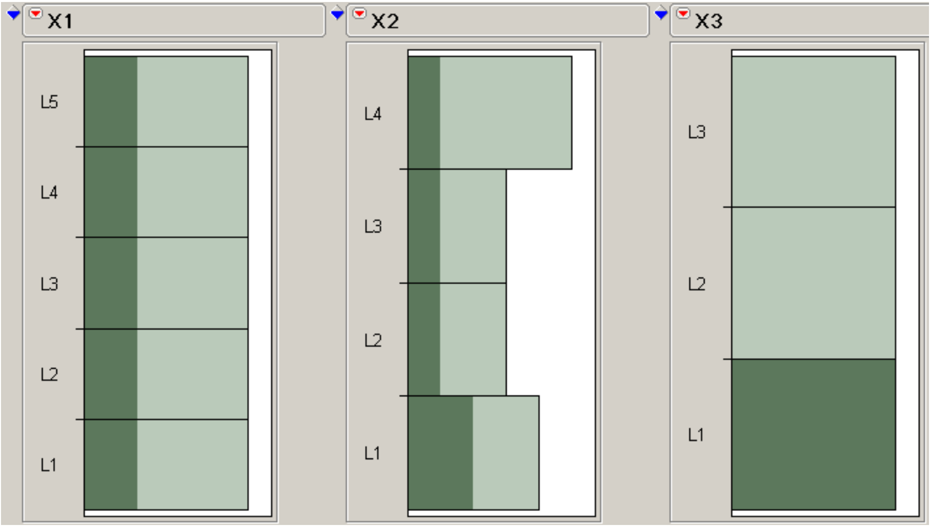
- Sometimes insisting on an orthogonal design is problematic
- Suppose that we have five factors:
  - A is categorical at 5 levels
  - B is categorical at 4 levels
  - C is categorical at 3 levels
  - D & E are continuous with 2 levels
- A full factorial has 240 runs and is orthogonal
- But would you really seriously consider running this experiment?

The Case for Non-orthogonal Designs

- What about a one-half fraction?
  - 120 runs
  - Not orthogonal, but very close
- What about a one-quarter fraction?
  - 60 runs
  - Not orthogonal, but close
- The 30 run design isn’t orthogonal, but it’s close
- We only need 11 degrees of freedom to estimate the main effects?
- What can we do with 15 runs?

JMP Demo

X1	X2	X3	X4	X5
L4	L4	L1	-1	1
L4	L3	L2	1	1
L2	L4	L3	1	1
L4	L1	L3	1	-1
L5	L1	L2	-1	1
L1	L3	L3	-1	1
L3	L1	L1	1	1
L3	L2	L3	-1	-1
L3	L4	L2	-1	-1
L2	L1	L1	-1	-1
L5	L4	L3	1	1
L1	L2	L1	1	1
L2	L2	L2	1	1
L1	L4	L2	1	-1
L5	L3	L1	1	-1



References

Hall, M. Jr. (1961). Hadamard matrix of order 16. *Jet Propulsion Laboratory Research Summary,* ***1,*** 21–26.

Johnson, M.E. and Jones, B., (2010) "Classical Design Structure of Orthogonal Designs with Six to Eight Factors and Sixteen Runs" accepted by *Quality and Reliability Engineering International*.

Jones, B. and Montgomery, D., (2010) "Alternatives to Resolution IV Screening Designs in 16 Runs." *International Journal of Experimental Design and Process Optimization*, 2010; Vol. 1 No. 4: 285-295.

Jones, B. and Nachtsheim, C. J. (2011) "A Class of Three-Level Designs for Definitive Screening in the Presence of Second-Order Effects" accepted by *Journal of Quality Technology*.

Jones, B. and Nachtsheim, C. J. (2011) "Efficient Designs with Minimal Aliasing" accepted by *Technometrics*

Plackett, R. L., and Burman, J. P. (1946), "The Design of Optimum Multifactor Experiments," *Biometrika, 33, 305–325.*

Montgomery, D.C. (2009) "Design and Analysis of Experiments" 7th Edition, Wiley Hoboken, New Jersey.

Sun, D. X., Li, W., and Ye, K. Q. (2002), "An Algorithm for Sequentially Constructing Non-Isomorphic Orthogonal Designs and Its Applications," Technical Report SUNYSB-AMS-02-13, State University of New York at Stony Brook, Dept. of Applied Mathematics and Statistics.

Module 3 - Conclusions

- The traditional approach to screening is to use regular fractional factorial designs.
- Recent research in design has found alternative designs that are strong competitors to these designs.
- In any case where two-factor interactions are likely and you cannot afford to run a resolution V design, these new designs are preferred.

Module 4 - Blocking

- Many experiments involve factors that affect the response, but the experimenter isn’t really interested in them for the purposes of system or process control.
- Sometimes these are called nuisance variables
- Examples include batches of raw material, operators, and time (shift, day of week, etc)
- Blocking is a design technique to separate the effect of a nuisance factor from the other sources of variability.

Tire Wear Study

- We have 4 brands of Tires
  - Michelin, Continental, Goodyear and Firestone
- We want to evaluate each brand with respect to tread wear using a road test
- How should we design this study?
- Let’s consider some possibilities…
  - We will use JMP to explore various possible plans.

Blocking

- **Blocking** is a technique for dealing with **nuisance factors**
- A **nuisance** factor is a factor that probably has some effect on the response, but it's of no interest to the experimenter…however, the variability it transmits to the response needs to be controlled or minimized
- Typical nuisance factors include batches of raw material, operators, pieces of test equipment, time (shifts, days, etc.), different experimental units
- **Many** industrial experiments involve blocking (or should)
- Failure to block is a common flaw in designing an experiment (consequences?)

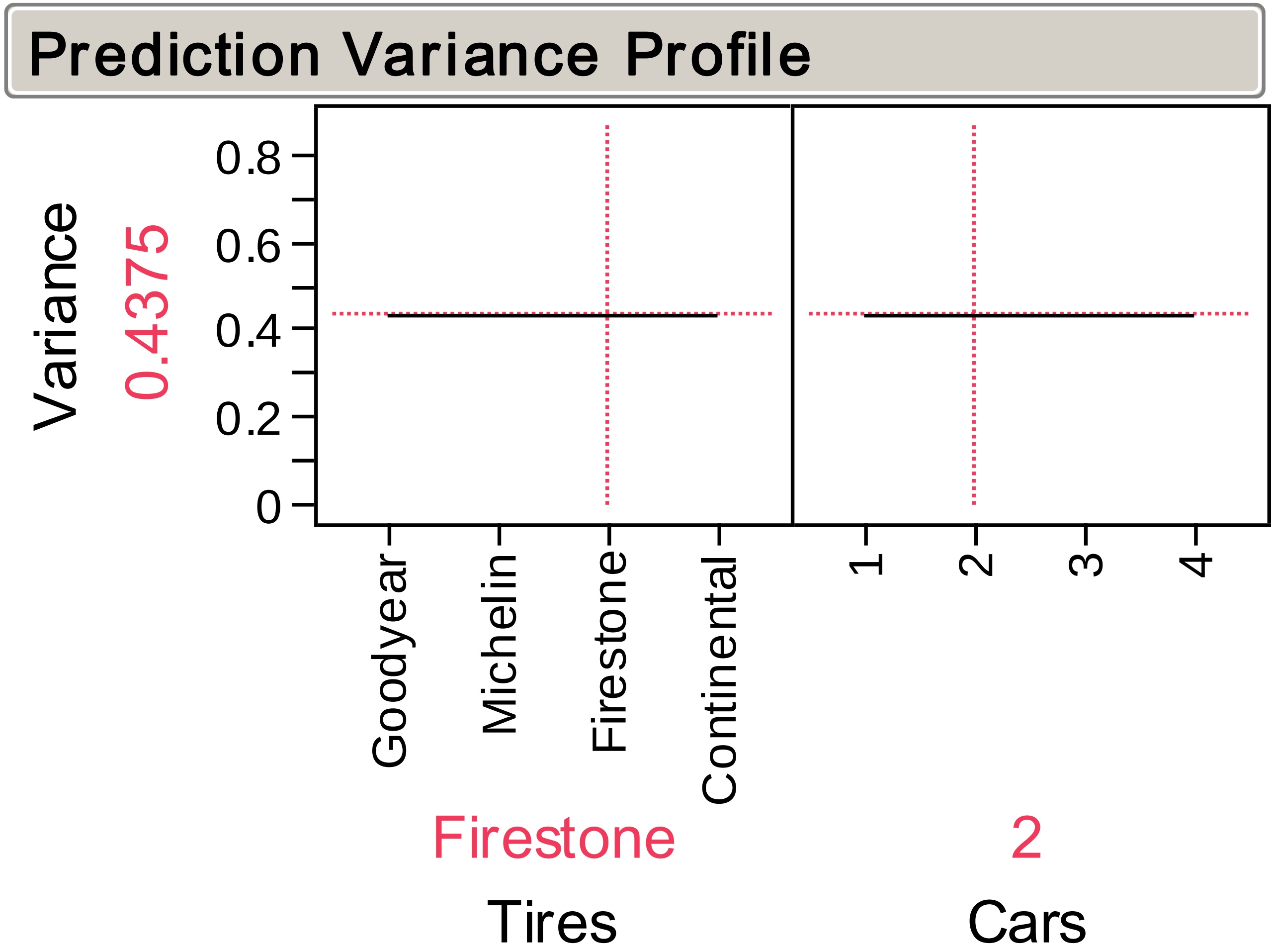
An example of blocking

- The tire mileage experiment
- Four brands of tires (Firestone, Goodyear, Continental, Michelin)
- Do the tires differ with respect to mean mileage performance?
- Suppose that we have four cars available for the experiment
- Let’s consider some possible designs

The randomized complete block design (RCBD) – using an optimal design tool

- Every block contains a complete replicate of the experiment (all treatment combinations)
- Blocks are orthogonal to treatments
- This design completely removes the block effects from the treatment comparisons
- What about our “bad assumptions” about the wheel positions?

JMP Demo


Prediction Variance Profile
Variance
0.4375
0.8
0.6
0.4
0.2
0
Goodyear
Michelin
Firestone
Continental
1
2
3
4
Firestone
2
Tires
Cars

The Latin Square Design – Using an Optimal Design Tool

Wheel Positions

Cars

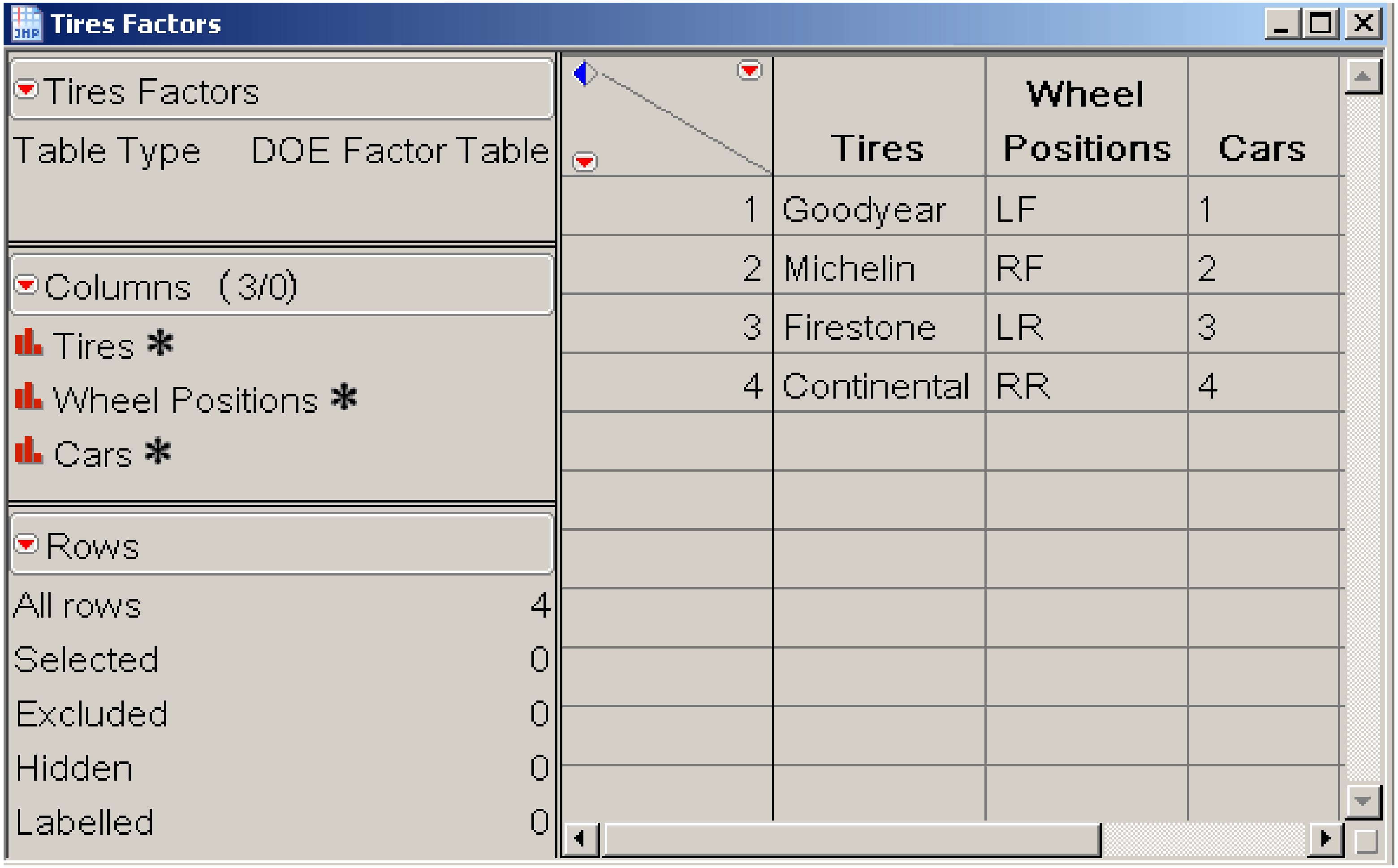
	RF	RR	LF	LR
1	F	G	C	M
2	M	F	G	C
3	C	M	F	G
4	G	C	M	F

What to do if you think wheel position could also matter.

The Latin Square Design

- This is also an orthogonal design
- The effects of both nuisance factors are balanced out
- The Latin square is actually a fractional factorial, a 4^{3-1}
- But we can find this design with an optimal design tool.

JMP Demo


Tires Factors
Tires Factors
Table Type DOE Factor Table
Columns (3/0)
Tires *
Wheel Positions *
Cars *
Rows
All rows 4
Selected 0
Excluded 0
Hidden 0
Labelled 0
Tires
Wheel Positions
Cars
1 Goodyear LF 1
2 Michelin RF 2
3 Firestone LR 3
4 Continental RR 4

Another Example of a Latin Square

- The Latin square design can be used with more complex treatment structures.
- The radar experiment (DOX 7E, DCM, 2009)
  - Two different filters
  - Three different levels of ground clutter
  - Response variable – intensity level at detection
  - Nuisance variable (1) operators
  - Nuisance variable (2) we can only run 6 tests per day

Another Example of a Latin Square

- The treatment structure is a factorial; 2 levels of one factor and 3 levels of another.
- Each replicate requires 2 x 3 = 6 runs.
- The Latin square design will require 6 operators (easy to do; there are lots of operators)
- Six test days will be required

Treatments for the 6 x 6 Latin square:

A = f1g1

B = f1g2

C = f1g3

D = f2g1

E = f2g2

F = f2g3

where fi = filter type i, g1 = ground clutter low, g2 = ground clutter medium and g3 = ground clutter high

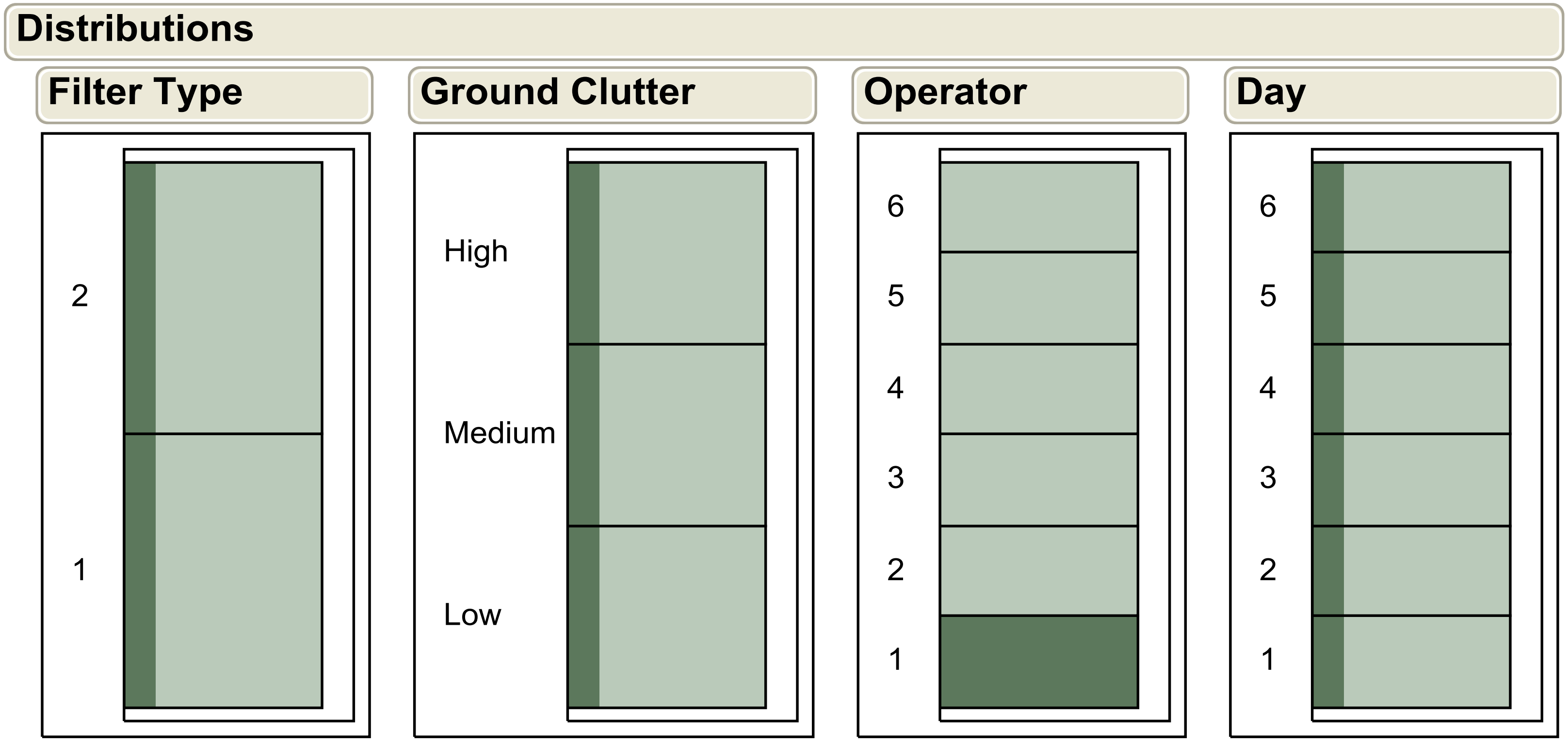
■ **TABLE 5.23**

Radar Detection Experiment Run in a 6 × 6 Latin Square

	Operator					
Day	**1**	**2**	**3**	**4**	**5**	**6**
1	$A(f_1g_1 = 90)$	$B(f_1g_2 = 106)$	$C(f_1g_3 = 108)$	$D(f_2g_1 = 81)$	$F(f_2g_3 = 90)$	$E(f_2g_2 = 88)$
2	$C(f_1g_3 = 114)$	$A(f_1g_1 = 96)$	$B(f_1g_2 = 105)$	$F(f_2g_3 = 83)$	$E(f_2g_2 = 86)$	$D(f_2g_1 = 84)$
3	$B(f_1g_2 = 102)$	$E(f_2g_2 = 90)$	$G(f_2g_3 = 95)$	$A(f_1g_1 = 92)$	$D(f_2g_1 = 85)$	$C(f_1g_3 = 104)$
4	$E(f_2g_2 = 87)$	$D(f_2g_1 = 84)$	$A(f_1g_1 = 100)$	$B(f_1g_2 = 96)$	$C(f_1g_3 = 110)$	$F(f_2g_3 = 91)$
5	$F(f_2g_3 = 93)$	$C(f_1g_3 = 112)$	$D(f_2g_1 = 92)$	$E(f_2g_2 = 80)$	$A(f_1g_1 = 90)$	$B(f_1g_2 = 98)$
6	$D(f_2g_1 = 86)$	$F(f_2g_3 = 91)$	$E(f_2g_2 = 97)$	$C(f_1g_3 = 98)$	$B(f_1g_2 = 100)$	$A(f_1g_1 = 92)$

In general, if there are *p* treatment combinations in the factorial design, a *p x p* Latin square will be required to handle the two nuisance factors

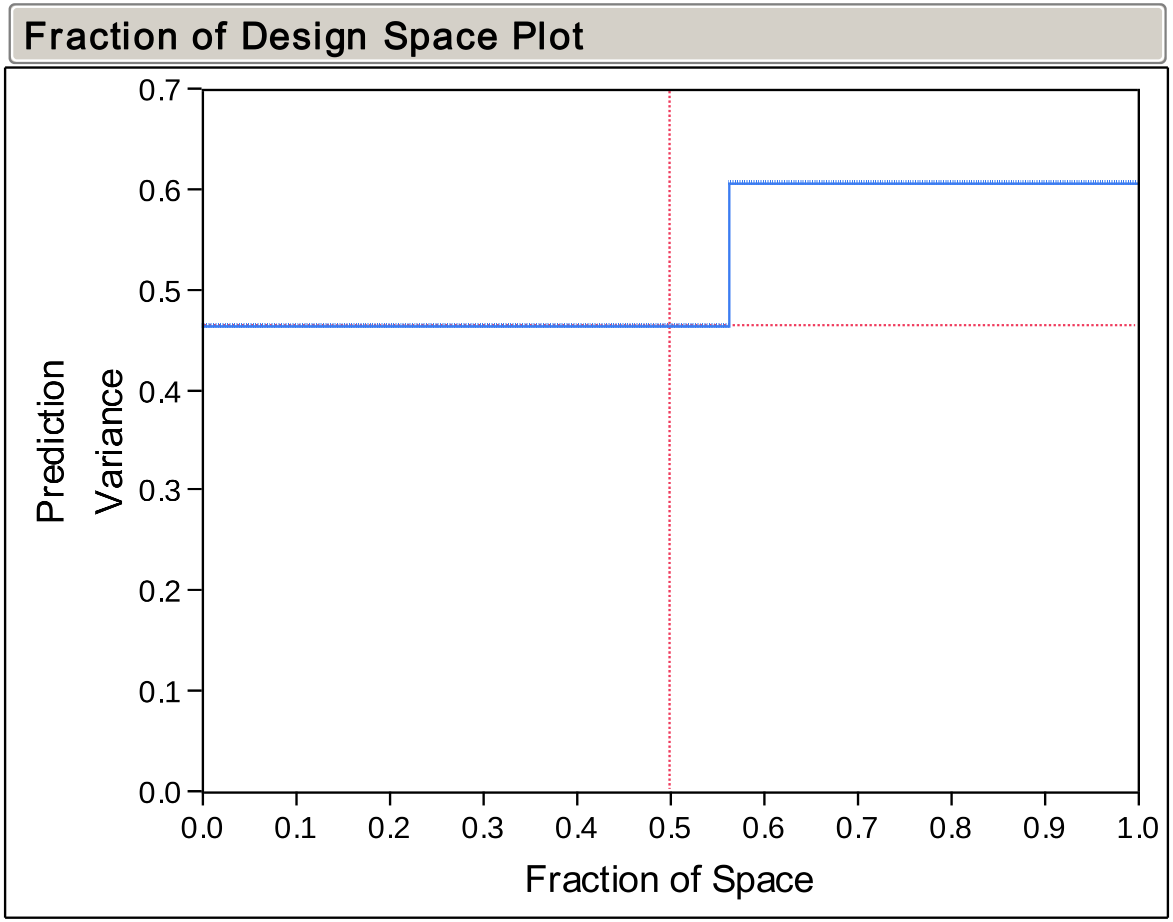
JMP Demo


Distributions
Filter Type
2
1
Ground Clutter
High
Medium
Low
Operator
6
5
4
3
2
1
Day
6
5
4
3
2
1

Back to tire testing

- Suppose that we have more tire brands, say seven brands
- What do we do now?
- Can we find cars with seven wheel positions?
- Balanced incomplete block designs
- Widely used in agricultural experiments

JMP Demo



Fraction of Design Space Plot
Prediction Variance
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0.0
0.0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1.0
Fraction of Space

Module 4 – Summary

- Most design problems have factors that are ripe for use as blocking variables.
- Ignoring these variables can make it hard to detect the real effects of the control factors due to the inflation of the error variance from the effect of the blocking factor.
- Traditional blocking structures are also optimal.
- These structures can be reproduced using optimal design algorithms.
- However, these algorithms also work in situations where non-standard block and/or sample sizes are required.

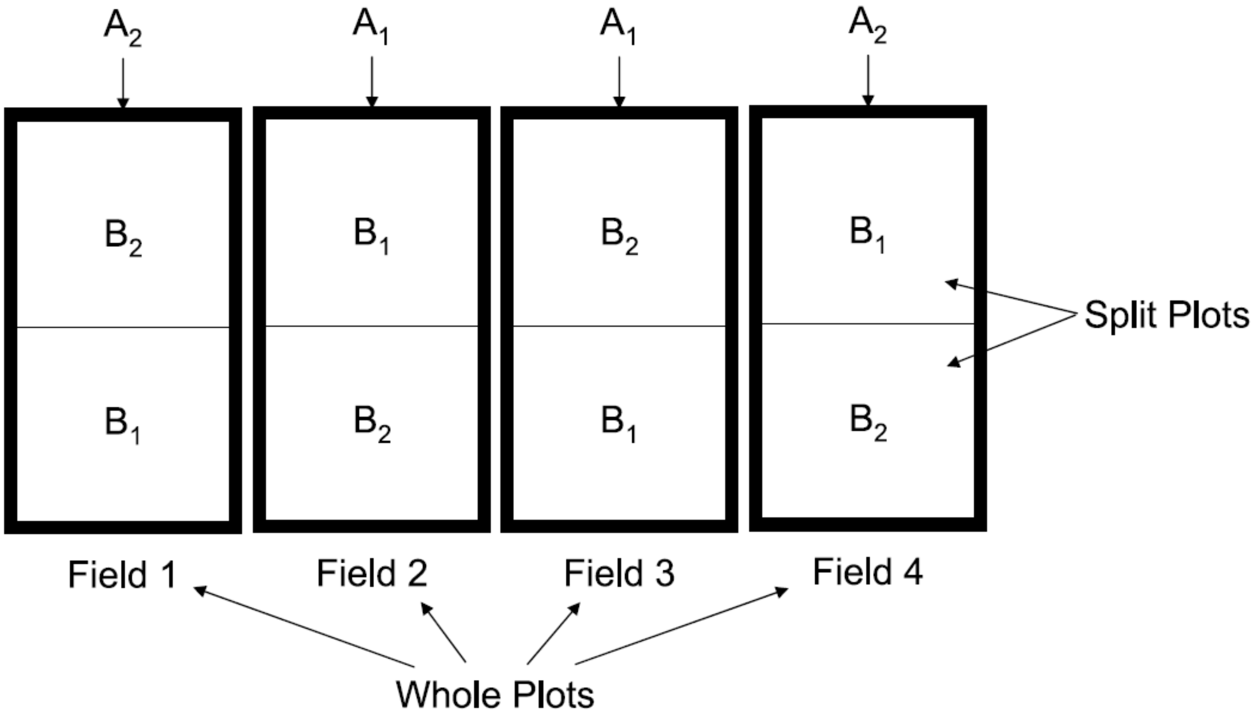
DOE Course – Module 5

Designed Split-plot Experiments

Goals

1. Introduce the idea behind split-plot experiments.
2. Develop a model for the design of split-plot experiments.
3. Compare random blocked to split-plot experiments.
4. Provide an example of a split-plot experiment.

Split-plot Graphic Definition



Split-plot Definition

A split-plot experiment is a blocked experiment, where the blocks themselves serve as experimental units for a subset of the factors.

Jones, B. and Nachtsheim, C. (2009) “Split-plot Designs: What, Why and How” *Journal of Quality Technology*, Vol. 41 #4

Model for Split-plot Experiments

$$\boldsymbol{Y} = \mathbf{X}\boldsymbol{\beta} + \mathbf{Z}\boldsymbol{\gamma} + \boldsymbol{\varepsilon}$$

Estimator for β $\quad \hat{\boldsymbol{\beta}} = (\mathbf{X}'\mathbf{V}^{-1}\mathbf{X})^{-1}\mathbf{X}'\mathbf{V}^{-1}\boldsymbol{Y}$

$$\mathbf{Z} = \begin{bmatrix} 1 & 0 & \cdots & 0 \\ \vdots & \vdots & \ddots & \vdots \\ 1 & 0 & \cdots & 0 \\ 0 & 1 & \cdots & 0 \\ \vdots & \vdots & \ddots & \vdots \\ 0 & 1 & \cdots & 0 \\ \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & \cdots & 1 \\ \vdots & \vdots & \ddots & \vdots \\ 0 & 0 & \cdots & 1 \end{bmatrix}$$

$$\begin{aligned} \mathbf{V} &= \mathrm{var}(\mathbf{Z}\boldsymbol{\gamma} + \boldsymbol{\varepsilon}) \\ &= \mathrm{var}(\mathbf{Z}\boldsymbol{\gamma}) + \mathrm{var}(\boldsymbol{\varepsilon}), \\ &= \mathbf{Z}[\mathrm{var}(\boldsymbol{\gamma})]\mathbf{Z}' + \mathrm{var}(\boldsymbol{\varepsilon}), \\ &= \mathbf{Z}[\sigma_\gamma^2 \mathbf{I}_b]\mathbf{Z}' + \sigma^2 \mathbf{I}_n, \\ &= \sigma_\gamma^2 \mathbf{Z}\mathbf{Z}' + \sigma^2 \mathbf{I}_n, \end{aligned}$$

Split-plot versus Random Blocks

1. Split-Plot Designs are a special case of Random Block design.
2. The difference is that in split-plot designs, certain factors (the “whole plot” factors) do not change within the blocks but only between blocks.
3. In ordinary random block designs, all the factors may change within each block.

Split-plot Design Set Up

General procedure

1. Specify the number of whole plots, n_w.
2. Specify the number of split plots per whole plot, n_s.
3. Specify the response model, $f(w, s)$.
4. Specify the prior estimate for d
5. Use computer software to construct the design for (1) through (4) that maximizes the D-optimality criterion.
6. Study the sensitivity of the optimal design to small changes in d, n_w, and n_s.

Split-plot Design Objective Functions

D-optimality Criterion

$$C_D(\mathbf{X}, d) = \left|\frac{\mathbf{X}'\mathbf{V}^{-1}\mathbf{X}}{N}\right|$$

I-optimality Criterion

$$C_I(\mathbf{X}, d) = \frac{N}{\int_R d\mathbf{x}} \int_{\mathbf{R}} f'(\mathbf{x})[\mathbf{X}'\mathbf{V}^{-1}\mathbf{X}]^{-1} f(\mathbf{x}) d\mathbf{x}$$

Split-Plot Example

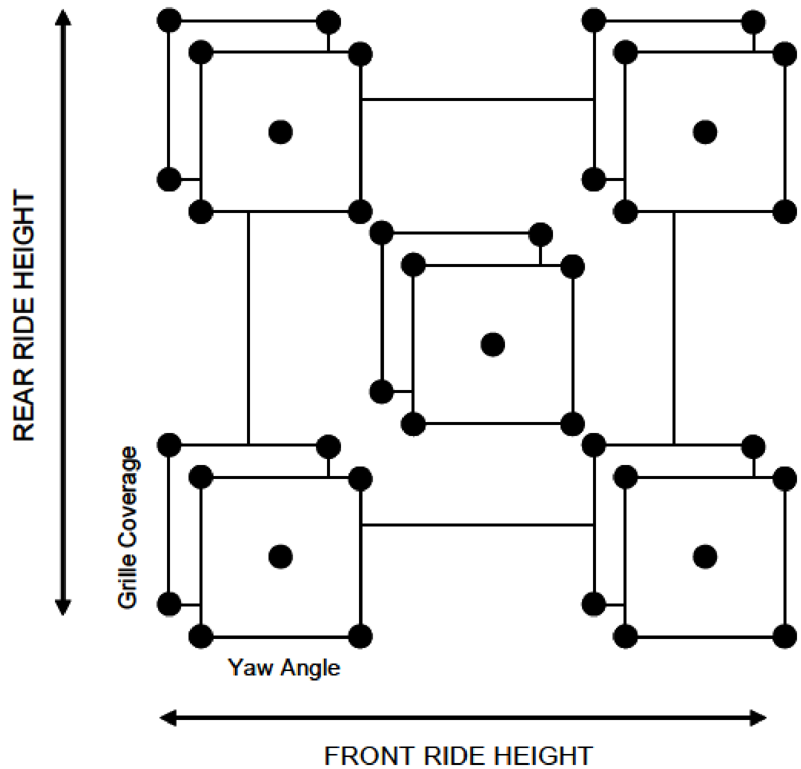
Scenario

1. Four factors
2. Two are hard-to-change and two are easy-to-change
3. Hard-to-change factor design can only have 10 runs.
4. Budget of 50 runs for the full design.

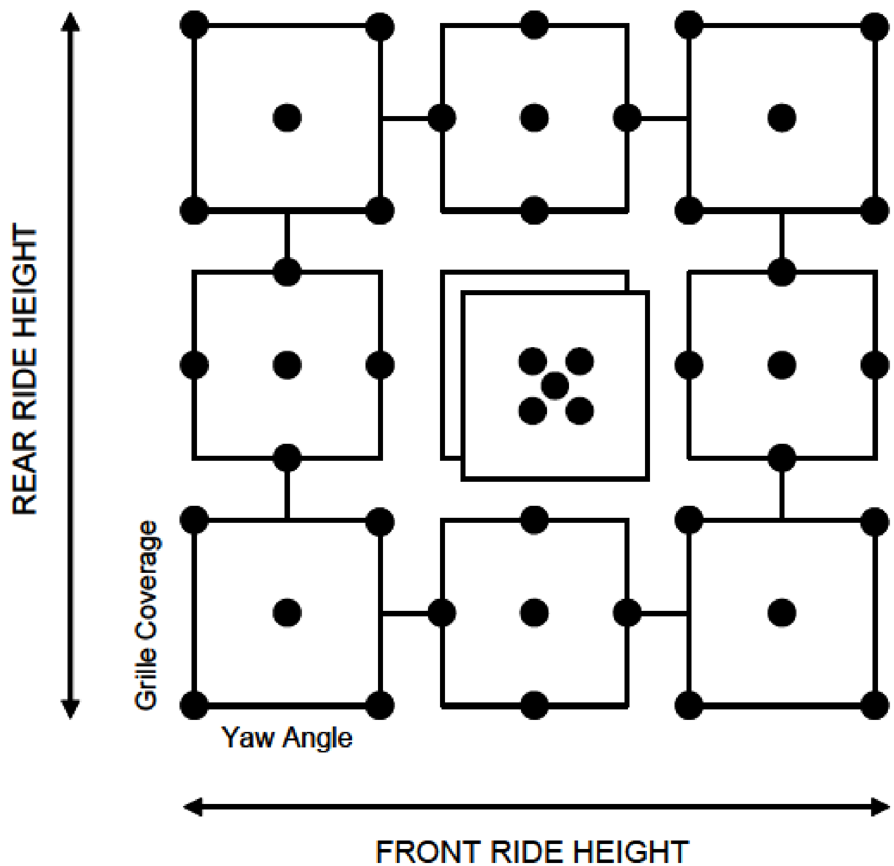
Factor Table

Factor (label)	Type	Low level	Center level	High level
Front ride height (FRH)	Hard-to-change	3.0 in	3.5 in	4.0 in
Rear ride height (RRH)	Hard-to-change	34 in	35 in	36 in
Yaw angle (yaw)	Easy-to-change	-3.0°	-1.0°	+1.0°
Grille tape coverage (tape)	Easy-to-change	0%	50%	100%

Ad hoc Design #1



Ad hoc Design #2



I-optimal Split-Plot Design

Hard-to-Change Factor Setting	Front Ride Height	Rear Ride Height	Yaw Angle	Grille Coverage
1	−1	−1	−1	−1
1	−1	−1	1	1
1	−1	−1	0	0
1	−1	−1	1	−1
1	−1	−1	−1	1
2	0	−1	1	1
2	0	−1	0	0
2	0	−1	1	−1
2	0	−1	0	1
2	0	−1	−1	0
3	1	−1	1	−1
3	1	−1	1	1
3	1	−1	−1	−1
3	1	−1	−1	1
3	1	−1	0	0
4	−1	0	−1	0
4	−1	0	0	1
4	−1	0	1	0
4	−1	0	0	−1
4	−1	0	−1	1
5	0	0	0	−1
5	0	0	1	0
5	0	0	0	0
5	0	0	−1	1
5	0	0	0	0
6	0	0	1	0
6	0	0	0	0
6	0	0	0	0
6	0	0	0	0
6	0	0	−1	−1
7	1	0	0	−1
7	1	0	1	1
7	1	0	0	0
7	1	0	0	1
7	1	0	−1	0
8	−1	1	−1	−1
8	−1	1	1	−1
8	−1	1	0	0
8	−1	1	1	1
8	−1	1	−1	1
9	0	1	−1	0
9	0	1	0	−1
9	0	1	0	0
9	0	1	0	1
9	0	1	1	1
10	1	1	−1	1
10	1	1	1	−1
10	1	1	−1	−1
10	1	1	1	0
10	1	1	0	1

Comparison of Coefficient Variances

Intercept	0.429	0.454
Front Ride Height (FRH)	0.200	0.200
Rear Ride Height (RRH)	0.200	0.200
Yaw Angle	0.042	0.032
Grille Coverage	0.042	0.032
FRH * RRH	0.300	0.300
FRH * Yaw Angle	0.050	0.046
FRH * Grill Coverage	0.050	0.046
RRH * Yaw Angle	0.050	0.046
RRH * Grille Coverage	0.050	0.046
Yaw Angle * Grille Coverage	0.063	0.042
FRH * FRH	0.554	0.523
RRH * RRH	0.554	0.523
Yaw Angle * Yaw Angle	0.125	0.102
Grille Coverage * Grille Coverage	0.125	0.102
Average (Including Intercept)	0.189	0.180
Average (Excluding Intercept)	0.172	0.160

Left column is for ad hoc design #2, right column is for I-optimal split-plot design.

OLS vs GLS Data Analysis

Effect	Estimate	St. Error	D.F.	t Ratio	p Value
β_0	0.9014	0.0046	42	194.83	0.0000
β_1	−0.0607	0.0038	42	−16.05	0.0000
β_2	0.0529	0.0038	42	14.04	0.0000
β_3	−0.0237	0.0038	42	−6.28	0.0000
β_4	0.0756	0.0037	42	20.32	0.0000
β_{22}	0.0241	0.0060	42	4.03	0.0002
β_{13}	0.0097	0.0045	42	2.15	0.0374
β_{14}	−0.0103	0.0044	42	−2.34	0.0244

The indices 1, 2 and 3 in the first column of the table refer to the front ride height, the rear ride height, the yaw angle and the grille coverage, respectively.

OLS Analysis

Effect	Estimate	St. Error	D.F.	t Ratio	p Value
β_0	0.9160	0.0068	6.99	135.38	0.0000
β_1	−0.0607	0.0087	6.99	−6.94	0.0002
β_2	0.0524	0.0087	6.99	5.99	0.0005
β_3	−0.0246	0.0028	35.07	−8.82	0.0000
β_4	0.0743	0.0028	35.18	26.85	0.0000
β_{13}	0.0102	0.0033	35.03	3.08	0.0040
β_{14}	−0.0107	0.0033	35.07	−3.29	0.0023
β_{24}	0.0078	0.0033	35.08	2.39	0.0226

The indices 1, 2 and 3 in the first column of the table refer to the front ride height, the rear ride height, the yaw angle and the grille coverage, respectively.

GLS Analysis

Module 5 – Summary

1. Split-plot designs are common in industry.
2. They are not commonly recognized as being split-plot designs.
3. As a result, these designs are mistakenly analyzed using OLS.
4. Explicitly, taking randomization restrictions into account makes the design process more economical, often more statistically efficient and more likely to produce valid analytical results.

Module 6 – Introduction to RSM

- Define RSM
- Introduce the standard RSM model
- Illustrate coordinate exchange algorithm

The Response Surface Framework for Industrial Experimentation

- Response Surface Methods (RSM) are a collection of mathematical and statistical design/model building techniques useful for developing, improving, and optimizing systems

- RSM employs a sequential strategy to explore the relationship between the response variables of interest and the independent variables in the process

- RSM dates from the late 1940s

- Mechanistic Models versus Empirical Models

- The response surface and the associated contour plot - refer to Figure 1.1, pg. 2 (RSM 2009, Myers, Montgomery & Anderson-Cook)

$$E(y) = f(\xi_1, \xi_2)$$

The response surface

The contour plot

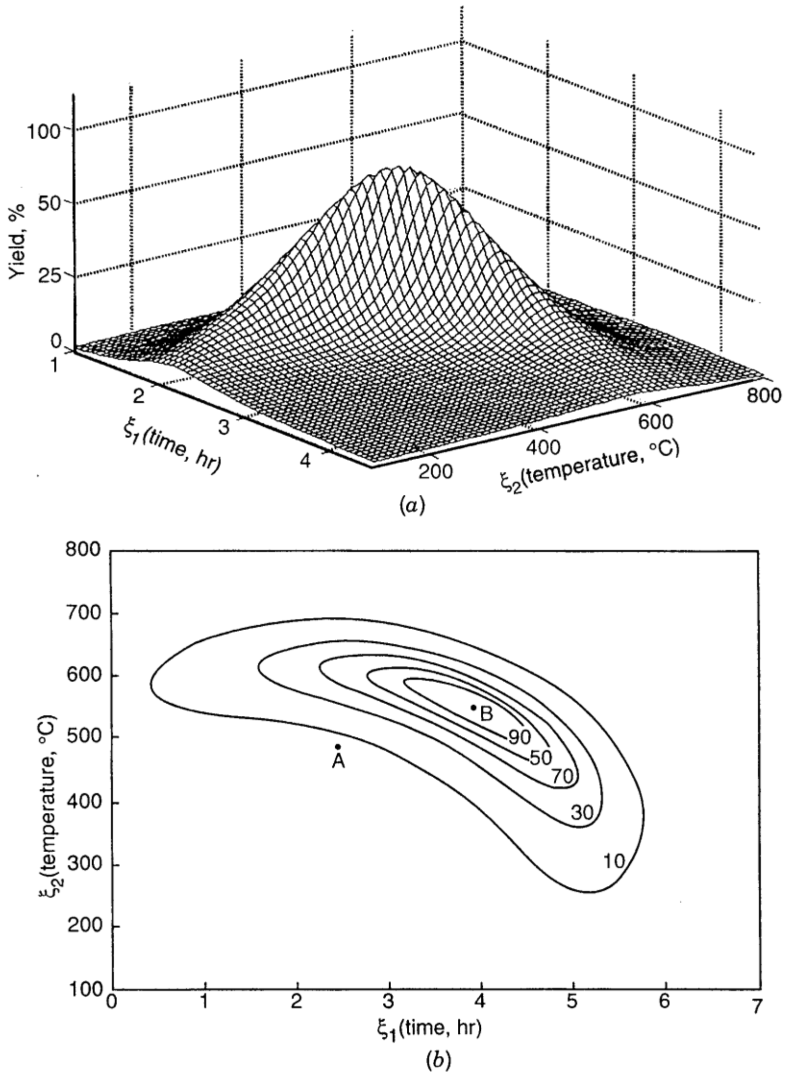


Figure 1.1 (*a*) A theoretical response surface showing the relationship between yield of a chemical process and the process variables reaction time (ξ_1) and reaction temperature (ξ_2). (*b*) A contour plot of the theoretical response surface.

Response Surface Methodology

- The physical mechanism is almost always unknown and must be approximated, usually with a low-order polynomial
- Polynomial approximation:
  - first-order model $$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \varepsilon$$
  - second-order model

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_{12} x_1 x_2 + \beta_{11} x_1^2 + \beta_{22} x_2^2 + \varepsilon$$

- Once the approximating model is fit, optimum conditions are determined

Response Surface Methodology

- Why do we use second-order models in RSM?
  - They are flexible
  - It is easy to estimate the parameters
  - There is a lot of empirical evidence that they work
- Philosophy of using low-order polynomials is based on a Taylor series analogy

Confidence Intervals (Page 36)

CI on individual model parameter:

$$b_j - t_{\alpha/2,n-p}\,se(b_j) \le \beta_j \le b_j + t_{\alpha/2,n-p}\,se(b_j)$$

Joint confidence region on model parameters:

$$\frac{(\mathbf{b}-\boldsymbol{\beta})'\mathbf{X}'\mathbf{X}(\mathbf{b}-\boldsymbol{\beta})}{pMS_E} \le F_{\alpha,p,n-p}$$

Elliptically-shaped region

Tricky to construct

Conceptually very useful

CI on the mean response at a point of interest:

$$\mathbf{x}_0' = [1, x_{01}, x_{02}, \cdots, x_{0k}]$$

Point of interest – not necessarily a design point

$$\hat{y}(\mathbf{x}_0) = \mathbf{x}_0'\mathbf{b}$$

Estimate (unbiased) of the mean response at the point of interest

$$V[\hat{y}(\mathbf{x}_0)] = \sigma^2 \mathbf{x}_0'(\mathbf{X}'\mathbf{X})^{-1}\mathbf{x}_0$$

Variance of the mean response at the point of interest

The CI is:

$$\hat{y}(\mathbf{x}_0) - t_{\alpha/2,n-p}\sqrt{\hat{\sigma}^2 \mathbf{x}_0'(\mathbf{X}'\mathbf{X})^{-1}\mathbf{x}_0} \le \mu_{y|\mathbf{x}_0} \le \hat{y}(\mathbf{x}_0) + t_{\alpha/2,n-p}\sqrt{\hat{\sigma}^2 \mathbf{x}_0'(\mathbf{X}'\mathbf{X})^{-1}\mathbf{x}_0}$$

Mean response at the point of interest

The Sequential Nature of RSM

Phases of an RSM Study:

- Factor screening (phase zero)
- Seeking the region of the optimum (phase 1)
- Determination of optimum conditions (phase 2)

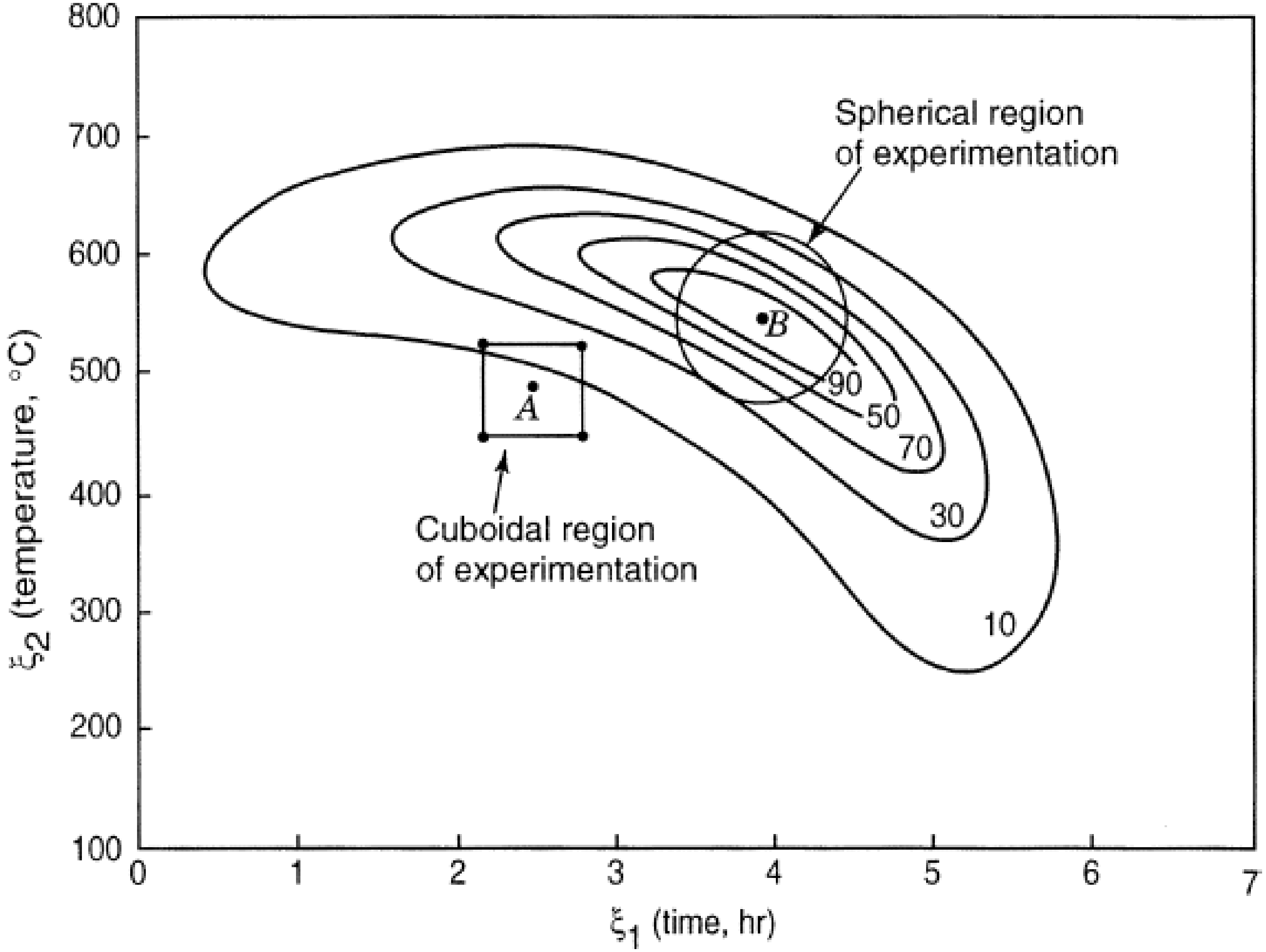


Figure 1.6 The region of operability and the region of experimentation.

Three Typical Applications of RSM

- **Mapping** a response surface over a region of interest
- **Optimization** of the response
- Selection of **operating conditions** to achieve specifications or customer requirements
  - May not correspond to a stationary point on the response surface
  - This often involves multiple responses

RSM Applications

- "**Classical**" RSM problem
- Product **formulation** or **mixture** problems
- "**Robust parameter design**" or RPD problem
  - How to select the parameters of a system so as to make the response insensitive to factors that are difficult to control
  - Process robustness studies

Useful References on RSM

- Box, G.E.P. and Wilson, K.B. (1951), "On the Experimental Attainment of Optimum Conditions", *Journal of the Royal Statistical Society B*, Vol. 13, pp. 1-45
- Myers, R.H., Montgomery, D.C. and Anderson-Cook (2009), *Response Surface Methodology*, 3rd edition, Wiley, NY
- Montgomery, D. C. (2009), *Design and Analysis of Experiments*, 7th edition, Wiley, NY
- Myers, R. H., Montgomery, D. C., Vining, G. G., Borror, C. M., and Kowalski, S. M. (2004), "Response Surface Methodology: A Retrospective and Literature Survey", *Journal of Quality Technology*, Vol. 36, No. 1, pp. 53-77.

Designs for the Second-Order RS Model

- The basic RSM second-order designs
  - The central composite design (CCD)
  - The Box-Behnken design (BBD)
- These designs are very useful in “standard” RSM settings
  - The region of interest is either a cube or a sphere
  - No significant restrictions on the number of runs

Categorical and Continuous Variables in RSM

- Most of the work in RSM and RSM designs assume that all design factors are continuous
- There are situations where a combination of continuous and categorical are encountered
- There are no standard designs for these situations
- Optimal designs are very appropriate here

Module 6 - Summary

- RSM is all about prediction and optimization.
- This naturally leads to minimizing the average variance of prediction as an appropriate design criterion (I-optimality)
- In many practical applications of RSM, the structure and constraints of the problem make it impossible to use traditional RSM designs. In such cases, an optimal design approach is useful.

Module 7 – RSM with Factor Constraints

Goals

1. Explain the practical need for RSM designs when there are constraints on the design factors
2. Provide an example of inequality constraints
3. Give an example for avoiding infeasible factor combinations

Situations where Standard Designs may not be Appropriate

- Constraints on the design region
- Nonstandard model

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_{12} x_1 x_2 + \beta_{11} x_1^2 + \beta_{22} x_2^2$$
$$+ \beta_{112} x_1^2 x_2 + \beta_{1112} x_1^3 x_2 + \varepsilon$$

- Unusual sample size or blocking requirements

In these situations computer-generated or "optimal" designs are useful

A problem with a constrained design region – amount of adhesive and cure temperature

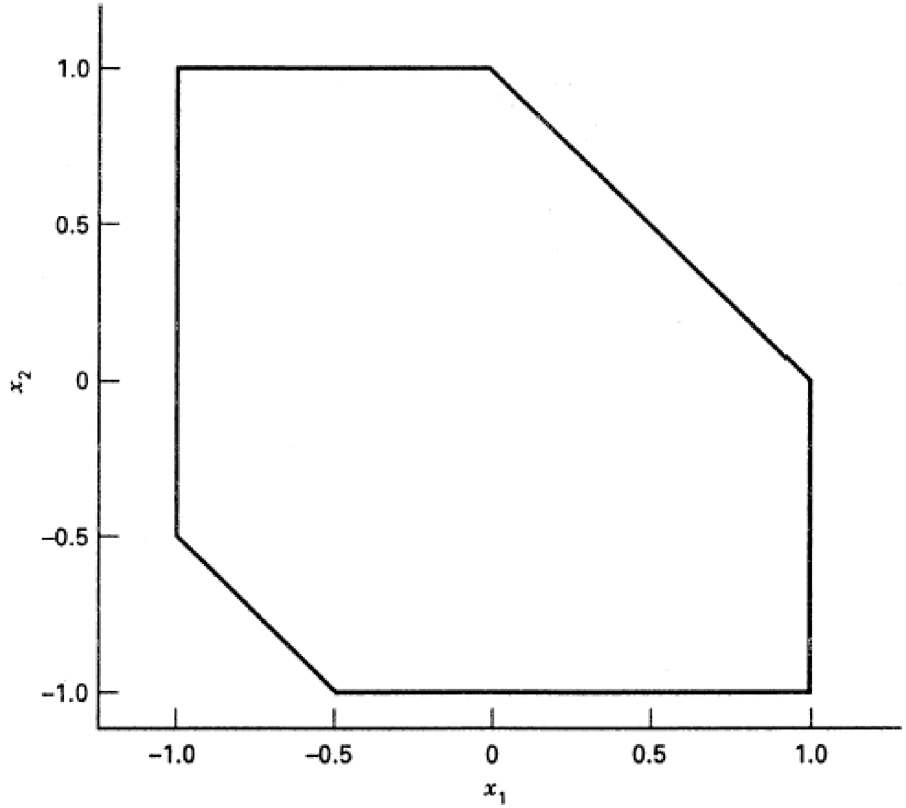


Figure 8.1 A constrained design region in two variables.

Page 391 & 392

$$-1.5 \le x_1 + x_2$$

$$x_1 + x_2 \le 1$$

How would we design an experiment for this problem?

- “Force” a standard design into the experimental region
  - May lead to a case of the “square peg and the round hole”
- Generate a unique design just for this particular situation
  - Need criteria for constructing the design
  - Computer implementation essential

JMP Default RSM Design

Custom Design

Responses

Factors

Define Factor Constraints

Add Constraint

1 X1 + 1 X2 ≥ -1.5

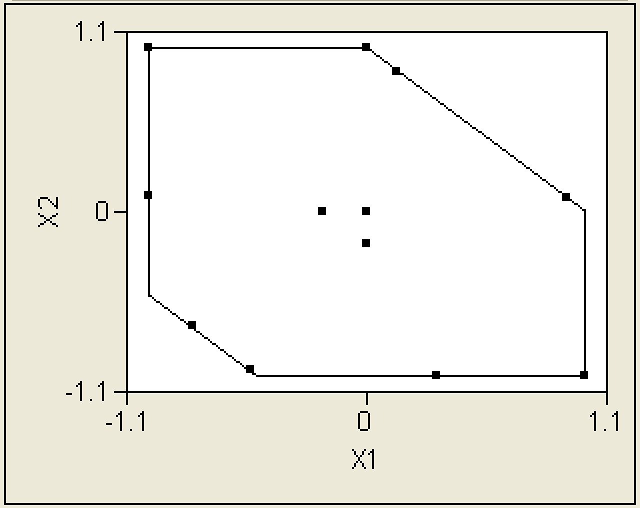
1 X1 + 1 X2 ≤ 1

Model

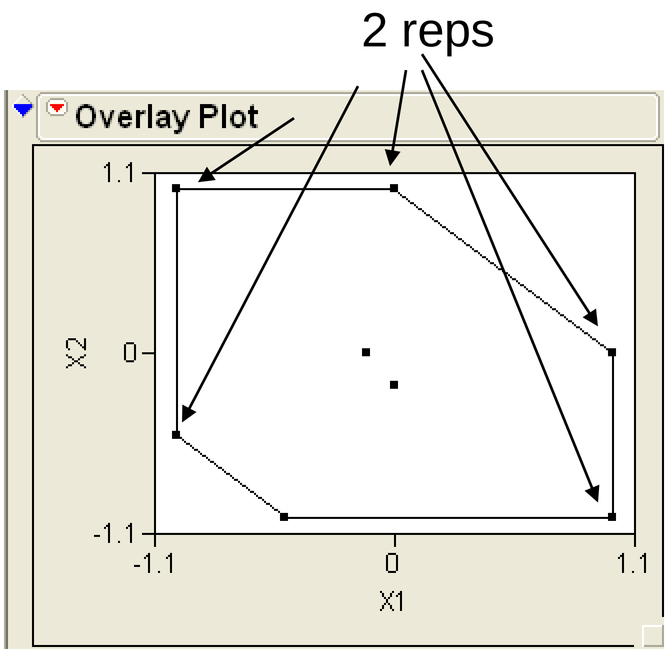
Design

Run	X1	X2	Y
1	-1	0.1	.
2	-1	1	.
3	-0.8	-0.7	.
4	-0.536	-0.964	.
5	-0.2	0	.
6	0	-0.2	.
7	0	0	.
8	0	0	.
9	0	1	.
10	0.141189	0.858811	.
11	0.32	-1	.
12	0.92	0.08	.
13	1	-1	.

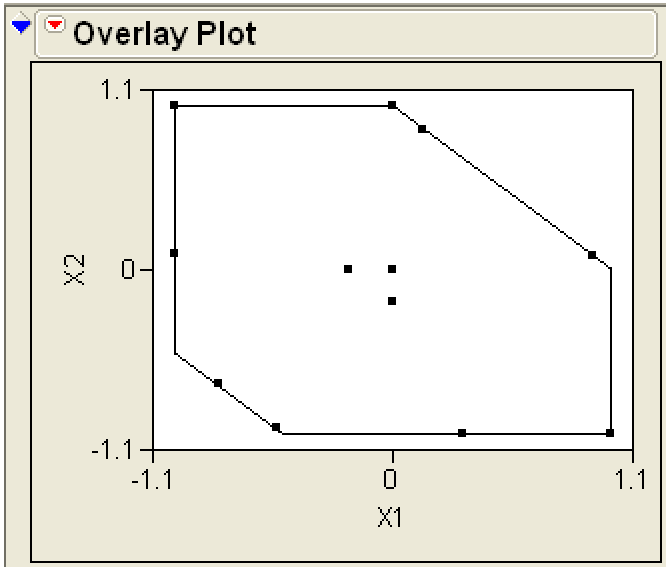
Overlay Plot



Design Comparison

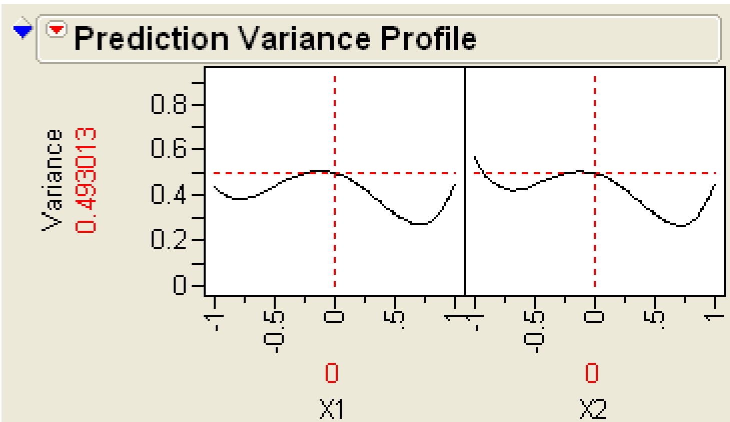


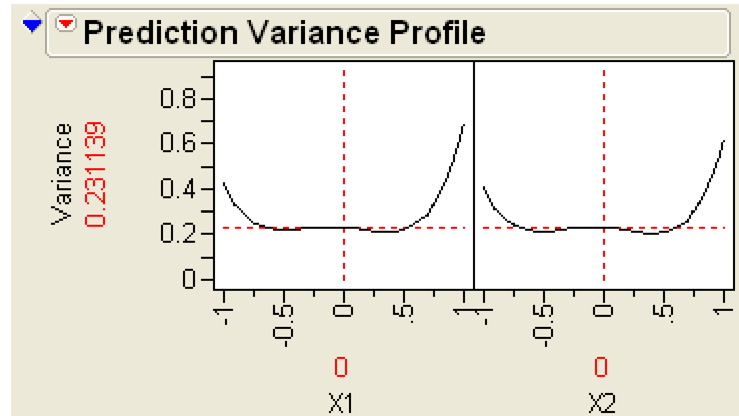
D-optimal Design Points

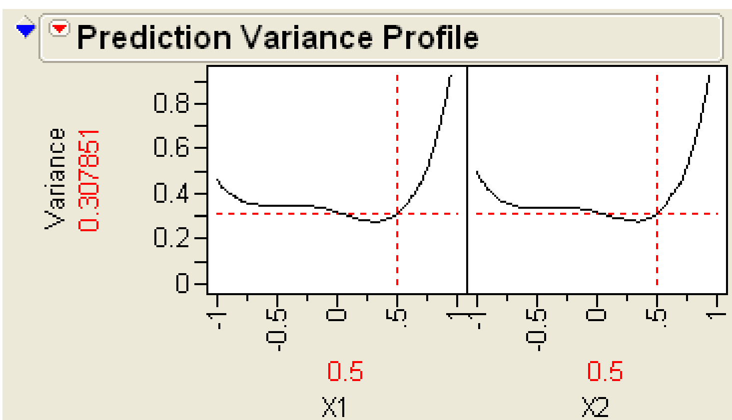


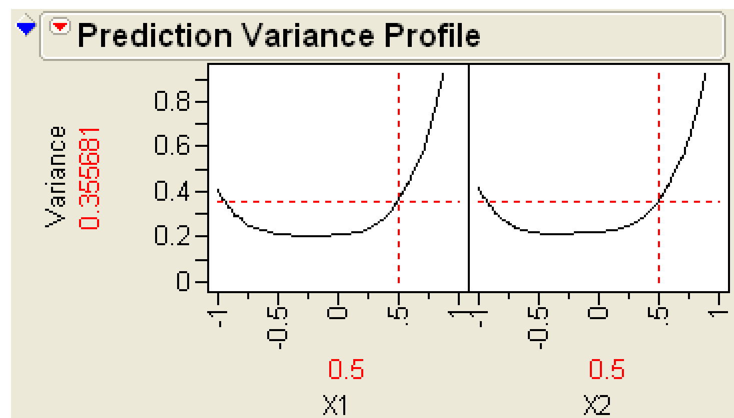
I-optimal Design Points

Design Comparison cont.









D-optimal Design Variance Profiles

I-optimal Design Variance Profile

Infeasible Factor Combinations

Especially when there are categorical factors with multiple levels, it is often the case that certain factor combinations are either infeasible or even impossible to run.

For example the Navy attack aircraft, A4, could not operate at night. The A6 was able to operate day or night. Suppose you want to run an experiment with both aircraft testing three different weapon systems under varying light conditions.

How can we accomplish this given the problem with the A4 not being able to fly at night?

Module 7 - Summary

- Constraints on design factors, unusual blocking requirements, and non-standard models are common in RSM
- Optimal designs are a logical way to solve these problems.
- Objective is to use a design that is customized to the specific problem

Module 8 – Robust Design

- Goals
  - Introduce the robust design problem
  - Illustrate control factor and noise factors
  - Show how to model the variability transmitted from noise factors
  - Illustrate how to achieve robustness – trading off mean performance and transmitted variance

Robust Parameter Design and Process Robustness Studies

- Origins of the RPD problem
- Taguchi and the American Supplier Institute
- RPD – proper choice of controllable factors to achieve robustness, or insensitivity to changes in uncontrollable noise variables
  - Control factors
  - Noise factors – these are factors that are uncontrollable in the system but controllable for purposes of a test
- An RPD problem in a manufacturing process is often called a process robustness study

Example of Noise Factors

Table 11.1 Some Examples of Control Variables and Noise Variables

Application	Control Variables	Noise Variables
Development of a cake mix	Amount of sugar, starch, and other ingredients	Oven temperature, baking time, amount of milk added
Development of a gasoline	Ingredients in the blend; other processing conditions	Type of driver, driving conditions, changes in engine type
Development of a tobacco product	Ingredient types and concentrations; other processing conditions	Moisture conditions; storage conditions on tobacco
Large-scale chemical process	Processing conditions, including nominal ambiant temperature	Deviations from nominal ambient temperature; deviations from other processing conditions
Production of a box-filling machine for filling boxes of detergent	Surface area; geometry of the machine (rectangular, circular)	Particle size of detergent
Manufacturing a dry detergent	Chemical formulation, processing variables	Temperature and relative humidity during manufacture

The Response Surface Approach

Section 11.4, page 552

Table 11.5 Experimental Data in a Crossed Array

Inner Array		Outer Array		Response Means
A	B	$C = -1$	$+1$	
-1	-1	11	15	13.0
-1	1	7	8	7.5
1	-1	10	26	18.0
1	1	10	14	12.0

Importance of the control-by-noise factor interactions

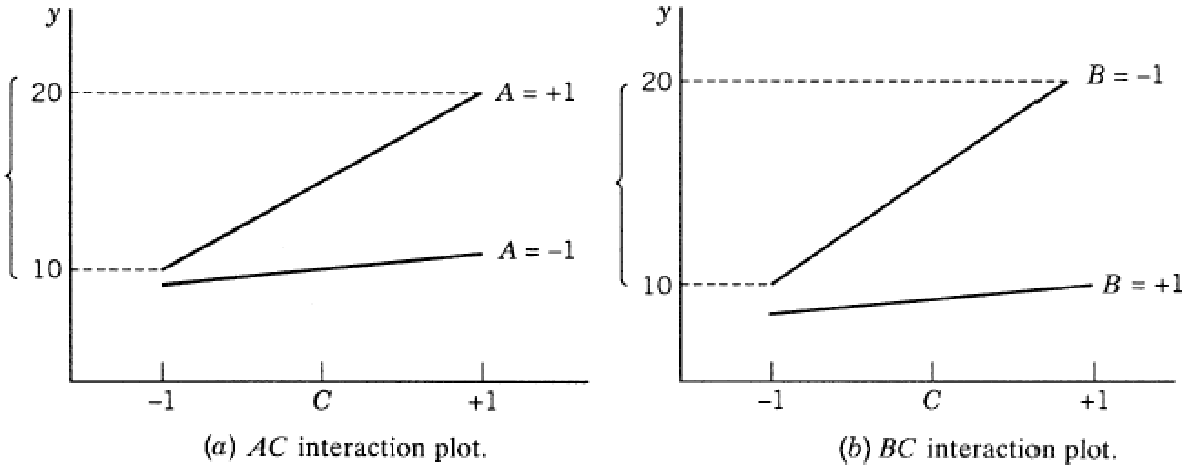


Figure 11.7 Interaction plots for the data in Table 11.5.

Both factors *A* and *B* have dispersion effects and location effects

A Modeling Approach that Includes both Control Variables and Noise Variables

Example 11.3 The Pilot Plant Experiment

Consider the experiment described in Example 3.2, where a 2^4 factorial design was used to study the filtration rate of a chemical product. The four factors are temperature, pressure, concentration of formaldehyde, and stirring rate. Each factor is present at two levels. The design matrix and the response data obtained from a single replicate of the 2^4 experiment are repeated for convenience in Table 11.7. The 16 runs are made in random order.

Temperature, z, is the noise variable

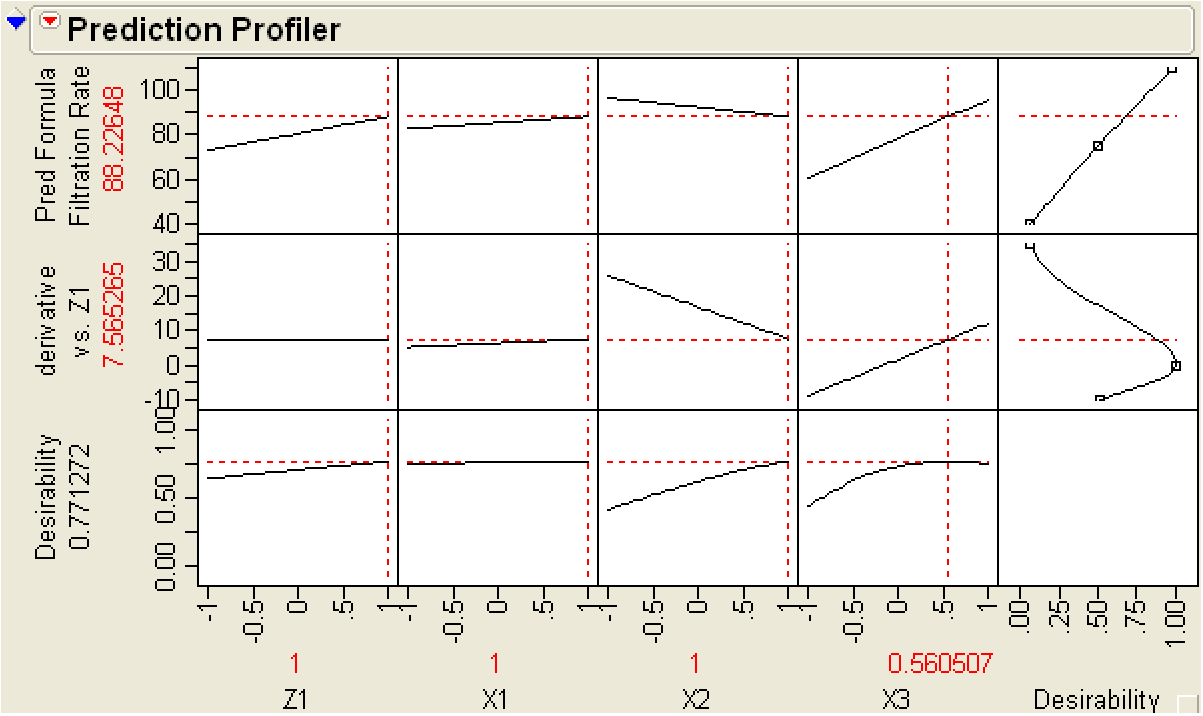
Table 11.7 Pilot Plant Filtration Rate Experiment

Run Number	Factor z_1	Factor x_1	Factor x_2	Factor x_3	Filtration Rate (gal/hr)
1	−	−	−	−	45
2	+	−	−	−	71
3	−	+	−	−	48
4	+	+	−	−	65
5	−	−	+	−	68
6	+	−	+	−	60
7	−	+	+	−	80
8	+	+	+	−	65
9	−	−	−	+	43
10	+	−	−	+	100
11	−	+	−	+	45
12	+	+	−	+	104
13	−	−	+	+	75
14	+	−	+	+	86
15	−	+	+	+	70
16	+	+	+	+	96

Combined array design

The x's are the control variables

Filtration Robust Processing Example

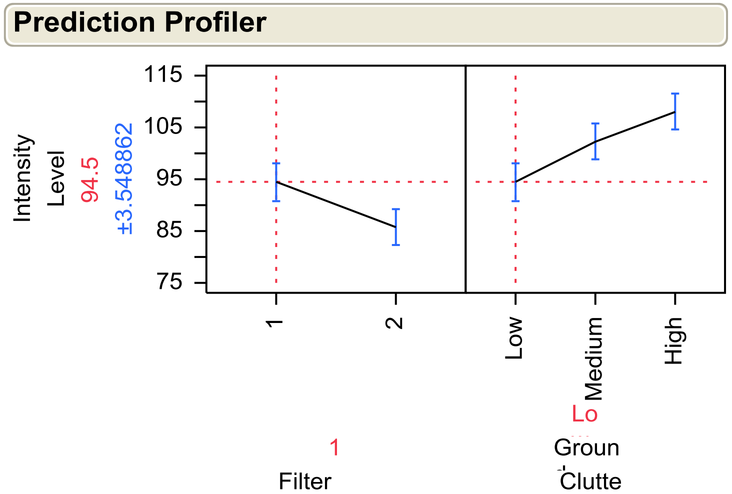


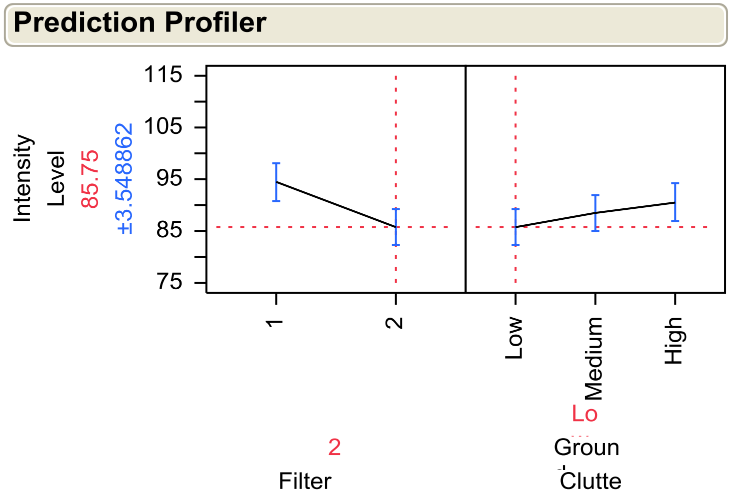
Radar Experiment

- Factors
  1. Filter Type
  2. Ground Clutter
  3. Operator

  The last two factors are noise factors…

JMP Demo


Prediction Profiler
Intensity Level
94.5
±3.548862
115
105
95
85
75
1
2
Low
Medium
High
1
Filter
Lo
Groun
Clutte


Prediction Profiler
Intensity Level
85.75
±3.548862
115
105
95
85
75
1
2
Low
Medium
High
2
Filter
Lo
Groun
Clutte

Module 8 - Summary

- By running an experiment that places both control and noise factors in the same design matrix we can develop a model for both the mean response and the transmitted variance
- In many cases it is possible to find settings for the control factors that reduce or even minimize the variability transmitted from the noise factors
- Optimal designs are good choices for the robust design problem
- Modern software makes this easy

Module 9 Mixture Designs

- Goals
  - Introduce mixture experiments
  - Design region for mixtures
  - Mixture models
  - Construction of mixture designs
  - Applications

Experiments with Mixtures

- A mixture experiment is a special type of response surface experiment where
  - The design factors are the components or ingredients of a mixture
  - The response depends on the proportions of the ingredients present
- The basic mixture constraint:

$$x_1 + x_2 + ... + x_q = 1$$

Mixtures occur in lots of settings

- Manufacturing – plasma etching in semiconductor manufacturing
- Product formulation
  - Paints, coatings, other industrial products
  - Personal care and commercial products
  - Pharmaceuticals
  - Food & beverages
    - Fruit juices, or finding the perfect Bordeaux blend

Mixture experiments involve a constrained region

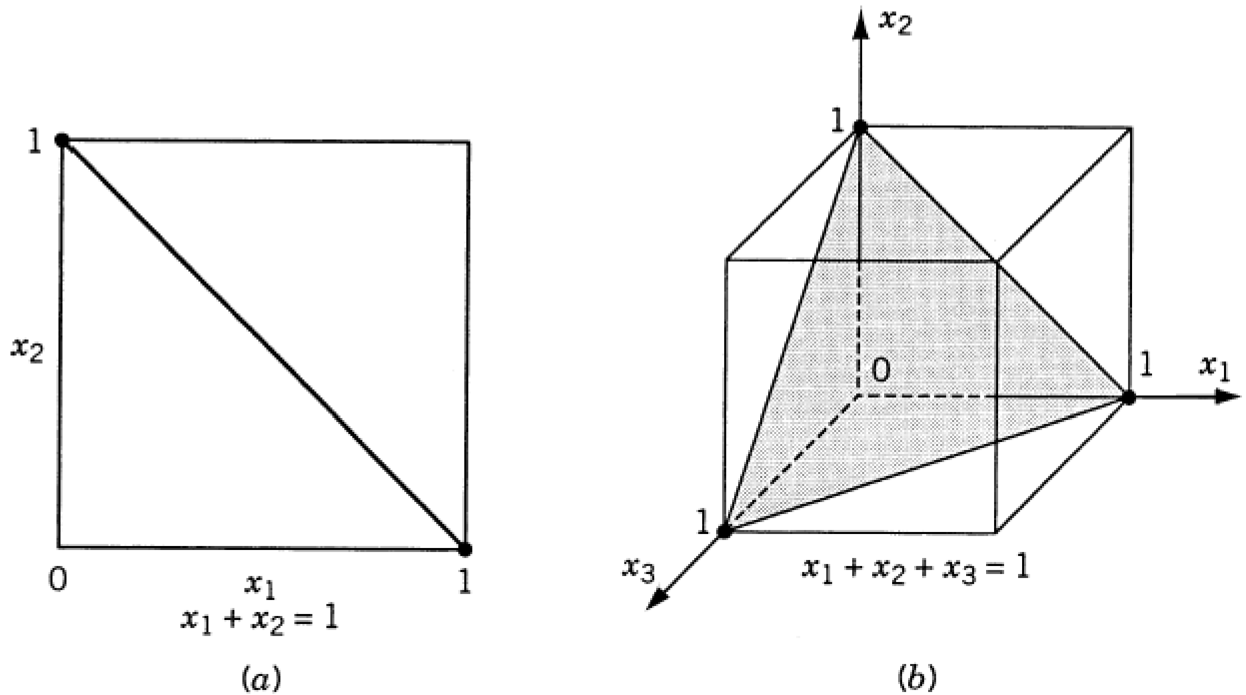
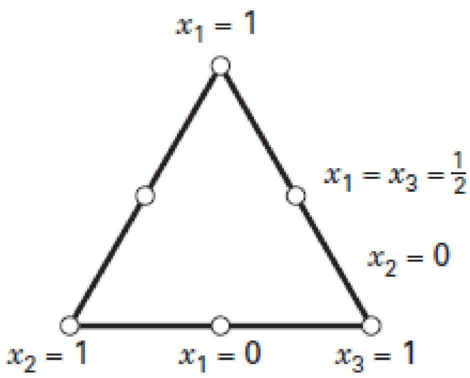
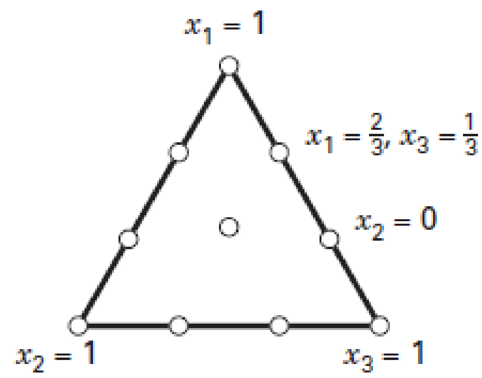


Figure 12.1 Constrained factor space for mixtures with (*a*) $q = 2$ components and (*b*) $q = 3$ components.

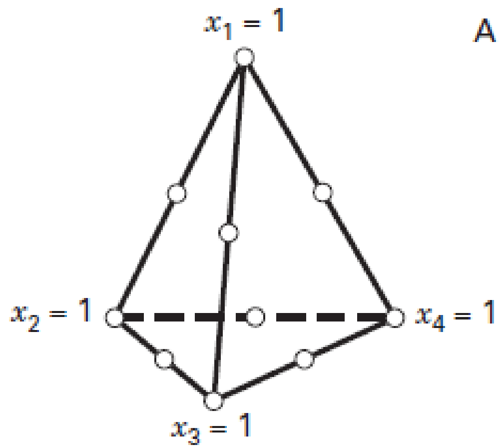
Simplex Designs



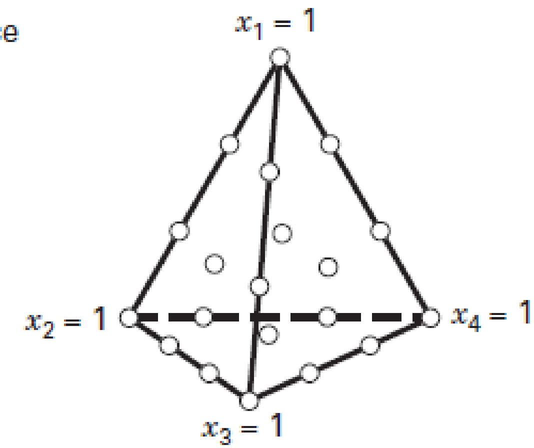
A [3,2] lattice



A [3,3] lattice



A [4,2] lattice



A [4,3] lattice

Simplex Designs are Optimal Designs

Constraints on the mixture components are common, often in the form of lower and upper bounds on component proportions

$$x_1 + x_2 + \cdots + x_q = 1$$

$$L_i \leq x_i \leq U_i, \qquad i = 1, 2, \ldots, q$$

The effect of these constraints is to alter the shape of the original simplex region

If there are only lower bounds, the simplex designs shown previously will still work

If there are both lower and upper bounds, simplex designs will not work for these types of problems

An experiment involving shampoo formulation

There are upper and lower bounds on each component proportion

The response variable is foam height

The experiment involves a constrained design region

An optimal design constructed by computer is a good choice

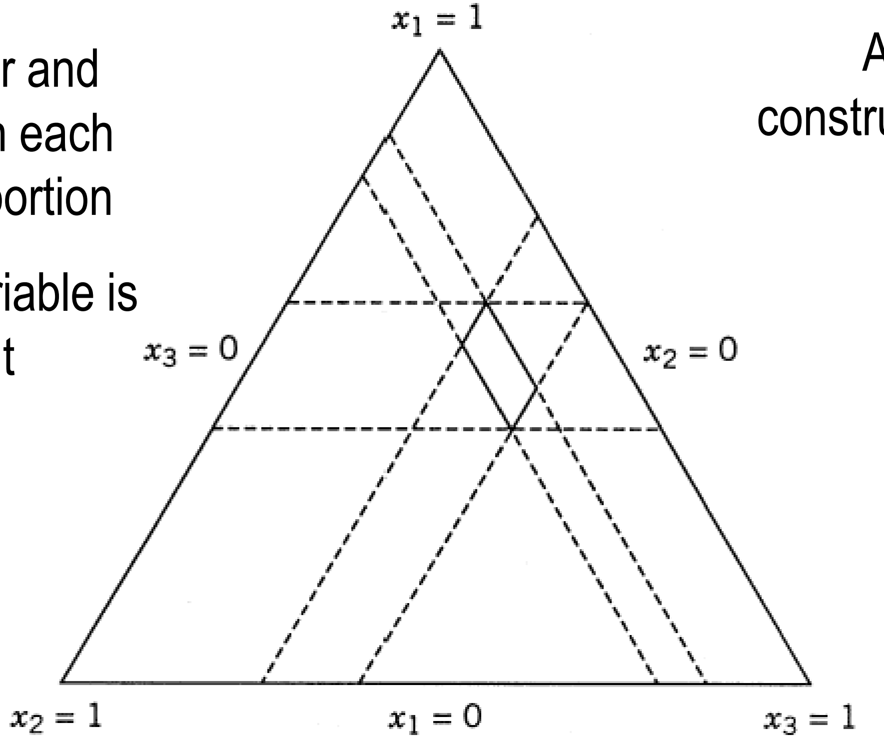


Figure 13.12 Feasible experimental region for the shampoo foam experiment.

An example: formulating the optimum three-component beverage

The constraints on the component proportions are:

$$x_1 + x_2 + x_3 = 1$$

$$0.3 \le x_1 \le 0.9$$

$$0.1 \le x_2 \le 0.7$$

The response variable is a rating, where the taster compares each blend to a “reference” blend and 0 - 4 indicates a blend that is inferior to the reference while 6 -10 indicates a blend that is superior

Makeup of an Aircraft Carrier Air Wing

- The air wing is composed of at least 6 aircraft types
  - Attack aircraft (bombers, like F/A-18)
  - Fighters (CAP, RESCAP, etc, like F-35)
  - Helos (SH-60, SAR, plane guard, etc)
  - ASW (S-3)
  - Ship-to-shore (think the C-2)
  - Electronics (E-2C, EA-6B)
- Space is limited – you can only have a maximum of 85 aircraft of all types
- A computer simulation model will be used to evaluate combat effectiveness for different air wing configurations

Mixture Constraints:

$$X_1 = \text{ attack}, X_1 \geq 25$$

$$X_2 = \text{ fighters}, X_2 \geq 25$$

$$X_3 = \text{ helos}, \; X_3 \geq 10$$

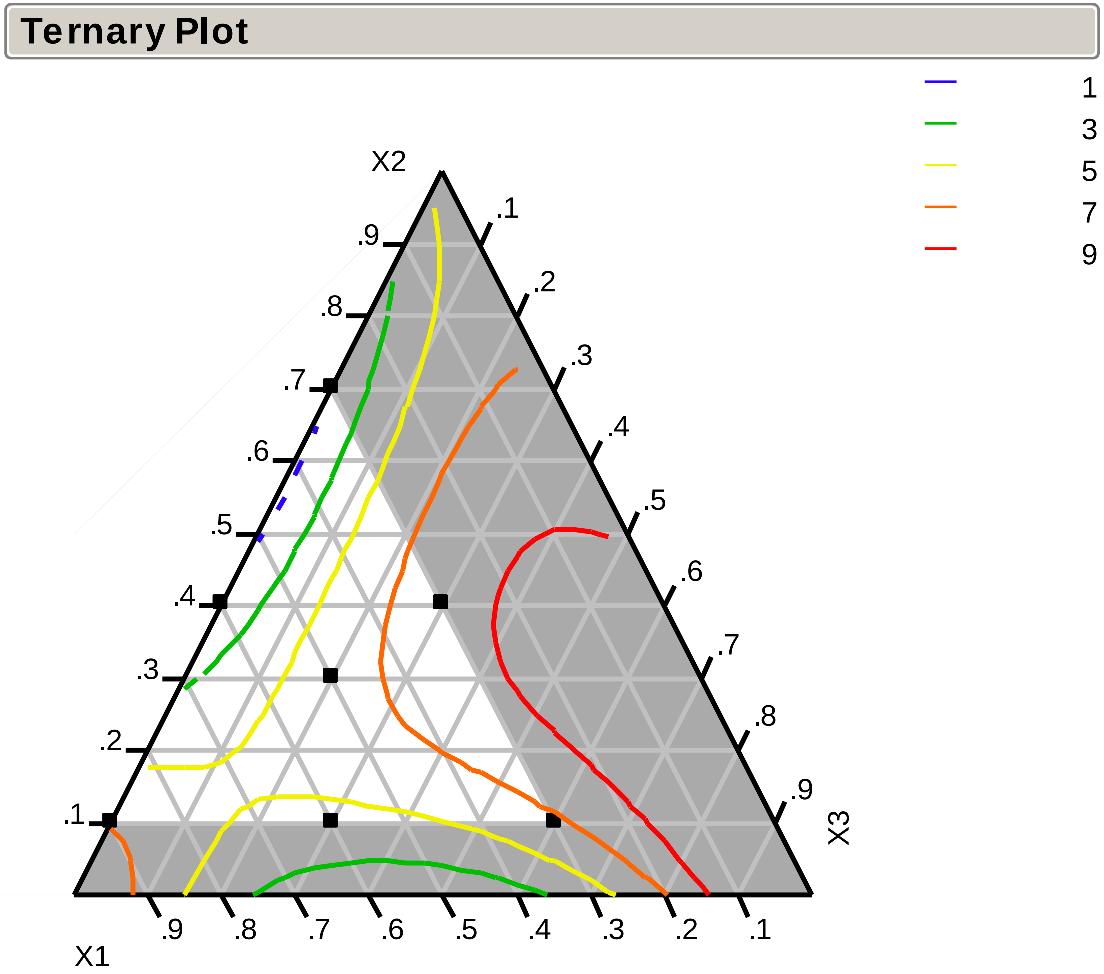
$$X_4 = \text{ ASW}, 8 \leq X_4 \leq 20$$

$$X_5 = \text{ ship-to-shore}, 1 \leq X_5 \leq 4$$

$$X_6 = \text{ electronics}, 10 \leq X_6 \leq 20$$

$$X_1 + X_2 + X_3 + X_4 + X_5 + X_6 = 85$$

Mixtures in JMP



Module 9 - Summary

- Mixture experiments are just a special type of response surface experiment
- Mixture experiments involve a constrained design region which will always require a custom design
- Mixture experiments occur in many settings – once you know about mixtures you will be surprised at how common they are

Module 10 – Covering Arrays

- Goals
  1. Introduce covering array concept
  2. Demonstrate their efficiency for detecting failure conditions
  3. Provide examples of their use

Scenario

Suppose we are testing a system with 10 components.

For simplicity, let each component have two settings.

(This constraint can be relaxed)

We want to make sure that each pair of components has all 4 possible combinations tested.

We want to perform as few system tests as possible.

(Guess the minimum number of necessary tests)

Note: it is not necessary to fit a model – just demonstrate that pairwise combinations work

Covering Array Definition

A *covering array* **CA**(N; t, k, v) is an *N*   *k* array such that the *i*-th column contains v distinct symbols. If a **CA**(N; t, k, v) has the property that for any *t* coordinate projection, all v^t combinations of symbols exist, then it is a ***t***-covering array (or strength t covering array). A ***t***-covering array is optimal if N is minimal for fixed t, k, and v.

N is the number of tests. (find minimum N)

k is the number of factors. (k=10)

t is the number of factors such that all t-factor combinations are tested. (t=2)

v is the number of levels of each factor. (v=2)

Minimum Covering Array

The size of a covering array is the *covering array number* **CAN**(t, k, v),

$$\mathbf{CAN}(t, k, v) = \min\{N: \exists \mathbf{CA}(N; t, k, v)\}.$$

The minimum covering array is the covering array with the fewest runs.

For (t,k,v)=(2,10,2), N = 6!

Covering arrays

1	1	1	1	1
2	2	2	2	2
2	2	2	1	1
2	1	1	2	2
1	2	1	2	1
1	1	2	1	2

CA(6:2,5,2)

1	1	1	1	1
1	1	2	1	1
1	2	1	1	2
1	2	2	1	2
2	1	1	2	1
2	1	2	2	1
2	2	1	2	2
2	2	2	2	2
1	1	1	2	2
2	2	1	1	1
2	1	2	1	2
1	2	2	2	1

CA(12:3,5,2)

Do these have the fewest possible runs?

Covering arrays and software testing

Let `foo(m,n,p,q)` denote a software system with four input parameters each of which has two possible values 1, 2.

Thorough testing would require a test case for each point in the input space (i.e. 16 test cases).

What if you can only afford 8 (or fewer) test cases?

1	1	1	1
1	1	1	2
1	1	2	1
1	1	2	2
1	2	1	1
1	2	1	2
1	2	2	1
1	2	2	2
2	1	1	1
2	1	1	2
2	1	2	1
2	1	2	2
2	2	1	1
2	2	1	2
2	2	2	1
2	2	2	2

Covering arrays and software testing

m	n	p	q
1	1	1	1
1	1	1	2
1	1	2	1
1	1	2	2
1	2	1	1
1	2	1	2
1	2	2	1
1	2	2	2
2	1	1	1
2	1	1	2
2	1	2	1
2	1	2	2
2	2	1	1
2	2	1	2
2	2	2	1
2	2	2	2

```
//Example 1
if(m==2 & p==1 & q==2,
      ...
);

//Example 1
if(m==2 & p==1 & q==2,
      //stuff
      write("n=",n),
      //other stuff
      write("m=",m," n=",n," p=",p," q=",q)
)

//Example 2
if(m==2 & p==1,
      ...
)

//Example 2
if(m==2 & p==1,
      //stuff
      write("n=",n," q=",q),
      //other stuff
      write("m=",m," n=",n," p=",p," q=",q)
)
```

1	1	1	1
1	1	2	2
1	2	1	2
1	2	2	1
2	1	1	2
2	1	2	1
2	2	1	1
2	2	2	2

All 1, 2, 3-way plus 50% 4-way interactions.

***CA**(8; 3, 4, 2),*
***CAN**(3, 4, 2) = 8*

1	1	1	1
1	2	2	2
2	1	2	2
2	2	1	2
2	2	2	1

All 1, 2-way plus 63% 3-way, 31% 4-way interactions.

***CA**(5; 2, 4, 2), **CAN**(2, 4, 2) = 5*

Example - Air to ground missile system

Consider a software system controlling the state of an air to ground missile (Dalal & Mallows). The inputs are:

Altitude	Roll
Attack angle	Yaw
Bank angle	Ambient Temperature
Speed	Pressure
Pitch	Wind Velocity

Challenge: We are interested in deriving test cases to effectively assess “...response during attack maneuvering.”

Example - Air to ground missile system

Suppose we know the maximum and minimum values for each input. Thus, we could choose to have a set of equivalence classes, each corresponding to the range of an input.

Note: This is *equivalence partitioning*.

Select the maximum and minimum as two representative values for each of the 10 input parameters and denote these values by the symbols **1**, **2** respectively.

Example - Air to ground missile system

For complete coverage, we would need to do $2^{10} = 1024$ system tests. This is clearly not feasible.

Can covering arrays help?

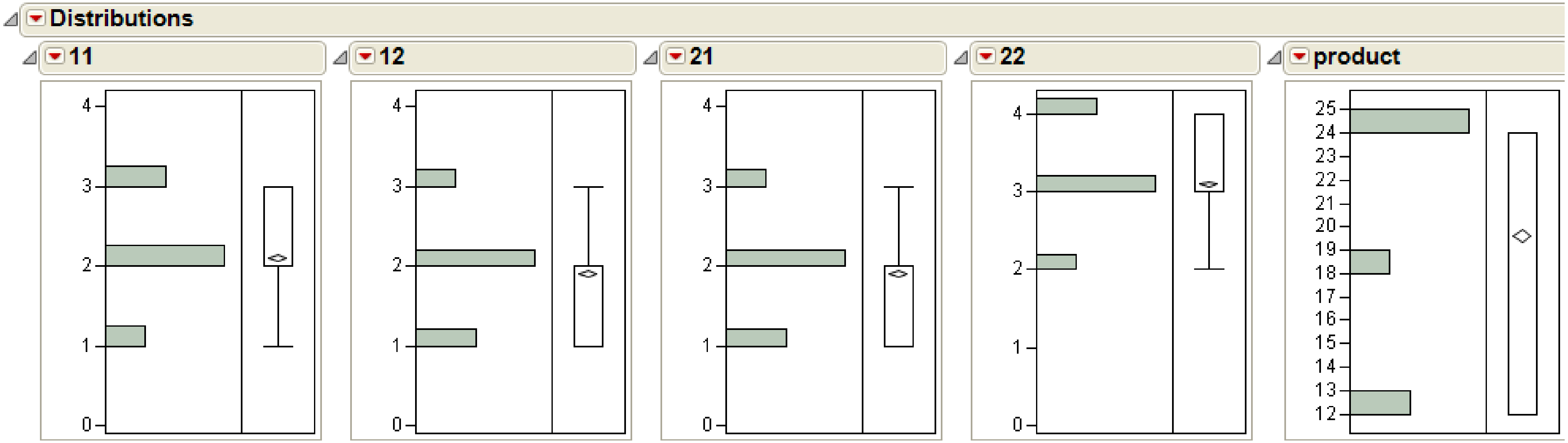
Of course!

JMP Demo of Air to Ground system test.

CA(N:2,k,2) Results

k	2-3	4	5-10	11-15	16-35	36-56	57-126	...	1717-2000
N	4	5	6	7	8	9	10	...	15

JMP Card Trick #1



References

- R. Brownlie, J. Prowse, & M. S. Padke. Robust testing of AT&T PMX/StarMAIL using OATS. *AT&T Technical Journal*, 71(3), 1992, pp 41-47.
- D. M. Cohen, S. R. Dalal, M. L. Fredman, & G. C. Patton, "The AETG System: An approach to testing based on Combinatorial Design," *IEEE TSE*, 23(7), 1997, pp 437-444.
- D. M. Cohen, S. R. Dalal, J. Parelius, & G. C. Patton, "The combinatorial design approach to automatic test generation," *IEEE Software*, 13(5), 1996, pp 83-88.
- S. R. Dalal, A. J. Karunanithi, J. M. Leaton, G. C. Patton, & B. M. Horowitz, "Model-based testing in practice," *Proceedings of the 21st ICSE*, New York, 1999, pp 285-294.
- S. R. Dalal & C. L. Mallows, "Factor-covering designs for testing software," *Technometrics*, 40(3), 1998, pp 234-243.
- I. S. Dunietz, W. K. Ehrlich, B. D. Szablak, C. L. Mallows, & A. Iannino, "Applying design of experiments to software testing," *Proceedings of the 19th ICSE*, New York, 1997, pp 205-215.
- M. Grindal, J. Offutt, & J. Mellin, "Handling Constraints in the Input Space when Using Combination Strategies for Software Testing," School of Humanities and Informatics, University of Skovde, Technical Report HS-IKI-TR-06-001, 2006.
- B. Hailpern & P. Santhanam, "Software Debugging, Testing, and Verification," *IBM Systems Journal*, 41(1), 2002.

References

- A. Hartman & L. Raskin, “Problems and algorithms for covering arrays,” *Discrete Math*, 284(1–3), 2004, pp 149–156.
- R. Mandl, “Orthogonal latin squares: an application of experiment design to compiler testing,” *Communications of the ACM*, 28(10), 1985, pp1054-1058.
- J. A. Morgan, G. J. Knafl, & W. E. Wong, “Predicting Fault Detection Effectiveness,” *Proceedings of the 4th ISSM*, 1997, pp 82-90.
- G. J. Myers, *The Art of Software Testing*, Wiley, 1979.
- N. J. A. Sloane, “Covering Arrays and Intersecting Codes,” *J. Combinatorial Designs*, 1(1), 1993, pp 51-63.
- M. Utting, A. Pretschner, & B. Legeard, “A Taxonomy of Model-Based Testing,” The University of Waikato, New Zealand, Technical Report 04/06, 2006.
- A. L. Williams & R. L. Probert, “A practical strategy for testing pairwise coverage at network interfaces,” *Proceedings of the 7th ISSRE*, Los Alamitos, 1996.
- W. E. Wong, J. R. Horgan, S. London, & A. P. Mathur, “Effect of Test Set Size and Block Coverage on the Fault Detection Effectiveness,” *Proceedings of the 5th ISSRE*, Monterey, 1994, pp 230-238.

Module 10 - Summary

- Covering arrays are the most efficient way to test all possible pairwise combinations of any number of factors
- Covering arrays are useful in software and system testing to assure that no pair of conditions will lead to a failure.
- Covering arrays can also be constructed that protect against triples or higher order combinations but these require more runs.
- JMP has state-of-the-art tools for creating covering arrays.

Module 11 – Supersaturated Designs

Goals

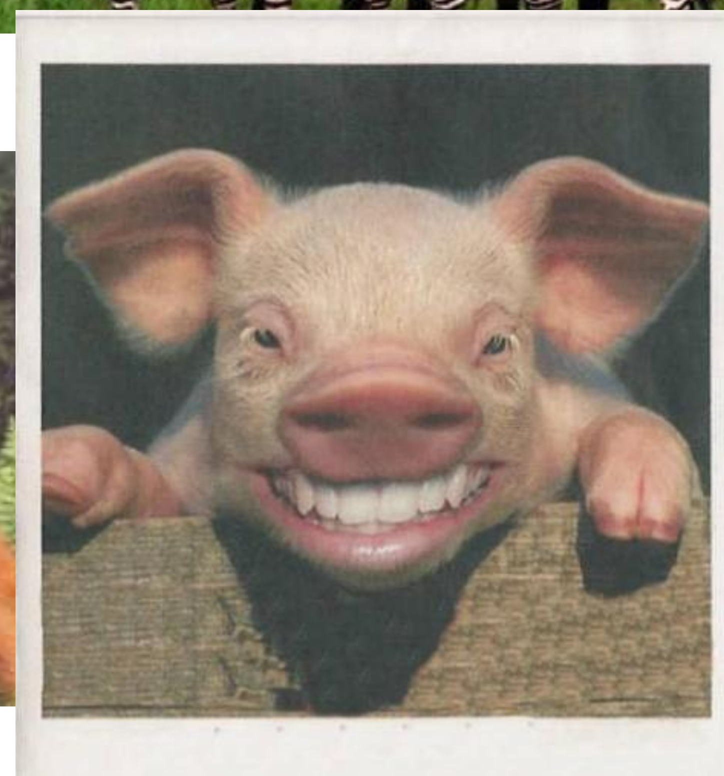
1. Introduce the idea of supersaturated designs
2. Show the theory for constructing them.
3. Give an example.

What is a supersaturated design?

Supersaturated designs have more factors than runs.

This may seem laughable…


DASH村



A more general definition…

Supersaturated designs have fewer runs than parameters of interest.

Supersaturated Design History

- Satterthwaithe (1959) – random balance experimentation
- Booth & Cox (1962) – computer search designs
- Lin (1993) – created new interest in the topic

Classical “supersaturated” designs

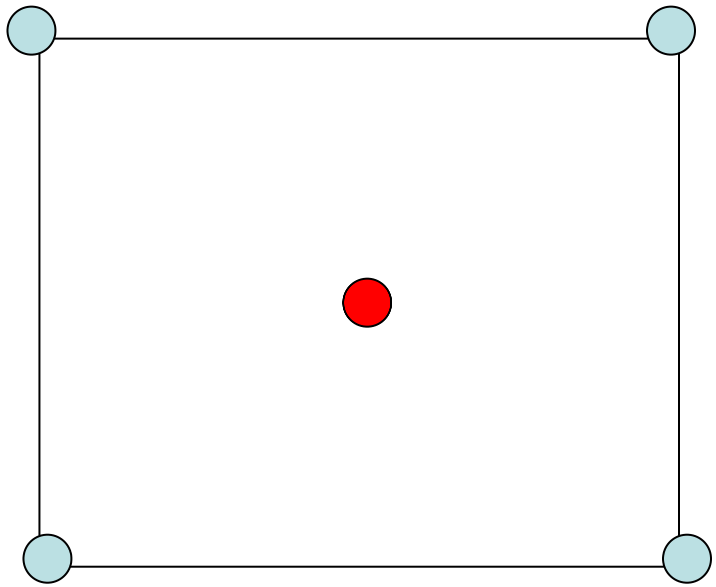
Examples of classical supersaturated design using the more general definition.

1. Adding center points to 2-level factorial designs.
2. Fractional factorial designs.

Case 1 – Center points.

2x2 factorial design with center points.

Supersaturated with respect to model with both quadratic effects.



Case 2 – Fractional Factorial Designs

1. Resolution III

   Supersaturated with respect to the model containing all two-factor interaction effects.

2. Resolution IV

   Supersaturated with respect to the model containing all two-factor interaction effects.

3. Resolution V

   Supersaturated with respect to models containing any three-factor or higher order interaction.

D-Optimal Design Definition

Given the usual linear regression model

$$y = X\beta + \varepsilon$$

find a design matrix, X, to maximize

$$\left| X^T X \right|$$

Problem

D-Optimal designs depend on the choice of the *a priori* model, i.e. X

Solution: Bayesian D-Optimality

Consider two kinds of effects:

Primary effects are ones you are sure you want to estimate. There are p_1 of these.

Potential effects are ones you are afraid to ignore. There are p_2 of these.

For sample size, n

$$p_1 < n < p_1 + p_2$$

Example

2^{6-2} Fractional Factorial Resolution IV design

intercept and main effects are primary

2-factor interactions are potential

$p_1 < n < p_1 + p_2$

$p_1 = 7 \quad p_2 = 15 \; n = 16 \;\; (7 < 16 < 22)$

Defining the K matrix

$$K = \begin{bmatrix} 0_{p_1 x\, p_1} & 0_{p_1 x\, p_2} \\ 0_{p_2 x\, p_1} & I_{p_2 x\, p_2} \end{bmatrix}$$

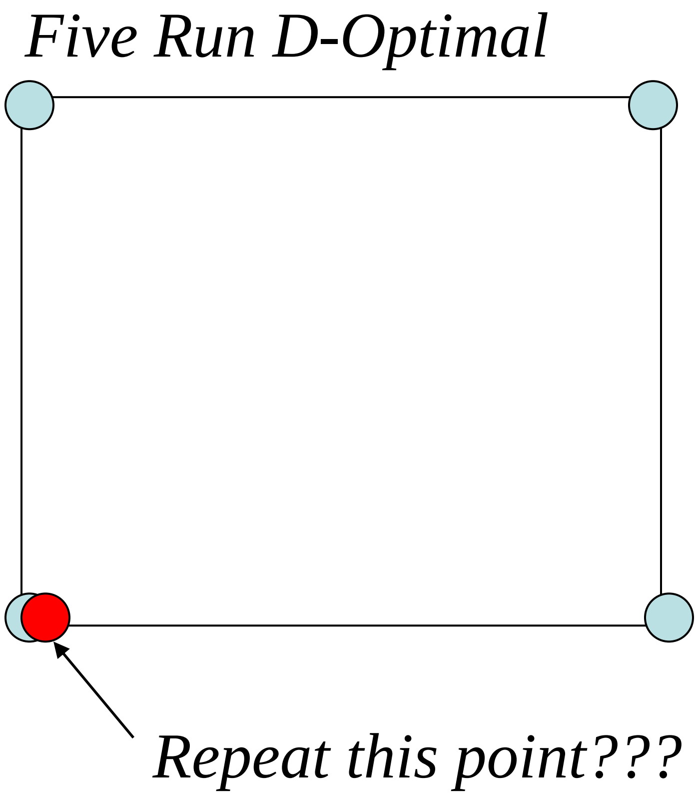
Bayesian D-Optimal designs

Find a design matrix, X, to maximize

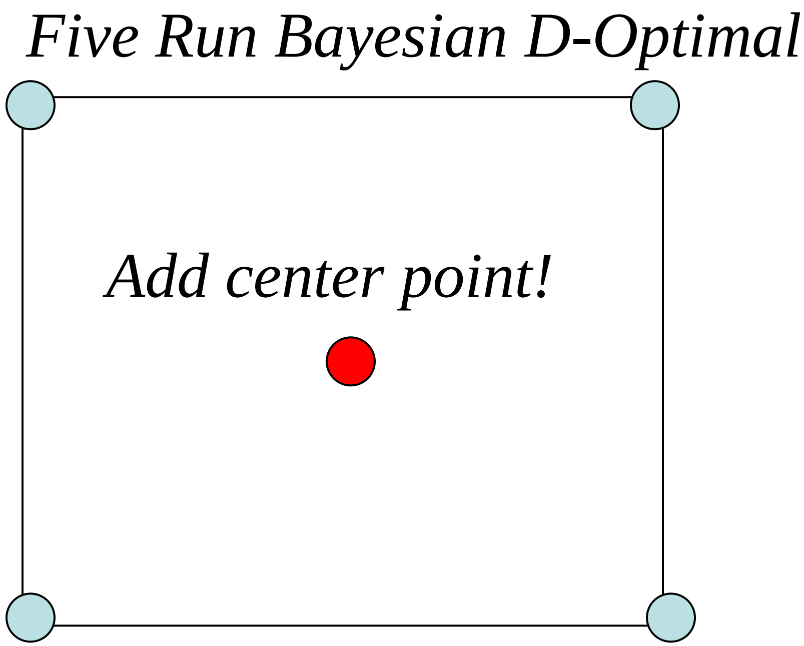
$$D_{Bayes} = \left| X^T X + K / \gamma \right|$$

where γ *is a tuning parameter.*

Comparison



Five Run D-Optimal
Repeat this point???



Five Run Bayesian D-Optimal
Add center point!

Question

Why should only higher order terms be potential?

$$y = \beta_0 1 + \beta_1 x_1 + \ldots + \beta_k x_k + \varepsilon = X\beta + \varepsilon$$

Inspiration: Allow main effects to be potential.

Result: Supersaturated designs using Bayesian D-Optimality.

Benefits of Bayesian D-Optimal Supersaturated Design

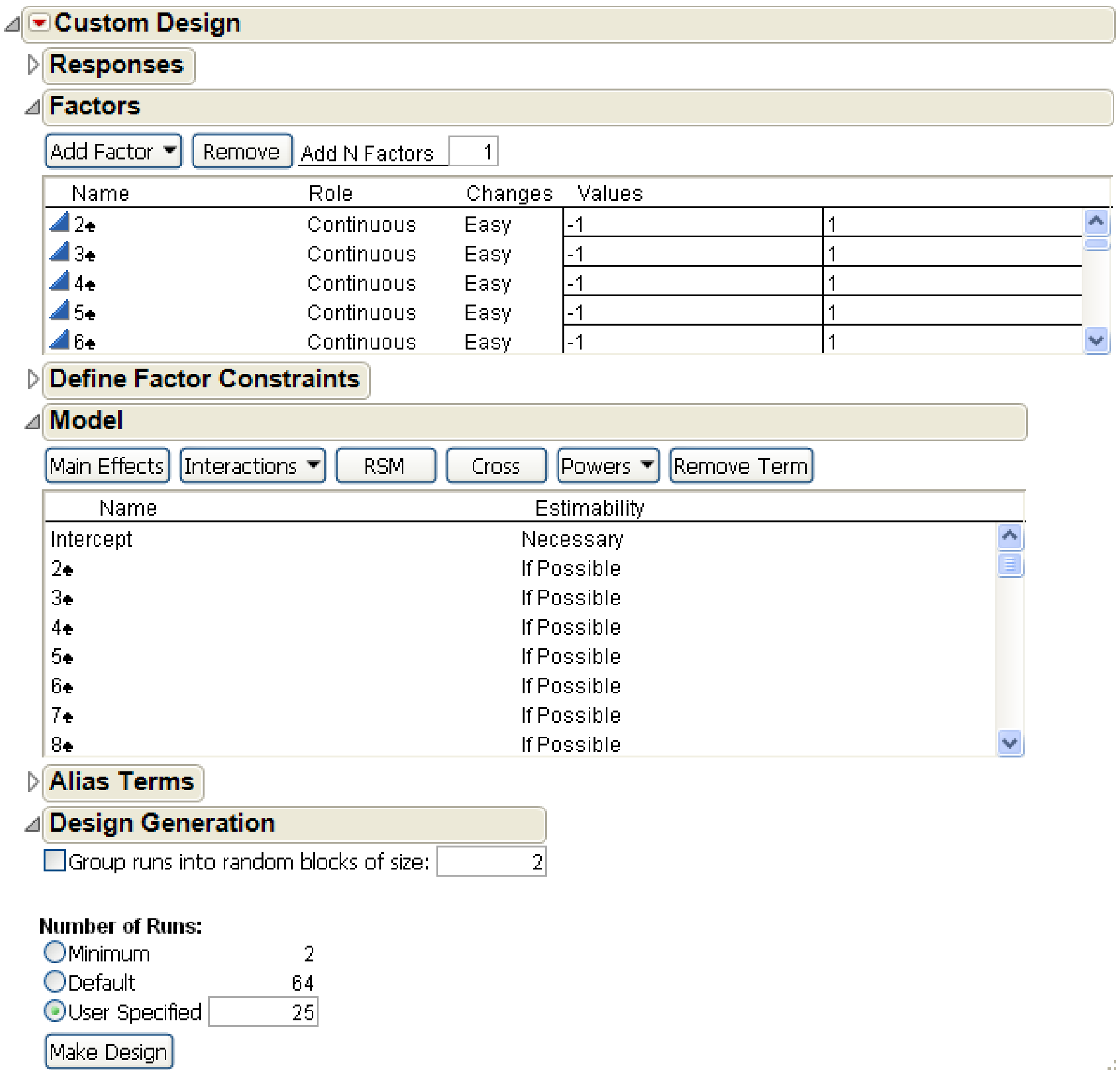
1. Easy and fast to compute
2. Flexible formulation (sample size, factor type, etc.)

References:

DuMouchel and Jones, Technometrics (1994) vol.36 #1 pp. 37-47.

Jones, B., Lin, D., and Nachtsheim, C. (2008) “Bayesian D-Optimal Supersaturated Designs.” *Journal of Statistical Planning and Inference*, 138, 86-92.

Card Trick in JMP



Module 11 - Summary

Supersaturated designs are not laughable.

It is time to start using them to solve real problems…

DOX Course – Final Thoughts

1. Optimal design framework is general and powerful for handling all kinds of DOX problems.
2. Modern software makes it easy to generate optimal designs for virtually any problem incorporating constraints on
    1. Factor combinations
    2. Model requirements
    3. Restrictions on sample size
    4. Restrictions on randomization
3. It is time to break away from traditional methods
4. Make the design fit the problem don't force your problem into the constraints of a classical design.