Unit 6: Orthogonal Designs Theory, Randomized Complete Block Designs, and Latin Squares

STA 643: Advanced Experimental Design

Derek S. Young
Learning Objectives

▶ Understand the basics of orthogonal designs theory
▶ Understand the use of blocking as a way to increase precision
▶ Become familiar with randomized complete block designs
▶ Know how to model and analyze blocking effects
▶ Become familiar with some variations on the randomized complete block design
▶ Know how to construct and model a Latin square design
▶ Understand the necessary arrangement of treatments for a Latin square design
▶ Understand replicated Latin square designs
▶ Be familiar with the usage of Graeco-Latin squares for more than two blocking
Outline of Topics

1. Orthogonal Designs Theory

2. Blocking, Randomized Complete Block Designs, and Relative Efficiency

3. Latin Squares

4. Replicated Latin Squares and Graeco-Latin Squares
Outline of Topics

1. Orthogonal Designs Theory

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4. Replicated Latin Squares and Graeco-Latin Squares
Orthogonality in the Linear Model

- In the one-way model, it is fairly straightforward to compute the least squares estimators of the parameters for both the full and the reduced models.
- However, for many more complicated ANOVA models associated with a given experimental design, it can be difficult to find the estimators for the full or reduced models.
- Many of the models we consider are orthogonal designs, which are particular kind of linear model for which derivations can be made more easily.
- We state most of our results in terms of vectors and subspaces, and then illustrate those results on the one-way ANOVA – in particular, for the single treatment effects model.
- The results easily extend to most of our standard ANOVA models.
Orthogonal Direct Sum

- Let $T_1, \ldots, T_r$ be subspaces in $\mathbb{R}^n$.
- We say that $V$ is the **orthogonal direct sum** of the $T_i$ and write

$$V = T_1 \oplus T_2 \oplus \cdots \oplus T_r = \bigoplus_{q=1}^{r} T_q \equiv \bigoplus T_q$$

if

$$V = \left\{ v = \sum_{q=1}^{r} t_q, \ t_q \in T_q \right\}, \ T_q \perp T_s \ \forall q \neq s. \quad (2)$$

- Note that $T_i \subset \bigoplus T_q = V$, therefore $T_i \subset V$.
- Note also that

$$P_{T_q} t_q = t_q, \ P_{T_q} t_s = 0, \ \forall q \neq s. \quad (3)$$

- Therefore,

$$v = \sum t_q \Rightarrow P_{T_q} v = t_q,$$

so that the $t_q$ are uniquely defined in the above representation.
Orthogonal Direct Sums Theorem

**Theorem 1**

Let \( V = \bigoplus T_q \).

1. \( V \) is a subspace and \( \dim(V) = \sum_q \dim(T_q) \).
2. \( P_V Y = \sum_q P_{T_q} Y, \|P_V Y\|^2 = \sum_q \|P_{T_q} Y\|^2, P_V = \sum_q P_{T_q} \).
3. Let \( W_q = \bigoplus_{s \neq q} T_s \). Then \( V|W_q = T_q \).

**Proof.**

1. \( V \) is a subspace following (2) and the definition of a subspace. Next, \( \dim(V) = \text{tr}(P_V) = \sum_q \text{tr}(P_{T_q}) = \sum_q \dim(T_q) \).
2. Note that \( P_V Y \in V \implies P_V Y = \sum_q t_q, t_q = P_{T_q} P_V Y = P_{T_q} Y \). The remaining results are straightforward.
3. Note that \( W_q \subset V \) and

\[
P_{V|W_q} = P_V - P_{W_q} = \sum_{i=1}^{r} P_{T_i} - \sum_{i \neq q} P_{T_i} = P_{T_q}.
\]
An orthogonal design is a model in which we observe
\[ Y \sim \mathcal{N}(\delta_1 + \delta_2 + \cdots + \delta_r, \sigma^2 I), \quad \delta_q \in T_q, \ q = 1, \ldots, r, \ \sigma^2 > 0, \]
where \( \delta_1, \ldots, \delta_r \) and \( \sigma^2 \) are unknown parameters and the \( T_q \) are known subspaces satisfying
\[ T_q \perp T_s, \ q \neq s, \ \dim(T_q) = d_q. \]

To make the above model into a linear model, let
\[ \mu = \delta_1 + \cdots + \delta_r, \ V = \bigoplus T_q. \]

Then,
\[ Y \sim \mathcal{N}(\mu, \sigma^2 I), \quad \mu \in V, \]
so that this is a linear model.

Note that the values \( \hat{\delta}_1, \ldots, \hat{\delta}_r \) that minimize
\[ Q(\delta_1, \ldots, \delta_r) = \|Y - \mu\|^2 = \|Y - \sum_q \delta_q\|^2 \]
are the ordinary least squares estimators (as well as MLEs) of \( \delta_1, \ldots, \delta_r \).
Inference with Orthogonal Designs

We now state a theorem, without proof, regarding inference in orthogonal designs.

Theorem 2

1. For testing $\delta_q = 0$,

$$SSH = \|\hat{\delta}_q\|^2 = SS_q, \quad df_h = d_q, \quad \xi = \|\delta_q\|^2,$$

$$SSE = \|Y - \hat{\mu}\|^2, \quad df_e = N - \sum_q d_q.$$  

2. The Scheffé simultaneous confidence intervals for contrasts associated with the above hypothesis are given by

$$c_q^T \delta_q \in c_q^T \hat{\delta}_q \pm \sqrt{d_q \text{MSE}} \|c_q\|^2 F_{1-\alpha, d_q, N-\sum_q d_q}, \quad \forall c_q \in T_q.$$
Example: One-Way ANOVA

Consider the one-way ANOVA model for an unbalanced design with a single factor. We observe $Y_{ij}$, such that they are iid according to $\mathcal{N}(\mu_i, \sigma^2)$, $i = 1, \ldots, p$, $j = 1 \ldots, n_i$. Let

$$Y = (Y_{11}, \ldots, Y_{1n_1}, Y_{21}, \ldots, Y_{pn_p})^T, \quad \mu = \text{E}(Y) = (\mu_1, \ldots, \mu_1, \mu_2, \ldots, \mu_p)^T,$$

where $\mu_i$ is repeated $n_i$ times. To make this model an orthogonal design, let

$$N = \sum_i n_i, \quad \theta_i = \frac{\sum_i n_i \mu_i}{N}, \quad \alpha_i = \mu_i - \theta.$$

Then $\mu_i = \theta + \alpha_i$, $\sum_i n_i \alpha_i = 0$. Let $\delta_1 = \theta 1_N$, $\delta_2 = (\alpha_1, \ldots, \alpha_1, \alpha_2, \ldots, \alpha_p)^T$, where $\alpha_i$ is repeated $n_i$ times. Then,

$$\mu = \delta_1 + \delta_2, \quad \delta_1^T \delta_2 = \sum_i \sum_j \theta \alpha_i = \theta \sum_i n_i \alpha_i = 0,$$

so that $\delta_1$ and $\delta_2$ are orthogonal; i.e., $\theta$ and the $\alpha_i$ are orthogonal. Note that the constraint $\sum_i n_i \alpha_i$ is the only constraint which makes these effects orthogonal. There are one $\theta$ and $p - 1$ linearly independent $\alpha_i$, therefore,

$$d_1 = 1, \quad d_2 = p - 1, \quad df_e = N - (p - 1) - 1 = N - p.$$
Example: One-Way ANOVA

To find the OLS estimators of $\theta$ and $\alpha_i$, we must minimize

$$Q \equiv Q(\theta, \alpha_1, \ldots, \alpha_p) = \| Y - \delta_1 - \delta_2 \|^2 = \sum_i \sum_j (Y_{ij} - \theta - \alpha_i)^2,$$

subject to $\sum_i n_i \alpha_i = 0$. Note that the constraint on the $\alpha_i$ implies the following:

$$\frac{\partial Q}{\partial \theta} =$$

$$\frac{\partial Q}{\partial \alpha_i} =$$
Example: One-Way ANOVA

Hence, the OLS estimator of $\mu_{ij} = \theta + \alpha_i$ is

$$\hat{\mu}_{ij} = \hat{\theta} + \hat{\alpha}_i = \bar{Y}_i. \quad \Rightarrow \quad \text{SSE} = \| Y - \hat{\mu} \|^2 = \sum_i \sum_j (Y_{ij} - \bar{Y}_i)^2,$$

which gives us $\text{df}_e = N - p$ and $\text{MSE} = \text{SSE}/\text{df}_e$. Therefore,

$$\text{SS}(\theta) = \| \hat{\delta}_1 \|^2 = \sum_i \sum_j \hat{\theta}^2 = N \bar{Y}^2, \quad \text{df}_\theta = 1, \quad \text{MS}(\theta) = \text{SS}(\theta),$$

$$\text{SS}(\alpha) = \| \hat{\delta}_2 \|^2 = \sum_i \sum_j \hat{\alpha}_i^2 = \sum_i n_i (\bar{Y}_i - \bar{Y})^2, \quad \text{df}_\alpha = p - 1, \quad \text{MS}(\alpha) = \frac{\text{SS}(\alpha)}{\text{df}_\alpha}.$$

In particular, to test that the $\mu_i$ are all equal or, equivalently, that the $\alpha_i = 0$, we reject if

$$F^* = \frac{\text{MS}(\alpha)}{\text{MSE}} > F_{1-\alpha^*, p-1, N-p},$$

where the $\alpha^*$ on the right-hand side is the significance level. Note that this test is, of course, the same that we derived without orthogonal designs.
Outline of Topics

1. Orthogonal Designs Theory

2. Blocking, Randomized Complete Block Designs, and Relative Efficiency

3. Latin Squares

4. Replicated Latin Squares and Graeco-Latin Squares
Blocking and Randomized Block Designs

- **Blocking** is a mechanism whereby we group the EUs into homogeneous blocks to compare treatments within a more uniform environment.

- Block designs help maintain internal validity, by reducing the possibility that the observed effects are due to a confounding factor, while maintaining external validity by allowing the investigator to use less stringent restrictions on the sampling population.

- Thus far, we have focused on CRDs where we simply randomly assign the treatments to the EUs available for our experiment.

- In a **randomized block design** or **RBD**, EUs are grouped into blocks that are thought to be similar such that the random assignment of units to treatments is done separately within each block.
  - The rationale for doing this is that, in the resulting dataset, the proportion of units receiving each treatment is identical across blocks.
  - If the blocking factor is related to the outcome, then blocking can substantially increase the precision of treatment comparisons over a CRD.
Example: Cholesterol-Reducing Drug

A clinical trial will be conducted to compare the performance of a new experimental cholesterol-reducing drug (Compound X) against the industry’s current leader (Lipitor). Twenty subjects will be recruited to participate in the trial at each of ten sites, for a total of \( N = 200 \) subjects. In a CRD, the 200 subjects would be randomly divided into two groups of 100 subjects each. The first group would receive Compound X, the second group would receive Lipitor. The analysis would be a standard one-way ANOVA with two groups. The fact that the subjects came from different sites is not used in the design and does not need to be used in the analysis. The treatment effect can be tested by a standard one-way ANOVA with two groups; i.e. a pooled two-sample \( t \)-test, with \( \text{df}_E = 198 \).
Example: Cholesterol-Reducing Drug

In an RBD, the randomization would be performed separately within each site. In each site, we would randomly assign 10 subjects to Compound X and 10 to Lipitor. The advantage of doing it this way is that the treatment groups will be balanced in the sense that the proportions of subjects in Site 1, Site 2, ... , Site 10 within the Compound X group will be identical to the proportions of subjects in Site 1, Site 2, ... , Site 10 within the Lipitor group. If the randomization is done this way, then the blocking factor (site) should not be ignored in the analysis. We will have to fit a linear model that allows site effects to be present. It turns out that the test for a treatment effect based on this model will be equivalent to (a) computing

\[ \bar{Y}_{1j} = \text{mean response for Compound X in site } j \] and

\[ \bar{Y}_{2j} = \text{mean response for Lipitor in site } j, \]

for each site \( j = 1, \ldots, 10 \), and (b) comparing the \( \bar{Y}_{1j} \)s to the \( \bar{Y}_{2j} \)s by a paired \( t \)-test with error \( \text{df}_E = 9 \).
Randomized Complete Block Designs

- When we have a single blocking factor, we will do our best to utilize a randomized complete block design or RCBD, where the EUs are stratified into blocks of homogenous units and each treatment is assigned randomly to an equal number (usually one) of EUs in each block.

- Note that the difference between an RBD and an RCBD is that complete set of treatments appear in each block in the latter design.
  - More precise comparisons are possible among treatments within the homogeneous set of EUs in a block.
  - RCBDs are the simplest blocking designs used for controlling and reducing experimental error.

- Let us illustrate RCBDs with two examples.
Example: Newspaper Advertising

In an experiment on the effects of four levels of newspaper advertising saturation on sales volume, the EU is a city, and 16 cities are available for the study. Size of city is usually highly correlated with the response variable, sales volume. Hence, it is desirable to block the 16 cities into four groups of four cities each, according to population size. Thus, the four largest cities will constitute block 1, the next four largest cities will constitute block 2, and so on. Within each block, the four treatments are then assigned, at random, to the four cities, and the assignments from one block to another are made independently.

1. What is the response?
2. What is the EU?
3. What is the treatment?
4. What is the blocking factor?
Example: Chemical Reaction Rates

A chemist is studying the reaction rate of five chemical agents when added to 10 mL of water. Only five agents can be analyzed effectively per day. Since day-to-day differences may affect the reaction rate, each day is used as a block. All five chemical agents are tested each day in independently randomized orders.

1. What is the response?
2. What is the EU?
3. What is the treatment?
4. What is the blocking factor?
Criteria for Blocking

▶ To help recognize some of the characteristics of EUs that are effective criteria for blocking, we need a precise definition of an EU.

▶ All elements of the experimental situation that are not included in the definition of a treatment need to be assigned to the definition of an EU.
  ▶ For example, suppose the treatment in an experiment consists of a portion of a vegetable containing a particular additive served in a laboratory. The EU might then be defined as a homemaker of a given age, processed by a given observer on a specified day during a particular part of the day, and then served food from a given batch of cooked vegetable.

▶ Still, other elements of the experimental setting might be included in the definition of the EU, and should be if they could be the cause of material variability in the response.

▶ There are basically two types of blocking criteria:
  1. Characteristics associated with the unit – for persons: gender, age, income, intelligence, attitudes, etc.; for geographic areas: population size, average income, etc.
  2. Characteristics associated with the experimental settings – observer, time of processing, batch of material, measuring instrument, etc.

▶ Blocking by time often captures different sources of variability; e.g., learning by observer, changes in equipment, and drifts in environmental conditions.

▶ Blocking by observer often eliminates interobserver variability, while blocking by batch has a similar effect when the EU is some sort of material.
Advantages of RCBDs

1. It can, with effective grouping, provide substantially more precise results than a CRD of comparable size.

2. It can accommodate any number of treatments and replications.

3. Different treatments need not have equal sample size. For example, if the control is to have twice as large a sample size as each of the three treatments, blocks of size five would be used; three units in a block are then assigned at random to the three treatments and two to the control.

4. The statistical analysis is relatively simple.

5. If an entire treatment or a block needs to be dropped from the analysis for some reason (e.g., spoiled results, failure of machinery), the analysis is not complicated thereby.

6. Variability in EUs can be deliberately introduced to widen the range of validity of the experimental results without sacrificing the precision of the results.
Disadvantages of RCBDs

1. Missing observations within a block require more complex analysis.
2. The df for experimental error are not as large as with CRDs since one df is lost for each block after the first.
3. More assumptions are required for the model (e.g., no interactions between treatments and blocks, constant variance from block to block) than for a CRD model.
4. In the absence of any information on the effectiveness of potential blocking, a blocking criterion might have to be chosen in a manner such as through uniformity trials, where all EUs are assigned to the same treatment and then information can be obtained about the effectiveness of different blocking variables. However, such a strategy could be costly or have certain practical constraints.
How to Randomize in an RCBD

▷ While different treatments need not have equal sample sizes, the randomization of such a scenario is a bit more complicated.

▷ For now, we consider the random allocation of treatments to EUs in an RCBD such that each treatment occurs an equal number of times (once or more) within each block.

▷ A random permutation of the order in which the treatments are placed with the units in each block provides a random allocation of treatments to units.

▷ One permutation is randomly selected for each block since a separate randomization is required for each block.

    ▷ For example, if \( t \) treatments are to be allocated to \( t \) EUs in each block, then randomly generate a permutation of the integers \( 1, 2, \ldots, t \) to use for the allocation.
Example: Newspaper Advertising

Let us return to the newspaper advertising example to illustrate how to randomize in such an RCBD setting. The newspaper advertising saturation is the treatment, which has 4 levels. For convenience, denote these levels as A, B, C, and D. We will then assign the integers 1, 2, 3, and 4 to each treatment level. Recall that there are four blocks in this study based on population size, which we label as largest, second largest, second smallest, and smallest, respectively. We will randomly generate a permutation of the integers 1, 2, 3, and 4 for each block. This will determine how we assign the treatments to each EU in the blocks. A randomized assignment of treatments in this RCBD is given in the table below. Notice how the allocation in the second largest and smallest blocks are the same permutation. The same allocations can be selected since there are only \(4! = 24\) possible permutations of four treatments.

<table>
<thead>
<tr>
<th>Block</th>
<th>EU</th>
</tr>
</thead>
<tbody>
<tr>
<td>Largest</td>
<td>B A D C</td>
</tr>
<tr>
<td>Second Largest</td>
<td>B D C A</td>
</tr>
<tr>
<td>Second Smallest</td>
<td>C A B D</td>
</tr>
<tr>
<td>Smallest</td>
<td>B D C A</td>
</tr>
</tbody>
</table>
The RCBD model with no interaction effects, when both the block and treatment are treated as fixed effects, is as follows:

\[ Y_{ij} = \mu + \tau_i + \rho_j + \epsilon_{ij}, \]

where

- \( \mu \) is the overall mean (a constant);
- \( i = 1, \ldots, t \) and \( j = 1, \ldots, n \);
- \( \tau_i \) is the treatment effect at level \( i \) and is subject to the constraint \( \sum_{i=1}^{t} \tau_i = 0 \);
- \( \rho_j \) is the block effect (sometimes called row effect) at block \( j \) and is subject to the constraint \( \sum_{j=1}^{n} \rho_j = 0 \); and
- \( \epsilon_{ij} \) is the experimental error and are \( iid \) normal with mean 0 and variance \( \sigma^2 \).

Note that the RCBD model is identical to the two-factor, no interaction model, except now we have a treatment effect and block effect instead of two factor effects.
SS for RCBDs

▶ At the bottom is a table for a general RCBD.
▶ We use the following identity to break out the treatment and block effects:

\[(y_{ij} - \bar{y}. \cdot) = (\bar{y}_i \cdot - \bar{y}. \cdot) + (\bar{y}. j - \bar{y}. \cdot) + (y_{ij} - \bar{y}_i \cdot - \bar{y}. j + \bar{y}. \cdot)\]

▶ The components in the above decomposition are:

- total deviation: \((y_{ij} - \bar{y}. \cdot)\)
- treatment deviation: \((\bar{y}_i \cdot - \bar{y}. \cdot)\)
- block deviation: \((\bar{y}. j - \bar{y}. \cdot)\)
- experimental error: \((y_{ij} - \bar{y}_i \cdot - \bar{y}. j + \bar{y}. \cdot)\)

<table>
<thead>
<tr>
<th>Treatment</th>
<th>1</th>
<th>2</th>
<th>\cdots</th>
<th>n</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>(y_{11})</td>
<td>(y_{12})</td>
<td>\cdots</td>
<td>(y_{1n})</td>
</tr>
<tr>
<td>2</td>
<td>(y_{21})</td>
<td>(y_{22})</td>
<td>\cdots</td>
<td>(y_{2n})</td>
</tr>
<tr>
<td>\vdots</td>
<td>\vdots</td>
<td>\vdots</td>
<td>\ddots</td>
<td>\vdots</td>
</tr>
<tr>
<td>(t)</td>
<td>(y_{t1})</td>
<td>(y_{t2})</td>
<td>\cdots</td>
<td>(y_{tn})</td>
</tr>
<tr>
<td>Block Means</td>
<td>(\bar{y}_1)</td>
<td>(\bar{y}_2)</td>
<td>\cdots</td>
<td>(\bar{y}_n)</td>
</tr>
</tbody>
</table>
SS for RCBDs

- Taking the SS of the decomposition on the previous slide (where the sums of cross-products drop out), we get

  Block effect: \( \text{SSBlk} = n \sum_{i=1}^{t} (\bar{y}_i - \bar{y}.)^2 \)

  Treatment effect: \( \text{SSTr} = t \sum_{j=1}^{n} (\bar{y}.j - \bar{y}.)^2 \)

  Error: \( \text{SSE} = \sum_{i=1}^{a} \sum_{j=1}^{b} (y_{ij} - \bar{y}_i - \bar{y}.j + \bar{y}.)^2 \),

which can then be summarized in the ANOVA table:

<table>
<thead>
<tr>
<th>Source</th>
<th>df</th>
<th>SS</th>
<th>MS</th>
<th>E(MS)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Blocks</td>
<td>( n - 1 )</td>
<td>SSBlk</td>
<td>MSBlk</td>
<td>( \sigma^2 + t \frac{\sum_{i} \rho_i^2}{n-1} )</td>
</tr>
<tr>
<td>Treatments</td>
<td>( t - 1 )</td>
<td>SSTr</td>
<td>MSTr</td>
<td>( \sigma^2 + n \frac{\sum_{j} \tau_j^2}{t-1} )</td>
</tr>
<tr>
<td>Error</td>
<td>((n - 1)(t - 1))</td>
<td>SSE</td>
<td>MSE</td>
<td>( \sigma^2 )</td>
</tr>
<tr>
<td>Total</td>
<td>( nt - 1 )</td>
<td>SSTot</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Inference About Treatment Means

- The standard error estimate for a treatment mean is
  
  \[ s_{\bar{y}_i} = \sqrt{\frac{\text{MSE}}{n}} \]

- The standard error estimate of a difference between any two treatment means is
  
  \[ s_{\bar{y}_i - \bar{y}_{i'}} = \sqrt{\frac{2\text{MSE}}{n}}, \quad i \neq i' \]

- The \( F \)-statistic for testing
  
  \[ H_0 : \text{all } \tau_j = 0 \]
  
  \[ H_A : \text{at least one } \tau_j \text{ does not equal 0.} \]

  \[ F^* = \frac{\text{MSTr}}{\text{MSE}}, \]

  which follows an \( F \)-distribution with df \( df_{\text{Tr}} = (t - 1) \) and \( df_{\text{E}} = (n - 1)(t - 1) \).
Example: Decision Making Study

In an experiment on decision making, executives were exposed to one of three methods \((t = 3)\) of quantifying the maximum risk premium they would be willing to pay to avoid uncertainty in a business decision. The three methods are the *utility method* \((U)\), the *worry method* \((W)\), and the *comparison method* \((C)\). After using the assigned method, the subjects were asked to state their degree of confidence in the method of quantifying the risk premium on a scale from 0 (no confidence) to 20 (highest confidence). \(N = 15\) subjects were used in the study. They were grouped into \(n = 5\) blocks of three executives, according to age. Block 1 contained the three oldest executives, block 2 contained the next three oldest executives, and so on. Below is the layout for the RCBD for this study:

<table>
<thead>
<tr>
<th>Experimental Unit</th>
<th>1</th>
<th>2</th>
<th>3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Block 1 (oldest executives)</td>
<td>C</td>
<td>W</td>
<td>U</td>
</tr>
<tr>
<td>Block 2</td>
<td>C</td>
<td>U</td>
<td>W</td>
</tr>
<tr>
<td>Block 3</td>
<td>U</td>
<td>W</td>
<td>C</td>
</tr>
<tr>
<td>Block 4</td>
<td>W</td>
<td>U</td>
<td>C</td>
</tr>
<tr>
<td>Block 5 (youngest executives)</td>
<td>W</td>
<td>C</td>
<td>U</td>
</tr>
</tbody>
</table>
Example: Decision Making Study

Below is the data for the experiment on decision making:

<table>
<thead>
<tr>
<th>Block (i)</th>
<th>Method (j)</th>
<th>( \bar{y}_j )</th>
<th>( \bar{y}_i )</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Utility</td>
<td>Worry</td>
<td>Comparison</td>
</tr>
<tr>
<td>1</td>
<td>1</td>
<td>5</td>
<td>8</td>
</tr>
<tr>
<td>2</td>
<td>2</td>
<td>8</td>
<td>14</td>
</tr>
<tr>
<td>3</td>
<td>7</td>
<td>9</td>
<td>16</td>
</tr>
<tr>
<td>4</td>
<td>6</td>
<td>13</td>
<td>18</td>
</tr>
<tr>
<td>5</td>
<td>12</td>
<td>14</td>
<td>17</td>
</tr>
<tr>
<td>( \bar{y}_j )</td>
<td>5.6</td>
<td>9.8</td>
<td>14.6</td>
</tr>
<tr>
<td>( \bar{y}_i )</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Example: Decision Making Study

- To the right is a treatment means plot of the decision making data.
- The $x$-axis has the decision making strategy factor, the $y$-axis has the degree of confidence response, and the profiles are broken out by the blocking factor.
- Clearly, there appears to be a significant blocking effect.
Example: Decision Making Study

Analysis of Variance Table

Response: rating

<table>
<thead>
<tr>
<th>Df</th>
<th>Sum Sq</th>
<th>Mean Sq</th>
<th>F value</th>
<th>Pr(&gt;F)</th>
</tr>
</thead>
<tbody>
<tr>
<td>block</td>
<td>4</td>
<td>171.333</td>
<td>42.833</td>
<td>14.357</td>
</tr>
<tr>
<td>method</td>
<td>2</td>
<td>202.800</td>
<td>101.400</td>
<td>33.989</td>
</tr>
<tr>
<td>Residuals</td>
<td>8</td>
<td>23.867</td>
<td>2.983</td>
<td></td>
</tr>
</tbody>
</table>

Signif. codes:  0 *** 0.001 ** 0.01 * 0.05 . 0.1  1

Above is the ANOVA output for the decision making study. As we noted with the treatment means plot, the block effect is highly significant with a \( p \)-value of 0.0010. While significant, how do we measure the increase in precision?
Measuring Increased Precision

- The expectation of increased precision in the estimates of treatment means motivates us to use the RCBD.
- Planning and conducting an experiment with the RCBD requires extra effort relative to the CRD.
- We can turn to the relative efficiency measure as a way to evaluate the benefit of blocking for a particular experiment.
- The relative efficiency measure requires an estimate of $\sigma^2$ from the CRD (which was not used), which is estimated by

$$s^2_{CRD} = \frac{SSBlk + n(t - 1)MSE}{nt - 1}$$
Example: Decision Making Study

The estimate of $\sigma^2$ from the CRD is

$$s^2_{CRD} = \frac{171.333 + 5(3 - 1)(2.983)}{(5)(3) - 1} = \frac{201.163}{14} = 14.369$$

The relative efficiency estimate, without df correction for estimates of $\sigma^2$, is

$$RE = \frac{s^2_{CRD}}{s^2_{RCBD}} = \frac{14.369}{2.983} = 4.817$$

The correction for estimation of $\sigma^2$ by $s^2$ is

$$\frac{(f_{RCBD} + 1)(f_{CRD} + 3)}{(f_{RCBD} + 3)(f_{CRD} + 1)} = \frac{(9)(15)}{(11)(13)} = 0.944,$$

where $f_{RCBD} = 8$ and $f_{CRD} = 12$ are the error df for the RCBD and CRD, respectively. The correction reduces the RE to $(0.944)(4.817)=4.547$. The correction has little effect with moderately sized df for experimental error variance estimates.

From the above, we see that the RCBD for this experiment is estimated to be about 4.5 times as efficient as the CRD.
The assumption of no treatment with block interaction implies that the treatment and block effects are additive.

The differences among the treatments are assumed to be relatively constant from block to block as a consequence of additive block and treatment effects, even though the use of blocking may result in large differences in responses between EUs from different blocks.

To formally check for nonadditivity, one can use the Tukey’s test for nonadditivity (also called Tukey’s additivity test) and proceed to use that same strategy in the presence of a possible interaction.
Tukey’s Additivity Test

- The procedure assumes that the interaction term is of a particularly simple form, namely \((\tau \rho)_{ij} = \gamma \tau_i \rho_j\), where \(\gamma\) is some unknown constant.
- We then test \(H_0 : \gamma = 0\) versus \(H_A : \gamma \neq 0\).
- The test partitions the SSE into a single-degree-of-freedom component for nonadditivity (the interaction) and a component for the remaining error with \((t - 1)(n - 1) - 1\) degrees of freedom:

  nonadditivity: \(SSN = \left[ \sum_{i=1}^{t} \sum_{j=1}^{n} y_{ij} y_i \cdot y_j - y_\cdot \cdot \cdot (SSBlk + SSTr + y_\cdot \cdot^2 / (tn)) \right]^2\)

  remaining error: \(SSE_2 = SSE - SSN\)

- Calculate the \(F\)-statistic for the presence of an interaction as follows:

  \[ F^* = \frac{SSN}{SSE_2 / [(t - 1)(n - 1) - 1]}, \]

  which follow an \(F_{1, (t-1)(n-1)-1}\)-distribution.
Multiple EUs per Treatment in Each Block

- We can test the existence of interaction when more than one EU for each treatment is measured within each block; i.e., when replicates are available.
- The model is similar to the two-factor model with interactions:

\[ Y_{ijk} = \mu + \tau_i + \rho_j + (\tau\rho)_{ij} + \epsilon_{ijk}, \]

where the same assumptions in the non-interaction model hold, but we also have:

- \( k = 1, \ldots, m; \)
- \((\tau\rho)_{ij}\) is the interaction of treatment level \( i \) at block \( j \) and is subject to the constraint \( \sum_{i=1}^{t} (\tau\rho)_{ij} = \sum_{j=1}^{n} (\tau\rho)_{ij} = 0; \) and
- \( \epsilon_{ijk} \) is the experimental error and are iid normal with mean 0 and variance \( \sigma^2 \).

- The computations are the same as for the two-factor with interactions model presented earlier.
Outline of Topics

1. Orthogonal Designs Theory

2. Blocking, Randomized Complete Block Designs, and Relative Efficiency

3. Latin Squares

4. Replicated Latin Squares and Graeco-Latin Squares
Needing Two Blocking Factors

- In some experimental settings, two factors (other than treatments) may influence the response variable.
- Even more precision can be achieved if we can block the units on the basis of the two factors.
- **Latin squares designs** allow for two blocking factors and, thus, can be used to simultaneously control (or eliminate) two sources of nuisance variability.
- The Latin square arrangement derives from an arrangement of the Latin letters A, B, C, D, ... into a square array such that each letter appears once in each column and once in each row of the square design.
- In applications to experiments, the rows and columns of the array are identified with the two blocking criteria and the Latin letters are identified with the treatments.
Example: Car Tire Treatments

An experiment to test car tire treatments is a classic example used to illustrate the Latin square design. The experiment tests $t = 4$ car tire treatments (A, B, C, and D) on four tire positions of each car. The row and column blocking criteria are the cars and tire positions, respectively. See the table below for the design. Each treatment appears once in each row (the car) and once in each column (the tire position). The rationale for the blocking criteria is that the wear on the tires may differ among the cars and the positions in which the tires are mounted on the cars. (Note that the design below is just one of many $4 \times 4$ designs that you could create. You can make any size square you want for any number of treatments as long as it has the property that each treatment occurs only once in each row and once in each column.)

<table>
<thead>
<tr>
<th>Car</th>
<th>Tire Position</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1  2  3  4</td>
</tr>
<tr>
<td>1</td>
<td>A  B  C  D</td>
</tr>
<tr>
<td>2</td>
<td>B  C  D  A</td>
</tr>
<tr>
<td>3</td>
<td>C  D  A  B</td>
</tr>
<tr>
<td>4</td>
<td>D  A  B  C</td>
</tr>
</tbody>
</table>
Using Standard Latin Squares for Design Generation

- All Latin squares of a specified size can be generated form the **standard squares**, which involves writing the treatment symbols (e.g., A, B, C, etc.) in alphabetical order in the first row and in the first column of the array.

- Each treatment symbol occurs only once in each column and once in each row of the array.

- Only one standard square exists for \( t = 2 \) or 3 treatments.

- There are 4 standard squares with \( t = 4 \) treatments.
  - The car tire experiment used one of these standard squares.

- There are 56 standard squares with \( t = 5 \) treatments.

- The number of squares increases dramatically with the number of treatments; e.g., there are 9408 standard squares with \( t = 6 \) treatments!

- There are publications (including our textbook) that provide tables of some standard squares.
Constructing a Standard Latin Square

A standard square of any size can be generated by the following:

1. Write the first row of letters in alphabetical order.

2. In the second row, shift the letter A to the extreme right-hand position while shifting all other letters (treatments) one position to the left.

3. Following the order of the second row, in the third row we shift the letter B to the extreme right-hand position while shifting all other letters (treatments) one position to the left.

4. Continue the above process for the remaining rows.

Below is the standard $5 \times 5$ Latin square constructed using the above steps:

\[
\begin{array}{ccccc}
A & B & C & D & E \\
B & C & D & E & A \\
C & D & E & A & B \\
D & E & A & B & C \\
E & A & B & C & D \\
\end{array}
\]
How to Randomize the Design

▶ If all standard Latin squares of size $t \times t$ are available, randomization is accomplished using the following steps:

1. Randomly select one of the standard squares.
2. Randomly order all but the first row.
3. Randomly order all columns.
4. Randomly assign treatments to the letters.

▶ All possible randomizations can be generated without including the first row in Step 2 if a standard square is randomly selected.

▶ If all standard squares are not available for selection, then it is recommended in Step 2 that all rows be included in the randomization.

▶ Not all possible Latin squares can be generated using the above steps, but the number of possibilities is increased considerably.
Example: Randomizing a $5 \times 5$ Latin Square Design

We proceed to illustrate how to randomize a $5 \times 5$ Latin square design using the steps we just outlined.

Step 1 Suppose the standard square selected for this design is as follows:

$$
\begin{array}{|c|c|c|c|c|}
\hline
A & B & C & D & E \\
\hline
B & C & D & E & A \\
\hline
C & D & E & A & B \\
\hline
D & E & A & B & C \\
\hline
E & A & B & C & D \\
\hline
\end{array}
$$
Example: Randomizing a $5 \times 5$ Latin Square Design

**Step 2** Obtain a random permutation of numbers to order the last four rows:

<table>
<thead>
<tr>
<th>Permutation</th>
<th>Original Row</th>
</tr>
</thead>
<tbody>
<tr>
<td>3</td>
<td>2</td>
</tr>
<tr>
<td>4</td>
<td>3</td>
</tr>
<tr>
<td>2</td>
<td>4</td>
</tr>
<tr>
<td>1</td>
<td>5</td>
</tr>
</tbody>
</table>

The placement of the rows for the standard square with row 1 in its original position is:

<table>
<thead>
<tr>
<th>Original Row</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
</tr>
<tr>
<td>5</td>
</tr>
<tr>
<td>4</td>
</tr>
<tr>
<td>2</td>
</tr>
<tr>
<td>3</td>
</tr>
</tbody>
</table>
Example: Randomizing a $5 \times 5$ Latin Square Design

**Step 3** Obtain a random permutation of numbers to order the five columns from Step 2:

<table>
<thead>
<tr>
<th>Permutation</th>
<th>Original Column</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>4</td>
<td>4</td>
</tr>
<tr>
<td>5</td>
<td>3</td>
</tr>
<tr>
<td>2</td>
<td>5</td>
</tr>
<tr>
<td>3</td>
<td>1</td>
</tr>
</tbody>
</table>

The placement of the columns for the standard square is:

<table>
<thead>
<tr>
<th>Original Column</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>B</td>
<td>E</td>
<td>A</td>
<td>D</td>
<td>C</td>
<td></td>
</tr>
<tr>
<td>A</td>
<td>D</td>
<td>E</td>
<td>C</td>
<td>B</td>
<td></td>
</tr>
<tr>
<td>E</td>
<td>C</td>
<td>D</td>
<td>B</td>
<td>A</td>
<td></td>
</tr>
<tr>
<td>C</td>
<td>A</td>
<td>B</td>
<td>E</td>
<td>D</td>
<td></td>
</tr>
<tr>
<td>D</td>
<td>B</td>
<td>C</td>
<td>A</td>
<td>E</td>
<td></td>
</tr>
</tbody>
</table>
Example: Randomizing a $5 \times 5$ Latin Square Design

**Step 4** Obtain a random permutation to assign treatments to the letters. This assignment is not necessary if the standard square has been selected at random from all possible standard squares. The method of assignment is shown here for illustrative purposes. Suppose we use the treatment labels V, W, X, Y, and Z:

<table>
<thead>
<tr>
<th>Permutation</th>
<th>Treatment</th>
</tr>
</thead>
<tbody>
<tr>
<td>5 = E</td>
<td>V</td>
</tr>
<tr>
<td>1 = A</td>
<td>W</td>
</tr>
<tr>
<td>2 = B</td>
<td>X</td>
</tr>
<tr>
<td>3 = C</td>
<td>Y</td>
</tr>
<tr>
<td>4 = D</td>
<td>Z</td>
</tr>
</tbody>
</table>

The new treatment labels V, W, X, Y, and Z will replace the respective Latin square letters in the order E, A, B, C, and D. The final design is given by:

<table>
<thead>
<tr>
<th>Row Factor</th>
<th>Column Factor</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
</tr>
<tr>
<td>1</td>
<td>X</td>
</tr>
<tr>
<td>2</td>
<td>W</td>
</tr>
<tr>
<td>3</td>
<td>V</td>
</tr>
<tr>
<td>4</td>
<td>Y</td>
</tr>
<tr>
<td>5</td>
<td>Z</td>
</tr>
</tbody>
</table>
Latin Square Design Model

- The linear statistical model for an experiment with \( t \) treatments in a \( t \times t \) Latin square design is as follows:

\[
Y_{ij} = \mu + \rho_i + \gamma_j + \tau_k + \epsilon_{ij},
\]

where

- \( Y_{ij} \) is the observation on the EU in the \( i^{th} \) row of the \( j^{th} \) column;
- \( \mu \) is the overall mean (a constant);
- \( i = 1, \ldots, t, \; j = 1, \ldots, t, \; \text{and} \; k = 1, \ldots, t; \)
- \( \rho_i \) and \( \gamma_j \) are the row and column effects, respectively, such that \( \sum_{i=1}^{t} \rho_i = \sum_{j=1}^{t} \gamma_j = 0; \)
- \( \tau_k \) is the effect of the \( k^{th} \) treatment, such that \( \sum_{k=1}^{t} \tau_k = 0; \) and
- \( \epsilon_{ij} \) is the experimental error and are iid normal with mean 0 and variance \( \sigma^2. \)

- Note that there is no interaction between the treatment and the row or column effects.
- Column totals will be given by \( y_i. = \sum_j y_{ij} \) and row totals by \( y. j = \sum_i y_{ij}. \)
- Note that the EU is not indexed by the treatment, so we will represent treatment totals by \( y_k, \) which will be the sum of the observations over the \( t \) EUs receiving treatment \( k. \)
SS and ANOVA for Latin Squares Designs

▶ We use the following identity for the SS partition:

\[(\bar{y}_{ij} - \bar{y}_{..}) = (\bar{y}_i - \bar{y}_{..}) + (\bar{y}_j - \bar{y}_{..}) + (\bar{y}_k - \bar{y}_{..}) + (\bar{y}_{ij} - \bar{y}_i - \bar{y}_j + \bar{y}_k + 2\bar{y}_{..})\]

▶ The components in the above decomposition are:

- total deviation: \((\bar{y}_{ij} - \bar{y}_{..})\)
- row deviation: \((\bar{y}_i - \bar{y}_{..})\)
- column deviation: \((\bar{y}_j - \bar{y}_{..})\)
- treatment deviation: \((\bar{y}_k - \bar{y}_{..})\)
- experimental error: \((\bar{y}_{ij} - \bar{y}_i - \bar{y}_j - \bar{y}_k + 2\bar{y}_{..})\)

<table>
<thead>
<tr>
<th>Source</th>
<th>df</th>
<th>SS</th>
<th>MS</th>
<th>E(MS)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rows</td>
<td>(t - 1)</td>
<td>(t \sum_i (\bar{y}<em>i - \bar{y}</em>{..})^2)</td>
<td>MSR</td>
<td>(\sigma^2 + t \frac{\sum_i \rho_i^2}{t-1})</td>
</tr>
<tr>
<td>Columns</td>
<td>(t - 1)</td>
<td>(t \sum_j (\bar{y}<em>j - \bar{y}</em>{..})^2)</td>
<td>MSC</td>
<td>(\sigma^2 + t \frac{\sum_j \gamma_j^2}{t-1})</td>
</tr>
<tr>
<td>Treatments</td>
<td>(t - 1)</td>
<td>(t \sum_k (\bar{y}<em>k - \bar{y}</em>{..})^2)</td>
<td>MSTR</td>
<td>(\sigma^2 + t \frac{\sum_k \tau_k^2}{t-1})</td>
</tr>
<tr>
<td>Error</td>
<td>((t - 1)(t - 2))</td>
<td>SSE</td>
<td>MSE</td>
<td>(\sigma^2)</td>
</tr>
<tr>
<td>Total</td>
<td>(t^2 - 1)</td>
<td>(\sum_i \sum_j (y_{ij} - \bar{y}_{..})^2)</td>
<td></td>
<td>(\sigma^2)</td>
</tr>
</tbody>
</table>
Standard Errors and Testing

- The standard error estimate for a treatment mean is
  \[ s_{\bar{y}_k} = \sqrt{\text{MSE}/t} \]

  and the standard error estimate for a difference between two treatment means is
  \[ s_{\bar{y}_{k1} - \bar{y}_{k2}} = \sqrt{2\text{MSE}/t} \]

- For a Latin square design, the general linear model test of interest is for the hypothesis:
  \[ H_0 : \tau_k = 0 \quad \text{versus} \quad H_A : \tau_k \neq 0 \text{ for some } k \]

- The \( F \)-statistic for the above is
  \[ F^* = \frac{\text{MSTr}}{\text{MSE}} \sim F_{(t-1),(t-1)(t-2)} \]
Example: Rocket Propellant Experiment

A researcher conducted an experiment to study the burning rate of \( t = 5 \) different formulations of a rocket propellant. The formulations are mixed from raw material that comes in batches whose composition may vary. Furthermore, the formulations are prepared by several operators and there may be variability among the operators due to differences in their skills and experience. Thus, there are two (presumably) unrelated sources of error: different batches and different operators. We account for these errors using a Latin square design, where \( t = 5 \) operators are chosen at random (column effect) and \( t = 5 \) batches of raw material are selected at random (row effect), each one large enough for samples of all \( t = 5 \) formulations to be prepared. A sample from the one of the five batches (labelled at random I, II, III, IV, V) is assigned to one of the five operators (labelled at random 1, 2, 3, 4, 5) for preparation of one of the five formulations (labelled at random A, B, C, D, E) according to the following Latin square arrangement:

<table>
<thead>
<tr>
<th>Batches</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>( \bar{y}_{i}. )</th>
</tr>
</thead>
<tbody>
<tr>
<td>I</td>
<td>23 (A)</td>
<td>20 (B)</td>
<td>19 (C)</td>
<td>24 (D)</td>
<td>24 (E)</td>
<td>22.2</td>
</tr>
<tr>
<td>II</td>
<td>17 (B)</td>
<td>24 (C)</td>
<td>30 (D)</td>
<td>27 (E)</td>
<td>36 (A)</td>
<td>26.8</td>
</tr>
<tr>
<td>III</td>
<td>18 (C)</td>
<td>38 (D)</td>
<td>26 (E)</td>
<td>27 (A)</td>
<td>21 (B)</td>
<td>26.0</td>
</tr>
<tr>
<td>IV</td>
<td>26 (D)</td>
<td>31 (E)</td>
<td>26 (A)</td>
<td>23 (B)</td>
<td>22 (C)</td>
<td>25.6</td>
</tr>
<tr>
<td>V</td>
<td>22 (E)</td>
<td>30 (A)</td>
<td>20 (B)</td>
<td>29 (C)</td>
<td>31 (D)</td>
<td>26.4</td>
</tr>
</tbody>
</table>

\[ \bar{y}_{i}. = 25.4 \]

\[ \bar{y}_{j} = 21.4 \]
\[ \bar{y}_{k} = 28.6 \]
Example: Rocket Propellant Experiment

Below are the boxplots, broken out according to batches (left), operators (middle), and treatment (right). Clearly there is variability within each of the blocking factors and the treatment. One could proceed to try a variance-stabilizing transformation to improve the appearance of these data. However, we will forego such transformations and explore a direct analysis of a Latin square design. It appears that the greatest differences are observed with respect to the treatments.
Example: Rocket Propellant Experiment

Analysis of Variance Table

Response: response

<table>
<thead>
<tr>
<th></th>
<th>Df</th>
<th>Sum Sq</th>
<th>Mean Sq</th>
<th>F value</th>
<th>Pr(&gt;F)</th>
</tr>
</thead>
<tbody>
<tr>
<td>batches</td>
<td>4</td>
<td>68</td>
<td>17.000</td>
<td>1.5937</td>
<td>0.239059</td>
</tr>
<tr>
<td>operators</td>
<td>4</td>
<td>150</td>
<td>37.500</td>
<td>3.5156</td>
<td>0.040373 *</td>
</tr>
<tr>
<td>treatment</td>
<td>4</td>
<td>330</td>
<td>82.500</td>
<td>7.7344</td>
<td>0.002537 **</td>
</tr>
<tr>
<td>Residuals</td>
<td>12</td>
<td>128</td>
<td>10.667</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

---

Signif. codes: 0 *** 0.001 ** 0.01 * 0.05 . 0.1 1

As we mentioned earlier, we are typically interested in the test about the treatment means. In this case, the treatment is highly significant, with a p-value of 0.0025. Thus, the mean burning rate of rocket propellant differs according to the formulation used.
Did Blocking Increase Precision?

- The efficiency of the Latin square design with two blocking criteria is determined relative to the RCBD with only one blocking criterion.
- The relative efficiency measures can be computed separately for the row and column blocking criteria of the Latin squares.
- If only the row blocking criterion or column blocking criterion is used for blocking in a RCBD, the estimated MSE is, respectively,
  
  \[ s_{rcbd,\text{col}}^2 = \frac{(\text{MSC} + (t - 1)\text{MSE})}{t} \]
  
  \[ s_{rcbd,\text{row}}^2 = \frac{(\text{MSR} + (t - 1)\text{MSE})}{t}, \]

  which implies that the relative efficiency of column blocking and row blocking for the experiment is, respectively,

  \[ RE_{\text{col}} = \frac{s_{rcbd,\text{col}}^2}{s_{ls}^2} \]
  \[ RE_{\text{row}} = \frac{s_{rcbd,\text{col}}^2}{s_{ls}^2}, \]

  where \( s_{ls}^2 = \text{MSE} \) under the Latin square design.
- Then, the correction for estimating \( \sigma^2 \) by \( s^2 \) is given by

  \[ \frac{(f_{ls} + 1)(f_{rcbd} + 3)}{(f_{ls} + 3)(f_{rcbd} + 1)}, \]

  where \( f_{ls} \) and \( f_{rcbd} \) are the error df for the Latin square design and RCBD, respectively.
Example: Rocket Propellant Experiment

From the ANOVA output earlier, we know \( s_{ls}^2 = \text{MSE} = 10.667 \). The estimated MSE quantities when blocking only on row (batches) or column (operators) are, respectively,

\[
\begin{align*}
    s_{rcbd,\text{col}}^2 &= \frac{(37.500 + (4)10.667)}{5} = 16.034 \\
    s_{rcbd,\text{row}}^2 &= \frac{(17.000 + (4)10.667)}{5} = 11.934,
\end{align*}
\]

which means the respective relative efficiencies are

\[
RE_{\text{col}} = \frac{16.034}{10.667} = 1.503 \quad \text{and} \quad RE_{\text{row}} = \frac{11.934}{10.667} = 1.119
\]

Thus, there is a 50.3% gain in efficiency over the RCBD in which only the batches (row criterion) of the Latin square design is used for blocking, while there is a 11.9% gain in efficiency over the RCBD in which only the operators (column criterion) of the Latin square design is used. The correction for estimating \( \sigma^2 \) by \( s^2 \) is

\[
\frac{(f_{ls} + 1)(f_{rcbd} + 3)}{(f_{ls} + 3)(f_{rcbd} + 1)} = \frac{(12 + 1)(16 + 3)}{(12 + 3)(16 + 1)} = 0.969
\]

The correction reduces the \( RE_{\text{col}} \) from 1.503 to \((0.969)(1.503)=1.456\) and the \( RE_{\text{row}} \) from 1.119 to \((0.969)(1.119)=1.084\). Therefore, the correction has a slight effect on the efficiency estimates.
Outline of Topics

1. Orthogonal Designs Theory

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3. Latin Squares

4. Replicated Latin Squares and Graeco-Latin Squares
Suppose we have the same row and column levels. For instance, we might do this experiment all in the same factory using the same machines and the same operators for these machines. The first replicate would occur during the first week, the second replicate would occur during the second week, etc. Week one would be replication one, week two would be replication two, etc. You could use the same squares over again in each replicate, but we prefer to randomize these separately for each replicate. It might look like this:

<table>
<thead>
<tr>
<th>Row Effect</th>
<th>Column Effect</th>
<th>Row Effect</th>
<th>Column Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>A B C D</td>
<td>1</td>
<td>D A B C</td>
</tr>
<tr>
<td>2</td>
<td>B C D A</td>
<td>2</td>
<td>A B C D</td>
</tr>
<tr>
<td>3</td>
<td>C D A B</td>
<td>3</td>
<td>B C D A</td>
</tr>
<tr>
<td>4</td>
<td>D A B C</td>
<td>4</td>
<td>C D A B</td>
</tr>
</tbody>
</table>
Replicated Latin Squares – Case 1

We would write the model for this case as:

\[ Y_{hijk} = \mu + \delta_h + \rho_i + \gamma_j + \tau_k + e_{hij}, \]  

(5)

where \( h = 1, \ldots, n \) and \( i, j, k = 1, \ldots, t \). This is a simple extension of the basic model that we had looked at earlier. We have added one more term to our model. The row and column and treatment all have the same parameters, that we had in the unreplicated Latin square. In a Latin square, the error is a combination of any interactions that might exist and experimental error. Remember, we can’t estimate interactions in a Latin square. The df for the ANOVA table are as follows:

<table>
<thead>
<tr>
<th>Source</th>
<th>df</th>
</tr>
</thead>
<tbody>
<tr>
<td>Replicates</td>
<td>( n - 1 )</td>
</tr>
<tr>
<td>Rows</td>
<td>( t - 1 )</td>
</tr>
<tr>
<td>Columns</td>
<td>( t - 1 )</td>
</tr>
<tr>
<td>Treatments</td>
<td>( t - 1 )</td>
</tr>
<tr>
<td>Error</td>
<td>( (t - 1)[n(t+1) - 3] )</td>
</tr>
<tr>
<td>Total</td>
<td>( nt^2 - 1 )</td>
</tr>
</tbody>
</table>
Replicated Latin Squares – Case 2

In this case, one of our blocking factors, either row or column, is going to be the same across replicates whereas the other will take on new values in each replicate. For example, the levels of the row effect are all different, while we only use the same $t$ levels of the column effect. Modifying the two replicates we saw for Case 1, our current set-up might look like this:

<table>
<thead>
<tr>
<th>Row Effect</th>
<th>Column Effect</th>
<th>Row Effect</th>
<th>Column Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>A B C D</td>
<td>5</td>
<td>D A B C</td>
</tr>
<tr>
<td>2</td>
<td>B C D A</td>
<td>6</td>
<td>A B C D</td>
</tr>
<tr>
<td>3</td>
<td>C D A B</td>
<td>7</td>
<td>B C D A</td>
</tr>
<tr>
<td>4</td>
<td>D A B C</td>
<td>8</td>
<td>C D A B</td>
</tr>
</tbody>
</table>
Replicated Latin Squares – Case 2

We would write the model for this case as:

$$Y_{hijk} = \mu + \delta_h + \rho_{i(h)} + \gamma_j + \tau_k + e_{hij}, \quad (6)$$

where $h = 1, \ldots, n$ and $i, j, k = 1, \ldots, t$. Notice how we modify the Case 1 setting here. Namely, we have nested the row effect within the replicates. It should be clear how to modify the above if the row and column effects were switched. The df for the ANOVA table are as follows:

<table>
<thead>
<tr>
<th>Source</th>
<th>df</th>
</tr>
</thead>
<tbody>
<tr>
<td>Replicates</td>
<td>$n - 1$</td>
</tr>
<tr>
<td>Rows</td>
<td>$n(t - 1)$</td>
</tr>
<tr>
<td>Columns</td>
<td>$t - 1$</td>
</tr>
<tr>
<td>Treatments</td>
<td>$t - 1$</td>
</tr>
<tr>
<td>Error</td>
<td>$(t - 1)(nt - 2)$</td>
</tr>
<tr>
<td>Total</td>
<td>$nt^2 - 1$</td>
</tr>
</tbody>
</table>
Replicated Latin Squares – Case 3

In this case, both of our blocking factors (i.e., the rows and columns) have different levels for the replicates. Thus, both of the factors are nested within the replicates of the experiment. A design with \( n = 2 \) replicates might look like this:

<table>
<thead>
<tr>
<th>Row Effect</th>
<th>Column Effect</th>
<th>Row Effect</th>
<th>Column Effect</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>A  B  C  D</td>
<td>5</td>
<td>D  A  B  C</td>
</tr>
<tr>
<td>2</td>
<td>B  C  D  A</td>
<td>6</td>
<td>A  B  C  D</td>
</tr>
<tr>
<td>3</td>
<td>C  D  A  B</td>
<td>7</td>
<td>B  C  D  A</td>
</tr>
<tr>
<td>4</td>
<td>D  A  B  C</td>
<td>8</td>
<td>C  D  A  B</td>
</tr>
</tbody>
</table>
Replicated Latin Squares – Case 3

We would write the model for this case as:

\[ Y_{hijk} = \mu + \delta_h + \rho_{i(h)} + \gamma_{j(h)} + \tau_k + e_{hij}, \]  

(7)

where \( h = 1, \ldots, n \) and \( i, j, k = 1, \ldots, t \). Notice how we modify the Case 1 (or Case 2) setting here. Namely, we have nested both effects within the replicates. The df for the ANOVA table are as follows:

<table>
<thead>
<tr>
<th>Source</th>
<th>df</th>
</tr>
</thead>
<tbody>
<tr>
<td>Replicates</td>
<td>( n - 1 )</td>
</tr>
<tr>
<td>Rows</td>
<td>( n(t - 1) )</td>
</tr>
<tr>
<td>Columns</td>
<td>( n(t - 1) )</td>
</tr>
<tr>
<td>Treatments</td>
<td>( t - 1 )</td>
</tr>
<tr>
<td>Error</td>
<td>( (t - 1)[n(t - 1) - 1] )</td>
</tr>
<tr>
<td>Total</td>
<td>( nt^2 - 1 )</td>
</tr>
</tbody>
</table>
Comments on Replicated Latin Squares

- When using replicates, the choice of case depends on how you need to conduct the experiment.
- If you are simply replicating the experiment with the same row and column levels, you are in Case 1.
- If you are changing one or the other of the row or column factors, then you are in Case 2.
- If both of the blocking factors have levels that differ across the replicates, then you are in Case 3.
- Note that the df for the error grows very rapidly when you replicate Latin squares.
- The error is more dependent on the specific conditions that exist for performing the experiment.
Example: Car Tire Treatments

Let us return to the car tire experiment. Recall that the experiment tests \( t = 4 \) car tire treatments (A, B, C, and D) on four tire positions of each car. The row and column blocking criteria are the cars and tire positions, respectively. Suppose now that the tests are to occur at four times throughout the day (early morning, late morning, afternoon, and evening). If you knew that time of day would have been important for the test, then you could have controlled for it ahead of time. Since time of day should be considered, we want a design with 3 blocking factors: tire position, time of day, and tire treatment. One could conduct the entire experiment on one day and replicate it four times. But this would require \( 4 \times 16 = 64 \) observations, not just 16. Or, we could use a design to accommodate a third blocking criterion.
Graeco-Latin Squares

- A **Graeco-Latin square** is a set of two orthogonal Latin squares where each of the Greek and Latin letters is a Latin square and the Latin square is orthogonal to the Greek square.
- The Greek letters each occur one time with each of the Latin letters.
- A Graeco-Latin square is completely orthogonal between rows, columns, Latin letters and Greek letters.
- A $t = 4$ Graeco-Latin squares design example is given below:

<table>
<thead>
<tr>
<th>Row</th>
<th>Column</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
</tr>
<tr>
<td>1</td>
<td>A $\alpha$</td>
</tr>
<tr>
<td>2</td>
<td>B $\delta$</td>
</tr>
<tr>
<td>3</td>
<td>C $\beta$</td>
</tr>
<tr>
<td>4</td>
<td>D $\gamma$</td>
</tr>
</tbody>
</table>
Graeco-Latin Squares Model

- We want to account for all three of the blocking factor sources of variation, and remove each of these sources of error from the experiment, therefore we must include them in the model.

- The linear statistical model for an experiment with $t$ treatments in a Graeco-Latin square design is as follows:

  $$Y_{ijk} = \mu + \rho_i + \gamma_j + \beta_k + \tau_l + \epsilon_{ijk},$$

  where

  - $Y_{ijk}$ is the observation on the EU in the $i^{th}$ row of the $j^{th}$ column of the $k^{th}$ level of the third blocking criterion;
  - $i = 1, \ldots, t$, $j = 1, \ldots, t$, $k = 1, \ldots, t$, and $l = 1, \ldots, t$; and
  - $\epsilon_{ijk}$ is the experimental error and are iid normal with mean 0 and variance $\sigma^2$.

- This is a highly-efficient design with $N = t^2$ observations.
Hyper-Graeco-Latin Squares

- If you have more than two orthogonal Latin squares together, then these are referred to as **Hyper-Graeco-Latin squares**.
- When only including the blocking criteria and the treatment effects, analysis of these designs are straightforward.
- The ANOVA tables are fairly intuitive to construct, albeit cumbersome if writing them out by hand.
This is the end of Unit 6.