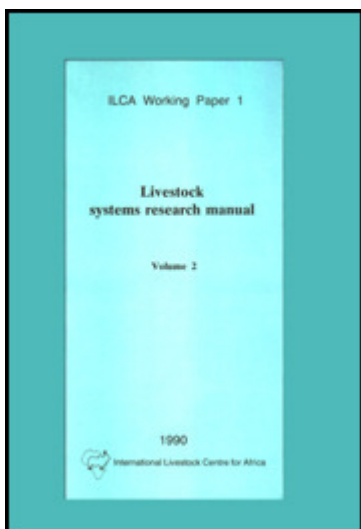


Livestock systems research manual - Volume 2



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International Livestock Centre for Africa

Addis Ababa, Ethiopia
December 1990

ILCA

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ILCA conducts its own research programme, works closely with national agricultural research systems (NARS) in collaborative research projects and seeks to develop the research capacities of NARS by providing specialised training programmes and a range of information services.

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The ILCA Working Paper series comprise informally reviewed data analyses, case studies, systems studies and reference materials dealing with livestock production in sub-Saharan Africa.

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Comments on this paper are invited, and should be addressed to the (senior) author.

Correct citation: ILCA (International Livestock Centre for Africa). 1990. *Livestock systems research manual*. Working Paper 1, Vol. 2. ILCA, Addis Ababa, Ethiopia. pp. 125.

ISBN: 92-9053-174-0

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Acknowledgements

Work on the *Livestock systems research* manual started in 1986 and has been conducted under the supervision of a Steering Committee of ILCA staff. The work was executed within the programme of the Livestock Policy and Resource Use Thrust.

The manual is primarily the product of Martin Doran who worked at it in stretches over a period of two years. He drew so much on the knowledge, skills and assistance of ILCA scientists that authorship has been attributed to ILCA as a whole rather than to any one individual. Nevertheless, Martin Doran drafted almost every word of the manual. He did so with the general support of Stephen Sandford who, as coordinator of the Livestock Policy and Resource Use Thrust, reviewed and commented repeatedly on almost every part of the manual. The third major contributor was Inca Alipui. She was instrumental in adding clarity and precision, and in making the manual easier to read went far beyond the normal role of a language editor.

Other substantial contributors to Section 1 of the manual were John McIntire, whose constructive comments shaped Module 3, and Barbara Grandin who helped with data and reference material; Trevor Wilson who provided data and extensive reference material for Module 5; John Tohill who was most helpful in the writing up of Module 6; Jess Reed and Douglas Little who commented on Module 7; and Brian Perry of ILRAD, Nairobi, without whose substantial input in the form of data and references Module 8 would have been incomplete.

Comments on the first draft were given by Ralph von Kaufmann (Module 3); Barbara Grandin and Solomon Bekure (Module 4); Ray Brokken (Module 5); Pieter de Leeuw and Pierre Hiernaux (Module 6); Michael Goe (Module 7); and Olajide Kasali and Eddie Mukasa-Mugerwa (Module 8). In addition to commenting on range resource evaluation, Pieter de Leeuw read the whole Section 1 of the manual for technical accuracy.

John Sherrington, ILCA's biometrician, rewrote the greater part of Module 11 in Section 1 and commented on the statistics in Section 2. Credit for the contents of the main statistical module (number 2) in Section 2 goes to Robin Sayers, his predecessor. Brian Perry's substantial contribution to the Animal Health Appendix to Module 2 is also gratefully acknowledged.





Preface

In its research work ILCA has always used and continues to use a farming systems approach. At the same time both in its mandate and as one of its operational goals ILCA seeks to improve the capacity of Africa's national agricultural research systems. This manual presents the fruits of both ILCA's own long experience of livestock systems research and of the expertise and efforts of others in this field.

The intended audience are primarily African scientists who are working for national research organisations and who are in need of reference materials and of practical examples of livestock systems research. The manual may also be of use to a wider audience, including those who need to study livestock systems in Africa in the context of development activities rather than research.

The manual has been produced as an ILCA Working Paper (in two volumes) for a number of reasons. One of these is that there will never be a 'final word' in livestock systems research, because new techniques will be continually invented and old ones adapted. At its first appearance there are parts of this manual which are already in need of updating.

Another reason is that the manual as a whole has not been formally reviewed as have other ILCA official publications and manuals, in an external peer review process. Of course, individual modules have been extensively reviewed both internally within ILCA and externally. But if anyone indeed existed who had the breadth of experience and knowledge to be able to review the manual as a whole, that person would be in such demand that he or she would not have the time to do the review.

ILCA has therefore decided to circulate this manual as a Working Paper, initially in a limited edition to a number of people from different backgrounds but who are known to be most active and most interested in livestock systems research. You are one of these. We should be most grateful for your comments, your proposals for amendment, and your suggestions about how it might be made more sound and more useful. We shall then, unless you tell us otherwise, present it to a wider audience.

All comments on the manual should be sent to ILCA marked 'For the attention of the Head of Livestock Economics Division'.





Section 2: Livestock on–farm trials

[Introduction](#)

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Introduction

On-farm trials are considered an integral part of the farming systems research approach to technological design and testing. This is because, to cite the reasons commonly given in literature (Harrington, 1980; Stroud, 1985; Gilbert et al, 1980; Norman and Collinson, 1985), they have:

- ***diagnostic function***

Insights gained during the on-farm trial phase should be confirmed and refined, and further contact with the farmer should broaden knowledge about the system. Our knowledge about resource interactions, the manner in which decisions are made and the role of management at the household level should therefore improve.

- ***prescriptive function***

This function indicates pathways for improvement. During on-farm trials, farmers are involved in technology identification, design and evaluation. This should improve the chances of adoption within each selected target group, if representative participant farmers are chosen.

- ***adaptive function***

"Solutions" developed on-station can be tested and adapted on-farm to determine their relevance to a particular situation. The relevance of technologies used elsewhere (e.g. in other similar agricultural production systems) can also be examined.

The success of any on-farm trial project will depend on a number of factors. In particular, there will be a need for the systems research team to:

- **review continually the approach used and to make an honest assessment of the problems likely to be confronted**
- **be practical in the approach adopted**

Attempts to be excessively systematic in the design of an 'ideal' farming system which has little hope of adoption should be avoided (Dillon and Anderson, 1984). Emphasis will normally be given to the adaptation of existing technologies, not to the design of new ones. When new technologies need to be designed, trials should normally be conducted on-station, not on-farm. There may also be circumstances in which it is more practical to conduct research on-station, rather than by on-farm trials (Part A of Module 2, Section 2).

- **maintain close contact with farmers and incorporate their views at every stage of the process**

This principle is repeatedly stated in the literature (e.g. Tripp, n.d.; Bernsten et al, 1984; Collinson, 1984; Conway, 1984). Agreements between farmers and the research team about management obligations should be clearly defined at the outset.

- **maintain close contact with on-station researchers and know when to utilise station facilities and other sources of available expertise (e.g. extension staff)**

The participation of, and communication with, research institutions and extension agents must be enhanced if the interest in the research conducted and the recommendations made is to be sustained (Butler, 1984).

- **maintain a multi-disciplinary approach to research throughout the process** (Gilbert et al, 1980).

These principles are generally applicable to crop and livestock on-farm trials. There are, however, factors peculiar to livestock on-farm trials which can alter the approach adopted. These factors need to be properly understood if useful results are to be achieved.

For instance, attempts to design livestock on-farm trials for the purposes of statistical analysis will often be fraught with problems because of the difficulty of obtaining large enough samples, or because adequate supervision cannot be assured. In such circumstances, objectives may need to be modified and alternative courses of action considered.

The problems commonly confronted and the manner in which they can be overcome are discussed in this section of the manual. Where appropriate, reference is made to cropping systems literature on the topic, but unnecessary repetition of what has already been written is avoided. The reader is, however, encouraged to use the support material listed. References such as Stroud (1985), CIMMYT (1986), Mutsaers et al (1986) and Steiner (1987) are widely available and particularly useful.

Section 2 comprises three modules:

- **Module 1, *Definitions, problems and initial considerations in planning livestock on-farm trials***

Module 2, Design, implementation, monitoring and evaluation of livestock on-farm trials, and

Module 3, Analysing data from livestock on-farm trials

The principles given in these modules can be applied to any kind of livestock on-farm trial. Considerations specific to feed and animal health on-farm trials are discussed in the appendices to Module 2 (Section 2).

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Section 2 - Module 1: Definitions, problems and initial considerations in planning livestock on-farm trials

[Part A: Definitions and concepts](#)

[Part B: Problems confronted](#)

[Part C: Justifying the need for on-farm trials](#)

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This module defines concepts related to livestock on-farm trials. It also discusses some of the problems which the researcher is likely to confront when livestock on-farm trials are being planned and implemented. It ends by outlining the principles which need to be considered when technologies are being selected for on-farm testing.

Part A: Definitions and concepts

The following terms will be used throughout the remainder of Section 2:

Trial. For the purposes of the discussion in this section, on-farm trials are classified as¹:

- **statistical trials**, in which emphasis is given to obtaining statistically analysable results

Statistical trials are designed along conventional statistical lines in order to facilitate the analysis of results (Module 3, Section 2) With livestock, these kinds of trials require relatively high levels of researcher supervision.

- **monitoring trials**, in which the main intention of the researcher is to observe and monitor farmers' reactions to the technology being tested

With these kinds of trials, farmers control the use of trial inputs and adapt the technology according to their own requirements. The results obtained are not statistically analysable in most cases, but other simple

methods of analysis can be used to assess the potential of the technology for the target area in question (e.g. partial budgeting, cash flow budgeting - see Module 3, Section 2).

¹ The range of possible trial types is large and the terminology used here does not correspond with that used in cropping systems research literature (e.g. Stroud, 1985, pp. 23-26). The categories of trials defined above are based on whether trials are likely to be statistically analyzable or not, since this is of major importance in livestock systems research.

Design. This is a stage in the livestock systems research procedure. In the context of livestock on-farm trials, 'design' is defined as that process which starts with an initial screening of the various technological options available and ends when the suitability of a technology for adoption by farmers has been established. The definition thus goes beyond (but includes) the conventional statistical use of the term². The design process is an iterative process which is likely to involve the need to re-design and re-try adapted versions of the original technology (Gilbert et al, 1980).

Test. A test is an informal study which assesses the practical implications and farmers' reactions to a particular trial.

Target population is the population of animals to which we would like to apply the results of a study.

Sampled population is the set of individual animals which would actually have a chance of being chosen for the purposes of the trial.

Sample is the set of animals selected for the purposes of the trial.

Sampling unit is the unit chosen for study (e.g. individual animals, pens of animals, herds).

Experimental variable is the variable whose effect is being examined in the trial (e.g. the effect of feed supplementation on weight gain).

In order to isolate the effect of the experimental variable, conventional research techniques require that the effects of other influencing factors (non-experimental variables) be held constant. In livestock on-farm trials, it can be difficult to measure and control non-experimental effects (e.g. management effects), and this problem increases as the level of control over the trial by the researcher declines (see pages 24-26 of Module 2, Section 2).

Treatment. The effect of an experimental variable can be examined by altering its level of application or the manner in which it is combined with other experimental variables. Each alteration constitutes a

treatment. Control treatments are used as a standard against which other treatments can be compared (e.g. animals receiving no supplement in a feeding trial) might be used as a 'control' (i.e. as the basis for comparison with those receiving some supplement).

Replication. Each repetition of a treatment is called a replication. In livestock trials, treatments may be repeated at the level of a pen, herd or individual animal. By increasing the number of replications, there is a better chance of detecting real treatment differences.

² It involves sample selection, treatment specification, treatment layout, replication requirements and the methods of analysis used.

Part B: Problems confronted

Most of the problems confronted in on-farm trials stem from the fact that resources available for research are limited. In particular this affects:

- **the supervisory capacity of the research team**

i.e. the capacity of the team to supervise adequately the farmers and enumerators taking part in the trial so that the data collected are useful.

- **sample size**

i.e. the ability of the team to obtain a large enough sample to ensure that statistical analysis of the results is meaningful.

Difficulties with respect to supervision and statistical analysis of the results obtained tend to be more pronounced with livestock than they are with crops, and the reasons for this are discussed in the literature (Bernsten et al, 1984; Sands et al, 1984; Gryseels, 1986; McIntire 1986). In brief, they relate to:

- **animal mobility**

In systems where livestock are highly mobile or where the quality of grazing resources is highly variable, treatment effects can be confounded by the effects of environmental variation. The more mobile and extensive the system, the more difficult it is to account for these sources of variation (McIntire 1986). To compensate, large samples are needed, but the supervision time required and the costs involved may be prohibitive. Options for technological improvement are also likely to be more limited in extensive production systems.

- **lifespan**

The lifespan of ruminant animals is relatively long.

Research aimed at improving breed performance therefore tends to be costly and beyond the scope of most systems research teams. Farmers are also inclined to lose interest in trials which continue for prolonged periods and require more supervision as a result. The risk that experimental animals will die or be sold also increases with the length of the trial period.

- ***life-cycle synchronisation***

Blocking sample animals into uniform classes on the basis of age, sex, weight and parity should always be attempted. This removes potential sources of variation between different sample animals and improves the chances of detecting treatment effects. However, in practice it can be extremely difficult to find large enough samples to 'block' animals in this way, which, in turn, imposes limitations on the ability of the researcher to obtain statistically analysable results (Module 2, Section 2).

- ***producers' attitudes to livestock***

Producers may be unwilling to divulge information about the livestock they own or hold and to participate in trials because of the high value they attach to individual animals (particularly cattle). When the herds or flocks owned or held are small, the inconvenience (and risk) associated with participation in trials may also be considered too great to warrant the effort. When animals are jointly owned or held or managed, further complications arise. Full commitment is, however, a pre-requisite to the success of on-farm trials.

- ***management variability***

Producers vary in their ability to manage livestock, and this can confound trial results. Blocking trials on the basis of differences in management can overcome this problem if sufficient animals within the same age, sex and productive class can be obtained on each farm for each treatment. When this is not possible, similar animals have to be obtained from many more farmers. Problems of supervision then tend to increase. The more widely dispersed the population, the more difficult it is to supervise the trial.

- ***communal land tenure systems***

Communal grazing can pose problems in the measurement of the effects of non-experimental variables (e.g. grazing resources and disease). It also limits the potential for certain types of technological improvement (e.g. breed and pasture improvement).

- **multiple outputs**

Livestock produce multiple outputs (meat, milk, draught, manure and skins) and some of these can be difficult to measure and value. This sometimes complicates data collection.

The implications of each of these problems are discussed in greater detail in Module 2 of this section.

Part C: Justifying the need for on-farm trials

Justifying the need for on-farm research requires consideration of essentially three issues:

- **the relevance of technology to the problems identified**
- **the appropriateness of on-farm trials, and**
- **the practicality of on-farm trials.**

The relevance of technology to the problems identified

Section 1 of this manual emphasised that correct description and diagnosis of the system is critical to the success of any on-farm trial project. It showed that diagnostic research should be directed towards the identification of 'pathways' for the improvement of production and income and that such improvements may come as a result of policy and of infrastructural, institutional or technological change.

On-farm trials may be justified if a technological solution by itself is considered appropriate to the problem(s) identified. In other circumstances, a new policy or an institutional reform may be required before technological solutions are considered (Caldwell, 1984).

The appropriateness of on-farm trials

Having identified the need for a technological solution, the need for on-farm trial work should then be definitely established. In some circumstances, on-station research may be sufficient or all that is possible. This would be true, for instance, if the problem(s) identified required long-term research unsuited to on-farm trial work (see part B above), or if technology suitable for on-farm adoption had not been developed.

There may be other situations in which **exploratory on-farm trials** are justified. Such trials are essentially diagnostic in intention, the aim being to obtain a better understanding of the system by more intensive contact with farmers. They are likely to be used by researchers who, although convinced of the need for technological change, "are still searching for an innovation to try" (Stroud, 1985). Exploratory trials are not discussed in this section.

The practicality of on-farm trials

In certain circumstances, on-farm research directed towards statistical analysis of

the results may be justifiable in theory but impractical, because it is too difficult to obtain a large enough sample or because adequate supervision cannot be assured. Alternative courses of action should then be considered (Module 2, Section 2), rather than attempting to conduct on-farm trials.

The research team may not be able to assess the need for and the practicality of on-farm testing immediately after the diagnostic phase. Some issues may only be properly clarified at the design stage, while others may require a preliminary testing stage. A flexible attitude and approach is therefore required at all stages of the research procedure.

Part D: Choosing the technology to test: the screening process

If a technological approach to the problem(s) identified is justified, it is necessary to **screen** the various options which exist and choose from them the technologies most relevant to the situation which we seek to ameliorate. In many cases, few (or no) options will be available. In others, a range of possibilities may exist and those most appropriate to on-farm testing will need to be selected.

Screening is essentially an on-going process of technology selection directed towards reducing the risk of negative or unproductive research results (Bernsten et al, 1984). It begins by considering each of the following issues:

- **the nature of the problem(s) confronted**
- **the technological options available to meet the problem(s) identified, and**
- **the technological options appropriate to the problem(s) identified.**

The nature of the problems confronted

The diagnostic techniques described in Section 1 and exploratory trials should give the researcher a reasonably good idea of the constraints limiting production and income in the target area. However, this is not enough: the nature and causes of the problems identified must be specified, by asking the following questions:

- Which particular production parameters are affected (e.g. mortality rates, reproduction rates, growth rates)?
- Which animal species and classes within each species are affected?
- What specifically causes the problem?

For instance, if the problem is a disease problem, we will need to identify the determinants of the disease (Module 8, Section 1). If it is a problem related to the nutritive value of the feed, we will need to determine which components of the diet (energy, protein or minerals) warrant particular attention (Module 7, Section 1). If it is a management problem, then we will need to find out which aspects of management require attention -watering and

herding practices, for instance (Module 10, Section 1).

Available technological options

When the nature and the causes of the problems identified have been specified, the next step is to identify the technological options which are available to solve the problems. Some options may need to be tested in on-farm trials, others may not (see below).

For instance, if labour is identified as the factor limiting watering frequency and hence production in a pastoral system (Module 11, Section 1), the options available for technological improvement (e.g. more evenly distributed water points) are likely to be fairly limited, offering little scope for trial work within the community.

For instance, if seasonal feed shortages are identified as limiting in a sedentary system, a wide range of options is likely to be available (e.g. various feed supplementation strategies, crop improvement strategies to increase stover production, pasture improvement schemes) (Module 7, Section 1) and the scope for on-farm testing is also likely to be greater.

Obtaining information about the various options available can be more difficult than expected. To overcome this problem, as many sources of information as possible should be used, including:

- ***farmers/pastoralists***

If consulted, farmers and pastoralists will often provide useful information about the nature of their production constraints and the solutions they consider appropriate. Their opinions should always be given full consideration but should be cross-checked against the opinions of others (e.g. extension officers).

- ***between and within-system comparisons***

An examination of the management practices adopted in the target area may point to potential solutions. Similarly, comparisons between similar systems might indicate suitable options for improvement (Shaner et al, 1982). For instance, if lambing percentages in one production system are consistently lower than those in an adjacent but similar production system, reasons for these differences should be examined. Producers in the latter area may, for instance, time the mating of ewes to coincide better with seasonally available feed supplies.

- ***other sources***, such as non-governmental organisations, extension officers and researchers, and

- ***secondary data sources*** (Module 1, Section 1).

Appropriate technological options

While the constraints confronted by small farmers and pastoralists are often similar (e.g. in terms of animal nutrition or health), the relevant solutions are often 'site-specific' because of cultural, institutional, economic, infrastructural and political influences. Available technologies should, therefore, be screened, using the following criteria (Peterson and Hayani, 1977; Dillon, 1979; Zandstra, 1980; De Boer, 1982):

- ***potential costs and benefits***

Low-cost innovations which generate obvious cash returns within a relatively short time period are likely to be attractive. If access to credit enables the farmer to 'spread' the capital costs of an investment (in equipment or livestock), longer-term options may also be attractive.

- ***Farmers' objectives/perspectives***

Farmers' and researchers' perceptions about the value of an innovation can differ quite markedly. Farmers, for instance, may be more interested in the survival of their livestock than in improved calving performance, milk production or weight gain, and this may affect their reactions to a technology and the manner in which it is applied (Behnke, 1984; Waters-Bayer and Bayer, 1987).

- ***risk***

Farmers will not adopt a technology if the risks involved are considered too great (Dillon and Scandizzo, 1978; Anderson et al, 1985).

- ***interactions***

Production decisions are influenced by various exogenous and endogenous factors which need to be carefully considered (see Parts A and B of this module).

- ***accessibility/availability of inputs***

Technologies requiring the use of purchased inputs will be inappropriate if those inputs are not available or accessible on a continuing basis.

In addition, the **broader social implications** of adoption should be considered on the basis of:

- ***equity***

For instance, what effect will adoption have on the distribution of income in the target area?

- **gender**

For instance, what effect will the adoption of a technology have on labour inputs required from women and on the distribution of benefits within the family. Will adoption be practical and will the overall welfare effects within the family be positive?

- **environment**

What impact will adoption have on the environment (e.g. in terms of range stability and sustainability)? (Module 6, Section 1) (Conway, 1985).

- **replicability**

For instance, is the technology likely to be applicable outside the target group? Within the group itself, will it be widely adopted? (Norman and Collinson, 1985).

A **decision matrix** can be a useful means of checking technologies against such criteria (Steiner, 1987). An example is given below. Module 1 in Section 1 describes an alternative but complementary approach suggested by CIMMYT (1986) for the screening of technological options.

Example: Assume there are three technological options available (A, B and C). Rate these in a decision matrix on the basis of the criteria given above using the symbols:

- (+) = fair to good
- (-) = poor
- (*) = fairly certain of effect
- (?) = uncertain of effect

Decision matrix

Criterion	Technological option		
	A	B	C
Costs/benefits	-/*	+/*	+/*
Farmers' objectives	-/*	+/?	+/?
Risk	-/*	+/*	+/*
Interactions	+/?	+/?	+/?
Accessibility/availability	-/?	+/*	+/*
Equity	-/*	+/?	+/?
Gender	+/?	-/*	-/?
Environment	+/*	-/*	+/*
Replicability	-/*	+/?	+/?

Of the three options considered available, option A is obviously inappropriate on the basis of the criteria used. Options B and C are

very similar but option B would be ruled out on the grounds of environment and gender. With option C, uncertainty about farmers' objectives, on-farm resource interactions and equity effects would strongly suggest the need for on-farm trials.

After screening, it is important to decide whether an on-farm trial approach is relevant to the circumstances. Technologies identified as 'appropriate' will normally need to be adapted or refined before wide adoption can be envisaged (e.g. Gilbert et al, 1980; Norman and Collinson, 1985; Mutsaers et al, 1986). This may involve the need for on-farm trials or on-station research work.

If on-farm trials are considered relevant to the circumstances, the research team will need to decide whether 'statistical' or 'monitoring' trials should be carried out. Module 2 (Section 2) outlines the factors which need to be considered when this is done. It also suggests when on-station research is perhaps the more relevant approach to adopt.

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Section 2 - Module 2: Design, implementation, monitoring and evaluation of livestock on-farm trials

[Part A: The role of on-station research](#)

[Part B: On-farm trials](#)

[Part C: Implementation](#)

[Part D: Evaluation and re-design](#)

[Appendix 1: On-farm feeding trials: Additional considerations](#)

[Appendix 2: On-farm animal health trials: Additional considerations](#)

[References](#)

In this module, the role of on-station research in the design and adaptation of technologies relevant to the target group has been emphasised. The discussion below focuses on the basic principles which need to be understood when livestock on-farm trials are being designed, implemented and evaluated. Additional considerations specific to on-farm feeding and animal health trials are discussed in Appendix 1 and 2 to this module.

Part A: The role of on-station research

On-station research plays an important role in technological development and evaluation, and access to research-station facilities and personnel will often decide the ultimate success of an on-farm trial project. There may be situations when livestock on-station research will be more practical and relevant to the prevailing circumstances than livestock on-farm research. This may be the case when:

- the circumstances favour rapid adoption of technologies developed on-station
- new technologies need to be developed before on-farm testing
- technical relationships need to be clearly understood
- long periods are required to develop technologies relevant to the target area, and
- the research team has insufficient resources to guarantee the implementation of useful on-farm trials.

Adoption of technologies without on-farm testing. There have been instances in sub-Saharan Africa when conditions were amenable to a rapid adoption of technologies developed on-station without carrying out on-farm trials. Examples of

such technologies are zero grazing of grade dairy cattle in the high-potential areas of Kenya (Kenya Government, 1985) and the introduction of crossbred cattle for dairying in Malawi (Agyemang and Nkhonjera, 1986).

For instance, the rapid adoption of zero grazing in the high potential areas of Kenya might have been expected because:

- land was in limited supply
- small-scale dairying was well established within the region
- grade dairy animals were readily available' and
- formal and informal market outlets for milk were well developed.

This is not to decry the value of on-farm trials. It simply demonstrates the need to take full account of the situation before moving forward to implement an on-farm trial project. A thorough understanding of the characteristics of the system could lead to the identification of technologies which need not be tested on-farm before they are widely adopted.

Development of new technology before on-farm testing. Technologies may not always be available-for adaptation within the system under study, and an initial period of on-station research may thus be needed to develop (or further screen) innovations which are **broadly applicable** to the problems identified during diagnosis (Gilbert et al, 1980).

However, it is not always possible to tailor this kind of research to the requirements of a particular target group, and on-farm trials to test the applicability of the technology (or technologies) developed will probably be required at a later date.

In these circumstances, the systems research team should maintain close contact with station researchers to ensure that all the important issues (e.g. constraints) are properly considered. The value of the on-station research will depend largely on the extent to which experiments are designed to correspond to smallholders' conditions within the target area.

The following general principles taken from Partenheimer (1983) should be borne in mind when advising on the design of on-station trials intended to help traditional production systems:

- Where possible, trials should be designed with a well defined group of farmers in mind and based on a particular constraint which had been clearly defined by diagnostic research.
- The experimental variables used in on-station trials should be at levels attainable by the target group.
- Non-experimental or 'fixed' variables should, where possible, be held at levels which correspond to those found on farms. With livestock it will often be very difficult to achieve this, particularly where more extensive production systems are involved.

Clear understanding of technical relationships. Relationships of a complex technical nature are best researched under station conditions where adequate control over both experimental and non-experimental variables can be ensured.

For instance, in area X, on-far. supplementary feeding significantly improved cattle liveweight gains, but researchers were unable to determine whether the improvement resulted from the addition of energy or protein to the diet.

Since it would have been extremely difficult to obtain the level of control required to determine the independent effects and interactions between the two factors, on-station trials under controlled conditions were conducted. After isolating the important determinants affecting weight gain, the supplement was altered and responses to the new ration were again tested on-farm.

The same reasoning applies to experiments which aim to show how output (e.g. milk production) changes as the amount of an input (e.g. concentrate)¹ changes. From such experiments, it is often possible to approximate input requirements for trials conducted on-farm. On-station research can, therefore, play an important complementary role to on-farm research in refining technologies to suit farmers' requirements.

¹ In literature on farming systems research, such trials are known as 'levels' trials.

Development of technologies relevant to the target area. Certain types of livestock technology require long periods of research before their relevance can be ascertained (e.g. breed improvement schemes). Research of this kind is unsuited for on-farm testing for the reasons discussed above under 'Development of new technology before on-farm testing'.

Insufficient resources. Even if on-farm trials are considered necessary, there will be little point in conducting them if the livestock systems research team cannot ensure useful results because the resources available are inadequate to supervise farmers and to collect meaningful data. On-station trials which attempt to simulate farm conditions as nearly as possible may, in these circumstances, be the next best option.

Part B: On-farm trials

[Initial considerations](#)
[Statistical trials](#)
[Monitoring trials](#)

On-farm research will, nevertheless, be justified in many circumstances. There are numerous reasons given in the literature and some of these are listed in the 'Introduction' to this section.

The need to maintain close links with on-station research facilities and to draw on all sources of available expertise during on-farm trials is also emphasised in the literature (e.g. Harrington, 1980; Gilbert et al, 1980; Stroud, 1985; Norman and Collinson, 1985; von Kaufmann, 1986). It can, however, be quite difficult to

develop and maintain these links, and formal mechanisms to strengthen collaborative efforts between on-station and on-farm researchers are required. A detailed discussion of what this implies in practice is given by Merrill-Sands and McAllister (1988).

Initial considerations

By working through the specific objectives and requirements of a trial before its implementation, initial designs can often be adjusted to account for difficulties which might arise. They should never be so rigid as to prevent adjustments being made even during implementation.

Irrespective of the type of on-farm trial being envisaged, one should begin by carefully considering:

- **objectives of the research**
- **resources**
- **management and supervision, and**
- **selection of trial participants.**

Objectives. Research without a clearly defined set of objectives is likely to be wasteful in terms of resources and to result in data collection which has no specific purpose. For any trial, one should specify the distinct phases into which the trial can be divided, the types of data to be collected, the frequency of data collection, the levels of researcher/farmer involvement, and the methods of analysis in each phase. One should also be clear about what constitutes the 'experimental' or 'observation units'. Such units could, for instance, be individual animals, groups of animals, plots of land, farmers or households.

Resources. To a large extent, trial objectives will be determined by the financial and manpower resources available and the ability of the livestock systems research team to access a research station and mobilise the support of extension officers on a continuing basis. Requirements such as trial equipment, vehicles, laboratory and computer facilities may not be easily met, and this can also influence the trial approach adopted (von Kaufmann, 1986).

Management and supervision. The responsibilities of the researcher and the participating farmers as regards trial management must be clearly defined. Before the trial starts, farmers must be made fully aware of their obligations and agree to abide by the conditions set.

In practice, however, it is extremely difficult to bind farmers to agreements like this, particularly when livestock are concerned. Thus, while it may be considered desirable to implement a 'statistical' trial (see page 7), attempts to do so will be futile if adequate supervision cannot be assured. In such cases, 'monitoring' trials (see page 26) may have to be implemented instead. Alternatively, it may be better to abandon attempts to conduct on-farm trials of any sort.

For the purposes of this module, three levels of supervision and farmer involvement are considered relevant (von Kaufmann, 1983). The trials are classified accordingly as:

- **Researcher-managed/researcher-executed trials** in which the

researcher controls the application of trial inputs (i.e. such experimental variables as feed or veterinary inputs), while the farmer controls other factors affecting animal performance (i.e. non-experimental variables, such as grazing time and watering frequency). Superimposed trials and cohort epidemiological studies fall under this category.

In *superimposed trials*, the researcher adopts the farmer's management practices and adds an input or alters the system otherwise (Stroud, 1985). Trials like this are kept fairly simple and are used when the researcher wants to evaluate the effect of an input under normal management conditions.

In *cohort epidemiological studies*, the animals selected for the study are assigned to two or more groups or cohorts and subjected to different treatments (e.g. vaccination or dipping or a combination of both).

In cohort studies, one group is always kept free and used as a control for the purposes of comparison (Putt et al, 1987) (Module 8, Section 1).

- **Researcher-managed/farmer-executed trials** in which the farmer administers the experimental inputs as prescribed by the researcher and controls all other factors related to livestock management.
- **Farmer-managed/farmer-executed trials** in which the farmer manages all experimental inputs in the manner in which he or she sees fit and the researcher observes the manner in which the technology is applied. In terms of the terminology used elsewhere in this module, such trials would usually be classified as 'monitoring' trials.

Selection of trial participants. This involves two steps:

- selection of households within the target area, and
- selection of members within those households that will actually implement the trial.

With the first, the households selected should be as representative of the area as possible if the technology is to have wide acceptance (Gilbert et al, 1980).² With the second, both the decision-maker and the user of the technology should be involved (where possible) in the testing process, if the implications of the on-farm trial are to be properly understood.

² Some authors (e.g Barlow et al, 1986) counter this view, advocating that progressive farmers are general!, preferred when selecting trial participants "since vest adoption studies show that less advanced farmers follow the technological lead of their sore advanced colleagues". Other authors (e.g. Sidahmed et al, 1985) suggest the use of farmers who have already participated in baseline or diagnostic surveys, because they will understand the purpose of the study and are, therefore, more likely to be willing to participate.

In many African societies, investment decisions related to technological adoption are made by men even when women are ultimately responsible for the manner in

which the technology is applied. In such cases, attempts should be made to involve both parties in the trial. Failure to do so could mean that important aspects affecting a household's ability to adopt a technology (e.g. the availability of female labour) will not be adequately accounted for.

It may, however, be difficult to identify the actual decision-maker and to ensure his or her involvement in the trial.

For instance, in countries where out-migration of males for urban or mine employment is common (e.g. Swaziland, Lesotho, Botswana, Mozambique), it is often difficult to know whether the woman has de facto decision-making rights or whether the man, though absent, makes the final choice about technological adoption or even the manner in which a new technology will be applied.

Furthermore, it cannot be assumed that technical information will be passed between household members in the manner desired by the researcher.

For instance, men offering to participate in a goat-feeding trial in western Kenya often failed to pass on information to their wives who bore ultimate responsibility for the management of smallstock (Sidahmed et al, 1985).

Statistical trials

The practicality of conducting on-farm statistical trials will mainly be determined by the ability of the research team to:

- obtain large enough samples which would enable meaningful statistical conclusions, and
- supervise farmers adequately so that treatment effects are not distorted and the collected data are accurate.

Each of these will, in turn, depend on such things as:

- the characteristics of the system being studied (e.g. sedentary versus pastoral)
- the characteristics of the variable being measured (e.g. its inherent variability)
- the complexity of the trial envisaged (e.g. the number of treatments per farm and the possible interactions between them)
- the resources available to the livestock systems research team, and
- the cooperation of the farmers involved.

All these are reflected in the three basic aspects influencing statistical on-farm trials with livestock, namely:

- **determination of sample size**
- **specification of treatment characteristics, and**
- **farmers' behaviour.**

Determination of sample size: Theoretical aspects

When determining sample size, it is necessary to distinguish between continuous and discrete data.

For *continuous variables* (e.g. weight, condition, milk production), the record obtained can take on a range of values.

For *discrete variables*, the record obtained can only take on one of two values (or a small number of values).

For instance, when mortalities are measured, the sample animal either lives or dies. Similarly, when reproduction is being measured, the animal either reproduces or fails to reproduce. If technology adoption is being measured, a farmer may adopt the technology or he may not

In general, much larger sample sizes are required when studying discrete variables. The question of sample sizes for discrete data is discussed on page 16.

CONTINUOUS VARIABLES. The sample size required for a trial with continuous variables will depend on two quantities:

- **the required precision of the experiment**

i.e. what size of difference should the experiment be able to detect, as measured by the least significant difference.

- **the inherent variability in the experimental material due to unknown or uncontrollable factors (random variation)**

which is measured by the standard deviation or the coefficient of variation.

Least significant difference (LSD). This is defined as the smallest difference between two treatment means which is statistically significant. This difference can be expressed either in absolute terms (i.e. in the same units as the relevant measurement) or in relative (percentage) terms³. The smaller the difference the researcher wishes to detect, the larger the experiment will need to be.

³ For the purposes of this module, differences will be expressed in relative terms.

Coefficient of variation (CV). This is a measure of the variability of the experimental units expressed in percentage terms (Module 11, Section 1). To estimate the required sample size corresponding to a given least significant difference, the coefficient of variation for the particular variable being measured (e.g. daily liveweight gain) will need to be determined. Results from previous experiments in the target area may provide the necessary data. Alternatively,

guesstimates based on research done elsewhere may be used. The larger the coefficient of variation, the larger the trial will need to be to detect a given difference.

The coefficients of variation will vary considerably, depending on:

- **the variable being studied**

For instance, milk yield per lactation say have & higher coefficient of variation than birth weight.

- **the type of experimental unit**

For instance, the coefficients of variation for mature sheep will be different from those for calves.

- **the selection criteria used**

For instance, if a trial is restricted to, say, animals of a given age, breed and sex, the coefficient of variation will be lower than if the trial included Dale and female animals of many ages and breeds. Also, if a study is limited to households with a given number of adults and children (and animals), the coefficients for the various measurements will be lower than for a study involving a cross-section of households.

- **the design of the experiment**, in particular, whether effective 'blocking' is used.

Table 1 gives coefficients of variation for selected production performance variables for cattle in sub-Saharan Africa.

Table 1. Coefficients of variation (CV) for selected cattle productivity variables.

Variable	Mean CV (%)	Range of CV	Number of estimates
Weight at			
birth	15	12-17	12
90 days	17	11-20	9
180 days	18	16-20	6
270 days	18	18-19	4
1 year	18	16-19	6
2 years	14	9-20	5
3 years	11	9-13	3
>3 years	13	10-20	5
weaning	11	8-13	3
Daily weight gain to 1 year	32	14-49	5
Age first calving	14	8-17	18

Calving interval	24	9-35	11
Lactation length	29	16-36	6
Extracted milk yield/lactation	41	24-52	5
Total milk yield/lactation	35	22-51	3
Productivity index	45	26-76	6

Table 2 gives production coefficients for small ruminants in two African countries (Mali and Sudan). The figures are indicative of what would be expected for small ruminants elsewhere in Africa.

Table 2. Coefficients of variation (CV) for selected productivity variables for sheep (Sudan) and goats (Mali).

Sheep		Goats	
Variable	CV (%)	Variable	CV (%)
Weight at		Weight at	
birth	18	birth	27
30 days	22	30 days	22
120 days (weaning)	20	150 days (weaning)	20
Age at			
1st conception	18	Parturition interval	34
1st lambing	14	Litter size	32
1st parturition	31	Annual reproduction rate	34
Litter size	33		
Lambing interval	35		
Annual reproduction rate	47		

Calculation of sample size for continuous variables. The formula, in percentage terms, for a least significant difference (d) is:

$$d = t \cdot CV \cdot \sqrt{(2/n)}$$

where

n = the number of experimental units per group, and

t = the tabulated t-value. For reasonable sample sizes (total number of units >20), t is approximately 2.0 at the 5% significance level.

If a guesstimate of the CV is available, and the desired least significant difference (d) is specified by the researcher, then the needed sample size (n) can be calculated as:

$$n = 2 \times (t \cdot CV/d)^2$$

For testing at the 5% level, t is approximately equal to 2, and then:

$$n = 2 \times (2 \text{ CV}/d)^2$$

which is the same as

$$n = 8 \times (\text{CV}/d)^2$$

Example: A researcher wishes to conduct a trial to determine the effect of two dry-season feed supplements on the weights of male calves at 90 days of age. He wishes to detect a difference of 10% between the two feeds (using a significance test at the 5% level). The coefficient of variation for weight gain in similar animals obtained from previous experiments in the area is 20%. Estimate the sample size required to detect the specified difference.

If difference (d) = 10%
CV = 20%, and
t = 2.0 (approx.)

Then

$$n = 2 \times (2.0 \times 20/10)^2 = 2 \times 16 = 32$$

i.e. 32 animals would be needed in each feed group. With two feed treatments, this would give a total of 64 calves required to detect a 10% difference.

The probability of detecting a difference needs to be considered in this context.

The formula given for difference $(d) = t \cdot \text{CV} \cdot \sqrt{(2/n)}$ calculates the least significant difference (d) for a given CV and experiment size (n). This is the smallest difference between treatment means in the experiment which will be declared statistically significant. The experimental treatment means are, however, only estimates of the 'true' means, and the experimental difference is only an estimate of the 'true' difference.

For any unbiased experiments, 50% of the experiments will overestimate the true difference and 50% will underestimate it.

For instance, if the 'true' difference between two treatments is 10%, and if a large number of identical experiments were carried out, then half the experiments will estimate the difference as less than 10%. If an experiment is large enough to detect a 10% difference, and the true difference is, in fact, 10%, then there is only a 50% chance that the experimental difference will be declared statistically significant,

Most researchers would not be satisfied with an experiment which has only a 50% chance of detecting a difference. The formula for the needed sample size (page 11) can easily be modified to increase this probability. This is done when

$n = 8 \times (\text{CV}/d)^2$ is generalised to

$$n = k \cdot (\text{CV}/d)^2$$

where the value of k is determined by both the significance level for statistical testing, and the probability of detecting a difference. Table 3 gives a range of values of k.

Table 3. Values of the constant k in the formula¹ for sample size estimation.

Probability of detecting a difference	Significance level		
	1%	5%	10%
50%	13	8	5
80%	23	16	12
90%	30	21	17
95%	36	26	22

$$^1 n = k.(CV/d)^2.$$

Example: The coefficient of variation is 20% and difference (d) is 10%, i.e. we wish to detect a difference of 10% between two treatments. If we carry out significance tests at the 5% level and wish to have an 80% chance of detecting a difference when the 'true' difference is d, then the value of k from Table 3 is 16 and the number of experimental units required per treatment is:

$$n = k.(CV/d)^2 = 16 \times (20/10)^2 = 64$$

Figures 1 and 2 relate sample size to the coefficient of variation and difference (d). When significance tests are done at the 5% level, for instance, Figure 1 shows an 80% chance of detecting a difference and Figure 2 a 90% chance.

Figure 1. Sample size required per treatment for an 80% probability of detecting a difference at the 5% level.

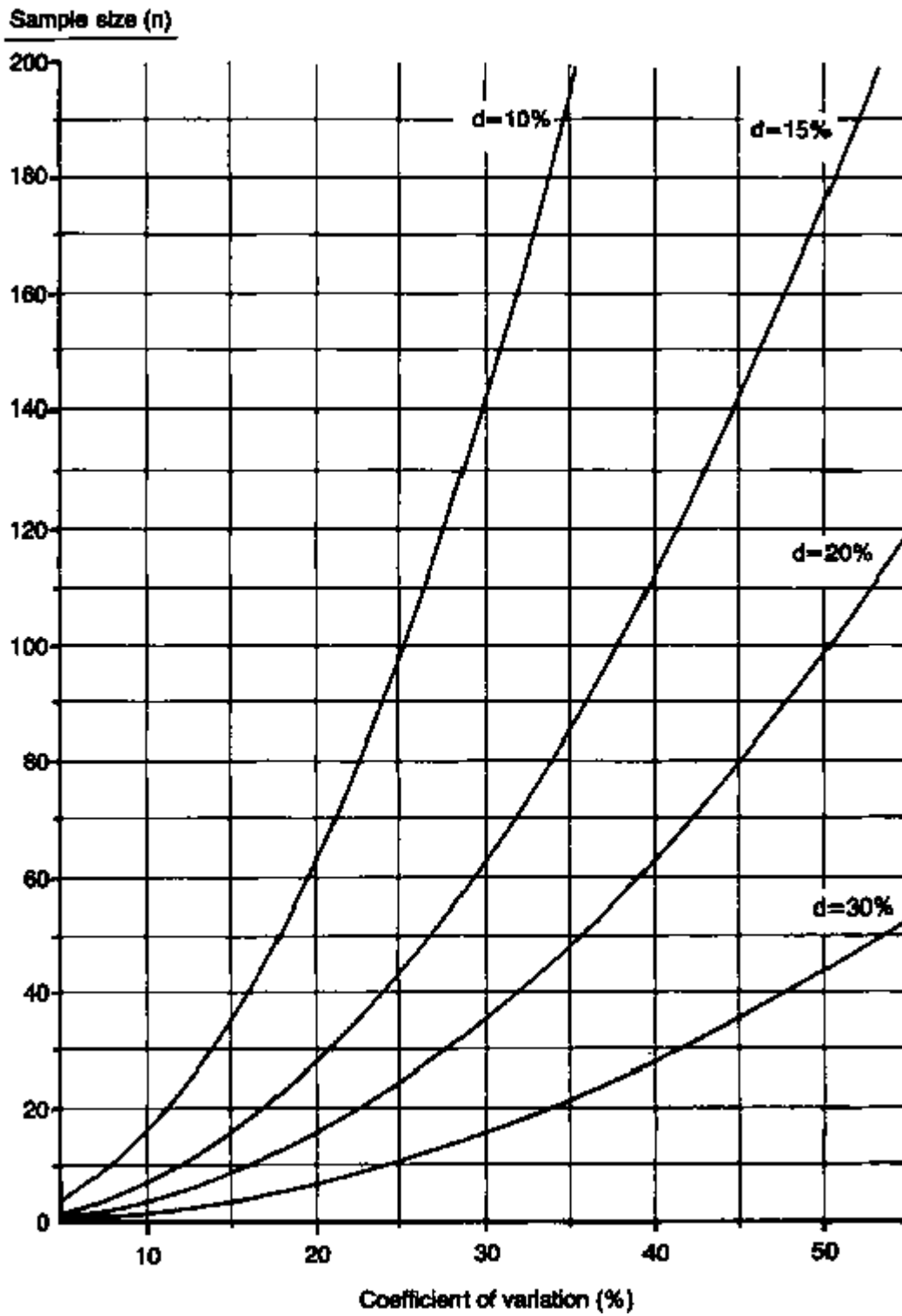
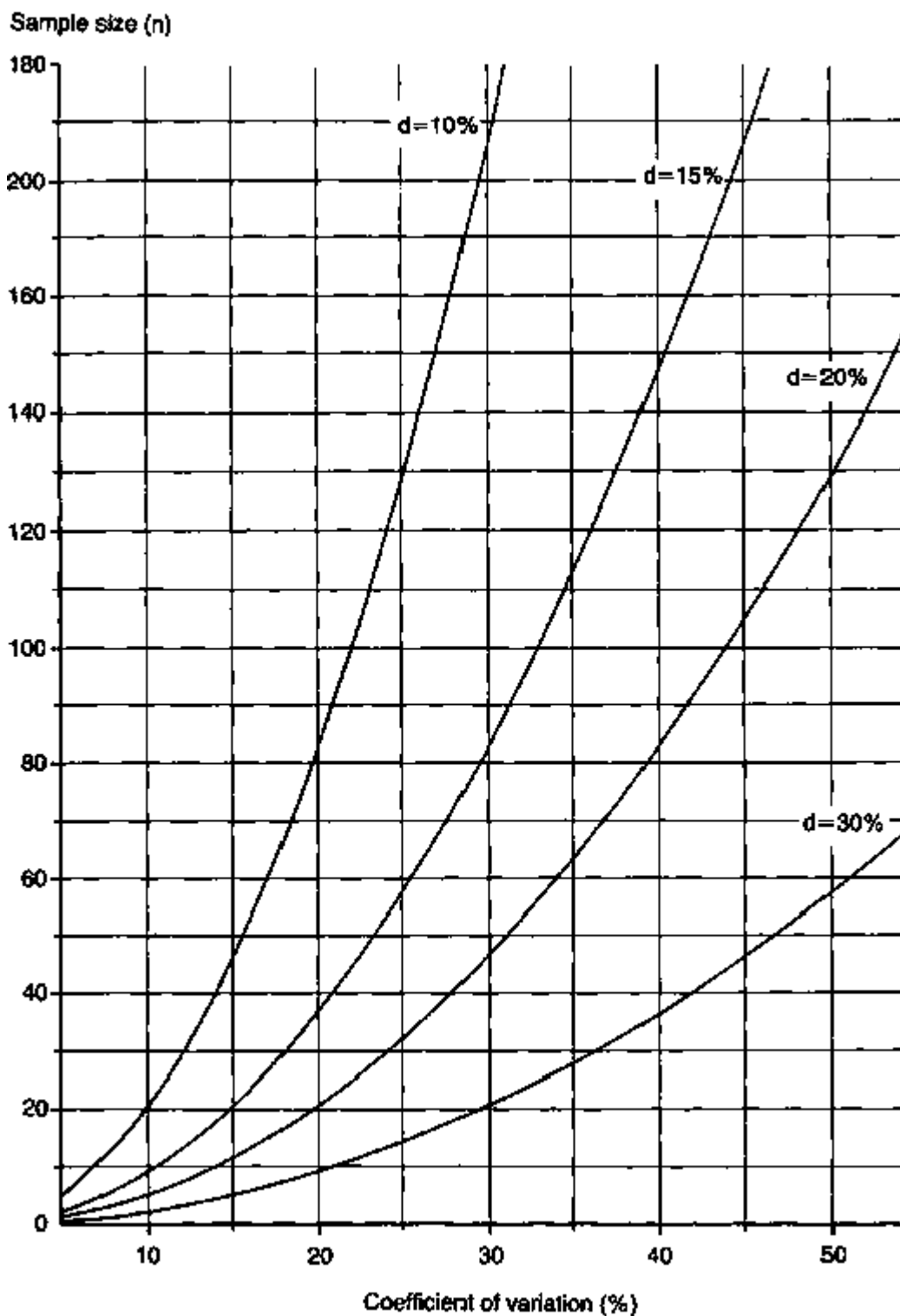


Figure 2. Sample size required per treatment for a 90% probability of detecting a difference at the 5% level.



Example: Suppose the coefficient of variation is 25% and we want the trial we are designing to have an 80% chance of detecting a difference between treatments of 15%. The correct figure to use in this case is Figure 1. Locate the coefficient of variation of 25% on the bottom axis of the figure and then follow the vertical line until it reaches the curve for $d = 15\%$. The samples size (read from the vertical axis) is about 45.

DISCRETE VARIABLES. Similar considerations apply when determining sample

size for discrete data. For instance, when comparing the effect of two treatments on calf mortality, the researcher will have to specify the size of the difference to be detected and he will have to guesstimate the likely level of mortality.

The calculation of the sample size for discrete variables depends on two proportions:

- P_1 , which is the proportion for treatment (group) 1, and
- P_2 , which is the proportion for treatment 2.

Example. We wish to detect a difference between two groups of animals which have mortality rates of 20% (P_1) and 40% (P_2), respectively. An approximate formula for calculating the minimum needed sample size is:

$$n = \frac{k \cdot [P_1(100 - P_1) + P_2(100 - P_2)]}{2 \cdot (P_1 - P_2)^2}$$

where k is taken from Table 3.

If we wish to have an 80% chance of detecting a difference between the 20% and 40% mortality rates (at the 5% significance level), the required sample size per treatment group is:

$$n = \frac{16 \times [(20 \times 80) + (40 \times 60)]}{(2 \times 20)^2} = 80$$

Thus we will need at least 80 animals per group to detect the difference in mortalities at the specified 5% significance level.

REDUCING SAMPLE SIZE. Each of the factors used to calculate sample size offers some scope for manipulation if the size of the sample needs to be reduced. This is particularly true about the level of precision required and the coefficient of variation for the measured variable.

Lever of precision. Factors such as the chosen least significant difference, the required level of statistical significance and the probability of detecting a difference are all a matter of choice. By opting for lower levels of precision (and accepting a greater risk that the difference wanted will not be detected), the researcher will be able to reduce sample size.

In most cases, larger sample sizes imply greater difficulties of supervision and hence, higher costs in terms of resource use. Any trade-off between precision and cost should always be balanced against the original objectives of the research project. With livestock on-farm trials in Africa, where control over non-experimental factors tends to be very difficult, high levels of precision will generally be impractical.

The coefficient of variation can be manipulated by:

- experimenting only with variables which have inherently lower coefficients of variation

- strict selection of experimental units
- 'blocking' experimental units, and
- data adjustment.

Variables with low coefficients of variation. For continuous variables, coefficients of variation differ markedly according to the type of variable being measured. For instance, the coefficients of variation for cattle weights at different ages tend to be much lower than those obtained for variables such as lactation length, milk production and calving interval (see Table 1).

This suggests that on-farm statistical trials may need to be confined to those variables whose coefficients of variation are relatively low. If we plan to experiment with variables which have high coefficients of variation (e.g. milk production), then we will probably have to accept either larger trials or lower levels of precision.

Selection of experimental units. The coefficients of variation of the data can be reduced by selecting only a narrow range of experimental units.

For instance, animals of only a particular breed, age and sex may be selected, or only households with a certain number of animals may be eligible for inclusion. Since these experimental units are deliberately chosen to be reasonably homogeneous, the measured characteristics should have less variability than in wider groups.

The drawback of this approach is that the results of the study will have a more limited interpretation, since they apply only to the kind of units chosen (which may not be representative of the whole population). Sometimes it is necessary to make a choice between precise results which can be applied to a limited section of the target population, or vaguer results with wider applicability.

Blocking experimental units. Blocking means grouping experimental units on the basis of important characteristics (e.g. sex, breed, weight, household size). The number of experimental units (e.g. animals or households) per block should, ideally, be equal to the number of treatments. Each treatment is then allocated to one unit in each group. This reduces inter-unit variation within a block and lowers the coefficient of variation accordingly (see 'Analysis of variance for a randomised block design' in Module 3 of this section).

Practical problems of blocking are:

- the availability of relevant information at the design stage of the trial
- the practical difficulty in the field to allocate animals or households to blocks and treatment groups, and
- the difficulty which may sometimes occur in obtaining the correct number of units per block.

If the number of animals or households per block does not equal the number of treatments, the statistical analysis becomes more complicated.

The use of the technique to remove sources of variation is discussed in detail on

pages 24 - 25.

Data adjustment. Even if blocking is used, there will be other sources of variation which may not be easily accounted for during experimental design. Covariance analysis⁴ can sometimes be used to remove the effects of these factors at the data analysis stage, thus increasing the precision of the results obtained.

⁴ Covariance analysis is not discussed in this manual, Statistical references which deal with the analysis in detail are Cochran and Cox (1957), Snedecor and Cochran (1961) and Mead and Curnow (1983).

For instance, blocking ewes on the basis of parity and breed in a feeding trial would remove two sources of potential variation. If weight at the end of the trial is also affected by initial weight, blocking on the basis of initial weight would remove yet another source of variation.

This is likely to be impractical in a typical African setting, since, normally, it would be extremely difficult to obtain enough animals of the same breed, weight and parity in each treatment. Alternatively, treatment means could be adjusted using covariance analysis, to take account of the effects of initial weight after the trial has been conducted.

Determination of sample size: Practical aspects

In practice, the ability of the livestock systems research team to obtain a large enough sample will be influenced by such factors as number of treatments, farmers' willingness to cooperate, animal species and system and treatment characteristics.

- **Number of treatments**

Simpler trials with fewer treatments offer scope for reducing the total number of animals or households required. They are also easier for the farmer to understand and for the researcher to supervise.

- **Contingency allowances**

Determining the number of animals required per treatment is only the start. Because animals die or are sold to meet cash needs, some allowance must be made for losses which are likely to occur during the implementation of the trial. Reynolds (1989), for instance, reports that for statistical trials with small ruminants in southern Nigeria, samples of 70 animals would need to be increased to 120 to cover such contingencies.

Note Sample estimates derived by the formulae given above should, therefore, be regarded as minima. Specific recommendations about the relative size of the contingency allowance cannot be made since this will depend on such things as farmers' cooperation, system characteristics and resource availability.

- **Farmers' cooperation**

This affects such things as the size of the contingency allowance required and the number of farmers who will be willing to offer their livestock in the first instance

- **Animal species**

In most cases, cattle will be more difficult to obtain for a trial than smallstock, Ibis is largely because of the high unit value attached to cattle in most societies, Farmers are usually reluctant to commit them to experimentation if they consider the risks too great.

- **System characteristics**

This is largely a logistical problem, The more extensive and mobile the system, the more difficult and costly it is to obtain the number of animals or households required. In closely settled areas, obtaining the required number is generally less difficult because farmers and livestock are concentrated in a relatively small area,

- **Treatment characteristics** (for details see below).

Summary

The total number of animals required for an on-farm statistical trial is a function of the following five factors:

- the number of treatments in the experiment
- the least significant difference specified by the researcher
- the level of confidence required in order to be able to declare that the difference obtained is statistically significant
- the probability of detecting a difference, and
- the coefficient of variation for the variable being measured, which may depend on the design of the experiment.

Specifying treatment characteristics for statistical trials

When designing statistical on-farm livestock trials, careful consideration should be given to the number and complexity of treatments to be used and to sources of variation.

NUMBER AND COMPLEXITY OF TREATMENTS. In general, on-farm livestock trials should be kept simple, involving not more than four treatments. More complex trials should be carried out on-station.

For instance, experiments conducted to determine how output (e.g. weight) changes as the amount of a given input (e.g. feed supplement) changes should initially be conducted on-station. After this, it is often possible to guesstimate a range of input levels relevant to farm circumstances and to confine on-farm treatments within these limits. Three or four treatments within the range prescribed may normally be sufficient.

Another essential principle is to use fewer treatments as the level of farmers' involvement in trial management increases. They should also be less complex. Two levels of management are considered appropriate to statistical trials with livestock. They are:

- **researcher-managed/researcher-executed trials**, and
- **researcher-managed/farmer-executed trials**.

Researcher-managed/researcher-executed trials. Up to three or four treatments can be recommended for such trials. Treatments may be specified in terms of the level of a given input (e.g. feed supplement) or they may involve combinations of a number of different factors thought to influence animal performance (so-called 'factorial' trials). In either case, animals should be randomly allocated to the different treatments used. (If blocking is used, this randomisation should be done within blocks.)

Example: With livestock, nutrition and health are often linked. Low levels of nutrition may predispose an animal to disease or, conversely, disease may affect intake. The independent effect of each factor on performance, as well as the manner in which they interact, can be studied by a factorial experiment. A 2 x 2 health/nutrition trial might be arranged in the following manner:

CONTROL	HEALTH
(no treatment)	treatment only
NUTRITION	NUTRITION x HEALTH
treatment only	treatment

On-farm factorial trials under researcher-managed/researcher-executed conditions should, as a general rule, have no more than four treatments, and the interactions examined should be simple and easy for the farmer to understand. More complex arrangements tend to confuse the farmer and are more difficult to supervise.

Factorial trials are 'efficient' in the sense that they enable the researcher to examine both the independent and the interaction effects of several factors at once (see example above)⁵. Compared with trials which examine factor effects separately, they can also be used to increase the precision of the results obtained.

⁵ For an explanation of an interaction, see Module 3 of this section.

Example: Suppose a researcher wishes to examine the effect of water and nutrition on calf growth. If, for some reason, he decides not to use a factorial experiment, he could control the watering regime and set up an experiment to test the effect of different levels of feed supplementation on growth. He could then vary the watering level in a second experiment, holding feed constant at the level identified as 'best' in the first experiment.

Assume now that two levels of supplementation (F_1 and F_2) and two watering regimes (W_1 and W_2) are being considered and that 18 animals are available for each experiment.

In the first experiment, the researcher decides to hold water constant (e.g. at W_1) in order to isolate the 'best' of the two feed alternatives. Two treatments would then be required:

Treatment 1: $W_1 F_1$

Treatment 2: $W_1 F_2$

Having isolated the 'best' feed in this experiment (say F_1), the two watering regimes could then be compared. This trial would have the following two treatments:

Treatment 3: $W_1 F_1$

Treatment 4: $W_2 F_1$

If 36 animals are available, only 9 could be used per treatment. The interaction effects of water and feed on calf weight could thus not be examined in an experiment like this.

Alternatively, all watering and feeding levels could be compared in a factorial experiment with four treatments, using the same 36 animals.

Treatment 1: $W_1 F_1$

Treatment 2: $W_1 F_2$

Treatment 3: $W_2 F_1$

Treatment 4: $W_2 F_2$

With this arrangement, more animals (18) are available to examine the main effects of water and feed, resulting in higher levels of statistical precision (i.e. there are 18 animals for each of the two water regimes and 18 for each of the two feed supplementation options). Examining all four treatments in one experiment reduces the time required for testing. In addition, the interaction effects of water and feed on calf weight could be examined by using the analysis of variation (ANOVA) technique (Module 3 of this section).

Researcher-managed/farmer-executed trials. As a general rule, not more than two treatments per farmer are recommended for this type of trial. Treatments should also be simple in themselves to ensure that they are easily understood (e.g. tests with complex feeding rations should be avoided), and animals should be randomly allocated between treatments. This randomisation should be done within each 'block' if blocking is used. Superimposed trials involving one treatment plus a control are an example of researcher-managed/farmer-executed trials.

The analysis of results is generally simple, involving the use of basic statistical techniques such as the t-test (Module 11, Section 1) or the paired t-test (Module 3 in this section).

BLOCKING - A METHOD TO REMOVE SOURCES OF VARIATION. If random variation in the experimental material can be reduced during the design of statistical trials, then this will either increase the precision of the experiment or reduce the number of replicates required per treatment.

As a general rule, because management practices tend to vary widely within any given area, selected farmers should each receive or be responsible for all experimental treatments. If this can be done, the effects of farmer variability (e.g. in terms of management) can be 'blocked out' in the analysis of variance, and treatment effects can be more clearly identified (Module 3 of this section).

In a situation such as this, each farmer comprises a 'block' of the experiment. Comparisons are made for each farmer, and so variation between farmers is excluded from the treatment comparison. However, this can be extremely difficult to achieve in practice, because farmers are rarely able (or willing) to assign the required number of animals to each treatment.

For instance, in sedentary systems where herds and flocks tend to be small, obtaining the required number of animals per farm can be problematic, particularly if small farmers (i.e. those who are representative of the system under study) are chosen to participate. If only the larger herd/flock owners are chosen to overcome the problem, results are likely to be biased and inapplicable to the target group as a whole. In extensive systems, herds and flocks tend to be larger but logistical difficulties can make trial administration impractical.

Also, farmers may not be able to manage trials where it is desirable to feed one diet consistently to some animals, while maintaining traditional practice with other animals.

Finding enough animals per farmer becomes even more difficult when additional blocking to reduce within-treatment variation is planned. Synchronisation on the basis of age, sex, weight, stage of lactation and parity can be difficult within an area as a whole, let alone on a per-farm basis (Gryseels, 1986). If, in addition, the farmer is made responsible for all the treatments in the experiments, blocking on the basis of such characteristics (particularly with cattle) is normally impractical.

However, since blocking on the basis of animal characteristics is desirable, researchers are often forced to obtain their trial animals from many different farmers. Often, it will not be practical to have one farmer using more than one treatment. If the sample required is large (say 100 animals) and farmers have only a few animals of the same class, a large number of farmers (say 40 to 50) may need to be involved just to obtain the required sample size. If the households are widely dispersed, trial supervision then becomes a major problem. There is also a risk that animals selected over a wide area may not come from the same population and that the resources of the participating farmers may be very different as a result.

When a farmer becomes responsible for only one treatment, management effects become a major source of variation which cannot be blocked out in the analysis of variance (Module 3 of this section). The residual variance and coefficient of variation for the experiment therefore tend to be high, requiring large samples to determine differences resulting from treatment effects.

Thus, trials like this are only likely to be practical when animals and households are highly concentrated (e.g. at village centres). Obtaining the required number of animals in each class will probably be less difficult and trial supervision less complicated.

For instance, ILCA conducted a successful on-farm feeding trial with livestock in The Gambia. It was a researcher-managed/researcher-executed trial in which animals were blocked on the basis of age, sex and liveweight. Supervision and the ability to obtain sufficient animals within each class were enhanced by the fact that animals were tethered each evening at the same village site (ILCA/ILRAD, 1988).

Farmers' behaviour

Apart from the problems associated with getting an adequate sample size and blocking out sources of variation, problems related to farmers' behaviour are commonly encountered in statistical livestock trials. They include:

- **unwillingness to participate**

The larger and more wealthy farmers are often the ones who show greatest interest in the trial, but this can result in conclusions which have narrow applicability within the area

Also, when superimposed trials are planned to test the effect of different animal health treatments on productivity, it can be very difficult to get farmers to cooperate and assign animals to the control (untreated) group.

- **moving animals across treatments**

Irrespective of the type of management involved, farmers are apt to move animals across treatments if they observe that a particular treatment is having a comparatively beneficial effect (e.g. control animals may be given a feed supplement) (Appendix 1). Such movements are difficult to monitor, even when the application of trial inputs is administered by supervising enumerators. There may also be cases when researchers unconsciously pass on their expectations to the participating farmers, who then give more attention to a particular group of animals so that a management effect rather than a treatment effect is recorded.

- **disposal of trial animals**

Farmers will often sell trial animals because they need the cash. This is unlikely to introduce bias into the results of the trial and affect the inferences made, unless the disposal of an animal is somehow related to a particular treatment effect (e.g. if in a feeding trial, treated animals were sold because they were in better condition than control animals) (McIntire, 1986). When there is no relationship, the chief problem is to ensure that the sample size is large enough to account for contingencies resulting from sale, transfers or deaths.

- **loss of interest**

With long-term trials, farmers are inclined to lose interest and, With time, to become less conscientious about the application of treatment inputs (Appendices 1 and 2). Such trials are thus better suited to on-station research.

Monitoring trials

When statistical trials are impractical, because adequate supervision cannot be ensured or because the samples are not large enough to give meaningful results, 'monitoring' trials may be an appropriate alternative, at least during the initial stages of on-farm research.

The broad objectives of monitoring trials are to:

- improve contacts between researchers and farmers
- increase the researchers' understanding of the farming system (diagnostic function)
- introduce technology thought to be appropriate to the target area (extension function)
- monitor farmers' reactions to the technology, and
- adapt and refine technology (through the above process) to suit better farmers' circumstances and objectives.

Monitoring trials have been used by ILCA in Nigeria for the extension and refinement of fodder banks and alley farming (von Kaufmann et al, 1984; Atta-Krah, 1985). They were also used to introduce new dairy and draught technologies in the Ethiopian highlands (Gryseels, 1986). The approach whereby diagnostic surveys are used to identify a suitable technology which is then progressively adapted as farmers' reactions become known through monitoring trials, is essentially an iterative approach to on-farm research.

Efficient communication among all the parties involved (i.e. the participating farmers, the livestock systems research team, on-station researchers and extension agents) is thus a prerequisite to the long-term success of monitoring trials (Atta-Krah, 1985; von Kaufmann, 1986; Waters-Bayer and Bayer, 1987). Refinements identified by the systems research team or suggested by the farmer will often need to be tested on-station before new adaptations can be re-tested on-farm. Access to research-station facilities must, therefore, be assured right from the beginning. As the emphasis shifts during the process of adaptation, a change in the composition of the livestock systems research team will often be required.

Monitoring trials are farmer-managed/farmer-executed and should be simple, particularly during the initial stages (i.e. they should have no more than one or two treatments). Farmers should be allowed to use and adapt the technology in the manner they see fit, with the researcher playing the role of an observer. On-farm statistical trials may come at a later point in the process, when adequate samples

or supervision can be assured and when there is more certainty about the applicability of the technology. Specific quantification of treatment differences may then be attempted under more controlled on-farm conditions.

Since statistical analysis of the results is not initially the objective, the introduction and testing of the technology can begin with just a few cooperative farmers. Ultimately, however, wider adoption of the technology in the area would be necessary if meaningful comparisons between adopters and non-adopters are to be made. Simple techniques of analysis such as the ordinary t-test (Module 11, Section 1), gross margins, partial budgets, whole-farm budgets and cash-flow budgets (Module 3 of this section) can then be applied to compare production performance. Other more complex techniques such as linear programming and simulation can also be used to examine the impact of changes introduced and to identify constraints (Gryseels, 1986).

Making meaningful comparisons will, however, be very difficult if cooperating farmers choose to apply the technology differently. In such circumstances, reasons for the different approaches used should be ascertained in diagnostic surveys. To minimise this problem, farmers selected for participation should have similar resource and objectives.

An additional problem with monitoring trials is that the livestock systems research team can become preoccupied with extension. Being convinced of the validity of the technology in the first place, they may be more concerned with promotion rather than with the observation of technology adoption and adaptation.

Part C: Implementation

[Operations and their phasing](#)
[Data collection](#)

Having decided on the approach to adopt, the research project needs to be implemented. During implementation, the major concerns are the phasing of various operations data collection.

Operations and their phasing

The researcher should be conscious of time throughout the entire systems research process. In on-farm trials, the starting and ending times of operations should be specified. By doing this, the research team is forced to think through the activities and requirements of the project in a sequential manner and to decide on those operations which are critical to its success. Operations will also need to be scheduled to suit seasonal conditions and coincide with periods when the farmers themselves are available and willing to cooperate (Sidahmed et al, 1985).

Identifying participating farmers and informing them of their obligations is one operation which will have an important bearing on the results obtained. Sample animals also need to be selected and positively identified (e.g. by ear-tagging). The need to allocate sufficient time to preparatory work of this nature should not

be underestimated. Failure to start a trial on time because of inadequate preparation can delay actual implementation for periods of up to one year (e.g. when seasonal feeding trials are being planned).

At this stage, it is also important to assign responsibilities to different members of the livestock systems research team. Each member needs to understand his/her obligations with respect to the trial and the time at which those responsibilities will become effective. Where external sources of support are envisaged (e.g. on-station staff), such personnel (and their activities) should also be included in the implementation plan.

Simple bar charts can be used to itemise and schedule crucial operations (Module 2, Section 1). Although in practice it may be difficult to adhere to an originally planned schedule (particularly as the level of farmers' involvement increases), attempts should, nevertheless, be made to keep operations under a reasonably tight control.

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Data collection

The first task here is to define the methods which will be used to collect data. The type of data collected will depend on the objectives of the trial, which, in turn, will be reflected in the level of farmers' involvement.

Statistical trials. In researcher-managed/researcher-executed trials, assessment of treatment effects is the main objective. Therefore, emphasis is given to the measurement of production performance in the farm environment, but under carefully supervised conditions of trial management. The various methods used to collect animal production data are described in Module 5 (Section 1) which also specifies how often measurements should be made for each performance variable.

In researcher-managed/farmer-executed trials, the measurement of performance is also important, but the effect of the technology on the use of farm resources (e.g. family labour and its allocation to other enterprises) requires more careful monitoring. Data on labour use and cropping patterns should be collected in farmer-executed trials to ensure that all issues affecting adoption are properly understood. However, as the farmers' involvement in trial management increases, so will also the requirements of data collection.

When statistical trials are being attempted in highly mobile production systems (e.g. superimposed trials using veterinary inputs in a pastoral area) there is always the risk that treatment effects may be confounded by variations in the grazing environment (McIntire, 1986). Grazing behaviour may then need to be monitored as part of the trial⁶. The same principle applies to other situations in which potentially confounding factors can be identified before the trial begins.

⁶ Methods used to monitor grazing behaviour are discussed in Nodule 7 (Section 1).

Monitoring trials. In farmer-managed/farmer-executed trials, the chief objective is to monitor farmers' reactions to the technology being tested. Again, besides measuring production performance, the researcher should also try to understand the interactions in resource use on the farm.

Farmers' opinions become more important as the level of farmers' involvement increases, and simple questionnaires need to be designed to ensure that information of this kind is collected both during and after the trial. Mutsaers et al (1986) suggest that these should be used:

- **when selecting farmers and livestock for the trial**

For instance, the information collected at this stage will include the name of household head, household size and structure, livestock owned or held by age, sex and productive function, cropping practices, other sources of income and assets. Information on animal characteristics (such as weight, sex, age) and means of identification can also be obtained.

- **during the trial**

During the trial, data should be collected on performance, inputs applied to each treatment group (if relevant) and on animal losses (deaths, sales, transfers). Other relevant data at this stage are farmers' observations and information on labour allocated to the trial and other activities, on potentially confounding factors (e.g. grazing resources, seasonal conditions) and on other problems (e.g. cooperation by farmers, logistics and supervision of enumerators).

- **at the end of the trial**

The data collected at this stage will include final records of performance and farmers' observations about the trial.

When enumerators are involved in the trial, periodic checks should be made to ensure that the right methods are being used and that data are being recorded correctly (Module 2, Section 1). Finally, weighing scales and other equipment should be regularly calibrated.

Part D: Evaluation and re-design

Evaluation is a step-wise process, ending with an assessment of the manner in which the technology has been adopted within (and outside) the target area. It will generally involve the need to consider some or all of the following issues:

Statistical significance. For on-farm statistical trials, treatments will initially be evaluated on the basis of their effects on production performance. The methods used to test the statistical significance of trial results are discussed in Module 3 of this section.

Financial attractiveness. A treatment may have a statistically significant effect on production without it being financially attractive to the farmer. The financial implications of the innovation should, therefore, be analysed at the farmer's level. In an appraisal of this kind, the main considerations are:

- the manner in which the inputs and outputs resulting from the application of a technology are valued, and
- the effect of adoption on the allocation of household resources to farm and non-farm activities, as well as its overall impact on farm income.

The methods commonly used to evaluate the financial attractiveness of an innovation at the farmer's level are discussed in Module 3 of this section, which also deals with the valuation of inputs and outputs.

Evaluation by farmers. While the productivity and financial effects of an innovation are important, they are not the only factors considered by the farmers. Issues such as the risks associated with adoption, the effect on intra-household control over resources and cultural acceptability will come into the decision as well.

Farmers must be involved in the whole process of design and evaluation. There

are numerous cases in Africa where apparently attractive technologies have not been adopted because these issues have not been adequately considered. This is particularly true for livestock-related technology (Behnke, 1984).

Adoption. The general applicability of an innovation for the target area will be indicated by the proportion of households which accept and adopt it, and by the rate of adoption.

Farmers' perception of the relevance of the technology will only be one of the factors affecting adoption. Other factors such as institutional and infrastructural support will also determine how widely and how rapidly the technology will be adopted.

Acceptability can be assessed in terms of an 'acceptability index'. The index is obtained by multiplying the percentage of farmers who adopt the new technology by the proportion of their livestock so affected, and by dividing the product by 100.

Example: In an area A, 10% of the farmers have adopted a particular health measure (e.g. the use of a vaccine) and 90% of the cattle owned or held by these farmers have been treated. Calculate the acceptability index.

$$\text{Acceptability index} = 10 \times 90/100 = 9$$

The acceptability index provides a measure of the importance of the technology to those who have actually adopted and of its potential for replication within the area (Shaner et al, 1982, p. 141). However, it can only be properly interpreted by examining the components used in its derivation.

For instance, the index calculated above indicates that only a small proportion of farmers have been impressed with the technology, but these have been convinced of its effectiveness. An index of 9 could also be derived if 90% of farmers used the treatment on only 10% of their animals. This would indicate wide use but cautious acceptance on the part of most households. A given value of the acceptability index can thus have many different interpretations.

Furthermore, the overall proportion of livestock affected ('coverage') will depend on the distribution of livestock holdings in the area concerned. If complete coverage is the aim (as would be the case with a vaccine), the acceptability index does not tell you much about the coverage actually achieved.

Example: If the 10% of the households which had accepted the use of a particular vaccine owned or held 50% of all the cattle in the area, and if they used the vaccine on 90% of their cattle, then the proportion of cattle actually affected would be:

$$\text{Coverage (\%)} = (\text{per cent of animals held by adopters} \times \text{per cent of these affected})/100 = 45\%$$

When high coverage is associated with a low acceptability index (as in the above example), the benefits would seem to be going to the wealthier cattle holders in the area, which may be considered undesirable on equity grounds.

Other evaluation criteria. The issues raised above show that an overall

evaluation of the benefits of a technology needs to take into account all the factors influencing adoption, including equity and environmental effects. Equity relationships in livestock systems research can be examined using the Lorenz curve (Module 11, Section 1). Environmental issues are discussed in Module 6 (Section 1).

Consideration of all the factors mentioned above may indicate the need for a re-design or modification of the technology. This may involve the use of research-station facilities to refine the innovation or further on-farm testing. The process of technological design is thus iterative and dependent on continuous feedback from the farmer to the systems research team and the research station.

Appendix 1: On-farm feeding trials: Additional considerations

The principles outlined in Modules 1 and 2 of this section are generally applicable to all types of livestock on-farm trial. Nevertheless, there are six additional considerations specific to on-farm feeding trials which need to be borne in mind when planning the experiments. They relate to:

- **supplementation, adjustment and compensatory gain**
- **seasonal effects**
- **feed variability**
- **interactions**
- **typical problems, and**
- **valuing feeds.**

Supplementation, adjustment and compensatory gain

Animals often require some time to adjust to a supplement before its effects can be positively stated. Periods of up to two months may be needed to allow such adjustment to take place. Several points can be made in this context:

- Unsupplemented control animals may perform better than supplemented animals during the initial stages of the trial (e.g. in terms of weight gain). The trial should, therefore, be long enough to allow supplemented animals time to adjust to the treatment (Riley et al, 1988).
- After adjustment, a period of compensatory gain among supplemented animals will often follow (Module 7, Section 1). If performance is being measured in terms of body weight, differences between treated and untreated groups may be temporarily distorted because of the effects of compensatory growth.
- Alternatively, the relative gains resulting from supplementation during periods of feed shortage (e.g. during the dry season) may only be temporary if compensatory growth occurs in unsupplemented animals during subsequent periods when grazing is relatively abundant (e.g. during the wet season).

This subsequent compensatory growth among

unsupplemented animals will be irrelevant when dry-season supplementation is geared towards either end-of-season market opportunities or the improvement of the body condition of draught animals at ploughing time. However, it becomes very important if the objective is to improve growth and condition in the longer term. However, in overgrazed environments, the potential for compensatory gain during the growing season may be very limited (Module 10, Section 1).

- Comparisons on the basis of liveweight alone can be misleading, since changes in weight can result from variations in gut or bladder fill rather than from changes in fat or muscle.

This problem can be mitigated by complementing weight measurements with condition scoring (Module 5, Section 1). Condition scoring is generally applied to cattle (Nicholson and Butterworth, 1986), but scoring techniques have recently been developed for small ruminants as well.

- The length of the trial period will depend on the objectives of the trial. For trials which aim to improve the growth performance of ruminants, periods of up to 100 days may be needed to account adequately for the effects of adjustment and compensatory gain.

Seasonal effects

Results can be markedly affected by inter-year or inter-season variations in moisture conditions when trial animals have access to communal grazing or other feed sources (e.g. crop residues) influenced by such conditions.

During dry seasons or years of low rainfall, for instance, differences between supplemented and unsupplemented animals may be highly significant. During wet seasons or years of high rainfall, unsupplemented animals may perform as well or even better than supplemented animals.

Feed variability

The nutritive value of locally available feed supplements tends to be highly variable **within** and **between farms**. Crop residue quality, for instance, can be affected by:

- seasonal conditions
- time of harvest
- the time lag between harvest and storage, and
- storage techniques.

Thus, treatment effects can be confounded by differences in the quality of the traditional feed supplement used by the farmers participating in a trial. To minimise this, attempts should be made to select farmers with feed supplements (e.g. hay, crop residues) of similar quality. Feed resources on each farm would thus need to be sampled and tested before commencing the trial (Module 7, Section 1).

Treatment effects can also be confounded by variations in animal characteristics (e.g. age, weight and sex).

Where possible, the researcher should attempt to remove, by blocking, potential sources of variation caused by:

- differences between feeds
- differences between animals, and
- differences between farmers.

However, this is likely to prove very difficult in practice.

For instance, blocking on the basis of animal characteristics often implies the need to select more farmers over a wider geographical area. However, the wider the area of selection, the greater the chances of variation in the traditional supplement used.

Thus, blocking on the basis of one factor (e.g. animal characteristics) may increase the likelihood of greater variation in another factor (e.g. feed supplement or farmer). In such cases, the residual variation in ANOVA is likely to be high, reducing the chances of obtaining a significant result.

Covariance analysis could be used to remove the effects of feed variability on individual trial farms if, for instance, a measure of 'average' quality per farm could be obtained by laboratory analysis. This would, of course, require periodic sampling of the supplement used and ready access to laboratory facilities.

Interactions

Responses to improved nutrition will often be determined by factors other than the feed itself. Intake, for instance, is affected not only by the digestibility and palatability of the feed given but also by such factors as disease, the availability of water, temperature, humidity and the physiological status of the animal (Module 7, Section 1).

Thus, nutrition trials are often more useful if attempts are made to understand the independent effects of influencing factors (including feed) on animal production performance and to determine the manner in which these different factors interact. Factorial trials are suitable for these purposes (Modules 2 and 3 of this section).

In order to determine the relative importance of the different factors and the interactions which occur between them, an initial period of on-station research will probably be required. Subsequently, when the important factors have been isolated, simple factorial trials (under researcher-managed conditions) could be used to examine their effects and interactions in the farm environment.

Typical problems

The problems most commonly confronted in feeding trials are:

- **a tendency, on the part of farmers, to move animals across treatments**
- **loss of interest**

- **difficulties in providing purchased supplements over long periods, and**
- **free provision of inputs.**

Farmers switch livestock between treatments because they observe differences between supplemented and unsupplemented animals.

Such behaviour sabotages statistical trials, and implies the need for a high degree of supervision by the researcher to prevent it. But with monitoring trials, where the aim is to observe how farmers use supplements and to understand the reasons for any changes made, moving animals from one treatment to another may be less serious, provided that the researcher is aware that it is being done.

When trials extend over prolonged periods, *farmers tend to lose interest* and are more likely to switch the treatment to the control group (as the effects of supplementation become more obvious) or to dispose of animals as and when the need for cash or food arises. Improvements which take a long time to take effect are also likely to be of less interest (Part B. Module 1, Section 2).

Purchased supplements, which were used in the trial, may not always be available on a continuing basis. As a result, the technology tested on-farm will have a limited period of applicability. Forecasting the availability of purchased feeds in the long term may not be easy, but an examination of sources of supply and their present and past reliability should provide a reasonable guesstimate.

Farmers' participation in a trial is often conditional upon the research team *providing the required feed inputs free of charge*. Such expectations then make it doubtful whether the farmers will continue to use the technology when they have to purchase the inputs themselves.

Valuing feeds

Module 3 in this section deals with the valuation of outputs which is necessary to assess the financial attractiveness of a new technology.

Feeding trials will essentially involve the use of home-grown feeds (e.g. hay, crop residues, grain, pasture, other forages) and purchased feeds (e.g. grains, agricultural byproducts). When valuing **homegrown feeds** as production inputs, the following principles should be borne in mind:

Grain products. If the household is a surplus producer, the amount of grain fed should be costed at:

- the price per unit at which it could be sold to a buyer, or
- its market price less the cost of taking it to the market, if this can be estimated.

If the household is a deficit producer (i.e. it has to buy grain on a regular basis), grain fed to animals should be costed at the price at which it can be purchased if delivered to the purchaser's farm.

Crop residues. Where there are formal or informal market outlets for crop residues, the value of the crop residue fed to animals is computed as indicated above for

grain. Where no such outlets exist, crop residues should be given an imputed market value. This is done by converting the feed into nutrient equivalents (e.g. energy or protein equivalents) and valuing it at the market price per unit for that particular nutrient. For very low-quality roughages, the value assigned will be close or equal to zero.

Home-grown forages. Where the land used to grow forages could be allocated to other cropping activities (e.g. grain cropping), the forage can be costed at its opportunity cost, i.e. in terms of the income which the farmer forgoes by using land for forage, not crop, production.

The valuation of forage legumes should also include the benefits of nitrogen fixation, improved soil structure and easier land preparation for subsequent cropping, all of which makes it more complex. Methods of analysis such as linear programming can be used to provide a unit value for forage legumes. However, these are beyond the scope of this manual.

Purchased feeds, such as molasses and cottonseed cake, are valued at the market price paid per unit of feed purchased.

Appendix 2: On-farm animal health trials: Additional considerations

When planning on-farm animal health trials, we will need to consider carefully such issues as:

- **participation**
- **incentives and expected assistance**
- **disposal of animals**
- **efficacy and availability of vaccines and drugs**
- **immunity and trial effects**
- **interactions**, and
- **prospective animal health studies.**

Participation

When the effect of an animal health intervention is not clearly understood, or when exposure to veterinary services has been limited, it can be difficult to obtain the required number of farmers to participate in the trial. Fear of losing animals subjected to a particular treatment (e.g. a new vaccine) can also result in reticence on the part of farmers. Farmers are also reluctant to have some of their animals treated for a disease, while the others (the control group) remain untreated.

Incentives and expected assistance

Because of the difficulties involved in getting the required number of participants, trial inputs (e.g. drugs) often need to be provided free of charge, at least initially. Additional incentives (e.g. in the form of general veterinary care) have sometimes been offered by researchers to ensure continued cooperation or to maintain credibility.

This can be both costly and time consuming, diverting resources away from the

originally prescribed tasks. It is also unlikely that NARS research teams will be able to offer such support because of the limited resources they usually have at their disposal. Therefore, government veterinarians should be involved in the trial programme, where possible.

During the trial, other issues may arise, including:

- requests for and purchase of treatment for control animals
- tempering with treatments
- dwindling interest in the trial, and
- unhelpful attitudes by veterinarians.

Treatment for control animals. Farmers with control animals often request that their animals be treated as well, when the benefits of the treatment become evident (e.g. reduced mortality or increased productivity). In such a case, the researcher can take one of two possible courses of action.

If the treatment is a vaccination, he may give the animals placebo (saline solution) injections instead of the real treatment. Of course, the danger here is that the farmer will detect the deception (e.g. if the animals die of the disease being 'treated') and refuse to cooperate. Needless to say, the credibility of the whole research team will be severely damaged.

Alternatively, farmers with control animals may be offered other forms of veterinary care for their animals to induce them to cooperate. This, however, raises the costs of the trial.

In the extreme case that no assistance is offered, farmers with control animals may resort to purchasing the treatment being tested from formal outlets or on the black market.

Treatment dilution. Farmers responsible for both treated and control animals may be tempted to dilute the treatment, spreading its use over both groups. Results then tend to be unconvincing, and the farmer loses interest in the trial. Detecting this kind of behaviour is virtually impossible even under heavily supervised trial management.

Dwindling interest. This is common in trials which require regular sampling. If farmers see no concrete benefits resulting from their participation in the trial, they tend to start regarding regular sampling as an inconvenience which has no real purpose and often refuse to continue to cooperate.

For instance, taking blood samples for trypanosomiasis detection tends to be unpopular precisely because it does not seem to lead to any immediate benefits.

Unhelpful attitudes. Researchers working on issues related to animal health may experience resistance from locally appointed animal health officers if, for instance, these officers do not have access to the drugs/vaccines being tested by the systems research team. Involving local veterinarians in the trials from the beginning can overcome this problem.

Disposal of animals

The sale of animals during trials is a common problem (Module 2, Section 2). With health trials, the probability of death in the untreated (control) group is likely to be greater than it is for animals assigned to treatment groups. Farmers with control animals are more likely to sell or slaughter sick animals rather than wait for them to die, and this can destroy the usefulness of trial results.

The levels of sale and slaughter may, therefore, be higher in control groups when animal health interventions are being tested (Reynolds and Francis, 1988). To correct this, researchers may need to offer therapeutic treatment for control animals when symptoms leading to death become evident. The objective of the trial will then be to observe differences in other performance measures, not mortalities.

For instance, assistance of this nature has been offered to Gambian farmers cooperating With the African Trypanotolerant Livestock Network in order to study differences in the performance of animals under high, medium and low trypanosomiasis risk and to determine the frequency and magnitude of the treatments required under the different trial regimes.

Efficacy and availability of vaccines and drugs

Introducing animal health interventions into Africa can meet with several obstacles, the most important of which are listed below.

Imported drugs and vaccines are not always effective under the different environmental conditions. They may also deteriorate before arrival. Their efficacy should, therefore, be tested before on-farm trials begin.

Recurrent funds for the operation of veterinary services have declined in real terms in many African countries (Addis Anteneh, 1983; 1985; ILCA, 1989). This has placed severe restrictions on the effectiveness of veterinary services, so that, in many cases, continuous support for on-farm testing of animal health interventions cannot be ensured.

Shortages and high costs of drugs mean that veterinarians and farmers often dilute applications, reducing the effectiveness of health interventions in the long term.

Immunity and trial effects

Animals may acquire temporary immunity to diseases prevalent in an area. If the trial period corresponds with the period of immunity, treatment differences will not be detected. Serum or blood samples would then be required to identify susceptible groups within the target population, but these are not always effective.

With serum sampling, false positive and negative results can occur when animals show a natural or induced tolerance to antigens and, therefore, do not produce antibodies when challenged with the disease agent. With blood smears, parasites may be easily detectable during the early stages of infection but may be less so during later stages (e.g. in carriers) (Part B. Module 8, Section 1). There is, therefore, danger that trial animals identified as susceptible may, in fact, be immune, and the effects of treatment will then be distorted.

For instance, with peste de petits ruminants (PPR), offspring can acquire impunity from the dam which lasts for three months. Immunity acquired by adult goats from a survived PPR infection lasts for about three years (Obi et al, 1983).

Interactions

Disease can affect the intake of feed and the level of nutrition an animal receives. Alternatively, the amount of feed eaten and its nutritive quality can affect the animal's susceptibility to disease. Breed characteristics, environmental conditions and management practices can also influence the susceptibility of animals to particular diseases (e.g. N'Dama cattle in West Africa have a natural tolerance to trypanosomiasis).

It is not always easy to determine the direction of causality between these factors or the manner in which they interact (Modules 7 and 8, Section 1).

Nevertheless, an understanding of such relationships is important if appropriate interventions are to be identified.

The precise nature of many of the environmental and genetic influences and the manner in which they interact can best be determined under controlled conditions on station. On-farm trials will, however, be necessary to understand the effects of the interactions between management practices (which are likely to be quite different on research stations from what they are on smallholdings) and disease. The use of factorial trials for these purposes was discussed on pages 21-23 above. Module 3 in this section shows how the results obtained can be analysed.

Prospective animal health studies

Prospective or cohort studies⁷ are often used to examine the effect of a disease determinant⁸ on different groups of animals. Animals selected for the trial are typically divided into two groups, with the treated group being subjected to or having the determinant in question.

⁷ Prospective studies aim to establish relationships between diseases and their determinants as they occur. Animals are normally separated into groups or 'cohorts' in which the determinant of the disease is either present or absent, or where its frequency of occurrence varies (Module 8, Section 1).

⁸ A determinant is any factor or variable that can affect the frequency with which a disease occurs within a given animal population (Putt et al, 1987) (module 8, Section 1). Determinants may be introduced (e.g. vaccine) or they may occur as a result of natural influences (e.g. breed, age, sex, climate, soils).

Under on-station conditions it is a relatively simple matter to isolate and quantify the effects of the determinant. Animals in each group can be paired on the basis of age, weight, sex or breed, and the paired 't' test can then be used to test for differences between groups (Module 3, Section 2).

Under the less controlled circumstances on the farm, the investigator can follow

essentially two courses of action:

- **study the influence of a determinant which occurs naturally**

For instance, one can study the effect of breed on disease susceptibility and production performance.

The problem with this type of study is that confounding factors (e.g. management practices) can make it very difficult to isolate the causes and effects and to come up with useful recommendations.

- **study the influence of an artificially introduced determinant**

An artificially introduced determinant could be a vaccine which is administered to a group of animals While another group is left untreated (control). Trials which have treatment and control groups are classified as superimposed trials.

In studies of this kind, animals in each group should be blocked on the basis of similar characteristics, but this often results in problems with sampling or supervision.

If the effect of the determinant is measured in terms of mortality (which is a discrete variable), the sample sizes required are likely to be very large and may be beyond the scope of most livestock systems research teams. Therefore, studies of artificially introduced determinants tend to be confined to the measurement of continuous variables such as weight and milk production.

To examine the effects of the determinant on mortality, two approaches may be used:

- offer therapeutic treatment to the farmer's control animals when it is certain that they would otherwise die. This may entail difficulties in ensuring adequate supervision.
- purchase animals and attempt to simulate traditional management practices, observing the effects of the determinant on treated and untreated groups.

Such observations have been carried out in veterinary epidemiology and in farming systems research studies, using sentinel herds (Fadlalla and Cook, 1985; Putt et al, 1987). However, the costs of setting up and administering trials of this nature are likely to be prohibitive in most circumstances.

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Section 2 - Module 3: Analysing data from on-farm trials

[Part A: Statistical analysis of on-farm trials](#)

[Part B: Financial analysis of on-farm trials](#)

[References](#)

This module provides a brief outline of the methods most commonly used to analyse data obtained from on-farm trials. It is broken into parts A and B.

Part A, entitled '*Statistical analysis of on-farm trials*', discusses methods of analysis applicable to data obtained from statistical trials. Module 11 (Section 1) and Putt et al (1987) complement the material given here.

Part B, entitled '*Financial analysis of on-farm trials*', discusses methods used to appraise the financial attractiveness of technologies introduced at the farm level. These methods are applicable to the analysis of data obtained from both statistical and monitoring trials.¹

¹ The terms 'statistical' and monitoring' trials are defined in Module 2 of this section.

Part A: Statistical analysis of on-farm trials

[The paired t-test](#)

[Analysis of variance \(ANOVA\)](#)

[One-way analysis of variance](#)

[Analysis of variance for a randomised block design](#)

[Detailed treatment comparisons](#)

[Quantitative treatments](#)

[Presentation of results](#)

[ANOVA and factorial experiments](#)

[Unbalanced data](#)

The paired t-test

When there are only two groups of animals being compared in an on-farm trial (e.g. a control and a treated group), differences in mean production performance

can be tested by using the ordinary t-test which is described in Part B of Module 11 (Section 1).

The statistical sensitivity of the tests being carried out can be greatly improved if animals are paired so that, within each pair, they are as alike as possible at the outset. One animal in each pair is then assigned at random to one group, and the other animal is assigned to the other group. This is analogous to 'blocking' which was described in Module 2 of this section. The paired t-test can then be used to make comparisons between groups.

Example: In an on-farm feeding trial, two groups of 10 lambs were identified and paired on the basis of age, weight, sex and parity. Lambs from each pair were then allocated to a control and a treated group, respectively, and their weights at three months were compared. The results of the trial are summarised in Table 1 below:

Table 1. Mean liveweights of lambs in treated and control groups.

Pair	Mean liveweight (kg)		Difference (d)
	Treated	Control	
1	12.2	11.4	0.8
2	10.4	10.6	-0.2
3	10.8	9.6	1.2
4	11.8	10.4	1.4
5	12.6	12.8	-0.2
6	12.0	12.6	-0.6
7	11.4	10.2	1.2
8	10.2	9.6	0.6
9	12.4	10.2	2.2
10	11.8	10.0	1.8
Mean	11.56	10.74	0.82
SD	0.84	1.16	0.92

These data could be analysed (inefficiently) by the ordinary t-test, as follows:

$$t = \frac{\text{treated mean} - \text{control mean}}{\text{standard error of difference}} \quad (1)$$

The standard error of the difference (SED), which is defined in Part B of Module 11 (Section 1), is calculated from the standard deviations (SDs) for the treated and control groups.

In this example, SED = 0.451 and the t-test is:

$$t = (11.56 - 10.74)/0.451 = 1.82$$

This value is compared with the tabulated value for 18 (n - 2) degrees of freedom, which is 2.10 at the 95% confidence level (Table 10, Module 11, Section 1). The calculated value (1.82) is smaller than 2.10, i.e. there is a more than 1 in 20 chance that such a value of t could arise in cases where samples are taken from the same population, and

so we cannot conclude that the treatment affects the weight of lambs. The difference is not statistically significant.

A 95% confidence interval for the difference can be calculated as:

$$\text{Difference} \pm t \cdot \text{SED} \quad (2)$$

$$\text{i.e. } (11.56 - 10.74) \pm (2.10 \times 0.451)$$

$$\text{i.e. } 0.82 \pm 0.95$$

$$\text{or } -0.13 \text{ to } 1.77$$

so the effect of treatment could be anything between decreasing lamb weight by 0.13 kg to increasing it by 1.77 kg. (The fact that the confidence interval includes the value zero is equivalent to the statement that the difference is not statistically significant, i.e. zero is a likely value of the difference).

However, since the animals were paired before the trial had begun, a more efficient analysis is possible by making comparisons for each pair. This eliminates variation from pair to pair.

The way to do this is to calculate:

- **the difference for each pair** as in the last column of Table 1

Note that the minus signs are important here,

- **the mean and standard deviation of these differences**

Note that the mean of the differences is the same as the difference between the two means (both equal 0,82 in this example).

The next step is to calculate the standard error of the difference (SED) as:

$$s / \sqrt{n} \quad (3)$$

where:

s = the standard deviation of the differences, and
n = the number of pairs.

In our case, s = 0.92 and n = 10, giving:

$$\text{SED} = 0.92 / \sqrt{10} = 0.291$$

The formula for the t-test is exactly as before

$$t = \frac{\text{mean difference}}{\text{standard error of difference}}$$

except now the standard error of the difference is calculated differently, i.e.

$$t = 0.82/0.291 = 2.82.$$

The tabulated value for n-1 degrees of freedom (= 9 df) at, say, the 5% level is 2.26. Our calculated value (2.82) is larger than 2.26, indicating that the difference is larger than would be expected if there was no treatment effect. Therefore, the difference is statistically significant.

Again, we can calculate a 95% confidence interval for the difference, as follows:

$$\text{Difference} \pm t.SED$$

which is

$$(11.56 - 10.74) \pm (2.26 \times 0.291)$$

$$\text{i.e. } 0.82 \pm 0.66$$

or 0.16 to 1.48

The main difference between the paired and the unpaired case above is that the standard error of the difference is now much smaller (0.291 compared to 0.451). The smaller the standard error, the narrower the confidence interval and the more precisely a difference is estimated. So, by taking the pairing into account, we have dramatically improved the precision of our estimate. The tabulated t-value has also changed (from 2.1 to 2.26), but this is only important for very small samples.

The above example helps to demonstrate the gains in efficiency which can be achieved when experiments are carefully planned and designed. By removing some potential sources of random variation at the outset, the benefits (or otherwise) of an introduced technology can often be more clearly ascertained.

However, pairing does have its difficulties, the two major ones being that, in traditional production systems, it is not always easy to obtain:

- **animals with similar characteristics**

Difficulties of this nature are likely to be more pronounced with cattle than with small ruminants (Module 2 of this section).

- **the sort of information required for efficient pairing**

For instance, farmers are not always sure about parity' type of birth, stage of lactation etc. Age is more easily determined (module 5, Section 1).

Analysis of variance (ANOVA)

The t-test can be used to compare the performance of two groups of trial animals, subjected to different treatments (e.g. two levels of feed supplementation). When several treatments are involved, real differences in performance between groups can be tested by using the analysis of variance (ANOVA) technique.

Many statistical textbooks (e.g. Cochran and Cox, 1957; Dagnelie, 1975; Snedecor

and Cochran, 1980; Steel and Torrie, 1980; Mead and Curnow, 1983; Gomez and Gomez, 1984) cover this topic. In particular, Gomez and Gomez (1984) give step-by-step details of the necessary calculations, while Cochran and Cox (1957) discuss more complex cases in addition to the basic ones.

One-way analysis of variance

The basic principles involved in the use of the ANOVA technique can be best explained by use of examples. The example which follows is taken from ILCA (1989a, Module 3).

Example: Assume that 5 groups of six sheep were identified for a feed supplementation trial. The objective of the trial was to test whether protein supplementation increases wool production. Each group of animals received one of the following treatments:

- Treatment 1: natural grazing (control)
- Treatment 2: grazing + extra maize
- Treatment 3: grazing + maize + protein supplement (S_1)
- Treatment 4: grazing + maize + protein supplement (S_2)
- Treatment 5: grazing + maize + protein supplement (S_3)

After completion of the trial, wool yields for the different treatment groups were as given in Table 2.

Table 2. Wool yields of sheep under different feed supplementation regimes.

	Wool yields (kg) for treatment group:				
	1	2	3	4	5
	2.4	2.8	3.0	3.2	3.3
	4.1	2.9	5.1	4.5	4.5
	2.9	2.4	3.5	3.6	4.1
	2.3	3.8	3.9	3.7	4.2
	2.6	2.4	2.9	2.1	2.6
	3.2	3.7	4.4	3.9	4.7
Treatment total (T_i)	17.5	18.0	22.8	21.0	23.4
Mean	2.9	3.0	3.8	3.5	3.9

The results suggest that supplementation may have had a beneficial effect on wool production, but the ANOVA technique is needed to determine whether the differences are real or whether they could be due only to sampling variation.

To determine this, factors causing variation between the different animals in the trial must be separated. In a one-way ANOVA, total variation is said to result from:

- **treatment effects**

i.e. the performance of animals may differ because of different treatments having different effects.

- **residual effects**

i.e. the performance of animals may differ because of unexplained (or unmeasured) influences (e.g. genetic differences).

We can say, loosely, that:

$$\text{Total variation} = \text{variation due to treatments} + \text{residual variation} \quad (4)$$

The ANOVA technique uses formulae to partition the total variation into these two components.

If there is no difference between the treatments, the variation due to treatments will be purely random variation, and therefore similar to the residual variation. If there are differences between the treatments, then the treatment variation will be larger than the random variation.

Variation is measured in terms of a 'mean square', which is another term for variance. The first step is to calculate 'sums of squares'. The total sum of squares and treatment sum of squares are calculated directly from the data, and the residual sum of squares is then calculated from these two figures.

Each sum of squares has an associated degree of freedom, and the mean squares (ms) are then calculated as:

$$\text{Mean square} = \frac{\text{sum of squares}}{\text{degrees of freedom}} \quad (5)$$

If we denote an individual measurement by y and the total number of experimental units N (animals, in our case), the total sum of squares (ss) is:

$$\text{Total ss} = \sum y^2 - \frac{(\sum y)^2}{N} \quad (6)$$

Using the data in Table 2,

$N = 30$, and

$$\sum y = 2.4 + 2.8 + \dots + 3.9 + 4.7 = 102.7$$

therefore

$$(\sum y)^2/N = 102.7^2/30 = 351.58, \text{ and}$$

$$\sum y^2 = 2.4^2 + 2.8^2 + \dots + 3.9^2 + 4.7^2 = 370.61$$

therefore

$$\text{Total sum of squares} = 370.61 - 351.58 = 19.03$$

Note: Sums of squares must always be positive numbers.

The treatment sum of squares (ss) is calculated from the totals for each treatment. If T_i is the total for treatment i and n_i is the number of units (animals) receiving that treatment, then:

$$\text{Treatment ss} = \sum \left(\frac{T_i^2}{n_i} \right) - \frac{(\sum y)^2}{N} \quad (7)$$

The second term on the right-hand side of equation 7, $(\sum y)^2/N$, has already been calculated above as 351.58. Using the totals for each treatment (T_i) which are given in Table 2, and if n_i has the value 6 for all treatments, then:

$$\sum \frac{T_i^2}{n_i} = \frac{17.5^2}{6} + \frac{18.0^2}{6} + \frac{22.8^2}{6} + \frac{21.0^2}{6} + \frac{23.4^2}{6}$$

and

$$\text{Treatment ss} = 356.44 - 351.58 = 4.86$$

When the total and treatment sums of squares are known, we can calculate the residual sum of squares as:

$$\text{Residual ss} = \text{total ss} - \text{treatment ss} \quad (8)$$

$$= 19.03 - 4.86 = 14.17$$

If

$$\text{Total degrees of freedom} = N - 1 = 29 \quad (9)$$

$$\text{Treatment df} = \text{number of treatments} - 1 \quad (10)$$

$$= 5 - 1 = 4$$

then

$$\text{Residual df} = \text{total df} - \text{treatment df} \quad (11)$$

$$= 29 - 4 = 25$$

Mean squares (ms) are then calculated as:

$$\text{ms} = (\text{sum of squares})/\text{df} \quad (12)$$

and are given in Table 3 overleaf.

Table 3. Conventional analysis of variance table for an on-farm sheep supplementation trial.

--	--	--	--	--

Source of variation	df	Sun of squares (ss)	Mean square (ms)	F-value
Treatments	4	4.86	1.215	2.14
Residual	25	14.17	0.567	
Total	29	19.03		

The F-value is calculated as follows:

$$F = \frac{\text{treatment ms}}{\text{residual ms}} = \frac{1.215}{0.567} = 2.14 \quad (13)$$

If there are no differences between the five treatments, the variation between treatments (as measured by the treatment mean square) will be due only to random variation. We would then expect the treatment mean square to be similar to the residual mean square which also measures random variation. Therefore, the F-value should be close to 1.0. On the other hand, if there are treatment differences, the treatment mean square will be larger than the residual mean square and so the F-value will be considerably larger than 1.0.

To test for a significant treatment effect, the calculated F-value is compared with tabulated values. Tables 4 and 5 give these values for the 5% and 1% levels. The tabulated value depends on both the treatment and residual degrees of freedom (4 and 25, respectively, in this example). From Table 4, the F-value for our example is 2.76 at the 5% level.

The calculated value (2.14) is smaller than the tabulated value (2.76) and, therefore, we cannot conclude that the treatments are different; the observed variation between treatments could be due simply to random variation.

Table 4: Percentage points of the F distribution: Upper 5% points.

Note: It is important to look up the table in the correct direction; treatment df is the column across the top of the table, while residual df is the row at the side of the table.

Table 5: Percentage points of the F distribution: Upper 1 % points.

Note: Treatment df is the column across the top of the table, while residual df is the row at the side of the table.

Analysis of variance for a randomised block design

By blocking on the basis of characteristics such as age, sex, weight or parity, estimates of treatment differences can be made more precise and significance tests more sensitive (Module 2, Section 2).

Example: Suppose that the researcher in the sheep supplementation trial had, in fact, decided to block animals on the basis of weight, because it is known that weight affects wool production. This means that the five heaviest animals had been randomly assigned to the five treatments. The five next heaviest animals were similarly assigned, and so on. It should then be possible to determine the effect of feed supplementation more precisely, since much of the variation due to differences in body weight are eliminated.

The grouping of the animals on the basis of weight is shown in Table 6. Note that, unlike

in the one-way analysis of variance, we now need to have the same number of animals in each treatment group.

Table 6. Wool yields (kg) of sheep under different feed supplementation regimes when blocked on the basis of pre-trial body weight.

Weight group	Treatment					Block totals (B_j)
	1	2	3	4	5	
1	2.4	2.8	3.0	4.5	3.3	14.7
2	4.1	2.9	5.1	4.5	4.5	21.1
3	2.9	2.4	3.5	3.6	4.1	16.5
4	2.3	3.8	3.9	3.7	4.2	17.9
5	2.6	2.4	2.9	2.1	2.6	12.6
6	3.2	3.7	4.4	3.9	4.7	19.9
Treatment totals (T_i)	17.5	18.0	22.8	21.0	23.4	
Means	2.92	3.00	3.80	3.50	3.90	

For this analysis, the only additional steps are to calculate the block sum of squares and mean square, and then recalculate the residual sum of squares and mean square.

If B_j is the total for block j , and t is the number of animals in each block (which must be the same as the number of treatments), then:

$$\text{Block sum of squares} = \sum \left(\frac{B_j^2}{t} \right) - \frac{(\sum y)^2}{N} \quad (14)$$

The second term on the right-hand side of equation 14, $(\sum y)^2/N$, has already been calculated as 351.58. Using the totals for each block (B_j) which are given in Table 5. and if $t = 5$, then the first term on the same side of the equation is:

$$\sum \left(\frac{B_j^2}{t} \right) = \frac{14.7^2}{5} + \frac{21.1^2}{5} + \frac{16.5^2}{5} + \frac{17.9^2}{5} + \frac{12.6^2}{5} + \frac{19.9^2}{5} = 361.75$$

and

$$\text{Block sum of squares} = 361.75 - 351.58 = 10.17$$

The new residual sum of squares (ss) is: (15)

$$\text{Residual ss} = \text{total ss} - \text{block ss} - \text{treatment ss}$$

and, therefore, the residual for degrees of freedom is: (16)

$$\text{Residual df} = \text{total df} - \text{block df} - \text{treatment df}$$

where block df = number of blocks - 1

These calculations are summarised in Table 7 overleaf.

Table 7. Analysis of variance table for an on-farm sheep supplementation trial With animals blocked by weight.

Source variation	df	Sum of squares (ss)	Mean square (ms)	F-value
Blocks	5	10.17	2.034	-
Treatments	4	4.86	1.215	6.09
Residual	20	4.00	0.200	-
Total	29	19.03		

Note that the most important effect of blocking was to reduce the residual mean square from 0.567 in the one-way analysis to 0.200. Since the residual mean square is a measure of the random variation, and the precision of treatment comparisons depends on its value, blocking has increased the precision of the experiment.

This is reflected in the F-value for treatments which is 6.09. The tabulated value (Table 4) for 4 and 20 df is 4.43 at the 1% level. The variation between treatments ($ms = 1.215$) is much larger than the random variation ($ms = 0.200$) and, therefore, the treatment differences are statistically significant.

The residual mean square is, in fact, the variance of the data after removing all treatment and block effects. It is often called the **residual variance (s^2)** and its square root, s , is referred to as **the residual standard deviation**.

The one-way analysis was carried out purely for the purposes of demonstration, to compare it with the randomised block analysis. In practice, only the randomised block analysis would have been done, since the animals had been 'blocked' by weight in the original experimental design. If this had not been done, the one-way analysis would have been the correct one. This emphasises the point that good experimental design can increase the precision of an experiment for little or no extra cost.

Detailed treatment comparisons

In the above analyses, the F-test examines whether or not there are significant differences between treatments. If significant differences are detected, the F-test gives no information about where the differences are occurring.

In order to obtain this information it is necessary to calculate first treatment means (if not already calculated). In our case, these are given in Table 6. Once treatment means are available, any two of them can be compared by using a t-test, though this may give rise to the problems of interpretation discussed below. Confidence intervals can also be obtained as in Part B of Module 11 (Section 1).

The standard error of the difference between two treatment means (SED) is defined as follows:

$$SED = \sqrt{s^2 \left(\frac{1}{n_1} + \frac{1}{n_2} \right)}$$

(17)

where:

s^2 = the residual variance (mean square), and
 n_1, n_2 = the numbers of observations in each mean.

If, as in our example, all means have the same number of observations, n , then equation (17) simplifies to:

$$SED = \sqrt{(2s^2 / n)} \quad (18)$$

For instance, if $n=6$ (i.e. there were 6 sheep in each treatment group), and $s^2=0.200$ (the residual mean square from Table 7), then:

$$SED = \sqrt{(2 \times 0.200 / 6)} = \sqrt{0.0667} = 0.258$$

The standard formula for a confidence interval can now be used, i.e.

$$\text{Difference} \pm t.SED \quad (19)$$

where t is the tabulated value.

The degrees of freedom for this t -value are the residual degrees of freedom from the analysis of variance. (This is because the variance estimate used to calculate the SED is the residual mean square with these degrees of freedom).

For instance, the tabulated t -value with 20 degrees of freedom at the 5% level is 2.09, therefore a 95% confidence interval for any treatment difference is:

$$\text{Difference} \pm (2.09 \times 0.258) = \text{difference} \pm 0.54$$

In our trial With sheep, treatment 2 is grazing + extra maize (mean wool yield = 3.00 kg) and treatment 3 is the same with protein supplement S_1 (mean wool yield = 3.80 kg) The difference between these treatments (0.80 kg) represents the effect of protein supplement S_1 . A confidence interval for this difference is:

$$0.80 \pm 0.54 \text{ or } 0.26 \text{ kg to } 1.34 \text{ kg}$$

So we can state that, at the 95% confidence level, the protein supplement S_1 increases wool yield by between 0.26 and 1.34 kg

A t -test gives a similar, but more limited, conclusion. A t -value can be calculated using the usual formula:

$$t = \frac{\text{difference}}{SED} = \frac{0.80}{0.258} = 3.10 \quad (20)$$

Since the calculated t -value is larger than the tabulated value (2.09), the difference is larger than would be expected by chance alone, and

so is statistically significant.

To avoid calculating a number of t-values and comparing them with tables, the above formula can be inverted. A difference will be statistically significant if:

$$\frac{\text{Difference}}{\text{SED}} > t \text{ (from the tables)}$$

(21)

This is equivalent to:

$$\text{Difference} > t \cdot \text{SED}$$

(22)

Equation (22) gives the **least significant difference (LSD)**.

In our example, SED = 0.258 and t is 2.09 (from tables for 20 df at the 5% level). Therefore, the LSD at 5% level is $2.09 \times 0.258 = 0.54$, i.e. any treatment difference larger than 0.54 is statistically significant at the 5% level.

There is a potential problem with t-tests and the equivalent LSDs if a rigorous approach is not adopted to tests of statistical significance.

For instance, in our example with five treatments there are 10 possible differences which could be tested (treatment 1 vs treatment 2, treatment 1 vs treatment 3, treatment 1 vs treatment 4 ... treatment 2 vs treatment 5 etc). With seven treatments, there would be 21 possible differences, with 10 treatment 45 differences, and with 15 treatments 105 differences

The nature of a significance test at the 5% level is that if there is no genuine difference, the test will erroneously indicate significant differences in 5% of cases (i.e. one time in 20) From this it can be seen that even with seven treatments, the problem of spurious statistical significance is serious A t-test comparing the best treatment with the worst is quite likely to be taken as significant, even when there is no genuine difference

If there are treatment comparisons which are specified before the experiment or are obvious from the nature of the treatments (pre-defined comparisons), then there is no real problem.

Example: In our trial with sheep (see page 6), obvious comparisons would be:

Treatment 1 vs treatment 2, the difference representing the effect of extra maize.

Treatment 3 vs treatment 2, the difference representing the effect of protein supplement S_1

Treatment 4 vs treatment 2, the difference representing the effect of protein supplement S_2 , and

Treatment 5 vs treatment 2, the difference representing the effect of protein supplement S_3 .

Another comparison of interest may be the mean of treatments 1 and 2 compared with the mean of treatments 3, 4 and 5. This compares the treatments with protein supplementation versus those without supplements. The comparison is straightforward, involving the calculation of an appropriate SED, using the general formula:

$$SED = \sqrt{s^2 \left(\frac{1}{n_1} + \frac{1}{n_2} \right)}$$

Example: If we have 6 animals per treatment group, the mean of treatments 1 and 2 is calculated from 12 animals, and so n_1 is 12. Similarly, n_2 is 18 for the mean of three treatments. The residual mean square (s^2) in Table 7 is 0.200, and so:

$$SED = [0.200 \times (1/12 + 1/18)] = (0.200 \times 0.139) = 0.167$$

This figure can then be used for confidence intervals or t-tests in the usual way.

Such statistical comparisons should only be used to examine pre-planned comparisons; they should not be used indiscriminately to see if any significant differences can be found between any treatments. This is because they are meant to assist scientific thinking, not to replace it!

Quantitative treatments

In some trials, the treatments may be different levels of the same factor, e.g. a feed supplement, as shown below:

- Treatment 1: unsupplemented control
- Treatment 2: supplement providing 25% of estimated crude protein (CP) requirement
- Treatment 3: supplement providing 50% of estimated CP requirement
- Treatment 4: supplement providing 75% of estimated CP requirement, and
- Treatment 5: supplement providing 100% of estimated CP requirement.

Here, there are five levels (0, 25, 50, 75 and 100) of a single supplement, which differs from the previous example in which the five treatments were five different diets. Obviously in such a trial, the overall response to supplementation is of interest, and comparisons such as 'treatment 2 vs treatment 4' are irrelevant.

The analysis of variance can be used as before, but, in addition, other more powerful tests can be carried out to detect trends due to treatment level. Details of such tests can be found in most good statistics textbooks, often under the intimidating heading 'Orthogonal polynomials'.

Basically, the use of orthogonal polynomials is the same as using a regression analysis of response (e.g. weight gain) on a level of treatment. (See Module 11 in

Section 1 for a description of regression analysis). The technique can detect linear trends and also test for non-linearity. And since it is looking for particular kinds of treatment effect, it is more powerful and sensitive than the general F-test.

Presentation of results

When presenting results in a report or scientific paper, it is not usually necessary to include the analysis of variance table. All that is necessary is:

- **treatment means**
- **a measure of the precision** (e.g. the standard error of the difference, SED) or the least significant difference (LSD)), and
- **an indication of statistical significance.**

Table 8 gives the result of the randomised block trial with 6 sheep per treatment, using the above three elements.

Table 8. Average wool yields for sheep on different feed supplementation regimes.

Treatment	Wool yield (kg)
Natural grazing	2.92
Grazing + extra maize	3.00
Grazing + maize + S ₁	3.80
Grazing + maize + S ₂	3.50
Grazing + maize + S ₃	3.90
SED (20 df)	0.258
F-test significance	P<0.01

ANOVA and factorial experiments

The merits of using a factorial design in on-farm experimentation have been discussed in Module 2 of this section. As already stated, the main advantage of factorial experiments is that interactions between the different factors can be examined in an ANOVA, and this is demonstrated by the example below which was taken from ILCA (1989a, Module 3). The calculations of sums of squares, mean squares and standard errors follow the general principles described for the randomised block design above.

Example: A trial was set up to examine the effect of water and nutrition on calf growth. Three watering regimes and two energy sources (local forage and sugar-cane residue) were used. This gave six treatment groups to each of which five animals were allocated. The mean weight gains during the trial period are shown in Table 9, together with standard error of the differences and statistical significance. The analysis of variance is given in Table 10.

Table 9. Mean weight gains of calves under different feeding and watering

regimes.

Treatment	Energy source	Watering regime	Mean weight gain (g LW/day)
1	local forage	1	119.6
2	local forage	2	138.6
3	local forage	3	143.2
4	sugar-cane residue	1	36.6
5	sugar-cane residue	2	71.4
6	sugar-cane residue	3	57.0
SED (24 df)			9.55
Significance of energy source			P<0.001
Significance of watering regime			P<0.01
Significance of energy x watering			n.s. ¹

¹ n.s. = not significant.

Table 10. Analysis of variance of calf weight gain.

Source of variation	df	Sum of squares	Mean square	F statistic
Main effect of energy source	1	46571	46571	204
Main effect of watering regime	2	4105	2053	9.0
Energy x watering	2	517	259	1.1
Residual	24	5482	228	
Total	29	56675		

In a factorial experiment, the total variation is partitioned into components due to treatment and the residual. (If the trial had been blocked at the design stage, there would also be a component for blocks). In our case, total variation (29 df) was partitioned into variation due to treatment (5 df) and the residual (24 df).

The treatment component can now be further partitioned into the following components:

- **main effect of energy source (1 df)**, which represents a comparison between local forage and sugar-cane residue (averaged over the three watering regimes)
- **main effect of watering regime (2 df)**, which represents a comparison among the three watering regimes (averaged over the two energy sources)
- **interaction between energy source and watering regime (2 df)**, which tests whether the effects of energy source and watering are independent of each other. If there is no interaction, then the difference between the two energy sources is the same for each watering regime. (Conversely, the differences between the watering regimes would be the same for each energy source.)

The statistical significance of these components can now be assessed by comparing the calculated F values with those tabulated for the relevant degrees of freedom. The F-test statistics for each component are given in Table 10.

To give an example, the F-test statistic for the main effect of watering regime is calculated using the formula:

Watering regime mean square

Residual mean square

which, in effect, is the ratio of the variation between the scans of the three watering regimes and the random variation.

If watering regime had no effect on weight gain, the only variation between the three means would be purely random variation. In such a case, the watering regime mean square would be similar to the residual mean square, and so the calculated F-value would be close to 1

If, on the other hand, watering regime affects weight gain, then the variation between the three watering regime scans will be considerably larger than random variation, i.e. the calculated F-value will be larger than 1.

The tabulated F-values for the main effect of energy source is:

F (1 and 24 df) at the 0.1% level = 14.03

and the values for the effect of watering regime or the interaction are:

F (2 and 24 df) at the 5% level = 3.40
at the 1% level = 5.61
at the 0.1% level = 9.34

The tabulated F-values tell us how large the calculated F-values have to be before we can be confident that the effects are genuine and not just due to random variation. In this example, the calculated F-value for the main effect of watering regime is 9.00 and the tabulated value at the 1% level is 5.61. We can, therefore, say that the main effect of watering regime is statistically significant ($P < 0.01$).

Having done an F-test, we may be interested in more detailed treatment comparisons. Standard errors and confidence intervals can be calculated in the usual manner (see pages 16-17), using the residual mean square which, in our example, was $s^2 = 228$ with 24 degrees of freedom.

The standard error of the difference between two individual treatment means (each based on five animals) is:

$$SED = \sqrt{2s^2 / n} = \sqrt{2 \times 228 / 5} = 9.55$$

The least significant difference (using t with 24 df at 5% level = 2.06) is:

$$LSD = t.SED = 2.06 \times 9.55 = 19.7$$

Therefore, using the treatment means in Table 9 we can conclude that:

- local forage gives higher weight gains than sugar-cane residue (for all watering regimes), and
- watering regimes 2 and 3 give higher weight gains than regime 1 (for both energy sources).

In the above experiment there was no significant interaction effect, though the effects of both energy source and water regime were statistically significant. However, interactions can often have significant effects in factorial experiments, and when this occurs, it can be misleading to put much emphasis on main effects. This is because the main effect is the effect of one factor averaged over the level of the other factor(s). In contrast, when the interaction is significant, the effect of the factor depends on which level of the other factor(s) is being considered. An average effect may, therefore, be of limited use.

Example (ILCA, 1989a, Module 3): A trial was set up to determine the crude protein (CP) concentration of dry matter at three levels of fertiliser application used in combination with four legume/grass seed mixtures.

In this example, both of the main effects and the interaction are significant (Table 11).

Table 11 Analysis of variance for crude protein concentration of dry matter.

Source of variation	df	Sum of squares	Mean square	F statistic	Significance level
Main effect of fertiliser level	2	66.3	33.2	13.94	P<0.001
Main effect of seed mixture	3	1721.5	573.8	241.10	P<0.001
Fertiliser x mixture	6	69.1	11.6	4.88	P<0.01
Residual	12	28.5	2.38		
Total	23	1886.0			

Let us now examine the treatment means in more detail. First, it is useful to calculate standard errors of differences and least significant differences using the same procedure as described for the randomised block analysis. The calculations are all based on the residual mean square (s^2) which, in our example, is 2.38 with 12 degrees of freedom.

The standard error for the difference is calculated as:

$$SED = \sqrt{(2s^2 / n)}$$

where n is now the number of individual observations (plots) making up the means.

Thus, in our trial:

n = 8 for fertiliser means
 n = 6 for seed mixture means, and
 n = 2 for individual treatment means.

In other words, each individual treatment mean is the mean of two plots and, for instance, each fertiliser mean is the mean of four individual treatment means, and, therefore, the mean of eight plots.

Therefore, SED for comparing fertiliser means is:

$$SED = \sqrt{(2 \times 2.38 / 8)} = 0.77$$

The t-value for 12 df at the 5% level is 2.18 and so the least significant difference for fertiliser means is:

$$\text{LSD} = t \cdot \text{SED} = 2.18 \times 0.77 = 1.67$$

Table 12 below gives mean CP concentrations under different treatments together with the relevant LSDs.

Table 12. Mean crude protein concentration of dry matter.

Seed mixture	Fertiliser level			Mean
	F1	F2	F3	
	g/kg DM			
M1	7.0	8.0	6.5	7.1
M2	8.0	8.0	9.5	8.5
M3	16.0	16.0	22.0	18.0
M4	26.5	24.5	33.5	28.2
Mean	14.4	14.1	17.9	15.5

LSD at 5% significance level for:

fertiliser means = 1.67

seed mixture means = 1.94

individual means = 3.36

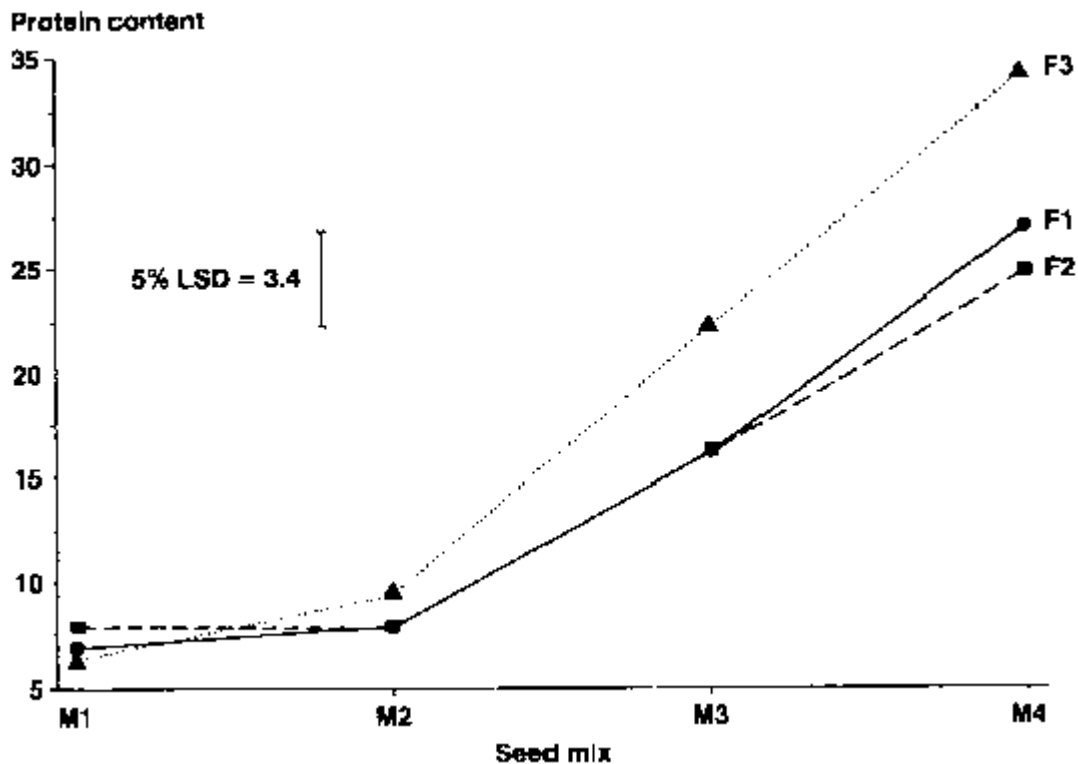
Comparing the means for the three levels of fertiliser application, we can see that F3 (with a mean of 17.9) is higher than the other two fertiliser means (14.4 and 14.1). Since the LSD is 1.67, the difference between two fertiliser means has to be larger than 1.67 to be statistically significant. (This is averaged over the four seed mixtures).

Similarly, the mean for seed mixture 4 (M4) is higher than the M3 mean which, in turn, is higher than the M1 and M2 means. There is no significant difference between M1 and M2. (These comparisons are averaged over the three fertiliser levels).

However, because the interaction is significant, the situation is slightly more complicated. Examining the individual treatment means, we can see that, with LSD = 3.4, there is no fertiliser effect for seed mixtures 1 and 2, and that F3 gives much higher protein content than F1 and F2.

There is no routine method to determine which treatments give rise to a significant interaction. Often than not, it is a matter of examining the table of means and their LSDs, and of determining how the response to one factor depends on the level of the other factor. Sometimes, simple graphs can be helpful, as can be seen from Figure 1. Separate lines are given for each fertiliser level, and each line shows how protein content depends on the seed mixture for that fertiliser level. A small bar also shows the 5% LSD.

Figure 1. Graphical display of interaction.



The overall effect of seed mixture is obvious from the rise in all the lines from M2 to M4. The lines for F1 and F2 are very similar. The line F3, however, while being the same for M1 and M2, is different for M3 and M4. This suggests that while there was no fertiliser effect for seed mixtures M1 and M2 with F3, there was an effect with M3 and M4 which, in addition, was greater than with the other two fertiliser levels. If there were no interaction, the three lines would be parallel.

Unbalanced data

In all the analyses of variance described above, the experiments have been 'balanced', i.e. every treatment was applied to the same number of experimental units (animals or plots). In general, it is best to design experiments in this way, since balanced designs make the most efficient use of experimental resources and are more straightforward to interpret.

In practice, however, experiments are often unbalanced, particularly in the case of animal experiments on-farm. This can arise from a number of causes:

- There may not be the right number of animals (or farmers) available at the start of the trial for equal replication, but it may be desirable to use all rather than eliminate some from the trial.
- In a survey, as opposed to a designed experiment, the researcher will not have control over the number of animals or households in each group.
- There may be good scientific reasons for having more animals on some treatments than on others.

- Animals may die during the trial, from reasons unrelated to the treatments.
- Farmers may stop participating in the trial, for reasons unrelated to the treatments.

In the last two cases, we would have problems interpreting the results if the reasons for dropping out were dependent on the treatments.

For instance, in a trial carried out to examine the effect of parasite control on weight gains it may happen that more animals die in the untreated group than in a group receiving a positive treatment. Analysing the data for surviving animals would, in this case, ignore the effect of the treatment on the mortality rate (which may be one of the major benefits of treatment) and the treatment comparisons could be severely biased.

Even without this problem, analysing unbalanced experiments is problematic. Firstly, one would need a sophisticated statistical analysis programme to analyse results from such experiments. Secondly, even with suitable computing facilities, the interpretation is not straightforward, and presentation of results in a concise form is not always possible.

It is beyond the scope of this manual to go into the details of such analyses, and most of the textbooks which cover this subject have a strong mathematical orientation. Nevertheless, an artificial example from Snedecor and Cochran (1980) is given below to illustrate the main problem.

Example: Imagine a feeding trial with male and female animals under two diets. The number of animals in each of the four groups and the total weight gains per group are given in Table 13. (The data are artificial)

Table 13. Total weight gains by diet and sex: Unbalanced experiment.

	Weight gain (g LW/day)			
	Female	Male	Total	Mean
Diet 1				
Total weight gain	160	60	220	22
(Number of animals	8	2	10)	
Diet 2				
Total weight gain	30	200	230	23
(Number of animals	2	8	10)	
Total				
Total weight gain	190	260	450	
(Number of animals	10	10	20)	
Mean weight gain	19	26		22.5

While there are 10 animals on each diet and 10 of each sex, the trial is still unbalanced. Diet 1 has eight females and two males, and this ratio is reversed for Diet 2. The simple mean weight gain for the 10 animals receiving Diet 1 is 22 (220/10), and for Diet 2 it is

23, suggesting that Diet 2 is (slightly) better than Diet 1. This suggestion is misleading and wrong.

The mean weight gains per animal for all four groups are given in Table 14.

Table 14. Treatment means for the data in Table 13.

	Weight gain (g LW/day)		
	Female	Male	Mean
Diet 1	20	30	25
Diet 2	15	25	20
Mean	17.5	27.5	22.5

For females, Diet 1 gives 5 units of weight gain more than Diet 2 (20 compared with 15). The effect for males is the same, animals on Diet 1 gain 5 units more than animals on Diet 2. The obvious and correct conclusion is that animals on Diet 1 gain 5 units more than those on Diet 2, irrespective of which sex they are. The simple means from Table 13 lead to the wrong conclusion that animals on Diet 1 gain 1 unit less than those on Diet 2.

The confusion is due to the imbalance of sexes in the two diets. It is fairly obvious in this artificial example, but may not be so obvious in larger, more complex practical situations. The problem of analysing unbalanced factorial experiments cannot be resolved by analysing each factor separately.

A valid method of analysing unbalanced experiments is to carry out a one-way analysis of variance, with each 'cell' of the factorial structure as a treatment.

For instance, our artificial diet x sex example could be analysed as an experiment with four treatments.

This approach will give correct means and standard errors for individual treatments. However, its major drawback is that the effects of the various factors and their interactions cannot be separated. (That is why a factorial design is usually preferred). The other problem is that main-effect means and standard errors cannot be obtained. Finally, in many practical experiments, the number of treatments (or cells) is quite large. A significant overall F-test would indicate that not all treatments are the same, but would be of little use in determining where the differences are occurring.

Part B: Financial analysis of on-farm trials

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Improved technical performance (e.g. higher daily weight gain) does not necessarily coincide with financial attractiveness. Therefore, it is important to consider the financial implications of the adoption of new technology at the farm level.

For instance, a technology tested on-farm may have the potential to increase production but, at the same time, be financially unattractive. If the returns obtained are not sufficient to cover the costs involved, or if there are more attractive opportunities available elsewhere for the resources (e.g. labour, land, capital) involved (e.g. off-farm employment or interest on bank deposit accounts), wide adoption could not be expected.

The following discussion deals with some of the simpler techniques used to assess the financial attractiveness of innovations being tested in on-farm trials. The methods outlined are also applicable to technological assessment during the pre-screening stages of livestock systems research (Module 1, Section 1; Module 1, Section 2).

Techniques such as simplified programming, linear programming and simulation are not covered here, but they are useful when complex interactions between farm resources are envisaged. Whilst applicable in terms of the underlying principles involved, production economics theory (e.g. input/output relationships) is beyond the scope of this manual. For a discussion of the theory of production economics the reader can refer to any basic economics text.

Definitions

To facilitate the discussion which follows, a few terms need to be defined at the outset. They are:

ENTERPRISE. In the present context, the term enterprise denotes the production of a particular commodity or group of commodities for the purposes of home consumption or sale (e.g. livestock enterprise, cropping enterprise), but it does not specify the method of production involved.

ACTIVITY. For every enterprise, there may be various ways of producing a commodity. Each possibility represents an activity (e.g. zero-grazing dairy activity, free-roaming goat activity).

INTERMEDIATE PRODUCT. Some farm products are neither sold nor used for home consumption but are used as **inputs** in the production of another commodity (e.g. forages grown for the purposes of fattening or milk production). These are known as intermediate products and costs incurred in their production can normally be allocated wholly or in part to a particular farm activity. Examples of how intermediate products are valued are given in Module 4 (Section 1) and Appendix 1 of Module 2 (Section 2).

OUTPUT. This is the amount of product produced by an activity (e.g. the amount

of milk produced by dairying). It may be sold or retained for home consumption. The manner in which output is valued will depend on whether the household is normally a surplus or deficit producer of the commodity.

For a surplus producer, output should be valued at the price per unit at which it could be sold to a buyer at the producer's farm, or it could be valued at the local market price less the cost of taking it to the market (if this can be estimated).

For a deficit producer (i.e. one who has to buy the product on a regular basis), the amount produced should be valued at the price at which it can be purchased if delivered to the farm (module 4, Section 1). Black market prices may need to be used if they are more appropriate to the circumstances (Barlow et al! 1986).

If output is exchanged for goods or services in kind (e.g. for labour), then it should be valued on the basis of the exchange item used.

For instance, if an animal is exchanged for labour, then its value is equivalent to cost incurred when hiring the same amount of labour from other sources at that time of the year.

GROSS ENTERPRISE (ACTIVITY) INCOME. This is the amount of output produced multiplied by the price which is relevant to the particular commodity. In livestock enterprises it may, when applicable, include notional income arising (declining) from changes in the value of the herd kept for the purposes of enterprise/activity.

OPPORTUNITY COST. This is the return (extra income) that would be earned by using one unit of a factor of production in the best alternative use of it to the one being considered. Opportunity costs may be lower or higher than market prices.

VARIABLE COST. A variable cost is a cost which varies directly with the level of output produced.

For instance, costs incurred for vaccines, drugs and dipping, feed supplements, labour hire and marketing services fall into this category. Again, it may be appropriate to use black-market prices for the valuation of variable inputs in some circumstances. The principles involved in the valuation of variable inputs are discussed in Part B of Module 4 (Section 1).

FIXED COSTS. A fixed cost is one which remains constant irrespective of the level of output produced.

For instance, annual depreciation on assets such as ploughs and carts, rents and maintenance costs, are fixed costs.

CAPITAL COSTS. A capital cost can be defined as an investment cost incurred for the purposes of increasing **future** productive capacity.

For instance, investments in fencing, pasture establishment, breeding cows, land and machinery are all investment costs.

GROSS MARGIN. The total gross margin (TGM) for a particular enterprise or activity is defined as:

TGM = total gross income - total variable costs = (total output x price) - total variable costs

Total **farm** gross margin is the sum of gross margins for all individual enterprises or activities. Note that the gross margin for an activity excludes fixed (or overhead) costs. It therefore represents the specific contribution made by an activity to farm profit.

Gross margins

Gross margins can be used to indicate the relative profitability of a technology being tested during the on-farm trial phase. They are easy to calculate and, for this reason, are often used to assess the financial attractiveness of new alternatives being introduced at the farm level.

A gross margin is normally calculated for a full production year. For small ruminants, which breed three times every two years on average, output in terms of kids/lambs born in one year should be expressed as an annual average over the two-year period (e.g. 1.5 kids/doe/year).

In order to make meaningful comparisons between the various options available, gross margins need to be expressed in **comparable terms** (e.g. on a per hectare, per livestock unit, per hour of labour basis). Expressed in this way, they provide an indication of the returns that can be expected if alternative activities are expanded by the use of one unit of the resource concerned.

Comparisons are most useful when they are made in terms of returns to the resource which limits the household's income most. New production techniques are likely to provoke interest in the target area if the gross margin per unit of that resource (factor) is higher than it is for other options presently available.

For instance, if labour is the scarce resource limiting production, it is relevant to compare activities on the basis of gross margin per man-day, if land is limiting, comparisons should be made on the basis of gross margin per hectare.

However, there are considerations other than returns to the most limiting factor which influence the adoption of a new technology. They are risk, capital and time-lag.

Risk. An activity with a potentially high gross margin may also be relatively risky and, therefore, unattractive.

Capital. If the activity requires additional capital or credit it may be beyond the reach of the farmer, despite its apparent profitability.

Time-lag. The gross margin (which is usually an estimate of the relative profitability of a fully established activity) gives no indication of the time period involved before full potential is reached. If the technology requires a long 'gestation' period (as may be the case with some breeding schemes), the activity

may be unattractive even if returns at full establishment appear relatively attractive.

To deal fully with issues such as these, more complex methods of economic analysis are required, but these are beyond the scope of this manual.

A general gross margin table is given overleaf. The gross margin derived will depend on the assumptions built into the calculation. The best practicable estimates of output levels, prices and costs should be sought and sensitivity tests should be carried out to examine the effect of altering the assumptions made.

Note that for livestock gross margins, closing and opening numbers (values) of animals in the herd/flock are included to allow for annual variations in the number and value of stock owned or held. When the herd or flock is in a steady state (i.e. there is no net change in the number owned/held), and when there is no reason to assume that values/head have increased or decreased, the two values cancel out in the gross margin calculation. In such cases, their inclusion in the gross margin table is a matter of choice.

Gross margin table

Output	Value
	Sales
Amount sold/exchanged (by item) x price/unit (including livestock and livestock products, e.g. milk, hides, skins)	
	+ Home consumption
Amount consumed x value/unit	
	Total gross value of output
	+ Closing value of the herd/flock
Number of animals in herd/flock by age/sex class x estimated average value	
	- Opening value of herd/flock
Number of animals in herd/flock by age/sex class x estimated average value	
	= Gross income
	- Variable costs
Variable inputs used (itemised) x price paid/unit	
	Total variable costs
	Total gross margin
	Gross margin per LU
	(or ha or man-day)

Example: Suppose that small-scale dairying has been introduced in a traditional farming area and that the average farmer involved does not supplement his animals with concentrates. Herds, on average, consist of two grade dairy cows stocked at the rate of one cow per 2 hectares of top-dressed natural pasture. Calves are sold soon after birth

and artificial insemination is used as a common practice. A 50% weaning rate is commonly encountered. Milk is marketed through the local cooperative and a percentage marketing fee is levied. An average operation of this kind requires about 125 man-days of labour. The average gross margin per cow, per hectare and per man-day have been calculated as follows:

Output	Value (\$)
Sales	
600 litres milk/cow x 2 cows x \$0.50/litre	600.00
1 calf x \$60	60.00
+ Home consumption	
200 litres milk x 2 cows x \$0.50/litre	200.00
Total gross value of output	860.00
+ Closing value: 2 cows x \$300 (average value)	600.00
- Opening value: 2 cows x \$300 (average value)	600.00
Gross income	860.00
Less variable costs	
Veterinary expenses (vaccines, drugs, care @ \$10/cow/year x 2 cows)	20.00
+ Artificial insemination @ \$10/cow/year x 2 cows	20.00
+ Fertiliser 100 kg nitrogenous fertiliser @ \$1.50/kg	150.00
+ Marketing costs (milk) @ 5% of marketed milk value	30
Total variable costs	220.00
Total gross margin	640.00
Gross margin/cow	320.00
Gross margin/ha	160.00
Gross margin/man-day	5.12

Suppose now that a series of on-farm trials have been conducted to assess the effect of feed supplementation with a protein/energy concentrate on milk production for the kind of management practices encountered. The aim is to improve the income by measures considered accessible to the average farmer.

Based on the trials, concentrate feeding at 100 kg/cow/year was recommended. This was assumed (again on the basis of the trials) to increase marketed milk offtake/cow by 50% but it would also result in extra time required for milking and marketing of the additional milk produced.

Man-day requirements/farm for the average operation will thus increase by 20%, and labour is considered the limiting factor in the area. All other outputs and costs remain unchanged. Calculate the gross margin per man-day and decide whether the technology would be recommended.

Output	Value (\$)
Sales	
900 litres milk/cow x 2 cows x \$0.50/litre	900.00
1 calf x \$60	60.00
+ Home consumption	
200 litres milk/cow x 2 cows x \$0.50/litre	200.00
Total gross value of output	1160.00
+ Closing value: 2 cows x \$300 (average value)	600.00

- Opening value: 2 cows x \$300 (average value)	600.00
Gross income	1160.00
Less variable costs	
Veterinary expenses @ \$10/cow/year x 2 cows	20.00
+ Artificial insemination @ \$10/cow/year x 2 cows	20.00
+ 100 kg nitrogenous fertiliser @ \$1.50/kg	150.00
+ 100 kg concentrate/cow @ \$1.00/kg x 2 cows	200.00
+ Milk marketing costs @ 5% of mark, milk value	45.00
Total variable costs	435.00
Total gross margin	725.00
Gross margin/cow	362.50
Gross margin/ha	181.25
Gross margin/man-day	4.83

Note that while total gross margin and the gross margins per cow and hectare have increased as a result of the use of concentrate, the gross margin per man-day has fallen. With labour being the limiting factor it is, therefore, unlikely that the new technology will be attractive to the average operator. Even if there were to be a significant increase in returns per man-day, three additional factors would need to be considered before making the recommendation on a wider basis, namely:

- returns per man-day in other farm and non-farm activities (e.g. crops, off-farm employment). If other, more attractive options were available, it is unlikely that farmers would be interested in recommendations to use concentrate.
- the sensitivity of results to variations in prices, costs and output levels. Highly sensitive results should be treated with caution.
- the availability of concentrates on a continuing basis.

Partial budgets

Partial budgeting is an extension of gross margin analysis. It is used to assess the financial worth of a planned incremental (or partial) change in farm organisation which normally involves the need to make additional capital expenditure at the outset. The analysis conducted is concerned **only** with those **annual costs and returns directly affected** by the change. A return to the extra capital invested is calculated to permit comparisons between various alternative investment possibilities.

Where the capital investment requires a 'gestation' period before full establishment is reached (as is common with livestock), partial budgeting provides an indication of the annual financial viability of the proposal **at full establishment**. It therefore ignores the period of capital development required before this phase is reached. This time period may be very important in the assessment of the viability of a technology.

If the partial budget indicates that the proposal is not viable, then further analysis will be unwarranted. If the result is viable, the use of cash-flow budgeting techniques (described below) will probably be needed to assess further the worth of the project. In such cases, partial budgeting should be seen as a **preliminary**

indicative step in the identification of potentially viable changes on the farm.

The technique is simple and is commonly used in the screening and appraisal of cropping technologies during the diagnostic, design and testing phases of livestock systems research (Byerlee and Collinson, 1980; Barlow et al, 1986). With livestock, however, other complementary techniques of analysis may be required to assess the long-term worth of a proposal.

Four basic questions in partial budgeting

When assessing a partial change in farm organisation which may or may not involve additional capital expenditure, four basic questions will be asked. They are:

- What **extra annual costs** (variable and fixed) result from the change?
- What **extra annual gross income** will be obtained as a result of the change?
- What **extra annual gross income** will be foregone as a result of the change?
- What **extra annual costs** (variable and fixed) will be foregone or saved as a result of the change?

Example of a layout of a partial budget for a change in farm organisation resulting from the introduction of a new technology.

Benefits	Costs
1. Extra annual gross income	3. Extra annual costs
2. Annual costs saved	4. Gross income foregone

Note that:

- All questions relate to the **annual** effect of the change at **full establishment**.
- Hidden benefits and costs incurred as a result of the change must be taken into account when doing the analysis. These include the costs which will be **saved** by making the change as well as any income which will be given up or foregone.

Hidden benefits and costs may be important if, for instance, the introduction of one activity means that another needs to be altered in some way. Thus, the introduction of new technologies at the farm level will often have implications for other farm and non-farm activities, and the hidden costs and benefits associated with it can sway the decision for or against a proposal. (See also 'Whole-farm budgeting').

- When doing partial budget analysis, all annual extra costs

associated with the change should be fully accounted for. These include both overhead and variable costs.

As regards the additional capital costs involved, it is not correct to charge the whole cost to the annual partial budget statement. Capital costs must be 'spread over the life' of an asset, and an annual allowance for depreciation and replacement must be calculated. This is commonly known as a '**depreciation allowance**' and is defined in this manual² as follows:

² Other more complex formulae and methods of calculation can be used but the formula given here is sufficient for the purposes of this manual.

$$\text{Annual depreciation allowance} = \frac{\text{original value} - \text{estimated salvage value}}{\text{estimated life of the asset (years)}}$$

Example: Assume that on-farm trials have been conducted to examine the benefits of supplementing oxen at the end of the cropping season. Farmers participating in the trial were asked to allocate a hectare of arable land to a fodder crop (e.g. Napier grass) for this purpose.

The underlying objective of the trial was to improve the condition of draught animals during the dry season, thereby increasing the chances of early ploughing at the start of the rainy season. This, it was assumed, would improve the yield of maize (the staple food) significantly.

In order to make the change, farmers were required to:

- **invest in fencing equipment at a cost of approximately US\$ 300**

It was estimated that fences would have an expected life of 20 years after which they would be worth nothing.

- **use an area previously allocated to maize production to grow Napier grass**

The costs of establishing the fodder crop were estimated at US\$ 135/ha/year. The gross margin from maize in the target area averages US\$ 100/ha, with variable costs per hectare being in the order of US\$ 40 (i.e. the opportunity cost of land is US\$ 100/ha). The average household in the area crops 4 ha under maize every year.

- **plough at the onset of the first rains**

The poor condition of animals at the end of the dry season results in late planting which is said to be the major constraint to improved maize production.

Trial results indicated that if these requirements were met, maize gross margin on the remaining 3 hectares of land would increase by approximately 100%. Based on these results, should the change be recommended for an average farm in the target area?

A partial budget for the improvement of maize production by supplementary feeding of oxen is shown in Table 15.

Table 15. Testing the financial advantages of early ploughing.

BENEFITS	COSTS
Extra annual gross income	Extra annual costs
Maize: 3 ha @ \$100/ha - \$300	Fencing: annual depreciation $(\$300 - \$0)/20 = \$15$
	Fodder crop: Establishment costs = \$135
Annual costs saved	Gross income foregone
Maize 1 ha (variable costs) @ \$40/ha \$40	Maize: 1 ha (gross returns) @ \$140/ha = \$140
Total benefits - \$340	Total costs - \$290
Net annual benefit resorting from the change: + US\$ 50	
<p>Net returns to the extra capital invested in the proposal can be estimated as follows:</p> <p>Returns to capital (%) = $(\text{Net benefits}/\text{Total capital invested}) \times 100$</p> <p>$= 50/300 \times 100 = 16.6\%$</p> <p>Although the proposal would result in a net improvement in annual income (by 12.5%), it is highly unlikely that it would be recommended for the following reasons:</p> <ul style="list-style-type: none"> • the net gains in absolute terms (US\$ 50) resulting from the change are small • minor changes in the assumptions made about maize production would lower the net gains substantially <p>For instance, an assured increase in the gross margin of maize by 80% (not 100%) would effectively wipe out any increase in income. The result is thus highly sensitive to the assumptions made, making the innovation a risky proposition.</p> <ul style="list-style-type: none"> • relative returns (i.e. 17%) to the extra capital invested are comparatively low and would probably be higher by investing in other alternatives (e.g. cattle). 	

The example serves to demonstrate the importance of considering the financial as well as the technical implications of proposed technologies tested on-farm. While apparently attractive in terms of its technical potential, the innovation fails on financial grounds and could not therefore be recommended. Of course, there would be little point in conducting on-farm trials if the indications beforehand were that the proposal would never pass on this or any other basis. For this reason, all new technologies should be carefully screened during the design phase, using partial budgeting.

Whole-farm budgeting

This manual has stressed the importance of recognising the various linkages which exist within a system. Interventions such as those outlined above will almost invariably affect other farm and non-farm activities carried out by the household, and it may be the effect on these activities which ultimately influences the farmer's decision to accept or reject a proposal.

For instance, if labour is a limiting factor on the farm, and if the technology being proposed implies a shift in labour use away from other activities considered important, then the innovation may be

unattractive if labour cannot be hired or obtained from other sources to compensate for its lack

A whole-farm approach is thus often necessary to assess the full impact of a new technology. Linkages which may otherwise be ignored can be more clearly recognised by the use of whole-farm budgets which involve the calculation of gross margins for all activities before and after the change is made.

A whole-farm budget is derived as:

(d All farm activity total gross margins) - (d farm overheads)

When this expression is calculated, the **net farm income** accruing to the whole farm is derived.

The derivation of a whole farm budget for the situation before and after adoption may also point to implied shifts in the composition of gross income and the manner in which it is received (e.g. its seasonal distribution), all of which can also affect farmers' attitudes towards change.

One further advantage of the whole-farm approach is that, by deriving gross margins for all other farm activities (crop and livestock), management weaknesses in other areas of production can often be isolated. By identifying these, further scope for improvement can sometimes be identified. Gross margins thus have a **diagnostic** as well as a **prescriptive** function.

Cash-flow budgeting

Minor activity changes with little or no investment of capital involved will often increase farm income in a relatively short time period. When this is the case, simple methods of analysis such as those outlined above will adequately assess the financial viability of a proposed intervention.

In other cases, improvements take longer and substantial injections of capital are required. Cash-flow budgeting methods should then be used to assess the financial viability of the proposal being considered. Those appropriate to the analysis of on-farm trials are briefly outlined below³.

³ For details, the reader is referred to Gittinger (1982), Putt et al (1987) and Chisolm and Dillon (1988).

Net cash flow

The first step in cash-flow budgeting is to derive a net cash-flow (NCF) budget, by **considering all the cash costs and benefits which accrue directly** to the proposal. This requires the calculation of all expected expenditures and receipts resulting from the implementation of the proposal at the farm level. The layout of the NCF calculation is shown overleaf.

Note that all benefits and costs should be itemised over time and be entered in the budget as they occur over time. The components of the NCF calculation and other relevant aspects are discussed below in some detail.

Example of a conventional layout for the derivation of the net cash flow for a farm project:										
	Year									
Item	1	2	3	4	5	6	7	8	9	10
A. Gross benefits/income										
	Total benefits									
Less										
B. Costs										
	- variable costs									
	- overhead costs									
	- capital costs									
	Total costs									
C. Net cash flow (A - B)										

The net cash flow for a particular year is the difference between gross benefits and costs in that year. It can be either positive or negative. Negative figures for any one year are usually indicated by brackets (). When a period of development is needed, as is often the case with new technologies requiring substantial investment or changes in management (which we shall hereafter refer to as 'projects'), negative figures are commonly encountered during the initial few years after which the net cash flow becomes positive.

Benefits. The benefits which can easily be identified are those where output is sold through formal and informal market channels. These financial benefits are equivalent to the volume of sales of all extra outputs resulting from the investment, multiplied by the price received for those outputs. Where output is not sold but used for other purposes (e.g. home consumption), an 'imputed' cash benefit is included in the benefit stream.

Some innovations do **not** lead to marketable output in the conventional sense; they **save costs** instead (e.g. labour costs). When these cost savings can be quantified, they should be included as part of the benefits stream. If it is difficult or impossible to quantify some benefits, they should at least be stated in the final assessment of the proposed innovation.

Costs. Most costs are straightforward and can be entered into the budget as they are expected to occur. Depreciation of capital equipment is not entered as an annual cost since this is an **allowance**, not a monetary cost. It is, however, reflected in the budget by entering both the original cost of the equipment (i.e. in full at the time of expenditure) and its replacement cost (minus anything received upon sale or disposal).

When funds are borrowed by the farmer to finance the capital required to introduce a new technology, there are two ways of treating the cash flows involved. Normally, the correct way is to include the interest payments and repayments of capital in the cash flow at the time when these occur, but not to include in the cash

flow the cost of that part of the capital investment (e.g. in the form of equipment or works) which is financed by borrowed funds.

For instance, if all of the cost of fencing (which is carried out in year 1 of the project at a cost of \$300) is financed by a loan whose payments of interest and capital take place in nine equal instalments of \$57 per year in years 2 - 10 inclusive, usually the correct thing to do is to ignore the payment of \$300 to the fencing contractor in year 1 but to include the annual \$57 loan-service payments to the bank in years 2 - 10.

Occasionally, it may be correct to include the payment of \$300 in year 1 and to ignore the annual payments. It is never correct to include them both. However, this is not the place to explain when it is core appropriate to include the original capital cost rather than the loan-servicing payments.

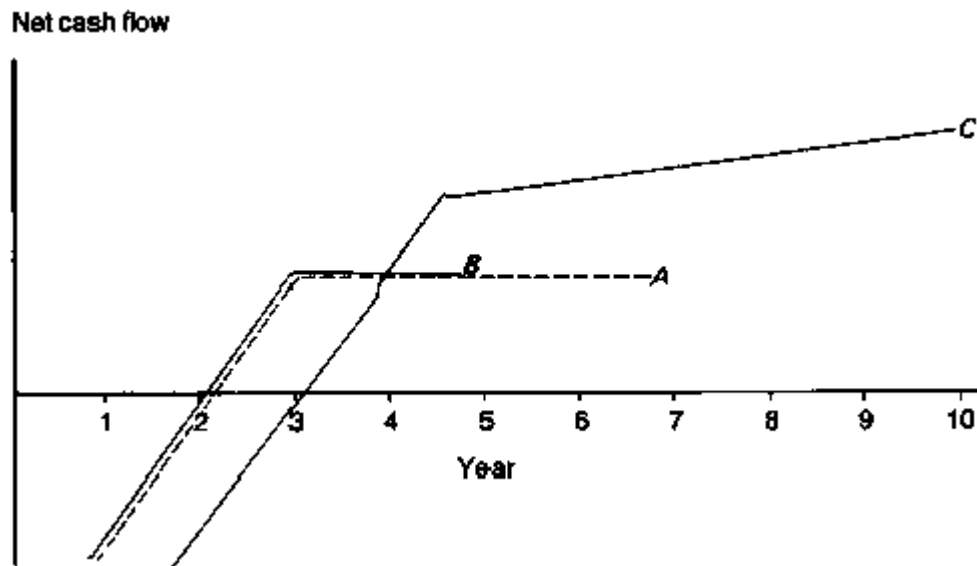
Salvage values. For assets which are retained till the end of the project period and which at that time have some value, it is important to enter their depreciated value (normally the value at which they could be sold) in the benefits stream in the last year of the cash flow. Part B of Module 4 (Section 1) shows how the depreciated value of an asset can be calculated.

Length of project period. The length of the project period (i.e. the number of years included in the cash flow budget) is a matter of choice. There is only one guiding principle - the period should extend over a number of years after the project has reached full establishment. Normally, a period of 7-10 years beyond this point will be sufficient. Reasons for this will become clearer later on.

One further point should be borne in mind - price, cost and output predictions become more imprecise as time proceeds. Attempts to draw up cash-flow budgets which are unnecessarily long therefore tend to be rather meaningless.

The net cash flow for a project proposal can be depicted as a graph to provide an indication of the manner in which net returns are generated through time (Figure 2). Sometimes, an examination of the net cash-flow pattern would be sufficient to permit the ranking or selection of different alternative proposals on financial grounds. However, more often than not it is impossible to rank investment opportunities by mere inspection of the cash-flow pattern, and, in such cases, additional analytical tools will be required. (See Figure 2 and following commentary).

Figure 2. Net cash-flow patterns for three potential farm investment options.



In Figure 2, investment option 'A' is obviously superior to 'B' in terms of its long-term financial contribution to the household. Despite the fact that the net cash flow is identical for the two projects during the first 5 years, 'A' continues for a longer period and would, therefore, be preferred.

The choice between options 'A' and 'C' is, however, less straightforward. While 'C' costs more initially (has a greater negative cash flow), and takes longer to reach full establishment, it generates greater net cash flow than 'A' once that point is reached. For such projects, a common basis for comparison (other than by visual inspection) must be established. Some of the methods of financial analysis applicable to such situations are discussed below.

Financial analysis of long-term projects

Three methods are applicable, and they will only be discussed very briefly here. They are:

- **net present value (NPV)**
- **benefit: cost analysis, and**
- **internal rate of return (IRR).**

Which method is the most applicable depends on the particular circumstances of the case. The issue is discussed by Gittinger (1982) to whom the reader is referred for further advice.

All the three methods rely on the use of discounting procedures. Discounting is used to express costs and benefits paid or received in the future in present-value terms.

The issue is very complex and cannot be dealt with satisfactorily here. In brief, it is generally recognised that, even in the absence of general inflation, a sum of \$100 now is more valuable to most people than \$100 in the future, say in 10 years' time. To put this another way, \$100 in 10 years' time is less valuable than \$100 now, i.e. it is 'discounted' in comparison to its value now. It is less valuable then than now for two main reasons:

- If I have \$100 now, I can invest it (e.g. in an interest-paying bank account), so that \$100 placed in such an account will, at 10% interest rate, grow to be \$259 in 10 years' time, or, to put it another way, \$39 put into this account now will, at 10% interest rate, grow to be \$100 in 10 years' time.

This reason can be summarised in the expression '**the opportunity cost of capital**', indicating that tying up capital in one activity prevents its being used in another activity where it can earn a return, e.g. interest.

Therefore, if activity A will give me a benefit of \$100 in 10 years' time, this sum must be 'discounted' at a rate equivalent to the interest I could have earned by investing it at the beginning of the project in the next best alternative use, say activity B.

For most farmers in Africa, investment in their own on-farm or off-farm enterprises is more profitable than putting money in the bank, yet they cannot borrow from the bank as much as they would like to. The opportunity cost of capital to them is, therefore, the return they can obtain from investing the money in the best of their own enterprises.

- Offered a choice between consuming (or otherwise enjoying) something now or later, many people will prefer to do so now. It follows that they will have to be offered a little more later than they will get now if they are to be persuaded to defer voluntarily their enjoyment.

The ratio between what is offered now and what has to be offered later in order to voluntarily defer consumption is known as the '**subjective time preference**' and is usually expressed as an annual rate of discount, in which case it may be referred to as the '**personal discount rate**'.

For instance, if an individual is indifferent to US\$ 100 received today and US\$ 110 received in a year's time, he/she is effectively discounting US\$ 110 by 10%. In other words, the individual would be discounting the US\$ 110 by a factor of 0.9091, i.e. $110 \times 0.9091 = \text{US\$ } 100$.

Although acknowledging that the discount rates that reflect the opportunity cost of capital to a farmer and his personal discount rate may, in theory, differ from each other, economists normally use a single rate to represent them both in practice. However, some farmers may be able to borrow capital from others to finance some kinds of farm activities at rates which are lower than their personal discount rates. This is one reason why earlier we said that when money is borrowed to finance the capital cost of a new technology, it is normally right to include the loan-servicing payments in the cash flow rather than the capital cost of the investment for which the money was borrowed.

Discount tables are available for the analysis of projects which extend over time. They can be found in Gittinger (1982) and Chisolm and Dillon (1988) and will not be reproduced here.

The net present value criterion

Projects with different net cash-flow patterns ('A' and 'C' in Figure 2, for instance) can be compared in terms of their net present value, by using discounting procedures.

Example: Assume that the following net cash-flow table has been derived for a project being considered for wider recommendation in the target area:

Year	1	2	3	4	5	6	7	8	9	10
NCF (\$)	-	-	-	100	200	300	700	700	700	700

If the discount rate to be used is 5%, the following tabulated **discount factors** (DF) would apply:

Year	1	2	3	4	5	6	7	8	9	10
DF	-	-	-	0.8227	0.7835	0.7462	0.7107	0.6768		0.6139

Applying these discount factors to the NCF figures above, we obtain a **net present value** for the project of:

$$(0 + 0 + 0 + 82.3 + 156.7 + 223.9 + 497.5 + 473.8 + 451.2 + 429.7) = \text{US\$ } 2315.1$$

Thus, expressed in present-value terms and given a discount rate of 5%, the value of the project would be about US\$ 2315.

The net present value itself doesn't mean much but it can be used as a basis for comparison between mutually exclusive technologies. Comparisons on the basis of the net present value can have several problems:

- the discount rate is usually chosen by the analyst on the basis of weak evidence
- projects viable at one rate, may not be viable at a higher rate
- project ranking can alter with the use of different discount rates

One way to test a project's sensitivity to the interest rate is to run several NPV calculations at varying interest levels. You can be fairly confident in the result if a project remains viable and rankings remain unaffected each time.

- smaller, highly attractive projects may have lower net present values than larger, marginally acceptable projects. This is because the net present value gives an absolute measure of profitability, not a relative one.

Benefit: Cost analysis

Because net present values are expressed in absolute terms, and because of the weaknesses associated with this, benefit: cost ratios are often used instead when long-term projects are being assessed. The ratio is expressed as:

$$\text{Benefit: cost ratio} = \frac{\text{present value of gross benefits}}{\text{present value of gross costs}}$$

Projects are ranked on the basis of the size of the benefit: cost ratio. At a given discount rate, a benefit: cost ratio of 1 or greater is considered viable.

As with the net present value criterion, the benefit: cost ratio suffers from the problem of discount rate selection. It also tends to discriminate against projects with relatively high gross returns and operating costs, even though these may be shown to have greater wealth-generating capacities than other projects with better benefit: cost ratios. Because of these disadvantages, the internal rate of return is often preferred for project appraisals of this kind.

Internal rate of return

This method involves finding that interest rate **which makes the net present value equal to zero**. The rate of interest, so found, indicates the actual rate of return on the investment, calculated independently of the cost of borrowing capital.

For instance, an internal rate of return (IRR) of, say, 10% means that a project will recover all operating and capital costs and pay the investor 10% for the use of his/her money in the meantime.

The interest rate derived can also be used as a basis of comparison. A project is said to be viable if the IRR obtained is greater than the opportunity cost of capital (i.e. greater than the interest rate which could be obtained from by investing the capital used in the next best available alternative, e.g. cattle or fodder banks).

The calculation of the internal rate of return is tedious if done by trial and error. A simple formula has therefore been derived (Gittinger, 1982):

$$\text{IRR} = \text{lower discount rate} + \frac{\text{difference between the two rates selected} \times \frac{\text{NPVs for}}{\text{absolute difference betw}}}{\text{absolute difference betw}}$$

The procedure is to select first an interest rate which gives a value 'close' to zero on the positive side. Next, find an interest rate (a higher one) which gives an NPV 'close' to zero on the negative side, then solve the equation.

Computer packages are available to reduce the time spent in computation, but the above formula is useful when only hand calculators are available.

Other considerations in cash-flow analysis

These include:

Forecasting costs and prices. Obviously forecasting costs and prices for 10-20 years in advance is fraught with problems. The analyst should, therefore, avoid being rigid in the interpretation of results, and projects should be tested for their sensitivity to variations in cost and price assumptions.

Inflation. In net cash flow analysis, normally no adjustment should be made for

expected future price rises caused by general inflation, i.e. where all the prices involved are expected to rise at the same rate. In such cases, the prices current at the time the calculations are made should be forecast to hold in the future also.

However, where (due to market forces such as shortages or large changes in production capacity) the prices of some inputs or outputs are expected to rise or fall relative to others, then adjustments for these relative future price changes should be included in the net cash flow forecast.

If a country suffers from high inflation rates, caution is appropriate with respect to the following:

- Where the discount rate being used in the calculations is based on the rates of return which farmers have historically been able to achieve from investing in their own enterprises, such a discount rate tends to over-estimate the real rate of return that was achieved because the initial investment occurred at a lower general price level than the subsequent revenues generated by it. Such historically based discount rates may need to be adjusted downwards.
- Where a new investment is expected to be financed by a loan at a rate of interest which will be constant during the project's life (while all other prices are rising), this is equivalent to believing that the 'price' of loan-servicing payments in the cash flow is falling relative to the prices of other inputs and outputs. An adjustment should, therefore, be made to reflect this.

Incremental benefits and costs. Note that with all the above methods of analysis, we were interested in benefits and costs which accrue solely to the project, i.e. the incremental effects of the project. This is a simple matter to determine, particularly if there are no displacement effects involved.

However, if implementing the project implies that some other farm activities need to be foregone (or displaced or altered) to make way for the project activity itself, then we need to calculate an **incremental net cash flow** budget. This budget is derived by deducting the 'without-project net cash flow' from the 'with-project net cash flow'. The resulting incremental net cash flow is then analysed using one or more of the methods described above.

Herd projection models. When designing livestock projects of long duration, it is necessary to know how to project herd/flock changes over time, as these will often form the basis for estimating the benefits and costs involved. The method used for such projections is described in detail in ILCA (1989b). ILCA research staff have also devised a computer programme which is useful for the projection of herd/flock dynamics over time. The interested user may contact the Head of Livestock Economics Department of ILCA, Addis Ababa, Ethiopia, for further information about the package.

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International Livestock Centre for Africa

PO Box 5689, Addis Ababa, Ethiopia

**Tel: (251-1) 61-32-15 • Telex: 21207 ILCA ET • Telefax: (251-1) 61-18-92 •
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ISBN: 92-9053-174-0

