TECHNIQUES FOR DESIGN AND ANALYSIS OF ON-FARM EXPERIMENTATION

FSR/E TRAINING UNITS: VOLUME II

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PREFACE

One of the major objectives of the Farming Systems Support Project is to provide training, and support for training activities, in FSR/E methodology. This collection of training units has been produced in response to an absence of available training materials which could be used in training practitioners in the skills necessary for implementing the FSR/E approach to agricultural development.

This collection of training units is not a course. Rather, it is a set of resources which supports FSR/E courses. It is an attempt to provide the trainer and practitioner trainee with a wide variety resources for teaching and learning specific content and skills needed for implementing FSR/E successfully.

Volume One, Diagnosis in FSR/E, contains nine units for introducing trainees to various diagnostic steps in the FSR/E approach. It stresses, but is not limited to, initial diagnosis. Volume One also contains units which detail on-going, or continuous, diagnosis throughout the FSR/E process. Links between social and biological science disciplines are stressed, as are considerations of intra-household and socio-cultural issues.

Volume Two, Techniques for Design and Analysis of On-Farm Experimentation, contains six units which detail the farm trial design and analysis process. A statistical analysis unit is included so that trainers do not need to depend solely on outside materials in this critical area of trial design and analysis. Volume Two considers the links between biological and social science disciplines in on-farm research and, like Volume One, addresses intra-household and socio-cultural issues.

The units have not been developed to be exhaustive texts of the the topics presented. Rather, they have been developed to convey basic information in a format as complete and concise as possible. It is our hope that both trainers and trainees will search out more information on specific topics covered in the training units. Each unit has learning objectives and key points which focus on the main essence of the unit. Many units are divided into sub-units, sections and sub-sections, each with its own set of learning objectives, key points and discussion. Suggested learning activities accompany the units, and each activity has separate instructions for trainers.

Each volume of the collection includes an introduction discussing some of the options on how to use the units. In the introductions, a logical sequence of presentation is discussed and any prerequisites to units, subunits, or sections are described. The introductions are viewed as guides for helping the trainer gain an appreciation for the material available in the volume and are in no way to be viewed as absolutely the only way for the units to be used.

The units are not the final word. Rather, they have been developed as the foundation of developing training units in FSR/E. Your comments,

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adaptations, additions, and suggested activities are welcomed and encouraged. The best measure of the usefulness of a product is given from those who use the product. The best way to improve a product is to listen to the users. At the end of this introduction you will find a one page evaluation sheet. We hope that you will use this form to send us your comments. This is not meant to limit your comments, and we encourage detailed comments, but rather to encourage you, the user, to let us know what you think and suggest. We anticipate an updated version to be produced early in 1987 based on user feedback and reviews.

FSSP recognizes the need for other training materials that provide useful information and has included some of these as supplemental items. The supporting documents included with the collection of training units are:

CARDI, April, 1984, "On-farm Experimentation: A Manual of Suggested Experimental Procedures.

CIMMYT, revised November, 1985, "Introduction to Economic Analysis of On-Farm Experiments", Draft Workbook, CIMMYT Economics Program, .

FSSP, 1985, "Bibliography of Readings in Farming Systems, volume 1,"

Poey, F. et. al, 1985, "Anatomy of On-Farm Trials: A Case Study From Paraguay", FSSP.

Hildebrand, P. and F. Poey, 1985, On-Farm Agronomic Trials in Farming Systems Research and Extension", Lynne Rienner Publishers, Inc., Boulder Colorado.

The FSSP would like to thank CIMMYT Economics Program and CARDI for their permission to include their work in economic analysis and on-farm experimental design respectively, as supplemental materials to the training units.

Current development of additional Case Studies are underway. The FSSP has developed and initially tested a case study based on Dominican Republic data from the Las Cuevas region which gives trainees the opportunity to interview farmers and develop research priorities. Some minor revisions are being made, and the final product will accompany the training units in the future.

The FSSP/Population Council case study project "Intra-Household Dynamics and Farming Systemns Research and Extension" is producing eight case studies designed primarily for the training of agricultural researchers. These case studies were "envisioned as a mechanism to examine the assumptions of FSR/E with regard to intra-familial input to farming systems, including land, labor and capital and production constraints within the household" (Feldstein and Poats, 1985)

The eight cases represent on-going field projects which have reached a level of development through, at least, the first three stages of FSR/E

(diagnosis, design, and on-farm experimentation and evaluation). The cases also represent a number of "issues" facing the whole state of the art of FSR/E, such as the relationship of agroforestrey to FSR/E, dryland on-farm cropping methods, the tendency towards "over-surveying" and the research extension linkage (Feldstein and Poats, 1985). The cases are listed below:

1. SAFGRAD project in Burkina Faso, written by Joseph Nagy, Sibiri Sawadogo and Herbert Ohm.

2. CARDI project in St. Lucia, written by Vasantha Chase.

3. A CIAT project in Colombia, written by Jacqueline Ashby.

4. TROPSOILS project in Sitiung, Indonesia, written by Vickie Sigman and Carol Colfer.

5. Lake Balinsasayao project in the Philippines, written by Eva Wollenberg.

6. ARPT project in Zambia, written by Charles Chabala and Robert Nguiru.

7. CARE/ICRAF Project in Siaya District, Kenya, written by Diane Rocheleau.

8. ATIP project in Botswana, written by Doyle Baker.

These case studies will accompany the training units upon their completion.

Throughout the development process of the FSR/E training units, from the planning, writing, initial editing, reviewing, testing, revising, to the final production, many individuals have been involved. FSSP would like to acknowledge their efforts. The individuals are listed below with their current affiliations.

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The draft edition of the Volume Two, Techniques for Design and Analysis of On-Farm Experimentation, was used for the first time in the FSSP/Gambia Agricultural Diversification workshop on On-Farm Experimentation in May, 1985. Parts of Volume One, Diagnosis in FSR/E, were used for the first time in the Jamaica Farming Systems Research Workshop, June, 1985. Feedback received during this initial testing was used, along with other feedback, in the revising effort.

Richard Bernsten, Michigan State University, presented the FSSP training units for review at the "Farming Systems Research Socio-Economics Monitoring Tour/Workshop," held September 16 - 28, 1985, at IRRI, Los Banos, Philippines, at the request of Marlin Van Der Veen, IRRI. Comments from that session, as well as detailed comments by Richard Bernsten, were very useful in revising both volumes. Susan Almy, Rockefeller Foundation, also provided very detailed comments. Additional review comments were made by Peter Hildebrand, University of Florida. Martha Gaudreau, University of Minnesota, played an important role in the revision of the Diagnostic Unit. Klaus Hinklemann, Virgina Polytechnic Institute, provided valuable consultation on some statistical aspects of the units.

The FSSP acknowledges the above contributions and those of others who may have been inadvertently omitted. I would like to gratefully acknowledge the patience and hard work of the FSSP secretaries, especially Lana Bayles, who typed the majority of the training units.

Lisette Walecka
Coordinating Editor
March, 1986

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EVALUATION (FEEDBACK) TRAINEE

Your comments are encouraged. Please feel free to write your comments and send them to the FSSP at the address listed on the back of this form. Being specific about the unit, sub-unit or sections which you are discussing will assist us in our efforts to provide quality materials.

(optional) NAME: DATE:

LOCATION:

1. How did you find the units most/least useful?

most:

least:

2. How was the content most.....

useful? _____

relevant? _____

3. Was the level of presentation appropriate?

4. Was the volume organized appropriately?

5. In the future editions what would you want to see.....

added?

expanded?

shortened?

omitted?

6. How useful were the existing activities provided in the unit?

PLEASE MAKE ANY ADDITIONAL COMMENTS, OR SUGGESTIONS. THANK YOU!

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PREFACE (TRAINERS' NOTES)

The set of materials included in the collection of FSR/E training units has been assembled by the FSSP to provide support for training in FSR/E methodology. As Farming Systems practitioners are aware, the first step in the FSR/E process is diagnosis. The same is true of developing training programs or courses. The first step is to find out what is needed by the potential trainees (audience). Performing a needs assessment should preceed all training, and a training course should be planned based on the needs assessment. Needs will vary by participants, location, familiarity with FSR/E, and a multitude of other factors. The FSSP realizes that due to the variability of needs form one short course to the next, the design of a "standard" course would not be the best approach to providing support. Instead, the development of training resources on a range of skills necessary for the implementation of the FSR/E approach was envisioned.

The training units were developed to provide flexibility in course design. By picking and choosing the relevant units or parts of units, based on the needs of the clientel, the trainer has the opportunity to provide a wide variety of short courses. The collection of training units provides an array of information on FSR/E, from diagnosis to the analysis of on-farm trials, from which a trainer can draw to develop short courses and workshops on a variety of aspects of FSR/E. It is not presented as a course outline. Each training course or workshop will differ depending on the objectives, content, trainees, trainers and other elements of a training activity.

Besides flexibility, another basic premise for the development of the training units was the need for participatory activities. Activities encouraging participation and hands-on experiences accompany most of the training units. There is a need for many more of these types of activities and we encourage you to submit any activities which you have used and would like to see included in future editions of the training units to the FSSP. Address your comments to:

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As a basis for understanding the process of the development of these training units, and their intended use, the following criteria were established.

1. Objectives - the objectives of the training units and specific sub-units are skill oriented emphasizing developing the abilities of field level practitioners to carry out Farming Systems Research and Extension.

2. Content - the content of the training units is technical in nature, addressing specific "tools" to be used by practitioners in FSR/E.

3. Process - the training processes to be included are to be varied

and essentially experiential in nature, allowing for maximum participation by the participants.

4. Audience - the training units are to be directed toward field level practitioners who will have had, ideally, at least a certificate level education and who will have had no more than two years field experience. While the training units will be directed toward this specified audience, trainers will be encouraged to adapt the materials therein to other levels as they feel necessary. Any units, sub-units or sections requiring pre-requisites for their use are clearly indicated at the beginning of each unit.

5. Trainers - the training units have been developed with the idea that they will be used by at least two trainers (one FSR/E content expert, one training specialist). These trainers will be expected to have language capability in the language of delivery, be at least diploma level, and have FSR/E experience (at least previous participation in the course being delivered).

Essentially, the training units are designed as participant manuals. These manuals are supplemented by trainers' notes where ever possible. These notes are printed on blue paper to facilitate easy location. The unbound, looseleaf format of the two volumes reflects their flexibilty. They appear in this form to allow for easy mix and match by the trainer as well as to facilitate photocopying efforts.When photocopying the manual for participant use, the blue pages should be excluded. We encourage you to photocopy the manual as needed. The only item which we cannot authorize photocopying permission is Peter Hildebrand and Federico Poey, On-Farm Agronmic Trials in Farming Systems Research and Extension, 1985 which is published by Lynne Rienner Publishers, Inc., Boulder, Colorado. This publication can be ordered from Lynne Rienner Publishers, Inc., 948 North Street, Boulder, Colorado 80302. It is distributed ouside of North and South America and Japan by Frances Pinter (Publishers) Ltd, 25 Floral Street, London WC2E 9DS, England.

As mentioned earlier, your feedback is essential for the future improvement of the training units. A general evaluation form is included here for your convenience. Please feel free to elaborate. Thank you for your support in this effort.

EVALUATION (FEEDBACK) TRAINER

Your comments are encouraged. Please feel free to write your comments and send them to the FSSP at the address listed on the back of this form. Being specific about the unit, sub-unit or sections which you are discussing will assist us in our efforts to provide quality materials.

(optional) NAME: DATE:

LOCATION:

1. How did you find the units most/least useful?

most:

least:

2. In planning to use this unit (subunit, section) again what would you:

expand:

add:

shorten:

omit:

3. How useful were the existing activities provided in the unit? Please list the activities and indicate whether or not you chose to use them and why or why not.

UNIT	ACTIVITY	USED (YES/NO)	REASON

4. What supplementary materials did you find useful that were not listed in the unit(subunit, section)?

PLEASE MAKE ANY ADDITIONAL COMMENTS, OR SUGGESTIONS. THANK YOU!

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INTRODUCTION

This volume presents techniques useful in the design and analysis of on-farm experimentation. On-farm experimentation moves through a series of steps. At each step there are various choices to make. No one choice is right all the time. Each choice has advantages and disadvantages. Sometimes one choice is better than another under one set of farmer conditions and a given problem to solve. The next time, under different farmer conditions, or for a different problem to solve, another choice may be better. The objective of this volume is to help farming systems teams make better choices.

The steps in on-farm experimentation help farming systems teams and farm households answer several questions. Each unit in this volume addresses one of these questions.

Unit I. What Kind of Testing To Do?

Initial diagnosis usually reveals more farmer problems than a team can address at one time. Prioritization of farmer problems is necessary. Prioritization is the link between initial diagnosis and design of on-farm experimentation.

Types of trials for priority problems can be classified by three criteria:

a. The basis of the production system (yam-based, rice-based, etc.)
b. The cropping or grazing pattern (monoculture, crop association, pasture rotation, etc.)
c. The subject of the problem (nutrition, disease, spacing, etc.)

On-farm experimentation can take two pathways. One pathway is based on spontaneous farmer experimentation. Another pathway is based on researcher-planned experimentation. Researcher planned experimentation moves through a sequence of trials. Farmer management increases as the trial sequence progresses.

Unit II. What Treatments to Test, and Where?

Many different treatments may be possible for a priority problem. Some will be more useful to farmers than others. The amount of land available on each farm can limit the number of treatments. More replications on each farm can also limit the number of treatments. Conversely, more treatments may limit the number of replications possible on each farm. Input from farmers is important in making choices among experimental treatments, control treatments, treatment specifications, and replications.

Unit III. How to Design the Trial to Obtain Analyzable Data?

Farmers' fields are variable even without experimental treatments.

Statistics is a set of techniques for comparing treatment differences against natural variation. Experimental designs allow a team to analyze trial data using statistical techniques. Different designs result in different layouts of treatments within each farm, and from one farm to another. The different types of designs each have advantages and disadvantages.

Unit IV. How to Carry Out the Trial?

Dialog with farmers is needed to determine where to place treatments in fields. Timing of planting is critical. Some data gathered from on-farm trials are different from on-station trials. Which data to collect, how to collect it, how to sample, and recording and handling all involve different choices.

Unit V. How to Analyze and Interpret the Trial Results?

There are various techniques for analysis. Which one is appropriate depends on the design used. Interpretation of results includes biological, economic, and social interpretation. Analysis and interpretation help the team and farm households make better decisions for future activities.

Unit VI. How to Manage and Administer FSR/E at the Field Level?

There are many commonly encountered management and administrative problems which interfere with the smooth implementation of the FSR/E approach. Recognizing the major areas of problems encountered by field-level practitioners will help in the attempt to operationalize a FSR/E approach.

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This volume consists of a collection of notes on techniques and choices. The volume has two objectives:

1. Skill-building: to enable farming systems team members to use different techniques.

2. Decision-making: to enable farming systems team members to make better choices among different techniques.

Both objectives are important. Team members cannot choose from techniques which they do not know how to use. Skill-building thus increases choices. Making good choices is equally essential. Using the wrong technique slows down the progress of on-farm experimentation.

The sections are arranged in the order of the steps of on-farm experimentation. Trainers do not need to present all the material at once. However, material later in the sequence often depends on material earlier in the sequence. Sections may have "prerequisites." These indicate the material that the section depends on. For example, analysis of variance (ANOVA) for factorial arrangements of treatments depends on understanding what are designs with factorials. Of course, if participants already understand the design principles (perhaps from a previous workshop), it is not necessary to repeat the prerequisite material.

Some techniques are less common than others. The less common techniques are marked "optional." They can provide team members with more choices. Teams can still do good on-farm experimentation without them. Trainers can choose how much to include depending on participants' needs.

Many sections have participant exercises. Trainers can use the exercises "as is" when appropriate. Or, trainers can use them as a guideline to make their own exercise with local examples.

The authors of this volume would like to improve it. They especially welcome new exercises and case studies. They would appreciate suggestions to improve the text. They would also like to hear about how participants make choices in field, and which techniques from this volume participants find most useful. The authors hope to incorporate this feedback into a revised edition in 1987.

UNIT I
WHAT KIND OF TESTING TO DO

(I)

WHAT KIND OF TESTING TO DO

OUTLINE

1. Ways to Decide Which Problems to Test First for Answers
2. Ways to Select Cooperating Households and Farmers
3. Ways to Classify Trials
4. Pathways Possible for On-Farm Experimentation
5. How Researcher-Planned Trials Change Over Time
6. Ways to Allocate Resources and Responsibilities

PREREQUISITES:

Should be familiar with information in Volume I. Diagnosis in FSR/E.

PARTICIPANT LEVEL:

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES:

After completing this unit the participants will be able to:

1. Recognize that different types of trials are needed to handle research priorities for the production system, farmer management practices and farm setting in a given domain.

2. Describe the sequential trend of researcher planned trials over time.

KEY POINTS:

1. Prioritization of farmers' problems for research is the link between intial diagnosis and design of on-farm experimentation.

2. The purpose of on-farm experimentation is to test alternative production practices to solve problems identified by farm households.

3. Two basic pathways in on-farm experimentation are: a) observing and making inferences from farmer initiated experimentation, and b) observing and making inferences from researcher planned experimentation.

4. Researcher planned trials change over time following a general sequential trend.

DEFINITIONS:
agricultural production system
analysis of variance (ANOVA)
arcsine transformation
border rows
causal agent
complete factorial
control treatment
diagnosis
error mean square (MSE)
evaluation
factorial
farmer initiated experimentation
household
interaction effects
intervention
logarithmic transformation
main plot error
natural variability
normal distribution
on-farm experimentation
probability distribution
production system
randomized complete block design (RCBD)
research domain
researcher planned experimentation
response curve
split plot arrangement
statistics
sub plot error
superimposed trial
test plot
variability

DISCUSSION

1. WAYS TO DECIDE WHICH PROBLEMS TO TEST FIRST FOR ANSWERS

Prioritization of farmers' problems is the link between initial diagnosis and design of on-farm experimentation. The preliminary result of the diagnostic process is problem identification. The diagnostic stage has been discussed in detail in volume I of the FSSP training units. The final unit in volume I specifically addresses problem identification. Please refer to that unit for more information.

Usually, initial diagnosis reveals more farmer problems than a team can address at one time. Setting priorities for research problems is necessary and will determine the types of testing needed to answer questions related to the priority problems. Research priorities should be a function of the farmers' situation and available information. The farmers' pre-acceptability of a trial, management practices and production

system are among the items to consider which may affect the priority setting decision.

Farmers' pre-acceptability and understanding of the treatments and objectives of the trial are determining factors in designing meaningful trials. Farmers should be informed about the variables considered for testing and consulted on probable degree of acceptance in case some of them prove experimentally to be good alternatives. Based on their response, the team is in a better position to define variables and levels, and to promote the new alternatives if this is justified after testing them. This is discussed in greater detail in (IV,A and B).

Management practices may affect the decision of whether or not to conduct a given type of trial. These should be carefully investigated before proposing a trial design. Existing management practices should be followed as much as possible in the design and management of the trials. For example, if the trial is designed to evaluate bean varieties and farmers plant beans as a relay crop after maize, the bean trial should be planted in a maize field at the same time that farmers plant their commercial bean crop. The farm setting can affect the decision to conduct a given type of trial. One example would be the land tenure of the farm households in the domain. A soil conservation oriented experiment might not be attractive to farmers that do not own land and rent land to farm.

The production system of the identified research domain serves as the basis for grouping components according to the researchable objectives. As much as possible, experimental designs should combine components in exploratory trials to study new variables and practices not used in the production systems. As the research process advances and experimental alternatives have been reduced in number, they should be handled independently, in order to obtain socio-economic data and avoid confounded results that are difficult to interpret. For example, an exploratory trial could include two levels each of nitrogen fertilizer, planting distance, and weed killer application in a 2 factorially arranged, randomized complete block design, while at a later trial, three levels of nitrogen could be tested in larger plot sizes that would allow a cost/benefit analysis of treatments. In the first case, plot size would be smaller but more replications would be recommended.

2. WAYS TO SELECT COOPERATING HOUSEHOLDS AND FARMERS

In selecting trial collaborators, the field team should first choose households representative of the research domain intended for each trial. A research domain is a problem focused environmental (agroecological and socioeconomic) range throughout which it is expected that hypothesized solutions to a defined problem could have potential applicabilty.

More specifically, care must be exercised to ascertain which of

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the household members are decision makers. Trial cooperators should be selected from among those who are responsible for the specific enterprises targeted for trials. It is always important, however, to involve the whole household in the evaluation of a new technology. Decisions to adopt a new technology may be made in consultation with members not directly responsible for the production activities. For example, in some locations women may not be involved in producing a certain food crop, but since they process it and prepare it, their preferences may decide whether new varieties are acceptable or not.

In some instances, the most representative farmers may not be willing to cooperate. In such cases, the FSR/E team may choose from among the most cooperative farmers in an attempt to attain the desired representation. More detailed information on the trial cooperator is given in (IV,A and V,B,3).

3. WAYS TO CLASSIFY TRIALS

a. Basis of the Production System

Farmers' production systems help make-up the physical setting in which trials will be set. The term "production system" is an abbreviation for "agricultural production system". The agricultural production system consists of the crop and animal production activities of the farming system. Whereas diagnosis and evaluation consider the entire farming system, including household, non-farm, and off-farm production and consumption activities, design and testing of interventions are generally based on the agricultural production system. Farm households in a given research domain share the same production system, and have similar problems and researchable priorities in that production system.

The following classification framework can help the multidisciplinary team accurately identify the predominant production system and thereby adapt trials to farmers' existing practices. Two major components of the production system need to be considered. First is the predominant type of crop or animal that forms the basis of the production system. (This collection of unit places major emphasis on crop-based production systems). In crop-based production systems, the second major component is the cropping pattern of the predominant crop. The different combinations of the first component, the basis of the production system (for example, maize), and the second component, the cropping pattern (for example, intercrop), identify different specific production systems (for example, maize intercropped with beans).

The FIGURE I.1 shows a matrix with examples of how the two components can be combined to identify five different specific production systems. First, predominant crop bases include: cereals, legumes, roots and tubers, fruiting and leafy vegetables, ornamentals, other field crops, pastures, trees, and

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others. Second, the predominant crop type in turn may be grown in one of the following different cropping patterns: sole crop, relay, intercrop, rotation or crop/animal mixture (where the crop is the main activity). If the animal is the main activity, the mixture is referred to as an animal/crop mixture.

Figure I.1 Matrix Depicting Possible Basic Cropping Systems and Patterns

CROP	PATTERNS				
	Monocrop	Relay	Intercrop	Rotation	Crop/Livestock
Cereals			A	B	
Legumes					C
Roots and Tubers					
Vegetables		D			
Ornamentals					
Other field Crops					
Pastures					
Trees					E
Other					

A = Maize/Bean
B = Rice - Legume
C = Groundnut/Cattle
D = Tomato/Sugar Cane
E = Lueceua/Calves

The results of diagnosis have indicated which specific component or components of the predominant crop and its cropping pattern are the researchable priorities for trial design. Treatment selection and choice of experimental design focus on those specific components. Other components can affect treatment responses and evaluation. The different types of components to consider include the following:

1. Variety
2. Cultural practices
   a. Spacing

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b. Planting time, intercropping, and crop rotation
c. Crop care
3. Plant nutrition (fertilizer)
4. Plant protection
5. Residual products

b. Basis of the Problem.

The most commonly identified researchable priorities include variety evaluation, plant nutrition, plant protection, and other cultural practices.

1. Variety Evaluation

Testing improved genetic material is a common type of research in farmers' fields. The following five considerations are important in variety testing:

a. Control treatments should include the recommended variety for the region as well as one or more local materials used by the farmers. Comparison of experimental varieties against these controls enables the team to reach more meaningful conclusions. This is because the team is interested not only in identifying higher yielding varieties, but in identifying other characteristics that farm household members consider in their assessment of varieties.

b. The farmers' own agronomic practices should be respected. The main objective of on-farm evaluation of new varieties is to know their potential under real farm conditions. Therefore special "experiment station" handling of these trials should be avoided.

c. Experimental varieties selected for testing should include as many available alternatives with a theoretical potential of excellence as is possible for the trial to accomodate. This means that the experimental varieties from national research should be tested along with local materials, varieties from private research programs, and material from neighboring nations, regional or international centers, and private seed companies.

d. Randomized complete block design (RCBD) is the experimental design most often appropriate for these types of trials.

e. Each experimental unit should be protected from environmental bias coming from growth habit of neighboring varieties. In maize, for example, where varieties may differ widely in plant size, extra rows of the same variety at each side of the experimental unit should be added. Those border rows are not harvested for experimental purposes. A common practice in maize is to plant four rows of each variety but only use the

inside two rows for harvest area and yield measurements.

2. Plant Nutrition

Fertilizer response trials are commonly conducted as site-specific experiments. Information on soil characteristics, previous management, and soil analysis should be determined before specific placement of the trial. Generally, at least three levels of each factor should be considered in order to estimate a response curve. Experimental designs should allow for measurement of interaction effects which are common in fertilizer response trials. Completely factorial arrangements offer a better estimate of interactions among factors than split-plot arrangements. The reason is that, in analysis of variance (ANOVA) for a completely factorial arrangement, the error mean square (MSE) is estimated with more degrees of freedom. The split-plot arrangement has the same number of degrees of freedom for interactions as the completely factorial arrangement, but residual degrees of freedom have to be distributed between the main plot error and the subplot error.

Special care must be used in field design to avoid fertilizer runoff effects from adjoining plots. Border rows or ample distance should be established between experimental units.

Control treatments should reflect local practice. When the local practice is <u>not</u> to use fertilizer, the control treatment should reflect that practice. When farmers' practices include <u>some</u> fertilizer use, the control treatment should not be a zero check, but should be based on the level used by farmers.

3. Plant Protection

Evaluation of insect, weed, and disease problems is more difficult than the other types of trials discussed so far. The main reason is that causal agents vary in intensity and mode of action, not only from year to year, but also within the plot area. Therefore, pest management trials require large experimental units with many replications, repeated for various crop cycles or seasons. Either complete factorial arrangements or split-plot arrangements in randomized complete blocks can be used. Frequently, a pest management trial is established by superimposing it on farmers' fields.

The probability distribution of counts or percentages of pest or disease damage does not usually follow a normal distribution. Sample data, therefore, need to be transformed in order to approximate the normal distribution, which is a theoretical requisite for the valid use of common statistical procedures. The most frequent transformations for these kinds of data are: (1) logarithmic [log(X) for count data that cover a wide range of values but have no zeros, or log(X + 1) when zero values are present]; (2) square root of x for values consisting of small whole numbers or percentage values between either 0% and 30%, or

70% and 100%, and square root of (X + 0.5) for small whole numbers with zeros; and (3) the angular or arcsine transformation for data with percentage values that overlap the ranges of 0-30, 30-70, and 70-100%. For percentage data within the range 30-70%, no transformation is normally necessary.

4. Cultural Practices

These include different between-the-row and in-the-row spacing, planting times and sequences, crop care practices, and water management. When several typical farmer practices are to be compared, a superimposed field trial may be appropriate. On the other hand, when some treatments will be dramatically different from typical practices, conventional field trials should be established. A split-plot arrangement can be appropriate when working with more than one variable. For example, when one variable requires different row arrangements, or if there is a large border effect and the experimental unit size is large, that variable can be assigned to the main plot. The other variables, such as in-the-row spacing, varieties, or secondary crop alternatives can then be assigned to the subplots. Precision will be greater for the variables in the subplots because more degrees of freedom are associated with subplots than with main plot error. An economic interpretation of these types of trials is mandatory since the crops involved generally have different market values, making individual yields alone an inadequate criterion of measurement.

c. Organizing Principles for the Minimum Data Set.

People, animals and plants respond to the environment in which they live. Often we do not understand the response reaction. Therefore it is very important to record as much about the environment in which the subjects live as logistically possible. When the variables have been measured and subjected to statistical analyses, the researcher must interpret the results. Often this interpretation is facilitated by information recorded on the environment affecting the research.

What environmental factors influence the outcome of an experiment? One of the characteristics of FSR/E research is the fact that often the work is removed from a center of research and disciplinary expertise may be unavailable when and where it is needed. To avoid the problem of not being able to interpret data because insufficient data was recorded, it is desirable to arrive on the research site with a list of the kinds of environmental information which should be gathered. This list can be considered a minimum data set. Those in charge of FSR/E research should not be expected to know all the factors, from several disciplines, which may influence the results or the interpretation of their research. To meet this need, field researchers should develop a list of environmental factors which may influence the results, and then record data for those factors. It is not enough to simply report results.

Professionals in the scientific disciplines will want to know the circumstances surrounding the research. For more details on the "minimum data set" refer to (IV,C).

4. WHAT ALTERNATE PATHWAYS ARE POSSIBLE FOR ON-FARM EXPERIMENTATION?

The purpose of on-farm experimentation is to test alternative production practices to solve problems identified by farm households. Three conditions must be met for on-farm experimentation to be useful:

1. Focus on real problems to which farmers want answers
2. Compare alternate practices under real farm conditions, and
3. Enable farmers to predict the likelihood that alternate practices will give improved results.

The first condition means that the team has understood farmers needs. Many techniques allow researchers to do this. These techniques are called diagnosis. Volume I discusses these techniques in detail. A team always begins with diagnosis, but diagnosis never ends. As it discovers more about farmer problems, the team may need to use diagnostic techniques in the middle of on-farm experimentation. In fact, on-farm experimentation itself is partly a diagnostic technique. This is because a team improves its understanding of farmer problems by observing how farmers compare practices the team suggests with their own traditional practices.

One diagnostic technique is to identify experimentation that farmers do themselves. For example, a team may find that some farmers are planting several varieties side by side or intermixed. Or, a team may find some farmers introducing new crops in an alley cropping pattern. These farmer-initiated changes can suggest ideas for more on-farm experimentation.

The second condition means that the team distinguishes between changes to be tested, and changes not to be tested. If a change is not for comparison purposes, it should not be made. Otherwise, the changes to be tested will be tested in the presence of other changes. Those changes will make test plots or animals different from farmer plots or animals. For example, if the team wants to test varieties, but farmers do not use fertilizer, test plots should also not use fertilizer. By not using fertilizer on the test plots, the team can compare the new varieties with farmer varieties even outside the test area. Farmers can see if the new varieties are different than their own under the same conditions their own varieties face.

This condition is likely to be met if farmers themselves initiate the experimentation. The team has to be careful not to violate this condition when it plans experimentation.

The third condition means that the team understands

variability. There will always be variability within farmers' fields. For example, consider two rows or two plots side-by-side, planted at the same time, with the same varieties, and grown the same way. Each plant will not have exactly the same height, nor will the yield from the two rows or two plots be exactly the same. The same would be true for two animals born to the same mother. It is unlikely that they would weigh the same, for example.

Now consider different farmers' fields of the same crop (or crop combination), or animals born to different mothers of the same breed. The variability will be even greater. All this is natural variability which is present even without on-farm experimentation.

In on-farm experimentation, farmers and the team are looking for differences among alternate practices. Suppose the team does find differences. The question then is, are the differences among alternate practices greater than differences just due to natural variation? What inferences can farmers and the team draw from the experiments? The answer is not definite.

The team and farm household members want to know what the chances are that the observed differences are really due to the alternate practices. Perhaps there is only a 50:50 chance. Are farm households willing to gamble on a 50:50 chance of an alternate practice being better? Or are they unwilling to gamble unless the chance is higher? How much higher does the chance have to be for them to be willing to gamble, and, furthermore to plant even one-tenth of a field using an alternate practice?

Statistics is a set of techniques to determine the chance that differences are real, and not just due to natural variation. Statistics consists of many techniques. A team needs to choose the best techniques for each situation.

What are the choices among statistical techniques? First, there are two basic pathways in on-farm experimentation. Each pathway leads to use of different types of statistical techniques. The two pathways are:

1. Observing and making inferences from farmer initiated experimentation.
2. Observng and making inferences from researcher planned experimentation.

Following the first pathway, the team does not plan the experimentation. There must be enough farmer-initiated experimentation to allow meaningful comparisons to be made among several farms. One useful technique is to group farms based on similar comparisons. Techniques to allow researchers to do this are currently under development.

Following the second pathway, the team plans the

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experimentation. The team may add treatments to farmer-initiated experimentation. Or, the team may design the experimentation and seek farm households willing to collaborate. The design may involve simply doing some practices differently in fields farm households have already planted. Such experimentation generally consists of superimposed trials. For example, the team may test several different times or methods of weeding. Or, the design may involve the team and farmers together putting out a test plot from the beginning of the season.

Statistical techniques for the second pathway are different than for the first. While techniques for the first pathway involve grouping observations, those for the second involve testing planned comparisons against one another. There are many ways to test planned comparisons The best technique to use depends on many criteria. The rest of this volume presents some of the most important criteria and some of the most widely-used techniques from which practitioners can choose.

5. HOW RESEARCHER-PLANNED TRIALS CHANGE OVER TIME

a. Trial Function in the Research Extension Process

According to their function in the research/extension process, trial types follow a general sequential trend as follows:

1. Exploratory testing

These are trials conducted when little is known about the domain or about possible treatment effects in the domain. They can be complementary to, or part of, the characterization of the domain and usually precede refinement trials. These trials normally provide more qualitative than quantitative information about several factors. Frequently, two levels of each factor are included and few replications are used. The most common designs used include the 2^n factorial and "add-on" or "take-off" trials. This type of trial can sometimes be superimposed on farmers' fields without the necessity of special preparation of the experimental area.

2. Refinement testing

Two kinds of trials can be included in this stage: site specific trials and regional trials.

a. Site specific trials are trials similar in design to conventional trials, but usually fewer treatments are involved. Perhaps as many as 20 to 25 treatments can be included, although this is not recommended unless a more complex type of design (e.g., a lattice or Latin square) is used to keep the experimental error at an acceptable level. Because of the requirement for intensive researcher's management, only a few of

these trials are normally conducted in a given domain. The most common design is randomized complete blocks (RCBD) with four replications.

b. Regional trials are trials that are amenable to both agronomic and agro-socioeconomic analysis. They are designed to expose the best treatments from site-specific trials to a much wider range of environments within a domain. Perhaps six treatments may be included, and five to ten sites can be utilized. A recommended design is randomized complete blocks (RCBD) or incomplete blocks (IBD) with two to four replications per site. ANOVA, regression, or modified stability analysis can be utilized. Combined analysis with site as a source of variation can be used in ANOVA to quantify treatment-environment interactions.

3. Validation testing

These trials provide the opportunity for the farmers themselves to manage and the farm households to evaluate the one or two most promising interventions identified in refinement testing. Large plots with no replications within farms are used. The purpose of these trials is for the farmers to compare the interventions with their own practices, so one plot with existing practices can be included in the design. This individual farmer control plot serves the researchers more than the farmers, because the farmers will be able to evaluate results based on their own fields. If researchers wish to measure results of the farmers' own practices, they can also sample the farmers' fields. However, agronomic and economic records of the farmers' practices must be kept to provide the necessary information. It is desirable to have at least 30 farmers conducting these trials in a given domain. The larger numbers improve the precision of the evaluation of the degree of acceptance by farm households of the new technology.

b. Researcher-Farmer Management Sharing

The relative participation of the multidisciplinary research team and farmers in conducting trials leads to another classification that will influence the number of trials of each kind in a given time and resource situation. There is a close correlation between management type and trial function.

1. Researcher Planted/Researcher Managed

This category includes those trials that represent a high economic risk to farm households because of the unpredictable or unknown behavior of intervention treatments under farmer conditions. Normally these trials would either be conducted in the experiment station, or if planted in a farmers' fields, the total cost of labor and inputs should be covered by the project. These trials are most common in exploratory and refinement

testing. For example, testing an array of new weed killers.

2. Farmer Planted/Researcher Managed

This category includes "superimposed" trials where treatments are placed on fields which have already been planted and are being managed by the farmers themselves. Treatments are marked by stakes or other means, and individual treatments are installed either by the researchers or the farmers. Together, researchers and farmers harvest the crop when it is mature. The design of a superimposed trial should be simple. Replications should be used at each location, although data from designs without replications at each site can be combined for regional analysis and interpretation. These trials are also most common in exploratory and refinement testing. For example, fertilizer redress application in a maize field.

3. Farmer Planted/Farmer Managed

Trials completely handled by farmers must include the following characteristics: a) the technology must be simple enough for farmers to comprehend and manage; b) farmers must use their own resources so they can understand all implications of the alternatives; and c) design of the trial must be simple enough that farmers can observe differences in treatments and/or measure them, with their own means of measurement. These trials are most common in validation testing. For example, testing of a new cultivar under the farmers' normal planting and cultivating procedures. The farmers pay all their usual costs plus the cost of the seed of the new variety.

6. WAYS TO ALLOCATE RESOURCES AND RESPONSIBILITIES

Trials established on the experiment station and trials established on farmers' fields are not substitutions for one another, but rather complement each other.

a. Station

Basic research trials are probably the only type of trial that should be planted solely on station. Other types of trials can be established on both the station and on farms. For example, trials of little known variables or treatments may be properly handled on either the station or as a researcher-planted, researcher-managed on-farm trial. Also, in a series of trials to expose treatments to a wide range of environments, the station can represent a "good" location to be considered in a combined analysis of results from various locations.

b. On-Farm

Homogeneous or uniform experimental areas are the rule rather

than the exception on the experiment station. The opposite is true on farms. Nevertheless, agronomists can reduce experimental error on farms by following a few common sense rules. For example, it is never wise to locate a research area adjacent to a house unless that is the environment in which the crop in the trial is going to be planted normally. Likewise, paths, canals, large trees and other conditions which are not a part of the environment for most of the crop should be avoided. If the crop is usually planted in these special environments, of course, it is appropriate to locate the experimental area in them. (IV,B).

The number of trials of each type of experiment will be related inversely to the relative participation of the team in each case. The more the research team's control, the fewer the number of trials. In cases of farmer planted, farmer-managed trials in the validation testing managed trials in the validation testing, the number of trials may exceed 50 per research domain, while trials on the exploratory testing may be only 3-5.

The planning of the activities and the personnel involved in conducting the trials needs to be well defined, financed and managed. This is discussed in more detail in (VI).

1. Framework for Interinstitutional Planning

Activities should be programmed to include the necessary interaction within and among the institutions involved. These activities should include:

Work plan session
Results presentation sessions

At these sessions, multidisciplinary and interinstitutional representatives should be present in addition to the field team who carry on the main responsibilities. Specific working sessions with research program personnel, extensionists and other interested persons should be programmed within the period of activities. By institutional planning, specialists from different disciplines and institutions are led to interact. If this interaction does not have a committed framework, by strictly voluntary participation the motivation and continuous level of participation tends to weaken.

2. Scheduling and Assignment of Resources and Responsibilities to Personnel

At the field team's level a detailed schedule of activities should be prepared for the specific assignment of each member, including a timetable indicating the beginning and completion date of each activity (VI).

The administration of personnel and the availability of inputs required to conduct the trials programmed should also be indicated in the overall schedule of activities. Although ideally

the multidisciplinary field teams should include personnel from the various biological sciences (agronomy and/or horticulture, plant protection, animal production, etc.) and social sciences (economics, sociology, anthropology, geography, etc.), this is seldom possible. Therefore, an approximation to the ideal situation becomes the logical alternative. This implies that the actual members of the field team need to include activities in the missing discipline(s) to the best of their abilities.

UNIT II

WHAT TREATMENTS TO TEST, AND WHERE

ACTIVITY ONE TRAINERS' NOTES
DEVELOPING TREATMENT OBJECTIVES

OBJECTIVE:

After completing this activity the participant will be better able to:

1. Develop a treatment objectives statement for an exploratory experiment.

TIME: approximately 20 minutes

MATERIAL:

1. Participant instruction sheet provided in their manual

INSTRUCTIONS:

1. Ask the participants to read the background information given below (this information appears in their instruction sheet) and write a short treatment objectives statement.

   Background Information: Most farmers growing yams plant on mounds, up to three feet high spaced between 3 x 3 feet and 5 x 5 feet apart. Present practice is to apply a small amount of compound fertilizer, one to two ounces per mound, either at planting or about one month after vine emergence. When applied after emergence, it is left on the surface, and heavy rains may cause wash off. Field station research on the same variety suggested that yields increased linearly with up to six ounces per mound. There is a potential export market for any increase in production.

PROCESSING:

1. Have the participants discuss their objective statements. The objective statement should stress fertilizer response, with the aim of estimating an economic optimum rate for recommendation to farmers.

ON-STATION AND ON-FARM LINKAGES

PREREQUISITES

None

PARTICIPANT LEVEL

Researchers, technical assistants, extensionists and administrators

LEARNING OBJECTIVES

After completing this sub-unit the participants will:

1. Better understand the linkages between on-farm and on-station research.

2. Know how and where these linkages take place in the course of conducting FSR/E.

3. Understand why effective FSR/E depends on good research station products.

KEY POINTS

1. FSR/E, through on-farm research, can greatly assist research stations in setting priorities for on-station research and experimentation, based on farmer needs and priority problems.

2. The results of on-station research can provide FSR/E with potential solutions (technologies) which may be able to solve farmer problems.

3. Both of these contributions point to the essential linkages between on-station and on-farm research.

DISCUSSION

FSR/E is an approach which research and extension institutions can use to increase their effectiveness in planning and implementing programs appropriate to farmers. Some people have feared that FSR/E is presented as a substitute for station based research. However, this is most certainly not the case. FSR/E is a complement to, and not a substitute for, station research. There is no necessary separation of station research and FSR/E. In fact, good FSR/E depends on effective and efficient station research. The same people may and often are involved in both. Many field teams employing FSR/E methods are based at research stations and carry out experiments on-station while simultaneously conducting on-farm trials. Rather than

being viewed as a separate effort, on-farm research should be part of the entire research and extension continuum. The diagram, shown in Figure II,A.1, and example demonstrate how these linkages operate.

Figure II,A.1 shows the interactions between station-based technical research and on-farm research (Collinson 1982). Franzel provides an example of how the diagram works under actual conditions (Franzel 1984).

The diagram depicts the interactions between station research and on-farm research as taking place in a series of stages. Stage One is a diagnostic survey of farmers in an area where maize-bean intercropping prevails. In Stage Two, low soil fertiltiy is identified as a constraint. During this stage, many potential solutions are proposed. Some, such as compound fertilizer, are rejected as not appropriate (no economic benefit to be gained, farmers lack cash purchase fertilizer, fertilizer not available even if farmers have case, etc.). Other solutions, such as the use of animal manure, are judged as potentially acceptable to farmers and feasible. Since work has been done on-station and much is known about manure composition and use, it is decided that experiments can be conducted on farmers' fields to test both the crop response to the manure and the different methods of application under farmer circumstances.

At this point, the team moves in two directions. First of all, concerning the potential solution of utilizing animal manure, the design of appropriate on-farm experiments begins (Stage Three). These will be conducted with farmer cooperators within the target group. If experiments are successful, recommendations are formulated and extended to farmers.

At the same time, the team decides that not enough is known about the other potential technology solutions to be able to proceed directly to on-farm trials. Rather, the team decides to send their proposals to the research station for further research and testing (Stage Four).

Taking one of these research proposals (the introduction of a green manure crop as a relay crop into standing maize following the bean harvest) as an example, the decision to continue on-station research before moving to on-farm trials does not eliminate the need to consult with farmers. Researchers determine that the crop would have to be plowed under before planting season. Since this would require additional labor and use of animal traction, researchers need to consult with farmers about the potential suitability of such shifts before investing in agronomic and biological study (Stage Five). Researchers discuss the proposal with farmers and find that farmers are very enthusiastic, despite the need for shifts in labor and animal use required by the technology. Researchers decide to test this innovation on-station in order to establish which possible forage crops appear most appropriate (i.e., add the most nitrogen,

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interfere least with the standing maize, etc.) The green manure crops are tested in Stage Six and those which respond best and seem most appropriate become part of the body of materials potentially suitable for farmers in the area.

These results then become part of the potential solutions which the team can consider in designing the next series of on-farm experiments. When once again at Stage Two, researchers decide to take these green manure crops from Stage Six and test them in Stage Three.

This example demonstrates that the linkages between on-station and on-farm research are very important. In Stage Four, the FSR/E team provides station researchers with ideas for experimentation based on their identification of farmer problems. Where research funds are scarce and research stations must focus on priority problems of farmers in a particular region, the referral of problems from a FSR/E team can greatly help prioritize problems for the allocation of these scarce funds. In Stage Six, the station supplies the FSR/E team with potential solutions to farmers' problems. The example demonstrates the interdependence of on-farm and on-station research. Neither can function effectively without input from the other.

It is important to remember that extension staff play important roles in all of these stages (see also VI, and IV,B). Simply conducting on-farm experiments does not necessarily mean one is doing FSR/E. An important part of FSR/E is that practitioners (researchers and extensionists) are interacting with farmers and are using their holistic understanding of farmers circumstances to plan and modify technology experimentation.

Figure II,A.1 Interactions Between Station-Based Technical Research and On-Farm Adaptive Research.

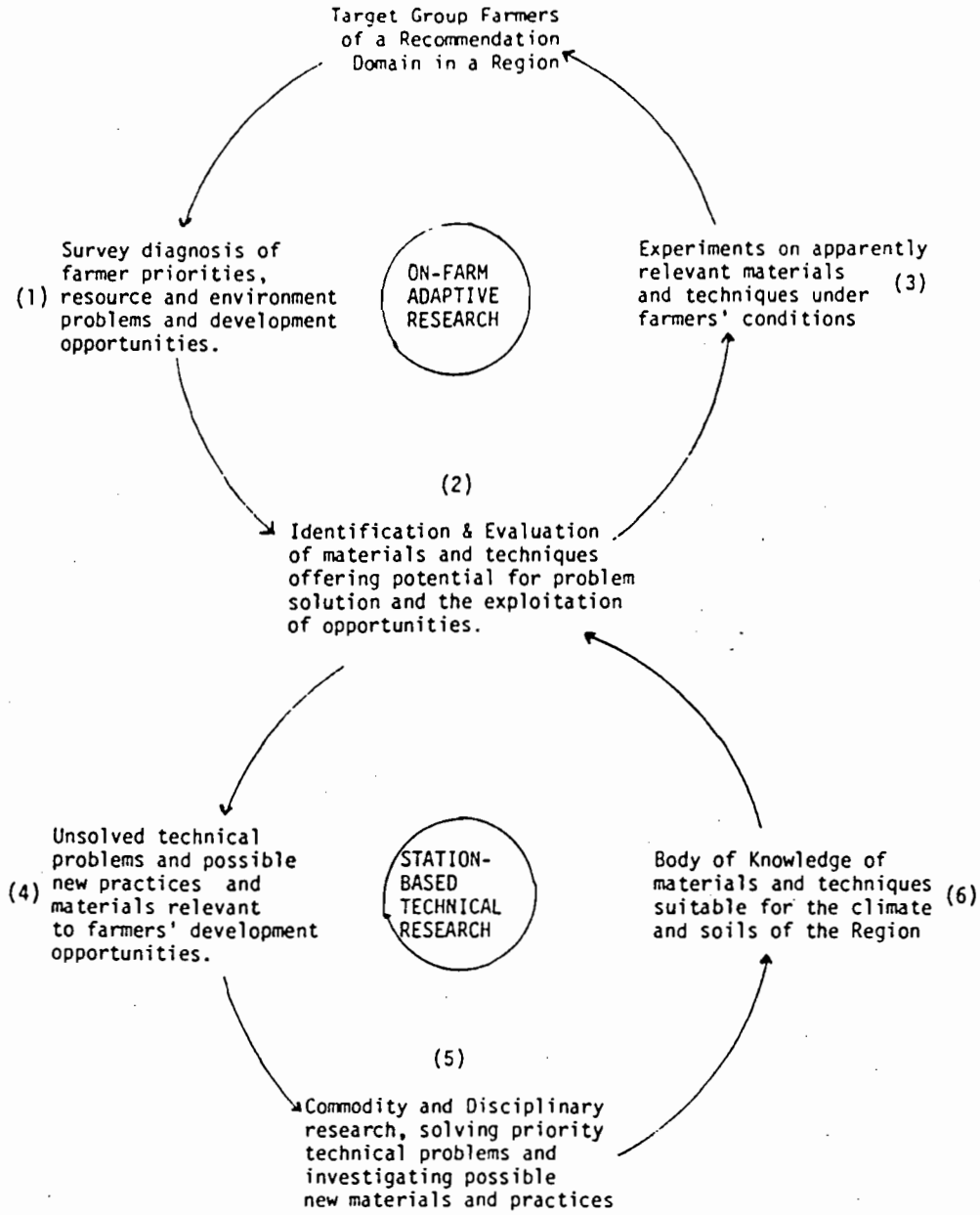

Figure 1. Interactions between Station-based Technical Research and On-Farm Adaptive research.

Source: Collinson, M. Farming Systems Research in Eastern Africa: The Experience of CIMMYT and Some Agricultural Research Services, 1976, 1981, MSU International Development Papers, No.3, 1982, page 5.

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WHAT KINDS OF FIELDS ARE AVAILABLE FOR TESTING

OUTLINE

1. Differences Within Farms
2. Differences Across Farms
3. Summary

PREREQUISITES

I What Kind of Testing to Do

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this sub-unit, participants will be able to:

1. Identify sources of differences within and among fields.

2. Determine the minimum size of a block.

3. Identify different possible combinations of block size and number within and across farms.

KEY POINTS

1. Soil, topography, land fragmentation, farm size, and household ability to take risk all affect block size.

2. Block size may vary from one farm to another.

3. Unequal block sizes across farms require either more complex designs or reduction of the number of treatments.

DEFINITIONS

ANOVA
block
contiguous blocks (plots)
experimental design
plot
trials
replication
treatment

DISCUSSION

1. DIFFERENCES WITHIN FARMS

Fields differ in many ways. Here are some examples:

Way a Field May Differ	Example
1. Soil texture	One end is sandier than the other end.
2. Soil depth	Even a light plowing turns up more clay at one end.
3. Slope	Low spot in the center.
4. Previous crops	Half left fallow but the other half planted in cowpeas.
5. Previous mangement	Ran out of fertilizer and only side dressed the first five rows.

Farmers know their fields well. They would not expect plants in the sandy part of a field to be the same as plants in a more silty part. The team should identify where to divide fields so that each part has no obvious differences. Parts with no obvious differences are uniform and homogeneous.

Sometimes, two parts of a field may be similar even though they are separated. For example, there may be a path in the middle of a field, but both sides are level and sandy.

A block is a uniform area with different treatments. Each block is subdivided into plots. A plot (experimental unit) is an area with only one treatment. A treatment is something the team wants to test. For example, a treatment may be a new variety, a high rate of fertilizer, or intercropping one row of groundnuts between each row of corn. A treatment might also be the farmer variety, a low rate of fertilizer, or corn planted at random among groundnuts.

Each part of the field that has no more obvious differences can be a block. Two parts of the same field separated by a path can also be one block. This would be a block with non-contiguous (separated) plots. In both contiguous and non-contiguous blocks, plots within the block will be about the same before the team applies the treatments. Obviously the plots in a block will be very different after the team applies the treatments. But they will not be very different in any other way. This is an important rule: blocks are uniform except for treatment differences.

Within each block, plots may not be exactly the same size or shape. One question is: how much difference in size or shape is acceptable? Some teams have found 20% to be a useful rule of thumb for maximum variability within blocks.

A block cannot be larger than a uniform part of a field.

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However, it can be smaller. A large uniform part of a field may be divided into two or three blocks. This means the same treatment can occur more than once. When a treatment occurs more than once, it is replicated. A replication is a complete set of treatments. More treatments means more plots in a replication. More plots in a replication means larger blocks. Larger blocks may mean a uniform part of the field can only be divided into two blocks, rather than into three blocks. That will reduce replications from two to three. Or, even larger blocks may mean the uniform part of the field cannot be divided at all. Then, the farm will have only one replication. Other replications of treatments will have to occur on other farms. (I) and (III,B) discuss why replication is important.

If more replication within each farm is important, the team may need to reduce the number of treatments. When is this necessary? That depends on the objectives of the trial and the treatments. (I,C,1) and (III,A) discuss how to make decisions about determining objectives.

If a team needs to reduce the number of treatments, how does it do so? (II,C,2) and (II,C,3) give some ways to reduce the number of treatments.

2. DIFFERENCES ACROSS FARMS

Farms can differ in the same way as fields. Sometimes part of a field on one farm will be similar to part of a field on another farm. For example, both fields may have one end where even light plowing turns up clay. Those two ends can be paired in incomplete block designs.

Some farms may have many small parcels of land. Other farms may have only one or two larger pieces of land. The small parcels may all be different, but the larger pieces of land may be more uniform within each larger piece. If so, larger blocks will be possible on the farms with the larger pieces of land.

The amount of land which the farm households will allocate for experimentation differs from farm to farm. Farms with more land often are willing to experiment on a larger field, or on a greater number of small parcels. Farmers with less land will probably not be willing to experiment on as much of their land. Some teams have found that a useful rule of thumb for deciding the maximum area to use for experimentation on each farm is to use no more than ten percent of the farm's total area.

Some farm households can take even less risk. For example, farm households with only a woman parent and no off-farm income will depend entirely on their land, crops, and animals. They may be able to put only one small parcel in an experiment. Farm households with two or three generations of adults, both men and women, may have many off-farm sources of income. They may be willing to take more risk and put a larger percentage of land in

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an experiment.

Where should a team place trials when farms differ? There are several decisions to make. The first is:

1. Should one kind of trial go on one kind of farm?

Whether the answer to this question is yes or no depends on the objectives of the trial. (III,A) will help to determine the answer to this question. Whether the answer is yes or no affects design and analysis choices:

1a: YES: The trial is a site-specific trial (see Hildebrand and Poey, 1985: 45-56). More than one replication is needed on the farm. This usually means a fairly large amount of land is needed on one farm. Designs for site-specific trials are similar to station trials. In fact, a station trial is simply a specialized site-specific trial, where the station is the team's "farm."

In station site-specific trials, blocks are usually touching, or contiguous. In on-farm site-specific trials, however, blocks might not touch. This means they are non-contiguous. Figure II,3.1 compares contiguous and non-contiguous blocks. Both have four blocks, I, II, III, and IV. Each block has four treatments, a, b, c, and d. In the non-contiguous example a path separates blocks I and II from III and IV.

Completely random or randomized complete block designs are most common (III,C,1). Analysis is by Analysis of Variance (ANOVA). The type of ANOVA used depends on design and treatments. (II,C and II,E) present treatment choices. (V,A) explains ANOVA procedures appropriate for different designs and treatment choices.

Figure II,B.1 Contiguous and Non-Contiguous Blocks in Site-Specific Trials

Contiguous

I	c	b	d	a
II	c	b	a	d
III	a	c	d	b
IV	b	a	c	d

Non-Contiguous

I	c	b	d	a
II	c	b	a	d

PATH

III	a	c	d	b
IV	b	a	c	d

1b: NO: The trial is a regional trial (see Hildebrand and Poey, 1985:7). The questions that follow can help the team decide where to place regional trials.

Regional trials mean the team places treatments across many farms. Where should the team place treatments when farms differ? There are several more questions to answer:

2. How much do farms differ in the amount of land available for trials?

2a: Small differences, this means blocks can be the same size on each farm.

2b. Large differences, this means block sizes may be different among farms. Some farms may have enough land for several blocks, but other farms may have enough land for only one block.

3. How many treatments does the team want to test?

3a. Small number (equal to smallest block size among the farms), this means every block can have a complete set of treatments.

3b. Large number (greater than the smallest block size among the farms), this means some blocks cannot have all treatments. The team has two choices:

(1) Use more complex designs.
(2) Reduce the number of treatments to equal the smallest block size and use simple designs.

What combinations of block size and number are possible in regional trials? What designs and analysis procedures are appropriate for different combinations? The next series of questions looks at several different possible combinations of block size and number across farms:

4. How many blocks can go on each farm?

4a. Only one block on every farm.

4b. More than one block on every farm with the same number of blocks on all farms.

4c. Unequal block numbers on different farms.

5. Are blocks the same size on each farm?

5a. Yes, farms have equal block sizes.

5b. No, farms have unequal block sizes.

For each answer to question 4, the team needs to ask question 5. There are six possible combinations. Which is the best combination? The best combination is different for each situation. The answer depends on the objectives of the trial and the treatments. (II,C,1) and (III,A) help to determine how to make decisions about objectives. The number of treatments also affects which combination the team will choose. (II,F) can help identify trade-offs among treatments and replications.

Which combination the team chooses in turn affects design and analysis procedures. A team needs to consider the design and analysis procedures in deciding which combination is best in each situation. Figure II,B.2) shows an example for each combination:

4a. Only one block on every farm.

5a. Equal block sizes, each block is a replication. This means each farm is also a replication. For example, farm A is also replicaiton I. Each farm has both treatments, a and b. Randomized complete block design is appropriate (III,C,1,b). In this example with only two treatments, a and b might be farmer variety versus a new variety. With more than two treatments on each farm, analysis can be by modified stability analysis. (V,A).

5b. Unequal block sizes, each farm may not be a replication. For example, farms C and D have larger blocks and could have a third treatment c in addition to treatments a and b. Treatment c, for example, might be a second new variety. Farms A and B would not have the second new variety. This combination requires use of an incomplete block design (III,C,b). The alternate is to eliminate the extra treatments (in this example, treatment c) so that all farms have equal block sizes (combination 4a + 5a).

4b. More than 1 block on every farm, with the same number of blocks on all farms.

5a. Equal block sizes, each farm has the same number of blocks. Each block has the same number of treatments, so each block can be a replication. For example, farm A has two replications, I and II. Farms B, C, and D similarly each have two replications. Each replication has all four treatments, a,b,c, and d. Randomized complete block design (III,C,1) is appropriate. Analysis of variance with combined analysis (V,A) can be used to test whether treatments perform the same on all farms or not. Modified stability analysis (V,A,) may also be used.

5b. Unequal block sizes, each farm has the same number of blocks, but some farms have smaller block sizes. For example, block III on farm B can take all four treatments, but replication IV can only take three treatments. Farm C is like farm B. On farm D, both replications VII and VIII can take only three treatments. This situation may arise when some farms have

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smaller amounts of land available for trials. This combination requires use of an incomplete block design (Unit III,C,1,b). The alternative is to reduce the number of treatments to equal the smallest block size. In this example, this would reduce treatment number from four to three. This change would then allow a randomized complete block design to be used, as in combination 2b + 3a.

Figure II, B.2 Examples of Combinations of Block Number and Block Size in Regional Trials Across Farms

Examples
(A,B, ... = farms)
(I, II, ... = blocks)
(a, b, ... = treatments in plots)

Combination	Block No.	Block Size				
2a + 3a	Only 1 on every farm	Equal	A I [a b]	B II [b a]	C III [a b]	D IV [b a]
2a + 3b	Only 1 on every farm	Unequal	A I [b a]	B II [a b]	C III [ab c]	D IV [ab c]
2b +3a	More than 1 block, in equal numbers,	Equal	A I [b a c d]	B III [d b c a]	C V [a c b d]	D VII [d b c a]
	on every farm		II [d a b c]	IV [d c b a]	VI [b a c d]	VIII [c b d a]
2b + 3b	More than 1 block, in equal numbers	Unequal	A I [b a d c]	B III [c b a d]	C V [a c d b]	D VII [a c b]
	on every farm		II [a d c b]	IV [a d b]	VI [c d b]	VIII [d a c]
2c + 3a	Unequal block numbers on	Equal	A I [a d b c]	B III [b d c a]	C V [a c d b]	D VI [b c a d]
	different farms		II [d c a b]	IV [b d a c]		
2c + 3b	Unequal block numbers on	Unequal	A I [c d b a]	B III [c d b]	C V [a b d c]	D VI [b c a]
	different farms		II [a b d]	IV [a c d]		

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4c. Unequal block numbers on different farms

5a. Equal block sizes, each block has the same number of treatments, so each block can be a replication. However, some farms have more blocks than others. This may happen if some farms have less land available for trials. For example, farms A and B each have two blocks, but farms C and D have only one block each. Randomized complete block design (III,C,1,a) can be used. Analysis of variance with combined analysis (V,A) can be used but analysis is more complex.

5b. Unequal block sizes, both number of blocks and size of blocks differ among farms. For example, farm A has two blocks, but only block one can take all four treatments. Block II can take only three treatments. Farm B also has two blocks, but each can take only three treatments also. Farm C has only one block, which can take all four treatments. Farm D has only one block, and it can take only three treatments. This situation may occur with greater diversity among farms in land available for trials. However, this combination requires an incomplete block design (III,C,1,b). The alternative is to reduce the number of treatments to the smallest block size (in this example, 3). This change would allow a randomized complete block design to be used, as in combination 2c + 3a.

Note: In the examples shown in figure II,B.1, blocks are not touching each other. These are non-contiguous blocks. Non-contiguous blocks are the rule in regional on-farm experimentation. This is different from station experimentation.

3. SUMMARY

Farms can differ in many ways. Uniform parts of fields are called blocks. Blocks are divided into plots. Each plot has a treatment.

Farms can differ in the amount of land available for trials. They can also differ in size and number of blocks.

Teams can place trials on only one farm or across farms. Trials on any one farm are site-specific trials. They are similar to station trials. Trials placed across farms are regional trials. Blocks are located on many farms. Block size and number may be equal or unequal among the farms. Equal block size and number allow simple designs and analysis procedures. Unequal block size and number require either more complex designs or reduction of treatment number.

OTHER SECTIONS THAT CAN HELP

II,C,1	Defining Treatment Objectives
II,C,2	What to Consider in Selecting Subsets of Treatments
II,C,3	(Optional) Statistical Techniques for Selecting Subsets

	of Treatments
II,E	Examples of Treatments for Different Types of Problems
II,F	Looking Ahead: What Are Some Trade-offs Between Treatments and Replications
III,A	How Objectives Change in the Research-Extension Process
III,B	What Designs Can Do
III,C,1	Ways to Replicate Treatments Within and Across Farms
V,A	How to Analyze and Evaluate Trial Data

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DEFINING TREATMENT OBJECTIVES

OUTLINE

1. Focusing on Priority Problems
2. Developing a Treatment Objectives Statement
3. Using Previous Research to Provide Clues
4. Checking with Farm Households

PREREQUISITES

I What Kind of Testing to Do

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this section, participants will be able to:

1. List steps useful in developing a treatment objectives statement.

2. Identify differences in treatment objectives common in exploratory, refinement, and validation testing.

3. Write a treatment objectives statement.

KEY POINTS

1. A treatment objectives statement provides a set of criteria for selecting individual treatments.

2. Biological criteria are relatively more important in treatment objectives statements for exploratory and refinement testing than for validation testing.

3. Social and economic criteria are relatively more important in treatment objectives statements for refinement and validation testing than for exploratory testing.

DEFINITIONS

block
ex ante analysis
exploratory trials (testing)
factor
farming system
plot

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probe
refinement trials (testing)
response
response Curve
treatment
validation trials (testing)

DISCUSSION

1. FOCUSING ON PRIORITY PROBLEMS

In researcher-planned experimentation, the team focuses on a priority problem of farm households. For example, the priority problem may be land preparation techniques to better conserve water from early rains. The team wants to test different ways of land preparation. How does a team decide what to test for the as the priority problem? How many different ways of land preparation should it test? What other associated practices should it test at the same time? For example, should it also test fertilizer placement methods? Perhaps fertilizer placement methods should change with different land preparation methods. There are many decisions a team needs to make.

Each different type of land preparation could be a treatment. For example, flat cultivation without ridges would be one treatment. Making ridges with hoes at one month would be a second treatment. Making ridges with donkeys at one month would be a third. Changing the time of making ridges would result in still more treatments. Other treatments could include different fertilizer placement methods. Many treatments are possible.

Some treatments are related. For example, flat cultivation, hoe ridging, and donkey ridging are all land preparation methods. Farm households would do only one of these. Fertilizer placement in hills or in bands are also two related treatments. Again, farm households would do either one or the other.

A set of related treatments is called a factor. The land preparation methods are one factor, and the fertilizer placement methods are another. Many combinations of the different treatments in two factors (sets of treatments) are possible. For example, flat cultivation can be done with fertilizer placed either in hills or in bands.

Another example of two factors would be kinds of fertilizer elements. Nitrogen would be one factor, and phosphorous another. Many different nitrogen rates would be possible at each phosphorus rate.

Combining two factors is often necessary if one factor affects the other. For example, incereasing nitrogen may have little effect at a low phosphorus rate, but have a large effect at a high phosphorus rate. This is called an interaction between two factors.

Usually, all the operations are done the same way over large areas. For example, farmers would usually not use four different land preparation methods on the same field. In experimentation, however, fields are divided into different parts. These parts are called blocks and plots. Each plot gets one treatment or treatments. This way, the team and farm household members can compare the different treatments. (II,B) discusses ways to choose fields and divide them for treatments. (III,C,1 and 2) describe different ways to place treatments in fields within and across farms, in order to obtain analyzable data.

The first step in deciding what to test for a priority problem is to develop a treatment objectives statement. This statement outlines what the team and the farm households want to learn from the trial. The statement provides a set of criteria for choosing among many treatment possibilities.

2. DEVELOPING A TREATMENT OBJECTIVES STATEMENT

How does a team develop a treatment objectives statement? Four steps are useful:

1. Reviewing what the team has learned in diagnosis about the priority problem.
2. Reviewing what the team knows from previous station and on-farm research on the priority problem.
3. Writing a preliminary statement.
4. Checking the preliminary statement with farm household members.

The priority problem is only one part of everything farm households do. Everything that farm households do is called the farming system. For example, land preparation is only part of growing sorghum. Sorghum is only one crop that farm household members grow. Besides all the crops, there may be donkeys, oxen, or perhaps other animals. There is also milling, working, selling of the sorghum, and so on.

In reviewing what the team has learned in diagnosis about the priority problem, the team needs to consider both the problem itself, and all the other activities related to the problem. Some questions to consider are:

1. What are the current management practices (for example, flat cultivation or donkey plowing)?
2. What are current levels of input use (for example, renting of donkeys, fertilizer use, etc.)?
3. What other activities compete for labor and inputs (for example, yams planted at the same time)?
4. How is the product used (for example, what mix of home consumption, informal barter, sharing, or sale)?
5. Who receives the benefits of the product (for example, is money from sales retained by one individual or is it redistributed throughout the household)?

6. How are the farm households organized (for example, ages and gender of different members, etc.)?
7. How available are inputs and markets (for example, credit, distance, etc.)?
8. Who in the household has access and control over inputs?

3. USING PREVIOUS RESEARCH TO PROVIDE CLUES

How much does the team already know about what can happen with different possible treatments? Here is where reviewing of previous station and on-farm research can provide clues. These clues can save time. Here are some useful questions:

1. What is known about the biological principles underlying the problem (for example, soil characteristics)?
2. What is known about biological effects of different kinds of inputs (for example, growth differences with different fertilizer placement methods)?
3. What is known about biological effects of different levels of inputs (for example, stalk versus grain yield over a range of nitrogen rates, lodging over a range of nitrogen rates, etc.)?
4. What is known about biological effects of different factors varied at the same time (for example, nitrogen and phosphorus, weeding and plant spacing, or different rotations)?
5. What is known about economic and social effects of different kinds of inputs (for example, use of traction freeing household labor for more cash crop vegetable production)?
6. What is known about economic and social effects of different levels of inputs (for example, yield increases paying for one but not two weedings)?
7. What is known about the economic and social effects of different factors varied at the same time (for example, one plant spacing reducing labor for weeding, and making it possible for the household to plant a larger area)?
8. What is known about farm household acceptability of different treatments?

When less is known about biological principles or effects, treatment objectives may be to clarify these first. For example, four fertilized rates may be needed to make a good response curve. With a good response curve, it is possible to find the highest rate. In figure 1a, with only three points, the curve is still rising. Perhaps a higher rate will still increase yield. Perhaps it will not. The experiment does not give a clear answer. In figure II,C,1.1(b) with four points, the curve has flattened. Now it is clear that the third level is high enough. There is no gain at higher rates. The higher rate is nevertheless useful, to probe the response.

Clarifying biological principles and effects is most common in exploratory testing. The objective is to explore the biological effects. Sometimes the objective is just to explore

which factors have any effect or response at all. At other times, the objective is to explore principles or responses for one or two most promising factors.

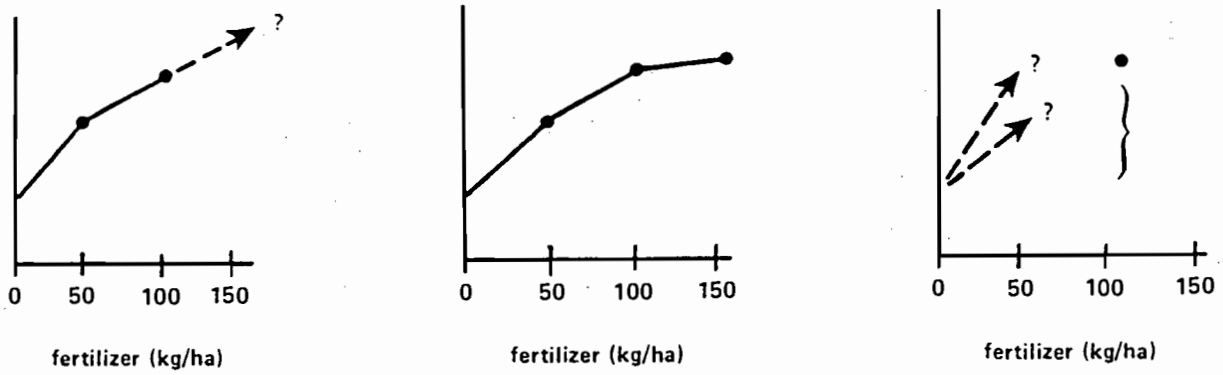
In refinement testing, clarifying biological effects is often still important. This is especially true if exploratory testing has eliminated some factors, but one or two others had a biological effect. The objective may be to find the cut off point of the biological effect. For example, the exploratory testing may simply have shown that 100 kg increases yield over no fertilizer. Another trial is necessary to construct a curve like Figure II,C,1.1(b).

Clarifying economic and social effects becomes more important in refinement testing. For example, in Figure II,C,1.1(b) the 100 kg gives more yield than the 50 kg, but the increase is not as great as from 0 to 50 kg. Yet each increase of 50 kg will cost the same. When does it stop paying to add 50 kg, at 50 or 100 kg? Or, perhaps the additional labor needed to apply even 50 kg is better spent on yam production. Who in the household provides the cash for the fertilizer, who provides the additional labor, and who benefits from the increased yield?

The team can ask many questions to help clarify possible economic and social effects before trying treatments. Sometimes the answers to these questions will eliminate a treatment before testing. This type of analysis is called ex ante analysis. Ex ante means "before". That is, before actually trying the treatment. Here are some useful questions for ex ante economic and social analysis:

1. Could farmers adopt and use a recommendation or technology based on these treatments?
2. Could farm households afford the money to buy the necessary inputs?
3. Are the required inputs, if any, locally available? If not, are they likely to become available? Does the farmer have access to these inputs?
4. Does the farm family have the labor resources necessary to adopt a technology recommendation arising from this experiment? Is hired labor available?
5. Is there anything in the treatments that could pose cultural problems to farmers, or to some farmers among the target group?
6. Are the expected benefits from the adoption of the recommendations expected to come out of the experiment likely to be attractive to farm households? Are yield increases, labor savings, etc., sufficient to attract farm households to adopt the expected recommendation?

Figure II,C,1.1 Determining Responses



1a: Incomplete curve

1b: Complete curve with probe at 150 kg/ha

1c: Exploratory results

4. CHECKING WITH THE FARM HOUSEHOLDS

All of the above examples and questions can help a team write a good treatment objectives statement. There is still one more step before testing. Even the best ex ante analysis may not reveal possible problems with treatments. A treatment objectives statement is only preliminary until the team checks it with farm household members. Here are some points to check:

1. Can farm household members understand the treatment objectives statement?

1a: If yes, then check point 2.

1b: If no, the team needs to record the statment. Recording in turn helps the team explain experimentation in farmers language. This promotes better communication.

2. Do farm household members agree with the treatment objectives?

1a: If yes, then check point 3.

1b: If no, the team needs to determine why not. This can help the team understand farm households objectives and constraints better. The team may need to change treatment objectives.

3. How much risk from the treatments can farm households accept? There are two kinds of risks to consider.

3a: Ordinary risk, associated with normal farmer practices (due to weather, diseases, pests, uncertain input supplies, unstable market prices, etc.)

3b: Experimental risk, associated with new practices (due to uncertain adaptability).

Farm households understand ordinary risk. They are willing to (or forced to) accept it as a normal part of farming. This kind of risk should not affect treatment objectives and treatment choices. Farmers will understand losses due to normal risk and would not expect compensation (although they would probably accept it if offered). In order to avoid paternalism in the research process, it is better not to plan for compensation for these cases. Farm households sometimes also take some experimental risk. On-farm experimentation allows them to take more of this kind of risk, because the research and extension organizations share the experimental risk with the farm households.

How much experimental risk should farm households take, and how much should the team take? The answer will depend on each situation. As a general principle, though, farm households should take enough experimental risk to feel that the trial is

theirs. Farm households should not have to take so much experimental risk that their well-being is jeapordized. The team should be prepared to bear input costs that would place undue strain on farm households. The team should also be prepared to provide compensation or additional support for the costs of treatments that are obviously not adapted. If the team cannot afford the compensation or additional support for such treatments, then it needs to reconsider the treatment objectives.

The preliminary treatment objectives statement is the first set of criteria for choosing treatments. Checking the preliminary treatment objectives statement with farm households helps finalize those criteria. The team prepares the criteria with the many treatment possibilities. How does the team reduce the number of treatment possibilities to match the criteria? (II,C,2) and (II,C,3) discuss methods for selecting subsets of treatments.

OTHER SECTIONS THAT CAN HELP

II,B	What Kinds of Fields are Available for Testing
II,C,2	What to Consider in Selecting Subsets of Treatments
II,C,3	(Optional) Statisical Techniques for Selecting Subsets of Treatments
III,C,1	Ways to Replicate Treatments Within and Across Farms
V,B,3	Social Science Perspectives and Farmer Participation

ACTIVITIES

ACTIVITY ONE: DEVELOPING A TREATMENT OBJECTIVES STATEMENT

Material useful for practical application exercises is available in the FSSP Paraguay Case Study Training Document, practica 1 and 2.

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OBJECTIVE:

After completing this activity you will be better able to:

1. Develop a treatment objectives statement for an exploratory experiment.

INSTRUCTIONS:

1. Read the background information given below and write a short treatment objectives statement.

   Background Information: Most farmers growing yams plant on mounds, up to three feet high spaced between 3 x 3 feet and 5 x 5 feet apart. Present practice is to apply a small amount of compound fertilizer, 1 to 2 ounces per mound, either at planting or about 1 month after vine emergence. When applied after emergence, it is left on the surface, and heavy rains may cause wash off. Field station research on the same variety suggested that yeilds increased linearly with up to 6 ounces per mound. There is a potential export market for any increase in production.

2. Be prepared to discuss your statement.

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WHAT TO CONSIDER IN SELECTING SUBSETS OF EXPERIMENTAL TREATMENTS

OUTLINE

1. Why Selecting Subsets of Treatments is Important
2. Agronomic Criteria
3. Economic Criteria
4. Social Criteria

PREREQUISITES

I What Kind of Testing to Do
II,C,1. Defining Treatment Objectives

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this section, participants will be able to:

1. Give reasons why it is often necessary to select a subset of treatments.

2. Write a complete list of treatment options.

3. Identify agronomic, economic, and social criteria for selecting subset of treatments.

KEY POINTS

1. Writing a complete list of possible treatment options is the first step in selecting the best subset of treatments.

2. Economic criteria affect how agronomic criteria are applied for selecting the best subset of treatments.

3. Social criteria include not only taboos, but also effects within the household (or compound), effects across households (in the community or village as a whole), and preferences for specific practices, characteristics, and uses.

DEFINITIONS

ex ante analysis
exploratory trials (testing)
factorial array
farming system
partial budgeting

probe
production system
refinement trials (testing)
response
sensitivity analysis
treatments

DISCUSSION

1. WHY SELECTING SUBSETS OF TREATMENTS IS IMPORTANT

There are often a large number of possible treatments or treatment options that could be included in an experiment. For example, we might list ten herbicides that could be used for pre-emergence weed control in corn. Each could be tested at two sites, and we might want more than one control treatment. This could give us 22 or more treatments. We could combine such a trial with varieties, or fertilizer level, etc.

Testing all the treatment options at once is usually not practical. Too many treatments mean a large trial. Large trials require either more land on each farm, or more complex designs to spread the treatments across many farms. Large trials on each farm make the trial difficult for the farm household to understand and manage. The team may then try to compensate by increasing its own role in management. This requires more time and more money for travel. It also reduces farmer management and makes the trial more artificial. That defeats an important objective of on-farm experimentation: testing treatments under real farm conditions.

The task is then to select the best possible subset of treatments. How does a team do this? Three steps are useful:

a. Write a complete list of possible treatment options.
b. Develop agronomic, economic, and social criteria based on the treatment objectives statement (II,C,1).
c. Use the agonomic, economic, and social criteria to decide which treatments to eliminate, and which treatments to include.

Let's look at some examples of criteria for selecting treatment subsets, and consider their use.

2. AGRONOMIC CRITERIA

Some of the agronomic criteria a team might use for selecting treatment subsets include:

a. Adaptability of the variety, practice, or input level to the environment;
b. Acceptability of the characteristics of the crop product in the local market, or in an export market;
c. Availability of seed and other inputs locally, or the

probability that these could become available;
d. Dependency on purchased inputs for acceptable performance;
e. Compatibility with the existing production system and its cropping pattern.

The complete list of treatment options might be, for example:

a. A list of varieties (cultivars) with some desirable characteristics, such as resistance to a specific disease or increased yield potential;
b. A full factorial array of fertilizer treatments for N, P, and K.
c. A list of pesticides with the capacity to control a specific pest or group of pests.

Example 1: Varieties

Let's apply these criteria to the first of these examples, the list of varieties. For example, suppose it is a list of tomato varieties:

a. Which of these varieties are known to be tolerant of our targeted environment? That is, tolerant of high temperatures, or wet soil conditions? This may eliminate many.
b. Are the tomato fruits acceptable on the local market or on a specified and available export market in terms of color, shape and size? If for an export market, which varieties handle the best?
c. Would seed of these be available locally? Is seed available overseas in commerical quantities? This may eliminate some "breeding lines".
d. Which of these varieties has a high requirement for other inputs (II,D,3)? Are some susceptible to the diseases common in the domain environment, thus requiring heavy inputs of pesticides?
e. Would these varieties fit into the production system we have targeted and its cropping pattern? Is there evidence that they could not be intercropped, for instance, if intercropping is the current practice?
f. Are there other useful characteristics we could use to select a subset of varieties? Resistance to one or more diseases or pests, extended bearing season, or ability to stand without staking are some possible criteria.

Many of these agronomic criteria depend on economic criteria. For example, market factors would determine acceptable fruit color for criteria b. Input costs and availability affect criteria c and d. Other examples are also possible. This illustrates an important concept in farming systems research: interactions among parts of the system. Each part (for example, tomato production) depends on other parts (for example, markets and inputs).

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Example 2: Fertilizer

For a fertilizer trial, the complete list of treatments might be 12 treatments:

1. Three rates of nitrogen (for example, average farmer rate, a higher rate recommended by the extension service, and an even higher rate as a probe).
2. Two rates of phosphorus (for example, with and without).
3. Two rates of potassium (for example, also with and without).

Combining the three nitrogen treatments, the two phosphorus treatments, and the two potassium treatments gives 3 x 2 x 2 = 12 total treatment combinations. This is called a full, or complete, factorial array (III,C,2,a).

Agronomic selection criteria might lead to the following questions:

a. Which fertilizer component is most critical: nitrogen, phosphorus, or potassium? Which is least critical? Could we eliminate the one least critical component?
b. What fertilizers or fertilizer carriers are available locally? Are 'straights' or single element fertilizers (like ammonium sulfate or triple superphosphate) available locally? If only compound fertilizers (like 10-10-10) are available locally, testing the factorial array will only be useful for understanding biological response to the three elements. Would it be more useful to test two or more rates of the compound carriers locally available instead? The answer will depend on the treatment objectives statement. In exploratory testing, the objective may be to determine the biological response under farm conditions to the individual elements. In that case, testing treatments for individual elements (nitrogen, phosphorus, and potassium) may be appropriate. In refinement testing, however, the objective may be to select the best carrier and rate combination.
c. How might the natural environment affect increased fertilizer use? For example, is high rainfall likely to leach out increased basal inorganic nitrogen? Would testing different frequencies of sidedressing be more useful?
d. Would increased fertilizer use require higher levels of other inputs? For example, is increased use of nematicides necessary for uptake of increased level of fertilizer?
e. Is increased fertilizer use likely to affect other crops or animals in the production system? Will any such effects be beneficial or harmful? For example, will residues prove beneficial to following crops?

Example 2 shows, as did example 1, how agronomic criteria often depend on economic criteria.

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3. ECONOMIC CRITERIA

Most farm households aim to make money from at least part of their farming, although subsistence farming may be an important part of their farming systems. Subsistence production also can have an economic basis. Supplies of a staple crop may be uncertain, or, prices may be highly variable, especially in "hungry" periods. Economic criteria include linkages between commercial farming activities and subsistence farming activities. Some of these criteria include:

a. Labor needs for subsistence food crops at the same time as proposed treatments.
b. Cash needs for food or for inputs for food crop production at the same time as proposed treatments.
c. Effects of proposed treatments on land area available for food crops or animals.
d. Effects of proposed treatments on wild crops, animals, or fish used as food. For example, will increased pesticides kill off fish in irrigation canals, or make them unsafe to eat?
e. Effects of increased yield from proposed treatments on supply and prices in the community as a whole. For example, will local merchants hoard food stocks more, and thereby drive up prices?

A team can use various methods of economic analysis before beginning the trial to select treatment subsets. Such analysis before testing is called ex ante analysis. Some methods include:

a. Partial budgeting to compare anticipated increased costs and increased benefits. (V,C) This leads to;
b. Projected benefit/cost ratios: does any one treatment show a substantially higher ratio than others?
c. Sensitivity analysis to examine possible changes in the ranking of treatments depending on changes in costs of inputs and prices of products.
d. Input availability.
e. Market opportunities and marketing systems: increased production may increase marketing problems or force down prices. It might be better to aim at increased productivity of land, labor or inputs at the same yield level.
f. Communications, especially roads as they affect access to markets, may hamper agricultural production.

Because of linkages between commercial and subsistence farming activities, a team may need to use these methods not only for the trial crop or animal, but also for other crops or animals that the trial treatments may affect. For example, if prices of a food crop increase, farm household members may have to reduce purchases of the food crop. They may then increase land and labor used for the food crop. That can reduce land and labor available for the trial crop.

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A good reference on how to do partial budgeting, calculate projected benefit/cost ratios, and do sensitivity analysis is the following: Perrin, R., et.al., From Agronomic Data to Farmer Recommendations An Economics Training Manual, CIMMYT.

(V,C) also explains how to do partial budgeting. The example in that section uses data after trials have been done, but the same techniques can also be used for ex ante analysis.

4. SOCIAL CRITERIA

Farm household members belong to many different groups at the same time. Some of these groups may also overlap. For example, the farm household and the community (or village) may overlap. Women may work on compound fields as a member of their household. They may also work on private fields of other women as a member of a group that shares labor. Social criteria include both effects within the household (or compound), and across households (in the community or village as a whole).

Some social criteria within the household include:

a. Effects of changes in production of trial crops or animals on food consumption by different household members. For example, would increased production and income fRom a women's crop improve children's diet?
b. Effects of changes in production of trial crops or animals on food preparation. For example, would new varieties require less cooking time? Would this reduce labor needed for gathering fuelwood?
c. Effects of changes in production of trial crops or animals on other household activities. For example, would more frequent weeding reduce time available for breast feeding?
d. Effects of changes in production of trial crops or animals on other crop and animal production sub-systems. For example, would the introduction of irrigated rice and hybrid varieties to male farmers in a household take away from female farmers the rights and access to farm swamp and mangrove rice on the same land? How would this affect household subsistance?

Other social criteria involve the community as a whole. Some of these involve changes in the community that might result from proposed treatments. Some of these criteria include:

a. Effects of proposed treatments on labor-sharing or food-sharing networks.
b. Effects of proposed treatments on differences in power and status among different people in the community. For example, will increased labor needs at planting or harvest time provide more work opportunities for landless laborers more dependent on tenant farm households?

Other community-based social criteria involve preferences and taboos. These community preferences and taboos can affect

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individual decisions about the acceptability of proposed treatments. Some of these criteria include:

a. Cultural or regligious preference for specific crop management practices (for example, planting mungbean during the first week of August because of religious significance).
b. Cultural or religious taboos against specific crop management practices (for example, prohibition against removing volunteer corn plants in the field with a succeeding crop, or against thinning extra corn plants in a hill, because of a belief that they are "children of God.")
c. Cultural or religious preferences for specific product characteristics (for example, preference for green-skinned tomatoes).
d. Cultural or religious preferences for specific product uses (for example, preference for using soybean as a coffee substitute rather than as a vegetable eaten with rice; preference for rice over sweet potato as a staple and use of sweet potato only as a snack food).
e. Cultural or religious taboos against specific product uses (for example, prohibitions against pigs and pig meat for Muslims or Seven Day Adventists.

Social criteria can also affect economic and agronomic criteria. A preference for green-skinned tomatoes may mean lower prices for red-skinned tomatoes. The team may decide to eliminate red-skinned lines. Or if resistance to a major disease (for example, bacterial wilt) is found only in red-skinned tomatoes, a breeding program to transfer the resistance from red-skinned to green-skinned tomatoes may be needed first, before moving to on-farm experimentation. Here, social criteria lead to a new linkage between station research and on-farm research.

OTHER SECTIONS THAT CAN HELP

III,2,a	Complete Factorials
V,C,	Economic Analysis of Treatment Differences

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ACTIVITIES

ACTIVITY ONE: DEVELOPING A COMPLETE LIST OF TREATMENT OPTIONS

ACTIVITY TWO: SELECTING A TREATMENT SUBSET

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ACTIVITY ONE TRAINERS' NOTES
DEVELOPING A COMPLETE LIST OF TREATMENT OPTIONS

Summary Description: This exercise uses the same example as the activity 1 in (II,C,1). There, the objective of the exercise was simply to develop a treatment objectives statement. Here, the objectives of the exercise are now to make a complete list of treatment options and then select the best subset.

OBJECTIVE:

After completing this activity the participants will be able to:

1. Develop a complete list of treatment options.
2. Select one appropriate subset.

TIME 20 minutes

MATERIALS:

1. Participant instruction sheet provided in manual.

INSTRUCTIONS:

1. Have the participants read the background information given on their instruction sheet. This information is repeated here for your convenience.

   Background information: Most farmers growing yams plant on mounds up to 3 feet high spaced between 3 x 3 feet and 5 x 5 feet apart. Present practice is to apply a small amount of compound fertilizer, 1 to 3 ounces per mound, either at planting or about 1 month after vine emergence. When applied after emergence, it is left on the surface, and heavy rains may cause wash off. Field station research on the same variety suggested that yields increased linearly with up to 6 ounces per mound. There is a potential export market for any increase in production.

2. Ask the participants to make a complete list of treatment options, and select an appropriate subset.

PROCESSING:

1. You should discuss the answers with the group after they have completed the activity. Possible answers include:

A complete list of possible treatments could well be as many as 36, as follows:

3 mound spacings (3 x 3, 4 x 4, 5 x 5)
3 fertilizer rates (2, 4, oz per mound)
2 application times (at planting vs later)

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2 methods of application (on surface or covered)

There would give a 3 x 3 x 2 x 2 factorial array.

The question is - what subset to use? This is an exploratory experiment. The simplest subset would be one spacing (say 4 x 4) and one method of application (covered) but three rates and two times of application. This would give six treatments as follows:

2 oz at planting	2 oz later
4 oz at planting	4 oz later
6 oz at planting	6 oz later

Based on local experience, trainees might argue for some other subset, but since the objective is to look at fertilizer response, fertilizer rate must be one component.

ACTIVITY ONE TRAINEE INSTRUCTIONS

DEVELOPING A COMPLETE LIST OF TREATMENT OPTIONS

OBJECTIVE:

After completing this activity you will be able to:

1. Develop a complete list of treatment options.
2. Select one appropriate subset.

INSTRUCTIONS:

1. Read the background information given below:

   Background information: Most farmers growing yams plant on mounds up to 3 feet high spaced between 3 x 3 feet and 5 x 5 feet apart. Present practice is to apply a small amount of compound fertilizer, 1 to 3 ounces per mound, either at planting or about 1 month after vine emergence. When applied after emergence, it is left on the surface, and heavy rains may cause wash off. Field station research on the same variety suggested that yields increased linearly with up to 6 ounces per mound. There is a potential export market for any increase in production.

2. Make a complete list of treatment options, and select an appropriate subset.

3. Be prepared to discuss your answer.

SELECTING A TREATMENT SUBSET

Summary Description: This exercise requires some simple economic analysis.

OBJECTIVE:

After completing this activity the participant will be able to:

1. Select a treatment subset from the list appropriate for a refinement trial.

2. Conduct single ex ante economic analysis.

TIME:

MATERIALS:

1. Participant instruction sheet provided in manual.

INSTRUCTIONS:

1. Have the participants read the background information given on their instruction sheet. The information is repeated here for your convenience.

   Background information: Earlier exploratory experiments showed that yields of eggplant could be substantially increased, and the bearing period extended, by weeding 3 times, compared with farmer practice which is 2 weedings. Application of about 300 lbs. per acre of sulfate of ammonia twice, at the first and second weedings, also incresed yields. Results are shown below. The price of eggplant is between 19 and 23 ct. per lb. Sulphate of ammonia costs $45 per 100 lbs. There does not appear to be any additional labor costs.

Weed control and fertilizer treatment	Yield (llb/ac)	% increase in yield
Weeded at 4, 8 & 12 weeks:		
no fertilizer	12,800	133
+ fertilizer	16,750	174
Weeded at 3, 8 & 13 weeks:		
no fertilizer	13,400	139
+ fertilizer	18,500	192
Weeded at 5, 9 & 13 weeks:		
no fertilizer	10,100	105
+ fertilizer	11,400	118
Farmer practice	9,650	

2. Tell them to study the suggested treatment options and to list one treatment subset including farmer practice. You

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should ask them to be prepared to defend their ideas.

PROCESSING:

1. You should have the participants discuss their conclusions with the rest of the class. Some helpful information follows:

This exercise requires some simple economic analysis: it is essential to cost the treatments and select those that give a substantial return above farmer practice. It is logical to look at the returns with eggplant at 19ct per lb only - since the higher price will simply increase net income.

At 19 ct per lb, net returns are, respectively (reading top to bottom of the table), 2432, 3048, 2546, 3380, 1919, 2031, and 1834 for farmer practice.

The selected treatment array might well be as follows:

Weeded at 4, 8 and 12 weeks:

(1) no fertilizer
(2) + fertilizer

Weeded at 3, 8 and 13 weeks:

(3) no fertilizer
(4) + fertilizer
(5) Farmer practice.

A simpler array would be to use (3), (4) and (5). Treatments (3) and (4) give the highest net returns. If trainees include (1) and (2) they should be asked to justify their inclusion.

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SELECTING A TREATMENT SUBSET

OBJECTIVE:

After completing this activity you will be able to:

1. Select a treatment subset from the list appropriate for a refinement trial.

2. Conduct single ex ante economic analysis.

INSTRUCTIONS:

1. Read the background information given below:

   Background information: Earlier exploratory experiments showed that yields of eggplant could be substantially increased, and the bearing period extended, by weeding 3 times, compared with farmer practice which is 2 weedings. Application of about 300 lbs. per acre of sulfate of ammonia twice, at the first and second weedings, also incresed yields. Results are shown below. The price of eggplant is between 19 and 23 ct. per lb. Sulphate of ammonia costs $45 per 100 lbs. There does not appear to be any additional labor costs.

Weed control and fertilizer treatment	Yield (llb/ac)	% increase in yield
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Weeded at 3, 8 & 13 weeks:		
no fertilizer	13,400	139
+ fertilizer	18,500	192
Weeded at 5, 9 & 13 weeks:		
no fertilizer	10,100	105
+ fertilizer	11,400	118
Farmer practice	9,650	

2. Study the suggested treatment options.

3. List one treatment subset including farmer practice.

4. Be prepared to defend your ideas.

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(II,C,3)

(Optional): STATISTICAL TECHNIQUES FOR SELECTING SUBSETS OF TREATMENTS

OUTLINE

1. Why Statistical Techniques for Selecting Subsets Can Be Useful
2. Structured Subsets Using Fractional Replication
3. "Take-off" Subsets
4. "Add-on" Subsets

PREREQUISITES

I	What Kind of Testing to Do
II,C,1	Defining Treatment Objectives
II,C,2	What to Consider in Selecting Subsets of Treatments
III,C,2	Ways to Combine Treatments Within Replications
V	How to Analyze and Interpret Trial Data

PARTICIPANT LEVEL

Extension subject matter specialist
Agricultural college-based adaptive research specialist

LEARNING OBJECTIVES

After completing this section the participants will be able to:

1. Construct a full set of treatment combinations for factorial experiments.

2. Identify which statistical technique is most appropriate to reduce a full set of treatment combinations for different farm household problems and different previous research information.

3. Construct and interpret tables of treatment combinations with treatment content rows and treatment effects rows for structured subsets with fractional replication, "take-off" subsets, and "add-on" subsets.

KEY POINTS

1. Statistical techniques for selecting subsets of treatments can help make more meaningful comparisons of treatments.

2. A clear treatment objectives statement is necessary to make a good decision about which statistical technique to use for selecting subsets of treatments.

3. Understanding experimental designs is necessary to use

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statistical techniques for selecting subsets of treatments correctly.

DEFINITIONS

"add-on" trial
alias
error
exploratory trials (testing)
fractional replication
interaction
refinement trials (testing)
"take-off" trial
treatment
treatment combination

DISCUSSION

1. WHY STATISTICAL TECHNIQUES FOR SELECTING SUBSETS CAN BE USEFUL

The number of treatment options for on-farm trial research is often too large. The full number may require more land than farm households are willing to use for trials, or, it may make the trial too complex. (II,C,2) discusses what to consider in selecting subsets of experimental treatments. That section presents several agronomic, economic, and social criteria to use in selecting treatment subsets. The criteria in turn are based on the treatment objectives statement (II,C,1).

What kind of treatment subset remains? What are the relationships among the treatments in the subset? There may be no special relationships among many treatments. For example, the final set of variety treatments may consist of two standard varieties and four new varieties. All four new varieties may have come from a neighboring country's breeding program. All have performed well in the neighboring country. The treatment objectives are to see how well they perform under farm conditions here in comparison with the two standard varieties, in terms of yield and taste acceptability by farm household members. The new four varieties start out on an equal basis going into the trial.

In many cases, however, the team can choose a treatment subset with a statistical relationship among the treatments. This is often possible in factorial experiments. In exploratory trials, the team may include one or two treatments to help understand the mechanisms' underlying responses. For example, in a fertilizer trial, the team may include a higher level than would be considered practical, in order to estimate a response curve. Or, it may include an additional factor (for example, P or K) to test for an interaction with the factor of primary interest (for example, N).

In refinement trials, the team may include one or two

treatments for economic comparisons. For example, in a weed control trial, the team may include a weed-free control treatment to estimate the loss of yield attributable to weeds, and to determine if weeding is economically justified. HO = weeding economically justified. HO can only be addressed if any control is compared to non-control, not if Ha = weed free. Other comparison is Ha1 = control 1 vs. Ha2 = weed free condition. Ha2 not a NORMAL option for farmer, but Ha3 = no weeding has been and will be observed. In either case, choosing a treatment subset with a statistical relationship among the treatments can help make more meaningful comparisons. This is because the team can plan the comparisons in advance. It can establish hypotheses for specific comparisons among the treatments, based on the treatment objectives statement. A goal of on-farm research is to match treatment objective statements as closely as possible to realistic farmer alternatives.

Three techniques are available for selecting subsets with statistical relationships among the treatments including structured subsets using fractional replication, "take-off subsets, and "add-on "subsets.

2. STRUCTURED SUBSETS USING FRACTIONAL REPLICATION

In a complete factorial arrangement, there is a statistical relationship among the treatments. For example, suppose the treatment objective is to determine whether combining weed, disease, and insect control together for cocoa is better than using any one of the three control practices without the other two (for example, just using weed control but no disease or insect control). This is a 2^3 factorial. A 2^3 factorial has eight possible treatment combinations:

Number	Treatment
1	No control at all
2	Only weed control
3	Only disease control
4	Only insect control
5	Weed and disease control, but no insect control
6	Weed and insect control, but no diesase control
7	Disease and insect control, but no weed control
8	All three: weed, disease, and insect control.

Logically, we might consider several possible subsets of treatment combinations:

1,2,3,4: no control vs. each type of control
2,3,4,8: each type of control vs. all controls together
5,6,7,8: revising one type of control vs. all controls together

Among these three subsets, the last two have a structured

statistical relationship based on the factorial nature of the eight treatment combinations. TABLE II,C,3.1 shows why. In TABLE II,C,3.1, each treatment combination is a column. Each effect is a row. Each treatment combination contributes to all seven effects, either positively (with a + in each effect's row), or negatively (with a "-" in each effect's row).

TABLE II,C, 3.1 Example of Structured Subset Using Fractional Replication

Factorial effect estimated	Treatment Combinations: Subset I				Subset II				Treatment content rows
	2	3	4	8	1	5	6	7	
	a	-	-	a	-	a	a	-	(weed control)
	-	b	-	b	-	b	-	b	(disease control)
	-	-	c	c	-	-			(insect control)
A (weed control)	+	-	-	+	-	+	+	-	
B (Disease control)	-	+	-	+	-	+	-	+	
C (insect control)	-	-	+	+	-	-	+	+	
AB (weed x dis. con.)	-	-	+	+	+	+	-	-	
AC (weed x insect con.)	-	+	-	+	+	-	+	-	
BC (dis. x insect con.)	+	-	-	+	+	-	-	+	
ABC (weed x dis. x insect con.)	+	+	+	+	-	-	-	-	

How are the "+" 's and "-" 's derived? For each main effect, put in a " + " if the corresponding treatment content row contains a small letter. Put in a " - " if the corresponding treatment content row contains a " - ". Consider treatment combination two. The weed control treatment content row contains a small "a," so the A effect row receives a " + ". The disease control treatment content row contains a " - " , so the B effect row also receives a " -," likewise, the C effect row receives a " - ."

The interaction effect rows are then derived by the multiplication of the main effects rows. Continuing with the treatment combination 2 example, the AB row is derived by multiplying the " + "of the A row by the " - " of the B row: a " + " times a " - " is " - ". So row AB also receives a " - ." Row AC similarly receives a " - ," While the BC row receives a " + ," (multiplying the " - "of row B by the " - " of row C gives a " + ." Finally, row ABC is obtained by multiplying the " + " of the BC row by the " + " of the A row.

The effects for all the other columns are derived the same way. Treatment combinations 2,3,4, and 8 are a structured subset. Why? The answer can be seen by comparing the rows under subset

I. The ABC row (the effect of all three control practices together) is all positive (four +'s). All the other rows have some combination of two +'s and two -'s. Thus, the ABC row is called the defining contrast for the other rows effects).

Note, however, that the A row and the BC row are the same, that is, "+, -,-,+" in subset I. This means that the two rows are equal. This means that the effects A and BC are confounded. This is called an alias. If there is a positive BC interaction (that is, disease and insect control together are better than the sum of just disease and just insect control), then the effect of A (just weed control) will appear to be greater in the analysis of results. However, if the team knows (from a prior experiment, for example) that the BC interaction is negligible, then the analysis will show the true effect of A.

Other effects in Subset I also have aliases. Row B (disease control only) and row AC (weed and insect control together) are the same (" -,+,-,+"). Likewise, the row C (insect control only) and row AB (weed and disease control together) are the same ("-,-,+,+"). So B and AC are aliases, and C and AB are aliases.

In Subset II, the aliases are not apparent. However, they are there. Look at rows A and BC. Row A is the inverse of row BC. That is, all the signs in row A are exactly reversed in row BC. This still qualifies as an alias. The only difference is that a positive BC interaction will cause the A effect to be smaller in the analysis, instead of larger as in Subset I.

Note that in Subset II, the defining contrast is also the ABC row, but with all "-'s". In both Subset I and Subset II, as the defining contrast, the ABC effect cannot be determined.

Also note that in both Subsets I and II, each individual treatment occurs twice among the four treatment combinations. For example, in Subset I, weed control occurs in treatment combinations 2 and 8. The occurrence of weed control is shown in TABLE II,C,3.1 under each treatment combination number with a small "a" in the treatment content row (the first row directly underneath the treatment number). Note that the three treatment content rows (weed control, disease control, and insect control, labelled at the right, only explain the treatment combinations. These three rows do not refer to the effects measured by the treatment combination. Those effects are the rows with "+"'s and "-"'s, labelled at the left.

Likewise, the absence of weed control occurs twice in treatment combinations 3 and 4. This is shown in TABLE II,C,3.1 with a "-" underneath the treatment combination number in the treatment content row. The other treatments (disease control, absence of disease control, pest control,and absence of insect control) also occur twice in Subset I. Thus, every individual treatment is replicated twice (occurs twice) within Subset I. This is called fractional replication. Each individual treatment is also

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replicated twice in Subset II. Thus, all six individual treatments are balanced within each structured subset.

What can the treatment combinations be tested against? If the four treatment combinations are replicated on several farms, an experimental error term can be obtained. (III,B) explains experimental error. (V,A) explains how to carry out the necessary calculations to estimate an experimental error.

3. "TAKE-OFF SUBSETS"

Another technique is called the "take-off" subset. This technique is based on single-degree-of-freedom contrasts. In the cocoa example, treatment combinations 8, 7, 6, and 5 form a "take-off" subset. Why are these treatment combinations listed in reverse order? TABLE II,C,3.2 shows why.

TABLE II,C,3.2 Example of a "Take-off" Subset

Effect estimated	Treatment Combinations				Treatment Comparisons			Treatment content rows
	8	7	6	5	8-7	8-6	8-5	
	a	-	a	a	a	(a)	(a)	(Weed control only)
	b	b	-	b	(b)	b	(b)	(Disease control only)
	c	c	c	-	(c)	(c)	c	(Insect control only)
A (weed control)	+1	-1	+1	+1	+2	0	0	
B (Disease control)	+1	+1	-1	+1	0	+2	0	
C (Insect control)	+1	+1	+1	-1	0	0	+2	
AB (weed x dis control)	+1	-1	-1	+1	+2	+2	0	
AC (weed x insect control)	+1	-1	+1	-1	+2	0	+2	
BC (dis x insect control)	+1	+1	-1	-1	0	+2	+2	
ABC (weed x disease x insect control)	+1	-1	-1	-1	+2	+2	+2	

"( )" indicates "in the presence of."

First look at the three treatment content rows directly underneath the treatment combination numbers. Compare treatment combinations 8 and 7. Treatment combination 8 has all three individual control practice treatments: weed control (shown with a small "a"), disease control (a small "b"), and insect control (a small "c"). In treatment combination 7, however, the weed control treatment combination row has a " - ". This shows the absence of weed control: one control practice has been removed. Similarly, in treatment combination 6, disease control is removed (shown with a "-" in the disease control treatment content row). In treatment combination 5, insect control is removed. Thus, in comparison with treatment combination 8, each of the other 3

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treatment combinations "takes-off" one control practice.

How do we analyze this subset? To answer this question, first look at the effects rows (A, B, C, AB, BC, and ABC) for the first four columns, (treatment combinations 5, 6, 7 and 8). Note the difference in notation in these seven rows as compared to TABLE II,C, 3.1

In TABLE II,C,3.1, the effects rows had only +'s and -'s, but in Table II,C,3.2, these are shown with +1's and -1's. The +'s and +1's mean the same thing. Likewise, the -'s and -1's mean the same thing. Note that no effects row in table 2 had all +1's or all -1's. There is no defining contrast.

Next, note that there are three new columns not found in TABLE II,C,3.1. These are the treatment comparison columns labelled "8-7", "8-6", and "8-5". How are these columns derived? Consider column "8-7" first. This column is derived by subtracting the number in each row in the 7 column from the number in each corresponding row in 8 column as follow is shown in TABLE II,C,3.3.

TABLE II,C,3.3

Effect estimated	Treatment combinations		Treatment comparison
	8	7	8-7
	a	-	a
	b	b	(b)
	c	c	(c)
A	+1	-1	+2
B	+1	+1	0
C	+1	+1	0
AB	+1	-1	+2
AC	+1	-1	+2
BC	+1	+1	0
ABC	+1	-1	+2

For example, in the first row (the A effect row) the -1 in the 7 column is subtracted from the +1 in the 8 column: +1 - (-1) = 1 + 1 = +2. The result is a +2 in the "8-7" column. In the second row (row B), the +1 in the 7 column is subtracted from a+1 in the ABC row: +1 - (+1) = 1 - 1 = 0. In the third row (row C), a -1 in the BC row is also subtracted from a-1 in the ABC row: -1- (-1) = -1 + 1 = 0, and so forth.

The numbers in treatment comparisons column "8-6" are derived the same way by subtracting the number in each row of column 6 from the corresponding number in each row of column 8. Finally, column 8-5 is derived by subtracting the number in column 5 from the corresponding number in column 8.

Now look at the three treatment content rows. Let's take the "8-7" column first. For weed control, column 8 has weed control,

represented by a small "a." Column 7 does not, and so the weed control row contains a " -." However, both columns 8 and 7 have disease control, shown by a small "b" in each. Both columns 8 and 7 also have insect control, shown by a small "c" in each. Thus, the difference between column 8 and column 7 is the effect of weed control in the presence of disease and insect control. For this reason, the weed control row for column "8-7" has a small "a," while the disease control row has a small "(b)", where "( )" indicates "in the presence of." Similarly, the insect control row has a small "(c)" again to indicate "in the presence of". The treatment content rows thus tell us how we define the effect.

Columns "8-6" and "8-5" are similar to column "8-7". Each shows the effect of another control practice in the presence of the other two control practices.

What do the effects rows mean for the three new columns? Take column "8-7" first. Notice that only one main effect is positive. This is the A effect: the weed control effect. The B effect (disease control) row and the C effect (insect control) row are both 0. However, the AB, AC, and ABC rows are also positive. Those interactions will magnify the effect of A if they are positive, or diminish the effect of A if they are negative. They are like aliases. If we know (from a prior experiment, for example) that these interactions are negligible, however, then the AB, AC, and ABC effects will disappear. In that case, the "8-7" column can be used to test for the A effect, weed control.

Note that the main effects row for A (weed control) simply indicates that the effect is positive. It does not say how the effect is defined. It is the treatment content row that tells us how the A effect is defined: in the presence of disease control and insect control.

The test using the derived treatment comparison "8-7" requires an appropriate error term. To obtain a measure of error, the team needs to replicate the four treatment combinations more than once.

Similarly, if the AB, BC, and ABC interactions are negligible, the "8-6" column can be used to test for the B effect, disease control, in the presence of weed and insect control. Also if the AC, BC, and ABC interactions are negligible, the "8-5" column can be used to test for the C effect, insect control, in the presence of weed and disease control.

The "take-off" subset can be used to test different hypotheses than the structured subset. For example, treatment comparison "8-7" can be to test the following hypothesis: Does it matter whether we practice weed control or not, when we do practice disease and insect control? Treatment comparisons "8-6"

and "8-5" can test similar hypotheses about disease and insect control when we use the other two control practices. On the other hand, the structured subset asked: Is any single control practice better than all three combined together?

Which hypotheses do we want to test? That depends on the treatment objectives statement. And remember that the treatment objectives statement simply tells us what farm households want to know. Recall that the treatment objectives statement is not only based on the results of diagnosis, but is then checked again with farm household members.

Suppose farm households in one domain are not using any of the control practices, but are facing low yields and low returns due to weeds, diseases, and pests. Station research has shown promising yield increases with all three control practices combined. However, farm households have little free cash to buy fungicides or insecticides. In addition, labor for weeding cocoa competes with off-farm employment. The question that farm households want an answer to is: into which type of pest control should scarce resources be put weed control, disease control, or insect control? In this case, the structured subset is a powerful technique to help answer the farm households' question. It tests the hypothesis they are interested in with only four treatments, instead of eight.

Now consider a different situation. Suppose another domain consists of farm households who have been using all three control practices. However, these farm households are now facing a cost-price squeeze. Cocoa prices are not increasing as fast as the cost of fungicides and insecticides. They have sometimes used hired labor for weeding, combined with family labor, but labor costs are also rising. The question that these farm households want an answer to is different. These farm households want to know: What is the effect of stopping one practice? Which one would hurt yields and returns the least if we eliminated it? To answer their question, the "take-off" subset is better.

4. "ADD-ON" SUBSETS

The third technique is called the "add-on" subset. It , like the "take-off" subset, is based on single-degree-of-freedom contrasts. However, as the name suggests, it proceeds in a reverse manner. Let's use the cocoa example again. Treatment combinations 1, 3, 7, and 8 are an "add-on" subset. TABLE II,C,3.4 shows why.

Look at the three treatment combination rows. Compare treatment combinations 3 and 1. Treatment combination 3 has added disease control which is shown with a small "b". Both treatment combinations 3 and 1 lack weed and insect control. These are shown with a " -." Thus, first disease control is added without any other control practices.

Next compare treatment combinations 7 and 3. They are the same except for insect control. Treatment combination 7 has added insect control, shown with a small "c." Both treatment combinations 7 and 3 have disease control (the small "b" in each), but neither have weed control (the "-" in each). Now insect control has been added in the presence of the first control (disease control), but without the final pest control (weeds).

TABLE II,C,3.4. Example of an "Add-on" Subset

Effect Estimated	Treatment combinations				Treatment comparisons				Treatment content rows
	1	3	7	8	3-1	7-3	7-1	8-7	
	-	-	-	a	-	-	-	a	(weed control)
	-	b	b	b	b	(b)	b	(b)	(disease control)
	-	-	c	c	-	c	c	(c)	(insect control)
A (weed control)	-1	-1	-1	+1	0	0	0	+2	
B (disease control)	-1	+1	+1	+1	+2	0	+2	0	
C (insect control)	-1	-1	+1	+1	0	+2	+2	0	
AB (weed x dis. control)	+1	-1	-1	+1	-2	0	-2	+2	
AC (weed x insect control)	+1	+1	-1	+1	0	-2	-2	+2	
BC (dis. x insect control)	+1	-1	+1	+1	-2	+2	0	0	
ABC (weed x disease x insect control)	-1	+1	-1	+1	+2	-2	0	+2	

Finally, compare treatment combinations 8 and 7. They are the same except for weed control. Treatment combination 8 has added weed control, with a small "a," where treatment combination 7 had a " -." Both treatment combinations 8 and 7 have disease and insect control, shown with a small "b" and small "c" in each column. Thus, weed control is added in the presence of the first two controls, disease and insect control.

In moving from treatment combination 1 to treatment combination 8, we have "added-on" new control practices one-by-one. How do we analyze these four treatment combinations? Again, we do not have a defining contrast. Each effects row under treatment combinations 1, 3 ,7, and 8 is a mixture of "+"'s and "-"'s.

In TABLE II,C,3.4 we also have new columns not found in TABLE II,C,3.1. These are the treatment comparison columns labelled "3-1", "7-3", "7-1", and "8-7". Like the new columns in TABLE II,C,3.2 these new columns in TABLE II,C,3.4 are also derived from the original treatment combinations. Column "3-1" is derived by subtracting the number in each row in the 1 column from the number in each corresponding row in the 3 column. For example, in the first row (the A effect row), the -1 in the 1 column is subtracted from the -1 in the 3 column. This gives 0

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ACTIVITY TWO TRAINERS' NOTES
IDENTIFYING CHANGES IN AND "ADD-ON" SUBSET

OBJECTIVE:

After completing this activity the participants will be able to:

1. Identify changes in an "add-on" subset needed, given that trial results in the previous year suggest a different pathway than that in the text example.

2. Construct a table of treatment combinations and treatment comparisons with treatment content and effects rows.

INSTRUCTIONS:

1. Have the participants read the background information given below (included in trainee manual) and study the suggested treatment options.

2. Explain the tasks to the participants:

   a. Develop an appropriate "add-on" subset.

   b. Make a table of treatment combinations and treatment comparisons for the treatment content and effects rows.

   c. Explain your table and why you choose the "add-on" subset that you present in your table.

Background Information

The domain is the same as in the "add-on" subset of TABLE II,C,3.4 in the text. That is, farm households are not using any of the 3 control practices for cocoa, but they are facing both low yields and returns due to weeds, diseases, and insects. A trial was done in year 1 using the structured subset, but the results were as follows: Greatest economic returns came from insect contol, followed by weed control. Disease control gave only a marginal increase in returns.

PROCESSING:

1. Use the following information to help discussion of the participants solutions.

These year 1 trial results suggest a different pathway: add insect control first, add weed control later when farm households have adequate resources, and only add disease control under the least limiting resource conditions. The "add-on" subset for this pathway would be:

1. No control practices
2. Pest control only

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3. Pest and weed control
4. Pest, weed, and disease control.

The table of treatment combinations and treatment comparisons with treatment content and effects rows could be:

TABLE II,C,3.8.

Effect measured	Treatment combinations				Treatment comparison				Treatment content rows
	1	4	6	8	4-1	6-4	6-1	8-6	
	-	-	a	a	-	a	a	(a)	(weed control)
	-	-	-	b	-	-	-	b	(disease cont)
	-	c	c	c	c	(c)	c	(c)	(insect cont)
A (weed control)	-1	-1	+1	+1	0	+2	+2	0	
B (disease cont)	-1	-1	-1	+1	0	0	0	+2	
C (insect control)	-1	+1	+1	+1	+2	0	+2	0	
AB (weed x disease control)	+1	+1	-1	+1	0	-2	-2	+2	
AC (weed x insect control)	+1	-1	+1	+1	-2	+2	0	0	
BC(disease x insect	+1	-1	-1	+1	-2	0	-2	+2	
ABC(weed x disease x insect control)	-1	+1	-1	+1	+2	-2	0	+2	

The numbers used to label the treatment combinations are the same as those used in TABLES II,C,3.1, II,C,3.2, and II,C,3.4. If participants develop their tables using different numbers, then point out what the original numbers would be, and compare with TABLES II,C,3.1 and II,C,3.4.

Participants should also note the interactions for each treatment comparison that must be negligible to use the treatment comparison for a test of a main effect. Participants should also note treatment comparison "6-1". This treatment comparison tests for the main effects of weed control (main effect A) and insect control (main effect C) together. The AC interaction, on the other hand, is 0. Participants should distinguish between these two main effects together (the A and C rows, each with positive signs), and their interaction (the AC row, with a 0).

Participants should also explain how each main effect is defined in the treatment comparisons. In treatment comparison "4-1", insect control (main effect C) is tested in the absence of weed and disease control. In treatment comparison "6-4", weed control (the A main effect) is tested in the presence of insect control but in the absence of disease control. In treatment comparison "6-1", weed and insect control (the A and C main effects) are tested in the absence of disease control. Finally, in treatment comparison "8-6", disease control (the B main effect) is tested in the presence of weed and insect control.

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ACTIVITY THREE TRAINERS' NOTES
SELECTING TREATMENT SUBSETS

OBJECTIVES:

After completing this activity the participants will be able to:

1. Use statistical techniques to select an appropriate treatment subset.

2. Construct a table of treatment combinations with treatment content and effects rows.

INSTRUCTIONS:

1. Have the participans read the background information below and study the suggested treatment options (this appears on their instruction sheet).

2. Explain the tasks to the participants

   a. Make a full set of treatment combinations first. Then choose an appropriate subset using statistical techniques.

   b. Make a table of treatment combinations with treatment content and effects rows for your subset.

   c. Present the full set of treatment combinations, indicate what type of subset you have chosen and why you chose that subset, present the subset, and explain your table for the subset.

Background Information:

Farm households currently grow mungbean in a random planting pattern. Their predominant variety is susceptible to cercospera leafspot. A new variety with resistance to cercospera is available from the central breeding station. Station research has shown a positive response to a light side dressing of N at hovering when the plant's demand for N apparently may exceed the ability of the nodules to fix N biologically from the soil.

The random planting pattern makes weeding and early spraying for beanfly difficult, but it does allow broadcasting. Broadcasting can be done sooner after rains during dry spells in August in the rainy season. This is a traditional planting time for mungbean. Farm households are often short of cash in August, because their rice crop has not yet been harvested. However, if they buy urea for rice which is (planted in June or July), some extra may be left to be used for their small areas of mungbean at flowering time in September.

Station research did not show any interaction between the new variety and improved plant spacing in rows, or between the new

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variety and sidedressing.

PROCESSING:

1. The following information will help in discussing the paricipants solutions.

The background information suggests that substituting the new variety for the old would be the easiest change to make. Farm households might not be able to buy fertilizer, but the information suggests they may already have some N on hand for the light sidedressing. The information also implies that mungbean area is less than rice area. On the other hand, a change to row planting might change planting time options. Thus, suggested pathways for improvement of mungbean production would be either:

(1) variety - sidedressing - plant spacing
or
(2) sidedressing - variety - plant spacing

Participants might argue for either (1) or (2), but the information does not suggest starting with spacing.

A full factorial set of treatment combinations would be a 2^3 factorial, with the following 8 treatment combinations:

TABLE II,C,3.9

Treatment combination number	Treatment combination		
	Variety	Side dressing	Plant spacing
1	Farmer	No	Random
2	Resistant	No	Random
3	Farmer	Yes	Random
4	Farmer	No	In rows
5	Resistant	Yes	Random
6	Resistant	No	In rows
7	Farmer	Yes	In rows
8	Resistant	Yes	In rows

Rather than put all eight treatments out, however, participants could use an "add-on" subset for either pathway (1) or pathway (2).

For pathway (1), the treatment combinations would be 1, 2, 5, and 8. The table of treatment combinations and treatment comparisons, with treatment content and effects rows, would be:

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TABLE II,C,3.10

Effect estimated		Treatment combinations				Treatment comparisons				Treatment content rows
		1	2	5	8	2-1	5-2	5-1	8-5	
		-	a	a	a	a	(a)	a	(a)	(variety)
		-	-	b	b	-	b	b	(b)	(sidedressing)
		-	-	-	c	-	-	-	c	(plant spacing)
A	variety	-1	+1	+1	+1	+2	0	+2	0	
B	sidedressing	-1	-1	+1	+1	0	+2	+2	0	
C	plant spacing	-1	-1	-1	+1	0	0	0	+2	
AB	variety x sidedressing	+1	-1	+1	+1	-2	+2	0	0	
AC	variety x plant spacing	+1	-1	-1	+1	-2	0	-2	+2	
BC	sidedressing x plant spacing	+1	+1	-1	+1	0	-2	-2	+2	
ABC	variety x sidedressing x plant spacing	-1	+1	-1	+1	+2	-2	0	+2	

Particpants should note the interactions for each treatment comparison that must be negligible to use the treatment comparison to test a main effect. Station research indicates that the AC and AB interactions can be ignored. We do not have information, however, on the BC or ABC interactions.

Participants should also note treatment comparison "5-1". This treatment comparison tests for the main effects of variety (main effect A) and sidedressing (main effect B) together. The AB interaction, on the other hand, is 0. Participants should distinguish between these two main effects together (the A and B rows, each with positive signs) and their interaction (the AB row, with a 0).

Participants should also explain how each main effect is defined in the treatment comparisons. In treatment comparison "2-1", variety (main effect A) is tested in the the absence of siddressing and the absence of plant spacing in rows (that is, with random plant spacing). In treatment comparison "5-2", sidedressing (main effect B) is tested in the presence of the new variety but in the absence of plant spacing in rows (that is, with random spacing). In treatment comparison "5-1", variety and sidedressing (the A and B main effects) are tested in the absence of plant spacing in rows (that is, with random spacing). Finally, in treatment comparison "8-5", plant spacing (main effect C) is tested in the presence of the new variety and

a1

sidedressing.

If participants choose pathway (2), the treatment combinations would be 1, 3, 5, and 8. Note that the only difference is that treatment combination 3 substitutes for treatment combination 2. Pathway (2) adds sidedressing first, and then adds the new variety second. Pathway (1) was just the reverse: it added the resistant variety first, then sidedressing. Both pathways (1) and (2) thus need treatment combination 5 as an intermediate step before adding row plant spacing last.

The table of treatment combinations and treatment comparisons, with treatment content and effects rows, would be:

TABLE II,C,3.11

Effect estimated	Treatment combinations			Treatment comparisons				Treatment content rows
	1	3	5	8	3–1	5–3	5–1	8–5	
	–	–	a	a	–	a	a	(a)	(variety)
	–	b	b	b	b	(b)	b	(b)	(sidedressing)
	–	–	–	c	–	–	–	c	(plantspacing)
A (variety)		–1	–1	+1	+1	0	+2	+2	0
B (sidedressing)		–1	+1	+1	+1	+2	0	+2	0
C (plant spacing)		–1	–1	–1	+1	0	0	0	+2
AB (variety x sidedressing)		+1	–1	+1	+1	–2	+2	0	0
AC (variety x plant spacing)		+1	+1	–1	+1	0	–2	–2	+2
BC (sidedressing x plant spacing)		+1	–1	–1	+1	–2	0	–2	+2
ABC (variety x sidedressing x plant spacing)		–1	+1	–1	+1	+2	–2	0	+2

Participants should note the interactions for each treatment comparison that must be negligible to use the treatment comparison to test a main effect. Participants should also note treatment comparison "5–"1, and distinguish between the two main effects together (the A and B rows, with positive signs) and their interaction (the AB row, with a 0).

Participants should also explain how each main effect is defined in the treatment comparisons. In treatment comparison

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"3-"1, sidedressing (the B main effect) is tested in the absence of the new variety and the absence of plant spacing in rows (that is, with random plant spacing). In treatment comparison "5-3", variety (the A main effect) is tested in the presence of sidedressing but in the absence of plant spacing in rows (that is, with random plant spacing). The effects in treatment comparisons "5-1" and "8-5" are identical in definition as in pathway (1).

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ACTIVITY ONE TRAINERS' NOTES

SELECTING SUBSET OF TREATMENTS FOR AN ON-FARM EXPLORATORY TRIAL

OBJECTIVE:

After completing this activity the participants will be able to:

1. Select an appropriate subset of treatments including an appropriate control treatment, for an exploratory on-farm trial.

TIME:

MATERIALS:

INSTRUCTIONS:

1. Have the participant read the background information provided below (it appears in their manual).

2. Ask them to list those varietites you would include in an exploratory trial. What would they use as control (or check) treatment(s)?

Background Information: Tomatoes are difficult to produce during the wet season in the humid tropics. Fusarium wilt is serious, and also bacterial wilt and late blight. There are three races of Fusarium - races 1, 2 and 3 (although the presence of the latter has not been confirmed, it is suspected). High night temperature tolerance is also needed, since many varieties show flower drop if night temperatures are high. Local preference is for a medium-sized fruit. The following information has been obtained from seed catalogues. Select varieties for an exploratory trial.

TABLE II,C,4.1

Variety	Fusarium resistance	Bact. wilt resistance	Late blight resistance	High night temp. resistance	Fruit size
A	1,2	No	No	Yes	Large
B	1,2,3	Yes	Yes	Yes	V.Large
C	1	Yes	Yes	Yes	Medium
D	1,2	Yes	No	Yes	Medium
E	1,2	Yes	Yes	No	Large
F	1,2,3	Yes	No	No	Medium
G	1,2	No	Yes	Yes	Medium
H	1,2,3	Yes	No	Moderate	Medium
I	1,2	Yes	Yes	Yes	Large
J	1	Yes	No	Moderate	Medium
K	1,2 (3?)	Yes	Yes	Yes	Medium
L	1,2	No	Yes	Yes	Medium
M	1,2 (3)?	Yes	Yes	Yes	Small
N	1,2	No	Yes	Moderate	Meduim
O	1,2	No	No	Yes	Large
P	1,2,3	Yes	Yes	Yes	Medium

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PROCESSING:

1. In discussing participants' solutions the following information will help:

   Clearly the varieties chosen should be those showing maximum disease resistance and high night temperature tolerance with the preferred fruit size.

   Only one satifies all these criteria - variety P. But in an exploratory experiment we should include a few others: K, H and l perhaps. Perhaps we should try B, in spite of its very large fruit size, and possibly M? As an average farmer control, the most common locally grown variety may be included. An individual farmer control would also be included at each farm, but not used in statistical analysis. The team might also include a recommended practice control if that variety is not the most common locally grown variety.

ACTIVITY TWO TRAINERS' NOTES
SELECTING SUBSET OF TREATMENTS FOR AN ON-FARM VALIDATION TRIAL

OBJECTIVE:

After completing this activity the participants will be able to:

1. Select a subset of treatments including an appropriate control treatment for an on-farm validation trial.

TIME:

MATERIALS:

INSTRUCTIONS:

1. Have the participants read the background information given below (it is provided in the trainee instructions).

2. Ask them to select those treatments you feel should be inlcuded in a validation experiment. What control (or check) treatment (s) would you propose? Justify your selection.

Background Information: Farmers presently use hand weeding for pigeon peas, but the crop usually requires three weedings at intervals of about 6 weeks. The total labor required is 15 - 18 days per acre. If labor is hired, the cost is $20 per day. Many farmers would use family labor, however, with no cash outlay required. Exploratory research has shown that five herbicides, applied pre-emergence, give-up to four months of good weed control, reducing the hand weeding needed.

Select an appropriate array from the following list, based on the following data, and suggest appropriate control treatments.

Herbicide	Unit price	Rate/acre	Hand weeding needed (days)
Anti	$20	5 lb.	4 - 5
Blast	$80	3 pints	5 - 6
Confuse	$22	5 pints	2 - 3
Destroy	$100	2 1/2 lb.	4 - 6
Electric	$40	2 lbs.	5 - 6

PROCESSING:

1. The following information will be useful in discussing the participants' solutions.

   Some "economic analysis" is needed: this should include labor costs and material costs. It is enough to use the maximum labor cost (i.e. the larger of the range given for handweeding).

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Handweeding costs (up to) $360 per acre: blast and destroy cost about the same in total cost. Confuse has the lowest total cost, but not the lowest material cost. It does not require the least weeding, however:

Herbicide	Materials Cost	Labor Cost	Total Cost
Anti	$100	100	200
Blast	$240	120	360
Confuse	$110	60	170
Destroy	$250	120	370
Electric	$80	120	200
Farmer practice	-	360	360

in the "3-1" column to the right. Likewise, in the second row (the B effect row), the -1 in the 1 column is subtracted from the +1 in the 3 column: +1 - (-1) = 1 + 1 = 2. This gives +2 in the "3-1" column. The numbers in the remaining rows in the "3-1" column are derived in the same way.

The numbers in the "7-3" column are derived by subtracting the numbers in each row of the 3 column from the corresponding numbers in each row of the 7 column. Column "7-1" is derived by subtracting column 1 from column 7. Column "8-7" is derived by subtracting column 7 from column 8.

Now look at the treatment content rows. Column "3-1" has a small "b" for disease control, and a "-" for weed and insect control. If we compare column 3 and 1, both have a " -" for weed and insect control. Column 3 has a "b" for disease control, while column 1 does not. Disease control is the only difference between columns 3 and 1. Thus column 3 - 1 gives the effect of disease control <u>in the absence of weed and insect control</u>.

Now compare columns 7 and 3. Column 7 has added insect control. Both columns have disease control, shown by a small "b". Neither have weed control. The difference is insect control in column 7, but not in column 3. So column "7-3" gives the effect of insect control in the presence of disease control (small "(b)" and in the absence of weed control (a " -").

We can also compare column 7 with column 1. Now both disease and insect control are added. Neither have weed control. So column 7-1 shows the effect of disease and pest control together in the absence of weed control. Note that this effect is not the interaction of disease and insect control. It is simply the additive effect of the two control practices used together.

Finally, compare columns 8 and 7. Both have disease and pest control. Column 8 adds weed control. So column 8 shows the effect of weed control in the presence of disease control (shown with a small "(b)" and pest control (shown with a small "(c)".

In each of the derived columns, the main effect measured has a +2. In column "3-1", the B effect (disease control) has a +2, but the A effect (weed control) and the C effect (pest control) have 0's. However, several interactions are either positive or negative. These interactions are like aliases. However, if we know (from a prior experiment, for example) that these interactions are negligible, then the interaction effects (AB, BC, and ABC for column "3-1") will disappear. Column "3-1" can then be used to test for the B effect, disease control.

Similarly, the "7-" column can be used to test for the C effect, if the AC, BC, and ABC interactions are negligible. Note that the treatment content rows tell us how the C effect is defined: in the presence of disease control, but in the absence

of weed control.

Likewise, the "7-1" column can be used to test for the B and C effects together, if the AB and the AC interactions are negligible. Note that the BC interaction is 0, as is the ABC interaction. The effects rows thus show the difference between the sum of two effects CB and C rows (both positive) and the interaction of two effects (BC row equal to 0).

Finally, the "8-7" column can be used to test for the A effect, if the AB, AC, and ABC interactions are negligible. Note again that the treatment content rows tell us how the A main effect is defined for their treatment comparison: in the presence of both disease control and pest control.

Under what conditions would the "add-on" subset be useful? Let us consider the same domain for which we used the structured subset. This was the domain where farm households were not using any of the three control practices, but were facing low yields and returns due to weeds, diseases, or insects. Suppose also that in the first year we had done trials using the structured subset. The results showed greatest economic returns when disease control was added. Insect control was second. Weed control gave only marginal increses in returns.

These results suggest a pathway of improvement: first add disease control, then add insect control when farm household resources permit. Save weed control for last. Now one question is: how can we test this pathway? The "add-on" subset is a way to test this pathway, again using only four treatments, instead of eight.

Each of these three examples, the structured subset, the "take-off" subset, and the "add-on" subset, are designed to answer different questions. Each is efficient, because they reduce the number of treatments needed to answer specific questions. To choose the correct tool, we must know what questions need to be answered. The structured subset, for example, will not help us test the pathway of adding new control practices of the "add-on" subset. On the other hand, the "add-on" subset starting with disease control would not be appropriate if the first year's trial results had suggested greatest returns to insect control, rather than disease control. Thus, the statistical techniques for selecting subsets of treatments are only tools to assist you in doing a job more efficiently. They help us to think more carefully about what the job to do is. But in the end, the team must choose the right statistical tool, to do whatever job farm households identify as their primary need.

OTHER SECTIONS THAT CAN HELP

III,B	What Designs Can Do
V,A	How to Analyze and Evaluate Trial Data

ACTIVITIES

ACTIVITY ONE: USING STATISTICS TO SELECT TREATMENT SUBSETS

ACTIVITY TWO: IDENTIFYING CHANGES IN AN "ADD-ON" SUBSET

ACTIVITY THREE: SELECTING TREATMENT SUBSETS

USING STATISTICS TO SELECT TREATMENT SUBSETS

OBJECTIVES:

After completing this exercise the participants will be able to:

1. Use statistical techniques to select an appropriate treatment subset.

2. Construct a table of treatment combinations with treatment content and effects rows.

INSTRUCTIONS:

1. Have the participants read the background information given below and study the suggested treatment options.

2. Explain the task. Tell the participants to make a full set of treatment combinations first. Then choose an appropriate treatment subset using statistical techniques. Make a table of treatment combinations with treatment content and effects rows for the subset. Present the full set of treatment combinations, indicate what type of subset you have chosen, and why you chose that subset. Present the subset. Explain their table.

   Background Information

   Farm households have traditionally used the fertilizer 10-10-10 at the rate of 1,000 kg/ha for basal fertilization of tomato. They usually sidedress once with urea at 4 weeks. Besides 10-10-10 and urea, 0-20-20 and ordinary super phosphate are also available locally. Some farm households wonder if they could just get by with 0-20-20 for basal fertilization instead of 10-10-10. Using 0-20-20 at half the rate of 10-10-10 would be cheaper.

   On the other hand, a recent soil mapping survey has shown that potassium levels are generally high, while phosphorus levels are low. The team wonders if perhaps potassium could be eliminated from basal fertilization, instead of N. Station research has shown a response to N applied basally, but not to K. Results with P have been mixed.

PROCESSING:

1. The following information will be helpful in discussing the participants solutions.

   The background information suggests there is value in looking at all three fertilizer elements. Farmer practice already uses all three, but farm households are interested in

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possibly eliminating N. The team thinks perhaps K could better be eliminated.

A complete factorial would be a 2^3 , with eight treatment combinations as follows:

TABLE II,C,3.5

Treatment combination Number	Treatment combination content		
	N	P	K
1	0	0	0
2	100 kg/ha	0	0
3	0	100 kg/ha	0
4	0	0	100 kg/ha
5	100 kg/ha	100 kg/ha	0
6	100 kg/ha	0	100 kg/ha
7	0	100 kg/ha	100 kg/ha
8	100 kg/ha	100 kg/ha	100 kg/ha

Note that these rates are based on 1,000 kg/ha of 10-10-10. Rather than use all 8 treatments, the background information suggests a "take-off" subset. Treatments could be:

8. 10-10-10 at 1,000 kg/ha
7. 0-20-20- at 500 kg/ha
5. Urea (45-0-0) at 222 kg/ha and super phosphate (0-22.5-0) at 444 kg/ha.
6. Urea (45-0-0) at 222 kg/ha and muriate of potash (0-0-60) at 167 kg/ha.

Note that these carriers each supply 100 kg/ha of the respective elements. The treatment combinations table, with treatment content and effects rows, would be:

TABLE II,C,3.6

Effect measured	Treatment combinations				Treatment comparisons			Tr.content rows
	1	2	3	4	1-2	1-3	1-4	
	n	-	n	n	n	(n)	(n)	N
	p	p	p	-	(p)	(p)	p	P
	k	k	-	k	(k)	k	(k)	K
N	+1	-1	+1	+1	+2	0	0	
P	+1	+1	+1	-1	0	0	+2	
K	+1	+1	-1	+1	0	+2	0	
NP	+1	-1	+1	-1	+2	0	+2	
NK	+1	-1	-1	+1	+2	+2	0	
PK	+1	+1	-1	-1	0	+2	+2	
NPK	+1	-1	-1	-1	+2	+2	+2	

The above is a complete "take-off" set, analogous to TABLE II,C,3.2 in the text. However, some participants might argue that treatment combination 4 is not needed, because the background information does not suggest that P could be

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eliminated. It also does not indicate that muriate of potash is available locally. A partial "take-off" subset would also be possible. It would include only treatment combinations 1, 2, 3, and derived treatment comparisons 1-2, and 1-3.

Some participants might suggest a different treatment combination to substitute for treatment 4: only P. The following table could result:

TABLE II,C,3.7

Effect measured	Treatment combinations 1	2	3	5	Treatment comparisons 1-2	1-3	3-5	Tr content rows
	n	-	n	-	n	(n)	n	N
	p	p	p	p	(p)	(p)	(p)	P
	k	k	-	-	(k)	k	-	K
N	+	-	+	-	+2	0	+2	
P	+	+	+	+	0	0	0	
K	+	+	-	-	0	+2	0	
NP	+	-	+	-	+2	0	+2	
NK	+	-	-	+	+2	+2	+2	
PK	+	+	-	-	0	+2	0	
NPK	+	-	-	+	+2	+2	+2	

Note that this table tests N in two different ways:

1-2: N in the presence of P and K
3-5: N in the presence of P but without K.

In both tables, participants should also note the interactions that must be negligible for the main effects tests to be valid.

ACTIVITY ONE TRAINEE INSTRUCTIONS
USING STATISTICS TO SELECT TREATMENT SUBSETS

OBJECTIVES:

After completing this exercise you will be able to:

1. To use statistical techniques to select an appropriate treatment subset.

2. To construct and interpret a table of treatment combinations with treatment content and effects rows.

INSTRUCTIONS:

1. Read the background information given below and study the suggested treatment options.

2. Make a full set of treatment combinations first.

3. Choose an appropriate treatment subset using statistical techniques.

4. Make a table of treatment combinations with treatment content and effects rows for your subset.

5. Present the full set of treatment combinations, indicate what type of subset you have chosen, and why you chose that subset.

6. Present the subset and explain your table.

Background information

Farm households have traditionally used the fertilizer 10-10-10 at the rate of 1,000 kg/ha for basal fertilization of tomato. They usually sidedress once with urea at 4 weeks. Besides 10-10-10 and urea, 0-20-20 and ordinary super phosphate are also available locally. Some farm households wonder if they could just get by with 0-20-20 for basal fertilization instead of 10-10-10. Using 0-20-20 at half the rate of 10-10-10 would be cheaper.

On the other hand, a recent soil mapping survey has shown that potassium levels are generally high, while phosphorus levels are low. The team wonders if perhaps potassium could be eliminated from basal fertilization, instead of N. Station research has shown a response to N applied basally, but not to K. Results with P have been mixed.

ACTIVITY TWO TRAINEE INSTRUCTIONS
IDENTIFYING CHANGES IN AND "ADD-ON" SUBSET

OBJECTIVE:

After completing this activity you will be able to:

1. Identify changes in an "add-on" subset needed, given that trial results in the previous year suggest a different pathway than that in the text example.

2. Construct a table of treatment combinations and treatment comparisons with treatment content and effects rows.

INSTRUCTIONS:

1. Read the background information given below and study the suggested treatment options.

2. Develop an appropriate "add-on" subset.

3. Make a table of treatment combinations and treatment comparisons for the treatment content and effects rows.

4. Explain your table and why you choose the "add-on" subset that you present in your table.

Background Information

The domain is the same as in the "add-on" subset of TABLE II,C,3.4 in the text. That is, farm households are not using any of these control practices for cocoa, but they are facing both low yields and returns due to weeds, diseases, and insects. A trial was done in year 1 using the structured subset, but the results were as follows: Greatest economic returns came from insect contol, followed by weed control. Disease control gave only a marginal increase in returns.

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ACTIVITY THREE TRAINEE INSTRUCTIONS
SELECTING TREATMENT SUBSETS

OBJECTIVES:

After completing this activity you will be able to:

1. Use statistical techniques to select an appropriate treatment subset.

2. Construct a table of treatment combinations with treatment content and effects rows.

INSTRUCTIONS:
1. Read the background information below and study the suggested treatment options.

2. Make a full set of treatment combinations first. Then choose an appropriate subset using statistical techniques.

3. Make a table of treatment combinations with treatment content and effects rows for your subset.

4. Present the full set of treatment combinations, indicate what type of subset you have chosen and why you chose that subset, present the subset, and explain your table for the subset.

Background Information:

Farm households currently grow mungbean in a random planting pattern. Their predominant variety is susceptible to cercospera leafspot. A new variety with resistance to cercospera is available from the central breeding station. Station research has shown a positive response to a light side dressing of N at hovering when the plant's demand for N apparently may exceed the ability of the nodules to fix N biologically from the soil.

The random planting pattern makes weeding and early spraying for beanfly difficult, but it does allow broadcasting. Broadcasting can be done sooner after rains during dry spells in August in the rainy season. This is a traditional planting time for mungbean. Farm households are often short of cash in August, because their rice crop has not yet been harvested. However, if they buy urea for rice which is (planted in June or July), some extra may be left to be used for their small areas of mungbean at flowering time in September.

Station research did not show any interaction between the new variety and improved plant spacing in rows, or between the new variety and sidedressing.

CHOOSING CONTROL TREATMENTS

OUTLINE

1. Researcher Control
2. Current Recommendation Control
3. Average Farmer Control
4. Individual Farmer Control
5. Examples of Different Types of Controls and When to Use Them

PREREQUISITES

I	What Kind of Testing to Do
II,B	What Kinds of Fields are Available for Testing
II,C.1	Defining Treatment Objectives
II,C,2	What to Consider in Selecting Subsets of Treatments

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this section participants will be able to:

1. Identify the purposes and types of trials for which researcher controls, current recommendation controls, average farmer controls, and individual farmer controls are used.

2. Distinguish the characteristics and uses of consistent and non-consistent controls.

3. Select appropriate controls for trials for specific farm household problems.

KEY POINTS

1. Different types of controls have different purposes: determining biological principles or effects, determining economic and social effects, and determining acceptability by farm household members.

2. Only consistent controls can be used in statistical analysis of treatments.

3. Non-consistent controls may be useful in determining acceptability by farm household members.

DEFINITIONS

average farmer control
block

check treatment
control treatment
domain
exploratory trials (testing)
individual farmer control
plot
recommendation domain
recommended practice control
researcher domain

DISCUSSION

There are four different types of control treatments. Each type can meet different purposes. The choice of control treatments therefore depends on their purpose. The purpose in turn depends on the treatment objectives statement.

1. RESEARCHER CONTROLS

Example of researcher controls include:

a. A treatment with no fertilizer.
b. A treatment with a very high rate of fertilizer (a probe treatment).
c. A treatment with no spraying for diseases or pests.
d. A treatment with no weeding.
e. A treatment sprayed as many times as necessary to prevent all diseases or eliminate all pests completely.
f. A treatment kept completely weed-free.

The purpose of researcher controls is for comparative analysis. Often the purpose is to determine biological response curves. For example, treatments a and b may be used to generate a fertilizer response curve as discussed in section C. 1. Or the purpose may be to estimate effects of disease, pests, or weeds on yield, or to determine economic costs and benefits of disease, pest, or weed control, such as with treatments c, d, e, or f. Plots with no pest or disease control (treatments c or d) may interfere with other plots, however, by providing a source of reinfection or reinfestation for sprayed plots.

2. CURRENT RECOMMENDATION CONTROL

This is the practice that the extension service or experiment station is currently recommending. The purpose of this control is to see if new practices tested in other treatments might substitute for the current recommendation. It is a special type of researcher control.

3. AVERAGE FARMER CONTROL

Determining the average farmer control requires a clear understanding of farming practices and their variation within the research domain of the trial. The average farmer control may be based on the most common practice or level used. For example, if

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most farm households weed twice, and only a few weed once or more then twice, then two weedings would be a good average farmer control. In other cases, the team may have to synthesize an average control. For example, an average farmer control for fertilizer may be the average of several commonly used rates.

The purpose of average farmer control is to compare experimental treatments against a common control through two or more years. Recommendation domains can be confirmed or modified by the relative consistency of this type of control.

4. INDIVIDUAL FARMER CONTROL

This is the practice of each individual collaborating farm household. Its purpose is usually for farm households to determine if a new practice is acceptable in comparison with their current practice.

Depending on the treatment objectives statement, a team may include more than one type of control. For example, it may include both an average farmer control and a recommended practice control in a refinement trial. More than one type of control is more likely to be necessary in exploratory and refinement testing than in validation testing.

The first three types of controls are all consistent. That is, they will be the same across all farms. The fourth type, individual farmer control, is not consistent. It may be different on each farm. It may, however, have demonstration value to collaborating farmers, and the researchers may learn from it. It cannot usually be included in statistical analysis because it is not consistent. There is no one treatment label that can be given for all the different individual farmer controls.

To include all these controls would increase the number of treatments, and usually also increase block size. A useful form of analysis is to calculate the variance of each treamtent to see which is the most variable between farms. Exploratory trials are likely to include a "researcher" control, an "average farmer practice" control, and an "individual farmer" control. The analysis of variance would probably exclude the latter control treatment. Its value lies in its demonstration both to the research team - to provide an insight into variations in farmer practice and farmer yield - and to individual farmers. In validation testing the only control needed is "individual farmer practice." Its variability does not matter at this stage. In intermediate refinement testing, both "average farmer practice" and "individual farmer practice" might usefully be included. If the latter proved extremely variable, it could also be excluded from the analysis of variance.

5. EXAMPLES OF DIFFERENT TYPES OF CONTOLS AND WHEN THEY ARE MOST USEFUL

a. Researcher Control

Examples include: a zero fertilizer treatment; clean weeding; weekly sprays of a pesticide; no weed control, etc. These are most useful in exploratory testing trials.

b. Current Recommendation Control

Examples include: an improved variety, a specific NPK recommendation, a particular chemical sprayed on a specified timetable, etc. These are most useful in refinement trials (site-specific or regional).

c. Average Farmer Control

Examples include: a small but specific quantity of fertilizer, a specified number and frequency of hand weedings, one or more pesticide sprays at specified times, etc. These are most useful in regional refinement testing trials.

d. Individual Farmer Control

This is whatever the farm household does. Farmers determine this, but the research team needs to carefully document this on each farm, so that it can be accurately described.

OTHER SECTIONS THAT CAN HELP

(V,B) Ways to Interpret Treatment Differences

ACTIVITIES

ACTIVITY ONE: SELECTING SUBSET OF TREATMENTS FOR AN ON-FARM EXPLORATORY TRIAL

ACTIVITY TWO: SELECTING SUBSET OF TREATMENTS FOR AN ON-FARM VALIDATION TRIAL

ACTIVITY ONE TRAINEE INSTRUCTIONS

SELECTING SUBSET OF TREATMENTS FOR AN ON-FARM EXPLORATORY TRIAL

OBJECTIVE:

After completing this activity you will be better able to:

1. Select an appropriate subset of treatments including an appropriate control treatment, for an exploratory on-farm trial.

INSTRUCTIONS:

1. Read the background information provided below.

2. List those varietites you would include in an exploratory trial. What would you use as control (or check) treatment(s)?

Background Information: Tomatoes are difficult to produce during the wet season in the humid tropics. Fusarium wilt is serious, and also bacterial wilt and late blight. There are three race of Fusarium - races 1, 2 and 3 (although the presence of the latter has not been confirmed, it is suspected). High night temperature tolerance is also needed, since many varieties show flower drop if night temperatures are high. Local preference is for a medium-sized fruit. The following information has been obtained from seed catalogues. Select varieties for an exploratory trial.

TABLE II,C,4.1

Variety	Fusarium resistance	Bact. wilt resistance	Late blight resistance	High night temp.resistance	Fruit size
A	1,2	No	No	Yes	Large
B	1,2,3	Yes	Yes	Yes	V.Large
C	1	Yes	Yes	Yes	Medium
D	1,2	Yes	No	Yes	Medium
E	1,2	Yes	Yes	No	Large
F	1,2,3	Yes	No	No	Medium
G	1,2	No	Yes	Yes	Medium
H	1,2,3	Yes	No	Moderate	Medium
I	1,2	Yes	Yes	Yes	Large
J	1	Yes	No	Moderate	Medium
K	1,2 (3?)	Yes	Yes	Yes	Medium
L	1,2	No	Yes	Yes	Medium
M	1,2 (3)?	Yes	Yes	Yes	Small
N	1,2	No	Yes	Moderate	Meduim
O	1,2	No	No	Yes	Large
P	1,2,3	Yes	Yes	Yes	Medium

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ACTIVITY TWO TRAINEE INSTRUCTIONS

SELECTING SUBSET OF TREATMENTS FOR AN ON-FARM VALIDATION TRIAL

OBJECTIVE:

After completing this activity you will be better able to:

1. Select a subset of treatments including an appropriate control treatment for an on-farm validation trial.

INSTRUCTIONS:

1. Read the background information given below.

2. Select those treatments you feel should be inlcuded in a validation experiment. What control (or check) treatment(s) would you propose? Justify your selection.

Background Information: Farmers presently use hand weeding for pigeon peas, but the crop usually requires three weedings at intervals of about six weeks. The total labor required is 15 - 18 days per acre. If labor is hired, the cost is $20 per day. Many farmers would use family labor, however, with no cash outlay required. Exploratory research has shown that five herbicides, applied pre-emergence, give-up to four months of good weed control, reducing the hand weeding needed.

Select an appropriate array from the following list, based on the following data, and suggest appropriate control treatments.

Herbicide	Unit price	Rate/acre	Hand weeding needed (days)
Anti	$20	5 lb.	4 - 5
Blast	$80	3 pints	5 - 6
Confuse	$22	5 pints	2 - 3
Destroy	$100	2 1/2 lb.	4 - 6
Electric	$40	2 lbs.	5 - 6

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SPECIFICATION OF EXPERIMENTAL TREATMENTS: FIXED AND FLUID SPECIFICATIONS

OUTLINE

1. Why Treatment Specifications Are Necessary
2. How to Write "Fixed" Treatment Specification
3. How to Write "Fluid" Treatment Specifications

PREREQUISITES

I	How Researcher-Planned Trials Change Over Time
II,B	What Kinds of Fields are Available for Testing
II,C,1	Defining Treatment Objectives
II,C,4	Selecting Control Treatments.

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this section, participants will be able to:

1. Identify reasons why treatment specifications are necessary.

2. List different types of information used in treatment specifications.

3. Write clear treatment specifications for different types of fixed and fluid treatments.

KEY POINTS

1. Treatment specifications must be consistent across plots with the same treatment if treatments are to be subjected to statistical analysis.

2. Treatment specifications must be documented when treatment specifications vary from plot to plot for the same treatment.

3. "Fluid" specifications require clear criteria to be consistent.

DEFINITIONS

average farmer control
control treatment
domain
exploratory testing
individual farmer control

intervention
plot
production problem
refinement testing
researcher control
treatments
validation testing

DISCUSSION

1. WHY TREATMENT SPECIFICATIONS ARE NECESSARY

A treatment is something the team wants to test. There are several types of treatments. The different types of treatments have different objectives and require different specifications.

a. Interventions

These are changes in production practices in a given domain. The changes are designed to solve a production problem. A production problem limits the growth or productivity of crops or animals. Because of the production problem, farm households do not achieve their goals as well as they might. For example, a farm household goal may be adequate yield of millet to insure food stock through the "hungry" season. The production problem may be decreased length of the rainy season, resulting in dry conditions during grain maturation and reduced yield. The intervention may be a new variety that matures earlier. Or, the intervention may be a new land preparation method that permits earlier planting dates and hence earlier harvest.

b. Researcher Controls

Like interventions, researcher controls are also changes in production practices. Unlike interventions, however, they are not designed directly to solve a production problem. Instead, they are designed to clarify principles or effects (either biological or economic). Indirectly,this can help solve a production problem. For example, a very early planting date may be included as a researcher control to determine what is the earliest date when planting can be done.

c. Average Farmer Controls

These represent the most common farmer practices in a domain. For example, the most common planting date may be an average farmer control.

d. Individual Farmer Controls

These represent the practice of each farm household. Continuing the same example as above, these would be the different planting dates of each individual farm household.

(II,C,4) points out the difference between the researcher controls, average farmer controls, and the individual farmer controls. This difference reflects different types of treatment objectives. The objectives of the first two types of controls are to clarify biological principles or biological, economic, and/or social effects across farms in the domain. Statistical and economic analyses are important tools used to clarify principles and effects. (III,B) explains how statistics can be useful in clarifying principles and effects. These analytical procedures require that treatments be consistent across farms. Treatment specifications are designed to insure this consistency.

The objectives of individual farmer controls differ from the other two controls. The objectives of individual farmer controls are to evaluate farm household acceptability. Statistical analysis is usually not important, consistency from farm to farm is not needed. However, the team must understand each individual farmer control. This is because the individual farmer control is the standard of judgment used by the farm household. The farm household uses this standard to judge the acceptability of the interventions. The team therefore, needs to document the specifications of each individual farmer control.

What about interventions? The objectives of interventions change from exploratory and refinement testing to validation testing. In exploratory and refinement testing, the researchers' share of trial management is greater. The objectives of interventions in exploratory and refinement testing are to assess biological, economic, and social effects of possible solutions to production problems across farms in the domain. This assessment also uses statistical and economic analysis. Like average farmer controls, these interventions are average solutions. For example, a new planting date may be tested. These average solutions must also be consistent across farms in order to use statistical and economic analysis procedures. The new planting date must be the same on all farms. Treatment specifications for interventions that are tested as average solutions are designed to insure consistency.

In validation testing, however, the objectives of interventions change. In validation testing, the farmers' share of trial management is greatest. The farmers' share includes setting the final specifications of the intervention. Each farm household modifies the intervention to suit its conditions and preferences. For example, the new planting date may vary several days, either earlier or later, on each farm. Each farm household may also modify the land preparation techniques that permit the new planting date. The result is an individual intervention on each farm. Each farm household compares their individual intervention against their own original practice, represented by their individual farmer control. On that basis each farm household evaluates the acceptability of the intervention. The team, therefore, also needs to document the specifications of each individual intervention.

2. HOW TO WRITE FIXED TREATMENT SPECIFICATIONS

Treatment specifications describe in detail each treatment. For average interventions, researcher controls, and average farmer controls in exploratory and refinement testing, the team writes the specifications in advance. The specifications must be clear and easy for farm household members to understand.

For individual interventions and individual farmer controls, the team writes the specifications based on what farm household members do. If possible, the team should observe what farm household members do. For example, the team might observe a farm household member filling a kerosene can half-full with fertilizer and spreading it on a plot. The team can then determine the rate by weighing the same volume of fertilizer later and converting that weight per plot area to a weight in kilograms per hectare.

When the team cannot observe a treatment application, then it needs to ask farm household members what they did. Again, the team may need to convert from farmer units of measurement (cans/plot, etc.) to standardized units of measurements (kg/ha, etc.)

Different treatments require different information to write clear specifications. The different types of information include:

a. Method of land preparation (hand vs. animal, flat vs. beds or ridges, height and width of beds or ridges, etc.)
b. Rates of each application (fertilizer, pesticide, etc.)
c. Timing of a single application (pre-plant vs. post-emergence, etc.)
d. Number and timing of multiple applications (pesticides, side - or top-dressings, etc.)
e. Method (s) of application (broadcast, banded, foliar in a ring, etc.)
f. Carrier or type (ammonium sulfate vs. ureao, granular vs. liquid, etc.)
g. Variety used
h. Timing of planting
i. Method of planting (broadcast, incorporation after broadcasting, depth of incorporation; row seeding, depth of planting; transplanting, age of transplants, depth of planting, use of transplant water; use of mulch, mulching material; intercropping, alley cropping)
j. Spacing (in the row and between rows; between other intercropped plants)
k. Crop care practices (staking, mulching, hilling-up, pruning, vine-turning, etc.)
l. Water management practices (timing, amount, methods of application)
m. Harvesting, storage, and processing methods (timing, frequency, implements, etc.)

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Several different types of information are combined to write each treatment specification. Examples are:

a. Intercrop Spacing Treatment

"Corn at 1 x .75 m., three seeds per hill, with cowpeas in a single row along the middle of the corn rows, with seeds 10 cm apart."

b. Fertilizer Treatment

"Four ounces of compound fertilizer (12-18-12) per mound six weeks after planting, in a one foot diameter ring around the vine" (or "one condensed milk can-full of compound fertilizer...").

c. Herbicide Treatment

"Pre-emergence application at 2.5 lb/acre active ingredient at 30-40 gal/acre, within two days of planting."

d. Pesticide Treatment

" Three sprays at one week intervals, starting at first flowering, with a spray of 10 ml per litre of Destroy."

All these are clear, and describe exactly what the treatment comprises. Diagrams may be useful. Note that these specifications avoid using the team "recommended rates", or expressing fertilizer rates on a per acre basis when mound spacing is the unit for application.

3. HOW TO WRITE "FLUID" TREATMENT SPECIFICATIONS

The specifications of some treatments depend on what happens in the field. The treatment may specify a certain level of weed control but how to achieve that level depends on weed growth. Another example would be pest threshold levels. If pests appear in numbers greater than the threshold level, then the treatment calls for a spray application. The number of sprayings will depend on how quickly pest numbers reach the threshold level after each spraying.

Such treatment specifications are called "fluid" specifications. What is "fluid" in the treatment? The treatment is not totally fluid. It must also have clear, fixed criteria to be consistent. The team must decide the criteria in advance. What is fluid is the way to achieve the criteria. The team cannot decide in advance exactly how to achieve the criteria of the treatment, because that depends on what happens in the field during the course of the experiment. The treatment does not change from farm to farm.

For example, an experiment to develop weed control

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recommendations (as opposed to screening herbicides) will require criteria which state the crop growth stage, weed growth stage, level of weeding, etc., at which the herbicides should be applied. It may be necessary to specify that hand weeding be used if the herbicides prove ineffective, or some weeds prove resistant. The crop or weed growth stage is the fixed criteria. The team decides this in advance. The timing and frequency of herbicide applications and hand weedings are the "fluid" methods. The team does not know what the timing and frequency should be in advance. That depends on how fast the weeds grow after each application. An example is: "Wham will be applied at two litres per ha. in 850 l. water, when the beans have at least two trifoliate leaves, and the weeds are not more than 10 cm. tall. The application will be repeated at the same rate as soon as weed cover is about 50% and/or weeds are about 10cm. tall. Hand weeding will be used if the control is unacceptable: if 50% cover develops within ten days of spraying".

Such specifications require researcher judgments, but clear criteria are set. It would, of course, be important in the above example to record the number of applications and dates of each, and the amount of hand weeding needed. A number of herbicide treatments might have similar specifications. The important thing is to set the critiera for the timing of sprays and for hand weeding.

OTHER SECTIONS THAT CAN HELP

II,D,3 Inputs and Calculations
III,B What Designs Can Do

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(II,D,2)

SPECIFICATION OF NON-EXPERIMENTAL VARIABLES

OUTLINE

1. What Are Non-Experimental Variables
2. Level of Mangement of Non-Experimental Variables in Exploratory and Refinement Testing
3. Variation in Non-Experimental Variables From Farm-to-Farm in Exploratory and Refinement Testing
4. Variation in Non-Experimental Variables From Plot-to-Plot in Exploratory and Refinement Testing
5. Non-Experimental Variables in Validation Testing
6. How to Write Specifications for Non-Experimental Variables

PREREQUISITES

I	How Researcher-Planned Trials Change Over Time
II,B	What Kinds of Fields are Available for Testing
II,C,1	Defining Treatment Objectives
II,D,1	Specification of Experimental Treatments: Fixed and Fluid Specifications

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this section, participants will be able to:

1. Distinguish between experimental and non-experimental specifications.

2. Select appropriate criteria for specification of non-experimental variables in exploratory, refinement, and validation testing.

3. List different types of information used in specification of non-experimental variables.

KEY POINTS

1. Specification of non-experimental variables is based on average farmer practice in exploratory and refinement testing, and based on individual farmer practice in validation testing.

2. Reducing variation in non-experimental variables from farm to farm can improve the precision of statistical analysis of differences between treatments, but analyzing variation in non-experimental variables from farm to farm can improve

understanding of acceptability of solutions among farms.

3. Non-experimental variables should be consistent from plot to plot within a block or a farm.

DEFINITIONS

block
diagnosis
domain
environment
exploratory testing
factor
interaction
intervention
plot
production problem
refinement testing
response
treatments
validation testing

DISCUSSION

1. WHAT ARE NON-EXPERIMENTAL VARIABLES

Growing a given crop or crop association requires many steps. Each step is a production practice. From land preparation, fertilization, choice of variety, and planting method, to weed and pest control, harvest, storage, and processing, there are many production practices. The same is true for raising a given type of animal. Again there are many production practices.

In a trial, the team selects only one practice, or perhaps a few practices, to test different ways of doing that particular practice. Each practice selected is a priority production problem of farm households, as identified in diagnosis. Each practice that the team varies is a factor. Each different way of doing that practice is a treatment. Another word for factor is experimental variable. The treatments of each experimental variable differ from plot to plot. (II,C,1) explains how factors and treatments differ.

To test different ways of doing one or a few practices (that is, to test different treatments for one or a few factors), farm household members and the team need to grow the crop or raise the animals. For example, suppose the team will test three treatments that are different land preparation methods to conserve moisture better after early rains. To test these three treatments, farm household members and the team must decide on all the other practices. What variety will they use? What will be spacing in the row, and between rows? How will fertilization be done? How will weeds and pests be controlled? And so on. All of these are non-experimental variables. The team does not

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vary the non-experimental variables from plot to plot.

Suppose there are three plots on each farm. Each plot gets a different land preparation, but all will have the same variety, spacing, fertilization, and weed and pest control. Suppose also that ten farms will have this trial. There are three important questions for the team and farm households to decide:

a. What should be the level of management of the non-experimental variables?
b. How much variation in non-experimental variables can there be from farm to farm?
c. How much variation in non-experimental variables can there be from plot to plot?

The answers to these three questions differ for exploratory, refinement, and validation testing.

2. LEVEL OF MANAGEMENT OF NON-EXPERIMENTAL VARIABLES IN EXPLORATORY AND REFINEMENT TESTING

The answers to the question of what the level of management of the non-experimental variables should be for exploratory and refinement testing shows an important characteristic of on-farm research. In exploratory testing, when the experiment is "researcher managed" the team may be tempted to use a high level of managment. For example, the team may be tempted to use fertilizer rates or weed, disease and pest control practices that may be "ideal" but out of reach of farmers, and far in advance of their present practices. It would be more realistic, and more likely to contribute useful information, to adopt a management level based on average farmer practice. In refinement testing in the research sequence, average farmer practice may also be the appropriate management level.

Average farmer practices are the appropriate criteria for specification of management levels of non-experimental variables in exploratory and refinement testing because the objective of on-farm research is to develop recommendations acceptable to farm households in a domain. The recommendations will be acceptable only if they are realistic. To be realistic, they must be based on what farm households are already doing. The recommendations involve changes in only a small part of what farm households are doing.

3. VARIATION IN NON-EXPERIMENTAL VARIABLES FROM FARM TO FARM IN EXPLORATORY AND REFINEMENT TESTING

Question two asks how much variation there can be from farm to farm in exploratory and refinement testing. Other related questions arise. How much should the team standardize the level of non-experimental variables? Why not let each farm household set the level of non-experimental variables independently?

Deciding the amount of acceptable variation depends on each situation. The best decision is to balance the reasons for reducing variation with the reason for leaving the existing variation. There are two reasons why the team would want to reduce variation from farm to farm and one reason why the team would want to leave some variation.

The first reason for reducing variation in non-experimental variables from farm to farm is that exploratory and refinement testing seek to develop an average solution to the priority problem of the domain. (II,D,1) explains how average solutions are tested against average farmer controls. The same principle applies for non-experimental variables in exploratory and refinement testing: average farmer practice is used. The team needs to understand the differences among farms in the domain. For example, most farms may use the same variety, but there may be a range in fertilizer rates. Average farmer practice might be the rate used on the greatest number of farms (i.e., the modal rate).

The second reason for reducing variation in non-experimental variables from farm to farm is statistical: the less the amount of variation in non-experimental variables, the more treatment differences will stand out. Exploratory and refinement testing use statistics as a tool for comparing treatments. The treatments are average solutions and average farmer controls. As (III,B) explains, statistics compares treatment differences (between average solutions and average farmer controls against natural variation. The less the variation in non-experimental variables, the less is the natural variation.

The reason for the team not to eliminate all the variation across farms even though it specifies an average farmer level of non-experimental variables is related to the concept of the recommendation domain. Recall that in initial diagnosis, the team groups similar farms into domains (Volume I:IV). However, even similar farms in the same domain are not exactly the same. The objective of on-farm research is to develop a recommendation acceptable by all the farms in the domain. This means that the recommendation must be acceptable even considering differences among farms in the domain. The team can expect some variation from farm to farm to remain and through dialog with farm household members, it can identify that variation.

Dialog with farm household members about non-experimental variables helps in other ways, too. First, it helps farm household members understand the research process. Explaining why the team proposes average level of non-experimental variables, rather than individual farmer practice, helps farm household members see how changes they make from their individual practices contribute to research for many farm households. Second, the dialog helps the team evaluate the average level of non-experimental variables it proposes. The team may find through the dialog that what the team thought was average farmer

practice is really not reasonable. The team may then revise their specifications for the average level of non-experimental variables.

The team can also test to see if the variation from farm to farm represents statistically significant differences across farms. (III,C,1) explains how to design trials to test for differences across farms. (V,A) explains how to do the calculations. The results of these analysis procedures can help the team better understand variation from farm to farm in the domain of the trials.

If the variation is not too great, there will be no significant differences across farms. This means the test indicates that all the farms belong to the same domain, in spite of the presence of some variation from farm to farm. However, if differences in non-experimental variables are very great across farms, differences across farms may be statistically significant. Also, treatments may have different responses on some farms compared with others. One variety may be better on some farms but not on others. Seed dressing of groundnut may be effective on one farm but not on others. This is an example of an interaction between an experimental variable (variety) and non-experimental variables. A useful term for non-experimental variables is environment. We sometimes call this interaction a treatment-by-environment interaction.

When differences among farms are significant, it means that different farms have different environments. Therefore, we can also call a treatment-by-environment interaction a treatment-by-farm interaction.

TABLE II,D,2.1 illustrates these points with three examples. In all three examples, there are three varieties: a farmer variety and two new varieties. In all three cases, the new variety 'Baro' yields less than the farmer variety, while the new variety 'Napintas' yields more.

In example 1, there is only a little variation in the mean yields among the farms (the right-most column). There is a little variation among farms in the yields of each variety, but 'Napintas' is better than 'Sigurado' on every farm, while 'Baro' is poorest on every farm. The team would probably recommend 'Napintas' over 'Sigurado'.

In example 2, there is a lot of variation from farm to farm. The farms appear to fall into two groups. All varieties do more poorly on farms G, H, and I than on farms J, K, and L. Perhaps farms G, H, and I suffered more from a pest infestation. The ranking of varieties is constant, however, 'Napintas'is always better than 'Sigurado', and 'Bano' is always worst. Thus there is a difference among farms due to the difference in pest environments. However, there is no treatment-by-environment interaction.

In example 3, there is again a lot of variation from farm to farm. The farms again fall into two groups. Note that there is also a treatment-by-farm interaction. 'Napintas' does not do as well as the farmer variety 'Sigurado' on farms M, N, and O. Perhaps farms M, N, and O suffered more from drought. 'Napintas' is less tolerant of drought. On farms P, Q, and R, however, 'Napintas' has higher yields than 'Sigurado'. Perhaps P, Q, and R suffered less from drought, and 'Napintas' was more efficient in taking up fertilizer under less drought-stressed conditions. Thus, there is a treatment-by-environment interaction.

TABLE II,D,2.1 Treatments Examples 1, 2, and 3

Farm		Treatments			
		Farmer variety 'Sigurado'	New variety 'Baro'	New variety 'Napintas'	Mean for each farm
Example 1	A	95	70	120	95
	B	105	80	140	108
	C	110	75	130	103
	D	95	70	115	90
	E	95	75	125	98
	F	100	80	120	100
	Mean for each variety	100	75	125	
Example 2	G	70	55	95	73
	H	75	60	100	78
	I	65	50	85	67
	J	130	85	165	127
	K	135	90	160	128
	L	125	80	145	117
	Mean for each variety	100	75	125	
Example 3	M	70	55	60	62
	N	75	60	70	68
	O	65	50	55	57
	P	130	85	190	135
	Q	135	90	195	140
	R	125	80	180	128
	Mean for each variety	100	75	125	

When there is a treatment-by-environment (treatment-by-farm) interaction, the team may partition the domain into two or more domains. The farms in each new domain will have a level of non-experimental variables different from farms in the other new domains. (V,A) and (V,B) show some techniques of analysis and interpretation that may result in the partitioning of a domain. Pages 37-43 in the CARDI manual give another example of significant differences among farms and a significant treatment-by-environment (treatment-by-farm) interaction.

4. VARIATION IN NON-EXPERIMENTAL VARIABLES FROM PLOT TO PLOT IN EXPLORATORY AND REFINEMENT TESTING

The third question which must be considered is how much variation in non-experimental variables there can be from plot to plot. In the dialog with farmers, the team needs to remember the difference between plots and blocks. (II,)B explains this difference which is very important for non-experimental variables.

Within blocks, non-experimental variables should be as consistent as possible from one plot to the next. Explaining this in the dialog with farmers will help the farmers understand the research process. The principle of consistency of non-experimental variables from plot to plot within a block is really very simple and logical. For example, farmers might plant a corner of a field with a new variety. They know the comparison with their own variety would be a poor comparison if they put less fertilizer on the new variety, or weeded their own variety but not the new variety.

Often each farmer will have more than one block. The key is to keep non-experimental variables consistent within each block. Differences in non-experimental variables from block to block within a farm can be handled by an appropriate experimental design, as explained in (III,C,1) Of course, if each farm has only one block (and farms are blocks), the non-experimental variables should be consistent from plot to plot within each farm.

5. NON-EXPERIMENTAL VARIABLES IN VALIDATION TESTING

Validation testing starts with an average solution found promising in refinement testing. Validation testing seeks to evaluate the acceptability of that average solution by farm households. (II,D,1) explains how validation testing lets each farm household modify the average solution and test it as an individual solution against their individual farmer control. This principle applies for non-experimental variables in validation testing also: individual farmer practice is used.

Since the management level in validatiton testing is individual farmer practice, it varies from farm to farm. In validation testing, each farm is usually a block, so

non-experimental variables should be consistent from plot to plot on each farm. Ususally this only involves a few (most typically two) field-sized plots on each farm.

6. HOW TO WRITE SPECIFICATIONS FOR NON-EXPERIMENTAL VARIABLES

Writing specifications for non-experimental variables is similar to writing specifications for experimental variables. It involves a mixture of writing specifications in advance, and documenting what farm households do. The degree of mixture differs in exploratory, refinement, and validation testing. In exploratory and refinement testing, the team first writes the specifications in advance, based on average farmer practice. The team then also documents variations that it observes from its specifications. In validation testing, each farm household determines its own specifications. The team needs to document these for each farm.

Documentation should be based on actual observation whenever possible. When observation is not possible, the team can ask farm household members what they did. In both cases, the team may need to convert from farmer units of measurement (for example, cans of fertilizer per plot) to standardized units of measurement (kg/ha, etc).

The types of information needed to write specifications for non-experimental variables involve all the steps in growing the crop or crops, or raising the animals, in the trial. For crops, these include:

a. Plot history: previous crops, previous fertilizer or manure application etc.
b. Land preparation methods: tools to be used, timing and sequence, bed or mound or ridge formation (including widths, heights), etc.
c. Cultivar (variety), spacing, method of establishment (i.e., direct seeding, transplanting, type and size of vegetative planting material, etc.), intercropping and intercrop spacings, time of planting, etc.
d. Fertilizer or manure applications: quantity, timing, method of application, etc.
e. Pest and disease control methods: pesticides, application rates, timing, method of application, etc.
f. Weed control methods: herbicides used (if any), timing, frequency, methods, etc.
g. Crop care practices: staking, mulching, hilling-up, pruning, vine turning, etc.
h. Water management practices: timing, amount, and methods of water application; rainfall.
i. Harvesting methods: timing, frequency, handling, etc.

These types of information are similar to those needed to write specifications for experimental variables. The difference is that only one type, or at most a few types, will be

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experimental variables. For those, there will be several specifications, one for each treatment, and each treatment on different plots. All the rest will be non-experimental variables. As this section has explained, each of the non-experimental variables will be consistent from plot to plot within blocks. The team will make decisions about consistency from farm to farm depending on trial objectives.

OTHER SECTIONS THAT CAN HELP

II,D,3	Inputs and Calculations
III,C,1	Ways to Replicate Treatments Within and Across Farms
V,A	Ways to Analyze and Interpret Data

INPUTS AND CALCULATIONS

OUTLINE

1. Deciding on Which Investment to Make
2. Calculating Costs of Single Nutrient Fertilizers
3. Other Variables to Consider
4. Calculations on Population Densities

PREREQUISITES

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this section the participants will be able to:

1. Recognize the importance of viewing treatments from the farmers point of view.

2. Understand the need to be aware of the cost of inputs to the farmer.

KEY POINTS

1. Input recommendations should not be made without being aware of and taking into consideration the actual costs of the inputs to the farmer.

2. Each environment may be unique in its limiting factors, and farmers can and will provide substantial information on their particular environmental constraints.

DEFINITIONS

incremental treatments

DISCUSSION:

1. DECIDING ON WHICH INVESTMENT TO MAKE

Few producers have the resources needed for optimum production. Farmers are faced with the question of which investment (organic or chemical fertilizer, amount of fertilizer, type of fertilizer, better seed, better weed control, new tools, another animal, a cart, or any of many other possible inputs)

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will give the greatest return. With limited capital, which input will give greatest "profit". Some inputs are relatively inexpensive while others are quite costly. One approach to this follows. If the research is considering different types and amounts of inputs, as FSR/E research often does, the treatments should be equated in terms of costs, or should be incremental (for example, $10, $20, $30, etc.). This implies that before treatments are defined the cost and availability of the treatment inputs must be determined.

To illustrate: fertility trials are common in FSR/E research. Rather than comparing the effect of, say 80 kg/ha of nitrogen with 80 kg/ha of phosphate, or 80 kg/ha of potash, compare the effect of $20.00 (increments or multiples) of nitrogen with $20.00 of phosphate, $20.00 of potash, and $20.00 worth of combinations of the three primary nutrients. It should be noted however, if one wants to increase grain yield of plants growing in poor soils, it may take a minimum of a nutrient (possibly 40 kg/ha) to get a grain yield response. Putting on less may result in increased vegetative growth and no grain yield response.

With fertility trials as common as they are, it is worth looking at the cost of actual nutrient in locally available fertilizers. Often there are substantial differences between nutrient costs from one chemical fertilizer to another. Further, the fertilizer which is most economical at one site may not be economical at another, or may change from season to season. Marketing and transportation systems affect the relative costs of nutrients in the various available formulations. The FSR/E researcher should be able to determine actual cost of nutrient for any of the fertilizers available in local co-ops. Here is the basic information that is needed to determine cost of actual nutrient:

First, bagged chemical fertilizers can be expected to bear some type of label indicating what the percent nutrient content is, as well as the source or compound used for that nutrient. That label can be expected to be of the form XX-YY-ZZ. The "XX" refers to the (elemental) nitrogen content as a percent of the total. The "YY" refers to the percent phosphate P_2O_5 (which is 44% phospohrus) and the "ZZ' refers to the percent potash (K_2O, 83% potassium). An example of a commonly found nitrogen fertilizer is ammonium nitrate, 33.5-0-0. A 100 pound bag of ammonium nitrate would contain 33.5 pounds of nitrogen.

2. CALCULATING COSTS OF SINGLE NUTRIENT FERTILIZERS

Now, for the cost calculations. Let us continue using ammonium nitrate to illustrate. Let us say that the 100 pound bag of ammonium nitrate costs $10.00. How much does one pound of nitrogen, from ammonium nitrate, cost? Divide the cost of the commercial bag of fertilizer by the number of pounds (or whatever unit of weight you want to use) in bag, then apply the following

general formula:

$$\text{Actual lb. cost of nutrient} = \frac{\text{Cost of a pound of the commercial fertilizer}}{\text{percent of nutrient in commercial fertilizer}}$$

Then, by comparing the cost of actual nutrient in various commercially available fertilizers, one can select the most economical fertilizer. If costs are similar, then one may want to consider the other factors associated with the fertilizer formulations. The same procedure we applied to determine the nitrogen would also apply to determining the cost of nutrients in fertilizers containing phosphate or potassium.

The procedure is simple and straight forward when comparing single nutrient fertilizers. The procedure gets a bit more complicated when we want to determine the economics of fertilizers containing two or all three of the primary nutrients. The procedure involves comparing the cost of the commercially available multiple nutrient fertilizer and what it would cost to purchase the same amount of nutrients as found in the commercially available multiple nutrient fertilizer and mix them on site.

3. OTHER VARIABLES TO CONSIDER

It should be noted that cost is only one of many variables associated with fertilizer use. Others include solubility (if the nutrients are not readily soluble they are unavailable to the plants), volatility (some fertilizers, for example urea, must be incorporated immediately to prevent loss), acid-formation, nutrients other than N-P-K in the fertilizer bag (calcium and sulfur are two agronomically important nutrients which may be included). Calcium is more important when soils are acid and sulfur is more important when soils are basic. Basic soils (calcareous and sodic) are common in arid areas where the basic cations have not been leached out of the soil by percolating water.

Two general comments will be made concerning observations of the chemical fertilizer problem in the developing world to encourage the FSR/E practioner to look at the fertilizer cost issue at the research site. First, commercially mixed, complete fertilizers often are substantially more expensive than the same nutrients in single nutrient and some multiple nutrient high analysis fertilizers. Second, because of such factors as freight costs, low analysis fertilizers (like 6-6-6), cost more per pound of nutrient at the farmgate than the higher cost per bag, higher analysis (like 16-44-0) fertilizers. A word of caution must be included relative to the high analysis (and probably lower cost per pound nutrient) fertilizers. If farmers have not had experience with high analysis fertilizers, the tendency is to over-fertilize and damage plants.

In addition to the amount and types of fertilizer to be applied there is also the issue of timing. Again, the FSR/E approach demands that the economics of the situation be carefully weighed. In general, the best time to put the fertilizer on is when the plant needs it, which usually is not at planting. In fact, applying fertilizer at planting can reduce germination and emergence. Also, if we are working in a high rainfall environment, the soluble fertilizer may be leached from the root profile before the plant has a chance to use the nutrients.

Fertilization has been used for illustration here. Similar situations exist for other inputs. No hard and fast rules can be given about the construction of treatments other than to try to view them from the farmer's viewpoint.

4. CALCULATIONS ON POPULATION DENSITIES

Plants compete with each other (and with weeds) for sunlight, nutrients, and moisture. The amount of moisture, nutrients, and occasionally sunlight will determine the carrying capacity or a unit of land. Too few plants result in greater weed competition and lower seed/fruit yields. Too many plants may result in reduced yield and lower product quality. Therefore, it is important to be able to determine desired plant spacings and actual plant populations to be able to follow recommendations from crop specialists or "trouble-shoot" when there is a problem. Furthermore, as human population densities increase, farmers will be looking for ways to get more production from their land and often will be increasing inputs like fertilizer and possibly changing varieties which may necessitate changes in the optimum plant population density. Here are the calculations and information needed to determine population density (the method applies to any land unit even if a unit other than a "hectare" or an "acre" is used.

One hectare is 100 meters x 100 meters, or 10,000 square meters. If you have your units as hectares and want acres, multiply the hectare value by 2.471 to get acres.

One acre is equal to 43,560 square feet and multiplying by 144 gives 6,272,640 square inches. If you want to convert acres to hectares, multiply by 0.405.

It may be helpful to know that one can convert kg/ha (or quintal/hectare) to lb/acre (or hundredweight/acre) by multiplying by 0.892. One can go from lb/acre to kg/ha by multiplying by 1.121. To understand the concept of population calculations, it is desirable to think "how much space does the typical plant occupy?" This is determined by dividing the distance between two plants into two halves, one for each plant. If one takes half the distance to each of the plant's neighbors, one should be able to determine how much land is occupied by the typical plant. Although there is a range of values for individual plants in a field, it is still useful to have an

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average value.

If there is a row pattern as there usually is even when hand planted, one can use the following relationship:

$$R \times D \times P = K$$

where:
R = distance between rows
D = distance between plants, within row (same unit as R)
P = Population per unit area (usually expressed in number of plants in one hectare or one acre)
K = a constant, and must agree with the units of R and D above:
— if meters are used in R an D, and we want population per hectare, then K is 10,000.
— if feet are used in R and D, and we want population per acre, then K is 43,560.
— if inches are used in R and D, and we want population per acre, then K is 6,272,640 and so forth.

To illustrate: a farmer has rows spaced about one meter (100 cm) apart and plants about 0.25 meter (25 cm) apart in the row. What is the plant population per hectare? Plug it into the formula:

$$1.0 \text{ m} \times 0.25 \text{ m} \times (\text{unknown population}) = 10{,}000 \text{ m}^2$$

$$\text{unknown population} = \frac{10{,}000}{(1.0) \times (0.25)}$$

unknown population = 40,000 plants per hectare

Another question that is asked is as follows: a farmer is advised to plant 15,000 plants per hectare and will plant in rows spaced one meter apart. How far apart should his plants be placed within the row? Plug values into formula:

$$1.0 \text{ m} \times (\text{unknown distance, in meters}) \times 15{,}000 = 10{,}000 \text{ m}^2$$

$$\text{unknown distance} = \frac{10{,}000}{(1.0) \times (15{,}000)}$$

unknown distance between plants in row = 0.67m

A point needs to be made. Often farmers who plant by hand will plant two or more seeds per "hill" (one for the worms, one for the birds, and one to grow). If, however, the farmer expects most seed to grow and produce plants, the calculations are not done on number of seed, but rather, number of hills.

Another point: most plants benefit from careful placement relative to each other. Rather than a square planting

arrangement (see Figure II,D,3.1 left), it is more advantageous to use an offset planting arrangement (see Figure II,D,3.1 right). The hexagonal arrangement will allow the farmer to plant about 15% more plants per unit area without changing the minimal plant distance.

Figure II,D,3.1 Planting Arrangements

EXAMPLES OF TREATMENTS FOR DIFFERENT TYPES OF PROBLEMS

OUTLINE

1. Variety Evaluation Trials
2. Plant Nutrition (Fertilizer) Trials
3. Plant Protection Trials
4. Cultural Practices Trials
5. Integrated Techpack Trials

PREREQUISITES

I	What Kind of Testing to Do
II,C,1	Defining Treatment Objectives
II,C,4	Choosing Control Treatments
II,D,1	Specification of Experimental Treatments
II,D,2	Specification of Non-Experimental Variables

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

By the end of this sub-unit, participants will be able to:

1. Propose likely experimental treatments and controls for different types of production problems.

2. Identify likely changes in experimental treatments and controls as trials move from exploratory and refinement to validation testing.

3. Identify specification needs that may arise for particular types of trials.

KEY POINTS

1. Exploratory and refinement trials usually focus on multiple levels of a few factors.

2. Interactions among factors may require that different levels of two or more factors be studied together in exploratory or refinement trials.

3. Validation testing may involve combining the best levels identified in exploratory and refinement testing of several factors into a single "integrated techpack" treatment.

DEFINITIONS

control treatment
exploratory testing
factor
factorial experiments
individual farmer control
interaction
intercropping
level
refinement testing
relay cropping
researcher control
rotation
techpack
validation testing

DISCUSSION

This section suggests the treatment sets that may be appropriate for different types of problems along with the controls that might be used at different stages in the research sequence.

1. VARIETY EVALUATION TRIALS

These typically comprise a comparison of several improved varieties against one or more locally used varieties. Improved varieties may require higher management levels than local ones. For example, there may be a variety-by-fertilizer interaction, or a variety-by-spacing interaction. A variety trial thus may be combined in exploratory testing with fertilizer level or spacing factors. (III,C,2) presents ways to design trials where treatments for different factors are combined together.

In validation testing, usually only one or two varieties will be tested against the farmers' own varieties which are possibly a part of an improved techpack.

2. PLANT NUTRITION (FERTILIZER) TRIALS

These trials include rates, time, or method of application, or a combination of two or more of these types. A point to be checked is how farmers apply fertilizer. They may apply it per plant or per mound, regardless of spacing. Treatment specifications should be expressed in these units.

3. PLANT PROTECTION TRIALS

a. Weed Control Trials

In exploratory testing, these may include herbicide screening, timing of weedings, etc., but refinement trials are

more likely to compare systems of weed control, such as combinations or sequences of land preparation methods, pre-plant, pre-emergence and/or post-emergence herbicides, and cultural (mechanical or manual) methods.

b. Pest and Disease Control

In exploratory and refinement testing, these controls may involve insecticide and fungicide screening; times, frequencies, rates, and methods of application; and pesticide sequences or rotations. Validation testing will usually combine several factors in a single techpack of practices (e.g. a recommendation of one or more pesticides with specific rates, timing and application methods).

4. CULTURAL PRACTICES TRIALS

a. Spacing Trials

These may compare regular vs. random or various combinations of between-row and in-the-row spacings at equal or different plant populations, planting patterns, number of plants per hill, or most simply various intra-row spacing with inter-row spacing unchanged. Spacing may be a component of a techpack in validation testing, including perhaps a new variety, a fertilizer and pest control recommendation, and a spacing recommendation.

b. Planting Time and Intercropping Trials

These trials may test intercrop species, plant populations, planting patterns, and time of planting, or examine variations in the planting pattern and population of the dominant crop. Other trial types may include relay cropping as a treatment. Exploratory intercropping trials can become extememly complicated in terms of a treatment array. These also include crop rotation or crop sequence trials, which may include fertilizer level as a factor: fertilizer applied to one crop may affect the following crop. Complex interactions can result.

c. Crop Care Trials

These trials include studies of staking (e.g., height or spacing of stakes, stakes vs. trellises, etc.), mulching, hilling-up, pruning, and vine-turning, etc.

d. Irrigation Trials

These trials include rate, frequency, and method or ways of water application.

5. INTEGRATED TECHPACK TRIALS

As indicated in several examples above, these combine practices for several variable factors into one treatment. These

are most common in validation testing, where they are usually compared against individual farmer controls. On occasion, a "researcher control" techpack including some practices currently recommended by the experiment station or linked to a government credit program but not found to be beneficial in earlier trials may be included in late refinement testing. The primary purpose of this is to demonstrate to policy makers the need to change recommendations or credit policies.

OTHER SECTIONS THAT CAN HELP

III,C,2 Ways to Combine Treatiments Within Replications

LOOKING AHEAD: WHAT ARE SOME TRADE-OFFS BETWEEN TREATMENTS AND REPLICATIONS

OUTLINE

1. Why an Increased Number of Treatments May Be Desirable
2. Why Increased Replication Can Be Important
3. Problems With Increasing Both the Number of Treatments and the Number of Replications.

PREREQUISITES

I	What Kind of Testing to Do
II,B	What Kinds of Fields Are Available for Testing On-Farm
II,C,1	Defining treatment objectives
II,C,2	What to Consider in Selecting Subsets of Experimental Treatments
II,C,4	Selecting Control Treatments
II,E	Examples of Treatments for Different Types of Problems

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this sub-unit, participants will be able to:

1. Identify reasons why a large number of treatments may be desirable.

2. Identify reasons why increased replication can be important.

3. Identify problems and choices with increased numbers of treatments and replications.

KEY POINTS

1. An increased number of treatments may be desirable. This often depends on the existence or possibility of interactions, the need for additional controls, and the need for adequate number of response points.

2. Increased replication can improve precision in identifying treatment differences and enhance the ability to identify treatment-by-farm interactions.

3. Increased numbers of treaments and replications require

additional decisions. These decisions can address trade-offs among complexity of design and analysis, size and complexity of trials at each farm, researcher-farmer management sharing, team travel time and costs, representativeness of farms, and amount of information which should be obtained.

DEFINITIONS

block
control
domain
experimental design
exploratory testing
factorial experiment
interaction
plot
probe
refinement testing
replication
response
statistics
2^n factorial
treatment
treatment combination
validation testing

DISCUSSION

1. WHY AN INCREASED NUMBER OF TREATMENTS MAY BE DESIRABLE

There are several reasons why an increased number of treatments may be desirable. Some of these reasons relate to the basis of the problem. Other reasons relate to the nature of the research process. Here are some of the reasons:

a. Nature of the Problem

In exploratory testing, the basis of the problem may suggest looking at many different possibilities for solving the priority problem of farm households. Here are some examples:

1. Varieties

The priority problem of farm households may, for example, be decreased length of the rainy season. An earlier maturing variety is needed. The team may be able to obtain a large number of advanced breeding lines and releases. Some may be breeding lines from neighboring countries and appear promising because of similarity of climatic conditions. Others may be releases from other countries or international centers that have done well there, but adaptability to local conditions is not certain. the team may want to try all of these in an initial exploratory trial.

In a different domain, the priority problem may be used for disease resistance. A number of lines or releases may be reported to have resistance, but adaptability and taste acceptability may be uncertain. Again the team may want to include all of the lines in an exploratory trial.

2. Weed, Disease, or Pest Control

The priority problem may be a need for disease or pest control where no good resistance is available. Or, the priority problem may be an increased labor cost for weeding due to off-farm employment opportunities. In either case, the team may want to screen a large number of possible fungicides, pesticides, or herbicides.

b. Existence or Possibility of Interactions

1. Exploratory 2^n Trials

Before doing an exploratory trial, the team may want to do a 2^n factorial trial to determine if possible interactions interactions are present, and select the most promising factors to develop improved production practices for. Each increase in the number of factors doubles the number of treatment combinations in a 2^n factorial experiment:

no.of factors	type of experiment	no. of treatment combination
2	2^2	$2^2 = 4$
3	2^3	$2^3 = 2 \times 2^2 = 2 \times 4 = 8$
4	2^4	$2^4 = 2 \times 2^3 = 2 \times 8 = 16$
5	2^5	$2^5 = 2 \times 2^4 = 2 \times 16 = 32$
6	2^6	$2^6 = 2 \times 2^5 = 2 \times 32 = 64$

(II,B) explains how to set up 2^3 factorial experiments.

2. Other Exploratory Factorial Experiments

In other situations, before doing an exploratory trial, the team may have prior information that suggest or indicate interactions among factors. This information may come from a variety of sources. Observations of crops or animals during diagnosis may indicate interactions. Farm household members may describe problems that suggest interactions. Also, interactions may be indicated by previous station research.

The team may want to confirm the interactions in an exploratory trial. For example, nitrogen costs may have increased and farmers may want to know what the trade-offs are between reduced yield and reduced costs for lower rates of nitrogen. Station research may have shown nitrogen interactions with phosphorus and potassium. The team may consider a factorial

experiment with four rates of phosphorus (with or without), and two rates potassium (with or without). This would be a 4 x 2 x 2 factorial, with 4 x 2 x 2 = 16 treatment combinations.

Interactions may also be a focus of exploratory intercropping trials. For example, the team may want to investigate three intercropping ratios (2:1, 1:1, and 1:2 ratios of rows of the two crops) at 3 in-the-row spacings for each crop. This would be a 3 x 3 x 3 factorial, with 3 x 3 x 3 = 27 possible treatment combinations.

In refinement testing, the team may also have prior information that indicates interactions. This is often the case if a 2^n factorial exploratory trial has identified several factors with interactions. Results of a 2^n factorial simply show if there is an interaction at two levels (usually, with vs. without). The team may want to pinpoint cut-off points for both factors simultaneously. For example, the exploratory trial may have shown an interaction between plant population (low vs. high population) and nitrogen (with vs. without). The team may want in the exploratory trial to test four rates of nitrogen at three plant populations. This would be a 4 x 3 factorial, with 4 x 3 = 12 treatment combinations.

c. Need for Additional Controls

In exploratory and refinement testing, the team may wish to add additional controls. (II,C,4) gives examples of different types of researcher controls that may be desirable, such as a no fertilizer treatment, no weed disease, pest control treatments, probe treatments, or a weed-free treatment. The team may wish to add a current recommendation control. More than one average farmer control may also be desirable. For example, if 50% of the farm households plant variety 'Gagangay' and 40% plant variety 'Napigsa,' it might be desirable to include both.

d. Need for an Adequate Number of Response Points

In exploratory or refinement testing, the objective may be to generate a response curve (II,C,1). At least three points are needed for a response curve, and four points are a more desirable number.

In refinement testing, data from a yield response curve may be converted to net benefit values and an economic response curve constructed. The CIMMYT manual, "From Agronomic Data to Farmer Recommendation," and the Shaner text give examples of how to construct economic response curves. To construct these curves, at least three points, and preferably four points, are desirable.

In all of the above examples, increasing the number of points means increasing the number of treatments. For example, instead of using just three nitrogen treatments of 0, 150, and 300 kg/ha, the team may want to use four treatments of 0, 100, 200, and 300

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kg/ha. If the experiment is a factorial experiment, each increase in the number of treatments of one factor increases the total number of treatment combinations in the whole experiment by the number of treatment combinations of the other factors. Suppose the experiment also includes two phosphorus treatments (0 and 150 kg/ha) and two potassium treatments (0 and 100 kg/ha). The total number of treatment combinations in the experiment will increase by 4:

	No. of treatments per factor			No. of treatment combinations
	N	P	K	
(1)	3	2	2	3 x 2 x 2 = 3 x 4 = 12
(2)	4	2	2	4 x 2 x 2 = 4 x 4 = 16
Difference in (2)-(1)	1			4

Note that the increase in treatment combinations equals the total number of treatment combinations of the other factors:

Treatment combination	Treatment level P (kg/ha)	K (kg/ha)
0,0	0	0
0,100	0	100
150, 0	150	0
150,100	150	100

2. WHY INCREASED REPLICATION CAN BE IMPORTANT

A plot is an area with only one treatment (or treatment combination in factorial experiments). A complete set of all the treatments or treatment combinations in an experiment is a replication. (II,B) explains these terms in more detail.

Each replication is like a complete experiment. If there are three replications in an experiment the team places each treatment (or treatment combination) in three different plots. If there are four replications, the team places each treatment (or treatment combination) in four different plots.

Why would a team want to increase the number of replications? Why, for example, would it want to place each treatment in four different plots, rather then in three? There are two reasons for this. One reason is to improve precision in identifying treatment differences. The other reason is to be able to identify treatment-by-farm interactions. Let's look more closely at these two reasons.

a. Improved Precision in Identifying Treatment Differences

Two adjacent groundnut plants may not be the same height, even though they have the same fertilizer, weeding, pest and disease control, etc. In a row of 20 plants, it is very unlikely that all 20 will have exactly the same heights. Suppose a farmer harvests those 20 plants and weighs the hay to give to the animals. Then the farmer harvests the next row, also with 20 plants, and weighs the hay in that row. It is also very unlikely that the two weights will be exactly same. A similar example could be made for weights of kids in the same litter from the same mother goat. The same would be true for litters from several mothers in the same herd. This is natural variation. Natural variation is always present in plants and animals.

A team applies each treatment to a plot. Plots are in uniform areas called blocks. (II,B) explains how to choose fields or parts of fields for blocks and plots. The team may use the best of its scientific knowledge, together all the experience and practical knowledge of farm households members, to choose areas that are as uniform as possible. Even so, natural variation will still remain in the uniform area. The weight of stalks in the first row will still probably be different from the weight in the next row.

Now, suppose the first row had one treatment (seed treatment) and the second had another treatment (no seed treatment). How do farm households know whether the difference in weights is due just to natural variation, or in fact due to the difference in treatments? Was increased weight in the row with seed dressing really due to the seed dressing? Or, would that increased weight have happened anyway, even if the second row had no seed dressing, because of natural variation? How can the team help farm household members judge to which the increased weight is due, seed dressing, or natural variation?

The way the team can help is by replicating the treatments. Each replication gives an estimate of natural variation. The more replications, there are the better is the estimate of natural variation. The team can compare treatment differences against an overall estimate of natural variability for the experiment. The better the estimate of natural variability, the more the treatment differences will stand out. The better the estimate of natural variability, the more confidence the team will have in helping farm household members to judge from which treatment the increased weight resulted. (III,B) explains in more detail how to compare treatment differences with natural variation. The way to make this comparison is called statistics.

Statistics is simply a set of rules on how to compare treatment differences with natural variation. There are many ways to make the comparison. These are called experimental designs. Each design has its advantages and disadvantages. Each design also has its own set of rules. The rules tell the team how to place treatments in plots and blocks to obtain analyzable data. Analyzable data means data that the team can make an

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objective judgment about the comparison between treatment differences and natural variation. This is a powerful way for a team to help farm households.

(III,C,1 and 2) explain the different designs. Understanding the advantages and disadvantages of each design can help a team make a better decision about which design is best in each situation. Understanding the rules for each design can also help a team make a better decision. Understanding the priority problem, resources, constraints, and goals of farm households in the domain is the basis for the team to evaluate the advantages and disadvantages of each design and make the best decision in each situation.

b. Ability to Identify Treatment-by-Farm Interactions

In diagnosis, the team groups similar farm households into domain (Volume I:IV). Even in each domain, farm households will still differ. This is similar to two rows differing in a uniform field. In other words, there is also natural variation among farm households.

The objective in grouping farm households into a domain is to develop recommendations that will be acceptable to all the farm households in the domain. This is why many people use the word "recommendation domain." On-farm testing is the method that farming systems research/extension uses to develop acceptable recommendations.

Because of natural variation among farm households, two treatments may have different responses from one farm household to another. For example, in a refinement trial, perhaps a higher plant population gives higher yield for one farm household, but the lower plant population gives higher yield for another. What is this due to? Is this the just to natural variation among farm households? Or, is there a reason for the difference? Is the difference in treatment responses (higher yield with higher plant population in one case, higher yield with lower plant population in the other case) due to a systematic pattern of slightly lower rainfall for farm households located further north? Or, is the difference due to a systemtic pattern pf poorer management by farm households with fewer off-farm employment opportunities?

If the difference is due just to natural variation, the team can still make the same recommendation for all the farm households in the domain. On the other hand, if the difference is systematic, and the team can identify the reason, then the team might consider making one recommendation for the first set of farm households, and a different recommendation for the other set. For example, if poorer management by farm households with fewer off-farm employment opportunities is the reason for the difference in treatment responses, the team might consider making two different recommendations:

(1) Higher plant population for farm households with more off-farm employment opportunities (and hence better management, with consequent positive response to the higher plant population).

(2) Lower plant population for farm households with fewer off-farm employment opportunities (and hence poorer management, with consequent negative response to the higher plant population):

This is called partitioning the domain. The team partitions the farm households in the original domain into two groups:

(1) Farm households with more off-farm employment opportunities.

(2) Farm households with fewer off-farm employment opportunities.

Before on-farm testing, the team would not really be sure if it could make a recommendation acceptable to all the farm households in the original domain. That is why, strictly speaking, the original domain is basically a researchable domain.

After partitioning, the team would probably design a simple validation experiment for the two new domains. Let's consider just what could happen with the new domain consisting of farm households with more off-farm employment opportunities. The team would select some farm households from this new domain to cooperate in a validation trial comparing higher and lower plant populations. If the cooperating farm households found the higher plant population acceptable, then all the farm households with more off-farm employment opportunities in the original researchable domain would now be a recommendation domain for the higher plant population. The results of on-farm testing would indicate that extension personnel could recommend the higher plant population to other farm households in the new domain, not just those that participated in the validation trial. Even though the other farm households did not participate they would also have more off-farm employment opportunities like the farm households that did participate.

Now how does the team decide whether to partition or not? How does it judge whether the difference in treatment response in the refinement trial example above is really due to the management difference, and not due just to natural variation among farm households?

A wrong judgment could be serious. There are two ways that a wrong judgment could be serious:

(1) The team is "pushy." There is no real difference due to management, but the team partitions anyway. The result of being "pushy" may be a surprise in the validation trial that follows.

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For example, farm households with better management may also find the higher plant population acceptable. The team has to re-evaluate the domain. The team decides to partition after all and to do one more validation trial. Time and money are lost. The team could have reached the same conclusion earler if it had made the right decision on partitioning after the refinement trial.

(2) The team is "shy." There really is a difference due to management, but the team doesn't recognize the difference, so it doesn't partition. The team decides to drop higher plant population for the whole domain. That result is unfortunate. The higher plant population really could help farm households with better management, but the team fails to discover this. Extension then also doesn't make the recommendation. Later, some farm households with better management discover that higher plant population is better anyhow. It takes longer for other farm households to learn of this. It takes still longer for extension to learn and adjust their recommendation. The public research and extension process appears less useful to farm households. Nobody is happy: farm households wish public research and extension had been more useful, and research and extension personnel wish they could have done a better job.

(The terms "pushy" and "shy" were taken from text orginally written by Clive Lightfoot, titled, "Statisical Analysis for On-Farm Agronomic Data").

Statistics can help here also. Statistics can help the team make an objective judgment of whether or not to partition. Of course, the objective judgment is still only a judgment. The team can still be wrong. But statistics provides a way for the team to know the chance of being wrong. It can also discuss with farm household members what chance of being wrong they are willing to accept. (III,B) explains how statistics can estimate the chances of being wrong.

How does statistics help in making judgments about partitioning? One way is to estimate treatment-by-farm interaction. How is that possible? Recall that each replication is a complete set of treatments (or treatment combinations). If each farm has only one replication, there is no way to separate treatment-by-farm interaction from natural variation for the experiment as a whole. This is because there is no estimate of natural variation within each farm. However, if each farm has more than one replication, then it is possible to estimate natural variation within each farm. The team can test for variation within farms against an estimate of natural variation for the whole experiment. The team can also test for treatment-by-farm interaction against an estimate of natural variation for the whole experiment. (III,C,2) explains how design experiments to test for treatment-by-farm interaction.

The ability to test for treatment-by-farm interaction can

help a team make better decisions about recommendations, and avoid mistakes. Increased replication makes this possible. Therefore, this is another reason why increased replication can be important.

3. PROBLEMS WITH INCREASING BOTH NUMBER OF TREATMENTS AND NUMBER OF REPLICATIONS

The total number of plots in an experiment equals the number of treatments or treatment combinations times the number of replications if all treatments or treatment combinations are replicated the same number of times. By adding just one treatment, the number of plots increases not by one, but by the number of replications. Suppose the team wants to test five new lines against the farmer variety, to solve a maize lodging problem. This gives six plots per replication. Now suppose the team receives seed of a sixth new line. Adding the sixth new line in a site-specific exploratory trial with four replications increases the number of plots by four, not one:

Plan	No. of treatments			No. of	No.of
	New	Farmer	Total	Replications	plots
(1)	5	1	6	4	24
(2)	6	1	7	4	28
(2)-(1)	1		1		4

Adding another replication increases the number of plots by the total number of treatments or treatment combinations. Suppose the team wants to test three planting patterns at two plant populations. This is a 3 x 2 factorial, with six treatment combinations. Suppose this will be a refinement trial. The team decides to have two replications per farm, to test for treatment-by-farm interaction. The number of plots increases by six on each farm:

Plan	No. of treatment combinations	No. of replications per farm	No. of plots per farm
(1)	6	1	6
(2)	6	2	12
(2)-(1)			6

(II,B) discusses some of the problems with increased numbers of plots per farm. Farmer management capability and parcel fragmentation may limit block size on some farms. If treatment number exceeds the smallest block size, the team has two design choices. Each design choice has advantages and disadvantages:

a. Design Choice 1: Randomized Complete Block (RCB)

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Use a randomized complete block design (III,C,1,a). Every farm has all the treatments or treatment combinations.

advantages:
- easy to plan
- easy to analyze
- fewer farms to visit

disadvantages:
- farms without enough land cannot be included unless treatment number is reduced
- only larger farms may be less representative of domain
- reducing treatment number reduces the amount of information from the trial
- too many treatments and plots may require more team management at each farm.

b. Design Choice 2: Incomplete Block Design (IBD)

Use an incomplete block design (III,C,1,b). Each farmer may have only some of the treatments or treatment combinations:

advantages:
- can accomodate unequal block sizes
- allows fewer treatments on each farm without having to reduce the number of treatments in the experiment as a whole
- fewer plots on each farm can make trial simpler on each farm and reduce team management time at each farm.

disadvantages:
- more difficult to plan
- more difficult to analyze
- requires more farms to obtain the same number of replications of treatments (or treatment combinations).
- more farms require more travel time and budget.

<u>OTHER SECTIONS THAT CAN HELP</u>

III,B	What Designs Can Do
III,C,1,a	Ways to Replicate Treatments Within and Across Farms
III,C,1,b	(optional) Incomplete Block Designs Across Farms
III,C,2	Ways to Combine Treatments Within Replications

UNIT III
HOW TO DESIGN TRIALS TO OBTAIN ANALYZABLE DATA

ACTIVITIES:

ACTIVITY ONE: COMPARISON OF CRD AND RCBD

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ACTIVITY ONE
COMPARISON OF CRD AND RCBD

TRAINERS' NOTES

OBJECTIVE:

After completing this activity the participants will be able to:

1. Compare results of non-identical comparisons between CRD and RCBD.

TIME:

MATERIALS:

1. Sample designs with treatments assigned (1 design/page)
2. List of treatments and maps of plots and farms
3. Pencils and erasers
4. Flip chart and marker pens

5. If trainer has time to do before workshop (a) Sample designs reproduced onto flip chart pages, (b) maps of plots and farms reproduced onto flip chart.

6. If trainer has access to overhead machine in workshop, (a) sample designs reproduced onto overheads, (b) maps of plots and farms reproduced onto overheads, (c) blank overheads.

INSTRUCTIONS:

1. Pass out CRD and RCBD sample designs and ask participants to place the 2 pages side-by-side.

2. Ask participants to count how many times each treatment appears in each design.

3. Ask participants to count how many times each treatment appears in each farm.

4. Ask participants to find any place in either design where the same treatment appears in adjacent plots in a horizontal row.

5. Explain that this can occur, although may not always occur, with randomization in CRD, but can never occur within each block in RCBD. Then say, "Let's see why this is so."

6. Pass out list of treatments and 2 maps: 16-plot map for CRD, and 4-plots/farm, 4 farms total map for RCBD.

7. Explain how to use random number table. *Point out also that writing numbers on pieces of paper, shaking in a hat, bowl, etc., and drawing the pieces of paper out one at a time is an alternative procedure for randomization.

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8. Have participants draw random numbers in range of 1-4 and assign in sequence to treatments a, b, c, and d.

9. Have participants draw random numbers, with replacement, in range of 1-4, and make 2 columns: column 1 with sequence of random numbers drawn; and column 2 with the treatment letter corresponding to each number.

10. Have participants write treatment letters in plots of 16-plot map, starting at top left, going across, then returning across next row from right, and so forth in S-fashion for rows 3-4.

    * Explain why randomization is necessary, because of natural variation.

11. Ask participants to find any place where the same treatment appears in adjacent plots in the same row. Write in column 1 on this chart: aa, bb, cc, dd. Ask how many participants had 1 set of a'a's and write the number of participants who did in column 2. Similarly, write the number of participants with 2 sets of aa's in column 3, the number with 3 sets in column 4, and the number with 4 sets in column 5 (note: columns 3-5 will probably be zeros). Repeat above process for bb's, cc's, and dd's. Then also do for the 6 non-identical ab, ac, ad, bc, and cd (or their reverses, ba, ca, etc.).

12. Have participants repeat step (8), explaining to them that this will be the sequence for randomization for the RCBD.

13. Have participants draw random numbers without replacement (explain) in range of 1-4 and make 3 columns: Column 1 with letter A for farm A; column 2 with the sequence of 4 random numbers; and column 3 with the treatment letter corresponding to each number.

14. Repeat step (13) for farms B, C and D.

15. Have participants write treatment letters in plots for each farm, starting at lower left and working up or across. Point out that plots may be non-contiguous.

16. Repeat step (11), using same columns, with a line drawn between the top half (label CRD) and the bottom half (label RCBD).

17. Ask participants to compare results of aa - dd comparisons between CRD and RCBD (note: there will be no aa's, bb's, etc., for RCBD).

18. Ask participants to compare results of non-identical comparisons between CRD and RCBD (note: there will be more for RCDB than for CRD).

PROCESSING:

1. In discussing the solutions with the participants, the following example will be useful.

CRD

1	2	4	1	row 1
1	4	4	2	row 2
4	3	1	3	row 3
3	3	2	2	row 4

RCDB

2	1	4	3	I
2	3	4	1	II
1	2	4	3	III
4	2	3	1	IV

Each row is not a block. Some rows may duplicate treatments, and even have the same treatment in adjacent plots in the same row.

Each row is a block. Each block has all four treatments.

adjacent plots in rows	CRD		RCBD	
11	0			0
22	1			0
33	1	3	0	0
44	1			0
12 or 21	1			2
13 or 31	2			1
14 or 41	2			2
23 or 32	1	9	12	2
24 or 42	2			2
34 or 43	1			3

note: 1 = a, 2 = b, 3 = c, 4 = d, so 11 = aa, 22 = bb, etc.

(III,A)

HOW OBJECTIVES CHANGE IN THE RESEARCH-EXTENSION PROCESS

OUTLINE

1. Characteristics for Comparing Trial Types
2. Characteristics and Design Options of Exploratory Trials
3. Characteristics and Design Options of Refinement Trials
4. Characteristics and Design Options of Validation Trials

PREREQUISITES

I	What Kind of Testing to Do
II,B	What Kinds of Fields Are Available for Testing On-Farm
II,C,1	Defining Treatment Objectives
II,F	What Are Some Trade-offs Between Treatments and Replications

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this section, participants will be able to:

1. Identify changes in response and replication in exploratory, refinement, and validation testing.

2. Identify changes in requirements for plot size, block size, and researcher-farmer management showing due to the different objectives of exploratory, refinement, and validation testing.

3. Identify design options appropriate for the different objectives and requirements of exploratory, refinement, and validation testing.

KEY POINTS

1. Biological response objectives decrease in importance as trial functions change from exploratory to refinement to validation testing.

2. Socio-economic response objectives increase in importance as trial functions change from exploratory to refinement to validation testing.

3. Replication within each farm decreases, and replication across farms increases, as trials move from exploratory to

refinement and validation testing.

4. The best choice among design options depends on trial response and replication objectives, farm setting, and researcher-farmer management sharing.

DEFINITIONS

acceptability index
ANOVA
block
combined analysis
confidence intervals
confounding
continuous data
discrete data
domain
experimental design
exploratory testing
factor
factorial experiments
2^n factorial experiments
fractional replication
guard rows
incomplete blocks
incomplete block design (IBD)
modified stability analysis
partial budgeting
RCBD
refinement testing
response
sensitivity analysis
split-plot experiments
superimposed trials
techpack
t-test
treatment
treatment combination
validation testing

DISCUSSION

1. CHARACTERISTICS FOR COMPARING TRIAL TYPES

The objective of on-farm research is to generate technology acceptable to farming unit members. Researcher-planned trials change over time in order to reach the final objective of acceptable technology. (I,F) presents the basic changes in trial function and researcher-farmer management sharing. It presents the three basic trial functions:

a. Exploratory testing
b. Refinement testing

c. Validation testing

This section compares these three types of trials based on several characteristics. These characteristics help the team choose the most appropriate experimental design for different farm household problems, circumstances, and trial functions.

Each characteristic asks a different question. The characteristic and their questions are:

a. Response Objective

- What do the team and farm household members want to learn from the treatments tested in the trial?
- How many treatments and plots would be desirable to obtain data to meet trial objectives?

b. Replication Objective

- How similar (homogeneous) does the team think farms are in the domain?
- What does the team want to learn about homogeneity in the domain?
- How many replications at each farm would be desirable to obtain data to meet trial objectives?
- How many replications across farms would be desirable to obtain data to meet trial objectives?

c. Plot Size

- How large do plots have to be to obtain data for the response objective?
- Do different treatments require different plot sizes?

d. Block Size

- How large would block size need to be for a complete replication of all treatments (or treatment combination)?
- How much diversity is there in block size among farms?

e. Researcher-Farmer Management Sharing

- Who plants the trial?
- How many treatments and plots can farm households manage?
- How many sites can the team travel to, given time and travel budget constraints?
- How many treatments and plots can the team help manage at each site?

f. Design Options

- How simple is the design to plan?
- How simple is the design to analyze?
- Can the design test for interactions among factors?

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- Can the design test for treatment-by-farm intervention?
- Can the design allow different sized plots for different treatments?
- Can the design allow for different sized blocks (different numbers of plots per block)?
- Can the design use more than 1 block to replicate treatments?

2. CHARACTERISTICS AND DESIGN OPTIONS OF EXPLORATORY TRIALS

In exploratory testing, the team wants to identify treatments that might be appropriate for wider testing over one or several domains.

Characteristics include:

a. Response Objectives

The response objective is primarily biological response to component treatments. The response objective may be qualitative: "Yes, there is a response." "No, there is not a response." Note the following examples:

1. Postive Response, "Irrigated plots yielded more than non-irrigated plots."

2. Negative Response, "Pruned plots had less yield than unpruned plots."

3. No response, "Random planting and row planting had the same yields."

Often, in exploratory testing the objective is to determine if there is a qualitative response to many factors. Factorial experiments can have large numbers of treatment combinations. For example, if testing four factors is desirable, the total number of treatment combinations would be

$$2^4 = 2 \times 2 \times 2 \times 2 = 16.$$

Other exploratory trials may compare many treatments for one factor. Variety trials and disease, pest, or weed control trials are good examples. The response objective may also be qualitative: "This increase in the factor increases yield this much." For example:

"The first 50kg/ha of nitrogen increases yield by 0.3 tons. The second 50 kg/ha increases yield by 0.1 tons. The third 50 kg/ha has no effect."

With more than one factor, these trials can also have many treatment combinations.

b. Replication Objectives

In exploratory testing, the team usually seeks to identify heterogeneity among farms. For qualitative factorial response objectives, replications are few within each farm (often only ten, but many across farms. For other qualitative and quantitative response objectives, replication is only within farms, so enough replications within farms (often four) are needed to give a good estimate of natural variation. (II,F) discusses natural variation, and (II,B) explains how statistics estimates natural variation.

Farms which the team selects should represent the range of environments present in the target area.

c. Plot Size

Because the response objective is biological, plot sizes can be small. Small areas may also be used as super-imposed plots.

The nature of the treatments also affects plot size. Plant protection and fertilizer rate trials require larger areas for border or guard rows, to minimize effects crossing over treatment boundaries.

Land preparation, crop protection, spacing and intercropping, treatments usually require larger plots than varieties, fertilization, or crop care treatments. Crop type can also affect plot size. Generally, field crops occupy larger areas than vegetable or ornamental crops.

d. Block Size

Because of the large number of treatments typical of exploratory trials, a block needs many plots for a complete replication of all treatments.

Overall farm size may place an upper limit on the area of land available for trial use. This is because trials involve risk to the farming units, which depends on their land for food and income. A good rule of thumb is to use no more than 10% of the total land of each farm. This can be a problem in exploratory testing, because of the larger number of treatments and replications within farms common in many exploratory trials.

e. Researcher-Farmer Management Sharing

Exploratory trials are complex, because of the large number of treatments. The researcher share of management is high. The team may superimpose the treatments on fields farmers have already planted if only a qualitative response is the objective. Otherwise, the team usually plants the trial.

f. Design Options

For qualitative factorial trials, 2^n factorial experiments are most common. The trials are usually superimposed on already planted farmers fields. Randomized complete block designs with few replications per farms but more replications across farms are common. (II,C,1,a) can help in designing these trials.

If the number of treatment combinations is too large, fractional replication is an alternative option. (II,C,2) explains this option. Other exploratory trials are usually site-specific trials. Randomized complete block designs are most common.

Treatment arrangements depend on type of treatment and number of factors.

TABLE III,A.1 summarizes design options for exploratory trials. For each type of response objective, numbered 1, 2, and 3 in the first column, the most common design and type of replication are shown first, in the second row and third column. For different management and/or treatment choices within each type of response objective in the first column, the most appropriate treament arrangement is shown in the fourth column. The sections that explain the designs and treatment arrangements are shown in the fifth (last) column.

3. CHARACTERISTICS AND DESIGN OPTIONS OF REFINEMENT TRIALS

In refinement testing, the team wants to expose the best treatments identified in exploratory testing to a much wider range of physical and management environments with each domain. Refinement testing in many ways is the heart of on-farm testing, because it bridges the gap between site-specific exploratory trials and validation trials. Site-specific exploratory trials focus on biological response objectives. They are similar to on-station trials in design and management. Validation trials focus on farm household acceptability as a resource objective. They are simple in design and farmer-managed. Refinement testing is in-between. In refinement testing, the team balances both biological and socio-economic response objectives. It also balances both researcher and farmer management sharing. For the best balance, the team has many choices to make. There are also some new design options developed specifically for on-farm research. Refinement testing can be an exciting opportunity for innovative research design.

a. Response Objectives

Response objectives include not only biological performance, but also socio-economic evaluation. For these reasons, the number of treatments are fewer. A useful target is six, and

TABLE III,A.1 Design Options for Exploratory Trials

Response objective, management, and treatment choices	Design	Replication	Treatment arrangement	Section to refer to for specifics
1. Qualitative 2^n factorial	RCBD	Superimposed: across>within		III,C,1,a
a. Treatment number managable				
(1) NPK, varieties			Random within blocks	III,C,2
(2) land preparation, crop protection, spacing, intercropping			split plot	III,C,2
b. Treatment number too large			fractional replication	III,C,2
2. Qualitative or qualitative with few factors but many treatments per factor	RCBD	Site-specific		III,C,1,a
a. NPK, varieties			Random within blocks	III,C,2
b. Land preparation, crop protection, spacing, intercropping			split-plot	III,C,1,a
3. Quantitative or qualitative with only 1 factor but many treatments	RCBD	Site-specific	Random within blocks	III,C,1,a III,C,2

15-20 would be a maximum number. Both qualitative and quantitative factors may be examined.

b. Replication Objectives

In refinement testing, the team hypothesizes homogenity among farms within domains identified in exploratory testing. The team designs trials separately for each domain in the target area. At this stage, however, the team is still learning about each domain and will not be certain as to the degree of homogeneity among farms. For this reason, the team needs to test homogeneity in each domain. Replication increases across farms (5-10), and often includes the experiment station as the researchers' "farms". Replication within farms decreases, often to two blocks per farm. Two blocks per farm are preferred, rather than only one, in order to identify treatment-by-farm interactions that may suggest partitioning of the farmer grouping or domain.

If biological and economic responses to treatments differ among farms enough to result in a significant treatment-by-farm interaction, these results can help to identify separate researchable domains in the target area. (II,F) discusses treatment-by-farm interactions and partitioning.

c. Plot Size

Plot size increases in refinement trials for two reasons. The first reason is because labor data will be inaccurate and highly variable if taken from the small plots used in typical on-station biological research. The second reason is because farmers typically manage larger pieces of land than the small plots used in typical on-station research. To enable farmers to evaluate the acceptability of new technology, they must be able to test it under conditions that are as similar as possible to what they would otherwise do on their own in the absence of the new technology.

d. Block Size

Increased replication across farms may mean more diversity among farmers. (II,B) discusses some of the causes of diversity among farms. For example, diversity within each farm can place limits on block size. If each farm is fragmented into many parcels with non-uniform environments, parcel size may limit block size. Slope, location on terraces, and presence of physical barriers such as canals, paths, groves of trees, etc., can also limit block size. Parcel fragmentation and physical barriers can sometimes reduce plot size needs, however, by providing natural non-plot border areas, and eliminating the need for guard rows within plots. Reduced plot size can then allow larger block size (more plots per block). (IV,B) gives more detail about guard (or border) rows and other layout

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techniques. Differences in farm household management capability and ability to take risk can also limit block size. These different causes of diversity can also result in differences in block size from farm to farm.

e. Researcher-Farmer Management Sharing

Refinement trials require greater farmer sharing of management, in order to obtain a realistic evaluation of socio-economic response. The team and farmers may plant the trial together, but farm households have a large share of subsequent management. This can limit treatment number and block size at each farm, as pointed out above.

f. Design Options

Refinement trials are usually planned as regional trials. This means the team replicates treatments across many farms. Several situations are possible. Block size and number may be the same on every farm in some situations. In other situations, block size or number, or both, may be different from farm to farm. (II,B) discusses six possible combinations of block number and size.

Even with equal block number and size, the number of treatments the team wishes to test may exceed block size on each farm. The team may decide to reduce the number of treatments to match block size. (II,C,1,2 and 3) can help here. Reducing treatment number to match block size then allows the team to use a simple randomized complete block design. Or, the team may decide to use an incomplete block design, in order to use the larger set of treatments across all the farms.

Treatments may be single factor discrete (for example, different varieties) or single factor continuous (for example, spacing). Or treatments may be factorial (for example, different varieties and different spacings). Incomplete block design techniques are different for single factor and factorial treatments.

TABLE III,A.2 summarizes design options for refinement trials. The table is a decision tree. Each decision involves a separation in the first column. There are several levels of separation. The first level of separation is by treatment type: single factor (numbered 1) or factorial (numbered 2). Within each treatment type, the second level of separation is by block size diversity (numbered a and b). Within the two types of block diversity (equal vs. unequal), the third level of separation is by treatment number vs. block size comparison (numbered (1) and (2)). Within each treatment number vs. size comparison, further separation is by reduction of treatment number and treatment type (for factorials). The most appropriate design and treatment arrangement for each final separation device is shown in the second, third, and fourth

columns. The sections that explain the designs and treatment arrangements are shown in the fifth (last) column.

If the same treatments are placed on all farms in RCBD, the team can use modified stability analysis for biological performance, and confidence intervals and response surfaces for socio-economic evaluation. Alternatively, with either RCDB or IBD, the team can use ANOVA for biological performance and partial budgeting (including sensitivity analysis) for socio-economic evaluation. Both ANOVA and partial budgeting are based on combined analysis over farms.

TABLE III,A.2 Design Options for Refinement Trials

Treatment type, block size diversity, and treatment number vs. block size comparison	Design	Replication	Treatment arrangement	Section to refer to
1. Single factor				III,C,2
a. Block sizes equal across farms				
(1) treatment no. = block no.	RCBD	Across> within	Random within blocks	III,C,1,a
(2) Treatment no. > block size	IBD	Across> partial within	Random across blocks	III,C,1,b
b. Block sizes not equal across farm				
(1) Treatment no. = num. block size	RCBD	Across> within	Random within blocks	III,C,1,a
(2) Treatment no. > num. block size				
c. Reduce treatment no. to num. block size number	RCBD	Across> within	Random within blocks	III,C,1a

(TABLE continued...)

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Treatment type, block size diversity, and treatment number vs. block size comparison	Design	Replication	Treatment arrangement	Section to refer to
(ii) Treatment no. > num. block size	IBD	Across> partial within	Random within blocks with size > treatment no., random across smaller blocks	III,C,1b
2. Factorial a. Block sizes equal across farms				
(1) Treatment combination no. = block no.	RCBD	Across > within		III,C,1a
(i) NPK, varieties			complete factorial, random within blocks	III,C,2
(ii) land preparation, plant protection, spacing, intercropping			split plot	III,C.2
(2) Treatment combination no.> block no.				
(i) reduce to logical sub-set with treatment no. = min block size	RCDB	Across> within		III,B II,C,3
(a) land prepartion, plant protection, spacing, intercropping			split-plot	III,C,2

(TABLE continued...)

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Treatment type, block size diversity, and treatment number vs. block size comparison	Design	Replication	Treatment arrangement	Section to refer to
(ii) confounding or fractional replication	IBD	Across> partial within	Random within incomplete blocks	III,C,2
b. Block sizes not equal across farms				
(1) Treatment combination no. = min. block size	RCBD	Across> within		III,C,1,a
(i) NPK, varieties			complete factorial, random within blocks	III,C,2
(ii) land preparation, plant protection, spacing, inter-cropping			split-plot	III,C,2
(2) Treatment combination no. > min. block size				
(i) Reduce to logical sub-set with treatment no. = min. block size	RCBD	Across within		III B II,C,3

(TABLE continued...)

Treatment type, block size diversity, and treatment number vs. block size comparison	Design	Replication	Treatment arrangement	Section to refer to
(a) NPK, varieties			complete factorial, random within blocks	III,C,2
			split-plot	III,C,2
(b) land preparation, plant protection, spacing, intercropping				
(ii) confounding or fractional replication	IBD	Across> partial within	Random within incomplete blocks	III,C,2

4. CHARACTERISTICS OF VALIDATION TRIALS

At the end of refinement testing, the team can identify technology that it judges suitable for wider dissemination. The team does not rely only on its own judgment. The team seeks the help of farm households in checking its judgement. The team designs a validation trial for farm household members to evaluate the acceptability of the technology. If the technology is acceptable, the team's judgement is validated. If the technology is not acceptable, the team seeks to discover why, so it can change the technology to improve acceptability. Characteristics of validation testing include:

a. Response Objective

The response objective is primiarily to evaluate acceptability by farming unit members of the technology. For these reasons, the number of treatments is often only two, and at most no more than four.

Often the new technology will represent a combination of the best treatments from several factors, combined into a "techpack" (II,E). The new technology is compared against individual farmer control (II,C,4).

In some cases, the best treatment and an individual farmer control will be compared for two factors. This gives a 2 x 2 factorial, with four treatment combinations.

b. Replication Objectives

In validation testing, the team tests the technology developed in refinement testing on many more farms in each domain. The team tests the match between the technology and the farmer households in the domain in the presence of all the heterogeneity (differences among farms) remaining in the domain. If there is a good match between the technology and the domain, the team uses the domain as the focus for wider dissemination. If the match is poor, the team may again partition and do further refinement testing for a particular sub-group of farm households. A 2 x 2 factorial may be used if the team suspects there is still treatment-by-farm interaction for the best treatments of two factors.

With these replication objectives, there is no replication within farms, but replication across farms increases (30 or more).

c. Plot Size

Large plots are used, just as farm households would do even without a trial.

d. Block Size

Each farm has only one block, with only two - four plots. Block size is not usually an issue.

e. Researcher-Farmer Management Sharing

Researcher input is minimal, and primarily involves monitoring by observation and questioning farm household members about the trial. Extension personnel participate actively in the monitoring. Farm household members make all the management decisions and carry out the trial.

f. Design Options

Validation trials are also regional trials. All use RCBD. Treatment arrangement depends on treatment type (single factor or factorial). TABLE III,A.3 summarizes the options.

Analysis of biological performance is either by paired t-test or modified stability analysis. Analysis of acceptability is by observation or directed surveys, and is often followed by calculation of the acceptability index. Economic analysis is by confidence intervals for discrete treatments or response surfaces for continuous treatments.

TABLE III,A.3 Design Options for Validation Trials

Treatment type and number	Design	Replication	Treatment arrangement	Section to refer to
1. single factor	RCBD	Across farms only	Random within pairs	III,C,2,b
2. 2 x 2 factorial	RCBD	Across farms only	Random within blocks	II,C,1,a

OTHER SECTIONS THAT CAN HELP

III,B	What Designs Can Do
III,C	What Designs are Possible
IV,B	How to Lay Out Trials
V,A	Ways to Analyze and Interpret Data
V,B	Ways to Interpret Treatment Differences

WHAT CAN DESIGNS DO

OUTLINE

1. The Problem: Treatment Differences and Natural Variability
2. A Tool for Making Better Judgments: Principles of Statistics and Designs

PREREQUISITES

II,B	What Kinds of Fields are Available for Testing?
II,C	Refining Treatment Objectives
II,F	Looking Ahead: What are Some Trade-Offs Between Treatments and Replication?
II,D,2	Specification of non-experimental variables.

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this sub-unit, participants will be able to:

1. Identify two types of problems in interpreting treatment differences: natural variability of mean treatment differences, and treatment-by-environment interactions from one farm to another.

2. Explain how the F-ratio assesses the probability that treatment differences are real and not due just to random variation.

3. Explain why both randomization and replication are necessary in order to measure random variation.

KEY POINTS

1. Random (natural) variability is always present in biological data, no matter how uniform fields, farms, or herds of animals may appear.

2. Randomization allows us to assume any sample comes from a normal population, while replication allows us to measure the random variation of such a sample.

3. Designs give rules for randomization and replication to achieve different objectives.

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DEFINITIONS

control
environment
F-ratio
plot
randomiztion
replication
recommendation domain
researchable domain
sample
treatment
variance
variance ratio

ACKNOWLEDGEMENT

This section incorporates in part and with modification, text originally written by Clive Lightfoot, titled, "Statistical Analysis for On-farm Agronomic Data," and presented at the Visayas State College of Agriculture (VISCA), Philippines, July 1985, under the Farming Systems Development Project - Eastern Visayas (FSDP-EV). The editor thanks Clive Lightfoot and the FSDP-EV for sharing this material.

DISCUSSION

1. THE PROBLEM: TREATMENT DIFFERENCES AND NATURAL VARIATION

Most often people think that statistics is not particularly easy and not particularly useful. This section attempts to show that statistics is not so difficult and is very useful. It is useful because both farmers and researchers want to know if improved practices, or experimental treatments, yield more than existing farmers' practices, or the farmer control.

After conducting an experiment you need to interpret your data. More specifically, what does your data tell you? Sometimes, though rarely in on-farm experiments, data is so decisive that statistics has little role to play. For example, imagine a greenhouse experiment on chemical control of rice blast. Here, eight pots of rice plants are infected with blast, four of which are sprayed with a new chemical and four"control pots" are not sprayed.

The results obtained are shown in TABLE III,B.1 on the following page.

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TABLE III,B,1.

SPRAYED TREATMENT	CONTROL TREATMENT
2172	167
1750	195
1784	259
1902	174

In these data, it is not difficult to interpret what has happened. Spraying increased yield. This is obvious even though there is variation in the data for each treatment. On the other hand, what about this data from an on-farm experiment on maize?

TABLE III,B.2

FARM	FARMER CONTROL	EXPERIMENTAL TREATMENT
1	215.5	136.1
2	2562.5	944.4
3	775.9	439.8
4	1183.0	650.0
5	1011.6	330.0
6	1417.0	1578.7
Total	7165.5	4079.0
Mean (Average)	1194.2	679.8

These data show that the farmer control has a higher mean (average for the six farms) than the experimental treatment, but the degree of overlap is considerable.

The mean yield of the farmer control at 1194.2 is greater than the mean yield for the experimental treatment at 679.8 but there is considerable overlap in yields obtained: the experimental treatment ranges from a low of 136.1 to a high of 1578.7, while the farmer control ranges from a low of 215.5 to a high of 2562.5. Looking at the data there is no clear cut picture. It is difficult to know whether one is better than the other because both have high, low and middle yields. In fact, the high yield for the experimental treatment, 1578.7, is greater than the mean yield for the farmer control, 1417.0. On the other hand, the low yield for farmer control, 215.5, is less than the mean yeild for the experimental treatment, 679.8.

In this circumstance, one problem is to determine if the degree of overlap is so great that there is no consistent difference, on the average, between the performance of the farmer control and the mean performance of the experimental treatment. In other words, how likely is it that this difference in mean yields is due to the true effects of the treatment and not just to random chance coming from natural variation.

There is a second problem we might have in interpreting the data. Perhaps we might decide that indeed it is likely that the farmer control is better than the experimental treatment. After all, on the average, across all six farms, the farmer control yields almost twice as much (1,194.2 versus 679.8). We might decide not to recommend the new practice of the experimental treatment.

But what about farm 6? Here, just on farm 6, the result is the opposite of the result when we compared means. Instead of the farmer control being better, it is not as good as the experimental treatment (1,417.0 versus 1,578.7).

How do we interpret this result? Is the result on farm 6 just an accident of chance? For example, if we ran the experiment again, is it likely that the farmer control would be better on farm 6 the next time, but perhaps the experimental treatment by chance better on farm 4 the next time? Is our best conclusion to say, all six farms are similar, and the farmer control will usually be better, although occasionally perhaps not?

Another interpretation is also possible. Perhaps farm 6 is really different than the other farms. Perhaps if we ran the experiment again, we would again get similar results, with the farmer control better on farms 1-5, but not on farm 6.

For example, suppose the farmer problem identified in diagnosis is poor soil fertility. The farmer control is to apply fertilizer. The experimental treatment is to plant hedgerows, reducing erosion of topsoil while adding organic matter from the hedgerow trees. Perhaps there is less erosion of topsoil on farm 6, because of better maintenance of hedgerows. This means farm 6 has a different environment from farm 1-5. On farm 6, and other farms like it, farm households can save money by eliminating the fertilizer. On farm 1-5, however, maintenance of hedgerows is poorer, and there is more erosion. The experimental treatment is not effective. We have a treatment-by-farm interaction. (II,D and II,F) explain this type of interaction in more detail.

In this example, which interpretation is correct makes a big difference. Again our problem is one of natural variability. How likely is it that this difference in results on farm 6 versus farms 1-5 is due to a true difference in the environments of farm 6 versus farms 1-5, and not just to random chance coming from natural variation?

We have seen two types of problems in interpreting treatment differences: natural variability of mean treatment differences, and treatment-by-environment interactions from one farm to another. How do we make the right judgments? This is where statistics and designs can help. Statistics and designs give us a tool for making better judgments, and for knowing the chance that our judgment may be correct or incorrect.

2. A TOOL FOR MAKING BETTER JUDGMENTS: PRINCIPLES OF STATISTICS AND DESIGNS

Statistics is the science of variability: Statistics analyzes variability and applies objective standards for us to make judgments about variability in data.

Statistics cannot be applied to all data. It can only be applied when we can measure the random or natural variability of the data. We must follow certain rules to be able to measure random variability. These rules tell us where to place treatments. These rules are called designs.

Let's look at several principles of statistics and designs including probability, random assignment, and replication within and across farms.

a. Probability

The objective standards of statistics are based on the probability of obtaining unusual values. Let us return to the maize yield example again. Each farm had two treatments, the farmer control and the experimental treatment. Since there were six farms, there were twelve total data values.

Statistics looks at the data values in two different ways. First, it says, let's consider all twelve values as coming from the same type of plot. Looking at the data this way, we could rank the values from smallest to largest without regard to which farm or treatment they came from. However, to help us compare the rearranged data with the original data, we can add the place where the data came from, to the right of the values.

TABLE III,B.3

Data No.	Value	Where from Farm	Where from Treatment	Group
1	136.1	1	Experimental	A
2	215.5	1	Farmer	A
3	330.0	5	Experimental	B
4	439.8	3	Experimental	B
5	650.0	4	Experimental	B
6	775.9	3	Farmer	B
7	944.4	2	Experimental	C
8	1,011.6	5	Farmer	C
9	1,183.0	4	Farmer	C
10	1,417.0	6	Farmer	C
11	1,578.7	6	Experimental	C
12	2,567.5	2	Farmer	D
Total	11,244.5			
Mean	937.0			

How variable is this data? We could look at the range for measure of variability. The range is the difference between the largest and the smallest values, or 2,562.5 - 136.1 = 2,426.4. A

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better measure is called the standard deviation. For this data, the standard deviation is is 690.0. The overall mean plus the standard deviation is 1,597.0. The overall mean minus the standard deviation is 277.0.

Now let's group the data into four groups. The groups are labelled in the right hand column above.

A. This group has all the data with values less than the overall mean minus the standard deviation, or less than 277.0 There are two data values in it.

B. This group has all the data with values between the overall mean minus the standard deviation, 277.0, and the overall mean, 937.0. There are four data values in their group.

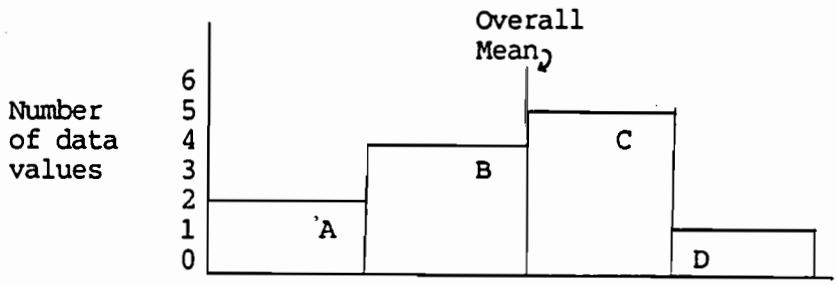
C. This group has all the data with values between the overall mean, 937.0, and the overall mean plus the standard deviation, 1597.0. There are five data values in group C.

D. This group has all the data with values greater than the overall mean plus the standard deviation, or greater than 1597.0. There is one value in group D.

The bar graph in Figure III,B,1 shows what the data looks like if we plot it by the four groups. the vertical axis shows how many data values each group has. a line between groups b and c shows the overall mean. this divides the graph into two halves. note that the two halves are roughly similar in appearance: larger next to the line, smaller away from it. we say that two halves are roughly symmetrical about the mean.

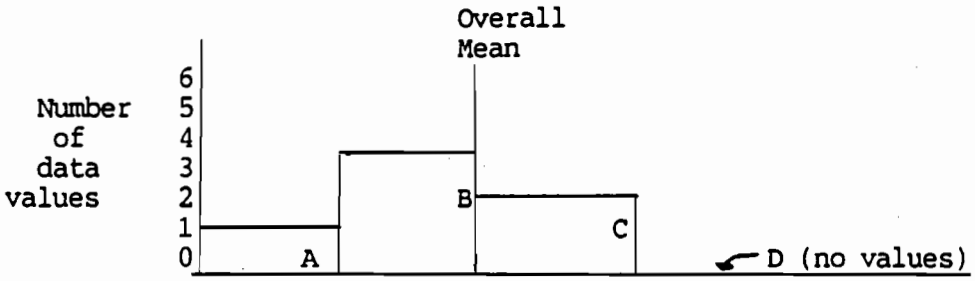
Statistics assumes that if we took data from all possible plots of the same type, the data values would be symmetrical about the mean. This is called "normal" data: data from a normal population of values. We thus expect most samples to also look similar, if they are from the same normal population. That is, we would expect data from a sample of another six plots (on more farms in the same area) to also be symmetrical about the same mean. Only unusual samples would not be symmetrical.

Figure III,B,1. Distribution of all 12 data values



Now let's look at the data another way. Let's consider the data values from experimental treatment as coming from a different type of plot than the data values from the farmer control. We can also count how many data values from the experimental treatment fall into the same original groups that were based on all 12 data values. This graph looks different:

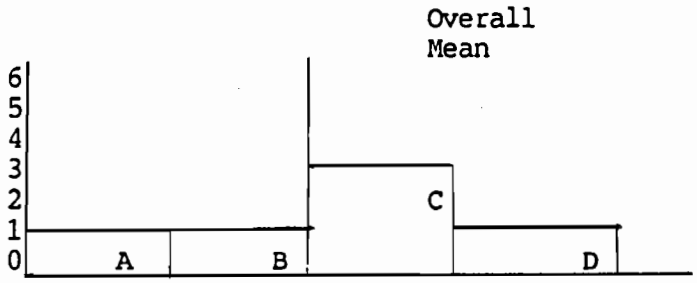
Figure III,B,2 Distribution of six experimental treatment data values



The left-hand side shows how many values are less than the overall mean. It is larger than the right-hand side, which shows how many values are greater than the overall mean. The data are not symmetrical about the original overall mean.

We can likewise count how many data values from the farmer control fall into the same original groups based on all twelve data values. This graph looks different from either of the 2 previous graphs:

Figure III,B,3



Here, the right-hand side showing how many values are greater than the overall mean is larger then the left-hand side showing how many values are less than the mean. These data are also not symmetrical about the original mean.

The graph for all twelve data values was roughly symmetrical about the overall mean, but this was not true for the graph for the six experimental treatment values, or for the graph for the

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six farmer control values. If the six experimental treatment values are a sample from the twelve, they are an unusual sample. What is the chance of drawing an unusual sample like the second graph, with more values to the left? What is the chance of drawing an unusual sample like the third graph, with more values to the right?

A basic method of statistical analysis is to compare variation due to treatments with natural variation. Variation due to treatments shows how different the treatment means are from the overall mean. In this example, the farmer control mean was 1,194.2, while the experimental treatment mean was 679.8. On the other hand, natural variation shows how different the twelve individual values are from the overall mean.

The two types of variation can be compared by a ratio of variances from the overall mean. This ratio is called the F ratio (the F comes from Fisher, the discoverer of the ratio). The ratio is:

$$F = \frac{\text{variance of treatment means}}{\text{variance of individual values}}$$

Because the F ratio uses variances, it is sometimes also called the variance ratio. (V,A) explains how to calculate the variances.

The F ratio will be large if the variance of treatment means is large. This will occur if the treatment means are very different, like the two bar graphs for the experimental treatment and the farmer control. Statistical textbooks contain tables that tell the probability of obtaining large ratios. That is, the tables give the probability of obtaining samples that are unusual. Computers have these tables built into their memory.

Statistics cannot prove that the farmer control is better than the experimental treatment. It indicates that the chance that farmer control is better than the experimental treatment has a certain probability: that it is likely to be better than the experimental treatment in 19 out of 20, or in 99 out of a 100 instances. The team in turn decides on what rate of being wrong is acceptable. In farming systems research, that risk depends on what risk is acceptable to farm households.

Statistics is not the end point of analysis. Based on the judgment of probability that statistics gives, the team may also do economic and social analysis of the treatments and the data must be interpreted and use made of the conclusions.

b. Random Assignment of Treatments

Replication means the same treatment appears more than once. This is necessary to obtain a measure of natural variation. This

measure is only valid however, if treatments have an equal chance of occurring in different locations. Let's take an example to see why this is so.

II,B explained ways in which one part of a field may be different from another part. It also introduced the concept of a homogeneous or uniform part.

Suppose a farm household plants a uniform part of a field with one crop, using the same seed of one variety. Farm household members apply the same fertilizer, weed the same, etc., throughout the field. Before they harvest, however, the research team divides the field into six small plots (Figure III,A.4). Then the farm household and the research team harvest and measure the yield from each plot individually. What can we expect the result to be? It is very unlikely that each plot's yield would be the same. Even though the whole area of six plots appeared uniform before planting, and the farm houshold grew the whole crop the same way, there is still natural variation in yield. Figures III,A.5 shows the variation in yields that occurred.

Figure III,B.4 Division of a uniform area into plots.

11	21
12	22
13	23

Figure III,B.5 Variation in yields in a uniform area where one crop is grown the same way.

	1320	1410	1365 (plots 11 and 21)
	1340	1380	1360 (plots 12 and 22)
	1300	1330	1315 (plots 13 and 23)
Means	1320 (plots 11, 12, and 13)	1370 (plots 21, 22, and 23)	

Now suppose the farm household had fertilizer for only half of the area. Since the white uniform area had six plots, they could put fertilizer on only three plots. Which three would they choose?

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Suppose the farm household put fertilizer on plots 11, 12, and 13 (Figure III,A.4). There is a problem here, though. These plots without fertilizer had lower yields than plots 21, 22, and 23. The mean was 1320, compared with 1370. Perhaps the fertilizer would increase yields on plots 11, 12, and 13, so that the mean of those plots was equal to the 1370 mean yield from plots 21, 22, and 23. However, we chose a uniform area, sowe would not know in advance that plots 11, 12, and 13 would have lower yeilds without fertilizer. We might instead conclude simply that fertilizer had no effect.

What if the farm household put the fertilizer on plots 21, 22, and 23? These plots had higher yields even without fertilizer. But we would not know that in advance. So we might conclude that fertilizer had a big effect, but part of the difference would be due to the fertilizer.

(II,C,1) defined treatments. In this example, there are two treatments: fertilizer, and no fertilizer. In the examples with figures 4 and 5, we had problsma in interpreting the results because we do not know in advance all the differnces in an area that appears uniform. There is only one solution to this problem: assign the two treatments at random to the plots.

What does at random mean? It means we do not decide. Chance decides. Only chance decides whether plot 11 gets fertilizer, or not, for chance means we use a randomization procedure. Randomization procedures must be mechancial. One mechanical way is to flip a coin. Another is to write numbers on pieces of paper, put them in a hat or box, shake them up, and draw them one at a time while not looking inside. The best way is to use a table of random numbers. Statistics texts have these tables and explain how to use them.

Randomization may sometimes be unlucky. It <u>may</u> result, by accident, in placing all the fertilizer treatments in plots 11, 12, and 13. As we saw earlier, this will hide the effect of fertilizer. But as long as the placement of fertilizer in plots 11, 12, and 13 is only by accident, then we can still use the data values. We are allowed to assume that the data values come from a normal distribution, and this allows us to test whether we have an unusual sample.

With randomization, the unlucky placement of treatments is only an accident. The next time we draw numbers from the box, fertilizer probably would go on plots 11, 12, and 13 again. Maybe we would be unlucky in the opposite way, and fertilizer might go on plots 21, 22, and 13. This would give us another sample of data values. In this sample, the effect of fertilizer would appear larger.

If we draw the numbers a third time, it is likely that we would get another combination. Maybe the third time, fertilize would go on plots 12, 22 and 13. We could draw the numbers many

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times, and repeat the experiment many times. If we added up all the samples of data values from plots with fertilizer and all the samples of data values from plots, without fertilizer, the differences just due to the plots would cancel out, and we would see the true effect of fertilizer. The variation in data values would be normal. Randomization is what allows us to assume any sample, even an unlucky one, comes from the underlying normal population which shows the true effect of fertilizer.

c. Replication Within and Across Farms

In the discussion on probability, we compared the variance of treatment values with the variance of individual means. Let's apply this principle to the previous example of the field with six plots in Figures III,B.4 and III,B.5.

Suppose we only divide that field into two large plots, the right-hand half (plots 11, 12, and 13) and the left-hand half (plots 21, 22, and 23). We also apply the principle of randomization, by flipping a coin. We say that heads will be fertilizer, tails no fertilizer, and the right-hand side of the field will get the treatment corresponding to the side of the coin that lands face up. The "tails" side lands face up, so we put "tails" (no fertilizer) on the right-hand side of the field, and fertilizer on the left-hand side (figure 6).

Figure III,B.6 No replication with a farm

right-hand: none	left-hand: fertilizer

At the end of the season, we help farm household members weigh the yield from each side. The yields are:

Right-hand side (no fertilizer)	1320
Left-hand side (fertilizer)	1500
Overall mean	1410

Since we have only one value for each treatment, that is the mean value of the treatment. The difference between treatment means and the overall mean is the same as the difference between individual values and the overall mean. We cannot calculate an F-ratio. Randomization without replication does not help.

With six plots, however, we can put fertilizer on three plots. The field might look like Figure III.B.6 after randomization (III,C.1) discuss how to do randomization for this example.

Figure III,B.7. Replication within a farm

11 fert.	21 none
12 none	22 fert.
13 none	23 fert.

At the end of the season, again we help farm household members weigh yields from each plot. The yields are:

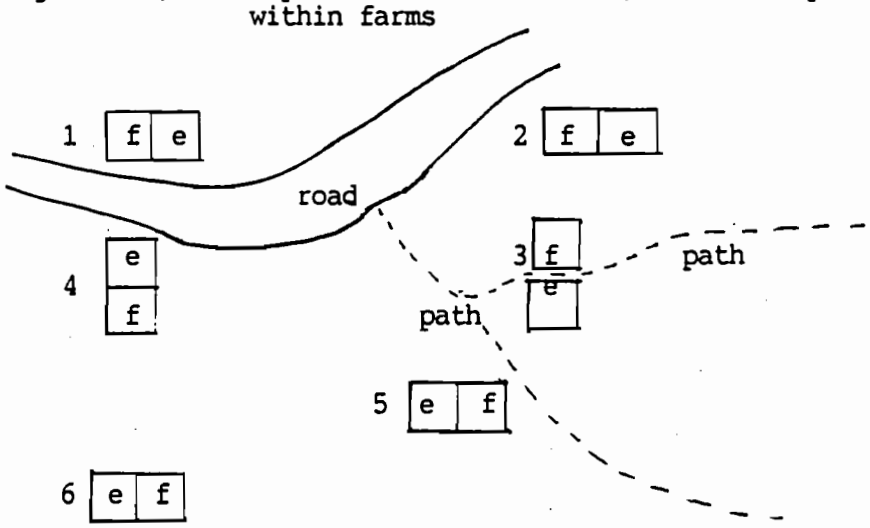
Figure III,B.8 Variation in yields.

			Fertilizer	No Fertilizer
fert. 1520	none 1410		1520	1410
none 1340	fert. 1560		1560	1340
none 1300	fert. 1510		1510	1300
Means			1530	1350

We now have three data values for each treatment, and a treatment mean. Each treatment is replicated three times. We can also compute the overall mean, from all six values, of 1440. Now we can compute separately the two variances for the F ratio: the variance of the two treatment means from the overall mean, and the variance of the six individual values from the overall mean.

In Figures III,B.7 and III,B.8 the replication is all within one field. We can say nothing about treatment effects on other farms from this experiment. Earlier, however, we looked at data from an on-farm maize experiment. There, the data came from six farms. The arrangement of treatments might be like Figure III,B.9 on the following page.

Figure III,B.9 Replication across farms, without replication within farms



The letters for each plot refer to the two treatments, "f" for "farmer control" and "e" for "experimental". The numbers refer to the six farms. Note that plots are non-contiguous (III,B) on farm 3. The location of the treatments ("e" or "f") on each farm is determined by randomization (this is in fact an example of a randomized complete block design, as explained in III,C,1,).

Are the treatments replicated? If we look only at farm 1, they are not. There is no replication within farms. However, if we look at all six farms as a whole, the treatments are replicated, six times total. Natural variation B measured for each treatment from one farm to another, but not within each farm. This is an example of replication across farms, but without replication within farms.

Replication across farms allows us to measure variation within the whole researchable domain. The value of this measure depends on how representative the farms are of the farmer grouping making up the researchable domain. If the grouping is heterogeneous, but the trial farms are still similar, then the replication among farms will not reflect all the variation in the grouping. If the farms are representative of the variation of the whole grouping then the trial provides a valid measure of the degree of homogeneity in the domain of the farm households' management of the treatments.

In the maize example, however, we had a second problem in interpreting the data. This was the case of farm six. Overall, the experimental treatment was not as effective as farmer control, when we looked at the means for the two treatments. This was also true for the individual treatment values on farms 1-5. Only on farm 6 was the experimental treatment better.

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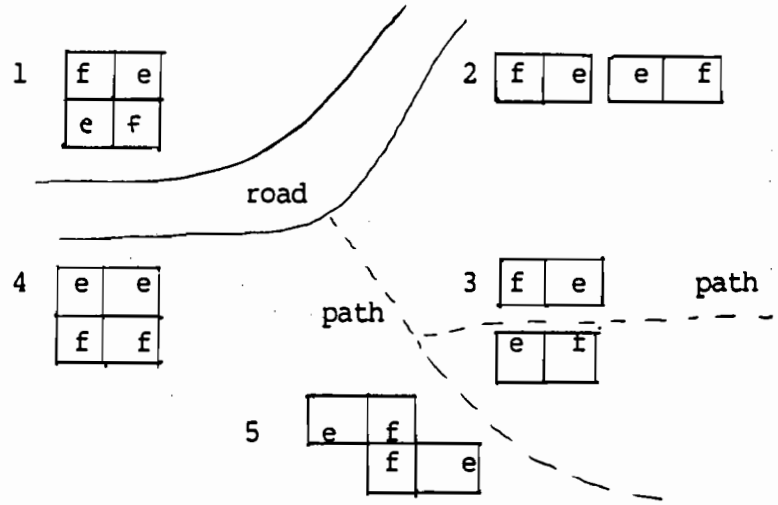
This appears to be an example of a treatment-by-farm interaction. The only way to measure this interaction is by comparing it against natural variation within each farm that requires replication within farms, in addition to replication across farms. The simplest case would be two replications per farm, as in Figure III,B.10 Designs with replication across and within farmers allow us to assess treatment-by-environment interactions.

Figure III,B.10. Replication within and across farms



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ACTIVITIES

ACTIVITY ONE: PRACTICAL CALCULATIONS

OBJECTIVE:

After completing this exercise you will be better able to:

1. Interpret treatment differences.

INSTRUCTIONS:

Assume three treatments (a, b and c) were each replicated four times, at random. The data are shown:

	Treatments		
	a	b	c
	4	9	16
	3	10	17
	5	11	16
	4	10	15
Totals	16	40	64
Means			

Looking at these data, we would intuitively feel confident that c was better than a and b, and b than a. The ranges of values are respectively 3 to 5, 9 to 11 and 15 to 17. Clearly there is no overlap.

1. Calculate means
2. Calculate ranges
3. Calculate overall mean and range
4. Calculate treatment means
5. Rank and make bar graphs
6. Compare treatment means and ranges
7. Discuss

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Assume now that the yields had been as follows:

	Treatments		
	a	b	c
	5	9	5
	10	2	8
	3	4	17
	6	5	6
Total	24	20	36

1. Calculate means
2. Calculate ranges
3. Calculate overall mean and range
4. Calculate treatment means
5. Rank and make bar graphs
6. Compare treatment means and ranges
7. Discuss

(III,C,1,a)

WAYS TO REPLICATE TREATMENTS WITHIN AND ACROSS FARMS: COMPLETELY RANDOM DESIGNS AND RANDOMIZED COMPLETE BLOCK DESIGN

OUTLINE

1. Completely Random Designs
2. Randomized Complete Block Design

PREREQUISITES

I	What Kind of Testing to Do
II,B	What Kinds of Fields Are Available for Testing
II,F	What Are Some Tradeoffs Between Treatments and Replications
III,B	What Designs Can Do

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this sub-section participants will be able to:

1. Identify situations when completely randomized design (CRD) and randomized complete block design RCBD are appropriate.

2. Use a random number table for randomization with replacement (CRD) and randomization without replacement (RCBD).

3. Translate randomization sequences onto field maps (plot plans).

4. Better understand why trial efficiency can be increased by orderly trial design (RCBD) rather than strictly random placement in a trial.

KEY POINTS

1. CRD can be used in site-specific trials if the field has no known gradients.

2. RCBD should be used in site-specific trials if their is a known or possible gradient.

3. RCBD can be used in regional trials, with each farm having one or more blocks.

4. The randomized complete block design is more efficient at detecting real differences between treatments than is a completely random design.

DEFINITIONS

block
completely randomized design (CRD)
exploratory trials
plot
randomization
randomized complete block design (RCBD)
refinement trials
treatment

DISCUSSION

1. COMPLETELY RANDOMIZED DESIGN (CRD)

In CRD, the team does not group plots into blocks. The team assigns treatments with randomization over all plots. Any treatment has an equal chance of being assigned to any plot. There is no pattern to the replication of treatments. Every treatment is replicated the same number of times.

CRD can be used in site-specific trials if the field for the trial has no gradients. A site-specific trial is an exploratory trial conducted at only one location - (see Hildebrand and Poey). A gradient means that the field has systematic differences in a given direction. For example, soil fertility may be highest at the top of the field and decrease steadily as one moves down the field (figure 1 field B). In such cases, the team should use randomized complete block design instead. In field A in figure III,C,1,a.1, CRD would be appropriate.

Figure III,C,1,a.1 Fields appropriate or not appropriate for CRD.

similar fertility
similar fertility
similar fertility

Field A: Appropriate

high fertility
medium fertility
low fertility

Field B: Not appropriate

Randomization over all plots proceeds in four steps:

1. Draw plots on a map of the field. The number of plots equals the number of treatments times the number of replications.

2. Assign numbers to treatments.

3. Draw random numbers with replacement. If using a random number table, ignore any numbers larger than the highest treatment number. Drawing with replacement means that the same treatment number can appear more than once before other treatment numbers. When the same treatment number appears as many times as it will be replicated, then ignore that treatment number in the remaining drawings.

4. Write the treatment numbers in the plots in a systematic manner, according to the random treatment sequence.

Example 1: Site-specific CRD with three replications (figure 1 field A).

Step 1: Draw plots on a map

No. of plots = no. of treatments x no. of replications
= 3 x 3
= 9

plot no's.

1	2	3
4	5	6
7	8	9

Step 2: Assign numbers to treatments

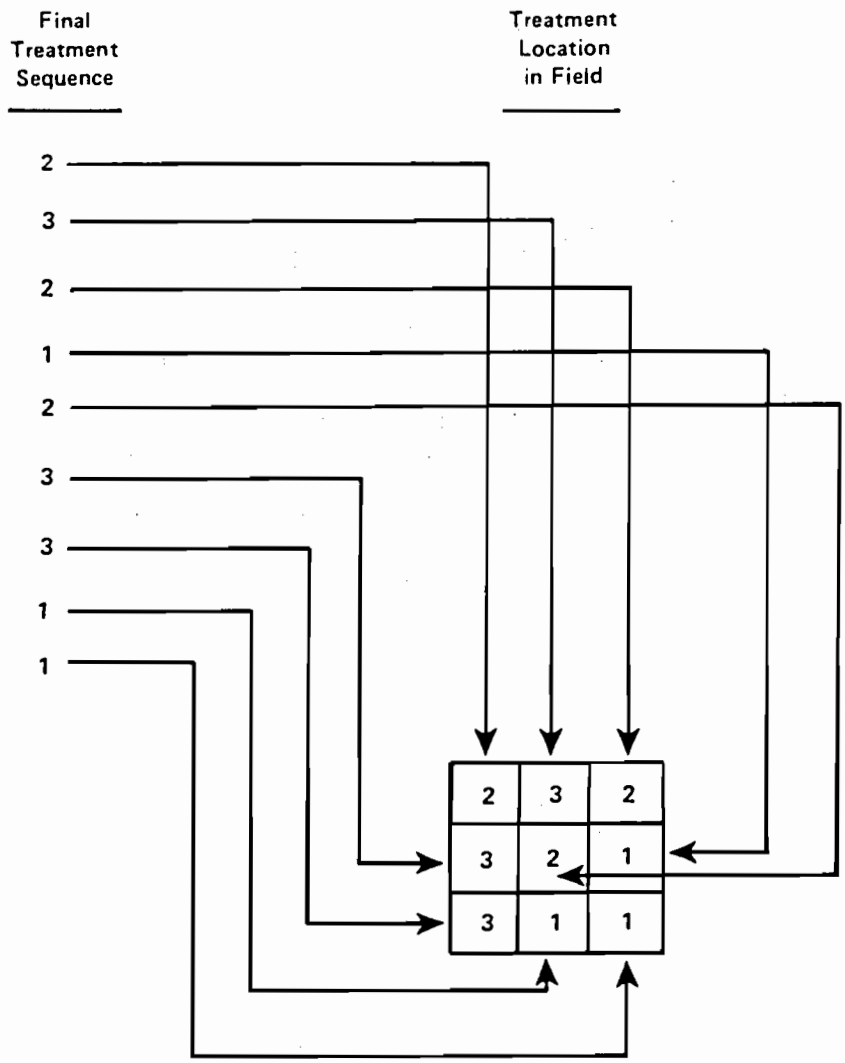
Treatment no.	Treatment
1	no weeding
2	weeding by hand
3	pre-plant application of 'Knock-out'

Step 3: Draw random numbers with replacement

sequence of random numbers from table of random numbers	action taken	final treatment sequence	no. of times treat-ment has appeared
2	assign to plot 1	2	1
5	ignore		
5	ignore		
4	ignore		
7	ignore		
3	assign to plot 2	3	1
7	ignore		
8	ignore		
2	assign to plot 3	2	2
9	ignore		
6	ignore		
1	assign to plot 4	1	1
0	ignore		
5	ignore		
2	assign to plot 5; note that treatment no. 2 now replicated three times	2	3
6	ignore		
8	ignore		
0	ignore		
9	ignore		
3	assign to plot 6	3	2
8	ignore		
9	ignore		
6	ignore		
4	ignore		
3	assign to plot 7; note that treatment no. 3 now replicated three times.	3	3
(stop drawing random numbers)	Only two plots remain, and treatment no. 1 has only been replicated one time, so plots eight and nine must get treatment no. 1	1 1	2 3

Step 4: Write treatment numbers in a systematic manner (for example, in 5-fashion: start from top left, go across, return across next row from right, then go across last row from left)

Figure III,C,1,A.2 Final Treatment Sequence and Treatment Location In Field



Result

hand weeding	'knock-out'	hand weeding
'knock-out'	hand weeding	no weeding
'knock-out'	no weeding	no weeding

2. RANDOMIZED COMPLETE BLOCK DESIGN (RCBD)

In RCBD, the team first groups plots into blocks. The number of plots in each block equals the number of treatments. The team assigns treatments with randomization within each block. Each block is a replication.

RCBD can also be used in site-specific trials. A team should always use RCBD if it can identify a gradient in the field (like figure 1, field B). If a team is unsure if there is a gradient, it is probably safer to use RCDB.

Randomization within each block requires starting the randomization process four times. Each time one can start at a new location in the table. The steps are:

1. Block the field in the direction of the gradient.
2. Divide blocks into plots, with number of plots per block equal to number of treatments.
3. Assign numbers to treatments.
4. Draw random numbers <u>without</u> <u>replacement</u>. If using a random number table, ignore numbers larger than the highest treatment number. Also ignore the same treatment number once it has been drawn.
5. When all treatment numbers have been drawn once, this completes block I. Then repeat step two for blocks II, III, etc.
6. Write the treatment numbers in each block according to the random treatment sequence.

Example 2: Site-Specific RCBD With Three Replications (figure III,C,1,a.1 field B)

Step 1: Block field

blocks		
I	higher fertility	direction
II	medium fertility	↓ of
III	lower fertility	gradient

Step 2: Divide blocks into plots

I	1	2	3
II	1	2	3
III	1	2	3

Step 3: Assign numbers to treatments

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treatment no.	treatment
1	no weeding
2	weeding by hand two times
3	pre-plant application of 'knock out'

Step 4-5: Draw random numbers without replacement, with a new start for each block

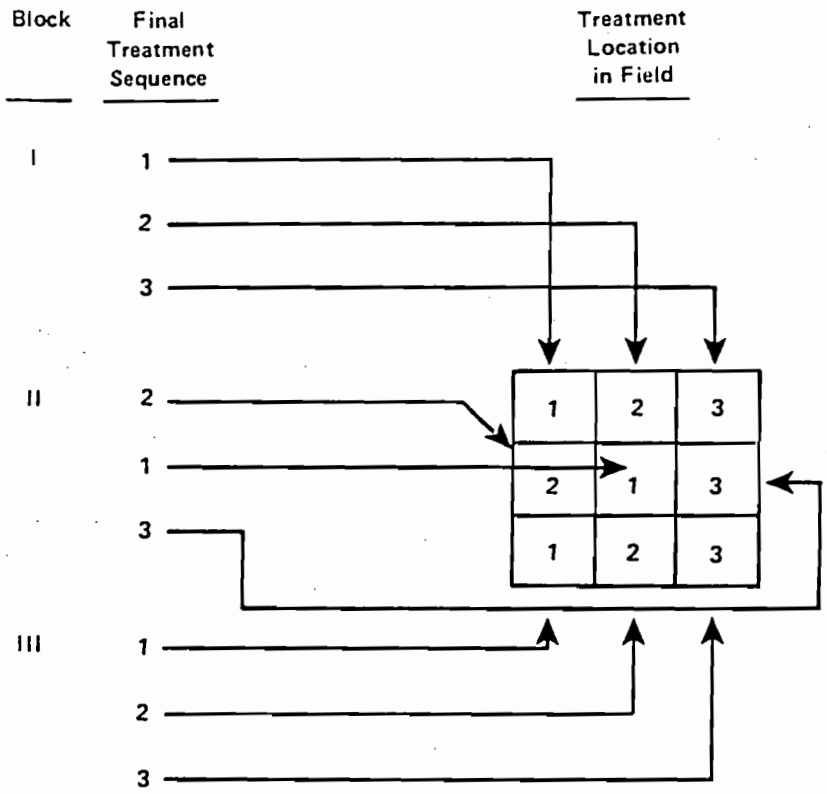
sequence of random numbers from table of random numbers	action taken	final treat-ment sequence	no. of times treatment has appeared
5	ignore		
1	assign for plot 1,blockI	1	1
8	ignore		
5	ignore		
1	ignore, since treatment no. 1 already drawn for block I		
8	ignore		
4	ignore		
1	ignore (already drawn)		
9	ignore		
7	ignore		
6	ignore		
1	ignore (already drawn)		
6	ignore		
9	ignore		
4	ignore		
5	ignore		
7	ignore		
4	ignore		
2	assign to plot 2, block I.	2	1
(stop drawing random numbers)	only one plot remains in block 7, so plot 3 in block I must get treatment no 3:	3	1
(new start)			
2	assign to plot 1,block II	2	2
7	ignore		
8	ignore		
7	ignore		

(table continued)

sequence of random numbers from table of random numbers	action taken	final treat-ment sequence	no. of times treatment has appeared
4	ignore		
2	ignore (already drawn in block II)		
0	ignore		
0	ignore		
1	assign to plot 2, block II	1	2
(stop drawing random numbers)	only one plot remains in block II, so plot 3 in block II must get treatment no. 3:	3	2
(new start)			
4	ignore		
1	assign to plot 1, block III	1	3
4	ignore		
5	ignore		
2	assign to plot 2, block III	2	3
(stop drawing random	only 1 plot remains in block III, so plot 3 in block III must get treatment no. 3:	3	3

Step 6: Write treatment numbers in blocks

Figure III,C,1,a.3 Block Final Treatment Sequence



Result

I	no weeding	hand weeding	'knock-out'
II	hand weeding	no weeding	'knock-out'
III	no weeding	hand weeding	'knock-out'

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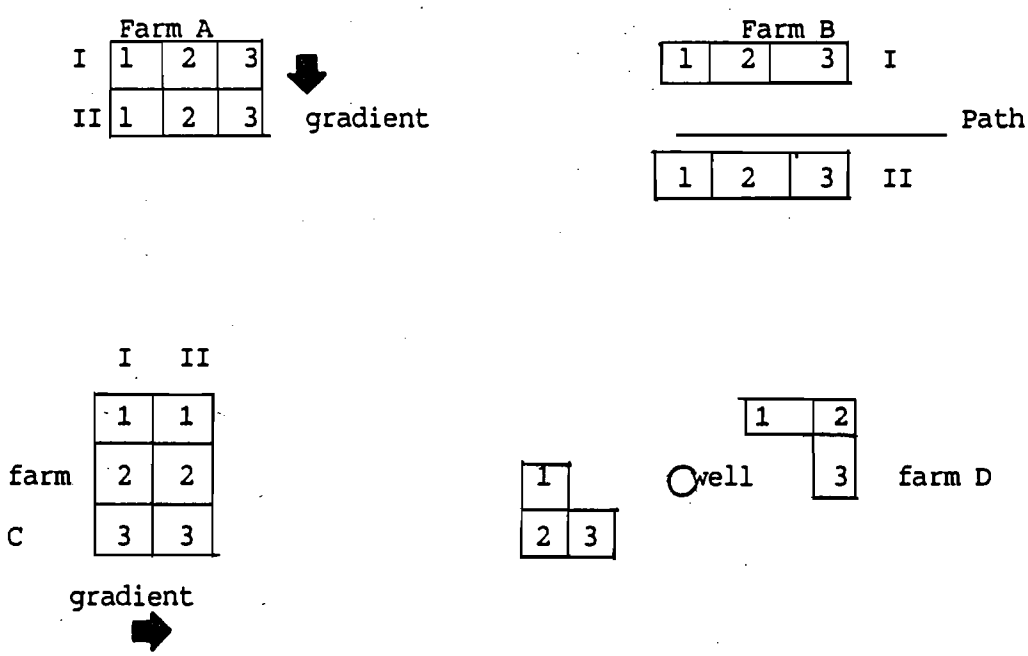
Site-specific trials are like on-station trials: plots and blocks are usually all contiguous. Site-specific trials are more common in exploratory testing. In refinement testing, regional trials are more common. Here, each farm can be a block, or each farm can have 2 or more blocks.

Suppose the same weed control trial would be tested on four farms, and each farm would have two blocks. Example 3 shows how to randomize for this RCBD.

Example 3: Regional RCBD with six farms,
two blocks/farm.

Step 1: Block each farm and divide blocks into plots



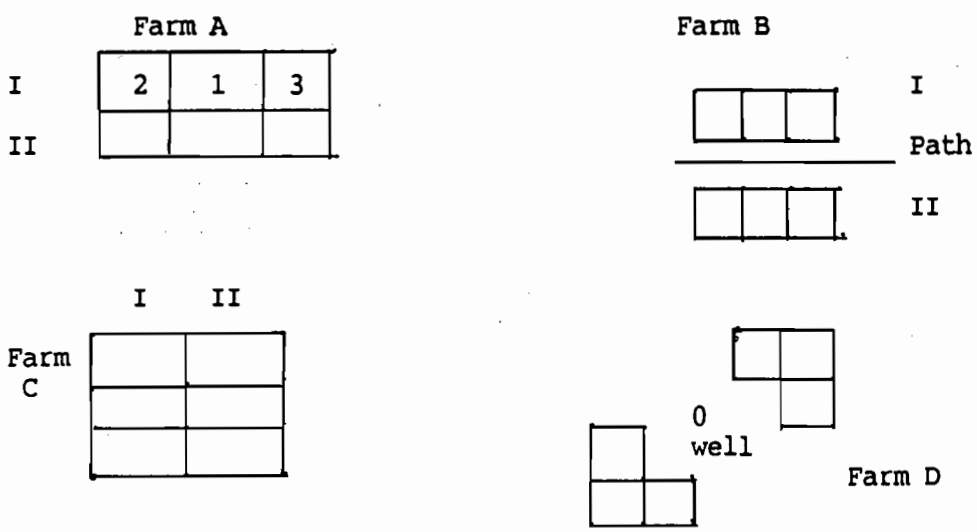
Step 2: Assign numbers to treatments

treatment no.	treatment
1	No weeding
2	Weeding by hand two times
3	Pre-plant application of 'knock-out'

Step 3: Draw random numbers without replacement, with a new start for each block.

sequence of random numbers from table of random numbers	action taken	final treat-ment sequence	No. of times treat-ment has appeared
4	Ignore		
6	Ignore		
7	Ignore		
8	Ignore		
2	assign to plot 1, block I, farm A	2	1
1	assign to plot 2, block I, farm A	1	1
(stop drawing numbers)	plot 3, block I; must get treatment no. 3:	3	1
(new start each block on each farm)	(details not shown; participants should complete)		

Step 4: Write treatment numbers in blocks on each farm



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OTHER SECTIONS THAT ARE USEFUL

Additional information may be found in the CARDI manual, pp. 5-8.

(III,C,1,b)

(OPTIONAL) INCOMPLETE BLOCK DESIGNS ACROSS FARMS

OUTLINE

1. Incomplete Block Designs

PREREQUISITES

II,B	What Kinds of Fields Are Available for Testing
II,F	What Are Some Trade-offs Between Treatments and Replications
III,B	What Can Designs Do
III,C,1,a	Ways to Replicate Treatments Within and Across Farms

PARTICIPANT LEVEL

College or experiment station researcher
Extension subject matter specialist

LEARNING OBJECTIVES

After completing this sub-section, participants will be able to:

1. Apply balanced designs from statistical texts to farms in regional trials.

2. Assign control and new treatments in augmented designs to farms.

KEY POINTS

1. Balanced designs place restrictions on the relationships among numbers of treatments, plots per block, and replications, but they have desirable statistical properties.

2. Augmented designs have one or more control treatments in every block, but other treatments do not appear in every block.

3. An incomplete block design may be necessary when complete designs are too large to fit in an average farm.

4. An incomplete block design may be useful when not all inter and comparisons are hypothesized as being important or of use to the farmers or research team.

DEFINITIONS

randomization

DISCUSSION

1. INCOMPLETE BLOCK DESIGNS (IBD)

In IBD, the team first groups plots into blocks. The number of plots in each block does not equal the number of treatments. Below are four sub-types of IBD:

a. Balanced Lattices
b. Balanced Incomplete Blocks
c. Incomplete Blocks With Supplemented Balance
d. Incomplete Blocks of Unequal Size

Randomization in all incomplete block designs can use treatment arrangement plans developed by statisticians. These plans assure that each pair of treatments appears together equally often within some block. Randomization using such plans follows three steps:

a. Randomization of replications from the plan
b. Randomization of blocks within each replication from the plan
c. Randomization of treatments within each block of the plan.

In balanced lattices, the number of treatments is an exact square (9,16,25, etc..). The number of plots in each block (block size) equals the square root of the number of treatments (3,4,5,etc..). The total number of plots in each replication equals the number of treatments (9,16,25,etc.). The number of replications equals the block size plus one (4,5,6, etc.).

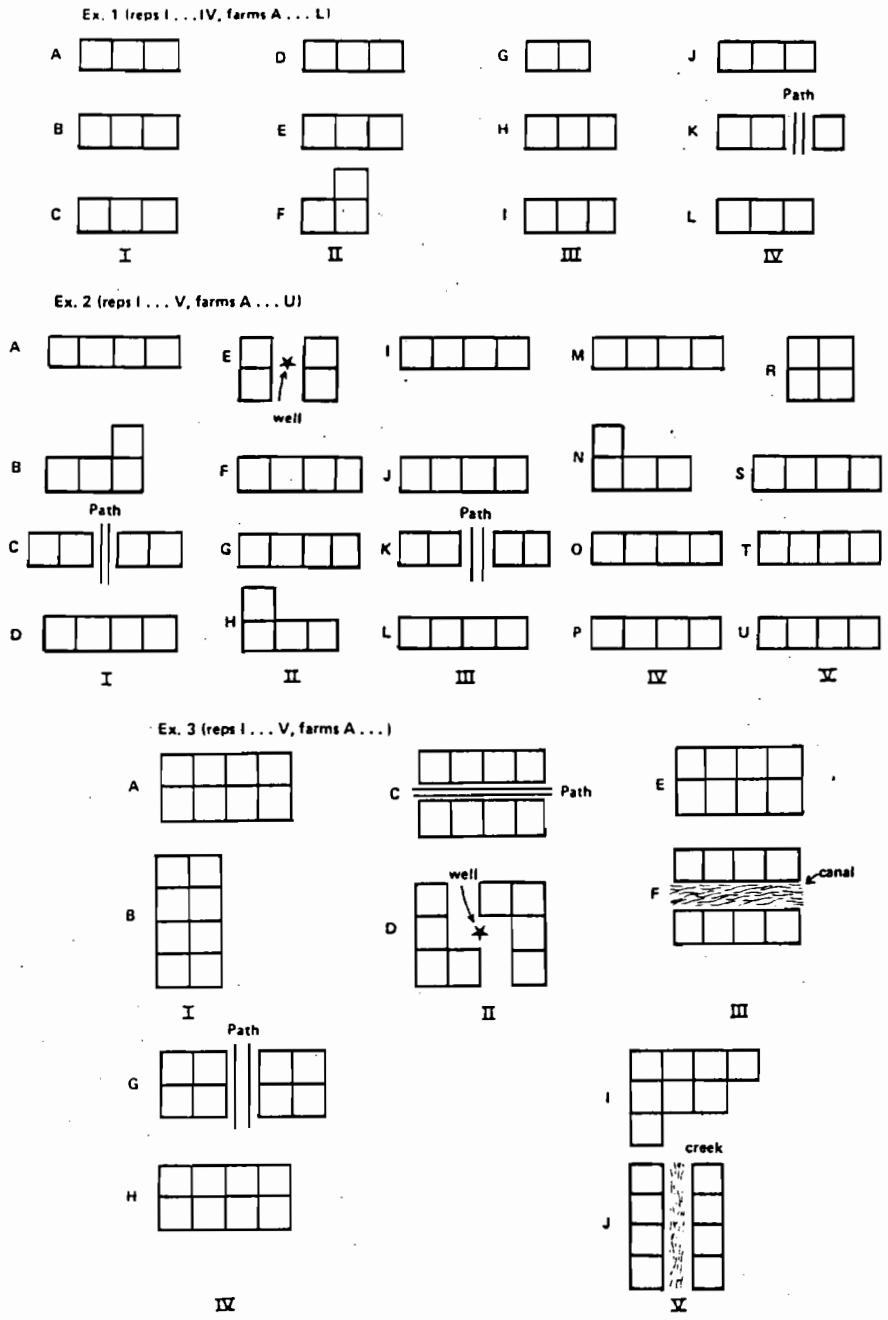
The above restrictions result in some desirable statistical properties. On the other hand, they require a match between number of treatments and number of farms. Table III,C,1,b.1 and figure III,C,1,b.1 show three examples of how balanced lattices could be applied to regional trials. Note that the farms in each replicate should be more similar to one another.

The team assigns treatments with randomization using a plan from Gomez and Gomez, or Cochran and Cox publications.

TABLE III,C,1,b.1 Examples of balanced lattices applied to regional trials.

Example	Total farms	repli-cations	farms/ replic.	blocks/ farm	plots/ block	treatments/ replication
1	12	4	3	1	3	9
2	20	5	4	1	4	16
3	10	5	2	2	4	16

Figure III,C,1,b.1 Examples of balanced lattices applied to regional trials.



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Partially balanced lattices are also possible. Requirements for treatment number, block size, and number of blocks in each replication are the same as for balanced lattices, but the number of replications is not fixed. There are several randomization procedures (Gomez and Gomez, pp. 52-53), and analysis is more complex.

In balanced incomplete blocks, the number of treatments does not have to be an exact square. The number of plots in each block varies. To find the number of blocks in a replication, first divide the number of plots in each block into the number of treatments. If the number obtained from that division is an even number, then that number is also the number of blocks in each replication. The total number of plots in each replication equals the number of treatments. The team assigns treatments with randomization using a plan from Cochran and Cox (pp. 469-482). Table III,C,1,b.2 gives some examples of the application of balanced incomplete block designs to regional trials.

If the number of plots in each block does not divide evenly into the number of treatments, then multiply the number of plots in each block by the number of treatments. That product is the total number of plots in a group of replications. Within each group of replications, the number of replications of each treatment equals the number of plots per block.

TABLE III,C,1,b.2 Application of balanced incomplete block designs to regional trials.

Ex.	Plan*	NO. of					
		total farms	repli-cations	farms/ repli-cation	blocks/ farm	plots/ farm	treat-ments/ repli-cation
1	11.1	6	3	2	1	2	4
2	11.3	15	5	3	1	2	6
3	11.5	20	10	2	1	3	6
3	11.6	15	10	3	1	4	6

* Cochran and Cox, pp. 471-472

In incomplete blocks with supplemented balance, the team first assigns one or two control treatments with randomization to every block. The team then assigns the remaining treatments with randomization over the remaining plots.

In incomplete blocks of unequal size, the number of plots in some blocks equals the number of treatments. For those blocks, the team assigns treatments with randomization within each block. In the remaining blocks, the number of plots is less than the number of treatments. The team assigns treatments in the remaining blocks using one of the incomplete block designs discussed above.

OTHER SECTIONS THAT CAN BE USEFUL

Additional information can be found in the CARDI manual pages 11-19 and 32-36.

An IDB example is given on pp 327-328 Shaner (1982).

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(III,C,2)

WAYS TO COMBINE TREATMENTS WITHIN REPLICATIONS

OUTLINE

1. One Type of Treatment: Discrete or Continuous
2. More Than One Type of Treatment: Complete Factorials
3. (Optional) Handling Many Types of Treatments: Incomplete Factorials
4. Paired Comparisons of Only Two Treatments

PREREQUISITES

II,B	What Kinds of Fields Are Available for Testing
II,F	What Are Some Tradeoffs Between Treatments and Replications
III,B	What Designs Can Do

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After finishing this section the participants will be able to:

1. Distinguish between discrete and continuous variables.

2. Determine the number of treatments applied to each plot and the total nunmber of treatment combinations for complete factorials.

3. Assign treatment combinations or levels of factors of complete factorials in factorial and split-plot arrangements.

4. (optional section) Identify four alternative ways of handling factorials with large numbers of treatment combinations.

5. (optional section) Explain the method of construction, method of randomization, advantages, and disadvantages of confounding based on one interaction and confounding based on several interactions.

6. (optional section) Explain the method of construction, method of randomization, advantages, and reasons for caution in use of fractional replication.

KEY POINTS:

1. The number of treatments which the team applies to each plot

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differs for single factor and complete factorial arrangements.

2. Randomization of complete factorials depends on the method of arrangement.

3. (optional) The choice of the best confounding plan depends on prior information about interactions between or among the factors.

4. (optional) The choice of the best fractional replication plan depends on prior information about interactions between or among the factors.

DEFINITIONS:

aliases
complete factorials
completely factorial arrangement
confoundry
CRD
experimental design
factor
factorial experiment
fractional replication
IBD
incomplete blocks
incomplete factorials
randomization
RCDB
refinement trials
replication
split-plot arrangement
treatment
treatment combinations
validation trials

DISCUSSION:

1. ONE TYPE OF TREATMENT: DISCRETE OR CONTINUOUS

In single factor arrangements, the researcher applies only one type of treatment to each plot. Treatments may be discrete or continuous. Examples of discrete factors are varieties and pesticide types. The team would assign a different variety or pesticide to each plot. Examples of continuous factors include nitrogen fertilizer rates, or dosage levels of a given pesticide. The team would assign a different nitrogen rate or dosage level of a pesticide to each plot. The team can use any design, including CRB, RCBD, or IBD, for single factor arrangements, depending on trial function, farm setting, field size and management considerations.

2. MORE THAN ONE TYPE OF TREATMENT: COMPLETE FACTORIALS

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In factorial arrangements, the team assigns two or more different types of treatments to the same plot. Each type of treatment consists of a group of related treatments, called a factor. The number of related treatments making up each factor is the number of levels of the factor. The total number of treatment combinations equals the product of number of levels of each factor.

In complete factorials for RCBD, the number of plots in each block equals the number of treatment combinations. In factorial arrangements, the team assigns each treatment combination with randomization within each block:

Step 1: Obtain treatment combinations by multiplying levels of factor A by levels of factor B (and so forth for higher-order factorials).

Step 2: Label and assign a number to each treatment combination.

Step 3: Draw random numbers to determine the sequence of treatment combinations in block II. Repeat for each succeeding block.

Step 4: Write treatment combination labels in plots according to sequence obtained for each block.

Alternatively, in split-plot arrangements of complete factorials, the team assigns levels of one factor to mean plots with randomization within each block. The team then assigns levels of the second factor to subplots with randomization within each main plot:

Step 1: Label and assign a number to each main plot level.

Step 2: Draw random numbers to determine the sequence of main plot levels in block I. Then draw random numbers anew to determine the sequence of main plot levels in block II. Repeat for each succeeding block.

Step 3: Write labels for the main plot levels next to each main plot in each block.

Step 4: Label and assign a number to each subplot level.

Step 5: Draw random numbers to determine the sequence of subplot levels in the first main plot of block I. The draw random numbers anew to determine the sequence of subplot levels in the second main plot of bolck I. Repeat for each succeeding main plot, if any, in block I.

Step 6: Repeat the process of step 5 for each succeeding block.

Step 7: Write labels for subplot levels in plots of each main

plot in each block.

3. (OPTIONAL) HANDLING MANY TYPES OF TREATMENTS: INCOMPLETE FACTORIALS

If either the number of levels for each factor is large, or the number of factors is large, then there will be many treatment combinations. If the number of treatment combinations is greater than the number of plots in each block for RCBD, the team has four choices:

a. Select a logical subset of only the combinations likely to be meaningful to farmers and assign treatments to plots in RCBD.

b. For continuous variables, select treatment levels using the central composite technique, and assign treatments to plots in RCBD (Hildebrand and Poey, 1985: 61-62).

c. Use an IBD with appropriate confounding of all treatment combinations.

d. Use appropriate fractional replication.

In confounding, the team has prior information on, or hypotheses about, which interactions are likely to be weak, and divides the treatment combinations into groups based on those interactions.

e. Step 1: Obtain treatment combinations
The number of treatment combinations in each group equals the number of plots in each block. The team assigns treatment combinations with randomization within each block, and randomization of blocks within each replication.

If the team chooses to confound only one interaction, the team uses the same groups of treatment combinations in every replication. This allows complete replication of the other interactions, but no test is possible for the completely confounded interaction. Alternatively, the team can confound several interactions. In this case, the team uses different groups of treatment combinations in each replication. Tests can be made for the partially confounded interactions, but they are based on fewer replications, and analysis is more complex.

In fractional replication, the team uses only one group of treatment combinations, based on a higher interaction of lesser importance. This group of treatment combinations is a whole fraction (half, one fourth, etc.) of all treatment combinations of the full factorial. The team then divides the remaining treatment combinations into sub-groups based on remaining interactions of less importance, as with confounding. The number of treatment combinations in each sub-group equals the number of plots in each block, and the team assigns treatment combinations

with randomization in each block.

Since the experiment uses only a sub-set of all the treatment combinations of the complete factorial and often does not replicate the sub-set, the experiment cannot distinguish among some effects. These effects are called aliases.

Confounding and fractional replication are discussed further, with examples, in the CARDI manual, pp.20-22, 23-31, and Hildebrand Poey, p.37.

4. PAIRED COMPARISONS OF ONLY TWO TREATMENTS

For paired-t tests, the trainee is to refer to the CARDI manual, pp 7-9.

UNIT IV
HOW TO CARRY OUT TRIALS

ACTIVITY ONE TRAINERS' NOTES
INPUT MEASUREMENTS AND PLOT PLAN PREPARATION

OBJECTIVES:

After completing this activity the participants will be able to:

1. Prepare the necessary seeds and assemble the necessary materials and inputs to allow them to lay out a series of systematic trials in farmers' fields.

2. Weigh out samples of dry inputs and measure out samples of liquid inputs with proper consideration of safety rules, water measurement, mixing of inputs, and calibration of simple back-pack spray equipment for small plot treatments.

TIME:

MATERIALS: Refer to instruction #3 for detailed listing of needed materials.

INSTRUCTIONS:

1. The following field exercise requires some preparation in advance by yourselves and the trainees. Regarding the field exercise, you may either follow it or develop your own more relevant exercise. Any exercise used should be adapted to the local situation, conditions, cropping systems, methods of planting, etc. This will require you to:

   a. Have site-specific knowledge of the area where the field exercise will be carried out, or

   b. Take a day or two to acquire the necessary knowledge of the local situation.

   Regardless of your personal situation, if the field exercise is to take place on a collaborating farmer's field, you will have to set the arrival time with him or her in advance, and verify the field trip the afternoon before it actually occurs.

   If the exercise is to be simulated on an experiment station, you will need to obtain a location from the station manager, and confirm with that individual the date of the exercise.

2. Divide the workshop participants along disciplinary lines into work groups of approximately four each. It is best not to have more than four total work groups.

3. Working back from the date for the field exercise, you will need to accomplish these two tasks:

a. Prepare the necessary materials and inputs for the trainees to complete the field exercise. This task includes assembling the following supplies for each work group, or at least confirming that it is available to you and the workshop participants during the morning or afternoon of this field exercise preparation activity:

— a simple laboratory balance, pan type (may be shared between groups),

— assorted measuring devices: graduated glass or plastic cylinders, containers for dry measures, plastic buckets (approximately 10 liters in size), spatulas, scoops, spoons, etc.,

— necessary seed (tuber, cutting, root, etc.) varieties in sufficient quantities for all sets of trials,

— sufficient seed envelopes (or other containers) in which to place counted seeds, tubers, cuttings, roots, etc.,

— markers (pens, pencils) for envelopes and field books,

— inputs required (fertilizer, insecticide, fungicide, nematocide, herbicide, etc.) in their original containers, in the proper formulations, and in sufficient quantity for all work groups,

— equipment for handling inputs and planting materials: plastic buckets, cans, burlap bags or plastic carrying bags, backpack sprayers, stirring sticks, etc.

b. Assemble the required equipment and supplies for each group to carry out the field trial lay out and planting exercise on the day after this field exercise preparation activity. Each work group will need the following (or a substitute for it):

— a 50m or 100m steel field tape measure,

— a roll or two of hemp or nylon twine,

— stakes (or flags) to mark plot corners <u>or</u> a machete-like tool to cut plot marker stakes on or <u>near</u> the farmer's land,

— necessary spray equipment (including measuring buckets, etc.): this may be same equipment as used above in the preparation exercise,

— blank field books or field book sheets,

— plot markers (plot tags),

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— a large hammer or mallet for driving stakes into the ground (if they are to be used),

Optional (Helpful but not vital) Equipment/Supplies:

— inclinometer

— jugs for carrying drinking water

— altimeter

— shovels and/or hoes

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ACTIVITY TWO TRAINERS' NOTES
TRIAL LAY OUT AND MEASUREMENTS TECHNIQUES

OBJECTIVES:

After completing this activity participants will be able to:

1. Lay out a systematic set of trials in farmer's fields under a range of different conditions.

2. Explain the major differences between station trials and farm-level trials.

3. Understand field sampling issues and be equipped with procedures for handling the measurements necessary for observing a group of on-farm trials.

INSTRUCTIONS:

A. Logistical Instructions

1. Pre-select one or two farmers willing and able to allow the workshop participants to spend a day on their farm practicing trial lay out and planting problems, sampling issues and procedures.

2. Arrange assured transportation and, if necessary, the necessary fuel to move the participants, in either 1 large or 2 smaller groups, to and from the selected farmer(s) field(s).

3. Arrange necessary meals for participants during the field trip including, if necessary, provisions for breakfast and dinner if the field trip starts and/or ends before or after normal meal times.

4. Assemble all the necessary field trial support equipment:

— tape measures,

— plot marking stakes or flags,

— a simple, inexpensive, portable laboratory scale (pan type balance),

— various types and sizes of carrying bags,

— extra envelopes,

— markers: pens and pencils,

— extra paper (field book sheets),

— a hand-held calculator,

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— machete (or similar cutting tool),

— pocket knife,

— etc.

5. Explain to the participants the day before the field exercise the following:

   a. They will all be going to the field on the following day
   b. They should arrive adequately clothed for such an activity
   c. They must arrive on time for the trip.

   The latter point is especially important when the field trip departure time is significantly earlier than the normal workshop starting time.

6. Give each participant a copy of the field exercise during the morning preceeding the actual field trip. Be sure to state to the workshop participants that each group may wish to discuss the field exercise before leaving on the field trip. Allow sufficient time for work group interaction to accomodate this need, either late in the morning session or early in the afternoon before the field preparation exercise.

7. Make a fall-back plan in case "guaranteed" transportation becomes unavailable at the last moment. Such a plan could include a visit to a "remote" section of a near-by experiment station where the field exercise may be carried out. In such a situation, the trainers may need to role-play as farmers to provide interaction with the work groups, especially at the beginning and near the end of the lay out and planting exercise.

B. Field Instructions

1. The participant work groups are expected to accomplish the following tasks during this field exercise:

   a. Randomize treatments
   b. Mark treatments on trial (plot) map before planting
   c. Lay out the trial
   d. Plant (or superimpose) the trial
   e. Make all necessary observations and measurements

2. Randomizing treatments: encourage participants to use simple methods to carry out randomization. Pulling numbers from a "hat", paper currency registration numbers, random number generator table or from hand-held calculator, etc.

3. Marking treatments: if they have not done so previously, encourage participants to pre-number their experimental plots

in a systematic order (either sequential or serpentine), beginning (1) from the upper left hand corner, (2) the lower left hand corner, (3) the upper right hand corner, (4) the lower right hand corner or (5) the corner nearest the normal entry point to the trial. Encourage participants to code their treatments in their field books and then use these codes each time a treatment is selected and further mentioned (refer to Figure IV.1 for an example).

4. Lay out the trial: determine whether the trial will be layed out along traditional square (or rectangular) shape, or along the natural contours and boundaries of the farmer's terrances or field. Determine whether the trial will be layed out using contiguous or non-contiguous replicates and/or plots. These are the two major trial lay out decisions.

   a. Rectangular or square plots:

   Use the first corner stake or flag as the "base" from which all subsequent measurements will be made. Participants should use the geometric principle "the sum of the squares of the two legs of a right triangle equal the square of its hypotenuse" — specifically the "3-4-5" right triangle, to assure a "square" trial lay out. Participants should estimate the whole trial size — including any necessary border rows, alleys, etc. — in advance of setting the first corner stake or flag, because they may have to shift the trial in one direction or another.

   The team can begin planting a researcher-planted trial before the whole trial is prepared for planting by the farmer. While row lengths are easily set at pre-determined lengths, the team will probably wish to allow planting width to vary to accomodate the farm situation (refer to Figure IV.2 for illustration). Allow the farm family to prepare the necessary width for at least two parcels (plots) before the work group measures the right angle in preparation to planting (they will need the twine, tape measure and a supply of stakes or flags at this point).

5. Plant (or superimpose) the trial: As noted above, it is often possible to begin planting a farm trial before the farm family finishes any necessary land preparation. If the trial is to be planted without land preparation at the time of lay out (examples include planting grains with planting sticks or transplanting rice), the decision must be made as to whether the farm family, including the regular hired help (if any), will carry out the planting, or whether the participant farm trial researcher group will do it. This decision should be made in advance of arrival in the farmer's field.

   Encourage the participants to reconfirm that the treatments they are laying out for planting or superimposing (seed or some other input) match the randomized pattern they have

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recorded on their field maps before placing them in the trial.

When the trial has fully been layed out, all necessary plot corner markers should be in place. Observations and measurements can now occur.

Figure IV.1

11 V_9	12 V_3	13 V_6	14 V_8	15 V_5	16 V_1	17 V_4	18 V_{10}	19 V_2	20 V_7
10 V_3	9 V_7	8 V_4	7 V_9	6 V_6	5 V_{10}	4 V_2	3 V_1	2 V_8	1 V_5

KEY 10 = plot number

V_1 = variety 1 (name); or T_1 = treatment 1 (specify what)

V_{10} = variety 10 (name); or T_{10} = treatment 10 (specify what)

NORMAL ENTRY TO TRIAL

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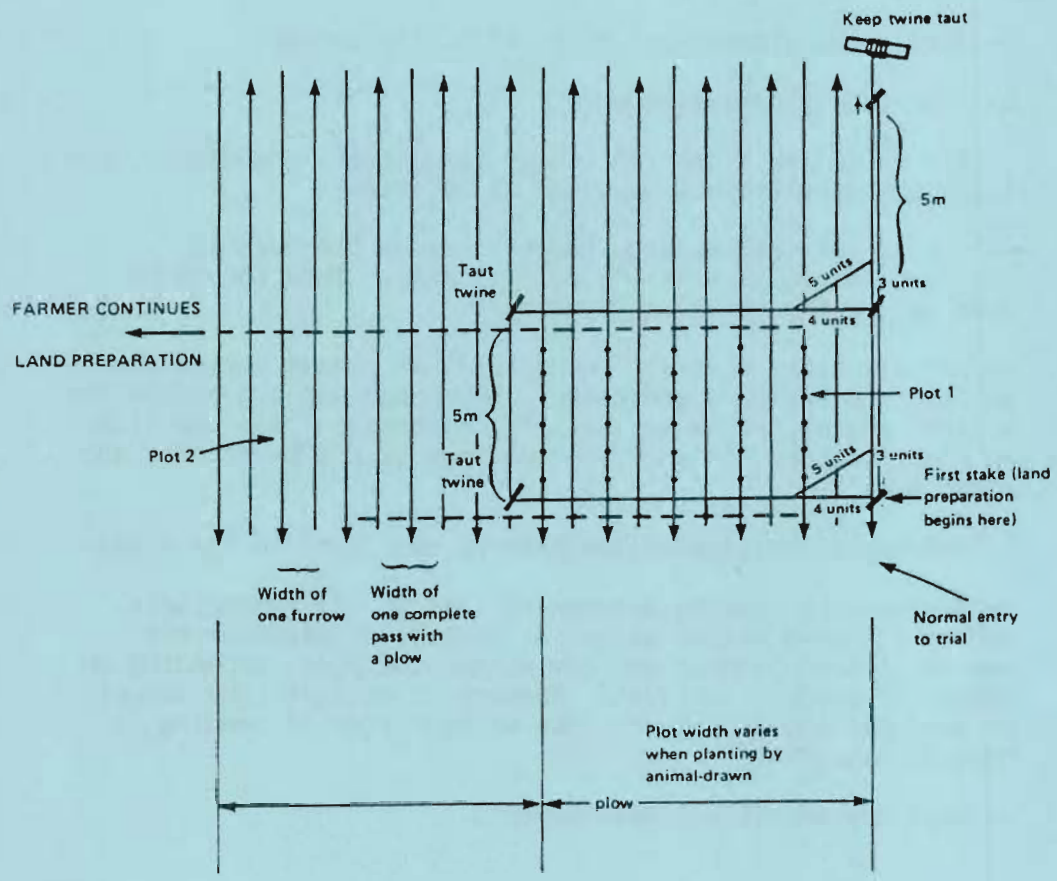
Figure IV.2


Keep twine taut
5m
Taut twine
5 units
3 units
4 units
FARMER CONTINUES
LAND PREPARATION
Plot 1
5m
Plot 2
Taut twine
5 units
3 units
First stake (land preparation begins here)
4 units
Width of one furrow
Width of one complete pass with a plow
Normal entry to trial
Plot width varies when planting by animal-drawn
plow

6. Make all necessary observations and measurements:

a. Necessary observations:

During the process of trial lay out, there is usually time for one (or more) of the team members to observe and record the following:

— major soil type(s) across the trial and in the field,

— cropping pattern history (obtained from farm family),

— important farming practices,

— # of seeds planted per hill (if hill planting),

b. Necessary measurements:

Either during trial lay out or immediately thereafter, the following measurements may need to be taken:

— obtain soil sample from the trial area, if such an activity is required of the participants. This should be done <u>before</u> the trial is layed out.

— average slope of field where trial is placed (using either an inclinometer or a protractor, plumb bob and two boards for a right angle). Take at least three separate readings with an inclinometer; six separate readings with a protractor and plumb bob.

— measure (or estimate) the size of each plot in the trial.

— measure the spacing between (1) hills, (2) transplants, and/or (3) rows within plots, or measure or estimate the amount of seed planted per row within the plot, depending on method of seeding employed. Measure or estimate the amount of seed planted in a given (pre-marked) area if seeding is done by broadcast.

— make any additional measurements.

SAMPLE HANDOUT: TRIAL LAY OUT AND MEASUREMENTS TECHNIQUES

(This is an optional sample for your information) The trainees should be informed of the field trip at least the morning before the actual trip.

INSTRUCTIONS FOR TRAINEES:

A. Logistical Instructions

1. Tomorrow morning all of you will go on a field trip to _____(area of the country), a region of _____(name of country) which is located approximately _____(km or mi) _____(direction: NW, SE, etc.) of _____(locale where the workshop is being held). We will all be leaving from _____(a specific location known to all participants) at precisely _____(time: 5:30am, etc.) in _____(type of vehicles to be used).

Since we will be going to the field, please dress appropriately (field shoes, hats, rain gear, sunscreen, insect protection, etc.).

Finally, we (will be back in time for the regularly-scheduled dinner) OR (will be stopping for dinner on the way back to the center).

Please remember to bring sufficient cash with you to cover any meals purchased away from the center.

2. Attached to these instructions is a proposed field trial lay out plan. Review it at your leisure this evening. Your work group may also wish to review tomorrow's field lay out and planting exercise today. Adequate time (at the end of this morning's session) OR (at the beginning of this afternoon's session) will be provided for each group to plan its own activities and strategies.

3. Do not be concerned with equipment and supplies for trial lay out and installation. The items necessary will be provided to your work group tomorrow.

4. Enjoy this exercise.

— important farming practices,

— # of seeds planted per hill (if hill planting),

b. Necessary measurements:

Either during trial lay out or immediately thereafter, the following measurements may need to be taken:

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— obtain soil sample from the trial area. This should be done before the trial is layed out.

— average slope of field where trial is placed (using either an inclinometer of a protractor, plumb bob and two boards for a right angle). Take at least 3 separate readings with an inclinometer; 6 separate readings with a protractor and plumb bob.

— measure (or estimate) the size of each plot in the trial. Refer to the appendix for a method of estimating areas of non-rectangular plots.

— measure the spacing between (a) hills, (b) transplants, and/or (c) rows within plots, or measure or estimate the amount of seed planted per row within the plot, depending on method of seeding employed. Measure or estimate the amount of seed planted in a given (pre-marked) area if seeding is done by broadcast.

— make any additional measurements.

WAYS TO INVOLVE FARM HOUSEHOLD MEMBERS

OUTLINE

1. Getting to Know Farmers' Opinions About the Trials
2. Learning From Farmers About Their Farms: History of Plot, Field, Farm, and Farming Practices
3. Coordinating the Timing of Trials With the Farmers
4. Implementing the Trial With the Farmer
5. Measuring Farm Labor Requirements

PRE-REQUISITES

None

LEARNING OBJECTIVES

After completing this subunit the participants will be able to:

1. Identify the roles of the farm family, extension and research in a variety of farm-level agronomic and livestock trial types. Treatments should be selected that are consistent with farmers' interests and trial objectives should be understood.

2. Select trial cooperators that represent the major production systems and/or research domains in the target area. The cooperators should grow the specific crops, or raise the specific livestock types, upon which the team focuses farm-level trials.

3. Explain to cooperating farmers the extent of their commitments with, and expected collaboration from, the field team in trials conducted on their farms.

KEY POINTS

1. Farmers' bases for making decisions about their farming practices, cropping and livestock patterns and systems almost always differ considerably from those used by station-based researchers.

2. Farmer participation begins before trial implementation, and includes input into treatment selection and trial design.

3. Successful trial implementation and evaluation require mutual understanding between farmer collaborators and the team. Exact timing of practices and/or treatments, and assigned responsibilities for each trial activity, must be discussed.

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DEFINITIONS:

border rows
cluster
confounding
control treatment
contiguous plots (reps)
domain
elementary sampling unit
experimental design
experimental unit
exploratory trials (testing)
guard rows
harvestable plot
intervention
minimum data set
multistage (cluster) random sampling
open-pollinated variety (new definition)
plot
plot maps
primary experimental data
primary sampling unit
probability sampling
random sample
recommendation domain
refinement trials (testing)
rep
replication
representative sample
research domain
response
sample
sampling unit
secondary experimental data
simple random sampling
split-plot arrangement
stratified random sampling
stratified multistage random sampling
stratum
superimposed trials
treatment
validation trials (testing)
variable (factor)
xenia

DISCUSSION

1. GETTING TO KNOW THE FARMERS' OPINIONS ABOUT TRIALS

On-farm trials are designed by the interdisciplinary field team shortly after the diagnostic stage is initially over. Such trials are based on the major constraints and predominant problems faced by farm households in a given domain. They usually revolve around the predominant (or key) crops or

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livestock types grown or raised by farmers in these households. Dialog is initiated between the team and the actual farmers of these predominant crops and livestock types, regardless of whether the farmers are male or female.

Once the team has designed farm-level trials for the given domain, dialog with farmers should be repeated. The team should return to the farmers and explain to them the objectives and treatments of each trial they have designed. Farmers should be encouraged to provide the team with feedback and opinions about both the objectives and the treatments. This is the process of filtering treatment specifics back through interviewed farmers, and is the only way to eliminate potentially useless treatments before trials are begun. It is also the only way to allow the team to refine or fine-tune treatments left in trials based on further input or feedback.

Example 1: Trial to Control Corn Foliar Insects

The team designed a trial to address a common farmer-identified problem with fall armyworm foliar damage in corn. The treatments involved are the farmer check (no control) (II,C,4) versus applications of a specific contact insecticide at three post-emergent stages during the growing season. The team must then check to confirm that the proposed insecticide is readily available in the domain, is relatively inexpensive, and is both effective and safe for general farmer use. The team must then ask for farmers' opinions about the treatments and the timings. It is possible that the farmers have tried out the proposed insecticide and found out that it does not work well. It is also possible that some of them have tried the same, or a different, insecticide at some, or all, of the proposed times during some past seasons, and have not obtained satisfactory control of fall armyworm. It is also possible that, if the insecticide requires a great deal of water for mixing, some or all of the farmers are located too far from a water source to make the trial meaningful to them. Finally, insecticide application equipment may be locally unavailable or prohibitively expensive. Without the extra step of researcher-farmer dialog after trial design, none of these potential problems may have been identified until after trial installation.

Example 2: Trial to Provide Protein Supplement to Cattle

The team designed a trial to provide protein supplement in the form of a purchased source of protein supplement commonly available in the capital city for cattle. The treatments include the farmer check (normal cattle diet) and feeding of the protein supplement for one year in addition to the normal cattle diet. The team must check to see that the supplement is readily available in the domain, and that its price is not prohibitive to farm households. The team may wish to pass the price information along to their domain's farmers in a further meeting, letting the farmers decide if the price is reasonable and that they can

afford it. Experiences in some livestock trials with farmers have shown that often cattle are simply lacking for sufficient calories. In such situations, protein supplements of any kind are simply broken down into added calories, and, instead of testing the intervention of added protein to an otherwise adequate diet, the trial becomes one of supplementing calories at a very high cost per caloric unit. In addition, the team may wish to consider locally-available alternatives, both to the protein supplement and for added calories. Perhaps an edible leguminous border tree intervention is more practical than a purchased protein supplement. Again, only further dialog with the actual farmers of the domain will assist the team in fine-tuning or modifying livestock interventions in such a situation.

Farmers also need to give the team feedback on the implications of the proposed field or livestock practices to accompany each trial and treatment. If a specific practice the team proposes to use is not appropriate to the reality of the farmers' situation, it may have to be changed at this time.

Example 3: Trial to Reduce Weediness in Rainfed Rice

The team has designed a trial to reduce weediness in upland, rainfed rice in a domain. The treatments include the farmer's control (normal weeding practices during the growing season) and reducing the row spacing from approximately 100 cm (1m) to 60 cm. The team has even adapted a typical wooden plow from the domain to the new narrower row spacing. The trial is based on the rationale that farmers identified labor constraints at weeding to be their most severe problem in upland rice culture. The theory behind the trial is that the reduced row spacing will lead to increased shading of weeds, and farmers will be able to eliminate one of the two to three hand weedings during the season. Upon further dialog with the farmers, the team learns that an even bigger constraint is access to a pair of oxen at planting and, since the proposed intervention adds essentially 100% more to land preparation and planting time in rice, the farmers do not believe it a practical solution. In such a case, the team may fall back on an herbicide trial, again confirming that the proposed herbicide(s) is/are (1) locally available, (2) not too costly and (3) effective and safe for farmer use.

Example 4: Trial to Measure Weight Gain in Livestock Due to Supplementary Feeding

The team has designed a trial which uses locally available feed sources to supplement cattle feed in the highlands of a country (highland domain). To verify the weight gain of the supplementally fed group over that of the farmer's control group, each animal is marked before the trial begins. Since the trial is being conducted with several farmers in a traditional village range with no natural fencing but with natural confinements, the team decides to pen the supplementally fed livestock near the

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scales they will be using to measure weights. The control group will be housed near-by in a locally constructed pen. The new pen is made up in the typical Western design of a corral with wooden pole construction and a hinged gate. Typical animal penning in the domain is by use of locally-available rocks and stones piled upon one another, with cracks remaining being filled in with mud and/or animal dung. At the end of the first year, the farmer control animals have collectively gained more weight than the experimental group. Further dialog with farmers during the winter months reveals that the local penning design cuts winter winds much better than the western corral design, and that differences in weight gains are due to the experimental group burning off many more calories trying to keep warm, not simply to the diet supplement. In the following year, the trial is repeated, with both groups being penned in local corrals. However, additional dialog with farmers before the first year of the trial may have revealed the purpose of the natural material corrals, and may have led the team to reject the construction of the inappropriate western-style corral.

2. LEARNING FROM FARMERS ABOUT THEIR FARMS: HISTORY OF PLOT, FIELD, FARM AND FARMING PRACTICES

a. Crop Based Trials

When the team arrives to plant a given farm trial, or to superimpose a trial, they need to obtain information about the history of crops and practices in the field where they carry out their research with each farmer. Such a history should go back at least one or two crop years, and should include all locally relevant variables. Examples of such variables include fertilizers used, pesticides applied (with special emphasis placed on herbicide types), mulches used, manures used and locations of all manure storage areas near or in the field assigned to the researchers, areas subject to standing water (water-logging potential), areas subject to seasonal drought (possibly high spots), etc.

The team should also be wary of having a trial located in or near the following places:

1. Village or school paths or animal trails
2. Large overhanging trees or vines
3. Corners of fields (unless there is no alternative)
4. Low (or high) spots in otherwise relatively uniform fields
5. A typically steep slopes in otherwise gently-sloping fields
6. Hill crests or hill bottoms in a field which begins on a hilltop and ends at the hill bottom
7. Across what appear to be slightly different major soil types, especially if this soil condition appears to be confined to the specific field being considered.

b. Livestock Based Trials

When the team arrives to begin an animal based trial with farmers, some historic information must be obtained on both the herd and, as far as possible, each animal selected for the trial. It would not do, for example, for all animals for one treatment to be selected from a herd which had recently received innoculations against a predominant fatal disease common in the domain, while the animals selected for the other treatment came from a herd which had never received innoculations for this disease. Such a situation is asking for differential mortality during the trial, when such mortality is probably not due to the experimental variable, but the particular histories of the two herds. Likewise, the team should verify that animals volunteered for either the control or the experimental groups are not sickly or recently recovered from some severe disease. In addition, in so far as practically possible, farmer practices for selected animals should be similar.

The team should be wary about accepting an animal or bird into a trial which:

1. Appears to be much smaller or weaker than the rest of the animals or birds in the herd or flock
2. Appears to be sickly or listless (at least more so than others from the same herd or flock)
3. Walks or runs with an obvious limp or other physical problem, or has trouble flying (for birds)
4. Has a dubious medical history

3. COORDINATING THE TIMING OF TRIALS WITH FARMERS

Timing of trial installation should correspond to the regular planting routine of the farmers growing that crop-based system in the domain. Trials planted on any given day, under rainfed conditions, should consider as their check plot only farmers' crops planted that same day. Even a one day difference in planting date under rainfed conditions may lead to excessive potential yield differences in the comparisons. Such variability may first be expressed by vastly differing seedling emergence rates, which may result in different yields.

Timing of superimposed trials should correspond either to the typical time of farmers' practice in the domain, or to agronomically recommended timing. For example, if the team is testing either a new tool to control weeds, or a post-emergence herbicide, they should time their first intervention to correspond with the first weeding by farmers. On the other hand, if the super-imposed trial involves an input with which farmers of the domain have no practical experience (such as a top-dressing of urea 20 days after emergence), the team should demonstrate the agronomically best application technique and the best timing. Even though the super-imposed trial may be conducted during the exploratory (first) phase of on-farm experimentation, proper technique should always be used by the

team.

Timing of field level observations should be a balance between routinely scheduled monitoring trips (every two weeks, for example) and trips for monitoring specific items (such as seedling emergence, insect damage, diseases, other stand establishment reducers, bird and/or rodent damage in crop-based trials and weight gain, general appearance and reaction to medications in animal-based trials). Routine monitoring trips must be scheduled so that too much time does not pass between visits during critical plant, animal or bird growth stages.

Timing of pre-harvest observations and/or measurements, and of harvest itself, are as crucial as timing of planting. Both involve a great deal of coordination within the field team and with collaborating farmers. If trials depend on harvested yield estimates, it is best to do all possible to avoid farmer misunderstanding and risk of premature harvest. This requires sensitivity by the field team anticipating the approach of harvest, so that plans can be made and verified with each individual collaborating farm household.

4. IMPLEMENTING THE TRIAL WITH THE FARMER: ROLE OF COOPERATORS IN TRIAL IMPLEMENTATION

Farmer participation relates to all stages of the FSR/E approach. Farm household members are not only the object of the approach, but should also be involved in the major and minor decisions about the types of trials, the treatments therein, and the practices involved in the relevant systems. Farmers are the center piece of trial implementation methodology on their farms. During the process, a close and permanent interaction should develop between collaborating farmers and the field team. Farmers and team members both learn about new production alternatives. Farmers teach team members about existing production practices. Farmers also teach the team about the values and attitudes they hold and use in evaluating trials.

The degree of farmer participation in trials varies according to the FSR/E stage. During exploratory trial work, farmer participation is more passive in that most activities are carried out by the team field. Obvious exceptions are those practices unaffected by the trial's treatments, and best carried out by the farmer and/or his or her hired laborers. As results from exploratory trials become available, they may suggest regional evaluation and acceptability by farm households of selected trial variables.

During the validation of trial phase, which focuses on socio-economic evaluation and farmer acceptance, experimental designs and treatments may be adjusted to each farm or herd. The appropriate check treatment then becomes each farmer's normal practice, not the average practice of the domain. More of the

management of the trial is given over to the farm family. After the team and farmers have defined and agreed upon their various roles and participation commitments, the team is in a better position to propose variables for testing, as well as suggesting levels of variables.

Farmers should be aware from the beginning exactly what to expect from the relationship with the research team. Above all, farmers must be informed that they are collaborating in research from which both they and the research team will learn. They are not involved in a demonstration designed to show them how much better the researchers can do what the farmers are already doing. Farmers must be aware of who will be expected to provide what, who will take what risks, and who will get what product. It is critical that farmers understand the timing of the various activities, and whether each activity is to be undertaken at their initiative or at the initiative of the researchers. For example, in a yellow maize area, if some white varieties are to be used in an exploratory trial, farmers should be informed that they can expect the researchers to provide them with some yellow maize in return for the white maize they will not want to consume. Farmers should agree to include white maize in the trial, after researchers explain why it should be included. Farmers must also know who will provide each input to be used, when it must be available, and who will pay for its purchase.

Management practices and field conditions on most farms differ substantially from those found on experiment stations. These differences need to be considered in any strategy to obtain meaningful experimental data from on-farm trials. On-farm trials are not meant to try to simulate experiment station conditions in farmers' fields. Rather, they are designed to help detect differences under typical farmer management practices and environmental conditions. Therefore, non-experimental variables and other crop management practices should not differ from those normally used by farmers in their own fields.

5. MEASURING FARM LABOR REQUIREMENTS

For measurements of labor inputs into trials to be meaningful, plot size must be sufficiently large so as to make the performance of each labor activity fall into the routine category. Most experimental plots (5m x 5m) are too small to allow for routine labor measurements. For example, timing a weeding operation in a plot which is 3m x 5m is just too artificial. Timing a weeding operation (at least one done by hand or using animal-drawn implements) in a 10m x 10m plot is more realistic. However, weeding usually occurs down the row, and farmers give little thought to stopping at plot boundaries. This makes animal assisted weedings difficult to measure in square plots. Long, thin plots are more efficient for timing labor requirements but not for agronomic efficiency. Plot shape is generally determined by the primary objective of any trial. Once a field team progresses along to testing where labor time

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differences may be critical for adoption, thought should be given to changing plot shape and size to accomodate the critical labor measurements.

Other labor timings such as, the tediousness of a newly-introduced planting technique, double row versus single row planting, spraying, thinning, transplanting, various types of weedings, pre-harvest preparations, harvesting, etc, may or may not be important to the adoption of an intervention or a set of interventions. Most farming systems interventions will be evaluated by each farmer and by the farming community, and will be accepted or rejected under their own unique multiple acceptance criteria. Thus, possible labor bottlenecks and labor operations which add significantly to the existing labor needs of the system must be evaluated early in the FSR/E process. In such cases, a deeper dialog with participating (and observing) farmers may assist the team to decide if such interventions appear to be practical from the farmers' perspective. See also section (V,B.3) for further information on this topic.

Figure IV,A.1 General Characteristics of Farm Trial Types

Characteristic (in general)	Trial Type: Exploratory	Refinement	Validation
Agronomic parameters			
# of trials	relatively few: 5	more: 10	most: 15-30
# of treatments	most: 5-12	less: 2-6	least: 1 or 2
# of replicates	several: 2-4	fewer: 2-3	none: 1
Plot size (harvest)	smallest	intermediate	largest
Proportion farmer field	no more than 10%	about 10%	20-100%
# of control plots	most: up to 4	intermediate	each farmer's only
Management parameters			
Total "work" involved	most	intermediate	least
Farmer participation	least		most
Researcher participation	most		least
Extension participation	least		most
Socio-economic parameters			
Risk to researchers	most	intermediate	least
Risk to farm household	most	intermediate	least
Exposure to others	least	intermediate	most
Informal dialogue import	most	intermediate	least
Formal survey importance	less	most	less
Farmer acceptance import	least	intermediate	most
Cost to researchers	most		least
Cost to farmer	least		most
Management combinations			
R-planted, R-managed	common	possible	non-existent
F-planted, R-managed	rare	common	possible
F-planted, F-managed	non-existent	possible	common

HOW TO LAY OUT TRIALS

OUTLINE

1. Deciding Where to Locate Trials in Fields
2. Determining Which Blocking Techniques to Use
3. Layout Techniques
4. Border Rows

PRE-REQUISITES

None

LEARNING OBJECTIVES

After completing this subunit the participants will be able to:

1. Explain many of the practical issues involved in laying out plots for agronomic and livestock trials in farmers' fields under a range of conditions.

2. Lay out trials in farmers' fields, or with farmers' livestock, under a range of conditions.

3. Determine appropriate field measurement techniques and estimate sample sizes for different types of variables under a range of farmer conditions.

KEY POINTS

1. The team should be prepared to assume the additional economic risk associated with farm-level treatments, especially during the exploratory trial phase and especially when such treatments are new to a region. Farm households should be prepared to assume the normal economic risks associated with normal crop or livestock practices or treatments which are not affected by the trial's treatments.

2. FSR/E teams must be able to obtain and use site-specific, localized knowledge of each field, farm and farm household in a defined, relatively homogeneous area or zone. This requires a high degree of sensitivity to the farm household's conditions, constraints and problems.

3. The measurements necessary for the successful management and/or harvest of crop-based or livestock-based trials must be done on the most statistically sound, inexpensive and timely basis as possible.

DEFINITIONS

border rows
cluster
confounding
control treatment
contiguous plots (reps)
domain
elementary sampling unit
experimental design
experimental unit
exploratory trials (testing)
guard rows
harvestable plot
intervention
minimum data set
multistage (cluster) random sampling
open-pollinated variety
plot
plot maps
primary experimental design
primary sampling unit
probability sampling
random sample
recommendation domain
refinement trials (testing)
rep
replication
representative sample
research domain
response
sample
sampling unit
secondary experimental data
simple random sampling
split-plot arrangement
stratified random sampling
stratified multistage random sampling
stratum
superimposed trials
treatment
validation trials (testing)
variable (factor)
xenia

DISCUSSION

1. DECIDING WHERE TO LOCATE TRIALS IN FIELDS

Obtaining the best possible parcel in the field is not the key to locating a trial in a farmer's field. Rather, the key is to obtain the most representative parcel. The team should avoid locating the trial in those areas which are not representative of that farmer's field.

In locating trials in farmer's fields, the research team

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working in non-terraced, sloping, rainfed areas must be aware of what is located uphill from their trial. For example, runoff from fertilizer use, herbicides, other pesticides, or stored animal manure, may all provide extreme confounding and uncontrollable interactions with a given trial during the subsequent cropping season. The team should not select the farmer's most inaccessible field unless it is the only field the farmer is willing to let the team use. On the other hand, if proximity to major roads or paths is correlated with farmer practices, the team must be very careful to select a representative sample of fields for trials. The sample should not be totally dominated by either inaccesible or accessible fields, but should contain a representative number of each type.

2. DETERMINING WHICH BLOCKING TECHNIQUES TO USE

a. Slopes and Other Problems in Fields

Once the team and the farmer have agreed as to where the trial will be placed in the field, the team must determine how to lay out the trial. If a significant change in slope is encountered, blocks (or reps) of treatments must be located along the slope gradient, not up and down the gradient (Figure IV,B.1). The same blocking technique should be used when the team encounters obvious soil type differences. In such a case, locate blocks (or reps) of treatments along such soil type differences not across them (Figure IV,B.2).

The team must consider several additional factors in trial layout. Again refering to Figure IV,B.2, it can be seen that the team should avoid placing trials (1) in corners of fields, (2) under overhanging trees, (3) where some treatments are affected by either people or livestock paths, or (4) where they may be exposed to damage by livestock (outside of fenced areas where fencing is common).

b. Plot Location

There is no reason why all treatments of any given trial must be located together in a farmer's field. If space is very limited, or if the designed trial is too long to fit into a given farmer's field, the treatments can be separated spatially. If a local flooding problem, soil type difference, or major slope change, is going to cut across a trial corner or several treatments, the trial should be separated so that treatments do not fall into the less representative areas. Depending on field size, it is even possible that different treatments of the same trial may be located in different (but contiguous) fields.

Figure IV,B.1 Trial Layout Under Changing Slope Conditions (Non-Terraced Hillsides)

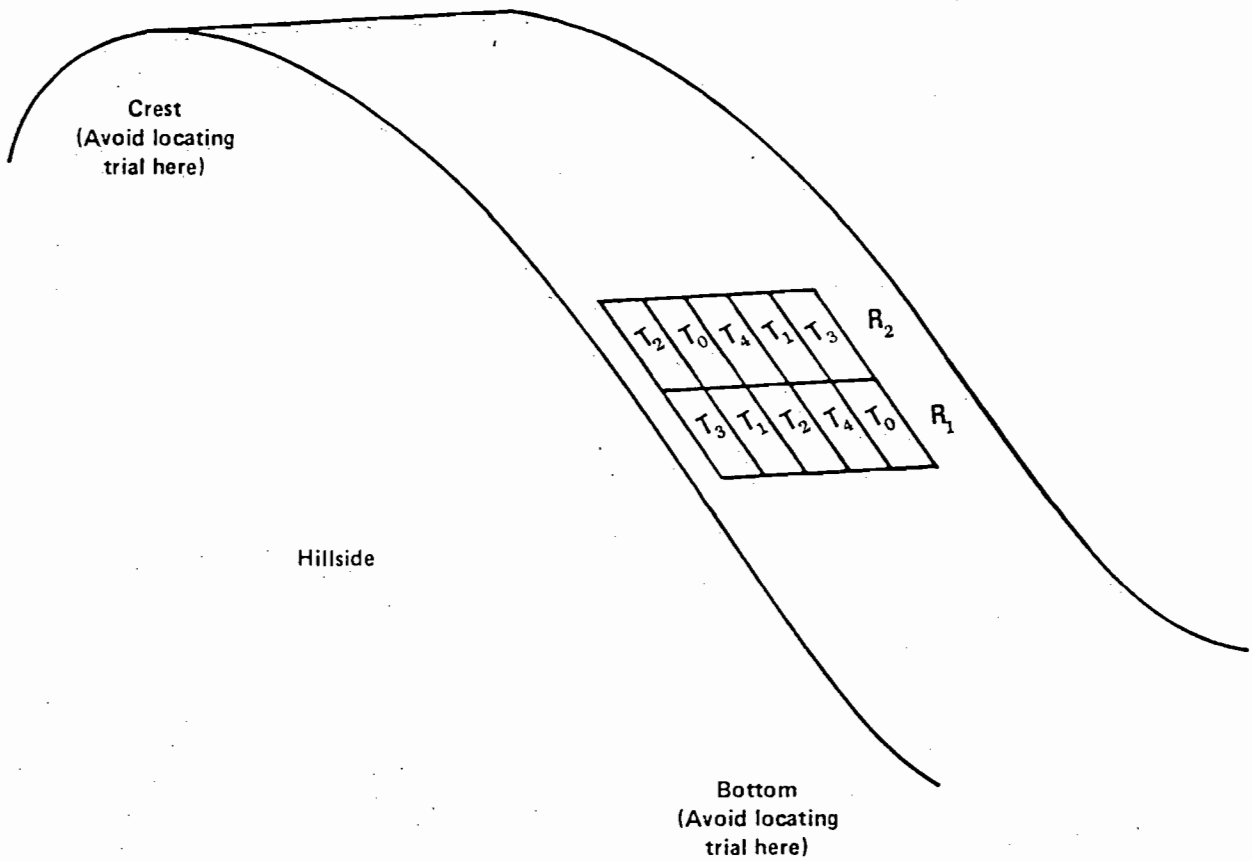

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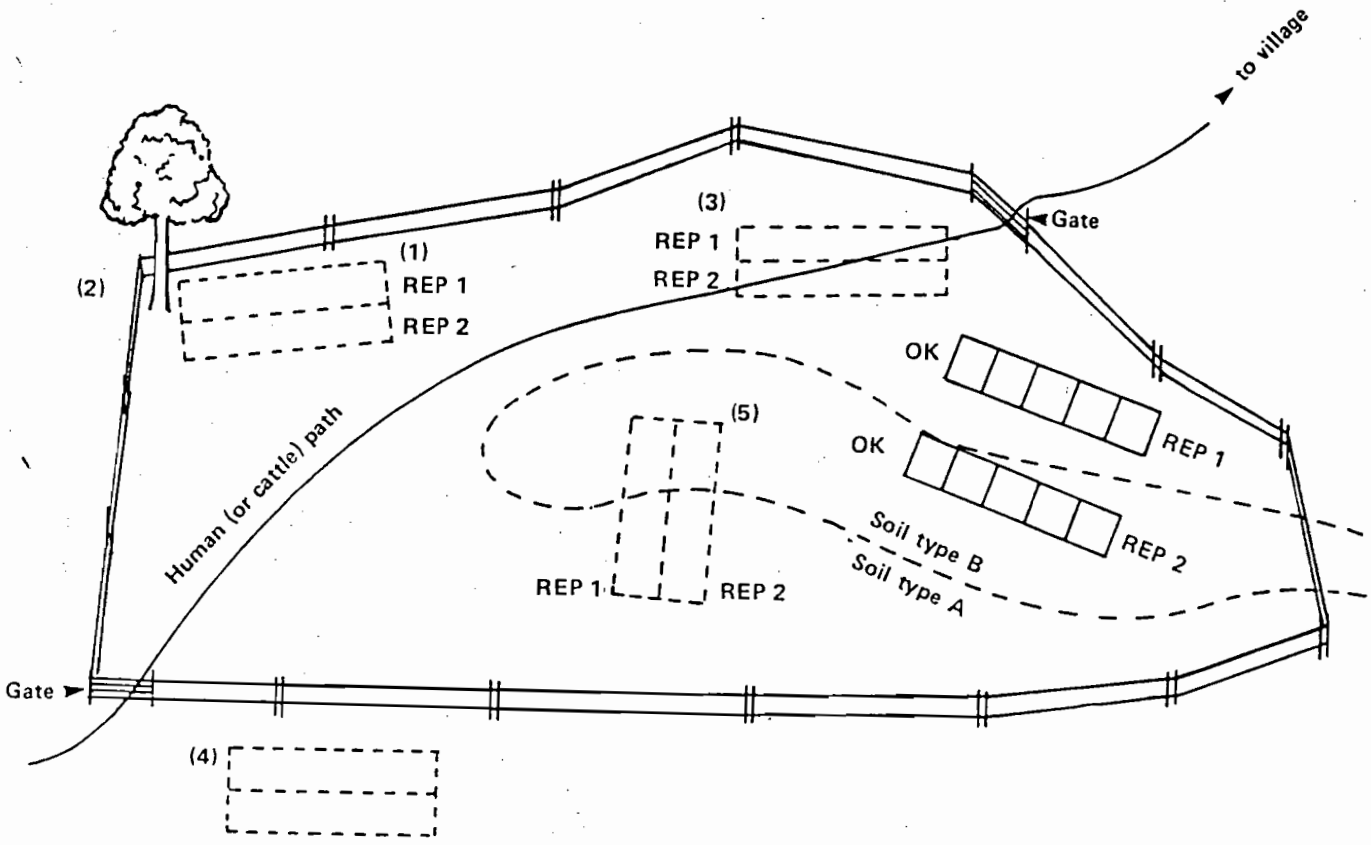
Figure IV,B.2 Additional Crop-Based Trial Layout Issues to Consider

Additional Crop-Based Trial Layout Issues To Consider

Locations to avoid:

(1) Corners of the field (unless unavoidable)
(2) Trees or vines providing excess shade or root competition for water or nutrients
(3) Human (or cattle) paths through fields
(4) Unfenced areas frequented by livestock
(5) Improper blocking layout across major soil (or drainage) differences



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c. Row Spacing and Planting Distance

With the exception of researcher-imposed trials, both row spacing and planting distance should generally follow the practice of each trial collaborator. Trials, especially during the initial FSR/E exploratory phase, should generally minimize changes in spacing without first examining the reasons behind current farmer row spacing and planting distances. There are often excellent reasons why current spacings are what they are. Such factors may be determined by animal type or plow width used in land preparation and planting; by the length of a farmer's average pace; or by traditional planting equipment. Changing such traditional factors without verified improvements which require no more labor or which do not add significantly to expense or inconvenience should be done with either great caution or not at all.

3. LAYOUT TECHNIQUES

Laying out any set of systematic on-farm trials requires a great deal of planning and preparation and the assembling of equipment and supplies. Seeds must be collected or purchased from suppliers, national production programs, or from commodity heads. Germination percentages should be determined for each variety used. Remaining seed should be saved in case replanting is either a common or acceptable practice, and becomes necessary after emergence. Each treatment usually requires its own seed packet.

Each field team should have a place to store and assemble equipment and supplies. Each field team should also have adequate and guaranteed transportation to and from the field during the labor peak at planting time. Each field team will need to have a minimum of planting materials and trial layout aids. Such items as steel tape measures, twine, corner marking stakes, envelopes, (paper and plastic of varying sizes), machetes (or other typical cutting devices), notebooks, marker pens and pencils, a simple, inexpensive laboratory scale, and various measuring devices (for input measurement and packaging) are all essential. Other items are quite helpful. These include inclinometers, soil augers, field tripods, water-proof note paper, etc. Near harvest, each field team must have access to a practical field scale, and, if necessary, a practical device for measuring grain moisture percentage.

Each team, for each cropping system, will generate its own method of installing field trials. However, frequent interaction with collaborating farmer, is indispensible to the process. It is hard to overemphasize the importance of planting at the same time as each collaborating farmer. Their assistance, help and understanding is essential. Many misunderstandings arise when farmers are ignored or overlooked during trial planting. This time period is often the origin of a feeling by farmers that the trial is not "theirs" and, this being the case, they can ignore

it in terms of normal management decisions and practices. (IV,A) discusses the importance of knowing farm household members.

The mechanics of trial layout are often determined by a combination of the number and complexity of treatments and replications of the on-farm trials, as well as the given situation of the farmers of the domain. For example, in Asia, where many fields are quite small, installing a complex trial with many treatments and/or replications in a lowland or terraced, irrigated rice-based system will require a great deal of logistical manipulation to get all treatment plots into a contiguous arrangement. However, an advantage of small fields and small plots located close together is that it should not be difficult to arrange to have similar trials with several farmers in a relatively tight geographical area. Indeed, in such a situation, 10 trial replicates could usually be located no more than one to two kilometers apart.

It may be much easier to physically fit trials on farmers' fields. Under rainfed conditions in parts of Latin America or Africa, however, it is also much more difficult to locate such trials conveniently from a geographically logistical visit point of view. Ten trial replicates may be located with a kilometer or more between each one. This has obviously adverse logistical implications.

Where appropriate, teams should be aware of simple methods of insuring that trials can be installed with right-angled corners (using any available twine and the right triangle principle that (3 units) squared + (4 units) squared = (5 units) squared: i.e., 3 + 4 = 5 (9 + 16 = 25). Field trial teams should also be aware that trials may often have to follow existing contours, and that the team may have to estimate plot size for plots which are neither square nor rectangular.

Teams should especially be aware that they should not violate certain farmer practices which may invalidate the extendability of their trial's results. Insisting that the rows of a given trial should run along the contour, when the collaborating farmer and all neighbors plant up and down the slope, and when this practice change is not a designed intervention, is not recommended and, if forced upon farmers against their will, may turn out to have varying degrees of disaster in it for the team during the season.

4. BORDER ROWS

The purpose of border rows in field trials is to buffer the harvestable mid-plot rows from the confounding effects of neighboring varieties and/or treatments. The number of border rows necessary varies with (a) distance between rows, (b) crop, and (c) treatment. For example, spray treatment trials require greater numbers of border rows than do variety trials.

In farmer's fields, it is best to separate varieties of open-pollinated species which are of obvious different colors because of the xenia affect. One way to assist this isolation is to group same colored varieties in one part of the trial and those of a different color in another. The two groups of different-colored varieties should then be separated by more than the usual number of border rows. The reason for separation is a cosmetic one: farmers may not use multicolored varieties and reject the intervention, regardless of its other potential benefits.

Plot protection is a major issue in trials which involve spray treatments. More buffer rows between treatments are generally required than with variety or fertilizer trials. In addition, it is relatively easy for researchers to stay within wind velocity maximums in spraying trials on-station, because it is easy to return the next morning to that part of the station where the trial is located. It is much more difficult for a team to return to a farm trial 27 km away from their home base. Decisions need to be made in the field regarding the trade-offs between wind velocity in excess of the recommended maximum, the distance back to the site from either another farm or from the research base, and the number of additional farms which are still to be visited that day or that week.

There is little a team can do to protect whole trials and individual plots from field-level damage. In some parts of the world, ranging cattle, other livestock and wild animals may do considerable damage to field-level trials. In other parts of the world, rodent and bird damage are endemic. Some trials will be damaged by villagers passing by (or through) the trial. It is generally much too costly and impractical to fence field trials. It is preferable to avoid as many potential problems as possible in laying out trials. For example, assuring that the trial will not cross a community path or cattle crossing area (under rainfed conditions) is high priority (Figure IV,B.2). As far as other field damages are concerned, the team must be prepared to be flexible enough to (a) identify the source of damage, (b) dialog with the participating farmer as to the "average severity" of the problem this year versus other years, (c) be prepared to quantify such damage in a meaningful way and (d) perhaps control economic damage. Control decisions differ by trial purpose in the FSR/E sequence and objective, as well as subjective team feelings and the opinions of collaborating farmers.

HOW TO OBTAIN AND HANDLE DATA FROM TRIALS

OUTLINE

1. Types of Data
2. Minimum Data Set
3. Choosing Which Types of Data to Collect
4. Determining Who to Collect Data and When
5. Measuring Techniques
6. (Optional) Sampling Techniques
7. Recording Techniques
8. Data Processing Techniques and Equipment

PRE-REQUISITES

None

LEARNING OBJECTIVES

1. Determine the data and information that should be collected at the different stages of on-farm trials in the FSR/E approach.

2. Use effective sampling procedures to collect relevant agronomic, livestock and household data.

3. Select the proper techniques to analyze and interpret the data and information collected from the trials to allow next-step decision-making.

KEY POINTS

1. The nature of on-farm trials conducted in FSR/E leads to differences in the extent and the characteristics of data and information that should be collected as compared to an experiment station setting.

2. The minimum necessary data should be identified for collections that are consistent with trial objectives and resources available to the research team and which insure that an adequate number of trials are conducted. It is recommended that the minimum data set be used.

3. FSR/E involves a sequence of on-farm trials. As the trial sequence progresses, the importance of social and economic information to analyze farmer reaction and to draw comparisons with the local farming practices dramatically increases.

DEFINITIONS

border rows
cluster
confounding
control treatment
contiguous plots (reps)
domain
elementary sampling unit
experimental design
experimental unit
exploratory trials (testing)
guard rows
harvestable plot
intervention
minimum data set
multistage (cluster) random sampling
open-pollinated variety
plot
plot maps
primary experimental data
primary sampling unit
probability sampling
random sample
recommendation domain
refinement trials (testing)
rep
replication
representative sample
research domain
response
sample
sampling unit
secondary experimental data
simple random sampling
split-plot arrangement
stratified random sampling
stratified multistage random sampling
stratum
superimposed trials
treatment
validation trials (testing)
variable (factor)
xenia

DISCUSSION

1. TYPES OF DATA

The nature of the information and data collected from on-farm trials has many similarities to that normally collected for trials conducted at experiment stations. However, in FSR/E, there is a progressive sequence of on-farm trials that involve

different design considerations and objectives, leading to varying information and data collection requirements. The extent and nature of the data and information to be collected will vary as testing progresses through exploratory to refinement and validation trials, and the analysis and interpretation of the data will likewise change.

Four categories of data need to be collected for on-farm trials. Such collection can fit into the minimum data set previously presented. The four data types to be collected follow.

a. Environmental Setting Information

The first category is environmental setting information. This involves the physical and biological environment of the area, the farm and the field on which the trial is planted. It includes such factors as cropping history, weather conditions, soil characteristics, location of the field in relation to the homestead, position in the topography and any other relevant factors needed to describe the farm and field where the trial is conducted. All factors which may affect the plants' response should be noted, such as presence of and direction of slope, soil type, cropping history, fertilization (chemical and/or organic) practices, PH (inexpensive kits will give close approximation), any irrigation history, etc. This information is largely taken for granted for trials planted on an experiment station and should be collected for all on-farm trials in all stages in the trial sequence.

b. Primary Experimental Data

The second category of data for collection is primary experimental data. This is the data on selected variables which reflect response of the experimental material to experimental treatments. These are measurements from trial plots of biological and physical variables directly related to the trial's objectives. At a minimum, this will be a measurement of productivity or yield but may include other measurements which will aid in the understanding and interpretation of differences. Such variables could include plant height, days to flowering, days to maturity (harvest), etc.

c. Secondary Experimental Data

The third category of data to collect is that information which can be used to help interpret primary experiment data and which relates to the whole experiment. This information would include data, or observations, on time of planting, rate and date of fertilizer application, time of irrigation, moisture stress periods, pest incidence, interference from farm animals, etc.

d. Social and Economic Data

The fourth category includes social and economic data that is essential to evaluate the potential for farmer household adoption of the new technologies under test in the trials. Much is information not taken directly from trial plots, such as farm size, farm household demographic characteristics, household availability and alternative uses of labor and power, household cash availability and/or access to credit, costs of agricultural inputs product market prices, and farmer reaction to the technologies under test and future plans for adoption of the technologies. Collection of information concerning current farmer practice and measurements of their productivity which is needed to make meaningful comparisons of the trial treatment results may also be included in this category. Some can be taken from trial plots, such as amounts of inputs used, measurements of labor time, or observations of the gender and age of household members for specific treatments.

2. MINIMUM DATA SET

Development of the minimum data set began during the FSSP Annual Meeting at KSU, October 1984. Subsequently, the minimum data set was expanded upon by Kenneth Buhr, Peter Hildebrand and Susan Poats at the University of Florida. An intermediate version was field-tested in Burkina Faso in the fall of 1985. The current version is being designed to fit into a practitioner's field book. An example of the minimum data set is shown in Figure IV,C.1. Please refer to it and feel free to use it in your FSR/E approach or project. It is also discussed in (I).

The minimum data set is a necessary screen or check-list to enable FSR/E practitioners to account for major site-specific, region-specific and project- or country-specific variables which identify and distinguish their trials. The minimum data set contains those physical, meteorological, agronomic, social and economic variables considered by practitioners to be necessary to allow classification of a set of farm-level trials as FSR/E. Again, the minimum set includes three nested sets of data based on

1. The country (or project) level -- most macro-level
2. The region (or domain or homogeneous zone) level -- intermediate
3. The individual farm level -- most micro-level.

The FSSP supports the use of the minimum data set concept to assist practitioners in recording and measuring the minimum types and amounts of data necessary for a set of farming systems trials, and to allow the reporting of results of such trials in a more systematic format and basis. The FSSP further asserts that such a simple tool as the minimum data set should form the basis

of all further recording and reporting of FSR/E approaches. Toward this end, if you, as a practitioner, use the minimum data set and have some concrete suggestions regarding its format or modification, please send your observations and comments to: Dr. Ken Buhr, Department of Agronomy, 2183 McCarty Hall, University of Florida, Gainesville, FL 32611.

The "minimum data set" should be developed by an interdisciplinary group before the research is begun so the proper recording equipment and materials can be taken to the site. Also, it is extremely important to record the data on time and properly. The best way to accomplish this is to have all of the minimum data, treatments, plot layouts, etc. recorded in a simple and durable field book. Making a backup copy of all important information is good insurance for the unexpected. Below is a set of factors which will describe a substantial portion of the environment in which farm and station research is conducted and which will help the researchers and the research community interpret results. For example refer to Figure IV,C.1.

3. CHOOSING WHICH TYPES OF DATA TO COLLECT

Different data may be collected and the degree of depth of data collection may vary according to trial type and objective. The three major types of trials considered below are exploratory, refinement, and validation. General types of data collection for each type is specified below.

a. Exploratory Trials

These on-farm trials evaluate the potential of a large number of treatments in the targeted area. Generally, the requirements for primary trial data and secondary trial data at this trial stage is similar to trials conducted on experiment stations. These trials involve small plot sizes, are replicated within each field, and are scattered throughout the target area, this introduces time and manpower constraints upon data collection that are different from an experiment station setting. It is important to identify the minimum data and information requried that is directly relevant and will be used to evaluate treatment performances. Priority should be given to primary and secondary experimental data as well as environmental setting information, but judicious decisions can be made to include useful background social and economic data if such considerations are useful in trial analyses. Again, refer to the minimum data set in the Figure IV,B.1. In addition, pages from a Nepali example of a field book Figure IV,C.2 provide examples of types of primary trial data, secondary trial data, environmental setting information, and social and economic data.

Figure IV,C.1 Minimum Data Set

Minimum Data, FSR/E Agronomic Trials
PERSONS(S) RESPONSIBLE FOR DATA
_____
Address _____
LOCATION: Country_____
Prov/Dept/State_____
Other_____
ENVT: Lat._____ Elev._____ Temps:
Annual Max/Min___C ___C,Trial temps
(Daily Max/Min)_____
Ppt: Mean Annual_____mm,During trial
Period___mm, Stress Period(s)?_____
_____Duration_____
Evapotrans./humidity values_____
_____Irrigation?_____
Amt._____mm_____
_____

SOIL: pH___OM%___Texture_____
Class._____Color_____
Fertility_____Slope%_____
Other_____
_____
_____

SOCIO-ECONOMIC: Farm size range for
locale____-___ha, Mean size___ha,
Range/mean for trials___-___ha,___ha;
Land tenure_____,Ethnic group
____Lang.___Market/supplies access?
___Distance____Km, Transport?_____
Form_____How Mkt?_____Credit?_____
Terms____% ____Other_____
_____
_____
_____

LABOR REQTS: Male/female____/_____
Seasonal Availability_____
Source (hired/family)_____
Energy rgt (manual,animal,mechanical)
Cash rqts(prices of key inputs,i.e.,
$/Kg N, etc.)_____
_____
_____
Value of products_____
_____
_____

Other field-household interactions
_____
_____
_____

TRIAL DETAILS: Crop(s)%_____
Previous crop(s)/mgt_____
Tillage/land prep_____
_____

Cultivars_____
Date planted_____/_____/_____Date (s)
Harvested ___/___/___,___/___/___,___
Plot Design _____Reps_____Plot
Size_____Harvested area_____
Treatments _____
_____
Fertilizer Applied _____kg/ha,
When_____
How _____
Weeds/control/when/how? _____
_____
Insects/control/when/how?_____
_____
Diseases/control/when/how?_____
_____
Genus/species of Ins. & Dis. Pests
_____
Other pests/control/when/how?
_____
_____
_____
Farmer input_____
_____
_____
_____
_____
_____

FACTORS TO RELATE THE TRIAL BACK TO
FARMING SYSTEM: What is problem/
Limiting factor (hypothesis for the
intervention)?_____
_____
_____
_____
_____

Infrastructure/Policy Implications?
_____
_____
_____
_____

Farmer assessment of result(adoption?)
_____
_____
_____
_____
_____

OTHER UNUSUAL, OR RELEVANT FACTORS OR
CIRCUMSTANCES_____
_____
_____
_____
_____

b. Refinement Trials

These trials are used to test and define the performance of a smaller number of relevant treatments in larger plots with the active participation of farmers and the team. In these trials, primary experimental data should be collected, but more priority should be assigned to collection of social and economic data as compared to secondary trial data, especially when data collection resources are limited. Monitoring and recording farmers and farm household members' reaction to the treatments being tested and the comparison of the treatment performances with current farmer practices assumes an important role at this trial stage. The larger plot size allows both farmers and researchers to develop a more realistic perception of the farm level performances of the technologies being tested. The team should develop the concept that it is better to conduct a larger number of trials with minimal but carefully selected data collection requirements as opposed to fewer trials involving more extensive data collection.

c. Validation Trial,

Trials at this stage are planned to verify and extend one or two of the most relevant technologies identified in the previous trial stages to a large number of farmers in the targeted area. They are usually farmer-planted and farmer-managed and involve large plots. In these trials, the priority for collecting primary and secondary experimental trial data will be reduced, and higher priority will be assigned to collection of social and economic data. Emphasis should be given to evaluation of farm household reaction to the technologies being tested and to comparison of their performance to current farmer practices. This would also include the determination of the future plans for technology adoption by farmers who participated in the trial, and by selected neighboring farmers.

In summary, the three general types of trials detailed above exploratory, refinement and validation can be characterized by several parameters which usually typify them. Caution must be used to say that these general characteristics are always found according to these trial types, because exceptions to these generalizations abound. However, looking across the whole process of trials, beginning with researcher-planted and researcher managed and ending with farmer-planted and farmer-managed, most of these characteristics hold in a relative sense.

TABLE IV,C.1 presents these general characteristics according to four major groups. The first group consists of agronomic parameters, such as number of trials and plot sizes. The second group consists of management parameters (that is, upon whom does most of the work fall), such as the amount of participation expected by farmers, researchers and extension. The third group consists of socio-economic parameters, such as risk (both to farmers and to researchers), importance of farm household dialog,

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importance of acceptance of interventions by farmers, and costs. The fourth and final group shows different combinations of management commitments: that is, the degree of researcher versus farmer involvement in the trials across trial type.

4. DETERMINING WHO TO COLLECT DATA AND WHEN

Collection of routine (systematic) data and data on an emergency basis (in reaction to an unanticipated pest problem or other potential seasonal disasters) is always a time consuming process. The team must carefully consider each field or household measurement to be taken. Each measurement costs money in terms of time spent on it and alternative activities not undertaken because of the measurement.

Once a team has decided upon the types of measurements to be taken based on the minimum data set and a list of agreed-upon seasonal monitoring measurements, the measurements themselves must be made. There is usually a definite learning process that anyone must go through to collect any individual datum. Systematic data collection is earlier, as individual measurements of individual parameters become much easier to make with a little practice.

In most cases, data are collected by field level researchers or their trained assistants. This is especially true in exploratory trials. In some cases, survey data may be collected by trained researchers who have responsibility for this task across several field sites. Farmers or other household members may be trained to collect and record some data, such as daily rainfall or spotting obvious insect damage. This tends to be more risky, and requires an in-depth understanding by the farmer of farm household member of exactly what is to be done and why.

In some societies, there are cultural and social barriers which hinder or prevent men who are unrelated to a household to speak with female members of the household. When this is the case, and women are engaged in production or decision making activities regarding the enterprise involved in a trial, it may be necessary to have female researchers or assistants involved in the data collection. In some instances, these social and cultural barriers can be overcome with specific training for male data collectors but often the best route is to have male and female reserchers and assistants involved in the field testing and data collection.

For primary and secondary experimental data, there is very little flexibility as to when data should be collected. Some occurrences such as plant emergence days to flowering, time of crop management activities, harvest of yield samples, birthing of livestock, etc., occur at set times and should be measured or observed at that time.

TABLE IV,C.1

Characteristic (in general)	Trial Type: Exploratory	Refinement	Validation
Agronomic parameters			
# of trials	relatively few: 5	more: 10	most: 15-30
# of treatments	most: 5-12	less: 2-6	least: 1 or 2
# of replicates	several: 2-4	fewer: 2-3	none: 1
Plot size (harvest)	smallest	intermediate	largest
Proportion farmer field	no more than 10%	about 10%	20-100%
# of control plots	most: up to 4	intermediate	each farmer's only
Management parameters			
Total "work" involved	most	intermediate	least
Farmer participation	least		most
Researcher participation	most		least
Extension participation	least		most
Socio-economic parameters			
Risk to researchers	most	intermediate	least
Risk to farm household	most	intermediate	least
Exposure to others	least	intermediate	most
Informal dialogue import	most	intermediate	least
Formal survey importance	less	most	less
Farmer acceptance import	least	intermediate	most
Cost to researchers	most		least
Cost to farmer	least		most
Management combinations			
R-planted, R-managed	common	possible	non-existent
F-planted, R-managed	rare	common	possible
F-planted, F-managed	non-existent	possible	common

Farmer recall of events can be used to collect some types of data. These data are primarily discrete data. It is imperative that plans be made well in advance as to who is responsible for what data collection, and that a regular schedule is established for field visits to make measurements or observations at the correct time.

Most environmental setting information can and probably should be collected in advance of trial planting. However, the timing of collecting most types of these data is not so rigid. For example, soil type and slope of field change much more slowly than does a plant insect infestation or disease outbreak. The same can be said for most social and economic data. Much data in this category relies on farmer or other household member recall during interviews. Such interactions should occur on a weekly or bimonthly basis at least. However, such data as labor and power measurements, for example, are best made at the time they occur. The team should not overlook training local data gatherers. Often, such individuals as sons or daughters of collaborating farmers, or village school teachers, make excellent assistants in collecting socio-economic data, especially through the systematic use of simplified farm record books or forms.

5. MEASURING TECHNIQUES

There are three basic keys to measuring anything on-farm. They are:

1. Making sure that the measurement is necessary
2. Making sure that the sample size for the measured variable is neither too large nor too small
3. Making sure that the measurement is taken at the appropriate time during the season.

Defining which measurements are necessary may require a lot of interaction between team members and between the team and the farmers of the domain. Such interaction is important to allow derivation of a list of measurements which are generally agreed upon as being essential.

All too often a team takes a sample of 30 plants per plot for a given measurement without ever asking if 30 plants is the appropriate sample. If the same mean value for the variable can be obtained by sampling 10 plants, the team can save a lot of measurement time over a 30-plant sample. Empirical comparisons during the first measurement time can allow the team to determine proper sample size, even when underlying variability in the variable being measured is unknown initially.

Often, measurements are made at inappropriate times during the season. For example, in working with corn, researchers may take their final plant counts when the plants are approximately 20cm high. They later convert those post-emergence count into an established plant count and the effective trial densities by

plot. However, experience has shown that corn plant numbers continue to decrease at the farm level as much as two to three months following emergence, depending on the length of the growing cycle of the corn varieties. Pre-harvest plant densities may be 50% less than post-emergence plant densities. Likewise, monitoring either insect or disease pest occurance and severity is extremely difficult, and timing is the key element to their measurement and/or effective control.

The objectives of any trial determine which measurements constitute the set of necessary experimental data. Likewise, trial objectives relegate other measurements to an "optional" category. For example, during exploratory trials, it may be necessary for the team to record plant heights, since some of the varieties they are working with may never have been tested off-station before. However, during validation, such measurements are not necessary.

Seasonal pest damage monitoring can be frustrating. Pest damage may be absent for nearly the whole season. Then, when (or if) such damage occurs, it may take both the team and the farmers by surprise. Since timing is such a crucial factor in assessing any type of pest damage or in recommending controls, the team may spend more time during exploratory trial stages monitoring trials for potential insect and disease damage and causes. Such intensive monitoring may be less necessary in later stages of the FSR/E sequence. The more frequent monitoring required during exploratory stages should help the team to minimize the potential disastrous effects of a major pest outbreak.

Nearing harvest, most teams should record several variables. Depending on the crop, one such measurement is plant density. Another such variable may be plant height. Finally, depending upon occurance, plant lodging severity and timing should be measured.

Harvested grain (or yield) needs to be weighed or otherwise measured as soon as possible after harvest. Where grain is the harvested variable most important as a trial objective, a moisture percentage for it needs to be determined as soon as possible after harvest. Grain samples may be measured for moisture in the field (using portable moisture measuring devices), or may be returned to an experiment station for measurement. In the latter case, the team must protect the grain sample from either acquiring moisture (if it is raining during harvest) or losing moisture (if it is extremely sunny and dry during harvest). The most practical field measuring device for grain moisture determination is the Dole moisture meter, given its adaptability to wide ranges of moisture contents and grain types.

The area of yield measurement is which must be understood so as not to err. Most of the common yield measurement errors overestimate yield. That may not be serious if all treatments

receive the same bias. In terms of yield measurement, if the inexperienced researcher will remember that a plant "occupies", or is allotted half the space between it and the next plant, one of the most common errors can be avoided. If the crops are planted in rows, one must remember to begin measuring yield area from the middle of the row and from the midpoint between two plants within the row.

Another consideration is the issue of moisture content in harvested grain. Usually, treatment differences are not large and can be comfortably ignored. However, if the trial is a variety test, or possibly a fertility test, the late-maturing treatments often have an unfair advantage because seed of the later maturing material may be expected to contain more moisture at weighing and appear better than they really are.

Two examples of field books are included in this section for informational purposes. One example is from Asia (Figure IV,C.2) the second from Latin America (Figure IV,C.4). These examples should not be considered to be complete, nor should either be used without first being adapted to the specific situation of a given field team. These two examples provide field measurements considered important by two groups of farm level researchers.

Figure IV,C.2 Example 1, Nepal Field Book

Form 1: Plot record:

1. Site: Ward: Village:
2. Farmer's name:
3. Diagram of cropping pattern to be planted in this plot (calendar):

Jan.	Feb.	Mar.	Apr.	May	Jun.	Jul.	Aug.	Sep.	Oct.	Nov.	Dec.

4. Field description:

5. Lowland: Upland:

Form 2: Crop record:

1. Crop(s) type:
2. Crop(s) variety:
3. Cropping pattern:
4. Previous crop:
5. Date of first land preparation:
6. Plot size:
7. Compost used for this plot (volume) (weight)
8. Date seeded:
9. Date transplanted:
10. Age of seedlings:
11. Seed germinated: yes no
12. Planted in rows: yes no
13. Row spacing: plants/hill
14. Date of emergence;
15. Stand at emergence good poor
16. Flowering (50%) date:
17. Fertilization:

Type	Composition	kg/ha applied:	Application date:
			Application method:

18. Pest occurrence (insects, weeds, diseases, rodents, birds, other):
19. Harvest date:
20. For rice, wheat finger millet: No. of productive tillers, 1 meter square area:
21. For maize and other crops: No. of plants in harvested area:
22. Degree of lodging: Date of lodging:
23. Plant height at harvest time:
24. Sample: Harvest time: Area:
    Sun-dried straw yield, kg:
    Grain yield, kg:
    Grain moisture (5):
    Calculated grain yield:
25. Comments and/or remarks:

Figure IV,C.3 Cropping Systems Program Research Manage Trial Data Compilation Form

CROPPING SYSTEMS PROGRAM

RESEARCH MANAGE TRIAL DATA COMPILATION FORM

A. GENERAL INFORMATION:

01. Site : _____; 02. Panchayat: _____; 03. Ward # : _____; 04. Farmer's Name: _____

05. Cropping Pattern: _____; 06. Season Trial is to be Planted: _____; 07. Previous Crop: _____

08. Fertilizer Used in Previous Crop: a) Inorganic (NPK) kg/h: _____; b) Compost Ton/Hectare: _____

09. Land Type: _____; 10. Soil Texture : Light [ ], Medium [ ], Heavy [ ]

11. Drainage : Good [ ], Fair [ ] Poor [ ] ; 12. Irrigated [ ] or Rainfed [ ]

13. Source of Irrigation : Canal [ ] or Well [ ] None [ ]

B. TRIAL INFORMATION

01. Type of Trial : _____; 02. Crop and Variety : _____; Sole [ ], Relay [ ], Intercrop [ ]

03. Land Preparation : _____; 04. Method of Planting : _____
(No. of Plowing & Plankings)

05. Spacing (cm) : Between rows: _____, Between seeds or hills: _____; 06. Seeding Date : _____

07. Seeding Rate : _____kg/ha; 08. Transplanting Date : _____; 09. Age of Seedlings : _____

10. Weeding: Number _____, Dates _____; 11. Accidental Flooding of Plots: Yes [ ] No [ ], How Many times _____,

Dates _____; 12. No. of Irrigation and Dates of Irrigation : _____

13.

Fertilizer Applied:	Kind	Amount a.i. kg/ha.	Dates of Application
Basal:			
1st Top Dress:			
2nd Top Dress:			
Insecticide			
Herbicide			

Contd...../.....

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Repli-cation	Plot No.	Treatment	Plot Size (m^2)	Date of Emer-gence	Stand at Emer-gence*	Crop Damage Report**				Plant Population***		Date of Harvest	Days to Matu-rity	Sample Area (m^2)	Fresh Wt. (kg)	% Moi-sture	Yield at SMC (kg/ha)	Sun Dried Straw Yield (kg/ha)
						Weeds	Lodg-ing	Ins-ects	Oth-ers	Total	Productive Parts							
	01																	
	02																	
	03																	
	04																	
	05																	
	06																	
	07																	
	08																	
	09																	
	10																	
	11																	
	12																	
	13																	
	14																	
	15																	
	16																	
	17																	
	18																	
	19																	
	20																	

* - Poor, fair, good

** - Rating scale for crop damage - 0 - No economic damage, 1. Slight economic damage, 2. Moderate economic damage, 3. Severe economic damage,

*** - Plant Population - For Rice, Wheat and Finger Millet, count total number of tillers and number of productive tillers in 1 m^2. For Lentil, total number of plants in 1 m^2. For Maize, total number of plants and ears in whole sample area. For Potato, total number of plants and marketable tubers in whole sample area. For all other crops, total number of plants in sample area.

Prepared By: _____

Checked By: _____

IADS:da

Figure IV,C.4 Cropping Systems Program Cropping Pattern Testing Data Compilation Form

CROPPING SYSTEMS PROGRAM
CROPPING PATTERN TESTING
DATA COMPILATION FORM

A - GENERAL INFORMATION: 01. Site : _____; 02. Panchayat : _____; 03. Ward Number _____
04. Season : _____; 05. Cropping Pattern : _____
06. Land Type : _____

B - TECHNOLOGY:
01. Crop : _____ 02. Variety : _____; 03. Seed Rate (Kg/ha) : _____; 04. Spacing (Cms): _____
05. Planting Method : _____; 06. Seedlings/Hills : _____; 07. Total Fertilizer (NPK, kg/ha) : _____
08. Basal Fertilizer (Dose) : _____; 09. First Top-Dressing (Dose) : _____; 10. Second Top-Dressing (Dose) : _____
11. Insecticides (Dose, Date of Application by Location) : _____

C - DATA FOR EACH LOCATION :	Location 1	Location 2	Location 3	Location 4	Location 5	Average
01 FARMER'S NAME						
02 PLOT SIZE (M^2)						
03 ESTIMATED COMPOST APPLIED TO PLOT (KG)						
04 SEEDING DATE (DAY - MONTH - YEAR)						
05 TRANSPLANTING DATE (DAY - MONTH - YEAR)						
06 AGE OF SEEDINGS (DAYS)						
07 HARVESTING DATE (DAY - MONTH - YEAR)						
08 DAYS FROM SEEDING TO HARVEST						
09 RAINFALL DURING GROWING PERIOD (MM)						
10 NUMBER OF IRRIGATIONS						
11 SAMPLING AREA (M^2)						
12 PLANT POPULATION AT HARVEST IN SAMPLING AREA (FOR RICE, WHEAT AND F. MILLET, NO. OF PRODUCTIVE TILLERS/M^2)						
13 GRAIN SAMPLE FRESH WEIGHT (KGS)						

Contd.../....

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CONTD.....FROM COMPILATION FORM

DATA FOR EACH LOCATION	Location 1	Location 2	Location 3	Location 4	Location 5	Average
14 GRAIN MOISTURE CONTENT (%)						
15 GRAIN YIELD (KG/HA) AT ____ % S.M.C.						
16 SUN-DRIED STRAW WEIGHT (KG/HA)						
17 COMMENTS ABOUT TRIAL:						

D - COMPONENT TECHNOLOGY (SUPERIMPOSED TREATMENTS)

YIELD TONS/HA

	Location 1			Location 2			Location 3			Location 4			Location 5			AVERAGE
	REP I	REP II	-X	REP I	REP II	-X	REP I	REP II	-X	REP I	REP II	-X	REP I	REP II	-X	
T_1																
T_2																
T_3																
T_4																
T_5																
T_6																

PREPARED BY: _____ CHECKED BY: _____

IADS: ds
August 15, 1984

Figure IV,C.5 Example 2, Honduras Field Book

A. Farm/Field Information Section, the following information is provided for each farm in a given domain or homogeneous zone:

1. Farm No.:
2. Farmer's name:
3. Farming system:
4. Date(s) of planting (including replanting):
   Crop 1:
   Crop 2:
   Crop 3:
   Crop 4:
5. Date(s) of harvest (by crop in system):
   Crop 1:
   Crop 2:
   Crop 3:
   Crop 4:
6. Slope:
7. Soil type:
8. Crops grown, previous season:
9. No. of years continuous cultivation:
10. Planting details:

B. Field Map Section, Each trial map includes:

1. Treatment number (keyed to treatments and defined in the field book),
2. Compass orientation of the trial, and additional helps (trial in relation to obvious landmarks or in relation to farmer's house),
3. Plot widths, lengths and calculated areas,
4. Number of hills per crop (if relevant),
5. Number of seeds per hill per crop
6. Number of rows wide all plots are, as well as total row width of trial.

C. Seasonal Monitoring Section, subsequent pages of the field book include the seasonal monitoring observations taken. All such measurements are keyed back to the original plot map and plot and treatment numbers. Examples of routine seasonal measurements taken on trials for a (corn + sorghum) system were:

1. General condition of each trial and plot.
2. Weediness.
3. Plant heights.
4. Dates to 50% flowering.
5. Observations of standing water in rainfed plots as well as dates.
6. Plant diseases and incidents.
7. Insect damage types.
8. Husk coverage for corn only.
9. Bird damage evaluations at harvest (both corn husks and sorghum heads).

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6. (OPTIONAL) SAMPLING TECHNIQUES

a. Sample Size

It may be more difficult for the team to agree upon the appropriate sample size than to agree upon which measurements must be taken. In practice, most teams agree to err on the side of safety. Thus, if the sample size is not known, the team may agree to sample 30 plants rather than 10 or 15.

While in theory it is statistically possible to determine the sample size quite precisely, with a knowledge of the underlying variability across which one is measuring, this is usually not done before field measurements are taken. This is usually because the team is too busy to go through this exercise as a group, or because the team does not possess enough cumulative statistical knowledge or have access to a biometrician or statistician.

Generally speaking, the less variability expressed by the variable being measured, the smaller is the necessary sample size to obtain an unbiased estimate of its mean and variation. For example, less plants need to be sampled for plant height, a heritable trait, than for foliar insect damage. An even larger plant sample is needed to non-random events in field plots. A good example of the non-random phenomenon is cutworm seedling damage (or even fall armyworm damage) in corn or sorghum. Such damage is rarely, if ever, randomly distributed in the field, and larger samples are required to pick up and account for the "lumpy" nature of such damage. Finally, the more frequent a variable appears in the population, the smaller is the required sample to quantify its incidence. For example, if 75% of rice plants are infected by brown plant hoppers, a much smaller sample is needed to verify this incidence than if only 5% of maize plants are affected by cutworms.

b. Sample Types

The question of what to sample is not always a simple one to answer, especially when such sampling is to be done under diverse conditions in farmer's fields. A listing of those types of crop-based variables which should normally be monitored or sampled during the growing season generally include:

1. Germination percentage of seeds, or viability percentage of tubers, roots or cuttings

2. Soil samples for nutrient levels and/or moisture levels

3. Crop emergence percentage or rate of "take " of grafts

4. Types of crop or seedling losses, which may be caused by (a) damping off diseases, (b) cut worms, (c) subsurface insects, (d) rodent and/or bird damage (both may uproot and eat

emerging seedlings), etc.

5. Seasonal crop/leaf/fruit damages, which may be caused by (a) foliar diseases, (b) plant diseases, (c) foliar insects, (d) plant boring insects, (e) ear (or fruit) diseases (rots and smuts), (f) ear (or fruit) insects (ear worms and occasionally pre-harvest weevil damage in basic grains, (g) lodging severity and timing, (h) other seasonal occurances, such as time of branching, time of flowering, plant (and/or) ear heights, propensity to shatter, etc.

6. Post-emergence stand density

7. Pre-harvest stand density

8. Measurement of tillering

9. Seasonal observations on trial weediness (coverage and predominant types or species of weeds)

10. Harvest measurements of (a) total yield, (b) marketable yield (if different from (a), (c) moisture percentage (for grains), (d) stand density, (50 harvest time damages, including those caused by diseases, insects and rodents/birds.

TABLE IV,C.2 "Guidelines for Sampling Variables" at the end of this subunit applies only to crop-based farm trials, and is arranged according to three columns. The first column contains the general variable to be sampled and the second column gives general advice about what (or how) to sample, while the third column provides very rough estimates of how large the given sample should be. Remember that this table is provided as a very rough guide only, and that each team should determine the appropriateness of (1) the variable to be sampled, (2) what (or how) to sample and (3) sample size.

Often samples in fields are drawn following a zig-zag or serpentine path. Start of the sampling path is randomly selected. Other approaches to sampling are also available. Dividing fields by soil type and sampling within each soil type is an example of stratified random sampling. Drawing a random sample of fields in a domain and then sampling rows within each field would be an example of multistage random sampling. Multistage random sampling may be useful if the team is managing a large number of trials.

What is the sample size required? There are not easy answers to this question. The sample size necessary to assure the determination of the mean level of any type of pest damage is related to three factors:

1. Its frequency in the population to be sampled and

2. Its degree of randomness in the same population

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3. The level, or degree, of reliability the researcher is willing to accept for the sample

A general formula to determine sample size is provided in Cochrane and Cox, 1957. An approximation for determining a minimum sample size is to start with a sample which is known to be too large. Assume, for example, that each plot contains approximately 60 plants and that the initial sample size, for variable n, will be 25 plants per plot. Pre-mark your field books or data collection sheets, with room for this 25-plant sample. In addition, save room at the bottom of this sheet for calculations of the means and standard deviations for (a) the 25-plant sample, (b) the first 20 plants sampled, (c) the first 15 plants sampled and (d) the first 10 plants sampled. Then, when the sampling process is finished, calculate and compare both the means and standard deviations for all four of the sample sizes. Then do a paired t-test for the means of each plot based on the 25 and 10 plant samples. If, at the 0.05 level of significance, the per plot means do not differ between the 25 and the 10 plant samples, then the field team would be perfectly justified in carrying out 10-plant samples for all future observations of variable n. Not only would the 10 plant sample be almost as accurate as the 25-plant sample, but it is also much less time-consuming for the field team to carry out. The team must remember that sampling any variable takes time, and the larger the sample size, the longer the team must spend in the field gathering these data.

c. Livestock Sampling

The following comments are drawn from a discussion which occurred at a Workshop on Livestock in Mixed Farming Systems: Research Methodologies and Priorities, held at ILCA (International Livestock Centre for Africa), June 24-27, 1985. The participants agreed that sound methods of collecting and preparing livestock samples must be followed both on-station and on-farm. Since more variability is encountered at the farm level, samples from that source should be greatest in number as compared to sampling on-station or in the laboratory. Example: collect two-three forage samples for analysis at the farm level, and analyze each rather than collecting one forage sample and analyzing it three times by splitting the sample in the laboratory.

While researchers can collect a great number of samples in the field, participants stressed that samples should be collected only when analysis of the measured parameter has significant meaning to the trial outcome. Example: is it worthwhile analyzing blood serum for calcium concentrations when the homeostasis mechanisms of the animal control this level very closely even in deficiency?

(The FSSP wishes to acknowledge the trip report of Dr. L.R. McDowell, Department of Animal Science, Center for Tropical

Agriculture, University of Florida, as the source of the above comments. Any possible misinterpretation of Dr. McDowell's trip report must lie with the FSSP).

7. RECORDING TECHNIQUES

When the nature of the data to be collected for each trial has been identified, appropriate field record books and data compilation forms should be developed well in advance of trial planting. The field record books should allow the team and assistants to record required data and observations in an orderly and concise way. Adequate space should be left in the field record book so that the researchers can draw an accurate map indicating the location of the trial and the treatments in each field. Time spent in developing simple field record forms is well repaid by time saved in recording data in the field. Again, use of the mimimum data set is recommended.

Both field record books and data summary forms should allow for photocopies to be made to prevent data loss. An office copy of all data collected from all trials must be maintained to guard against the accidental loss of a field book. Considerable thought must be given to developing a system for organized data filing and storage to allow easy, future reference to the results from previous trial data. An innovative method for FSR/E data storage has recently been developed and implemented by CARDI (the Caribbean Agricultural Research and Development Institute), operating in the Windward and Seeward Islands of the Caribbean. CARDI personnel refer to this process as TIF, which stands for Technology Improvement File. The TIF contains four major categoties of materials relating to FSR/E trials, arranged physically in filing cabinets and placed in manila folders. All trial-related materials are arranged according to the following scheme:

1. File 1: Target area farm systems description
2. File 2: Recommendations (all proposed trials, designs, analyses and results)
3. File 3: Rationale (for each trial and/or treatment)
4. File 4: On-going research (references back to station-based research, informal trials, ideas for next year, etc.)

For more information about the TIF process of data storage, contact either Calixte George, Robert Hart or John Hammerton, CARDI, Castries, St. Lucia, West Indies.

8. DATA PROCESSING TECHNIQUES AND EQUIPMENT

Simplicity is the key to data processing. Use of complicated data collection or summary forms is generally counterproductive and very time-consuming. In general, a much quicker and more reliable analysis can be carried out using hand-held calculators at the team level than can be obtained by coding data and relying

upon a central data processing facility to conduct the needed analyses.

In on-farm research, the magnitude and diversity of experimental variation is usually larger than for experiment station trials. Many times some of this greater variability can be explained and logically adjusted to reduce variability if the data is examined to identify outliers. Upon identification, the team should check to determine the cause of the extreme value of each outlier. If the value is due to incorrect transcription from the original field record book, the correct value can be entered onto the compilation form or into the data processor. If the outlier is due to incorrect recording in the field book (such as several rows of data being written one row below their corresponding treatments, a decimal point left off, and extra zero written in etc.) data can be matched with the correct treatment numbers or otherwise corrected where possible, or treated as a missing plot where not possible. If no transcription or recording errors can be found, then the outlier data point should be retained in the analysis, and the team should try to identify the field or farm conditions which caused the extreme value. Only researchers and assistants who have intimate knowledge of the trial conditions can make these decisions at the time of data analysis.

Finally, data should be analyzed as rapidly as posssible because FSR/E is a continuing, year-round activity. Trial analysis and interpretation of results should be immediately available to the team to allow rapid decisions to be mde concerning the next series of trials. This argues for the development of a data analysis capacity by researchers and assistants operating at the field site.

Equipment to handle research data is constantly being improved. The availability of hand-held calculators, micro computers and main frame computers, however, varies greatly from country to country. As a minimum, each researcher should have an inexpensive, programmable, hand-held calculator. With training, researchers and their assistants will be able to handle most simple data analysis. These calculators are particularly appropriate for village-based researchers where electricity may not be available or may be suvject to severe power surges, outages, or where extremes of temperature and/or humidity exist and cannot be modified.

Micro (desk top) computers can play an important role in data analysis, data storage, report preparation, etc. However, care should be taken not to fall into the situation where trial data from field sites are only recorded there and blindly forwarded to a central location for processing or analysis. Many data-based decisions can only be made by team members who have a close relationship to the farm sites from which data are obtained.

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TABLE IV,C.2 Guidelines For Sampling Variables

Variable to Sample	What (or How) to Sample	Number to Sample
Viability/ germination %	Remaining seed, cuttings, roots, tubers or stems	100 seeds/variety (include farmers')
Soil sample	Regular recommended procedure as per farm. Draw 2 (or more) if there are 2 obviously different soil types	Three samples per trial site; 6 for 2 major soil types
Crop emergence	Count emerged seedlings 1-2 wks after emergence. Take care if replanting is a common practice	At least 3-4 hills per row and all rows per harvestable plot
Tillering		
Lodging	Count both lodged/non-lodged plants for each treatment	All plants in the trial; or all plants in the harvestable plot
Branching time		
Flowering time	A sub-sample per variety and replicate	20 plants per variety
Plant height	A sub-sample per variety and replicate	10 plants per variety
Ear height	A sub-sample per variety and replicate	10 plants per variety
Foliar insect damage	Determine rating scale if any, to be used. Apply it at critical times during season	25 plants per harvestable plot for grains
Foliar diseases damage	Determine rating scale if any, to be used. Apply it at critical times during season	10 plants per plot for grains
Ear insects	At harvest, decide on incidence, severity or both in the harvested sample	Entire harvested sample is to be graded for insect damage incidence, severity or both
Ear diseases	At harvest, decide on incidence, severity, or both in the harvested sample	Entire harvested sample is to be graded for insect damage incidence, severity, or both
Fruit insects	At harvest, decide on incidence, severity, or both in the harvested sample	Entire harvested sample is to be graded for insect damage incidence, severity, or both
Fruit diseases	At harvest, decide on incidence, severity, or both in the harvested sample	Entire harvested sample is to be graded for insect damage incidence, severity, or both

ACTIVITIES

ACTIVITY ONE:	INPUT MEASUREMENTS AND PLOT PLAN PREPARATION
ACTIVITY TWO:	TRIAL LAY OUT AND MASUREMENT TECHNIQUES
ACTIVITY THREE:	WORKSHEET FOR "STANDARD" TRIAL DESIGN
ACTIVITY FOUR:	WORKSHEET FOR NON-CONTIGUOUS REPLICATIONS TRIAL DESIGN
ACTIVITY FIVE:	WORKSHEET FOR NON-CONTIGUOUS PLOTS TRIAL DESIGN
ACTIVITY SIX:	WORKSHEET FOR "STANDARD" TRIAL DESIGN WITH SPLIT PLOT ARRANGEMENT
ACTIVITY SEVEN:	WORKSHEET FOR "STANDARD" TRIAL DESIGN WITH SUPERIMPOSED TREATMENTS
ACTIVITY EIGHT:	WORKSHEET FOR "STANDARD" TRIAL DESIGN WITH 2 SUPERIMPOSED TREATMENTS
ACTIVITY NINE:	WORKSHEET FOR "STANDARD" TRIAL DESIGN IN TERRACED, NON-RECTANGULAR AREAS

ACTIVITY ONE TRAINEE INSTRUCTIONS
INPUT MEASUREMENTS AND PLOT PLAN PREPARATION

OBJECTIVES:

After completing this activity you will be able to:

1. Prepare the necessary seeds and assemble the necessary materials and inputs to allow them to lay out a series of systematic trials in farmers' fields.

2. Weigh out samples of dry inputs and measure out samples of liquid inputs with proper consideration of safety rules, water measurement, mixing of inputs, and calibration of simple back-pack spray equipment for small plot treatments.

INSTRUCTIONS:

1. This afternoon you will be handling seeds (tubers, cuttings, roots, etc.) in preparation for tomorrow's trial lay out and planting exercise in a farmer's field. You will also be measuring out liquid inputs (herbicide, etc.) and weighing out dry inputs (fertilizer, insecticide, etc.) to be used in the field exercise on-farm. Please follow all of the handling instructions and take all the precautions listed on the label of each input during this preparatory exercise and during the field exercise tomorrow. As researchers, you must be extra careful and aware of your influence on farmers when you work with or near them in their fields, especially in the way you handle and respect pesticides. Proper handling and disposal techniques are essential, both for your own safety and for the instructional impact you will have on the observing farmers.

2. Please read over carefully the attached field exercise. Much of what will be learned today may be intuitive to many of you. However, please accept this preparation exercise as it is. Certain differences between the station and farm situations may not be obvious to you now, but will become clearer later.

3. This preparatory exercise will be carried out by each of your assigned work groups. Each member of your work group will be given the chance to participate in each of the counting or measuring exercises in preparation for tomorrow's farm trial lay out and planting exercise. Trainers will circulate between groups to observe and to answer any questions which may arise.

4. Complete the attached handout #1 "Field Exercise Preparation Worksheet" during your group's preparation for the field lay out and planting exercise this afternoon, and hand it in at the end of the day to one of your trainers.

PREVIOUS PAGE BLANK

INPUT MANAGEMENT AND PLOT PLAN PREPARATION (WORKSHEET)

A. Trial details:

1. Trial type:
2. Trial design:
3. # of reps:
4. # of treatments per rep:
5. Treatment specifics:

T1:

T2:

T3:

T4:

T5:

T6:

T7:

T8:

T9:

T10:

T11:

T12:

6. Draw below your working group's trial design, including replications, treatments and plot numbers. Use a logical key to save time and space. Your work group may number your farm trial plots

_____From left to right
_____From right to left
_____In serpentine fashion

PREVIOUS PAGE BLANK

B. Planting material details:

1. Type of material to be planted:
2. # seeds/cuttings/tubers/roots per plot:
3. # seeds/cuttings/tubers/roots per trial:
4. # (or %) of 3. saved as remnant:
5. # seeds/cuttings/tubers/roots subjected to viability:
6. Was planting material placed in separate envelopes/containers for each plot? _____Yes _____No.
7. If 6. answered "No," why not?_____
8. Was each plot envelope/container marked? _____Yes _____No.
9. Was the rep # used in marking envelopes/containers? _____Yes _____No.
10. What will your work group's strategy be if, when your trial is layed out tomorrow, you do not have enough seed/cutting/tuber/root material to plant all of
    a. the trial_____?
    b. any given plot_____?
11. Comments:

C. Input details:

Input 1

1. Type of input:
2. Formulation:
3. Amount to apply per ha/ac/etc.:
4. Estimated size of each treatment plot:
5. # plots per any given amount of input:
6. Amount of input needed per treatment:
7. Amount of input needed per plot:
8. Describe the process of measuring this input on either a per plot or on a treatment basis:

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Input 2

1. Type of input:
2. Formulation:
3. Amount to apply per ha/ac/etc.:
4. Estimated size of each treatment plot:
5. # plots per any given amount of input:
6. Amount of input needed per treatment:
7. Amount of input needed per plot:
8. Describe the process of measuring this input on either a per plot or on a treatment basis:

Input 3

1. Type of input:
2. Formulation:
3. Amount to apply per ha/ac/etc.:
4. Estimated size of each treatment plot:
5. # plots per any given amount of input:
6. Amount of input needed per treatment:
7. Amount of input needed per plot:
8. Describe the process of measuring this input on either a per plot or on a treatment basis:

ACTIVITY TWO TRAINEE INSTRUCTIONS
TRIAL LAY OUT AND MEASUREMENT TECHNIQUES

OBJECTIVES:

After completing this activity you will be able to:

1. Lay out a systematic set of trials in farmer's fields under a range of different conditions.

2. Explain the major differences between station trials and farm-level trials.

3. Understand field sampling issues and be equipped with procedures for handling the measurements necessary for observing a group of on-farm trials.

INSTRUCTIONS:

1. Your work groups are expected to accomplish the following during today's field exercise:

   a. Randomize treatments.
   b. Mark treatments on trial (plot) map before planting.
   c. Lay out the trial.
   d. Plant (or superimpose) the trial.
   e. Make all necessary observations and measurments.

2. Randomizing treatments: use simple methods to carry out randomization.

3. Marking treatments: number your experimental plots in a systematic order (either sequential or serpentine), beginning (a) from the upper left hand corner, (b) the lower left hand corner, (c) the upper right hand corner, (d) the lower right hand corner or (e) the corner nearest the normal entry point to the trial. Code your treatments in your field books. Use these codes each time a treatment is mentioned.

4. Lay out the trial: determine whether the trial will be layed out along traditional square (or rectangular) shape, or along the natural contours and boundaries of the farmer's terrances or field. Determine whether the trial will be layed out using contiguous or non-contiguous replicates and/or plots. These are the two major trial lay out decisions.

   a. Rectangular or Square Plots:

   Use the first corner stake or flag as the "base" from which all subsequent measurements will be made. Use the geometric principle, "the sum of the squares of the two legs of a right triangle equal the square of its hypotenuse" — specifically the "3-4-5" right triangle, to assure a "square" trial lay out. Estimate the whole trial size, including any

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necessary border rows, alleys, etc., in advance of setting the first corner stake or flag, because you may have to shift the trial in one direction or another in order to fit it into the available space.

To minimize researcher management confounding, any pre-plant final land preparations should be those of the hosting farm family. For example, instead of measuring in advance a trial plot which is 5m long by 5m wide, your work group should measure out plots which are 5m long, but allow the planting width to vary to accomodate the farmer practice of that particular farm.

Allow the farm family to prepare the necessary width for at least two parcels (plots) before you measure the right angle in preparation for planting.

5. Plant (or superimpose) the trial: It is often possible to begin planting a farm trial before the farm family finishes any necessary land preparation. If the trial is to be planted without land preparation at the time of lay out (examples include planting grains with planting sticks or transplanting rice), they decide whether the farmer and his or her family, including the regular hired help (if any) will carry out the planting, or whether your work group will do it.

When the trial has fully been layed out, all necessary plot corner markers should be in place. Now make the necessary observations and measurements.

6. Make all necessary observations and measurements: a

a. Necessary observations:

During the process of trial lay out, there should be time for one (or more) of your work group to observe and record the following:

— major soil type(s) across the trial and in the field,

— cropping pattern history,

— important farming practices,

— # of seeds planted per hill (if hill planting),

b. Necessary measurements:

Either during trial lay out or immediately thereafter, the following measurements may need to be taken:

— obtain soil sample from the trial area. This should be done <u>before</u> the trial is layed out.

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— average slope of field where trial is placed (using either an inclinometer of a protractor, plumb bob and two boards for a right angle). Take at least 3 separate readings with an inclinometer; 6 separate readings with a protractor and plumb bob.

— measure (or estimate) the size of each plot in the trial. Refer to the appendix for a method of estimating areas of non-rectangular plots.

— measure the spacing between (a) hills, (b) transplants, and/or (c) rows within plots, or measure or estimate the amount of seed planted per row within the plot, depending on method of seeding employed. Measure or estimate the amount of seed planted in a given (pre-marked) area if seeding is done by broadcast.

— make any additional measurements.

ACTIVITY THREE TRAINEE INSTRUCTIONS
WORKSHEET FOR "STANDARD" TRIAL DESIGN

INSTRUCTIONS:

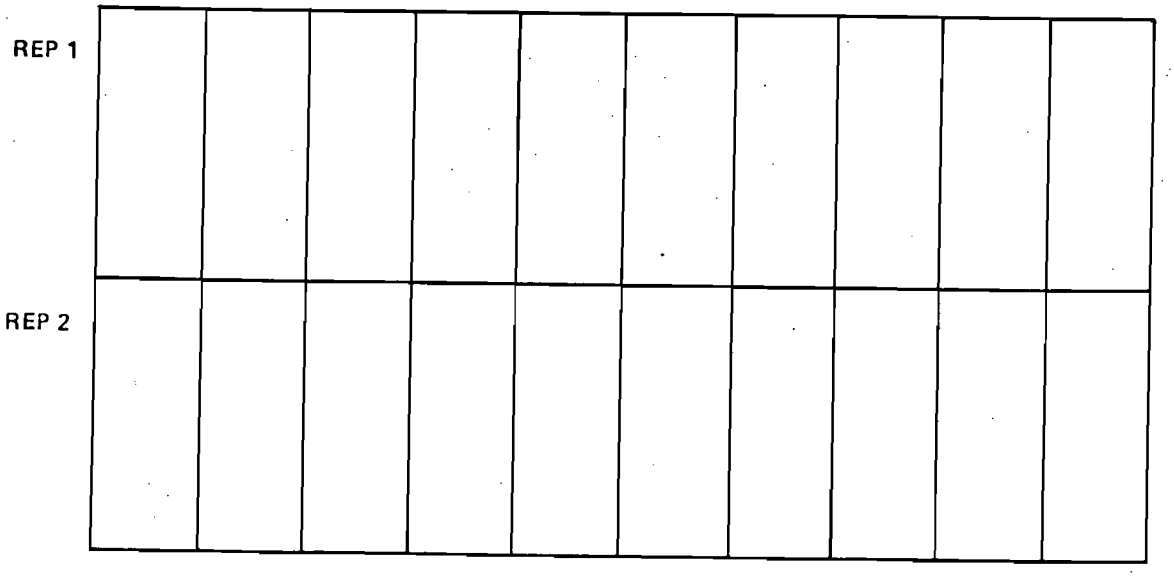
1. This optional trial exercise accomodates a "standard" trial design which may contain as many as ten treatments and two replications in a randomized complete block design.

   You have five specific tasks to accomplish today:

   a. Randomize treatments
   b. Mark treatments on the trial (plot) map before the planting exercise
   c. Lay out the trial
   d. Plant (or superimpose) the trial
   e. Make all necessary observations and measurements.

   Time exercise began:_____
   Time exercise ended:_____
   Total time required:_____

Plot Map



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WORKSHEET FOR NON-CONTIGUOUS REPLICATIONS TRIAL DESIGN

INSTRUCTIONS:

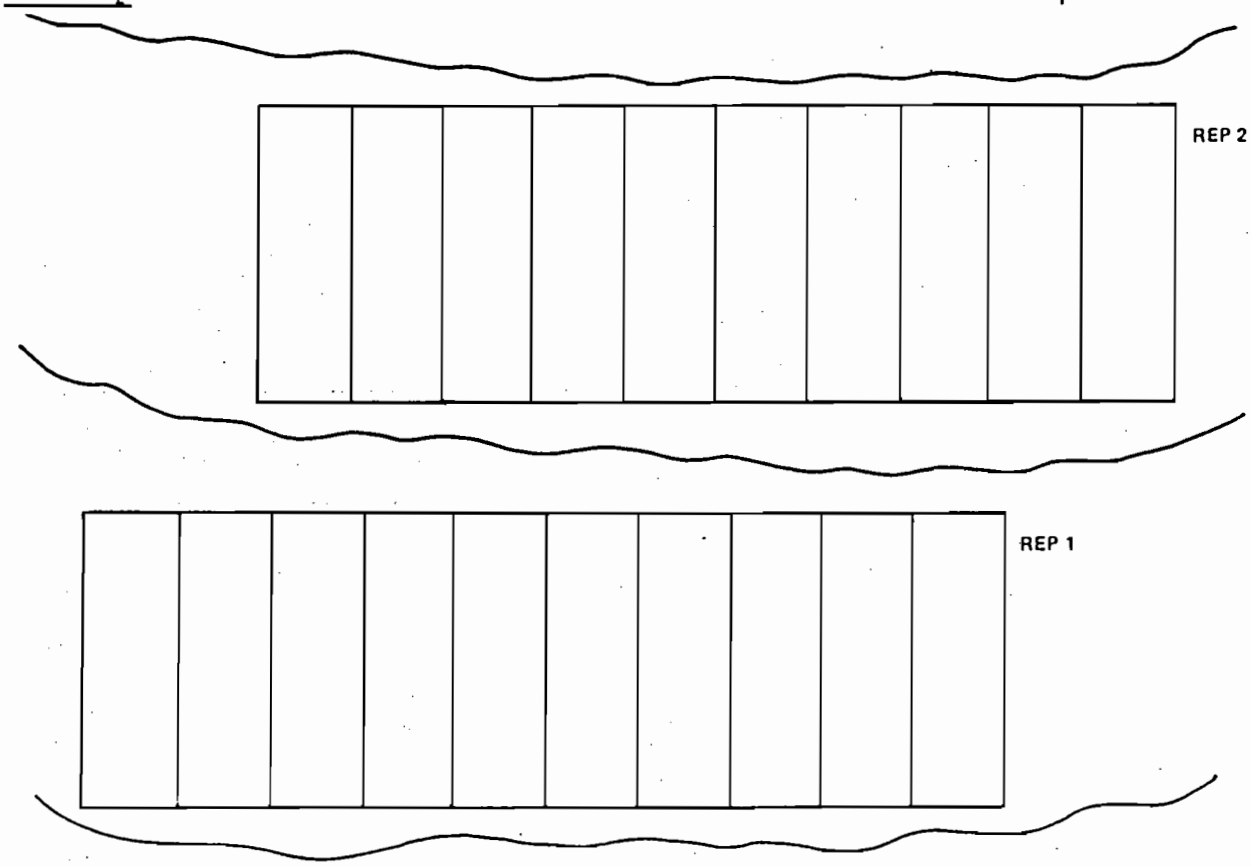
1. This optional trial exercise accomodates a "standard" trial design which may contain as many as ten treatments and two replications in a randomized complete block design where replications must be separated in the farmer's field for reasons of insufficient space (non-contiguous reps).

   You have five specific tasks to accomplish today:

   a. Randomize treatments,
   b. Mark treatments on the trial (plot) map before the planting exercise
   c. Lay out the trial
   d. Plant (or superimpose) the trial
   e. Make all necessary observations and measurements.

   Time exercise began:_____
   Time exercise ended:_____
   Total time required:_____

Plot Map



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ACTIVITY FIVE TRAINEE WORKSHEET
WORKSHEET FOR NON-CONTIGUOUS PLOTS TRIAL DESIGN

INSTRUCTIONS:

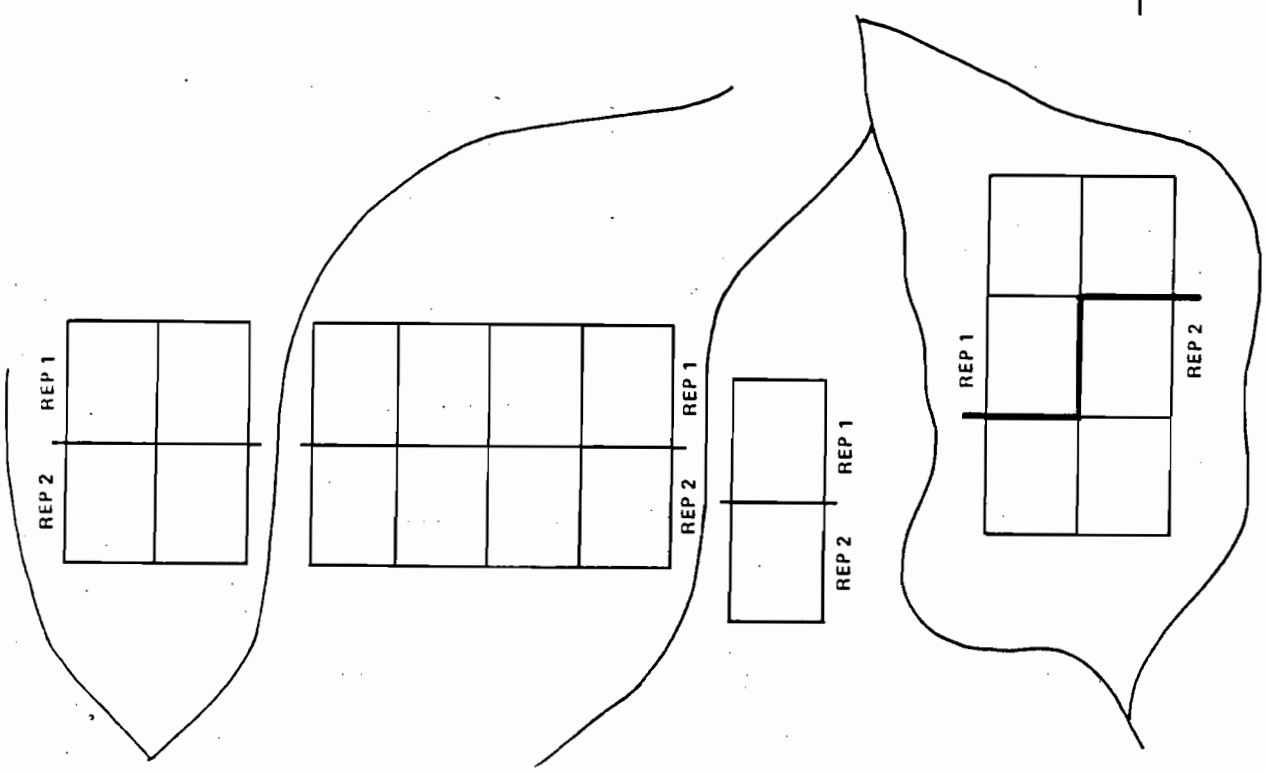
1. This optional trial exercise accomodates a "standard" trial design which may contain as many as ten treatments and two replications in a randomized complete block design which must be separated by plot because of insufficient space in the farmer's field.

   You have five specific tasks to accomplish today:

   a. Randomize treatments
   b. Mark treatments on the trial (plot) map before the planting exercise
   c. Lay out the trial
   d. Plant (or superimpose) the trial
   e. Make all necessary observations and measurements

   Time exercise began:_____
   Time exercise ended:_____
   Total time required:_____

Plot Map



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WORKSHEET FOR "STANDARD" TRIAL DESIGN WITH SPLIT-PLOT ARRANGEMENT

INSTRUCTIONS:

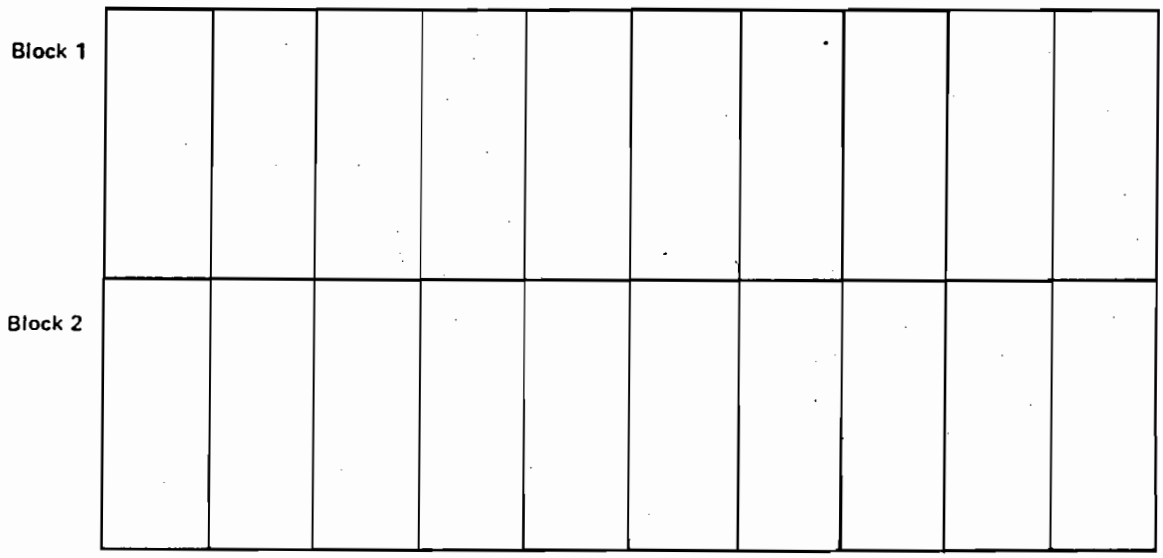
1. This optional trial exercise accomodates a "standard" trial design which may contain as many as ten treatments and two replications in a split plot design.

   You have five specific tasks to accomplish today:

   a. Randomize treatments
   b. Mark treatments on the trial (plot) map before the planting exercise
   c. Lay out the trial
   d. Plant (or superimpose) the trial,
   e. Make all necessary observations and measurements.

   Time exercise began:_____
   Time exercise ended:_____
   Total time required:_____

Plot Map



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ACTIVITY SEVEN TRAINEE INSTRUCTIONS

Worksheet FOR "STANDARD" TRIAL DESIGN WITH SUPERIMPOSED TREATMENTS

INSTRUCTIONS:

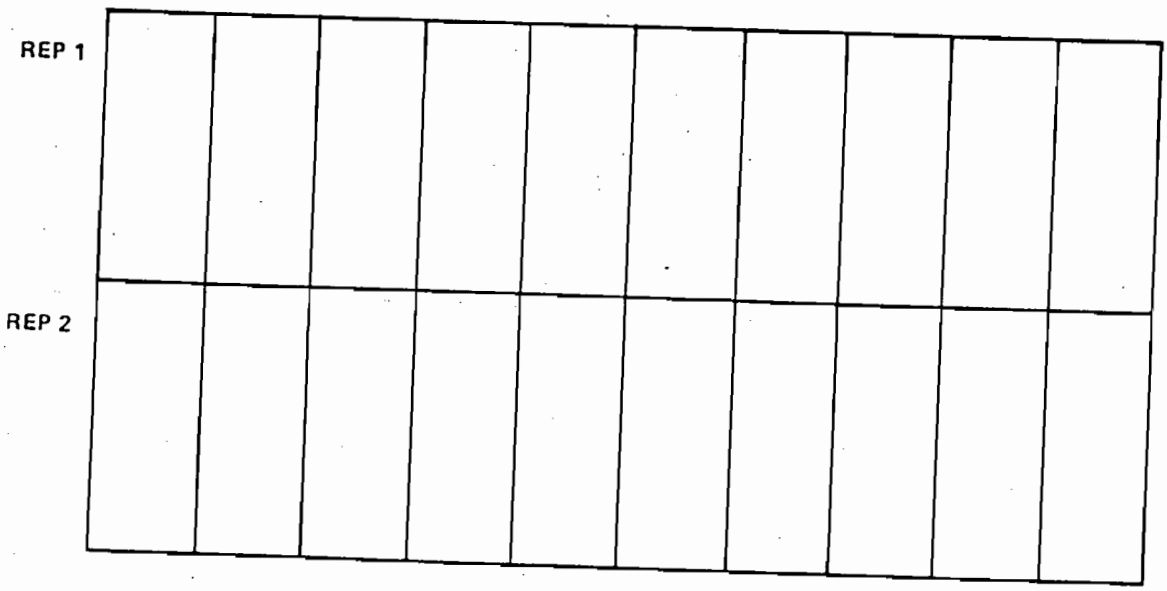
1. This optional trial exercise accomodates a "standard" trial design containing superimposed treatments and two replicates in a cropping pattern trial format.

   You have five specific tasks to accomplish today:

   a. Randomize treatments
   b. Mark treatments on the trial (plot) map before the planting exercise
   c. Lay out the trial
   d. Plant (or superimpose) the trial,
   e. Make all necessary observations and measurements.

   Time exercise began:_____
   Time exercise ended:_____
   Total time required:_____

Plot Map



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ACTIVITY EIGHT TRAINEE INSTRUCTIONS

WORKSHEET FOR "STANDARD" TRIAL DESIGN WITH 2 SUPERIMPOSED FACTORS

INSTRUCTIONS:

1. This optional trial exercise accomodates a "standard" trial design containing two factors of superimposed treatments (N-P, for example) and two replicates in a cropping pattern trial format.

You have five specific tasks to accomplish today:

a. Randomize treatments
b. Mark treatments on the trial (plot) map before the planting exercise
c. Lay out the trial
d. Plant (or superimpose) the trial,
e. Make all necessary observations and measurements.

Time exercise began:_____
Time exercise ended:_____
Total time required:_____

Plot Map

REP 1										
REP 2										

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ACTIVITY NINE TRAINEE INSTRUCTIONS
WORKSHEET FOR "STANDARD" TRIAL DESIGN IN TERRACED, NON-RECTANGULAR AREAS

INSTRUCTIONS:

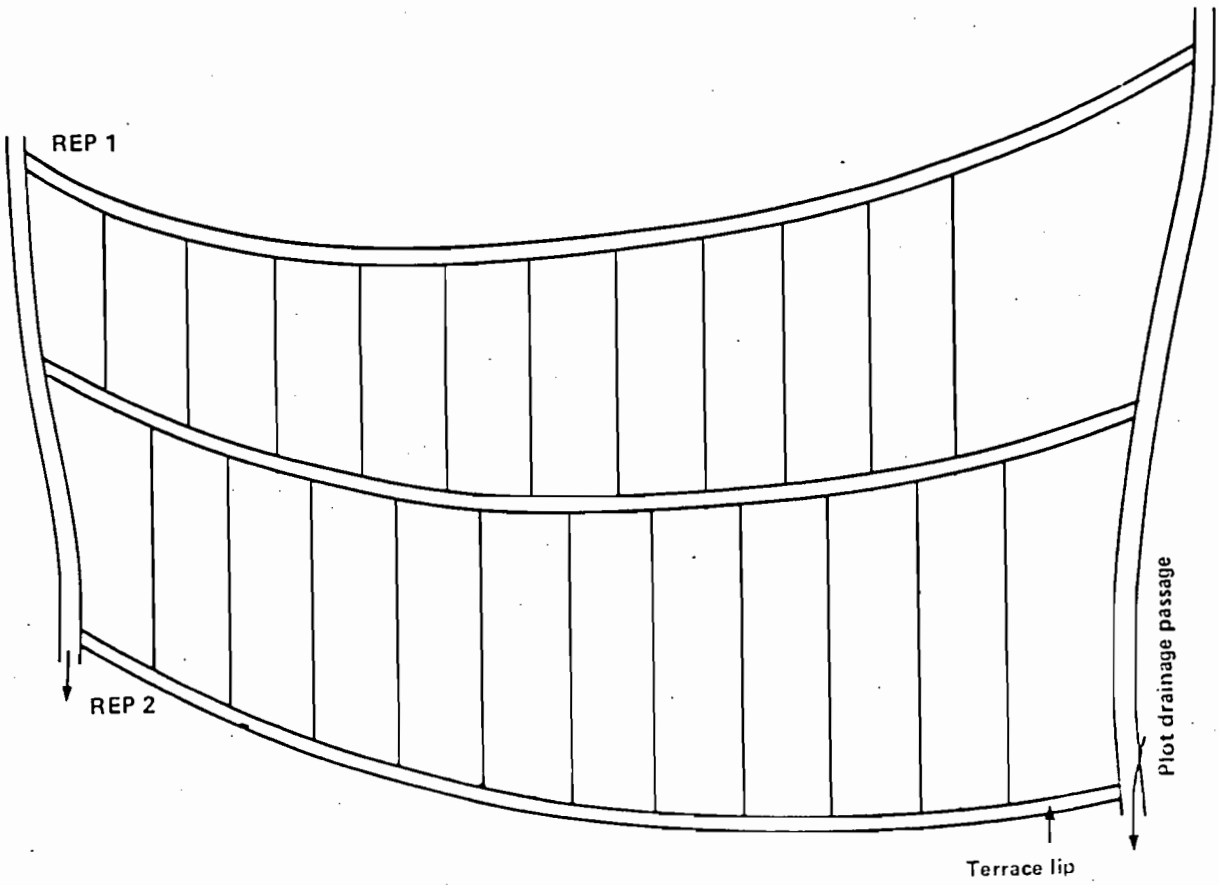
1. This optional trial exercise accomodates a "standard" trial design for placement in terraced, non-rectangular areas.

   You have five specific tasks to accomplish today:

   a. Randomize treatments
   b. Mark treatments on the trial (plot) map before the planting exercise
   c. Lay out the trial
   d. Plant (or superimpose) the trial,
   e. Make all necessary observations and measurements.

   Time exercise began:_____
   Time exercise ended:_____
   Total time required:_____

Plot Map



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UNIT V
HOW TO ANALYZE AND INTERPRET TRIAL DATA

HOW TO ANALYZE AND EVALUATE TRIAL DATA

OUTLINE

1. Ways to Analyze Biological Data
   a. Analysis of Variance for Simple Designs
   b. Combined Analysis Across Farms
   c. (Optional) Analysis of Variance with Inconmplete Block Designs or Incomplete Factorials.
   d. Modified Stability Analysis

2. A Way to Handle "Damaged" Data: Analysis of Covariance

3. Ways to Evaluate Trial Data
   a. Levels of Significance
   b. Coefficient of Variation

PREREQUISITES

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this subunit participants will be able to:

1. Determine the data and information that should be collected at the different stages of on-farm trials in Farming Systems Research and Extension.

2. Develop methods to handle the collection and processing of trial data.

3. Select the proper techniques to analyze and interpret the data and information collected from the trials to allow the next step decision making.

KEY POINTS

1. The nature of on-farm trials conducted in Farming Systems Programs leads to differences in the extent and the characteristics of data and information that should be collected as compared to an experiment station setting.

2. The minimum necessary data should be identified for collections that are consistent with trial objectives and resources available to the research team and which insure that an adequate number of trials are conducted.

DEFINITIONS

ANOVA
combined analysis
confidence intervals
continuous treatments
covariance
CV
discrete treatments
environmental setting information
FSR/E
interaction
LSD
modified stability analysis
outlier data
partial budgeting
1^0 experimental data
regression analysis
response surfaces
2^0 experimental data
significant levels
single df contrasts
exploratory trials (testing)
refinement trials (testing)
validation trials (testing)
confidence intervals
distribution of confidence intervals
modified stabilty analysis

DISCUSSION

1. WAYS TO ANALYZE BIOLOGICAL DATA

Researchers involved in Farming Systems Research and Extension have at their disposal the variety of data analysis techniques that are available to any other researchers. The key is to recognize the peculiar conditions and problems involved in conducting on-farm trials and to know how to select the appropriate techniques to contend with them. In addition, the function of on-farm trials at progressive stages of the research process address different objectives. Different analysis techniques, therefore, will be used to analyze the results of trials at the different stages. For many refinement trials and especially for validation trials, the importance of "traditional" agronomic statistical analyses techniques decreases while social and economic analyses assume a more important role. Therefore, farming systems researchers need to know how to analyze data not only using standard statistical techniques but also using techniques that incorporate the criteria farm household members use in making their own judgments about the technologies being tested as potential interventions.

a. Analysis of Variance for Simple Designs

1. Completely Random Design

Analyses of variance procedures are appropriate when we want to evaluate differences between qualitative treatments such as different varieties, different herbicides, different tillage treatments, etc. Analysis of variance should not be used when treatments constitute quantitative differences or levels of a factor, such as different rates of nitrogen fertilization, or different rates of a pesticide. For analysis of this type of information, regression procedures, involving least squares curve fitting, are more appropriate.

Example 1. Assume three treatments (a, b and c) were each replicated four times, at random, to a set of twelve plots as shown below. This would be a completely randomized design. The data are shown also.

a	c	b	a
b	a	c	c
a	b		
b	c		

	Treatments		
	a	b	c
	4	9	16
	3	10	17
	5	11	16
	4	10	15
Totals	16	40	64
Means	4	10	16

Looking at these data, we would intuitively feel confident that c <u>was</u> better than a and b, and b than a. The ranges of values <u>are</u> respectively 3 to 5, 9 to 11 and 15 to 17. Clearly there is no overlap.

Example 2. Assume now that the yields had been as follows:

	Treatments		
	a	b	c
	5	9	5
	10	2	8
	3	4	17
	6	5	6
Total	24	20	36
Means	6	5	9

Intuitively we would <u>not</u> feel confident that there was any real difference between treatments: the within treatment variation is too large. The ranges 3 to 10, 2 to 9 and 5 to 17, overlap. The range is not a good indicator of variation, however, because it is overly dependent on extreme values. Instead, the variance or mean square is used.

The completely randomized design (CRD) is the basic tool of statistical analysis. In a completely randomized experiment - as in Examples 1 and 2 - we have two sources of variation - variation due to treatments and variation due to residual. The analysis breaks the total variation into these two components. The degrees of freedom (df) are allocated by subtracting one from the number of treatments and one from the total number of plots; the df for residual are derived by difference. The sums of squares (SS) are calculated as follows. The total sum of squares (Total SS) is (the Σ in the following equations stands for sigma or summation):

$$\Sigma x^2 - \frac{(\Sigma x)^2}{n}$$

and the treatment SS is:

$$\frac{\Sigma T^2}{r} - \frac{(\Sigma x)^2}{n}$$

[x represents the value of any single plot, so Σ x is the grand total; n is the total number of plots; T is any treatment total; r is the number of replicates] The term $\frac{(\Sigma x)^2}{n}$ is the correction factor and is used in most calculations of sums of squares.

The actual calculations for Example 2 are

$$\text{Total SS} = (5^2 + 10^2 + 3^2 + 6^2 + 9^2 \ldots + 6^2) - \frac{(80)^2}{12} = 176.7$$

and

$$\text{Treatment SS} = \frac{(24 + 20 + 36)^2}{4} - \frac{(80)^2}{12} = 34.7$$

The analysis of variance is then:

Source of variation	SS	df	MS	F
Treatment	34.7	3-1=2	17.4	1.10
Residual	142.0	11-2=9	15.8	
Total	176.	12-1=11		

The residual SS is derived by difference. The mean squares (MS) are derived by dividing the sums of squares by the degrees of freedom. The F-ratio or variance ratio for treatments is calculated by dividing the treatment MS by the residual MS. In comparison with published statistical tables, the F ratio is not statistically significant. There is a very high probability that the treatment differences occurred due to natural variation in the field. It confirms our intuitive feeling.

The term "error" is perhaps unfortunate: it does not necessarily imply errors on the part of researchers - but it may. Carelessness will contribute to the residual term (error) by increasing variation among replications of the same treatment. The same treatment should give exactly the same response when replicated two or more times. Except in rare cases, there will be a difference in the measured response of some characteristic which has no explanation other than "residual", "error", or "unexplained variation" in the anlaysis of variance context.

"Field experiments are rarely planted as completely randomized designs. Most fields have a slope, or some other gradient which would benefit from blocking. However, once planted in a randomized complete block design and it is found that the "blocks" component of the analysis of variance is insignificant, then the experiment can be analyzed as a completely randomized design. The advantage of the completely randomized design, over the randomized complete block design, when there is no significant block effect, is that there are more degrees of freedom assigned to residual. To illustrate, assume that we are going to test six varieties in four replications, at one location. We will have 24 plots. The ANOVAS will be:

Completely Randomized Design

Source of variation	df
Treatments	5
Residual	18
Total	23

Randomized Complete Block Design

Source of variation	df
Blocks	3
Treatments	5
Residual	15
Total	23

If the sums of all of the plots planted in each block are identical, or nearly so, nothing has been gained by blocking and it would be desirable to have those degrees of freedom (three, in this case) in the residiual term, raising it from 15 to 18, and we would expect to get a smaller residual mean square term when

the residual sums of squares is divided by 18 rather than 15. The smaller the residual (error mean square) term (the denominator in our F ratio, which represents whether or not the treatments are significant or not) the better our chances of statistically detecting differences in treatments.

Completely randomized designs have their advantage in two distinct situations: 1) where there is not likely to be any type of gradient, possibly in growth chamber or greenhouse studies, and 2) where there is unequal replication. Unequal replication occurs when those treatments of interest may be replicated many times and certain treatments, possibly exploratory, only a few times. The completely randomized analysis is desirable if several plots are lost, for one reason or another. The analysis differs, from the completely randomized analysis shown above, only for the calculation of the treatment sums of squares. Total sums of squares is calculated as illustrated previously however, treatment sums of squares is calculated as follows:

$$\text{Treatment SS} = \left[\frac{(\text{treatment A})^2}{\text{reps in A}} + \frac{(\text{treatment B})^2}{\text{reps in B}} + \text{etc.} \right] - \text{c.f.}$$

If there are six treatments, there will be six terms being summed, each treatment sum of squares being divided by the number of times it was replicated in the experiment.

2. Randomized Complete Block Design

An experimental design more appropriate to FSR/E is the randomized complete block design (RCBD). In this design, the plots are "blocked" - assigned to blocks on the basis of similarity in soil type, slope, cropping history, etc. It does not matter if blocks differ from each other in these characteristics, but within blocks, plots must be as uniform as possible. Plots within blocks are often contiguous, but do not need to be. In RCBD each treatment occurs once in each block. In FSR/E, blocks may occur on different farms, so that variation between blocks, as measured by the Block SS (and MS) may be extremely large, and larger than the variation between treatments.

Example 3. Assume that the three treatments (a, b and c) had been applied as an RCBD blocking as shown. The data remain the same.

a	c	b	a
b	a	c	c
a	b		
b	a		

Block	Treatments a	b	c	Totals
1	3	2	5	10
2	5	4	8	17
3	6	5	6	17
4	10	9	17	36
Totals	24	20	36	80
Means ($\bar{X}$)	6	5	9	

Note that block total vary widely - as they might well do in on-farm experiments. The analysis of variance now has another source of variation - blocks. The only new calculation is that of Blocks SS. This is given by:

$$\frac{\Sigma B^2}{b} - \frac{(\Sigma x)^2}{n}$$

Where B is any block total, and b is the number of blocks, or plots per block. This gives:

$$\frac{(10^2 + 17^2 + 17^2 + 36^2)^2}{3} - \frac{80}{12} = 124.7$$

The analysis of variance is now:

Source of variation	SS	df	MS	F
Blocks	124.7	3	41.6	14.85
Treatments	34.7	2	17.4	6.21
Error	17.3	6	2.8	
Total	176.7	11		

The F-ratio (variance ratio) for blocks is significant at a probability (P) of less than 1% while that for treatments is significant at a probability of less than 5%. That is to say, there is a better then 5% or 1 in 20 chance that the differences between treatments are real.

Note that in the RCBD the error component represents the interaction of blocks and treatments. If, for whatever reason, treatment a is the best in blocks 1 and 2 but the poorest in blocks 3 and 4, the error SS will be larger than if a is consistently the best. This may arise in on-farm experimentation, particularly if there is an element of heterogeneity among the farms. With one block per farm; the farm X treatment interaction is confounded with the error (residual component). To measure and quantify this interaction, it is necessary to include more than one complete block per farm.

3. Randomized Complete Block Design: Complete Factorials

So far we have assumed that our treatments (a, b and c in examples 1, 2 and 3) represent different types of treatments, such as three varieties; three kinds of pesticides; or nitrogen, phosphate, and potash. With only two treatments, they may simply represent the presence or absence of a factor. However, when a treatment array compares all combinations of two or more factors each at two or more levels, it is known as a complete factorial array. the term "factorial" refers to the way treatments are designed, and is not a design. A factorial experiment can be put into the field in any of the designs we have discussed, and several others. The great advantage of the factorial experiment is that it can test the contribution of a factor (say nitrogen) at two or more levels of one or more factors (say phosphorus and/or potassium). The factorial allows the researcher to detect and test the significance of interactions between factors. Interactions are quite common. Examples of common interactions include: the amount of nitrogen fertilizer that can be profitably used may depend on the performance of the variety being used, or the variety being planted may depend on the date of planting.

The simplest example is an 2^2 factorial: Two factors each at two levels. If we had two factors each at three levels it would be termed a 3^2 factorial. We could have three factors each at two or three levels; or, respectively, 2^3 and 3^3 factorials.

Example 4. A 2^2 factorial has four treatments. Assume the two factors are herbicide application and insecticide application and the levels are "absence" and "presence". They could equally be two rates of each, or one and two applications. The four treatments are thus four treatment combinations (using conventional notation, a = herbicide, b = insecticide).

(1)	a	b	ab
no herbicide	herbicide	no herbicide	herbicide
no insecticide	no insecticide	insecticide	insecticide

The data set is as follows, with five complete blocks, each with four plots.

	Treatments				
Block	(1)	a	b	ab	Totals
1	7	13	5	18	43
2	9	14	8	17	48
3	8	14	7	15	44
4	10	12	11	16	49
5	8	14	8	17	47
Totals	42	67	39	83	231

We can now develop the ANOVA table: we have three df for treatments, one for the effect of a (denoted by the capital A), one for the b effect (B), and one for the ab interaction effect (AB), so the ANOVA table is:

Source of Variation	SS	df	MS	F
Blocks		4		
A		1		
B		1		
AB		1		
Residual		12		
Total		19		

The following table shows the contribution of each treatment combination to the effects.

	Treatment			
Effect	(1)	a	b	ab
A	−	+	−	+
B	−	−	+	+
AB	+	−	−	+

The A effect is computed from those treatments with a, minus those without. The B effect is computed similarly. The AB effect is derived by "Multiplying" the A and B lines (note, therefore, that minus (−) times minus(−) becomes a plus(+)for the (1) treatment.

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The SS for A is calculated as follows, using treatment totals.

$$\frac{(a + ab - (1) - b)^2}{(2\ r)^2}$$

Similar formulae can be written for SSB and SSAB, based on the above table. For SSA the calculation is:

$$\frac{(67 + 83 - 42 - 39)^2}{20} = \frac{692}{20} = 238.1$$

Note the correction factor is not needed for these SS's, but is needed for Block and Total SS. The SSB is 8.5 and SSAB is 18.1. The ANOVA is now:

Source of Variation	SS	df	MS	F
Blocks	6.7	4	1.68	<1.00
A	238.1	1	238.1	111.78
B	8.5	1	8.5	3.99
AB	18.1	1	18.1	8.50
Residual	25.6	12	2.13	
Total	297.0	19		

The effects of A and AB are significant at, respectively, P = <0.01 and P + < 0.05. There is no need to calculate a LSD since the treatment effects each have only 1 df. The CV is

$$\frac{\sqrt{2.13}}{11.55} \times 100 = 12.6\%$$

More complex factorials may have more than one df for treatment effects, so the analysis is more complex.

b. Combined Analysis Across Farms

On-farm trials are designed to estimate the range of adaptability of new production technologies compared to current farmer practice over a range of on-farm conditions. To do this it is useful to combine the results of a given trial with the same treatments over a number of locations and/or years. If there is only one replication of each trial at each location, it may be advisable to use modified stability analysis to evaluate the performance of each treatment over the range of farms where the trials were conducted. Analysis of variance can be applied to the combined location data but it will not be able to assess treatment by location interaction if there is only one replication at each location.

Where there is more than one replication at each location, or in each year, analysis of variance of the combined results from the locations can be performed which will allow an estimate of treatment-by-location, or treatment-by-year interactions. This will give an indication of the potential stability of the treatments across the environments included for the trial locations and years. The variation can be further partitioned within the analysis of variance to identify the homogeneous sites and assist the research team in further delineation of recommendation domains. The result of the analysis will allow researchers to identify the treatments that perform best and identify their range of adaptability.

Combined analysis is discussed in detail in Hildebrand, P. and F. Poey, On-farm Agronomic Trials in Farming Systems Research and Extension, 1985, pp. 63 - 71.

c. (Optional) Analysis of Variance with Incomplete Block Designs or Incomplete Factorials. ANOVA for incomplete block design (IBD) differs depending on the type of incomplete block design. Let us consider four cases (see II, C, 1, b also):

a. Balanced IBD with equal block sizes, equal replication of treatments, and single replication.
b. Balanced IBD with equal block sizes, equal replication of treatments, and group replication.
c. IBD with equal block sizes and supplemental balance (unequal replication of treatments).
d. IBD with blocks of unequal size.

II, C, 1.b discusses the difference between single replication and group replication, and page 11 of the CARDI manual presents two examples of IBD with group replication. Page 13 of the CARDI manual presents two ANOVA tables balanced IBD: one with simple replication, and the other with group replication.

Pages 334-335 of Shaner et al., also present calculations for an IBD example.

Pages 18 and 32-33 of the CARDI manual discuss ANOVA for IBD with equal block sizes and supplemented balance or unequal replication of treatments. Pages 33-34 discuss ANOVA for IBD with unequal block sizes. These pages also refer to a general method of ANOVA for IBD with unequal block size and unequal replicaiton of treatments, on pages 70-77 in the appendix of the CARDI manual. The data of the example presented in the CARDI appendix has been analyzed using the general linear models (GLM) procedure of the statistical analysis system (SAS) package on a mainframe computer. The SAS analysis gave results essentially identical to those obtained by the general method. This method uses only multiplication, division, addition, and subtraction, and so can be done with simple hand calculators. Although not shown in the CARDI appendix, the method can also be extended for partitioning the treatment sums of squares to obtain single degree-of-freedom contrasts. This method thus gives FSR/E teams the ability to analyze data from IBD without dependence on a mainframe computer with SAS GLM.

Pages 24-26 of the CARDI manual present examples of ANOVA tables for RCBD with confounding, and pages 30-31 present examples of ANOVA tables for designs with fractional replication. The general method of ANOVA presented in the CARDI appendix may not be valid for designs with confounding. Assistance from a biometrician or a biological scientist with experience using such designs should be sought first.

d. Modified Stability Analysis

There has been a need for a method of analysis that fully considers some of the unique characteristics of on-farm research. These characteristics may include (1) trials for which blocks are considered as individual farmer fields, (2) variation in farmer management, (3) variation in soils and climate inherent across trials, and (4) data processing requirements that are capable of being easily handled by institutions in developing countries. The analysis method should help researchers evaluate treatment responses and partition farmers into explicit recommendation domains.

The modified stability analysis possesses most, if not all, of these characteristics. It is based on the technique of regression analysis. Mean treatment yields at each location are used as an environmental index. Individual treatment results are regressed on this environmental index using simple linear regression analysis. By fitting each treatment independently to the simple linear regression equation, and then plotting the yield response to the environmental index on the same graph, it is possible to visually compare the performance of the treatments. A graphic distribution of confidence intervals within partitioned groups helps in selecting superior treatments

in different environments.

Modified stability analysis is discussed in detail in Hildebrand,P. and F. Poey, On-farm Agronomic Trials in Farming Systems Research and Extension, 1985, pp. 126 - 142. The process of calculation is discussed and concrete examples are given.

2. (OPTIONAL) A WAY TO HANDLE "DAMAGED" DATA: ANALYSIS OF COVARIANCE

In many situations the experimental plots comprising an on-farm trial are heterogeneous. This results from inherent differences (in soil fertilty, for example) within the field, because of uneven application of common management practices across the trial (usually through management errors), or because of differences in such factors as weed, insect, disease or accidental animal grazing that are not directly related to treatment effects. Covariance analysis allows the treatment means to be adjusted to correct for these unequal effects on treatments.

To use covariance analysis to adjust treatment means yields to correct for plants damaged in a random fashion by accidental cattle grazing, for example, the researchers would need to measure both yield and plant population for each treatment in each plot. Conducting covariance analysis using yield as the primary data of interest and plant population as the covariate will then allow the researchers to control experimental error and to adjust the treatment mean yields on the basis of a uniform plant population.

Covariance analysis can also be used to estimate missing plots and as an aid in the interpretation of experimental results. Its main role will be in exploratory and refinement trials where more emphasis is given to collecting and analyzing Primary and Secondary Trial Data. The other types of data that are collected besides yield can be used as covariates in covariance analyses, to assist in the understanding of differences in treatment performances in trials.

Analysis of Covariance is discussed in detail in Hildebrand,P. and F. Poey, On-farm Agronomic Trials in Farming Systems Research and Extension, 1985, pp. 22 - 28. A detailed example is used to illustrate its use.

3. WAYS TO EVALUATE TRIAL DATA

a. Levels of Significance

This is an indication of the level of probability that actual differences in fact do exist between treatment means, as indicated by the F-test associated with analysis of variance. Most experiment station biological researchers utilize a

probability level of 1% to indicate that highly significant differences are present between treatment means, and a level of 5% to indicate significant differences. The level of significance does not indicate, however, which means are significantly different.

On-farm trials usually have higher experimental error (residual). Therefore, the probability levels of 1% and 5% commonly used for trials in experiment stations may not detect differences between treatments in some trials on farmers' fields. In this case, it may be useful to test for treatment differences with levels of significance having higher probabilities such as 10-15%. In this way trials can provide useful information to identify potential superior treatments for further study. The research team, however, should try to explain the reasons leading to the high residual term.

Traditionally, researchers reported levels of significance as the 1% and 5% levels of probability. More recently, the trend has been to report the probability of achieving the F-ratios calculated in the experiment by chance. Few researchers question the observation that identical treatments in on-farm trials tend to differ more than would be expected at research stations. There generally is more variability contributing to higher mean squares in analyses of variance which result in higher denominators in the F-ratio equation resulting in lower F-values which are less likely to "be significant" at the 1% or 5% levels. Depending on the cost of the treatment to the farmer the farmer may adopt input treatments found to be significant only at the 50% level. If there is no cost involved and if the farmer can expect to gain one year out of two, he is likely to adopt the practice. On the other hand, if input costs are high, such as certain fertilizer compounds, certain chemicals, or tillage practices, the farmer may not be interested even if the treatment is significant at the 1% level, or even the 0.1% level.

The on-farm trial researcher should do everything possible (blocking, plot, border rows, experiment border rows, good records, timely and careful cultural practices, etc.) to keep the residual mean square (the unexplained variation, or "experimental error") to a minimum.

b. Coefficient of Variation

This statistic indicates the degree of precision by which the treatments can be compared and is a good index of the reliability of the experiment. It expresses the square root of residual as a percentage of the overall mean for the experiment. The higher the value of the coefficient of variation, the lower is the reliability of the experiment. The acceptable level for the coefficient of variation encountered for on-farm trials is usually higher than for experiment station trials because of more diverse sources of uncontrollable variation on farmers' fields. Experiment station researchers may accept an upper limit of 10%

for the coefficient of variation. Levels of 20-25% or more can be acceptable for on-farm trials. Research team should however, seek to identify the courses sources of variation leading to the high coefficient of variation in on-farm trials.

It may be helpful to see the formula for the coefficient of variation to understand how it varies for the trait (characteristic) being evaluated (for example, yield, number of fruit per plant, etc.).

$$CV = \frac{\sqrt{\text{residual mean square}}}{\text{mean of all plots}} \times 100$$

The residual mean square is influenced by the variability due to chance, to poor record taking, to improperly applied treatments, etc. The mean plot yield is influenced by the environment and the treatments selected. By taking very good care of the plots one can elevate the mean yield and decrease the CV. Researchers like to see low CV's, meaning that there was good within-experiment repeatability. However, the nonuniformity of small farmers fields is inescapable. Use of the CV's in judgment decisions must be accompanied by a good understanding of what CV's mean.

ACTIVITIES

1. For examples and material for exercises in working with the modified stability analysis please refer to the Hildebrand/Poey publication included in the FSSP training materials package, pp. 126-142.

2. Material for working examples are available in the Paraguay Case Study included in the FSSP training materials package, pp. 126-142.

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(V,B,1)

WAYS TO INTERPRET TREATMENT DIFFERENCES: BIOLOGICAL

OUTLINE:

1. Linear Regression and the Correlation Coefficient
2. Least Significant Difference (LSD)
3. (Optional) Multiple Range Tests
4. (Optional) Single Degree of Freedom Contrasts

PREREQUISITES:

PARTICIPANT LEVEL:

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES:

After completing this section participants will be able to:

1. Explain the principles of statistical methods, blocking, and replication.

2. Perform simple statistical analyses of experimental data from on-farm trials.

3. Identify modifications and differences in the statistical methods necessitated by the nature of on-farm research.

4. Select the proper techniques to analyze and interpret the data and information collected from the trials to allow next-step decision making.

KEY POINTS:

1. Many researchers have an "inherent" fear of mathematics and statistics, and either avoid using some of the analyses available and appropriate (other than the analysis of variance) or rely on someone else to analyze the data. This other person is often not given any guidance as to what analyses are required, nor what contrasts are of major interest.

2. In farming systems research/extension, field teams may lack statistical support services, or there may be a considerable delay in receiving completed analyses from such a service. Yet it may be necessary to take planning and design decisions at an early date following completion of an experiment. This urgency may be greater in exploratory and refinement testing than in validation testing later in the sequence, where statistical analyses are of lesser importance compared to

other forms of analysis.

3. Farming Systems Research/Extension is characterized by variability between farms, which must be addressed by appropriate designs and analyses.

4. Farming Systems Research/Extension involves a sequence of on-farm trials. As the trial sequence progresses, the importance of social and economic information to analyze farmer reaction and to draw comparisons with the local farming practices dramatically increases.

DEFINITIONS:

ANOVA
block
correlation
correlation coefficient
RCBD
CRD
Coefficient of variation (CV)
degrees of freedom (df)
effect
error (residual sum of squares (SSr)
exploratory trials (testing)
factorial array
factorial experiment
F-test
interaction
least significant difference (LSD)
level
linear regression
Mean Square (MS)
Multiple range test (MRT)
plot
randomization
replication
refinement trials (testing)
residual
single degree-of-freedom comparisons
sums of squares (SS)
t-test
treatment array
treatment combination
treatment
validation trials (testing)
variance ratio
variance

DISCUSSION:

Results of the analysis of on-farm trials must be interpreted before the research team can determine which tested technologies

can pass from one trial function to the next and finally to the extension stage for wider dissemination to farmers. (V,B,1 through 3) consider the biological, economic and social aspects of on-farm trial interpretation. The interpretation will be directly related to the methods that have been used to analyze the data. The interpretation will range from objective identification of superior treatment means based on statistical analyses to more subjective identification of superior technologies based on assessment of farmer and farm household members' perceptions of the value of the new technologies compared to current practices between the means at defined probability levels are encountered. Several methods for comparison of means are available.

a. Linear Regression and the Correlation Coefficient

Several categories of data are normally recorded for on-farm trials. Many times it is useful to determine the association between a selected data type (yield, for example) with one or more of the other types of data that have also been recorded from the trial plots (level of nitrogen, for example). Regression analysis describes the effect of one or more variables (designated as the independent variable) on a single variable (designated as the dependent variable). For example, in trials on yield response to nitrogen levels, yield is obviously the dependent variable and nitrogen level is the independent variable. When levels of input are being evaluated, regression analysis, rather than analysis of variance, should be used. Regression analysis in this example would attempt to indicate how yield is associated to the level of nitrogen applied. It would answer the questions "Does yield increase as nitrogen level increases and how are they related?"

Correlation analysis, on the other hand, provides a measure of the degree of association between the variables. Regression and correlation analyses can be classified according to the number of variables involved and the form of the curve obtained by plotting the relationship between the dependent variable and the independent variables. It is simple regression and correlation if only one dependent and one independent variable are involved and multiple if more than one independent variable is involved. It is termed linear if the curve indicating the relationship is a straight line and non-linear if otherwise.

Simple linear or non-linear regression and correlation analyses will likely be more commonly used for on-farm trials although certain conditions may call for multiple regression and correlation analyses.

Two measurements may be related, either coincidentally or causally. Crop yield, for instance, may be related to plant height, to soil nitrogen levels, and to total rainfall during a critical period of the growth cycle. Such a relationship may not be simple: yield may increase with increases in rainfall to a

certain quantity of rainfall, but rainfall over and above this may decrease yields. Yield of course is dependent on many variables, and not on a single one. The simplest relationship is linear with one dependent variable (usually labeled y) increasing or decreasing linearly with increase in one independent variable (usually labeled x). This relationship can be expressed as, $Y = a + bx$ (or $Y = a - bx$), where a is a constant, and b is the regression coefficient, the rate of change of y with unit change in x. On a graph of y against x, a is the intersect on the y-axis and b the slope of the regression line. The calculations are shown below.

The two sums of squares and the sum of products (SPxy) must be calculated. This latter is defined as

$$SPxy = \Sigma xy - \frac{\Sigma x \Sigma y}{n}$$

The sums of squares are:

$$SSx = \Sigma x^2 - \frac{(\Sigma x)^2}{n}$$

and

$$SSy = \Sigma y^2 - \frac{(\Sigma y)^2}{n}$$

example 1.

	y	x
	21	1.5
	26	1.7
	27	1.9
	25	2.0
	32	2.1
	37	2.4
	39	3.0
	45	3.4
	47	3.9
	51	4.5
Total	350	26.4
Mean	35.0	2.64

$$SSy = 13220 - 12250 = 970$$
$$SSx = 78.94 - 69.70 = 9.24$$
$$SPxy = [(21 \times 1.5) + (26 \times 1.7) + \ldots + (51 \times 45)] - \frac{(350 \times 26.4)}{10}]$$
$$= 91.8$$

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The regression coefficient (b of y on x is calculated thus

$$b = \frac{SPxy}{SSx} = \frac{91.8}{9.24} = 9.94$$

This indicates that y changes by 9.94 units for every unit change in x. Note that SPxy can be negative. The statistical significance of b can be tested by an analysis of variance, separating the SSy into a regression component and an errror component. The regression SS is given by:

$$\frac{(SPxy)^2}{SSx}$$

The analysis is as follows:

Source of variation	SS	df	MS	F
Regression	912	1	912	125.8 (significant P = < 0.01)
Error	58	8	7.25	
Total	970	9		

Given b, the value of a can also can be calculated, where $\bar{Y}$ is the mean of the Y values and $\bar{x}$ is the mean of the x values:

$$a = \bar{y} - b\bar{x}$$

In this example,

$$a = 35.0 - 9.94\ (2.64)$$
$$= 8.8$$

Having calculated a and b, it is now possible to estimate the value of y for any value of x lying between the minimum and maximum observed values of x. The estimated value of y, denoted by $\hat{Y}$, is:

$$\hat{Y} = a + bx$$

In the above data, therefore:

$$\hat{Y} = 8.8 + 9.94x$$

The data in example 1 above do not include an observation at x = 2.7, but we can estimate the value of y at x = 2.7 with the

above equation and the values for a and b calculated above:

$$\hat{Y} = 8.8 + 9.94\ (2.7)$$
$$= 35.6$$

Given b, $\hat{Y}$ can also be calculated directly from the data table of example 1, since,

$$\hat{Y} = a + bx$$

$$a = \bar{y} - b\bar{x}$$

then,

$$\hat{Y} = \bar{y} - b\bar{x} + bx$$

In the above example,

$$\hat{Y} = 35.0 - 9.94\ (2.64) + 9.94\ x$$
$$= 35.0 - 26.2 + 9.94\ x$$
$$= 8.8 + 9.94\ x$$

which is identical to the answer obtained by using the previously calculated value of a.

Finally, it is important to note that $\hat{Y}$ will not necessarily equal the observed value of $\hat{Y}$ for a given observed value of x. This is because the equation for Y is an estimated relationship between y and x. For example, for x = 2.4,

$$Y = 8.8 = 9.94\ (2.4)$$
$$= 32.7$$

whereas the observed value of y at x = 2.4 was actually 37. The difference between Y and observed y is called the deviation from regression. The closeness of the relationship can also be expressed by the correlation coefficient, denoted by r. This has values from -1 to +1, with 0 (nil) indicating no relationship at all. The correlation coefficient is calculated as:

$$r = \frac{SPxy}{\sqrt{(SSy)(SSx)}} = \frac{91.8}{\sqrt{(9.24)(970)}} = 0.97$$

The statistical significance of this is tested by reference

to tables of r, for different probability levels with (n-2) df. The greater the deviations from regression, the closer to zero will be the value of r, and the more likely r will be non-significant.

In this case, with 10 observations,

$n = 10$
$n - 2 = 8$

At n-2 = 8, tabulated r = 0.632 at the 5% probability level. Since the calculated r of 0.970 is greater, we would declare r to be significant: the probability that x and y are not associated according to the formula Y = 8.8 + 9.94 x is less than 5 %.

If r is significant, then the square of its value provides an estimate of the percentage of variation in y that is due to variation in x.

In this case,

$r = (0.970) \times 100\%$
$= 94\%$

This means that 94% of the variation in y is accounted for by the variation in x, over the range of 1.5 - 4.5. Note that this relationship is only valid within this range of observed values of x; Y cannot be estimated for values of x less than 1.5 or greater than 4.5

b. LEAST SIGNIFICANT DIFFERENCE (LSD)

The least significant difference (LSD) test is the simplest and most commonly used procedure for comparing pairs of means. It is most appropriate for making planned comparisons and, strickly speaking it is not valid for comparing all possible pairs of means, especially when the number of treatment means is large. If it is used to compare all possible pairs of means, apply it only when the treatment F-test is significant and the number of treatments is less then six.

The LSD from treatments is calculated from the formula:

$$t \times \sqrt{\frac{2\,(\text{MS for residual})}{r}}$$

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where r is the number of replicates, or blocks. 't' is taken from tables for a given probability, usually P = 0.05 or P = 0.01 (5% or 1%), and for the degrees of freedom for residual. The value of t for given probability decreases as the residual df increase.

As an example take the date of Example 3. For P = 0.05, and with six df for residual, "t" (from tables) is 2.447. The LSD is therefore:

$$t \times \sqrt{\frac{2 \times 2.8}{4}} = 1.18 \times 2.447 = 2.89$$

We can now rank and compare our means against our LSD of 2.89:

b = 5.0
a = 6.0
c = 9.0

The difference a-b is less than the LSD and so is not significant, but b-c is larger than 2.89, so this difference is significant. The comparison a-c cannot be made unless it was a pre-planned comparison. Only pairs ranked next to one another can legitimately be tested with LSD.

C.(OPTIONAL) MULTIPLE RANGE TESTS (MRT)

Because the LSD should not be used indiscriminately among large numbers of treatment means, a multiple range test (MRT) should be used. For trials that require the evaluation of all possible pairs of treatments and where the number of treatments is larger (greater than five), there are several multiple range tests that can be used. Duncans' multiple range test is the most widely used. It is similar in computation to the least significant difference. However, unlike the least significant difference in which only a single value is required for any possible comparison of a pair of means, Duncan's multiple range test requires computation of a series of values, each corresponding to a specific pair of treatment mean comparisons.

Duncan's MRT involves the calculation of a series of "shortest significant differences" (SSD's) which increase the wider apart is the ordered (ranked) array of treatment means. The SSD is given by R x LSD for each relative position in the array. Adjacent means are considered to have a relative position of 2. R is the significant studentised factor, taken from tables.

The values of R for relative positions of 2 to 7 are shown

below, along with the SSD's.

	Relative Position					
	2	3	4	5	6	7
R	1.00	1.05	1.08	1.10	1.12	1.13
SSD	1.74	1.83	1.88	1.91	1.95	1.97

This example shows a set of data (for example, yields of seven varieties) for which Duncan's MRT is appropriate.

Variety	Mean yield
1	15.3
2	14.2
3	11.4
4	9.7
5	9.1
6	8.7
7	8.1

The steps for determining which means are significantly different from each other using Duncan's Multiple Range Test are as follows:

Step 1a.

Starting with the most extreme comparison, we can now compare the difference between each pair of treatments with the SSD: 15.3 - 8.1 = 7.2 which is greater than 1.97 (and significant therefore); 15.3 - 8.7 =6.6 which is larger then 1.95;.....;15.3 - 14.2 = 1.1 which is not larger than 1.74 and therefore not significant.

In this example, the mean yields of 15.3 and 14.2 are not significantly different from each other. However, had that group, which now includes two treatment means (15.3 and 14.2), included three or more means, step 1b would be needed. However, the fact that in this example there are only two means, step 1b is unnecessary and in this example you would go directly to step 2a. Step 1b is presented here for completeness.

Step 1b.

Calculate the range between the remaining treatment means (those whose values are larger than or equal to the difference between the largest mean and the largest SSD value). Compare this range with the SSD value for the number of treatment means in the group. If the calculated range is smaller than the corresponding SSD value the treatment means in the group are declared not significantly different from each other.

Step 2a.

We now compare the next largest mean as follows: 14.2 - 8.1

= 6.1, which is larger than 1.95 and significant;.....;
14.2 - 11.4 = 2.8 which is larger than 1.74 and so is significant.

Step 2b.

Should there be more than two treatment means in the group resulting from step 2a, you would need to proceed as in Step 1b. Again, in this example this step is unneccesary because there are only two means (11.4 and 9.7).

Step 3.

Repeat Step 2 until no means are found to be significantly different from each other.

These significant differences are either indicated by a line joining means that are not significant, or by letters, such that means with a letter in common are not significantly different.

Variety	Mean yield
1	15.3a
2	14.2a
3	11.4 b
4	9.7 bc
5	9.1 c
6	8.7 c
7	8.1 c

d. (OPTIONAL) SINGLE DEGREE OF FREEDOM CONTRASTS

This technique for making comparisons between treatment means involves partitioning the treatment sum of squares into meaningful components. The procedure is similar to that of the analysis of variance where the total sum of squares is partitioned into a fixed set of components directed by the experimental design used. For example, the total sum of squares in the randomized complete block design has three sources of variation; blocks, treatment and residual. With further partitioning of the treatment sum of squares into one or more sources of variation associated ith individual treatments specific causes of the differences between treatments can be determined and the most important ones identified.

Even with the non-factorial array, it is sometimes useful to break down the df for treatment into single df, or df for at least a partial breakdown. Suppose we have four treatments, a nil (check) treatment (0) and three nematicide treatments (p, q and s), so that there are 3 degrees of freedom for treatments. The 3 degrees of freedom can be divided into three single-degree-of freedom contrasts, as follows:

1. O vs the rest (p, q, and s)
2. P vs q and s
3. q vs s

To estimate the SS the following procedures and formulae are needed:

Contrasts	Treatments					
	0	p	q	s	m^2	m
0 vs rest	+3	-1	-1	-1	12	0
p vs qs	0	+2	-1	-1	6	0
q vs s	0	0	+1	-1	2	0

The column m^2 is the sum of the squares of the coefficients in the table

(e.g., [$(-3)^2 + (-1)^2 + (-1)^2 + (-1)^2 = 12$ and so on). The SS (o vs rest) is calculated from the treatment totals (To, Tp, etc.)as follows:

$$SS = \frac{(+3\ To - 1\ Tp - 1\ Tq - 1\ Ts)}{\Sigma m^2 r}$$

where r is the number of observations in each treatment (equal to the number of replications).

For SS (p vs q + s),

$$SS = \frac{(+2Tp - 1\ Tq - 1Ts)^2}{\Sigma m^2 r}$$

One point is necessary concerning derivation of the coefficients: coefficients must sum to zero within lines, and the products of any two lines must sum to zero (e.g., lines 1 and 2 give $(0 - 2 + 1 + 10 = 0)$.

Example. The product of the coefficients of the following data set refer to the case above.

Blocks	o	q	r	s	Totals
1	5	9	7	11	32
2	4	8	10	12	34
3	6	11	10	14	41
Totals	15	28	27	37	107

SS (o vs rest) then becomes:

$$SS = \frac{[3\ (15)-1(28)-1(37)]^2}{[3^2 + (-1)^2 + (-1)^2 + (-1)^2] \times 3}$$

$$= \frac{(-47)^2}{(12) \times 3}$$

$$= \frac{2209}{36}$$

$$= 61.4$$

SS (p vs q + s) and SS (q vs s) can be similarly calculated from their formulae. SS for blocks and total can be calculated as shown earlier, and SS for residual divided by subtraction of the SSs for blocks and the 3 contrasts from SS for total.

The ANOVA then becomes:

Source of variation	SS	df	MS	F
Blocks	11.2	2	5.6	5.49
o vs rest	61.4	1	61.4	60.20
p vs q + s	3.6	1	3.6	3.53
q vs s	16.6	1	16.6	16.27
Residual	6.1	6	1.02	
Total	98.9	11		

The effects of blocks, o vs rest, and q vs s are all significant at $P < 0.05$. These results indicate, without further tests, that nematicides increase yields, and that s is better than r. One could derive other contrasts if necessary.

ACTIVITIES

ACTIVITY ONE:	STATISTICAL ANALYSIS USING ANOVA
ACTIVITY TWO:	STATISTICAL ANALYSIS USING REGRESSION AND THE CORRELATION COEFFICIENT
ACTIVITY THREE:	STATISTICAL ANALYSIS IN MEANS COMPARISONS USING LSD, MRT, AND SINGLE DF CONTRASTS

ACTIVITY ONE TRAINERS' NOTES
STATISTICAL ANALYSIS USING ANOVA

OBJECTIVE:

After completing this activity you will be better able to:

1. Develop skills in statistical analysis using ANOVA.

TIME: 20 minutes

MATERIALS: Statistical tables, Graph paper.

INSTRUCTIONS:

Trainees should work individually, or in groups, if they have problems.

1. Ask the participants to carry out an analysis of variance on the data set given in their instruction sheet. This is repeated below for your convenience. The four treatments were laid out in a randomized complete block design, with one block per farm.

2. Have the participant plot the data on a graph (with treatments a, b, c and d on the horizontal axis, and the data on the vertical axis. Plot the data for each block with a separate line and symbol).

Data set:

	Treatments			
Farm	a	b	c	d
1	14	15	21	23
2	11	10	15	18
3	15	13	18	24
4	17	15	18	12
5	13	15	14	17
6	19	21	16	11
7	16	18	25	22

PROCESSING:

1. The answers are provided below:

Source of variation	SS	df	MS	F.
Farms	124.4	6	20.73	1.55
Treatments	63.3	3	21.10	1.58
Error	240.7	18	13.37	
Total	428.4	27		

There are no significant differences; it is unncessary therefore to calculate LSD's. The graph will show that treatments a and b give lower values than c and d on farms 1, 2, 3 and 7, (solid lines), but not on farms 4, 5, and 6.(broken lines with symbols in breaks).

ACTIVITY ONE TRAINEE INSTRUCTIONS
STATISTICAL ANALYSIS USING ANOVA

OBJECTIVE:

After completing this activity you will be better able to:

1. Develop skills in statistical analysis using ANOVA.

INSTRUCTIONS:

1. Carry out an analysis of variance on the data set given below. The four treatments were laid out in a randomized complete block design, with one block per farm.

2. Plot the data on a graph (with treatments a, b, c and d on the horizontal axis, and the data on the vertical axis. Plot the data for each block with a separate line and symbol).

Data set:

	Treatments			
Farm	a	b	c	d
1	14	15	21	23
2	11	10	15	18
3	15	13	18	24
4	17	15	18	12
5	13	15	14	17
6	19	21	16	11
7	16	18	25	22

ACTIVITY TWO TRAINERS' NOTES

STATISTICAL ANALYSIS USING REGRESSION AND THE CORRELATION COEFFICIENT

OBJECTIVE:

After completing this exercise the participant will be able to:

1. Complete statistical analysis using regression and the correlation coefficient.

TIME: 30 minutes

MATERIAL: Statistical Tables, Graph paper, Pocket/hand held or desk top calculators.

INSTRUCTIONS:

1. Ask the participants to calculate the linear regression equation and the correlation coefficient from the following data set.

2. Have the participants do an analysis of variance on the regression coefficient.

3. Finally, ask them to plot the data on graph paper and draw in the regression line (Hint: calculate, from the equation, the values of y for x = 1 and x = 4).

Data set:

y	x
8.5	1.6
9.3	2.3
8.4	1.6
10.7	3.2
10.5	3.3
4.3	0.5
11.7	4.2
8.9	2.7
14.2	6.1

PROCESSING:

1. The answers are provided below:

Ey	=	86.5	Y	= 9.61
Ex	=	25.5	x	= 2.83
SSy	=	58.91	a	= 5.14
SSx	=	21.68	b	= +1.577
SPxy	=	34.18	r	= +0.956

Source of variation	SS	df	MS	F.
Regression	53.89	1	53.89	74.85
Error	5.02	7	0.72	
Total	58.91	8		

$$y = 5.14 + 1.577\ x$$

The regression coefficient is significant at $P<0.01$ (1%)
The regression equation accounts for 91% of the variation in y.

ACTIVITY TWO TRAINEE INSTRUCTIONS

STATISTICAL ANALYSIS USING REGRESSION AND THE CORRELATION COEFFICIENT

OBJECTIVE:

After completing this exercise you will be better able to:

1. Complete statistical analysis using regression and the correlation coefficient.

INSTRUCTIONS:

1. Calculate the linear regression equation and the correlation coefficient from the following data set.

2. Do an analysis of variance on the regression coefficient.
3. Plot the data on graph paper and draw in the regression line (Hint: calculate, from the equation, the values of y for x = 1 and x = 4).

Data set:

y	x
8.5	1.6
9.3	2.3
8.4	1.6
10.7	3.2
10.5	3.3
4.3	0.5
11.7	4.2
8.9	2.7
14.2	6.1

ACTIVITY THREE TRAINERS' NOTES

STATISTICAL ANALYSIS IN MEANS COMPARISONS USING LSD, MRT, AND SINGLE DF CONTRASTS

OBJECTIVE:

After completing this activity the participants will be able to:

1. Complete statistical analyses in means comparisons using LSD, MRT, and single df contrasts.

MATERIALS: Tables of F, t and significant studentised factors.

INSTRUCTIONS:

1. Ask the participants to analyze the following data set (appears on their instruction sheet) which are weed weights per unit area from an experiment with 4 residual herbicides (a-d) and an untreated control (e). Two analyses are required :

   (i) an ANOVA with (t-1) df for treatments: calculate a LSD and do a MRT.

   (ii) an ANOVA with single df treatment contrasts.

Data set:

	Treatments					
Block	a	b	c	d	e	Total
1	5	8	17	3	25	
2	4	8	16	3	21	
3	8	9	15	4	19	
4	7	6	22	5	27	
Total						

PROCESSING:

1. The answers are provided below:
   Answers for part (i):

Source of variation	SS	df	MS	F
Blocks	25.2	3	8.4	1.65
Treatments	1090.3	4	272.6	53.45
Error	61.3	12	5.1	
Total	1176.8	19		

$$LSD = 2.1790 \times \sqrt{\frac{2\,(5.1)}{4}} = 3.48$$

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MRT:

Treatment	Value
e	23.0 a
c	17.5 b
b	7.8 c
a	6.0 cd
d	3.8 d

Note that in this example, the LSD and the MRT would declare the same pairs of means (a and b, and a and d) non-significant.

Answer for part (ii):

Source of variation	SS	df	MS	F
Blocks	25.2	3	8.4	1.64
e vs rest	649.8	1	649.8	127.41
c vs abd	408.3	1	408.3	80.06
a vs b,d	0.2	1	0.2	<1.00
b vs d	32.0	1	32.0	6.27
Error	61.3	12	5.1	
Total	1176.8	19		

ACTIVITY THREE TRAINEE INSTRUCTIONS

STATISTICAL ANALYSIS IN MEANS COMPARISONS USING LSD, MRT, AND SINGLE DF CONTRASTS

OBJECTIVE:

After completing this activity you will be better able to:

1. Complete statistical analyses in means comparisons using LSD, MRT, and single df contrasts.

INSTRUCTIONS:

1. Analyze the following data set which are weed weights per unit area from an experiment with 4 residual herbicides (a-d) and an untreated control (e). Two analyses are required :

   (i) an ANOVA with (t-1) df for treatments: calculate a LSD and do a MRT.

   (ii) an ANOVA with single df treatment contrasts.

Materials: Tables of F, t and significant studentised factors.

Data set:

	Treatments					
Block	a	b	c	d	e	Total
	1	5	8	17	3	25
	2	4	8	16	3	21
	3	8	9	15	4	19
	4	7	6	22	5	27
Total						

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WAYS TO INTERPRET TREATMENT DIFFERENCES: ECONOMIC

OUTLINE

1. What is Economic Analysis of Technological Alternatives?
2. What is Partial Budgeting?
3. Uses of Partial Budgeting in FSR/E
4. Application of Partial Budgeting Analysis in FSR/E

PREREQUISITES

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

LEARNING OBJECTIVES

After completing this section participants will be able to:

1. Define economic analysis of technological alternatives.

2. Identify components of a partial budget and understand data requirements for partial budget analysis.

KEY POINTS

1. The major critical issues associated with the use of the partial budget analysis of adopted technology are:

    a. Valuation of farmer owned resources used in a farm household.
    b. Stability of input and output prices.
    c. Ready availability of inputs which are critical to the alternative technology.

DEFINITIONS

explicit cost
implicit cost
negative impact
opportunity cost
partial budget
positive impact

DISCUSSION

1. WHAT IS ECONOMIC ANALYSIS OF TECHNOLOGICAL ALTERNATIVES?

Economic analysis is an evaluation of the "productivity" of the technological alternative from the point of view of the resources most scarce to the farmers involved. Critical

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considerations include biological or economic returns to cash, labor at critical times, seed, land, irrigation water or any of the otherr resources needed to produce the technology. One of the most common measures used in economic analysis is profit.

Many examples of the economic analysis tools currently used by FSR/E practitioners are presented in "Introduction to Economic Analysis of On-Farm Experiments: Draft Workbook", CIMMYT, 1985, and practical exercises are provided. This document has been included in the FSSP Collection of Training Materials for your convenience. Partial budgeting is only one of the many economic analysis tools used. What it is and how it is used is discussed below.

2. WHAT IS PARTIAL BUDGETING?

In many developing economies, the allocation of the resource endowments are influenced by political, social, and economic factors. At the farm level, many resource allocations decisions are imbedded in the goals, objectives and aspirations of the farm families.

The end product of FSR/E is a recommendation of an alternative technology or intervention which is expected to relax resource constraints in a given system or increase opportunities in the system to improve productivity as viewed by the farmer. Given the assumption that small scale, limited resource farmers in developing economies are rational decision-makers and that their practices reflect an appropriate management adjustment to their natural resource endownment, the state of technology and the risks and uncertainties associated with their natural environment, it is imperative that potential interventions are evaluated using farm household criteria. One of these criteria is always an economic evaluation.

A partial budget is a techinque used to assess the economic impact of an incremental change in a business or farm enterprise. The technique is useful in analyzing potential alternative technology or interventions for a given recommendation domain or assessing the impact of an adopted technology.

Fundamental to a partial budget analysis is determining aggregate values of the positive and negative impacts associated with the alternative technology or intervention. If the aggregate value of the positive impacts is greater than the aggregate value of the negative impacts, then the technique suggests that the alternative technology is likely to be economically acceptable. The merit of the economic soundness of the alternative technology depends heavily on the reliability/stability of price and yield values and real cost of capital utilized in the analysis.

A partial budget format consists of positive and negative impacts components. The positive impacts include all the

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increased returns and reduced costs that are associated with the alternative technology. The negative impacts consist of the increased costs and reduced returns resulting from the alternative technology. The difference between total positive impacts and total negative impact is the expected change in net return.

3. USES OF PARTIAL BUDGETING IN FSR/E

The usefulness of partial budgeting is limited to instances where the change anticipated in the farm enterprise is incremental in nature, and that the given change does not affect other parts of the farm system. Strictly speaking, this condition can never be fully met. However, the incremental changes expected from FSR/E vary depending on the constraints and/or opportunties in a given system. Shaner, et al. (1982), list the following situations:

1. Increasing output from a given level of resources.
2. Increase the level of resource utilization with subsequent output increases enough to justify increased input.
3. Reduction of farmers' risk through a more reliable inputs or stable prices.
4. Reduction of inputs to produce a given output level.
5. Increasing farmers' satisfaction in other ways, i.e. food security, health improvement, etc.

A partial budget analysis could be applied to any of the above situations. If properly applied, partial budgeting can provide a useful guide in designing and prioritizing alternative technologies. Its usefulness becomes more important as one gets involved in reseacher managed on-farm research. The use of the technique becomes imperative as one moves to farmer managed on-farm research and subsequently as alternative technology is recommended for farmer adoption. The results and interpretation of a partial budget analysis also serve as an important tool in the farmers' management decision making process.

a. Data Requirements

The components of a partial budget format are simple. However adapting its components to a complex farming environment complicates the generation of the appropriate data for the analysis. Some of the critical areas essential to a proper application of the technique are: (1) How to treat fixed cost, (2) Imputation of value of resources owned and used by the farm family, and (3) Determination of output prices at different spacial and time context.

b. Example of Positive and Negative Impacts of New Technology

Maize is intercropped with beans. The current arrangement is maize with single row of beans intercropped. An alternative practice under consideration is maize with double rows of beans.

The increased returns will be the greater value of beans produced; the reduced costs will be variable cost associated with decrease in maize cultivated; the increased costs will be value of variable cost associated with beans and maize cultivated; and the reduced returns is the decreased value of maize output.

c. Fixed Cost

Fixed costs are usually valued at zero in partial budget analysis. However, such valuation is correct only when the usage of a durable input is not affected by the alternative technology. If the alternative technology affects the use of a durable input then the appropriate value of the fixed cost should be incorporated in the analysis. In the example given above, there is no fixed cost associated with the alternative technology.

d. Value Of Farmer Owned Resources

The cost of farm business could be classified into explicit and implicit costs. Explicit cost may be defined as the direct out of pocket expenses that one pays for the use of a resource. Implicit cost is the value of a resource owned by a family and used in the farm business. Its value is usually inputed as the opportunity cost of that resource. Such inputed values may vary by production season (peak or slack period), traditional nonworking days in a specific culture, competing need from nonfarm enterprises, etc.

The value that farmers put on their resources vary and if the magnitude of implicit cost in the total cost is high, it will make a lot of difference in his or her acceptance or rejection of an alternative technology. In many developing economies, especially the Sub Saharan Africa region, where limited "modern" inputs are utilized in small farm activities, a large proportion of the production cost is implicit in nature. An imaginative profile analysis of critical inputs (labor, management and cash) is needed to better approximate the value of farmers' resources. The closer one approximates to a farmer's value of his or her critical resource(s), the better a partial budget analysis will predict farmer acceptance of alternative technology.

The explicit cost determination for some inputs, especially labor, at times have additions which are often overlooked. Such additions may include meals, transportation to and from the field, etc. It is important to ascertain of such extra costs are part of the normal system during the initial survey of the study area.

e. Output (Yield) Prices

The value of output (product) varies with time, place, and use. The prices of most agricultural products tend to be relatively low at times of harvest and very high just before harvests. Farm gate prices are usually less than market prices.

Perrin et al (1976:6-12) provides an excellent section on handling such issues.

f. Input Levels

The value of inputs should reflect their flow values but not their stock values. Normally, the actual level of input that went into a production activity is used in the budget. For example, the time it takes one to move from house to farm should not be considered as a production cost, likewise time spent on breaks and meals. An exception in the situation facing independent small farmers and indivisible, perishable inputs. Pure urea fertilizer may be avaialbel to farmers only in 100-pound sacks. If a plot requires 2-1/2 sacks, the farmer is still forced to purchase three sacks. If he or she cannot re-sell the excess, and if the research team estimates that the urea will be worthless next year (because of moisture absorption and product deterioration), the full cost of the three sacks should be used as the relevant variable input cost.

g. Stability of Critical Factors

The risks that usually characterize agriculture in many developing countries make it impossible to have stabilized values for prices of inputs and output as well as yields from trials. Horton (1982:5) emphasizes this as follows:

> "It may be risky to recommend a new technology to farmers only on the basis of one or a few successful on-farm trials. Where ever possible, on-farm trials should be continued over several years. Even when a technology looks promising on a wider practical scale, it may still not be adopted by farmers, for example, because there is no reliable supply of a recommended input, or because credit is not available when needed. An obvious, but often forgotten, rule is that only readily available inputs should be recommended to farmers."

The problem of price and yield variability is usually handled through a sensitivity analysis. However, trials should be conducted under all possible conditions prevalent in a given recommendation domain.

4. APPLICATION OF PARTIAL BUDGET ANALYSIS IN FSR/E

The degree of rigor in the application of the technique depends on the phase of FSR/E:

a. Design of Alternate Technology

At this stage in the FSR/E process the purpose of partial budget analysis is to gain a very general appreciation of how

consistent the solution may be with regard to internal and external resources available, rough estimates of yields and prices. The application is less rigorous at this stage.

b. Researcher/Farmer Managed On-Farm Trials

At this point a systematic data collection mechanism should be in place to monitor the input, output, and prices associatied with the trial. A methodology needs to be developed to assess farmers' perception and attitude towards risk, and values of resources at critical stages of the production cycle. This enables one to formulate a mechanism for determining implicit costs of family owned resources and implicit prices of that proportion off output that does not enter the market system. An assessment of the conduct of financial institutions (formal and informal) would assist in determining the real cost of investment capital associated with the research. The determination of quantity of output in the format should be based on average values of all treatment with necessary adjustments. The use of marginal analysis on the returns to the trial is essential in the refinement of recommendations. Applications of partial budgeting to on-farm research are found in Harringtion, Horton, Perrin et al and CIMMYT.

The primary use of partial budget analysis in on-farm research is to help refined an alternative technology to be recommended for adoption to a target group of farmers. The recommendation package for the alternative technology should include a budget which estimates the net return of the alternative technology. After the alternative technology has been adopted by some farmers, it is still necessary to monitor the adoption practices of selected farmers who have accepted the technology.

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ACTIVITIES

Practical exercises are found in the CIMMYT publication:

"Introduction to Economic Analysis of On-Farm Experiments: Draft Workbook," CIMMYT, El Batan Mexico.

(V,B,3)

WAYS TO INTERPRET TREATMENT DIFFERENCES: SOCIAL SCIENCE PERSPECTIVES AND FARMER PARTICIPATION

OUTLINE

1. The Role of Social Science
2. Planning Ahead for Social Evaluation and Farmer Participation
3. Collecting Relevant Social Data
4. Communication with Farmers During On-Farm Experimentation
5. Seven Key Questions in the Farmers Evaluation
6. Farmers' Index of Acceptability
7. Intra-household and Gender Sensitive Evaluation of On-Farm Experiments

PREREQUISITES

Basic understanding of FSR/E process is necessary as section begins with on-farm experimentation. Social science background is not necessary.

PARTICIPANT LEVEL

FSR/E practitioners (researchers, research assistants or extension agents).

LEARNING OBJECTIVES

After completing this section the participants should be able to:

1. Understand how and why social science perspectives are necessary in the interpretation and evaluation of the results of on-farm trials.

2. Determine what data should be collected in order to understand farmer evaluations of on-farm trials.

3. Understand the conceptual framework and methodological tools for social science analysis and interpretation of on-farm trials.

DEFINITIONS

farmer environment
farmer feedback
household
intra-household
inter-household

KEY POINTS

1. Farmers adapt and adopt new technologies, not plants or animals. FSR/E practitioners must learn to plan, view and

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evaluate their work from the farmer perspective, to "see the world throught their eyes".

2. By actively involving farmers at each step of the process (designing, testing and evaluating alternative solutions to problems), FSR/E practitioners will better understand farmerr perspectives on proposed technological improvements.

DISCUSSION

"That farmers should participate in developing and evaluating technology for their own use is so evident that it has generally been ignored." R. Tourte (1984)

1. THE ROLE OF SOCIAL SCIENCE

The need for farmers to participate in the diagnosis, design and testing of new agricultural technology has been stressed throughout the previous units in this training manual. This section will focus on the use of social science perspectives and methods to further engage farmers in the evaluation of agricultural technology tested on farm, and to ensure that new technologies are evaluated not only on their production and economic merits, but also from the farmer perspective, "through farmers' eyes."

Social science deals primarily with "intentionality", that is, the "why" or purpose of human behavior. Social scientists observe activities and collect data on what people do and then provide reasons for why people behave as they do. In FSR/E, non-economic social science methods, particularly anthropological methods, are especially well-adapted to obtain both the perceptions and the actual behaviors of farmers for use in evaluating potential technology. Rhoades (1985) explains that asking why and exploring farmer behavior is an "amorphous activity" and cannot be equated with determining cost-benefit ratios or monetary value. Social Science evaluation goes beyond the economic evaluation of farming as a business and takes the holistic perspective, viewing the perceived and actual impact of proposed technologies on "the whole household in its ecological, physical and sociocultural environment" (Rhoades 1885).

Because social science in FSR/E focuses on the farmer (he or she), rather than the plants, animals or their interactions, it is difficult to separate social science perspectives in evaluation from farmer perspectives and evaluation of technology. In many ways, a major goal of social science input to FSR/E is to insure that the farmer perspective is considered.

2. PLANNING AHEAD FOR SOCIAL EVALUATION AND FARMER PARTICIPATION

Just as one plans in advance the types of analyses which will be conducted on the biological variables resulting from an on-farm trial, so must FSR/E practitioners plan ahead to collect

the social or cultural data needed for appropriate social and farmer evaluation. Since much of the information necessary to make a social evaluation involves observing farmer behavior during the course of the production cycle, practitioners cannot wait until the harvest to collect the appropriate data.

Many kinds of information will need to be gathered on a regular basis much in the same way that biological data will be collected. In the design of a field book, entry spaces can be added to include social observations. Some of these are already noted in the Minimum Data Set included in this volume (IV,C). The types of observations and data which will be useful in evaluation can be obtained by asking yourself the following questions:

- Who does each task in the production cycle? Who decides which tasks should be done and when?

- Does the proposed technological solution require more labor or resources than required under traditional production methods? How do households meet these additional needs?

- Does the technology being tested require new farmer skills? Are they difficult to learn?

- What other activities do the farm household members perform in addition to those directly related to technology being tested? Does the experimental technology compete against other important uses of household labor, time or resources?

Some of the information needed is observed or collected at a single point in time and can be ticked off a check list. Other information will require a continuous observation at several points in time (such as household labor inputs). Practitioners should design their field books to collect both single point and continuous data.

3. COLLECTING RELEVANT SOCIAL DATA

Practitioners should be flexible in their definition of "data" collected during an on-farm trial. Not all data are quantitative. Much of the valuable information which can be gleaned from observing and talking with farmers as the trial progresses, is qualitative, and takes the form of opinion, speculation or feelings. Practitioners monitoring on-farm trials should always try to visit the trial in the company of the cooperating farmer. Practitioners should ask farmers what they think about a trial or whether they have suggestions for changes in trial procedures for the following season. Farmers who may have passively agreed initially to cooperated in an on-farm trail, may feel more relaxed and willing to talk during subsequent interactions with FSR/E practitioners.

"Some of the farmer's impressions may be irrelevant

by-products of plot sizes and designs which seem odd to the farmer, but some of the observations will help to point out the practical problems of integrating the next technology into the previous system, and help to point out the conceptual problems that the farmer may have in understanding why a technique works. It is at this stage, that the extension person should start thinking about the terms which should be used to describe to farmers how a new technique works."

CIMMYT OFR Draft Training Manual p.6

Informal interactions with farmers during the course of a trial will often help to correct mistaken impressions of social behavior gained during the initial diagnostic activities. Sensitive information such as land tenure, animal ownership, capital resources and decision-making often become apparent, or can be discussed with greater ease once practitioners and farmers are on more familiar terms.

Practitioners should take care in the collection of data to look not only at what is happening with the on-farm trial, but also what is going on in adjacent fields. Researchers often find out (too late) that their conceptions of appropriate timing for planting, weeding or harvesting may be based on research station experience, which could be influenced by mechanization logistics, larger field size or input applications. Farmer timing of activities may follow a very different logic based on generational knowledge (passed down from ancestors) on ecological, climatic or other local factors. FSR/E practitioners can improve their on-farm trial designs by observing and questioning farmers as they conduct their own field activities. Care should also be taken to observe surrounding farms and farmer activity, as well as that on the farms where trials are located. Do the neighbors farm like the cooperator or are there marked differences between them? Why? (see IV,A for related information).

Remember, the important keys to collecting good and useful social science and farmer data during on-farm experimentation is to follow each question of "What is going on?" with a "Why?".

4. COMMUNICATION WITH FARMERS DURING ON-FARM EXPERIMENTS

Social science embodies the learning of certain skills which enable a social scientist to use interviews and dialogs as tools to gain useful information on the "whys" of farmer behavior. There is a certain "art" in the skills and methods of successful interviewing, especially in maintaining an informal atmosphere around the interview. Not everyone can learn to be a highly skilled interviewer, but FSR/E practitioners can enhance the quality of their communications with farmers by using the following guidelines (see also volume I:VII)

FROM CIMMYT EAST AND SOUTHERN AFRICAN ECONOMICS PROGRAM - OFR TRAINING MANUAL, DRAFT, 1985.

"Talk along with the farmers, not down to the farmer."

1. Communicate with both men and women. The man may be the head of household and major decision maker but the woman may be the main farm worker and have a major influence on the decision maker. Make it a point to understand family relationships and how they affect farm management.

2. Determine communication links in the community and within the individual farm situation.

3. Try to use language, terminology, units of measure, etc. that farmers understand.

4. Consider literacy problems of the farmer. Devise ways to minimize problems of communication. For example, a demonstration of what you want the farmer to do may be more effective than giving him or her written instructions.

5. Distinguish research from extension. Explain to the farmer your intentions in several different ways at several times.

6. Express willingness to learn from the farmer.

7. Don't promise quick results.

8. Maintain flexibility and a sense of humor at all times.

9. Establish a reciprocal relationship by trying to elicit honest opinions, encourage discussion about lots of subjects; plan to visit when the farmer is present; plan so that the farmer is not inconvenienced and explain the results of any trial.

5. SEVEN KEY QUESTIONS IN THE FARMER EVALUATION

Rhoades suggests that the following seven basic questions can help practitioners to come closer to their clients by trying to "think like a farmer." A fuller explanation of the implications of these questions can be found in Rhoades' excellent article "Understanding Small-Scale Farmers," Journal of Agronomic Education 13:64-68, 1984. Some discussion of these questions is presented below.

1. Is the Problem to be Solved Important to Farmers?

Scientists sometimes project their values or preferences too much into the farmer's circumstances. What may be scientifically

important may not be important to farmers. Farmers may cooperate with on-farm experimentation because they feel they have too, and not because they view the problem as a high priority. If this is the case, the experiment may be successful, but no one will adopt the solution. On-farm trials are an excellent way to test whether a problem really is a problem for farmers.

2. Do Farmers Understand the Trials?

This question raises others. Was the trial clearly explained? Was the number of experimental variables too large? Were there too many replications? Was the technology too complicated or sophisticated? Did farmers understand the utility of the new technology? "Technologies which build on existing, traditional practices will probably stand the best chance of being understood."

3. Do Farmers Have Time, Inputs, and Labor Required by the Improved Technology?

Practioners running on-farm trials must always consider the logistics of the trials from the farmer viewpoint. Farmers do not have research stations or projects to supply inputs, additional labor, or vehicles to carry supplies to the field or purchase them in town. Farmers weigh each new technology by the resources they control or to which they have access. These resources include land, labor and capital. Within the household, there may be competition for these resources. Farmers may have sufficient land, but lack the capital or credit for the inputs needed for new technology. More subtle is the question of labor and time. Farmers may have to weigh allocating family labor to the new technology against the time already needed to collect fuel and water, herd animals, collect feed for livestock, scare birds from fields, weeding or many other tasks which must be completed. FSR/E practitioners should not assume that because a household member is not engaged in agricultural tasks, that their time is free, because many other tasks must be completed to sustain the household.

FSR/E practitioners must also remember that households are not egalitarian units. Resources and benefits are not always distributed equally. Differences in access and control over resources and benefits often exist in terms of age and gender. Children, unmarried adults and elderly members of the household, even if they farm their own fields, often do not have the same access to farm inputs such as seed, fertilizers or animal traction, as the head of the household and his or her spouse(s) do. More striking and important in the design and potential adoption of new technology is the fact that gender often distinguishes the access or control of a farmer over the resources needed to farm or the benefits gained from farming. Women are often denied access to credit and membership in cooperatives which supply seeds, fertilizers or machinery. Women are often overlooked by extension services even when they are the

primary farmers of a household. Women may have great potential to gain from new technology but are denied the ability to use it. On the other hand, women's needs for technology may differ from men's and therefore new technology may be inappropriate or fail to address their production problems.

The key point to remember here is when asking the question whether farmers have time, inputs and labor required by the improved technology, practitioners should always ask further if all farmers (young and old, male or female) have the needed requirements. If not, should adjustments be made in the proposed technology?

4. Does the Proposed Technology Make Sense Within the Present Farming System?

A change in one part of a system, caused by the introduction of a new technology, will cause changes in the rest of the system. Will the new technology or proposed changes fit within the system? Will it cause changes in other areas which will have a negative impact on farmers? Will it negatively impact certain farmers (women, children, elderly)? Will it fit within the existing rhythms of production, such as the time needed for harvesting major cash crops or times when family labor is moved to a different agricultural zone to work for wages? Understanding whether a proposed technology will make sense requires close examination, probing within the system, observation and talking with farmers. Again, a technology may make perfect sense to the scientist who conducts an evaluation at field level, but it may make no sense at all in terms of the whole farming system.

5. Is the Mood Favorable for Investing in New Technologies or Crops in a Region?

Rhoades points out that "this question suggests understanding farmers' orientations toward investment or innovation in crop production brought about by broader economic conditions. If trials are conducted when prices have hit rock bottom and have stayed there for two or three seasons, promoting changes could be a losing battle. Even if farmers believe a change may be beneficial, they may respond with general pessimism."

6. Is the Proposed Change Compatible with Local Preferences, Beliefs, or Community Sanctions?

FSR/E practitioners should remember that taste or color preferences of foods, superstitutions and ceremonies are as important to farmers as they are to everyone else. Rather than viewing these as quaint or as obstacles, practitioners should see where they fit in the farming system as a whole. As Rhoades points out, "planting days tied to religious festivals may be an ingenious way of guaranteeing that work is done by a certain day." Technologies designed to take these aspects into account

are more likely to be acceptable, thus facilitating FSR/E work rather than hindering it. Preferences for food color, shape, size and taste must always be considered in the design of agricultural technology and can be critical in determing the evaluation of technology. The social science perspective in the evaluation may uncover that though the technology increased yields by 200%, the color of the new variety was unacceptable to farmers and consumers and therefore no one was interested in adoption. Finally, farming systems are linked to larger community and government systems. The evaluation of new technology for the farming system must always consider the potential impact on the community at large and whether government may pose restrictions on the utility of the technology. Can local marketing boards handle increased production? Can government suppliers of credit and inputs handle increased demand? Will the new technology create an advantage for only some farmers while creating a disadvantage for others?

7. Do Farmers Believe the Technology will Hold Up Over the Long Term?

Rhoades points out that "a farmer's view is normally based on long-term needs, not on a couple of seasons, and sometimes on generations of experience with the crop and land." Researchers and extension agents may view a technology favorably based on the results of three or four years of testing, but unless the records for the area are exceptionally good or they have lived and farmed themselves in the area for a long time, they cannot estimate how the technology will respond to the longer tests of time. Farmers who have farmed in the area for a long time, or who have learned farming skills from generations of farmers in the area, can evaluate technological performance using many more criteria than researchers. Creating opportunities for farmers, even those not directly involved in a trial, to assess technology being tested today in the field can provide an evaluation based on generations of agro-ecological, economic and social criteria. Standing up against the rigors of such a test will often yield technology far more likely to be readily adopted by farmers.

Posing these seven questions will greatly assist FSR/E practitioners in assuring that a social science perspective and a farmer perspective are incorporated in the testing and evaluation of new technology. In the same article, Rhoades also reminds us of a very important fact in evaluating technology:

> "In the end, the acceptability of a technology depends on what the farmers actually do. This can only be discovered in a final stage of farmer testing where farmers themselves take over the new technology and incur all risks, costs and benefits. Until this final step is taken, all other evaluations remain only suggestive of the technology's potential."

7. FARMERS' INDEX OF ACCEPTABILTY

The farmers' acceptance of a technology is the true test of its value. By defining the degree of acceptance of technology as the level of its use by participating farmers after the trial period, the concept can be operationalized in quantitative terms. In other words, the degree to which a farmer uses a technology that has been introduced can serve as a measure of his or her acceptance of that technology. The concept of an index of acceptability is discussed in Hildebrand, P. and F. Poey, On-Farm Agronomic Trials in Farming Systems Research and Extension, pp. 121 -125.

8. INTRA-HOUSEHOLD AND GENDER SENSITIVE EVALUATION OF ON-FARM EXPERIMENTS

Throughout this section, an effort has been made to highlight the need to consider and evaluate new technology from the farmer perspective. It has been demonstrated that a social science perspective can enhance the ability of FSR/E practitioners to view technology "through farmers eyes." Equally important in achieving the farmer perspective, is acknowledging and working with the fact that male and female farmers may operate different farming systems, may have different technological needs and may therefore react differently to the same technology being tested on-farm. Recent research among farmers in a wide variety of geographical and agro-ecological locations has shown that understanding the differences between male and female farmers, and discerning the dynamics of the relationships between farmers and individuals within households (intra-household) and between households (inter-household) is crucial to successful application of the FSR/E approach. However, the problem of how to go about gaining and incorporating this knowledge still remains.

One way to help practitioners to incorporate intra-household and gender sensitivity in their evaluation of on-farm experiments will be to acquire new analytical skills by working through the "Case Studies on Gender and Intra-Household Dynamics in Farming Systems Research and Extension" which form a part of the overall training package which includes this manual. Feldstein and Poats (1985) developed a conceptual framework for the case studies to provide a guidelines by which information on gender and the intra- and inter-household aspects of farming systems may be gathered, analysed and applied to the design of improved technologies for agricultural and livestock systems. It covers the information necessary to model a farming system and the process by which farmers (men and women) are included in the research and extension activities in a given area. Some of the key issues and questions provided in the conceptual framework are summarized here regarding the evaluation of on-farm trials.

First of all, what are intra- and inter-household dynamics and variables? What do they contribute to the analysis and

evaluation of on-farm experiments?

The basic notion underlying these terms in that a 'household' is not an undifferentiated grouping of people with a common production and consumption function, i.e. with shared and equal access to resources for and benefits from production. Rather, individual members of households or families share some goals, benefits and resources; are independent on some; and in conflict on others. Individuals are also members of other groups through which they may gain access to productive resources or benefits and to which they may have obligations. Poor rural households often depend on a number of activities, on and off farm, and alliances for survival. Farm management decisions on any enterprise are affected by the interplay of the roles and resources of the individuals connected with that enterprise as investors, laborers and beneficiaries. Thus, there are patterns of activity within the household and between households which relate to the ways in which members make choices and carry out activities.

What we face is complexity, not homogeneity. In a particular farming system or a single enterprise within that system, the pattern of resources and incentives must be discovered, not assumed. The conceptual framework is designed to assist in this discovery.

The way the conceptual framework operates is to examine the four areas of knowledge important to FSR/E to which a consideration of intra-household dynamics can make a contribution: labor, non-labor resources, incentives, and the process by which farmers are included in FSR/E. These areas are considered for each stage of FSR/E (diagnosis, design, on-farm experimentation and evaluation, and recommendations) by asking a series of questions. We will consider here only those appropriate to experimentation and evaluation activities.

a. Labor

What changes in labor allocation, in time or task, are actually associated with on-farm experiments? Do these contribute to or detract from increases in productivity or income for this enterprise? Do changes in labor allocation impact on other enterprises including household production? Do they fit what was predicted in the design?

b. Access and Control of Non-Labor Resources

How and <u>to whom</u> have new resources been supplied? Who has/has not <u>used them</u>? What networks of relationship or exchange have been used to garner any additional resources needed? Can further constraints in access to resources by particular groups be identified as result of the testing?

c. Incentives

What motivates people's decisions about the allocation of labor and other resources to farm production, home production and alternative uses? What incentives/disincentives are there for farmers (men and women) to modify practices concerning the enterprise in question? What incentives/disincentives are associated with the particular modifications being tested? Are ther incentives or disincentives associated with being a cooperating farmer? How do the technologies being tested affect individual income streams?

d. Inclusion

Are women as well as men included as cooperating farmers in on-farm research? For particular enterprises? Fields? In the management of trials? Are they included in interviews evaluating the trials? Are there factors which inhibit the participation of particular categories of farmers?

This framework is flexible and can be used to describe a farming system or the variables affecting a particular enterprise. People are often overwhelmed when confronted with a new list of questions to consider as they analyse and evaluate a situation. The questions presented in this section on social science and farmer perspectives are not designed to burden FSR/E practitioners with interesting but irrelevant detail. Instead, the purpose is provide practitioners with the tools and skills to better understand the nature and processes of farming systems in order to identify better solutions to the problems confronting all farmers today.

UNIT VI
HOW TO MANAGE AND ADMINISTER FSR/E AT THE FIELD LEVEL

(VI)

HOW TO MANANGE AND ADMINISTER FSR/E AT THE FIELD LEVEL

OUTLINE

1. Intra-Institutional Issues
2. Inter-Institutional Linkages
3. Costs
4. Logistical Issues
5. Human Resources

PREREQUISITES

PARTICIPANT LEVEL

Agricultural research assistant
Extension technology verification technician

This unit attempts to sensitize aspiring or new FSR/E teams to many of the management and administrative problems. The unit is aimed at the field-level practitioner team. However, the unit should also be read by their teams superiors as it may serve to remind them of the many problems encountered by field teams which are unrelated to agricultural technology advances.

LEARNING OBJECTIVES

After completing this unit, participants will be able to:

1. Anticipate and plan for many of the common management and administrative issues and impediments which are encountered at the field level by FSR/E practitioners.

2. Develop practical, working relationships with fellow researchers, extension personnel and superiors within their host ministry.

3. Be aware of, and assist mid- to upper-level administrators in their host ministry to resolve, inter-institutional problems which arise from bottle-necks that cut across ministries.

KEY POINTS

1. There are five major areas of problems encountered by field-level practitioners as they attempt to operationalize a FSR/E approach. These major areas are:

    (a) Intra-institutional issues (those problems most commonly encountered within the ministry or department which hosts the FSR/E effort),

    (b) Inter-institutional issues (those problems which

occur between the FSR/E host ministry or department and other institutions including, but not limited to, other ministries or departments),

(c) Fixed and recurring costs involved in institutionalizing the FSR/E approach,

(d) Logistical issues (revolving around guaranteed team mobility to interact with farm households and monitor systematic farm-level crop or livestock trials), and

(e) Development and deployment of necessary human resources to allow successful operationalization of the FSR/E approach.

2. There are many commonly-encountered management and administrative problems which interfere with the smooth implementation of the FSR/E approach. There are also many management and adminstrative problems which are unique to any given nation and the specific ministry or department which hosts the FSR/E approach.

3. Common problems which may have tried and tested solutions should be distinguished from problems which are unique to your situation. The latter group of problems will require interdisciplinary team deliberation and may require innovative solutions.

DISCUSSION

One of the most neglected areas in the FSR/E process is that of field-level management and administration. Problems which arise from many of the bottle-necks which exist within a given FSR/E host ministry, as well as between such a ministry and other institutions, take up the precious time of all FSR/E team members, and require even more of the time of team leaders and their immediate superiors and regional counterparts. This unit attempts to sensitize aspiring or new FSR/E teams to many of these management and administrative problems.

Many factors and issues influence the success and rate of operationalizing and institutionalizating a FSR/E approach. Issues which are of particular concern to mid- and upper-level decision-makers and administrators are considered in the FSSP Project Guideline Handbook (see especially Draft 3, Chapter II and Appendix F).

This unit is aimed at all field-level practitioners of FSR/E, either those involved in projects or programs. The intended audience ranges from the Chief of Party (or team leader or station manager) to all host country counterparts or program participants, and includes materials relevant to both researchers and extension personnel. This draft draws upon the experience

and observations of three former FSR/E practitioners. However, the FSSP admits that the unit is a working draft and, for this reason, encourages all practitioners exposed to the material herein to submit additional experiential materials and/or anecdotes to be used to update these training materials and make them more relevant.

1. INTRA-INSTITUTIONAL ISSUES

Intra-institutional issues are defined as those issues which internally influence the particular ministry or department which acts as host for the FSR/E approach. The major issues a team may expect to face include the following ones.

a. Location of the FSR/E Team

The team may be composed of persons recruited specifically for FSR/E activities, or may consist of researchers and extension personnel called out from more traditional activities and placed together for purposes of on-far research. The team will have a physical location which will be either close to the target farmers (regional placement) or further away (capital city placement). Regardless of location, the team requires staff support: secretaries, assistants, etc. In addition, the team will require additional support in terms of office space, supplies and equipment. How these are provided may be an issue to address early on in the process.

In addition, the FSR/E team will usually be assigned to an experiment station. The team may or may not be physically located at that station. If it is physically located there, the space and support issues above apply to this regional level as well. Regardless of base location, the team needs to come to terms with the normal routine of the given station base, both in terms of the station manager and the field assistants available. Avoiding station labor bottle-necks and accomodating the station-based research of the team are areas of concern which are not resolved automatically. Negotiation and frequent team and research station based personnel meetings are a necessary part of the process. Assignment of equipment and personnel may add unforseen burdens to the life of any station manager. The team and those above them in the structure need to assist any station manager address these new needs. Additional equipment and supplies may be needed at the outset of any new FSR/E approach.

b. The FSR/E Team-Researcher Interface

The FSR/E team must interface with four major sets of personnel: (1) their immediate superiors in the organization, (2) commodity researchers, (3) disciplinary specialists and (4) extension. Each will be considered separately.

1. Superiors

The team is essentially responsible for their technical work to their superiors in research. They must effectively communicate to them their research objectives, plans and results. Small problems must be resolved quickly before they progress into large ones.

2. Commodity Researchers

The team most depend on these researchers to provide new sources of germplasm for any trials which may include varietal testing. This group of researchers is also very keen on being informed of farm-level realities — such as pest and farmer reactions to their improved genetic materials. Team members must remember that commodity researchers have their own commodity improvement programs and are busy. Adequate lead time is necessary when requesting any quantity of improved varieties.

3. Disciplinary Specialists

The team may need the time commitment of entomologists, pathologists, weed scientists, economists, and statisticians in the design and analysis of their trials. During the growing season, the team may have to call upon the services of one or several of these individuals in a consultative expert role, either in the farmer's fields or in the office. Team members must again remember such resource personnel have their own research agendas and are busy people.

4. Extension

The team will work with extension at least at the local and regional levels. Often, upper-level FSR/E management personnel, including the head of research, will work with the national head of extension. These relationships are considered further under Inter-institutional issues.

2. INTER-INSTITUTIONAL ISSUES

Inter-institutional issues are defined as those issues which influence the particular ministry or department hosting the FSR/E approach and all other ministries or departments with which it must interact, including interactions with bilateral and multilateral donor agencies and international agricultural research centers (IARCs).

a. Research-Extension Linkages

The team needs to work with local extension agents from the outset of their work in FSR/E. The team must come to an understanding with regional and district extension supervisors or coordinators as well, but such an understanding must be worked at with national heads of both research and extension, so that both parties will have a shared understanding of the working relationships. At the local (village) level, the team will work

with the existing agents. Such relationships always range from open and mutually reinforcing to cold and hostile. The team must always encourage local extension participation at each stage of the process, but cannot be expected to guarantee it. Just as each researcher will vary in his or her committment to the FSR/E approach, so will each extension agent have a varying understanding and committment to the team/extension working relationship.

1. Formal/Informal Linkages.

Formal linkages are defined as those which are agreed upon between the respective heads of research and extension to the degree that they have been institutionalized in both the research and extension departments or divisions via memorandums or other operational directives, regarding the official working relationships of the FSR/E team and the local extension agents. Informal linkages are defined as those which occur between each researcher on the FSR/E team and each local extension agent: the personification of the formal linkages, or, in other words, the real working relationships that develop at the field level.

2. Research-Extension Working Relations.

While substantial and mutually-beneficial work may be accomplished through the informal (personal) working relationship route, many approaches consider the seconding of extension personnel to the FSR/E team or approach to be a realistic way of involving extension personnel with the FSR/E approach. The effort should be made at the upper administrative levels of both research and extension to show and stress the importance of joint FSR/E activities in the national approach to research and extension, so that the FSR/E team is not used as a dumping ground for extension personnel from the least productive end of the spectrum of extension competence.

Planning of diagnostic and trial activities, and planning of next year's work activities and plans, should be jointly undertaken by the FSR/E team, the commodity and disciplinary specialists most intimately involved in the on-farm research, and the extension personnel working with the unit. Such joint activities can go a long way toward solidifying the work plans and the approach of all actors in the process.

b. Ministry of Agriculture External Linkages

1. Ministry of the Budget

Each nation contains either a ministry or a department responsible for the national budget and for disbursing funds to each other ministry or department. It is vital that administrative understanding at the upper level of the FSR/E unit understand the fund trasfer and accounting procedures and requirements of the Finance Ministry. It is equally important

that those in the Finance Ministry in charge of FSR/E account realize that all purchases cannot be precisely planned for annually, and to authorize a small rotating fund for field-level emergencies.

2. Ministry of Livestock

Some nations separate crop and livestock research at the ministerial level; some separate them at the department (or division) level within a given ministry. Regardless of the specific of the division, incorporation of a working relationship in the FSR/E team with livestock is very high priority. In many cases, inclusion of a livestock specialist on the team is both logical and necessary.

3. Ministry of (Agro)Forestry

Many nations have a separate ministry to consider forestry or the interface of agro-foresty issues. Some nations have place a major emphasis on agro-forestry in these ministries. Where this is the case, it is important that mid- to upper-level administrators realize, accept and promote the natural joint activities of agricultural and agro-forestry research. Again, inclusion of formal links with, or representation of, agroforestry on the FSR/E team is desirable from the outset.

4. Other Ministries

Significant linkages and mutual understanding may also need to be developed between the Ministry of Agriculture and, for example, the ministries of fisheries, public health and nutrition, education, etc., depending on the interactions between the FSR/E approach and the areas of potential overlap between them.

5. Bilateral contractors

Through the Ministry of Agriculture, the FSR/E team needs to be aware of all bilateral contractors working in the country, especially those working in (a) the same (or similar) agro-climatic zone, or (b) with overlapping or complementary goals, objectives, approaches or outputs. There is never a need to "reinvent the wheel". Anything appropriate to the problems being faced by the FSR/E team, which has already been addressed by any of these other groups, should be incorporated into the experience of the FSR/E team during the collection of relevant secondary information. Conversely, any bilateral contract entering the nation after the FSR/E approach has begun should interface with the approach leaders to avoid duplication of effort and/or territorial disagreements at a later date.

6. Bilateral Donors

Ministries of Agriculture and Finance need to be increasingly

concerned with coordinating donors activities and projects, and to be less and less concerned with getting assistance of any type. At times, the total absorptive capacity of a nation is swamped because either too many donors have unrelated projects, or too much donor-provided money has come into the nation too quickly.

7. Multilateral donors

These include the World bank and the various regional banks. Again, planning must include these organization's demands on the time and matching budget for separate projects.

8. International Agricultural Research Centers (IARCs)

The IARCs operate regional commodity programs throughout the world. Some place more emphasis on one region than another (IRRI's concentration on Asia; IITA's concentration on Africa). Many of the IARCs also have farming systems approaches and offer training in FSR/E. Some training is conducted at the center level; other training is brought and tailored to individual country needs. Those planning to initiate FSR/E for the first time are strongly urged to contact the personnel in charge of FSR/E training at those IARCs closest to, or most active in, their country and/or region. Such contacts are necessary to arrange for training of team FSR/E personnel. (The FSSP also conducts tailored FSR/E training)

3. COSTS

Fixed costs may be formally defined as "costs which in the short-run do not vary with output", or "the cost ... which goes on regardless of the amount of production." While such costs include rentals, they may be thought of generally as those costs which occur on a one-time basis, usually near the front end of a project, or as a research approach is first begun. Examples include experiment station construction, purchase of laboratory equipment and work vehicles for researchers or extension personnel.

Recurring costs may be formally defined as "the expenses of a business which keep on recurring, such as wages..." Less formally, recurring costs are those costs which may occur over and over again during the life of a project or over many years during which an approach is being opperationalized. Examples include maintenance of experiment station fences, having a laboratory scale re-balanced, or the purchase of oil, filters and petrol for a work vehicle.

a. Fixed Costs

1. Experiment Station

In implementing FSR/E, the costs added to normal fixed costs

of an experiment station are minimal or negligible. Most added costs are recurring ones. Indeed, in the most extreme case, FSR/E can be carried out with no experiment station interaction. However, this greatly limits the efficiency of the effort, as feedback to researchers is minimal, as are contacts between commodity researchers, disciplinary specialists and FSR/E team members.

2. Work Vehicles

The added inventory of vehicles assigned to a given region's station may be supplemented by FSR/E team vehicles. In addition, the demand for station-based vehicles may be increased in the initial stages of FSR/E implementation, as lags in provision of equipment means that the team may be required to approach either the station manager or individual researchers for assistance with mobility early in the FSR/E process. Such borrowing/lending interactions may be viewed hostilly by traditional, station-based researchers, and FSR/E team members are urged to use tact in such negotiations.

3. Equipment and supplies

It is very likely that the team will require several sets of equipment (including backpack sprayers, plot marking twine and/or stakes, etc.) early on in the FSR/E process. Likewise, additional laboratory supplies may be needed. Examples include a simple laboratory balance, various petrochemicals, desks or work tables, chairs, typewriters and/or word processors, etc. Again, borrowing from existing supplies may have to occur early on in a project or shift in approach. Such cases are not always looked upon kindly by those being asked to lend or give up equipment or supplies.

b. Recurring Costs

1. Rotating Fund

It is impossible to run a responsive, flexible and relevant FSR/E approach without creating a small field accessible rotating cash fund. Such a fund should be located at the appropriate regional locale (either the experiment station or the regional director's office), so that the team has daily access to it. This issue must be agreed upon between top administrators in the Ministry of Agriculture and Finance, and between mid-level administrators (the regional director and/or station manager and the FSR/E team leader), to the degree that authorized use, replenishment procedures, etc., are agreed to in advance and adhered to by all affected parties.

2. Petrol

Traditionally in short (and lumpy) supply to any given research program, this commodity is often the key factor to

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providing the necessary mobility to perform programmed and spontaneous FSR/E diagnosis and trial and household monitoring in farmer's fields and homes. Even if the local team is physically living in the village area near the farmer's fields, they may need petrol for motor cycles or for a shared vehicle. Also, the backstopping members of the team not located at the village level must be able to make routine visits to the field level team members, and to respond to emergency consulting requests which simply cannot be anticipated. An example of this response might be a visit from the regional entomologist when a severe outbreak of an unknown insect pest is reported.

3. Additional Support to Mobility

Vehicle maintenance and repair cannot be overstressed. During the season, some monitoring visits cannot be postponed. Motorpool arrangements for emergency repairs must be possible. Funds from rotating fund must be available for such repairs and non-programmed services.

Monetary incentives, inducements and/or adjustments to local professional and/or support staff may have to be considered by administrators. It is difficult to require FSR practitioners at any level to subject themselves to the larger hours required, as well as being separated from their families, without some type of compensation. Projects or programs must be more innovative in this area.

4. Human Resources

The human resources available in any given country will help determine whether the normal number of researchers and extension agents will have to be added to for successful introduction of FSR. In some very small countries, the Ministry of Agriculture cannot afford to pick up the salaries, in the long run, paid in the short run by bilateral donor project funds. This dilemma raises serious questions about the preconditions necessary in any country to consider the FSR approach on a country-wide basis. Regardless of the size or wealth of the country, training and educating research and extension personnel will cost both time and money.

4. LOGISTICAL ISSUES

Logistical issues are defined as those issues of mobility, work circumstances, and support staff which can either assist greatly, or adversely affect, the most effective deployment of both physical and human resources during agricultural research and extension.

a. At the Local Level

1. Guaranteed Mobility

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Guaranteed mobility is necessary to allow visits to farms and farmers whenever necessary.

2. Adequate Supplies and Equipment

Adequate supplies and equipment are needed to allow research and monitoring observations in the field and at the household levels to occur according to schedule.

b. At the Regional Level

1. Routine Maintenance/Needed repairs

Routine maintenance/needed repairs are needed for vehicles on a timely basis.

2. A viable work vehicle policy

A viable work vehicle policy needs to be developed and each member must have a clear understanding of the policy for work vehicles assigned or loaned to them.

3. Joint Work Plan Development

Researchers and extension personnel in a given region or domain need to develop annual work plans in together so that each group knows what the other plans to do and so that each group's plan can complement the other's.

4. Station Equipment Use and Personnel Deployment

Understanding must be reached between the regional director and/or station manager and the FSR/E team about (1) the use of station equipment and supplies, and (2) request procedures for, and use of, station and field assistants.

5. Use of Regional Rotating Fund

The regional rotating fund is to be used for emergency — i.e., not routinely planned — expenditures only. Its use should be restricted to purchase of those items needed to keep vehicles running between routinely-scheduled maintenances, and to facilitate trial implementation and responses to monitoring.

6. Assuring Quick Data Turn-Around

Quick data turn-around must be assured in any FSR/E approach. More and more data is being processed at a sub-national level. When this occurs, results must get back to the farmers and the team. Examples of times when rapid data analysis turn-around is called for are (1) following the initial diagnostic phase in planning

for initial trial design; (2) following the initial trials for use in planning the follow-up trials; and (3) during farm record management. In the latter case, it is ideal to be able to provide farmers with sets of analyzed data based on major crop or livestock activities (such as land preparation, weeding, or harvesting; or birthing, purchases or sales of livestock).

c. At the National Level

1. Major Work Vehicle Repairs

National level decision-makers must promptly honor requests for vehicle repairs which are above the limit for the local rotating cash fund of the region.

2. Guaranteeing the Greatest Level of Farm-level Mobility

National level decision-makers should attempt to guarantee the greatest level of farm-level mobility for the FSR/E team. Such support should extent, but not necessarily be limited, to (1) replacement of worn-out work vehicles; (2) providing FSR/E team members with official letters of introduction for specific local officials (village heads, extension coordinators and local agents, etc.); (3) assistance with rents for locally-based staff (this may include authorizing monitary assisstance for two rental units for those staff unable to relocate their families. It may also include some financial assistance for relocation of staff and their families. Some consideration should be made for the added hardships faced by such staff.

5. HUMAN RESOURCES

Human resources are defined as all of the manpower available or potentially available to a given FSR/E project or approach. Human resource development is defined as the formal education and formal and informal training of personnel to carry out effective FSR/E. Human resource deployment is defined as the way in which FSR/E personnel are assigned to both sub-regions and work tasks in any given national FSR/E setting.

a. Developing Human Resources

1. Formal Education

Adopting a FSR/E approach usually means that a given nation will have to invest more in formal education of personnel. While much of the emphasis will be at the B.S. and M.S. levels, a certain number of new PhD's will be necessary to assure the continuity of the

approach, especially if the FSR/E approach is being introduced to the country by a bilateral contract. There is no substitute for quality-trained personnel. Host country decision-makers should place great emphasis upon trying to guarantee the most relevant education possible for each individual trained officially for the FSR/E approach. Top consideration should be given to applicants interested in working at the farm level in an interdisciplinary team, and to those interested in the agronomic, pest management, breeding, agricultural economics, anthropology and rural sociology disciplines who are also interested in learning about what other disciplines have to offer. Minimize use of rote lists of candidates whose "turn" it is to receive further education, unless they fit well into these two categories.

2. Formal Training

Maximal use should be made of the medium-term formal training course offered by some of the international agricultural research centers such as IRRI (rice production and breeding) and CIMMYT (wheat and maize production and breeding). Both institutes also offer FSR/E trainings based on their mandate crops.

3. Informal Training

The type of training which many believe is most cost-efficient is short course informal training. Such training includes, but need not be limited to, (1) short courses offered by most, of the international agricultural research centers, (2) short courses offered by third countries, (3) short courses offered on a regional basis (both CIMMYT and the FSSP have been active in this area) and (4) short courses which can be offered in a given host country and which can be tailored to meet the locally-defined needs of a specific target audience of participants. Again, CIMMYT, IRRI and the FSSP have been proactive in encouraging tailored, country or region-specific training.

b. Deploying Human Resources

1. Placement of First-time Practitioners

a) At the Regional Level, at least one person of similar rank, title, status and/or experience to that of the regional-level adminstrator is necessary on any FSR/E team. This post is needed to facilitate the necessary interation with regional bosses, including, but not limited to, commodity researchers and heads of commodity programs, experiment station directors, and regional extension supervisors.

b) At the Local Level, generally, local placement can be of two types: (a) regionally-based technicians with guaranteed mobility — vehicles or motorcycles — to the village and farm level, or (b) village-based technicians with sufficient village-level mobility but needing guaranteed contact and mobility linkages back to regional contacts and FSR/E personnel. Regardless of the placement specifics, administrators need to be aware that a guaranteed two-way logistical and communicative flow must occur in practice, because it is this level of project or program staff which has daily contact with the farmer clientele and access to their problems and priorities.

2. Placement of Subsequent rounds of Practitioners

If the project or program has a choice in placement of subsequent FSR/E personnel, it will probably extend slowly, region by region, placing a critical mass of staff in each region before moving into another new area. A critical mass may be defined for the sake of convenience as at least two, and probably not more than six or eight. Placing single individuals trained in FSR/E in diverse regions may appear to be more cost-efficient, but such an approach lacks a critical mass of interdisciplinary approach.

3. Multiplying Human Resources

Extending the FSR/E methodology to other distinct regions and other locales within the pilot (or initial) region requires more human resources. A program or project may be requested to provide FSR/E training to researchers, extension personnel and/or technicians already located in a given region to expand the human resource base equipped with the FSR/E approach. In such a situation, one solution is to train or develop a core staff of trainers to carry out training in the FSR/E approach and methods. Staff can then react by setting forth a logical training program on a region-by-region basis.

If the team is also fortunate enough to have access to a regional training facility near its base, training of regional participants from more than one region at a time may occur. Otherwise, trainers will probably have to visit each region sequentially. A compromise, requiring a greater amount of logistical input, would be to combine training for two or more regions, but base it in a given region, which would act as host to participants from the other region(s).

ACTIVITIES:

ACTIVITY ONE: TYPICAL MANAGEMENT AND ADMINISTRATION DILEMMAS
A reading discussing the typical management and administration dilemmas encountered by a team implementing FSR/E.

ACTIVITY TWO: DEVELOPMENT OF HOST COUNTRY ORGANIZATIONAL DIAGRAM AND DISCUSSION OF PIVOTAL LINKAGES
Appropriate for the whole set of participants, or for small working groups, this exercise allows trainees to develop host country organizationnal diagrams with an end to discussing key (pivotal) linkages in the institutionalization of any FSR/E approach.

ACTIVITY THREE: A POTENTIAL CONFLICT IN THE MANAGEMENT OF AN EXPERIMENT STATION
An active role play situation mixing small and large group action. This exercise concentrates upon resolution of potential management conflicts of an experiment station involved in research and extensions linkage issues.

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TYPICAL MANAGEMENT AND ADMINISTRATION DILEMMAS ENCOUNTERED BY A TEAM IMPLEMENTING FSR/E

This reading provides a one-year summary of typical management and administration issues which may affect the operationalization of any new FSR/E approach. It is arranged according to fine-tuned FSR/E stages — consider them to be sub-stages if it helps — and is meant to be suggestive of an actual experience of an interdisciplinary FSR/E team. Examples provided are experiential and are not meant to be all-inclusive. They are provided as illustrations of the major management and administration issues listed in the previous sub-units. Practitioners are encouraged to submit additional management and administration experiences encountered while implementing the FSR/E approach. The FSSP will attempt to incorporate them into these training materials.

1. COLLECTION AND REVIEW OF SECONDARY INFORMATION

Please refer to Volume 1:VI for additional information on secondary data sources.

a. Macro-Level Statistics on Agricultural Crop and Livestock Production

This type of information is generally obtained in key decision-maker's offices and at planning sections and/or libraries within the Ministry of Agriculture (MOA). Problem: the FSR/E team may not be able to obtain their own copy of the most current statistics. Photocopying may be a solution. Someone may have to authorize such photocopying charges or the purchase of production statistics, if all publications are distributed on a fee basis.

b. Maps

Types to consider include:(1) potential regions, (2) parcels of farmers within selected domains, (3) major soil groups, (4) official government aerial photos of selected domains, etc. Locating these documents may be more difficult and time-consuming than locating the above secondary information. A charge will usually be made for a copy of each map. Official letters of authorization and/or introduction may be required even before access to certain types of maps can be attained. Some nations prohibit access to certain types of maps because of politically sensitivities. In such cases, the FSR/E team must do without some of these helpful secondary sources of information.

c. Detailed Soil Type Maps and/or Reference Books or Manuals

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Once the team has selected the region for work and the domain(s) within the region, it needs more detailed soil information. Such information may or may not be available. If it is, access to it should be obtained. Again, there is usually a charge for such information.

d. Relevant Station and Commodity Research Results

Once the team is headed for a specific region, it needs access to resent research conducted there. Normally obtainable by speaking with key station-based researchers, reviewing their field books, and through annual research reports, the team may initially need a letter of introduction from the appropriate MOA official to allow them to have access to station and regional research literature. The entire FSR/E team-commodity researcher linkage begins with the interest and willingness of the team members to see what their fellow researchers have accomplished lately in the major crops and livestock of the region. The FSR/E should always stress the complementarity between farm-based and station-based research.

e. Additional Secondary Data

Many additional types of materials — ethnic histories of a given region, anthropological and/or historical writings, histories of bilateral or multilateral development projects, etc. — may be extremely valuable to an interdisciplinary team first moving into a region. Much of this information is obtainable by borrowing and/or photocopying. What is needed administratively is the encouragement and time to do the necessary location of such information. Probably one to two months is sufficient.

f. Initial Interpersonal Relations

This period of time marks the initial interaction between the team and the regional director and/or station manager. Therefore, it is crucial for the team to maintain a low-key, positive approach. It is a big mistake for a team to be either condecending or critical during these initial contacts. Both station-based researchers and station managers have full agendas and all are very busy people. The FSR/E team needs to be very much aware of these realities, and be careful how they interact with established researchers and station managers. To make the most of the FSR/E effort, each will require the assistance and blessing of the other several times during any given season. Alienating these mid-level decision-makers is only done at great risk to the logistical and technical success of the FSR/E approach.

2. BASE OF OPERATION

An interdisciplinary FSR/E team may operate out of (a) the capital city, or (b) a major regional city. The team may or may not be assigned to an experiment station. The team may or may

not have direct access to an experiment station. The team may divide along senior/junior lines, with senior team members stationed in either the capital or in a major regional city, while junior members are located physically in villages or communities being served by the approach. In any case, the team leader and all team members need to work through the proper protocols for their relations with the regional director and/or station manager. In addition, they may need permission to work and/or live in, selected villages. Either village councils or village head men may need to be approached by the team. Official letters of introduction may be needed.

The issue of basing a rotating cash fund near the FSR/E team arises here. The fund should fall within the region of operation, but its administrative control should remain under the team leader, not under the regional director or the station manager. The fund should be located as close to the team as possible to minimize slippage between identifying the needed emergency expense and approving funds to cover it. This cannot be done if a trip to the capital is required every time a rock rips a hole in a work vehicle's tire, for example. Fiscal accountability procedures, and the appropriate monthly or quarterly reporting procedures, need to be worked out in advance between the team leader and his or her immediate superior in the MOA, so that team members and their superiors understand the ground rules of rotating fund use from the team's first day in the field.

3. INITIAL DIAGNOSTIC PHASE

Once a team is working in a given region it will begin the initial diagnostic steps of the FSR/E process. Whether the process begins with a sondeo (or rapid rural appraisal), RRA, or formal survey, is largely irrelevant from a management point of view. The team requires guaranteed access to:(a) supplies (such as paper for photocopying questionnaires), (b) sufficient enumerators to gather the necessary data, (c) mobility to move the enumerators around, and (d) an appropriate mechanism for quick data analysis and turn-around. A potential bottle-neck exists at the point of each of these requirements.

a. Supplies

If unavailable or of limited availability within the MOA, the team needs to make a rapid decision as to whether such supplies (photocopy paper, etc.) constitute an appropriate rotating fund purchase the first year.

b. Sufficient Enumerators

If the team is not large, or if it is opperating under a severe time constraint, more human resources may be needed to carry out a RRA. Possible sources of supplementary human resources include fellow researchers, key commodity heads, key

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disciplinary specialists, and extension personnel. If the team plans to involve others, it must consider at least these issues:(1) the timing of the activity, (2) the MOA administrative channels for agreeing to such joint activities, and (3) the logistical and financial requirements necessary to assure success. Most researchers and extension personnel cannot attend such activities without provisions being made well in advance for covering per diem and petrol costs, if they use their own research or extension vehicles. Cross-regional activities (supplementing staff of a given region with staff from neighboring regions) are conceptually ideal, but may raise many questions about use of a second region's personnel and equipment to assist a first region. Directors and researchers of other regions often wonder what is in it for them. This is a legitimate question, and needs to be addressed. Can a quid pro quo relationship be worked out or established? That is, at a later date, the FSR/E team can assist those in the second region with an activity requireing more manpower than normal.

c. Mobility to Move Enumerators Around

Seldom if ever does the FSR/E team have suffiecient physical resources to move all required enumerators around. The ideal situation occurs where the team has a locally-based component which is sufficiently large to provide all the complementary activites required to carry out initial diagnostic activities. Where this is not the case, and additional vehicles are needed, permission has to be asked, and granted, to mobilize sufficient vehicles. Such permission may be internal to the region (requested of the regional director or station manager) if the region has sufficient vehicles and their use has not been fully scheduled during this diagnostic activity. Otherwise, such permission may have to originate at either the regional or national level, and be directed to other regional directors or station managers. Regardless of the source, vehicles used in diagnosis must be maintained if breakdowns occur, and petrol must be available on a timely basis.

d. Quick Data Analysis and Turn-Around

More FSR/E projects have floundered initially on this requirement than on any other factor. Usually questionnaires are too long or detailed, too many farm families are interviewed, the linkage with a mainframe computer for analysis is tenuous at best, etc. The first key to success is to make the survey instrument as streamlined as possible, by eliminating all but essential items. A second key is to plan for the analysis as the survey instrument is designed. A third key is to collect data in such a way as to minimize the hassle of the analytical process. A fourth key is to establish the analytical method and responsibilities in advance of the initial diagnostic activity. A fifth key is to pretest and modify the survey instrument (in the case of a formal questionnaire) or the master list of questions (in the case of RRA). A sixth key is to submit the

questionnaires for analysis immediately upon completion of the diagnostic activity. A seventh key is to mandate that the whole team be involved in the analysis as a group. This final key not only guarantees a quick product, it also reinforces the interdisciplinary interactions which occur during data analysis. At this point, the team leader may have to reinforce the decision to have a team analytical exercise, as a tendency may be to assign the analytical process to only some members of the team. However, such a tendency dilutes the interdisciplinary nature and significance of the task. Additional information on informal survey techniques can be found in Volume I: Unit 7.

4. INITIAL TRIAL DESIGN

The only significant constraint at this point of the process may be one of management. Social scientists on the team may feel like skipping this phase. They should not be allowed to do so. In addition, major commodity and livestock heads and disciplinary specialists (such as pest management specialties, agricultural economists, statisticians, etc.) should be encouraged to participate in the design process. If the team leader cannot get access to their time (due to the parallel nature of his or her administrative position in the MOA), the administrator immediately superior to the team leader and the commodity heads should issue the trial planning invitation. Agreement should be reached shortly thereafter about per diem allowances for those attending from outside of the region.

As a joint activity, the team leader should take the lead role in bringing discussion of each trial's to closure. The team should reach agreement upon the design of each trial, its objecrtives and treatments to be included, level(s) of treatments, inputs (if any) needed, check plot definition, and agreements to be reached with collaborating farmers concerning trial conduct and seasonal management.

5. PREPARATION FOR TRIAL IMPLEMENTATION

Usually, this sub-stage of the FSR/E process requires a high degree of logistical activity by various team members. Supplies and equipment for each village-level team needs to be identified, located or purchased, and assembled prior to planting or trial lay-out. Many items are essential (such as twine, field tape measures, etc.), and many more are desirable (such as stakes or flags to mark plot corners, altimeters, inclinometers, etc.). Someone must be designated to accumulate all team equipment and supplies prior to trial installation. The team leader must be able to either locate such equipment and supplies within the MOA system, or authorize its purchase using rotating cash funds. A supply/equipment "kit" must be prepared for each different village-based team. While certain equipment may be shared (for example, soil bore equipment, altimeters and inclinometers), other equipment must remain with each team (twine, tape measure, clip boards and/or field books, hammer or equivalent, a cutting

device like a machete, etc.).

Seed packets for variety trials may need to be prepared. Inputs, both traditional and innovative, may need to be provided for cropping trials. Livestock interventions may need to be located and/or purchased. Improved pasture or legume sources may need to be located, and sufficient quantities obtained to begin livestock-crop interactive trials. The team may have to assemble some equipment to supplement that available at the experiment station, if the need is great enough. For example, the station may not have enough field scales to measure harvested yield. In such a case, rather than borrowing, the team may have to purchase additional scales.

The key to all of these preparational activities is to start them with sufficient lead time to allow completion before the first trial must begin on the first farmer's field. Thus, the team must work back from expected planting dates, allowing sufficient buffer time to respond to early rains, should the team be based in a rain-fed region or domain. For this sequencing to take place properly, expected planting dates must be used to structure both the trial planning exercise and the initial diagnostic activities. The same rationale holds true for livestock-based trials, except that the likely timing of the livestock intervention must be determined first, with backward planning required for preparational team activities.

6. TRIAL IMPLEMENTATION

As expected planting dates draw near, it is likely that the FSR/E team will be introducing trials in more than one locale simultaneously. All local teams may be planting or superimposing trials in their own respective villages on the same day and at the same time. For this reason, each team needs a minimum amount of equipment and supplies (see above section) of its own.

Contacting farmers and assuring coordination of planting or superimposing trials in farmer's fields, or introducing livestock interventions, is one of the two most difficult activities for any team to accomplish. (The second difficult activity is assuring coordination of harvest with farmers). This time is crucial in the process, and not just for each planting or superimposed treatment scheduling. Also at this time, each participating farm household needs to come to agreement with the team as to the objectives of the research work, the expectations by each side, who pays for what and who is expected to do what during the season, including agreement as to compensation or no compensation for samples removed at harvest time. Very much a function of effective communication, this can be a stressful process and time for both the team and the farm families.

The entire period is characterized as one of hurry up and wait, especially if the region is entirely rain-fed and planting begins following the first significant rain-fall. If serious

delays occur in rains, it may be extremely difficult or impossible for the team to be able to stagger planting with collaborating farmers across only a very few days. If all farmers are awaiting the rains, they may view it as an unreasonable request to wait four more days for the team. Team management is likely to be streached to the breaking point at this time. The team needs all of the administrative and managerial assistance it can obtain from those above it in the administrative structure of the MOA during this period. The team may also need to have the flexibility to either research station field assistants, or to hire, using rotation funds, additional manual laborers from the labor pool in each village.

7. TRIAL MONITORING, HOUSEHOLD INTERACTIONS AND CONTINUED DIAGNOSIS

a. Trial Monitoring

Generally, such activities can be scheduled in advance by the team. Routine visits — to confirm germination and emergence, to assess insect and/or disease damage, to monitor weeds, to note physiological stages of plant or animal maturity, to monitor animal or bird mortality,to quantify animal weight gain or loss, etc. — are agreed to in a joint team meeting shortly following trial design. Scheduled monitoring may be augmented or reduced during the season, depending upon what happens in the trials. The distribution of human resources and vehicles during these activities is best described as routine: these are agreed-upon monitoring activities which have been planned for, and managing vehicular and personnel assignments should not be difficult.

However, as with most everything in the FSR/E process, there are exceptions. Almost always something unexpected will occur during the growing season or during the relevant livestock monitoring period. To accomodate the unexpected, the team needs to:(1) agree that the unexpected is important enough to warrant monitoring, (2) monitor it and (3) analyze the results. Those steps require management decisions to be taken quickly at the team level. Once the decision to monitor an unexpected occurrance has been made, reallocation of human resources and vehicular assignments may be necessary to accomodate this decision. Team members and other researchers or extension personnel need to be aware that they do NOT have simple scopes of work, even after field-level monitorings are planned. Each person must be aware that he or she may be called upon in emergency situations to participate in monitoring some problem in another village or even region at a time which may interfere with a routine observation or activity in his or her village or region. Deciding upon those trade-offs is part of the job of the team leader, and may involve consultation with his or her superior(s) in the MOA.

b. Household Interactions

Such interactions may take place formally or informally each time the team visits the farmer's field, herd or flock. Also, such interactions may take place consistently on a routine basis during an entire year, if the team uses the farm record-book approach to continuing diagnosis. Again, in either case, the human resources and work vehicle assignments needed to accomodate either of these farm household interactions should be programmable by the team, and worked into the seasonal calendar.

Unexpected difficulties may arise in the collection of data based on farm household record-keeping. Especially at the beginning of any such effort, extra visits to each household may be necessary to assure that data is being collected and entered on the appropriate forms in a consistent manner. This does not occur as easily as one may expect. For this reason, the team leader and the social science team members need to make data classification decisions. In addition, the socio-economic team members may require more than usual mobility during the first two-three months of a farm record-keeping process.

c. Continued Diagnosis

Problems may arise during the first season's crop trials or livestock interventions which may require the gathering of more detailed diagnostic information on a systematic basis. When this occurs, teams may need to meet in a plenary session to agree upon and develop the questions to be asked, and to reach agreement on the instrument with which to ask them. Again, this activity, and the subsequent survey itself, may be unscheduled activities.

Often each local team will be too busy to address these new diagnostic questions during the first growing season. And if they are not too busy with field activites, such additional activities must be scheduled for a slack time in terms of other tasks being carried out by each local field team. Generally, such supplemental diagnostic activities must be carried out during the off-season (assuming that there is one), or during the second year. Again, these issues may require team decisions, facilitated by the team leader and agreed to by all.

8. TRIAL PRE-HARVEST ACTIVITIES

Many of these activities involve taking final monitoring observations which were agreed upon before and during the season. Again, they normally do not present unexpected management difficulties.

The most difficult and stressful management time during this sub-stage in the FSR/E process is once again coordinating harvests with farmer collaborators. Each village level team should be well aware that even with two pre-harvest visits to farmers, one to plan the day and time and the other for confirmation, between 10-20% of trials may be lost because of unexpected premature harvests. The job of the team is to manage

this activity — scheduling with farmers — so as to minimize the number to trials lost to premature harvesting in each village setting. Such management is normally up to each village team, and their rapport and mutual understanding developed with their farmer collaborators should go a long way toward minimizing this potential problem.

9. TRIAL HARVEST

Actual trial harvests should be fully-scheduled activities and hold few surprise for the FSR/E team. The team leader, in consultation with either the regional director or the experiment station manager, should arrange to have all team work vehicles checked over quickly by contracted (normal) maintenance personnel, so that preventable breakdowns can be avoided during this crucial sub-stage.

All necessary supplies needed — plastic or burlap bags for collectingsamples, tripods and field scales, sheers for sheep fleecing, pens for marking samples, grain moisture meters, etc. — should be assembled in anticipation of this activity. Again, certain items should be provided to each local team, while others may be shared. The issue of compensation of farmers in kind or in cash for samples removed must be reconfirmed with each farm household. Each member of the farm household to be involved in the harvest process must have the same understanding of the agreement with the team. This again is a function of effective communication between the team and their collaborating farmer households.

10. TRIAL AND FARM RECORD ANALYSIS

The team leader must insist that all team members meet together as soon after harvest as possible to go through the process of trial and farm record book analysis. Agronomists should not feel that the analysis of farm records can only be done by economists or other social scientists. Likewise, the social scientists should not feel that they cannot learn from and contribute to farm trial analysis. Done jointly, these activities lead to the synthesis of diagnostic and agronomic analyses, and lead to more fine-tuning of trials for next year, as well as to feedback to station-based researchers and to design of additional trials based on year one's results. In terms of management, the team leader may appoint sub-teams of two persons each to undertake these two major types of analyses. After completing their activities, each sub-group can participate in the major plenary session to define trials for next year (or season), and to fine-tune (or eliminate) the collection of supporting socio-economic data.

11. SYNTHESIS, PLANNING AND TRIAL REDESIGN

a. Synthesis

This is the final step or stage of a given year's FSR/E processes. This is one of the most critical points for joint team work. By this time, the team has been working together for nearly a year and a high level of joint productivity should be much more automatic. In terms of management, the team leader needs to assure that the trial results are synthesized with the results from the farm household records and with any additional results from tailored, follow-up surveys performed by the team during the season.

b. Planning

Once these results from vaious sources are synthesized and discussed, the team plans the following season's (or year's) activities. When these activities have been planned and agreed upon, the team is ready for trial redesign.

c. Redesign of Trials

Trial redesign should again be a joint activity, with the team leader taking the lead role in bringing each trial's discussion to closure with team agreement upon each trial's design, treatments, inputs (if any), check plot definition, and agreements to be reached with collaborating farmers concerning trial conduct and seasonal management issues. This redesign step normally concludes the team's field work for the first season (or year). The subsequent stages in the FSR/E approach are similarly managed and administered by the field-level team in concert with their immediate superiors and co-workers in research and extension.

ACTIVITY TWO TRAINERS' NOTES

DEVELOPMENT OF HOST COUNTRY ORGANIZATIONAL DIAGRAM AND DISCUSSION OF PIVOTAL LINKAGES

Summary description: The audience can include host-country participants at any administrative level, counterparts to host-country participant(if applicable). This activity is appropriate for the whole set of participants, or for small working groups. It allows trainees to develop host country organizational diagrams with an end to discussing key (pivotal) linkages in the institutioanlization of any FSR/E approach.

OBJECTIVE:

After completing this activity the participants will be able to:

1. Better understand the host country organization and pivotal linkages.

TIME: 2 hrs or 1/2 day

MATERIAL:

1. Blackboard (with white and colored chalk, eraser) or a flip-chart with marker

2. 2-4 flip-charts for sub-groups, with markers provided for each in 2 or 3 colors

3. Setting: A large working group or two (or more) smaller working groups. This depends on the wishes of the participants and the availability of facilitators and room. If more than one room is available, and/or if more than one facilitator is present, small working groups can be formed for this exercise. If small work groups are formed, the general instructions for the activity should be read to all paricipants during the initial plenary session. In addition, sufficient time should be allowed (at least one-half hour) for the small groups to reconvene in a closing plenary session to discuss their separate accomplishments and/or suggestions.

INSTRUCTIONS:

1. You may say something like the following to workshop participants:

   "Now we're going to address some of the real management and administration problems unique to _____ (name of country) as they relate to a FSR/E approach. We've found that one way to do this is to consider the organizational diagram for the (Ministry or Department) of Agriculture (whatever the official name is). Once we have developed the organizational diagram, we can begin to pin-point where management and

administration bottle-necks are likely to occur in implementing FSR/E. We should also be able to identify what the specific management and/or administration problems are likely to be at each bottle-neck. Once this has been done, we will devote some time to considering possible solutions to these problems."

2. At this point, you need to begin the exercise, using either the blackboard or the flip-chart, if participants continue to meet in the large group. If participants will be divided into smaller groups, this division needs to be made here. Each small group should have a designated facilitator (the facilitator may be one of the participants) and a designated meeting place. Each small group should also have the necessary material aids to carry out the activity (either a blackboard and chalk or a flip-chartand markers).

   All tasks to be performed by the small groups need to be clearly explained. Have the participant groups: Announce clearly and loudly when the small groups are expected to reconvene in closing plennary session.

   a. Develop organizational diagram. During the activity, the groups should quickly be able to develop the country's organizational diagram. Such a diagram should begin at least with the Minister of Agriculture and progress to the level of the farmer. It should include both research and extension administrative and technical linkages, especially if the latter are different from the former (for example, in some countries, researchers at the sub-national level are administratively responsible to the regional director (or station manager), but are technically responsible to either the director of research or the head of a particula commodity). If needed for reference, a generic organizational chart is included at the end of this exercise, and can be used as an introductory hand-out for participants. Time required: approximately one-half hour.

   b. Discuss administrative bottlenecks. To facilitate discussion of potential management and administrative bottlenecks, discussion may begin anywhere on the chart. However, it makes most logical sense to begin at the farm level, with the Minister of Agriculture, or with the FSR/E team. Time required: approximately one-half to one hour--Identify potential management and administrative bottlenecks with a different colored marker, or by putting them in parenthsis () if you are working on a blackboard. Use of colored chalk is a big help.

   c. List problems creaated by administrative bottlenecks Once several potential management and administration bottlenecks have been identified by the participants, move on to list the problems created by them. The two

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processes may not be distinct, and participants may prefer to identify management and administration problems as they identify each potential bottleneck. Be flexible and accomodate yourself to their preferences. Again, list problems in a third color of marker, or set apart with another symbol (*) or another color of chalk. Time required approximately one-half to one hour).

d. Develop potential solutions to administrative problems When participants seem to have identified most of the management and administration problems, or have begun to tire of the activity, have them begin to develop potential solutions. If sufficient time remains, ask the participants to prioritize the management and administration problems, from most to least important. Again, any list of "most to least important" management and administration problems will depend largely upon the point of view of the participants. If your group or sub-group contains mainly mid- to upper-level administrators, the list of priority problems is likely to be quite different from the priorities given them by a group largely composed of field-level practitioners. Time required: between one-half and one and one-half hours, depending on the time devoted to the total activity).
--If your participants are working in small groups, visit each with 10-15 minutes left in the small work sessions to inform them of the time.

PROCESSING:

1. Reconvene the large group plennary session. Have the participants give a summary of small (or large) group findings (time required: between 15-30 minutes).

   Begin the summary with approximately 15-30 minutes left in the session. Encourage the participants to: (1) point out the major management and administration bottlenecks, (2) identify the major management and administration problems, (3) discuss the solutions to the major management and administration problems, and (4) discuss where solutions need to be found for those problems which had none. Again, it is best to direct this summary, while either selecting participants (facilitators of small groups make a logical choice) or asking for volunteers. If time is very short, you may have to make the summary yourself.

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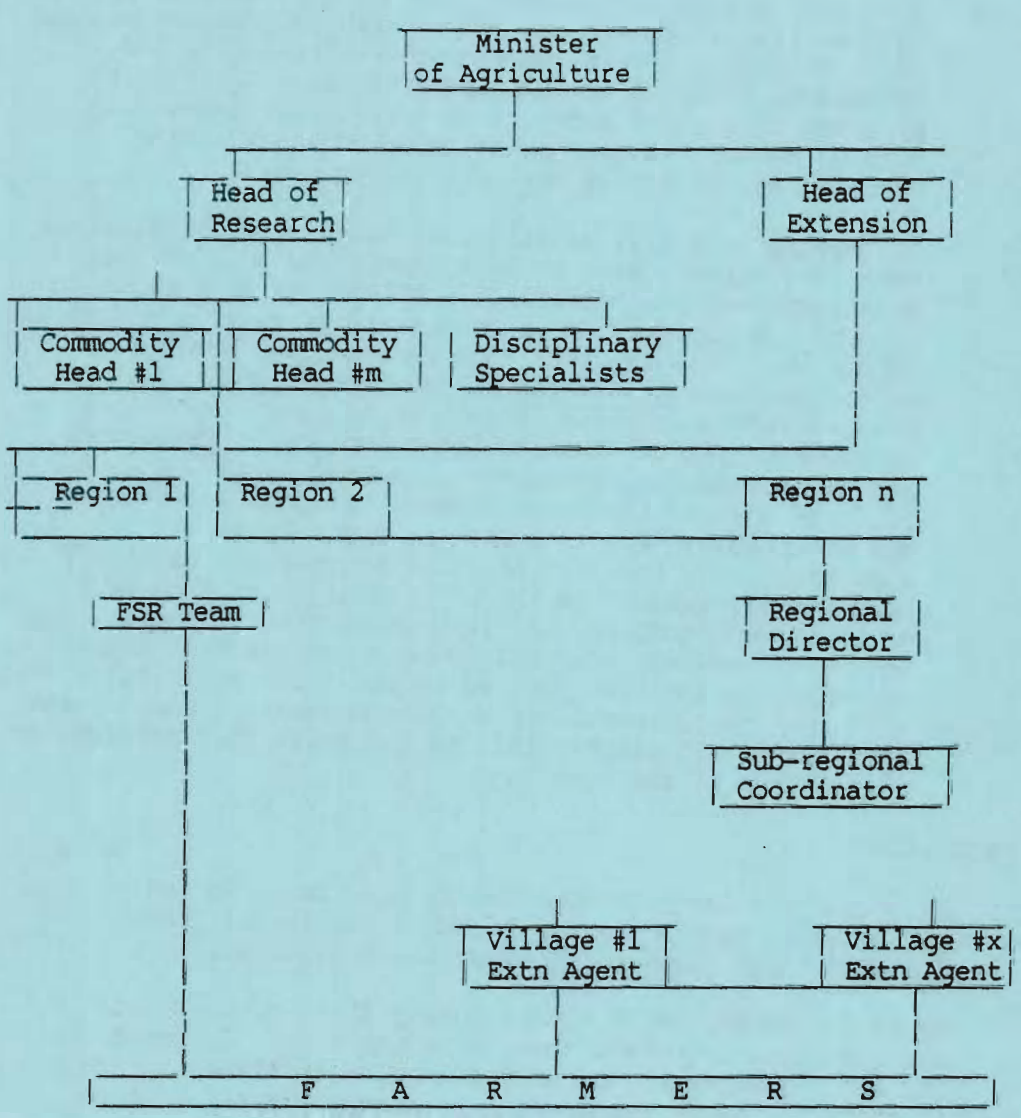
Figure VI.1

GENERIC ORGANOGRAM



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ACTIVITY THREE TRAINERS' NOTES

A POTENTIAL CONFLICT IN THE MANAGEMENT OF AN EXPERIMENT STATION

OBJECTIVES:

After completing this activity the participants will be able to:

1. Identify potential problems in communication between major actors in the management of an experiment station.

2. Develop ways to limit the development of potential problems.

3. Improve ability to resolve management problems related to implementation of the Research/Extension process.

TIME: 1 hour

MATERIALS:

1. Flip chart
2. Role descriptions to be given to individual actors.
3. Map of experiment station to be displayed during role play.

INSTRUCTIONS:

1. Before breaking from the previous session, five volunteers should be recruited from the participants.

2. Each volunteer should be given a description of his role (stated below) along with a map and introduction to the role play. Ideally this will be done during coffee break so that the players will be prepared at the beginning of the next session. It is imperative that none of the players communicate with each other before the role play.

3. Before introducing the role play, the trainer give a short introduction of the Agricultural Experiment Stations being sure to cover the following key points: (5-10 min)
   a. Experimental Stations are technical support units of science based operations.

   b. Experiment stations are useful as:

      1. nuclei of work sites, so that experimental work can be located near facilities such as storage, warehousing for equipment and supplies laboratories.

      2. centers of work in specific ecological zones.

      3. centers so that work of different kinds can be grouped for convenience of administration and for use of facilities.

      4. as sites where conditions can be controlled in order

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to reduce variation that affects expeimental results.

5. sites to maintain collections of biological materials used in plant breeding.

c. Experiment stations are costly to establish and maintain.

d. They are designed for use by several discipline and commodity groups.

e. They are designed to serve different groups with diverse needs.

f. They serve both adiministrative and technical functions

g. They are usually managed by a Station Director responsible to a person at a higher hierarchical level thatn the programs which they serve.

h. The position for the station director may be rated higher, the same or lower than the heads of programs.

i. Conflict is common between station directors and heads of programs.

4. The facilitator should introduce the role play by referring directly to the objectives and instructions to the trainees. This is important in order to set some direction, or frame work for the follow up discussion of the activity.

5. Role play should begin by the director of the experiment station inquiring as to the problems encountered with the proposed layout of experimental plans. The players are to maintain their roles as indicated on their role description as much as possible. The objective of the meeting is for the director of the experiment station to come up with a mutually agreeable distribution of experimental plots (as much as possible). (10 min)

PROCESSING:

The facilitator should follow-up the role play with open discussion including all session participants and observers. Be sure to cover at least the lead questions laid out in the trainer instructions. Determine a)problems and b)possible solutions.(40 min)

Possible solutions to potential problems should include at least:

1. Keep station managers informed, and do it ahead of time.

2. Station Director should take initiative to get the

information he/she needs with sufficient lead time. One solution might be for the Station Director to chair a meeting at the appropriate time of year with the participation of Heads of Programs, for the assignment of plots at the experiment station. If such a meeting, chaired by the Station Director, is not likely to solve the conflict, the Director of Research, who is the boss of both the Station Director and the Heads of Programs, might chair a meeting to assign the plots.

3. Setting up some administrative rules to inform the interested parties as to how decisions will be made.

4. Developing a management system that gives rapid recourse to higher management levels for settling differences of opinion and coming to decisions before they expand into major conflictive ones.

7. The follow-up discussion should lead to "real-life" examples of problems and solutions in the participant work experience.

Instructions for role play participants appear on the following pages.

YOU ARE THE HEAD OF THE NATIONAL BEAN PROGRAM

The following is a map of the proposed plan for distribution of experimental plots of land. As head of a national commodity research group, you are interested in obtaining the best possible plot for your research. Ideally that land would be uniform, accessible, secure, level, and have access to water. However, each of you has a specific set of priorities that may differ from the rest.

Problem plots include:

5,6 isolated, poor shape, far from experimental station headquarters and not secure. Neighbors have been known to harvest ears of corn form these plots.
1,2,3,4,5,11,12 lack of irrigated water (rainfed only)
2,3,4 lack uniformity, have slight slope

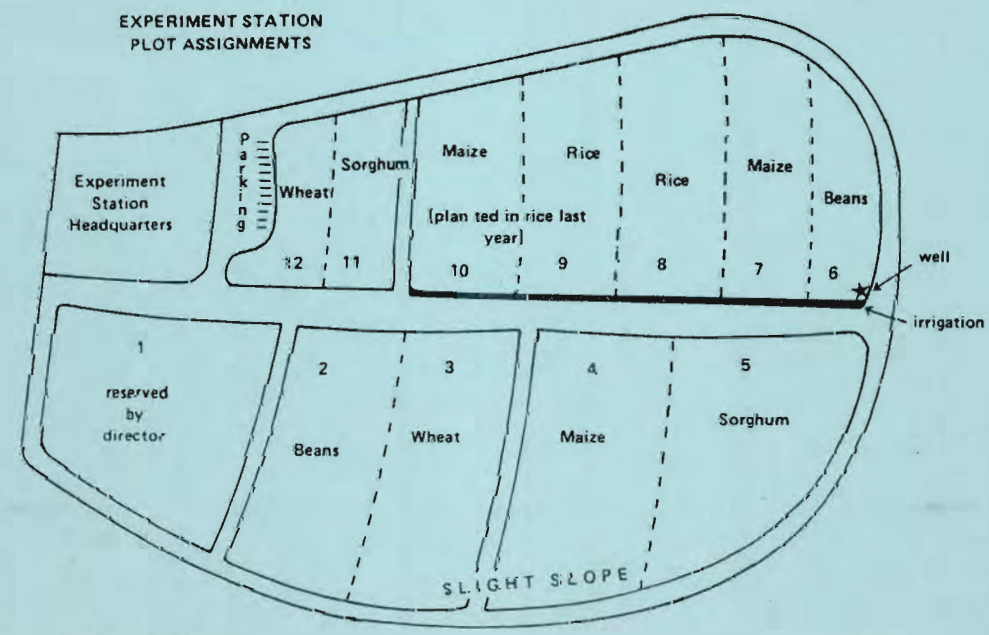
Preferred plots include:
7,8,9,10 level, water, uniform, accesible, secure

YOU ARE THE HEAD OF THE NATIONAL BEAN PROGRAM

1. Your major concern is testing intercropping of beans and corn.

2. You're a cranky, disagreeable sort and feel that, although it is not critical, you want a plot on the north side of the road.

3. You know that your plots should not be adjacent to experimental maize plots because of cross-pollination.



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YOU ARE THE HEAD OF THE NATIONAL RICE PROGRAM

The following is a map of the proposed plan for distribution of experimental plots of land. As head of a national commodity research group, you are interested in obtaining the best possible plot for your research. Ideally that land would be uniform, accessible, secure, level, and have access to water. However, each of you has a specific set of priorities that may differ from the rest.

Problem plots include:

5,6 isolated, poor shape, far from experimental station headquarters and not secure. Neighbors have been known to harvest ears of corn from these plots.
1,2,3,4,5,11,12 lack of irrigated water (rainfed only)
2,3,4 lack uniforminty, have slight slope

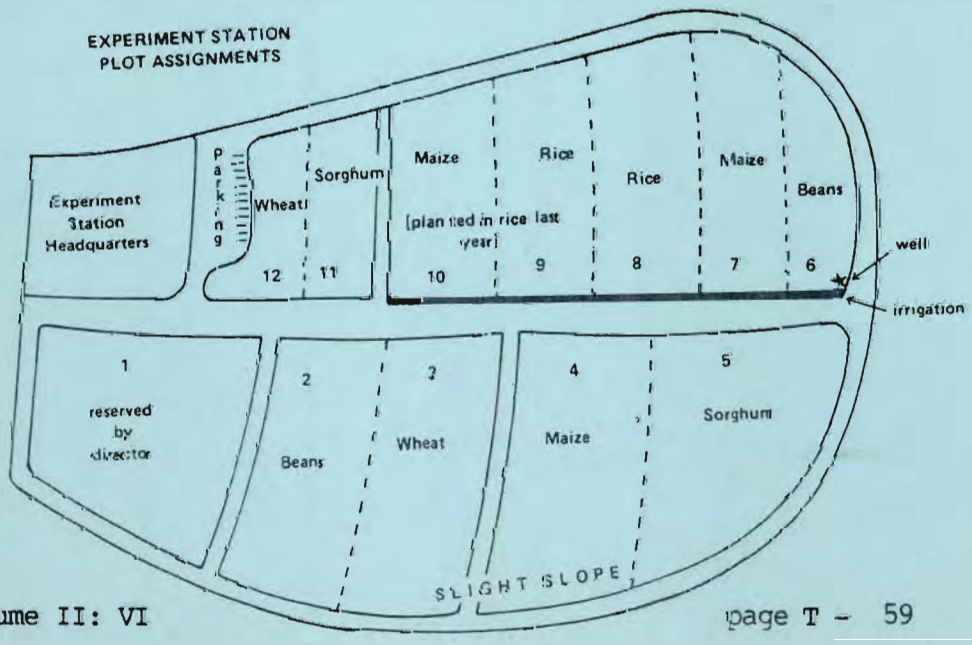
Preferred plots include:
7,8,9,10 level, water, uniform, accesible, secure

YOU ARE THE HEAD OF THE NATIONAL RICE PROGRAM

1. You must have two of the following three plots (6,7,8) because they fulfill your critical needs of water, level land, and no voluntary rice.

2. You cannot use the same plots that were planted in rice last year (9,10) and refuse to do so.

3. As a last resort argument, you note that government policy priorities emphasize rice and there is a large popular support from the rice farmers in this region. If not satisfied, you will go to the minister of Agriculture (who, by the way is your brother-law).



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YOU ARE THE HEAD OF THE NATIONAL MAIZE PROGRAM

The following is a map of the proposed plan for distribution of experimental plots of land. As head of a national commodity research group, you are interested in obtaining the best possible plot for your research. Ideally that land would be uniform, accessible, secure, level, and have access to water. However, each of you has a specific set of priorities that may differ from the rest.

Problem plots include:

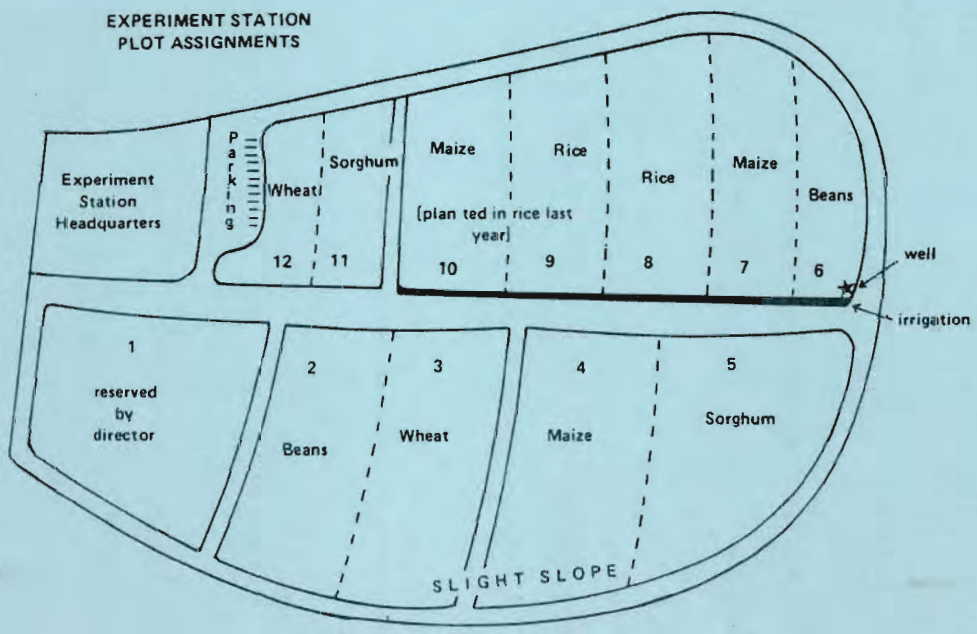
5,6 isolated, poor shape, far from experimental station headquarters and not secure. Neighbors have been known to harvest ears of corn form these plots.
1,2,3,4,5,11,12 lack of irrigated water (rainfed only)
2,3,4 lack uniforminty, have slight slope

Preferred plots include:
7,8,9,10 level, water, uniform, accesible, secure

YOU ARE THE HEAD OF THE NATIONAL MAIZE PROGRAM

1. You must obtain plots that are separated from adjacent corn by at least the road or another field. This especially refers to the corn in the bean fields.

2. You absolutely refuse to be located next to a bean field.



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YOU ARE THE HEAD OF THE NATIONAL SORGHUM PROGRAM

The following is a map of the proposed plan for distribution of experimental plots of land. As head of a national commodity research group, you are interested in obtaining the best possible plot for your research. Ideally that land would be uniform, accessible, secure, level, and have access to water. However, each of you has a specific set of priorities that may differ from the rest.

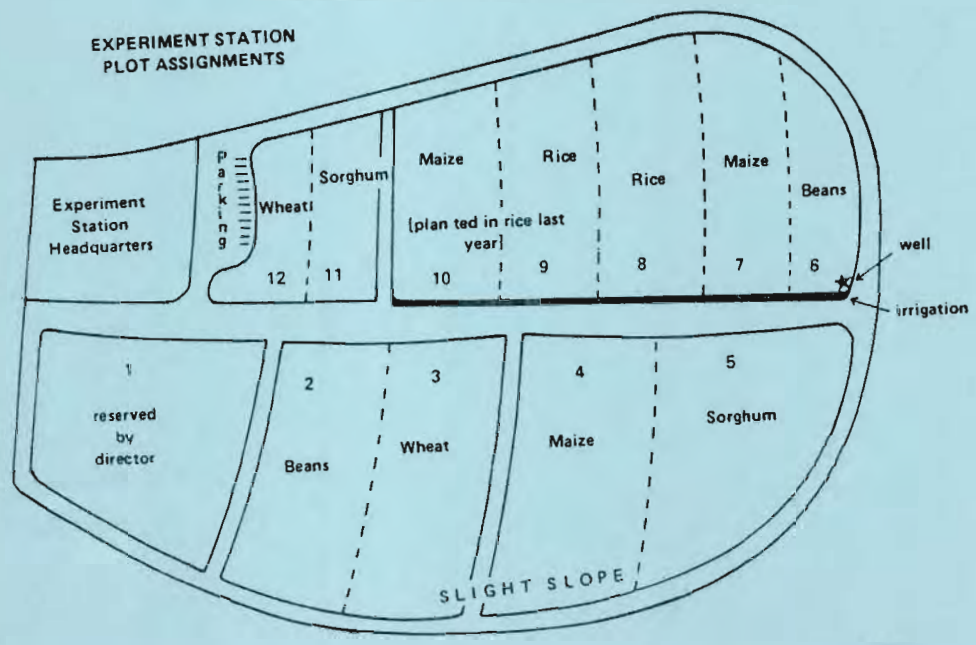
Problem plots include:

5,6 isolated, poor shape, far from experimental station headquarters and not secure. Neighbors have been known to harvest ears of corn form these plots.
1,2,3,4,5,11,12 lack of irrigated water (rainfed only)
2,3,4 lack uniforminty, have slight slope

Preferred plots include:
7,8,9,10 level, water, uniform, accesible, secure

YOU ARE THE HEAD OF THE NATIONAL SORGHUM PROGRAM

1. You have no critical priorities in plot location but feel that you should have equal crack at the "best" spots. However, you are amenable to any changes, although you don't offer any suggestions and are not overly enthusiatic about the changes suggested.



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YOU ARE THE DIRECTOR OF THE EXPERIMENT STATION

The following is a map of the proposed plan for distribution of experimental plots of land. As head of a national commodity research group, you are interested in obtaining the best possible plot for your research. Ideally that land would be uniform, accessible, secure, level, and have access to water. However, each of you has a specific set of priorities that may differ from the rest.

Problem plots include:

5,6 isolated, poor shape, far from experimental station headquarters and not secure. Neighbors have been known to harvest ears of corn form these plots.
1,2,3,4,5,11,12 lack of irrigated water (rainfed only)
2,3,4 lack uniforminty, have slight slope

Preferred plots include:
7,8,9,10 level, water, uniform, accesible, secure

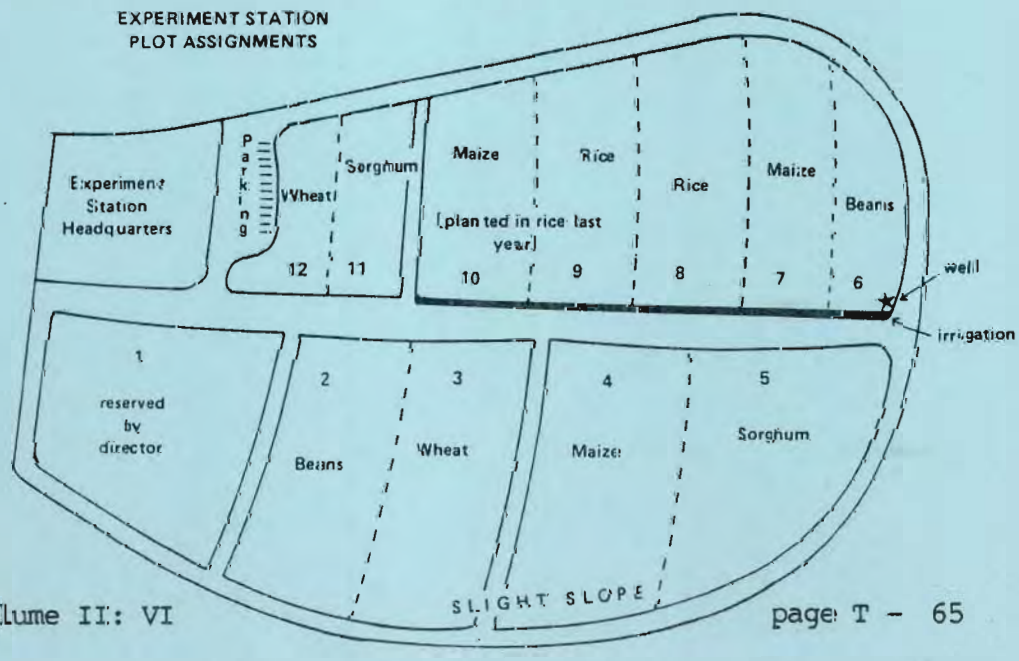
YOU ARE THE DIRECTOR OF THE EXPERIMENT STATION

1. It is your first year on the job and you are anxious to please everyone.

2. Your boss , the director, previously worked in the maize program and favors that program. He told you to be extra responsive to the maize program requests.

3. You argue that rice should not get the best plots, because they had the best plots last year.

4. You are willing to give up part of the reserved plot (1), but do not initiate the offer.



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ACTIVITY THREE TRAINEE INSTRUCTIONS

A POTENTIAL CONFLICT IN THE MANAGEMENT OF AN EXPERIMENT STATION

OBJECTIVES:

After completing this activity you will be able to:

1. Identify potential problems in communication between major actors in the management of an experiment station.

2. Develop ways to limit the development of potential problems.

3. Improve ability to resolve management problems related to implementation of the Research/Extension process.

MATERIALS:

1. Flip chart
2. Role descriptions to be given to individual actors.
3. Map of experiment station to be displayed during role play.

INSTRUCTIONS:

Agricultural Experiment Stations are essentially no more than technical support units of science based operations just as libraries, storage facilities, and warehuses are support units. Of course the utility of such units is derived from widley varying functions.

Experiment stations are useful as:

1. nuclei of work sites, so that experimental work can be located near facilities such as storage, warehousing for equipment and supplies laboratories.

2. centers of work in specific ecological zones.

3. centers so that work of different kinds can be grouped for convenience of administration and for use of facilities.

4. as sites where conditions can be controlled in order to reduce variation that affects expeimental results.

5. sites to maintain collections of biological materials used in plant breeding.

Since experiment stations are costly to establish, and even more costly to maintain, they are usually designed for use by several discipline and commodity groups. Also, since thety are designed to server both administrative and technical functions, many times of a broad scope, they usually are managed by a Station Director responsible to a person at a higherr

hierarchical level than the program which they serve. The position of the station director may be rated higner or lower than the heads of programs. Conflict is common between station directors and heads of programs.

Background for role play exercise:

The director of the experiment station has determined the location of the experimental plots for each of the national commodity groups. He has sent a copy of the map showing the location of the experimental plots to each of the heads of the national commodity programs for their approval. The heads of the programs are not pleased with their assignments and wish to discuss possible changes with the director of the experiment station.

You are to observe the meeting of the director and the heads of the national commodity programs. The objective of the meeting is for the director of the experiment station to come up with a mutually agreeable distribution of experimental plots (as much as possible). As this meeting is taking place you should try to note problems that materialize that would be typical in such a situation. What are some of the main concerns? How did each of the persons involved deal with the problem? What are some of the ways that this type of confrontation could be avoided? What did the individuals do right? What did they do wrong?

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GLOSSARY

GLOSSARY

"ADD-ON" TRIAL: A 2n trial in which the higher level of each factor is added one factor at a time, giving one treatment combination at all lower levels and a series of treatment combinations each with at least one factor at the higher level.

AGRICULTURAL PRODUCTION SYSTEM: See production system

ALIASES: Two or more different effects (factors or combinations of factors) that a fractional replication experiment cannot distinguish between.

ANOVA (ANALYSIS OF VARIANCE): A statistical method for comparing variance in responses among treatments with variance reflecting natural variation in fields, and assessing the probability that the differences among treatments are due to the effects of the treatments themselves, rather than to natural variation.

ARCSINE TRANSFORMATION: Also known as angular transformation, may be used on data which may not be consistent with assumptions for the analysis of variance. Data in this category include count data expressed as percentages or proportions of total sample.

AVERAGE FARMER CONTROL: A practice used as a treatment, which is an average rate or a composite technique of the actual rates or techniques used by individual farmers in a given domain.

BALANCED LATTICE: A type of incomplete block design in which the number of treatments is an (exact) square (16, 25, etc.), the number of plots per block (block size) is equal to the square root of the number of treatments (4, 5, etc.), and the number of replications of each treatment is one more than the block size.

BLOCK: A group of plots similar in certain characteristics (e.g., soil type, fertility, plant stand, etc.) prior to the application of the treatments.

BORDER (ROWS): Rows on the edge of the treatment plot which are not harvested because they may be influenced by the treatment in the adjacent plot.

CAUSAL AGENT: Organism responsible for a given condition.

CENTRAL COMPOSITE: A method of incomplete factorials for fertilizer levels.

CLUSTER: See primary sampling units.

COEFFICIENT OF VARIATION: A measure of reliability of any experiment.

COMBINED ANALYSIS: A type of analysis of variance for data from more than one location and/or year.

COMPLETE BLOCKS: Every block has all treatments in the experiment, so the number of replications equals the number of blocks.

COMPLETE FACTORIALS: Experiments which replicate all combinations of levels, in either complete or incomplete blocks.

COMPLETELY FACTORIAL ARRANGEMENT: See complete factorials

CONFIDENCE INTERVALS: The range between a minimum value and a maximum value, between which the probability of occurrence of the true mean value is estimated by statistics.

CONFOUNDING: Using incomplete blocks in a factorial experiment.

CONTIGUOUS PLOTS (OR REPLICATES): Normally, replicate 1 is next to replicate 2 is next to replicate 3, etc., and plot 1 in rep 1 is next to plot 2 in rep 1 is next to plot 3 in rep 1, etc. Such an arrangement is not mandatory under farmer's conditions, leading to non-contiguous plots (or replicates).

CONTINUOUS DATA: Data on which the data points can be any value, and any number of intermediate values are possible (e.g., yield in kg/plot: between the value 1 kg/ha and the value 2 kg/ha, intermediate values such as 1.25, 1.5, etc., hg/ha are possible).

CONTROL TREATMENT: A standard or baseline treatment, against which the others are to be compared. Also called check treatment.

CORRELATION: The tendency of two variables to be related; the correlation coefficient (r) measures the closeness of the relationship.

CORRELATION COEFFICIENT: A measure of the degree of association between two variables; does not indicate direction of cause-and-effect.

COVARIANCE ANALYSIS: An extension of analysis of variance in which treatment means of the variable of interest are adjusted to values they would have if there were no variation in the values of a second variable, called the covariate.

CRD: Completely randomized design. The research team randomizes treatments over all plots, and there are no blocks.

CROPPING PATTERN: The arrangement of crops on a given field over a given period of time (usually 12 months).

DEGREES OF FREEDOM (DF): The number of comparisons possible; always one less than the number of blocks, treatments, plots,

etc.; the divisors for each sum of squares in an analysis of variance.

DIFFUSION DOMAINS: Interpersonal communication networks through which newly acquired knowledge of agricultural technologies naturally flow. From farmer to farmer, neighbor to neighbor, store operator to patron, information about new ideas moves through a farming community. Awareness of a new technology being verified in on-farm trials, and of its response under local conditions, takes place among farmers and families not directly involved in the on-farm research.

DISCRETE DATA: Data in which the data points are whole numbers, and intermediate values are not possible (e.g., number of pods on a sample plant: between the value 1 pod and the value 2 pods, there cannot be intermediate values such as 1.25, 1.5 etc., pods).

DOMAIN: Used to describe any of four terms based upon relative homogeneity: (a) homogeneous agroecological zone, (b) research domain, or (c) recommendation domain, or (d) diffusion domain. Homogeneous groupings of farm households into domains may be based upon similarities of (a) cropping patterns and/or systems, (b) animal systems, (c) crop-animal (or animal-crop) systems, (d) land size and tenure, (e) access to input and/or product markets, (f) political/administrative boundaries, (g) irrigation districts, (h) watershed basins, or other socio-cultural criteria (religion, caste, etc.).

EFFECT: The average difference in measured responses to two or more levels of a factor.

ELEMENTARY SAMPLING UNIT: The actual sampling units on which observations or measurements are made and data collected.

ENVIRONMENT: Biophysical and management conditions on a farm which affect the response to treatments.

ENVIRONMENTAL MEAN SQUARE: From the analysis of variance, that value which serves as the denominator in F-ratios to determine significance or source of variation.

ENVIRONMENTATL SETTING INFORMATION: Information on the physical and biological environment of the farm and trial field.

ERROR: See Residual

EX ANTE ANALYSIS: Analysis of the expected biological, economic and/or social benefits of trial and/or treatment choices prior to conducting the trials, in order to make decisions about trial type and treatments.

EXPERIMENTAL DESIGN: How to arrange treatments in fields and among farms so that researchers can analyze the differences in

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responses among treatments using statistical methods.

EXPERIMENTAL UNIT: A plot assigned a unique treatment or treatment combination.

EXPLICIT COST: Direct out of pocket expenses that one pays for the use of a resource.

EXPLORATORY TRIALS (TESTING): Trails which test a large number of variables, in order to identify which variables are more likely to contribute to a solution to the researchable priority. These trials come before refinement trials.

F-TEST (F-RATIO): The ratio of one variance (or MS) to the error variance (or MS); this ratio is compared with tabulated ratios to determine significance. Also called the variance ratio.

FACTOR: A group of related treatments.

FACTORIAL: Refers to manner in which treatments are assembled. A complete factorial experiment includes all possible combinations of the selected levels in two or more factors. Analysis of variance includes interaction term(s). Not a design.

FACTORIAL ARRAY: A set of treatment combinations in a completely factorial arrangement.

FACTORIAL EXPERIMENTS: Experiments which place combinations of levels of 2 or more factors on each plot.

2^n FACTORIAL EXPERIMENTS: Experiments which compare n factors, where each factor has only two levels.

FARMER ENVIRONMENT: The physical, biological, economical and socio-cultural conditions under which a farmer operates his or her farming system.

FARMER FEEDBACK: The assessment by the farmer of technology or methodology being tested or demonstrated by extension or research.

FARMING SYSTEM: Most of these practitioners agree with Shaner et al. that the "farming system" is a: reasonably stable arrangement of farming enterprises that the household manages according to well-defined practices in response to the physical, biological and socioeconomic environments and in accordance with the household's goals, preferences and resources. These factors combine to influence output and production methods. The farming system is part of larger systems—e.g. cropping {or livestock} systems (1982: 16).

FIELD BOOK: A notebook dedicated to the systematic recording of field plot data for farm trials in a given domain. A field book always includes plot and field maps for each farm with each trial

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type.

FRACTIONAL REPLICATION: Experiments which compare only certain combinations of levels of factors, so that not all combinations occur but each level represented in the combinations is replicated.

GUARD (ROWS): See border rows.

HARVESTABLE PLOT: That portion of a plot which will be harvested; usually the plot minus the border (or guard) rows or hills.

HOUSEHOLD: Households represent one system of resource allocation, and are composed of individuals, often related through kinship, most of whom live most of the time in close proximity. Household members share some goals, benefits and resources; are independent on some; and in conflict on others.

IMPLICIT COST: The value of a resource owned by a family and used in the farm business. There may or may not be a market value for such resources.

INCOMPLETE BLOCKS: Each block does not have all treatments.

INCOMPLETE FACTORIALS: Experiments with only selected combinations of levels.

INDIVIDUAL FARMER CONTROL: The practice of an individual farmer which may be used as one type of control in an experiment.

INTER-HOUSEHOLD: Refers to interactions and relationships between two or more households and their members.

INTERACTION: a) A change in the response to levels of one factor depending on the levels of another factor, b) A change in the response to treatments depending on location or year.

INTERACTION EFFECTS: A significant interaction occurs when the response at one factor (input) is dependent upon the presence (level) of one or more different factors.

INTERCROPPING: Growing two or more simultaneously in the same plot in different, but (proximate) stands. In this system, one crop system is part of the other crop's environment.

INTERVENTION: A type of "treatment": a technological or management change from the "average" farmer practice in a given domain, designed to solve a production problem.

INTRA-HOUSEHOLD: Refers to interactions and relationships among members of the same household.

LATIN SQUARE: A type of experimental design which controls for

natural variation in two directions.

LEAST SIGNIFICANT DIFFERENCE (LSD): The smallest difference between any two means that can be accepted as statistically significant.

LEVEL: The individual treatments of a factor.

LINEAR REGRESSION: The relationship between two variables expressed as an equation relating a dependent variable to an independent variable. Graphically, the relationship is a straight line.

LOGARITHMIC TRANSFORMATION: Used on certain data which do not conform to assumptions made in analysis of variance. Used on data whose standard deviations are preportional to means.

MAIN PLOT: A large plot to which a level of one factor is applied, and which is subdivided into small plots, called sub-plots.

MAIN PLOT ERROR: Refers to unexplained variation in the analysis of variance for main (large) plots of a split plot experimental design.

MANAGEMENT LEVEL: The level of non-varying or basal factors applied uniformly to an experiment

MEAN: The mean, or the sample mean (also called the mean of the sample) is the arithmatic average of a given sample. The mean is calculated by dividing the sum of the observations by the number of observations.

MEAN SQUARE (MS): An estimate of the variance; the sum of squares divided by the degrees of freedom.

MODIFIED STABILITY ANALYSIS: A type of regression analysis in which several individual treatments at each location are used as the values of an independent variable called the environmental index.

MONOCULTURE: (a) (agronomic definition) growing only one crop on the same piece of land in one year; (b) (economic definition) growing only one crop repetitively on the same piece of land or the same farm.

MULTIPLE RANGE TEST (MRT): Any of several techniques for determining statistically significant differences among a large number of treatment means.

MULTISTAGE (CLUSTER) RANDOM SAMPLING: Sampling in which the underlying population is divided into primary sampling units, each which in turn consists of the elementary sampling units of the population. Random sampling is first done from the primary

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sampling units, and then either data is taken from all the elementary sampling units of the selected primary sampling units, or separate random sampling of elementary sampling units is done within each selected primary sampling unit. The procedure can be extended to any number of stages. The primary sampling units are analagous to main plots and the elementary sampling units to sub-plots in field experiments.

NATURAL VARIABILITY: Variation which occurs when identical treatments are replicated; also known as unexplained variation.

NEGATIVE IMPACT: Increased costs and/or reduced returns resulting from an alternative technology or intervention.

NON-EXPERIMENTAL VARIABLES: See non-varying factors.

NON-VARYING FACTORS: Those factors that do not vary over the experiment (see management level).

NORMAL DISTRIBUTION: Any distribution of outcomes which center about a mean, and have a variance that describes the chance of occurrence of outcomes different from the mean, such that the chance of their occurrence decreases as their differences from the mean increases; used as the basis for statistical estimates of variability.

OPPORTUNITY COST: The value of a resource in its best alternative use.

OUTLIER DATA: Data points that are exceptionally larger or smaller than the bulk of the data points, or which lie removed from the remaining data points in a simple regression scatter diagram. These are first examined for possible errors before analyses are performed.

PARTIAL BUDGETING: A method of economic analysis which compares changes in variable costs and returns to assess the economic benefit of treatment differences.

PLOT: The name given to the physical location of each treatment or treatment combination in a given trial: any one of the smallest whole experimental units of a given trial.

PLOT MAPS: These maps (a) orient the researchers and site visitors to each trial and (b) include all treatments, along with appropriate keys, to prevent treatment mix-ups in the field during planting, the season, and at harvest time.

POSITIVE IMPACT: Increased returns and/or reduced costs resulting from an alternative technology or intervention.

PRIMARY EXPERIMENTAL DATA: Measurements of variables necessary to distinguish treatment responses.

PRIMARY SAMPLING UNITS (PSU): Groups of elementary sampling units, that are sampled first in multistage random sampling. Also called clusters, especially if the grouping is done on a geographical or area basis.

PROBABILITY SAMPLING: Sampling in which each selected sampling unit has a known chance of being selected from the underlying population. Probability sampling includes simple random sampling, stratified random sampling, and multistage (or cluster) random sampling. All types of probability sampling involve drawing one or more random samples.

PROBE: A treatment at a higher rate than practical, or of greater risk than normally assumed by farm households, included in a trial for purposes of comparison, generation of response curves, and exploration of potential responses.

PRODUCTION PROBLEM: A condition or situation created by a group of factors that limit the growth and/or productivity of plants or animals.

PRODUCTION SYSTEM: Abbreviation for agricultural production system. How farming units use land to grow the same type of crops and/or raise the same type of animals. The main type of crop or animal defines the production system.

RANDOM SAMPLING: (a) Sampling in which each sampling unit has an equal chance of being selected from a defined subset of the population, such as a stratum in statified random sampling or a primary sampling unit in multistage random sampling. (b) Abbreviation for simple random sampling.

RANDOMIZATION: The allocation of treatments to plots by a random process to avoid subjective bias.

RCBD: Randomized complete block design. The research team randomizes treatments within blocks.

RECOMMENDATION DOMAIN: Two definitions, one narrow and one broad one given here: (a) (broad definition) The phase "recommendation domain" was originaly used in the first CIMMYT Economics Manual (Period et al, 1976) "A group of farmers within an agro-climatic zone whose farm are sufficiently similar and who follow sufficiently similar practices that a given recommendation is applicable to the entire group. (b) (narrow definition) (Wotowiec, Poats, Hildebrand, 1986) A recommendation domain is a group of farmers (or farmers and their fields) with a common problem for which a tested solution meets their (the farm decision-makers') biophysical and socioeconomic requirements for adoption. Recommendation domains can be based not only upon farm households, but also upon their separate fields which are not contiguous, but widely dispersed in location and altitude. Each household might fall into several recommendation domains depending upon; (1) where, along the agroecological gradient of

the mountainside, their fields are located; (2) the climate-related crop management decisions made for each of those fields; and, (3) the particular problem solutions to be tested: the broad definition covers both "researchable domain" and the narrow definition of "recommendation domain."

REFINEMENT TRIALS (TESTING): Trials which test a limited number of variables that have been identified as likely to contribute to a solution to the researchable priority, in order to determine the best level of each variable, or the best combination of variables for a potential solution. These trials follow exploratory and proceed validation trials.

REGRESSION: A statistical method for assessing the relationship between one or more independent variables and a dependent variable.

REGRESSION ANALYSIS: A statistical method for assessing the relationship between one or more independent variables and a dependent variable.

RELAY (INTERCROPPING): Growing two or more crops in sequence, seeding or transplanting the succeeding one some weeks before the harvest of the preceding crop.

REP: Abbreviation for replication.

REPLICATION: The repeating of treatments in more than one plot.

REPRESENTATIVE SAMPLE : A sample which is so selected, and which is large enough, to provide a true representation of the underlying population from which it is taken.

RESEARCH CONTROL: A practice used as a treatment, which is unrealistic for farmers, but which can provide an estimate of response.

RESEARCH DOMAIN: A research domain is a problem-focused environmental (agroecological and socioeconomic) range throughout which it is expected that hypothesized solutions to a defined problem could have potential applicability and therefore should be tested. Research domains are determined during the initiation of research activities, largely by consideration of biophysical (agroecological) factors, with some attention to socioeconomic and gender issues. Research domains are comprised of one or more agro-socioeconomic recommendation domains, which are tentatively defined based upon the response of a specific technology to the actual agro-socioeconomic conditions found on farms.

RESEARCH PRIORITY: A production problem which the multidisciplinary team identifies in diagnosis as a priority for design and testing of an intervention.

RESEARCHER PLANNED EXPERIMENTATION: Trial, or sets of trials,

designed by researchers and/or extension personnel.

RESIDUAL (ERROR): A statistical measure of random variability not accounted for by treatments, blocks, or other sources of variation imposed or accounted for by researchers.

RESPONSE: What the multidisciplinary team observes or measures on the crop or crops after the team applies a treatment or treatments.

RESPONSE CURVE: A curve which shows the relationship of one variable (for example, yield) as a continuous function of another variable (for example, N rate); the curve is generated by regression analysis for a limited number of actual data points.

ROTATION: A sequence of crops grown one after another, with the sequence repeated over several years (corn-beans-corn-beans-...); often done with the objective of improving or maintaining soil condition.

SAMPLE: (a) A set of measurements which constitute part of a population; (b) A small collection from some larger aggregate about which we wish information.

SAMPLING UNIT: The object of observation, measurement, or data collection. Examples include pods on a plant, plants in a field, rows in a field, plots in field, fields in a domain, farm households in a domain, etc.

SECONDARY EXPERIMENTAL DATA: Measurements of variables that may be useful in the interpretation of treatment responses and/or differences among fields and farms as they affect treatment responses.

SENSITIVITY ANALYSIS: Partial budgeting done using input and/or product prices different from those actually observed during the experiment, but which have occurred in the past or could be expected to occur at given probabilities in the future.

SIGNIFICANCE LEVELS: The probability that an observed difference will be declared to be due to the treatments when it is actually due to random variation, expressed as a percentage. The percentage. (100 minus the significance level), is the probability that the observed difference is due to the treatments.

SIMPLE RANDOM SAMPLING: Sampling in which all selected sampling units are drawn from the entire underlying population, with each sampling unit having the same chance of being selected as any other sampling unit.

SINGLE DEGREE OF FREEDOM COMPARISONS: An extension of analysis of variance which tests hypothesized relationships among treatments.

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SPECIFICATIONS: The detailed description of the proposed treatments and the intended non-varying factors.

SPLIT-PLOT ARRANGEMENT: Experiments which place levels of one factor in large plots (called main plots), which are sub-divided into small plots (called sub-plots), onto which levels of a second factor are placed. Typically, each main plot thus contains all the levels of the second factor.

SPLIT-PLOT EXPERIMENTS: Experiments in which levels of one factor are randomized among sub-plots of larger plots of levels of another factor.

STATISTICS: The science of the systematic collection, organization, and mathematical analysis of quantifiable data so as to present descripture information about data to induce characteristic of a larger population of which the data is construed as representative or to infer the significance of underlying factors whose effects are reflected in the data.

STRATIFIED MULTISTAGE RANDOM SAMPLING: Multistage random sampling in which the primary sampling units, the elementary sampling units, or both, are first divided into 2 or more strata, and random sampling is done within each stratum. This is analogous to nesting in field experiments.

STRATIFIED RANDOM SAMPLING: Sampling in which the underlying population is divided into to 2 or more strata, and random sampling is done independently within each stratum. The strata are analagous to blocks in field experiments.

STRATUM (PLURAL=STRATA): A subdivision of a population of sampling units from which a random sample of units is taken. The strata cannot overlap but together must include the entire underlying population of sampling units, primary sampling units, or elementary sampling units of primary sampling units.

SUB-PLOT: A small plot to which 1 level of a factor is assigned; several sub-plots are contained in each main plot.

SUB-PLOT ERROR: Unexplained variation in the sub-plots, within the main plot, of a split plot experimental design.

SUM OF SQUARES: Any of several types of mathematical values calculated by summing squared values from individual plots, treatment totals, block totals, and/or other totals, depending on the design used.

SUPERIMPOSED TREATMENTS: Treatments added to fields already planted by farmers.

SUPERIMPOSED TRIALS: Trials in which researchers add treatments (such as N sidedressing, pesticide applications, etc) to fields already planted by farmers.

"T" TEST: A method for comparing two means; it is often the second step following an analysis of variance

"TAKE-OFF" TRIAL: A 2^n factorial trial in which the higher level of each factor is removed one factor at a time, giving one treatment combination at all higher levels, and a series of treatment combinations each with at least one factor at the lower level.

TECHPACK: Abbreviation for technological package: a combination of superior practices for several different variable factors.

TRANSFORMATIONS: A mathematical operation, e.g., taking the square root, that is applied uniformly to every value in a data set, in order to convert the data set to a modified data set with properties (such as equality of variances of treatment means) that better fit the assumptions that must be met to use statistical analysis of treatment differences.

TREATMENT: What researchers do to one area of the crop or crops, in order to compare with other areas to which they do something different or nothing at all. Treatments can include interventions, researcher controls, average farmer controls, and individual farmer controls.

TREATMENT ARRAY: The set or subset of treatments selected from the total set of possible treatments; the first step in designing an experiment.

VALIDATION TRIALS (TESTING): Trials which test one or at most two potential solutions against individual farmer practices in order to determine the acceptability of the solution by farmer households. These trials follow both exploratory and refinement trials.

VARIABILITY: Differences in responses of treatments. May be partitioned (by correct experimental design) into one or more of the following: treatment, block (replication), interaction error.

VARIABLE (FACTOR): An experimental treatment, or a component of an experimental treatment.

VARIANCE: A mathematical measure of variability estimated as the mean square (MS).

VARIANCE RATIO: The ratio of one variance (or MS) to the error variance (or MS); this ratio is compared with tabulated ratios to determine significance. Also called the F-test or F-ratio.

XENIA: Referring to the situation in which the genotype of the pollen influences the developing embryo or the maternal tissue of the fruit so as to produce a phenotypically demonstrable effect upon the seed.

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