

AGL/MISC/26/2000

**GUIDELINES FOR ON-FARM
PLANT NUTRITION AND SOIL
MANAGEMENT TRIALS AND
DEMONSTRATIONS**

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FOOD AND AGRICULTURE ORGANIZATION OF THE UNITED NATIONS
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Preface

One of the goals of the Land and Water Development Division (AGL) is to alleviate the problems many countries face of low soil productivity and unsustainable land use. It seeks to achieve this by promoting, *inter alia*, integrated plant nutrition management, soil fertility enhancement, sound soil management, and soil and water conservation. An important task of the division is, therefore, to provide guidelines for the effective planning and sound management of land, water and plant nutrient resources in agriculture.

To this end, this technical guide presents some aspects of effective planning for improved plant nutrition, soil fertility and soil management (that related to the selection of appropriate management practices through on-farm experimentation). The guide provides researchers with options for selection of sites, treatments, layout, experimental designs and methods of analysis. It also gives guidelines for the interpretation of results, and for drawing conclusions and making recommendations based on them. In addition, it emphasizes the importance of completing all stages of experimentation, including analysis, conclusions and recommendations, at the end of each season before the next crop.

The guide also attempts to provide some insight into the problems which may arise in conducting trials and demonstrations on farmers' fields. It is not meant to substitute consultation with a qualified biometrician or statistician, especially where difficulties arise due to missing data, unusual shape or size of experimental area, difficult terrain or limited resources. However, the guide does provide simple tools to assist users and to help them recognize when they should seek professional statistical assistance.

The guide is intended not only for FAO programmes and projects, but also for researchers and others actively involved in on-farm experimentation who do not have ready access to statistical or biometric assistance. These persons should have some training in field experimentation and elementary statistics. Readers should also be familiar with commonly used statistical terms such as treatment, block, replicate, randomization, analysis of variance (ANOVA), as well as the more frequently used experimental designs, i.e. completely randomized design, randomized block design and Latin squares.

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Chapter 1

Introduction

The agronomic productivity of land is highly dependent on the application of effective soil and plant nutrition management. In addition to climatic conditions that facilitate growth, plants require both a medium for physical support and anchoring of the root system and also the provision of water and essential nutrients. However, soils vary considerably in their potentials to retain and supply the required water and nutrients. Soil and plant nutrition management is geared towards modifying the plant nutrition regime and improving the capacity of the soil in order to ensure the provision of these essential inputs. While crop yield depends on the complex interaction of all plant growth and development factors, the role of prudent soil and plant nutrition management is particularly important to ensure a sustained level of productivity for any given cropping system.

PRINCIPLES OF PLANT NUTRITION MANAGEMENT

By definition, soil fertility recognizes the ability of soils to provide the essential elements for plant growth in the right amounts and at the right time. Few soils fit required specifications perfectly and most require some additional inputs of nutrients to meet plant needs. This is particularly the case when considering the long-term sustained productivity of the land. In developing a nutrition management system for a cropping system, the following factors need to be considered:

- the uptake by the crop for a reasonable yield;
- the amount that can potentially be supplied by the soil without degrading it;
- the efficiency of utilization of the nutrient from its source.

To satisfy these considerations, a soil may actually require not just the three major elements, N, P and K, but any or a combination of all the others needed to guarantee the best level of nutrition for the crop. Apart from N, P and K, the parent material from which the soil is formed largely dictates the levels of other essential elements present. To a lesser extent, factors determining the development of the profile, such as climatic conditions and topography also influence the nutrient status of soils; these becoming more important as the soil matures. In particular, N and P are associated with the organic matter component in soils and are therefore affected by any activity that influences the status of organic matter. Despite their occurrence in a 15-centimetre layer at levels of thousands of kilograms per hectare, only a small fraction of this amount is actually available for plant uptake. Consequently, N, P and K may need to be supplied to meet crop needs.

The actual application of these nutrients also depends on an understanding of their behaviour in the soil. Application issues include: the nutrient carrier or source, the timing of the application, the method of application, and the type of mineral nutrient source.

INTEGRATED PLANT NUTRITION SYSTEMS

Integrated plant nutrition systems (IPNS) is the maintenance or adjustment of soil fertility and of plant nutrient supply to sustain a desired level of crop production. It is achieved by optimizing the

benefits from all possible sources of plant nutrients and by improving the overall management of the farm.

The main objectives of IPNS are to rationalize plant nutrition management in order to:

- upgrade the efficiency of the plant nutrient supply in terms of: (i) relationships between crop yields and the quantity of plant nutrients applied; and (ii) the adequacy of the flows of plant nutrients passing through the soil/crop system and the crop's demand for nutrients at any point in time;
- maintain and improve the stock of plant nutrients in the soil/crop system;
- limit plant nutrient losses;
- provide the highest possible economic rate of return for the farmer.

By applying IPNS, farmers will improve their production capacity and income using the best combination of agronomic efficiency and economic profitability. In addition, they will have a more sustainable soil environment for crop growth.

The IPNS methodology is developed at three levels: plot, farm and village/community. Plant nutrient management at the plot level focuses on determining crop response to various application rates, times of application, forms and sources of plant nutrients. Some trials may also examine the residual effects of plant nutrients applied in previous seasons. At the farm level, it addresses the alternative sources of plant nutrients available to the farmer and suggests a suitable mix of them to fit production objectives. Finally, the village level considers the wider farming community, encompassing communal natural resources, environmental issues and farmer groups.

PRINCIPLES OF SOIL MANAGEMENT

Physical soil management practices are directed towards: (i) modifying the rooting environment, thereby enhancing the uptake of water and plant nutrients; (ii) reducing competition from weeds; and (iii) minimizing surface soil loss. Issues to be addressed include tillage, mulching, soil amendment and conservation practices. Tillage loosens the surface layer of the soil and helps to improve tilth/structure. This allows easier movement of water into the soil, which in turn leads to better plant growth. Tillage operations also facilitate the application of soil amendments and fertilizers. No till or minimum tillage systems where crops are planted in undisturbed soil may reduce production costs, preserve soil organic matter, reduce soil compaction and reduce soil loss on hillside farms. Mulching controls weeds and improves water use efficiency. Additional benefits of mulching include: structural improvement at the surface layer, arising out of a stimulation of microbial activity; reduction in post-plant operations, such as moulding; and reduction in the transmission of soil-borne pathogens to the plant. Soil conservation practices are necessary on hillsides and on gently sloping areas. The significant erosion that would otherwise occur would carry with it valuable topsoil and all the inputs applied to the soil surface, including the expensive inorganic and organic fertilizer and soil amendments (such as lime or gypsum). There are also side effects, such as silting of rivers and flooding of low-lying areas.

THE IMPORTANCE OF ON-FARM EXPERIMENTATION

Many of the above factors and issues need to be considered when recommending appropriate soil and plant nutrition management practices on the farm. Those outlined above also interact with and are influenced by other management practices, such as the irrigation and drainage system adopted for the farm. Therefore, some form of experimentation is required to determine the practices which

best suit the farmers' conditions. These experiments on soil management and IPNS studies at the farm level should be conducted on-farm.

On-farm experiments serve as a link between researchers, extensionists and farmers. They allow all parties to learn from one another. Researchers can assess the crop's response to the proposed technology under farmers' conditions, which may be quite different from on-station conditions. Researchers can also evaluate farmers' management and acceptance of the proposed treatments. By so doing, they are able to determine the limitations of the treatment and make adjustments to enhance its applicability. Extensionists and farmers gain a greater appreciation of the relevance of researchers' work. In addition, because of their involvement even in the planning stages, they may feel more inclined to cooperate.

CONTENTS OF THE GUIDE

The remaining chapters of this guide deal with various aspects of conducting trials and demonstrations on-farm. Chapter 2 defines the design of the on-farm trial including: the population or framework under investigation, choice of possible improvements, treatments, sites and observations. Chapter 3 addresses the main considerations in the various phases of on-farm trials and demonstrations: before, during and at the end of the trials. Chapter 4 deals with management of the results and data analysis. It provides some worked examples of the types of analyses researchers use in processing data. It also addresses statistical and economic issues. Chapter 5 discusses interpretation of the results. Finally, Chapter 6 provides further guidelines for analysing already existing data.

As most research institutions and agriculture ministries have personal computers, the emphasis is not on the mathematical computations required for data analysis. Instead, the publication concentrates on the main considerations in conducting computer analysis and the interpretation of the output from computer analysis of the data.

Chapter 2

Defining the design

This chapter examines the preliminary work involved in planning on-farm trials.

DEFINING THE FRAMEWORK OF THE STUDY

This defines: the population or area of study; the conditions in that population or area; and the units of the population where the soil and plant nutrition management interventions will be conducted. These units may be a single crop on a farm, an intercrop, or a crop rotation under a particular set of conditions. For plant nutrition studies, crop rotations are recommended over a sufficient period to enable the evaluation of the medium and long-term impact on crop response of factors such as climatic conditions, the incidence of pests and diseases, and management.

BOX 1: Data requirements of main categories for study area characterization

Physical environment

Climate: e.g. rainfall, temperature, wind, sunny days.

Soil: e.g. type and physical, chemical and hydrological conditions.

Topography: e.g. slope, floodplain.

Irrigation: e.g. water source and quality, means and frequency of delivery, on-farm practices.

Biological environment

Weeds, insects, diseases, other pests, crop yields.

Socio-economic environment

Resource availability: e.g. land, labour, cash, means of traction.

Infrastructure: e.g. supply of farm inputs, markets for farm produce, transportation.

Market data: e.g. prices of farm inputs and commodities.

Socio-cultural characteristics: e.g. land tenure and inheritance systems, gender division of labour in agriculture, decision making on the farm, willingness to change, production objectives.

Political and economic structures: e.g. national regulation, community groups, cooperatives, farmer organizations.

Production systems and land use

Major crops and livestock, cropping patterns, livestock characteristics, management practices, use of FYM, sources of organic materials and soil amendments on the farm.

The framework of the study can only be defined after initial discussions with farmers, extension agents and research personnel in the area, and also after a rapid reconnaissance survey of the area. The survey should be conducted by a multidisciplinary team of plant and livestock scientists, social scientists and economists. The area under study should be characterized according to the physical and biological environment, socio-economic environment, farming systems and land use. Box 1 provides details of the data requirements under each of these headings. In addition to these characteristics, there should be discussions with the farmers to determine their production objectives and constraints, as well as to obtain a general idea of farmers' perceptions and living conditions.

Once the area has been described, it may be possible to identify predominant sub-groups or strata within the population. These strata can be targeted for further study.

One possibility is to stratify the population. The main purpose of stratification is to subdivide the participants into homogeneous subsets which can provide more reliable information about the scope of the technologies being demonstrated and hence their applicability over a range of contrasting situations.

In selecting the strata the following factors should be considered:

- What factors are likely to affect the response of the enterprise to the technology being demonstrated?
- What factors are likely to affect the farmers' adoption of the technology?
- Is there the information available to allow stratification of the participants according to these factors?

Some examples of variables which could be used for stratification include: gender, farm size, system of land tenure (i.e. owns, rents, leases, etc.), cropping system (e.g. intercrop vs monocrop), soil type, land cover, system of water control, and slope. This list is by no means exhaustive but gives an indication of the type of information needed for stratification.

The following sources may provide information for identifying important strata:

- censuses;
- baseline surveys;
- rapid reconnaissance surveys;
- previous studies of the area under consideration;
- discussions with researchers, farmers groups and village leaders.

Stratification is best done before selecting the farmers. If it is not possible to do so, then some attempt should be made to stratify the farmers who have agreed to participate in the trials or demonstrations.

IDENTIFICATION AND SCALE OF THE PROBLEM

Probably the most difficult thing is to determine the problem responsible for low farm productivity. Unless there are obvious plant symptoms, such as those resulting from serious moisture stress or nutrient deficiencies, it is not easy to determine why one farmer is obtaining half the yield of another with the same crop and cropping system. In such circumstances, it may be necessary to conduct rigorous soil and crop testing to aid diagnosis, or to source technical assistance from experienced personnel. If the aim is to provide better aeration, moisture and nutrient regimes for plant growth and development, then assessment of the status of these in the soil should reveal the problem that exists. While this type of service may be either too difficult or too costly for many to access, it is sometimes the only means of accurate diagnosis.

For more obvious problems, perhaps relating to direct inaccessibility of resources and inputs, the difficulty lies in identifying the solution. Some of these problems may be due to an unavailability or reduction in the supply of plant nutrient sources in remote regions, or the increased cost of tillage operations, or greater division of an already scarce water resource. Solutions to these problems are usually found in cultural practices that were once traditional methods of soil management. The FAO Guidelines on Participatory Diagnosis of Constraints and Opportunities for Soil and Plant Nutrition Management provide useful tools for diagnosing the problems and deriving possible solutions.

Some problems extend across farms while others are localized and specific to individual farms. Where there is an organized body of farmers or where there are agricultural extension workers, the scale of the problem can be determined by participatory rural appraisal or other appropriate survey methods. This should be a necessary prerequisite in developing a programme of farm trials to investigate solutions.

LISTING THE POSSIBLE MANAGEMENT AND CROPPING SYSTEM IMPROVEMENTS

Further discussions with farmers in each specific stratum will allow the identification of the main production problems in the area of study. Researchers, in consultation with the farmers, will then be able to suggest a list of possible improvements to the cropping system. Many options may exist and problems can be tackled from many different angles.

An example is that of a group of farmers in a village who complain that they are unable to purchase large quantities of mineral fertilizer because of its increasing price. Consequently, their maize yields are declining owing to the reduced input of fertilizer. This problem suggests the need for improved plant nutrient management to: (i) evaluate alternative, cheaper, locally available sources of plant nutrients; and (ii) improve the efficiency of fertilizer use. The list of possible improvements should include:

- varying the sources of plant nutrients, i.e. organic, inorganic and biological;
- varying rates of application of plant nutrients;
- varying the time of application of plant nutrients;
- varying the crop rotation to include legumes or a fallow period in the rotation.

Another example is that of very acid soil. Solutions to the problem may involve finding a range of soil amendments, such as lime or dolomite, for the specific cropping system. Alternatively, the farmers may opt to live with the acidity problem, if it is slight, but to identify a crop or cropping system that can produce well in such a situation. Within these two broad approaches, many possibilities exist.

Depending on the scale of the problem, the number of options to investigate varies. The more widespread the problem, the fewer the number of alternative solutions that can be investigated within a manageable framework, particularly if monitoring of trials across farms is to be employed.

DEFINING THE OBJECTIVES OF THE STUDY

This is the most important consideration in planning the experiment. It determines why the trial or demonstration is being conducted, or the questions the researcher wants the experiment to answer. The objective must be clearly defined and the hypotheses to be tested must be clearly stated and within the farmer's or researcher's means to implement.

Not all experiments are conducted to test hypotheses. In most cases, the researcher is interested in estimating the value of a particular parameter (e.g. the effect of a particular treatment on yield). The parameters to be estimated must then also be clearly stated and defined.

CHOOSING THE TREATMENTS

There are numerous possible management improvements for a specific problem. However, it may not be possible to apply all these improvements in one experiment, especially in one conducted on-

farm. Therefore, the researcher must select and test a small number of treatments from the list of alternatives.

Once the possible solutions have been agreed upon, the next step is to prepare for actual investigation on the farm or farms. At this stage, the major consideration becomes the design and layout, i.e. the statistical approach to the trials at the farm level.

The choice of treatments is determined primarily by: the objective of the experiment; the researcher's knowledge of the crop response to the treatment in the location where the experiment is being conducted; the resources available to the farmer for applying the treatment; and the long-term effect of the treatment on the soil and environment. Depending on the production objective, this last aspect may conflict with the farmer's economic objective. A cost-benefit or ex-ante analysis may therefore be necessary before deciding on the appropriate treatments.

For example, if a trial or demonstration requires relatively expensive mulching treatments and the target group is resource poor farmers, then the treatment may not be affordable. However, if the mulching is not done, the farmers may run the risk of severe weed infestation and/or soil erosion and even greater economic losses in the long term. Consequently, as a compromise, a low-cost mulching material could be used.

For on-farm investigations to examine the benefits of applying soil amendments, the following questions may be asked:

1. Choice of soil amendment
 - a. Which amendments are to be chosen?
 - b. Why will they be chosen?
 - c. Which amendments are readily available (especially those naturally occurring in the farming area)?
 - d. Are any of these amendments expensive or environmentally unsound? This can refer not only to harmful runoff but also to the use of an amendment that might spoil a natural resource.
2. How are the amendments to be tested?
 - a. Are the amendments to be tested at one level or several? What level or levels?
 - b. When and how often are they to be applied?
 - c. What is the method of application?
3. What is the standard (control) against which the test treatments are to be compared?

Two further, more complicated problems require consideration:

- Reduced supply and accessibility of organic fertilizers;
- Declining yields due to soil acidity development and soil organic matter decline.

The former may affect farms with more than one soil type. A solution is to use available organic sources, such as chicken manure, green manure and sludge. If little is known about the response of the crop to these sources, then different (probably three or four) rates of application need to be tested. Thus, a 3 x 4 factorial, plus control treatments, needs to be set up. This is an experimental station trial not a farm trial. However, there may not be an experimental station or stations representing all the soil types. It will be necessary to find farms on which to conduct experimental station type trials. The results will lead to recommendations to take to large groups of farms.

The latter could well be a single farm problem. Unless the results are likely to benefit a larger group of farmers, it is probably best to advise the farmers on appropriate practices and avoid costly experimentation.

Most on-farm trials/demonstrations will contain no more than four or five treatments and many will contain fewer (i.e. three or even only two treatments). If after careful consideration of points 1-3 above, more than five treatments are required, then a more complex experiment on a few farms is needed.

For on-farm experiments, it is recommended that:

1. For farmer managed and executed trials, there should be no more than six plots. On larger farms where the researcher supervises and personally executes the trials, a maximum of eight to ten treatments is permissible if the farm can accommodate them. These treatments may be non-factorial (e.g. five levels of a single factor) or factorial (e.g. 2 x 2, 3 x 3, 2 x 2 x 2) provided the number of treatment combinations does not exceed the maximum.
2. For the sake of simplicity, the choice of treatments may be the same at all locations. As the final aim of the experiment is to have estimates of yield increase, efficiency, nutrient loss, etc., due to the treatments, such estimates are easily possible if all treatments are present everywhere (this guide contains some simple examples). More complicated designs which allow for unequal replication may be used. However, competent statistical or biometric assistance, where available, should be sought with the more complex designs.
3. All treatments should be defined precisely (including the control). For example, if the treatment is time of application, the time should be clearly expressed as days after sowing or transplanting, or precisely related to the vegetation stages.
4. For trials where the primary objective is to derive response curves, a minimum of three and preferably four levels of the particular plant nutrient should be used. Where the trial includes more than one plant nutrient, this could lead to a large experiment which may be better placed on an experimental station.
5. A control comprising the farmer's traditional method may be included as one of the treatments, especially in demonstrations. This control is a point of reference for comparing or estimating the effect of the treatment being tested or demonstrated.

Control treatments

Control (or check) treatments are a source of confusion. A do nothing control, such as add no soil amendment, is what is required if present farming practice is just that. However, if the farmers already practice some form of application of amendments, then the do nothing treatment may not be useful, unless the researcher considers it a good alternative to present practice. Thus, the control may be present practice rather than do nothing. Sometimes there may be several controls. If present practice and do nothing are both included, then both may be considered controls.

In some experiments, the control may be the treatment in which the farmer does the most rather than does nothing or does the least. For example, with minimum tillage the control of farmers' practice will be the treatment in which the land preparation is more intense than in the other treatments. There will probably be an intermediate treatment with land preparation somewhere between the control and minimum tillage.

Sometimes not all farmers have the same practice. In this case, farmer's control is not the same on each farm. One way to deal with this problem is to decide on one or two treatments which represent the most typical farming practice and use them as the control. Farms which do not normally use this typical practice may then have an extra control plot for their own practice, in addition to the typical practice control. At the analysis stage there is usually little to be gained from including treatments which are unique to particular farms. However, if the treatments used in the experiment perform much better than the farm practice, then the demonstration effect to the farmer

will have been achieved. Conversely, if the unique farm practice does as well as the experimental treatments, then the researcher may need to think again and perhaps learn something from the farmer.

Factorial experiments

A factorial experiment tests the effects of two or more treatments (factors) simultaneously. Each factor occurs at more than one level, and all possible combinations of levels are tested. An example of a factorial experiment is a trial with two fertilizer types: urea and calcium ammonium nitrate (CAN), each type being applied at two rates: 0 and 50 kg/ha. All possible combinations would be: no fertilizer (i.e. 0 kg/ha urea and 0 kg/ha CAN), urea only (i.e. 50 kg/ha urea and 0 kg/ha CAN), CAN only (i.e. 50 kg/ha CAN and 0 kg/ha urea), and both urea and CAN (i.e. 50 kg/ha urea and 50 kg/ha CAN).

Researchers sometimes fail to recognize a factorial structure. For example, it might be decided to experiment with improved land preparation and mulching. There are, therefore, four treatments: land preparation unimproved and no mulch (farmer's practice or control); land preparation unimproved and mulch; improved land preparation and no mulch; and improved land preparation and mulch. The 2 x 2 factorial structure here is often not noticed; that is, land preparation at two levels and mulching at two levels.

This may not matter too much at the design stage. However, at the analysis stage there is a ready (and easily obtained on practically all software packages) subdivision of the three treatment degrees of freedom into meaningful single degree of freedom contrasts representing land preparation, mulching and the interaction.

Factorial experiments are useful for determining the interaction between factors and where production functions must be derived.

It may be possible to use more complex designs, such as factorial designs with partial or total confounding in no more than eight to ten plots. The design and analysis of the results for such complex designs require the assistance of a competent statistician. These designs should, therefore, not be attempted on-farm unless such assistance is available and there is a high level of supervision, especially in the planning and layout.

Split-plot designs are applicable where there is more than one factor and one of the factors requires a large area for a single experimental plot, e.g. trials involving irrigation, soil conservation or land preparation treatments. The main plots will be the treatments requiring a large area with the other treatments assigned to the sub-plots.

Split-plot designs are not recommended for simply accommodating additional factors in factorial experiments. A good rule of thumb is never to use split-plots unless they are necessary for reasons of cultural practices.

Because there are fewer replicates of the larger main plots, main plot treatment effects are measured with less precision than sub-plot ones. There are also problems with analysis, especially when there are missing values.

The layout of split-plot designs requires a high level of supervision.

Step-by-step or add-in designs

In this type of trial, the various innovations to the farmer's traditional practice are added step by step to show the farmer the incremental effect of each new technology or factor. For example, a trial may consist of three treatments as follows:

Treatment	Description
A	Farmer's traditional practice
B	Farmer's practice + FYM
C	Farmer's practice + application of FYM + inclusion of legume in crop rotation

This type of trial is appropriate where:

- the results of a factorial experiment show that some treatment combinations are consistently not significant and can therefore be eliminated from the experiment;
- the extensionist or researcher wants to demonstrate the cumulative effect of the technologies, so that the farmer can assess and select the combination which is best suited to the production objectives and available resources.

One disadvantage of this design is that the order of inclusion of the factors may affect the outcome. Indeed, if the order of inclusion is wrong, no useful result will be obtained.

SITE AND LOCATION SELECTION

The choice of sites and locations is made after the framework of the study has been clearly defined. This set of locations should be similar to a random sample with the sites of the trial being as representative as possible of the range of conditions under which the study is being conducted. The sites should also be representative of the farmers' conditions. In this way, it is possible to obtain the full scope of responses to the proposed interventions as well as assess the risks under a given set of conditions which are relevant to the farmers' situation.

Important considerations in site selection include:

- Are the crops selected normally grown in the region where the experimental site is located?
- What is the history of the site (i.e. previous crops, fertilizer, organic manure or soil amendment application)?
- Is the site representative of the conditions under which farmers cultivate the crop? For example, if most farmers grow the crop on slopes under rainfed conditions, it would be unwise to select a site in lowland areas where irrigation is applied, unless the objective is to determine the crop's response under irrigated conditions.
- How many farms are to be chosen? If only a few farms are involved in the experiment, more replicates are required and, hence, more land is needed for each trial on the farm. The level of cooperation by the farmer will also need to be higher.

It is necessary to choose farms where the farmer is willing to cooperate. It is useful to work with persons who are respected in their communities, as they will pass on the knowledge they gain to other farmers.

Notwithstanding the need to do on-farm trials in typical farming conditions, the farms chosen should have ample land available for the plots required. The number of plots per farm may be small, but these plots will need to be large enough to give a reasonable assessment of the treatment applied. Some consideration should be given for the need for guards between plots if interference between treatments applied to adjacent plots is probable. An example of this is where fertilizers applied on sloping ground may run downhill.

Sample size or number of farms

The minimum sample size or number of farms is determined by estimates of variability of important indicator variables such as yield or dry matter (Box 2). The formula used for calculating an approximation of the number of trials or demonstrations is:

$$n = (z c/p)^2$$

where: n is the number of samples or sample size to be taken, z is the value from the normal distribution tables corresponding to the specified confidence level, c is the coefficient of variation as a percentage, and p is the precision (percent) with which it is desired to estimate the mean.

BOX 2: Example of calculation of sample size or number of farms

If the coefficient of variation of yields of an improved technology (obtained from measures of variability such as coefficients of variation or standard errors of past experiments) is 10 percent and the aim is to estimate the yield of the improved technology to within 5 percent of its actual value with 95 percent confidence, the sample size would be:

$$n = (1.96 \times 10/5)^2 = 15.4 \text{ or approximately } 16$$

To estimate the yield with a precision of 10 percent, the sample size would be:

$$n = (1.96 \times 10/10)^2 = 3.8 \text{ or approximately } 4$$

More trials or demonstrations are required if the results are highly variable. If no suitable estimates of variability are available, the following rules of thumb are suggested:

- A minimum of 10 two-replicate trials or 20 single-replicate demonstrations.
- It is advisable to conduct more than these minimum numbers to allow for the failure of some experiments. There is also the added advantage of more farmers receiving exposure to the technology, and of testing the technology over a wider range of conditions.
- As weather conditions vary from one year to the next, trials and demonstrations should be conducted over more than one year (2-4 years is adequate), preferably with the same number of trials and demonstrations each year. If one of the objectives of the trial is to examine the residual effects of the treatments applied, then the trial should continue on the same experimental plots.
- Although the same treatments and designs may be used on each farm, a different randomization should be applied at each location. Table 1 gives an example of the randomizations for a trial with four treatments (A, B, C and D) arranged in a randomized complete block design with two replicates at ten sites. Appendix 1 provides a table of permutations of numbers.

TABLE 1

Assignment of treatments to plots at ten sites

Rep	Plot	Site number									
		1	2	3	4	5	6	7	8	9	10
1	1	B	D	A	D	C	A	B	B	C	D
1	2	C	C	D	A	B	B	A	D	D	A
1	3	A	A	B	B	A	C	C	C	A	C
1	4	D	B	C	C	D	D	D	A	B	B
2	5	D	A	B	C	A	C	A	B	D	B
2	6	B	D	A	B	C	B	C	D	C	A
2	7	A	B	C	D	B	A	D	C	A	D
2	8	C	C	D	A	D	D	B	A	B	C

SELECTING THE DESIGN AND EXPERIMENTAL PLOTS

After selecting the treatments and farms, it is then possible to determine: the experimental design; the number, location and arrangement of the plots on the farm; and the plot size and shape.

There is considerable literature available on the design of on-farm trials (see above for brief descriptions of factorial experiments, split-plots and step-by-step or add-in designs). Researchers are advised to choose simple experimental designs, especially where statistical software and assistance are not readily available. In this way, they will still be able to conduct the analysis using hand calculators or by simple calculations on a spreadsheet program. Moreover, missing observations do not adversely affect the analysis of these simple designs.

An important aspect of design definition is that of the persons responsible for managing and executing the experiments. Experiments may be:

1. managed and executed by researchers on farmer's fields;
2. managed by extension officers but executed by the farmer;
3. managed by researchers but executed by the farmer.

Each type of experiment has different design requirements which need consideration during this planning stage.

Type 1 experiments are no different from on-station trials and may be necessary if the conditions required for conducting the trial are not available on the research station. For example, the research station may be situated on a flat piece of land and the researcher may want to conduct a soil conservation trial requiring a sloping field. If a farmer's holding is on sloping land, that farm may be selected for conducting the trial. The usual guidelines for on-station trial designs are applicable here and are not given in this publication.

Type 2 experiments use simple designs, such as single-replicate or randomized complete block designs with two replicates and six or fewer plots. Chapter 3 presents a possible layout of such a design. One of the treatments should be the control or check with the farmer's traditional practice.

Type 3 experiments use the randomized complete block design. This is one of the easiest and most commonly used designs and is therefore recommended, especially where biometric assistance is not available. Studies of on-farm trials and spatial variability have shown that where the source of variation is not apparent, row-and-column designs such as the Latin square or Youden square are more efficient. Incomplete block designs are applicable where there are limitations on the number of plots that can be accommodated on some farms compared to the number of possible treatments.

In addition to classification by farmer/researcher, on-farm investigations for IPNS, soil fertility and soil management also form three categories:

- Trials conducted on a single farm. Literally, this means using the farm as an experimental station.
- Trials with several replications at each of a few farms (three or fewer farms).
- Trials with a few replications at many farms (certainly more than four, probably more than six).

It is usually recommended that most on-farm trials should fall into the third category. However, many plant nutrition and soil management trials may fall into the second category. This is because soil management requirements may vary from farm to farm according to topography, soil type and other environmental factors. This is particularly so in tropical countries where on-farm trials are thought to be more relevant than trials on uniform field stations. Trials of the first category should not be ruled out. There may not be a field station available with suitable conditions to perform

initial testing of several treatments before moving to several farms with treatments which seem to be promising.

Design of trials at a single farm

Designs here follow many of the recommendations for field station trials in classical statistical texts. These generally recommend some form of blocking structure, with randomized blocks usually predominating. This is also what is usually found in practice.

Row-and-column or double blocking designs, usually Latin square, are sometimes used but the researcher should be aware of the limitations of Latin square designs. These are the constraints on the numbers of replications (and rows and columns) determined by the number of treatments. However, a number of other row-and-column type designs are available. Perhaps the most useful of these is the incomplete Latin square or Youden square, where the number of rows equals the number of treatments but the number of columns is flexible. Modern computing facilities mean that the overwhelming predominance of Latin squares for row-and-column designs is not necessary.

Moreover, the more the blocking, the fewer the degrees of freedom for the residual (or error) terms in the ANOVA tables. Thus, a double blocking design (as in a row-and-column) will only be efficient if there really are two types of variation to control. Blocking in general (single or double) is desirable if it controls variation. However, if there seems to be little variation at the experimental farm or if the variation is difficult to identify, then blocking is only necessary for management purposes.

Completely randomized designs are rarely used for field trials (on-station or on-farm). However, post mortems on completed experiments often show that blocks were not necessary. Nonetheless, an experiment without blocks may introduce experiment management problems. This is because blocks are useful when dividing up the tasks to be performed. For example, operations such as planting, weeding, fertilizer application, or harvesting cannot always be completed in a single day. Blocks are then useful divisions for the day's labour. Moreover, different personnel sometimes perform the same task. This practice is best avoided. If it is unavoidable, then the different teams should be allocated different blocks.

Design of trials at a few farms

As soil management problems may vary from location to location, it may be difficult to find a large number of farms to test certain practices. There is also the situation where two or three progressive farmers will be able to assist with the conduct of a trial with a relatively large number of replications on each farm.

When the trial is conducted on more than one farm, the different farms provide a type of blocking. However, at each farm there need to be several replications of the treatments. It is not necessary to have the same number of replications at each farm. If one of the sites can only accommodate three replications whereas the others can manage four, then that arrangement is acceptable. Nor is there any reason why the blocks should look the same at each farm.

The type of design chosen for each site (e.g. completely randomized, randomized block, row-and-column, etc.) depends on the same type of considerations as for a single site trial. If the sites do not all lend themselves to the same type of design, then it may be necessary to have different designs at different farms. For example, blocks on some farms but completely randomized at others. At the analysis stage, the 'blocks within farms' term in the ANOVA will only have relevance for those farms with blocking. Similarly, if one farm, but not the others, is thought to need a row-and-column design, then this is how the trial should be designed. At the analysis stage,

the farms with blocks will be considered to have rows and columns, but there will only be one column present (see example in Chapter 4).

However, if split-plot design arrangements are thought necessary, then these will apply to all the sites.

Design of trials at several farms

These are the classic types of on-farm experiment. The guidelines for designs of this type are given above, i.e. a small number of plots (ten or fewer) on several farms. Because there are only a few plots available, not many treatments can be included. The maximum number of treatments sometimes recommended is four and this rules out all factorial designs except 2 x 2. However, there are some important points to note. Rules of this kind are never carved in stone and if the farmers selected can understand and manage five treatments (one of which will almost certainly be a control), then there is no reason for not using five treatments. The same considerations may even apply to more than five treatments. Nevertheless, there will usually be four treatments or fewer, particularly in soil management experiments.

If there will usually be fewer than ten plots per farm, then there is not much opportunity for large numbers of replicates per farm even with fewer than four treatments. Two or three replications are usually the maximum that can be managed. Single replicates on-farm are sometimes found, but the estimation of the farm by treatment interaction becomes difficult to calculate; few of the better-known software packages do it readily. Researchers who are not familiar with the methodology for estimating this interaction in the single replicate per farm case are advised to ensure that a biometrician is available to help. One methodology for calculating this interaction is also available.

Supplemented balance designs

A design that can be important in on-farm trials of this type is a simple supplemented balance. A supplemented balance design is one where there are two groups of treatments with balance within groups but not between groups. One of the treatment groups can consist of a single treatment. Thus, there may be one or more treatments that are replicated on each farm and another group that do not appear on each farm, but each pair of that group appears on the same farm the same number of times. Figures 1, 2 and 3 show some supplemented balance designs. For practical use, the designs in the figures should be replicated at least two or three times (i.e. using more farms or using more blocks on farms so that there are 12 or 18 blocks instead of 6).

Supplemented balance designs are useful where it is desired to test more treatments than can be accommodated on each farm (or block). Figure 1 shows how blocks with three plots can accommodate four treatments. It also shows how more than one block can be placed at each farm; the small block size (three plots) allowing within farm variation to be controlled. Figure 2 extends the principle to five treatments, but the block size is still three plots.

In the examples in Figures 1 and 2, treatment a is a control which is important to replicate in each block. However, there will sometimes be more than one control. For example, in addition to a control of farmer practice there may be a researcher control which may be a 'best' treatment against which alternative less intense treatments are tested. Figure 3 shows how to accommodate two controls in a supplemented balance arrangement.

Control plots

Conditions in control plots should be as similar as possible before the treatments are applied.

FIGURE 1

Supplemented balance design on three farms, four treatments

Two blocks at each farm, two groups of treatments: 1. Treatment a replicated in each block. 2. Treatments b, c and d, of which each pair of treatments appears twice in the experiment

a	b	c
Farm 1, Block 1		
a	b	d
Farm 1, Block 2		
a	c	d
Farm 2, Block 3		
a	b	c
Farm 2, Block 4		
a	b	d
Farm 3, Block 5		
a	c	d
Farm 3, Block 6		

FIGURE 2

Supplemented balance design on three farms, five treatments

Two blocks at each farm and two groups of treatments: 1. Treatment a replicated in each block. 2. Treatments b, c, d and e, of which each pair appears in the same block once

a	b	c
Farm 1, Block 1		
a	b	d
Farm 1, Block 2		
a	b	e
Farm 2, Block 3		
a	c	d
Farm 2, Block 4		
a	c	e
Farm 3, Block 5		
a	d	e
Farm 3, Block 6		

FIGURE 3

Supplemented balance design on six farms

One block per farm and two groups of treatments: 1. Treatments a and b replicated on each farm. 2. Treatments c, d and e, of which each pair appears on the same farm twice

a	b	c	d
Farm 1			
a	b	c	e
Farm 2			
a	b	d	e
Farm 3			
a	b	c	d
Farm 4			
a	b	c	e
Farm 5			
a	b	d	e
Farm 6			

Where possible, data (e.g. yields or descriptions of the farming system) should be also collected from farmers adjacent to the demonstration who are growing the same crop using traditional technology. This enables an assessment of whether the yields obtained from the trial and demonstration plots are comparable with those of neighbouring farmers.

Plot size and shape

This is usually limited by the land available for the experiment, and by the amount of labour and inputs of other available resources.

Plots should not be less than 50 m² for trials and 100 m² for demonstrations. Very large plots are not required. If land is available, it is preferable to use more blocks/replications rather than increase the plot size. To allow valid comparisons, the harvested area of the plot on a particular farm should be the same size for each treatment. Plots need not be square or rectangular.

Allowance should be made for guard rows to avoid interplot interference, especially in plant nutrition experiments. However, guard rows may be unnecessary in some types of experiments, especially if plots are not contiguous or are surrounded by the crop which has minimal edge effects.

DEFINING THE OBSERVATIONS

The objectives of the experiment and the resources available to the researcher to measure the variables determine the data to collect. The choice of observations depends ultimately on the researchers as they know the main factors which could influence the results. In soil management trials, the focus is on the measurement and analysis of two types of indicators, namely: crop performance and soil status.

Crop performance is probably of major interest of the farmer and is measured by parameters such as yield quantity and quality, and also crop growth measurements.

Soil status may include assessment of the level of nutrients in the soil and other soil parameters, or it may mean estimation of soil and water runoff during and possibly after the duration of the experiment. This is of interest to the researcher who is trying to explain the performance of the crop. Plant status means examination of the level of nutrients within the various plant parts.

Box 3 presents a possible minimum data set which may be collected in on-farm experimentation.

The variables, the stage at which they should be measured and the units of measurement should all be listed. Where variables are measured by scoring, research staff and their assistants need to be properly trained so that there is general agreement on the meaning of the scores. The range of scores should not be too narrow as to affect the distribution of responses. It is also better to have several persons score the same plot and use the mean of their scores. It is recommended that the researcher responsible for the study make a simple list of all measurements which need to be taken during the course of the experiment. This ensures that a complete set of observations is collected at every site and location.

SAMPLING

Plant nutrition and soil management trials can create unusual sampling problems. One reason for this is that it is often necessary to monitor the nutrient status of soil and or plants both before and after application of treatments.

BOX 3: Minimum data set for on-farm trials and demonstrations

Village level:

Daily rainfall (mm), temperature (°C), agro-ecological zone;
Input prices;
Wage rate during the season;
Farm produce prices, end of season.

Site level:

Soil texture (sandy, medium, heavy) at two depths: 10 and 15-30 cm;
Soil pH, N, P, K and OM, aggregation, and bulk density in the case of tillage trials, colour at 10 and 15-30 cm;
Slope and position on slope, e.g. steep on top, medium in the middle, etc.;
Crop management information which is not held constant and is not part of the treatments (planting date, field history, land preparation, varieties, plant arrangement, pest control);
Age and gender of farmer;
Location of farm and demonstration (including geographic coordinates).

Plot level:

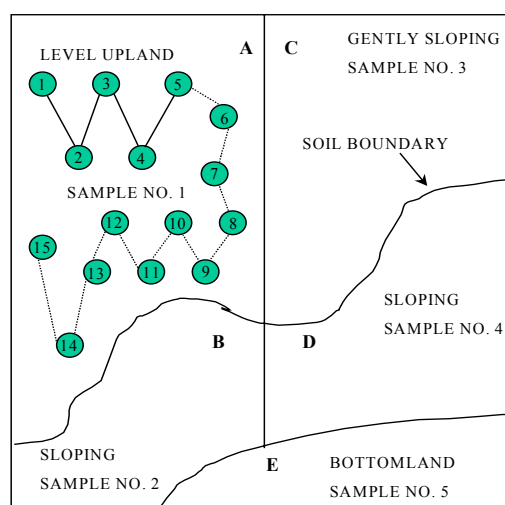
Establishment, mid-season and final stand counts;
Density of secondary crops;
Crop yields or other suitable indicator;
Variable inputs (i.e. inputs which differ between treatments, including labour);
Farmer assessment.

Soil sampling

Obtaining accurate figures for the nutrient status of the soil can be difficult. Two samples of soil taken from almost the same place can differ in important aspects when subjected to laboratory analyses. There is also the question of the cost of analysing samples. There are also sometimes doubts about the reliability of some laboratories.

Although some of these problems are almost impossible to eliminate, the well-known sampling technique of using many, but not necessarily large, samples can help minimize them. Thus, taking small amounts of soil from different locations in the field is more effective than taking large amounts from a few locations. It may be too costly to analyse each of the many samples individually, so it will then be necessary to mix up some of the samples. The area to be sampled must first be examined to determine whether it is uniform or not. The more uniform the soil, the larger the area which can be sampled as a unit. If the field varies in slope, texture, colour, drainage or crop management, then the field should be subdivided into uniform sampling units and each unit sampled separately. Composite samples should be taken from relatively uniform areas. Figure 4 shows an example

FIGURE 4
Diagram of field showing sampling units and sampling pattern for one unit



of a field subdivided into five sampling units because of its topography. It also shows the sampling pattern for one sampling unit.

Soil sampling depth is dependent on soil type and crop species. Soils have distinct profiles and may have varied thickness of their horizons. Additionally, organic matter, N, P or K may be concentrated in the top 10 cm, and thus mixed samples from a 0-30-cm layer can grossly underestimate the true status of these variables.

In many cases, it may be advisable to sample separately at many different depths to be sure of determining the true soil status. Sampling at different depths will be necessary if soil profile studies are part of the investigation. When sampling at different depths, it is usually convenient to sample all the required depths at the same sampling point.

The frequency and purpose of soil sampling vary between short-term and long-term studies. Short-term studies may require more frequent sampling than long-term studies.

Short-term studies will probably be examining the more dynamic forms of the nutrients and other soil variables (e.g. available or extracted nutrients). Long-term studies should monitor the less dynamic and more stable forms (total N, organic carbon, total P, CEC, bulk density, permeability, etc.).

Plant sampling

Obtaining the nutrient status of plants involves removing or destroying either whole plants or parts of plants. Studies of leaves or branches entail the removal of some leaves or branches, but the rest of the plant may be unaffected. On the other hand, studies of stems or roots will require removal of entire plants.

There is a clear distinction between destructive and non-destructive sampling. Destructive sampling may not only eliminate a plant from a plot, but the space left could alter the performance of plants close by. Thus, future sampling or observations from neighbouring plants may not represent what would have happened if the original plant had not been removed.

Therefore, if destructive sampling is necessary before the conclusion of the experiment, then plot sizes need to be made larger to allow unbiased observations of plants that are not destroyed in earlier samples.

Non-destructive sampling removes only parts of plants. Thus, it must be determined whether non-destructive sampling will affect the future performance of that plant. If a few leaves are removed from a large fruit tree, future yields are not much affected; nor will there be any problem finding more leaves for subsequent sampling. However, with smaller plants, removal of parts of a plant may mean that the same plant should not be used again for sampling or other observations. Nevertheless, although that plant cannot be used, immediate neighbours may not be affected and can be used. There are many possible scenarios depending on type of plant and type of sampling. A clear guideline is to only observe or sample from plants or parts of plants not influenced by any previous sampling activity.

The principle referred to above of many small samples rather than a few large ones also applies to plants. Again, this may cause a conflict as there may be a temptation to sample from several adjacent plants and leave the rest of the plot clear. However, this is really taking a large sample from one area rather than several small samples from different areas and it may lead to biased estimates that are not representative of the plot as a whole. Compromise will again be necessary. It is advisable to take as the sample size at least two, but preferably more, plants per plot. The resources available will dictate the number of plants sampled.

Sampling problems are rarely trivial and general guidance may not always be suitable for every experiment.

PLANNING THE METHOD OF ANALYSIS

This consideration is often ignored during the planning stage of an experiment, but it is important in determining how the data will be processed once collected. The method of analysis should provide the answers to the hypotheses tested, or give valid estimates of the parameters, and consequently assist in achieving the objectives of the study. For example, before collecting the data, it is wise to prepare skeleton ANOVA tables to ensure that the relevant hypotheses will be tested and parameters estimated.

PREPARING THE PROTOCOL

A protocol should be written for each experiment. This protocol serves as a permanent record to which researchers can refer for all relevant information about the experiment. All protocols should contain the following information:

- objectives of the study;
- location of the experiment;
- treatments applied;
- experimental design, including the randomization;
- field plan showing the size, orientation and location of the plots (including footpaths and guard rows);
- observations to be taken (primary, concomitant, management and production system);
- the skeleton ANOVA table, as well as a brief description of the types of statistical analysis to be conducted on the data;
- a responsibility table showing the person responsible for each activity associated with the experiment.

Appendixes 2 and 3 are examples of minimum protocols for two experiments. For the skeleton ANOVA table, two columns are given for the degrees of freedom: one with the formula for calculating the degrees of freedom, and the other with the specific values for the particular experiment. Appendixes 4 and 5 elaborate further on the analysis of the data collected from these experiments.

Chapter 3

Conducting the trials

This chapter addresses the activities which can only be conducted in the field. It also discusses how to resolve problems which may arise during the trials.

BEFORE AND ESTABLISHING THE TRIALS

Location of plots in the farmer's field

Plots should be located so that they are representative of soil and other field conditions of the particular farm. Attempts should not be made to make plots artificially uniform, e.g. residues from the previous crop should not be removed if these are normally left in the field.

Sources of variation, such as shade or change in soil type, should be recorded and marked on the field plan for presentation in the report to assist in the interpretation of the results.

With block designs, plots within blocks need not be contiguous provided that conditions among plots within each block are not visibly variable. Similarly, blocks need not be adjacent, but could be situated on different fields on a particular farm. Figure 5 shows the layout of an experiment with non-contiguous plots and blocks.

Field plans

Once the experimental design has been decided, field plans should be drawn up. These should show the allocation of treatments, gross and net plot size, and the location of plots and blocks in the field in relation to standard landmarks such as trees or buildings. Where possible, landmarks should also be indicated on the field plan to enable anyone who is not familiar with the experiment to locate the trial or demonstration. Figure 6 is an example of a field plan.

Plot management

Each plot should have a plot number and that number should be written clearly on labels in the field to enable ready identification of them. The numbering system of the plots is not important. However, the system should be consistent for the whole set of trials or demonstrations, and it should be reflected in the field plan. Labels on demonstration plots should also include a brief description of the treatment.

All operations conducted on the experiment should be done for a complete replicate or block at a time. Consequently, it is better to weed a complete replicate in one day than to weed only some of the treatments in all replicates over the same period. Similarly, it is advisable to assign one worker to all treatments in a single replicate rather than assign a different worker to each treatment.

DURING AND AT THE END OF THE TRIAL

Supervision and management of the trial

One of the main reasons for the failure of on-farm trials is lack of proper supervision and follow-up. All persons involved in the experiment, such as researchers, extension officers and farmers, need to be aware of their respective roles for the duration of the experiment.

FIGURE 5
Randomized complete block design, two blocks per farm, four treatments (a-d) on three farms

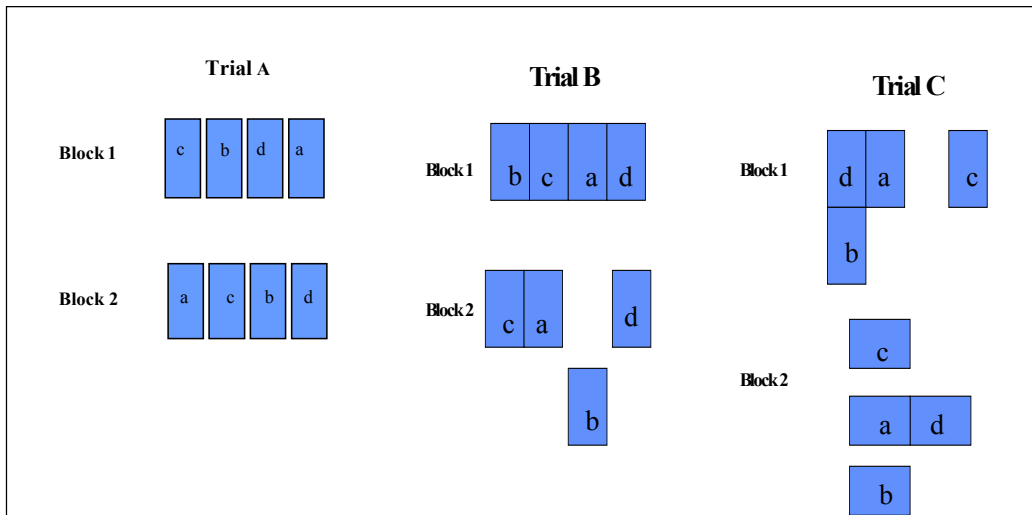
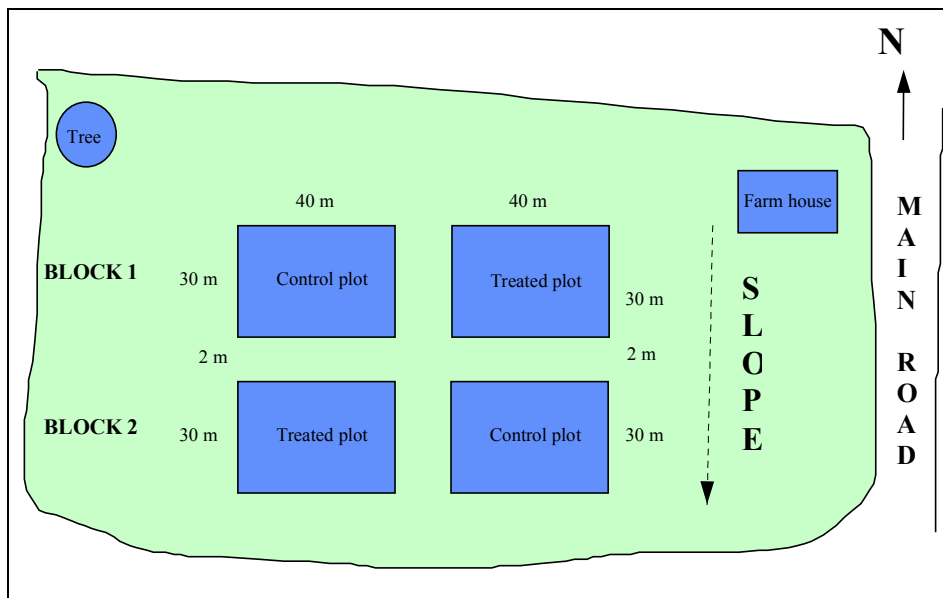


FIGURE 6
Example of a field plan



There should be a chart showing all the operations required during the course of the experiment and who is responsible for each operation (Chapter 2). In particular, the arrangements for harvesting the crop are crucial to the quality of the data collected. Therefore, overall supervisors of experiments should devise a method to remind themselves and all other officers of their responsibilities so that they are executed in a timely manner.

Data recording

Supervision and management are important, not only to ensure timeliness of operations, but also to ensure reliability of the data. In this regard, a data recording system should be designed so that the data are collected on essential variables as well as on secondary and concomitant variables which could enhance the interpretation and precision of the analysis. It is advisable to prepare a standard data recording sheet or procedure and field book for the persons responsible for data collection to ensure that they collect the same data at each site.

Persons responsible for data recording should be properly trained in the correct use of all measuring devices. Moreover, to safeguard the accuracy of the data, the measuring devices themselves should be checked and calibrated periodically to ensure that they are in good working order.

The data collected in the field should be checked immediately for abnormal values. Reasons should be sought to explain the abnormalities and these reasons noted on the data collection sheets. Only where abnormal values result from errors in recording should they be corrected.

Missing data

During the course of the trial, problems sometimes arise which are beyond the control of the researcher or extension officer and these result in missing or erroneous data. Some common causes and possible solutions are as follows.

Misallocation of treatments

Causes

When one or more experimental plots do not receive the intended treatment, or when an incorrect dose of the treatment is applied and observations are made as though the treatments were correctly applied.

Solutions

If the error is discovered and the researcher wishes to retain the modified treatment, then the analysis can be adjusted accordingly. In the case of the plot or plots receiving the unintended treatment, the data analysis could be that of an unbalanced design. For the incorrect dose, if only a few plots are affected, then those observations are discarded and the plot treated as a missing plot. Where all plots receiving that particular treatment at a particular site are affected, the analysis for that site would not be affected but that treatment should be excluded in the combined analysis.

Destruction of experimental plants

Causes

Diseases, pests, cattle or other animals, or bad weather (e.g. strong winds) may cause some experimental plants to lodge, or poor germination rates may reduce plant stands at harvest.

Solutions

Where losses are treatment related, the data should not be considered as missing. For example, if one of the objectives of the experiment is to determine the rates of N fertilizer which result in lodging of rice, then the yield in plots where plants were lodged should be considered zero and not missing. On the other hand, if lodging occurred in only one plot in a particularly windswept section of the field, then the data from that plot should be omitted and considered missing. In the case of seed germination, if there are a few plants missing from each plot, then all plots can be thinned to

a uniform plant population. However, if one treatment consistently has poor germination rates in each plot, those data collected from plots with that treatment are valid and should not be omitted from the analysis.

Loss of harvest

Causes

Predial larceny (theft), or the farmer may sell the crop before final measurements are taken, or part of the harvested samples may be lost due to malfunctions in the drying ovens.

Solutions

If data are lost from the entire trial or demonstration, no estimates of the missing values can be made. If data are lost from a few plots, it is possible to estimate the missing value using formulae from standard statistical texts, or to apply the analysis of an unbalanced design. Where one or more entire blocks are lost at some sites, depending on the number of farms affected, it may not be possible to estimate the effect of the farms nor the farm-by-treatment interaction. If only a few farms are affected, then the treatment effect and interaction may be estimated for the remaining farms.

In the case where data on the final yield are not available (because of larceny or damage), if readings were taken on a variable which gives some approximation of yield prior to the final harvest (e.g. number of fruits), then the analysis can be conducted on this variable instead. Therefore, it is advisable to include the measurement of such variables in the experimental protocol.

Chapter 4

Analysing the results

This chapter deals with the treatment of the data after they have been recorded in the field.

COLLECTING AND MANAGING THE RESULTS

Data should be re-examined after data collection to detect any additional errors. Queries should be made as to the sources of the errors and the appropriate measures taken to correct or eliminate the data as necessary.

There is a wide range of statistical software packages available for conducting the analyses. It is up to the individual researcher to decide on an appropriate one (Box 4). It is not necessary to purchase an expensive package with features which researchers are unlikely to use.

BOX 4: Tips on selecting statistical software

List of desirable features:

- User friendly;
- Easy to understand with quality documentation;
- Numerical accuracy;
- The appropriate analysis, i.e. it does what it says it can do;
- The desired output; few hand computations;
- Capability to interface data analysis with graphics;
- Capability to input files created by spreadsheet and word processing programs;
- Capability to output files to be used by word processing and desktop publishing programs;
- Fast.

For ANOVA applications:

- The analysis should be model driven rather than simply one-way or two-way, etc.;
- It must be able to handle multiple error terms for split-plot and other designs;
- Balanced and unbalanced data sets (including missing observations);
- Provide expected mean squares;
- Use specific contrasts;
- Work with covariates.

To these may be added for ANOVA and regression analysis applications:

- Provide residuals;
- Provide standard errors;
- Provide options for testing the assumptions of the analyses

Generally, the software packages favoured by professional biometricians are better at analysing on-farm trials covering more than one farm. At present, statistical analysis routines in most popular spreadsheet programs are inadequate for this type of work.

The software used and the structure of the data file should be the same for all trials and demonstrations. In addition, the data should be easily transferable to a central database as well as exported directly to statistical and graphics packages without transcribing (the more times data are

transcribed, the greater the chance of error). Data should also be secured so that they are available even when the project is no longer operational.

Data should be stored in appropriate spreadsheet or data base programs so that they can be easily manipulated and retrieved when needed for analysis. The choice of software for storage and analysis is at the discretion of the officer in charge.

Data storage and preliminary analysis should be initiated as soon as the data collection begins rather than at the end of the experiment. It is advisable to make at least two copies of the data, both on diskette and in hard copy. These copies should be stored in different places to safeguard against total data loss. At the end of each season, the entire data set should be analysed and the results interpreted so that conclusions can be drawn and recommendations made before the next crop.

PRELIMINARY DATA ANALYSIS

Once the data have been validated, it is advisable to do a preliminary analysis and data summary. This serves as a further check for anomalous values and helps form an initial impression of the data and the kinds of problems that are likely to arise with the detailed analysis.

A useful tool for such summaries is a table of treatment means and their corresponding standard deviations or variation coefficients. This table indicates whether the variability within treatments was the same and also shows the stability of the treatment.

Another valid tool for preliminary analysis, especially if the appropriate statistical software is available, is the box-plot (also called a box-and-whisker-plot). Box-plots display the spread and distribution of the observations, and identify unusually large or small observations.

DETAILED DATA ANALYSIS

Detailed analysis entails:

- ANOVA of individual site data;
- combined analysis of data from all sites;
- analysis of results from demonstrations.

Guidelines and examples are also given for analyses of data derived from trials where the same design or treatment combination was not used on all farms and for cases where existing data have not been previously analysed.

Analysis of data from individual sites

In conducting the analysis for the individual sites, the F test for replicates is usually not performed as it is assumed that the blocks were properly laid out.

It is advisable to include probability or P values in the ANOVA table as they give an indication of the degree of significance of the result. These P values can be estimated from statistical tables or calculated exactly from a computer routine. The convention is that if the P value for a particular F test is 0.05 (i.e. 5 percent) or less, it can be concluded that there is some evidence of a treatment effect. A P value of 0.01 (i.e. 1 percent) or less indicates that there is strong evidence of a treatment effect, and a value of 0.001 (i.e. 0.1 percent) or less indicates very strong evidence of a treatment effect.

The 5, 1 and 0.1 percent levels of significance are guidelines and should not override common sense in interpreting the results of the study. Before drawing conclusions, it is always necessary

to consider whether the results make sense and whether there is a logical explanation for them. If the results do not make sense, it is necessary to go back to the data set and try to find a more plausible explanation for the result.

Table 2 presents the ANOVA table of the data from an individual site for a single-factor trial comprising a randomized complete block design with two replicates and four levels of fertilizer. Appendix 2 gives the protocol for this experiment while Appendix 4 gives data and summaries of the results for the other sites. Appendix 3 gives the protocol for a two-factor experiment while Appendix 5 gives data and summaries for an individual site for a factorial experiment.

TABLE 2

ANOVA for data from Site F

Source of variation	Df	SS	MS	F	P*
Blocks	1	0.605	0.605	1.41	
Fertilizer	3	16.445	5.482	12.80	0.032
Fertilizer x blocks	3	1.285	0.428		
Total	7	18.335			

* P values are calculated from statistical routines and can also be obtained by using a spreadsheet program

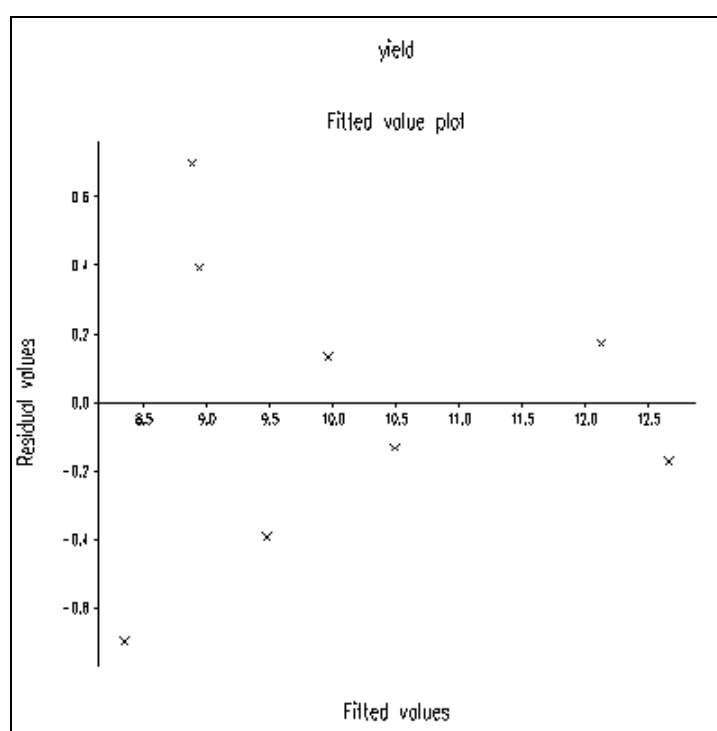
Residuals are the difference between the fitted model and the actual data. They provide a good indication of whether the assumptions of the analysis have been met. The residuals generated from this analysis were examined to determine if there were any serious violations of the assumptions of ANOVA. Figure 7 shows the resulting plot.

Although the plot shows a wider variation among the lower values than the higher values, the difference in magnitude of the residuals is very small. Therefore, it can be assumed that there were no serious violations of the assumptions of ANOVA and it can be concluded that there was evidence of a treatment effect.

Significance testing is usually not the only objective of plant nutrition trials. Researchers may be interested in estimating either the effect of the treatment or the optimum level of plant nutrient to be applied.

FIGURE 7

Residual plot for analysis of data from Site F



Analysis of combined trials

For the combined trials, the error terms differ depending on the effect being tested. The example given below presents the results of the ANOVA of the combined trials of the single-factor experiment described above.

TABLE 3
Results of the combined ANOVA of ten single-factor trials

Source of variation	Df	SS	MS	F	P
Sites	9	2 011.420	223.492	33.93	<0.001
Blocks within sites	10	65.873	6.587		
Fertilizer	3	107.562	35.854	4.14	0.015
Fertilizer x sites	27	233.966	8.665	2.19	0.019
Fertilizer x blocks within sites	30	118.398	3.947		
Total	79	2 537.222			

As seen in Table 3, the sites effect is tested against the blocks within sites term, the fertilizer effect is tested against the fertilizer x sites interaction, and the fertilizer x sites interaction is tested against the fertilizer x blocks within sites term.

The results of the analysis of combined trials for the factorial experiment, described in Appendix 3, are presented in Appendix 5.

Analysis of data derived from single-replicate demonstrations

For single-replicate demonstrations, if there is no information about the sites where the demonstrations are located or if it is suspected that the treatment effect will be similar at each site, then the analysis will be similar to that in Appendix 4. Instead of replicates, there will be a term for sites or farms, with the corresponding appropriate degrees of freedom.

However, if the sites vary in characteristics, such as soil type, farming system, or rainfall pattern, or if a different variety is grown at some sites, then the analysis should be adjusted to test for these other sources of variation.

An example is the case of a researcher who lays out nine demonstrations at various sites. Each demonstration comprises a single replicate of the same four treatments: no fertilizer, 60 kg/ha N, 120 kg/ha N and 180 kg/ha N. In this case, where no additional information is known about the sites, the ANOVA would be as shown in Table 4. However, if it is also known that the sites are located on three different soil types, e.g. Sites 4, 5 and 9 on soil type A, Sites 6 and 7 on soil type B, and all other sites on soil type C, then the analysis can be amplified to reflect this additional information. Table 5 shows the amplified analysis where the degrees of freedom for sites are further partitioned into soils and sites within soils. Similarly, the error term is partitioned into the treatments x soils interaction and treatments x sites within soils. The appropriate error term for testing the effect due to soils is the mean square for sites within soils. The appropriate error term for testing the treatment effect is the mean square for the treatments x sites within soils term.

TABLE 4
ANOVA of single-replicate demonstrations at nine sites

Source of variation	Df	SS	MS	F	P
Sites	8	56.874	7.109		
Treatments	3	17.632	5.878	17.92	<0.001
Treatments x sites	24	7.866	0.328		
Total	35	82.372			

TABLE 5
Amplified ANOVA of single-replicate demonstrations at nine sites

Source of variation	Df	SS	MS	F	P
Sites	8	56.874	7.109		
Soils	2	6.826	3.413	0.409	0.68
Sites within soils	6	50.048	8.341		
Treatments	3	17.632	5.878	19.330	<0.001
Treatments x sites	24	7.866	0.328		
Treatments x soils	6	2.390	0.398	1.309	0.303
Treatments x sites within soils	18	5.476	0.304		
Total	35	82.372			

The results indicate no evidence of an effect due to soils but very strong evidence of a treatment effect and no evidence of a treatment x soils interaction, thereby implying a similar effect of the treatments in the various soils.

Analysis of data derived from trials on a few farms

The analyses in the preceding sections refer to trials and demonstrations where the same design and the same number of replications were used on each farm. However, there may be instances where this is not the case (Chapter 2). For example, a more progressive farmer with more land may decide to accommodate an additional replicate, or the farm may not lend itself to a randomized complete block design because there is another source of variation in the field. In these cases, a row-and-column design is more appropriate.

Some of these designs might present problems at the analysis stage using some software packages. To inexperienced users, these packages often appear to be able to analyse only the common types of textbook experimental design. However, it is usually possible to analyse most types of design. For this reason, the examples below concentrate on designs that may not, at first glance, appear easy to analyse.

The examples below are for trials conducted on a small number of farms. Again, this is for demonstration purposes as the points raised and explained are best described in relatively small studies. In addition, because of the small number of treatments and replicates used on the individual farms in the following examples, the degrees of freedom for the error terms would be too small to give a reliable test of the treatment effects. Consequently, the ensuing sections present only the combined trial analysis.

Randomized block design with unequal replication on each farm

An example of a trial using soil amendments is used to describe this scenario. The purpose was to demonstrate the need to add a suitable soil amendment on acid soils in order to improve tomato yield. However, more importantly, the purpose was to investigate the feasibility of using one of two forms of soil amendment: lime or dolomite. The three treatments were:

- T1: Lime applied at the recommended rate;
- T2: Dolomite applied at the recommended rate;
- T3: No amendment.

The design used was simple randomized block on each of five farms. There were two randomized blocks on four of these farms. At the fifth farm, the farmer had more land available and three randomized blocks were planted. Appendix 6 presents the data.

Most available computer software is able to handle the combined trial analysis for this design. However, some software routines are not comfortable with the hierarchical (or nested) structure of blocks within farms. The important farms x treatments interaction can also give problems with

simple unsophisticated routines. However, using many general linear model routines, the following model fits correctly:

$$\text{Yield} = \text{Farms} + \text{Blocks} + \text{Treatments} + \text{Farms} \times \text{treatments}$$

With some other routines, knowledge of the software limitations and an ability to programme in the language of the software will solve the problem.

With some packages, it may be necessary to build up the ANOVA as follows:

- (1) Fit model Yield = Farm
- (2) Fit model Yield = Farm + Block
- (3) Fit model Yield = Farm + Block + Treatment
- (4) Fit model Yield = Farm + Block + Treatment + Farm x treatment

Fitting model (1) above will give the sum of squares for farm with degrees of freedom. Model (2) will give the sum of squares for farm plus that for blocks with the corresponding degrees of freedom similarly summed. Therefore, subtraction of the model (2) sum of squares from the model (1) sum of squares will give the correct value for the sum of squares of blocks within farm; the degrees of freedom are similarly subtracted. This process then continues: subtraction of the sum of squares (and degrees of freedom) for model (3) from those for model (2) will give the treatment terms in the ANOVA. Finally, subtraction of the values generated in model (4) from those in model (3) will give the interaction values. Model (4) also gives the values for error and total in the final ANOVA table. Mean square and F values are obtained in the usual way and P values can be estimated from statistical tables or calculated exactly from a computer routine. Figure 8 presents the results.

Large differences among tomato yields on each farm are apparent in Figure 8. The first line of the ANOVA (Table 6) explains these differences. The F value (8.49) on this line has a probability of less than 0.012. Perhaps more interesting are the treatment differences and interaction of farms and treatments (Table 7).

TABLE 6

ANOVA of tomato yields per plot (kg) in soil amendment trial on five farms

Source of variation	Df	SS	MS	F	P
Farms	4	191.20	47.80	8.49	0.012
Blocks within farms	6	33.78	5.63		
Treatments	2	16.01	8.01	24.32	0.000
Farms x treatments	8	42.52	5.32	16.15	0.000
Error	12	3.95	0.33		
Total	32	287.46			

Table 7 shows that the soil amendment application was successful on two of the five farms (Farms 1 and 4). On these two farms, particularly Farm 4, dolomite was better than the control. An economic analysis is required to investigate the relative benefit of T1 and T2.

TABLE 7

Treatment means by farm for soil amendment trial

Treatment	Farm					Overall
	1	2	3	4	5	
T1	6.3	4.5	1.1	9.7	6.4	5.6
T2	8.2	4.5	0.9	10.8	6.8	6.2
T3	4.2	4.8	1.0	4.7	7.1	4.4
sem	0.41	0.33	0.41	0.41	0.41	0.17

sem: standard error of means

Figure 9 shows the plots of the standardized residuals against the fitted values. There is some concern here about the fan-like shape of the plots. This indicates that the variance of the weights of the tomato increases with size, which is not uncommon when dealing with agricultural yields. As one of the assumptions of an ANOVA is that the variance of the responses is constant, then there could be concern that the ANOVA is not valid.

Table 7 gives a further caution. At Farm 3, the standard error is about 50 percent of the mean values. The usual advice in such circumstances is to transform the response data (in this case yields) and reanalyse. A suitable transformation to stabilize variances in this case is the square root. When this is done, the ANOVA table probabilities look very similar and the conclusions are much the same.

There is an indication of the robustness of the ANOVA technique. In a situation such as this where a transformation does not alter the conclusions drawn from the analysis, it is quite suitable to present the results from the untransformed analysis. However, it should be borne in mind that the standard error calculated for Farm 3 is almost certainly an overestimate of the true variation on that farm.

Different experimental design at each site

A tillage trial on two farms provides an example for the analysis of this trial design.

This trial investigated five methods of tillage in plots planted with cucumber. The control was no tillage, and there were four other treatments that formed a 2 x 2 factorial for ploughing and harrowing. The two depths of ploughing were 15 and 25 cm; the two harrowing levels were no harrowing and harrowing after ploughing. Thus, the five treatments can be described as follows:

T1: Control; no tillage;

T2: Ploughing 15 cm; no harrowing;

FIGURE 8
Tomato yields per plot (kg) in a soil amendment trial on five farms

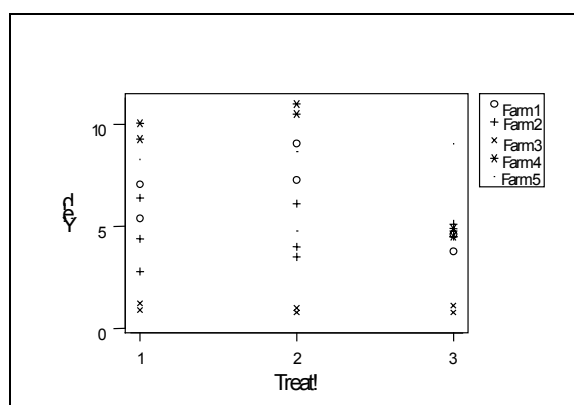
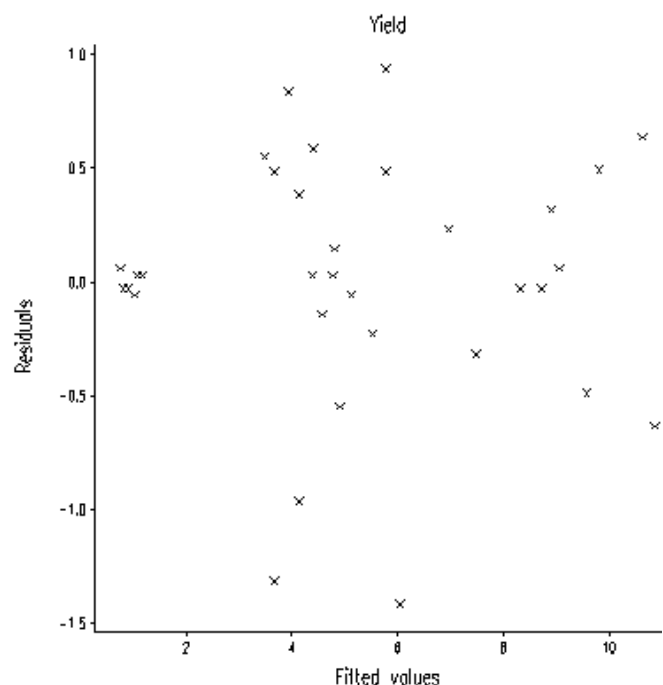


FIGURE 9
Residual plot for analysis of data from soil amendment trial



- T3: Ploughing 15 cm; harrowing;
 T4: Ploughing 25 cm; no harrowing;
 T5: Ploughing 25 cm; harrowing.

The trial was conducted at two farms where the soil was thought suitable for a no till situation. At the first farm (Farm 1), three replicates of a randomized block were set up. At the second farm (Farm 2), the layout of the land indicated clearly the need for a row-and-column design. There was a distinct slope and a known fertility gradient at right angles to the slope. Therefore, a 5 x 3 Youden square was set up.

Appendix 7 presents the data spreadsheet and the coding for the various sources of variation. Figure 10 presents the results and evidences the general superiority of yields at Farm 2, and the success of the row-and-column arrangement in controlling environmental variation at Farm 2.

Table 8 shows the ANOVA for the cucumber yields. These yields were the totals of several harvests.

This ANOVA was achieved by using a general linear model routine. The general linear model fitted was:

$$\text{Yield} = \text{Farms} + \text{Rows} + \text{Columns} + \text{Treatments} + \text{Farms} \times \text{treatments}$$

Again, it may be necessary to build up this model one step at a time using some software.

The computer may give a number of warnings about various rows and/or columns being aliased; these can probably be ignored. However, when the ANOVA is produced, the degrees of freedom should be checked.

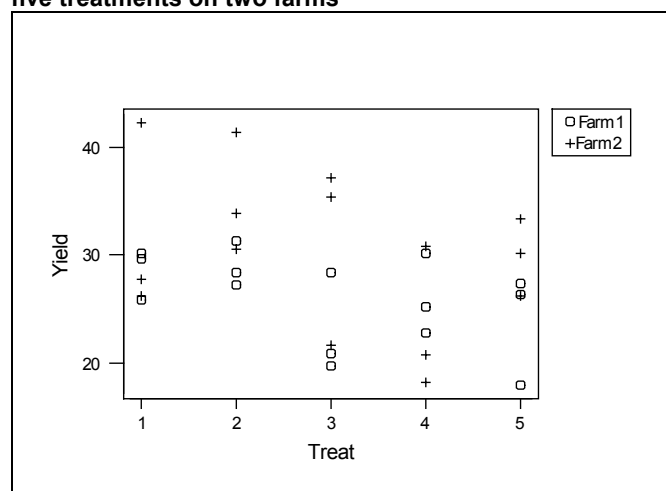
TABLE 8
ANOVA for plot yield of cucumber tillage experiment

Source of variation	Df	SS	MS	F	P
Farms	1	139.1	139.10		
Blocks (Farm 1)	2	16.9	8.50		
Rows (Farm 2)	4	359.9	90.00		
Columns (Farm 2)	4	247.6	61.90		
Treatments	4	91.5	22.98	1.88	0.1787
Farms x treatments	4	88.0	22.00	1.80	0.1937
Error	12	146.1	12.20		
Total	29	1 089.0			

The rows were separated into blocks on Farm 1 and rows on Farm 2 by running a separate model for Farm 1 only as follows:

$$\text{Yield} = \text{Rows}$$

FIGURE 10
Yields per plot (kg) of cucumber in a tillage experiment with five treatments on two farms



This gave the sum of squares for the blocks effect in Table 8. The rows effect was obtained by subtracting this value from the one obtained for rows when the full data set was analysed.

The P values for treatments and farms x treatments interaction in Table 8 indicate that the method of tillage did not have much influence on cucumber yield in this experiment.

Mean separation

It is possible to split the four treatments and four farms x treatments interaction degrees of freedom into contrasts which take account of the 2 x 2 factorial structure. This would give single degrees of freedom for two main effects and an interaction. The remaining degree of freedom is a contrast comparing the control with the overall mean of the other four treatments.

One way to achieve this is to eliminate the control treatment data from the spreadsheet and add coded columns for ploughing and harrowing (Appendix 7). The following model would be fitted:

$$\text{Yield} = \text{Farms} + \text{Rows} + \text{Columns} + \text{Ploughing} + \text{Harrowing} + \text{Ploughing} \times \text{harrowing} + \text{Farms} \times \text{harrowing} + \text{Farms} \times \text{ploughing} + \text{Farms} \times \text{ploughing} \times \text{harrowing}$$

When the computer produces the ANOVA, the first three terms (farms, rows and columns) can be ignored. The other terms are the required main effects and interaction and the interaction of these with farms. The contrasts for control vs overall mean of other treatments and the interaction of this with farms can be obtained by subtracting the original sum of squares (with five degrees of freedom) from the sum of the corresponding three single degrees of freedom on the second model. Table 9 shows the full ANOVA this generates.

TABLE 9

Full ANOVA (with contrasts) for plot yields of cucumber in tillage experiment

Source of variation	Df	SS	MS	F	P
Farm	1	139.1	139.1		
Blocks (Farm 1)	2	16.9	8.5		
Rows (Farm 2)	4	359.9	90.0		
Columns (Farm 2)	4	247.6	61.9		
Ploughing	1	27.4	27.4	2.25	0.1595
Harrowing	1	4.8	4.8	0.39	0.5440
Ploughing x harrowing	1	49.1	49.1	4.02	0.0681
Control vs other treatments	1	10.2	10.2	0.83	0.3802
Farm x ploughing	1	12.4	12.4	1.02	0.3325
Farm x harrowing	1	60.3	60.3	4.94	0.0462
Farm x ploughing x harrowing	1	6.5	6.5	0.53	0.4806
Farm x (control vs other treatments)	1	8.8	8.8	0.72	0.4128
Error	12	146.1	12.2		
Total	29	1 089.0			

Table 9 indicates that there may be an interesting interaction between farm and harrowing; the interaction between ploughing and harrowing is also close to the usual significance level of $P = 0.05$. Thus, it is necessary to examine the two-way tables of means which investigate these interactions (Tables 10 and 11).

TABLE 10

Investigation of ploughing x harrowing interaction. Mean cucumber yields per plot (kg) over both farms

Treatment	Depth of ploughing (cm)	
	15	25
No harrowing	32.1	24.6
Harrowing	27.2	26.7

Control (no tillage) 30.3; sem = 1.43

TABLE 11

Investigation of farm x harrowing interaction. Mean cucumber yields per plot (kg) over both ploughing depths

Treatment	Farm		sem
	1	2	
No harrowing	27.5	29.2	1.43
Harrowing	23.4	30.7	1.43
Control (no tillage)	28.5	32.1	2.02

The main interest in both these tables is in the good performance of the control treatments (no tillage). However, this has not caused the interactions. Table 10 shows that the best yields were obtained with no harrowing at the shallow depth of ploughing. Although this was scarcely any better than the control, it may have been marginally better than the other three non-control treatments. The farm x harrowing interaction is explained by the somewhat better performances of the no harrowing plots on Farm 1. At Farm 2, where yields were generally higher, the deleterious effect of harrowing did not seem to be present.

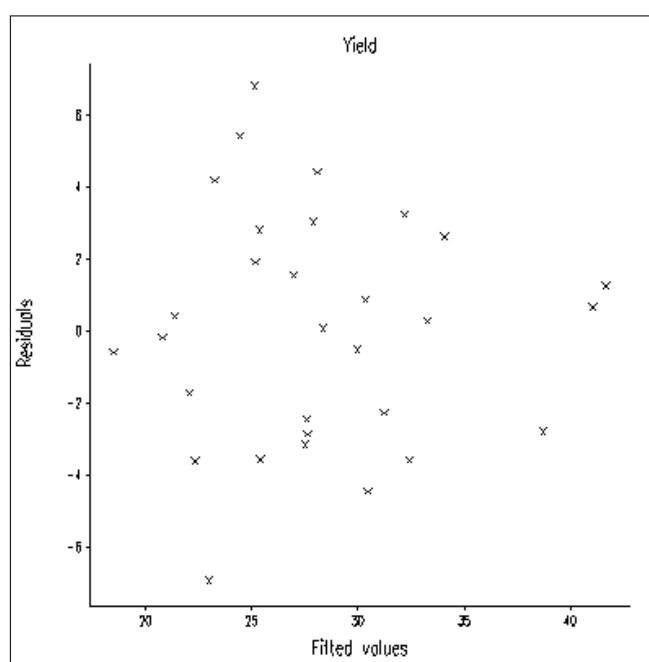
The plot of the fitted values versus the residuals (Figure 11) did not reveal any irregularities for investigation. The major result of this trial was that tillage was not advantageous on the farms selected.

Supplemented balance design

A soil conservation trial is used as the example for this design. The investigation was set up at four farms to try to control soil runoff where pigeon peas are grown on sloping land. There were four treatments:

- T1: Control treatment - farmer's practice where little is done to prevent soil runoff;
- T2: Pigeon peas intercropped with pumpkin planted at intervals of 0.5 m;
- T3: Pigeon peas intercropped with pumpkin planted at intervals of 1 m;
- T4: A drainage ditch was dug along the contour below each row of planted pigeon pea.

FIGURE 11
Residual plot for analysis of data from tillage trial



Three farms were found to be suitable, but on each farm it was thought that four treatments were too many. Therefore, the supplemented balance design of Figure 1 was used. It was decided to use T4, the probable successful treatment in each block, with the other three treatments appearing in only four of the six blocks at the three farms.

The objective of the experiment was to see whether either T2 or T3 was a feasible alternative to T4 (an unattractive proposition to farmers because of the manual work and time involved in the extra land preparation).

Soil runoff was measured by placing two soil catchment boxes at the foot of each plot. These boxes were placed exactly one-third and two-thirds, respectively, along the length of the bottom edge. Thus, the soil runoff data were a sample estimate of the total runoff as there was no attempt to capture all the soil runoff.

After each heavy shower of rain, the farmer removed each box and replaced it with another. The boxes were labelled clearly to indicate their placement position in the field.

Figure 12 plots the total soil runoff collected during the experiment in kilograms per unit of area against treatment for each farm. As expected, this figure indicates the superiority of T4; intercropping of pumpkin (T2 and T3) may have helped to control soil runoff, but not as successfully as T4.

The computer software fitted the following general linear model:

$$\text{Runoff} = \text{Farms} + \text{Blocks} + \text{Treatments} + \text{Farms} \times \text{treatments}$$

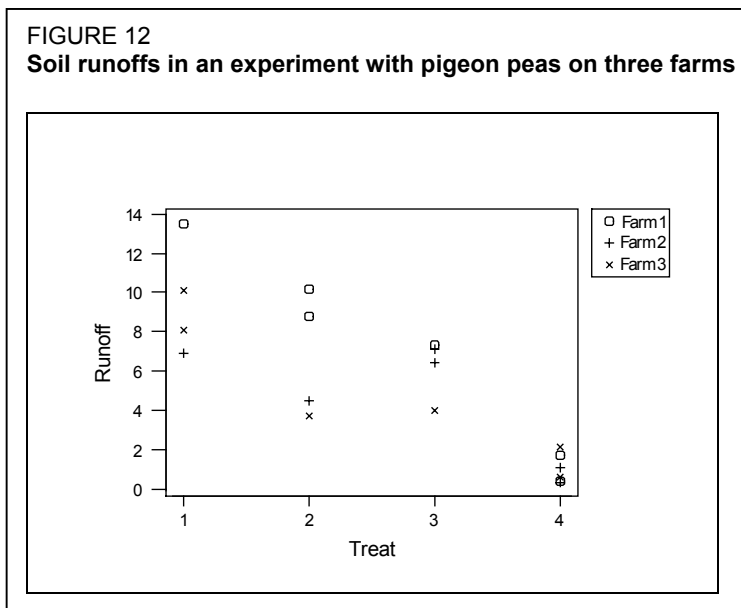


Table 12 shows the ANOVA table generated. It can be noted (and should have been noticed by the researcher at the design stage) that the error or residual degrees of freedom are very few. This is common in the type of on-farm trial that forms more of a demonstration to the farmers than a scientific inquiry to the research team. The demonstration purpose of this experiment has probably been achieved, but the scientific value may not be too considerable.

TABLE 12
ANOVA of soil runoff in pigeon pea on-farm trial

Source of variation	Df	SS	MS	F	P
Farms	2	23.64	11.82	14.49	
Blocks within farms	3	9.36	3.12	3.82	
Treatments	3	196.03	65.34	80.10	0.0023
Farms x treatments	6	30.63	5.11	6.26	0.0804
Error	3	2.45	0.82		
Total	17	262.11			

TABLE 13
Treatment means by farm for soil runoff (kg) from pigeon pea plots

Treatments	Farm		
	1	2	3
T1	13.5*	6.9*	9.1
T2	9.8	4.5*	3.7*
T3	7.3*	6.8	4.0*
T4	1.0	0.7	1.4

* Only one plot on the farm was allocated to the treatment; other treatments are means of two replicates

Table 13 shows the means for the treatments at each farm and the overall means. These means should be treated with caution because of the small numbers of replications per farm. For the same reason, standard errors could be misleading and so are not reproduced. However, the table shows the clear superiority of T4. The effectiveness of the two treatments with pumpkin over the do nothing control seems to be somewhat variable; this explains the large interaction between farm and treatment.

In practice, experiments of this nature often give large farm x treatment interactions. This is because the potential maximum runoff at each farm varies considerably according to factors such as slope, soil types and conditions, and rainfall. These may vary even between sites located quite close together.

Analysis of already existing data

Sometimes researchers inherit data sets from experiments already conducted that have not been analysed. Alternatively, an analysis may have been conducted but failed to provide the type of information needed. The data will therefore have to undergo another analysis. This section outlines the steps involved in conducting analyses on already existing data.

The first thing to do is to obtain additional information about the trials and demonstrations which yielded the data, such as the objectives, field plans, field books and a description of the treatments. Where the demonstrations were conducted on-farm, additional information which may be useful concerns: (i) the farm location, such as soil type, slope, rainfall and other physical characteristics; and (ii) the production system, such as varieties used, planting dates, use of organic matter and application of pesticides. Such information helps in stratifying the farms according to important categories. Chapter 2 provides examples of some useful stratification factors.

The second step is to examine the data carefully and validate them to identify any abnormally high or low observations. Field notebooks can be useful in determining the cause of these anomalous observations and in deciding whether to discard the data. Another good method of preliminary data screening is to plot graphs of the response variable (i.e. yield) against treatment or site. These graphs may show natural stratification of the data. Box-plots are also useful in identifying outliers.

Not all data may be utilizable for further analysis. After data screening and validation, some data sets may have to be discarded because of poor quality. The method of analysis applied to the remaining exploitable data will depend on the amount of additional information available, especially for single-replicate trials.

For trials with more than one replicate per farm, the method of analysis will depend on the experimental design. If a randomized complete block design is used, the analysis is similar to that in Appendix 4 or 5, depending on whether the treatment structure is factorial or not. Analyses are first conducted for the individual trials, followed by a combined analysis.

For single-replicate demonstrations, if no additional information is available to allow prior stratification of the data, and if the same treatments were applied at each location, then the results will need to be analysed as a randomized block design. Each farm will be considered a block (see example above).

If no additional information is available to allow prior stratification, and if there are some common treatments at each location, but the non-common treatments differ from one location to another (i.e. no replication of the non-common treatments), then the simplest analysis should be conducted only on the common treatments. This analysis will be as above, a randomized block design with each location as a block. Alternatively, a statistician may be needed to assist with the analysis.

Where additional information is available to allow prior stratification, subsets of the data can be analysed according to the predominant categories identified. Analysis may take the form of ANOVA or regression analyses, depending on the number and nature of the treatments, as indicated in Chapter 3.

Additionally, the categories may be used as factors which could then be tested using ANOVA or regression techniques to determine their effects on the response variable.

OTHER STATISTICAL ISSUES

Checking the assumptions of the analysis

An important and often overlooked aspect of the analysis is the checking of the assumptions of the analysis. The three main assumptions are: normality, independence, and homogeneity of variance of the error. The assumption of independence is related to randomization. Hence, it is important to allocate treatments to plots randomly and to use a new randomization at each site. For on-farm data, the consequences of violating the assumption of normality of on-farm data are not serious. However, violation of the assumption of homogeneous variances can lead to erroneous conclusions. Therefore, the assumption of homogeneity of variances should always be tested. This is easily done by examining the residual plot. If the plot shows a non-random scatter of points about the zero line, this implies that the assumption has been violated. In this case, remedial measures should be taken to address the problem. For example, the residual plot shown in Figure 11 depicts a non-random scatter.

For the combined analysis, the assumption of homogeneity of variances is more important as it affects decisions on the adaptability of the treatments over a range of sites. An easy way to check for heterogeneity of variances is to draw a scatter plot of the variances (or standard deviations) against means. A scatter plot of the logarithm of the variance or standard deviation and means may also be useful. If the scatter plot shows no evidence of a relationship between the means and standard deviations and little difference in the magnitude of the variances or standard deviations, then it can be assumed that the variances are homogeneous. If the plot shows evidence of a difference among site variances, then the treatments x sites interaction is not valid. In this case, the analysis should be adjusted by combining sub-groups of sites which have homogeneous variances and conducting the combined analysis on those sub-groups or using transformations.

Transformations

Transformations may be applied where there are serious violations of the assumptions of the analysis. Box 5 summarizes the types of transformations and the circumstances under which they are generally applicable.

BOX 5: Transformations and where they may be applicable

Log

- Where the standard deviations (not variances) of the samples are roughly proportional to the means;
- Where effects are multiplicative rather than additive;
- Where residual plot has the shape of a right opening megaphone (like this: <).

Square root

- Where variances are proportional to the means of the samples.

Arc sine or angular

- Where data are expressed as percentages or proportions.

When presenting results where data have been transformed, it is advisable to back transform the means (reverse the transformation applied to the data). This gives an idea of the results in the scale in which the data were measured. Both transformed and back transformed means should be presented as the transformed standard errors must be used to make the treatment comparisons.

Mean separation

In some trials, the treatments are qualitative (e.g. fertilizer type such as urea, CAN or ammonium sulphate) rather than quantitative (e.g. rates of fertilizer: 0, 10, 20 kg/ha). Thus, the objective of the trial is not to derive a response curve, but to investigate the nature of effects of different plant nutrient sources. Instead of making all possible comparisons of treatments, the treatments should be selected to allow meaningful comparisons of interest between sub-groups.

Site F presented in Table 2 (table of means in Table A-3 in Appendix 4) can serve as an example. The treatments are a control and three different sources of N: organic matter, urea or CAN. Thus, from Table A-3, the mean yield of the control plot was 8.6 t/ha, for plots treated with organic N it was 12.4 t/ha, and for urea and CAN the yields were 9.2 and 10.2 t/ha respectively. A possible set of meaningful comparisons could be:

- control vs the rest;
- organic vs mineral (i.e. organic vs urea and CAN);
- between mineral sources (i.e. urea vs CAN).

Then the treatment effect of the ANOVA can be further subdivided to test these comparisons as shown in Table 14.

TABLE 14
ANOVA showing specific comparisons

Source of variation	Df	SS	MS	F	P
Blocks	1	0.605	0.605		
Treatments	3	16.445	5.482	12.8	0.032
Control vs rest	1	5.802	5.802	13.60	0.034
Organic vs mineral	1	9.541	9.541	22.30	0.018
Urea vs CAN	1	1.102	1.102	2.57	0.207
Treatments x blocks	3	1.285	0.428		
Total	7	18.335			

The results indicate evidence of a difference between the control and the mean response of the plots which received fertilizer. The control plot had a mean yield of 8.6 t/ha while the corresponding mean for the fertilized plots was 10.6 t/ha (i.e. the average of 12.4, 9.2 and 10.2). There was also some evidence of a difference in effect of the organic and mineral fertilizers. Plots treated with the organic plant nutrients had a mean yield of 12.4 t/ha, while those treated with mineral fertilizer had a mean yield of 9.7 t/ha (the average of 9.2 and 10.2). There was no evidence of a difference in effect between the two mineral fertilizers.

Missing data

The main point to remember about analyses with missing data is that the standard error of difference between treatment means varies depending on whether the comparison involves treatments with missing plots or not. Therefore, output from computer analysis should be examined to determine whether the computer program reflects this difference. Otherwise, the appropriate standard errors will need to be calculated manually.

Response curves

Where treatments are quantitative, one possible objective may be to derive the equation for the response curve or production function. These curves or production functions may be derived for a single factor or more than one factor.

Single factor

The first step in determining the most appropriate model is to make a scatter plot of the treatment means vs the respective treatment levels. This scatter plot gives an idea of the possible models which can be used to describe the data.

Where the response is non-linear, as is often the case in plant nutrition studies, four models are commonly used: quadratic, square root, broken stick and Mitscherlich. This publication discusses only the first two models.

Figure 13 shows possible graphs of the four functions mentioned in Table 15. Appendix 4 summarizes the fits of the quadratic and square root regression models for the data at Site H.

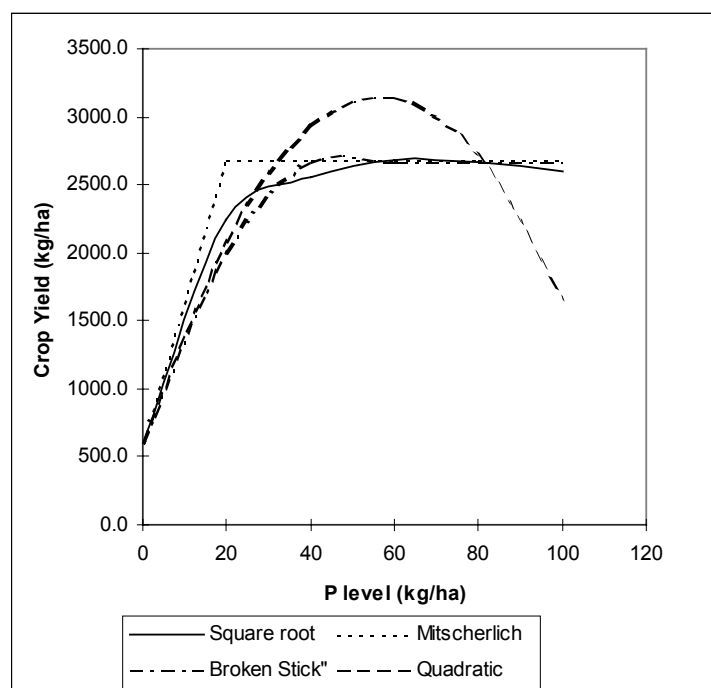
TABLE 15

Function forms of models used in plant nutrition studies

Model	Functional form
Quadratic	$Y = a + b_1X + b_2X^2$
Square root	$Y = a + b_1X + b_2 \sqrt{X}$
Broken stick	$Y = a + b_1(X - b_2 - X - b_2)$
Mitscherlich	$Y = a + b_1 \exp(b_2X)$

FIGURE 13

Graph of alternative functions for modelling crop response to plant nutrients



Multiple regression

The steps involved in fitting the regression with more than one factor are similar to those of the simple regression:

1. Draw scatter plots of the dependent variable (e.g. yield) against each factor individually to form an idea of the terms to include in the model.
2. Include interaction terms if they are thought to be important. In determining the variables to be included in the model, care should be taken that the number of variables does not exceed the number of observations used to fit the regression.
3. Use all possible subsets or step-by-step regression analysis to derive the most important variables which should be included in the response function.
4. Draw a scatter plot of the residual of the 'best' model to check for violations of assumptions of regression analysis.

Appendix 5 presents a worked example.

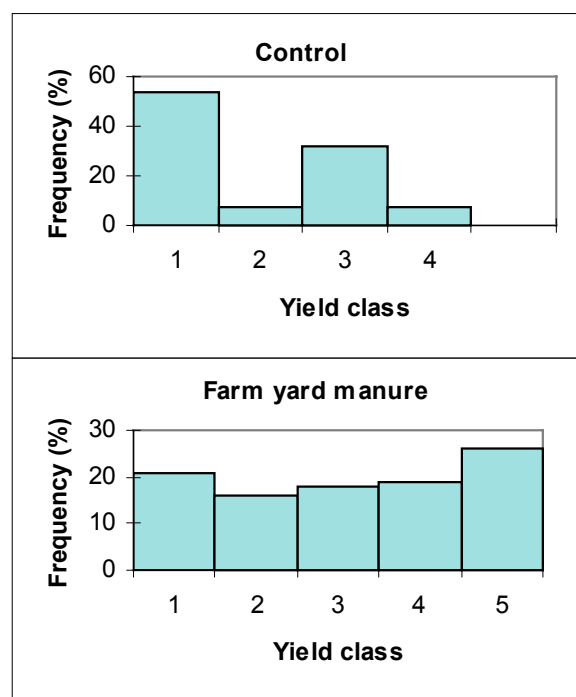
Frequency distributions

Frequency distributions give a good indication of the number of farmers attaining a specified level or range of productivity, gross margin or farm income. This information is useful in determining the likelihood of farmers accepting a particular technology. Frequency distributions constructed from data for successive years also show how stable the technology is, and, therefore, how risky investment in the technology is likely to be.

Figure 14 shows that using the farmer's practice the majority of farmers obtained low yields, whereas using FYM about 26 percent of the farmers obtained yields over 5 t/ha.

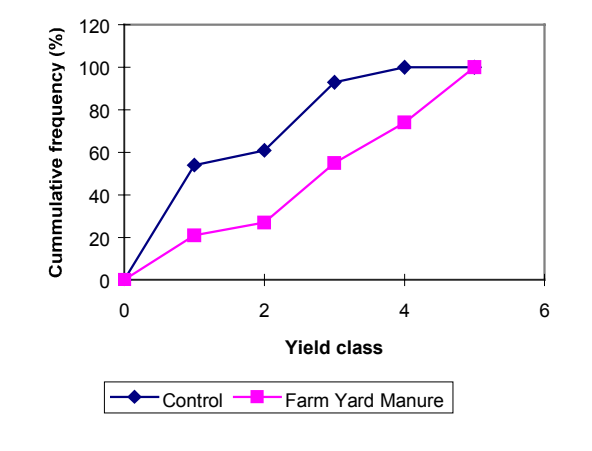
Cumulative frequency distributions can be used to estimate the probability of obtaining a certain level of production, productivity or gross margin. This probability value is also a good index of the risk involved in applying the proposed technology.

FIGURE 14
Frequency distributions of maize yield using the farmer's plant nutrition (control) vs FYM



Yield classes: 1 = < 2 t/ha, 2 = 2-3 t/ha, 3 = > 3-4 t/ha, 4 = > 4-5 t/ha, 5 = > 5 t/ha

FIGURE 15
Cumulative frequency distribution of maize yield using farmer's plant nutrition (control) and FYM



Most computer programs generate frequency distribution and cumulative frequency distributions.

Figure 15 presents the cumulative frequency distribution for two previously mentioned treatments. The graph shows that the control, with the steeper slope, was less risky, but that the farms with the FYM treatment were able to attain higher yields.

ECONOMIC EVALUATION

Economic evaluation aims to identify the treatments, quantity of input, or combination of inputs which satisfy the farmer's production objective, e.g. maximize financial returns, or reduce hired labour. To conduct this evaluation, additional information is required, such as the quantities and prices of all farm inputs and products, to derive farm budgets. Gross margins and returns to inputs such as land, labour or capital can then be calculated.

The data derived from the farm budgets can then be used for further analysis such as:

- ANOVA of gross margins;
- Marginal rate of return (MRR), value/cost ratios and production index;
- Estimation of optimum inputs - single and multiple factors.

ANOVA of gross margins is identical to that of yield. It is useful in identifying the treatment with the highest gross margins.

MRR indicates the additional gross margin obtained per unit of additional variable cost. It is calculated as follows:

$$\text{MRR} = \text{marginal increase in gross margin} / \text{marginal increase in variable costs}$$

Using the example in Appendix 4, Site F, and the table of means in Appendix A4.3, Table 16 shows how the MRR is calculated. It assumes a fertilizer price of US\$0.52/kg, and a product price of US\$160/t.

TABLE 16

Calculation of marginal rate of return

Treatment	Average yield (t/ha)	Gross income ¹	Fertilizer cost ² (US\$)	Gross margin ³ (US\$)	Marginal income ⁴	Marginal cost ⁵	Marginal rate of return ⁶
Control	8.6	1 376	-	1 376.00	-	-	-
40 kg N/ha	12.4	1 984	20.80	1 963.20	587.20	20.8	28.2
80 kg N/ha	9.2	1 472	41.60	1 430.40	(532.80)	20.8	(25.6)
120 kg N/ha	10.2	1 632	62.40	1 569.60	139.20	20.8	6.7

1. Gross income = average yield x product price

2. Fertilizer cost = rate of application x fertilizer price (this column may be replaced by total variable cost)

3. Gross margin = gross income - fertilizer cost

4. Marginal income = gross margin for a specific rate of application - gross margin for the previous rate of application

5. Marginal cost = fertilizer cost for a specific rate of application - fertilizer cost for the previous rate of application

6. Marginal rate of return = marginal income / marginal cost

Using the same example for Table 17, the calculations for the productivity index and value cost ratio (VCR) are:

$$\text{VCR} = \text{value of yield increment due to a specific input} / \text{cost of the input};$$

$$\text{Productivity index} = \text{yield} / \text{quantity applied to obtain the yield}.$$

TABLE 17

Calculation of productivity index and value cost ratio

Treatment	Average yield (t/ha)	Yield increase ¹	Value of yield increase ² (US\$)	Productivity index ³	Fertilizer cost ⁴	VCR ⁵
Control	8.6	-	-	-	-	-
40 kg N/ha	12.4	3.8	608	310	20.8	29.2
80 kg N/ha	9.2	0.6	96	115	41.6	2.3
120 kg N/ha	10.2	1.6	256	85	62.4	4.1

1. Yield increase = yield for a particular treatment - control plot yield

2. Value of yield increase = yield increase x product price

3. Productivity index = yield increase / quantity of fertilizer applied (in this example, yield x 1 000 to convert it to kilograms per hectare)

4. Fertilizer cost = quantity applied x fertilizer price

5. VCR = value of yield increase / fertilizer cost

Economic optimum application rates can also be derived from response curves. For a single factor, the optimal rate is the rate at which profit is maximized. This is where the first derivative of the response curve (dy/dx) is equal to the ratio of the input (P_x) and product (P_y) prices (expressed in the same units), i.e. where:

$$dy/dx = P_x/P_y$$

Table 18 gives the equations for calculating optimal rates for the square root and quadratic models described above. Appendix 4 contains a worked example.

TABLE 18

Formulae for calculating optimal rates for square root and quadratic growth models

Model	Optimal rate = X
Quadratic	$X = (E_x - b_1)/2b_2$
Square root	$X = [0.5 b_2/(E_x - b_1)]^2$

 b_1, b_2 = regression coefficients of the respective models E_x = input and product price ratio (P_x/P_y)

EVALUATION OF FARMER RESPONSE TO THE TECHNOLOGY

One of the main reasons for conducting on-farm research is to examine the farmers' response to the new technology. Therefore, evaluation of farmer response to the technology should be an integral part of the analysis.

This evaluation need not be very formal. It can be done through discussions on a farm visit or at a farmer group meeting. Important points to consider are:

- Do the farmers understand and feel confident about implementing and managing the new technology?
- Do the farmers think that the new technology is better than their current practice? If so why and if not why not?
- What do the farmers like about the new technology?
- What do the farmer dislike about the new technology (especially with respect to labour and other inputs)?
- Will the farmers try all or part of the new technology in the next cropping season? If not why not?

Chapter 5

Coming to conclusions

This is probably the most important step in the entire experimentation process and must, therefore, be done at the end of each growing season, as soon as the results are analysed. The conclusions drawn may be used to derive recommendations for farmers. Researchers should only make recommendations after testing the technology over a range of conditions for more than one season. Moreover, the results could indicate the need for further fine-tuning of the treatments in subsequent trials.

In coming to conclusions, the following checklist should be used as a guide to determine whether the trial was appropriate and whether the results are valid and utilizable.

- Are the objectives of the trial or demonstration clearly defined and relevant to the problem to be solved?
- Will the technologies selected solve the problem and show the results expected?
- Are the farmers selected to host the demonstrations representative of the target group?
- Are the sites selected on farmers' fields typical of the farmers' farming conditions and farming systems?
- What check or control plot data are used for comparing the new technologies? Are they appropriate (e.g. using data from the previous season's crop is not recommended because the climatic conditions may have been significantly different from those of the trial or demonstration)?
- Are the data collected from plots of similar size?
- Is there replication of the same technologies on other farms (i.e. same set of technological components used on each replicate farm)?
- Is this replication adequate (i.e. at least 10 or 20 farms testing the same technologies)?
- Are the indicator variables measured appropriate for testing the response to the interventions (i.e. variables other than yield that would allow economic analysis of the results, such as family, female or hired labour, or days to harvest)?
- Are additional, concomitant variables measured to allow stratification of farmers for identification of agronomic constraints and interpretation of the results?
- Is farmer assessment of the technologies measured to determine their acceptance or rejection of the technologies?
- Are the methods of analysis appropriate for obtaining the information needed?

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Appendix 1

Table of random permutations of numbers

90	78	82	54	47	20	83	80	10	41	35	22	23	03	98	79	74	41	35	05	78	73	95	47	83
78	58	68	87	41	11	08	81	29	89	71	23	10	01	79	25	06	00	45	80	64	70	95	34	29
51	42	21	03	88	20	05	35	93	00	68	12	09	55	09	36	54	95	22	82	48	30	09	56	87
93	15	07	60	86	67	37	94	24	35	82	44	19	92	96	21	84	29	04	29	83	32	05	10	48
27	12	31	66	62	09	54	17	31	23	27	30	37	36	79	75	50	39	57	12	67	23	22	09	33
79	44	83	55	47	96	50	93	56	82	58	16	35	18	87	64	08	22	47	93	86	43	43	30	17
89	73	43	91	03	57	91	35	40	64	13	61	94	37	16	09	93	96	25	87	30	23	42	54	31
29	30	90	00	58	15	99	93	33	67	80	08	59	21	66	13	54	56	85	25	05	32	03	52	52
97	33	17	26	25	04	73	18	10	05	34	40	32	65	07	28	68	29	31	97	89	57	95	55	16
07	15	44	92	47	28	50	93	03	53	37	70	19	68	59	95	39	87	90	46	98	64	46	24	71
82	50	35	50	80	23	67	81	25	02	83	08	12	70	00	25	31	33	80	06	19	86	14	59	27
59	21	86	16	30	27	85	16	26	34	50	15	87	22	69	71	36	95	90	76	90	99	79	63	21
04	19	60	33	05	29	02	33	74	56	38	84	21	07	35	93	54	70	18	47	14	62	75	45	02
96	91	44	09	94	06	89	50	88	83	82	50	11	82	51	30	68	91	06	28	86	65	17	45	20
31	71	03	53	38	94	02	52	72	15	44	49	53	42	43	00	36	97	67	64	12	27	46	00	18
03	70	22	67	59	98	10	64	68	08	79	06	89	48	41	85	72	10	87	24	96	04	20	68	00
08	45	79	46	89	74	73	67	60	15	70	37	61	44	07	67	89	81	54	26	57	17	63	27	74
37	80	05	75	64	48	51	68	68	27	71	75	45	32	27	76	35	26	58	88	67	74	48	90	94
90	63	56	69	37	19	74	48	63	31	52	36	84	40	66	72	66	03	41	87	65	29	12	36	64
22	69	38	02	88	89	71	43	01	87	41	79	42	99	29	41	08	47	32	19	45	29	59	69	90
05	79	69	67	64	36	14	82	65	26	40	51	63	42	48	85	48	34	12	04	33	26	52	26	52
48	91	53	03	82	64	24	06	31	03	97	44	82	24	89	88	48	66	54	10	41	27	09	11	61
94	64	97	27	25	62	23	94	40	54	56	32	97	78	90	58	86	41	75	19	42	90	85	36	68
15	85	82	52	08	52	96	26	92	88	93	11	03	23	52	78	23	57	85	43	53	90	42	22	22
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43	83	99	02	76	12	16	45	52	66	35	70	93	09	52	75	40	34	35	62	65	42	27	20	59
31	98	09	80	62	75	26	64	57	26	46	41	47	90	97	99	46	10	51	42	73	28	98	89	91
81	35	42	62	84	37	02	59	78	16	17	96	05	71	39	88	05	34	05	92	22	43	89	66	89
97	95	56	39	75	65	47	61	86	33	14	88	55	33	69	70	87	79	94	46	17	61	72	27	01
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39	87	38	16	06	82	92	62	32	75	67	64	50	49	39	29	55	53	92	97	04	48	60	53	90
37	73	01	84	87	42	88	30	93	75	01	18	34	73	30	28	44	28	18	01	00	38	26	38	57

15	62	38	72	92	03	76	09	30	75	77	80	04	24	54	67	60	10	79	26	21	60	03	48	14
77	81	15	14	67	55	24	22	20	55	36	93	67	69	37	72	22	43	46	32	56	15	75	25	12
18	87	05	09	96	45	14	72	41	46	12	67	46	72	02	59	06	17	49	12	73	28	23	52	48
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97	35	74	05	75	42	13	49	48	38	74	19	06	42	60	20	79	90	81	77	18	51	71	27	27
53	09	93	28	29	80	19	68	30	45	94	49	49	71	21	93	93	71	30	34	52	65	83	40	13
26	36	68	48	09	37	69	26	22	80	23	34	10	45	70	83	51	07	37	44	62	96	74	42	64
49	16	57	15	79	56	63	22	94	28	11	39	69	55	38	53	06	97	20	42	09	14	90	43	48
03	51	79	78	74	75	23	73	75	98	47	85	07	26	02	61	28	01	22	16	14	12	15	67	22
21	88	87	28	48	23	44	03	03	80	53	89	07	87	93	30	17	84	17	74	16	53	31	39	01
56	41	73	33	41	59	16	59	50	98	24	24	87	06	75	99	52	09	88	05	86	25	43	50	94
72	39	19	70	17	01	04	01	22	33	04	84	63	27	65	84	39	45	55	31	95	88	93	90	37
97	28	25	81	49	71	69	22	04	51	56	46	56	15	10	69	59	99	50	29	33	50	16	93	09
18	87	02	72	08	74	52	16	03	82	20	19	66	23	62	37	51	04	89	31	32	19	59	85	57
53	40	11	75	45	13	56	85	31	37	09	17	71	96	79	39	50	79	27	62	71	14	95	53	03
60	49	03	41	56	78	33	77	28	92	21	90	10	62	01	97	06	45	01	19	95	12	24	18	52
09	16	12	75	04	39	69	95	00	48	26	85	28	73	08	66	92	10	66	75	62	61	27	82	57
64	20	19	87	54	88	15	12	54	24	06	99	57	07	28	51	34	54	98	50	70	88	02	86	48
31	28	07	58	77	03	98	26	76	09	10	44	57	61	28	60	29	85	70	79	80	29	19	98	92
80	04	28	47	76	35	73	67	78	28	09	39	88	63	74	41	26	92	42	33	06	80	06	33	84
24	60	22	51	19	34	54	08	24	73	86	72	11	44	69	76	90	81	17	85	57	47	35	16	84
59	16	11	26	29	18	97	78	44	43	58	92	78	70	80	09	65	32	68	26	65	73	90	50	46
58	54	29	98	27	40	51	92	07	13	58	41	59	56	94	16	32	51	42	54	77	37	13	85	19
20	18	34	22	73	57	40	67	17	28	63	57	74	36	18	65	55	25	50	68	35	90	00	03	38
53	90	46	56	19	50	58	33	84	53	14	74	17	40	73	86	11	04	02	04	02	28	49	62	36
97	16	93	94	65	70	95	95	83	20	91	42	57	95	63	00	86	29	02	53	02	27	86	70	95
72	55	71	70	92	04	22	53	19	29	67	29	13	56	70	45	73	45	05	04	32	43	30	93	41
99	19	72	58	35	49	09	26	00	74	26	42	94	52	02	83	31	85	65	66	31	97	67	52	15
48	21	49	72	97	79	19	64	81	82	78	92	51	96	51	28	79	13	20	82	34	81	39	46	86
52	37	68	15	53	22	98	30	16	31	83	24	87	69	29	24	85	44	25	50	75	62	83	95	41
97	50	52	53	52	26	78	21	68	69	57	79	42	40	89	55	81	75	24	52	51	32	79	97	05
36	05	09	18	11	71	01	63	17	60	11	65	19	43	07	44	86	19	58	92	23	71	32	96	19
20	79	70	09	30	81	14	53	80	93	71	94	10	18	14	83	69	76	53	25	27	36	65	65	05
13	07	89	72	08	00	37	75	14	94	83	85	06	72	66	07	47	30	17	11	16	02	63	97	30
94	26	82	37	43	34	23	00	14	50	96	85	41	17	71	69	20	15	98	82	79	69	68	50	31
13	55	88	38	43	75	37	43	83	85	53	74	54	62	99	68	93	74	43	95	06	26	79	78	87
02	44	24	97	71	97	93	12	70	89	42	52	33	24	91	05	87	53	15	77	49	92	83	97	80
34	90	96	63	54	22	84	36	38	99	85	36	25	03	27	49	24	72	10	50	95	14	18	26	64
13	67	06	34	98	04	20	80	12	54	01	18	54	20	76	92	10	47	04	65	54	45	82	42	90
18	75	55	82	66	34	77	27	71	79	67	65	85	92	68	16	43	83	18	74	12	48	68	87	22
91	25	52	57	15	21	54	40	05	50	67	51	66	45	69	84	72	74	32	30	17	70	40	90	24
76	24	00	14	92	14	29	12	17	73	77	46	44	24	30	48	50	36	30	24	93	08	01	39	37

Appendix 2

Protocol of a single-factor experiment

Title: Fertilizer rate trial on maize

Objective: To determine the response of maize to increasing levels of N fertilizer.

Location: Ten trials distributed in XYZ region.

Treatments: 1 = Control without fertilizer
 2 = 40 kg N/ha
 3 = 80 kg N/ha
 4 = 120 kg N/ha

Experimental materials:

Local variety used by farmers - seeds distributed by extension service
 Unit field dimensions: 100 m x 100 m

Observations:

Type of data	Variables measured
Primary	Total grain yield (kg/plot converted to t/ha)
Concomitant	Number of plants per plot, time of flowering, lodging, pest and disease incidence, levels of soil N, P and K
Management	Date of land preparation, planting, thinning, fertilizer application, weeding, spraying, harvesting, etc.
Farming system	Rainfall, soil type, slope, cropping history, intercrops, crop rotations, major weeds, pests and diseases

Experimental design:

Randomized complete block design with 2 blocks of 4 plots each, blocks need not be contiguous.

Scheme for analysing results:

For each plot, dry grains to 14°C

For each trial and each variable measured or calculated, the ANOVA is:

Source of variation	Df (general)	Df (specific)
Blocks	$b - 1$	1
Fertilizer	$f - 1$	3
Fertilizer x blocks	$(b - 1)(f - 1)$	3
Total	$bf - 1$	7

For the group of trials and for each variable measured and calculated, the ANOVA table is:

Source of variation	Df (general)	Df (specific)
Sites	$s - 1$	9
Blocks within sites	$s(b - 1)$	10
Fertilizer	$f - 1$	3
Fertilizer x sites	$(f - 1)(s - 1)$	27
Fertilizer x blocks within sites	$s(f - 1)(b - 1)$	30
Total	$sbf - 1$	79

The fertilizer effect should be compared with the fertilizer x sites term in the ANOVA table.

Other analyses:

Plot mean yield vs fertilizer level for each site to determine whether there is an optimum level of fertilizer.

Curve fitting to obtain response function.

Estimation of optimum fertilizer level to apply.

Responsibilities

Activity	Person responsible
Procurement of fertilizer, seeds and other inputs	
Distribution of fertilizers and seeds among farmers	
Selection of experimental plots and marking out of plots, randomization	
Weighing out fertilizer for application to plots and application to plots	
Supervision between implementation and harvest	
Data collection	
Validation of data collected in the field	
Statistical analysis	
Interpretation of results and making recommendations	

Appendix 3

Protocol of a two-factor factorial experiment

Title: Effect of N and P on yields in maize

Objectives: To determine the response of maize to increasing doses of N and P₂O₅ fertilizer

Location: Ten trials distributed in the ABC region.

Treatments: All 4 combinations of 2 levels of N fertilizer and 2 levels of P₂O₅ fertilizer
 N levels: 0 and 60 kg N/ha
 P levels: 0 and 40 kg P₂O₅/ha

Experimental material:

Unit parcel dimensions: 30 m x 30 m

Observations:

Type of data	Variables measured
Primary	Total grain yield (kg/plot converted to t/ha)
Concomitant	Number of plants per plot, time of flowering, lodging, pest and disease incidence, levels of soil N, P and K
Management	Date of land preparation, planting, thinning, fertilizer application, weeding, irrigation, spraying and harvesting
Production system	Rainfall, soil type, slope, cropping history, intercrops, crop rotations, major weeds, pests and diseases

Experimental design:

Randomized complete block design with 4 plots per block and 2 blocks.

Scheme for data analysis:

For each trial and each variable measured or calculated, the ANOVA is:

Source of variation	Df (general)	Df (specific)
Blocks	$b - 1$	1
Treatments ¹	$np - 1$	3
N	$n - 1$	1
P	$p - 1$	1
N x P	$(n - 1)(p - 1)$	1
Treatments x blocks	$(b - 1)(np - 1)$	3
Total	$bnp - 1$	7

1. Sums of squares and Df for treatments are partitioned into N, P and N x P interaction.

For the group of trials and for each variable measured and calculated, the ANOVA table is:

Source of variation	Df (general)	Df (specific)
Sites	$s - 1$	9
Blocks within sites	$s(b - 1)$	10
Treatments	$np - 1$	3
N	$n - 1$	1
P	$p - 1$	1
N x P	$(n - 1)(p - 1)$	1
Treatments x sites	$(np - 1)(s - 1)$	27
Treatments x blocks within sites	$s(np - 1)(b - 1)$	30
Total	$sbnp - 1$	79

Sites are compared with blocks within sites.

Treatments are tested against treatments x sites.

Treatments x sites tested against treatment x blocks within sites.

Responsibilities

Activity	Person responsible
Procurement of fertilizer, seeds and other inputs	
Distribution of fertilizers and seeds among farmers	
Selection of experimental plots and marking out of plots, randomization	
Weighing out fertilizer for application to plots and application to plots	
Supervision between implementation and harvest	
Data collection	
Validation of data collected in the field	
Statistical analysis	
Interpretation of results and making recommendations	

Appendix 4

Data analysis of a single-factor experiment

Data

TABLE A-1

Raw data - maize yields (t/ha)

Rep	Treatment (kg/ha)	Site A	Site B	Site C	Site D	Site E	Site F	Site G	Site H	Site I	Site J
1	Control	16.8	20.3	8.8	9.7	4.7	7.7	4.6	7.7	17.3	14.9
1	40	18.0	22.7	9.8	4.9	5.4	12.3	11.5	11.2	10.7	14.9
1	80	20.2	23.4	11.5	16.7	4.3	9.3	7.9	13.6	15.3	10.2
1	120	23.5	24.9	12.7	20.3	4.9	10.1	8.0	13.8	13.5	13.3
2	Control	20.6	19.5	6.5	4.1	4.9	9.6	5.4	7.9	12.0	14.0
2	40	22.0	20.0	7.4	8.7	5.2	12.5	12.5	12.6	12.4	9.1
2	80	22.1	20.2	11.5	10.5	5.4	9.1	7.2	14.2	12.1	15.3
2	120	23.2	23.7	12.7	10.6	5.1	10.4	5.2	14.7	15.7	14.1

Analysis of data from individual sites

TABLE A-2

Summary of ANOVA for individual sites

Site	Blocks	Treatments	Error
A	11.045	7.783 (0.148)	2.008
B	7.801	6.705 (0.045)	0.668
C	2.761	11.315 (0.034)	0.960
D	39.16	40.41 (0.243)	16.67
E	0.211	0.101 (0.625)	0.151
F	0.605	5.482 (0.032)	0.428
G	0.361	17.988 (0.037)	1.541
H	1.201	17.545 (0.001)	0.128
I	2.645	4.208 (0.648)	6.795
J	0.080	2.302 (0.873)	10.160

Values are mean squares; values in parentheses are probability values for the F-test of corresponding terms.

Figure A-1 depicts the box-plot for the data for all ten sites. The line in the middle of each box is the median. The upper and lower boundaries of the box represent the distance between the upper and lower quartiles. The whiskers attached to either end of the box depict the minimum and maximum observations. If outliers are present, they are situated outside the whiskers and are indicated by the number of the observation (an observation is considered an outlier if it lies further than 2.5 times the quartile or outer edge of the box).

The plots reveal that Site D had a wider range of responses and that Site E had a narrower range of responses than the other sites. At Sites F and I, the distributions seem to be skewed to the lower values, while the distributions are skewed towards the higher values at Sites C, H and J.

FIGURE A-1
Box-plot of data from ten sites

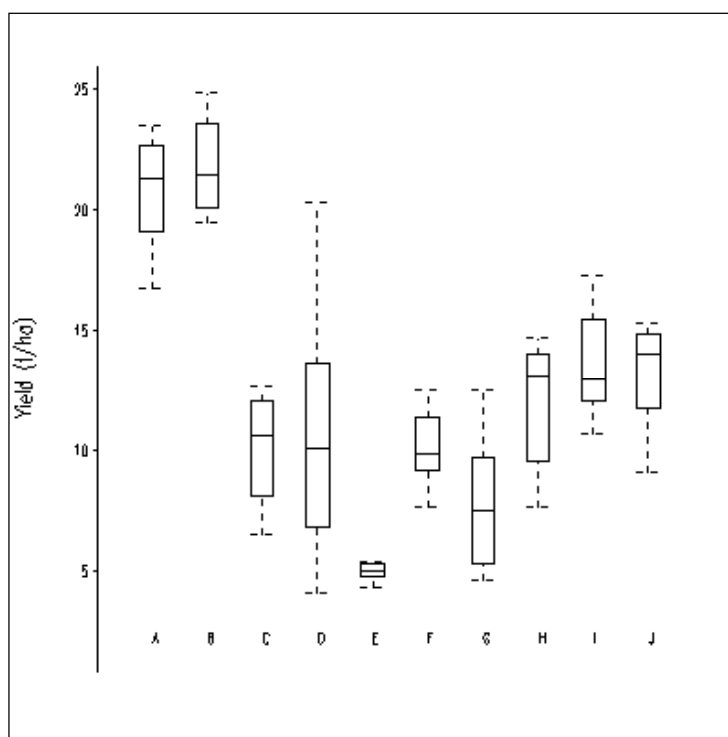


TABLE A-3

Treatment means and the standard errors of the difference between two means for individual sites

Site	Treatments (kg N/ha)				sed*
	0	40	80	120	
A	18.7	20.0	21.2	23.4	1.42
B	19.9	21.4	21.8	24.3	0.82
C	7.6	8.6	11.5	12.7	0.96
D	6.9	6.8	13.6	15.5	4.08
E	4.8	5.3	4.9	5.0	0.39
F	8.6	12.4	9.2	10.2	0.65
G	5.0	12.0	7.6	6.6	1.24
H	7.8	11.9	13.9	14.3	0.36
I	14.6	11.5	13.7	14.6	2.61
J	14.4	12.0	12.7	13.7	3.19

* sed - standard error of the difference between two treatment means

Analysis of combined trials

Table A-4 presents the results of the combined analysis of data collected from all ten sites.

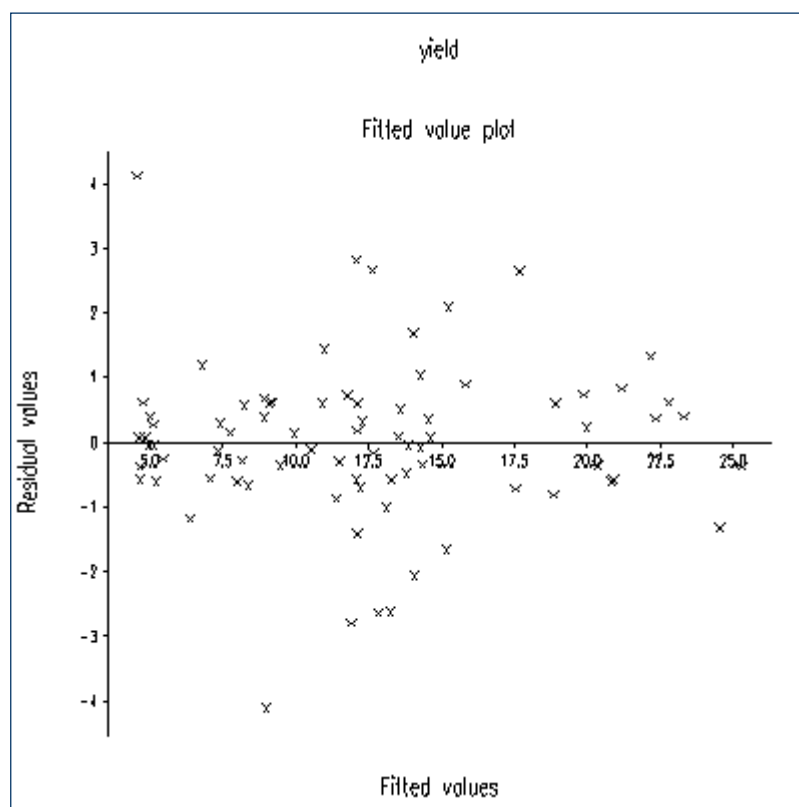
TABLE A-4

Results of the combined ANOVA of the ten trials

Source of variation	Df	SS	MS	F	P
Sites	9	2 011.42	223.49	33.91	<0.001
Blocks within sites	10	65.87	6.59		
Fertilizer	3	107.56	35.85	4.14	0.015
Fertilizer x sites	27	233.97	8.66	2.19	0.019
Fertilizer x blocks within sites	30	118.40	3.95		
Total	79	2 537.22			

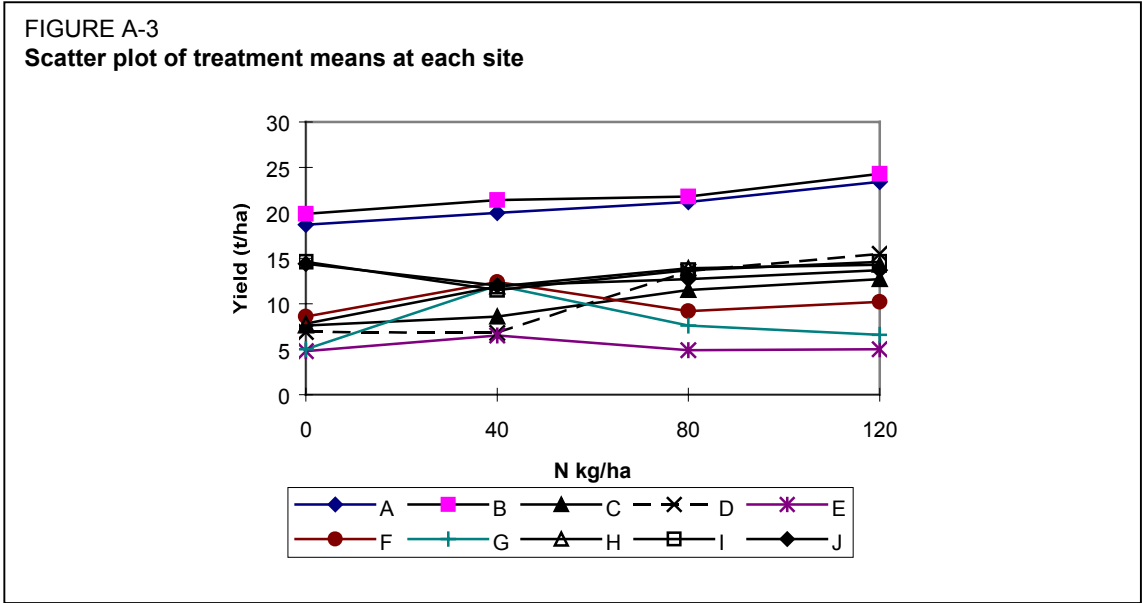
FIGURE A-2

Residual plot of ANOVA of combined analysis



The residual plot shows some large residuals, particularly two points with residuals of more than + or -4. Examination of the residuals revealed that these results occurred in Location 4, for Treatment 2. They should be further investigated to determine whether they are true responses or whether there was a mishap; this to decide whether to retain them in the analysis or not.

There is also some evidence of a fertilizer x sites interaction, indicating that the response to N fertilizer differed among sites. Figure A-3 attempts to show the nature of the interaction.



The figure shows that the sites interaction can be subdivided into two main sub-groups with Sites 1 and 2 having higher yields than the other sites.

As a further analysis, it may be useful to redo the analysis, omitting the results of Location E which were much lower than any of the other sites (see box-plot in Figure A-1).

Model fitting

This worked example uses the data for Site H because the ANOVA for this site (Table A-2) indicated strong evidence of a treatment effect. The scatter plot of the treatment means gives the response to N as shown in Figure A-4.

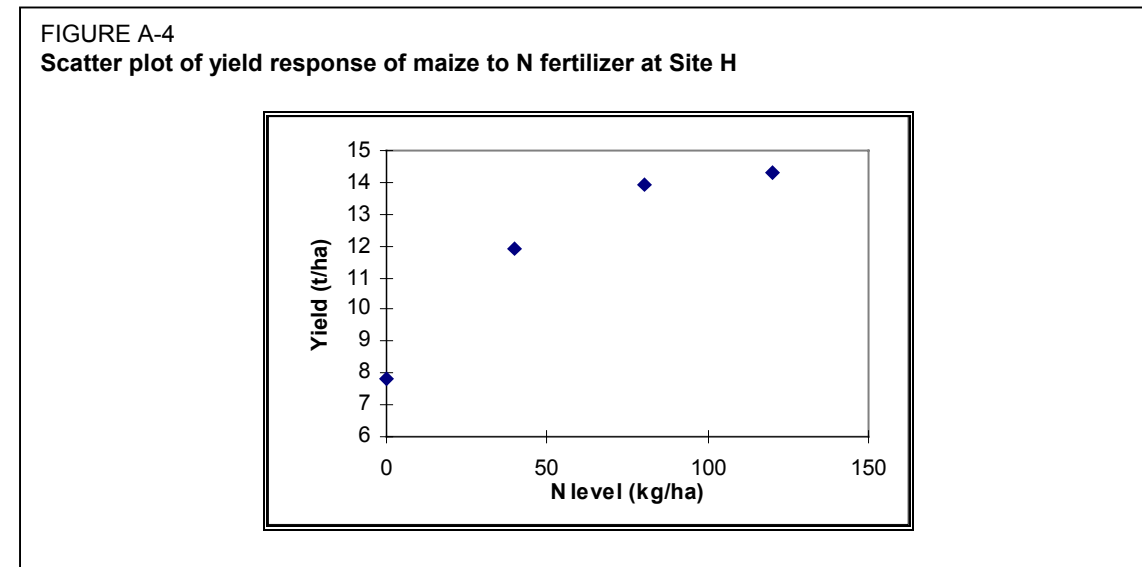


Table A-5 summarizes the results of the regression analyses using the quadratic and square root models. The results show that the quadratic model is superior to the square root. The scatter plot of the residuals (not shown) indicated no violations of the assumptions of the regression analysis.

TABLE A-5

Summary of results of fits of regression models

Criterion	Square root	Quadratic
b0 value	7.771 (0.476)	7.82 (0.098)
b1 value	-0.0177 (0.0194)	0.124 (0.0039)
b2 value	0.802 (0.211)	-0.00059 (0.00003)
F value	57.33	1 299.1
R2 value (%)	97.4	99.9
Error mean square	0.2275	0.0101

Values in parentheses are the standard errors of the respective regression coefficients

Estimation of optimum level

Using the results of the analysis of the previous section, the most appropriate production function for the response of maize to N fertilizer at Site H is:

$$Y = 7.82 + 0.124 N - 0.00059 N^2$$

This production function estimates the yield in tonnes per hectare. To estimate the yield in kilograms per hectare, each coefficient is multiplied by 1 000.

Assuming a cost of US\$160/kg for N fertilizer and a maize price of US\$10/kg, the price ratio on a per kilogram basis is: $160/10 = 16$

Using the equation for optimal rate of the quadratic model given in Table 15 in the main text, the optimal N rate is:

$$(16 - 124)/(2 \times (-0.59)) = 91.5, \text{ or about } 92 \text{ kg/ha of N fertilizer.}$$

Overall conclusions

Sites A and B had higher yields than the other locations, with Site E having substantially lower yields. The response to N fertilizer varied among sites. There was no evidence of a response to N fertilizer at Sites A, D, E, I and J. There was some evidence of response at Sites B, C, F and G. There was very strong evidence of a quadratic response at Site H. The optimum application rate of N fertilizer at that site was 92 kg/ha.

Appendix 5

Data analysis of a two-factor experiment

Data

TABLE A-6

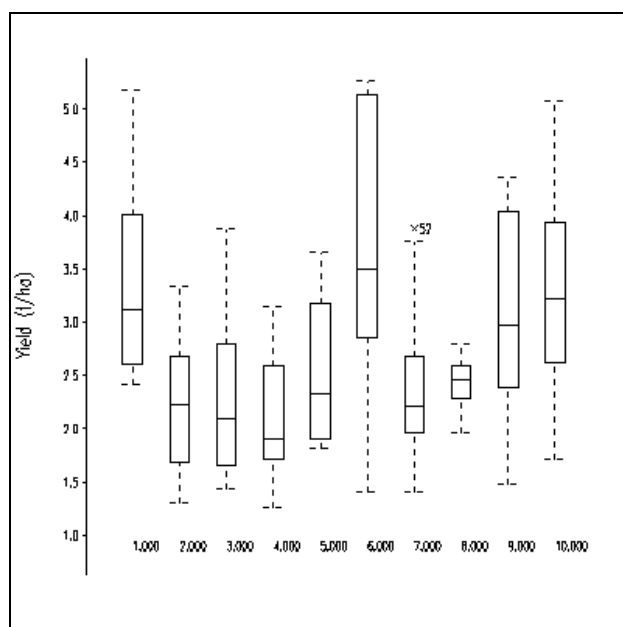
Raw data - maize yields (t/ha)

Rep	N level	P level	Site 1	Site 2	Site 3	Site 4	Site 5	Site 6	Site 7	Site 8	Site 9	Site 10
1	1	1	2.42	1.31	1.62	1.26	1.84	3.59	1.84	1.96	1.48	1.72
1	1	2	2.75	1.79	2.03	1.86	2.70	5.27	2.32	2.44	2.46	3.28
1	2	1	3.38	2.13	2.15	1.77	3.66	5.08	2.68	2.44	2.34	4.12
1	2	2	4.64	2.51	1.70	1.96	3.64	5.18	3.88	2.49	2.44	5.08
2	1	1	2.46	1.58	1.43	1.67	1.82	1.41	1.41	2.58	3.47	2.32
2	1	2	2.94	2.32	2.90	2.06	2.34	2.44	2.10	2.13	3.88	2.92
2	2	1	3.30	2.85	2.68	3.14	2.32	3.40	2.68	2.80	4.36	3.16
2	2	2	5.18	3.33	3.88	3.11	1.96	3.26	2.08	2.61	4.19	3.76

Analysis of data from individual sites

FIGURE A-5

Box-plot of data from the ten sites



The box-plot revealed that the distributions for sites were generally skewed towards the lower values, especially at Sites 4, 5, 6 and 9. The spread of values for Sites 1, 6, 9 and 10 was very wide,

while that of Site 8 was very small. There appears to be a possible outlier at Site 7 (X 57), which needs further investigation.

TABLE A-7

Summary of ANOVA for individual sites

Site	Blocks	N	P	N x P	Error
1	0.060	4.396 (0.002)	1.950 (0.005)	0.679 (0.023)	0.036
2	0.684	1.824 (0.004)	0.541 (0.023)	0.016 (0.511)	0.029
3	1.437	0.738 (0.308)	0.865 (0.277)	0.160 (0.609)	0.492
4	1.225	1.225 (0.070)	0.165 (0.384)	0.086 (0.516)	0.160
5	1.445	1.037 (0.165)	0.125 (0.570)	0.387 (0.345)	0.310
6	9.267	2.216 (0.024)	0.891 (0.074)	0.945 (0.069)	0.123
7	0.750	1.665 (0.110)	0.392 (0.355)	0.041 (0.748)	0.329
8	0.078	0.189 (0.217)	0.002 (0.898)	0.004 (0.843)	0.078
9	6.444	0.520 (0.035)	0.218 (0.098)	0.266 (0.078)	0.039
10	0.520	4.322 (0.039)	1.730 (0.113)	0.045 (0.744)	0.352

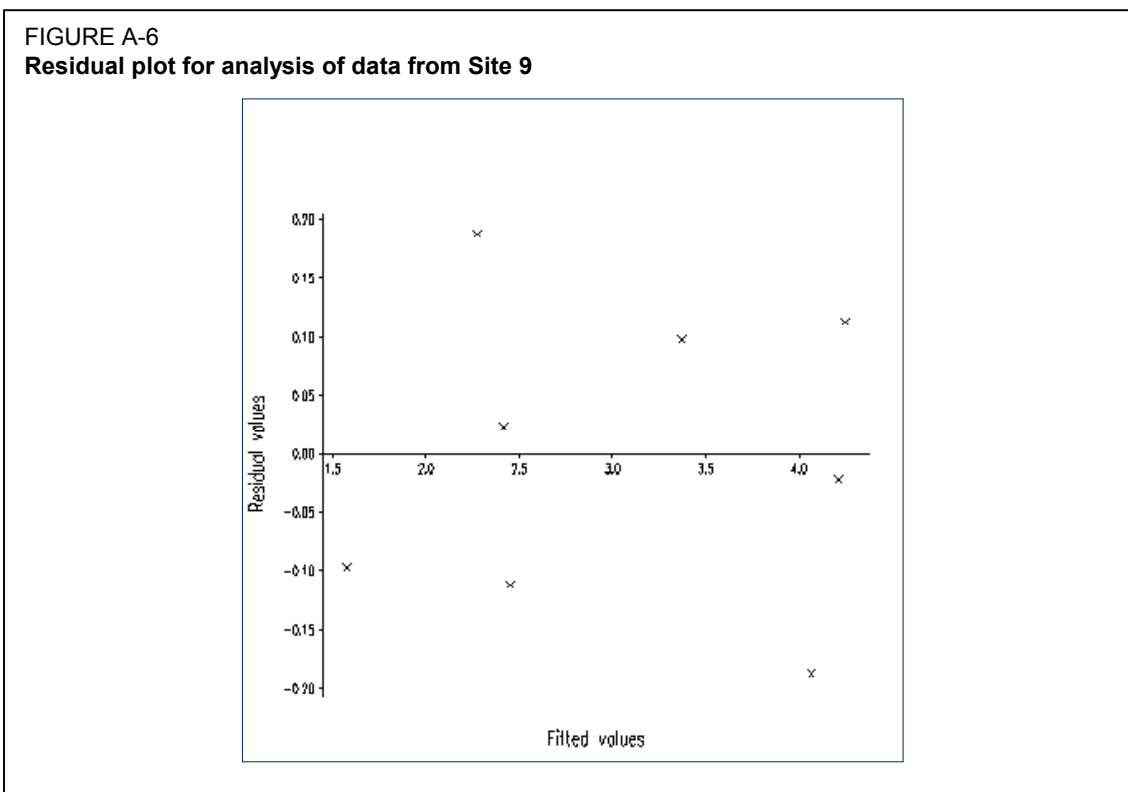
Values are mean squares; values in parentheses are probability values for the F-tests of corresponding terms

TABLE A-8

Table of means and standard errors of difference between two means

N level (kg/ha)	P level (kg/ha)	Site 1	Site 2	Site 3	Site 4	Site 5	Site 6	Site 7	Site 8	Site 9	Site 10
0	0	2.44	1.45	1.53	1.47	1.83	2.50	1.63	2.27	2.48	2.02
	40	2.85	2.06	2.46	1.96	2.52	3.86	2.21	2.29	3.17	3.10
60	0	3.34	2.49	2.42	2.45	2.99	4.24	2.68	2.62	3.35	3.64
	40	4.91	2.92	2.79	2.54	2.80	4.22	2.98	2.55	3.32	4.42
sed N x P		0.190	0.171	0.702	0.400	0.556	0.350	0.573	0.279	0.196	0.593
Mean N	0	2.64	1.75	2.00	1.71	2.17	3.18	1.92	2.28	2.82	2.56
	60	4.13	2.71	2.60	2.50	2.89	4.23	2.83	2.59	3.33	4.03
sed N		0.134	0.121	0.496	0.283	0.393	0.248	0.405	0.198	0.139	0.420
Mean P	0	2.89	1.97	1.97	1.96	2.41	3.37	2.15	2.45	2.91	2.83
	40	3.88	2.49	2.63	2.25	2.66	4.04	2.59	2.42	3.24	3.76
sed P		0.134	0.121	0.496	0.283	0.393	0.248	0.405	0.198	0.139	0.420

Figure A-6 shows the residuals for the analysis of the data from Site 9. The plot shows no serious violations of the assumptions of ANOVA. Therefore, it can be concluded that there is some evidence of N effect, but no evidence at the 5 percent level of interaction or a P effect.



Analysis of combined trials

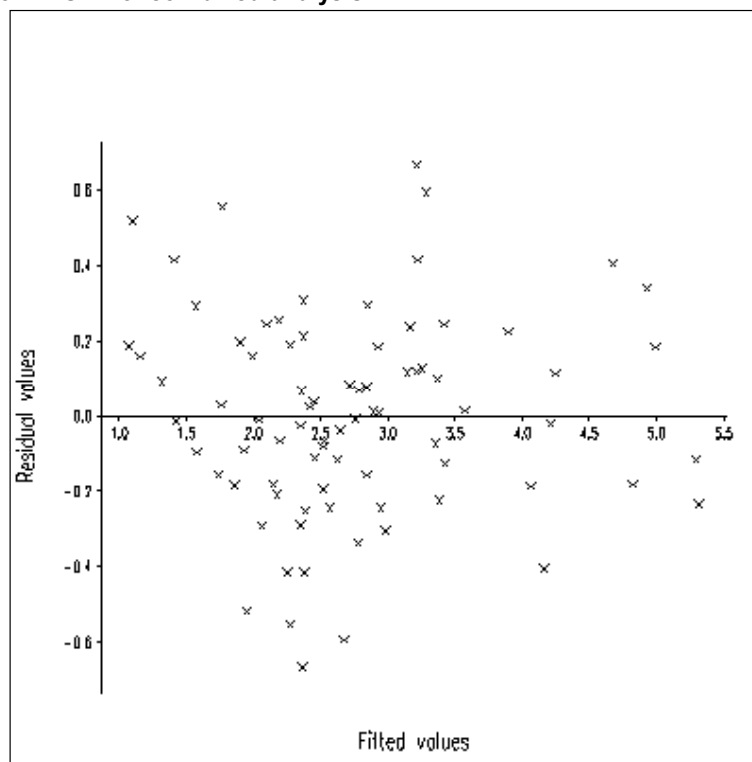
Table A-9 presents the results of the analysis of the combined data.

TABLE A-9
 Results of the combined ANOVA of the ten trials

Source of variation	Df	SS	MS	F	P
Sites	9	23.190	2.577	1.18	0.399
Blocks within sites	10	21.909	2.191		
Treatments	3	21.244	7.081	29.9	<0.001
N	1	15.488	15.488	65.4	<0.001
P	1	5.090	5.090	21.5	<0.001
N x P	1	0.666	0.666	2.81	0.105
Treatments x sites	27	6.394	0.237	1.21	0.305
Treatments x blocks within sites	30	5.842	0.195		
Total	79	78.579			

Figure A-7 presents the residual plot for this analysis. There appears to be no serious violation of the assumption of homogeneity of variances, nor is there evidence of a treatments x sites interaction.

FIGURE A-7
Residual plot of ANOVA of combined analysis



Overall conclusion

From the analysis, it can be concluded that there is very strong evidence of an N effect and a P effect but no evidence of any interactions. The addition of 60 kg/ha of N fertilizer resulted in an average yield increase over all sites of 0.88 t/ha, while the corresponding increase was 0.50 t/ha with the addition of 40 kg/ha of P₂O₅.

Transformations

As indicated in Chapter 4, it is important to check whether the assumptions of the analysis have been violated. For example, an analysis of the residual plots of the data from the individual sites revealed that the assumptions of the analysis were not upheld at Site 1.

The spread of the residuals resembles a right opening megaphone, thereby indicating a violation of the assumption of homogeneity of variances. Therefore, the data were transformed using the natural logarithmic function, and ANOVA was performed on these transformed data. Table A-10 summarizes the results of the analysis.

The residual plot for this analysis (not shown) indicated no serious violations of the assumptions of ANOVA. Therefore, it can be concluded that there is strong evidence of an N x P interaction. In the presence of this interaction, the main effects will not be discussed. Table A-11 shows that the effect of N fertilizer was greater when P₂O₅ was applied. The yield increase in the absence of P₂O₅ was 0.91 t/ha (from 2.44 to 3.35 t/ha), compared with 2.04 t/ha when 40 kg/ha of P₂O₅ was applied (from 2.86 to 4.90 t/ha).

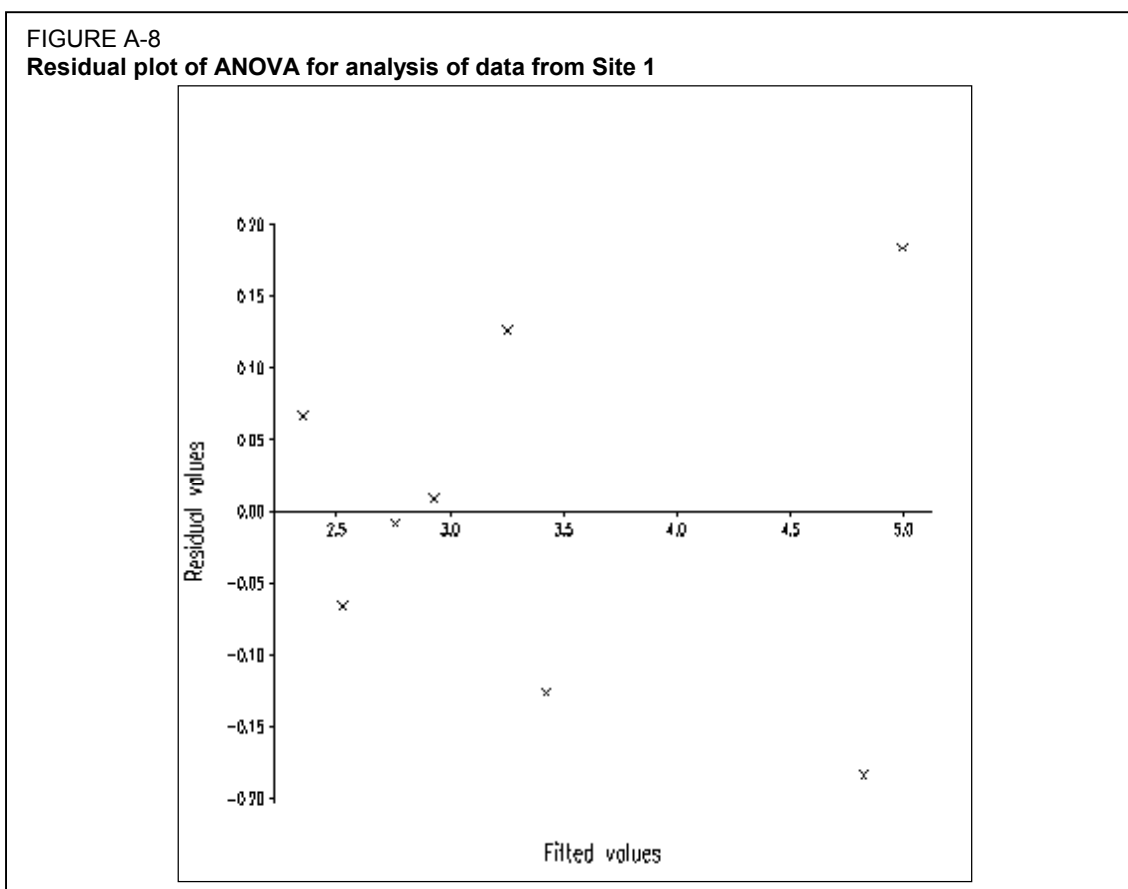


TABLE A-10

Results of ANOVA of transformed data for Site 1

Source	Df	SS	MS	F	P
Blocks	1	0.0036	0.0036		
Treatments	3	0.5394			
N	1	0.3687	0.3687	215.7	<0.001
P	1	0.1441	0.1441	84.3	0.003
N x P	1	0.0266	0.0266	15.58	0.029
Treatments x blocks	3	0.0051	0.0017		
TOTAL	7	0.5482			

TABLE A-11

Effect of N and P₂O₅ fertilizer on maize yields at Site 1

N level (kg/ha)	P ₂ O ₅ level (kg/ha)		Mean
	0	40	
0	0.89 (2.44) ²	1.05 (2.86)	0.97 (2.64)
60	1.21 (3.35)	1.59 (4.90)	1.40 (4.06)
Mean	1.05 (2.86)	1.32 (3.74)	1.18 (3.25)

sed¹ (N effect) = 0.029, sed (P effect) = 0.029, sed. (N x P) = 0.041

1. sed = standard error of the difference between two treatment means (of the transformed data)
2. Values in parentheses are back transformed means calculated by computing the exponential of the mean transformed value, i.e. 2.44 = exp(0.89)

Appendix 6

Soil amendment trial

Data

Farms	Blocks	Treatments	Yield
1	1	1	5.4
1	1	2	7.3
1	1	3	3.8
1	2	1	7.1
1	2	2	9.1
1	2	3	4.6
2	3	1	6.4
2	3	2	6.1
2	3	3	5.1
2	4	1	2.8
2	4	2	4.0
2	4	3	4.5
2	5	1	4.4
2	5	2	3.5
2	5	3	4.8
3	6	1	1.2
3	6	2	1.0
3	6	3	1.1
3	7	1	0.9
3	7	2	0.8
3	7	3	0.8
4	8	1	10.1
4	8	2	10.5
4	8	3	4.9
4	9	1	9.3
4	9	2	11.0
4	9	3	4.5
5	10	1	8.3
5	10	2	8.7
5	10	3	9.1
5	11	1	4.4
5	11	2	4.8
5	11	3	5.1

To prepare the data entry spreadsheet in either a spreadsheet program or statistical package, each column of the spreadsheet should represent a design factor or a response. Each row represents a plot from the experiment. In this example, rows are needed for farms, blocks, treatment, yield and any other responses recorded for analysis (e.g. days to fruiting). Farms are usually represented by codes such as 1, 2, 3, 4 and 5 for the five farms. Most packages require the block code to number the blocks differently from farm to farm, i.e. Farm 1 would have Blocks 1 and 2; Farm 2, Blocks 3 and 4, etc. The treatments are coded 1 to 3.

Appendix 7

Tillage trial

Data

Farm	Blocks (Farm 1)/ Rows (Farm 2)	1(Farm 1)/ Columns (Farm 2)	Treatment	Yield
1	1	1	1	25.8
1	1	1	2	31.3
1	1	1	3	20.8
1	1	1	4	30.1
1	1	1	5	17.9
1	2	1	1	29.6
1	2	1	2	27.2
1	2	1	3	28.4
1	2	1	4	25.2
1	2	1	5	27.4
1	3	1	1	30.1
1	3	1	2	28.4
1	3	1	3	19.7
1	3	1	4	22.8
1	3	1	5	26.3
2	4	2	1	42.3
2	4	3	2	41.4
2	4	4	3	37.2
2	4	5	4	30.8
2	4	6	5	33.4
2	5	2	2	30.5
2	5	3	1	27.8
2	5	4	4	20.7
2	5	5	5	26.2
2	5	6	3	21.6
2	6	2	3	35.4
2	6	3	5	30.1
2	6	4	2	33.8
2	6	5	1	26.2
2	6	6	4	18.2

Column 1 is coded 1 or 2 for farms. Column 2 is labelled 'Rows' for blocks (Farm 1) and rows (Farm 2). This was coded as 1-3 for the three blocks on Farm 1 and 4-6 for the rows on Farm 2. Column 3 is for the columns in the design. This was coded as 1 for all of Farm 1; and 2-6 for the five columns of Farm 2. Column 4 is for the treatment codes (1-5).

Appendix 8

Example of multiple regression analysis

Data

The data for this analysis were extracted from a fertilizer trial to determine the effect of N and P fertilizer on rice yields.

N rate (kg/ha)	P rate (kg/ha)	Yield (t/ha)
75	20	5.3
75	60	5.4
225	20	6.0
225	60	6.5
150	40	6.2
0	40	4.5
300	40	6.9
150	0	5.9
150	80	5.5

Source: Hildebrand and Poey, 1985

Step 1: Scatter plots

Scatter plots were first drawn of yield vs N rate and yield vs P rate (Figures A-9 and A-10).

The scatter plots show a linear relationship between yield and N rate, but not much evidence of a relationship between yield and P rate.

Step 2: Inclusion of interaction terms

The NP interaction term is included in the model by creating a new variable (NP) and inserting the product of N rate and P rate in that term. Thus, the NP interaction for N = 75 and P = 20 is $75 \times 20 = 1\,500$, etc.

Step 3: Regression analysis

Regression analyses were conducted for all possible combinations of independent factors versus yield. Table A-12 summarizes the results.

The results in Table A-12 indicate that the 'best' fitting model was the regression with yield vs N rate. Regressions with yield vs N rate and NP rate and yield vs N rate and P rate were also good. However, further examination of the regression coefficients in Table A-13 shows that the regression coefficients for P rate and NP rate in the respective models were not significant.

FIGURE A-9
Scatter plot of yield vs N rate

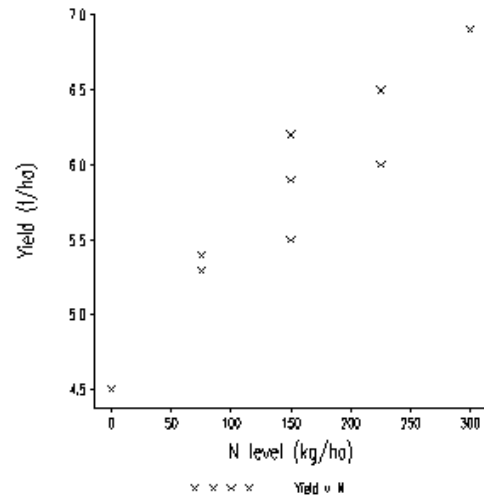


FIGURE A-10
Scatter plot of yield vs P rate

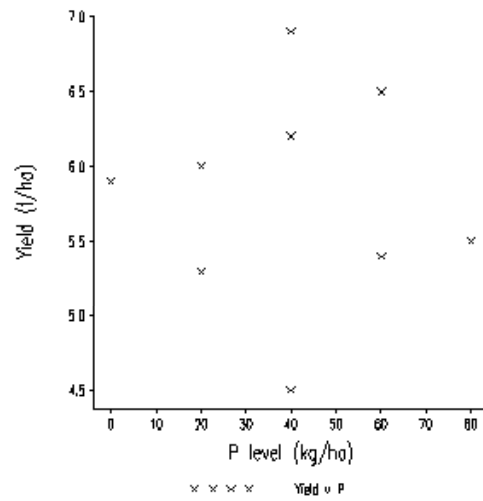


TABLE A-12

Summary of results of regression analysis

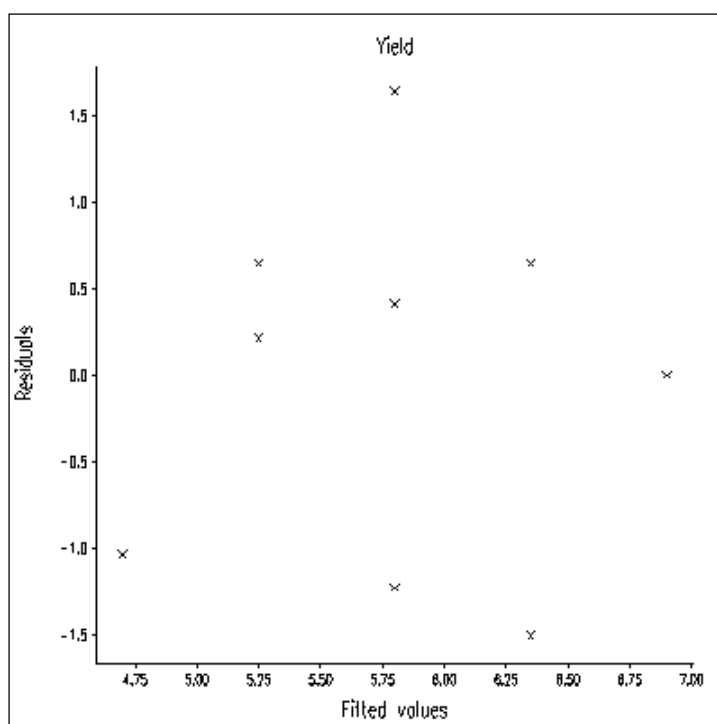
Independent variable(s)	F (prob. value)	R ²	Error mean square
N	54.6 (<0.001)	86.9	0.067
P	0.01 (0.942)	0.07	0.585
NP	5.17 (0.057)	34.4	0.337
N, P	23.36 (0.001)	84.8	0.078
N, NP	23.17 (0.002)	84.7	0.078
P, NP	19.79 (0.002)	82.4	0.090
N, P, NP	14.35 (0.007)	83.3	0.085

TABLE A-13

Summary of regression coefficients

Model	Intercept (a)	Regression coefficient (b)	Regression coefficient (c)
Y = a + bN	4.70 (0.173)*	0.007 (0.0010)	
Y = a + bN + cP	4.73 (0.246)	0.0073 (0.0011)	-0.0008 (0.0040)
Y = a + bN + cNP	4.7 (0.187)	0.0073 (0.0015)	0 (0)

Figure A-11 presents the residual plot for the best fitting model yield vs N rate. The plot shows no serious violations of the assumptions of regression.

FIGURE A-11**Residual plot of regression of yield vs N**

Conclusion

There is strong evidence of a linear relationship between yield and the N rate.

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19. Fertigation/Chemigation. 1991.
20. Secondary Nutrients. 1992.
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22. Planning Sustainable Management of Land Resources: The Sri Lankan Example. 1999.
23. Integrated Soil Management for Sustainable Agriculture and Food Security in Southern and East Africa. 1999.
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