Food and Agriculture Organization
of the United Nations

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FACILITATING SCIENTIFIC METHOD[1]

as follow-up for FFS graduates

ABOUT THIS MANUAL

Studies conducted by farmers are and will be a driving force for community-IPM in Asia. Farmer studies are sources of innovation and provide a vital evaluation of location-specific variables. Experience has shown that farmers are eager to share and distribute their study results in meetings and through networks for the benefit of others.

This manual is meant for those involved in facilitating farmer studies to farmer field school graduates. The tools and principles provided here improve planning and analysis of field studies and prepare trainers to facilitate scientific method to farmers. The manual is for study by individual trainers or for discussion at technical workshops for trainers. It is also recommended for use at season-long training-of-trainers courses where trainees have the opportunity to conduct field studies. This manual only discusses the methodological aspect of farmer studies. Equally important are organisational and social aspects of farmer studies; these are discussed in detail in a separate contribution, "Farmer Field Research: An Analysis of Experiences from Indonesia".

WHAT IS SCIENCE

To facilitate scientific method, start by asking the participants what science is. Responses could vary from “research with lots of replicates”, to “learning through experiments”, “studies to solve problems”, “knowing through observation”, etc. The objective is to determine that, essentially, science is not reserved for professional scientists, but that farmers are able to do

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science and may have been doing science without realising it. It is useful to compare "farmers doing science" with "farmers receiving technology".

SCIENCE AT THE FARMER FIELD SCHOOL

Ask how farmers first learned to do science in the farmer field school (FFS). In the farmer field school farmers learn about the basic principles and processes in their crop ecosystem. They do simple studies, compare treatments, and learn through their own observations. They learn to ask questions and to answer these questions by finding out themselves. In other words, the farmer field school teaches a scientific attitude.

SCIENCE AS FOLLOW-UP ACTIVITY

The farmer field school functions like a primary school, after the completion of which more serious or more applied activities can start. Experience has shown that FFS graduates often require follow-up training to develop their newly acquired knowledge and skills according to the local circumstances. Farmer studies is a key follow-up activity; community-level planning is another important follow-up activity. Specific training on field study skills help farmers to conduct studies in an independent and sound manner.

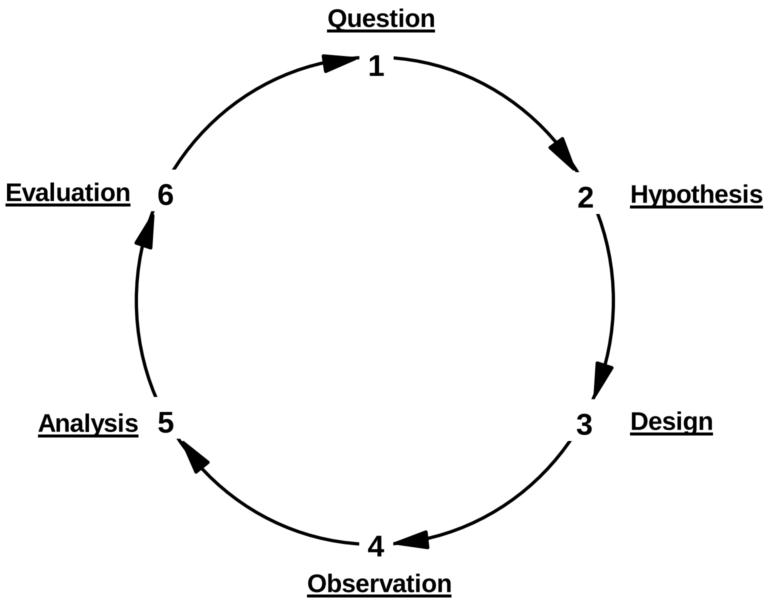
THE FACILITATOR'S ROLE

The facilitator plays a crucial role in farmer studies. If he guides too much, the study will be planned how *he* had in mind, not according to the participant farmers. This is a common problem. Even though the farmers do participate, they are not in charge at the planning stage, and will not have the sense of 'ownership' over the study. When asked about why they are doing such-and-such, they may answer “because the trainer told us so”, but strictly speaking, the study is not theirs. Conversely, if the facilitator plays a laissez-fair role, leaving everything up to the farmers while keeping to the background, some opportunities for learning will be missed.

The key, it seems, is to provide <u>tools</u> for guiding farmers to use the right methods, and to introduce basic <u>principles</u> for enriching content and understanding. If used correctly, these tools and principles bring forward the potential skills, creativity and knowledge of the group of the participating farmers. This is what facilitating is about. To facilitate farmer studies a certain confidence is necessary, which is gained through regular practice and through direct involvement in field studies.

THE LEARNING CYCLE

Ask the participants what different steps are needed for conducting a field study, using an example situation. Which is the first step; how does it start? After that, what next step should be taken, and so on. The chart depicts six essential steps for conducting a study. It resembles an experiential learning cycle, adapted for use in a field study. The following sections elaborate on each step. Tools and principles be introduced.



1. QUESTION

As field persons, we have to learn to ask questions about our crop ecosystem that require answers. Only if we are curious and eager to find out something about the world around us can we be scientists. This curiosity is encouraged among professional scientists but not among farmers and trainers because projects and programmes expect them to follow their recommendations (as formulated by the 'professionals'). Training and experience is necessary to develop 'scientific' curiosity as one of our senses through which we ask questions to be tested by our own observations. By doing a study new questions emerge, which close the learning cycle.

To help farmers formulating a question about their crop, the Topic-Selection Matrix is introduced. This tool is appropriate when a field study is planned. It has five columns to be worked out by the participants. In the first column the problems or the causes of low yield are listed. In the second column the current farmers' practice is described for each problem. The potential for improvement of each practice is described in the third column, and what constraints the improvement in the fourth. The suggested topics for study are put in the last column. Finally, the group discusses the selection of the best topic while taking into account the potential for improvement and the constraints. An example of Topic-Selection Matrix is given below; the selected topic in this example is on the use of urea.

Topic-selection Matrix

Problems (or, causes of low yield)	Current practice	Potential for improvement	Constraint	Suggested topics
Poor establishment of seedlings	*Broadcast seeding* *Uncertified seed*	*Transplanting may be better* *Certified seed*	*Extra labor not available* *Cost*	- *Seed comparison*
Improper application of N	*Low use of urea*	*More urea may improve yield*	*Probably increases costs*	*Use of urea*
Weeds	*2x mechanical weeding*	*Increased weeding* *Increased flooding*	*Labor costs* *Lack of control over irrigation*	*Intensity of weeding* -
Rats	*No control*	*Area-wide baiting; studies*	*Time, cost, collaboration*	-
And on…				

The Topic-Selection Matrix is meant for farmers who are planning a field experiment, but not for a more detailed kind of study (e.g. on some aspect of the biology of a certain insect). Another method of planning is so-called 'Participatory Planning', which is a more comprehensive process of community-level planning; a field study is just one of its possible outcomes.

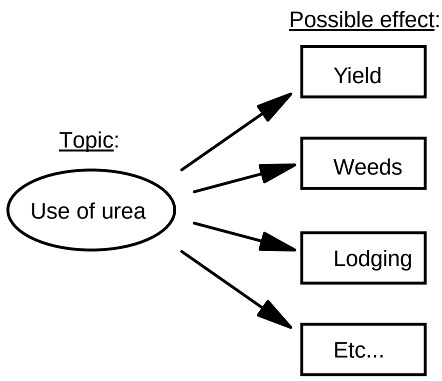
The Topic-Selection Matrix is problem-based. As an alternative to listing the problems, the agricultural operations are listed in the first column, from seeding through planting till harvest. This method is considers all stages of farming to help farmers select their topic for study, but it is lengthier.

Agricultural operation	Current practice	Potential to improvement	Constraint	Suggested topics
Land preparation	*Shallow ploughing*	*Deep ploughing*	*Disk plough expensive*	-
And on…				

2. <u>**HYPOTHESIS**</u> (IDEAS TO BE TESTED)

After selecting the topic for study, farmers have to specify what exactly they want to find out. They have to define a hypothesis, an idea which needs to be tested. For example, a scientist who selected 'the use of urea' as the topic for his study in rice thinks that "more urea will produce more yield". This is his hypothesis; this is the idea he wants to test. This particular scientist only has a single hypothesis ("urea will affect *yield*"). Possibly because he was busy or not very interested, he failed to ask if there are other positive or negative consequences of the increased use of urea on the crop, on the ecosystem, or on the costs made by the farmer. Changing one aspect of the agro-ecosystem may influence several other variables, either directly or indirectly.

It is better to start a study by having not just one main hypothesis but also several alternative hypotheses. For instance, urea may encourage weeds, demanding intensified weed control, and urea may also cause lodging. Considerations like these should be taken into account when planning a study. Only after determining the various hypotheses or ideas, we are able to do a thorough study which addresses all possible aspects associated with –in this example– the increased use of urea.



The Idea Matrix is a tool which encourages farmers to consider all possible effects of the selected topic; it avoids the single hypothesis. The Idea Matrix is prepared *after* a topic for study has been determined. It consists of three columns. In the first column farmers describe their ideas about the selected topic, by asking: "*What possible influences will the topic of the study have on the crop system as a whole?*". These ideas should address influences on the crop, on the ecosystem, or on social and economic aspects (see example below).

In the second column farmers specify the source of these ideas; some ideas may be proven facts, others just thoughts not based on any facts, or they may be proven under different circumstances. In the third column farmers write what they think about each idea; is it true, is it reliable, is it relevant or applicable to the local situation; this is to determine whether the idea needs to be tested.

Idea Matrix on "Use of urea"

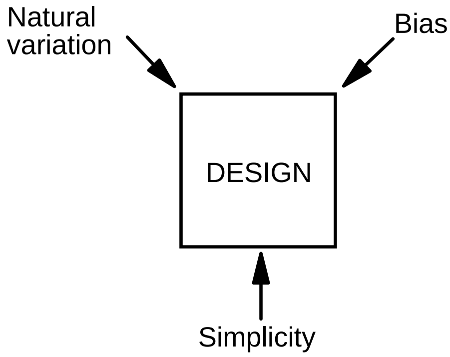
Ideas (*"What possible effects will the topic of study have?"*)	Source of each idea	What do we think about each idea? (*"Does it need to be tested?"*)
Increased use of urea will increase yield of rice	*Extension officer*	*Not convinced; needs to be tested locally*
Weeds increase with more urea	*Other farmers*	*Probably; need to observe*
Plant growth and tillering will increase with increased urea	*Experience of one of the farmers*	*Surely, but to what extent?*
With increased urea, other nutrients become deficient	*Experience from FFS*	*Need to observe*
More urea will be washed out into the canal	*Newspaper*	*Yes, but how to observe?*
Certain pests might become more dominant	*One of the participants*	*Not everyone agrees; need to observe*
Natural enemies might feel more at home in taller plants	*Just a thought*	*Not everyone agrees; need to observe*
More labor and money is required to apply the extra fertiliser	*Farmers' provisional calculations*	*Needs to be tested*

A Idea Matrix is of central importance for a study. These are the ideas which need testing. Farmers can use this matrix as a basis to plan their observations: Are yield samples sufficient, or should additional observations be made on weeds, plant growth, and insect levels? After completion of the study, the test results for each of the ideas is evaluated. Therefore, farmers should retain the Idea Matrix throughout the length of the study.

3. DESIGN

The optimal design for a field study depends on the topic of study, on the condition and size of the field, and on the intensity of the study. Hence, no standard design can be given. Instead, farmers should understand the basic principles of field design to allow them to do the designing by themselves.

Three principles are important for the design of a field study: Natural Variation, Bias and Simplicity. If farmers consider these principles they are able to design better experiments.

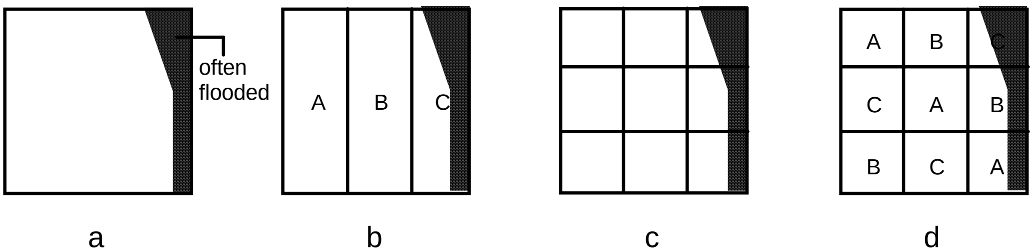


a. Natural variation

Natural variation is found between plants within a plot, between different parts of a plot, and between field plots. A study should compare treatments under the same conditions. However, field conditions are never uniform, and this can confuse the study results. Farmer researchers should understand the natural variation in their fields in order to design better experiments.

To facilitate the principle of natural variation, one could start by asking the participants to examine a field plot (or a square piece of lawn) to observe the different types of variation within that plot, present it in a drawing and explain what causes the variation. Farmers may mention differences in land level, plant stand, weed density, soil compactness, soil fertility, non-uniform drainage or water supply. Thus, there are different types of natural variation; some are causes, others are effects. Discuss how natural variation interferes with the experiment and why it is important to reduce natural variation.

To deal with the problem of natural variation, the following steps (illustrated in the diagram) are suggested.



i First, it is important to select a field plot which is as uniform as possible. At the time of planting, however, some sources of variation may be hidden (e.g. soil fertility, compactness, seed bank of weeds).

ii Suppose we selected the plot in Figure a. Part of the field is regularly flooded whereas the other part remains dry. This is one source of variation. (In reality, there are several sources of variation, some of which hidden.)

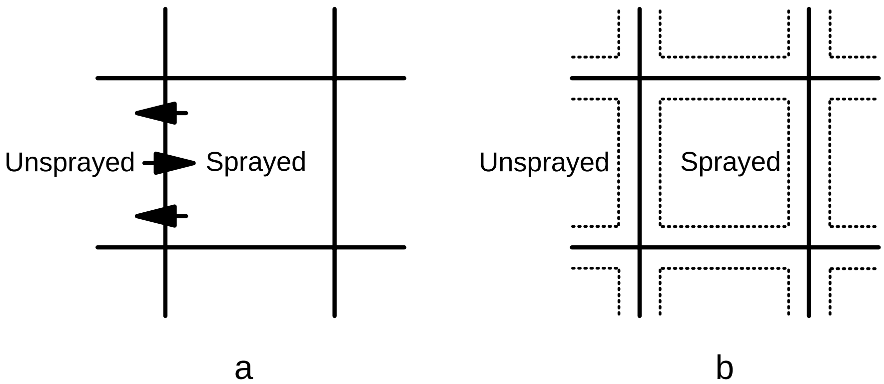
iii To design a study on urea, we could divide the plot into three parts, or three treatments: 0 kg urea, 50 kg urea and 100 kg urea/ha. But, what is the problem with the design in Figure b? How will the results be affected?

iv What can be done to overcome this problem? We could draw the treatments horizontally but there may be sources of variation unknown to us as we make the design. Better if we could replicate each treatment (Figure c).

v Replicates of a treatment should be distributed evenly over the plot, in good as well as bad parts of the plot. Hence, the different replicates give a reasonable representation of the entire plot. Treatments may be distributed randomly or regularly over the plot, but for small studies with few replicates (as in farmer studies) a regular distribution is recommended. In a regular distribution, treatment plots do not border other plots of the same treatment (see Figure d).

b. <u>Bias</u>

A treatment plot which is bordered by a plot with another treatment may well be affected by the neighbouring treatment and thus become biased. Bias, or interferrence, influences the quality of our results and occurs in the form of insecticide drift, fertiliser drift, movements of insects, etc. The principle of Bias is best illustrated in a study on pest management, as explained below.



i Suppose the central treatment plot is sprayed (Figure a), but it is bordered by an unsprayed control. What problems do you foresee? Spray may drift, pests may move away from the sprayed area, or natural enemies may get trapped in the sprayed area. As a result, the control is no longer a pure control but it has become biased.

ii Do you expect bias in a study on fertilisers? How about a study on plant spacing? The extent of bias apparently depends on the topic of study and the type of treatments.

iii How can we overcome bias? First, bias can be reduced by increasing *plot size*. A study on pest management, where bias is strong, would require larger plots than a study on plant spacing. Second, bias is most important near the border of a plot and, to reduce bias, we could leave a *border zone* (around 1-2 m at each side) unsampled while we restrict our samples to the centre portion of each plot (Figure b). If we expect a bias through water flow (e.g. fertiliser drift) we should erect *bunds* as a barrier between plots (beware not to use topsoil for the bunds).

c. <u>Simplicity</u>

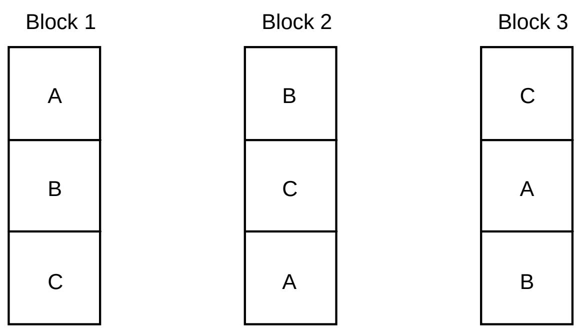
The study design should be kept as simple as possible. There is a value in simplicity because it implies clarity of results. A simpler design allows for more intensive and more comprehensive observations than a complex design and leads to stronger conclusions.

First, the experiment should address only a single aspect, or <u>factor</u> (e.g. the factor 'urea dosage'). If we compare a combination of factors, for example fertiliser A at close plant spacing with fertiliser rate B at wider spacing, we are unable to understand the role of each factor (was yield increase due to the fertiliser rate or due plant spacing?). To increase understanding, we need to study the factors one-by-one: for example, with a study on fertilisers or one on plant spacing. In special cases it is justifiable to combine two factors in one study (for instance to compare variety A at close spacing with variety B at wider spacing), but only after each factor was previously investigated in separate, single-factor studies.

Second, the <u>number of treatments</u> should be kept to a minimum or the study becomes too complex, which jeopardizes the quality of observations and conclusions. Only 2 to 4 most important and most distinctive treatments should be considered. Ask critically about the relevance of each treatment. The control is the treatment against which the other treatments are compared; it could be the current practice or the treatment where a certain practice is *not* applied (e.g. no spray).

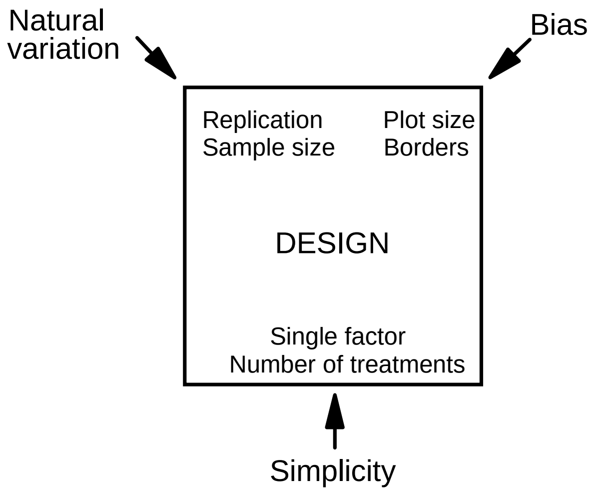
Third, the number of replicates (when farmers decide to use replication at all) should likewise be kept to a minimum. Three replicates are optimal for farmer studies (and allow for the use of the "Overlap Test", as discussed in the section on Analysis). A 3-by-3 design (3 treatments, 3 replicates) is generally a good compromise with regard to limited plot sizes, within-field variability, and ease of observation and analysis by farmers.

We have already discussed the importance of selecting a uniform field for our design. But farmers' fields are frequently too small to be divided into different plots, while reducing the plot size only increases the level of bias. Therefore, there may be a need for 'blocks' in certain situations. A block is a complete set of treatments (see diagram) which is separated from other blocks. Because of the separation, each block has its own natural conditions (e.g. different elevation, different soil, different timing of irrigation). Hence, by using blocks we increase the extent of variation in our study results which



makes it more difficult to obtain clear results. Moreover, the actual *effect* of a treatment, say, the use of urea, is not constant but may well depend on the conditions in each block: If one of the blocks was irrigated too late, urea would not have its normal effect on the crop, in contrast to the other blocks with timely irrigation. It is advisable to avoid the use of blocks, if possible, by using a uniform plot in one location.

In summary, the design of a study is influenced by three principles. An understanding of Natural Variation helps farmers deciding on the need for replication and on the sample size (to be discussed in the next session). An understanding of Bias helps them to plan the appropriate plot size and border zone. Finally, the principle of Simplicity helps to reduce the design to the essentials only, with a single factor and a limited number of treatments. This summary is illustrated in the following diagram.



4. Observation

What should be observed? How should observations be made? When should observations be made? These considerations require careful planning by farmers.

To determine *what* should be observed (what kind of observations should be made) we utilise the Idea Matrix in which we already identified the different components of the ecosystem that need to be considered in our study. If we expect that increased urea influences any other component of the crop ecosystem (weeds, insects, etc.) these components should be observed. We often realise only afterwards that we did not consider a certain component, and thus an opportunity for learning is lost.

How to make observations depends on what is practical and what is accurate. Plant height measurements, for example, require observations of individual plants, while yield measurements are best taken by crop cuts (the larger the area the better). Whatever we observe, our samples should give us a reasonably accurate estimate from each replicated plot, realising that there is variation between plants, and between the different parts of each plot. A representative sample consists of a number of observations; and this number depends on the type of observation. In case of individual plant observations (with clear variations between plants) a sample must consist of at least 10 plants per treatment in order to be representative. But for large yield measurements (e.g. 5 x 5 m), one crop cut at the centre of each plot replicate will suffice.

When to make observations depends on the type of observations. Yield measurements are taken at crop maturity or at harvest, while observations on weeds may be most important during the early crop stages. Observations on insects, diseases and plant development are ideally made weekly during the entire season because their incidence and condition change.

These three aspects –what, how and when to observe– can be planned using an Observation Matrix. An example of an Observation Matrix based on the Idea Matrix of the study on increased use of urea is given below. Explain to the participant farmers the three columns of the matrix to enable them to plan their own observations. An example:

Observation Matrix on "Use of urea"

WHAT should be observed?	HOW?	WHEN?
Yield	*5 x 5 m crop cuts*	*At harvest*
Weeds	*50 x 50 cm samples*	*Weekly during first 5 weeks*
Plant length	*Observe 15 hills per treatment*	*Weekly*
Number of tillers	*Observe 15 hills per treatment*	*Weekly*
Insects/diseases	*Observe 15 hills per treatment*	*Weekly*
Natural enemies	*Observe 15 hills per treatment*	*Weekly*
Inputs	*Calculate and record costs*	*When inputs are made*

Different types of observations produce a complexity of records. Separate records should be kept per treatment, and records of each sampling occasion should be summarised. At the end of the season, records could be summarised over all sampling occasions (to produce a seasonal *average* of, say, stemborer incidence; or the seasonal *maximum* of plant height) to allow for easy comparison between treatments.

5. ANALYSIS

Replicates are necessary to confront natural variation in farmers' fields. Replicates function like different measurements of a treatment obtained from different locations of the field plot. Each measurement gives a different result due to natural variation, but the average of all measurements provides a reasonable sample of the field plot under that particular treatment.

Not only the average is important. It is equally important to understand the variation between the individual measurements. Highly variable measurements are suspect and should be treated with caution before any conclusions are drawn. Uneven field conditions or poor observations can obscure our results. To determine whether two treatments have clearly different results we need to examine the variation between the measurements of each treatment. If variation is not inspected, premature or faulty conclusions may be drawn as regularly happens in farmer studies. Therefore, a statistical tool, the "Overlap Test", was developed for farmers. The test consists of two steps (see worked-out example below).

i Is the difference between treatments large?

ii Is there any overlap between minimum-maximum ranges of treatments?

In step 1 the average is calculated (or the sum, if that is easier) for each treatment to determine whether the difference between treatments is small or large. In step 2 we examine how variable or how uniform the measurements are. For each treatment we determine the replicate with the minimum value and the one with the maximum value. If the minimum and maximum values are close together, the variation between samples is limited. If, however, the minimum and maximum are far apart, they are likely to overlap with the minimum-maximum range of another treatment. In case of an overlap between treatments (or in case the same value occurs in both treatments), it is concluded that the results of those treatments are *not* clearly different. To facilitate Step 2, the following steps are suggested.

i Draw a horizontal scale; indicate for each treatment the minimum and maximum value, connected with a line; what does the line indicate?

ii Determine whether the treatments overlap or not; what does this mean? What do you conclude from the drawing?

The Overlap Test helps farmers to draw better conclusions from their study results. If data are uniform (with little variation between replicates) we may find a clear difference between treatments, but if data are highly variable a difference between treatments is easily obscured by an overlap. Uniform plots and intensified observations reduce the extent of variation to give clearer and more convincing results. The test is easily used for analysing measurements of yield, but could also be used for other types of measurements (e.g. plant height, insect incidence) if summarised on a *per-plot* basis.

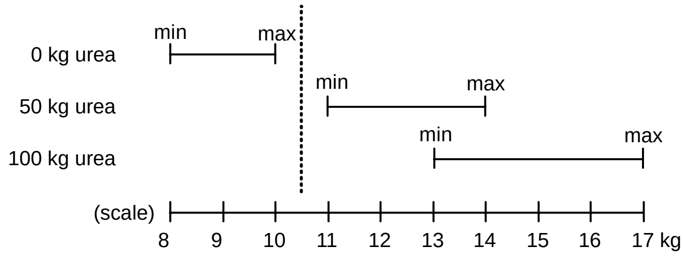
Box 1: A worked-out example of the Overlap Test

Suppose farmers conducted a study on increased use of urea in rice, with three treatments: 0 kg urea, 50 kg urea and 100 kg urea per ha, each treatment with three replicates. At harvest, yield measurements (in kg per 5 x 5 m) are taken from each replicate. The following table of results is prepared:

Treatment	Replicate 1	Replicate 2	Replicate 3	(Average)
0 kg urea	8	10	9	(9 kg)
50 kg urea	11	14	13	(13 kg)
100 kg urea	13	17	15	(15 kg)

Step 1: Is the difference between treatments large? Averages are calculated for each treatment. Yield in treatment 1 is almost half that in treatment 3. Yield in treatment 2 is in between the other treatments.

Step 2: Is there an overlap between the minimum-maximum ranges of treatments? To answer this question the farmers draw a horizontal scale from the lowest to the highest observed value (from 8 to 17 kg), and for each treatment the minimum-maximum range, as shown in the following figure.



For 0 kg urea, the minimum value is 8, the maximum value 10. For 50 kg urea, the minumum is 11, the maximum 14. For 100 kg urea, the minumum is 13, the maximum 17. It appears from the constructed figure that there is no overlap between treatment 1 and the other two treatments; treatment 1 can be separated from the other treatments by the dotted line. Therefore, it can be concluded that 0 kg urea gives a clearly lower yield than either of the other treatments with urea.

Treatment 2 and treatment 3 have overlapping minimum-maximum ranges; treatment 2 has a maximum value that reaches within the value range of treatment 3. Because of this overlap, it is concluded that treatment 2 and treatment 3 are not clearly or convincingly different. This study has not proven that 100 kg urea produces higher yields than 50 kg urea. Even though the average for treatment 3 is higher, it is not convincingly higher, and may be due to natural variation in the field. This experiment showed too much variation to be able to differentiate between treatment 2 and treatment 3. More uniform plots and/or more detailed observations could reduce this variation.

Box 2: When using 'blocks'…

Suppose farmers conducted a study on increased use of urea in rice, as in Box 1. But instead using a uniform plot at one location, the study was divided into three separate sub-studies or 'blocks', each in a different field at some distance from one another. A block is a complete set of the three treatments. And suppose that the following yield results were obtained:

Treatment	Block 1	Block 2	Block 3	(Average)
0 kg urea	6	12	9	(10 kg)
50 kg urea	8	15	16	(13 kg)
100 kg urea	11	19	16	(15 kg)

Obviously, block 1 was in a field with much poorer growing conditions than block 2 and 3, and this difference between blocks inflated the amount of variation in our data. When using the Overlap Test on these data, we find an overlap between all treatments, suggesting that urea causes no *convincing* yield increase. Try drawing the graph as in Box 1 and observe how the treatments overlap.

We know, however, that part of this large variation was due to the different natural conditions in each block. In situations where we have distinct blocks, we can use what we shall call the '<u>Consistency-test</u>', which is a different way of looking at natural variation. With the Consistency-test we examine whether a treatment is *consistenly* better or worse than other treatments across the 'blocks'. It is less accurate than the Overlap Test, but better than not looking at variation at all!

Look at each separate block: which is the 'losing' treatment and which the 'winning' treatment? Treatment 1 (0 kg urea) is the 'loser' in block 1, block 2 *and* block 3 (see underlined data); it is a *consistent* 'loser' and therefore, according to our test, *clearly worse* than the other treatments. However, there is no consistent 'winner' in our results: Treatment 3 (100 kg urea) is winner in block 1 and block 2 (see bold data), but block 3 has no winner at all. Therefore, the decision of the Consistency-test is that urea clearly increases yield (because 0 kg urea is the consistent 'loser'), but there is no clear difference between 50 and 100 kg urea.

Treatment	Block 1	Block 2	Block 3
0 kg urea	<u>6</u>	<u>12</u>	<u>9</u>
50 kg urea	8	15	16
100 kg urea	**11**	**19**	16

The Consistency-test makes reasonable decisions for studies with 2-3 treatments and 3 replicates, but as soon as we add treatments or replicates the chances of having a consistent 'winner' or consistent 'loser' decrease. This is important to realise. But if we were to compare, say, the IPM and Farmer Practice treatments across a large number of sites, we could use the Consistency-test in a more 'loose' manner, to determine whether the difference between treatments is 'slightly consistent' or 'very consistent'.

The Overlap Test resembles conventional statistical methods (*t* test at $P < 0.05$) if we use *three* replicates. But in case of four or more replicates, the test is less accurate and should not be used[2]. Experience has shown that farmers, though not all farmers, are able to understand and use the Overlap Test after some exercise. Farmers are usually quite aware of sources of variation in their fields, and often go to great lengths to explain differences in crop stand or plant vigour within their fields.

In those cases where the study is divided into separate 'blocks' (see section on Design), each with its own natural conditions, an alternative test is used to examine the data. This so-called 'Consistency-test' is explained in Box 2.

6. EVALUATION

Different types of observations produce different types of results, such as yield measurements, plant growth measurements, insect levels, and input costs. After all observations have been made, an evaluation of the complete set of data is necessary in order to draw final conclusions. Weekly observations must be summarised to provide a single average (or maximum[3]) value for each treatment.

An Evaluation Matrix helps to evaluate the data-set. It evaluates the ideas formulated at the start of the study (from the Idea Matrix), by adding the test results and drawing a conclusion about each idea.

Ask the participants to explain the various results (e.g. why was plant growth and weed density affected by urea; why were pests more common in treatment 3). One could also ask: Which differences were observed between the treatment with the highest yield and the one with the lowest yield? An example of an Evaluation Matrix is given below.

Economic analysis is important but, to avoid complexity, it should consider only the costs which *differ* between the treatments (e.g. input of urea & labour cost to apply the urea). The differences in cost are then compared against the outputs (value of harvest) of the treatments. (Bear in mind, however, that in the example there was no clear yield difference between treatment 2 and 3, and hence there was no clear difference in their outputs). Calculations are either on a per-plot basis, or per standard unit area (acre,

[2] The more replicates the more *likely* there will be some kind of extreme result from one of the replicates causing an overlap between the treatments -- even if the treatments are convincingly different.

[3] E.g. in case of plant height measurements.

ha). The treatment with the highest yield is not necessarily the one which is most beneficial.

Evaluation Matrix on "Use of urea"

Ideas to be tested (at the start of the study)	Results: Treatment 1: 0 kg urea	Results: Treatment 2: 50 kg urea	Results: Treatment 3: 100 kg urea	Conclusion
Increased use of urea will increase yield of rice	*9 kg per sample*	*13 kg*	*15 kg*	*Urea increased yield but no clear difference between 50 & 100 kg*
Weeds increase with more urea	*10 per sample*	*12*	*15*	*True, especially at the highest dosage*
Plant growth and tillering will increase with increased urea	*65 cm; 12.1 tillers*	*74 cm; 15.5 tillers*	*77 cm; 16.5 tillers*	*Adding more urea increased plant height and tillers*
With increased urea, other nutrients become deficient	*Casual observations did not indicate a deficiency*			*Studies on other nutrient are required*
More urea will be washed out into the canal	*No data*			*This was not tested*
Certain pests might become more dominant	*Few pests, but slightly more leafhoppers and stemborers in treatment 3*			*A tendency towards increased insects at high dosage of urea*
Natural enemies feel more at home in taller plants	*Almost the same in all treatments*			*No clear effect*
More labor and money is required to apply the extra fertiliser	*No extra inputs*	*Extra inputs Rp 10,000/ha*	*Extra inputs Rp 17,000/ha*	*Most inputs required at 100 kg urea*
Any added ideas….				

In drawing the final conclusion of the study, the farmers should not only consider their records, but also social aspects (e.g. labour availability), environmental pollution and human health. These aspects can be in conflict with an increased economic benefit.

Finally, now that the study has been completed, it is important to ask:

1. Which aspects remain unknown?
2. Which new questions are raised, and how could they be addressed?