STAT22200 Spring 2014 Chapter 13A

Yibi Huang

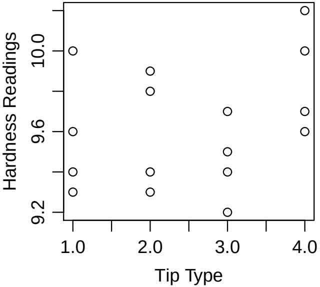
May 27, 2014

Example – Hardness Readings (1)

A hardness testing machine operates by pressing the tool tip into a metal test coupon, then determine the hardness of the coupon based the depth of the resulting depression. To test the consistency of the readings, four tool tips are used in an experiment, each tip presses four metal coupons made of same material.

Tip Types	Metal Coupons 1	2	3	4
1	9.3	9.4	9.6	10.0
2	9.4	9.3	9.8	9.9
3	9.2	9.4	9.5	9.7
4	9.7	9.6	10.0	10.2

Reading = hardness in Rockwell C scale minus 40



Questions: Which factor is the treatment factor? Tip types or coupon?

Example – Hardness Readings (2)

If we ignore the structure in the units and regard as a completely randomized design (CRD) with 4 treatments (tip types), the ANOVA table would be

```
> hardness = read.table("hardness.dat",header=T)
> attach(hardness)
> summary(aov(readings ~ as.factor(tiptype)))
                   Df Sum Sq Mean Sq F value Pr(>F)
as.factor(tiptype)  3  0.385 0.12833   1.702   0.22
Residuals          12  0.905 0.07542
```

As the P-value 0.22 is big, we could NOT reject H_0 that the four treatment groups have a common mean.

> Wait! What does the ANOVA test mean in the context of this experiment?

Recall the Means Model for CRD

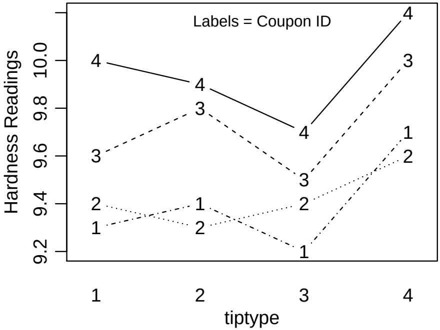
$$y_{ij} = \mu_i + \varepsilon_{ij},$$

in which

- y_{ij} = the hardness reading of tip type i on metal coupon j
- μ_i = mean hardness readings of tip type i
- ε_{ij}: errors, i.i.d. $\sim N(0, \sigma^2)$

Does the model $y_{ij} = \mu_i + \varepsilon_{ij}$ make sense in this context?

Different coupons may have different hardness, and should have different readings.
Why do we expect same reading when we are measuring different metal coupons?




```
> ?interaction.plot
> interaction.plot(tiptype,coupon,readings,type="b",
+ ylab="Hardness Readings",legend=FALSE)
> legend("top","Labels = Coupon ID",bty="n",cex=.8)
```

We can see that coupon 4 always have the highest reading, regardless of tip types. Coupon 3 is the second. Coupon 1 and 2 always have lower readings. The four coupons indeed differ in hardness.

A More Reasonable Model

$$y_{ij} = \mu + \alpha_i + \beta_j + \varepsilon_{ij}$$

in which

- y_{ij} = the hardness reading of tip type i on metal coupon j
- μ = the grand mean of y_{ij}
- β_j = the coupon effect
- α_i = bias of readings from the true hardness if a tip of type i is used
- ε_{ij}: measurement errors, i.i.d. $\sim N(0, \sigma^2)$

Question: In the context of this experiment, which parameters are we more interested in? α_i's or β_j's?

Just like the models for factorial design, the model above is over-parameterized. some constraints must be imposed. The commonly used constraints are

$$\sum_{i=1}^{4} \alpha_i = 0 \quad \text{and} \quad \sum_{j=1}^{4} \beta_i = 0.$$

Block Designs

- A **block** is a set of experimental units that are homogeneous in some sense. Hopefully, units in the same block will have similar responses (if applied with the same treatment.)
- **Block designs**: randomize the units within each block to the treatments.
- **Randomized Complete Block Design (RCBD)**: Suppose there are g treatments and the size of all the blocks are of size k, and $k = rg$ is a multiple of g.
  - Mostly, block size $k = \#$ of treatments g. From now on, assume $k = g$.
  - Within each block, the $k = rg$ experimental units are randomized to the g treatments, r units per treatments.
  - The word "complete" indicates that every block contains all the g treatments.
  - The Match-Paired design in Chapter 2 is a special case of RCBD in which the block size $k = 2$.

Data Structure for a Randomized Complete Block Design (RCBD)

	Block 1	Block 2	$\cdots$	Block b
Treatment 1	y_{11}	y_{12}	$\cdots$	y_{1b}
Treatment 2	y_{21}	y_{22}	$\cdots$	y_{2b}
$\vdots$	$\vdots$	$\vdots$	$\cdots$	$\vdots$
Treatment g	y_{g1}	y_{g2}	$\cdots$	y_{gb}

Normally, data are shown arranged by block and treatment, as in the hardness reading example. You cannot see what was/was not randomized by looking at data only.

Advantage of Blocking

- Blocking is the second basic principle of experimental design after randomization.

  > "Block what you can, randomize everything else."

- If units are highly variable, grouping them into more similar blocks can lead to a large increase in efficiency (more power to detect difference in treatment effects).
- The choice of blocks is crucial

Commonly used blocking

- Block on batch (e.g., milk produced in a day)
- Block spatially
- Block on time
- Block when you can identify a source of variation (e.g., age, gender, and history blocks)

Caution: Blocking must be done at the time of randomization; you can't group units into blocks after the experiment has been run (That is called ANCOVA instead, not RCBD).

A Blocking Factor is Not A Treatment Factor

It is sometimes easy to confuse blocking variables with treatment factors. Remember that

- We can assign an experimental unit to any treatment, but cannot assign an unit to any block.
  Blocking variables are a property of the experimental units, not something we can manipulate.
  e.g., in the hardness reading example, we can change the tip type to measure the hardness of a unit, but cannot change the batch an unit comes from.

- Since we cannot experimentally manipulate the blocking variable, block effects are "observational". We cannot make causal inference to a blocking variable as to a treatment factor.

Example: Patio Sealers

- Goal: Want to study the effects of 3 different sealers on protecting concrete patios from the weather.
- Units: 15 unsealed patios are available spread across Chicago
- Which of the following two designs do you think will be better in this context? CRD or RCBD?
  - CRD: Randomly assign the 15 units to the 3 sealers, 5 units each. Apply the assigned sealer to all the surface of the patio. The degree of erosion of the 15 patios are evaluated after 2 years.
  - RCBD: Separate each patio into 3 portions, and apply the sealers (randomly) in such a way that each patio receives each sealer for 1/3 of the surface. The patio surface are evaluated after 2 years.

Patio (location) may be a important factor on the degree of erosion. Some patios may be better sheltered (by trees or nearby buildings etc.)

Parameter Estimates for the RCBD

The model for a RCBD

$$y_{ij} = \mu + \alpha_i + \beta_j + \varepsilon_{ij} \quad \varepsilon_{ij}\text{'s are i.i.d. } N(0, \sigma^2).$$

has the same format as the additive model for a balanced two-way factorial design. So the two models have the same least square estimate for parameters:

$$\begin{aligned} \widehat{\mu} &= \overline{y}_{\bullet\bullet} \\ \widehat{\alpha}_i &= \overline{y}_{i\bullet} - \widehat{\mu} = \overline{y}_{i\bullet} - \overline{y}_{\bullet\bullet} \\ \widehat{\beta}_j &= \overline{y}_{\bullet j} - \widehat{\mu} = \overline{y}_{\bullet j} - \overline{y}_{\bullet\bullet} \end{aligned}$$

for $i = 1, \ldots, g$ and $j = 1, \ldots, b$.
Questions: Why not including treatment-block interactions?

Fitted Values for RCBD

The fitted values for RCBD is then

$$\begin{aligned}\widehat{y}_{ij} &= \widehat{\mu} + \widehat{\tau}_j + \widehat{\beta}_j \\ &= \overline{y}_{\bullet\bullet} + (\overline{y}_{i\bullet} - \overline{y}_{\bullet\bullet}) + (\overline{y}_{\bullet j} - \overline{y}_{\bullet\bullet}) \\ &= \overline{y}_{i\bullet} + \overline{y}_{\bullet j} - \overline{y}_{\bullet\bullet} \\ &= \text{row mean } + \text{ column mean } - \text{grand mean}\end{aligned}$$

just like in an additive model for a balanced two-way design.

	Block 1	Block 2	$\cdots$	Block b	row mean
Treatment 1	y_{11}	y_{12}	$\cdots$	y_{1b}	$\overline{y}_{1\bullet}$
Treatment 2	y_{21}	y_{22}	$\cdots$	y_{2b}	$\overline{y}_{2\bullet}$
$\vdots$	$\vdots$	$\vdots$	$\ldots$	$\vdots$	$\vdots$
Treatment g	y_{g1}	y_{g2}	$\cdots$	y_{gb}	$\overline{y}_{g\bullet}$
column mean	$\overline{y}_{\bullet 1}$	$\overline{y}_{\bullet 2}$	$\cdots$	$\overline{y}_{\bullet b}$	grand mean $\overline{y}_{\bullet\bullet}$

Sum of Squares and Degrees of Freedom

The sum of squares and degrees of freedom for RCBD are just like those for the additive models:

$$SST = SS_{trt} + SS_{block} + SSE$$

where

$$\begin{aligned}
SST &= \sum_{i=1}^{g}\sum_{j=1}^{b}(y_{ij} - \overline{y}_{\bullet\bullet})^2 \\
SS_{trt} &= \sum_{i=1}^{g}\sum_{j=1}^{b}(\overline{y}_{i\bullet} - \overline{y}_{\bullet\bullet})^2 = b\sum_{i=1}^{g}(\overline{y}_{i\bullet} - \overline{y}_{\bullet\bullet})^2 \\
SS_{block} &= \sum_{i=1}^{g}\sum_{j=1}^{b}(\overline{y}_{\bullet j} - \overline{y}_{\bullet\bullet})^2 = g\sum_{j=1}^{b}(\overline{y}_{\bullet j} - \overline{y}_{\bullet\bullet})^2 \\
SSE &= \sum_{i=1}^{g}\sum_{j=1}^{b}(y_{ij} - \overline{y}_{i\bullet} - \overline{y}_{\bullet j} + \overline{y}_{\bullet\bullet})^2.
\end{aligned}$$

Total	Treatment	Block	Error
$dfT = bg - 1$	$df_{trt} = g - 1$	$df_{block} = b - 1$	$dfE = (g-1)(b-1)$

Expected Values for the Mean Squares

Just like CRD, the mean squares for RCBD is the sum of squares divided by the corresponding d.f.

$$\text{MS}_{trt} = \frac{\text{SS}_{trt}}{g-1}, \quad \text{MS}_{block} = \frac{\text{SS}_{block}}{b-1}, \quad \text{MSE} = \frac{\text{SSE}}{(g-1)(b-1)}.$$

Under the effects model for RCBD,

$$y_{ij} = \mu + \alpha_i + \beta_j + \varepsilon_{ij} \quad \varepsilon_{ij}\text{'s are i.i.d. } N(0, \sigma^2).$$

one can show that

$$\mathbb{E}(\text{MS}_{trt}) = \sigma^2 + \frac{b}{g-1}\sum_{i=1}^{g}\alpha_i^2$$

$$\mathbb{E}(\text{MS}_{block}) = \sigma^2 + \frac{g}{b-1}\sum_{j=1}^{b}\beta_j^2$$

$$\mathbb{E}(\text{MSE}) = \sigma^2$$

Thus the MSE is again an unbiased estimator of σ^2.

ANOVA F-Test for Treatment Effect

To test whether there is an treatment effect

$$H_0 : \alpha_1 = \alpha_2 = \cdots = \alpha_g \quad \text{v.s.} \quad H_a : \text{not all } \alpha_i\text{'s are equal}$$

is equivalent to testing whether all α_i's are zero

$$H_0 : \alpha_1 = \alpha_2 = \cdots = \alpha_g = 0 \quad \text{v.s.} \quad H_a : \text{not all } \alpha_i\text{'s are zero}$$

because of the constraint $\sum_{i=1}^{g} \alpha_i = 0$. This test is also equivalent to a comparison between 2 models:

full model: $y_{ij} = \mu + \alpha_i + \beta_j + \varepsilon_{ij}$ reduced model: $y_{ij} = \mu + \beta_j + \varepsilon_{ij}$

The test statistic is

$$F_{trt} = \frac{\text{MS}_{trt}}{\text{MSE}} \sim F_{g-1,\,(g-1)(b-1)} \text{under } \text{H}_0.$$

The ANOVA table is given in the next slide

ANOVA Table for RCBD

Source	d.f.	Sum of Squares	Mean Squares	F
Treatment	$g-1$	SS_{trt}	MS_{trt}	$F_{trt} = \frac{\text{MS}_{trt}}{\text{MSE}}$
Block	$b-1$	SS_{block}	MS_{block}	$(F_{block} = \frac{\text{MS}_{block}}{\text{MSE}})$
Error	$(b-1)(g-1)$	SSE	MSE	
Total	$bg-1$	SST		

- The F statistic F_{block} for testing the block effect, which is not interesting, and is often omitted.

ANOVA Table for CRD and RCBD

If we ignore the block effect, and analyzed the experiment as a CRD, the ANOVA table becomes

Source	d.f.	Sum of Squares	Mean Squares	F
Treatment	$g-1$	SS_{trt}	MS_{trt}	$F_{trt} = \frac{MS_{trt}}{MSE_{CRD}}$
Error	$bg-g$	SSE_{CRD}	MSE_{CRD}	
Total	$bg-1$	SST		

In the table SS_{trt} and MS_{trt} is the same in CRD and RCBD, but the variability due to block is now in the error term

$$SSE_{CRD} = SSE_{RCBD} + SS_{block}.$$

If SS_{block} is large, by considering the block effect, one can substantially reduce the size of noise. With a smaller MSE, it is easier to detect difference in treatment effects.

RCBD can be a very effective noise-reducing technique if SS_{block} is large.

Example – Hardness Reading

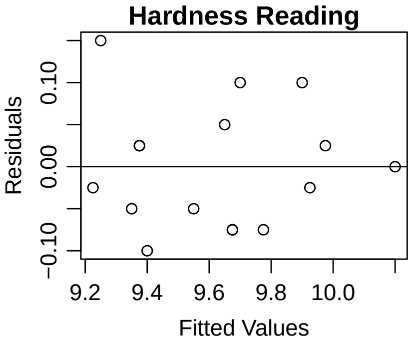
```
> anova(lm(readings ~ as.factor(coupon)+as.factor(tiptype)))
                   Df Sum Sq Mean Sq F value   Pr(>F)
as.factor(tiptype)  3  0.385 0.12833   14.44 0.000871 ***
as.factor(coupon)   3  0.825 0.27500   30.94 4.52e-05 ***
Residuals           9  0.080 0.00889
```

If we ignore the block effect,

```
> summary(aov(readings ~ as.factor(tiptype)))
                   Df Sum Sq Mean Sq F value Pr(>F)
as.factor(tiptype)  3  0.385 0.12833   1.702   0.22
Residuals          12  0.905 0.07542
```

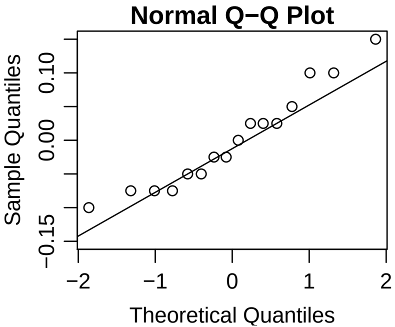
Model Diagnosis

Just like CRD, one should also do model checking after fitting a RCBD model, including all kinds of residual plots, normal QQ plot, time plot, Box-Cox, etc.

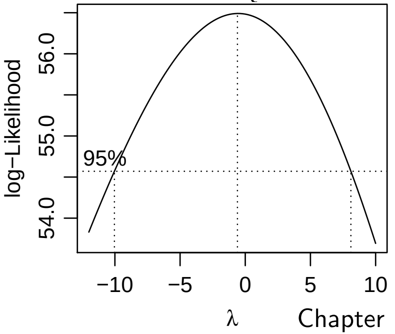


The residual plot shows nothing unusual, no clear evidence of non-constant variance or outliers.

Model Diagnosis



The QQ plot shows nothing unusual. It is slightly curved, but as there is only a few points, we are not sure if it's a real pattern or just random fluctuation.



The Box-Cox method gives a wide C.I. for λ (about -10 to 8), which means the data has no specific preference for transforming the response.

Contrasts in RCBD (1)

A natural estimator of a **contrast** $C = \sum_{i=1}^{g} w_i \alpha_i$ is

$$\widehat{C} = \sum_{i=1}^{g} w_i \widehat{\alpha}_i = \sum_{i=1}^{g} w_i (\overline{y}_{i\bullet} - \overline{y}_{\bullet\bullet}) = \sum_{i=1}^{g} w_i \overline{y}_{i\bullet},$$

which is identical to the estimator in CRD. (Note a contrast must have $\sum_{i=1}^{g} w_i = 0$). So is the SE of the estimator (group size n_i becomes b)

$$\text{SE}(\widehat{C}) = \sqrt{\text{MSE} \times \sum_{i=1}^{g} \frac{w_i^2}{b}}$$

The $(1-\alpha)100\%$ C.I. and test for C remain the same form, only the degrees of freedom changes from $N - g$ to $(g-1)(b-1)$.

$$\widehat{C} \pm t_{\alpha/2,(g-1)(b-1)} \times \text{SE}(\widehat{C})$$

Contrasts in RCBD (2)

As for testing

$$\begin{cases} H_0: & C = \sum_{i=1}^{g} w_i \alpha_i = 0 \\ H_a: & C = \sum_{i=1}^{g} w_i \alpha_i \neq 0 \end{cases}$$

the test statistic remain the same form,

$$t_0 = \frac{\widehat{C}}{\text{SE}(\widehat{C})} = \frac{\sum_{i=1}^{g} w_i \overline{y}_{i\bullet}}{\sqrt{\text{MSE} \times \sum_{i=1}^{g} \frac{w_i^2}{b}}} \sim t_{(g-1)(b-1)}$$

only the $t-$distribution changes its d.f. form $N-g$ to $(g-1)(b-1)$. At test level of α, reject H_0 when

$$|t_0| > t_{\alpha/2,\,(g-1)(b-1)}$$

For one-sided alternative, we reject H_0 at level α when

$$\begin{aligned} t_0 > & \quad t_{\alpha,\,(g-1)(b-1)}, \quad \text{if } H_a: C > 0, \\ t_0 < & -t_{\alpha,\,(g-1)(b-1)}, \quad \text{if } H_a: C < 0. \end{aligned}$$

Multiple Comparison

All the multiple comparison procedures apply to RCBD, just change the degree of freedom from $N - g$ to $(g-1)(b-1)$.

Method	*Family of Tests*	*Critical Value to Keep FWER* $< \alpha$
Fisher's LSD	a single pairwise comparison	$t_{\alpha/2,(g-1)(b-1)}$
Dunnett	all comparisons with a control	$d_\alpha(g-1,(g-1)(b-1))$
Tukey-Kramer	all pairwise comparisons	$q_\alpha(g,(g-1)(b-1))/\sqrt{2}$
Bonferroni	all pairwise comparisons	$t_{\alpha/(2r),(g-1)(b-1)}$, where $r = \frac{g(g-1)}{2}$
Scheffè	all contrasts	$\sqrt{(g-1)F_{\alpha,g-1,(g-1)(b-1)}}$

Power and Sample Size Calculation

When H_a is true, treatments have different effects, the ANOVA F-statistic also has a non-central F-distribution

$$F = \frac{\text{MS}_{trt}}{\text{MSE}} \sim \begin{cases} F_{g-1,\,(g-1)(b-1)} & \text{under H}_0 \\ F_{g-1,\,(g-1)(b-1),\delta} & \text{under H}_a \end{cases}$$

where the non-centrality parameter δ is

$$\delta = \frac{\sum_{i=1}^{g} b\alpha_i^2}{\sigma^2}.$$

Note that only the degrees of freedom changes from $N - g$ to $(g-1)(b-1)$

13.3 Latin Square Designs — Blocking Two Variations Simultaneously

Sometimes there are more than one source of variations or disturbance that can be eliminated by blocking.

Example Suppose in a farm there is a north-south variation in sunlight and east-west variation in soil humidity. It is natural to block on row and column position of plots.

For example, the treatments are 5 fertilizers A, B, C, D, E. Consider the design

	Column				
Row	1	2	3	4	5
I	A	B	C	D	E
II	C	D	E	A	B
III	E	A	B	C	D
IV	B	C	D	E	A
V	D	E	A	B	C

Each treatment occurs once in each row and in each column. This is called a **Latin square**. When we estimate treatment effects, the row and column effects will cancel out.

Example — Automobile Emissions

<u>Variables</u>

Additives: A, B, C, D (chemicals aimed at reducing pollution)

Drivers: I, II, III, IV

Cars: $1, 2, 3, 4$

Response: Emission reduction index measured for each test drive

<u>The experiment</u>

Additives as treatments ($i = 1, 2, 3, 4$)

Drivers as a block variable (row block $j = 1, 2, 3, 4$)

Cars as another block variable (column block $k = 1, 2, 3, 4$)

The combination (driver, car) as experimental units

Latin square of order 4 as the design of the experiment

Data and Design for the Automobile Emissions Example

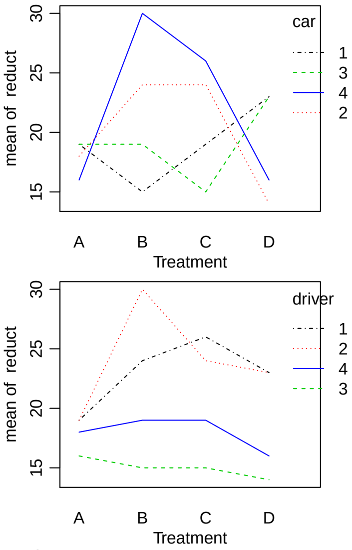
	Cars				
Drivers	1	2	3	4	driver average
I	A	B	D	C	
	19	24	23	26	23
II	D	C	A	B	
	23	24	19	30	24
III	B	D	C	A	
	15	14	15	16	15
IV	C	A	B	D	
	19	18	19	16	18
average					grand mean
per car	19	20	19	22	20

Treatment means

A: 18, B: 22, C: 21, D: 19

Data Set of the Automobile Emissions Example

	driver	car	trt	reduct
1	1	1	A	19
2	1	2	B	24
3	1	3	D	23
4	1	4	C	26
5	2	1	D	23
6	2	2	C	24
7	2	3	A	19
8	2	4	B	30
9	3	1	B	15
10	3	2	D	14
11	3	3	C	15
12	3	4	A	16
13	4	1	C	19
14	4	2	A	18
15	4	3	B	19
16	4	4	D	16



Are there block effects by drivers, by cars?
Which block effect is stronger?

Model For a Latin Square Design

If we label the two blocking variables j and k and the treatment i with $1 \leq i, j, k \leq g$, we write

$$y_{ijk} = \mu + \alpha_i + \beta_j + \gamma_k + \varepsilon_{ijk}$$

where

$$\sum_i \alpha_i = \sum_j \beta_j = \sum_k \gamma_k = 0, \qquad \varepsilon_{ijk} \sim i.i.d.\ N(0, \sigma^2)$$

We have g^2 experimental units. For given j and k we only have one value $i(j,k)$ corresponding to *Treatment* i.

The design is balanced, so we have the usual estimates:

$$\widehat{\mu} = \bar{y}_{\bullet\bullet\bullet}, \quad \widehat{\alpha}_i = \bar{y}_{i\bullet\bullet} - \bar{y}_{\bullet\bullet\bullet}$$
$$\widehat{\beta}_j = \bar{y}_{\bullet j\bullet} - \bar{y}_{\bullet\bullet\bullet}$$
$$\widehat{\gamma}_k = \bar{y}_{\bullet\bullet k} - \bar{y}_{\bullet\bullet\bullet}$$

Statistical analysis can be done using familiar R commands.

Why Do Latin Squares Design Work?

What is the mean for $\bar{y}_{1\bullet\bullet}$?

Decomposition of the Latin Square (Automobile Emissions)

	1	2	3	4
I	A 19	B 24	D 23	C 26
II	D 23	C 24	A 19	B 30
III	B 15	D 14	C 15	A 16
IV	C 19	A 18	B 19	D 16

=

$\bar{y}_{\bullet\bullet\bullet}$

20	20	20	20
20	20	20	20
20	20	20	20
20	20	20	20

+

$y_{ijk} - \bar{y}_{\bullet\bullet\bullet}$

-1	4	3	6
3	4	-1	10
-5	-6	-5	-4
-1	-2	-1	-4

$y_{ijk} - \bar{y}_{\bullet\bullet\bullet}$

-1	4	3	6
3	4	-1	10
-5	-6	-5	-4
-1	-2	-1	-4

=

$y_{\bullet j\bullet} - \bar{y}_{\bullet\bullet\bullet}$

3	3	3	3
4	4	4	4
-5	-5	-5	-5
-2	-2	-2	-2

+

$y_{\bullet\bullet k} - \bar{y}_{\bullet\bullet\bullet}$

-1	0	-1	2
-1	0	-1	2
-1	0	-1	2
-1	0	-1	2

+

$y_{i\bullet\bullet} - \bar{y}_{\bullet\bullet\bullet}$

-2	2	-1	1
-1	1	-2	2
2	-1	1	-2
1	-2	2	-1

+

residuals

-1	-1	2	0
1	-1	-2	2
-1	0	0	1
1	2	0	-3

$\mathbf{Y} - \bar{\mathbf{Y}}$	**D**	**C**	**T**	**R**
$SS_{Total} = 312$	$SS_{Driver} = 216$	$SS_{Car} = 24$	$SS_{Trtl} = 40$	$SS_E = 32$
$d.f. = 15$	$d.f. = 3$	$d.f. = 3$	$d.f. = 3$	$d.f. = 6$

ANOVA Table for a Latin Square — (w/o Replicates)

Source	d.f.	SS	MS	F-value
Row-Block	$g-1$	SS_{row}	$\mathrm{SS}_{row}/(g-1)$	$\mathrm{MS}_{row}/\mathrm{MSE}$
Column-Block	$g-1$	SS_{col}	$\mathrm{SS}_{col}/(g-1)$	$\mathrm{MS}_{col}/\mathrm{MSE}$
Treatment	$g-1$	SS_{Trt}	$\mathrm{SS}_{Trt}/(g-1)$	$\mathrm{MS}_{Trt}/\mathrm{MSE}$
Error	$(g-2)(g-1)$	SSE	$\mathrm{SSE}/[(g-2)(g-1)]$	
Total	g^2-1	SS_{total}		

where $(g-2)(g-1) = g^2 - 1 - 3(g-1)$.

$$
\begin{aligned}
SS_{row} &= \sum_{ijk} \widehat{\beta}_j^2 = \sum_{ijk} (\bar{y}_{\bullet j \bullet} - \bar{y}_{\bullet\bullet\bullet})^2 = g \sum_j (\bar{y}_{\bullet j \bullet} - \bar{y}_{\bullet\bullet\bullet})^2, \\
SS_{col} &= \sum_{ijk} \widehat{\gamma}_k^2 = \sum_{ijk} (\bar{y}_{\bullet\bullet k} - \bar{y}_{\bullet\bullet\bullet})^2 = g \sum_k (\bar{y}_{\bullet\bullet k} - \bar{y}_{\bullet\bullet\bullet})^2, \\
SS_{trt} &= \sum_{ijk} \widehat{\alpha}_i^2 = \sum_{ijk} (\bar{y}_{i\bullet\bullet} - \bar{y}_{\bullet\bullet\bullet})^2 = g \sum_i (\bar{y}_{i\bullet\bullet} - \bar{y}_{\bullet\bullet\bullet})^2, \\
SSE &= g \sum_i (y_{ijk} - \bar{y}_{i\bullet\bullet} - \bar{y}_{\bullet j\bullet} - \bar{y}_{\bullet\bullet k} + 2\bar{y}_{\bullet\bullet\bullet\bullet})^2 \\
&= SS_{total} - SS_{row} - SS_{col} - SS_{trt}, \\
SS_{total} &= \sum_{ijk} (y_{ijk} - \bar{y}_{\bullet\bullet\bullet})^2
\end{aligned}
$$

Example: ANOVA Table for Automobile Emissions Data

```
> lm1 = lm(reduct ~ as.factor(car)+as.factor(driver)+as.factor(trt))
> anova(lm1)
Analysis of Variance Table

Response: reduct
                  Df Sum Sq Mean Sq F value   Pr(>F)
as.factor(car)     3     24   8.000     1.5 0.307174
as.factor(driver)  3    216  72.000    13.5 0.004466 **
as.factor(trt)     3     40  13.333     2.5 0.156490
Residuals          6     32   5.333
```

Advantages of Latin Squares

- For the same number of experimental units as a randomized complete block design (RCBD) with g treatments and g blocks, we can simultaneously block for a second variable.
- Provide an elegant and efficient use of limited resources for a small experiment.

Disadvantages of Latin Squares

- Cannot identify block-treatment interactions.
- There may be few $d.f.$ left to estimate σ once both block and treatment effects are estimated.
  E.g., in a 3×3 square, block and treatment effects taken up 3×2 $d.f.$, allowing 1 $d.f.$ for the grand mean leaves 2 $d.f.$ for estimating σ, and this includes all the treatments.
- One can (and should) replicate Latin square (Next Lecture).

Coming Up Next...

There are several extensions of RCBD

- 13.2 RCBD (done!) .... 1 blocking variable
- 13.3 Latin Square Design (almost done!) 2 blocking variables
- 13.4 Graeco-Latin Square Design .... 3 blocking variables
- Chapter 14 Incomplete Block Design
  (what if block size $\neq$ number of treatments?)