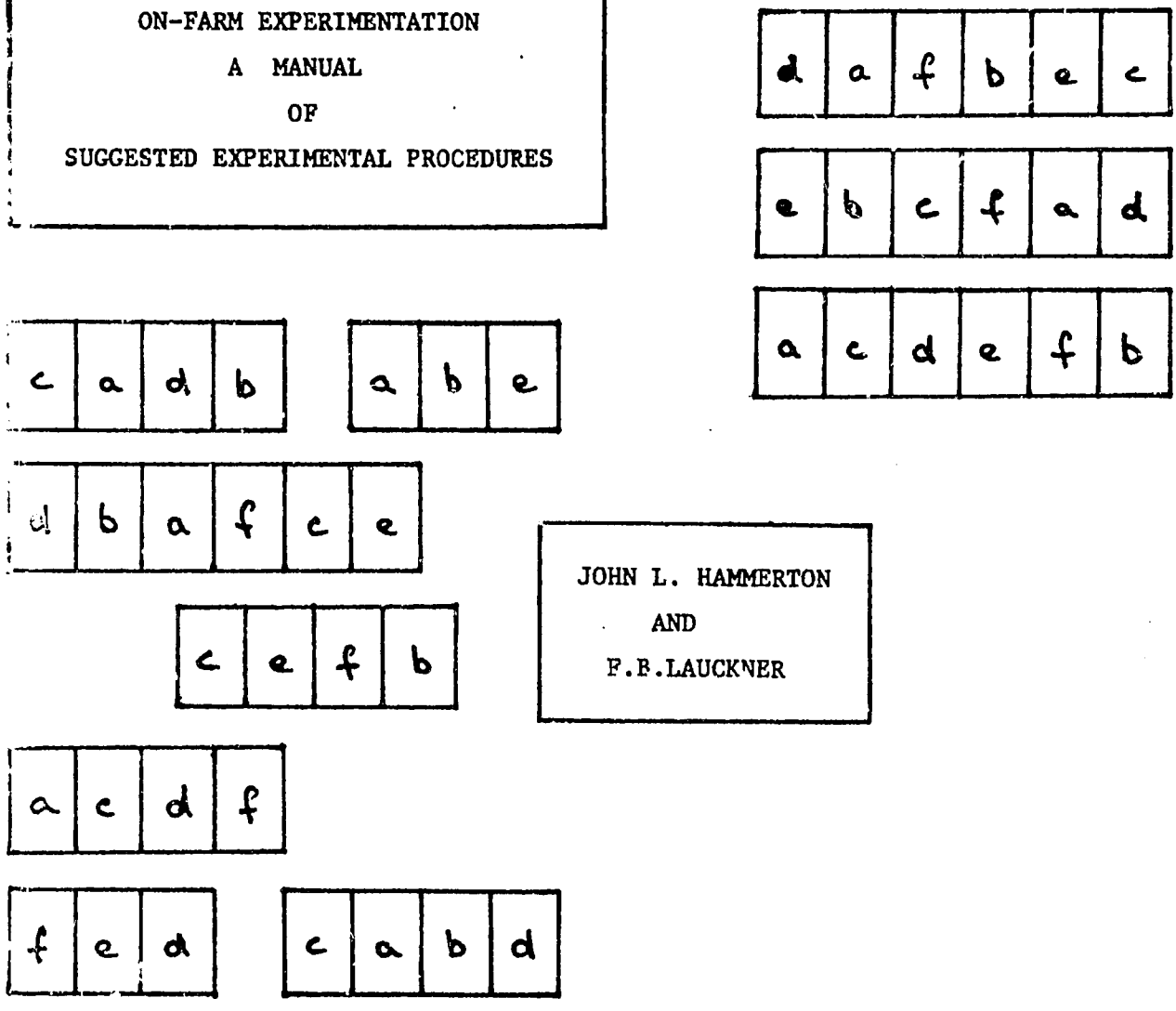
PN-AAF-622 67

CARIBBEAN AGRICULTURAL RESEARCH AND DEVELOPMENT INSTITUTE

ON-FARM EXPERIMENTATION
A MANUAL
OF
SUGGESTED EXPERIMENTAL PROCEDURES



JOHN L. HAMMERTON
AND
F.B.LAUCKNER

An output of
The CARDI/USAID FSR/D Project
#538-0099

PN-AAP-620

CARIBBEAN AGRICULTURAL RESEARCH AND DEVELOPMENT INSTITUTE
(CARDI)

ON-FARM EXPERIMENTATION

A MANUAL
OF SUGGESTED EXPERIMENTAL PROCEDURES

John L. Hammerton
and
F.B.Lauckner

An output of
The CARDI/USAID FSR/D Project
#538-0099

SAINT LUCIA APRIL 1984

PREFACE

The yield gap that exists in crop and animal productivity between Experimental Stations and farmers' fields in the Eastern Caribbean is currently exercising the minds of both agro-biological and socio-economic researchers in CARDI. CARDI's approach to agricultural research in the sub-region is based on its experience in attempting to concentrate its limited human and physical resources on finding solutions to the numerous biological, social and economic constraints that affect small and medium size farm households.

CARDI's Farming Systems Research and Development methodology lays particular emphasis on On-farm Experimentation at several stages, viz:

1) On-Farm Production Systems Analyses.
2) On-Farm Validation with Farmer Control and extensive supervision
3) On-Farm Testing with researcher control and supervision;
4) Applicability testing with Farmer Control and supervision.

The needs of all these various On-Farm Testing Schemes vary because objectives are different, thus necessitating varying design management and analysis considerations.

Since On-Farm experimentation is fundamental to CARDI's Farming System Research and Development methodology it follows that for success On-Farm experiments must be carefully planned and managed. Close collaboration with farmers and extension agents is also vital. However, none of these is sufficient unless the experimental designs are efficient and well conceived.

The nature of most small farms in the Eastern Caribbean - small size, prevalence of steep slopes, complexity of the farming systems - makes On-Farm Experimentation difficult and certainly far more difficult than traditional Field Station Experimentation.

This Manual provides guidelines that should enable both Country Teams and Technical Specialists to design, plan, manage and analyse On-Farm Experiments. The Manual stems basically from the experiences gained in the design, management and analysis of On Farm Experiments in the Eastern Caribbean. The Authors, Dr. John Hammerton, Technical Co-ordinator (Windward Islands) of the USAID funded Farming Systems Research and Development (FSR/D) Project and Mr. Bruce Lauckner, Biometrician at CARDI Headquarters, Trinidad, must be commended for their efforts in producing the Manual. It is hoped that the Manual will be of tremendous benefit to the scientists conducting On-Farm Experiments in the FSR/D Project as well as those others in the region interested in this approach to agricultural research and development.

CALIXTE GEORGE
Project Manager
Farming Systems Research and Development
Project #538-0099.

Page No.

Chapter 1. Introduction 1

Chapter 2. Terminology, and Experimental Designs 4

2.1. Terminology 4
2.2 Randomised Complete Block Designs 5
2.3. Incomplete Block Designs 10
2.4. Factorial Experiments 20
2.5. Confounding 23
2.6. Fractional Replication 27
2.7. Unequal Replication 32
2.8. One or more blocks per farm 37
2.9. Partitioning of the Error S.S. 42
2.10. Comparison of Zones 42
2.11. Summary 45

Chapter 3. Problems 46

3.1. Physical 46
3.2. Biological 46
3.3. Technical 47
3.4. Farmers 47
3.5. Planning and Design 49
3.6. Management 50
3.7. Data Collection 51
3.8. Missing plots (and blocks) 52

IV
(iii)

		Page No.
Chapter 4. Solutions		53
4.1.	Physical	53
4.2	Biological	53
4.3	Technical	54
4.4	Farmers	58
4.5	Planning and Design	59
4.6	Management	64
4.7	Data Collection	64
4.8	Missing plots (and blocks).	68
APPENDIX:	The Analysis of Variance a General Method forDesigns arranged in blocks and accommodating Incomplete Block Designs, Missing Plots and Unequal Replication	70
INDEX 1.	General	78
INDEX 2	Experimental Designs	83
INDEX 3	Analyses of Variance	86
FURTHER READING.		87

CHAPTER 1 INTRODUCTION

On-farm experimentation is basic to Farming Systems Research and Development (FSR/D). In CARDI's FSR/D methodology (Fig.1) on-farm experimentation is carried out at four steps in sequence of activities leading to the transfer of improved technologies These steps are :-

- On-farm Production Systems Analysis (Step 5) which includes exploratory experiments and technology screening. At this step the number of treatments in an experiment may be relatively large. These experiments are very much under the control of the researcher.

- On-farm Testing of Alternatives (Step 9) compares those technologies and components evaluated in steps 4 to 7. Design and synthesis of components is done at step 8. The number of treatments at this step is likely to be small, and to include a farmer practice control treatment. These experiments are also under the researcher's control, but with the farmer's active assistance and participation.

- On-farm Testing with farmer control (step 10) is likely to have no more than two treatments - farmer practice and one alternative selected from those tested at step 9. These tests are supervised by Extension Officers, but the researcher must be responsible for general oversight and for planning, treatment specification and the design of data collection.

- Applicability testing (step 11) may or may not involve experimentation an improved technology may be advocated as a replacement for the farmer's traditional technology, or a farmer involved in steps 9 and 10 may decide to adopt this improved technology.

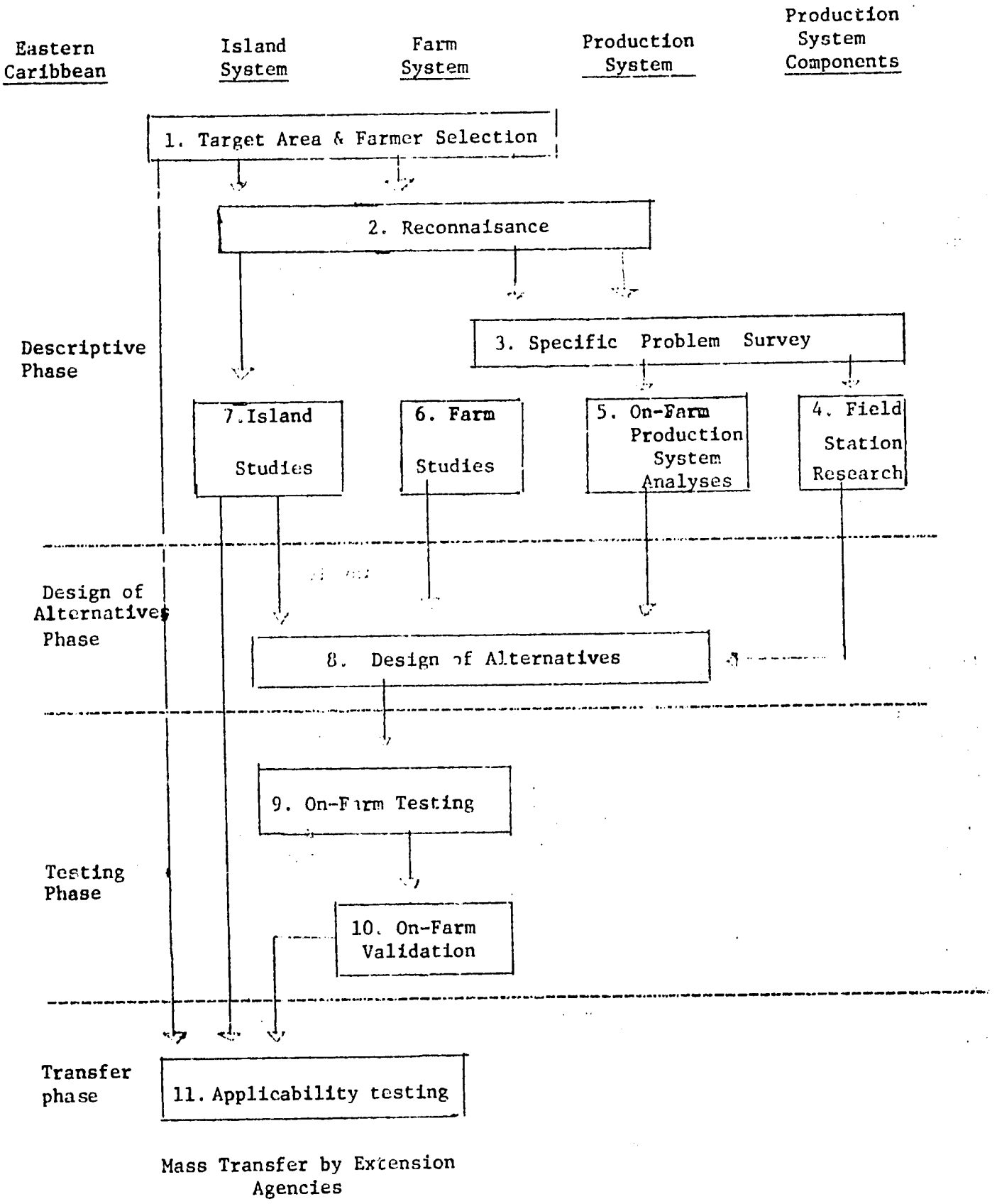
Thus it is likely that the number of treatments to be tested will decrease as work progresses from step 5 to steps 9, 10 and 11. Experimental designs eill accordingly become simpler.

The FSR/D approach to technology generation does not preclude Field Station and laboratory research - as Fig. 1 demonstrates. These types of research are complementary to, and supportive of, on-farm research. Some types of research are unsuited to on-farm experimentation: examples are varietal screening, livestock feeding trials with several rations and/or regimens, and the screening of pesticides to evaluate efficacy, optimal frequency of application and rates and so on.

It should be noted that Field Station research does not necessarily require the physical facility of a field station. Such research can be done on a farmer's land: it is the single site, the total control exercised by the researcher and, usually, the emphasis on a commodity and/or a discipline or a technology component that distinguishes field station - type research from on-farm research.

.../

FIG. 1 CARDI'S FARMING SYSTEMS RESEARCH AND DEVELOPMENT METHODOLOGY



Most researchers are familiar with at least a few experimental designs appropriate to field station research, the techniques for managing and conducting such experiments, and the basics of data analysis. On-farm research uses the same designs, techniques and methods of data analysis, but there are problems peculiar to such research, and designs which are not commonly used in field station research but may be appropriate to on-farm research. These problems and designs form the subject matter of this manual.

This manual is intended to stimulate thought, to encourage imaginative planning of experiements, and to avoid inadequate experimentation arising from ignorance of appropriate designs and the relevant analyses. It does not purport to provide definitive solutions to all the problems discussed, nor does it consider such topics as farm surveys, farm studies or island studies, although these steps are important components of the CARDI FSR/D methodology (Fig.1) .

Section 2 describes the main types of design likely to be useful. It includes some worked examples and discusses the pros and cons of the various designs, and some related topics such as replication and variability.

Section 3 poses some of the problems that can arise in on-farm experimentation without offering solutions. Some possible solutions are suggested in Section 4.

CHAPTER 2. TERMINOLOGY AND EXPERIMENTAL DESIGNS

This section defines the terminology of experimentation, and discusses those experimental designs appropriate to on-farm research.

2.1 Terminology

Experiment refers to the entire set of plots of a single investigation. An experiment comprises a minimum of two treatments which are replicated or repeated. Treatments are allocated to individual plots in some organized random fashion, and plots are arranged in blocks, except in the completely randomised design. This design is not suitable for on-farm experimentation however. Each block may comprise a complete replicate or set of treatments, when it is a complete block. Alternatively, blocks may comprise subsets of the treatments, when they are known as incomplete blocks, so that more than one block is necessary to make up a single replicate. In some designs blocks may contain more plots than there are treatments, with one or more treatments allocated to two or more plots in each block.

On-farm experiments may be sited on a single farm or distributed over several farms. The first arrangement of course is equivalent to a field station experiment, except that the blocks may be sited on different parts of the farm, and problems of the designs and management of this type of experiement will not be further discussed. Where an experiment is spread over several farms, individual farms may accommodate one or more complete or incomplete blocks.

Decisions as to the "best" design for an on-farm experiment must take into account the number and nature of the experimental treatments, the availability of land suitable for experimentation on the participating farms, and the resources - expecially of manpower - available. It is a basic principle that plots within blocks be as uniform as possible - with respect to slope, soil texture, weediness, plant number (except where this is a treatment variable), plant height and girth and so on, so that block size - the number of plots per block - is ofter a major factor determining appropriate designs. Plots within blocks need not be contiguous, and blocks can differ with respect to the above, and other, characteristics.

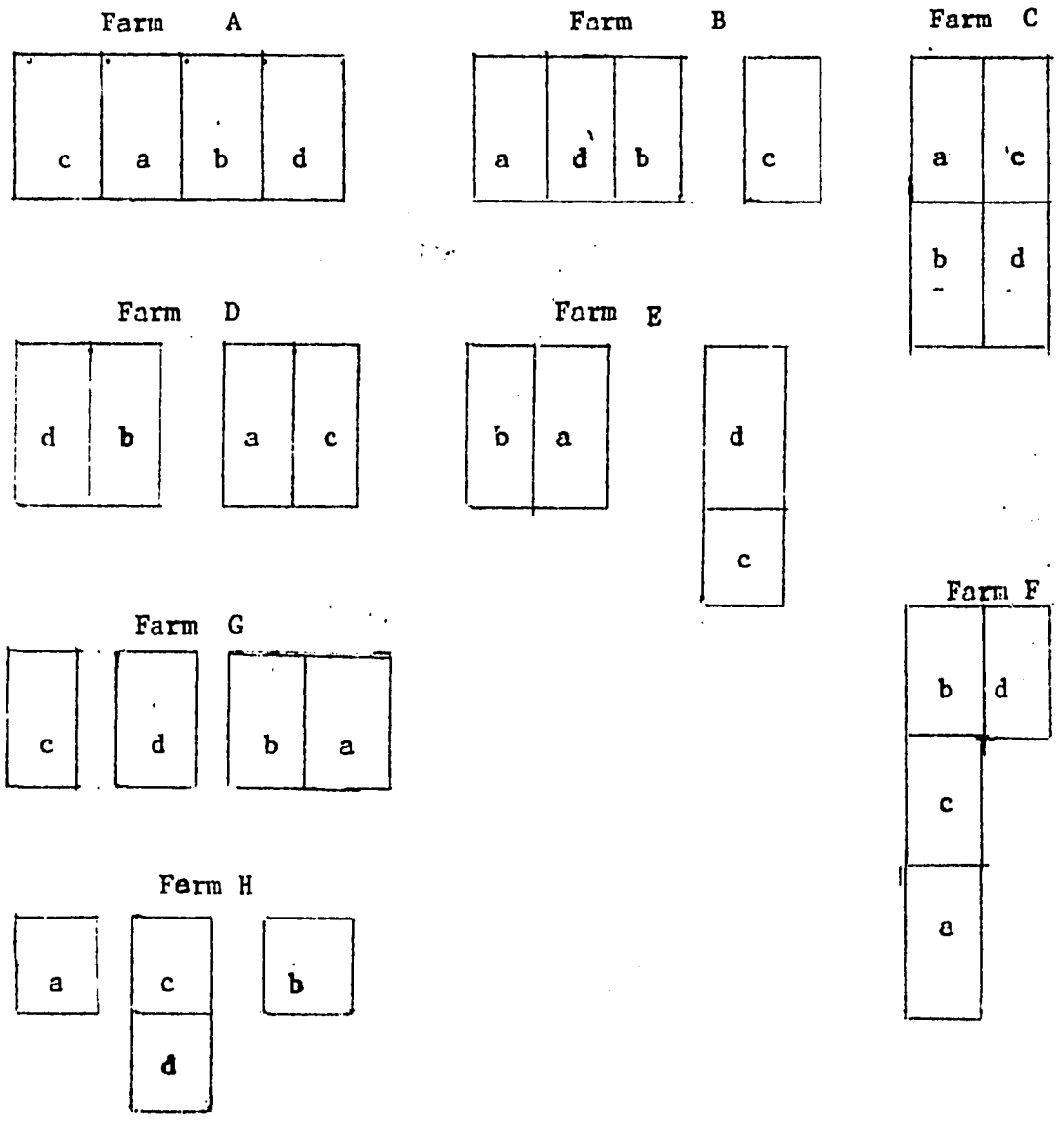
2.2. Randomised Complete Block Designs

The completely randomised design is the simplest. Treatments are replicated but allocated at random to the total array of plots. Such a design is appropriate only where all the plots are uniform. Clearly this condition will not be met where plots are spread over a number of farms. In such a case it is essential to take account of the expected differences between farms.

The simplest design for on-farm experimentation is therefore the randomised complete block design (RCB), with one (complete) block per farm. Fig. 2 gives an example with four treatments (a - d) and eight farms. The analysis of variance is straight-forward, and the apportionment of the degrees of freedom (d f.) is as follows , where b = number of blocks and t = number of treatments. With one complete replicate per block, the number of replicates (r) equals the number of blocks and the number of forms. A significant difference for blocks in the analysis of variance therefore indicates a significant difference between farms.

Blocks	(b - 1)	=	(r - 1)
Treatments	(t - 1)		
Error	(b -1)(t-1)	=	(r -1)(t-1)
Total	(bt -1)	=	(rt-1)

Fig. 2 A randomised complete block (RCB) design with one complete replicate per block. There are four treatments (a-d) and one block per farm on eight farms. Note the variations in block shape and non-contiguity of some plots in some blocks.

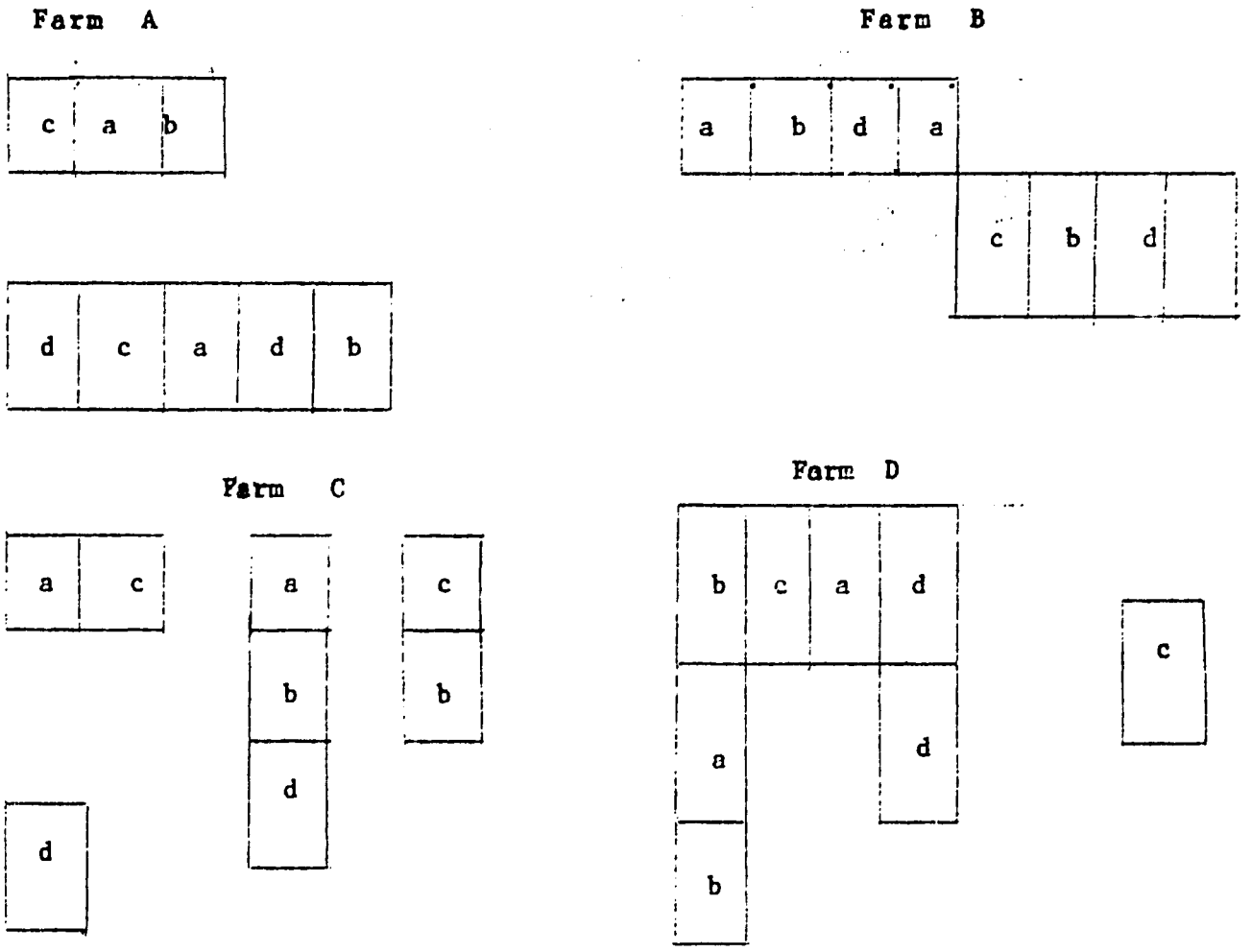


A variation of this design is to have two (or more) complete replicates per block. This design could be used where it is possible to find blocks of uniform plots such that the number of plots is twice, or some multiple of, the number of treatments. Fig. 3 gives an example of such a design, where t = 3, b = 4 and r = 8. The analysis of variance would be as shown above, but the divisors in the calculation of the sums of squares for treatments would be r and not b as would be the case with only one complete replicate per block. This variant is unusual, and unlikely to be important. More likely to be useful and practicable is two complete blocks per farm (see 2.8).

A randomised complete block experiment with only two treatments (i.e. two plots per block) can be analysed by a paired t - test. Equally an on-farm experiment with only two treatments - a comparison of a new technology against farmer practice at step 10 for example - can be designed as a paired t-test comparison with two plots per farm on a large number of farms. Such an analysis is not appropriate if there is more than one replicate (i.e. pair of plots) per farm (see 2.8)

Table 1 gives an example of a paired 't' test for a hypothetical data set. The mean difference , D, is divided by $S_{\bar{D}}$, the standard error of the mean difference, to calculate 't', with n-1 d.f., where n = number of pairs. The data have also been analysed by analysis of variance. Both 't' and F are highly significant in this instance.

Fig. 3. A randomised complete block (RCB) with two complete replicates per block. There are four treatments (a - d) and four blocks, one per farm. Note the variations in block shape and non-contiguity of some plots.



Note: this is distinct from the more usual and more useful design with two complete blocks per farm.

Table 1. A **paired** 't' test from a hypothetical experiment with 11 pairs of plots, (or 11 blocks of two plots). The two treatments are a and b.

Pair	a	b	D = (b - a)
1	7.3	9.8	2.5
2	5.8	7.7	1.9
3	6.1	9.4	3.3
4	8.3	10.2	1.9
5	9.9	14.3	4.4
6	6.8	8.8	2.0
7	8.2	11.3	3.1
8	9.1	12.5	3.4
9	11.0	12.9	1.9
10	8.9	12.0	3.1
11	10.5	15.3	4.8
Total	91.9	124.2	32.3
Mean	8.35	11.29	2.94

't' = test

$$t = \frac{\bar{D}}{S_{\bar{D}}}$$

$$S_{\bar{D}} = S_D / \sqrt{N}$$

$$S_D = \sqrt{\frac{\sum D^2 - (\sum D)^2/n}{n - 1}}$$

n = number of pairs

$$S_D = \sqrt{\frac{105.15 - 94.84}{10}}$$

$$= 1.0154$$

$$S_{\bar{D}} = \frac{1.0154}{\sqrt{11}}$$

$$= 0.3061$$

$$t = \frac{2.94}{0.3061} = 9.60 \quad ****$$

(with n - 1 d.f.)

Analysis of Variance:

	S.S.	d.f.	M.S.	F.
Blocks	80.63	10		
Treatments	47.42	1	47.42	92.08 ****
Error	5.15	10	0.515	
Total	133.20	21		

2.3 Incomplete Block Designs

As the number of treatments increases, block size in a RCB design obviously increases. The variability of terrain, soil depth and other factors on small farms may make it difficult to find blocks of adequate size, without sacrificing the criteria (of uniformity) for blocking. It then becomes necessary to use incomplete blocks, such that each incomplete block contains a subset of the treatments. Allocation to subsets (i.e. to incomplete blocks) follows certain rules, and some designs are available from reference works. Improved methods of analysis have resulted in greater flexibility in the choice of incomplete block designs.

The simplest incomplete block design is the balanced lattice. In this, the number of treatments must be an exact square and the number of plots per block is the corresponding square root. This design is therefore suitable for an experiment with nine treatments, with blocks of three plots. A design is shown in Fig. 4 it has four replicates, with a total of twelve blocks each of three plots. Every pair of treatments occurs once, and only once, in the same block, so that all pairs of treatments are compared with about the same precision, even though differences between blocks may be large. This might well be the case if single blocks were located on twelve different farms. As far as possible, similar blocks - to the extent that this can be ascertained - should be placed in the same replicate. Eight replicates, involving 24 farms, each with one block, could also be used.

Fig. 4. A balanced lattice design for nine treatments (a-i) in 12 blocks ((1) - (12)) of three plots with four simple replications (t = 9, k=3, b=12, r = 4)

(1)	a	b	c	(4)	a	d	g	(7)	a	e	i	(10)	a	h	f
(2)	d	e	f	(5)	b	e	h	(8)	g	b	f	(11)	d	b	i
(3)	g	h	i	(6)	c	f	i		d	h	c	(12)	g	e	c

Where the number of treatments is not a square, balanced incomplete block designs are available. Figs. 5 and 6 give two examples, and Table 2 lists some of the designs available for different number of treatments (t) and block sizes (k = number of plots per block).

There are also partially balanced designs, of which the simplest examples are the lattices. As for the balanced lattices, the number of treatments must be a square. Partially balanced designs are less suitable than balanced designs: the statistical analysis is more complex, and when variation among blocks is large, as must be expected in on-farm research, some pairs of treatments are compared less precisely than others.

Fig. 5 A balanced incomplete block design for five treatments (a-e) in ten blocks ((1) - (10)) of three plots with group replication (t = 5, k = 3, b = 10, r = 6).

Reps I, II & III

(1)	a	b	c
(2)	a	b	e
(3)	a	d	e
(4)	b	c	d
(5)	c	d	e

Reps IV, V & VI

(6)	a	b	d
(7)	a	c	d
(8)	a	c	e
(9)	b	c	e
(10)	b	d	e

Fib. 6 A balanced incomplete block design for seven treatments (a-g) in 21 blocks ((1) - 21)) of two plots with group replication (t = 7, k = 2, b = 21, r = 6)

Reps I & II

(1)	a	b
(2)	b	f
(3)	c	d
(4)	d	g
(5)	a	e
(6)	e	f
(7)	c	g

Reps III & IV

(8)	a	c
(9)	b	d
(10)	c	e
(11)	d	f
(12)	e	g
(13)	a	f
(14)	b	g

Reps V & VI

(15)	a	e
(16)	b	c
(17)	c	f
(18)	d	e
(19)	b	e
(20)	f	g
(21)	a	g

Table 2. Examples of balanced incomplete block designs for different numbers of treatments (t) and numbers of plots per block (k)

Number of Treatments (t)	Number of plots per block (k)	Number of replicates (r)	Number of blocks (b)	Total number of plots[1]	Efficency factor[2] (E)
5	2	4	10	20	0.62
	3	6	10	30	0.83
6	2	5	15	30.	0.60
	3	5	10	30	0.80
	4	10	15	60	0.90
7	2	6	21	42	0.58
	3	3	7	21	0.78
	4	4	7	28	0.88
9	2	8	36	72	0.56
	4	8	18	72	0.84
	5	10	18	90	0.90
	6	8	12	72	0.94
11	2	10	55	110	0.55
	5	5	11	55	0.88
	6	6	11	66	0.92

[1]Total number of plots = (t x r) = (k x b).

[2]The efficiency factor (E) is a lower limit to the efficiency relative to a randomised complete block (RCB) design. This assumes that a RCB design could have been used instead of a balanced incomplete block design. Usually this is not so: the incomplete block design is chosen because a RCB design cannot be used. The "true" efficiency therefore is greater than 1.00.

In Figs. 5 and 6 it will be noted that blocks are arranged in groups of replications. This arrangement does not apply to all incomplete block designs. Where the designs permits simple replication, the analysis of variance and allocation of degrees of freedom is as follows:

Replications	(r - 1)
Treatments (unadjusted)	(t - 1)
Blocks within replications (adjusted)	(b - r)
Intro-block error	(tr - t - b +1)
Total	(tr - 1)

An adjusted treatment sum of squares can be calculated and used for a significance test of treatment effects.

Where designs are arranged in groups of replications, the analysis of variance is as follows (where c = the number of groups, which is two in Fig. 5 and three in Fig. 6).

Groups of replications	(c - 1)
Treatments (unadjusted)	(t - 1)
Blocks within groups (adjusted)	(b - c)
Intra-block error	(tr - t - b +1)
Total	(tr - 1)

Some balanced incomplete block designs cannot be arranged in replications or groups of replications, so that these terms disappear from the analysis of variance. Note that the analysis requires the computation of adjusted sums of squares and means, and is consequently much more complex than for randomised complete block designs.

Recent developments in biometrics, computing and experimental designs, have resulted in greater flexibility of design and analysis, so that there can be much more flexibility in block size and in replication.

Another group of incomplete block designs are those with supplemented balance. In this group one or more control treatments occur in every block with different subsets of the other treatments. Fig. 7 gives examples for different numbers of treatments (t) and different block sizes (k) with either one or two control treatments per block. Such designs are useful where increased replication of a control treatment (or of more than one) is desirable. The occurence of the control treatment in every block also has considerable demonstration value. Table 3 gives further examples for different values of t and k, and of c, the number of control treatments.

The examples of Fig. 7 and Table 3, are based on the full array of possible treatment sets per block. If $t = 7$, $k = 5$ and $c = 2$, for example, sets of three treatments must be chosen from the five non-control treatments for each block, since two plots per block are already assigned to the control treatments (a and b). The number of combinations of three from five, which equals the number of blocks (b) is given by -

$$b = \frac{5 \times 4 \times 3}{3 \times 2 \times 1} = 10.$$

This design is shown in Fig. 7E. For

$$t = 8, \quad k = 4 \text{ and } c = 2, \text{ then}$$

$$b = \frac{6 \times 5}{2 \times 1} = 15$$

Fig. 7 Incomplete block designs with supplemented balance. Five examples are shown for different numbers of treatments (t) and numbers of plots per block (k), with either one or two control treatments (c) per block. Each design is a complete set.

A. $t = 4$, $k = 3$, $c = 1$

(1)	a	b	c
(2)	a	b	d
(3)	a	c	d

Replication: a = 3; b, c d = 2

B. $t = 5$, $k = 3$, $c = 1$

(1)	a	b	c	(4)	a	c	d
(2)	a	b	d	(5)	a	c	e
(3)	a	b	e	(6)	a	d	e

Replication: a = 6; c,d,e = 3

C. $t = 6$ $k = 3$, $c = 1$

(1)	a	b	c	(6)	a	c	e
(2)	a	b	d	(7)	a	c	f
(3)	a	b	e	(8)	a	d	e
(4)	a	b	f	(9)	a	d	f
(5)	a	c	d	(10)	a	e	f

Replication: a=10;
b,c,d,e,f = 4

D. $t = 6$, $k = 4$, $c = 2$

(1)	a	b	c	d
(2)	a	b	c	e
(3)	a	b	c	f
(4)	a	b	d	e
(5)	a	b	d	f
(6)	a	b	e	f

Replication: a, b = 6
c,d,e,f = 3

E. $t = 7, \quad k = 5, \quad c = 2$

(1)	a	b	c	d	e
(2)	a	b	c	d	f
(3)	a	b	c	d	g
(4)	a	b	c	e	f
(5)	a	b	c	e	g
(6)	a	b	c	f	g
(7)	a	b	d	e	f
(8)	a	b	d	e	g
(9)	a	b	d	f	g
(10)	a	b	e	f	g

Replication: a, b = 10 c, d, e, f, g = 6.

Note: Treatments are not randomised within blocks.

Table 3. Examples of incomplete block designs with supplemented balance for different numbers of treatments (t) and numbers of plots per block (k).

Number of Treatments (t)	Number of plots per block (k)	Number of control treatments (c)	Number of blocks[1] (b)	Total number of plots[2]	Replications of control(s)[3]	Replications of others
4*	3	1	3	9	3	2
4	3	2	2	6	2	1
5*	3	1	6	18	6	3
5	4	1	4	16	4	3
5	3	2	3	9	3	1
5	4	2	3	12	3	2
6*	3	1	10	30	10	4
6	4	1	10	40	10	6
6	3	2	4	12	4	1
6*	4	2	6	24	6	3
6	5	1	5	25	5	4
6	5	2	4	20	4	3
7	3	1	15	45	15	5
7	4	1	20	80	20	10
7	5	1	15	75	15	10
7	4	2	10	40	10	4
7*	5	2	10	50	10	6

[1]Number of blocks for the complete array of possible treatment combinations.

[2]Total number of plots = (k x b)

[3]Replication of controls = b, since the control treatment(s) occur in every block. Those designs marked with an asterick are illustrated in Fig. 7.

For some design arrays, replication may be excessive, and a subset may provide adequate replication. The subset should be balanced with equal replication of the non-control treatment. For example, blocks (1), (3) (5), (9) and (10) of Fig. 7C would give a balanced design, with five replicates of treatment (a) and two of (b) - (f). If replication is inadequate, as in the examples in Fig. 7 A and B, double or treble arrays could be used, or, where feasible, one-and-one-half arrays.

Supplemented balanced designs can accommodate double replication of one control treatment in each block. (e.g. a, a, b, c, d; a, a, c, d, e, etc) To be avoided is a disconnected design. (Fig. 8 A). The design of Fig. 8 B is analysable but unsatisfactory in other ways. Note the <u>overlapping</u> of treatments.

Recent developments in biometrics, computing and experimental design have resulted in much greater flexibility of design and analysis so that there can be much more flexibility in block size and in replication. Unequal replication is further discussed in 2.7.

Analysis of many of the incomplete block designs discussed is not simple. Some such analyses can be done on hand calculators, but large experiments with complex designs require computer analysis. The availability of suitable programmes should be considered in selecting a design. A general method of analysis is given in the Appendix.

Fig. 8. Two further incomplete block designs. A is <u>disconnected</u> with no overlap of treatments, and should never be used. B has an <u>overlap</u> of treatments but should be avoided.

A. t=6; k = 3; c = o

(1)	a	b	c
(2)	d	e	f
(3)	a	b	c
(4)	d	e	f

Replication: 2

B. t = 6; k = 4; c = o

(1)	a	b	c	d
(2)	a	b	e	f
(3)	c	d	e	f

Replication: 2

2.4 Factorial Experiments

Factorial designs are well known, particularly for fertilizer experiments. In such designs the effects of two or more factors are investigated simultaneously. The simplest case is a 2 x 2 or 2^2 factorial, comparing two factors each at two levels, one of which might be nil.

This clearly gives four treatment combinations. If there are three factors, each at two levels, there are eight treatment combinations, and this is a 2 x 2 x 2 or 2^3 factorial. There can be any number of factors, but clearly, even with only two levels, the number of treatment combinations becomes large. For instance a 2^6 has 64 treatment combinations. The number of levels can also vary, so that one might have a 3^2, 3^3 or 3^4 factorial, with respectively 9, 27 and 81 treatment combinations. Additionally, the design includes experiments in which the factors vary in the number of levels compared. Thus one can have a 2 x 3, a 2 x 2 x 4 or a 2 x 3 x 4 factorial with respectively 6, 16 and 24 treatment combinations.

Factorial designs estimate both main effects and interaction effects. For instance in a 2^3 factorial the main effects of the three factors A, B and C are estimated, plus the effects of the first-order interactions AB, AC and BC, and of the second-order interaction ABC. Two factors are said to interact when the effect of one is dependent on the presence or absence (or level) of a second. In example (1) below A and B do not interact: the effects of both A and B are independent of the presence of the other. In (2), however, A and B do interact: the average effect of A is nil, but A gives a lower value in the absence of B (i.e. 8 vs. 18), but a much higher value in the presence of B (i.e. 24 vs. 14). Similarly B gives a lower value in the absence of A, but a higher value in its presence.

(1)

	-A	+A	
- B	8	18	13
+ B	14	24	19
	11	21	

(2)

	-A	+A	
- B	18	8	13
+ B	14	24	19
	16	16	

Three (or more) factors can interact. For instance, factor B may only increase yields in the presence of A and C.

The analysis of variance, and apportionment of degrees of freedom (d.f) for a 2^3 factorial is as follows, assuming a RCB arrangement of plots (i.e. complete blocks of 2^3 = 8 plots) and four blocks or replicates.).

Blocks		3
Main Effects	A	1
	B	1
	C	1
Interactions	AB	1
	AC	1
	BC	1
	ABC	1
Error		21
Total		31

It should be noted that, with only two levels of each factor, there is a single degree of freedom (d.f) for each treatment effect. Generalizing to a 2^n factorial (i.e. n factors each at two levels), the analysis becomes, again assuming an RCB arrangement:

Blocks	$(b - 1)$
Main effects	n
Interactions	$(2^n - n - 1)$
Error	$(2^n - 1)(b - 1)$
Total	$(2^n b - 1)$

With more than two levels, the analysis of variance changes. For a 3^3 factorial -- three factors each at three levels with 27 treatment combinations - the analysis is as follows, (again assuming an RCB arrangement):

Blocks		$(b - 1)$
Main Effects	A	2
	B	2
	C	2
Interactions	AB	4
	AC	4
	BC	4
	ABC	8
Error		$(3^3 - 1)(b - 1)$
Total		$3^3b - 1$

A refinement of this analysis is to test for linearity of the main effects. Details of this latter type of analysis, and of more complex factorials of the 2 x 3 x 4 type, will not be given here.

2.5 Confounding

Factorials tend to have large numbers of treatments: the smallest is a 2^2, and if three factors or three levels are to be tested, treatment number rapidly become large. Imagine attempting to find 16, 27 or 32 plots per farm so as to lay down one complete block of respectively, a 2^4, 3^3 or 2^5 factorial! The problem is multiplied if it is wished to replicate the experiment over five or ten farms! One way of reducing block size in factorials is the technique of confounding.

This technique can be illustrated by taking the simple case of a 2^3 factorial. The eight treatment combinations can be split into two sets of four, so that an experiment with three replicates would be laid down in six blocks each of four plots. Figs. 9 and 10 give examples of such designs, and the derivation of the subsets of treatments is shown in Table 4. The latter shows the treatment combinations that contribute to the estimation of main and interaction effects: a priori the effect of A is the total of all treatments containing less the total of those from which a is absent and so on, Interaction effects are derived by "multiplying" the A and B lines, the A and C lines and so on. If the effect of ABC is considered likely to be small - and this is often so - the ABC interaction can be completely confounded with blocks by taking, as subsets of the eight treatment combinations, those with a plus sign (+) and those with a minus sign (-) in the ABC line of Table 2, and allocating these to separate blocks, as in Fig. 9. The analysis of variance

Fig. 9. A 2^3 factorial experiment, arranged in blocks of four plots, with completely confounded with blocks.

Rep. I		Rep. II		Rep. III	
b	ab	abc	(1)	a	bc
a	(1)	a	bc	c	ab
abc	ac	c	ab	abc	ac
c	bc	b	ac	b	(1)

Fig. 10. A 2^3 Factorial experiment, arranged in blocks of four plots, with AC, BC and ABC partially confounded.

Rep. I		Rep. II		Rep. III	
a	ac	(1)	c	abc	ac
c	(1)	ac	ab	a	b
abc	bc	b	bc	(1)	c
b	ab	abc	a	bc	ab
ABC confounded		AC confounded		BC confounded	

Table 4. Main and interaction factorial effects in terms of individual treatment combinations.

Factorial effects	Treatment combination							
	(1)*	a	b	c	ab	ac	bc	abc
A	-	+	-	-	+	+	-	+
B	-	-	+	-	+	-	+	+
C				+	-	+	+	+
AB	+	-	-	+	+	-	-	+
AC	+	-	+	-	-	+	-	+
BC	+	+	-	-	-	-	+	+
ABC	-	+	+	+	-	-	-	+

*(1) conventionally represents the combination of a, b, and c all at the lowest, nil or absence level.

Note that in analysis of variance, the sum of squares for the main effect of A can be computed directly, from the totals, as

$$S.S.A = \frac{(a + ab + ac + abc - (1) - b - c - bc)^2}{2^n r}$$

Similarly for B, C, and the interaction effects. The sum of squares for the effect of ABC, for example, would be given by

$$S.S.\ ABC = \frac{(a + b + C + abc - (1) - ab - ac - bc)^2}{2^n r}$$

now becomes:

Blocks		5
Main Effects	A	1
	B	1
	C	1
Interactions	AB	1
	AC	1
	BC	1
Error		12
Total		23

Note that no test of the effect of the ABC interaction effect can be made. The S.S. for ABC has been pooled with the blocks S.S. Loss of information on ABC is the price paid for the convenience of having smaller blocks.

Fig. 10 illustrates partial confounding: one replicate has ABC confounded, another AC and the third BC. The make-up of subsets derives, as explained above, from Table 4. The analysis of variance now allows a test of all interactions, but the tests for AC, BC and ABC are each based on <u>two</u> replicates only. (For ABC on replicates II and III, for AC on I and III add for BC on I and II).

Blocks		5	
Main Effects	A	1	
	B	1	
	C	1	
Interaction effects	AB	1	
	AC	1'	Based on only two replicates.
	BC	1'	
	ABC	1'	
Error		11	
Total		23	

Additional replication could be included in the experiment, with the degrees of freedom for blocks, error and total increasing from those shown in the example above, and with greater precision accorded to the tests of the partially confounded interaction effects.

A 2^4 factorial, which has 16 treatment combinations, can be grouped into balanced sets of eight or four. In the latter instance four blocks of four plots are needed for one complete replicate. Several interactions must be confounded, but if the experiment is large enough - with sufficient blocks - partial confounding can be used so that the effects of all interactions can be estimated. For example, a 2^4 factorial with six replicates (24 blocks each of four plots) can be partially confounded with first-order interactions (AB, AC, AD, BC, BD, CD), confounded in one replicate each, and second-order interactions (ABC, ABD, ACD, BCD) each confounded in three replicates.

As the size of the (confounded) factorial increases, so the need for replication decreases. A 2^6 factorial, which has 64 treatment combinations, can be laid down in eight blocks of eight plots, with no replication. Several of the higher order interactions would be confounded with blocks. The analysis of variance would be:

Blocks	7
Main Effects	6
First-order Interactions	15
Second-order interactions	20
Third-order interactions	15
Total	63

The d.f. and S.S. for the second and third order interactions would be pooled and used as the "error" to test the significance of the main effects and first-order interactions. Such an experiment could be laid out over eight farms but plot selection for blocking would require special care.

Although attention has been focused on 2^n designs, confounding can also be used for 3^n factorials, using blocks of nine plots, and for 2 x 3 x 4 type designs, using blocks of 12 plots and so on.

2.6 Fractional Replication

In a single replication of a 2^6 factorial, each main effect is averaged over 32 treatment combinations, and in a 2^5 factorial over 16 treatment combinations. Such precision of estimation may be totally unnecessary. Lest it be thought unlikely that a large factorial could be useful and relevant in on-farm research, consider the following two hypothetical experiments - both 2^5 factorials - in which the five factors listed are compared each at two levels (presence or absence):

Minimum tillage experiment	Micro-nutrient foliar spray experiment
cultivation	Cu
Labicide	Fe
Mulch	Mn
Soil insecticide	Mo
Fertilizer	Zn

It would be difficult - if not impossible - to lay down such factorial experiments on-farm as complete blocks (of 32 plots). Blocks of eight might be feasible for a confounded design, but the question remains: is it necessary to average main effects over 16 treatments and plots? One answer is fractional replication.

Fig. 11. A half-replicate of a 2^5 factorial in four blocks of four plots. ABCDE is the defining contrast and the first order interactions CD, CE and DE are confounded with blocks.

(1)	(2)	(3)	(4)
(1)	ac	ae	ad
ab	bc	be	bd
acde	de	cd	ce
bcde	abde	abcd	abce

Note that treatments are not randomised within blocks.

Fractional replication enables five factors - or four or six - to be tested in an experiment of practical size. Only a subset of the full factorial array of treatment combinations is used, with no replication of these combinations. An example of a half - replicate of a 2^5 factorial, in blocks of four plots, is shown in Fig. 11. Some first order interactions are confounded with blocks, and the analysis of variance is as follows:

Blocks	3
Main Effects	5
First-order interactions	7
Total	15

The interaction line is used as the error line for tests of significance of main effects.

To examine the derivation of the subset of treatment combinations used, it is convenient to consider a 2^3 factorial. Table 4 shows the 8 treatment combinations. Using the ABC interaction as the contrast to split the factorial into two halves, a, b, c and abc would form one subset and (1), ab, ac and bc the other. ABC is know as the defining contrast. In larger factorials, where a quarter or even one-eighth replicate is taken, there will be several defining contrasts.

Experiments with fractional replication are open to interpretation, or misinterpretation, in a way that does not occur with replicated designs. Table 5 shows the individual treatment combinations that contribute to the estimation of the main and interaction effects for the two subsets of a 2^3 factorial. Looking at subset 1, it is clear that the quantity used to estimate the effect of AB (i.e. (abc) + (c) - (a) - (b)) is the same as that used to estimate the effect of C. C and AB are know as aliases, and are written as C = AB. The Table further shows that A = BC and B = AC. Note that ABC cannot be estimated at all. The effect of using fractional replication then is to lose the effect of ABC entirely and to confuse main effects each with a first-order interaction. If a different defining contrast had been used, a different set of aliases would be found.

Aliases pose problems of interpretation: is the apparent effect of A due wholly to A, or to the BC interaction, or to a mixture of the two? If the researcher knows that the interaction of B and C is negligible, then the observed effect of A can be attributed to A. But if there is a positive interaction of B and C, then the effect of A will be over-estimated.

Subset 2 shows another sort of alias: the effect of A is equal to the BC interaction, with the signs changed. That is:

A = (ab) + (ac) + (bc) - (1), and

BC = -(ab) - (ac) + (bc) + (1), and the alias is now written as A = - BC.

If B and C positively interact, this will tend to make the effect of A apparently smaller than its true effect.

Table 5. The two subsets of treatments combination for a half replicate of a 2^3 factorial, with ABC as the defining contrast, and the main and interaction effects in terms of the treatment combinations.

Factorial Effect	SUBSET 1				SUBSET 2			
	a	b	c	abc	(1)	ab	ac	bc
A	+	-	-	+	-	+	+	-
B	-	+	-	+	-	+	-	+
C	-	-	+	+	-	-	+	+
AB	-	-	+	+	+	+	-	-
AC	-	+	-	+	+	-	+	-
BC	+	-	-	+	+	-	-	+
ABC	+	+	+	+	-	-	-	-

The choice of a fractional factorial design clearly requires some prior information on expected main effects and interaction effects. Since the sum of squares for the interaction effects is used as the error sum of squares it is important to be fairly sure that interactions are small or absent.

In a half-replicate of a 2^5 factorial, with defining contrast ABCDE, each main effect has a third order interaction as alias, and each first-order interaction has a second-order interaction as alias. The analysis of variance of a half-re replicate of a 2^5 factorial with four blocks of four plots (Fig. 9) is (as before)

Blocks	3
Main Effects	5
First order interactions	7
Total	15

Note that seven of the ten first-order interactions can be estimated: three are confounded with blocks.

If now two blocks of eight plots were used, confounding one first-order interaction, the analysis becomes:

Blocks	1
Main Effects	5
First Order interactions	9
Total	15

Finally, if there was one block of 16 plots, all first-order interactions could be estimated with a total of 10 degrees of freedom, and there would be no block effect.

Fractional replication assumes a single part-replication. However, it may be useful and sensible to select a subset of factorial treatment combinations and to replicate these on several forms. For example, if A, B and C are, respectively, pruning, fungicide application and fertilizer application to cocoa, it might be useful and economical of effort to test the subset a, b, c and abc only (excluding (1), ab, ac and bc). This makes sense if the primary objective of the experiment is to see if - and to demonstrate that - the abc combination is more effective than a, b or c alone. With b blocks (or farms) of four plots, the analysis of variance would be:

Blocks	(b - 1)
Treatments	3
Error	(3 (b -1))
Total	(4b - 1)

The 3 d.f. could be divided into single d.f. for main effects, but because of the aliases (A =BC, B = AC and C= AB) it would be preferable to test for significance among the four treatments (with 3 d.f.). Care would be necessary in interpretation of the results.

A subset of eight treatment combinations from a 2^4 factorial could also be tested on a number of farms, in blocks of four plots, with one block per farm. The two sets of four treatment combinations might be b, c, ad and abcd and a, bd, cd and abc. There would be several aliases, so that a prior knowledge of the more important interactions would be necessary in selecting the subset, in order to avoid problems of interpretation.

2.7 Unequal Replication

All the designs discussed above have had equal replication of all treatments there are circumstances however where it may be useful to adopt unequal replication. It may be desirable to increase replication of the 'nil control treatment in a crop protection experiment for example, so as to obtain a more accurate base against which to measure the performance of the treatments. Or it may be desirable to examine on a limited number of farms, the effect of 'probe' treatments - higher levels of fertilizer for instance - in order to obtain an indication of the response curve

In the former case it is usual to include two 'nil' control plots in each block (and there is no reason why there should not be three or more control plots except that this unduly increases block size) This situation with one treatment uniformly replicated more than once in each block is easy to analyse. An example will help assume a RCB experiment with four treatments (a d) and one block per farm, on 17 farms. Without extra replication of any treatment, the apportionment of d.f. is straightforward

Blocks	16
Treatments	3
Error	48
Total	67

With double replication of the control treatment (say, a), blocks now comprise 5 plots (with treatments a, a, b, c, d,), so that the experiment comprises 85 plots (17 x 5). Thirty-four of these would receive the control treatment d. The d.f. are now:

Blocks	16
Treatments	3
Error	65
Total	84

Note that the error d.f. are increased by 17. The treatment sum of squares (S.S.T) can be divided into two components : 'control versus the others (i.e. 'A. vs. C and D) with 1 d.f., and 'within others' (i.e. A vs B, C and D) with 2 d.f.

Computation of the treatment S.S. differs. Without the double replication, the formula is:

$$S.S._T = \frac{T_a^2 + T_b^2 + T_c^2 + T_d^2}{17} - \frac{G^2}{68}$$

where T_a, T_b, T_c and T_d are respectively the totals for treatments a to d, and G is the grand total. With double replication of treatment a the computation of treatment S.S. is:

$$S.S._T = \frac{T_b^2 + T_c^2 + T_d^2}{17} + \frac{T_a^2}{34} - \frac{G^2}{85}$$

The block S.S., total S.S. and error S.S. are derived in the usual way.

The case of an experiment with limited replication of a "probe" treatment is more difficult to analyse. Again an example will help. Assume 14 farms with one block per farm. All farms have the four treatments (a - d), but six have the extra "probe" treatment (e). Block size therefore varies: eight blocks or farms have four plots and six have five plots, giving a total of 62 plots. Two separate analyses could be done, firstly analysing the data from treatments a - d in all 14 blocks, and secondly analysing the data from treatments a - e from the six blocks with the five treatments.

Alternatively a single analysis can be done, computing treatment S.S. from:

$$S.S._T = \frac{T_a^2 + T_b^2 + T_c^2 + T_d^2}{14} + \frac{T_e^2}{6} - \frac{G^2}{62}$$

and block S.S. from:

$$S.S._B = \frac{B_1^2 + B_2^2 + B_3^2 + \ldots B_8^2}{4} + \frac{B_9^2 + B_{10}^2 + \ldots B_{14}^2}{5} - \frac{G^2}{62}$$

where B_1 to B_8 are the block totals for the eight blocks with four treatments, and B_9 to B_{14} are the block totals for the six blocks with five treatments, the apportionment of d.f. in the analysis of variance is as follows:

Blocks	13
Treatments	4
Error	44
Total	61

It is of course possible to partition the treatment S.S. in the above analysis into components. The divisor in computing the component S.S. would of course vary depending on the number of plots contributing to the treatment totals.

There is also the situation where blocks of equal plot numbers cannot be found, without compromising on uniformity within blocks. One option would be to take the smallest number of plots per block (k) that can be accommodated and use an incomplete block design based on this number. For instance, suppose the number of treatments is five, but of the twelve farmers willing to collaborate only three can accommodate blocks of five plots. Suppose that six farmers can accommodate blocks of four plots, and three have suitable land for blocks of three plots. A balanced incomplete block design based on blocks of three plots could be used. Table 2 gives a suitable design, which would have the effect of [illegible] plots, with each treatment replicated six times. An alternative is to use blocks of different sizes. The design should be "balanced", so as to give more-or-less equal replication of all treatments. A possible design is shown in Fig. 12. The analysis must take account of the differences in block size, and must also adjust treatments for block differences.

If it is considered desirable to have one of each treatment in every block, a design such as Fig. 13 could be used. This is an incomplete block design with unreplicated balance and blocks of varying sizes. Addition of further blocks would change the application, unless a balanced array was added (see 7.2, concerning Fig. 7 and Table 3).

Analysis of designs with unequal block sizes and unequal replication is not simple. A general method is given in the Appendix. If there are several data sets to be analyzed, it would be advisable to check the availability of computer programmes and of consulting services.

Best Available Document

Fig. 12. A design with block of different sizes, so that some blocks are incomplete. There are five treatments (a - e), with block sizes (k) of five, four and three plots.

t = 5, k = 5 and 3

(1)	a	b	c	d	e	(6)	b	c	d	e
(2)	a	b	c	d	e	(7)	a	b	c	e
(3)	a	b	c	d	e	(8)	a	b	e	
(4)	a	b	c	d		(9)	a	c	d	
(5)	a	b	d	e		(10)	c	d	e	

Replication: a - e = 8

Fig. 13 An incomplete block design with supplemented balance and blocks of different sizes. The number of treatments (t) is six (a -- f), block size is five, four and three plots, and one control treatment occurs in all blocks.

t = 6, k = 5, 4 or 3, c = 1

(1)	a	b	c	d	e
(2)	a	c	d	e	f
(3)	a	c	d	f	
(4)	a	b	d	e	
(5)	a	b	c	f	
(6)	a	b	e	f	
(7)	a	d	e		
(8)	a	b	f		
(9)	a	b	c		
(10)	a	e	f		
(11)	a	c	d		

Replication a = 11, b - e = 6

2.8 One or More Blocks per Farm?

Where a field experiment is laid down on a uniform area of land, nothing is gained by using a randomized complete block design instead of a completely randomised design. On such an area there are no grounds for blocking: plots do not differ in some characteristic that enables them to be allocated to blocks of similar plots. The analysis of variance will show very small and non-significant block effects. In the absence of any adequate grounds for blocking, superimposition of a RCB design may result in inappropriate blocking, resulting in a higher error mean square than would result from using a completely randomised design, or an appropriately blocked RCB design. A large and significant block effect in an analysis of variance of a field station experiment is an indication that the blocking was appropriate.

In on-farm research, farms are likely to be selected for a certain homogeneity of environment, cropping system and management. However, it is almost inevitable that, with blocks of different farms, large and statistically significant difference between blocks will be found in the analyses of variance. If there is one block per farm, so that block effects in the analyses of variance are farm effects, and if treatments and farms interact, then the error sum of squares will be large. This is because the error S.S. is in fact the block (or farm) X treatment S.S.

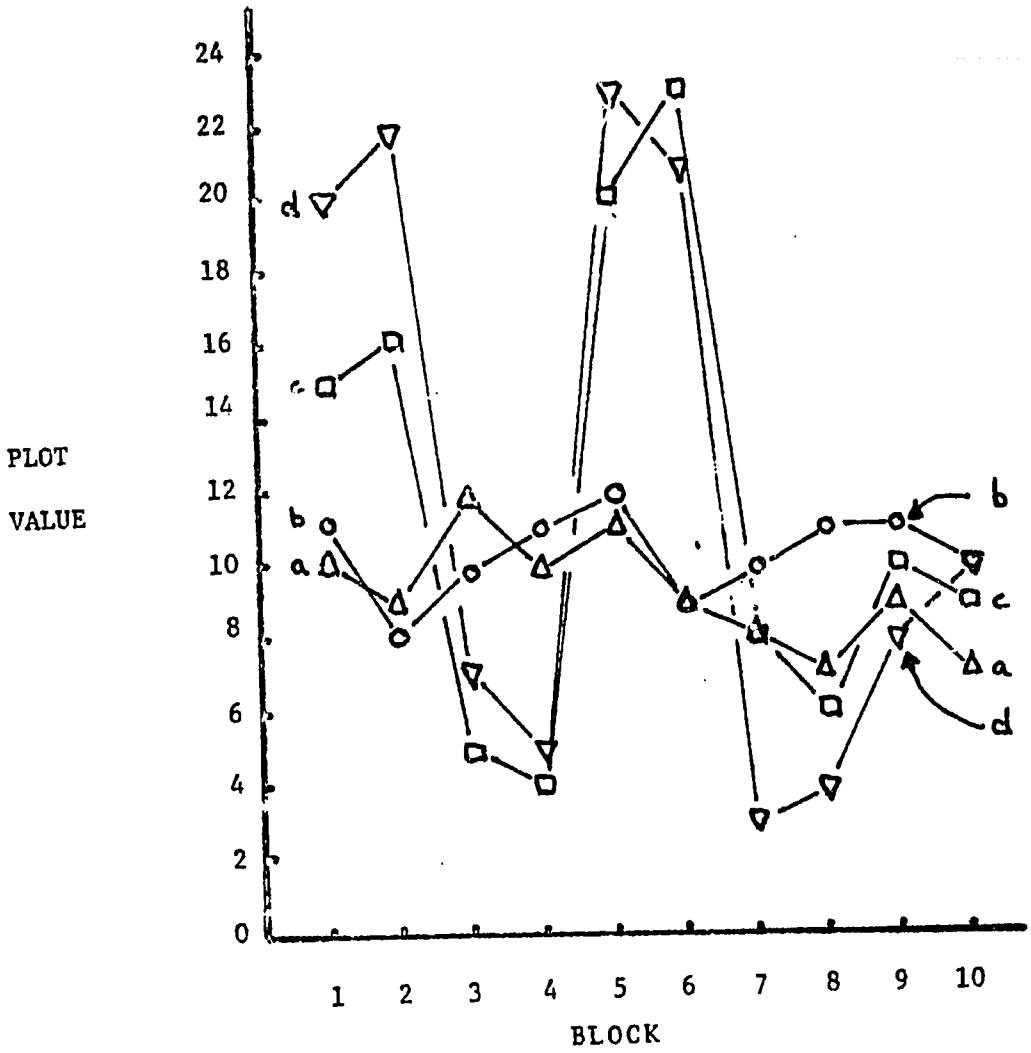
Table 6. Plot values (hypothetical) for a RCB experiment with four treatments (a-d) and ten blocks, considered both as an experiment with one block per farm, and with two blocks per farm.

Farms	Block #	Treatments a	b	c	d	Totals Blocks	Farms
A	1	10	11	15	20	56	111
	2	9	9	16	22	55	
B	3	12	10	5	7	34	64
	4	10	11	4	5	30	
C	5	11	12	20	23	66	128
	6	9	9	23	21	62	
D	7	8	10	8	3	29	57
	8	7	11	6	4	28	
E	9	9	11	10	8	38	74
	10	7	10	9	10	36	
							434

Farm X Treatment Totals

A	19	19	31	42	111
B	22	21	9	12	64
C	20	21	43	44	128
D	15	21	14	7	57
E	16	21	19	18	74
Treatment Totals	92	103	116	123	434

Fig. 14



Graph of plot values for the four treatments of Table 6 against blocks, to illustrate variability and interactions.

An example will illustrate this: the data are given in Table 6, and have been plotted in Fig. 14. It is clear that treatments c and d give higher values in blocks 3 and 4. There are only small differences between the four treatments in blocks 9 and 10. The data may also be looked at in another way: treatments a and b are less variable among blocks than c or d. Assuming one block per farm, so that there are ten farms involved, the analysis of variance is:

Source of variation	S.S.	d.f.	M.S.	F.
Blocks (= Farms)	486.6	9	54.07	2.70
Treatments	56.9	3	18.97	0.94
Error	539.6	27	19.99	
Total	1083.1	39	-	

M.S. is the mean square (i.e. the S.S. divided by the corresponding d.f.), and F is the variance ration (i.e. the M.S.s fivided by the M.S. for error). F. is used to test for significance by comparison with tabulated values.

Neither the effects of blocks - large as it is - nor that of treatments, are significant. If the experiment had been laid down on five farms with two blocks per farm (blocks 1 and 2 on Farm A, 3 and 4 on B and so on), the effect of the farm X treatment interaction could be tested: the error S.S., in the above analysis is now partitioned into an interaction and an error component. The analysis is as follows:

Source of variation	S.S.	d.f.	M.S.	F.
Farms	481.9	4	120.48	128.17***
Block-within-farms	4.7	5	0.94	
Treatments	56.9	3	18.96	9.92***
Farms X Treatments	510.4	12	42.53	21.81***
Error	29.2	15	1.95	
Total	1083.1	39	-	

The effect of farms is tested against the "blocks-within-farms" M.S., and the F. value is highly significant. Treatment, and farms X treatment effects are also highly significant. The co-efficient of variation (CV), which is the square root of the error M.S. divided by the overall mean and multiplied by 100, is 41.2% for the first analysis and 12.9% in the second analysis. Note that the error S.S. and d.f of the first analysis are indeed partitioned into two components (ie 539.6 = 510.4 + 29.2 and 27 = 12 + 15). Note also that the "blocks-within-farms" S.S. is derived from the blocks S.S. less the farms S.S.

From a practical viewpoint, farm X treatment interactions can arise because farms are not as homogeneous as originally thought, or because some treatments are not properly applied or are not applied uniformly over farms. Farms may be homogeneous in terms of soil, rainfall and major production systems, but may be heterogeneous in terms of cultivars and in at least some cultural practices. It follows that where homogeneity is in doubt, two blocks (or more)per farm are "safer" than one.

However, even if an experiment with one block per farm had been done, with results such as those of Table 6 showing no significant effect of treatments, the experiment should not be regarded as a loss. Useful information can be gathered from the data by asking, inter-alia, the following questions:

- Why do farms differ so markedly in total or mean values?
- Why do certain treatments perform well only on some farms?
- Why are some treatments more variable among blocks or farms than others?

Concomitant observations will assist in answering some of these questions. Highly variable treatments may be agronomically unsound or require an unrealistically high level of management: they should probably not be recommended as practices to farmers, at least not without further research. (for the data of Table 5 the standard errors of the means of the four treatments are respectively 1.54, 1.10, 6.22 and 7.77 which quantifies the observed variability).

The experiment with two blocks per farm does not in itself answer any of the above questions. In fact it poses the same questions. It does provide estimates of the magnitude of the interaction however. In less extreme cases than that illustrated the error S.S. might still be large (because the farm X treatment effect was relatively small so that its S.S. was small), in which case the variation between blocks within farms would be relatively large.

2.9 Partitioning of the Error S.S.

In many experiments, the treatment S.S. can be partitioned into components - as in the factorial designs already discussed - where main effects and interaction effects may each have a single d.f.. The error S.S. can also be partitioned, though this is seldom done. It is useful where variability is found in treatment effects - as in the illustration above. The computations are not discussed here, but it should be borne in mind that the technique exists and may be required in order to carry out valid t-tests, where treatments differ sufficiently in variability such that errors are not homogeneous.

2.10 Comparison of Zones

One final "arrangement" of blocks should be mentioned. Where it is desired to compare a set of treatments over a wide area - possibly island-wide - thus spanning several agro-ecological zones or several recommendation domains, there are two options. The first is to carry out separate experiments in each zone (with separate analyses of variance). The second is to carry out one large experiment with "zones" as a component in a single hierachical analysis of variance. If the number of farms that can be supervised is limited the second option is preferable, since the size of the separate experiments may be too small to give precise estimates of effects.

The allocation of degrees of freedom in the analysis of variance is shown below, assuming for simplicity, one complete block per farm and the same number of blocks (or farms) in each zone (z = number of zones):

Zones	(z - 1)	4
Blocks-within-zones	(b -)-(z-1)	10
Treatments	(t -1)	2
Zones X treatments	(z - 1)(t - 1)	8
Error	(t - 1) ((b-1)-(z-1))	20
Total	(bt - 1)	44

The right hand column gives the d.f. for an experiment where z = 5 b = 15 (with three blocks per zone) and t=3. The "blocks-within-zones" S.S. would be calculated as the difference between the blocks S.S. and the zones S.S. The "blocks-within-zones" mean square would be used to test the effect of zones. Separate analysis of five experiments (one per zone:"option one") would give the following analysis and d.f. allocation for each experiment:

Blocks	2
Treatments	2
Error	4
Total	8

Clearly these are too few d f. for error to give a precise test of treatment effects. The larger combined analysis also tests zone X treatment effects, and information on these may well be useful in planning further experiments zone by zone.

Should the number of farms vary from zone to zone, a larger combined analysis is still possible, but the computation of S.S.s will be slightly more complicated (see 2.7).

Table 7 Plot values (hypothetical) for a hierachical RCB experiment with three treatments (o - q) two blocks per farm and four farms in each of three agro-ecological zones.

Zone #	Farm #	Block 1				Block 2				Farm Totals			
		o	p	q	Total	o	p	q	Total	o	p	q	
1	1	5	6	7	18	4	6	7	17	9	12	14	35
	2	4	5	6	15	5	5	7	17	9	10	13	32
	3	5	5	8	18	4	5	7	16	9	10	15	34
	4	3	5	7	15	4	4	6	14	7	9	13	29
Zone Totals										34	41	55	130
2	5	7	9	5	21	6	10	5	21	13	19	10	42
	6	8	10	4	22	7	9	3	19	15	19	7	41
	7	9	9	6	24	10	11	5	26	19	20	11	50
	8	7	10	7	24	8	9	9	26	15	19	16	50
Zone Totals										62	77	44	183
3	9	9	6	7	22	10	5	5	20	19	11	12	42
	10	10	6	5	21	12	7	7	26	22	13	12	47
	11	11	7	7	25	10	5	6	21	21	12	13	46
	12	10	5	6	21	8	6	4	18	18	11	10	39
Zone Totals										80	47	47	174
Grand Totals										176	165	146	487

If there are two blocks per farm the hierachical analysis of variance has some additional components This can be illustrated by a numerical example (Table 7). The experiment compares three zones, with four farms per zone, and two complete blocks per farm There are three treatments (o, p and q). That is z = 3, f (farms) = 12, b = 24 and t = 3. The data have been devised so that q gives the highest values in Zone 1, p in Zone 2 and o in zone 3. In the analysis of variance (see below) the d.f. for "farms-within-zones" is given by (f-1)-(z-1), and for "blocks-within-farms" by (b-1)-(f-1). The corresponding S.S. are calculated from (farms S.S. - zones S.S.), and (blocks S.S. - farm S.S.), respectively. The "farms-within-zones M.S." is used to test the effect of zones. The analysis of variance is:

Source of variation	S.S.	d.f.	M.S.	F
Zones	67.0	2	33.50	13.40**
Farms-within-zones	22.5	9	2.50	
Blocks-within-farms	13.5	12	1.13	
Treatments	19.2	2	9.60	13.71***
Zones X Treatments	168.4	4	42.10	60.14***
Farms X Treatments	22.4	22	1.02	1.46
Error	14.0	20	0.70	
Total	327.0	71		

There are clearly significant differences between zones, and the effects of treatments and of zones x treatments are both highly significant.

It is worth noting that, with no hierachy and no partition into zones and farms, the analysis would be:

Source of variation	S.S.	d.f.	M.S.	F
Blocks	103.0	23	4.48	1.01
Treatments	19.2	2	9.60	2.16
Error	204.8	46	4.45	
Total	327.0	71		

Neither block nor treatment effects are significant.

As already pointed out, the analysis can accommodate different numbers of farms per zone. It should also be evident that the analysis of non-RCB designs where there is partition into zones, and where there is interest in zone X treatment and farm X treatment interactions is vastly more complicated than the analysis of RCB designs. This should not preclude or discourage the use of such designs where they are necessary, but great care in design and allocation of blocks would be necessary.

2.11 Summary

- For on-farm experimentation, the simplest, most familiar, and easiest to analyse design is the rendomised complete block (RCB).

- If the homogeneity of farms is in doubt it is safer to have two complete blocks per farm, rather than one, so that any farm X treatment effect can be estimated and separated from the error S.S.

- Where the block size (i.e. the number of plots per block) is limited by the availability of suitable uniform land on farms it may not be possible to use complete blocks.

- In such circumstances designs other than randomised complete block designs must be used. These include balanced lattices and balanced (and partially balanced) incomplete block designs. The analysis of these is more complet than for randomised complete blocks

- Where the treatments structure is factorial, confounding can be used to reduce block size, and fractional replication of factorials can also be so used.

- Increased replication of a control treatment, and testing one (or more) probe treatments with fewer replications, are ways of improving, or increasing, the information gathered.

- Where sufficient blocks of the desired size cannot be sited on farms, blocks varying in size (i.e. in number of plots) can be used. "Balanced" designs should be used, but the analysis is less straightforward than for randomised complete blocks.

CHAPTER 3. PROBLEMS

These are discussed under eight main heads, but topics inevitably overlap. Some of the problems have been referred to in Chapter 2 - with some solutions suggested - but are reiterated here for emphasis.

3.1 Physical

Small farmers in the Eastern Caribbean typically farm marginal lands on hillsides. Their lands are often characterised by -

- Slopes, which may be steep and irregular. Any one parcel may have areas differing in slope and in aspect.
- Gullies and rocky outcrops, and differences in depth of soil.
- The presence of trees, which offer uneven shading and root interference. If trees have been felled stumps usually remain.

If a part of the land has been terraced, the terraces may be narrow and uneven in width, and limited in area.

The selection of a number of uniform plots on such land is not easy. Even if the land is relatively flat, or uniform in slope, rocky outcrops and trees may be present. A consequence is that block size may have to be small. What must be avoided is excessive reductions in plot size, so as to increase the number of plots, relaxation of the criteria for blocking and selection of only those farmers with flat, ideal land; and the exclusion of those with more difficult lands.

3.2. Biological

The complexity of many of the cropping systems practiced by small farmers, both in time and space, also makes selection of uniform plots difficult. Adjacent areas of land may differ widely in cropping history: one area may have been weed fallowed with an adjacent area just out of bananas and another adjacent area may have carried a sequence of vegetables. These areas will differ in fertility, weed flora, quantity and quality of crop residues, cultivation history and so on. Some areas may have been grazed by tethered animals and so trampled and manured. There may be no clear evidence of previous cropping so that it is difficult to determine boundaries. This heterogeneity of history and use can lead to intra-plot variations, as well as to inter-plot (and intra-block) variation.

3.3. Technical

There is usually no difficulty on a field station in finding areas to accommodate rectangular or square blocks of contiguous rectangular or square plots. Marking out is relatively straightforward: blocks can be pegged out and divided into plots.

On farmer's lands it may be difficult to find areas of sufficient size into which contiguous and uniformly-shaped plots can be fitted - for the reasons outlined above. Contiguity is not essential, nor is equality of shape and size, although extreme variation should be avoided. The criterion of uniformity of plots within blocks must not be compromised however.

If terraces are to be used these may curve and vary in width, and the extent of admixture of subsoil with topsoil may vary both along terraces and between terraces.

Application of treatments uniformly to plots differing in size and shape and to plots that differ - between blocks - in slope and in the regularity of the terrain is also a problem. This is particularly acute with fertilizers and pesticides that must be applied at a specific rate per unit area. Calibration of a sprayer on flat or regular surfaces will underestimate the volume rate - and hence the application rate - applied to plots with slopes and irregular terrain..

3.4 Farmers

The farmer is a partner in on-farm experimentation, and so must be fully aware of the objectives of the experiment, of his responsibilities and expected contribution vis-a-vis those of the researcher. He should also understand the expected benefits, both short- and long-term.

Some farmer problems that may arise are:

- Reluctance to apply a particular treatment or treatment component, or to carry out certain basal operations, as and when required by the specifications of the experiment, because it conflicts with his way of doing things or with his beliefs For example, he may not believe in applying fertilizer to yams or may not believe in controlling weeds early.

- Conflict between the farmer's commercial and domestic needs and those of the experiment. Thus a farmer may reap only a few yam mounds at any one time, the number of mounds being determined by how much he can sell and use in the household. This poses problems in the collection of harvest data from a block of four plots each of ten mounds. On-farm research is usually - and should be - preceded by a thorough study of the target farmers' practices, but details - such as harvesting procedures - may be overlooked.

- Premature reaping -- in advance of the expected date - may be done as a response to a market opportunity or to a need for cash. At worst there may be total loss of data from one or more plots, or at best a partial loss, as with crops that are serially reaped (e.g. tomatoes, peppers). Even the most co-operative farmer may find it impossible to advice the researcher of his wish to start reaping earlier.

- Excessive helpfulness, when the farmer, with every good intention, hinders the collection of data or causes a loss of data. Examples onclude the farmer who clean-weeds all the plots of a herbicide experiment prior to the evaluation of weed control, and the farmer who reaps an entire experiment before the researcher's arrival, putting all the produce in one large heap.

- Premature adaption of the technology under test will lead to a loss of data, encouraging as it may be. An example is the farmer who, impressed with the logic of mulching, decides to mulch the unmulched plots as well.

- Bias in favour of one particular treatment sometimes occurs or is suspected. The farmer may favour what he regards as "his" plot, which will usually be one of the control treatments. This may be weeded first or more frequently than the others. This problem may arise because the farmers consider responsibility for the other plots to rest with the researcher.

- Drop-outs can occur for several reasons, resulting in the loss of one or more blocks from the experiment. Farmers may drop-out because they disagree with the practices required, or prefer to do things their way; or because they consider the demands of the plots on their time and resources excessive and unreasonable or because they fail to see any potential benefits from the experiment.

3.5 Planning and Design

There are several questions to be asked and answered.

- How many treatments to include in the experiment? This will depend in part on the objectives of the experiment and on the step in the programme. (Fig. 1) For instance, an experiment on fertilizer rates and times of application at Step 5 will have more treatments than an alternative system experiment at Steps 9 or 10. Not all the treatment combinations of a factorial array need to be tested a subset of those with most potential may be adequate.

- How many farms, and how many blocks or replicates per farm? This will depend on the logistics of laying-down and conducting the experiment, and on the homogeneity of farms within the agro-ecological zone. At step 5 it may be useful to cover a wide range of zones, in order to estimate and evaluate zone X treatment interactions. At steps 9 and 10 separate experiments in each zone are preferable, with these experiments differing perhaps in some details of the treatments. Where farms are expected or suspected to be heterogeneous more than one block per farm is to be preferred. It may be desirable to select farms with some heterogeneity - at least at Step 5 - to investigate interactions.

- Equal or unequal replication? In designing an on-farm experiment, it may be useful to increase replication of one or more treatments. These might, for example, be the farmer's practice and the basic recommended practice, with the other treatments - with less replication - being variations on the basic recommendations. In a crop protection experiment it may be useful to have increased replication of the untreated control treatment, so as to obtain a more precise basis against which to measure the performance of the treatments. Or on a subset of farms, selected perhaps because they can accommodate larger blocks, one or more "probe" treatments could be included. Both complete and incomplete block designs to accommodate unequal replication can be used, and appropriate methods of analysis are available.

- What control (or check) treatment (or treatments) to include? If the control is to be "farmer practice" how uniform is it? In a yam experiment for example, one treatment factor was "farmer bit size", but this ranged from 11 to 64 oz, and probably contributed to the large error S.S. in the analysis of variance.

- What plot shape and size? There are no hard and fast rules. Plot size must be adequate to represent the crop being grown and to provide reasonable estimates of yield, pest attack and so on. Plot shape should not vary so much within blocks that the intra-plot environment varies. A square plot and a long narrow plot comprising one crop row are clearly not comparable. Guard rows are necessary in some experiments, especially

crop protection experiments, but may be unnecessary in other types of experiment, particularly if plots are contiguous or surrounded by the crop so that edge effects are minimal. Plot shapes may have to differ within and between blocks to conform with the land areas available (Figs. 11 and 12).

- How much overall replication? How many plots and blocks can be accommodated on the selected farms? What experimental design to use? These questions are inter-related and depend on the answers to the above questions. Some rethinking of treatment numbers may be necessary, if block size must be small. Or an incomplete block design may be necessary if treatment number cannot be reduced. Attention must also be given to the expected heterogeneity of farms, and possible interactions.

Clearly, planning and the choice of the overall design must be an interative process.

3.6 Management

The farmer must be clear as to his responsibilities in the management and conduct of the experiment. Equally the researcher must be clear as to his responsibilities. Problems that can arise, for a variety of reasons include:

- Wrongly-applied treatments. The wrong plot may be treated, or the treatment may be applied at the wrong time, or may be applied to all the plots in the block or on the farm. Some information may be salvageable, but the block or blocks may be totally lost depending on the nature and extent of the error.

- Non-uniform application of experimental treatments to plots. This will lead to intra-plot variability and increase the error S.S. in the analysis of variance. Half a plot may be weeded today and the remaining half weeded next week, for example.

Non-uniform application of a non-varying (or basal) practice. This can increase both intra- and inter-plot (intra-block) variability. For instance a basal fertilizer application may be spread unevenly, or applied over a period of time to different plots.

3.7 Data Collection

This is the responsibility of the researcher. Even if Extension Officers are responsible for overseeing the experiment (as in Step 10: see Fig. 1) the researcher must decide what data is to be collected, how and when. He should prepare guidelines and proformae to ensure, as far as possible, uniformity of data collection and of recording.

Data collection costs time and money, so a problem to be addressed is what (or how much) data to collect. This must be resolved and determined at the planning stage.

The data and information to be collected is of four main types.:

- Primary data: that data essential to evaluate the experiment and achieve the objectives.
- Secondary data: that data which is desirable to assist in interpretation of the results.
- Supporting data and information: the calendar or diary of the experiment.
- Farm and production system information: information on each participating farm that may help in the design of alternative systems and in technology transfer.

Examples of these different types are given below. Specific needs will vary with the crop and the nature of the experiment. More data and information are necessary at steps 5 and 9 than at steps 10 and 11. Primary and secondary data must be collected plot-by-plot, but it will usually be sufficient to record supporting data by blocks or by farms.

Primary Data: Total yields, marketable yields, numbers, sizes, average weights, etc. In crop protection experiments primary data includes counts or estimates of weed numbers or cover crop damage, insect or lesion numbers and so on.

Secondary Data: Plant numbers, heights, branching, time of flowering, lodging, shattering, weeds and weediness, pest and disease incidence, soil nutrient levels, soil moisture levels and so on.

Supporting Data: Dates of land preparation, planting, thinning, fertilizing, weeding, spraying, reaping and so on.

Farm and Production System Information: Physical data on rainfall, soil type, slope, aspect; biological data on cropping history, other crops grown, intercrops, major weeds, pests and diseases; socio-economic data on farm and family size, labour resources, level of purchased inputs and sales; and information on cultural practices used.

3.8 Missing Plots and Blocks

Plots can be "lost" as a result of some of the problems discussed above. No data at all or incomplete data, may be obtained from one or more plots in an experiment.

An entire block may also be "lost", and where there are two or more blocks per farm, one or more entire farms may be "lost" or provide only partial data.

Data that is suspect because of suspected misapplication or wrong application of a treatment should be regarded as "lost".

Missing data complicates the analysis of variance and reduces the precision of significance tests.

CHAPTER 4. SOLUTIONS

There are no simple universal solutions to the problems posed above. Researchers engaged in on-farm research must be pragmatists, willing and able to develop solutions to problems as and when they arise. The following discussion provides suggestions only as to how some problems of on-farm experimentation can be addressed.

4.1 Physical

Thorough site inspection is essential to decide the siting of plots and blocks. Plots within blocks must be as uniform as possible with regard to soil type and depth, slope and so on. Some variation in plot size within blocks can be tolerated, and certainly such variation can be allowed between blocks and between farms. Perhaps a variation of up to 20% less than the desired or optimal size can be tolerated. Plots need not be contiguous and can be separated by outcrops, trees, ditches or gullies, etc., provided they satisfy the criteria for blocking.

If site inspection suggests that several of the selected farms cannot accommodate complete blocks of adequately sized plots, then three options are available:

- review the array of treatments to see if the number of treatments can be reduced, or if a subset could achieve the objectives of the experiments;
- look for farms that can accommodate complete blocks of the desired size;
- consider an incomplete block design.

4.2 Biological

Site inspection must take cognisance of shading, cropping history, crop and weed distribution and so on in siting plots and choosing blocks on farms. The farmer should be consulted on his cropping patterns and cropping history, use of fertilizer and so on, on-site.

4.3 Technical

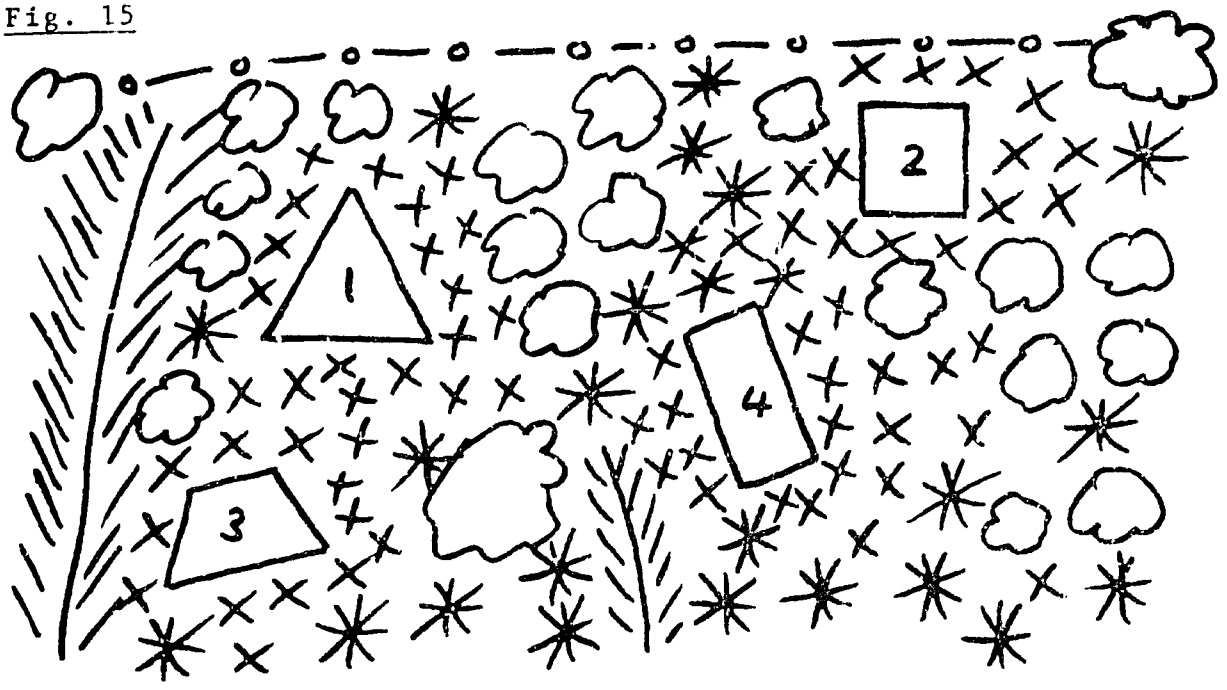
It has already been stated that plots within blocks do not need to be contiguous, nor exactly the same shape and size -- though extreme differences must be avoided. Apart from "statistical" considerations, the data from plots differing in area must be adjusted to a standard area before analysis. The need for guard rows should be critically examined for each experiment. Differences in plot shape and size between blocks are preferable to differences within blocks, but a flexible approach is necessary. Fig. 15. shows an extreme case where plots differing in shape might have to be used. Fig. 16 shows the simplest and ideal arrangement, but Fig. 17 is also perfectly satisfactory.

Marking out may require some ingenuity and will entail more work if plots are not contiguous. If plots of different shapes have to be used, then some simple calculations of the linear dimensions necessary to give equal areas will be necessary. A sketch map, with approximate dimensions, will help in deciding what plot shapes and sizes can be used. Where terraces vary in width and curvature, plots of very similar area can be marked out fitting geometric shapes (Fig 18). Alternatively, the "linear run of rows" can be used.

The problem of basal or treatment application at a specified rate to plots of different shapes and sizes can be resolved by application to a larger more conveniently shaped plot (Fig. 19), if space allows, or by application on a row-by-row or plant-by-plant basis where this is possible. Where application is by sprayer, on-site calibration is essential, adjusting the amount of concentrate accordingly.

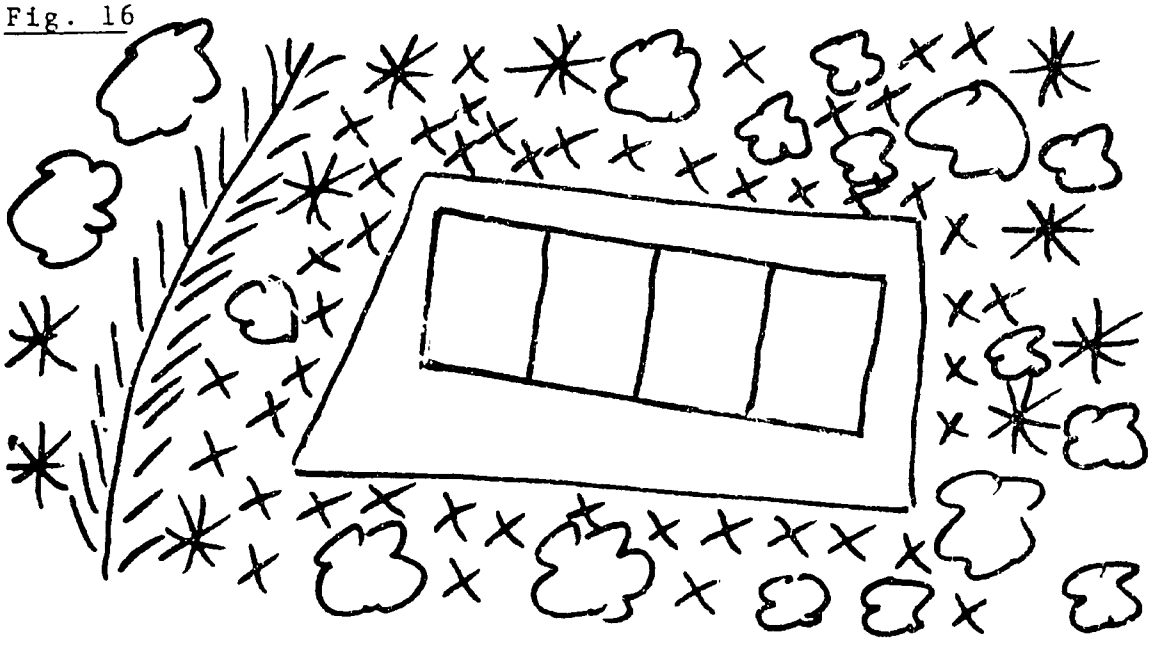
One obvious solution to these problems is to choose only farms with adequate space and easy terrain. Such farms may not be representative of all those in the domain, however. Additionally, it would be wrong to exclude co-operative and enthusiastic farmers on the grounds that their land was less than ideal. Where plots are scattered on a farm (i.e. not contiquous) proper pegging and labelling of each plot is essential.

Fig. 15



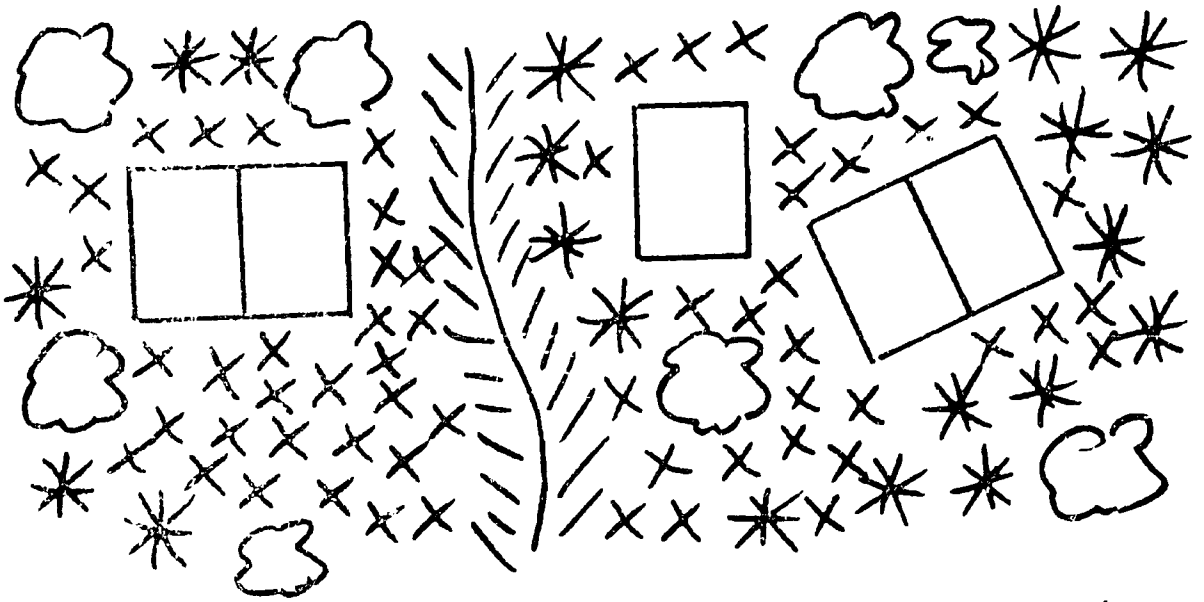
If the only relatively homogeneous areas are non-contiguous and differ in shape, could not these four plots constitute a block?

Fig. 16



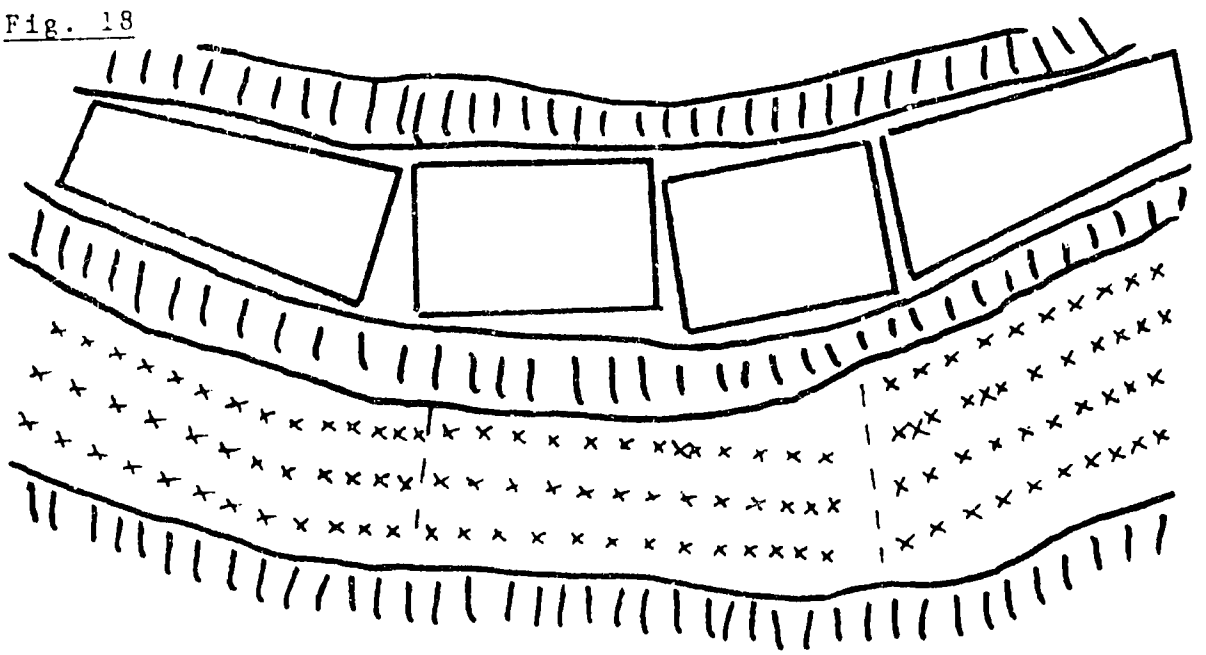
An ideal situation permitting several arrangements of contiguous plots.

Fig. 17



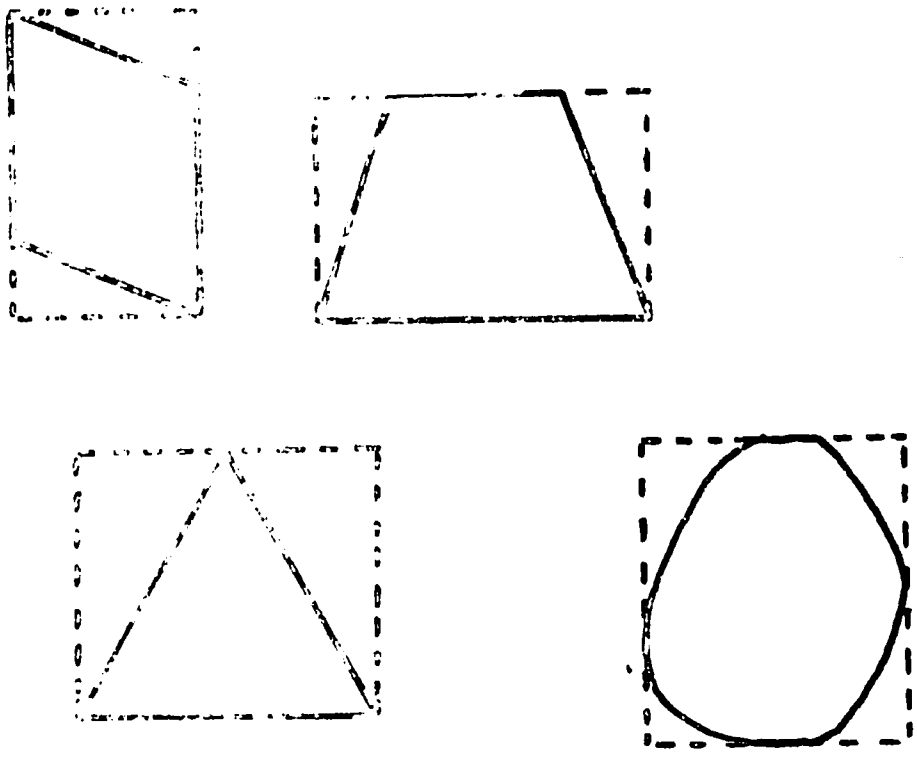
A situation where plots must be non-contiguous, but can be standard in size and shape.

Fig. 18



Curved terraces can be used for experimental plots. Plot shapes differ but areas are approximately equal. Plot size might comprise a specified length of crop row.

Fig. 19



Conversion of non-rectangular plots to rectangles for simplicity of applying treatments varying and non-varying, where space allows

4.4. Farmers

Most "farmer problems" can be avoided by clearly explaining to the farmers - either on an individual basis or in a small workshop meeting - the following:

- the objectives and rationale of the experiment,
- the expected long-term benefits that should result,
- the experimental procedures proposed, including the "critical" requirements,
- the anticipated contribution of the farmer (e.g. land, labour planting materials, etc.),
- the inputs, material and otherwise from the researcher,
- the short-term benefits to the farmers and any guarantees against failure or loss:
- the time schedule involved.

Farmer response may require modifications to the procedures. The farmers contribution will change as the programme advances: he will make greater inputs at step 10 than at steps 9 and 5. Extension officers should be involved from an early stage. They can assist in farmer selection, and in supervision of the experiments and in data collection. At step 10 extension officers are very much involved, more so than at the earlier steps, but should be involved at every step.

Specific points that require resolution are conflicts between farmers' practices and experimental and data collection procedures, and the willingness and competence of the farmer - and his workers - to carry out tasks which may be unfamiliar. It may be necessary for the researcher to demonstrate and train workers in such tasks.

Regular visits by the researcher must be scheduled, to monitor the experimental plots, to assist in unfamiliar tasks, to apply certain treatments - if this is not the farmers responsibility - and to collect data, including concomitant observations. The farmer may request advice on other aspects of his farming system, and willingness to give such advice may help to engender a spirit of co-operation and mutual goodwill.

4.5 Planning and Design

Some of the problems raised in 3.5 have already been addressed and solutions suggested.

The most appropriate design will depend on the type of on-farm test. Steps 9 and 10 may involve only two treatments but a large number of farms, whereas a step 5 test may have many treatments and require fewer replications. Some tests may require on-farm replications whereas others will not.

There may need to be a compromise between what is statistically ideal and what is possible given the resources available. An increase in the number of farms, for example, must be weighed against the increased mileage, manpower and materials required. It may also be desirable to carry out, say, five rather than four on-farm tests, so as to service a greater number of production systems and farmers.

One of the most important questions to be answered is the number of replicates. As a general rule, less replication is needed as the number of treatments increases. This is because there should be in the analysis of variance a "reasonable" number of d.f. for error. Table 8 shows the number of blocks (= number of replicates) required to give about 20 and about 30 error d.f. in a RCB design with one block per farm. Note that neither 20 nor 30 d.f. for error are advocated as the optimal numbers; but they are "reasonable" numbers.

With two or more complete blocks per farm and a RCB design, the d.f. are partitioned so as to test farm effects and farm X treatment effects (2.2). Table 9 shows the allocation of d.f. for different numbers of treatments, farms and blocks. Note that with more than one block per farm a greater total number of blocks is necessary, than with only one block per farm. For instance, with five treatments, seven blocks are necessary, with one block per farm, to give 24 d.f. for error, compared with 12 blocks with two each on six farms. However, the increased number of blocks and plots should not entail anything like a proportional increase in mileage or manpower requirements. Two blocks per farm also enables farm X treatment interactions to be evaluated. Increasing the number of blocks per farm to three increases the number of d.f. for error (Table 9), but if it is considered important to examine farm X treatment interaction effects, it would be preferable to include more farms with two blocks than fewer with three. If farms are relatively homogeneous - that is to say, the farm X treatment interaction effect is expected to be negligible - one complete block per farm should suffice. But where homogeneity cannot be assumed, or where farms are known to be heterogeneous, two blocks per farm are preferable and a useful insurance. It has already been suggested that exploratory experiments (as at step 5), it may be useful to cover several agro-ecological zones, or in other words to seek heterogeneity among farms. In other experiments the converse may be true.

TABLE 8. The number of blocks required in a randomised complete block design for different numbers of treatments, to give approximately 20 or 30 error d.f., assuming no partition of error.

For approximately 20 error d.f.							
No. of treatments	2	3	4	5	6	7	8
No. of blocks	21	11	8	6	5	4	4
Error d.f.	20	20	21	20	20	18	21

For approximately 30 error d.f.							
No. of treatments	2	3	4	5	6	7	8
No. of blocks	31	16	11	9	7	6	5
Error d.f.	30	30	30	32	30	30	28

TABLE 9. The allocation of d.f. in the analysis of variance for different number of treatments and farms, with two or three blocks per farm, assuming a randomised complete block design.

No. of treatments	2	2	2	2	3	3	3
No. of farms	12	20	8	12	6	12	6
Blocks per farm	2	2	3	3	2	2	3
Total No. of blocks	24	40	24	36	12	24	18
DEGREES OF FREEDOM (d.f.)							
Farms	11	19	7	11	5	11	5
Blocks -within farms	12	20	16	24	6	12	12
Treatments	1	1	1	1	2	2	2
[illegible]	11	19	7	111	10	22	10
Error	12	20	16	24	12	24	24
Total	47	79	47	71	35	71	53
No. of treatments	4	4	5	5	6	6	8
No. of farms	9	6	6	4	5	4	4
Blocks per farm	2	3	2	3	2	3	2
Total No. of Blocks	18	18	12	12	10	12	8
DEGREES OF FREEDOM (d.f.)							
Farms	8	5	5	3	4	3	3
Blocks-within farms	9	12	6	8	5	8	4
Treatments	3	3	4	4	5	5	7
Farms X Treatments	24	15	20	12	20	15	21
Error	27	36	24	32	25	40	28
Total	71	71	59	59	59	71	63

Note also that with a RCB design, as the number of d.f. for treatment increases, so the acceptable d.f. for error decreases. For instance, with 3 d.f. for treatment, 18 d.f. for error would be acceptable, but with d.f. for treatment, it would be preferable to aim for 24 d.f. for error. The two experiments would comprise 7 x 4 = 28 plots and 4 x 9 = 36 plots. Where experience suggests that coefficients of variation are high, additional replication (i.e. more d.f. for error) is advisable.

Experience with incomplete block designs for on-farm experimentation is limited. With one incomplete block per farm, block totals may vary widely, and no test for farm X treatment interaction can be made. Such a design should be used only where necessitated by the number of treatments and by limits to block size per farm. Farms should be reasonably homogeneous, however. The same constraints apply to several other designs including fractional replication of factorials, confounded factorials and designs with varying block sizes. In fact, more than one incomplete block, or more than one block of a confounded factorial could be located on a single farm. Nor is it essential for all farms to have the same number of blocks, complete or incomplete, although uniformity in block number per farm simplifies analysis and is to be preferred.

The choice of the control treatment(s) is important and requires careful thought. There are three options

- "researcher control" which might be unrealistic in practice, but which provides a base for comparisons. This might be no fertilizer, complete removal of weeds, no pest control etc.,

- "average farmer practice" which requires a full understanding of farmer practice and its variations,

- "individual farmer practice" that is the practice of each farmer collaborating in the experiment.

The first of these may be necessary, but may "interfere" with the other treatments, as in the example of no pest or disease control. If all farmers use fertilizer, use some pest and disease control and so on, this treatment can probably be dispensed with. The second option is perhaps the best control treatment, since the intention of the experiment is to improve upon farmer practice and this provides a consistent and uniform control. The third is valuable in demonstrating to individual farmers how his practices could be improved, and perhaps in showing the researcher how <u>his</u> practices could be improved. The control treatment may be very inconsistent between farms, however. The individual farmers' practices must be well-documented if this treatment is to be of value.

It would, of course, be possible to include all these control treatments in the experiment but this would increase block size. Not all need be included in the analysis of variance however: "individual farmer practice" might give extremely variable data. It would be important to calculate the variances of the individual treatments in any case, since the analysis

of variance model requires variances to be broadly similar. In determining "average farmer practice" it might be possible to separate farmers into homogeneous groups with respect to one or more component practices and to include this grouping in the analysis of variance (as zones or domains - see 2.10) .

Where the number of treatments is large, so that it will be difficult to find land for two complete blocks per farm, it may be desirable to reduce the number of treatments by further field station research. This will delay the start of on-farm experimentation, but should result in the rejection of some,at least,of the poorer treatments . If this cannot be done, then an incomplete block or confounded factorial design may be necessary so as to reduce block size. Such designs tend to require greater overall replication however, and therefore more plots, than complete block designs.

4.6 Management

Problems of management can generally be resolved by careful explanation to the farmer of the time schedule and of the importance of timeliness and uniformity of treatment and basal treatment application. Regular visits to on-farm plots are essential for proper monitoring and correction of any departures from the intended schedule.

4.7 Data Collection

The collection, recording and storage of data must be organised so as to avoid omissions, errors and losses. This is particularly so where several technicians are involved, each overseeing a subset of the farms.

Pro-formae will facilitate accurate recording, and can serve as reminders of the data to be collected and of observations to be made at each farm visit. They may need to be designed so as to be compatible with computer systems for data storage and analysis. Dates of recording must be noted. this is easily overlooked.

Regular observations of on-farm plots can often suggest concomitant variables that should be recorded. As far as possible, such observations should be anticipated and planned for. Something unexpected may show up however - for example the incidence of a disease or pest may appear to differ between treatments. Some standard method of observation should then be used. This might be a score or rating - and many scoring or rating schemes have been developed and widely used. Even if the pest or disease problem is serious on only a few farms the information is important and should be recorded.

An example of the value of concomitant observations and their use in co-variance analysis is given below. The data (Table 10) represent yields ( ) and weediness scores ( ) from a RCB experiment on five farms (A-E) with two blocks per farm. Weediness was estimated on each plot a few weeks before reaping on a scheme in which) = no weeds and 9 = complete weed cover. Analyses of variance show that treatments differed significantly both in yield ( ) and weediness ( ). Farms differed significantly in yield, but not in . weediness. To what extent is the effect of treatments on yield attributable to their effect on weediness, recognising that weeds affect yields? Is there any relationship between weediness and yield?

TABLE 10. Plot values (hypothetical) of yield (y) and weediness scores (x) for a RCB experiment with three treatments (a - c), two blocks per farm and five farms. (See 3.7).

Farm	Blocks	Treatment a	Treatment b	Treatment c	Totals (y)	Treatment a	Treatment b	Treatment c	Total (x)
A	1	11	5	9	25	2	8	4	14
	2	10	9	8	27	3	3	5	11
		21	14	17	52	5	11	9	25
B	3	14	7	12	33	1	8	3	12
	4	15	11	10	36	1	5	4	10
		29	18	22	69	2	13	7	22
C	5	13	7	9	29	2	6	5	13
	6	12	8	10	30	3	6	4	13
		25	15	19	59	5	12	9	26
D	7	9	8	7	24	3	5	6	14
	8	11	4	6	21	4	9	6	19
		20	12	13	45	7	14	12	33
E	9	12	11	13	36	4	6	3	13
	10	15	14	10	39	1	1	8	10
		27	25	23	75	5	7	11	23
TOTALS		122	84	94	300	24	57	48	129

TABLE 11 (A-D) Covariance analysis of the data of Tables 11 (A-D). Table 10 gives the sums of squares (S.S.y and S.S.x) and products (S.P.xy) . Tables 11B and D show the calculations of the regression co-efficients and of adjusted S.S. y's and M.S.'s, and Table 11 C shows the calculation of adjusted means.

Table 11A

	d.f.	S.S.y	S.P.xy	S.S.x
Farms	4	99.3	-29.7	12.5
Blocks-within-farms	5	5.4	- 6.0	7.8
Treatments	2	77.6	-67.2	58.4
Farm X treatments	8	14.1	-12.6	24.7
Error	10	39.6	-39.5	40.9
Total	29	236.0	-155.0	144.3

TABLE 11B

	S.S.y	S.P.xy	S.S.x	$\frac{(S.P.xy)^2}{S.Sx}$	$S.S.y - \frac{(S.P.xy)^2}{S.S.x}$	d.f.	M.S.	F.
Farms	99.3	-29.7	12.5	--	41.1	4	10.3	51.5
Blocks-within-farms	5.4	- 6.0	7.8	4.6	0.8	(5-1)	0.2	
Farms + Blocks within-farms	104.7	-35.7	20.3	62.8	41.9			

b' = -0.769 F = 4.6/0.2 = 23.0 (1 84 d.f.)

TABLE 11C

	x	$(x-\bar{x})$	$b'(x-\bar{x})$	y	$y-b'(x-\bar{x})$
A	4.17	-0.13	+0.10	8.67	8.57
B	3.67	-0.63	+0.48	11.50	11.02
C	4.33	+0.03	-0.02	9.83	9.85
D	5.50	+1.20	-0.91	7.50	8.41
	3.83	-0.47	+0.36	12.50	12.14

TABLE 11D

	S.S.y	S.P.xy	S.S.x	$\frac{(S.P.xy)^2}{S.S.x}$	$\frac{S.S.y-(S.P.xy)^2}{S.S.x}$	d.f.	M.S.	F
Treatment	77.6	-67.2	58.4		1.0	2	0.50	2.94
Error	39.6	-39.5	40.9	38.1	1.5	9	0.17	
Treatment + Error	117.2	-106.7	99.3	114.7	2.5			

b' = -0.966 F = 38.1/0.17 = 224(1&9 d.f.)

For analysis of co-variance, the sums of square for yield (S.S.Y) and for weediness (S.S.x) and the sums of products (S.P. xy) are required. The S.S. x's and S.S.y's will have been calculated for the analyses of variance. Note that the sums of products are all negative in this example (Table 11 A). Since one of the objectives of covariance analysis is to examine inter-relationships, the first steps are to estimate a regression co-efficient and determine its significance. Looking first at farms, the blocks-within-farms regression co-efficient (b') is calcualted from

$$b' = \frac{S.P.xy}{S.S.x}$$

using the blocks-within-farms S.S.x and S.P.xy (Table 11 B). In this example the value of b' = 6.0/7.8= -0.769. An F-test to determine if this value is significant; if it is not, further calculations are unnecessary. F is calculated - using the blocks-within-farms line only from

$$F = \frac{(S.P.xy)^2}{S.S.x} \Big/ \frac{S.S.y - ((S.P.xy)^2 / S.S.x))}{d.f. - 1}$$

In this example F = 4.6/0.2 = 230, which, with 1 and 4 d.f. is highly significant. Note that the 5 d.f. for blocks-within-farms is partitioned into 1 d.f. for the regression and a "residual" with 4 d.f..

The yield sums of square (S.S.Y) for farms is now adjusted for the regression. This is done in a roundabout way, using the farms and blocks-within-farms totals; from this line is calculated:

$$S.S.y - ((S.P.xy)^2 / S.S.x)$$

In the example, this is -

$$104.7 - (35.7^2/20.3) = 41.9:$$

from this is subtracted the comparable quantity calculated from the blocks-within-farms line, which is -

$$5.4 - (-6.0^2/7.8) = 0.8$$

to give the adjusted farms S.S.y (41.9 - 0.8 = 41.1). The mean squares are now derived, noting that the adjusted mean square for blocks-within-farms has one less d.f. than in the original analysis - one d.f. has been "lost" to the regression. The F value for farms (51.5) is highly signivicant. It might have happened that the effect of farms was no longer significant after adjustment, indicating that the differences in yield were largely accounted for by the differences in weediness.

Farm mean yields are now adjusted, as shown in Table 11C. Each farm mean for y is adjusted by an amount which varies according to the deviation of the farm mean for x from $\bar{x}$, the overall mean of x. Tests of significance of differences between pairs of adjusted means requires the calculation of separate 't' values, but this is not shown here.

Turning to the treatment and error lines (Table 11B), a comparable series of calculations is done. The value of b'' (-0.966) is highly significant, but the adjusted mean square for treatment is not significant. There is no point therefore in computing adjusted means. This result can be interpreted as indicating that differences between treatments in yield were largely due to differences in weediness. A cautionary note is necessary; covariance analysis does not necessarily indicate a casual relationship. Yield might have been determined before differences in weediness become apparent, and these differences in weediness may be due to differences in crop growth, plant form, shading and so on. Generalising, y may not be casually dependent on x, both y and x being casually related to an unsuspected (or unrecorded) variable z.

4.8 Missing Plots and Blocks

The best laid plans and most meticulous execution, cannot ensure no loss of data, but can reduce the likelihood. The loss of recorded data by carelessness is inexcusable and can be avoided. Loss of data from one or more plots in a block, from an entire block and from an entire farm can occur as a result of misunderstandings with the farmer, livestock damage, praedial largeny and so on. This should be borne in mind at the design stage: there should be sufficient d.f. for error to accommodate some loss.

A value for a single missing plot in a RCB experiment can be easily computed. If a' is the missing value (of treatment a), then an estimate of a' is given by -

$$a' = \frac{r B' + ET - G}{(r - 1)(t - 1)}$$

where B' is the total of the remaining plots in the block, T' is the total of treatment a from the remaining blocks and G is the grand total. If data from several plots are missing (a', b' and c' for instance) values are assumed for b' and c' and a value for a' estimated as above. Using this value, and the assumed value of c' an estimate is then made of b', and then c' using the estimated values of a' and b'. The calculations are repeated iteratively until the estimated values do not differ substantially from those found in the previous cycle. One error d.f. is lost for each estimated plot value.

For incomplete block designs more complex formulae than the above must be used. But the general method of analysis (Appendix) can accommodate some missing plots, and even blocks.

If the data from an entire block is lost, or from both blocks on a farm where the design has two blocks per farm, no estimation of the missing plot and block values can be made. However, the loss of, say, two plots out of four in one block should not cause the researcher to abandon the entire block: data from the two remaining plots is still useful, and methods of analysis are available to accommodate this situation.

A more serious situation arises where one or more entire blocks are lost, perhaps because the farmer reaps the plots without advising the researcher, or because of livestock damage. Suppose for example that in the data of Table 6, Farm E was entirely lost (e.g. both blocks reaped pprematuerly) and that blocks 6 and 8 were also lost, due to severe livestock damage. The data now comprise plot values from only six blocks, two each on Farms A and B, and one each (5 and 7) on Farms C and D. No longer can the effect of farms be estimated, nor the farm x treatment interaction effect, in the analysis of variance. The analysis now becomes:

Source of variation	S.S	d.f.	M.S.	F
Blocks	316.0	5	63.20	2.72
Treatments	40.5	3	13.50	1.0
Error	348.0	15	23.20	
Total	704.5	23		

lotting the data, as in Fig. 14 (see 2.8) would indicate that there is evidence of a block x treatment interaction.

Since methods of analysis are available for experiments with unequal replication, data from experiments with more than one missing plot per block from several blocks, can be analysed. Clearly, if data from only one or two plots of a particular treatment are available, that treatment cannot be included in the analysis. But if the loss of those plots was due to disease or lack of rainfall, that is a "significant" result indicating disease or drought susceptibility.

Clearly, every effort must be made to avoid loss of plots and blocks and of recorded data. This may require fencing (which is an added cost) or simply securing the full co-operation and understanding of the farmer. It is also advisable to increase the overall replication, so as to ensure, as far as possible, sufficient replicates, in the event that one or more replicates are lost. This will usually mean increasing the number of farms but in an experiment with one complete block per farm it may be possible to put additional blocks on two or three farms at minimal extra cost.

APPENDIX

The Analysis of Variance - A General Method for Designs arranged in Blocks, and accommodating Incomplete Block Designs, Missing Plots and Unequal Replication.

The general method of analysis described below can handle most designs arranged in blocks. Difficulties arise if there are disconnections in the design, that is, some treatments occur only in certain blocks with the other treatments occuring only in the other blocks. This can occur in confounded designs.

The data used as an example are arranged in two ways: Firstly, as an incomplete block design with some double replication within blocks (Fig. 20 A), and secondly, as a randomised complete block design (Fig.20B) Both designs have 20 plots, with five treatments replicated four times. The design of Fig. A has nothing to commend it, and is used for its illustrative value only.

The data of the incomplete block design give an incidence matrix as follows - to show the number of times each treatment occurs in each block.

	BLOCK		
	(1)	(2)	(3)
a	2	0	2
b	2	2	0
c	1	1	2
d	1	2	1
e	0	2	2

Block totals are 59, 81 and 83 based on 6, 7 and 7 plots respectively (Table 20 A.) Block means are therefore 9.833, 11.571 and 11.857. Treatment totals are: a, 30 ; b, 35; c, 46; d, 51; e, 61.

Fig. 20. An incomplete block design (A) and a randomised complete block design (B), to show the use of the general method of analysis of variance. The plot values are the same for the two experiments.

A

(1)	b	a	c	d	a	b
	10	8	12	14	7	8

(2)	c	b	e	d	b	e	d
	11	10	14	12	7	16	11

(3)	a	c	d	e	c	e	a
	6	13	14	15	10	15	9

B

(1)	e	b	a	d	c
	16	10	8	14	12

(2)	c	a	e	b	d
	11	7	14	8	12

(3)	b	c	d	e	a
	10	13	14	15	9

(4)	a	d	c	e	b
	6	11	10	16	7

Table 12. Plot values from Fig. 20 A and B arranged in order with block and treatment totals.

A. Incomplete block design (Fig. 20 A)

	Blocks (1)	(2)	(3)	Treatment Totals
	a, 8	b, 10	c, 6	a, 30
	a, 7	b, 7	a, 9	b, 35
	b, 10	c, 11	c, 13	c, 46
	b, 8	d, 12	c, 10	d, 51
	c, 12	d, 11	d, 14	e, 61
	d, 14	e, 14	e, 15	
		e, 16	e, 16	Grand total, 223.
Total	59	81	83	

B. Randomised complete block design (Fig. 20 B)

	Blocks (1)	(2)	(3)	(4)	Treatment Totals
a	8	7	9	6	30
b	10	8	10	7	35
c	12	11	13	10	46
d	14	12	14	11	51
e	16	14	15	16	61
Total	60	52	61	50	223

The adjusted treatment totals (Q) are calculated by taking each treatment total and subtracting the block means, weighing for the number of times the treatment occurs in each block. For instance:

$Qa = 30 - ((2 \times 9.833) + (2 \times 11.857)) = -13.380,$

$Qb = 35 - ((2 \times 9.833) + (2 \times 11.571)) = -7.808$

$Qc = 46 - (9.833 + (2 \times 11.571) + 11.857) = +0.882$

and so on. $Qd = +6.168$ and $Qe = +14.144$. The sum of the Q's should be zero, but rounding off may give a small deviation from zero.

The main part of the calculation is an iteration, shown below. This is continued until further repetition has no effect. The purpose of this iteration is to find the true effects of treatments after allowing for differences due to blocks. The vectors v_1, v_2, v_3 etc. are calculated for each treatment and in comparable fashion values for blocks (u_1, u_2, u_3, etc

The vector v_1 is estimated by dividing adjusted treatment totals by the number of plots receiving each treatment in this case 4 for all treatments. Thus for treatment a the value is $-13.380/4 = -3.345$, for $b = -7.808/4 = -1.952$ and so on. The u_1 values for blocks, are estimated from the v_1 values multiplying each element by the number of times it occurs in each block. For example, for block 1 u_1 is estimated from

$((2 \times -3.345) + (2 \times -1.952) + (+0.220) + (+1.542))/6 = -1.472$

and for block 2, from

$((2 \times -1.952) + (+0.220) + (2 \times +1.542) + (2 \times +3.536)) / 7 = +0.925$

and similarly for block 3.

Next v_2 is projected using the values in u_1. For treatment a, each element of u_1 is multiplied by the number of times a occurs in the block, thus,

$((2 \times -1.472) + (2 \times +0.338))/4 = -0.567$

and for treatment d,

$(-1.472 + (2 \times +0.925) + 0.338)/4 = +0.179$

The sum of the v_1 elements should be zero, allowing for small deviations due to rounding-off.

From v_2, the values of u_2 are projected. Thus for block 2,

$((2 \times -0.274) + 0.032 + (2 \times 0.179) + (2 \times 0.632))/7 = +0.158$

Then from u_2, the elements of v_3 are derived, and so on. At each step, the sum of the v_1, v_2, v_3 elements etc. should equal zero. Any sizeable deviation from zero should be checked for errors. The iteration continues until only zero values, or values close to zero are derived.

				v_1	v_2	v_3	v_4	v_5	v_6
2	0	2	(4)	-3.345	-0.567	0.097	-0.016	-0.003	0.000
2	2	0	(4)	-1.952	-0.275	0.044	-0.001	-0.001	0.000
1	1	2	(4)	+0.220	+0.031	+0.005	+0.001	0.000	0.000
1	1	2	(4)	+0.220	+0.031	+0.005	+0.001	0.000	0.000
1	2	1	(4)	+1.542	+0.178	+0.031	+0.006	+0.001	0.000
0	2	2	(4)	+3.536	+0.632	+0.105	+0.008	+0.003	0.000
(6)	(7)	(7)							
-1.472	+0.925	+0.338	u_1						
-0.245	+0.158	+0.053	u_2						
-0.041	+0.027	+0.009	u_3						
-0.007	+0.005	+0.002	u_4						
-0.001	+0.001	0.000	u_5						

The purpose of iteration is that the effects of treatments, known as the <u>treatment parameters</u>, are best estimated by the elements v_1, v_2, v_3.....

Thus for treatment a, the parameter is given by (-3.345) + (-0.567)+(0.097) + (-0.016) + (-0.003) = -4.028. So the treatment parameters are:

a -4.028; b -2.279; c. +0.257; d +1.757 e +4.294.

A check is to multiply each treatment parameter by the respective treatment replication and sum the products, which should sum to zero

(4 x -4.028 + (4 x -2.279) + (4 x 0.257) + (4 x 1.758) + (4 x 4.294) = - 0.008.

The deviation is accounted for by rounding-off errors. Note that with equal replication, there is no need to include the 4 in each bracket for this check.

The adjusted treatment means are given by adding the general mean (223/20 = 11.15) to each parameter:

a = 11.15 + (-4.028) = 7.122, and b = 8.871

c = 11.407 d = 12.908, e = 15.444.

The sum of squares for treatments can now be calculated by multiplying each adjusted treatment total (Q) by the corresponding parameter and summing the products, thus:

(-13.380 x -4.028) + (-7.808 x - 2.279) + (0.882 x 0.257) + (6.166 x 1.757) + (14.144 x 4.294) = 143.49.

An analysis of variance can now be done. The within-blocks S.S. is calculated from the sum of the squared individual plot values less the block totals squared and divided by the number of plots in each block:

$(8^2 + 7^2 + 10^2 \ldots 15^2 + 16^2) - ((59^2/6) + (81^2/7) + (83^2/7) =$

2667.00 - 2501.60 = 165.40

	S.S.	d.f	M.S.	F.
Treatment	143.49	4	35.87	21.22
Error	21.91	13	1.69	
Within-blocks	165.40	17		

The d.f. are 5-1 for treatments and within-blocks the total number of plots less one (19) minus 2 for blocks (3 - 1) = 17. So for error there are 17 - 4 = 13 d.f.. The F value of 21.22, with 4 and 13 d.f. is highly significant.

It is possible to adjust for blocks, in similar manner to the adjustment for treatments and to estimate residuals (i.e. the difference between the "expected" and actual values) for each plot. The sum of the plot residuals equals the S.S. for error.

Had the experiment been laid down as a RCB design, with plot arrangement and values as shown in Fig.20B, the above method of analysis could also be used. This example is given solely to illustrate the versatility of the general method.

$Qa = 30 - (12.00 + 10.40 + 12.20 + 10.00) = 14.60$

$Qb = 35 - 44.6 = -9.60$

$Qc = 46 - 44.6 = +1.40$ and

$Qd = +6.40$ and $Qe = +16.40$.

The incidence matrix comprises ones only, as shown.

					v_1	v_2
1	1	1	1	(4)	-3.650	0.000
1	1	1	1	(4)	-2.400	0.000
1	1	1	1	(4)	+0.350	0.000
1	1	1	1	(4)	+1.600	0.000
1	1	1	1	(4)	+4.100	0.000
(5)	(5)	(5)	(5)			
0.000	0.000	0.000	0.000	u_1		
				u_2		

The u_1 elements are all zero, and the v_2 elements are also all zero. Treatment parameters are therefore the v_1 elements only. The S.S. for treatments is therefore given by:

$$(-14.60x-3.650)+(-9.60x -2.400) + (+1.40 x +0.350) + (+6.400x +1.600) + (+16.40 x +4.100) = 154.30$$

and the within-block S.S. by

$$(8^2 + 10^2 + 12^2 + 14^2 \ldots 11^2 + 16^2) - (60^2 + 52^2 + 61^2+50^2)/5= 2667 - 2505 = 162.00$$

So the analysis is:

	S.S.	d.f	M.S.	F
Treatment	154.30	4	38.58	60.27
Error	7.70	12	0.64	
Within-blocks	162.00	16		

This may be compared with the more familiar method of analysis of a RCB design:

	S.S.	d.f.	M.S.	F.
Blocks	18.55	3	6.18	9.66
Treatments	154.30	4	38.58	60.27
Error	7.70	12	0.64	
Total	180.55	19		

This gives a blocks S.S., and the effect of blocks is clearly significant.

The general method of analysis is versatile and can handle, inter alia the following:

- incomplete block designs,
- designs with planned unequal replication;
- experiments with incorrect application of treatments (so giving unplanned unequal replication)
- data sets with missing plot values.

INDEX 1: GENERAL

	Page No.
adjusted treatment means	
- in analysis of covariance	66 - 67
- in analysis of variance	75
aliases	28 - 29
alternatives, testing of	1 - 2
analysis of covariance	64 - 68
analysis of variance	85 - 86
- confounded factorials	23- 26
- factorials	20 - 22
- [illegible] replication	27 - 31
- general method	70 -77
- hierachical	42
- incomplete block designs	10-19 ,62
- randomised complete block	5 - 9
applicability testing	1, 2
balance, supplemented	13 - 17
balanced incomplete block designs	10 - 19
balanced lattices	10
basal treatment application	54 , 57
bias	48
biological problems	46
blocks	
- c[illegible]	4-7,83
- different sizes	83
- incomplete	4, 10-19,83
- number per form	5,7,37-41, 59,60
- number required	59,60
blocks-within-farms	41,44
blocks-within-zones	42

	Page No.
calibration of Sprayers	47
coefficient of variation	41
completely randomised designs	5
concomitant observations	41, 64
	23 - 26
- complete	23 - 25
- partial	24 - 25
contiguity of plots	4,6,8
control treatments	
- choice of	49-62
- extra replication of	13-19
covariance analysis	64-68
cropping history	46,53
d.f.	
- allocation in analysis of variance	85
data	
- primary	51
- secondary	51
- supporting	51
data collection	51-52, 58,64-68
defining contrast	28
degrees of freedom see d.f.	
disconnected designs	18,19
efficiency factor (of incomplete block designs)	12

	Page No.
FSR/D methodology	1, 2, 3
F test	40
factorial	20-22
- 2^n	21
- 3^n	22
- cofounding	29,31
farm x treatment interaction	38,40
farmers	
- bias	48
- conflicts	47
- drop outs	48
- explanation to	58
- premature adoption	48
- premature reaping	48,58
- reluctance	47
- farming systems research and development	1, 2
farms, how many	49
fractional replication	27-31
group replication	11,13
gullies	46
half-replicate	27,28
hierachical analysis	42-45
homogeneity	
- of environment	41
- of error	42,63
incidence matrix	70,74,76
interaction effects	20
lattices	10,11
main effects	20-22
management	50,64
marking out	54
mean square	40
missing plots and blocks	52,68,69,77

	Page No.
non-uniform application	50
on-farm	
- production systems analysis	1, 2
- testing	1, 2
- testing of alternatives	1, 2
paired t - test	7, 9
partially - balanced designs	11
partition of	
- error S.S.	42
- treatment S.S.	20-22
physical problems	46
planning	35,36
plot	
- arrangement	5,6,7,55,57,83
- shape	49,55
- size	49
problems	
- biological	46
- physical	46
- technical	47
production systems analysis	
proformae	
randomised complete block designs	5 - 9
- with one block per form	5,6,7
- with two or more blocks per farm	59-62
- with two replicates per block	7, 8
replication	
- fractional	27-31
- how much ?	49,59-61
- unequal	32-36,49

	Page No
slopes	46
subsets of treatments	
- fractional replication	24,28-31
- incomplete block designs	10-19
sum of products	64-67
sum of squares	70-77,85
supplemented balance	13-17,36
t - test, paired	7, 9
technical problems	47
terminology	4
terraces	47,56
treatment application	54
treatment combinations	20,23,24,28
treatment parameters	74,76
treatments	
- how many	49,53
trees	47
unequal replication	32-36,49
uniformity of plots	46,47
variance ,analysis of	85,86
within-blocks S.S.	75,76
wrongly applied treatments	50
zones	42-65
zonex x treatment interaction	42,44

Page No.

Index 2 Experimental Designs

Balanced lattice (t = 9, k = 3 b 12, r = 4)*	Fig. 4	10
Balanced incomplete block (t = 5, k = 3 b = 10 r = 6)	Fig. 5	11
Balanced incomplete block (t = 7 k = 2 b = 21 r = 6)	Fig. 6	11
Blocks of different sizes (t = 5 k = 5 4 and 3, r = 8)	Fig. 12	34
Blocks of different sizes with supplemented balance (t = 6, k = 5 4 and 3 c = 1)	Fig. 13	36
Confounding complete of a 2^3 factorial	Fig. 9	23
Confounding partial of a 2^3 factorial	Fig. 10	24
Half-replicate (fractional replication) of a 2^3 factorial in 4 blocks of 4 plots	Fig. 11	27
Incomplete block with disconnection (t = 6 k = 3)	Fig. 7A	19
Incomplete block with overlapping (t = 6 k = 4)	Fig. 7F	19
Incomplete block with supplemented balance		
(t = 4 k = 3 c = 1)	Fig. 7A	14
(t = 5 k = 3, c = 1)	Fig. 7B	14
(t = 6, k = 3, c = 1)	Fig. 7C	15
(t = 6 k = 4 c = 2)	Fig. 7D	15
(t = 7 k = 5 c = 2)	Fig. 7E	16
Randomised complete block one replicate per block	Fig. 2	6
Randomised complete block two replicates per block	Fig. 3	8

* t = number of treatments k = number of plots per block (i.e. block size). b = number of blocks, r = replication and c = number of control treatments in each block. For RCB designs c = 1.

Table 2 (p. 12) lists balanced incomplete block designs available for various values of t and k.

Table 3 (p. 17) lists incomplete block designs with supplemented balance for various values of t,k and c.

Page No.

Index 3: Analyses of Variance

Factorial: randomised complete block design

2^3 x 4 blocks	21
2^n x b blocks	21
3^3 x b blocks	22

Factorial: complete confounding

2^3 x 3 replicates in 6 blocks	25
2^6 x 1 replicate in 8 blocks	26

Factorial: partial confounding

2^3 x 3 replicates in 6 blocks	25

Factorial fractional replication

2^5 half replicate in 4 blocks	28,30
2^5 half replicate in 2 blocks	30
2^3 subset with b replicates	30

General method*	70

Incomplete block design

simple replication (generalised)	13
group replication (generalised)	13

Randomised complete block design

generalised	5
one block per farm	
(t = 4, b = 17)	32
(t = 4, b = 17, but double replication of one treatment)	32
(t = 4 with an extra probe treatment, b = 14)	35
(t = 4, b = 10)*	39
two blocks per farm	
(t = 4, b = 10)*	39

*Indicates worked examples

	Page No.
hierachial classification	
zones	42
zones and farms within zones*	43,44
degrees of freedom	
t = 2-8: blocks required for 20 or 30 error d.f. with 1 block per farm	60
t = 2-8: error d.f. with 2 or 3 blocks per farm.	61
t test (paired)	9
Analysis of covariance*	64-68

*Indicates worked examples.

FURTHER READING

Cochran, W.G., and Cox, G.M. (1957) Experimental Designs. 2nd Ed. John Wiley, New York, U.S.A.

Fisher, R.A., and Yates, F. (1963) Statistical Tables for Biological, Agricultural and Medical Research. 6th Ed. Oliver & Boyd, Edinburgh.

Pearce, S.C. (1976) Field Experimentation with Fruit Trees and other Perennial Plants. Tech. Comm. #23, 2nd Ed. (Revised). Commonwealth Bureau of Horticulture and Plantation Crops, East Malling, Kent. Commonwealth Agric. Bureau, Slough, England.

Pearce, S.C. (1983). The Agricultural Field Experiment. 1st Ed. John Wiley, London, U.K.

Yates, F., Lipton, S., Sinha, P., and Das Gupta, K.P. (1959) An exploratory analysis of a large set of 3x3x3 fertilizer trials in India Emp. J. Exptl. Agric., 37, 263-275.