PN-ABA-941
58160

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PNABA-991
58160

ICRAF WORKING PAPER No.

52

# AN ECOLOGICAL APPROACH TO ON-FARM EXPERIMENTATION

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May 1988

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# AN ECOLOGICAL APPROACH TO ON-FARM EXPERIMENTATION*

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ABSTRACT

The need to obtain more bio-physical agroforestry data from on-farm situations is emphasized. Two methods are discussed: studies of single, established multipurpose trees and on-farm experimentation. Some of the complexities of the former are described and briefly illustrated. A new approach to on-farm experimentation is proposed which involves randomly sampling natural conditions on-farm which are selected to be relevant and useful comparisons ("ecological" treatments), and/or with added manipulations ("interference" treatments). The advantages of this ecological approach, using any "quadrats" designs, are discussed vis-a-vis classical conventionally designed experiments, which may not be perceived by the farmer as so relevant, nor lend themselves to his active participation so readily.

Key words: On-farm experimentation, agroforestry experimentation, multipurpose tree investigations, agroforestry surveys, quadrat designs.

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# 1. Introduction

## 1.1 General comments

The needs and opportunities to collect more *in situ* bio-physical data about agroforestry systems and components are becoming increasingly apparent; whether these are from surveys or from field experiments. Certainly, experimental questions have to be tested on farm at an early stage because the bio physical complexity of agroforestry systems demands a high level of site specific validation, and there is a critical need to be aware of the land user's precise reasons and attitudes to any introductions or changes involving the use of trees. Indeed, without the latter adoption potential can be very incorrectly estimated.

A complete on farm research programme, whether to introduce a technical intervention or a complete system, will necessarily involve a whole range of experimental, observational and information gathering activities. And it will be closely linked and overlap with on station work (for example, see Atta Krah and Francies, 1987). This paper introduces, and briefly discusses, two approaches that the authors feel are particularly needed for on-farm research concerned with agroforestry.

In the first part some of the factors involved in using a grid transect method to investigate the environmental interactions of an established tree with its immediate site are discussed. The second part is concerned with some of the short-comings in trying to adapt experimental techniques that originally were designed for on station experimentation to on farm situations, and a new approach is proposed. In this introduction we concentrate mainly on the need and justification for the latter.

## 1.2 Observations on trees

The upsurge of interest in multipurpose tree (MPT) species for use in agroforestry systems has created considerable demand for data

on their growth, phenology, production characteristics and yield; their potentials for beneficially modifying the micro-site; and their general compatibility with other plants, particularly with agricultural crops and grasses. For many MPI species there is a dearth of information on these aspects and, even when this can be obtained from various documented sources, it is often fragmentary and still in need of elaboration and/or validation. However, a vast body of such information still awaits collection from on-farm situations. The main problems here are to define, clearly, a set of limited, practical objectives for collecting such information, and to give guidelines on exactly how it can be obtained. These aspects are discussed in Section 2 below.

## 1.3 A new approach to on-farm agroforestry experimentation

This requires more explanation. Basically, on-station designed experiments, for which there is very wide choice of regularly-shaped field designs, use sets of treatments chosen to provide answers to clearly identified, well focussed questions. They are almost invariably done on flat or, at most, gently-sloping land. Their size and often rather formal structure do not make them suitable for on-farm trials with subsistence farmers having only small fields. Especially where such farmers are on slopes, sometimes even with terraces. Nor do such old fashioned and formal designs lend themselves to "miniaturization" either in size, complexity, or both. This is because at the on-farm scale they may become unsatisfactory both biophysically (e.g. plot size too small), and statistically (number of degrees of freedom for testing too few). Furthermore, farmers do not see such layouts as relevant to their normal farm conditions, or to their goals. So that this can markedly limit their impact.

These comments apply with additional force to on-farm agroforestry as compared with agricultural trials, because of the inherently larger size and complexity of the latter. Additionally, an agroforestry experiment can be expected to last for several to many years.

In general, two ways have been adopted in order to modify the design approaches to agricultural on-station research to make them more suitable for on-farm investigations. First, limiting the size of blocks is seen to be essential and, in consequence, the incomplete block layout is a very useful approach (e.g. Mead, 1988). Then again, the image that many temperate-based researchers have of what constitutes a "block" has had to be greatly modified, although possibly not as far as statisticians have been advocating. Indeed, the term "block" is not a happy choice. A "block" represents a similarity, in some obviously recognisable sense, of a group of units. Several authors (Caldwell and Walecka, 1986 and Mead, 1988), emphasise the primary requirement in a randomised block layout, complete or incomplete, for setting out each block of an experiment on an environmentally homogeneous area regardless of the contiguousness of the plots containing seperate treatments. To extend this idea somewhat, the plots in the field comprising any one block can even be quite widely scattered (Huxley, 1986a), although this implies some appropriate level of knowledge about the conditions within which "homogeneity" can be defined and, within practical limits, perceived.

Unfortunately, too many field experimenters assume that contiguousness over small areas ensures homogeneity. In tropical regions, where on-farm sites can vary markedly over even a few metre's distance, plots in a proposed block can often be found to be obviously not homogeneous. Nevertheless, the image of the regularly shaped on-station field experiment, for which blocking was originally proposed as a very effective and practical tool in temperate lands, has prevailed. Research stations wherever they may be are, or should be, chosen for their suitability for large scale field experimentation. Which automatically implies that they have several large environmental homogeneous areas on which locational error can be minimized. Small farm sites in many parts of the tropics inevitably do not have this attribute. We are thus almost invariably faced with a very difficult and thoughtful task if we wish to set down a controlled experiment using blocks on such a farm.

The on-station field experimenter sometimes carries out a uniformity (or non-uniformity?) trial using the intended or a substitute test crop, planted equally spaced over the whole area, and harvested either as a grid, or according to a pre-conceived idea of where blocks and plots will be situated. Co-variance is then used to adjust the yield of subsequent experimental plots. Such a careful approach is rarely found in tropical field research these days. In any case, for agroforestry, the different environmental resource-use characteristics of woody and non-woody plant components renders it inadequate in its usual form. For example, a locally adapted cereal cultivar is hardly likely to provide an adequate test of site variability for an experiment which is to include a deep rooting woody perennial.

We can undoubtedly agree that considerably more thought is required in dealing with locational error in field experiments carried out on farm in the tropics as compared with those on-station in temperate regions. And that the problems are further exacerbated with agroforestry field experimentation. Would it not be better, to look into ways to exploit rather than try to overcome this naturally occuring on-farm heterogenity? And to utilize it in order to compare relevant situations? Or, perhaps, after we have identified and characterized the situations to modify them in order to observe interactions of other kinds of effects over a wider range of conditions? Two different, and equally important objectives.

To achieve this we will have to change our attitudes and modify our approach towards that of the ecologist, one of whose major tasks is to investigate such situations in natural vegetation. The approach to agroforestry field investigations on-farm proposed in the second part of this paper is based on this.

## 2. Bio-physical surveys of existing multipurpose trees

### 2.1 The approach

This type of "on-farm" research (it could be done both on- and

off-farm) is designed to explore and compare the characteristics of existing trees, including their effects on the micro-site. Such trees could be either growing singly, in small clumps, or even as hedgerows/boundary plantings etc. In what follows, the single tree example has been chosen. This kind of operation would be researcher planned/ researcher managed.

The approach is basically that which ecologists use when studying any plant community, but the environment immediately around (and below) a single tree is what needs to be investigated. Often we may wish to examine seasonal changes of some factors (e.g. shade), and longer term influences, for example on soil characteristics, would be studied by selecting similar sites possessing trees of different ages.

As with many other kinds of research some of the measurements could well be similar to those needed on station when studying sample trees in plots. However, for single trees, in addition to any production from the tree itself, the spatial and temporal variations in different parameters can, perhaps, best be investigated using a grid transect method to study both the relevant above and below ground environmental aspects. Grid sampling beneath single tree plots has already been reported in an experimental agroforestry situation (e.g. by Newman 1984), and discussed in relation to the use of phytometers (Newman, 1988), but it is yet to be generally adopted.

Such an investigation on single trees could attempt to answer "simple" questions as such "In this area, and on such-and-such a soil type, is species A more or less soil-improving than B?" However, a question like this can contain many hidden conditions (e.g. "over what time scale?", "with what surrounding soil and crop management?" etc.). Nevertheless, even an outcome in relative terms (e.g. as a ranked order) could be very useful at this stage in agroforestry research. Single tree investigations of any kind are, alas, all too few (cf. Kellman, 1979) but some

detailed studies have recently been started by Belsky et al. (1988) and at ICRAF's Machakos Field Station (the latter, to study only methodology).

At present we know insufficient about the variability likely to be encountered to be able to come up with firm recommendations about sampling such situations. And there is a further need to simplify the expected assessment methodology because of the limitations to research resources.

## 2.2 Explanation of the methodology

The research methodology problem is that of selecting appropriate characteristics to measure, and to define how to measure them. For example, what sample sizes (e.g. grid areas) are to be used and how often should we sample? What number of replicates (trees) are needed? Finally, we have to solve any data analysis problems. The basic principles of experimental design are relevant to the planning of such sampling exercises. Thus if we are sampling from a grid of 64 squares around each tree and have to select sample quadrats from each tree on each of a number of occasions, the structural ideas of incomplete block designs and Latin squares can be utilized advantageously.

In this type of bio-physical survey the farmer is being used to provide MPT species/management situations that would not otherwise be available. The usual difficulties of discovering the exact management history of the study unit, and of characterising and recording current management practices or interventions will apply. The objective is to obtain valid information about tree environment interactions from an adequate sample of trees of that species at one or more defined kinds of sites.

In the first instance tree age and management history might well be standardized, as far as this can be done. Otherwise samples could be stratified accordingly, and then the tree-environment

interactions with time under different management circumstances examined. This is beginning to make the size and complexity of such a bio-physical survey rather large, but a study of changes with time is likely to provide important information.

What is needed are some sample sets of data so that some idea of the variability in such tree systems can be obtained. Only then will it be possible to give precise recommendations about how to carry out this kind of study. Figs. 1 and 2 give examples indicating hypothetical sampling schemes, bearing in mind the present scarcity of suitable data on which to base these suggestions. Sampling designs to fit various different restrictions can be constructed without difficulty. For a 64-quadrat grid the restrictions may include rough equality of sampling intensity in different rows, columns, quarters (for orientation effects) or distance from the centre (tree effects). Two designs (without optimality properties) are shown in Figures 1 and 2. In Fig. 1 times of sampling are allocated equally in each row, each column and the inner block of 16 quadrats. Such a design relates directly to latin squares. In Fig. 2 the strata within which allocation of sampling times is required to be equal are quarters and square boundaries about the centre. The analysis of data would involve fitting a regression model with terms corresponding to the strata effects and (possibly) distances from the centre and orientation, together with tree and sampling occasion effects. From this fitted model isoclines can be constructed.

Although at first sight it might all seem very simple the diagrams that follow show that it is, in fact, not! Fig. 3 is a plan view of a tree occupying a site. Figs. 4-6 show examples of the hypothetical distribution of some inputs; Fig. 7 the distribution of some outputs, similarly; and Fig. 8 the possible net long term effect on soil fertility. Table 1 lists some plant-environment characteristics that may need to be assessed.

FIG 1

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| F | G | H | C | B | E | D | A |
| D | B | C | H | A | F | G | E |
| E | F | A | B | C | D | H | G |
| A | C | E | F | G | H | B | D |
| H | E | F | A | D | G | C | B |
| G | D | B | E | H | C | A | F |
| C | A | G | D | E | B | F | H |
| B | H | D | G | F | A | E | C |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| H | A | B | E | D | G | F | C |
| F | D | E | B | C | H | A | G |
| G | H | C | D | E | F | B | A |
| C | E | G | H | A | B | D | F |
| B | G | H | C | F | A | E | D |
| A | F | D | G | B | E | C | H |
| E | C | A | F | G | D | H | B |
| D | B | F | A | H | C | G | E |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| H | B | D | A | G | F | C | E |
| C | G | A | D | E | H | B | F |
| F | H | E | G | A | C | D | B |
| E | A | F | H | B | D | G | C |
| D | F | H | E | C | B | A | G |
| B | C | G | F | D | A | E | H |
| A | E | B | C | F | G | H | D |
| G | D | C | B | H | E | F | A |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| B | D | F | C | A | H | E | G |
| E | A | C | F | G | B | D | H |
| H | B | G | A | C | E | F | D |
| G | C | H | B | D | F | A | E |
| F | H | B | G | E | D | C | A |
| D | E | A | H | F | C | G | B |
| C | G | D | E | H | A | B | F |
| A | F | E | D | B | G | H | C |

Fig. 1: Sampling scheme for 8 occasions with 64 grid points for four trees, each in the centre of its plot (see Fig. 3). Occasions (A, B, C, D, E, F, G, H) are arranged to occur in each column and inner/outer square boundaries with equal frequency, and in different positions for different trees - see text for further explanation.

FIG 2.

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| D | C | H | G | E | H | F | B |
| A | F | D | C | F | A | G | C |
| E | G | A | B | C | D | E | A |
| H | B | E | F | G | H | B | D |
| G | F | C | A | E | G | H | E |
| A | H | B | D | H | F | G | B |
| F | D | E | G | A | B | C | D |
| E | C | H | B | C | D | F | A |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| G | C | F | A | H | G | A | H |
| D | H | B | E | B | C | D | E |
| B | F | C | D | E | F | G | D |
| E | A | G | H | A | B | F | C |
| D | B | E | C | G | A | H | D |
| H | G | D | F | B | H | E | A |
| B | C | A | H | D | F | G | F |
| A | F | G | E | C | E | H | C |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| F | A | B | G | A | C | G | D |
| D | C | E | H | E | D | A | F |
| E | F | H | D | G | F | H | B |
| C | G | A | B | C | E | B | H |
| E | F | G | H | D | C | A | H |
| C | D | E | F | A | B | F | C |
| A | G | B | C | G | E | D | F |
| B | D | A | H | H | G | B | E |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| C | B | H | D | G | B | F | H |
| F | G | D | A | C | F | E | A |
| A | E | F | G | H | A | D | C |
| B | H | G | E | D | B | G | E |
| G | D | A | B | F | E | B | H |
| B | F | H | G | C | D | A | G |
| C | A | E | H | H | B | C | D |
| F | E | C | D | A | F | E | G |

Fig. 2: Another sampling scheme, also covering 8 sampling occasions. (A to H). In this example the strata within which the allocation of sampling times is required to be equal are quarters (for orientation effects) and square boundaries about the centre (for distance-from-tree-effects) - see text for further explanation.

SINGLE-TREE ENVIRONMENT INTERACTION STUDIES

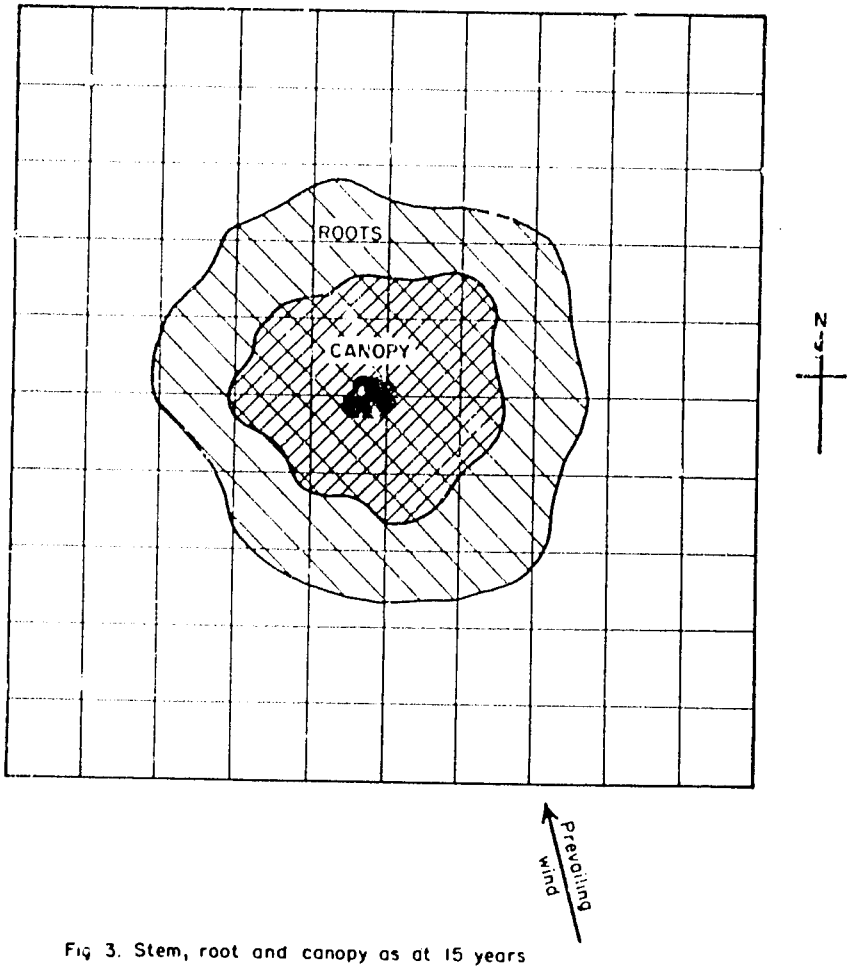


Fig 3. Stem, root and canopy as at 15 years
- a hypothetical case.

Notes:

1. Dimensions of individual grid squares will be dependant on degree of discrimination required.

2. Number of grid squares (overall size) will depend on (1) together with numbers of samples that can be handled in practice e.g. 8 x 8 1·5m squares for a medium sized tree?

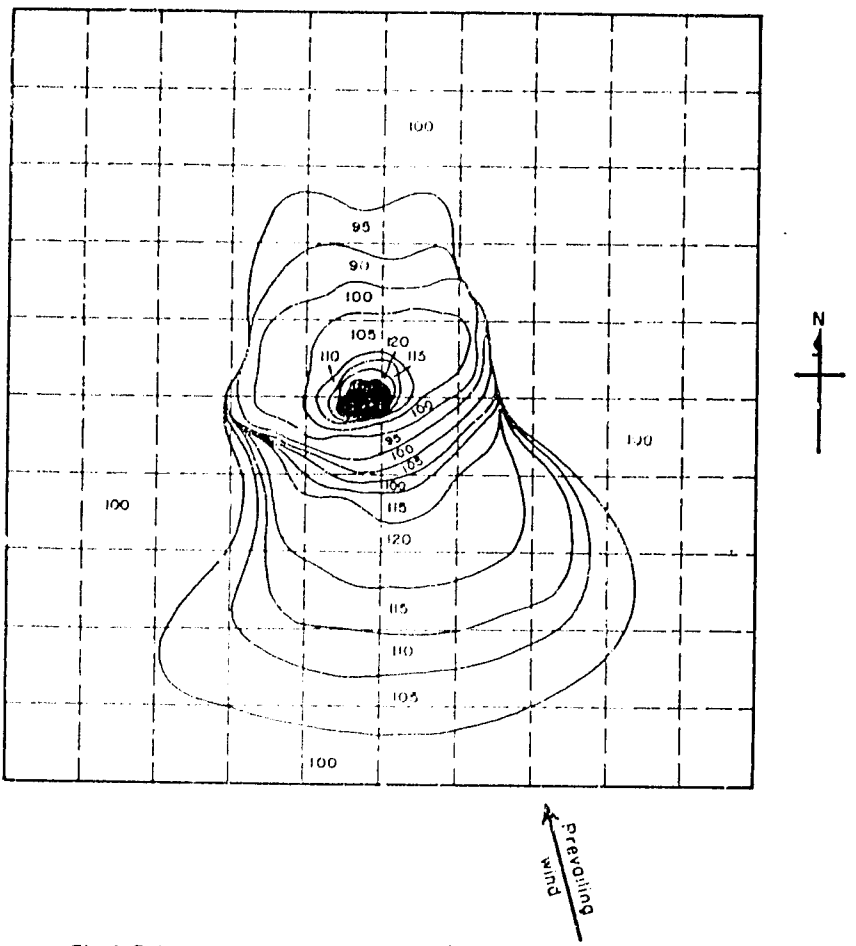


Fig 4 Rain received at ground level (% of open) - hypothetical - all else as for Fig. 3.

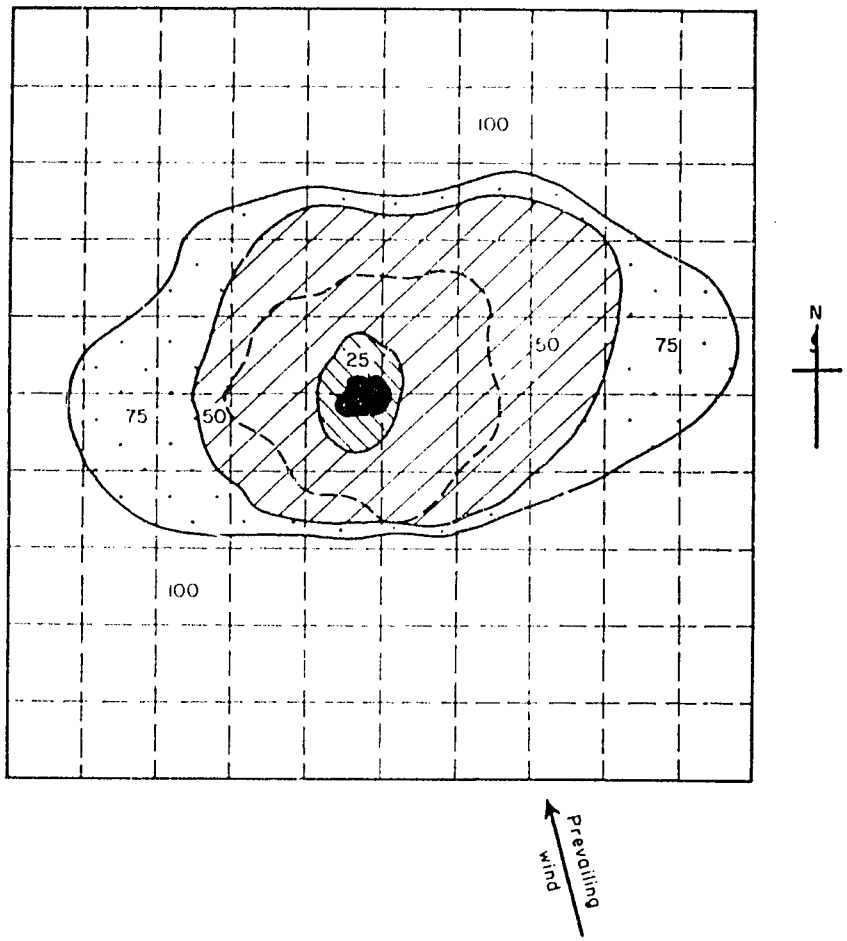


Fig. 5. % full daylight (ground level, per day). Latitude 5° S., July. – hypothetical (morning/afternoon soil surface temperatures will be modified accordingly).

– all else as for Fig. 3.

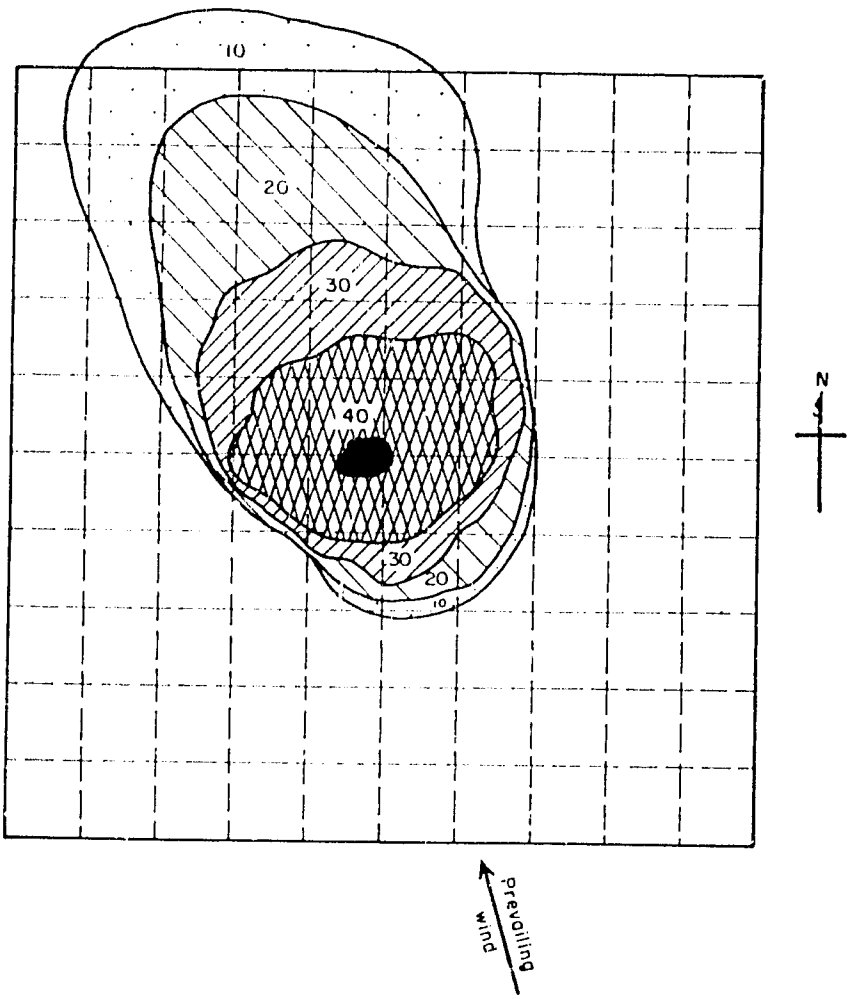


Fig. 6. % total litter deposition –hypothetical

–all else as for Fig. 3.

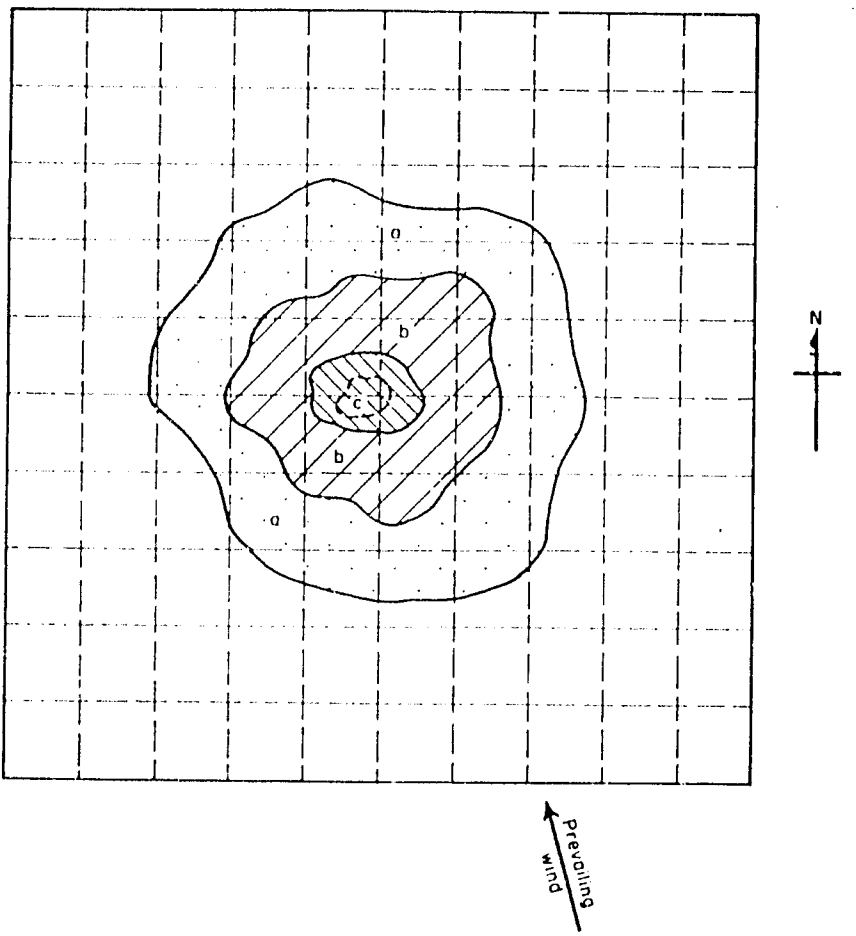


Fig. 7. Water/Nutrient uptake situations: -hypothetical

a High level in surface soil.

b Medium level in surface and deeper soil levels.

c Mainly deeper soil levels.

(but modified by litter and soil temperature changes).

-all else as for Fig. 3.

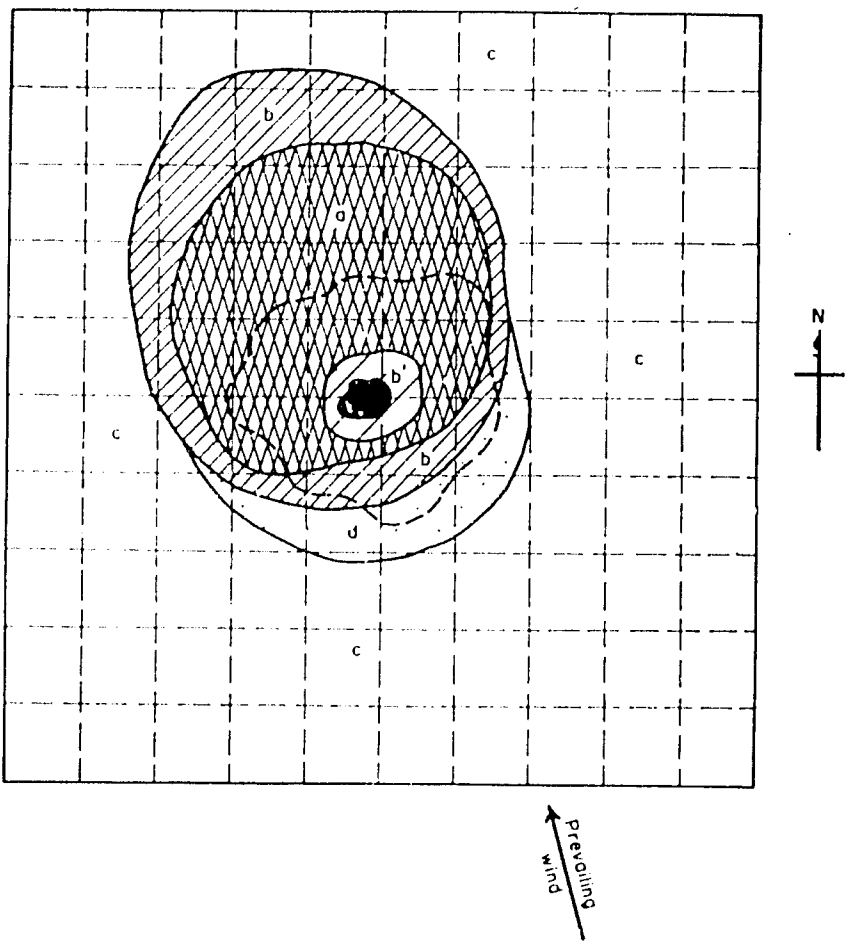


Fig. 8. Hypothetical overall topsoil fertility status after 15(?) years. (needs to allow for increasing canopy and root spread).

a — Highest level (most litter, less rain, some shade but high level of water/nutrient uptake)

b — Moderate level.

b' — Low level due to heavy stemflow leaching?

c — Normal level (outside tree influence).

d — Lowest level (high leaching, high topsoil temperatures, little litter disposition).

– all else as for Fig. 3

Table 1 List of potential plant environment assessment for Single tree investigations (* necessary, others are optional)

Information about seasonal changes are likely to be important so that, for each of the characteristics below, careful thought has to be given as to when and and how many times during the year an assessment should be made.

A. Above-ground assessments

On the tree: Crown size*,
(at beginning and end of season).
Leaf area index.
Stem diameter*.
Height.
Phenology*.
Non-destructive estimate of whole or part.
(e.g. of fuelwood, at beginning and end of season).
Chemical composition of foliage*
(during a growth flush and at least once, subsequently).
Litter
(amounts and chemica composition, litter degradation rate)*
Fruit yield
(if appropriate)

Climate: Rainfall*
(below and outside canopy in selected grid squares)
Stem flow, drip characteristics and chemical content of throughfall/ stemflow.
Radiation and light interception*
(% total daily integrated radiation and light(PAR*) received at ground level in selected grid squares on sample days).
Air movement (20cm below lowest branch elements and/or half that height from surface in selected grid squares on sample days (eventually to be replaced by 20cm above crop, grass or weed growth) as a % of air movement in the open at the same heights.

Temperature profiles, if required.

Humidity profiles, if required.

B. Below-ground assessment

Samples taken only at the beginning, middle and end of season are likely to be feasible.

1. On the tree: Root samples
   (in appropriate grid squares).
   Root phenology*
   (fine root growth activity).
   Fine root turnover
   (dry weight samples).
   Nodulation*
   (mycorrhizal associations).
   Nodule efficiency (ethylene-acetylene reduction).

2. Soil: Soil water*
   (profile determinations in selected grid squares and at selected depths to identify main season changes).
   Deep drainage
   (and its chemical content).

   New surface daily soil max. and min. temperatures(at two depths from the actual soil surface in selected grid squares), and soil surface itself (if required).

   Chemical composition*
   (every 3 years) bases, CEC, pH.

   Soil physical status*
   (every 3 years) bulk density in selected grid squares).
   Soil water infiltration rates
   (in selected grid squares).
   Soil fauna
   (seasonal sampling, biomass and nutrient content).

## 3. Replacing "classical" designed on-farm experiments

### 3.1 Limitation of "classical" designed experiments for on-farm studies

When planning to carry out such experiments on farm it is often assumed to be essential to "miniaturize". Not only are treatment numbers constrained, but the experimental unit is reduced in size. The former can lead to an experiment that is inefficient and, possibly, statistically inadequate (too few degrees of freedom for testing). The latter may result in plot sizes that are so small that they are all edge effects and/or severe mutual interference between plots take place; especially as the idea of guard rows is often abandoned for lack of space. In either case the end result can be both biologically and statistically unsatisfactory. It is like nothing that the farmer has ever seen and he views it, therefore, as having only dubious relevance to his situation. These problems arise because we start with the "classical" on station type of layout in mind and then try to adapt it. There are other alternatives, one of which is described below. However, before doing this it is as well as to say something about research objectives, because these should determine, to a large extent, the kind of on-farm experiment we do.

A designed experiment may be carried out in order to test either a single piece of technology (e.g. to compare two forms of soil management, such as with and without mulch combined with with and without tillage). Or it may attempt to compare different farmer-derived combinations of technologies which represent alternative sets of inputs. Investigating a single technology lends itself to a more precise evaluation of the effects and interactions of selected key factors, possibly at different levels. Comparing treatments as combinations of several factors confounds them, and thus defines the role of each less clearly and makes it more difficult to extrapolate to different places or management conditions. Investigating or assessing combinations of factors may appear to be more relevant to the farmer's situation,

but inevitably this provides little or no information on how the system works. The more complex any treatment composed of factor combinations becomes the more this applies. Because we are always dealing with at least two plant components in agroforestry systems (woody and non woody ones) the temptation to adopt the factor combination approach is often considerable and, at the scaled-down on-farm level, the returns in information could be rather limited and decidedly site specific. If enough sites are being used then this, of course, may not matter. ICRAF is adopting an observational "Prototype Systems Trial" approach (Huxley and Raintree 1987 see Appendix), principally aimed at extension workers, to accommodate this need to explore, in an initial and practical way, "best bet" agroforestry factor combinations. They will, of course, often lead to a clearer definition of exactly what precise experimentation is required, and can be particularly useful if the intending experimenter is still not adequately familiar with the plant components and management practices he wishes to study.

Factorial arrangements of experimental treatments can be a powerful way of investigating several factors, each perhaps at different levels. Factorially arranged experiments can be used, very efficiently, to explore main effects and their interactions. The size of a factorially arranged agroforestry experiment could readily get out of hand, because of the number of factors involved, but this difficulty can be accommodated through fractional replication. The factorial approach is likely to be most useful in initial investigations to identify the key factors to study in any particular situation, provided that the problem being addressed (a) needs a field experiment as distinct from a prototype system trial, and (b) that enough is already known about the agroforestry system and components under study to embark on resource demanding field experimentation.

It is up to the individual researcher in the particular circumstances to decide which strategy has to be adopted; although it is wise to use statistical advice in choosing the strategy because statistics is the science of obtaining and using

quantitative information efficiently. It is also important to recognise that different stages of a complete research plan will fit naturally into the research station and the on-farm situations

## 3.2 On-farm research and farmer objectives/goals

On-farm research will often have two sets of objectives. One is implicit in the discussion so far. This is to provide data to satisfy questions that the researcher wants answered, hopefully quickly to be adapted and passed back to the farmer through extension processes as practical recommendations. The other is to utilize the experience of the farmer himself to help design, monitor and evaluate the trial. The latter can be achieved at two levels, which are (or should be) essentially simple and clear responses by which we learn something about the potential value and adoptability of the proposed intervention. First, if the farmer "accepts", "likes", "is interested in", "want to know more about" all or part of the experiment then that, in itself, is a successful outcome. Even if the farmer "ignores", "resists applying", or even "alters a treatment", something has been learnt.

"Accepts", "likes", "is interested in" and "wants to know more about" are subjective terms which describe a decision or choice based almost certainly on an integrative, heuristic appraisal of the possibilities made in the context of a highly-localized situation. Some of these factors are quantifiable, but many of them are not, or at least not easily made so (risk, perference etc.). The overall impact of the experiment, in farmers's terms, may well be defined quantitatively just by the number who accept or reject the intervention in some form or another. A second, and more valuable outcome for the experimenter, will be to discover the reasons why the farmer adopts the view he does about the experiment, including exactly what experimental treatments have attracted the farmer's own form of re evaluation (was it something the experimenter overlooked?).

Clearly, an on farm experiment can be most cost effective if it fulfills both sets of objectives. But can a conventionally designed experiment do this very effectively? If it is a "miniatuarized" field layout testing technical interventions in a classical way, then this is unlikely. Is there a better approach and, if so, what design and assessment methodologies can be suggested?

### 3.3 "Quadrat" designs

These could equally well be termed "unsystematic" designs! This approach assumes that on-farm treatments can be selected and/or arranged in two ways: either by identifying relevant already occuring "ecological" (environmental) states and/or management conditions which exist in the type of on-farm situation to be found (or which come about as a natural consequence of promulgating it); or as some form of "interference" with the natural state of part of the system. Either or both could be present in any one experimental situation. The comparisons of experimental treatments would then consist of those between different sets of selected quadrats ("units", "plots").

There are some minimal requirements which apply to any form of experimentation from which conclusions are to be drawn which are to be regarded as scientifically (or technologically) defensible. These include:

a) The definition of the observational unit, in terms of size (which must be similar for different units), and location (if orientation varies it could be regarded as an ecological treatment).

b) Replication of units to the level where some numerical estimate of the variation between "identical" units treated identically can be obtained. Obviously this does not demand that we actually use identical units identically treated (a philosophical impossibility). Rather it requires that the

number of units (on a single farm, or on a group of farms) is clearly larger than the total number of differences (either observed ecological differences or applied treatments) which are of interest. Or which must be allowed for in the analysis of the variation in observed performance variables.

The proposed type of on-farm investigations will, therefore, be examining problems partly by observation - which will involve some random element in the choice of unit and recognition of environment, but not have any element of "interference"; and partly by experimentation - which will involve choice of units, perhaps control of environment, and "interference" (= applied treatments). The practice of superimposing simple agronomic treatment plots on fields being managed by farmers is not, of course new (e.g. Hildebrand and Poey, 1985). However, such fields are usually chosen to be generally representative of the farm situation in any particular area, and not from the point of view of exploring the available locational heterogenity. The choice and selection of both ecological and interference treatments will depend on the kind of comparisons to be made, whether or not the investigation is on an existing farm situation, or whether the farmer is about to initiate an agroforestry practice, i.e. it is to be newly planted so as to be examined later. Lopping management on existing hedgerows is clearly an interference treatment comparison, and newly planted sites could take into account the natural opportunities to examine soil, shelter, aspect or even the influence of other vegetation (e.g. existing trees) on selected quadrats.

In any case, comparative sets of "treatments" must obviously be chosen for their relevance, not merely because they are there! They will nearly always represent a few rather widely different comparisons (if not extremes?), a feature which will contribute both to overall simplicity and the clear interpretation of results.

It is not necessary to make detailed soil fertility measurements on potential plots before using them. A perfectly sensible

strategy would be to classify potential plots subjectively (with the farmer) and, where subsequent measurements of fertility or other physical characteristics are available, to use such measurements as covariates to adjust treatment comparisons.

The stages

For this type of on-farm experimentation we thus have the following stages.

a) Identify a large population of observational units, probably spread over several farms.

b) Each unit is classified according to its level for each of several environment treatment factors.

c) Each unit is further classified according to its level of each of several blocking factors which, unlike the environment treatment factors, are of no direct relevance to the questions the research is hoping to answer.

d) There may be interference treatments (= the type of treatment used in an on-station experiment) to be included in the on farm experiment.

e) Within blocks of similar units, where blocks are defined from (c), units to represent different environmental treatments (b) are randomly selected from a population of available quadrats (plots) of that kind and (if (d) is relevant) allocated a level of interference treatment.

## 3.4 An example

To elucidate we have taken a hypothetical hedgerow intercropping situation as an example, but the approach has a wide applicability and would be useful for any simple sets of comparisons (e.g. with 2 or 3 factors). The design proposal (see Section 3.5 and Figs. 9-11) is for investigating from the

farmer's viewpoint, the feasibility, value and adoptability of this particular agroforestry intervention (hedgerow intercropping into a chosen woody and a chosen non woody plant component). And to obtain data on tree crop interface effects at two ranges of soil fertility and two levels of hedgerow lopping, for the researcher.

With these twin sets of objectives in mind the farmer is to be asked, specifically, "is this form of crop association (in this case, say, leucaena hedgerows with field beans intercropped between) useful to you?". And, then to qualify this (if the answer is positive) by stating "what arrangements and/or management conditions do you consider to be best for your (stated) purpose?". The researcher has the specific technical comparisons to test of the interactive effects of the hedgerow, with its adjacent crop, of two different soil conditions and two kinds of hedgerow lopping practices (clearly, these would have been discussed with the farmer before being chosen). He must obtain data from randomly chosen replicates so as to draw statistically valid conclusions. One might add a third collaborator - the extension worker - who will probably be satisfied with critical observations (not essentially statistically validated), from which he can gain a great deal of practical information about what will or will not work in terms of management and farm organisation.

The farmer would be provided with a set of guidelines on how, generally, to manage the trial, but at the same time encouraged to adopt his own approaches to management for different parts, should he wish. Although, of course, not from the places where the researcher is going to collect his information.

Fig. 9 shows a hypothetical example of a possible on-farm situation from which the researcher is in the process of selecting relevant ecological treatments. Fig. 10 illustrates, for another farm site, what the outcome has been of selecting the randomly available treatment plots (same treatments as before).

Fig. 11 gives another example in which the natural farm situation has been utilized to test a set of simple (2 factor x 2 level) treatments, one of which is ecological (site condition) and the other interference (mulch management).

## 3.5 Points to consider

In setting up such a trial it must be clear that (a) sample plots representing any particular ecological treatment are as similar as possible, and (b) they are chosen to be a random sample of the possible available plots of that kind.

The classification of ecological plots, based on existing situations, would best be done by getting the farmer himself to select them, or to make this a joint exercise between him and the research team. Certainly, a farmer's classification of his soil, based on experience, could be less time and resource-consuming, and far more management and output-oriented than a scientific evaluation of soil characteristics. Although, the latter could be used, eventually, to validate similar clusters of treatment plots.

Treatments such as "distance from hedge" would need to be defined precisely and the plots chosen to minimize other potentially influential factors (e.g. orientation, sheltered or exposed side of the hedge etc.). Lengths of hedgerow without accompanying crop rows could be left if data on potential hedgerow biomass production are needed as a "control", and samples taken from appropriate sole crop areas, similarly.

The size of each sample plot (or length of sampled crop row) would need to be chosen in relation to the overall size of the experimental sites, the number of plots needed at any one site and the treatment being tested. It would be essential to try to standardize sample sizes.

Fig. 9

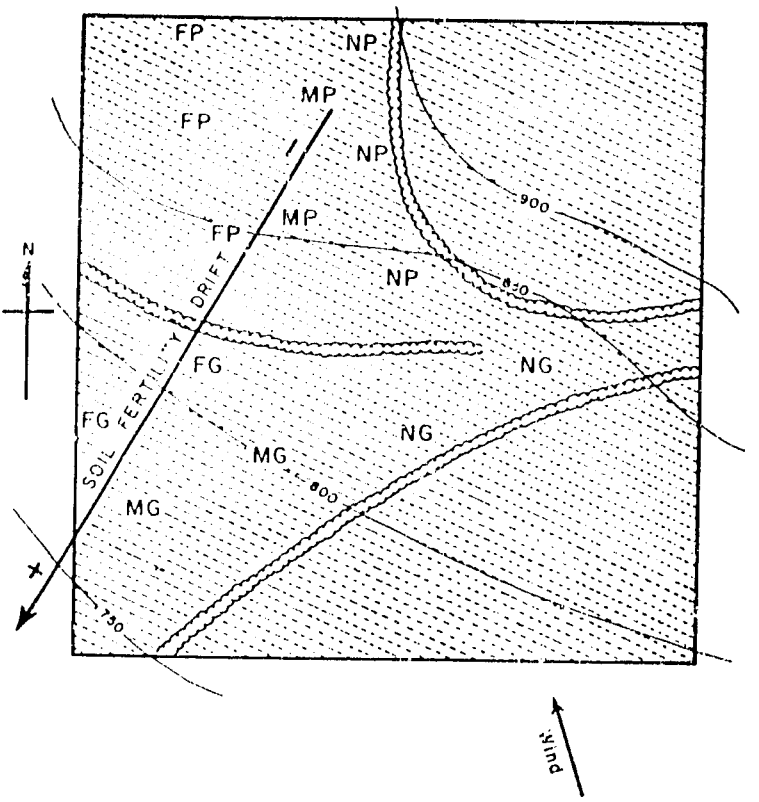


Fig. 9
Hedgerow intercropped farm site chosen to provide two kinds of "ecological" cropping situations: crop proximity to hedge being (a) near (N), moderate (M) and far (F), on (b) soil of good (G) or poor (P) fertility. Some of the possible "plots", which also conform to the requirement that otherwise they are similar, are shown. For example, all plots are on the sheltered and not the exposed side of hedgerows. Dotted lines indicate crop rows (they would not necessarily all be parallel), the hedgerow is indicated by convoluted lines. Should controls (crop alone, hedgerow above) be required suitable, similarly randomly chosen plots would be included.

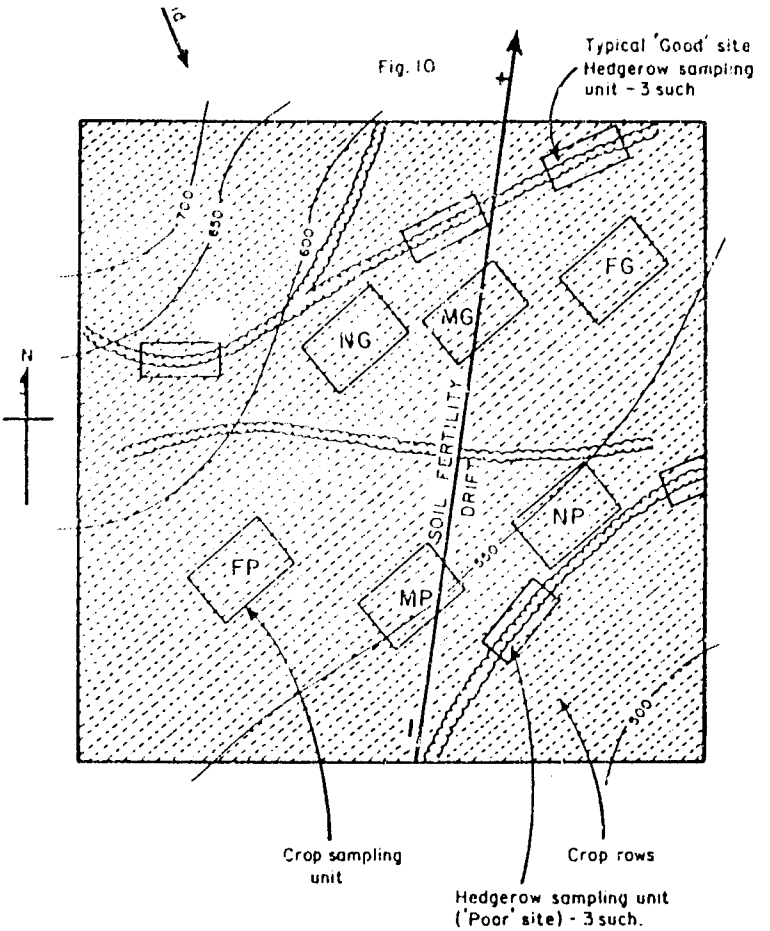


Fig. 10
A similar site indicating the equal-sized crop plots (quadrats) and different, but equal-sized, hedgerow plots which have been selected, at random, to represent the "ecological" situation under study. Hedgerows would be non-destructively measured for growth, as well as having fodder, fuelwood or mulch yields taken from those parts being subjected to a standard experimental management procedure. A limited number of hedgerow plots might be available for destructive sampling, if needed, as long as this did not interfere with any site characteristics (particularly the treatment plots). The farmer, similarly, could manipulate hedgerow or crop elsewhere on the site in any way that occurred to him.

Fig. 11

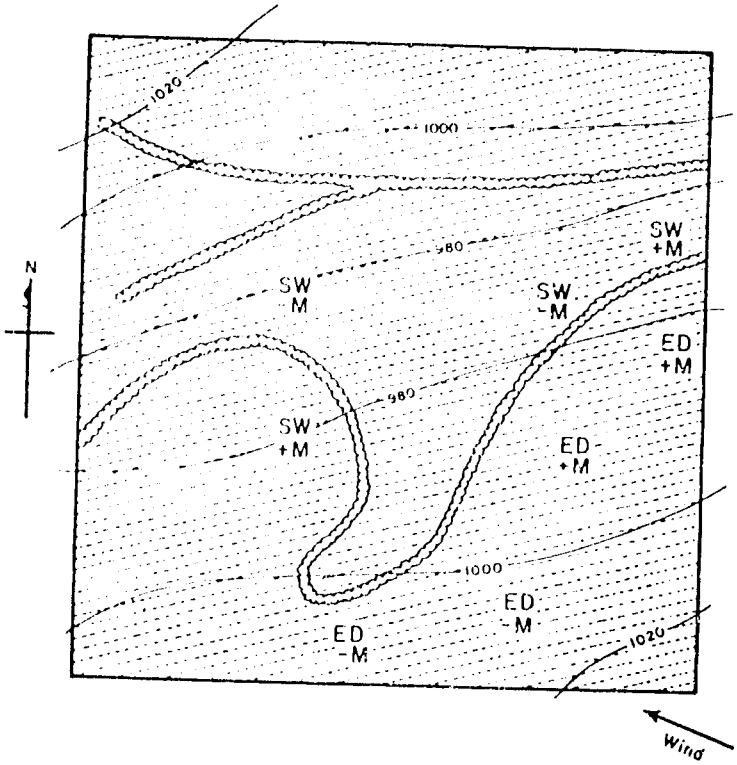


Fig. 11
Site providing another randomly chosen possible set of "ecological" comparisons i.e. crop sheltered and soil "wet" (i.e. clayey with poor drainage), versus crop exposed and soil "dry" (sandy, and free-draining). Clearly, all crop plots (as indicated) would need to be equidistance from a hedgerow in order to standardize this factor. A set of interference treatments (+M and -M) involving the use or not of standard amount of mulch obtained from an adjacent hedgerow are also indicated. This forms a 2x2 comparison which would be replicated on as many other similar sites as necessary.

It is important to distinguish between environmental classification of units as a basis for treatments, and blocking classification which will be based on all other characteristics of the plots (cf. the case-control concept in restrospective epidemilogical investigations). Thus "blocks" will be groups of plots similar in all other respects except that of the treatment characteristics chosen for comparison; irrespective of whether they are on the same or different farms. Clearly, our ability to select blocks, or for that matter treatment plots, will demand a high level of skill from the field investigation team (with the help of the farmer?). And our ability to do this satisfactorily in particular situations needs to be extensively tested. The allocation of subsets of treatments to form incomplete blocks of a more limited size can have the same advantage as it has in classically designed on-station experiments (e.g. Mead, 1988). Other design approaches (such as the use of split plots) could also be applicable and, in some cases where a relatively large number of sites can be used, regression analysis may be useful to discover the form of any relationship between two variables.

What the researcher can learn from such an experimental layout will depend on the skill with which it has been planned. The spatial arrangement of plant component of various kinds, range of management conditions and within-site environmental differences have to be carefully selected so that a previously, defined and limited set of identifiable experimental situations can be sampled, and reliable bio-physical data obtained. Clearly, each layout will be chosen (or designed if it is to be newly-planted on-farm) with a particular site and set of experimental objectives in mind. Furthermore, it has to be replicated on an adequate number of different sites which must first be surveyed selected, even if subjectively, for such aspects as comparable soil conditions, aspect/exposure, and so on.

Such layouts could be designed to give a very wide range of environmental and managemental situations indeed; far beyond the capacity of the research team to record and describe them. The

key issue becomes, then, a need to define the researchers' objectives very clearly. Within each investigation the exact combinations of environmental and interference treatment selections must be clearly specified. For example, tree-crop-interface effects can be examined at different orientations. Or particular soil management conditions can be studied (e.g. tilled versus untilled combined with mulch and unmulched situations using hedgerow residues). Again, site variability can be put to good use and poorly-drained versus well-drained situations chosen on which to chose sample plots (making sure that other conditions are kept "standard" or used as blocking factors). On sloping areas the design can be arranged across and along terraces so as to select sample plots at the front, middle and back. The range of possibilities is large.

Because of the difficulties of being able to select plots in which the treatments (= situations to be measured) are identical, it will certainly be best to make sure that treatments are markedly different from one another. For example, an experiment to discover the effects on crop growth of increments of hedge residue used as mulch would better be done on-station, certainly if detailed soil physical and chemical measurements are needed. But testing the effects of widely different mulch levels on crop and hedgerow growth ("none", "little" and "lots", later to be quantified) could suitably be done on-farm.

There are also possibilities for measuring experimental variables (covariates) in situ as they happen to occur and, using regression analysis, to adjust treatment means so as to improved precision.

## 3.6 The benefits

Depending on the resources available such an on-farm experiment can enable the researcher to examine a wide range of useful measurable conditions. He has limited control over the experiment as a whole but must demarcate and retain precise

control over the plots he needs to measure. He is using available the site heterogeneity in order to establish a set of ecological (or environmental) treatments. He may choose these as packages of variables, or as individual variables to be studied at different levels, depending on his prime objectives. In such a layout the block x treatment interactions (= error) will be high. So that this, again, is an argument for establishing a treatment range in which relatively large differences are to be expected. Indeed, such experiments are unlikely to be able to discriminate fine differences. However, if there are sufficient replications, and the selection of plots with closely similar bio-physical attributes (which can be precisely characterized) is achieved with a clearly defined random sampling component within each "block", then such an experiment may be both statistically and biologically more reputable than a miniaturized on-station layout. Furthermore, it will resemble something more familiar to the farmer's normal situation which can be easily fitted into a farm site, however awkard a site this may be.

3.7 Which alternative?

Agroforestry experimentation for low-input farm situations is likely to be dealing, to a large extent, with investigating the optimization of components and simple management techniques for prototype systems. It may be asked, therefore, whether on the one hand it is better to start with a randomized block design in which great care is taken to place sets of experimental plots into homogeneous but fragmented blocks? Or, on the other, create an appropriate set of environmental/management conditions which can be measured and compared as sampled "quadrats"? The answer to this will depend on the balance between environment treatment and interference treatments required and the degree of validation necessary to determine either (a) the homogeneity of individual blocks, or (b) the environmental suitability and similarity of quadrats to be used for any particular set of treatment comparisons. In fact, these concepts are really identical. Do we end up, then with a distinction without a difference?

Although conceptually the same, the choice of direction from which we approach the problem of making valid comparisons of environmental situations, either unencumbered by unwanted site variability, or by making use of it, is a very important practical issue. With the classical designed experiment approach one is trying to impose a set of clearly-defined, and limited treatments which we hope, with good management, can be carefully controlled in a situation on-farm that is inevitably locationally highly variable, and sometimes disasterously so. Sampling from quadrats uses the natural variability to identify and/or create sets of relevant conditions to be examined. Existing conditions can, themselves be recognised and, if necessary, modified. It offers an approach which is both more readily appreciated and understood by the farmer and, at the same time, could promote a wide range of treatment comparisons and interactions in a way that can equally well fulfill the researchers objectives. Furthermore, other kinds of data analysis techniques can be utilized. For example clustering and/or ordination techniques, commonly used by ecologists, would be one additional possibility.

The question is, therefore, not "either/or?" but for each situation "which point in the continuum will be most cost-effective?". On farms of sufficient size, which are sited on suitably homogeneous environmental areas, the conventional designs should be used with our modern armoury of knowledge about experimental design (i.e. probably not using Randomised Complete Blocks!). On small farm areas with considerable soil and/or climatic change the "ecological" approach offers a possible useful alternative which should be tested as widely as possible.

## 4. Conclusions

A challenge to agroforestry researchers everywhere arises from the difficulties of exploring woody/non-woody intercrop situations, of one kind or another. And of dealing with the testing and/or introduction of the very large range of

potentially useful agroforestry practices and components that could feasibly enhance the productivity and sustainability of tropical and subtropical landuse systems. The very nature of these generates a multitude of difficult questions, the ultimate answers to which can only be provided through very carefully derived sets of field experiments. Observational prototype systems trials may well generate some rapid answers as to what are "best-bets". But these, too, will need experimental validation in due course. New approaches and new possibilities have been proposed for on-station agroforestry field experiments, (for example, Huxley, 1985, 1987, 1988b; Huxley, Mead and Ngugi, 1987). Yet on-farm agroforestry experimentation demands, even more, that the researcher opens up his mind to all useful possibilities. In this paper we have proposed and discussed two of them which seem to have potential.

The multidisciplinary nature of the approaches to the descriptive aspects of agroforestry investigations needs, now, to be paralleled by a similar thrust into the experimental sciences - both biophysical and socio-economic. Many of the solutions required will probably be achieved through innovative research and/or combinations of ideas for research resulting from experiences from several disciplines. We have to take the best from the past and not be afraid to open up some new views into the future.

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APPENDIX

## FIELD EXPERIMENTS VERSUS PROTOTYPE SYSTEMS TRIALS*

P.A. Huxley and J.B. Raintree

Prototype systems testing using a Prototype Systems Trial differs from field experimentation, whether on-station or on-farm, in several important ways. The following is a brief note suggesting what the authors feel these differences should be.

### 1. The agroforestry field experiment

Designed field experiments should have precisely-focussed objectives based on examining the effects, and often also the interactions between carefully-selected key variables with all other standardized as far as possible. The greater space/time dimensions and management complexities of agroforestry systems, as compared with most agricultural ones, means that agroforestry field experiments can only deal with very few variables at a time. Because each step in an experimental sequence has to be properly-validated, a set of field experiments may take a number of years to complete.

The results are to provide sets of information on experimental treatments (= various combinations of the selected variables) in the form of statistically-analysable datasets which give meaningful comparisons of the strictly limited aspects of the subject under investigation. Through ICRAF's "D&D" approach this subject will have been selected as a potential system "leverage point", the modification of which could beneficially affect the outputs and/or stability of the landuse system of concern. This ensures that only relevant and priority experimental issues are addressed.

Field experiments can seldom be effectively modified unless the original experimental design has taken this into account (provision for split plots). Depending on the resources available and the

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* ICRAF In-house circular, November, 1987.

actual objectives established - an agroforestry field experiment may test just "What" happens or it may try to discover "How" the results come about. In the latter case this can facilitate extrapolation to other sites and/or changes in management. The continuum of On-station to On-farm field experimentation involves an increasing level of farmer participation, but the concept of testing selected variables is still implicit. The outcome of a field experiment is a set of data interpreted so as to scientifically convincing whilst, at the same time, hopefully of practical value. The customers are interested scientists and all those who are able to access, translate and extrapolate the information provided so as better to manipulate and manage the concerned system under their particular sets of circumstances.

## 2. The agroforestry prototype system trial

A prototype system trial is a simulation of a particular "best-bet" combination of components and management features. It represents a practical attempt to examine, in an observational manner and hopefully but not essentially with some few replicates, a whole system or part of a system. The objectives may be broader and related to developing an understanding and information about the general production/service output capacity of the system. Any one observational trial needs to be continued until either the system is "mature", or nothing further can be learnt from it, and/or it ceases to have any value as a demonstration. However, a relatively early return of information for effort/resources invested is to be expected from a prototype system trial.

The choice of system to test, and its precise design, will again arise from a "D&D" exercise, but the outputs will be a demonstration of an improvement, or set of improvements, over the existing landuse system and comparative data sets of inputs and outputs. This information must convince the primary customers, who are the extension service who will need to modify and adapt to their own farmers' precise circumstances.

A prototype system trial can readily undergo modifications/ improvements season-by-season as long as the original geometry and choice of woody species has been correctly made. Clues (but not statistically validated comparisons) as to the effects of external factors such as management and seasonal climate differences on the functioning of the system will be valuable observational information of scientists visiting to refine/thier field experiments and/or make them more relevant. In some cases, it may be decided that "D&D" exercises, of one kind or another, will lead to a set of best-bet recommendations that are to be issued directly to the extension services without the need to set down a trial to test them.

## 3. Conclusions

The prototype systems trial will be especially useful where agroforestry is being developed and there is little or no previous field experience of the intended system to go on. Conducted "On-station" such a trial is an agroforestry equivalent of the agricultural "look see" experiment from which the researcher can begin to select the most important variable on which to experiment and target in on appropriate levels of each. Agroforestry field experiments and prototype systems trials are thus two parallel activities - each essential and mutually supportive. Table 1, which follows, lists the basic characteristics of a field experiment and a prototype systems trial in a comparative way.

Table 1: Comparing the basic characteristics of agroforestry experiments and prototype systems trials

| CHARACTERISTICS | FIELD EXPERIMENTS | PROTOTYPE SYSTEMS TRIALS |
|---|---|---|
| Focus | Precisely focussed | Broad focus |
| Objectives | To study carefully-selected experimental variables (components/management practices, but only a few and others are standardized). | To observe and examine how well a system (or part of a system) functions. |
| Time course | Sequential set of investigations, each to be validated. As AF systems are complex completion of investigations could take a long time. | One (or a few) designs under test until maturity. Some early returns in information to be expected. |
| Potential for modification | Very little or none for the individual experiment | As long as original geometry and choice of woody species is broadly near optimum, then seasonal changes (e.g. in management) are permissable. |
| Design features: | | |
| (1) Replication | Essential -must be adequate to satisfy particular statistical requirements | Desirable, but can be very limited. |
| (2) Size | Difficult to prevent on AF experiment from becoming large and complicated. Block size must still be kept small to minimize variability due to locational errors. | Can be kept small as long as overall plot size remain "credible" to the farmer. |
| (3) Choice of treatments | Depends on objectives - but may often be necessary to select carefully from a relatively large number in agroforestry experimentation | Trial will be made much more valuable by having paired "compare and contrast" system's situations (e.g. same components and arrangements but two different levels of management inputs). |
| (4) Background information required | From "D&D" in order to make experiment relevant. From detailed scientific literature etc. for precise selection of treatments | From "D&D" and also requires technical/ scientific "tools" from which to make a prognosis of how the system will work. |

| | | |
|---|---|---|
| Management features: (1) Manpower & equipment needed | On-station, often requires high degree of skill and (for "How" experiments) scientific equipment which has to be calibrated and maintained. Even on-farm experiments usually require some scientific back up. | Minimal - just good field staff + notebooks and camera - estimate of growth and yield, soil changes etc desirable if facilities and staff available. |
| (2) Site | Needs to be representative of region/ecoclimate and easily accessable to all experimental staff concerned and their equipment (may need electrical power, laboratory facilities etc.) | Needs to be representative of region/ecoclimate and easily accessable to extension workers and a limited number of researchers. |
| Outcome | Detailed and statistically analysable data sets relating to sometimes a few key components/management factors; sometimes has only relatively little demonstration value. | Observational and demonstrational. Data sets on input/outputs but probably relatively small amounts of information on component measurements. |
| Primary Customers | Scientific community | Extension service (and so to farmers in appropriately modified forms). |
| Secondary Customers | Extension service (but often information so precise its practical implementation in isolation is difficult). | Scientific community |
| Extrapolability | If a simple "What happens" experiment then this is limited. If a "How does it happen" experiment (i.e. with appropriate sets of highly focused treatments and adequate sampling/instrumentation) than extrapolation should be possible | Prototype trial has to be designed to represent an improved landuse system broadly-suited to an area based on which local modifications can readily be made by the extension services. |
| Priority | Needed for the sound and long-term development of the AF System and to further the understanding of how, generally, to design and manage woody/non-woody plant associations productively and on a sustainable basis. | Especially needed where such AF systems have not been tried before (and it may not even be clear what the experiments should be), where local scientific information is sufficient to "try-out" a seemingly practical proposal, and where (if sufficient is known) a demonstration is required. |