SYMPOSIUM ON DESIGNS FOR ARGICULTURAL EXPERIMENTS AND COMPUTERISATION OF THEIR ANALYSIS

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A symposium on "Designs for Agricultural Experiments and Computerisation of their analysis" was organised during the Silver Jubilee Session of the Society held in New Delhi on 26th March, 1972. In all eight papers were presented. The speakers mainly concentrated on the analytical aspects of designs using computers, under various situations arising in the fields of Agriculture, Animal Sciences and Biological assays. Some discussion on the choice of optimum designs under these situations was also held. Extended summaries of the remarks made by the persons who participated in the symposium are given in what follows :

Analysis of Spoilt Experiments *by M.C. Chakrabarti, Department of Statistics, University of Bombay.*

1. We follow Hoyle (JRSS, A, 1971, 429-439) in including under spoilt experiments, experiments with (*i*) missing yields, (*ii*) mixed-up yields, (*iii*) extra observations. In all these experiments an attempt is made to take advantage of analysis of the original experiment by making suitable changes to meet the new situation.

Essentially the missing plot technique is the realisation of the facts

$$(i) \qquad \min_{x,\,\theta} [(y-A\theta)'(y-A\theta)+(x-B\theta)'(x-B\theta]$$

$$= \min_{x,\,\theta} [(y-A\theta)'(y-A\theta)+(x-B\theta)'(x-B\theta)]$$

$$= \min_{\theta} (y-A\theta)'(y-A\theta)$$

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$$(ii)\ \underset{\theta, x}{\text{Min}}\ [(y-A\theta)'\ (y-A\theta)+(x-B\theta)'(x-B\theta)\ |\ \theta^{(2)}=0]$$

$$=\underset{\theta, x}{\text{Min}}\ [(y-A\theta)'\ (y-A\theta)+(x-B\theta)'(x-B\theta)\ |\ \theta^{(2)}=0]$$

$$=\underset{\theta}{\text{Min}}\ [y-A\theta)'\ (y-A\theta)\ |\ \theta^{(2)}=0]$$

Thus the substitutions α, β in (i) and (ii) in order to get

$\underset{\theta}{\text{Min}}\ (y-A\theta)'\ (y-A\theta)$ and

$\underset{\theta}{\text{Min}}\ [(y-A\theta)'\ (y-A\theta)\ |\ \theta^{(2)}=0]$ are respectively

$$\alpha=B\hat{\theta} \qquad \text{where} \qquad A'y+B'\alpha=(A'A+B'B)\hat{\theta}$$

$$\beta=B_1\hat{\theta}^{(1)} \qquad \text{where} \qquad A_1'y+B_1'\beta=(A'A+B'B)\hat{\theta}^{(1)}$$

Thus the unknown substitute can be equated to their estimates in the completed table, the latter estimates being functions of the unknown values and the equations solved formally give the values of the substitutions. These substitutions give the correct unconditional and conditional error Sum of Squares, the difference between the second and the first gives the correct Sum of Squares for testing $\theta^{(2)}=0$ against the unconditional error Sum of Squares. It the Sum of Squares due to hypothesis is calculated from the first completed table, there is a positive bias and this is discussed in Section 2.

Bartlett recommended the use of covariance technique for analysis of designs with missing yields.

The set-up is :

$$E(y)=A\theta+0.\beta,$$

$$E(0)=B\theta-\beta.$$

It is to be noted that the equations for determining the regression coefficients in the set-up are the same as the equations for determining α, the substitutions which minimise the unconditional error sum of squares. Unlike the other method, the error sum of squares has to be suitably adjusted if we use Bartlett's method.

2. Kshirsagar, A.M. (Amer. Statistician, 1971, 47-50) indicates how the bias can be calculated in particular designs. The general expressions for the bias in any experiment and methods of calculation are given in this section. In the case of block designs, the second substitution is unnecessary. However in the case of more complex

designs, both the substitutions will be necessary. An alternative approach would be to analyse the design with 0 as substitutions for the missing yields and make the appropriate reductions to get the unconditional and conditional error sum of squares.

Theorem 1

In any block design where the ith block has actual total B_i and originally k_i plots of which yields in n_1 ($n_1 < k_i$) are missing, the bias in the Treatment Sum of Squares in the completed table with substitutions $x_{ij}(i=1, 2, \ldots, b; j=1, 2, \ldots, n_i)$ to get the correct Error Sum of Squares, is

$$\sum_{i=1}^{b}\sum_{j=1}^{n_1}(x_{ij}-\bar{x}_i)+\sum_{i=1}^{b}\frac{n_i(k_i-n_i)}{k_i}\left(\bar{x}_i-\frac{B_i}{k_i-n_i}\right)^2$$

Proof. Let y_{ij}'s be the existent yields. Then

Actual Treatment Sum of Squares

$$\sum\sum y^2_{ij}-\sum_{1}^{b}\frac{B_j^2}{k_i-n_i}-\left[\sum\sum y^2_{ij}+\sum\sum x^2_{ij}-\sum\frac{(B_i+n_i\bar{x}_i)^2}{k_1}\right.$$

$$\left.-\text{Treatment SS in the completed table}\right]$$

Bias = Treatment Sum of Squares in the completed table

$$-\text{Actual Treatment Sum of Squares}$$

$$=\sum\sum x^2_{ij}+\sum\frac{B_i^2}{k_i-n_i}-\sum\frac{(B_i+n_i\bar{x}_i)^2}{k_i}$$

$$\sum_{i=1}^{b}\sum_{j=1}^{n_i}(x_{ij}-x_i)^2+\sum_{1}^{b}\frac{n_i(k_i-n_i)}{k_i}\left(\bar{x}_i-\frac{B_i}{k_i-n_i}\right)^2$$

Corollary: If in any block design, there is a single missing yield occurring in ith block having originally k_i plots, then the bias in the treatment sum of squares in the completed table is

$$\frac{k_i-1}{k_i}\left(x-\frac{B_i}{k_i-1}\right)^2$$

where x is the substitution for getting the correct Error Sum of Squares.

A Fortran program for the computation of the bias, written by Dr (Mrs) B.I. Sanghvi is given as an appendix.

Theorem 2

Denoting by $E_1(x, y)$ and $E_2(x, y)$ the unconditional and conditional Error Sum of Squares, x as substitution in the plots with missing yields and y the existent yields,

$$\min_x E_1(x, y) = E_1(\alpha, y),$$

$$\min_x E_2(x, y) = E_2(\beta, y),$$

the bias in the sum of squares due to hypothesis in the completed table is $E_2(\alpha-\beta, 0)$.

Proof:

$$\text{Let } E(y) = A\theta,\ E(x) = B\theta, \theta = \begin{pmatrix} \theta^{(1)} \\ \theta^{(2)} \end{pmatrix}$$

$$A = (A_1, A_2),\ B = (B_1, B_2).$$

Then

$$\begin{aligned} E_1(x, y) &= y'y + x'x - (y'A + x'B)(A'A + B'B)^+(A'y + B'x) \\ &= y'[I - A(A'A + B'B)^+A']y + x'[I - B(A'A \\ &\quad + B'B)^+B']x - 2x'B(A'A + B'B)^+A'y \end{aligned}$$

$$\begin{aligned} E_1(\alpha, y) &= y'[I - A(A'A + B'B)^+A']y - \alpha' \\ &\quad [I - B(A'A + B'B)^+B']\alpha \end{aligned}$$

where

$$[I - B(A'A + B'B)^+B']\alpha = B(A'A + B'B)^+A'y$$

$$\begin{aligned} E_2(x, y) &= y'[I - A_1(A'_1A_1 + B'_1B_1)^+A'_1]y \\ &\quad + x'[I - B_1(A_1'A_1 + B_1'B_1)B_1']x \\ &\quad - 2x'B_1(A_1'A_1 + B_1'B_1)^+A_1'y \end{aligned}$$

$$\begin{aligned} E_2(\beta, y) &= y'[I - A_1(A_1'A + B_1'B_1)^+A_1']y \\ &\quad - \beta'[I - B_1(A_1'A_1 + B_1'B_1)^+B_1']\beta \end{aligned}$$

where

$$[I - B_1(A_1'A_1 + B_1'B_1]\beta = B_1(A_1'A_1 + B_1'B_1)A_1'y$$

Bias

$$\begin{aligned} &= [E_2(\alpha, y) - E_1(\alpha, y)] - [E_2(\beta, y) - E_1(\alpha, y)] \\ &= E_2(\alpha, y) - E_2(\beta, y) \\ &= (\alpha-\beta)'[I - B_1A_1'A_1 + B_1'B_1)^+B_1'](\alpha-\beta) \\ &= E_2(\alpha-\beta, 0) \end{aligned}$$

Corollary 1. $0 \leqslant \text{Bias} \leqslant (\alpha-\beta)'(\alpha-\beta)$.

Corollary 2. Let there be uu' plots arranged in u rows and u' columns and let v treatments be assigned to these uu' plots so that the sth treatment occurs r_s times. If $x_{ij}^{(1)}$ and $x_{ij}^{(2)}$ be the values which minimise E_1 and E_2 respectively and if $z_{ij}=x_{ij}^{(1)}-x_{ij}^{(2)}$ in the plots with missing yields and zero elsewhere then bias in the treatment sum of squares in the completed table

$$= \sum_{i=1}^{u} \sum_{i=1}^{u'} (z_{ij.}-z_{i}-z_{.j}+z_{..})^2$$

Corollary 3. If in the set-up of corollary 2, there is only one plot in which the yield is missing and if α and β be values which minimise E_1 and E_2 respectively, then the bias in the treatment sum of squares in the completed table

$$= (\alpha-\beta)^2 \frac{(u-1)(u'-1)}{uu'}.$$

Illustrative Examples

1. Kendall [Advanced Theory of Statistics, Vol 2, Charles Griffin & Co. Ltd. (1946), p. 230] gives an example of 10 randomised blocks, each having 8 treatments in which 9 plot yields are missing. The relevant portion of the data is recorded below :

Blocks	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*Total*
Treatments											
1			*b*								27·51
2							*f*				24·96
3											...
4								*h*			33·99
5	*a*										28·52
6							*g*	*i*			24·37
7						*d*					25·50
8					*c*	*e*					25·59
Total	20·48	...	19·38	...	25·08	21 92	22·39	19·10	...	...	223·85

By the method of iteration, we get values of a, b, c, d, e, f, g, h and i correct to 2 decimal places as follows :

a	b	c	d	e	f	g	h	i
2·88	2·58	3·73	3·33	3·76	3·32	3·61	3·89	3·22

Substituting these values, we get the biased Treatment Sum of Squares in the Completed Table = 6·5812.

The bias = 0·7423.

2. Latin Square with two missing yields :

$A(3)$	$B(5)$	$C(8)$	Dx_{14}	$16+x_{14}$	A 22
$B(4)$	$C(6)$	$D(6)$	$A(10)$	26	B 19
$C(9)$	$D(6)$	$A(5)$	$B(6)$	26	C 27
Dx_{41}	$A(4)$	$B(4)$	$C(4)$	$12+x_{41}$	D .... $12+x_{14}+x_{41}$
$16+x_{41}$	21	23	$20+x_{14}$	$80+x_{14}+x_{41}$	$80+x_{14}+x_{41}$

We have

$x_{14}^{(1)} = 6$, $x_{41}^{(1)} = 2$

$x_{14}^{(2)} = 6{\cdot}8$, $x_{41}^{(2)} = 2{\cdot}8$

	DF	SS					
Rows	3	24·0	0	0	0	·8	·8
Columns	3	8·5	0	0	0	0	0
Treatments	3	9·5	0	0	0	0	0
Error	4	26·0	·8	0	0	0	·8
Total	13	68·0	·8	0	0	·8	1·6

$$\text{Bias} = 1{\cdot}28 - \frac{1{\cdot}28}{4} + {\cdot}16$$

$$= 0{\cdot}80$$

Actual Treatment Sum of Squres

$$9{\cdot}5 - 0{\cdot}8 = 8{\cdot}7$$

Alternative method

$A(3)$	$B(5)$	$C(8)$	$D(0^*)$	16	A 22
$B(4)$	$C(6)$	$D(6)$	$A(10)$	26	B 19
$C(9)$	$D(6)$	$A(\cdot)$	$B(6)$	26	C 27
D(0*)	$A(4)$	$B(4)$	$C(4)$	12	D 12
16	21	23	20	80	80

Rows	38·0	Rows	38·0
Columns	6·5	Columns	29·5
Treatments	29·5	Error	67·5
Error	38·0	Total	11.2
Total	112		

$$\frac{6}{16}x_1-\frac{2}{16}x_2=2 \qquad \frac{9}{16}x_1'+\frac{1}{16}x'_2=4$$

$$-\frac{2}{16}x_1+\frac{6}{16}x_2=0 \qquad +\frac{1}{16}x_1'+\frac{9}{16}x_2'=2$$

$$x_1=6,\ x_2=2 \qquad x_1'=6{\cdot}8$$

$$x_2'=2{\cdot}8$$

Adjustment $=6{\cdot}2+2{\cdot}0=12$ Adjustment $=6{\cdot}8\times4+2{\cdot}8\times2=32{\cdot}8$

Unconditional Error SS Conditional Error SS

$=38-12=26$ $=67{\cdot}5-32{\cdot}8=34{\cdot}7$

Adjusted Treatment $SS=34{\cdot}7-26=8{\cdot}7$.

Let the yields of q plots be mixed up and only their total u be known.

Then

$$E(y)=A\theta$$

$$E\left(\frac{u}{\sqrt{q}}\right)=\frac{1}{\sqrt{q}}E_{1q}B\theta.$$

Let

$$Z'=(x_1, x_2, \ldots, x_{q-1}, u-x_1 \ldots x_{q-1})$$

$$x'=(x_1, x_2, \ldots, x_{q-1}).$$

Then it is easy to verify that

$$\underset{\theta}{\text{Min}}[(y-A\theta)'(y-A\theta)+\frac{1}{q}[U-E_{1q}B\theta)'(U-\mathrm{E}_{1q}B\theta)]$$

$$=\underset{\theta}{\text{Min}}\,[(y-A\theta)'(y-A\theta)+(Z-B\theta)'(Z-B\theta)]$$

and this happens when

$$Z^{(1)}=\frac{u}{q}E_{q,\,1}+\left(I_q-\frac{1}{q}E_{q,\,q}\right)B\hat{\theta}$$

$$A'y+\frac{u}{q}B'E_{q,1}=\left[A'A+\frac{1}{q}B'E_{q,q}B\right]\hat{\theta}$$

For testing $\theta^{(2)}=0$, we have to obtain the conditional error sum of squares by the substitutions

$$Z^{(2)}=\frac{u}{q}E_{q,1}+\left(I_q-\frac{1}{q}E_{q,\,q}\right)B_1\hat{\theta}^{(1)}$$

where

$$A_1'y+\frac{u}{q}B_1'E_{q,1}=\left[A_1'A_1+\frac{1}{q}B_1'E_{q,q}B_1\right]\hat{\theta}^{(1)}$$

I have indicated elsewhere [Chakrabarti (1963, J. Ind. Stat. Assoc., 50-52)] how Barlett's covariance technique can also be applied in this situation.

4. Under "additional observations" many situations may arise. Let us suppose that we have an arrangement of a BIBD with parameters v, b, r, k and it is decided to have an additional treatment in block, so that block sizes are now $k+1$ and there are $v+1$ treatments. The new C-matrix is

$$\begin{bmatrix}\frac{bk}{k+1}I_1-\frac{r}{k+1}E_{1v}\\ -\frac{r}{k+1}E_{v1},\ \frac{rk+\lambda}{k+1}I-\frac{\lambda}{k+1}E_{vv}\end{bmatrix}$$

$$C^{+}=\begin{bmatrix}\frac{k+1}{bk+r}I_1,\ 0\\ 0,\ \frac{k+1}{rk+\lambda}\left(I_v-\frac{r-\lambda}{r(v+1)}\right)\end{bmatrix}-\frac{r}{(k+1)(v+1)^2}$$

$$E_{v+1,\,v+1}$$

Programme (Fortran) for Bias in Treatment Sum of Squares in Block Designs with Missing Yields

```
      DIMENSION N(30), K(30), B(30), X(30, 12), XBAR (39)
      READ 10, IC
   10 FORMAT (I4)
      READ 20, (N(I), I=1, IC)
   20 FORMAT (20I4)
      READ 20, (K(I), I=1, IC)
      READ 40, (B(I), I=1, IC)
   40 FORMAT (10F 8·4)
      DO 150 I=1, IC
      M=N(I)
      READ 40, (X(I, J), J=1, M)
  150 CONTINUE
      XBAR2=0·0
      SUMX2=0·0
      DO 60 I=1, IC
      XBAR(I)=0·0
      SUMXI=0·0
      NI=N(I)
      DO 70 J=1, NI
      SUMXI=SUMXI+X(I, J)
   70 SUMX2=SUMX2+X(I, J)**2
      ANI=N(I)
      XBAR(I)=SUMXI/ANI
   60 XBAR2=XBAR2+ANI*XBAR(I)**2
      T1=SUMX2—XBAR2
      T2=0·0
      DO 80 I=1, IC
      ANI=N(I)
      AKI=K(I)
   80 T2=T2+(ANI*(AKI—ANI)/AKI)*(XBAR(I)—B(I)/(AKI
                                                —ANI))**2
   90 T=T1+T2
      PRINT 100, T
  100 FORMAT (F15·6)
      END
```

Instructions regarding data cards.

First card : The number of blocks (C) in which observations are missing is punched with the unit's digit in 4th column of the card.

Second set of cards : n_i is punched in such a way that the unit's digit of n_i is in $4i^{th}$ column.

Third set of cards : The values of K_i are punched in the same manner as in the second set.

Fourth set of cards : The value of B_i is punched anywhere in column numbers $[10(i-1)+1]$st column to $10\,i^{th}$ column with a decimal point.

Fifth set of cards : Subset i contains missing observations in the i^{th} block punched in the same way as fourth set of cards.

S.K. Raheja[2], R.K. Khosla[3], P.P. Rao[4], and Mahesh Kumar[5]

"Design and Analysis of Agricultural Field Experiments and Computer Use".

In any programme of agricultural field experiments, planning of experiments, collection and analysis of data and interpretation of results are the main components and the statistician has an important role to play in all these. Planning includes choice of levels of factors under study (for which results of earlier research would serve as useful guide), preparation of lay-out plans with provision for bunds, irrigation and drainage channels, etc., randomization of treatments in experimental plots and setting out procedure of analysis of data. Collection and analysis of data includes drawing up of proforma for recording yield and biometric data, instructions for field work and recording data, supervision of field work, detailed statistical analysis involving partition of treatment sums of squares into meaningful contrasts, adjustment for influence of extraneous factors, etc. Interpretation of results would include preparation of summary tables with standard errors and critical differences of treatment means, ranking of final averages, diagram atic representations of main findings and preparation of reports. A summary report giving brief resume of work with salient results would facilitate better understanding and appreciation of the programme.

The laborious computations involved in the analysis and summarisation of data of a large scale experimental pragramme

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usually results in considerable time lag between the conduct of the experiment and the availability of results for dissemination. The planning of experiment itself might be somewhat affected since sophisticated designs like incomplete block designs involving intricate procedure of analysis would be more cumbersome and time consuming. This probably explains why a majority of experiments conducted in the country till about a decade ago were generally based on simple designs like randomized complete blocks.

Today with the availability of Electronic Computer, not much attention is paid to the computational problems since there can be handled at a fantastic speed by the computer. However, it was not in the distant past that all computational work had to be done manually. In fact, monotony of computations has bothered man for centuries and the only mechanical device available for ages was perhaps the "Chinese Abacus" based on the system of 'fives' and 'twos'. The first usable calculating machine was made by Blaise Pascal in 1642. Fifty years later, G.W. Liebnitz made another machine which could multiply and divide as well. Surprisingly, however, not much progress was made in this direction for almost 200 years. It was around 1890 that Herman Hollerith, a census statistician perfected a system of Data Processing using punched card principle. This invention cut down the time requirement by two thirds. It was around this time also that calculating machine and comptometers were developed by other workers and production of these various machines started on a commercial scale. From then on the progress was fairly rapid and in 1944 an automatic sequence controlled calculator 'Mark-I' was built by the IBM. With the development of electronic industry. vacuum tubes were used in the world's first electronic computer called 'ENIAC" in 1946 which weighed 30 tonnes and occupied 15000 sq. ft. of space. Followed the invention of transistors by the Bell Laboratories in 1948 and the world's first fully transistorized electronic computer was manufactured by the IBM in 1959. The computer industry then developed at a fantastic pace with computation time being reduced to micro-seconds and now nano-seconds. Thus, although the first commercial computer was made less than two decades ago, we are already in the third generation of computers with more than 60 firms in the computer manufacturing business and another 60 making peripheral equipment all over the world. One reason for the initial slow progress in the development of computer was that it was primarily looked upon as an aid for scientific research without much scope of its use in business or other fields. No sooner was the need for an all purpose computer for

scientists and businessmen realized than the computer industry developed at an amazing pace. And the results were nothing short of being spectacular. Today application of computers are known in almost all the fields like medicine, science, agriculture, education and business etc. Within the scientific fields, further use of computer became more intensive since it could be of great help in all problems involving decisions based on rules of logic. Again, in view of tremendous speed at which results could be made available, sequential experimentation became a reality.

In agricultural field experiments, the need of electronic computer was felt not only for ease of computational work but also for obtaining high precision in results of experiments involving costly or rare material. The gain in time was considerable, although a part of this was offset by the time spent on preparation of computer programmes, preparation of card designs, punching of data cards and their verifications, etc. The concept of 'General Purpose Programme' developed during the last few years proved very valuable in this field, since in most of thea gricultural field experiments, standard designs are adopted for which analysis procedures are well-known.

The application of computer technology in the other two components of agricultural field experiments *i.e.* planning of experiments and interpretation of results is of a recent origin. The computer can be of great help in planning of experiments as also in the choice of appropriate levels of factors under study by a quick appraisal of earlier results. The preparation of lay out plans with randomised treatments can be similarly speeded up by using a Random Number Generator and printing out the complete lay-out plans directly from the computer. For proper interpretation of results, apart from mean tables and standard errors, graphs and curves could be drawn by use of 'Plotter' with the computer. It would thus appear that in the design and analysis of agricultural field experiments, the computer has a very important role in all aspects. This assumes great significance in the context of our fast developing agricultural research. For instance, in the Breeding programme of improved varieties, their relative performance needs to be tested in the shortest possible time and the results made available for formulation of recommendations for practical utility. An immediate application of the above concept could be in the field of all India Crop Improvement Projects of the ICAR under which planning of a large number of experiments, preparation of lay-out plans, conducting of experiments and data collection, analysis of data and preparation of reports have to be completed within one crop season.

In fact, it appears that taking into account the magnitude of work involved, a Systems Analysis And Design Study would be highly desirable to examine the needs of information at various levels including the planner, research scientist and farmer who is the ultimate beneficiary of all research in agriculture.

A.C. Kulshreshtha[6] "*Designs for Indirect Assays Based upon Qulantative Responses*"

For agricultural and biological research investigations we have mainly three categories of experimental designs, namely, (*i*) Designs for factorial experiments (*ii*) Designs for varietal trials and (*iii*) Designs for bio-assays. Several designs are available in literature for factorial experiments and varietal trials. These designs were developed largely to meet the needs of agricultural research and, perhaps, this is the reason that these designs and their analysis place much emphasis on tests of significance rather than the problems of estimation. However, for bio-assays, where both the problems of estimation and tests of significance need attention, not much work seems to have been done to evolve optimum designs that may be suitable in different situations.

A biological assay (bio-assay) involves a stimulus applied to a *subject* to get a response which is some measurable characteristic of the subject. The nature of a bio-assay is, in general, comparative and the purpose is to estimate the potency of a *test* (unknown) preparation relative to a *standard* (known) preparation of the stimulus. A certain number of doses from each of the preparations are applied to subjects and the response is measured. The *dose-response relationship* is used to obtain the estimate of relative potency. A design for a bio-assay describes the number and magnitudes of doses of each preparation to be tested, the number of subjects to be used at each dose, the system of allocating subjects to doses, the order in which doses will be administered and the response measured and other usual characteristics of an assay. The two preparations (standard and test) in a bio-assay contain the same effective ingredient and a completely inert diluent. Such assays are called dilution assays and in a dilution assay the relative potency of the test preparation is obtained as the ratio of two equipotent doses taken from each of

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the preparations. The job is simple in case of direct assays, where the doses of the standard and test preparations, which produce a specified response, are directly measured. However, when there is a time-lag between the administration of dose and the appearance of the response, it is not possible to get the two equipotent doses directly. In such situations indirect assay techniques provide a solution.

Based on two distlnct linearizing transformations, the two types of well known indirect assays are: (*i*) Parallel line assays and (*ii*) Slope ratio assays. For these types of assays, the expressions of different dose-contrasts ase known (see Finney, 1964). As a matter of fact, in these assays the doses from the two (or more) preparations can be considered as treatments and the interest lies in estimating with maximum precision, two or three contrasts of major importance. The contrasts of major importance are those which are either used in determining the estimate of relative potency or testing the fundamental validity of the assay. Thus for a parallel line assay the contrasts (*i*) preparation (*ii*) combined regression and (*iii*) parallelism are of major importance as the first two are used for estimating the relative potency and the third one is used for testing the fundamental validity of the assay. Similarly, for slope ratio assays, the contrasts (*i*) slopes, (*ii*) intersection and (*iii*) blank are of majorimportance.

The use of balanced incomplete block (BIB) desings, which provide equal precision for all primary treatment comparisons, may not be appreciated due to the fact that in bio-assays all contrasts are not of equal importance. In fact, none of the available incomplete block designs is optimum as such for bio-assays, but these designs become useful for bio-assays if certain modifications are made. For example, for parallel line assays Das and Kulkarni (1966) have given two series of incomplete block bio-assay (IBB) designs, based on BIB and circular designs respectively, by which the first two contrasts of major importance can be estimated with full accuracy. These IBB designs are quite useful and advantageous and have been greatly appreciatedby bio-assayers. The IBB designs, however, suffer from a drawback that the parallelism contrast, which is used for the fundamental validity test of the assay, is not estimated with full accuracy. It is not known whether, in general, an incomplete block design (proper and equireplicate) is available which can provide full information on all the three contrasts of major importance, but for even number of doses from each of the preparations of a symmetrical parallel line assay, a modified incomplete block bio-assay (MIBB) design is possible which provides full information on each of the contrasts of

major importance. When the number of doses in each of the preparations is odd, then again a MIBB design is possible but with blocks of unequal sizes.

For slope ratio assays, 'blank' and 'intersection' contrasts are used for the fundamental validity tests and the relative potency is obtained from the ratio b_t/b_s, where b_s, b_t are the 'slopes' of the regression lines of the two preparations. If an incomplete or supercomplete (proper and equireplicate) block design is used for a symmetrical slope-ratio assay, the two contrasts of major importance (b_s, b_t) are affected by the block differences. Das and Kulkarni (1966) have proposed a series of IBB designs based on BIB designs for SSR assays, which is quite analogous to the one for SPL assays and is more efficient in determining b_s and b_t as compared to a BIB design. It may be a point of interest to note that the more precise estimation of b_s and b_t separately does not imply more precise estimation of the relative potency. Therefore, the proper criterion of judging the superiority of a design for a SR assay will be the length of the fiducial interval of the estimate of relative potency. An ideal design would be one which will provide full information on the contrasts of 'blank' and 'intersection' and the shortest possible fiducial interval for the estimate of relative potency. The existence of such ideal designs has not been investigated in detail. It is, however, known that MIBB designs for SR assays, which provide full information on the 'intersection' contrast and provide shorter fiducial intervals for the estimate of relative potency than the IBB designs and randomized block designs with equal replication of non-zero doses, are possible for even number of doses from each of the preparations.

In view of the fact that in bio-assays all the contrasts are not of equal importance, one may immediately think of using a confounded design. Contrasts, which are not of major importance may be partially or completely confounded. A general link between the bio-assay and factorial contrasts has been established (*cf.* Kulshrestha, 1971*b*) and therefore we may make use of the conventional confounded factorial designs in bio-assays.

The multiple assays (assays with one standard and several test preparations) are preferred because they permit more economical use of experimental animals. In such assays all the contrasts which are used in the estimation of relative potencies and the contrasts used for the fundamental validity tests of the assays are of major importance. For multiple SPL assays IBB and MIBB designs are possible. The IBB designs for multiple assays are the generalizations of the

designs of Das and Kulkarni (1966). MIBB designs and some related problems have been discussed by Kulshreshtha (1968, 1969*a*, 1969*b*, 1969*c*, 1970*a*, 1971*a*, 1971*b*, 1971*c*). Optimal incomplete block designs for multiple SR assays are yet to be explored.

The problem of design in bio-assays is more complex than in agriculture. Here, no detailed advice can be given, as, the number of possible experimental situations is too great and an optimal design has to be carefully chosen in accordance with the available resources and needs of the projected assay.

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Damaraju Raghavarao[7], "*Transformations in the Analysis of Variance*".

Analysis of variance is a technique through which the total variation in the data can be partitioned into three major components called treatment, environment and error. The variation due to the treatments deliberately introduced by the experimenter is called treatment variation. The variation due to the differences in experimental units is called environmental variations. The unexplainable random variation of the data is attributed to error.

The analysis of variance and the tests of significance are made possible by the basic assumptions of (*i*) additivity of the effects in the assumed model, (*ii*) normal distribution of errors, (*iii*) independence of the distribution of the errors and (*iv*) equality of the error variances, also known as homoscedasticity of the error variances. Scheffe (1959) discusses variations of the assumptions and concludes as follows :

(*A*) Non-normality has little effect on inference about means but serious effects on inferences about variances of random effects whose kurtosis differs from zero .

(*B*) Inequality of variances in the cells of a layout has little effect on inferences about means if the cell numbers are equal, serious effects with unequal cell numbers.

(*C*) The effect of correlation in the observations can be very serious on inferences about means.

Since the correlated observations have serious consequences on the inferences, the statistician resorts to the indispensable tool of randomization which helps him in assuming the independence of errors.

The inequality of error variances is harmful while working with a non-orthogonal classification and it can be remedied through variance stablizing transformations. Curiously enough, the transformations which are primarily used for reducing the inequality of variances, also reduce non-normality and thus safeguard the experimenter from the consequences of the violations of non-normality and unequal variances of errors.

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Let y be the random variable of observations, having Var (y) $=\psi(\theta)$, a function of θ. If we put $z=f(y)$, then it is well known that

$$\text{Var}\,(z)=\text{Var}\,[f(y)]=[f'(\theta)]\psi^2(\theta) \qquad \ldots(2.1)$$

and this will be equal to a constant c^2,
when

$$z=f(y)=\int \frac{c\ dy}{\sqrt{\psi(y)}} \qquad \ldots(2.2)$$

If the observations y follow a Poision distribution then $E(y)=\mu=\text{Var}\,(y)$, and in that case

$$z=f(y)=\int_2^1 \frac{dy}{2\sqrt{y}}=\sqrt{y} \qquad \ldots(2.3)$$

stabilizes the variance at 1/4. However, the variance of $\sqrt{y}+\frac{1}{2}$ will be more stable than that of $\sqrt{y}$ (in this connection see Anscombe (1948), Bartlett (1947), Curtiss (1943), Freeman and Tukey (1950), Mosteller and Yautz (1961) and Rao (1965).)

If the observation y is a binomial proportion based on n Bernoulli trials, then $E(y)=p$, Var $(y)=p(1-p)/n$ and in that case

$$z=f(y)=\int \frac{\sqrt{n}\ dy.}{2\sqrt{n}\ \sqrt{y(1-y)}}=\text{Sin}^{1-}\sqrt{y} \qquad \ldots(2.4)$$

stabilizing the variance for p, the variance of the transformed variable being $1/4n$. However, if the number of trials of each observation are different, then the transformation (2.4) still retains inequality of error variance and a weighted analysis of variance has to be carried out. If y is 0, then it can be replaced by $1/4n$ and if y is 1, then it can be replaced by $1-1/4n$ (In this connection see Ancombe (1948), Bartlett (1947), Freeman and Tukey (1950) and Mosteller and Youtz (1961)).

When the effects in a model have a multiplicative effect, then the logarithmic transformation will help restoring the additivity of the effects as needed by the assumptions of the analysis of variance technique.

While the above three are commonly and widely used transformations in analysis of variance problems. there are other transformations like probit transformations [*cf.* Bliss (1935 *a*, *b*), Finney (1947), Fisher and Yates (1963], logit transformations [*cf.* Berkson (1944), Fisher and Yates (1963)], and Rankit transformations [*cf.* Bartlett (1947), Fisher and Yates (1963].

Two questions that the author is being constantly asked by the Agricultural and Biological workers are the following:

> 'Can we compute the treatment means or effects from the original data? Can we back transform the least significance difference into the original scale and test the significance of the treatment means or effects in the original scale?'

It is to be noted that the analysis of variance test and multiple comparison tests for the data have to be necessarily done in the transformed scale and the treatment effects or means will be obtained by reverting to the original scale, the treatment effects or means obtained in the transformed scale.

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A. Dey,[8] *"Same Designs for Agricultural and Animal Experimentation"*.

The use of Factorial experiments is now well recognised in the field of Agriculture. However, in many situations especially those in India, the designs used for such experiments are intended chiefly to study the 'effects' of different factors and very rarely a study of dose-response relationship is made. In a situation where the factors are quantitative in nature, a comprehensive conclusion may be drawn by making a study of the response dose relationship. Designs which adequately allow the fitting for response-dose relationship are known as response surface designs. Response surface designs are extensively used in industry especially in chemical technology. However, their use seems to be limited in Agriculture probably due to the fact that many response surface designs are not available in blocks of small and equal sizes. Recently attempts have been made to provide methods of construction of some important response surface designs split into blocks of reasonable sizes (Dey and Das, 1970). The blocking of response surface designs poses some problems. While blocking a response surface design, it is desirable that the 'parameters' of the response surface are estimable free of block effects and when a design satisfies this criterion, we say that we have an 'orthogonal' blocking arrangement. In many response surface designs, (especially Rotatable designs) orthogonal blocking arrangements are not economical in the sense that they require too many experimental units. This difficulty has been solved partially in the case of rotatable designs by introducing 'non-orthogonal' blocking arrangements. In 'non-orthogonal' blocking arrangements, not all surface parameters are estimable free of block-effects, but these 'affected' parameters can be estimated after adjusting for the block effects. 'Non-orthogonal' blocking arrangements reduce the size of the experiment to a considerable extent and the loss of information due to 'non-orthogonality' is also not appreciable (Dey and Das, 1970).

Rotatable designs form an important class of response surfaee designs, mainly because (*i*) they are more efficient in estimating the response in regions of interest than the conventional factorials, and (*ii*) their analysis is quite straightforward—a very simple computer programme can be written out for bulk analysis of date collected from such experiments. The only difficulty in applying rotatable designs seems to be the calculation of actual doses which are to be applied, as such calculations often require approximations. A class

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of response surface designs have been evolved recently (Dey, 1970) which have equispaced doses of the factors. These designs, though non-rotatable in nature, are found to be more efficient than the corresponding rotatable designs in measuring the response at certain specific points of interest. Such designs are available with blocks also. The analysis of these designs is fairly simple and proceeds more or less on the same lines as that of the rotatable designs.

Now we describe a design which might find use in experiments with Animals. A large number of experiments are conducted in the country on Animal Nutrition. Most of the designs adopted for such experiments are the usual Randomized block designs and in only a few cases, a switch-over design is adopted. It is well known that experiments with treatments showing residual effects may be planned by adopting a suitable switch-over design. However, in many experiments with animals, there is a reason to believe that the effect of a treatment differs over the different periods of time indicating the presence of treatment X periods interaction. Such experiments, where one suspects the presence of treatment X period interaction, are to be planned in a different manner than the usual switch-over experiments. An experiment on Lambs was conducted at the University of Sydney in 1967 for studying the effect of different nitrogen and sulphur intakes on live-weight gain and wool growth and the presence of treatment X period interaction was suspected. There were 4 treatments A, B, C and D and the design adopted was as given below :

	Animal No.															
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Period 1	A	A	A	A	B	B	B	B	C	C	C	C	D	D	D	D
Period 2	A	B	C	D	A	B	C	D	A	B	C	D	A	B	C	D

A rest period was inserted between the two periods of time in order to take care of the residual effects. Balaam (1965) has discussed such designs for any number of treatments involving only two periods. In his designs, a rest period has to be inserted between the periods, so that the residual effects do not appear in the model for analysis. These designs, for treatments require t^2 animals. Balaam's designs suffer from a drawback that these are only two period designs. Under similar circumstances, designs with higher number of periods may sometimes be desirable. Saha (1970) has given a large number of designs for estimating the treatment X period interaction with

number of periods more than two. The analysis of these designs does not pose special problem and a simple computer programme for the analysis of these designs can be written out. These designs may find use not only in animal nutrition experiments but also in pathological experiments where treatments are applied on different parts of animal body.

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R.K. Khosla[9], M.G. Sardana[10], M.P. Saksena[11] and M.L. Sahani[12], *A Review of Agricultural Experimentation in India.*

Experiment is the main tool of agriculture research. The value of crop varieties, manures, cultivation practices, insecticides and pesticides can be assessed only by testing them in well-planned and scientifically conducted field experiments. Keeping in view the importance of agricultural field experiments, Abraham *et al* (1960) prepared a critical review of agricultural field experiments conducted at various research stations of the country during 1948-59. Singh *et al* (1971) added further to the review, the experiments conducted upto 1964. A critical review of the experiments conducted during 1965-70 reveals that a total of 5762 experiments conducted at various research centres in the country during 1965-70 have so far been collected under the IARS scheme of National Index of Agricultural Field Experiments. Data on purely varietal trials were not collected under the scheme and hence such experiments are not included. For collection of data the country has been divided in 14 operational regions. A region consists of a state except in two cases where the neighbouring small states have been combined to form a region.

It is found that over one-fourth of the experiments were conducted on paddy crop. This was followed by wheat on which 11

per cent of the experiments were conducted. The number of experiments conducted on cotton and oilseed were about 10 per cent each. About 4 to 7 per cent of the experiments were conducted each on jowar, maize, sugarcane and fruits. The remaining about 22 per cent of the experiments were conducted on other crops.

The experiments were classified into 11 types :

(*i*) Manurial (M)
(*ii*) Manurial-cum-varietal (MV)
(*iii*) Cultural (C)
(*iv*) Cultural-cum-varietal (CV)
(*v*) Cultural-cum-manurial (CM)
(*vi*) Cultural-eum-manurial-cum-varietal (CMV)
(*vii*) Irrigational (I)
(*viii*) Irrigational-cum-manurial (IM)
(*ix*) Irrigational-cum-cultural (IC)
(*x*) Irrigational-cum-cultural cum-manurial (ICM) and
(*xi*) Insecticidal and pesticidal (D).

It is seen that the largest number of experiments (43 per cent) were purely manurial. The next important type of experiment was cultural, accounting for about 12 per cent. Of the factorial experiments, about 23 per cent of them involved manuring as one of the factors and about 16 per cent involved cultural treatments as one of the factors. Only about 14 per cent of the experiments were devoted to the study of control of diseases and pests. Experiments dealing with irrigation alone or in combination with the other factor(s) accounted for about 8 per cent of the experiments.

The distribution of the experiments according to the number of factors tried and the design adopted revealed that almost half of the experiments were uni-factor. About 30 per cent of the experiments involved two factors, while experiments involving three factors were about 17 per cent. Experiments involving more than three factors were only 4 per cent.

About two-third of the experiments were laid out using Randomised block design. A little over one-fourth of the experiments were canducted by using Split-plot design. The confounded designs, both symmetrical and asymmetrical were adopted in about 6 per cent of the experiments. The use of Completely randomised, Latin square and Strip-plot design was very limited.

There were 3001 factorial experiments. One of these experiments was laid out using completely randomised design, while in 9 experiments Strip-plot design was adopted. The distribution of the remaining 2991 experiments according to the design adopted is as follows :—

Split plot-1591, Confounded-317, R.B.D.-1083.

Nearly 50 per cent of the experiments involved more than 16 treatments. Of these nearly two-third were laid out using Split-plot design, a little over one-fifth using Confounded design and in the remaining experiments Randomised block design was used.

In about 40 per cent of the experiments, the number of treatments tried were 9 to 16. A little over half of these experiments were laid out using Randomised block design and the remaining, except 3 experiments, were conducted using Split-plot design.

The remaining about 10 per cent of the experiments involved at the most 8 treatments. Nearly 70 percent of such experiments were conducted using Randomised block design, while the remaining were laid out using Split plot design except in two cases where confounded designs were used.

On the basis of the design used, the experiments have been grouped in two classes—(*i*) Randomised block and confounded design and (*ii*) Split-plot design.

It is seen that in over 90 per cent of the cases of experiments belonging to group (*i*), where either Randomised block or Confounded design was used, the degrees of freedom for experimental error were at least 12. In the same group, nearly three-fourth of the experiments involved 4 or more replications.

In group (*ii*) where the experiments were laid out using Split-plot design, nearly two-third of the experiments involved at least 4 replications. About 95 per cent of the experiments provided 12 or more degrees of freedom for sub-plot error, while for the main-plot error the degrees of freedom did not exceed 6 in about 53 per cent of the cases. In only about 27 per cent of the experiments the degrees of freedom for main-plot error were 12 or more and in the remaining about 20 per cent of the cases the degrees of freedom available for main-plot error were between 7 and 11.

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Prem Narain[13], *"Processing and Evaluation of Animal Breeding Experiments on Computer"*.

There are two basic problems in the application of quantitative genetics to animal breeding which require the attention of statisticians. One relates to the analysis of the structure of the animal population in order to measure the relative importance of the different genetic and environmental sources of variation and covariation. The other is the estimation of breeding values for selecting the animals for further propagation. In the former category the animal breeder conducts experiments for estimating additive genetic variance for the various economic characters in which he is interested. These experiments give also the estimates of the covariation between the additive genetic values for the different characters. The estimation of these genetic parameters enable us to predict expected response to selection for individual characters as well as for an aggregate merit. These experiments are also so structured as to provide some information on the importance of non-genetic factors on the chosen characters. For example in cattle breeding the effect of age of cow or herd or the calving season on milk production can be worked out for adjusting the production data. The second category of problems are connected with the identification of genetically superior animals. In a cattle selection scheme, for instance, where a great deal of emphasis is placed on the artificial insemination, the experiments are required to be planned for evaluating the bulls for dairy characters from samples of progeny records. In poultry, on the other hand, the information from several relatives such as full-sibs and half-sibs are required to be combined with the individual performance for deciding about the criteria for selecting the birds.

While the straightforward methods of analysis for the animal breeding experiments carried out in relation to the above mentioned problems are available, the outlook of the statistician in this context is considerably changed in the recent days with the advent of the computer technology. He can now analyse extensive sets of data

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for several characters simultaneously under complex schemes wherein necessary assumptions are less restrictive than what they used to be previously. The use of general matrix arithmetic methods on the computer has opened up enormous possibilities of getting results not only in a shorter time but also on a more extensive scale. The animal breeding experiments conducted to measure genetic and non-genetic sources of variation are usually longterm. In cattle, for instance, a herd is established with limited number of sires and a large number of cows. Each sire is mated to random set of dams and daughters from each mating as well as the dams are scored for each of the several milk characters. The same set of sires are repeatedly used over a period of years before they become unfit for breeding purpose.

In such types of programmes which are both experimental as well as operational, the effects of various factors enter in simultaneously along with the main factor under study. The analysis of vast data collected can therefore be made in several ways depending upon the problem under consideration. For measuring genetic and environmental sources of variation and covariation a general purpose analysis of dispersion can be adopted.

The analysis of dispersion is, however, nothing more than an algebraic device and it has no biological significance other than that which we give it. It is not always useful and sometimes cannot overcome certain difficulties such as confounding of certain factors or non-estimable interactions between them. However, it is of sufficiently general utility and the computation of an analysis of dispersion is a primary task involved in analysing animal breeding data.

Various short cut methods are already available for the computation of analysis of variance in properly designed experiments. However, the nature of the animal breeding experiments is, in some respects, different from the experiments in the other fields. The experiments are long term and give rise to various types of records. For analysing these recorded data somewhat different techniques are often required. Available methods for analysis of single character involve data reduction technique on the computer. However, animal breeders are not interested in a single character but in several characters which determine an economically sound animal. It, is therefore, necessary that the general least squares methods may be extended to include the analysis of several characters simultaneously. This means, therefore, that the estimation of fixed effects and components

of variance and covariance for random effects will depend on the computation of analysis of dispersion.

The problem of computing an analysis of dispersion and conducting and performing associated estimations for the simplest model involving one-way classification does not pose any problem. However, when there are more than one classification, the computation of analysis of dispersion is not very straight forward, though such analysis can be tackled effectively on an electronic computer.

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Dr. M.N. Das, Director, I.A.R.S., participated in the discussion and made the following remarks :

In the recent past, a large amount of work has been done on Design of Experiments, especially on construction and combinatorial problems. However, it is felt that research in Design of Experiments should now be channelised more towards application oriented programmes. For example, with the

advent of high yielding varieties of major crops, it has become necessary to conduct uniformity trials on such varieties to obtain the optimum plot size required for experimentation on them. Practically no work seems to have been done on the determination of optimum plot size in the case of Horticultural crops, especially, tree crops. Experiments with such crops require special techniques. Proper attention should be given for developing suitable methodology for experiments involving tree crops. Data collected from such uniformity trials on tree and other crops can be analysed conveniently and completely with the help of computers.