ANOVA & DESIGN OF EXPERIMENTS

BY

TANUJIT CHAKRABORTY

Indian Statistical Institute

Mail : tanujitisi@gmail.com

ANALYSIS OF VARIANCE

- **Introduction:-** If observations are taken from a popln. with mean μ, all the observations will not be identical. They will fluctuate around the mean, due to random observational error. This is a natural inevitable variation. But, if, on the top of this, another source of variation or sources of variation are either deliberately introduced or are suspected to enter due to circumstances beyond our control. Hence, observations are heterogeneous or not homogeneous with respect to source or sources of variation. For example, if one wishes to assess the effect of a sleeping drug on the average amount of sleep of patients.

A deliberately introduced source of variation, e.g. a sleeping drug, is called "treatment" or "factor". Thus, certain patients do not receive the treatment from one group and the other groups by changing the 'dose' of the drug. Besides the drug, the patients can be classified according to some other factor such as age or sex.

The effect of these sources of variation, that is, treatment can be assessed by analysing the total variation and splitting it into components corresponding to these sources of variation.

Linear Models:- By linear model, we shall mean a mathematical equation that involves random variables, mathematical variables and parameter and that is also linear in parameters.

If the distribution of the RV is given, this will be considered as the part of the model and there may be unknown parameters in the distribution.

For examples:- If $\gamma_0, \gamma_1, \gamma_2$ are unknown parameters, then

(i) $y = \gamma_0 + \gamma_1 x + \gamma_2 x_2 + e$, where, $E(e) = 0$ and $Var(e) = \sigma^2$

(ii) $y = \gamma_0 + \gamma_1 e^{x_1} + \gamma_2 \log x_2 + e$, where, $e \sim N(0, \sigma^2)$

are examples of linear models.

An example of non-linear model is: $y = \gamma_0 e^{-x_1 \gamma_1} + e$, where $e \sim N(0, \sigma^2)$.

Illustrative Example of linear model:- Let $y_1, y_2, \ldots, y_n$ be n observable quantities. In all cases, we shall assume the observed value to be composed of two parts:

$$y_i = \mu_i + e_i \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

where, μ_i is the true value and e_i be the error. The true value μ_i is that part which is due to assignable causes, and the portion that remains is the error, which is due to various chance causes.

This set up, which is fundamental to analysis of variance, is called the linear model.

Remark:- It is possible that there may be association between errors of successive measurements, but we shall assume that the errors e_i are always independent random variables. These are also assumed to have zero expectations and to be homoscedastic.

Model Classifications: — Consider the linear model:

$$y_i = \mu_i + e_i ,$$

$$\therefore y_i = \beta_1 x_{i1} + \beta_2 x_{i2} + \cdots + \beta_p x_{ip} + e_i , i = 1(1)n$$

assuming μ_i to be a linear function of p unknown quantities, $\beta_1, \beta_2, \ldots, \beta_p$, called effects.

Assumption:— The random variables $\{e_i\}$ is that $E(e_i) = 0 \ \forall i = 1(1)n$ and $Var(e_i) = \sigma^2 \ \forall i = 1(1)n$.

i.e. $\{e_i\}$ are uncorrelated and have the same mean '0' and the variance σ^2 (unknown).

The purpose is to make inferences about the $\{e_i\}$ and some of the $\{\beta_j\}$ on the basis of the observations $\{y_i\}$.

- **Model-I :— [Fixed Effect model]**

We shall call a model fixed effect model in which all the effects $\{\beta_j\}$ are unknown constants.

It often happens that one of the $\{\beta_j\}$ is a constant which occurs with every observation with coefficient 1, i.e. $x_{ij} = 1 \ \forall i$ and for this one j. We may call such a β_j an additive constant. constant.

Examples:— Suppose we want to predict an individual's height (y) on the basis of his father's height (x_1) and his mother's height (x_2). Then the predicting formula is:

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + e \ , \text{ say.}$$

Here the effects $\beta_0, \beta_1, \beta_2$ are unknown fixed quantities, are the parameters. The model is a fixed effect model, and β_0 is an additive constant.

- **Model-II :— [Random Effect Model]**

A model in which all the $\{\beta_j\}$ are random variables, except possibly for one which is an additive constant, is called a random effect model.

Example:— In measuring the nitrogen content of the foliage on a certain tree, there are two major sources of variation: the variation of leaves on the tree, and the variation due to the measurement error. Suppose we take n leaves from the tree, where the actual nitrogen content of the i^{th} leaf is a_i. In this case, a_i's are r.v.s.

The model can be written as

$$y_i = \mu + a_i + e_i , \ i = 1(1)n;$$ where, μ is a constant which is average value of y_i, an additive constant and a_i's are r.v.s with $E(a_i) = 0$ and $Var(a_i) = \sigma_a^2$.

- **Model III :-** [Mixed Effect Model]

A model in which at least one β_j is a random variable and at least one is a unknown constant (parameter) except an additive constant, is called a mixed effects model.

Example :- Let, there are p-given varieties of rice, the production of which are experimented in q Indian districts chosen randomly. Let y_{ij} be the production of rice in the j^{th} district of the i^{th} variety ; $i = 1(1)p$, $j = 1(1)q$.

The effect of i^{th} variety α_i is fixed and the effect of j^{th} district b_j is random for all i, j.

Then the model can be written as

$$y_{ij} = \mu + \alpha_i + b_j + e_{ij}$$

The model is a mixed effects model.

Three kinds of Analysis :-

(a) The analysis of Variance (ANOVA) is a body of statistical methods of analysing observations, where variation is inherent in nature. The total variation in any set of numerical data is due to a number of causes which may be classified as : (i) Assignable causes and (ii) chance causes.

The variation due to assignable causes can be detected and measured where as the variation due to chance causes is beyond of control of human hand and can't be traced separately. According to Prof. R.A. Fisher, Analysis of variance (ANOVA) is the "separation of variance ascribable to one group of causes from the variance ascribable to other group". In general, in ANOVA technique, all factors are treated qualitatively.

- **Example :-** Suppose a researcher has developed a new variety of rice, which he wants to compare with a standard variety. He wants to examine the yield of the two varieties ; so he plants both under uniform conditions. If we let α_1, α_2 be the average yields of the new and standard variates, respectively, we can write the model for the observed yield as

$$y = \alpha_1 x_1 + \alpha_2 x_2 + e,$$ where, e is an error and x_1, x_2 take on the values 0 and 1.

If he wants new variety, then $x_1 = 1$ and $x_2 = 0$; the observed yield y is $y_1 = \alpha_1 + e$.

Hence, this is a case of analysis of variance.

(b)

Regression Analysis:- If the $\{x_{ij}\}$ are values taken on in the observations not by counter variable, but by continuous variables like t = time, T = temperature, t^2, e^{-T}, etc.

[these are called independent or concomitant variables and the observations $\{y_i\}$ are then said to be on a dependent variable y], then we have a case of regression analysis.

In general, in regression analysis all factors are quantitative and treated quantitatively.

Example:- Suppose we want to predict an individual's height (y) on the basis of his father's height (x_1) and on his mother's height (x_2). Then the predicting formula is

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + e.$$

Here x_1 and x_2 are continuous variables.

This is a case of regression analysis.

(c) **Analysis of Covariance:-** If there are some $\{x_{ij}\}$ of both kinds, we have an analysis of covariance. In other words, in AN COVA, some factors are present that are treated quantitatively and some that are qualitatively. (*)

Example:- Suppose, we want to compare different type of drugs and an experiment is made on patients to measure the effect of a drug. If the age of the patients is taken into account, then the model may be taken as

$$y_{ij} = \mu + \beta_i + \gamma x_{ij} + e_{ij},$$

where, β_i is the effect due to i^{th} drug, x_{ij} is the age of the j^{th} patient taking drug i. Here the factor 'drug' is treated qualitatively and the factor 'age' is quantitative.

This is a case of analysis of covariance. (**)

(*) This is an extension of ANOVA technique to cover the case where observations are taken on more than one variable from each experimental unit. The ANCOVA method controls the experimental error by taking into consideration the dependence of y on the x's.

(**) One real life example is:

The yield of a crop may depend on the number of plants per plot, and we may consider the number of plants as the concomitant variable and perform the analysis of covariance.

• What is linear hypothesis? How is such a hypothesis tested? [CU]

Solution:- Let $y_1, y_2, \ldots, y_n$ be n independent observations with a common variance σ^2 and having expectations

$$E(y_i) = x_{i1}\beta_1 + x_{i2}\beta_2 + \cdots + x_{ip}\beta_p, \quad i = 1(1)n. \quad \cdots\cdots (*)$$

where x_{ij} are elements of a specified matrix $X^{n\times p}$ and β_j are unknown parameters and let $\text{rank}(X) = r$.

If $S_1^2 = \min \sum_i (y_i - x_{i1}\beta_1 - \cdots - x_{ip}\beta_p)^2$.

when minimised with respect to the β_j, then S_1^2/σ^2 is χ^2 with d.f. $= (n-r)$.

Suppose the β_j are subject to s independent conditions, viz,

$$R_0: \begin{cases} r_{11}\beta_1 + \cdots + r_{1p}\beta_p = g_1, \\ \vdots \\ r_{s1}\beta_1 + \cdots + r_{sp}\beta_p = g_s \end{cases} \Leftrightarrow R^{s\times p}\,\underset{\sim}{\beta}^{p\times 1} = \underset{\sim}{g}$$

These linear restrictions can be assumed to be independent, for, if not, they can be placed by an independent set.

A linear hypothesis H_0, corresponding to the linear model (*), specifies the values of one or more linear function of parameters

$$H_0: \begin{cases} h_{11}\beta_1 + \cdots + h_{1p}\beta_p = \theta_1 \\ h_{21}\beta_1 + \cdots + h_{2p}\beta_p = \theta_2 \\ \vdots \\ h_{m1}\beta_1 + \cdots + h_{mp}\beta_p = \theta_m \end{cases} \Leftrightarrow H^{m\times p}\,\beta^{p\times 1} = \underset{\sim}{\theta}$$

The above m linear functions can be assumed to be independent. Suppose the parameters in the model (*) are known to satisfy the s linear restrictions R_0 given above. These conditions are also taken as independent.

Test of general linear hypothesis:— It is necessary that the vectors $(h_{i1}, h_{i2}, \ldots, h_{ip})$, $i = 1, 2, \ldots, m$, in H_0 be linearly dependent on the vectors $(x_{i1}, \ldots, x_{ip})$, $i = 1, 2, \ldots, n$, and $(r_{i1}, \ldots, r_{ip})$, $i = 1, 2, \ldots, s$, in order that H_0 may be tested.

Let, as before, $\text{rank} X = r$ and t be the number of independent vector in $(h_{i1}, \ldots, r_{ip})$, $i = 1, 2, \ldots, s$, that are linearly independent on the rows of A. Let $t+m$ be the number of independent vectors in $(h_{i1}, \ldots, h_{ip})$, $i = 1, 2, \ldots, m$ and $(r_{i1}, \ldots, r_{ip})$, $i = 1(1)s$, that are linearly dependent on $(x_{i1}, \ldots, x_{ip})$, $i = 1(1)n$.

Then $\sigma^2\chi^2_{R_0}$ is the minimum value of

$$\sum_i (y_i - x_{i1}\beta_1 - \cdots - x_{ip}\beta_p)^2$$

when β_j are subject to the condition R_0, is distributed as a $\sigma^2\chi^2$ with d.f. $= (n-r+t)$.

similarly, $\sigma^2\chi^2_{R_0+H_0}$, which is the minimum value of

$$\sum_i (y_i - x_{i1}\beta_1 - \cdots - x_{ip}\beta_p)^2$$

where β_j are subject to the conditions in R_0 and H_0, is distd. as $\sigma^2\chi^2$ with d.f. $= (n-r+t+m)$, provided H_0 is true.

Hence $\chi^2 = \chi^2_{R_0+H_0} - \chi^2_{R_0}$ is distributed as χ^2 with df = m under H_0, i.e. only if H_0 be true. Then a test for H_0 provided by

(1) $\chi^2_{R+H_0} - \chi^2_R$, which is a χ^2 with df = m, if σ^2 is known,

(2) $F = \dfrac{[\chi^2_{R+H_0} - \chi^2_{R_0}]/m}{\chi^2_{R_0}/(n-r+t)}$; which is distributed as an F with df = $(m, n-r+t)$, if σ^2 is not known.

Selection of Valid Error :-

It is important to note that $\chi^2_{H_0+R_0} - \chi^2_{R_0}$ follows a χ^2 with m d.f. and non-centrality parameter $\frac{1}{\sigma^2}(H\underset{\sim}{\beta} - \underset{\sim}{\theta})' D^{-1} (H\underset{\sim}{\beta} - \underset{\sim}{\theta})$

where $D = \text{Disp}(H\hat{\underset{\sim}{\beta}})$

Hence $E\left(\dfrac{\sigma^2(\chi^2_{H_0+R_0} - \chi^2_{R_0})}{m}\right) = \sigma^2 + \frac{1}{m}(H\underset{\sim}{\beta} - \underset{\sim}{\theta})' D^{-1} (H\underset{\sim}{\beta} - \underset{\sim}{\theta})$

$= E\left(\dfrac{\sigma^2 \chi^2_{R_0}}{n-r+t}\right) + \frac{1}{m}(H\underset{\sim}{\beta} - \underset{\sim}{\theta})' D^{-1} (H\underset{\sim}{\beta} - \underset{\sim}{\theta})$

Note that, $E\left(\dfrac{\sigma^2(\chi^2_{H_0+R_0} - \chi^2_{R_0})}{m}\right) = E\left(\dfrac{\sigma^2 \chi^2_{R_0}}{n-r+t}\right)$ if H_0 is true

$> E\left(\dfrac{\sigma^2 \chi^2_{R_0}}{n-r+t}\right)$, if H_0 is not true.

Clearly, $\dfrac{\sigma^2 \chi^2_{R_0}}{n-r+t}$ is the valid quantity to get the effect of $H_0 : H\underset{\sim}{\beta} = \underset{\sim}{\theta}$ and is an unbiased estimator of error variation σ^2 with or without H_0.

Hence, $\sigma^2 \chi^2_{R_0}/(n+t-r) = \underset{R_0}{\text{Min}} \{(\underset{\sim}{y} - X\underset{\sim}{\beta})'(\underset{\sim}{y} - X\underset{\sim}{\beta})\}$, the minimum error sum of square under the model, is the valid error in testing the linear hypothesis H_0.

Examples:- Let $y_1 = \alpha_1 + \epsilon_1$

$y_2 = 2\alpha_1 - \alpha_2 + \epsilon_2$

$y_3 = \alpha_1 + 2\alpha_2 + \epsilon_3$ where $\epsilon_i \overset{iid}{\sim} N(0, \sigma^2)$.

Find the least squares estimates of α_1 and α_2. Derive the F statistic for testing $H : \alpha_1 = \alpha_2$.

Solution:-

$$\begin{pmatrix} y_1 \\ y_2 \\ y_3 \end{pmatrix} = \begin{pmatrix} 1 & 0 \\ 2 & -1 \\ 1 & 2 \end{pmatrix} \begin{pmatrix} \alpha_1 \\ \alpha_2 \end{pmatrix} + \begin{pmatrix} \epsilon_1 \\ \epsilon_2 \\ \epsilon_3 \end{pmatrix}$$

$\Leftrightarrow \underset{\sim}{y} = X\underset{\sim}{\beta} + \underset{\sim}{\epsilon}$, where $X^{3\times 2}$ is of rank 2.

$H_0 : (1, -1)\begin{pmatrix} \alpha_1 \\ \alpha_2 \end{pmatrix} = 0 \Leftrightarrow H_0 : H\underset{\sim}{\beta} = 0$

Then $X'X = \begin{pmatrix} 1 & 2 & 1 \\ 0 & -1 & 2 \end{pmatrix} \begin{pmatrix} 1 & 0 \\ 2 & -1 \\ 1 & 2 \end{pmatrix} = \begin{pmatrix} 6 & 0 \\ 0 & 5 \end{pmatrix}$.

Then $\hat{\beta} = (X'X)^{-1} X'y = \begin{pmatrix} \frac{1}{6} & 0 \\ 0 & \frac{1}{5} \end{pmatrix} \begin{pmatrix} y_1+2y_2+y_3 \\ -y_2+y_3 \end{pmatrix}$

$$\Rightarrow \begin{pmatrix} \hat{\alpha}_1 \\ \hat{\alpha}_2 \end{pmatrix} = \begin{bmatrix} \frac{1}{6}(y_1+2y_2+y_3) \\ \frac{1}{5}(-y_2+y_3) \end{bmatrix}$$

Now, $L_\Omega = SSE\,[RSS] = y'y - \hat{\beta}'X'X\hat{\beta} = y_1^2+y_2^2+y_3^2 - 6\hat{\alpha}_1^2 - 5\hat{\alpha}_2^2$

Under H, let $\alpha_1 = \alpha_2 = \alpha$, we have $\epsilon'\epsilon = (y_1-\alpha)^2 + (y_2-\alpha)^2 + (y_3-\alpha)^2$

and $\frac{\partial \epsilon'\epsilon}{\partial \alpha} = 0$

$\Rightarrow \hat{\alpha}_H = \frac{1}{11}(y_1+y_2+3y_3)$

Hence, $L_{\Omega \cap H} = \min_H \epsilon'\epsilon = (y_1-\hat{\alpha}_H)^2 + (y_2-\hat{\alpha}_H)^2 + (y_3-\hat{\alpha}_H)^2$

Therefore, $SS(H) = L_{\Omega \cap H} - L_\Omega = \frac{30}{11}(\hat{\alpha}_1 - \hat{\alpha}_2)^2$

Hence $F = \frac{SS(H)/1}{SSE/1} = \frac{30}{11} \cdot \frac{(\hat{\alpha}_1-\hat{\alpha}_2)^2}{SSE} \sim F_{1,1}$, under H.

Reject H at 100 α% level of significance if observed $F > F_{\alpha;1,1}$.

Example:- A trial observations y_1, y_2, y_3 are made of angles θ_1, θ_2 and θ_3, respectively, of a triangle on the ground. If the observations are subject to independent normal errors with means 0 and common variance σ^2, derive a test statistic for the hypothesis that the triangle is an isosceles with $\theta_1 = \theta_2$.

Hints:-

$$\Omega: \quad \begin{aligned} y_1 &= \theta_1 + e_1 \\ y_2 &= \theta_2 + e_2 \\ y_3 &= \theta_3 + e_3 \end{aligned}$$

where, $e_i \overset{iid}{\sim} N(0, \sigma^2)$ and $\theta_1+\theta_2+\theta_3 = \pi$.

To minimize $e'e = \sum_{i=1}^{n} (y_i - \theta_i)^2$, subject to $\theta_1+\theta_2+\theta_3 = \pi$, under H.

To minimize $e'e = (y_1-\theta_1)^2 + (y_2-\theta_2)^2 + (y_3-\pi+\theta_1+\theta_2)^2$, under H, let $\theta_1 = \theta_2$, we want to minimize

$e'e = (y_1-\theta_1)^2 + (y_2-\theta_2)^2 + (y_3-\theta_3)^2$, subject to $2\theta_1+\theta_3 = \pi$

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ANALYSIS OF VARIANCE : ——

The analysis of variance is a statistical technique for analysing measurements depending on several kinds of effects operating simultaneously, to decide which kind of effects are important and to estimate the effects.

TYPES OF DATA:- As the method of analysis based on model depends on the type of data, we have defined below several types of data.

(I) One-way Classified data:- When a set of observations is taken on distributed over the different levels of a factor, they form one-way classified data. If there are k levels of a factor and let there are n_i observations denoted by y_{ij}, $j=1(1)n_i$, against the i^{th} level, $i=1(1)k$. Then the observations y_{ij} classified in k groups according to the k levels of the factor are said to form one-way classified data.

Factor A	Observations
Levels	
A_1	$y_{11}\ y_{12} \cdots y_{1n_1}$
A_2	$y_{21}\ y_{22} \cdots y_{2n_2}$
⋮	⋮
A_k	$y_{k1}\ y_{k2} \cdots y_{kn_k}$

(II) Two-way classified data:- If we take two factors simultaneously, say, A and B at number of levels p and q, respectively, then there are pq cells each of which is defined by one level of A and one level of B. Let there are n_{ij} observations taken from the $(i,j)^{th}$ cell defined by the i^{th} level of A and j^{th} level of B.

Let y_{ijk} denotes the k^{th} observation in the $(i,j)^{th}$ cell. Then the data $\{y_{ijk} : k=1(1)n_{ij},\ i=1(1)p,\ j=1(1)q\}$ arranged in the pq groups are called **two-way classified data**.

ANOVA for one-way classified data:- Let y_{ij} denote the j^{th} observation in the i^{th} level or group of a factor, $i=1(1)k$, $j=1(1)n_i$.

Model:- The observations from the i^{th} level are:
$\{y_{i1}, y_{i2}, \ldots, y_{in_i}\}$. Let μ_i be the mean effect of the i^{th} level. Then we may write $y_{i1} = \mu_i + e_{i1}$, $y_{in_i} = \mu_i + e_{in_i}$, where e_{ij}'s are errors. We make the assumption that
$y_{ij} = \mu_i + e_{ij}$, $i=1(1)k$, $j=1(1)n_i$ and $\{e_{ij}\}$ are independently $N(0,\sigma^2)$.

If μ is the general effect which is fixed of the factors, then we may write $\mu_i = \mu + \alpha_i$, where $\alpha_i = \mu_i - \mu$ is the (additional) effect due to the i^{th} group (level) over the general effect. Therefore our underlying assumption are equivalent to the mathematical model:

$$\Omega: \begin{cases} y_{ij} = \mu + \alpha_i + e_{ij}, \ i=1(1)k,\ j=1(1)n_i, \text{ where} \\ \{e_{ij}\} \overset{id}{\sim} N(0,\sigma^2),\ \sigma^2 \text{ is unknown.} \end{cases}$$

Model Classification :– The (additional) effects α_i's can be both fixed or random depending on how it has been chosen.

A. Fixed-effects model :– If a factor has only K levels, then each of the K levels has some fixed effects. If we consider all the K levels and y_{ij} is the j^{th} observation from i^{th} level, then our linear model is

$$\begin{cases} y_{ij} = \mu + \alpha_i + e_{ij}, & i = 1(1)K,\ j = 1(1)n_i, \\ \{e_{ij}\} \text{ are independently } N(0, \sigma^2), \sigma^2 \text{ unknown}, \\ \text{and } \alpha_i \text{ is the fixed effect.} \end{cases}$$

Hypothesis : We wish to test the hypothesis that all the K levels have the same effect or not. Therefore, we wish to test $H_0 : \mu_1 = \mu_2 = \cdots = \mu_K$

$\Leftrightarrow H_0 : \alpha_1 = \alpha_2 = \cdots\cdots = \alpha_K$.

Normal Equations and least Squares Estimators :–

The LS estimators of μ and α_i's are obtained by minimizing

$$L = \sum_{i=1}^{K}\sum_{j=1}^{n_i} e_{ij}^2 = \sum_{i=1}^{K}\sum_{j=1}^{n_i} (y_{ij} - \mu - \alpha_i)^2$$

The normal equations are :

$$(*) \leftarrow \begin{cases} 0 = \dfrac{\partial L}{\partial \mu} = 2\sum_i\sum_j (y_{ij} - \mu - \alpha_i)(-1) \Rightarrow \bar{y}_{00} = \mu + \dfrac{\sum_{i=1}^{K} n_i \alpha_i}{\sum n_i} \\ 0 = \dfrac{\partial L}{\partial \alpha_i} = 2\sum_j (y_{ij} - \mu - \alpha_i)(-1) \Rightarrow \bar{y}_{i0} = \mu + \alpha_i, \ i = 1(1)K \end{cases}$$

Note that, adding last K equations of (*), we get the first equation. Thus, we have really K independent equations in $(K+1)$ unknowns : μ, α_i $(i = 1(1)K)$. To get unique solution of μ, α_i's, we are short by one equation. By making assumption $\sum n_i \alpha_i = 0$, we get unique solution of μ and α_i's. Hence, from (*), we have

$$\hat{\mu} = \bar{y}_{00}, \quad \hat{\alpha}_i = \bar{y}_{i0} - \bar{y}_{00}, \quad i = 1(1)K.$$

[significance of the assumption $\sum_{i=1}^{K} n_i \alpha_i = 0$ is that $\sum n_i (\mu_i - \mu) = 0$

$\Leftrightarrow \mu = \dfrac{\sum n_i \mu_i}{\sum n_i}$ is the mean effect of all μ_i's.]

To test $H_0 : \alpha_1 = \alpha_2 = \cdots\cdots = \alpha_K$, we now rewrite the model under H_0 and get $y_{ij} = \mu + e_{ij}$, since the assumption $\sum n_i \alpha_i = 0$, gives the common value of α_i under H_0, say, $\alpha = 0$.

To minimize $L = \sum_i \sum_j (y_{ij} - \mu)^2$, under H_0, w.r.t. μ,

Normal Equation: $\frac{\partial L}{\partial \mu} = 0 \Rightarrow 2 \sum\sum (y_{ij} - \mu)(-1) = 0$, under H_0.

$$\Rightarrow \sum\sum y_{ij} = \mu \left(\sum n_i\right).$$

The LS estimator of μ, under H_0, is $\hat{\mu}_H = \bar{y}_{00}$.

Orthogonal splitting of total sum of squares :-

$$L_\Omega = \min_\Omega \sum_i \sum_j e_{ij}^2$$

$$= \sum_i \sum_j (y_{ij} - \hat{\mu} - \hat{\alpha}_i)^2$$

$$= \sum_i \sum_j (y_{ij} - \bar{y}_{i0})^2 \quad \text{and} \quad L_{\Omega \cap H_0} = \min_{\Omega \cap H_0} \sum\sum e_{ij}^2.$$

$$\text{So, } L_{\Omega \cap H_0} = \sum_i \sum_j (y_{ij} - \hat{\mu}_H)^2$$

$$= \sum_i \sum_j (y_{ij} - \bar{y}_{00})^2$$

Here L_Ω is the error sum of squares (SSE).

As a measure of effect on H_0 on the model Ω, we get,

$$SSH_0 = L_{\Omega \cap H_0} - L_\Omega = \sum\sum (y_{ij} - \bar{y}_{00})^2 - \sum\sum (y_{ij} - \bar{y}_{i0})^2$$

Note that, $y_{ij} - \hat{\mu}_H = (y_{ij} - \hat{\mu} - \hat{\alpha}_i) + (\hat{\alpha}_i + \hat{\mu} - \hat{\mu}_H)$, is an orthogonal splitting.

Therefore, $\sum_i \sum_j (y_{ij} - \hat{\mu}_H)^2 = \sum_i \sum_j (y_{ij} - \hat{\mu} - \hat{\alpha}_i)^2 + \sum_i n_i \hat{\alpha}_i^2$

$$\Rightarrow \sum_i \sum_j (y_{ij} - \bar{y}_{00})^2 = \sum_i \sum_j (y_{ij} - \bar{y}_{i0})^2 + \sum_i n_i (\bar{y}_{i0} - \bar{y}_{00})^2$$

$$\Rightarrow L_{\Omega \cap H_0} = L_\Omega + \sum_i n_i (\bar{y}_{i0} - \bar{y}_{00})^2 \text{ is an orthogonal splitting.}$$

Here $L_{\Omega \cap H_0} = \sum_i \sum_j (y_{ij} - \bar{y}_{00})^2$ is called the total sum of squares about the grand mean (SST).

$SSH_0 = \sum n_i (\bar{y}_{i0} - \bar{y}_{00})^2$ is also known as the sum of squares between levels or groups (SSB).

$L_\Omega = \sum\sum (y_{ij} - \bar{y}_{i0})^2$ is also refered to as SS within levels or groups (SSW). Now, we may write,

$$\boxed{SS(\text{Total}) = SS(\text{Within groups}) + SS(\text{between groups})}$$

Derivation of Test Statistic and Testing Procedure:-

From the model: $y_{ij} = \mu + \alpha_i + e_{ij}$, with $\sum n_i \alpha_i = 0$,
where $E(e_{ij}) = 0$, $Var(e_{ij}) = \sigma^2$,
we have $\bar{y}_{io} = \mu + \alpha_i + \bar{e}_{io}$, $\bar{y}_{oo} = \mu + \bar{e}_{oo}$

Then $E(SSE) = E\left[\sum_i \sum_j (y_{ij} - \bar{y}_{io})^2\right] = E\left[\sum\sum (e_{ij} - \bar{e}_{io})^2\right]$

$= E\left[\sum_i \sum_j e_{ij}^2 - \sum_i n_i \bar{e}_{io}^2\right] = \sigma^2 \left(\sum_{i=1}^{K} n_i\right) - \sum_{i=1}^{K} n_i \cdot \frac{\sigma^2}{n_i}$

$= (n-K)\sigma^2$, where $n = \sum_{i=1}^{K} n_i$

Hence, $E(MSE) = E\left(\frac{SSE}{n-K}\right) = \sigma^2$.

Now, $E[SSHo] = E\left[\sum_i n_i (\bar{y}_{io} - \bar{y}_{oo})^2\right] = E\left[\sum n_i (\alpha_i + \bar{e}_{io} - \bar{e}_{oo})^2\right]$

$= \sum_{i=1}^{K} n_i \alpha_i^2 + E\left[\sum_{i=1}^{K} n_i (\bar{e}_{io} - \bar{e}_{oo})^2\right]$

$= \sum_{i=1}^{K} n_i \alpha_i^2 + E\left[\sum_{i=1}^{K} n_i \bar{e}_{io}^2 - n\bar{e}_{oo}^2\right]$

$= \sum n_i \alpha_i^2 + \sum_{i=1}^{K} n_i \cdot \frac{\sigma^2}{n_i} - n \cdot \frac{\sigma^2}{n}$

$= \sum n_i \alpha_i^2 + (K-1)\sigma^2$.

Hence, $E[MSHo] = \sigma^2 + \frac{\sum n_i \alpha_i^2}{(K-1)} = E(MSE) + \frac{1}{K-1} \sum n_i \alpha_i^2$

Therefore, $E(MSHo) = E(MSE)$, under $Ho: \alpha_1 = \alpha_2 = \ldots = \alpha_K = 0$
$> E(MSE)$, if Ho is not true;

MSE is the valid quantity on the basis of which, we compare the effect of Ho (MSHo). Again, MSE is an UE of error variance with or without the hypothesis Ho. Hence, MSE is the valid error in testing Ho.

When Ho is not true, we have $E(MSB) > E(MSE)$ and we can expect on the average that, in the ratio $F = MSB/MSE$, the numerator is larger than the denominator, depending on the non-null quantity $\frac{\sum n_i \alpha_i^2}{(K-1)}$. Thus the large values of F indicate the departure from null hypothesis Ho. Hence, we reject Ho if $F > c$.

To find c, we are to derive the distribution of $F = \frac{MSB}{MSE}$,

under Ho. $\frac{SSB}{\sigma^2} \sim \chi^2_{K-1}$ and $\frac{SSE}{\sigma^2} \sim \chi^2_{n-K}$, independently,

Therefore, $F = \frac{MSB}{MSE} \sim F_{K-1, n-K}$, under Ho, and the null hypothesis Ho is rejected at the level of significance α if observed

$F = \frac{MSB}{MSE} > F_{\alpha;\, K-1, n-K}$.

The calculation of the analysis of variance are usually exhibited in a table — called the ANOVA Table.

Table: ANOVA of One-way Layout

Source of Variation	SS	DF	MS	F	Tabulated
Between groups	SSB (or SSHo) $= \sum_{i=1}^{k} n_i (\bar{y}_{i0} - \bar{y}_{00})^2$	$k-1$	$MSB = \frac{SSB}{k-1}$	$F = \frac{MSB}{MSE}$	$F_{\alpha; k-1, n-k}$
Within groups (Error)	SSW or SSE $= \sum_i \sum_j (y_{ij} - \bar{y}_{i0})^2$	$n-k$	$MSE = \frac{SSE}{n-k}$		
	$SST = \sum_i \sum_j (y_{ij} - \bar{y}_{00})^2$	$n-1$			

If the null hypothesis Ho is rejected. Naturally such rejection leads to further investigation to decide which means are different. We may test $H_{01}: \mu_i = \mu_{i'}$, with the help of the statistic

$$\frac{\bar{y}_{i0} - \bar{y}_{i'0}}{\sqrt{MSE\left(\frac{1}{n_i} + \frac{1}{n_{i'}}\right)}}$$ which has t-distribution with $(n-k)$ degree of freedom, under Ho.

The null hypothesis $H_{01}: \mu_i = \mu_{i'}$ is rejected at level α if the difference $|\bar{y}_{i0} - \bar{y}_{i'0}| >$ the critical difference $t_{\alpha/2,(n-k)} \cdot \sqrt{MSE\left(\frac{1}{n_i} + \frac{1}{n_{i'}}\right)}$

Thus we may compute all possible critical differences arising out of all possible pairs $(\mu_i, \mu_{i'})$, $i \neq i'$ and compare them with their observed differences. On the basis of such comparisons it will be possible to divide the k levels into different groups such that the levels in the same group have the same mean and those in different groups have different means.

Remark:-

(1) It is important to note that $\hat{\mu} = \bar{y}_{00}$, $\hat{\alpha}_i = \bar{y}_{i0} - \bar{y}_{00}$. The LS estimators, are BLUEs of μ and α_i, under the restriction $\sum n_i \alpha_i = 0$.

If the restriction is not satisfied, then $\bar{y}_{i0} = \mu + \alpha_i + e_i$,

$\Rightarrow E(\bar{y}_{i0}) = \mu + \alpha_i$, since $E(\bar{e}_{i0}) = 0$ and $\bar{y}_{00} = \mu + \frac{\sum n_i \alpha_i}{n} + e_{00}$

$\Rightarrow E(\bar{y}_{00}) = \mu + \frac{\sum n_i \alpha_i}{n}$.

Hence, $E(\hat{\mu}) = E(\bar{y}_{00}) = \mu + \frac{\sum n_i \mu_i}{n}$ and $E(\hat{\alpha}_i) = E(\bar{y}_{i0} - \bar{y}_{00}) = \alpha_i - \frac{\sum n_i \alpha_i}{n}$.

If the restriction is not satisfied, then $\hat{\mu}, \hat{\alpha}_i$ are not unbiased.

Write down shortnotes on Orthogonal Contrasts and Treatment Contrasts (Parametric Contrasts).

Solution:-

Observational Contrast :- Let $y_1, y_2, \ldots, y_n$ be n observations. The linear function $L = \sum_{i=1}^{n} c_i y_i$, where c_i's are given numbers such that $\sum_{i=1}^{n} c_i = 0$, is called a contrast of y_i's or an observational contrast.

Orthogonal Contrasts :- Two observational contrasts $L_1 = \sum_{i=1}^{n} c_i y_i = \underset{\sim}{c}'\underset{\sim}{y}$ and $L_2 = \sum_{i=1}^{n} d_i y_i = \underset{\sim}{d}'\underset{\sim}{y}$ are said to be orthogonal if $\underset{\sim}{c}'\underset{\sim}{d} = \sum_{i=1}^{n} c_i d_i = 0$. When there are more than two contrasts, these are said to be mutually orthogonal if they are orthogonal pairwise. For n values, $\exists$ $(n-1)$ orthogonal contrasts.

For example, when there are four observations y_1, y_2, y_3, y_4, we can write the following three mutually contrasts

(i) $y_1 + y_2 - y_3 - y_4$ (ii) $y_1 - y_2 - y_3 + y_4$ (iii) $y_1 - y_2 + y_3 - y_4$.

Sum of squares of contrast :- If $y_i \overset{iid}{\sim} N(\mu, \sigma^2)$, $i = 1(1)n$, then $L = \sum_{i=1}^{n} c_i y_i$ with $\sum_{i=1}^{n} c_i = 0$, has $N\left(0, \sigma^2 \sum_{i=1}^{n} c_i^2\right)$ distribution.

Note, $E\left(\frac{L^2}{\sum c_i^2}\right) = \sigma^2 \Rightarrow \frac{L^2}{\sum c_i^2} = \hat{\sigma}^2$ is an UE of σ^2.

The sum of squares due to contrast L is defined as $\left(\frac{L^2}{\sum c_i^2}\right)$.

Treatment Contrasts (Parametric Contrasts) :-

A contrast among the parameter $\beta_1, \beta_2, \ldots, \beta_p$ is a linear function of the β_j's, $\sum_{i=1}^{p} c_i \beta_i$, with known contrast coefficient subject to the condition $\sum_{i=1}^{p} c_i = 0$.

For example, $\beta_1 - \beta_2$, $\beta_1 + \beta_2 - 2\beta_3$ are contrasts.

The contrast $L = \sum_{i=1}^{p} c_i \beta_i$, $\sum_{i=1}^{p} c_i = 0$, can be expressed as $L = \underset{\sim}{c}'\underset{\sim}{\beta}$ with $\underset{\sim}{1}'\underset{\sim}{c} = 0$.

Elementary Contrasts :- Contrasts like $\beta_i - \beta_j$ are called elementary contrasts. A contrast $L = \underset{\sim}{c}'\underset{\sim}{\beta}$ with the contrast vector $\underset{\sim}{c}$ consisting only two non-zero elements, is called a elementary contrast.

- **Theorem:-** Any (treatment) contrast can be expressed as a linear combination of elementary (treatment) contrast.

Proof:- Consider a general contrast $L = \sum_{i=1}^{p} c_i \beta_i$ where $\sum_{i=1}^{p} c_i = 0$

Note that, $L = c_1\beta_1 + c_2\beta_2 + \ldots\ldots\ldots\ldots + c_{p-1}\beta_{p-1} + c_p\beta_p$

$= c_1\beta_1 + \ldots\ldots + c_{p-1}\beta_{p-1} + \left(-\sum_{i=1}^{p-1} c_i\right)\beta_p$

$= c_1(\beta_1 - \beta_p) + c_2(\beta_2 - \beta_p) + \ldots + c_{p-1}(\beta_{p-1} - \beta_p)$

which is a linear combination the elementary contrasts.

Further consideration of one-way Layout :-

(1) Contrasts in one-way fixed effect model :- In one-way layout a contrast among the mean effects μ_i's is $L = \sum_{i=1}^{k} c_i \mu_i$, with $\sum_{i=1}^{k} c_i = 0$.
The LS estimator $\hat{L} = \sum c_i \hat{\mu} = \sum c_i \bar{y}_{i0}$ is an UE of $L = \sum c_i \mu_i$.
Note that $Var(\hat{L}) = \sum_{i=1}^{k} c_i^2 Var(\bar{y}_{i0}) = \sigma^2 \left(\sum_{i=1}^{k} \frac{c_i^2}{n_i} \right)$, which is estimated by $\hat{\sigma}_L = MSE \left(\sum_{i=1}^{k} \frac{c_i^2}{n_i} \right)$. Hence, $t = \frac{\hat{L} - L_0}{\sqrt{MSE \left(\sum_{i=1}^{k} \frac{c_i^2}{n_i} \right)}} \sim t_{n-k}$,
under $H_0 : L = L_0$, is the proper test statistic for testing $H_0 : L = L_0$ Vs. $H_1 : L \neq L_0$.

(2) Estimability :- In the one-way fixed effect model, $\Omega : y_{ij} = \mu + \alpha_i + e_{ij}$, $e_{ij} \sim N(0, \sigma^2)$ independently, $j = 1(1)n_i, i = 1(1)k$, without the side condition $\sum n_i \alpha_i = 0$, what are the estimable functions ?

☞ Note that $\sum_{i=1}^{k} \sum_{j=1}^{n_i} a_{ij} E(y_{ij}) = \sum_i \sum_j a_{ij} (\mu + \alpha_i) = \sum_i c_i (\mu + \alpha_i)$,
let $\sum_{j=1}^{n_i} a_{ij} = c_i$. Thus the estimable functions are the totality of functions ψ of the form $\psi = c_1 \alpha_1 + \cdots + c_k \alpha_k + \left(\sum_{i=1}^{k} c_i \right) \mu$.
Note that neither is any of the parametric functions $\mu, \alpha_1, \ldots, \alpha_k$ estimable nor is $\sum_{i=1}^{k} n_i \alpha_i$. A linear function of the $\{\alpha_i\}$ will be estimable iff it is in the form $\sum_{i=1}^{k} c_i \alpha_i$ with $\sum_{i=1}^{k} c_i = 0$. Hence, without side condition, only parametric contrasts are estimable.

(3) Show that if the total number of observations to be taken is fixed, the average variance of the estimators of all elementary contrasts of the effects due to the levels of the factor is minimum if the number of observations on each level is the same.

Solution:- Elementary contrasts are $\mu_i - \mu_j$, $i \neq j$.
LS estimators are $\bar{y}_{i0} - \bar{y}_{j0}$ and their variances are $\sigma^2 \left(\frac{1}{n_i} + \frac{1}{n_j} \right)$
Average variance, $\bar{V} = \sum_{i \neq j} \sigma^2 \left(\frac{1}{n_i} + \frac{1}{n_j} \right) \Big/ k(k-1)$

$$= \frac{2(k-1)\sigma^2 \left(\sum \frac{1}{n_i} \right)}{k(k-1)} = 2\sigma^2 \left(\frac{\sum_{i=1}^{k} \frac{1}{n_i}}{k} \right) \geq 2\sigma^2 \frac{k}{\sum n_i},$$

applying AM $\geq$ HM. Equality holds iff $n_i = n_0$, $\forall i$
Hence, $\bar{V}$ is minimum iff $n_i = n_0$, $\forall i = 1(1)k$, and minimum $\bar{V} = \frac{2\sigma^2}{n_0}$ and in this case estimation and testing based on the estimates are efficient if $n_i = n_0$ $\forall i$.

Definition:- A one-way (and higher-order) classification is called balanced if the numbers of observations in the groups (or in cells) are equal. i.e. As smaller the variance, the better the estimate, the one-way ANOVA is most efficient, if $n_i = n_0$, i.e. if the classification is balanced.

B. Random Effects Model:- Suppose that, as a measure of quality control, an auto manufacturer tests a sample of new cars, observing for each car, the mileage achived on a number of occasions on a litre of Petrol. Suppose y_{ij} is the mileage of the i^{th} car on the j^{th} occasion.

Let m_i be the true mean for the i^{th} car selected. Then, we may write $y_{ij} = m_i + e_{ij}$, where e_{ij} is the error of i^{th} car on the j^{th} occasion. However, the manufacturer is interested in the performance of the thousands of cars to be produced that year and, for this reason, has drawn a random sample of cars for test. Hence, m_i's are the effects of the cars selected randomly from the large population of cars and the cars selected can be any cars from the population of cars; consequently m_i's are random.

Let μ be the fixed general effect and we define the effect of the i^{th} car in the experiment is $a_i = m_i - \mu$. Then the model becomes

$$y_{ij} = \mu + a_i + e_{ij}$$, where the effect a_i of the i^{th} car is there a random variable.

WLG, we can put $E(a_i) = 0$ since the mean can be absorbed into μ and $\text{Var}(a_i) = \sigma_A^2$.

Here our model is :

$$\begin{cases} y_{ij} = \mu + a_i + e_{ij}, \text{ where } \{a_i\}, \{e_{ij}\} \text{ are completely independent,} \\ \text{and } a_i \sim N(0, \sigma_A^2) \text{ and } e_{ij} \sim N(0, \sigma_e^2) \ \forall \\ i \text{ and } j. \end{cases}$$

The variance of an observation y_{ij} is $\sigma_y^2 = \sigma_A^2 + \sigma_e^2$ and so it is appropriate to call σ_A^2 and σ_e^2 the variance components; that is, the components of the variance of an observation.

The intra-class correlation coefficient ρ^* is the ordinary correlation between any two of the observations y_{ij} and $y_{ij'}$ $(j \neq j')$ in the same level (or car) i,

$$\rho^* = \frac{\text{cov}(y_{ij}, y_{ij'})}{\sqrt{\text{Var}(y_{ij})\text{Var}(y_{ij'})}} = \frac{E[(y_{ij} - \mu)(y_{ij'} - \mu)]}{\sigma_y^2}$$

$$= \frac{E(a_i + e_{ij})(a_i + e_{ij'})}{\sigma_y^2} = \frac{E(a_i^2)}{\sigma_y^2} = \frac{\sigma_A^2}{\sigma_A^2 + \sigma_e^2}.$$

The y_{ij}'s are dependent and their joint distribution, hence the estimation of σ_A^2 and σ_e^2, is greatly simplified if the model is assumed to be balanced, i.e., to satisfy, $n_i = r$, $\forall\, i = 1(1)k$.

We summarize the assumptions made for:

$$\Omega: \begin{cases} y_{ij} = \mu + a_i + e_{ij}, \text{ the } (k+rk) \text{ random variables} \\ \{a_i\} \text{ and } \{e_{ij}\} \text{ are completely independent, the} \\ \{a_i\} \sim N(0, \sigma_A^2) \text{ and } \{e_{ij}\} \sim N(0, \sigma_e^2). \end{cases}$$

Let $rk = n$.

Hypothesis:- The hypothesis usually tested in the present model for one-way layout is

H_0: $\sigma_A^2 = 0$; i.e., to test whether all the levels in the population have the same "true" mean.

Analysis:- [The Least square method and Testing of linear hypothesis, etc are applied for linear models, which are linear in parameters. In random effects model, $y_{ij} = \mu + a_i + e_{ij}$ is not a linear model as a_i's are random, not parameters. Therefore, the SS's occuring in fixed effects model, will not occur in the random effect models in the same way. The general procedure used to obtain tests and estimates with random-effects model and mixed models in balanced cases is to consider all Mean-squares in the usual ANOVA table with the corresponding fixed-effects model. Fixed effects model is the model for the population of all levels, but random effects model is the model for a random sample of levels from the population of all levels. We expect that the SS's occuring in the fixed effects model for the population of all levels are also valid for the random effects model arises from a random sample of levels.]

The SS's occuring in the ANOVA for one-way fixed effects model are

$$SSH_0 \text{ (or, SSA)} = \sum_{i=1}^{k} r\,(\bar{y}_{i0} - \bar{y}_{00})^2$$

$$SSE = \sum_{i=1}^{k}\sum_{j=1}^{r} (y_{ij} - \bar{y}_{i0})^2$$

Under the random effects model

$$\bar{y}_{i0} = \mu + a_i + \bar{e}_{i0}, \quad \bar{y}_{00} = \mu + a_0 + \bar{e}_{00}$$

We thus have

$$SSA = r\sum_{i=1}^{k} (\bar{y}_{i0} - \bar{y}_{00})^2 = r\sum_{i=1}^{k} (a_i + \bar{e}_{i0} - a - \bar{e}_{00})^2$$

and $$SSE = \sum_i \sum_j (e_{ij} - \bar{e}_{i0})^2 .$$

Define, $g_i = a_i + \bar{e}_{i0}$, $g_i \overset{iid}{\sim} N\left(0, \sigma_A^2 + \frac{\sigma_e^2}{n}\right)$.

Let, $\sigma_g^2 = \sigma_A^2 + \frac{\sigma_e^2}{n}$.

Therefore, $SSA = n \sum_{i=1}^{k} (g_i - \bar{g}_0)^2$.

Note that, $\sum_{i=1}^{k} \frac{(g_i - \bar{g}_0)^2}{\sigma_g^2} \sim \chi^2_{k-1} \Rightarrow SSA = n \sum_{i=1}^{k} (g_i - \bar{g}_0)^2 \sim n\sigma_g^2 \chi^2_{k-1}$.

$\therefore E[SSA] = n\sigma_g^2 (k-1) \Rightarrow E[MSA] = n\sigma_A^2 + \sigma_e^2$.

On the other hand, $\sum_{i=1}^{k} \left\{ \sum_{j=1}^{n} \frac{(e_{ij} - \bar{e}_{i0})^2}{\sigma_e^2} \right\} \sim \chi^2_{k(n-1)}$.

$\Rightarrow SSE \sim \sigma_e^2 \chi^2_{n-k}$, $kn = n$

$\Rightarrow E(MSE) = \sigma_e^2$.

Therefore, $E(MSA) = E(MSE) + n\sigma_A^2$

$$\begin{cases} = E(MSE) & \text{if } H_A: \sigma_A^2 = 0 \text{ is true} \\ > E(MSE) & \text{if } H_A \text{ is false} \end{cases}$$

We have $\frac{MSA}{MSE} \simeq 1$, under H_A and $\frac{MSA}{MSE}$ is > 1. When H_A is not true. Hence, MSE is not valid error for testing H_A. This suggests that $H_A: \sigma_A^2 = 0$ to be tested by using the ratio $F = \frac{MSA}{MSE}$.

Note, it is clear that SSA and SSE are statistically independent.

Here $F = \frac{MSA}{MSE} \sim F_{k-1, n-k}$; under H_A. We reject H_A at level of significance α if $F = \frac{MSA}{MSE}$ (observed) $> F_{\alpha; k-1, n-k}$.

Remark:- (1) Since $F = \frac{MSA}{MSE} \sim \frac{(n\sigma_A^2 + \sigma_e^2)\chi^2_{k-1}}{k-1} \Big/ \frac{\sigma_e^2 \chi^2_{n-k}}{n-k}$

$$\sim (1 + n\theta) \left\{ \frac{\chi^2_{k-1}}{k-1} \Big/ \frac{\chi^2_{n-k}}{n-k} \right\}$$

$$\sim (1 + n\theta) F_{k-1, n-k}.$$

The expected value of F-statistic is

$$E(F) = \left(1 + n \cdot \frac{\sigma_A^2}{\sigma_e^2}\right) E\left(F_{k-1, n-k}\right) = \left(1 + n \cdot \frac{\sigma_A^2}{\sigma_e^2}\right) \cdot \frac{n-k}{n-k-2} > 1$$

since $E(F_{n_1, n_2}) = \frac{n_2}{n_2 - 2}$.

For random-effects model in one-way classified data, the expected value of F-statistic can't be less than unity. But it is quite possible that an observed F is less than 1.

(2) If $F < 1$, then MSA < MSE. Here MSE is random variability which we have to allow in the experiment. If MSA > MSE then the variability due to 'A' is not random and it has some effect. Therefore, if $F < 1 \Leftrightarrow$ MSA < MSE implies variability due to A is also random and it has no effect.

(3) Point Estimation of Variance Components :- We have

$$E(MSE) = \sigma_e^2$$
$$E(MSA) = \sigma_e^2 + r\sigma_A^2$$
$$\Rightarrow E\left(\frac{MSA - MSE}{r}\right) = \sigma_A^2$$

Hence, $\hat{\sigma}_e^2 = MSE$ and $\hat{\sigma}_A^2 = \frac{MSA - MSE}{r}$ are UEs of σ_e^2 and σ_A^2.

ANOVA table for one-way random effects model :-

Source of Variation	SS	D.F.	MS	E(MS)	F	Tabulated F
Factor A	$SSA = r\sum_{i=1}^{k}(\bar{y}_{io} - \bar{y}_{oo})^2$	$k-1$	$MSA = \frac{SSA}{k-1}$	$\sigma_e^2 + r\sigma_A^2$	$F = \frac{MSA}{MSE}$	$F_{\alpha;k-1,n-k}$
Error	$SSE = \sum\sum(y_{ij} - \bar{y}_{io})^2$	$n-k$	$MSE = \frac{SSE}{n-k}$	σ_e^2		
Total	$SST = \sum\sum(y_{ij} - \bar{y}_{oo})^2$	$n-1$				

Distinguish between Fixed Effects model and Random Effects model:

(i) The model assumed in fixed effects model is

$$\Omega : \begin{cases} y_{ij} = \mu + \alpha_i + e_{ij}, \; i = 1(1)k, \; j = 1(1)n_i \\ \text{where, } \mu, \{\alpha_i\} \text{ are fixed effects (or parameters)} \\ \text{such that } \sum n_i\alpha_i = 0, \; e_{ij} \sim N(0, \sigma_e^2), \text{ independently.} \end{cases}$$

The model in random effects model is

$$\Omega : \begin{cases} y_{ij} = \mu + a_i + e_{ij}, \; i = 1(1)k, \; j = 1(1)n_i, \text{ where} \\ \{a_i\} \text{ and } \{e_{ij}\} \text{ are completely independent} \\ \text{random variables and } \{a_i\} \sim N(0, \sigma_A^2) \text{ and} \\ \{e_{ij}\} \sim N(0, \sigma_e^2). \end{cases}$$

(ii) In fixed effects model, $E(y_{ij}) = \mu + \alpha_i$, the observations in different levels have different means. In random effects model, $E(y_{ij}) = \mu$, all the observations have the same expectation.

(iii) In fixed effects model, $Cov(y_{ij}, y_{ij'}) = 0$, that is, all the observations are statistically independent.
In random effect model, $\rho = \frac{Cov(y_{ij}, y_{ij'})}{\sqrt{V(y_{ij})V(y_{ij'})}} = \frac{\sigma_A^2}{\sigma_A^2 + \sigma_e^2}$ and the observations are not statistically independent.

ANOVA of Two-way Classified data :—

Suppose that two factors A and B vary in an experiment or in observational material, for example, A may be machines and B may be operators or different variates (A) are plotted in different locations (B). If we have p variates and q locations in the second example, these are called the p levels of A and the q levels of B, respectively.

Model : Let y_{ijk} be the k^{th} observation on the "i,j treatment combination" where factor A is at the i^{th} level and B at the j^{th} level, $k = 1(1)n_{ij}$, $i = 1(1)p$, $j = 1(1)q$.

If we assume that the observations in the $(i,j)^{th}$ cell a random sample from a population corresponding to the cell. We shall denote the "true" mean of the $(i,j)^{th}$ cell by μ_{ij}.

Hence our model is —

$$y_{ijk} = \mu_{ij} + e_{ijk} \quad \text{where } e_{ijk} \sim N(0, \sigma^2) \text{ independently.}$$

We can think of μ_{ij} as being composed of the following parts :

$$\mu_{ij} = \mu + (\bar{\mu}_{i\cdot} - \mu) + (\bar{\mu}_{\cdot j} - \mu) + (\mu_{ij} - \bar{\mu}_{i\cdot} - \bar{\mu}_{\cdot j} + \mu)$$

$$= \mu + \alpha_i + \beta_j + \gamma_{ij}, \text{ where } \mu \text{ is some common}$$

quantity present in each observation, called general mean; α_i is the effect of the i^{th} level of A, the excess over general mean; β_j is the effect due to the j^{th} level of B; and γ_{ij} is called the interaction of the i^{th} level of A and j^{th} level of B.

Here μ is fixed constant but the effects α_i, β_j and γ_{ij} can be random or fixed depending on how it has been chosen.

- If $\gamma_{ij} = 0$, for all i,j. The model reduces to :

$$\eta_{ij} = \mu + \alpha_i + \beta_j$$

This is a case of no interaction and the model is called "no interaction" model. This is also called a case of additive effects.

(A) **Fixed Effects Model :—** If A and B have only p and q levels then their effects are fixed; that is, α_i, β_j, γ_{ij} are fixed unknown quantities. Then the linear model is :

$$y_{ijk} = \mu + \alpha_i + \beta_j + \gamma_{ij} + e_{ijk}, \quad e_{ijk} \overset{iid}{\sim} N(0, \sigma_e^2),$$

is the fixed effects two-way layout.

(1) The two-way layout with one observation per cell :—

In order to get exact test concerning the main effects, it is generally necessary for fixed effects model to assume that there are no interactions, based on one observation per cell. If y_{ij} denotes the single observation in the $(i,j)^{th}$ cell, we assume the following fixed effects model:

$\Omega : y_{ij} = \mu + \alpha_i + \beta_j + e_{ij}$ where $e_{ij} \sim N(0, \sigma_e^2)$ independently.

Null Hypothesis :– The hypothesis of chief interest are

H_A : all α_i's are equal

and H_B : all β_j's are equal

LS Estimators :– The least squares estimators are obtained by minimizing $L = \sum_{i=1}^{p}\sum_{j=1}^{q} e_{ij}^2 = \sum_{i=1}^{p}\sum_{j=1}^{q} (y_{ij} - \mu - \alpha_i - \beta_j)^2$ under Ω.

Normal equations are:

$$0 = \frac{\partial L}{\partial \mu} \Rightarrow \sum_i\sum_j y_{ij} = pq\mu + q\sum_i \alpha_i + p\sum_j \beta_j \quad \text{——— (1)}$$

$$0 = \frac{\partial L}{\partial \alpha_i} \Rightarrow \sum_j y_{ij} = q\mu + q\alpha_i + \sum_j \beta_j, \ \forall\ i = 1(1)p \quad \text{——— (2)}$$

$$0 = \frac{\partial L}{\partial \beta_j} \Rightarrow \sum_i y_{ij} = p\mu + \sum_i \alpha_i + p\beta_j, \ \forall\ j = 1(1)q \quad \text{——— (3)}$$

Adding p equations in (2), we get equation (1) and adding q equations in (3), we get equation (1). Hence there are $\{1 + (p-1) + (q-1)\} = p+q-1$ independent equations. The number of unknown quantities is $(p+q+1)$ and hence we need two side conditions or identifiable constraints.

Let $\sum_{i=1}^{p} \alpha_i = 0 = \sum_{j=1}^{q} \beta_j$.

Then the normal equations reduce to

$$\sum_i\sum_j y_{ij} = pq\mu \Rightarrow \hat{\mu} = \bar{y}_{00}$$

$$\sum_j y_{ij} = q\mu + q\alpha_i \Rightarrow \hat{\alpha}_i = \bar{y}_{i0} - \bar{y}_{00}$$

$$\sum_i y_{ij} = p\mu + p\beta_j \Rightarrow \hat{\beta}_j = \bar{y}_{0j} - \bar{y}_{00}$$

The hypothesis H_A and H_B reduce by the condition $\sum_i \alpha_i = 0 = \sum_j \beta_j$ as $H_A : \alpha_i = 0, \ \forall\ i = 1(1)p$ and $H_B : \beta_j = 0, \ \forall\ j = 1(1)q$.

SS's and Orthogonal splitting :-

We write $(y_{ij} - \mu - \alpha_i - \beta_j) = (y_{ij} - \hat{\mu} - \hat{\alpha}_i - \hat{\beta}_j) + (\hat{\mu} - \mu) + (\hat{\alpha}_i - \alpha_i) + (\hat{\beta}_j - \beta_j)$.

On squaring and summing over i, j ; we find that the cross-product terms vanish because of the side conditions :

$$\sum_i \alpha_i = 0 = \sum_j \beta_j \text{ and of } \sum_i \hat{\alpha}_i = 0 = \sum_j \hat{\beta}_j .$$

We get $L = \sum_i \sum_j (y_{ij} - \mu - \alpha_i - \beta_j)^2$

$$= \sum_i \sum_j (y_{ij} - \hat{\mu} - \hat{\alpha}_i - \hat{\beta}_j)^2 + pq(\hat{\mu} - \mu)^2 + q\sum_i (\hat{\alpha}_i - \alpha_i)^2 + p\sum_j (\hat{\beta}_j - \beta_j)^2 .$$

Under Ω, L is minimum if $\mu = \hat{\mu}$, $\alpha_i = \hat{\alpha}_i$, $\beta_j = \hat{\beta}_j$.

Hence, L_Ω (or SSE) $= \sum_i \sum_j (y_{ij} - \hat{\mu} - \hat{\alpha}_i - \hat{\beta}_j)^2$.

Now, under H_A : all $\alpha_i = 0$, $L = L_\Omega + pq(\hat{\mu} - \mu)^2 + q\sum_i \hat{\alpha}_i^2 + q\sum_j (\hat{\beta}_j - \beta_j)^2$.

This is obviously minimized by the values $\mu = \hat{\mu}$, $\beta_j = \hat{\beta}_j$ and minimum of L under H_A is

$$L_{\Omega \cap H_A} = L_\Omega + q\sum_{i=1}^{p} \hat{\alpha}_i^2 .$$

For testing H_A, the SS is SSHA which we shall call SS due to levels of factor A : $SSA = L_{\Omega \cap H_A} - L_\Omega = q\sum_{i=1}^{p} \hat{\alpha}_i^2$.

Similarly, SS due to the levels of factor B is $SSB = p\sum_{j=1}^{q} \hat{\beta}_j^2$

It is important to observe that

$y_{ij} - \hat{\mu} = (y_{ij} - \hat{\mu} - \hat{\alpha}_i - \hat{\beta}_j) + \hat{\alpha}_i + \hat{\beta}_j$, is an orthogonal splitting and hence we have $\sum_i \sum_j (y_{ij} - \hat{\mu})^2$

$$= \sum_i \sum_j (y_{ij} - \hat{\mu} - \hat{\alpha}_i - \hat{\beta}_j)^2 + q\sum_i \hat{\alpha}_i^2 + p\sum_j \hat{\beta}_j^2$$ as the orthogonal splitting of SST, i.e., $SST = SSE + SSA + SSB$.

Hence, SSA, SSB, SSE are independently distributed.

Derivation of Test Statistic and Testing Procedure :-

The d.f. of $SSA = q\sum_{i=1}^{p} \hat{\alpha}_i^2$ is $\overline{p-1}$, since we have one restriction $\sum_{i=1}^{p} \hat{\alpha}_i = 0$ and $(p-1)$, $\hat{\alpha}_i$'s are linearly independent. Similarly, the d.f. of SSB is $\overline{q-1}$ and d.f. of S.S.E. is $pq-(p+q-1) = (p-1)(q-1)$.

It can be shown that $E(SSA) = (p-1)\sigma_e^2 + q\sum_{i=1}^{p} \alpha_i^2$.

$$\Rightarrow E(MSA) = \sigma_e^2 + \frac{q}{p-1}\sum_{i=1}^{p} \alpha_i^2$$

Similarly, $E(MSB) = \sigma_e^2 + \frac{p}{q-1}\sum_{j=1}^{q} \beta_j^2$ and $E(MSE) = \sigma_e^2$.

[Note that $E(MSA) = \begin{cases} E(MSE) & \text{, under } H_A \text{; that is, if } H_A \text{ is true} \\ > E(MSE) & \text{, if } H_A \text{ is false.} \end{cases}$

Hence, MSE is the valid quantity to reflect (or compare) the effect of the levels of factor A and also note that MSE is an UE of error variance σ_e^2. Hence, MSE is the <u>valid error</u> for testing H_A: all $\alpha_i = 0$.]

When H_A is true, $F_A = \frac{MSA}{MSE} \simeq 1$. When H_A is not true,

$$E(F_A) = E\left(\frac{MSA}{MSE}\right) = E(MSA)\,E\left(\frac{1}{MSE}\right) > \frac{E(MSA)}{E(MSE)} = 1 + \frac{q\sum \alpha_i^2}{\sigma^2(p-1)} > 1.$$

Thus, the ratio $F_A = \frac{MSA}{MSE}$ gives some indication as to the "true state of affairs" regarding α_i's ; H_A is rejected if F_A is "significantly large".

It can be shown that $F_A = \frac{MSA}{MSE} \sim F_{p-1,(p-1)(q-1)}$, under H_A.

We reject H_A at level α if $F_A > F_{\alpha;\,p-1,(p-1)(q-1)}$.

Similarly, H_B: all $\beta_j = 0$, is rejected at α level of significance if

$$F_B = \frac{MSB}{MSE} > F_{\alpha;\,q-1,(p-1)(q-1)}.$$

ANOVA table for two-way layout with one observation per cell :-

Source of Variation	SS	DF	MS	F
Due to A	$SSA = q\sum_i (\bar{y}_{i0} - \bar{y}_{00})^2$	$p-1$	$MSA = \frac{SSA}{p-1}$	$F_A = \frac{MSA}{MSE}$
Due to B	$SSB = p\sum_j (\bar{y}_{0j} - \bar{y}_{00})^2$	$q-1$	$MSB = \frac{SSB}{q-1}$	$F_B = \frac{MSB}{MSE}$
Error	$SSE = \sum_i\sum_j (y_{ij} - \bar{y}_{i0} - \bar{y}_{0j} + \bar{y}_{00})^2$	$(p-1)(q-1)$	$MSE = \frac{SSE}{(p-1)(q-1)}$	
Total	$SST = \sum_i\sum_j (y_{ij} - \bar{y}_{00})^2$	$pq-1$		

(2) The two-way layout with equal numbers of observations in the cell :-

Let y_{ijk} is the k^{th} observation in the cell (i,j), $i=1(1)p$, $j=1(1)q$, $k=1(1)m$. The model appropriate for this type of data is the following fixed-effects model :

$$\Omega: \begin{cases} y_{ijk} = \mu + \alpha_i + \beta_j + \gamma_{ij} + e_{ijk}, \\ e_{ijk} \overset{iid}{\sim} N(0, \sigma_e^2), \ i=1(1)p, \ j=1(1)q, \ k=1(1)m, \text{ where } m>1. \end{cases}$$

Here (i) α_i is the fixed effect of the i^{th} level of A

(ii) β_j is the fixed effect of the j^{th} level of B

(iii) γ_{ij} is the interaction effect of the two factors which may arise due to the simultaneous occurance of the i^{th} level of A and j^{th} level of B and is in addition to the effects of α_i and β_j.

LS estimators and SS's :-

Define $L = \sum_i \sum_j \sum_k e_{ijk}^2 = \sum_i \sum_j \sum_k (y_{ijk} - \mu - \alpha_i - \beta_j - \gamma_{ij})^2$, under Ω.

LS estimators are obtained by minimizing L under Ω.

Normal equations are :-

$$(1) \longleftarrow 0 = \frac{\partial L}{\partial \mu} \Rightarrow \sum_i \sum_j \sum_k y_{ijk} = pqm\mu + qm\sum_i \alpha_i + pm\sum_j \beta_j + m\sum_i \sum_j \gamma_{ij}$$

$$(2) \longleftarrow 0 = \frac{\partial L}{\partial \alpha_i} \Rightarrow \sum_j \sum_k y_{ijk} = qm\mu + qm\alpha_i + m\sum_j \beta_j + m\sum_j \gamma_{ij}, \ \forall i=1(1)p$$

$$(3) \longleftarrow 0 = \frac{\partial L}{\partial \beta_j} \Rightarrow \sum_i \sum_k y_{ijk} = pm\mu + m\sum_i \alpha_i + pm\beta_j + m\sum_i \gamma_{ij}, \ \forall j=1(1)q$$

$$(4) \longleftarrow 0 = \frac{\partial L}{\partial \gamma_{ij}} \Rightarrow \sum_k y_{ijk} = m\mu + m\alpha_i + m\beta_j + m\gamma_{ij} \ \forall (i,j).$$

If we add the equations for $i=1(1)p$ in (4), we get the equations in (3), if we add the equations in (4), for $j=1(1)q$, we get the equations in (2). If we add the equations in (4) for all (i,j), we get equation in (1). Hence, the last set (4) of pq equations is the only set of linearly independent equations. The number of unknown quantities is $(1+p+q+pq)$. In order to get unique solutions, we impose $(p+q+1)$ linearly independent additional restrictions :

$$\sum_i \alpha_i = 0 = \sum_j \beta_j, \quad \sum_i \gamma_{ij} = 0 \ \forall \ j=1(1)q, \ \sum_j \gamma_{ij} = 0, \ \forall \ i=1(1)p.$$

Under these restrictions we get

$$\hat{\mu} = \bar{y}_{000}\ ,\ \hat{\alpha}_i = \bar{y}_{i00} - \bar{y}_{000}\ ,\ \hat{\beta}_j = (\bar{y}_{0j0} - \bar{y}_{000})\ ,$$
$$\hat{\gamma}_{ij} = y_{0j0} - \bar{y}_{i00} - \bar{y}_{0j0} + \bar{y}_{000}\ .$$

Hypothesis :- The hypotheses that we usually wish to test are

H_A : all $\alpha_i = 0$, H_B : all $\beta_j = 0$, H_{AB} : all $\gamma_{ij} = 0$

We write $(y_{ijk} - \mu - \alpha_i - \beta_j - \gamma_{ij}) = (y_{ijk} - \hat{\mu} - \hat{\alpha}_i - \hat{\beta}_j - \hat{\gamma}_{ij})$
$+ (\hat{\mu} - \mu) + (\hat{\alpha}_i - \alpha_i) + (\hat{\beta}_j - \beta_j) + (\hat{\gamma}_{ij} - \gamma_{ij})$.

Squaring and summing over i, j, k, we find that the cross-product terms vanish because of the side conditions :

$$\sum \alpha_i = \sum \hat{\alpha}_i = 0\ ,\ \sum \beta_j = \sum \hat{\beta}_j = 0\ ,\ \sum_i \gamma_{ij} = \sum_i \hat{\gamma}_{ij} = 0\ , \forall j$$
$$\sum_j \gamma_{ij} = \sum_j \hat{\gamma}_{ij} = 0\ , \forall i\ .$$

We get

$$L = \sum_i \sum_j \sum_k (y_{ijk} - \hat{\mu} - \hat{\alpha}_i - \hat{\beta}_j - \hat{\gamma}_{ij})^2 + pqm(\hat{\mu} - \mu)^2 + qm \sum_i (\hat{\alpha}_i - \alpha_i)^2$$
$$+ pm \sum_j (\hat{\beta}_j - \beta_j)^2 + m \sum_i \sum_j (\hat{\gamma}_{ij} - \gamma_{ij})^2$$

Under Ω, the minimum value of L is $L_\Omega = \sum\sum\sum (y_{ijk} - \hat{\mu} - \hat{\alpha}_i - \hat{\beta}_j - \gamma_{ij})^2$ which is attained when $\mu = \hat{\mu}$, $\alpha_i = \hat{\alpha}_i$, $\beta_j = \hat{\beta}_j$, $\gamma_{ij} = \hat{\gamma}_{ij}$.

Under H_A : all $\alpha = 0$, L becomes

$$L = L_\Omega + pqm(\hat{\mu} - \mu)^2 + qm \sum_i \hat{\alpha}_i^2 + pm \sum_j (\hat{\beta}_j - \beta_j)^2$$
$$+ m \sum_i \sum_j (\hat{\gamma}_{ij} - \gamma_{ij})^2$$

This is obviously minized by the values $\mu = \hat{\mu}$, $\beta_j = \hat{\beta}_j$, $\gamma_{ij} = \hat{\gamma}_{ij}$ and the minimum value of L under H_A is

$$L_{H_A \cap \Omega} = L_\Omega + qm \sum_i \hat{\alpha}_i^2$$

$$\Rightarrow SSA \text{ (or } SSH_A) = L_{\Omega \cap H_A} - L_\Omega = qm \sum_{i=1}^{p} \hat{\alpha}_i^2$$

Similarly, SSB (or SSH_B) $= pm \sum_{j=1}^{q} \hat{\beta}_j^2$

and $SS(AB)$ (or SSH_{AB}) $= m \sum_{i=1}^{p} \sum_{j=1}^{q} \hat{\gamma}_{ij}^2$

Orthogonal Splitting:- Note that $y_{ijk} - \hat{\mu} = (y_{ijk} - \hat{\mu} - \hat{\alpha}_i - \hat{\beta}_j - \hat{\gamma}_{ij}) + \hat{\alpha}_i + \hat{\beta}_j + \hat{\gamma}_{ij}$ is an orthogonal splitting; that is, the components are orthogonal.

Therefore, $$\sum_i \sum_j \sum_k (y_{ijk} - \hat{\mu})^2 = \sum_i \sum_j \sum_k (y_{ijk} - \hat{\mu} - \hat{\alpha}_i - \hat{\beta}_j - \hat{\gamma}_{ij})^2 + qm \sum_{i=1}^{p} \hat{\alpha}_i^2 + pm \sum_j \hat{\beta}_j^2 + m \sum_i \sum_j \hat{\gamma}_{ij}^2,$$

$\Leftrightarrow$ SST = SSE + SSA + SSB + SS(AB), is the orthogonal splitting of total SS. Therefore, SSE, SSA, SSB and SS(AB) are independently distributed.

Test Criterion and Testing Procedure:- The d.f. of SSA is $(p-1)$, since we have one linear restriction $\sum_{i=1}^{p} \hat{\alpha}_i = 0$ and $(p-1)$ $\hat{\alpha}_i$'s are linearly independent. Similarly, the d.f. of SSB is $(q-1)$ and d.f. of SS(AB) is $pq - (p-1+q-1+1)$ $= (p-1)(q-1)$. The d.f. of SSE is $pqm - 1 - (p-1) - (q-1) - (pq - p - q + 1)$ $= pq(m-1)$ which is positive if $m > 1$.

It can be shown that $E(MSA) = \sigma_e^2 + \frac{mq \sum_{i=1}^{p} \alpha_i^2}{p-1}$

$E(MSB) = \sigma_e^2 + \frac{mp \sum_{j=1}^{q} \beta_j^2}{q-1}$, $E[MS(AB)] = \sigma_e^2 + \frac{m \sum_i \sum_j \gamma_{ij}^2}{(p-1)(q-1)}$.

Note that $E(MS(AB)) = E(MSE)$, when H_{AB} is true.
$> E(MSE)$, when H_{AB} is false.

Hence, the ratio $F_{AB} = \frac{MS(AB)}{MSE}$ will give the test for H_{AB}.

We reject H_{AB} at α level of significance if the observed

$$F_{AB} > F_{\alpha;(p-1)(q-1),\, pq(m-1)}.$$

If H_{AB}: all $\gamma_{ij} = 0$ is accepted, the tests for H_A and H_B can be performed as follows:

H_A is rejected at α level of significance if the observed $F_A = \frac{MSA}{MSE} > F_{\alpha;\, p-1,\, pq(m-1)}$, and similarly, H_B is rejected if the observed $F_B = \frac{MSB}{MSE} > F_{\alpha;\, q-1,\, pq(m-1)}$.

Now, if H_A is rejected, one can find the level of A with the help of t-tests. Let $L = \sum_{i=1}^{p} c_i \alpha_i$, $\sum c_i = 0$, is a contrast among α_i's. The contrast L admits unbiased estimators

$\hat{L} = \sum_i c_i \hat{\alpha}_i = \sum_{i=1}^{p} c_i (\bar{y}_{i00} - \bar{y}_{000}) = \sum_{i=1}^{p} c_i \bar{y}_{i00}$ so that $Var(\hat{L}) = Var\left(\sum_i c_i \hat{\alpha}_i\right)$

$= Var\left(\sum c_i \bar{y}_{i00}\right) = \frac{\sigma_e^2}{qm} \sum_{i=1}^{p} c_i^2$. Hence, $t = \frac{\hat{L} - L_0}{\sqrt{MSE\left(\sum_i c_i^2\right)/qm}} \sim t_{pq(m-1)}$

under $H_0: L = L_0$, is the proper statistic to test $H_0: L = L_0$ Vs. $H_1: L \neq L_0$.

Similarly if H_B is rejected then the same procedure can be carried out for contrasts $\sum_{j=1}^{q} d_j \beta_j$, $\sum_j d_j = 0$.

▨ If H_{AB} is rejected ; that is, γ_{ij}, the interaction is present in the model, then testing H_A: all $\alpha_i = 0$ and H_B: all $\beta_j = 0$, have no meaning. To fix our ideas let us assume that the factor A represents machines and the factor B represents operators. If the interation γ_{ij} is present it means the performance of a machine depends not on itself but also on the operator who uses it. Comparison of two machines is therefore meaningless unless either we fixed the operator using them or if we need an overall comparison, we consider the preference of a machine averaged over all the operators and compare this "average" performance for two machines. The hypothesis of no differences among machines is then $H_A : \alpha_i + \frac{1}{q}\sum_{j=1}^{q} \gamma_{ij} = 0$ $\forall$ $i = 1(1)p$.

In the case of presence of interaction, it is reasonable to test whether the machines differ significantly when a particular operation using them. This is done by making an ANOVA for the one-way classified data obtained by taking the particular operator but all the machines.

It happens occasionally that the hypothesis of no interactions will be rejected by a statistical test but the hypothesis of zero main effects for both factors will be accepted. The correct conclusion is then not that no differences in the main effects has been demonstrated; if there are interactions there must be differences among the cell means. The conclusion should be that there are differences but that when the effects of the levels of one factor are averaged over the other, no difference of these averaged effects has been demonstrated.

Remark:- It is important to note that the LS estimators $\hat{\mu}, \hat{\alpha}_i, \hat{\beta}_j, \hat{\gamma}_{ij}$ are unbiased (infact BLUE's) estimators of $\mu, \alpha_i, \beta_j, \gamma_{ij}$, respectively, under the restrictions: $\sum_i \alpha_i = 0 = \sum_j \beta_j$, $\sum_i \gamma_{ij} = 0 = \sum_j \gamma_{ij}$.

If the restrictions are not satisfied, then

$$\bar{y}_{i00} = \mu + \alpha_i + \frac{1}{q}\sum_j \beta_j + \frac{1}{q}\sum_j \gamma_{ij} + \bar{e}_{i00},$$

$$\bar{y}_{000} = \mu + \frac{1}{p}\sum_i \alpha_i + \frac{1}{q}\sum_j \beta_j + \frac{1}{pq}\sum_i\sum_j \gamma_{ij} + \bar{e}_{000},$$

and $E(\bar{y}_{i00} - \bar{y}_{000}) = \alpha_i - \bar{\alpha}_0 + (\bar{\gamma}_{i0} - \bar{\gamma}_{00}) \neq \alpha_i$.

Therefore $E(\hat{\alpha}_i) \neq \alpha_i$, similarly $\hat{\mu}, \hat{\beta}_j, \hat{\gamma}_{ij}$ are not unbiased, if the restrictions are not satisfied.

ANOVA for two-way classified data with $m(>1)$ observations per cell:

Source of Variation	D.F.	SS	MS	F
Between levels of A	$p-1$	$SSA = qm\sum_i (\bar{y}_{i00} - \bar{y}_{000})^2$	$MSA = \frac{SSA}{p-1}$	$F_A = \frac{MSA}{MSE}$
Between levels of B	$q-1$	$SSB = pm\sum_j (\bar{y}_{0j0} - \bar{y}_{000})^2$	$MSB = \frac{SSB}{q-1}$	$F_B = \frac{MSB}{MSE}$
Interactions AB	$(p-1)(q-1)$	$SS(AB) = m\sum\sum(\bar{y}_{ij0} - \bar{y}_{i00} - \bar{y}_{0j0} + \bar{y}_{000})^2$	$MS(AB) = \frac{SS(AB)}{(p-1)(q-1)}$	$F_{AB} = \frac{MS(AB)}{MSE}$
Error	$pq(m-1)$	$SSE = \sum\sum\sum(y_{ijk} - \bar{y}_{ij0})^2$	$\frac{SSE}{pq(m-1)} = MSE$	—
Total	$pqm-1$	$SST = \sum\sum\sum(y_{ijk} - \bar{y}_{000})^2$		

[B] Random Effects Model:— If both the levels of A and the levels of B are random samples from a large number of levels, then the main effects and the interaction effects are not fixed quantities but they are random variables and so the model is now random effect model.

As an example in which such a model might arise, suppose that there are p machines and q workers in the experiment and each worker is assigned to each machine for m days. Further if the p machines are a random sample from a large population of machines and q workers are a random sample from a large population of workers.

Model:

$$\Omega: \begin{cases} y_{ijk} = \mu + a_i + b_j + c_{ij} + e_{ijk}, \\ \text{where, } \{a_i\}, \{b_j\}, \{c_{ij}\} \text{ are completely independently normal} \\ \text{with zero means and respective variances } \sigma_A^2, \sigma_B^2, \sigma_{AB}^2 \text{ and} \\ \text{also } e_{ijk} \sim N(0, \sigma_e^2) \text{ independently.} \end{cases}$$

Hypothesis: We wish to test $H_A: \sigma_A^2 = 0$, $H_B: \sigma_B^2 = 0$, $H_{AB}: \sigma_{AB}^2 = 0$.

Analysis: Here we use four SS's – SSA, SSB, SS(AB), SSE, for main effects of A, main effects of B, AB interactions and Error, are defined in terms of the observations $\{y_{ijk}\}$ are the same as the fixed-effects model.

If we substitute the model:

$y_{ijk} = \mu + a_i + b_j + c_{ij} + e_{ijk}$, into the definitions of SS's, we get

$$SSA = qm \sum_i (\bar{y}_{i00} - \bar{y}_{000})^2 = qm \sum_i (a_i + \bar{c}_{i0} + \bar{e}_{i00} - \bar{a}_0 - \bar{c}_{00} - \bar{e}_{000})^2.$$

$$SSB = pm \sum_j (b_j + \bar{c}_{0j} + \bar{e}_{0j0} - \bar{b}_0 - \bar{c}_{00} - \bar{e}_{000})^2$$

$$SS(AB) = m \sum_i \sum_j (c_{ij} - \bar{c}_{i0} - \bar{c}_{0j} + \bar{c}_{00} + \bar{e}_{ij0} - \bar{e}_{i00} - \bar{e}_{0j0} + \bar{e}_{000})^2$$

$$SSE = \sum_i \sum_j \sum_k (e_{ijk} - \bar{e}_{ij0})^2$$

Now, let $g_i = a_i + \bar{e}_{i00} + \bar{c}_{i0}$, then $g_i \overset{iid}{\sim} N(0, \sigma_g^2)$, where σ_g^2 equals to $\sigma_A^2 + \frac{1}{q}\sigma_{AB}^2 + \frac{1}{qm}\sigma_e^2$. Hence, $SSA = qm \sum_i (g_i - \bar{g}_0)^2 \sim \sigma_g^2 \cdot qm \chi_p^2$

$\Rightarrow E(SSA) = qm\sigma_g^2 (p-1) \Rightarrow E(MSA) = \sigma_e^2 + m\sigma_{AB}^2 + qm\sigma_A^2$.

Similarly, $E(MSB) = \sigma_e^2 + m\sigma_{AB}^2 + pm\sigma_B^2$.

To treat SS(AB), let $h_{ij} = c_{ij} + \bar{e}_{ij0}$, so $h_{ij} \overset{iid}{\sim} N(0, \sigma_h^2)$, where, $\sigma_h^2 = \sigma_{AB}^2 + \frac{1}{m}\sigma_e^2$. Hence, $SS(AB) \sim m\sigma_h^2 \chi^2_{(p-1)(q-1)}$.

$\Rightarrow E[MS(AB)] = \sigma_e^2 + m\sigma_{AB}^2$.

Further, $E(MSE) = \sigma_e^2$.

Test criterion and Testing Procedure:-

Under $H_A: \sigma_A^2 = 0$, $E(MSA) = E[MS(AB)]$ and when H_A is false. $E(MSA) > E[MS(AB)]$. The ratio $F_A = \frac{MSA}{MS(AB)}$ will give the test criterion for testing H_A.

Note that $F_A = \frac{MSA}{MS(AB)} \sim F_{p-1,(p-1)(q-1)}$, under H_A; H_A is rejected at α level of significance if $F_A > F_{\alpha; p-1, (p-1)(q-1)}$.

Similarly, H_B is rejected at α level of significance if

$$F_B = \frac{MSB}{MS(AB)} > F_{\alpha; q-1, (p-1)(q-1)}.$$

If $m > 1$, we can make inferences about σ_{AB}^2 by using the ratio

$$F_{AB} = \frac{MS(AB)}{MSE} \sim \frac{\sigma_e^2 + m\sigma_{AB}^2}{\sigma_e^2} \cdot F_{(p-1)(q-1), pq(m-1)},$$

under Ω. When H_{AB} is true, $F_{AB} = \frac{MS(AB)}{MSE} \sim F_{(p-1)(q-1), pq(m-1)}$

We reject H_{AB} if observed $F_{AB} > F_{\alpha; (p-1)(q-1), pq(m-1)}$.

ANOVA Table for two-way classified data with random effects model:-

Source of Variation	D.F.	S.S.	MS	F
Due to A	$(p-1)$	$SSA = qm\sum(\bar{y}_{i00} - \bar{y}_{000})^2$	$MSA = \frac{SSA}{p-1}$	$F_A = \frac{MSA}{MS(AB)}$
Due to B	$(q-1)$	$SSB = pm\sum(\bar{y}_{0j0} - \bar{y}_{000})^2$	$MSB = \frac{SSB}{q-1}$	$F_B = \frac{MSB}{MS(AB)}$
Due to AB	$(p-1)(q-1)$	$SS(AB) = \sum_i\sum_j m(\bar{y}_{ij0} - \bar{y}_{i00} - \bar{y}_{0j0} - \bar{y}_{000})^2$	$MS(AB)$	$F_{AB} = \frac{MS(AB)}{MSE}$
Due to Error	$pq(m-1)$	$SSE = \sum\sum\sum(y_{ijk} - \bar{y}_{ij0})^2$	MSE	—
Total	$pqm-1$	$SST = \sum\sum\sum(y_{ijk} - \bar{y}_{000})^2$		

Remark:- In our model σ_A^2, σ_B^2, σ_{AB}^2 and σ_e^2 are unknown quantities and their UEs are

$$\hat{\sigma}_A^2 = \frac{1}{qm}\{MSA - MS(AB)\}, \quad \hat{\sigma}_B^2 = \frac{1}{pq}\{MSB - MS(AB)\}$$

and $$\hat{\sigma}_{AB}^2 = \frac{1}{m}\{MS(AB) - MSE\}, \quad \hat{\sigma}_e^2 = MSE.$$

[C] Mixed Effects Model :-

An example of a two-way layout in which it is appropriate to treat one of the factors as having fixed effects and other as having random effects can be obtained by modifying the example concerning machines and workers so that the workers are still regarded as a random sample from the large population of workers; the machines are not, the interest being the individual performance of the machines.

Let y_{ijk} be the k^{th} observation when j^{th} worker is working in the i^{th} machine, $i = 1(1)p$, $j = 1(1)q$, $k = 1(1)m$.

Model :-

Ω : $y_{ijk} = \mu + \alpha_i + b_j + c_{ij} + e_{ijk}$, where $\sum_{i=1}^{p} \alpha_i = 0$, $\sum_{i=1}^{p} c_{ij} = 0$, for all j, the $\{b_j\}$, $\{c_{ij}\}$, $\{e_{ijk}\}$ are jointly normal; the $\{e_{ijk}\}$ are independently $N(0, \sigma_e^2)$ and independent of $\{b_j\}$ and $\{c_{ij}\}$, which have zero means and they are not independent.

Define $\sigma_A^2 = \frac{1}{p-1} \sum_{i=1}^{p} \alpha_i^2$, $\sigma_B^2 = Var(b_j)$, $\sigma_{AB}^2 = \frac{1}{p-1} \sum_{i=1}^{p} V(c_{ij})$.

Analysis :- Here we shall use the SS's which are obtained in two-way fixed effects model. If we substitute the model, into four SS's defined in fixed effects model, we get

$$SSA = qm \sum_{i=1}^{p} (\bar{y}_{i00} - \bar{y}_{000})^2 = qm \sum_{i=1}^{p} (\alpha_i + \bar{c}_{i0} + \bar{e}_{i00} - \bar{e}_{000})^2$$

$$SSB = pm \sum_{j=1}^{q} (\bar{y}_{0j0} - \bar{y}_{000})^2 = pm \sum_{j=1}^{q} (b_j + \bar{e}_{0j0} - b_0 - \bar{e}_{000})^2$$

$$SS(AB) = m \sum_i \sum_j (c_{ij} - \bar{c}_{i0} + \bar{e}_{ij0} - \bar{e}_{i00} - \bar{e}_{0j0} + \bar{e}_{000})^2$$

$$SSE = \sum_i \sum_j \sum_k (e_{ijk} - \bar{e}_{ij0})^2, \text{ since } \bar{c}_{0j} = 0, \forall j \text{ and hence } \bar{c}_{00} = 0.$$

These four SS's are pairwise independent except for the pair SSB, SS(AB).

We have $SSE = \sum_i \sum_j \{ \sum_{k=1}^{m} (e_{ijk} - \bar{e}_{ij0})^2 \} \sim \sigma_e^2 \chi^2_{pq(m-1)}$.

$\therefore E(SSE) = \sigma_e^2 pq(m-1) \Rightarrow E(MSE) = \sigma_e^2$.

Let $f_j = b_j + \bar{e}_{0j0}$, we get $SSB = pm \sum_{j=1}^{q} (f_j - \bar{f}_0)^2$, where $f_j \overset{iid}{\sim} N(0, \sigma_f^2)$ with $\sigma_f^2 = \sigma_B^2 + \frac{\sigma_e^2}{pm}$ and $SSB \sim \sigma_f^2, pm\chi^2_{q-1}$.
i.e. $SSB \sim (\sigma_e^2 + pm\sigma_B^2)\chi^2_{q-1}$. It follows that

$$E(MSB) = \sigma_e^2 + pm\sigma_B^2 .$$

It is convenient to define $\hat{\alpha}_i = \bar{y}_{i00} - \bar{y}_{000}$ so that $\hat{\alpha}_i = (\alpha_i + \bar{c}_{i0} + \bar{e}_{i00} - \bar{e}_{000})$, and hence $E(\hat{\alpha}_i) = \alpha_i$ and $Var(\hat{\alpha}_i) = Var(\bar{c}_{i0}) + Var(\bar{e}_{i00} - \bar{e}_{000}) = \frac{1}{q} Var(c_{ij}) + \frac{p-1}{p} Var(\bar{e}_{i00})$

$$= \frac{1}{q}\left[Var(c_{ij}) + \frac{p-1}{mp}\sigma_e^2 \right].$$

Hence, $E(SSA) = E\left[qm \sum_{i=1}^{p} \hat{\alpha}_i^2\right] = qm \sum_{i=1}^{p} E(\hat{\alpha}_i^2)$

$$= qm \sum_{i=1}^{p} \{ Var(\hat{\alpha}_i) + E^2(\hat{\alpha}_i) \}$$

$$= m \sum_{i=1}^{p} Var(c_{ij}) + (p-1)\sigma_e^2 + qm \sum_{i=1}^{p} \alpha_i^2$$

$$= (p-1)m\sigma_{AB}^2 + (p-1)\sigma_e^2 + qm(p-1)\sigma_A^2$$

$$\Rightarrow E(MSA) = \sigma_e^2 + m\sigma_{AB}^2 + qm\sigma_A^2 .$$

Also, it can be shown that $E[MS(AB)] = \sigma_e^2 + m\sigma_{AB}^2$.

Test Criterion and Testing Procedure :→

If $m > 1$, note that $E(MSB) \begin{cases} = E(MSE), \text{ if } H_B : \sigma_B^2 = 0 \text{ is true} \\ > E(MSE), \text{ if } H_B \text{ is false.} \end{cases}$

The ratio $F_B = \frac{MSB}{MSE} \sim F_{q-1, pq(m-1)}$, under H_0, will give a test for H_B.

The hypothesis $H_{AB} : \sigma_{AB}^2 = 0$ is tested using ratio

$$F_{AB} = \frac{MS(AB)}{MSE} \sim F_{(p-1)(q-1), pq(m-1)}, \text{ under } H_{AB}.$$

Note $E(MSA) \begin{cases} = E[MS(AB)], \text{ under } H_A : \sigma_A^2 = 0 \Leftrightarrow H_A : \text{all } \alpha_i = 0 \\ > E[MS(AB)], \text{ if } H_A \text{ is false.} \end{cases}$

Hence, the ratio $F_A = \frac{MSA}{MS(AB)}$ will provide a test for H_A. Even though MSA and MS(AB) are independent and under H_A, F_A doesnot in general have F-distribution. An approximate F-test with $p-1$, $(p-1)(q-1)$ d.f. may be performed for H_A with the ratio $F_A = \frac{MSA}{MS(AB)}$.

ANOVA Table for Two-way Mixed Effects Model :-

Source of Variation	D.F.	MS	E(MS)	F
Due to A (fixed)	$p-1$	MSA	$\sigma_e^2 + m\sigma_{AB}^2 + qm\sigma_A^2$	$F_A = \frac{MSA}{MSE}$
Due to B (Mixed)	$q-1$	MSB	$\sigma_e^2 + pm\sigma_B^2$	$F_B = \frac{MSB}{MSE}$
AB interactions	$(p-1)(q-1)$	MS(AB)	$\sigma_e^2 + m\sigma_{AB}^2$	$F_{AB} = \frac{MS(AB)}{MSE}$
Error	$pq(m-1)$	MSE	σ_e^2	

Remarks:- We have $E(MSB) = \sigma_e^2 + pm\sigma_B^2$

$E(MS(AB)) = \sigma_e^2 + m\sigma_{AB}^2$

$E(MSE) = \sigma_e^2$.

They are as usual valid without normality assumptions. they lead to the following unbiased estimators if $m > 1$:

$$\hat{\sigma}_B^2 = \frac{1}{pm}(MSB - MSE)$$

$$\hat{\sigma}_{AB}^2 = \frac{1}{m}(MS(AB) - MSE)$$

$$\hat{\sigma}_e^2 = MSE.$$

Example:- There are p given variates of rice of the production of which are experimented in q Indian districts chosen randomly. Here one are interested in the interaction effects along with the main effects. Write the model in details.

Solution:- Let y_{ijk} be the production of rice of the k^{th} plot in the j^{th} district of the i^{th} variety, $k = 1(1)m$, $i = 1(1)p$, $j = 1(1)q$. This is a two-way classified data with mixed-effects model.

Model :- $y_{ijk} = \mu + \alpha_i + b_j + c_{ij} + e_{ijk}$, where $\sum_{i=1}^{p} \alpha_i = 0$, $\sum_{i=1}^{p} c_{ij} = 0$, $\forall j$, the $\{b_j\}$, $\{c_{ij}\}$, $\{e_{ijk}\}$ are jointly normal ; the $\{e_{ijk}\}$ are independently Normal$(0, \sigma_e^2)$ and independent of $\{b_j\}$ and $\{c_{ij}\}$, which have zero means and they are not independent.

DESIGN OF EXPERIMENT

DESIGNS OF EXPERIMENTS

Concept of Experiments and design of Experiments :—

Experimentation and making inferences are twin essential features of general scientific methodology. After a statistical problem has been set-up, the next step is to perform experiments for collecting information on the basis of which inferences can be made in the best possible manner.

In the case of experimental data no such population exists in its own way. What exists is a problem and the data have, so to say, to be manufactured by proper experimentation so that an answer to the problem can be inferred from the data. Creation of controlled conditions is the main characteristic feature of experimentation. Proper designing is necessary to increase accuracy and sensitivity of the results.

Data obtained without regard to the statistical principles can't lead to valid inferences.

Experiment :— An experiment is a device or means of getting an answer to the problem under consideration.

A scientific experiment should be set up to answer a specific question or questions from interpretation of a set of obsns collected suitably. Precise formulation of the question (or questions) to be answered enables the experimentor to plan his experimental procedure more effectively. In planning an experiment we clearly state our objectives and formulate the hypothesis we want to test.

Treatment :— The problems or questions are usually in the form of comparisons among (a set) different procedures or objects. A general name 'treatment' is used, to denote the experimental material among which comparison is desired.

For example, in agricultural experiments, different varieties of a crop, different fertilizer doses, viz., variety, fertilizer may constitute the treatments.

Experimental unit :— The smallest division of the experimental material to which we apply the treatments and on which we make observations on the variable under study, is termed as experimental unit. In carrying out an experiment, the effects of different treatments are produced on different objects or units. An experiment unit is an object on which the effect of treatment is produced and measured. Equal sized plots of land, a single or a group of plants, etc. are used as experimental units.

Experimental Error:- The results of experiments are affected not only by the action of the treatments, but also by extraneous variations which tend to mask the effects of the treatments. Two main sources of this variation may be distinguished. The first is inherent variability in the experimental units (material) to which the treatments are applied. The second is the lack of uniformity in the methodology of conducting the experiment on in other words failure to standardise the experimental techniques, and the third is the lack of representativeness of the sample to the population under study.

The term experimental error is often applied to these variations, it provides a basis for the confidence to be placed in the inference about the population. So, it is important to estimate and control the experimental error.

THREE PRINCIPLES OF EXPERIMENTAL DESIGN :—

For the validity of statistical analysis and enhancing the precision of the experiments, three basic principles : (i) replication, (ii) randomization, (iii) local control are observed according to R. A. Fisher who pioneered the study of experimental design and illustrates the functions of the various principles.

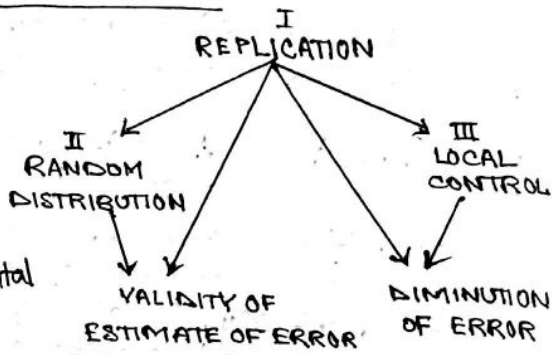


Fig:- Fisher's Diagram

Given a set of treatments which can provide information regarding the objective of an experiment, a design for the experiment defines the size and number of the experimental units, the manner in which the treatments are alloted to the units and also the appropriate type and the grouping of the experimental units. These requirements of a design ensure validity, interpretability and accuracy of the results. Write down a probability statement to estimated treatment differences:-

According to Fisher,

(i) Randomization which defines the manner of allocation of the treatments to the experimental units.

(ii) Replication which specifies the number of units to be provided for each of the treatments, are two required conditions for an experimental design.

This also means that randomization and replication are necessary to obtain a valid estimate of the error variation.

(iii) To get accurate conclusions on results, it is necessary to control the experimental error which increase the precision by choosing appropriate type of experimental units and also their groupings.

• What is the role of 'randomization' in the design of experiments?

ANS: The principle of randomisation, as advocated by Fisher, is essential for a valid estimate of the experimental error and also to minimise bias in the results. One of the vital assumption in the model of the analysis of variance is the independence of errors. If we consider agricultural experiments, it is a fact that soil fertility is not distributed at random and nearby plots happen to be correlated. Randomisation is a simple device to achieve this independence of errors. It also helps to attach a probability statement to estimated treatment differences, which is necessary for drawing inferences beyond data.

However, randomisation by itself is not sufficient for the validity of the experiment. Consider an experiment for comparing two diets for children and suppose there are only two children available for the experiment and they belong to different family. Then even if two diets be equally effective, but the one applied to the child in a better situation will give a better result despite random allocation of the diets of the children. So, randomization forms only a basis of a valid experiment. In order to ensure validity, it is necessary to have more than one child of each type and then to make the allocation of diets at random. Thus randomisation plus replication will be necessary for the validity of the experiment.

Each design has its own way of randomization.

• Why is 'replication' necessary in designing an experiment?

ANS:

If a treatment is allotted to 'r' experimental units in an experiment, it is said to be replicated r times. If in a design each of the treatment is replicated r times, the design is said to have 'r' replications.

Replication is necessary to increase the accuracy of estimates of the treatment effects. It also provides an estimate of error variation which is a function of the difference among observations from different experimental units under identical treatments. Though, the more the replications, the better it is, so far as precision of estimates is concerned, it can't be increased indefinitely as it increases cost of experimentation.

It is also necessary to attach probability statement to the treatment differences and to obtain a valid estimate of the error variation. Sensitivity of statistical methods for drawing inference also depends on the number of replications.

- Write a brief note on error control or Local control :- [CU]

Ans:-

The considerations in regard to the choice of the number of replications ensure reduction of standard error of the estimates of the treatment effects, since the standard error of the estimate of a treatment effect is $\sqrt{\frac{s^2}{r}}$ where s^2 is the error variance per experimental unit. But they can't reduce the error variance itself, though a large number of replications can ensure more stable estimate. It is, however, possible to devise methods for reducing the error variance. Such measures are called error control or local control.

One such measure is to refine the experimental technique and make the experimental units homogeneous. Another measure is to form several homogeneous groups by skillful grouping, and allowing variation between the groups.

- What is uniformity trial? Discuss its use in field experiments. [CU]

Uniformity trials enable us to have an idea about the fertility variation of the field. By uniformity trial, we mean a trial in which the field (experimental material) is divided into small units (plots) and the same treatment is applied on each of the units and their yields are recorded. From these yields, we can draw a 'fertility contour map' which gives us a graphic picture of the variation of the soil fertility and enables us to form a good idea about the nature of the soil fertility variation. The fertility contour map is obtained by joining the points of equal fertility through lines.

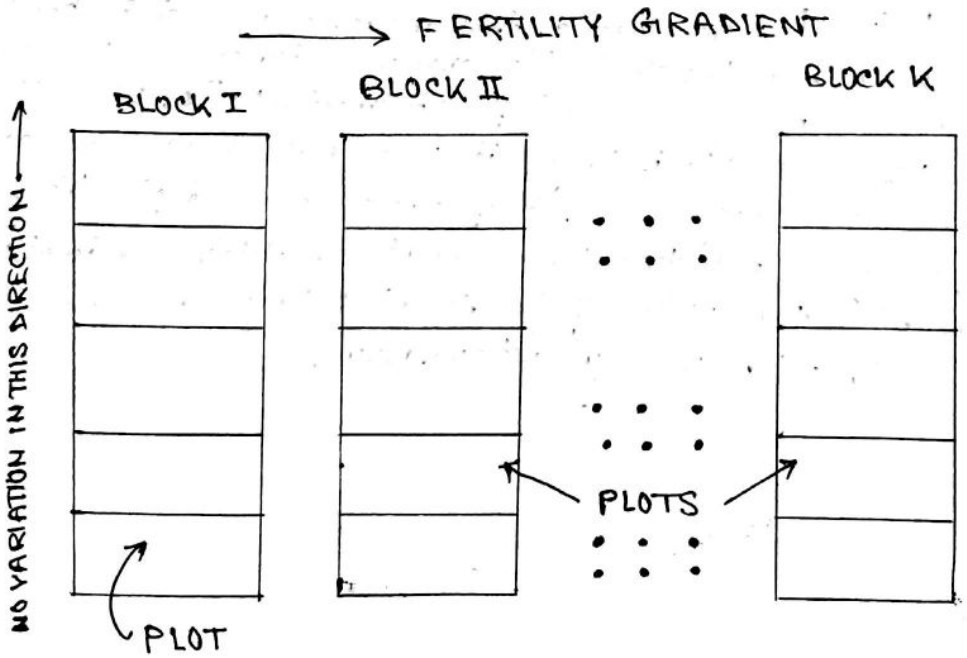
Accordingly, the field (which is expected to be heterogeneous with. fertility) can be divided into relatively homogeneous sub- groups (blocks) to control the experimental error. Incidently, uniformity trials also give us some idea about the shape and size of the plots to be used.

- **How do the size and shape of plots and blocks affect the result of a field experiment?** [CU]

The size of the plot depends on a number of factors such as the total experimental area available, the number of treatments, the number of replications of each treatment, the crop, and so on. If the total experimental area remains fixed, then an increase in the size of the plot will result in decrease in the number of plots and consequently result in an increase in the size of the block and decrease in the number of blocks. The shape and size of the blocks will usually depend upon the shape and size of the plots.

In order to control the experimental error, it is desirable to divide the whole experimental area into different sub- groups (blocks) such that within each block there is as much homogeneity as possible but between blocks there is maximum variation.

Further each block is to be divided into as many plots as the number of treatments. For maximum precision the plots should be rectangular in shape with their long sides parallel to the direction of the fertility gradient and the blocks should be arranged one after the other along the fertility gradient as shown in the Figure



• COMPLETELY RANDOMISED DESIGN (C.R.D.) :—

Designs are usually characterised by the nature of grouping of experimental units and the procedure of random allocation of treatments to the experimental unit. In a CRD the units are taken in a single group. As far as possible the units forming the group should be homogeneous. We shall sometimes use the word "plot" for unit.

Let there are k treatments in an experiment. Let the i^{th} treatment be replicated r_i times $(i=1,2,\cdots,k)$. The total number of experimental unit required for the design is thus $\sum_{i=1}^{k} r_i = R$ (say)

Replication:— Normally, the number of replications for different treatments should be equal as it ensures equal precision of estimates of the treatment effects. The actual number of replications is, however, determined by availability experimental resources and the requirement of precision and sensitivity of comparison. If the experimental material for some treatments is available in limited quantities, the numbers of their replication are reduced. If the estimates of certain treatment effects are required with more precision, the number of their replication are increased. This type of flexibility in the choice of number of replications is present only in this design.

Randomisation: (Layout) :— The term layout refers to the placement of experimental treatments on the experimental units according to the conditions of the design.

Let the number of the i^{th} treatment be written on r_i papers $(i=1,2,\cdots,k)$. The $\sum_{i=1}^{k} r_i$ pieces of papers are then folded individually so that the numbers written on them are not visible. These papers are then drawn one by one at random. The treatment which is drawn in the i^{th} draw is allotted to the i^{th} plot $(i=1,2,\cdots,R)$.

Local Control:— No local control measure as such is provided in this design excepting that the error variance can be reduced by choosing a homogeneous set of experimental units. When the number of treatments is large, it may not always be possible to get a large homogeneous set of units required for the experiment. It is, therefore, not desirable to adopt completely randomized designs when the number of treatments is large or when the experimental units are very heterogeneous.

Analysis:- This design provides a one-way classified data according to levels of a single factor, "treatment". For its analysis the following model is taken:

$$y_{ij} = \mu + t_i + e_{ij} \quad (i = 1(1)k,\ j = 1(1)n_i)$$

where y_{ij} is the observation from the j^{th} replicate of the i^{th} treatment, μ is the general mean, t_i is the fixed effect of the i^{th} treatment and $e_{ij} \overset{iid}{\sim} N(0, \sigma_e^2)$.

Assumption: $\sum_{i=1}^{k} n_i t_i = 0$.

The analysis in the present case is the same as that of one-way classified data as considered in ANOVA.

The ANOVA table:

ANOVA for a CRD:-

Source of Variation	D.F.	S.S	MS
Treatments	$k-1$	$\sum_{i=1}^{k} n_i (y_{io} - y_{oo})^2 = SST$	MST
Errors	$R-k$	$\sum_i \sum_j (y_{ij} - y_{io})^2 = SSE$	MSE
Total	$R-1$	$\sum_i \sum_j (y_{ij} - y_{oo})^2$	

The hypothesis that treatments have equal effects is tested by the F-test. We reject $H_0: t_1 = t_2 = \cdots = t_k$ at level α if

$$F = \frac{MST}{MSE} > F_{\alpha,\,(t-1),\,(n-t)}.$$

If F is significant, then the treatment effects are not equal. In such cases it becomes necessary to estimate and test individual treatment contrasts in which the experimenter may be interested.

Estimate any treatment contrast $\sum_{i=1}^{k} l_i t_i$, where t_i denotes the effect of the i^{th} treatment is obtained from $\sum l_i \hat{t}_i = \sum l_i \bar{y}_i$ where $\bar{y}_i = \sum_{j=1}^{n_i} y_{ij} / n_i$.

$\therefore \text{Var}\left(\sum l_i \bar{y}_i\right) = \sigma_e^2 \sum_{i=1}^{k} \frac{l_i^2}{n_i}$, where σ_e^2 is the error variance which is estimated by the error mean squares, MSE.

The significance of the contrast can be tested by t-test

where

$$t = \frac{\left| \sum_{i=1}^{k} l_i \bar{y}_i \right|}{\sqrt{MSE \sum_{i=1}^{k} \frac{l_i^2}{n_i}}} \qquad \text{with } R-K = \left(\sum_{i=1}^{k} n_i - k\right) \text{ d.f.}$$

Advantages and Disadvantages:—

The chief advantages of the completely randomised design are:

(i) It is easy to layout the design.

(ii) The design allocos for the maximum number of d.f. in the error sum of squares.

(iii) A completely randomised design has the simplest analysis of all experimental designs subject to statistical analysis.

(iv) Unequal number of replications for various treatments may be included without unduly complicating the analysis in most cases.

The chief disadvantages of the design is that

(i) it is usually suited for small numbers of treatments and for homogeneous experimental material.

(ii) When large numbers of treatments are included, a relatively large amount of experimental material must be used. This generally increases the variation among treatment responses.

(iii) If the variation over the entire experimental material is relatively large, it is possible to select more efficient designs than CRD.

Missing observations :— Missing observations do not make the analysis of this design complicated. If an observation from a treatment is missing in one or more replications, the actual number of replications where the treatment is not affected, is to be taken into account for computation.

• Randomised Block Design (RBD) :—

We have seen that in a completely randomised design no local control measure was adopted excepting that the experimental units should be homogeneous.

An improvement of the CRD's can be obtained by providing error control measures as described below. The resulting design is called randomised block design (RBD).

If the experimental material is not homogeneous, it may be possible to stratify or group the material into homogeneous groups. Let there are K treatments and each of the treatments is replicated the same number of times, say r, in this design. The number of experimental unit is Kr. These units are arranged into r groups, each of size K. The error control measure in this design consists of making the units in each of these groups homogeneous. the groups are commonly known as blocks. this type of homogeneous grouping of the experimental units and the random allocation of the treatments seperately in each block are the two main characteristic features of RBD. The number of blocks in the design is the same as the number of replications.

Randomization:— The treatments are first numbered from 1 to K in any order. The units in each block are also numbered, conveniently from 1 to K. The K treatments are then alloted at random to the K units in each block. Random allocation can be made either by consulting a random number table or by drawing of lots.

A column is first chosen. If the selected random number $\leq K$, the treatment corresponding to this number is allotted to the first plot in the first block. If it is zero or greater than K, it is omitted. Similarly, if the next number is not greater than K and has not been chosen earlier, the treatment corresponding to it is allotted to the second plot. When all the units in the first block are allotted, units in the second and subsequent blocks are allotted treatments similarly.

Local Control :— The principle of local control is adopted in this design by first forming homogeneous groups of the units and then allotting at random each treatment once in each group. this results in an increase in precision of estimates of the treatment contrasts, due to the fact that error variance which is a function of comparisions within blocks, is smaller because of homogeneous blocks. This type of allocation makes it possible to eliminate from error variance a portion of variation attributable to block differences.

Analysis:- The data collected from experiments with randomized block designs from a two-way classification, that is, classified to the levels of two factors, viz., blocks and treatments. There are Kn cells in the two-way table with one observation in each cell. We take the model $y_{ij} = \mu + t_i + b_j + e_{ij}$ $(i = 1(1)K, j = 1(1)n)$, where, y_{ij} denotes the observation from i^{th} treatment in the j^{th} block, μ be the general mean and t_i are effect of the i^{th} treatment and b_j are the effect of the j^{th} block. These effects are fixed and e_{ij} is the error component.

Assumptions:- $e_{ij} \overset{iid}{\sim} N(0, \sigma_e^2)$ $\forall (i,j)$ and $\sum_i t_i = \sum_j b_j = 0$.

Analysis of Variance of RBD:-

Source of Variation	D.f.	SS	MS	F
Blocks	$n-1$	$K\sum_j (y_{oj} - y_{oo})^2 = SSB$	MSB	$F = \frac{MST}{MSE}$
Treatments	$K-1$	$n\sum_i (y_{io} - y_{oo})^2 = SST$	MST	
Error	$(n-1)(K-1)$	$\sum_i \sum_j (y_{ij} - y_{io} - y_{oj} + y_{oo})^2 = SSE$	MSE	
Total	$nK-1$	$\sum_i \sum_j (y_{ij} - y_{oo})^2$		

The hypothesis that the treatments have equal effects, i.e.; $H_0: t_1 = t_2 = \dots = t_K$, is tested by F-test where H_0 is rejected at the level α if $F = \frac{MST}{MSE} > F_{\alpha;(K-1),(K-1)(n-1)}$.

When F is significant, we conclude that the treatment effects are different. We may then be interested to either compare the treatments in pairs or evaluate special contrasts depending on the objectives of the experiments.

Efficiency of a randomised block design:- In an RBD with n blocks and K treatments, let MSB and MSE denote the block MS and the error MS. Suppose we use the same treatment (a single treatment) in all the plots. Then the analysis for a single experiment with dummy treatment will be as shown below:

Due to	D.f.	Sum of Squares
Blocks	$(n-1)$	$(n-1)$ MSB
Error	$n(K-1)$	$n(K-1)$ MSE
Total	$nK-1$	$(n-1)$ MSB + $n(K-1)$ MSE

Here, we can get an estimate σ_e^2 from the pooled treatment and error components which the new error of the above table with d.f. $= (k-1) + (r-1)(k-1) = r(k-1)$.

Thus, the treatment of σ_e^2 from an RBD with a single treatment will be

$$E_{RBD} = \frac{r(k-1)\,MSE}{r(k-1)} = MSE.$$

Now, if we consider the above experiment in a CRD with a single treatment, there will be no separate Blocks component. This will be merged with error component. Thus the estimate of σ_e^2 from CRD with a single treatment will be

$$E_{CRD} = \frac{(r-1)\,MSB + r(k-1)\,MSE}{(rk-1)}$$

Measure of the efficiency of the RBD relative to a CRD is then the relative precision of the two estimates E_{RBD} and E_{CRD}, precision being defined as the reciprocal of the estimates of the error variance. Thus the relative efficiency of an RBD compared to a CRD is

$$\frac{E_{CRD}}{E_{RBD}} = \frac{(r-1)\,MSB + r(k-1)\,MSE}{(rk-1)\,MSE}$$

and this is $\gtrless 1$ according as $MSB \gtrless MSE$.

Advantages and Disadvantages :-

The RBD has many advantages over other design. It is quite flexible. It is applicable to a moderate number of treatments. If extra replications are necessary for some treatments, these may be applied to more than one unit per block. Since variability among replicates can be eliminated from experimental error, it is not necessary to use continuous blocks. Thus RBD is the most popular design with experimenters in view of its simplicity, flexibility and validity.

The chief disadvantages is that if the blocks are not internally homogeneous, then a large error term will result. As usually occurs in field experiments, with an increase in the number of treatments, the block size increases and so one has a less or control over error, for the block will include material of a more homogeneous nature. In such cases, special types of incomplete block designs are used to reduce the block size.

• **LATIN SQUARE DESIGN (LSD):-** It has pointed out earlier that the randomised block designs are improvements over CRD in the sense that they provide error control measures for the elimination of block variation. This principle can be extended further to improve randomised block designs by eliminating more sources of variation. Latin square design is one such improved design with provision for the elimination of two sources of variation.

For the LSD, two restrictions are imposed; namely, that for an experiment area divided into rows and columns, each treatment must appear once in a row and once in a column. Thus for latin squares, the treatments are grouped into replicates in two ways, once in rows and once in columns. Though the elimination of row and column effects from the within treatment variation the residual or error variance may be considerably reduced.

Let there be K treatments each replicated K times so that the total number of experimental unit required is K^2.

Let P and Q denote two factors whose variabilities are to be eliminated from the experimental error by having a suitable design. Each of the factors P and Q is taken at K levels. The total number of level combinations of the two factors is K^2. The K^2 experimental units are now so chosen that each unit possesses a different level combination of the two factors.

Examples:- (1) In an animal experiment with the object of comparing effects of four feeds, let young calves be the experimental units with their growth rate during a certain period as the variate under study. Let it be intended to eliminate the variation due to breeds and ages of the calves. So breed and age are the two factors P and Q. The calves are, therefore, to come from four breeds and four age groups. The 16 calves required for the experiment should each belong to a different breed-age combination.

(2) The effect of four ingredients (A, B, C, D) on the reaction time of a chemical process is being studied. Each batch of raw material is only large enough to permit four runs to be made. Furthermore, each run requires approximately two hours, so only four runs can be made in one day.

So, different parts of raw material and different shifts in a day are the two factors P and Q. The four ingredients (A, B, C, D) are applied on four different parts of raw material and four shifts. The 16 reaction time of a chemical process are to be observed for the experiment so that each belonging to a different part-shift combination. (CU'2005)

Randomization:- According to the definition of a LSD, treatments can be allocated to the k^2 units in a number of ways. There is, therefore, a number of latin squares of a given order. The purpose of randomization is to select one of these squares at random.

The totality of LSDs obtained from a single LSD by permuting the rows, columns and treatments (letters, say, A, B, C, etc) is called a transformation set. An $m \times m$ Latin square with the m letters A, B, C, ... in the natural order occuring in the 1st row and in the first column is called a standard square. Thus, the standard square corresponding to the square cited above is

A	B	C	D
B	C	D	A
C	D	A	B
D	A	B	C

From a standard $K \times K$ Latin square, one may obtain $K!(K-1)!$ different LSD's by permutating all the K columns and the $(K-1)$ rows except the first row. Thus, the total number of different LSDs in a transformation set is $K!(K-1)!$ times the number of standard LSDs in the set.

For randomisation, one select with equal probability one standard square from all the standard $K \times K$ LSDs and rows, columns of the selected squares are rearranged at random except the first row. If the latin square of order K, then random numbers $\leq (K-1)$ are selected by consulting a random number table. If the first number chosen is n_1, then the n_1^{th} row of the initial square is written as the second row. If n_2 is the 2nd number and not equal to n_1, then n_2^{th} row is written as the third row. this procedure is continued till all the rows of the initial square are placed at random to form another square. After row randomization is over, the columns of the row randomized squares are rearranged by following exactly a similar procedure as for row randomization.

Analysis:— In LSDs there are three factors, there are the factors P, Q and treatments. the data collected from this design are, therefore, analysed as a three-way classified data.

The LSD is actually an incomplete three-way layout, where all the three factors, viz., row, column and treatment, are at the same number of levels (K). For a complete three-way layout with each factor at K levels, there should be K^3 observations but because of the particular allocation of treatments to the cells, there is only one observation per cell instead of K in the usual three-way classified orthogonal data. In agricultural experiment, if there is soil fertility in two mutually perpendicular directions, then the adoption of an LSD proves useful. According, we take the model

$y_{ijs} = \mu + r_i + c_j + t_s + e_{ijs}$, where y_{ijs} is the observation in i^{th} row, j^{th} column and under s^{th} treatment. μ, r_i, c_j, t_s $(i,j,s = 1,2,\ldots,k)$ are fixed effects denoting in order the general mean, the row, the column and the treatment effects.

Assuming, $e_{ijs} \overset{iid}{\sim} N(0, \sigma_e^2)$, $\sum_{i=1}^{K} r_i = \sum_j e_j = \sum_s t_s = 0$.

ANOVA table for KXK LSD

Source of Variation	D.F.	SS	MS	F
Rows	$K-1$	$K\sum_i (y_{i00} - y_{000})^2 = SSR$		
Columns	$K-1$	$K\sum_j (y_{0j0} - y_{000})^2 = SSC$		
Treatments	$K-1$	$K\sum_s (y_{00s} - y_{000})^2 = SST$	MST	$F = \frac{MST}{MSE}$
Error	$(K-1)(K-2)$	by subtraction	MSE	
Total	K^2-1	$\sum_{i,j,s} (y_{ijs} - y_{000})^2$		

The hypothesis of equal treatment effects is tested by F-test. If F is significant, further studies to test the significance of any treatment contrast can be made in exactly the same way as discussed for CRD.

Efficiency of LSD :- It may be desirable to test whether the row classification or the column classification or both have led to increased precision in the experiment.

The ANOVA for LSD with K^2 - experimental units :

Due to	D.f.	M.S.
Rows	$K-1$	MSR
Columns	$K-1$	MSC
Treatments	$K-1$	MST
Errors	$(K-1)(K-2)$	MSE
Total	K^2-1	

Then the analysis for a single experiment with dummy treatments (i.e. under a uniform treatment condition) will be as shown below :

Due to	D.F.	Sum of Squares
Rows	$K-1$	$(K-1)MSR$
Columns	$K-1$	$(K-1)MSC$
Errors	$(K-1)^2$	$(K-1)MSE$
Total	K^2-1	

In the ANOVA table, the error MS have d.f. of treatment MS as well because the treatment variance is the as error variance under uniform treatment condition.

Instead of adopting an LSD, an experimental area with K^2 experimental unit, if an RBD with rows as blocks is adopted under a uniform treatment condition, the analysis could be as shown as below :

Due to	D.f.	Sum of squares
Blocks (rows)	$K-1$	$(K-1)MSR$
Error	$K(K-1)$	$(K-1)MSC+(K-1)^2MSE$
Total	K^2-1	

The efficiency of LSD relative to RBD with rows as blocks may be called column efficiency,

Thus, $$E_{(column)} = \frac{(K-1)MSC + (K-1)^2 MSE}{K(K-1)MSE} = \frac{MSC + (K-1)MSE}{K \cdot MSE}$$

Similarly, $$E_{(row)} = \frac{MSR + (K-1)MSE}{K \cdot MSE}$$

The efficiency of the LSD relative to CRD is estimated by

$$E = \frac{(K-1)MSR + (K-1)MSC + (K-1)^2 MSE}{(K^2-1)MSE} = \frac{MSR + MSC + (K-1)MSE}{(K+1)MSE}$$

Advantages of Latin-Square Design (LSD):—

1. With two-way grouping or stratification L.S.D. controls more of the variation than CRD or RBD. The two-way elimination of variation as a result of cross-grouping often results in small error mean sum of squares. Thus, in field experimentation if the fertility gradient is in two directions at right angles to each other then L.S.D. is more efficient than R.B.D. In fact, L.S.D. can be used with advantage of those cases where the variation in experimental material is from two orthogonal sources.

2. L.S.D. is an incomplete 3-way layout. Its advantages over the complete 3-way layout is that instead of m^3 experimental units only m^3 units are needed. Thus a 4×4 L.S.D. results in saving of $m^3 = 4^3 - 4^2 = 48$ observations over a complete 3-way layout.

3. More than one factors can be investigated simultaneously and with fewer trials than more replicated designs.

Disadvantages of Latin-Square Design:—

1. The fundamental assumption that there is no interaction between the three factors of variation may not be true in general.

2. Unlike RBD, in LSD the number of treatments is restricted to the number of replications and this limits its field of application.

3. In the field layout, RBD is much easy to manage than LSD, since the former can be performed equally well on a square or rectangular field or a field of any shape whereas for the latter approximately a square field is necessary.

• **The Split plot design:—** The very nature of the levels of one factor, say A, may be such as to exclude the use of small plots or units, or the experimenter may know that the levels of the factor usually differ in field. In such circumstances the levels of factor A ($A_1, A_2, \ldots, A_p$) may be laid out in relatively large units (whole plots) designed as an RBD or LSD. Since the whole plots are large by necessity or by design, it may be desirable to compare levels of another factor, say B, on each plot, the q levels are $B_1, B_2, \ldots, B_q$ being allotted to the split plot or sub-plots of each whole plot at random. This is done by splitting the plots (whole plots) of the factor A into as many sub-plots as there are levels of factor B.

Randomization:— The randomization procedure for the whole plots is determined by the particular design chosen (RBD or LSD). The sub-plot treatments are randomly allotted to the units within each whole plot. A different randomization is used within each whole plot.

Analysis:

(a) **RBD (Whole plots in an RBD):—** Suppose we have a factor A at p levels and which are arranged in an RBD using r blocks, and a second factor B at q levels, which are applied to the plots of a block after subdividing each plot into q subplots. So there are p whole plots in a block and q sub-plots in a whole plot.

The analysis will be based on the model:

$$y_{ijk} = \mu + b_i + \tau_j + e_{ij} + \delta_{jk} + \gamma_k + e_{ijk}$$, where

$i = 1(1)r$, $j = 1(1)p$, $k = 1(1)q$, and $\tau_j, \gamma_k, \delta_{jk}$ are the fixed effects due to the j^{th} level of A and the k^{th} level of B and the interaction between A_j and B_k, respectively, with

$$\sum_j \tau_j = \sum_k \delta_k = \sum_{j \text{ for all } k} \delta_{jk} = \sum_{k \text{ for all } j} \delta_{jk} = 0 .$$

The random components b_i, e_{ij} and e_{ijk} are independently dist'd. with zero means and respective variances $\sigma_b^2, \sigma_e^2, \sigma_{e'}^2$.

Initially, we use the analysis of RBD with p treatments in r blocks, where each plot value is based on the total of q sub-plots values.

The whole-plot ANOVA :-

Due to	D.F.	S.S.
Blocks (or Replicates)	$r-1$	$SS(\text{blocks}) = SS(\text{Replicates}) = pq\sum_i (\bar{y}_{i\cdots} - \bar{y}_{\cdots})^2$
Whole-Plot treatments (A)	$p-1$	$SSA = rq \sum_j (\bar{y}_{oj o} - \bar{y}_{ooo})^2$
Whole plot error (E_I)	$(r-1)(p-1)$	by subtraction
Total	$rpq-1$	$q\sum_i\sum_j (y_{ijo} - \bar{y}_{ooo})^2$

The sub-plot analysis within the whole plots :-

Due to	D.F.	SS
Sub-plot treatments (B)	$q-1$	$SSB = rp\sum_k (\bar{y}_{ook} - \bar{y}_{ooo})^2$
Interaction (AB)	$(p-1)(q-1)$	$SS(AB) = r\sum_j\sum_k (\bar{y}_{ojk} - \bar{y}_{ojo} - \bar{y}_{ook} + \bar{y}_{ooo})^2$
Sub-plot error (E_{II})	$p(q-1)(r-1)$	$\sum_i\sum_j\sum_k (y_{ijk} - \bar{y}_{ijo})^2$ (total between sub-plots within whole plots)
Total	$rpq-1$	

The two analyses of variance may be combined into one, in such a way that the constituent parts add up to the total sum of squares about the mean.

ANOVA for a split-plot experiment in a RBD :-

Due to	Df	SS	E(MS)	F
Blocks (Replicates)	$r-1$	$SS(\text{blocks})$	—	—
Whole plot treatments (A)	$p-1$	SSA	$\sigma_e'^2 + q\sigma_e^2 + \phi_1(\tau_j's)$	$\frac{MSA}{MSE_I}$
Block X treatment (A) (Error I)	$(r-1)(p-1)$	SSE_I	$\sigma_e'^2 + q\sigma_e^2$	—
Sub-plot treatments (B)	$(q-1)$	SSB	$\sigma_e'^2 + \phi_2(\gamma_k's)$	$\frac{MSB}{MSE_{II}}$
AXB interaction	$(p-1)(q-1)$	$SS(AB)$	$\sigma_e'^2 + \phi_3(\delta_{jk}'s)$	$\frac{MS(AB)}{MSE_{II}}$
Remainder or Error II	$p(q-1)(r-1)$	SSE_{II}	$\sigma_e'^2$	
Total	$pqr-1$	SST		

Remark :- Difference between split-plot and RBD is that, while in the split-plot the randomisation is done seperately for the whole-plot treatments to the whole plots of a block and the sub-plot treatments to the sub-plots of a whole plot, in the RBD all the combinations of the two factors are allotted at random to the plots of a block. This enables us to test the main effects of the sub-plot treatments (B) and the interaction of the whole-plot treatments and sub-plot treatments (AXB) more efficiently than the main effects of the whole-plot treatments (A) in a split plot design. On the other hand, the main effects and interaction are all tested equally efficiently than the two-factor experiment in an RBD.

(b) LSD (Whole plots in LSD) :- If the number of whole-plot treatments is not very large, say, less that 8, and the number of replicates can be made equal to the number of these treatments, it is possible to arrange the whole-plot treatments according to a Latin square, each whole-plot being divided into the requisite number of split plots for the split-plot treatments.

Randomisation :- The whole-plot treatments are arrange at random in a Latin-square in the usual way, each whole plot is divided into a requisite no. of split-plots, and the split-plot treatments are randomized within whole-plots.

Analysis :- The model for this experiment with p rows and p columns, p whole-plot treatments and q split-plot treatments is

$$y_{ijkl} = \mu + r_i + c_j + t_k + e_{ijk} + s_l + (ts)_{kl} + e_{ijkl} ;$$

where, $i, j, k = 1(1)p$, $s = 1(1)q$; the parameters t_k, s_l, $(ts)_{kl}$, r_i, c_j are the effect of k^{th} level of whole-plot treatment, l^{th} level of split-plot treatment, interaction of k^{th} level of whole-plot and l^{th} level of split-plot treatments, i^{th} row, j^{th} column, e_{ijk} and e_{ijkl} are independent normals with zero mean and respective variances σ_e^2, $\sigma_{e'}^2$.

ANOVA for split-plot experiment in an LSD :—

Due to	d.f	S.S	F
Rows (R)	$p-1$	$\frac{1}{pq}\sum_i Y_{i000}^2 - CF = SS(R)$	—
Columns (C)	$p-1$	$\frac{1}{pq}\sum_j y_{0j00}^2 - CF = SS(C)$	—
Whole-plot treatments (T)	$p-1$	$\frac{1}{pq}\sum_k y_{00k0}^2 - CF = SS(T)$	$\frac{MST}{MSE_I}$
Whole plot error	$(p-1)(p-2)$	by subtraction = $SS(E_I)$	
Total for whole plots	p^2-1	$\frac{1}{q}\sum_i\sum_j Y_{ij00} - C.F.$	—
split-plot treatments (S)	$q-1$	$\frac{1}{p^2}\sum_l y_{00\cdot l}^2 - CF = SS(S)$	$MS(S)/MSE_{II}$
Interaction (S×T)	$(p-1)(q-1)$	$\frac{1}{p}\sum_{k,l} Y_{00kl}^2 - CF - SS(T) - SS(S)$	$\frac{MS(AB)}{MSE_{II}}$
Error	by subtraction	SSE_{II}	
Total	p^2q-1	$\sum_i\sum_j\sum_k\sum_l y_{ijkl}^2 - C.F.$	

$$C.F. = \frac{Y_{0000}^2}{p^2q}$$, where

$$Y_{i000} = \sum_j\sum_k\sum_l y_{ijk}, \text{ etc.}$$

Efficiency :— (a) The efficiency of the split-plot where whole-plot is in an RBD, relative to the RBD on the B and AXB comparisons in

$$\frac{[(p-1)+(p-1)(r-1)]MSE_I + [(q-1)+(q-1)(p-1)+p(r-1)(q-1)]MSE_{II}}{(pqr-r)MSE_{II}}$$

$$= \frac{(p-1)MSE_I + p(q-1)MSE_{II}}{(pq-1)MSE_{II}}$$; disregarding the difference in the number of d.f.'s.

On the other hand, the efficiency on the A effect on the whole plot comparisons would be decreased, the formulae of efficiency in this case being $\frac{(p-1)MSE_I + p(q-1)MSE_{II}}{(pq-1)MSE_{II}}$

(b) The efficiency of the split-plot where plots (whole) in an LSD, relative to an RBD is estimated by

$$\frac{\{MSc + (p-1)MSE_I\}\frac{p-1}{p} + p(q-1)MSE_{II}}{(pq-1)MSE_I}.$$

It has been found that the split-plot where whole plots in an LSD is almost as precise as if an RBD has been used for pq treatment. This feature adds considerably to the attractiveness of split-plot design.

- **Strip-plot design:-** In a variant the split-plot treatments, instead of being randomised independently within each in each block (or replicate), are arranged in strips across each block (or replication). This layout may be convenient for field experiments where it is necessary to test both factors on relatively large areas and to leave free access at both ends.

In a strip-plot design, we divide each replicate into a number of rows (same as the no. of levels of one factor A) and a no. of columns (same as the no of levels of B). The rows and columns are strip. the 'p' levels of A and 'q' levels of B are randomised in rows and in columns respectively. Here an entire row receives a single level of A while an entire column receives a single level of B. the random allocations of the levels of A and B are done afresh for each of the 'b' blocks or replicate. Since both A, B are applied to strips (larger areas), so the main effects of A and B will have lower precision than A×B.

The model is:

$$y_{ijk} = \mu + r_i + \alpha_j + (r\alpha)_{ij} + \beta_k + (r\beta)_{ik} + (\alpha\beta)_{jk} + (r\alpha\beta)_{ijk}$$

where y_{ijk} is the yield of the plot receiving j^{th} level of A, k^{th} level of B in the i^{th} replicate.

Assumptions: (i) $\alpha_i, \beta_k, (\alpha\beta)_{jk}$ are fixed effects.

The errors $(r\alpha)_{ij} \overset{iid}{\sim} N(0, \sigma_1^2)$, $(r\beta)_{ik} \overset{iid}{\sim} N(0, \sigma_2^2)$ and $(r\alpha\beta)_{ijk} \overset{iid}{\sim} N(0, \sigma_3^2)$.

ANOVA Table :—

Due to	D.F.	S.S	E(MS)	F
Replicates (R)	$b-1$	SSR	—	
Treatments (A)	$p-1$	SSA	$\sigma_3^2 + q\sigma_1^2 + \frac{bq}{p-1}\sum_j \alpha_j^2$	MSA/MSE_I
Error I (RXA)	$(b-1)(p-1)$	SSE_I	$\sigma_3^2 + q\sigma_1^2$	—
Treatments (B)	$q-1$	SSB	$\sigma_3^2 + p\sigma_2^2 + \frac{bp}{q-1}\sum_k \beta_k^2$	MSB/MSE_{II}
Error II (RXB)	$(b-1)(q-1)$	SSE_{II}	$\sigma_3^2 + p\sigma_2^2$	—
Interaction (AXB)	$(p-1)(q-1)$	SS(AB)	$\sigma_3^2 + \frac{b\sum_{j,k}(\alpha\beta)_{jk}^2}{(p-1)(q-1)}$	$MS(AB)/MSE_{III}$
Error III (RXAXB)	$(b-1)(p-1)(q-1)$	SSE_{III}	σ_3^2	—
Total	$bpq-1$	SST	—	—

• Advantages and disadvantages of split-plot design:-

Advantages of SPD :-

1. The split-plot design is advantageous when:

(i) The main effects of one of the two factors are large enough to be deducted even a with a lower precision, so that the factor may be allotted to the main plots.

(ii) The main effects of the factor allotted to the main plots are not of much interest as compared to the effects of the factor allocated to the sub-plots and the interaction between the two factors is of primary interest.

2. In SPD, of the two errors, $SSE_2 < SSE_1$. This implies that, usually the main effect B and the interaction effect AB will be estimated and tested more precisely than the main effect A.

3. Overall precision SPD can, however, be increased by designing the whole-plot treatments in Latin square or in an incomplete Latin square.

Disadvantages of SPD :-

1. The whole-plot treatments are measured with less precision than they are in a randomised complete block design of pq treatments in each r replications.

2. The computation of two types of error sum of squares SSE_1, and SSE_2 makes the analysis more complex or difficult.

3. The different treatment comparisons have different basic error variances (SE's) which make the analysis more complex as compared with the corresponding RBD.

Missing Observation in RBD:—

It may happen that due to some unforeseen causes, obsn.s from some plots are missing. Consequently the data become non-orthogonal and they can't be analysed according to the method of analysis suggested for the design. Missing plot technique is based on the following theorem:

If the value 'f' in the augmented model $y_a = X\beta + \epsilon$, are chosen to minimize the SSE of this model, the resulting normal equations and SSE are the same as the normal equations and SSE in the model: $y_e = X_e\beta + \epsilon_e$ of the existing obsn.s y_e only.

RBD: In a RBD with k treatments and r replications, let one observation be missing from the i^{th} treatment and j^{th} block.

Let T_i' and B_j' denote the observation total to i^{th} treatment and j^{th} block taking zero for the missing obsn. Further G' denote the grand total of the obsn.s taking zero for the missing plot. sum of squares.

Due to	Sum of squares
Blocks	$\sum_{m \neq j} \frac{B_m^2}{k} + \frac{(B_j'+x)^2}{k} - \frac{(G'+x)^2}{rk}$
Treatments	$\sum_{n \neq i} T_n^2/r + \frac{(T_i'+x)^2}{r} - \frac{(G'+x)^2}{rk}$
Error	By Subtraction
Total	$\sum_{(m,n) \neq (j,i)} y_{nm}^2 + x^2 - \frac{(G'+x)^2}{rk}$

The error SS is then

$$SSE = C + x^2 - \frac{(T_i'+x)^2}{r} - \frac{(B_j'+x)^2}{k} + \frac{(G'+x)^2}{rk}$$

where C does not contain x.

The missing value is estimated by minimising this SSE w.r.t. x and this simply done by equating the derivative w.r.t. x to zero and solving for x, i.e.

$$2x - \frac{2(T_i'+x)}{r} - \frac{2(B_j'+x)}{k} + \frac{2(G'+x)}{rk} = 0$$

$$\Rightarrow x = \frac{kT_i' + rB_j' - G'}{(r-1)(k-1)}.$$

The data are then completed with this estimated value and analysed by the usual technique appropriate for RBD. From this analysis, the correct SSE is obtained but not the treatment S.S. Here the correct SST is obtained by subtracting the correct SSE from 'block SS' obtained from the incomplete data.

ANOVA for exact test

Due to	d.f.	Sum of squares
Blocks	$(r-1)$	$\sum_{m \neq j} \frac{B_m^2}{k} + \frac{B_j'^2}{k-1} - \frac{G'^2}{rk-1}$
Treatments	$(k-1)$	By subtraction
Error	$(r-1)(k-1)-1$	$SSE_0 - \frac{M^2}{R}$ = SSE with the estimated value
Total	$rk-2$	$\sum_{n \neq i} \sum_{m \neq j} y_{nm}^2 - \frac{G'^2}{rk-1}$

Notation:- SSE_0 and SST_0 denote the error and treatment SS obtained by taking zero for the missing value.

Remark:- Estimate of missing value $x = \frac{M}{R}$

Correct $SSE = SSE_0 - \frac{M^2}{R}$

Adjusted (or, correct) $SST = SST_0 + \frac{M^2}{R} - \frac{M'^2}{R'}$

where $M = \frac{B_j'}{k} + \frac{T_i'}{r} - \frac{G'}{rk}$, $R = 1 - \frac{1}{r} - \frac{1}{k} + \frac{1}{rk}$

$M' = \frac{B_j'}{k}$, $R' = \left(1 - \frac{1}{k}\right)$.

Variance of estimate of elementary contrasts in RBD:-

The estimate of the missing value in RBD is

$x = \frac{kT_i' + rB_j' - G'}{(k-1)(r-1)}$, where the value in the i^{th} treatment and j^{th} block, i.e. y_{ij} is missing.

The estimate of the i^{th} treatment effect is $\hat{t}_i = \frac{T_i' + x}{r}$. If $\hat{t}_p$ denotes the estimate of an unaffected treatment then $\hat{t}_p = \frac{T_p}{r}$.

Using the fact that the y_{ij} are distributed with common mean '0' and variance 'σ_e^2' and that $Var\left(\sum \lambda_{ij} y_{ij}\right) = \sigma_e^2 \sum \lambda_{ij}^2$ we find that

$$Var(\hat{t}_i) = \frac{\sigma_e^2}{r-1}\left\{1 + \frac{1}{r(k-1)}\right\} = \frac{\sigma_e^2}{r}\left[1 + \frac{k}{(r-1)(k-1)}\right]$$

Variance of any other treatment effect $\hat{t}_p$ is $\frac{\sigma_e^2}{r}$ and the treatment means are uncorrelated.

For any contrast among the treatment means, $\sum_n \lambda_n \hat{t}_n$ with $\sum_n \lambda_n = 0$, the variance of the contrast is

$$\frac{\sigma_e^2}{r} \sum_{n \neq i} \lambda_n^2 + \lambda_i^2 \frac{\sigma_e^2}{r}\left[1 + \frac{k}{(r-1)(k-1)}\right]$$

clearly, $Var\left(\hat{t}_i - \hat{t}_p\right) = \frac{\sigma_e^2}{r}\left[2 + \frac{k}{(r-1)(k-1)}\right]$

and $Var\left(\hat{t}_p - \hat{t}_q\right) = 2\sigma_e^2/r$.

Missing Observation in LSD:-

It may happen that due to some unforeseen causes, observations from some plots are missing. Consequently the data become non-orthogonal and they can't be analysed according to the method of analysis suggested for the design. Missing plot technique is based on the theorem: If the values 'f' in the augmented model $y_a = X\beta + \varepsilon$, are chosen to minimize the SSE of this model, the resulting normal equations and SSE are the same as the normal equations and SSE in the model: $y_e = X_e\beta + \varepsilon_e$ of the existing observations.

LSD: Let one obsn. is missing in a LSD on the plot in row u, column v and treatment w.

Let R_u' denote the total of the u^{th} row taking zero as the missing value. Similarly, C_v' and T_w' are the total of the v^{th} column and w^{th} treatment taking zero as the missing value. G' denotes the grand total taking zero as the missing value.

ANOVA Table:

Due to	D.F.	S.S.
Rows	$(k-1)$	$\sum_{i\neq u} \frac{R_i^2}{k} + \frac{(R_i'+x)^2}{k} - \frac{(G'+x)^2}{k^2}$
Columns	$(k-1)$	$\sum_{j\neq v} \frac{C_j^2}{k} + \frac{(C_j'+x)^2}{k} - \frac{(G'+x)^2}{k^2}$
Treatments	$(k-1)$	$\sum_{s\neq w} \frac{T_s^2}{k} + \frac{(T_w'+x)^2}{k} - \frac{(G'+x)^2}{k^2}$
Error	—	by subtraction
Total	—	$\sum_{(i,j,s)\neq(u,v,w)} y_{ijs}^2 + x^2 - \frac{(G'+x)^2}{k^2}$

The missing values is estimated by minimizing SSE

$$= c + x^2 + \frac{2(G'+x)^2}{k^2} - \frac{1}{k}\left\{(R_u'+x)^2 + (C_v'+x)^2 + (T_w'+x)^2\right\}$$

where c does not contain x.

Now, $$0 = \frac{\partial(SSE)}{\partial x} = 2x + \frac{4(G'+x)}{k^2} - \frac{2}{k}\left\{(R_u'+x) + (C_v'+x) + (T_w'+x)\right\}$$

$$\Rightarrow x = \frac{k(R_u' + C_v' + T_w') - 2G'}{(k-1)(k-2)}.$$

The data are completed with this estimated values and analyzed the data by the usual techniques appropriate for LSD. From this analysis the correct SS is obtained but not the treatment SS.

ANOVA for exact test

Due to	D.F.	S.S.
Rows	$k-1$	$\sum_{i \neq u} \frac{Ri^2}{k} + \frac{Ru'^2}{k-1} - \frac{G'^2}{k^2-1}$
Columns	$k-1$	$\sum_{j \neq v} \frac{cj^2}{k} + \frac{cv'^2}{k-1} - \frac{G'^2}{k^2-1}$
Treatments	$k-1$	by subtraction
Error	$(k-1)(k-2)-1$	$SSE_0 - \frac{M^2}{R}$
Total	k^2-2	$\sum_{(i,j,s) \neq (u,v,w)} y_{ijs}^2 - \frac{G'^2}{k^2-1}$

Notation:- SSE_0 and SST_0 denote the error and treatment SS, obtained by taking zero for missing value.

Remark:- $\hat{\alpha} = \frac{M}{R}$, correct $SSE = SSE_0 - \frac{M^2}{R}$,

Adjusted / correct Treatment SS $= SST_0 + \frac{M^2}{R} - \frac{M'^2}{R'}$

where $M = \frac{k(Ru' + cv' + Tw') - 2G'}{k^2}$, $R = 1 - \frac{3}{k} + \frac{2}{k^2}$

$M' = \frac{k(cv' + Ru') - G'}{k^2}$, $R' = 1 - \frac{2}{k} + \frac{1}{k^2}$

Variance of estimate of elementary contrasts:-

$$Var(\hat{t}_i) = \frac{\sigma_e^2}{r}\left\{1 + \frac{r}{(r-1)(r-2)}\right\}$$

Hence, $Var(\hat{t}_i - \hat{t}_p) = \frac{\sigma_e^2}{r}\left\{2 + \frac{r}{(r-1)(r-2)}\right\}$

and $Var(\hat{t}_p - \hat{t}_q) = \frac{2\sigma_e^2}{r}$, where $\hat{t}_p, \hat{t}_q$ denote the treatment effects uneffected by missing values.

ANALYSIS OF COVARIANCE (ANCOVA)

This is an extension of the ANOVA technique to cover the case from where observations are taken on more than one variable from each experimental unit. Interest, however, centers on one of these (y, called the devendent variable) and the question is whether the variation of the dependent variable over the classes is due to class effects or due to its dependence on the other variables (x's, called independent variable), which also vary from class to class. The ANCOVA controls the experimental error by taking into consideration the dependence of y on the x's.

Some simple examples where ANOVA technique are used:

i. The yield of a crop may depend on the number of plants per plot, and we may consider the number of plants as the concomitant variable and perform the analysis of covariance.

ii. In a study of the effect of drugs or diets on the growth of animals, the growth may depend on the initial condition (say, initial weight) of the animals and an analysis of covariance may be performed.

Remark:- The concomitant variable need not necessarily be measurable. Even if it is a quality characteristic which can't be measured quantitatively.

Example:- Intelligence, poverty, indifference, good/bad, presence/absence, etc. can be suitably converted into numerical scores, the use of ANCOVA results in a considerable increase in precision.

FACTORIAL EXPERIMENTS

In the foregoing experiments performed either CRD, RBD or LSD, we were primarily concerned with the comparison of the effects of different levels of a factor like varieties of Rice, varieties of a fertilizer, etc. Such experiments which deals with only one factor at a time may be called simple experiments.

In industrial applications frequently we know that the several factors may affect the characteristics in which we are interested, but we wish to estimate the effects of each of the factors and how the effect of one factor varies over the levels of other factors. The logical procedure could be to vary all factors simultaneously within the frame work of the same experiment. When we do so, we have what is now widely known as a factorial design.

In factorial design as the term indicates, effects of several factors of variation are investigated simultaneously, the treatments being all the combinations of different levels of the factors and they are then randomly allotted accordingly to CRD, LSD or RBD. For example, if there are p levels of factor A and q levels of factor B, each replicate contains all pq treatment combinations when they are allotted accordingly to CRD, RBD, LSD. Then this factorial experiment is called a pxq factorial experiment.

If a factorial design involves k factors with levels $p_1, \ldots, p_k$ then it is called $p_1 \times p_2 \times \cdots \times p_k$ factorial experiment. If the number of levels of each factor in an experiment is the same, the experiment is called symmetrical factorial; otherwise, it is called asymmetrical factorial.

NOTE: Prior to R.A. Fisher (1926) factorial experiments were called 'complex experiments'.

2^3 - experiment means an experiment with 3 factors at 2 levels each and 3^2 - experiment means an experiment with 2 factors at 3 levels each.

Advantages of factorial experiment: —

1. It increases the scope of the experiment and its inductive value and it does so mainly by giving information not only on the main factors but on their interactions.
2. When there are no interactions, the factorial design gives the maximum efficiency in the estimate of the effects.
3. When interactions exist, their nature being unknown a factorial design is necessary to avoid misleading conclusions.

Basic ideas and notions in the 2^n- factorial experiment :-

Sometimes experiments are conducted with n factors each at two levels. These are called 2^n- factorial experiments. The levels of a factor may be its presence and absence or a high and a low dose or even two modes of application of techniques.

A. 2^2 - factorial Experiment :- Following the notations due to Yates, let the capital letters A and B indicate two factors, under study and let the small letters a and b denote one of the two levels of each of the corresponding factors and this will be called the second level. The first level of A and B is generally expressed by the absence of the corresponding letter in treatment combinations. The four treatment combinations are denoted as follows:

$a_0b_0 \equiv 1$: Factor A and B both at the first level
$a_1b_0 \equiv a$: A at second level and B at first level
$a_0b_1 \equiv b$: B at second level and A at first level
$a_1b_1 \equiv ab$: A and B both at second level.

- Main and Interaction Effects :- These four treatment combinations 1, a, b, ab can be compared by laying out the experiment in CRD, RBD and LSD and ANOVA can be carried out accordingly.

Suppose the factorial experiment with $2^2 = 4$ treatments is conducted in r replicates. Let [1], [a], [b], [ab] denote the total yields of the r plots receiving the treatments 1, a, b, ab respectively and let the corresponding mean values obtained by dividing those totals by r is denoted by (1), (a), (b), (ab). The letters A, B, AB when they refer to numbers will represent the main effects due to the factors A and B and their interaction AB respectively.

Noth that ——

(i) The effect of A at the first level b_0 of B is
$$(a_1b_0) - (a_0b_0) = (a) - (1)$$

(ii) The effect of A at the second level b_1 of B is
$$(a_1b_1) - (a_0b_1) = (ab) - (b).$$

These two effects are called the sample effects of the factor A.

If two factors act independently of one another, one should expect the true expect the true effect of one to be same at either level of other. Under independence of A and B, one should expect that the two quantities observed, in (i) and (ii) are really the estimates of the same thing. Then the average observed effect of A over the two levels of B is called the main effect due to A

and is defined by :

$$A = \frac{(ab) - (b) + (a) - (1)}{2} = \frac{1}{2}(a-1)(b+1)$$, where the RHS is to be expanded algebrically and then the treatment combinations are to be replaced by treatment means.

Similarly, the main effect due to B is :

$$B = \frac{1}{2}\left[(ab) - (a) + (b) - (1)\right] = \frac{1}{2}(a+1)(b-1).$$

If the two factors were acting independently we could expect the two sample effects (i) and (ii), to be equal but in general they will be different and their difference is a measure of the extent in which two factors interact.

Hence a measure of the interaction of A with the factor B is $\{(ab) - (b)\} - \{(a) - (1)\}$ and a measure of interaction of B with the factor A is $\{(ab) - (a)\} - \{(b) - (1)\}$; they are same.

Therefore the interaction between A and B is either AB or BA and $\{AB + BA\} = \{(ab) - (b) - (a) + (1)\}$

$$\Rightarrow AB \text{ or } BA = \frac{1}{2}\{(ab) - (b) - (a) + (1)\}$$

$$\therefore AB \text{ or } BA = \frac{1}{2}(a-1)(b-1).$$

Remark :-

(1) The main effects and interaction effect of two factors A and B are three orthogonal contrasts of 4 treatment means (1), (a), (b), (ab).

Note that, the main effect of $A = \frac{1}{2}\{(ab) - (b) + (a) - (1)\}$

$$= \sum_{i=1}^{4} c_i t_i, \text{ where } \sum_{i=1}^{4} c_i = \frac{1}{2} + \left(-\frac{1}{2}\right) + \frac{1}{2} + \left(-\frac{1}{2}\right)$$

and t_i's are the treatment means. therefore the main effect of A is a contrast of 4 treatment means. $\therefore \sum_{i=1}^{4} c_i = 0.$

Similarly, the main effect of $B = \frac{1}{2}\{(ab) + (b) - (a) - (1)\}$

$$= \sum_{i=1}^{4} d_i t_i, \text{ with } \sum_{i=1}^{4} d_i = 0, \text{ is}$$

a contrast of 4 treatment means.

Also, note that $\sum_{i=1}^{4} c_i d_i = \left(\frac{1}{2}\right)\left(\frac{1}{2}\right) + \left(-\frac{1}{2}\right)\left(\frac{1}{2}\right) + \left(\frac{1}{2}\right)\left(-\frac{1}{2}\right) + \left(-\frac{1}{2}\right)\left(-\frac{1}{2}\right) = 0.$

Hence, the main effect of A and B are orthogonal contrasts. Similarly, it can be shown that the two main effects and the interaction effect of A and B are three mutually orthogonal contrasts of the 4 treatment means.

(2) Let M be the mean yield of the four treatment combinations. Then

$$M = \frac{1}{4}\{(ab) + (b) + (a) + (1)\}$$
$$= \frac{1}{4}(a+1)(b+1).$$

Note that
$$2M = \frac{1}{2}\{(ab) + (b) + (a) + (1)\}$$
$$A = \frac{1}{2}\{(ab) - (b) + (a) - (1)\}$$
$$B = \frac{1}{2}\{(ab) + (b) - (a) - (1)\}$$
$$AB = \frac{1}{2}\{(ab) - (b) - (a) + (1)\}$$

$$\Rightarrow \begin{pmatrix} 2M \\ A \\ B \\ AB \end{pmatrix} = \begin{pmatrix} \frac{1}{2} & \frac{1}{2} & \frac{1}{2} & \frac{1}{2} \\ \frac{1}{2} & -\frac{1}{2} & \frac{1}{2} & -\frac{1}{2} \\ \frac{1}{2} & \frac{1}{2} & -\frac{1}{2} & -\frac{1}{2} \\ \frac{1}{2} & -\frac{1}{2} & -\frac{1}{2} & \frac{1}{2} \end{pmatrix} \begin{pmatrix} (ab) \\ (b) \\ (a) \\ (1) \end{pmatrix}$$

The matrix of the transformation is orthogonal.
The relationship may be inverted, giving

$$\begin{pmatrix} (ab) \\ (b) \\ (a) \\ (1) \end{pmatrix} = \begin{pmatrix} \frac{1}{2} & \frac{1}{2} & \frac{1}{2} & \frac{1}{2} \\ \frac{1}{2} & -\frac{1}{2} & \frac{1}{2} & -\frac{1}{2} \\ \frac{1}{2} & \frac{1}{2} & -\frac{1}{2} & -\frac{1}{2} \\ \frac{1}{2} & -\frac{1}{2} & -\frac{1}{2} & \frac{1}{2} \end{pmatrix} \begin{pmatrix} 2M \\ A \\ B \\ AB \end{pmatrix}$$

, as $A^{-1} = A^T$ for an orthogonal matrix A.

$$\Rightarrow \begin{cases} (ab) = M + \frac{1}{2}\{A + B + AB\} \\ (b) = M + \frac{1}{2}\{-A + B - AB\} \\ (a) = M + \frac{1}{2}\{A - B - AB\} \\ (1) = M + \frac{1}{2}\{-A - B + AB\} \end{cases}$$

Here,
$$A = \begin{pmatrix} \frac{1}{2} & \frac{1}{2} & \frac{1}{2} & \frac{1}{2} \\ \frac{1}{2} & -\frac{1}{2} & \frac{1}{2} & -\frac{1}{2} \\ \frac{1}{2} & \frac{1}{2} & -\frac{1}{2} & -\frac{1}{2} \\ \frac{1}{2} & -\frac{1}{2} & -\frac{1}{2} & \frac{1}{2} \end{pmatrix}$$
$$\Rightarrow A^{-1} = A^T.$$

(3) Sign Table :—

Factorial Effects	Treatment means (ab)	(b)	(a)	(1)	Divisor
M	+	+	+	+	4
A	+	−	+	−	2
B	+	+	−	−	2
AB	+	−	−	+	2

These are the signs of the elements of the orthogonal mtx A. Clearly, from sign table, the effects are orthogonal.
The sign table may be constructed in the following rule:
" Give a plus sign to each of the treatment means whenever the corresponding factor is at the second level, otherwise give a negative sign, for the main effects. For the interaction, the signs to be attached to various treatment means are obtained by computing the signs of the corresponding main effects ".

SS due to factorial effects and tests of factorial effects :—

It is convenient to obtain the factorial effects and their SS's from the treatment totals rather than from the treatment means. We define the factorial effect totals as follows:

$$[A] = [ab] - [b] + [a] - [1]$$
$$[B] = [ab] + [b] - [a] - [1]$$
$$[AB] = [ab] - [b] - [a] + [1]$$

Then the factorial effects are as follows:

main effect of $A = \frac{1}{2}\{(ab) - (b) + (a) - (1)\}$

$$= \frac{1}{2r}\{[ab] - [b] + [a] - [1]\}$$
$$= \frac{[A]}{2r}$$

main effect of $B = \frac{[B]}{2r}$

main effect of $AB = \frac{[AB]}{2r}$, where r is the common replication number.

[A contrast among k treatment totals T_i from r replicates is $L = \sum_{i=1}^{k} c_i T_i$, with $\sum_{i=1}^{k} c_i = 0$.

Then SS due to the contrast L is an UE of σ_e^2 based on L.

Note that $E(L^2) = \sum_{i=1}^{k} c_i^2 \, Var(T_i)$

$$\Rightarrow E(L^2) = \sum_{i=1}^{k} c_i^2 - r\sigma_e^2$$

$$\Rightarrow E\left(\frac{L^2}{r\sum_{i=1}^{k} c_i^2}\right) = \sigma_e^2$$

Then SS due to the contrast L is $\left\{\frac{L^2}{r\sum_{i=1}^{k} c_i^2}\right\}$, with d.f. 1.]

Note that $[A] = [ab] - [b] + [a] - [1]$ and the SS due to the contrast $[A]$ is $\frac{[A]^2}{r\{(+1)^2 + (-1)^2 + (+1)^2 + (-1)^2\}} = \frac{[A]^2}{4r}$ with d.f. 1.

Therefore,

- SS due to main effect of A $(SS_A) = \frac{[A]^2}{4r}$ with d.f. 1.
- SS " " " " " B $(SS_B) = \frac{[B]^2}{4r}$ with df 1;
- SS " " " " " AB $(SS_{AB}) = \frac{[AB]^2}{4r}$ with df 1.

and the SS due to a factorial effect is $r \times (\text{factorial effect})^2$.

Statistical Analysis in 2^2 - factorial design:—

2^2 - Factorial experiments are conducted either in CRD, RBD or LSD with four treatment combinations 1, a, b and ab. It has been pointed out that main effects of A and B, and the interaction are mutually orthogonal contrasts of treatment means (or totals).

Hence, $\begin{cases} SS(\text{treatment}) = SS_A + SS_B + SS_{AB} \\ \text{d.f.} \quad 4-1 = 1 + 1 + 1 \end{cases}$

Hence, the yields in 2^2 - factorial experiment obtained from CRD, RBD or LSD can be analysed in the usual manner expect that in this case the treatment SS with 3 d.f. is partitioned into three orthogonal components SS_A, SS_B and SS_{AB} each with 1 d.f.

(i) 2^2 - factorial experiment in CRD: —

Four treatment combinations 1, a, b and ab are allotted at random into 4n plots with n replicates for each treatment, so that the design is a CRD.

Let y_{ijk} be the observed yield when factor A is at the i^{th} level and B is at the j^{th} level for the k^{th} replicate, $i=0,1$, $j=0,1$, $k=1(1)n$.

The data may be put in the following way:

Treatment	Replicate 1	2	.....	n
$a_0b_0 \equiv 1$	y_{001}	y_{002}	.....	y_{00n}
$a_1b_0 \equiv a$	y_{101}	y_{102}	.....	y_{10n}
$a_0b_1 \equiv b$	y_{011}	y_{012}	.....	y_{01n}
$a_1b_1 \equiv ab$	y_{111}	y_{112}	.....	y_{11n}

Model: — $y_{ijk} = \mu + \alpha_i + \beta_j + (\alpha\beta)_{ij} + e_{ijk}$, $i,j = 0,1$; $k = 1(1)n$

where, μ is the mean effect,

α_i is the fixed effect of i^{th} level of A, $\sum_{i=0}^{1} \alpha_i = 0$,

β_j is the fixed effect of j^{th} level of B, $\sum_{j=0}^{1} \beta_j = 0$,

$(\alpha\beta)_{ij}$ is the fixed effect of the interaction between α_i and β_j,

$\sum_{i=0}^{1} (\alpha\beta)_{ij} = 0 = \sum_{j=0}^{1} (\alpha\beta)_{ij}$ and $e_{ijk} \overset{iid}{\sim} N(0, \sigma_e^2)$ $\forall\, i,j,k$.

We have $SST = SS(tr) + SSE$, by CRD

$= \{SS_A + SS_B + SS_{AB}\} + SSE$, as an orthogonal partition of SS(total) in CRD

2^2 - Experiment in n completely randomised treatments:-

Source of Variation	d.f.	S.S.	M.S.	F
Main effect A } TREATMENT	1 }	$SS_A = \frac{[A]^2}{4n}$	$MS_A = SS_A$	$\frac{MS_A}{SSE} = F_A$
Main effect B } TREATMENT	1 } = 3	$SS_B = \frac{[B]^2}{4n}$	$MS_B = SS_B$	$\frac{MS_B}{SSE} = F_B$
Interaction AB } TREATMENT	1 }	$S_{AB} = \frac{[AB]^2}{4n}$	$MS_{AB} = SS_{AB}$	$\frac{MS_{AB}}{MSE} = F_{AB}$
Error	$4(n-1)$	SSE (by subtraction)	$MSE = \frac{SSE}{4(n-1)}$	—
Total	$4n-1$	$\sum_i \sum_j \sum_k y_{ijk}^2 - C.F.$		

Here $F_A = \frac{SS_A}{MSE} = \frac{[A]^2}{4n.MSE} \sim F_{1,4(n-1)}$; under $H_A : \alpha_0 = \alpha_1 = 0$

$F_B = \frac{[B]^2}{4n.MSE} \sim F_{1,4(n-1)}$, under $H_B : \beta_{01} = \beta_2 = 0$

$F_{AB} = \frac{[AB]^2}{4n.MSE} \sim F_{1,4(n-1)}$ under $H_{AB} : (\alpha\beta)_{ij} = 0 \; \forall \; (i,j)$

If observed F_A and F_B are greater than $F_{\alpha;1,4(n-1)}$, then the main effects of A and B are significant.

If $F_{AB} > F_{\alpha;1,4(n-1)}$, then the interaction effect is significant.

The above test of significance of the factorial effects may be simplified by computing the estimate of the standard error of a factorial effect total or a factorial effect.

Standard error of any factorial effect total [A], [B], [AB] is $\sqrt{4n\sigma_e^2}$.

Estimate of standard error of factorial effect total $= \sqrt{4n.MSE}$.

Hence, if |a factorial effect total| $> t_{\alpha/2,4(n-1)} \cdot \sqrt{4n.MSE}$ then the corresponding factorial effect is significant at level α.

A single replicate of 2^n factorial experiment: — For even a moderate number of factors, the total number of treatment combinations in a 2^n factorial design is large. Because resources are usually limited, so available resources only allow a single replicate of the design to be run.

A single replicate of a 2^n design is sometimes called an unreplicate factorial. With only one replicate, there is no interval estimate of error or pure error. One approach to the analysis of an unreplicated factorial is to assume that certain high order interactions are negligible and combine their mean squares to estimate the error. When analysing data from unreplicated factorial designs, occasionally high order interactions occur. The use of an error mean square obtained by pooling high-order interactions is inappropriate in these cases.

(ii) 2^2 – factorial design in RBD:—

Sometimes, it is not feasible or practical to completely randomized all of the runs in a factorial. Consider a 2^2– factorial experiment with two factors (A and B) in n replicates. Now, suppose that to run this experiment a particular raw material is required. This raw material is available in batches that are not large enough to allow all 4n treatment combinations to be run from the same batch. However, if a batch contains enough material for 4 observations, then an alternative design to be run each of the n replicates using a seperate batch of raw material. Of course, within a batch the order in which the treatment combinations are run is completely randomised. The presence of a nuisance factor may required that the experiment be run in blocks.

Suppose, the factorial experiment with 4 treatment combinations 1, a, b, ab is conducted in n blocks or replicates. Let y_{ijk} be the yield when factor A is at the level i and B is at the level j in the k^{th} block.

Model:- $y_{ijk} = \mu + \alpha_i + \beta_j + \delta_k + (\alpha\beta)_{ij} + \varepsilon_{ijk}$, where

α_i is the fixed effect of the i^{th} level of A, $\sum_{i=0}^{1} \alpha_i = 0$;

β_j is the fixed effect of the j^{th} level of B, $\sum_{j=0}^{1} \beta_j = 0$;

δ_k is the fixed effect of the k^{th} block, $\sum_{k=1}^{n} \delta_k = 0$;

$(\alpha\beta)_{ij}$ is the fixed effect of the interaction of the i^{th} level of A and j^{th} level of B, $\sum_{j=0}^{1} (\alpha\beta)_{ij} = 0 = \sum_{i=0}^{1} (\alpha\beta)_{ij}$;

and $\varepsilon_{ijk} \overset{iid}{\sim} N(0, \sigma_e^2)$.

We have $SS(\text{total}) = SS(\text{block}) + SS(\text{tr}) + SSE$

$= SS(\text{block}) + \{SS_A + SS_B + SS_{AB}\} + SSE$,

as an orthogonal partition of the SS(total).

df: $4n - 1 = n - 1 + \{1 + 1 + 1\} + 3(n-1)$.

ANOVA for 2^2– factorial design in RBD:–

Source of Variation	SS.	df	MS	F
Block	$SSB = \frac{1}{4}\sum y_{00k}^2 - CF$	$n-1$	$MSB = \frac{SSB}{n-1}$	—
A, B, AB = Treatment	$SS_A = \frac{[A]^2}{4n}$, $SS_B = \frac{[B]^2}{4n}$, $SS_{AB} = \frac{[AB]^2}{4n}$ = SS(tr)	1, 1, 1 = 3	$MS_A = SS_A$, $MS_B = SS_B$, $MS_{AB} = SS_{AB}$	$F_A = \frac{MSA}{MSE}$, $F_B = \frac{MSB}{MSE}$, $F_{AB} = \frac{MSAB}{MSE}$
Error	SSE (by subtraction)	$3(n-1)$	$MSE = \frac{SSE}{3(n-1)}$	—
Total	$\sum_i \sum_j \sum_k y_{ijk}^2 - C.F.$	$4n-1$		

$$C.F. = \frac{1}{4n}\left(\sum_i \sum_j \sum_k y_{ijk}\right)^2$$

To test

(i) H_A : no main effect of A , i.e. $H_A : \alpha_0 = \alpha_1 = 0$

(ii) H_B : no main effect of B , i.e. $H_B : \beta_0 = \beta_1 = 0$

(iii) H_{AB} : absence of interaction , i.e. $H_{AB} : (\alpha\beta)_{ij} = 0 \ \forall \ i, j$.

If the observed F_A, F_B are greater than $F_{\alpha;1,3(r-1)}$, then the main effects of A and B are significant.

If the observed $F_{AB} > F_{\alpha;1,3(r-1)}$, then the interaction effect is significant.

The tests of significance of the factorial effects can be carried out directly using the factorial effect totals without applying F-test in ANOVA table:

Note that $[A] = [ab] - [b] + [a] - [1]$

$$\text{Var}[[A]] = (+1)^2 \cdot r\sigma_e^2 + (-1)^2 \cdot r\sigma_e^2 + (+1)^2 \cdot r\sigma_e^2 + (-1)^2 r\sigma_e^2$$
$$= 4r\sigma_e^2 .$$

Hence, under H_A, $\dfrac{[A]}{\sqrt{4r\sigma_e^2}} = \dfrac{[A]}{\sqrt{4r.MSE}} \sim t_{3(r-1)}$.

Hence if $|\text{factorial effect total}| > t_{\alpha/2, 3(r-1)} \cdot \sqrt{4r.MSE}$,

then the corresponding factorial effect is significant.

(iii) 2^2 factorial design in LSD:- In case of two randomization restrictions, each with 4 levels, the number of treatment combinations in 2^2-factorial design exactly equals the number of restriction levels. Then the factorial design may be run in a 4X4 Latin square.

Consider the experiment given in RBD and the batches of material were considered as blocks. Suppose now that can be made per day. Thus, days become a second randomisation restriction.

Let, y_{ijkl} be the yield when A is at i^{th} level and B is at j^{th} level from the k^{th} row and l^{th} column.

Model:- (fixed effects)

$$y_{ijkl} = \mu + \alpha_i + \beta_j + (\alpha\beta)_{ij} + r_k + c_l + e_{ijkl} \ , \ i,j = 0,1 \ ; \ k,l = 1(1)4 .$$

[Notations have their usual meanings]

$$e_{ijkl} \overset{iid}{\sim} N(0, \sigma_e^2) .$$

Orthogonal Splitting:- $SS(\text{total}) = SSR + SSC + SS(tr) + SSE$

$$= SSR + SSC + \{SS_A + SS_B + SS_{AB}\} + SSE$$

d.f. : $16 - 1 = (4-1) + (4-1) + \{1+1+1\} + 6$

ANOVA table for 2^2 factorial design in LSD :-

Source of Variation	SS	df	MS	F
Row	$SSR = \frac{1}{4}\sum_{K=1}^{4} y_{00K0}^2 - CF$	4-1 = 3		
Column	$SSC = \frac{1}{4}\sum_{L=1}^{4} y_{00L0}^2 - CF$	4-1 = 3		—
A B AB } = treatment	$SS_A = [A]^2/4n$ $SS_B = [B]^2/4n$ $SS_{AB} = [AB]^2/4n$ } = SS(tr)	1 1 1 } = 3	$MS_A = \frac{SS_A}{1}$ $MS_B = SS_B$ $MS_{AB} = SS_{AB}$	$F_A = \frac{MS_A}{MSE}$ $F_B = \frac{MS_B}{MSE}$ $F_{AB} = \frac{MS_{AB}}{MSE}$
Error	SSE (by subtraction)	6	$MSE = \frac{SSE}{6}$	—
Total	$\sum_i \sum_j \sum_k \sum_l y_{ijkl}^2 - C.F.$	16-1 = 15		

$$C.F. := \frac{1}{2\times2\times4}\left(\sum_i \sum_j \sum_k \sum_l y_{ijkl}\right)^2$$

Now, if observed $F_A, F_B, F_{AB} > F_{\alpha;1,6}$, then the corresponding factorial effect has significant effect.

* ——— *

B.

2^3 - factorial Experiment :—

Suppose that three factors A, B and C, each at two levels, are of interest. Then the experiment is called 23-factorial. Following the notations due to Yates, let the small letters a, b and c denote one of the two levels of each of the corresponding factors and will be called the second level. The first level is generally expressed by the absence of the corresponding letter in treatment combinations. The eight treatment combinations are denoted as follows:

$a_0b_0c_0 \equiv 1$
$a_1b_0c_0 \equiv a$
$a_0b_1c_0 \equiv b$
$a_1b_1c_0 \equiv ab$
$a_0b_0c_1 \equiv c$
$a_1b_0c_1 \equiv ac$
$a_0b_1c_1 \equiv bc$
$a_1b_1c_1 \equiv abc$

Main and interaction effect: – The eight treatment combinations can be compared by laying out the experiment in CRD, RBD or LSD and the ANOVA can be carried out accordingly.

Suppose the 2^3-factorial experiment is conducted in r replicates. Let $[\cdot]$, $(\cdot)$ give the total and the mean yields of the treatment combinations based on r replicates.

The simple effects of A is given by the differences in mean yields of A as a result of increasing the factor from level a_0 to a_1, at other levels of B and C:

Levels of B	Levels of C	Simple effect of A
b_0	c_0	$(a_1b_0c_0) - (a_0b_0c_0) = (a) - (1)$
b_1	c_0	$(a_1b_1c_0) - (a_0b_1c_0) = (ab) - (b)$
b_0	c_1	$(a_1b_0c_1) - (a_0b_0c_1) = (ac) - (c)$
b_1	c_1	$(a_1b_1c_1) - (a_0b_1c_1) = (abc) - (bc)$

Main effect of A = $\frac{1}{4}$ (sum of the above four)

Thus $A = \frac{1}{4}[(abc) - (bc) + (ac) - (c) + (ab) - (b) + (a) - (1)]$

$= \frac{1}{4}[\{(abc) + (ac) + (ab) + (a)\} - \{(bc) + (c) + (b) + (1)\}]$

$= \frac{1}{4}(a-1)(b+1)(c+1)$

A measure of interaction AB is the difference between the average effects of A at the two levels of B.

Levels of B	Average effect of A
b_0	$\frac{(ac) - (c) + (a) - (1)}{2}$
b_1	$\frac{(abc) - (bc) + (ab) - (b)}{2}$

By convention, one half of the difference is called the interaction of AB and $AB = \frac{1}{4}[\{(abc)-(bc)+(ab)-(b)\}-\{(ac)-(c)+(a)-(1)\}]$

$= \frac{1}{4}[\{(abc)+(ab)+(c)+(1)\}-\{(bc)+(b)+(ac)+(a)\}]$

$= \frac{1}{4}(a-1)(b-1)(c+1)$.

Similarly, we have

$B = \frac{1}{4}(a+1)(b-1)(c+1)$, $C = \frac{1}{4}(a+1)(b+1)(c-1)$

$BC = \frac{1}{4}(a+1)(b-1)(c-1)$, $AC = \frac{1}{4}(a-1)(b+1)(c-1)$,

a minus sign appearing in any factor on the right if the letter is present in the left.

Again, { interaction AB at level c_0 of $c = \frac{1}{2}\{(ab)-(b)-(a)+(1)\}$

interaction AB at level c_1 of $c = \frac{1}{2}\{(abc)-(bc)-(ac)+(c)\}$

The interaction ABC is defined as the average difference between the interaction AB for the two levels of C.

Thus $ABC = \frac{1}{4}[\{(abc)-(bc)-(ac)+(c)\}-\{(ab)-(b)-(a)+(1)\}]$

Remark:- Let $M = \frac{1}{8}\{(abc)+(bc)+(ca)+(ab)+(b)+(a)+(c)+(1)\}$, the main yield of eight treatment combinations.

Sign Table for 2^3-factorial experiment:-

Factorial Experiment	(1)	(a)	(b)	(ab)	(c)	(ac)	(bc)	(abc)
8M	+1	+1	+1	+1	+1	+1	+1	+1
4A	-1	+1	-1	+1	-1	+1	-1	+1
4B	-1	-1	+1	+1	-1	-1	+1	+1
4AB	+1	-1	-1	+1	+1	-1	-1	+1
4C	-1	-1	-1	-1	+1	+1	+1	+1
4AC	+1	-1	+1	-1	-1	+1	-1	+1
4BC	+1	+1	-1	-1	-1	-1	+1	+1
4ABC	-1	+1	+1	-1	+1	-1	-1	+1

The matrix of numbers (or signs) is constructed such that the number (or sign) for A, B and C are easily written down, being +1 if the corresponding small letter is present and -1 if absent. The numbers (or signs) for AB are the product of the numbers for A and B and so on.

From the above matrix, the sum of elements in each rows except the first, is zero. Hence each factorial effects is a construct of the treatment means (or totals).

Also, note that the all the rows except the first are mutually orthogonal and consequently the 7 factorial effects A, B, AB, etc. are orthogonal constraints among treatment means (or totals).

Factorial effect total of A is

$$[A] = [abc] - [bc] + [ac] - [c] + [ab] - [b] + [a] - [1]$$

Main effect of $A = \frac{[A]}{4n}$

SS due to the contrast $[A]$ is $\frac{[A]^2}{8n}$ with d.f. 1.

$$\Rightarrow SS_A = \frac{[A]^2}{8n}$$

In general, SS due to factorial effect $= \frac{\{\text{Factorial effect total}\}^2}{8n}$, with d.f. 1.

Statistical Analysis in 2^3-factorial design:-

2^3-factorial experiments are conducted either in CRD; RBD or LSD with eight treatment combinations 1, a, b, ab, c, ac, bc, abc. It has been pointed out that the 7 factorial effects are mutually orthogonal contrasts in treatment means or totals.

Hence $SS(tr) = SS_A + SS_B + SS_{AB} + SS_C + SS_{AC} + SS_{BC} + SS_{ABC}$

d.f. : $8-1 = 1 + 1 + 1 + 1 + 1 + 1 + 1$

(i) **2^3-factorial experiment in CRD:-** Let y_{ijkl} be the observed yield when (A, B, C) is at (i, j, k) level for the l^{th} replicate, $i, j, k = 0, 1$, $l = 1(1)n$.

Model:-

$$y_{ijkl} = \mu + \alpha_i + \beta_j + \gamma_k + (\alpha\beta)_{ij} + (\beta\gamma)_{jk} + (\alpha\gamma)_{ik} + (\alpha\beta\gamma)_{ijk} + e_{ijkl}$$

where, $e_{ijkl} \overset{iid}{\sim} N(0, \sigma_e^2)$.

[The notations have their usual meanings].

ANOVA for 2^3-design in CRD:-

Source of variation	d.f.	SS	MS	F
A	1	$SS_A = [A]^2/8n$	$MS_A = SS_A$	$F_A = \frac{MS_A}{MSE}$
B	1	$SS_B = [B]^2/8n$	⋮	⋮
AB	1	SS_{AB} ⋮	⋮	⋮
BC	1	SS_{BC} ⋮	⋮	⋮
AC	1	SS_{AC} ⋮	⋮	⋮
C	1	SS_C ⋮	⋮	⋮
ABC	1	$SS_{ABC} = [ABC]^2/8n$	$MS_{ABC} = SS_{ABC}$	$F_{ABC} = \frac{MS_{ABC}}{MSE}$
Error	$8(n-1)$	SSE (by subtraction)	$MSE = \frac{SSE}{8(n-1)}$	—
Total	$8n-1$	$\sum_i \sum_j \sum_k \sum_l y_{ijkl}^2 - C.F.$		

(=7)

Here $F_A = \frac{SSA}{MSE} \sim F_{1,8(r-1)}$; under H_A : no main effect of A.

(ii) 2^3 - factorial experiment in RBD: — Let $y_{ijk\ell}$ be the observed yield when (A, B, C) is at $(i,j,k)^{th}$ levels in the ℓ^{th} block.

$$y_{ijk\ell} = \mu + \alpha_i + \beta_j + \gamma_k + (\alpha\beta)_{ij} + (\alpha\gamma)_{ik} + (\beta\gamma)_{jk} + (\alpha\beta\gamma)_{ijk} + \theta_\ell + e_{ijk\ell}$$, where θ_ℓ is ℓ^{th} block effect.

ANOVA for 2^3 - experiment in RBD:-

Source of Variation	d.f.	SS	F - ratio
Block	$r-1$	$SS(block) = \frac{1}{8}\sum\sum\sum y_{\cdot\cdot\cdot\ell}^2 - C.F.$	—
Treatment: A	1	$SS_A = [A]^2/8r$	$F_A = \frac{SSA}{MSE} \sim F_{1,7(r-1)}$
B	1	$SS_B = [B]^2/8r$	
AB	1		
C	1		
AC	1		
BC	1		
ABC	1	$SS_{ABC} = [ABC]^2/8r$	$F_{ABC} = \frac{SS_{ABC}}{MSE} \sim F_{1,7(r-1)}$
	=7		
Error	$7(r-1)$	SSE (by subtraction)	—
Total	$8r-1$	$\sum\sum\sum\sum y_{ijk\ell}^2 - C.F.$	

(iii) 2^3 - factorial Expt. in 8X8 L.S.D:-

The model is $y_{ijk\ell m} = \mu + \alpha_i + \beta_j + \gamma_k + (\alpha\beta)_{ij} + (\alpha\gamma)_{ik} + (\beta\gamma)_{jk} + (\alpha\beta\gamma)_{ijk} + \theta_\ell + \phi_m + e_{ijk\ell m}$, $i,j,k = 0,1$, $\ell, m = 1(1)8$,

where, θ_ℓ, ϕ_m are row and column effects.

ANOVA table:-

Source of Variation	d.f.	S.S.
Row	8-1	$\frac{1}{8}\sum_{i=1}^{8} R_i^2 - C.F.$
Column	8-1	$\frac{1}{8}\sum_{m=1}^{8} C_m^2 - C.F.$
Treatment: A	1	$SS_A = [A]^2/8r$
B	1	$SS_B = [B]^2/8r$
AB	1	
C	1	
AC	1	
BC	1	
ABC	1	$SS_{ABC} = [ABC]^2/8r$
	8-1=7	
Error	42	SSE
Total	64-1	SST = Raw S.S - C.F.

Here, $F_A = \frac{SSA}{MSE} \sim F_{1,42,etc.}$

C. 2^n - Factorial Experiment :- The results and notations of 2^2 and 2^3 experiments can be generalised to the case of 2^n - experiment. Here we consider n factors each at 2 levels. Suppose A, B, C, D, ..., K are the factors each at two levels (0,1). Corresponding small letters a, b, c, d, ..., K denote the corresponding factors at the second level, the first level of any factor being signified by the absence of the corresponding small letter. The various factorial effects are enumerated for 2^n - experiment as follows:

Main effects : ${}^nC_1$ in number
Two-factor interactions : ${}^nC_2$ in number
Three-factor interactions : ${}^nC_3$ in number
⋮ ⋮
n - factor interaction : ${}^nC_n$ in number

Hence, the total number of factorial effects in 2^n - experiment are :

$$ {}^nC_1 + {}^nC_2 + \cdots + {}^nC_n = \left[{}^nC_0 + {}^nC_1 + \cdots + {}^nC_n \right] - 1 $$
$$ = (1+1)^n - 1 $$
$$ = 2^n - 1 . $$

Main and interaction Effects :- As in the case of 2^2 and 2^3 - experiments the results for the main effect and interactions can be generalised to the case 2^n - experiment. Thus, for n factors A, B, C, D, ..., K, the main effects and interactions are given by the expression :

$$ \frac{1}{2^{n-1}} \left[(a \pm 1)(b \pm 1)(c \pm 1)(d \pm 1) \cdots (K \pm 1) \right] . $$

the corresponding sign in each factor being taken as negative if the corresponding factor is contained in the factorial effect whose value we want. As usual, the R.H.S. is to be expanded algebrically and then the treatments combinations are to be replaced by the corresponding treatment means. The factorial effect totals can be obtained every conveniently from treatment totals by the generalisation of Yate's method.

Remark :- 1. S.S. due to $(2^n - 1)$ mutually orthogonal factorial effects each with 1 d.f. will add up to treatment S.S.

2. the main effects and the interactions can be obtained in terms of factorial totals as follows :

$$ \text{Factorial effect (Main or Interaction)} = \frac{\text{Factorial effect total}}{n \cdot 2^{n-1}} $$

Analysis of 2^n design:-

It will be seen that all the factorial effects (main and interactions) are mutually orthogonal contrasts of treatment totals. Hence, having obtained the factorial effect totals by Yate's technique, the S.S. due to each factorial effect is given by:

$$\frac{[\ ]^2}{\sum_{i=1}^{2^n} r.1^2} = \frac{[\ ]^2}{r.2^n}, \text{ where } [\ ] \text{ is the factorial effect total.}$$

Table:- ANOVA Table for 2^n Experiment in r Randomised Blocks

Source of Variation	d.f.	S.S.	M.S.S.
Blocks	$r-1$	$S_R^2 = \sum B_j^2/2^n - C.F.$	$s_R^2 = S_R^2/(r-1)$
Treatments	2^n-1	$S_T^2 = \frac{\sum T_i^2}{r} - C.F.$	$s_T^2 = \frac{S_T^2}{2^n-1}$
Main effects			
A	1	$S_A^2 = [A]^2/r.2^n$	$s_A^2 = S_A^2$
B	1	$S_B^2 = [B]^2/r.2^n$	$s_B^2 = S_B^2$
⋮	⋮	⋮	⋮
K	1	$S_K^2 = [K]^2/r.2^n$	$s_K^2 = S_K^2$
Two-factor Interactions			
AB	1	$S_{AB}^2 = [AB]^2/r.2^n$	$s_{AB}^2 = S_{AB}^2$
AC	1	$S_{AC}^2 = [AC]^2/r.2^n$	$s_{AC}^2 = S_{AC}^2$
BC	1	$S_{BC}^2 = [BC]^2/r.2^n$	$s_{BC}^2 = S_{BC}^2$
⋮	⋮	⋮	⋮
Three-factor interactions			
ABC	1	$S_{ABC}^2 = [ABC]^2/r.2^n$	$s_{ABC}^2 = S_{ABC}^2$
ACD	1	$S_{ACD}^2 = [ACD]^2/r.2^n$	$s_{ACD}^2 = S_{ACD}^2$
⋮	⋮	⋮	⋮
n-factor interaction ABCD.....K	1	$S_{AB....K}^2 = [AB....K]^2/r.2^n$	$s_{AB...K}^2 = S_{AB...K}^2$
Error	$(r-1)(2^n-1)$	S_E^2 = By Subtraction	$s_E^2 = \frac{S_E^2}{(r-1)(2^n-1)}$
Total	$r.2^n-1$	Raw S.S - C.F.	

The block effects and the factorial effects (main and interactions) can be tested for significance by comparing their mean S.S. with error S.S.

CONFOUNDING IN FACTORIAL EXPERIMENT

The number of treatment combinations increases rapidly as the number of factors is increased in 2^n - factorial experiments. For 2^5 factorial experiments containing 32 treatment combinations and could require randomised blocks of 32 plots in order to compare them. As the no. of treatments increases it becomes exceedingly difficult to select replicates for a randomised complete block design which are relatively homogeneous.

Because the variation within replicates (blocks) tends to increase as the replicate (or block) size increases, resulting in a larger experimental error variance, it is desirable to keep block sizes small. It order to retain relatively small block sizes for a large number of treatments, only a portion of the treatments may be included in a small block. The resulting blocks are called incomplete blocks.

By a device known as confounding the necessity of including all treatments in each block (or row & column in Latin square) is side-stepped. The whole block or replicate is divided into the desired number of incomplete blocks. Consider a 2^3 factorial experiment, and note that interaction effect $ABC = \frac{1}{4}\left[\{(abc)+(a)+(b)+(c)\} - \{(1)+(ab)+(ac)+(bc)\}\right]$

Suppose that the 8 treatment combinations are arranged in 2 blocks, according to their sign in the ABC interaction:

Block I
1
ab
bc
ac

Block II
a
b
c
abc

The quantity we use to estimate the effect of $A = \frac{1}{4}\{-1+ab-bc+ac+a-b-c+abc\}$ is orthogonal to block totals:

$B_I = \{1+ab+bc+ac\}$ and $B_{II} = \{a+b+c+abc\}$.

Thus, the estimate of A will contain none of the additive block effects, can therefore be estimated and tested as it is done in completely randomised block without any difficulty.

The same is true for the other main effects and 2-factor interactions. Here 3-factor interaction is $\frac{1}{4}\{-(1)-(ab)-(bc)-(ac)+(a)+(b)+(c)+(abc)\}$ and this estimate measures not only the effect of ABC but also the block difference (B_2-B_1). It is not possible to separate the true interaction ABC from the block difference; consequently effect of ABC can not be tested and estimated.

The process by which unimportant treatments are deliberately mixed up or entangled with the incomplete block differences, for the purpose of assessing more important comparisons with greater precision, is called confounding. In other words, confounding is a technique of reducing the block sizes in a replicate at the cost of losing information on some effect which is not of much important.

Distinguish between complete and partial confounding.

[CU '11]

Ans:- If an effect is unimportant, it may be confounded with the incomplete block differences in all replicates. This system of confounding is known as complete confounding. Then the confounding effect is not estimable, we lose complete information on that effect from all the replicates, where the unconfounded effects are orthogonal to the blocks of the replicates and can be estimated and tested as in completely block design.

Sometimes it may be that we are not sure whether the highest order interaction is unimportant or absent and we shall be unwilling to sacrifice the entire information on this. We shall distribute the loss of information among more than one effects and shall get some information on each of them.

If an effect is confounded with incomplete block differences in replicate I, a second effect in replicate II, and a third effect or one of the first two in replicate III, these effects are then partially confounded with incomplete block differences; that is, they are confounded with incomplete block differences in some replicates and unconfounded in others. Some information is available on all comparisons, but some comparisons are more accurately determined, since they are made of all replicates instead of being made in only a portion of the replicates.

Consider a 2^3-factorial experiment with 4 replicates where a different interaction has been confounded in each replicate.

Replicate I		Replicate II		Replicate III		Replicate IV	
ABC Confounded		AB confounded		BC confounded		AC confounded	
1	a	1	b	1	c	1	c
ab	b	ab	a	bc	b	ab	a
ac	c	c	bc	a	ac	b	bc
bc	abc	abc	ac	abc	ab	abc	ab

Fig: Partial confounding in 2^3-design.

If the same set of effects is confounded in all the replicates, confounding is called complete. If, again, different sets of interactions are confounded in different replications, confounding is called partial. In complete confounding, the confounded effects are lost but in partial confounding, the confounded interaction can be recovered from those replications in which they are not confounded.

∴.

Complete confounding in 2^n-factorial experiments:-

If the higher order interactions or even main effects may have little or no meaning and it may be decided to confound these effects with incomplete block differences in all the replicates.

As for example, in 2^3-experiment suppose we decide to use two incomplete blocks of 4 plots each and to confound interaction ABC in all the replicates. Note that $ABC = \frac{1}{4}[\{(abc)+(a)+(b)+(c)\} - \{(1)+(ab)+(bc)+(ac)\}]$. Let us apply the four treatments with '−' signs in ABC to one block and the remaining four with '+' sign to other block.

Replicate	Block I	1	ab	bc	ca
	Block II	a	b	c	abc

Note that the contrast measuring the effect ABC is also measuring the block differences: $B_2 - B_1$; and hence ABC is mixed up or entangled or confounded with block effects and consequently we lose information on ABC. But the other effects A, B, C, AB, BC, AC are not influenced by the differences of blocks and can be estimated and tested without difficulties, we can confound one effect and applying the treatment combinations with '−' sign in that effect to one block and the treatment combinations with '+'ve signs in that effect to the other block. This will assure the confounding of the said effect with block differences and the orthogonality of the other effects to block differences.

This is a method of confounding a 2^n-factorial experiment in 2 blocks per replicate.

Complete confounding in 2^3 experiment in r replicates with two incomplete blocks: —

If ABC is completely confounded in 2^3 factorial experiment with two incomplete block differences in r replicates is given below:

Replicate I

B_1	B_2
1	b
ab	a
bc	c
ca	abc

↓ Key block

Replicate II

B_2	B_1
a	ac
b	bc
c	1
abc	ab

↓ Key block

, etc.

Randomisation:- The set of treatments in the key block and the other block should be randomly allocated to the incomplete blocks with a different random allocation for each complete block or replicate. The treatment combinations within each incomplete blocks are allocated to the plots at random.

[By randomly allocating the sets of treatments to the incomplete blocks, some information is available on the confounded effect.]

The partitioning of the d.f. & in the ANOVA is as follows:

Source of Variation	d.f.	SS
Blocks: Replicates	$2r-1 = r-1$	SS(blocks)
Blocks: ABC	$2r-1 = 1$	SS(blocks)
Blocks: Replicates × ABC	$2r-1 = r-1$	SS(blocks)
Treatments: A, B, AB, C, AC, BC	$G = 1, \ldots, 1$	$SS_A, SS_B, \ldots, SS_{AC}, SS_{BC}$
Errors	$G(r-1)$	
Total	$8r-1$	

For carrying out the statistical analysis, the various factorial effects and their SS are estimated in the usual manner with modification that neither SS due to confounded effect ABC is computed nor it is included in the ANOVA table.

Gain in information in confounding :—

Effects	Information	
	Unconfounded	confounded ABC
A, B, AB, C, AC, BC	$= \frac{2r}{\sigma_8^2}$	$= \frac{2r}{\sigma_4^2}$
ABC		

Let there be r replicates of the 2^3-experiment in each case and that the true error variance with blocks of 8-plots and 4 plots is σ_8^2 and σ_4^2, respectively. Due to small block sizes σ_4^2 will generally be less than σ_8^2, so that we have increased our information on every unconfounded effect in the ratio

$$\frac{\sigma_8^2}{\sigma_4^2} = \frac{2r}{\sigma_4^2} \div \frac{2r}{\sigma_8^2},$$

by using blocks of 4 plots at the expense of obtaining zero information on ABC instead of $\frac{2r}{\sigma_8^2}$ units.

2^n factorial experiment in 2^k blocks in each replicate in complete confounding: —

For a 2^4 factorial, two blocks per replicate are reasonable, in experiment with a larger number of factors, more blocks per replication are required, to keep the block size small.

Let us consider a 2^n factorial experiment conducted in 2^k blocks of equal sizes per replicate. Number of units (plots) in each block $= 2^n/2^k = 2^{n-k}$. This is known as $(2^n, 2^k)$ experiment. We have 2^{n-k} treatment combinations in each complete block and these are assigned at random within the plots of each incomplete block. In each replicate, there are 2^k block totals and it is possible to construct (2^k-1) mutually orthogonal block constrasts. These (2^k-1) block constrasts are actually orthogonal treatment constrasts giving rise (2^k-1) factorial effects; that is, there are (2^k-1) factorial effects which are confounded among the incomplete block difference in each replicate.

Advantages & Disadvantages of Confounding: —

The only and the greatest advantage of confounding scheme lies in the fact that it reduces the experimental error considerably by stratifying the experimental material into homogeneous subgroups. The removal of the variation among incomplete blocks within replicates often results in smaller error mean square as compared with a randomised completete block design, thus making the comparisons among some treatments more precise.

The major disadvantages are the confounded contrasts are replicated fewer times than are the other contrasts and as such there is loss of information on them and they can be estimated with a lower degree of precisions as the number of replications for them is reduced. In the confounding scheme, the increased precision is obtained at the cost of sacrifice of information (partial or complete) on certain relatively non-important interactions. It may be pointed out here that an indiscriminate use of confounding may result in complete or partial loss of information on the contrasts or comparisons of greatest importance. Secondly, a number of problems arise if the treatments interact with blocks.

Example 1:- Construct a 2^5 factorial design, confounding the highest order interaction ABCDE.

Solution:- Since only one interaction effect is to be confounded, we will lay out the 2^5 design in 2 blocks, each containing 16 treatment combinations.

To confound the interaction ABCDE, the principal block will contain, apart from control treatment (1), those treatment combinations which have either an even number of letters or no letters common with the confounded effect ABCDE.

Note that the number of treatment combinations having 2 letters common with ABCDE are ${}^5C_2 = \frac{5\times 4}{2} = 10$ and the treatment combinations having 4 letters common with ABCDE are ${}^5C_4 = 5$. The layout is given by:

Principal Block; (Block 1)	Treatment Combinations.
	(1), ab, ac, ad, ae, bc, bd, be, cd, ce, de, abcd, abce, abde, acde, bcde
Block 2	a, b, c, d, e, abc, abd, abe, acd, ace, ade, bcd, bce, bde, cde, abcde

Example 2:- Construct a 2^4 factorial design in 2 blocks of 8 treatments each confounding the effect ACD.

Solution:- In a 2^4 expt, there are 16 treatment combinations. Since the expt. is laid out in 2 blocks of 8 treatments each, the principal block will contain 8 treatment combinations. To confound the effect ACD, we take in the principal block, those treatment combinations which have an even number of letters or no letter common with the confounded effect ACD.

	Treatment combinations							
Principal Block (Block 1)	(1)	b	ac	ad	cd	abd	abc	bcd
Block 2	a	ab	c	d	acd	bd	bc	abcd

Example 3:- Given the principal block of 2^4 design as {1, ab, cd, abcd} Identify the confounded effects.

Solution:- Number of treatment combinations $= 2^4 = 16$, Block size $= 4$.
Number of block in a replicate $= \frac{16}{4} = 4$.
Since there are $2^2 = 4$ incomplete blocks, $2^2 - 1 = 3$ effects are confounded in a replicate, out of which 2 are independent and the third is their generalised interaction.

On careful examination we find that the treatment combination in the principal block have an even or no letters common with each of the following effects: (i) AB (ii) CD (iii) ABCD.

Ex.(4):- The following is the key block of a 2^4-factorial experiment: $\{1, bd, ac, abcd\}$. Write down the other blocks and identify the confounded effects.

Solution:-

Key block :	1	bd	ac	abcd
Block 2 :	$1 \times a = a$,	$bd \times a = abd$,	$ac \times a = c$,	$abcd \times a = bcd$
Block 3 :	$1 \times b = b$,	$bd \times b = d$,	$ac \times b = abc$,	$abcd \times b = acd$
Block 4 :	ab	abbd	bc	cd

On careful examination, we find that the treatment combination in the principal block have an even number or no letters common with each of the following effects:

(1) AC (2) BD (3) ABCD

Ex.(5):- Divide the 16 treatments of 2^4-factorial experiment into 4 blocks of 4 treatments each, confounding the interaction effects AB and CD completely with blocks. Which other interaction is automatically confounded in this layout?

Solution:- Since, the $16 = 2^4$ treatments are to be divided into blocks of size $4 = 2^2$, in the principal block besides (1), we shall write two independent treatment combinations which have either an even number of letters or no letter common with each of the confounded effects AB and CD (and their generalised interaction ABCD).

2^4-Design in 4 blocks in 4 Blocks, confounding AB and CD:-

Principal Block Block1	Block 2	Block 3	Block 4
(1)	a	c	ac
ab	b	abc	bc
cd	acd	d	ad
abcd	bcd	abd	bd

AH. Ques:- Construct a $(2^4, 2^2)$ factorial design, confounding the interaction effects AB and CD completely with blocks. Which other interaction is automatically confounded in this layout? [CU]