Learning on the Run

Incorporating trials and experimentation into the management of natural resources

South Australian Murray-Darling Basin Local Action Planning Groups

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Introduction

Introduction

Natural resource managers operate in an environment of rapidly changing and emerging issues and opportunities.

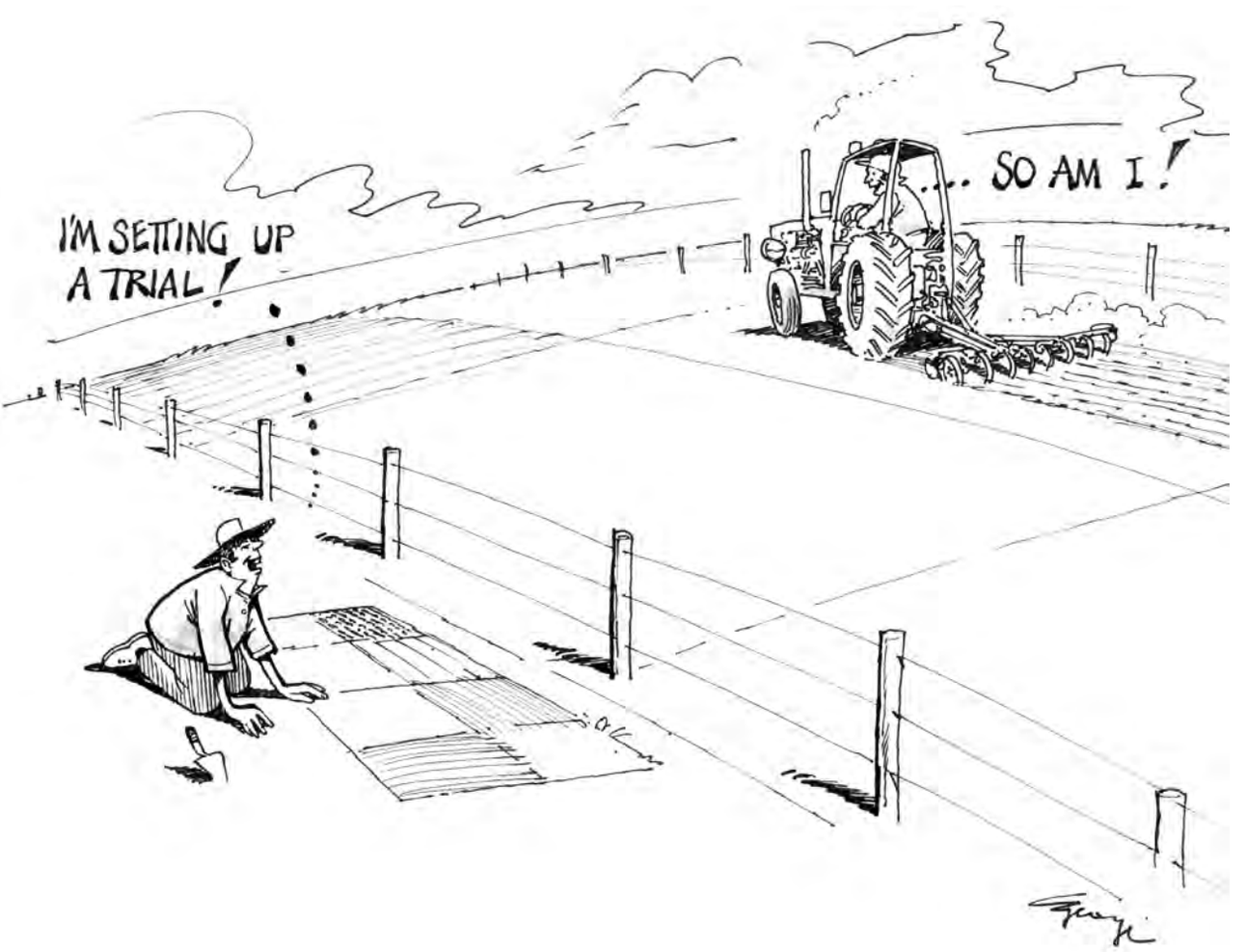
In response to the challenges of controlling pest plants and animals, stabilising erosion, restoring remnant vegetation, managing wetlands, protecting cultural sites and assets, recovering threatened species, revegetating cleared land or practising sustainable agriculture, natural resource managers regularly try new management techniques and compare different management options. The results of these 'trials' are then used to inform and refine their subsequent management activities.

These guidelines, which will lead you through the process of planning and designing a sound trial, aim to support and improve this approach to natural resource management by ensuring that the results used to support decision making are as correct and reliable as possible.

The guidelines are designed to be used for both setting up discreet trials or incorporating experimentation into full-scale management activities. They can be used for all natural resource management issues and are equally as relevant for setting up small on-farm trials as they are for establishing large regional-scale investigations.

The skills you gain by following these guidelines and incorporating well designed trials and experimentation into your management activities will help you:

- gain ***greater confidence*** in the results / outcomes of your trials;
- ***learn more about the system*** you are managing;
- ***learn more quickly*** about the system you are managing;
- make ***more efficient use of the resources*** available to you;
- make a ***contribution to the knowledge*** base and professionalism of natural resource management;
- ***reduce the risks*** associated with trying new things, and;
- ***empower yourself***, your organisation and your local community to tackle their own issues and answer their own questions.



These guidelines can be used for setting up small-scale trials or for incorporating trials into your full-scale management.

How to use these guidelines

How to use these guidelines

These guidelines provide a step-by-step process for planning, designing and undertaking natural resource management trials. They do not describe how to statistically analyse results from the trials—this is beyond the scope of the guidelines, as the analysis process can be complicated.

The guidelines are designed to be used by all natural resource managers dealing with a broad range of issues and questions. They contain examples to help illustrate the steps and concepts along the way. The guidelines progress through a series of questions and important information relevant to developing a trial. Your responses to these questions should be recorded on a photocopy of the *Trial planning and design record sheet* at the back of these guidelines (Appendix 1–page 59). These answers will be used in the final stage of designing your trial and will also be essential for documenting the rationale behind your design. This process will provide you with a rigorous, well thought out and well documented trial design.

It is recommended, especially when planning a large trial, that you have your plan checked by someone with a good knowledge of statistics. A statistician may help with the type of data that could be collected and the analysis that will need to be undertaken. This is especially important for large, expensive trials.

The guidelines are divided into **four** main sections. These sections lead you through all stages of planning, designing and undertaking a trial. The four stages are as follows:

Effective trials—the 8-Step Cycle

This section describes the 8-step cycle of an effective trial which involves assessing the problem/opportunity, setting an objective, planning, design and implementing the trial, collecting data, and evaluating and reviewing the results. This section gives an overview of the entire process and provides context for the more detailed information presented in Stages I, II and III.


Effective trials—
the 8-Step Cycle

Planning
your trial

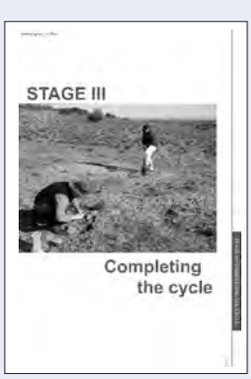
Stage I–Planning your trial

Stage I leads you through information and a series of questions that are important to consider when planning a trial. It focuses on assessing the problem/opportunity, setting the objective and planning the trial. It also describes the essential principles and concepts of experimental design. A record sheet is included at the back of these guidelines in Appendix 1 (page 59) to record your answers to the questions asked in this stage.

Finalising
your design

Stage II–Finalising your design

Stage II uses the information generated in Stage I to help you select between a number of common trial design types. For each of these design types, example layouts are presented for a range of different trial scenarios.



Completing
the cycle

Stage III–Undertaking the trial

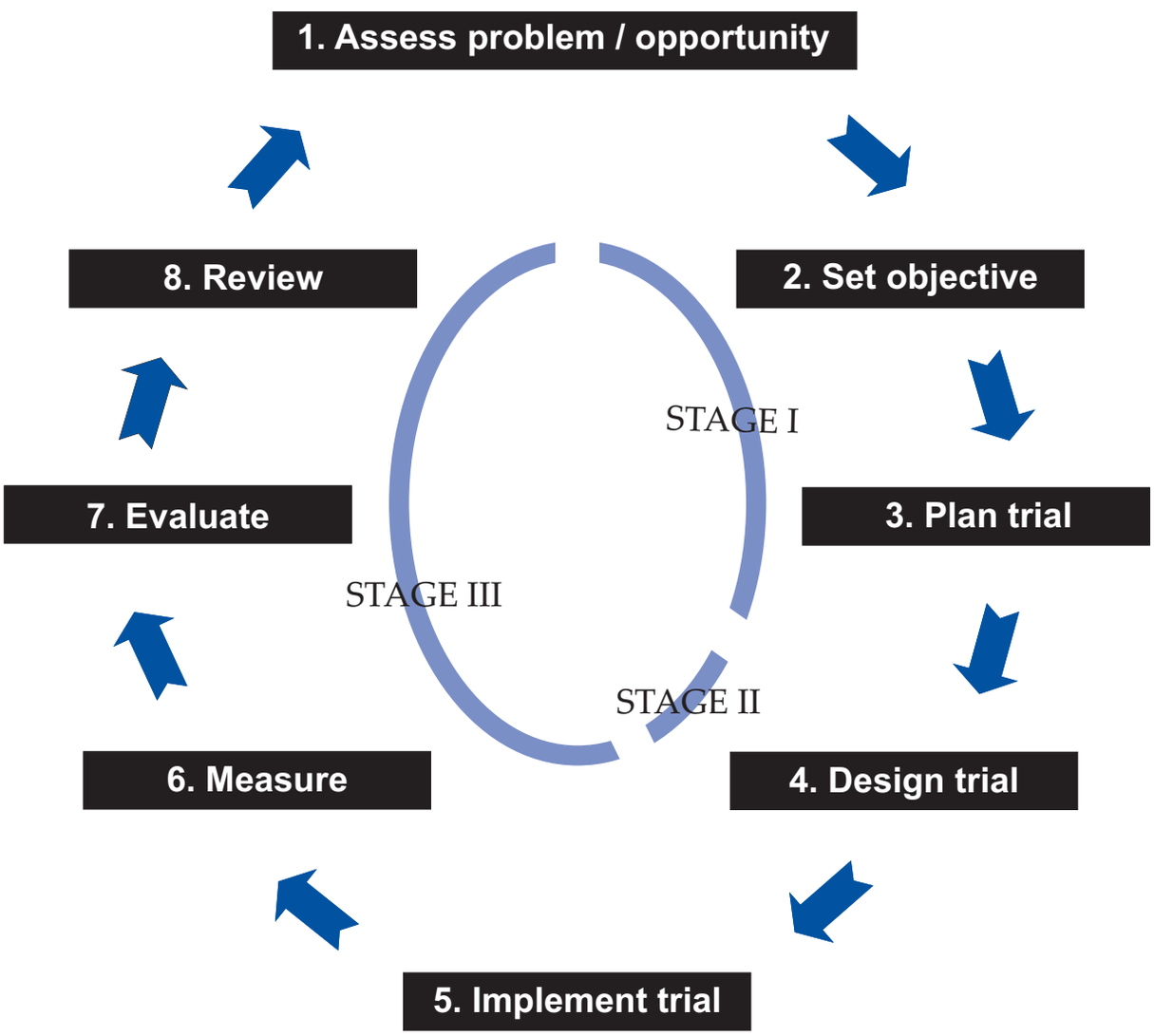
Stage III leads you through the steps of setting up the trial, collecting and analysing data, interpreting and evaluating the results and reviewing the outcomes in terms of your management objectives and options. The 8-step cycle may then be started again with this improved knowledge from the original trial.

Effective trials— the 8-Step Cycle

Effective trials—the 8-Step Cycle

There are eight critical steps involved in designing and implementing an effective trial. The steps start with the initial identification of a problem or opportunity and end when the trial results are reviewed and incorporated into management activities and future trials. Therefore, the eight steps form a cycle that may continue with further trials or experimental management.

Following the entire cycle encourages a thorough and disciplined approach to trial design, implementation and evaluation. Specific details on assessing the problem / opportunity, setting the objective and trial planning are given in Stage I. Details on designing your trial are given in Stage II. Information on implementation, taking measurements, evaluation and review are provided in Stage III.



Step 1—Assess the problem / opportunity

Trials are undertaken in an attempt to overcome a problem or to investigate an opportunity. To achieve a good understanding of the problem or opportunity it is important to understand what is already known, and what is not known, about the issue. This research will help to focus on the specifics of the question you want to answer.

Stage I (page 13) provides more information on assessing the problem/opportunity.

Assessing the problem of wheel cactus infestation and the limited options for control.

Step 2—Set the objectives

It is important to clearly specify the objectives of the trial. Sufficient time must be allowed for this step, and it may be necessary to revisit and reassess the objective at different stages of the trial. It is important to consult with everyone involved in the trial when setting the objectives. It is also important not to set too many objectives for any single trial—keep things simple or split one large trial into several smaller trials, each with a specific objective. Having simple objectives will help in the later stages of planning, designing and undertaking the trial.

Stage I (page 13) provides more information on setting the objective.

It is important to discuss the objectives of the trial with the stakeholders.

Step 3—Plan the trial

Planning a trial involves considering the concepts of experimental design (replication, randomisation, etc) as well as the practicalities of what to try, where to try it, and what to measure or what data to collect.

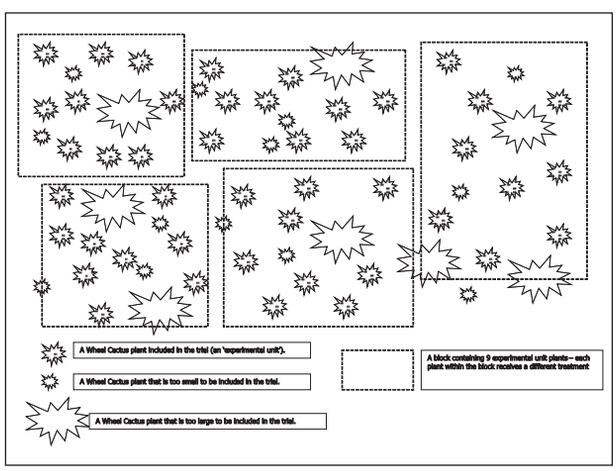
Stage I (page 13) provides more information and concepts for the trial planning process.

Good planning is one of the keys to a successful trial. Here the trial to compare wheel cactus control methods is being discussed.

Step 4—Design the trial

A small number of standard trial designs are suitable for most natural resource management trials. Having gone through the planning process it will be easy to identify which trial design type suits your situation.

Stage II (page 33) provides information on a number of different trial designs.



The success of a trial also relies on a sound trial design. Here is the design used for the wheel cactus control trial.

Step 5—Implement the trial

Having followed the previous four steps, the next step is to implement the trial, knowing that it is appropriately designed to answer the management questions of interest.

Stage III (page 51) provides information on implementing

the trial.

Implementing one of the treatments to be investigated in the wheel cactus control trial.

Step 6—Measure

The trial has now been established and measurements can be taken to determine the effect of the different treatments. As part of the trial planning process (Stage I), what to measure and when and how the measurements are to be taken would have been previously decided.

Stage III (page 51) provides more information on taking measurements for the trial.

Taking initial measurements of one of the cacti treated in the cactus control trial.

Step 7—Evaluate

Having collected relevant data it is time to analyse and understand the trial results. This may involve statistical analysis or simply weighing up the differences between different treatments. It may be necessary to get specialist help to either determine the best method to analyse the results or to do the actual data analysis.

Stage III (page 51) provides more information and guidance on evaluating your results.

The different treatments are evaluated, with some proving to be very effective in controlling wheel cactus (photos show same plant before and after treatment).

Step 8—Review

Having evaluated your trial it is important to consider if the management questions were answered, how this new knowledge can be incorporated into future management practices, how the information can be shared with others and what further investigations may be required.

Stage III (page 51) provides more information and guidance on reviewing your trial.

A highly effective new control method has been identified by the trial. This information can now be used to guide on-going cactus control work.

STAGE I

Planning your trial

Planning your trial

The success of your trial hinges on good planning. Taking the time to plan your trial well will ensure that your results are as clear and reliable as possible.

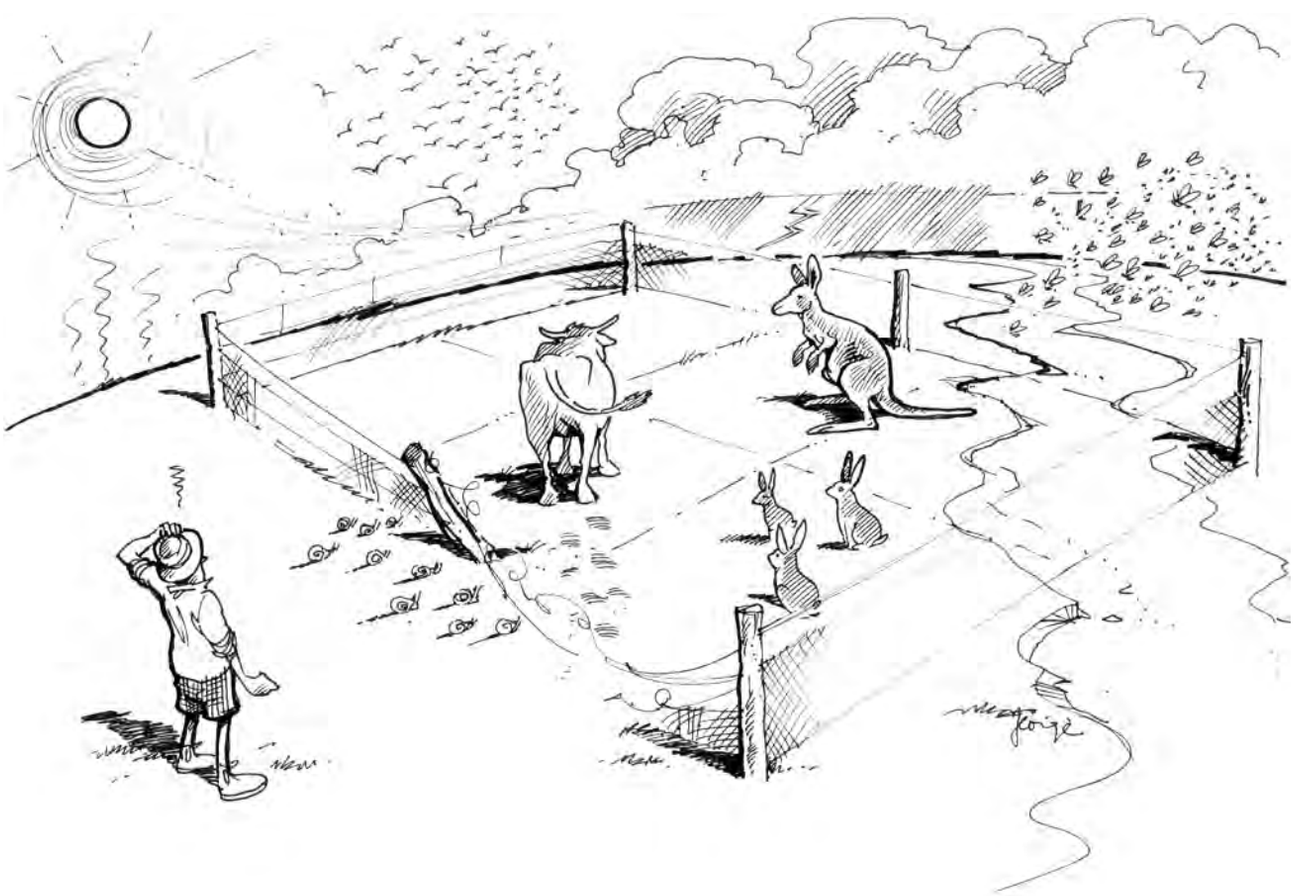
It is advisable to seek specialist advice from someone with a good knowledge of statistics if your trial is complicated or expensive.

Stage I will lead you through a series of essential considerations and decisions in the planning of your trial and incorporates steps 1, 2 and 3 from the 8-step cycle.

For each step in the planning process you will be presented with information and asked to answer a number of questions. The answers to these questions will be essential for finalising your design in Stage II. Your answers will also be essential for documenting the decision making process. A worksheet is provided at the back of these guidelines (Appendix 1–page 59) for recording your answers.

Although the following sections are presented sequentially, many of the steps are interrelated, with decisions made in one section influencing those in another. It is important to recognise that you will probably need to revisit your answers at the end of Stage I and adjust them if necessary. The steps are:

1. Assess problem / opportunity
2. Set the objectives
3. Select the treatments
4. Choose the trial site
5. Decide on the experimental unit
6. Account for variability
7. Decide on the number of replicates
8. Randomly allocate treatments
9. Develop a measurement program
10. Develop an implementation program
11. Calculate trial costs



Not only will good planning reward you with a reliable result, it will also help you to consider and avoid the many things that can go wrong when trials are undertaken in the field.

1. Assess the problem / opportunity

Why is the trial needed?

Before starting the trial it is important to take the time to document the need for your trial and the information already available on the problem or opportunity. This will help you to define the trial objectives and leave a record for future reference.

What is the problem or opportunity?

In most cases the problem or opportunity will be pretty clear—it will be the reason you decided to undertake the trial. However, it is well worth doing some homework to find out what is known about the issue and determine where your trial might extend the available knowledge or fill the knowledge gaps. Talk to experienced people in the area and read any available literature or search the internet.

A problem may be a major issue (e.g. dryland salinity) or it may be something more specific to your management operation (e.g. nothing will grow on my salinised land). As well as problems, there may also be opportunities for improvement to current operations. These may be in the form of some new technology that is available (e.g. a new direct seeding machine that you want to test) or an opportunity to compare different management options.

Question 1.1–What is the specific problem or opportunity you wish to address with this trial? (Record using Appendix 1–page 59)

Consider the scale of the problem or opportunity

An appreciation for the scale and importance of the problem or opportunity will help you to make decisions about the planning and implementation of your trial. The cost, level of background research required, methods of data collection / analysis and trial costs will be much different, for example, in a trial investigating the management of all wetlands along the River Murray than it would be for one comparing the effect of a couple of different fertilisers on your front lawn.

Question 1.2–What is the scale and importance of the problem or opportunity?

What information exists on the problem or opportunity?

Finding out what is already known about the problem or opportunity is an essential step in planning your trial. It will help you focus your efforts and reduce the chances of wasting time setting up a trial when the answer is already available. A review could include consultation with experts, a review of existing literature (e.g. government publications, past trial results, conference proceedings, scientific papers) and search of internet sites. It is also important to find out what the currently recommended practice is .

Question 1.3–What information / knowledge concerning the problem or opportunity already exists?

What are the knowledge gaps?

Having researched the problem or opportunity you may find some useful information that you didn't know was available. You may also discover a lack of information about the problem, especially for your local area and conditions. You will need to decide which knowledge gaps are important for your trial. This will help you determine your trial objectives.

Question 1.4–What are the gaps in existing knowledge that your trial aims to fill?

A trial might tackle a problem, such as erosion...

or investigate an opportunity such as this new direct seeder.

It is now time to consider the specific objectives of your trial.

2. Set the objectives

What are you trying to find out?

After working through the problem or opportunity and identifying the knowledge gaps that your trial could fill, you are in a good position to set your objectives. Clear, simple objectives are crucial for focussing in on a question of interest.

There are always more questions than answers and it is important to not set too many objectives for a single trial. Brainstorming potential objectives and then prioritising them may be a useful way of focussing on the most essential objectives. A good question to ask yourself is: *"If I could only try one thing, what would it be?"*.

This section discusses setting a broad management objective, a specific trial objective, a hypothesis and a null hypothesis.

Brainstorming may be useful in setting objectives.

Setting your management objective

Your management objective is a **broad statement** about the intended management outcomes that you hope will result from your trial. It is really what you hope to achieve by undertaking the trial. Some examples are:

- To improve the survival of direct seeded eucalypts in low rainfall areas.
- To maximise natural regeneration in pink gum woodland around Clayton.
- To increase the percentage of false caper controlled by spraying.
- To find out which species of tree is best for farm forestry on my farm.

Question 2.1–What is your management objective?

It is important to get expert input into setting objectives.

Setting your trial objective

Your trial objective is much more focussed on the specifics of your trial and the details of what you hope to discover. A good trial objective contains information about what is to be included in the trial and sets boundaries about the expectations of the trial. Some examples, leading on from the previous management objectives are:

- To evaluate the effects of wetting agent on the germination and survival of direct seeded *Eucalyptus socialis*, *E. gracilis* and *E. incrassata* in the Murray Mallee.
- To compare the effect of herbicide type, application rate and timing on the regeneration of *Eucalyptus fasciculosa* in veldt grass infested woodland near Clayton.
- To compare the difference in false caper density after spraying with herbicide 'A' and herbicide 'B'.
- To compare the growth rates of *Eucalyptus occidentalis* and *E. cladocaylx* on deep sand dunes near Yumali.

Question 2.2–What is your specific trial objective?

Setting your hypothesis

Your hypothesis is a statement of the expected outcomes of the trial. It involves a prediction of a difference between the treatments. For example:

- Wetting agent will increase the germination and survival of all the eucalypt species.
- A herbicide applied to veldt grass during spring will result in a greater % death of veldt grass and a greater regeneration of *E. fasciculosa* than the same

herbicide applied in Autumn.

- Herbicide 'A' will reduce false caper density more than herbicide 'B'.
- *Eucalyptus occidentalis* will grow faster than *E. cladocaylx.*.

Question 2.3–What is your trial hypothesis?

The more input into setting objectives the better.

Setting your null hypothesis

It is easier to disprove something than it is to prove it. For example, take the proposition that a certain herbicide will kill all of the boxthorn bushes it is sprayed on. You could spray and kill 1000 boxthorns and people could still legitimately say "there are millions out there how do I know that it will kill every, single, solitary, last one?" However, if you went out and sprayed one boxthorn and it did not die, you have just disproved the proposition that the herbicide will kill every boxthorn it is sprayed on.

Consequently, we phrase our hypothesis as a null hypothesis, stating that there will be no difference between treatments. We then establish the trial to disprove this null hypothesis. If we are successful in disproving that there is no difference (the null hypothesis), we have, by inference, proven the hypothesis that there is a difference.

Some examples of null hypotheses are:

- There will be no difference in the germination and survival of the eucalypt species if wetting agent is applied.

Indigenous input and cultural considerations will be important for many trials.

- The % death of veldt grass and the regeneration of *E. fasciculosa* will be the same whether herbicide is applied in Spring or Autumn.
- False caper density will be the same following the application of either herbicides 'A' or 'B'.
- There will be no difference in the growth rates of *Eucalyptus occidentalis* and *E. cladocaylx*.

Question 2.4–What is your null hypothesis?

Next we discuss the selection of treatments for your trial.

3. Select the treatments

What are you going to try?

Treatments are the different management actions that will be compared in the trial. Some treatments common in natural resource management trials include:

- Herbicide applications
- Fertiliser applications
- Grazing regimes
- Irrigation regimes
- Varieties of plants
- Planting densities
- Seed treatments
- Soil preparation techniques
- Timing of management actions
- Water regimes in wetlands
- Pest animal control techniques

After assessing the problem / opportunity and setting the objectives, the treatments to include in the trial will become more obvious. However, there are a number of things to consider when designing the most effective and appropriate trial.

Keep it simple

It is often tempting to try to test many treatments and combinations of treatments. However, testing too many treatment combinations can mean that your resources (time, land, labour, materials, consultants, etc), may be spread too thin for effects to be detected. It is most important to limit the treatments to a practical number in order to keep the trial simple and focussed on the hypothesis.

It is best to include only a small number of appropriate treatments.

Learn from others / check the literature

The best trials will build on available knowledge. Discuss treatment choices with colleagues and check available literature to best focus the trial with appropriate treatments.

Question 3.1–What have you learnt from the literature and from discussing the trial with others regarding appropriate treatments?

Local people are a great source of information.

Setting factors, levels and treatments

Consider the following example. A trial has been designed to evaluate the effect of three different fertilisers on five different varieties of lucerne. In this trial there are two factors:

Factor 1 *Fertiliser*
Factor 2 *Variety of lucerne*

> ***Factors*** are categories of action or manipulation into which the treatments can be grouped.

There are three levels within Factor 1 (three different types of fertiliser) and five levels within Factor 2 (five different varieties of lucerne):

Factor 1 *Fertiliser*
- **Level 1** *fertiliser type 1*
- **Level 2** *fertiliser type 2*
- **Level 3** *fertiliser type 3*

Factor 2 *Variety of Lucerne*
- **Level 1** *variety 1*
- **Level 2** *variety 2*

Level 3 *variety 3*
Level 4 *variety 4*
Level 5 *variety 5*

Levels are the different types or amounts of treatment within each factor.

Question 3.2–What are the factors in your trial? What are the levels within each of your factors?

The above example is known as a 3 x 5 factorial treatment structure (3 types of fertiliser x 5 varieties of lucerne). This means that there are 15 treatments in the trial (3 x 5 = 15). If the trial had included a third factor, irrigation, with 2 levels (2 rates of irrigation), the factorial treatment structure would have been written 2 x 3 x 5 with a total of 30 treatments.

Herbicide types are Levels within the Factor herbicide.

It is worth reflecting here on whether restrictions to the number of treatments caused by practical considerations (eg site size and availability of resources) have caused any deviation from the objective. It is also worth considering if some of the treatments (factor x level combinations) are not needed. For example, it may not be necessary to include the treatment with the slowest growing lucerne variety at the lowest level of fertiliser application if you know that it will not grow well.

Question 3.3–What is your factorial treatment structure (e.g. 3x5) and total number of treatments?

What control treatments are needed?

Controls are experimental units in your trial that do not receive any of the treatments. They are otherwise managed exactly the same as experimental units which have received one of the treatments. Controls are used to assess changes that would have occurred irrespective of the treatments (e.g. due to rainfall, season, light levels). For example, if comparing a range of direct seeding techniques near remnant vegetation, a control would be useful to detect whether any natural regeneration was occurring without seeding.

Different grazing regimes might form the treatments of a trial.

NOTE: Not every trial needs a control treatment. The need for a control is determined by the trial objective. For example, if a woodlot trial aimed to assess the potential advantages of using fertilisers at the time of planting, then a 'no fertiliser control treatment' would be needed. However, if it was known that a fertiliser was always needed for plant establishment and the trial objective was to compare the effect of a range of different fertiliser types, then there would be no need for a 'no fertiliser' control treatment.

Question 3.4–Does your trial require a control treatment?

What is the total number of treatments?

Having considered the above information, it is now possible to finalise the number of treatments. Write down the treatments, including the control treatments, in full and make sure that all treatments are necessary.

Question 3.5–How many treatments and control treatments are to be used in your trial? List them in full.

Control treatments are used to assess the changes that would occur irrespective of the treatments. For example, in a trial to control reeds it would be important to include plots where reeds were not controlled.

In the next section we discuss the trial site.

4. Choose the trial site

Where are you going to do the trial?

The trial site should be carefully chosen and matched to your specific objective. However, the availability of suitable sites may be limited and compromises may need to be made.

One of the most important characteristics of a good trial site is that it is relatively uniform, or that any variation across the site is recognised. Where there is a high degree of variability across a site, such as different soil types or different slopes, it can be more difficult to determine if the trial results are due to the treatments or due to the underlying site variability.

In most cases trial sites will contain a certain amount of variability. The following sections on *blocking*, *replication* and *experimental units* will help to account for this variability and reduce its impact on the trial.

Trial sites should be as uniform as possible, although good trial design can accommodate a certain level of variability.

The following should be considered when choosing your trial site or sites.

Choosing the number of sites

Although most trials will be carried out at a single site, it may be appropriate to include a number of sites in a single trial. For example, a trial could be conducted across a number of properties owned by different landholders.

Trials involving more than one landholder can provide added benefits such as increased participation by landholders, pooling effort and resources into a common issue and broadening the application and relevance of the trial results.

In addition, by involving a number of properties or sites, the amount of area taken up on any one property is minimised. In fact, for some trials it may be impossible to use just one site. For example, a trial investigating responses of wetlands to different wetting and drying regimes will require a number of wetlands if one treatment can only be applied at a single wetland.

When treatments affect a whole site, as may be the case for wetland management trials, multiple sites will be needed.

If multiple sites are to be used, however, it is important that they all are relatively similar to one another. If the sites are very different, a better result may be achieved by conducting a separate trial at each of the sites.

Question 4.1–Does your trial involve one or more than one site?

How consistent are conditions across the site?

A good understanding of the variability across the trial site is important when designing a trial. This will allow the effect of the treatments to be separated from background differences across a site or between sites. Differences across or between sites are inevitable in natural systems and will not necessarily be a problem if the trial is well designed. It is important to identify the variability so that it can be controlled / accounted for in the trial design.

Common sources of site variability include:

- Position on slope
- Aspect
- Soil type

- Soil depth
- Groundwater depth
- Previous management history (e.g. cultivation, herbicide use, grazing, water regime)
- Differences in existing native vegetation, crop, pasture or weeds
- Climatic effects (e.g. rainfall, temperature, evaporation) if more than one site is used
- Surrounding infrastructure and vegetation
- Light levels and temperature if part of the site is in shade or the trial is in a glasshouse

Position on slope and aspect are common sources of site variability.

Edge effects are another common source of site variability. Here the effect of the native vegetation on adjacent section of paddock (or vice versa) would need to be considered in the trial design.

Question 4.2–What is / are the main source(s) of variability at your site?

How representative is the trial site?

The results of a trial may be applicable to other sites if the trial site is similar to the other sites. If it is hoped that the trial results will be applicable elsewhere, it is worth

Trials in remnant native vegetation require good designs to control / account for the large amount of variability in these areas.

considering whether the site is typical by comparing factors such as:

- Soil type
- Aspect / slope
- Climatic conditions (e.g. rainfall and temperature)
- Plant community
- Stage of succession in vegetation community
- Habitat type
- Weeds present
- Wetland type (e.g. permanent, temporary, saline)
- Management history

The next section will discuss the individual subjects of your trial—the experimental units.

5. Decide on the experimental unit

What are you going to apply the treatments to?

Experimental units are the units of material to which a single treatment is applied.

Depending on the trial, experimental units could be individual leaves, individual plants, a group of 10 plants, an area of vegetation or crop, a section of lake or river bank, or even an entire wetland. A trial is analysed by comparing the different responses of the experimental units to each of the applied treatments.

Experimental material *is the material that is the subject of the trial e.g. an individual weed, seedling, tree or pasture plant.*

Experimental units *are the individual pieces or groups of experimental material to which a single treatment is applied e.g. a single tree or an area of pasture plants.*

It is not always immediately clear what the appropriate experimental unit is for any given trial. To assist in the selection of appropriate experimental units, the following should be considered:

Variability of experimental units

Variability between experimental units may be due to environmental differences across the trial site(s) or due to differences in experimental material used in the trial. Both of these sources of variability should be considered when deciding what will make up the experimental units.

Variability across the trial site

Where variability is high across a site, more experimental units will be required to account for it. If there is a limited area for the trial this may force your experimental units to be smaller so that more can be included. Section 6 will assist more with handling site variability.

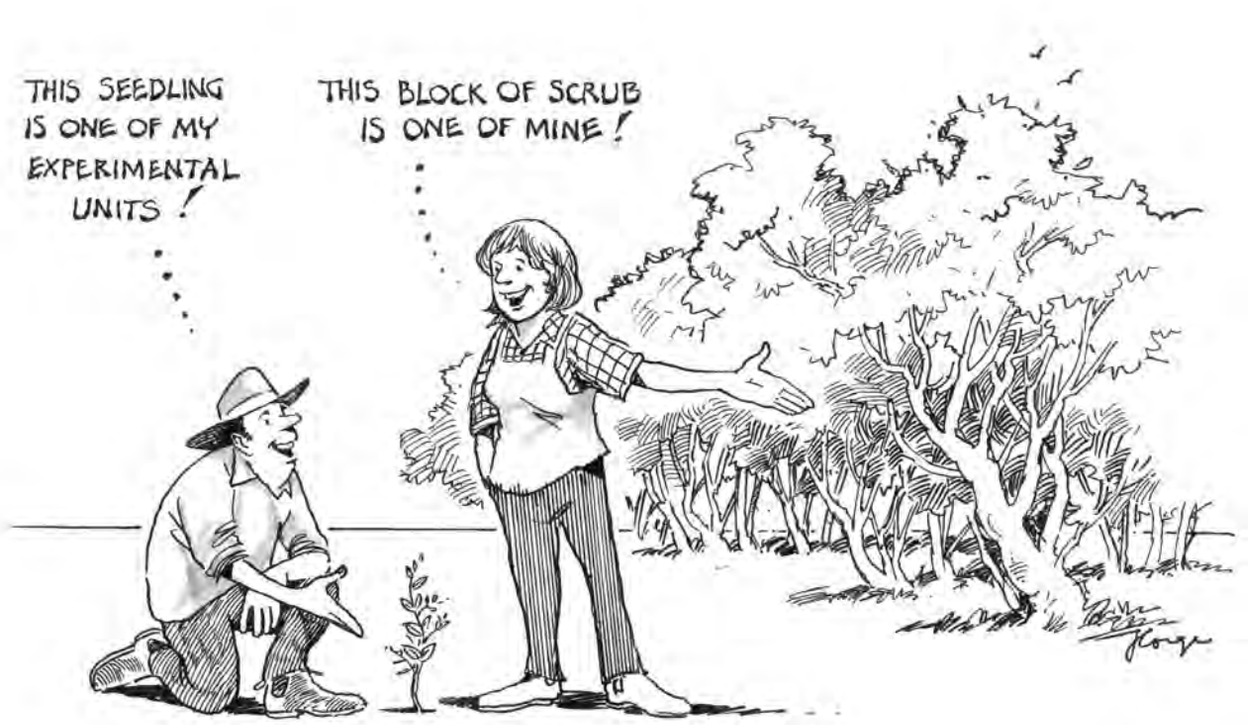
Example
The trial site is highly variable.

Recommended action
Use smaller experimental units.

Explanation
A greater number of experimental units can be used to account for this variability.

Variability in the experimental material

The variability in the experimental material to be used in the trial will also influence the experimental unit size. The greater the variability in this material (e.g. genetic variability between seedlings, age of mature trees, size of weeds) the greater the size of the experimental unit required to ensure that this variability is accounted for (i.e. more experimental material per experimental unit).



Depending on your trial the size of your experimental unit could vary greatly.

Example
A trial using cloned cuttings from a single plant

Recommended action
A single cutting can be used as an experimental unit

Explanation
Experimental units are similar

Example
A trial using seedlings grown from seed collected from wild populations of plants

Recommended action
An experimental unit would be made up of a large number of seedlings

Explanation
If experimental units were made up of single plants only, the genetic differences between plants would result in unacceptably high variability between experimental units.

Careful selection of experimental material to be included in the trial will reduce the amount of variability between experimental units.

Example
A trial of methods to control boxthorns in an area where the size of the plants varies from small seedlings to extremely large individuals.

Recommended action
Only include plants of a similar size to minimise the number of plants needed per experimental unit.

Explanation
Experimental units are similar.

If the amount of potential experimental material is limited (e.g. only a limited number of boxthorn plants available at the site), experimental units can be grouped into categories such as size to reduce variability (further discussed in Section 6).

Question 5.1–Will site variability and limited area or available experimental material impact on the size of experimental units? Briefly explain your answer.

Predicting the survival of experimental material

When dealing with living material in trials there is always a chance that some of the material may die. The loss of experimental material through death may have serious implications for the completeness of the trial and therefore enough material should be incorporated into an experimental unit to minimise the risk of its total loss.

Example
A trial involving planting seedlings of different species into a very harsh salt pan.

Recommended action
Use a high number of individual plants in each experimental unit.

Explanation
Mortality rate is expected to be over 50%.

Because the survival rate of these individual reeds was expected to be low, each experimental unit was made up of 16 reeds.

After considering the variability of the trial site and the variability and predicted survival rate of the experimental material, an informed decision about the size of the experimental unit can be made. The exact size may need to be fine-tuned later after considering the following sections.

Question 5.2–What is the experimental unit? What are the characteristics of the experimental units (e.g. size, age, spacing).

Interaction between experimental units

Every effort should be made to stop any interaction between the different treatments applied to the experimental units.

The chance of this interaction can be minimised by using buffer zones between experimental units and / or manipulating their size, shape or orientation.

Buffer zones are areas that are left between experimental units that do not received any treatment. This zone may need to be quite large to prevent interference between experimental units. For example, in a trial to compare the growth rates of two varieties of woodlot tree with and without irrigation, there is a danger that the roots of non-irrigated trees could run laterally and access extra moisture from neighbouring irrigated plants, thus affecting the results of the trial. In this case, the buffer zone distance would need to be larger than the potential growth of the roots.

In this sheoak establishment trial, the chance of interaction between inoculation treatments has been reduced by leaving wide buffer zones between experimental units.

The ***size of experimental units*** can also be increased to include an un-sampled buffer zone within the unit. This approach is particularly appropriate when it is not desirable to leave un-managed areas within the trial site. For example in a trial to compare different herbicides, it may be undesirable to have unsprayed strips of weeds remaining in the paddock. It may be better to allow treatment plots to abut one another but restrict measurements to a central part of each plot, away from any potential interaction between treatments (e.g. spray drift).

The ***shape of experimental units*** may also be manipulated to minimise potential interactions between treatments. For example, square plots of land, in comparison to linear ones, have a low perimeter to area ratio, minimising the shared edges with other plots. If there is a risk of interaction between treatments, square experimental units will be more appropriate than linear ones.

The ***orientation of experimental units*** can also be used to minimise potential interactions between treatments. For example, experimental units treated differently to stabilise a bare sand dune can be arranged in a single line perpendicular to the prevailing wind to minimise any interference from an experimental unit that failed to stabilise the sand.

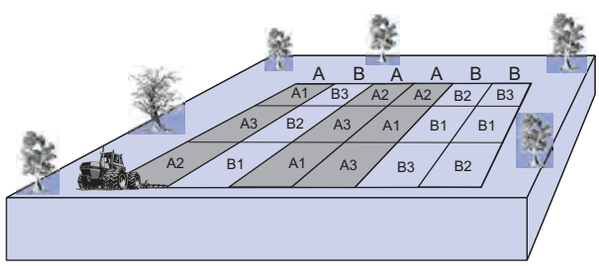
More than one of these approaches may be needed to minimise the chance of treatment interaction affecting the results.

Question 5.3–Is there the potential for interaction between experimental units? Can this be minimised by manipulating plot size, shape or orientation or by leaving a buffer zone around experimental units?

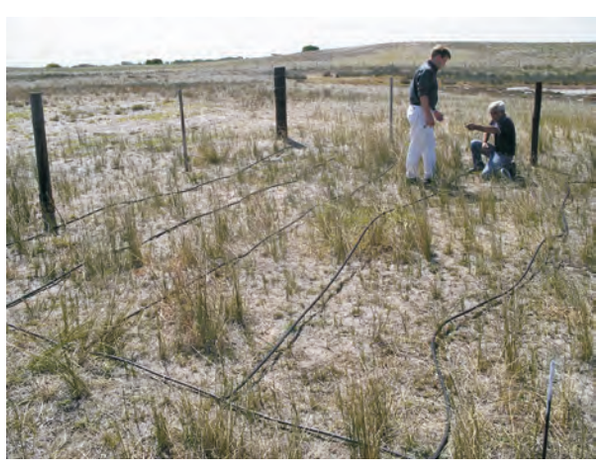
Using main-plots and sub-plots

Some trials involve applying more than one factorial treatment. These different treatment types may need to be applied at different scales. For example in a trial investigating different crop seeding machines and different fertilisers, the size of the experimental units for the seeding machines must be at least as wide as the machines, even though the fertiliser could be applied at a much smaller scale by hand. Issues of management practicalities and those of interaction, as discussed above, can be addressed by the use of main-plots and sub-plots.

Main-plots are large plots that are used for treatments that can only be applied at large scales (e.g. seeding machines) or for those that may interfere with neighbouring treatments (e.g. irrigation). Within the large main-plots, the other treatments are included at a smaller scale in sub-plots. For the seeding machine and fertiliser example, the seed would be sown in main plots or strips and the different fertilisers would be applied by hand in small sub-plots within each main-plot. For the irrigation example, large main plots would be established as either irrigated or non-irrigated (minimising the contact between the two areas) and within the main-plots smaller sub-plots would be established to plant two different plant varieties. In trials using main-plots and sub-plots, the sub-plots form the experimental units.



Here the main plots of treatments A and B, created by the tractor, are broken into 3 sub-plots for the application of smaller scale treatments (e.g. different fertilisers).



Setting up main-plots for irrigation and sub-plots for fertiliser.

Question 5.4–Is there a need to use main-plots and sub-plots? Which treatments will be the main plots and which will be the sub-plots?

The next section explains how to minimise or account for variation between experimental units.

6. Accounting for variability

Do you need to block?

The aim of a trial is to detect the effects of different treatments. These effects will be more difficult to detect if variability from other sources is present. It is therefore necessary to account for variability that is not due to the treatments.

The main sources of non-treatment variability will be from variability present at the site or in the experimental material. Variability may be present at the beginning of a trial (e.g. a gradient in soil type across a site), or may be introduced later due to the design of the trial (e.g. different amounts of shading over experimental units due to plant growth in other units).

If the variability between experimental units is too large because of variability present at the beginning or introduced during the trial, treatment differences may not be detected.

The following sections will help you to understand how to account for variability.

Accounting for site variability

Variation between experimental units can be accounted for in two ways: 1 - minimise the amount of variability between experimental units; and 2 - account for variability in the trial design. It may be useful or necessary to both minimise and account for variability.

1. Minimise the amount of variability between experimental units

The following examples describe potential sources of variability between experimental units and how these can be managed or avoided:

Example
One half of the experimental units are covered by leaf litter.

Recommended action
Remove the leaf litter.

Explanation
Experimental units are more similar.

Example
A revegetation trial site has some remnant trees casting shade.

Recommended action
Place all experimental units either under or away from the trees to ensure they receive approximately equal amounts of light.

Explanation
Experimental units are more similar.

Example
A revegetation trial along a roadway where there is run-off from the road.

Recommended action
Place all experimental units an equal distance from the road.

Explanation
Experimental units are more similar.

2. Account for variability in the in the trial design

It is not always easy or desirable to minimise all variability. For example, a manager may wish to apply the results of a trial to other sites where the variability across the site is known to be high. By recognising and accounting for variability at the trial site (by using appropriate trial design and analysis) it is possible to separate the background variability from the effects of the treatments.

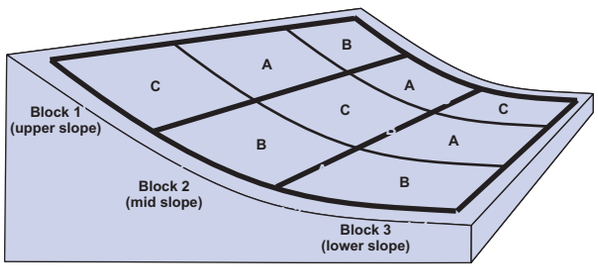
The standard approach to account for variability is to group experimental units to a similar background variability and apply each treatment to one experimental unit within this group. The group must then be replicated to ensure that each of the treatments is repeated. For example, if there are six groups, each treatment will be repeated six times, once in each group. Experimental units grouped in this way are called *BLOCKS*.

Blocking is a common technique used to account for the non-treatment sources of variability. It involves grouping experimental units into like areas or types and is useful when it is known or suspected that the site or site conditions are patchy enough to have different effects on different experimental units. Blocking ensures that treatments within a block are experiencing similar background conditions. It may be possible to explain some of the variation detected as differences between the blocks (i.e. background variation) and thus improve the chances of detecting differences due to treatments.

Example
A trial is situated on a hillside with conditions changing from the top to the bottom of the hill.

Recommended action
Group experimental units into four blocks: at the upper, middle and lower slopes.

Explanation
Experimental units within each block are more similar to each other than if they were randomly placed across the hillslope.



Here a trial site on a slope has been broken into 4 blocks at different elevations. A single replicate of the treatments A, B and C has been randomly allocated to the experimental units within each block.

Blocking may also be useful if:

- There is a reasonable chance that some experimental units will be affected differently than others during the trial (eg. grazing in one part of a pasture trial).
- There is a reasonable chance that part of the site is vulnerable to destruction of experimental units (eg. an eroding river bank). In this case it is important to try to ensure that approximately the same number of experimental units with each treatment are preserved.

Question 6.1–Do you need to block to account for site variability? What source of variability do you need to block for?

Site variability can occur over small distances. This trial site was broken into eight blocks to account for differences in soil type.

Accounting for other sources of variability

Variation between experimental units can also be caused by differences in the materials used in the treatments or differences in the basic materials of each experimental unit as previously discussed in section 5. For example experimental units in a revegetation trial may vary from one another due to the genetic differences between the seed or tubestock being used. Similarly, different batches of herbicide used to spray weeds may be mixed differently, contributing to differences between experimental units. All of these sources of variation can be accounted for to maximise the likelihood of detecting the changes of interest. One way to do this is to put all material from one source in a single block or treat all experimental units in one block with materials from the same source (or batch).

Example
A trial investigating the most successful method of growing an endangered plant from cuttings, might use plant material cut from 10 individual parent plants. There may be genetic differences between the 10 parent plants affecting cutting establishment, quite separate from any effect of the treatments applied.

Recommended action
Use parent plant as the blocking factor (ie. treat the cuttings from a given parent plant as a block of similar experimental units).

Explanation
Experimental units within each block are more similar to each other.

When variation between experimental units is high, more experimental units will be needed to detect the effects of treatments, especially if the effects are real but relatively small. However, in some circumstances, more experimental units may mean that each unit has less material (i.e. the more experimental units that the trial is broken into, the smaller each unit will become). It is important to ensure that experimental units do not become too small , however, and that enough material is available in each to adequately measure the effect of treatments in the trial.

Question 6.2–Do you need to block to account for other sources of variability?

It is also important to ensure that any non-treatment management actions made during the life of the trial (e.g. insecticide application) are applied to all experimental units to minimise introduced variability.

Next we investigate how many times you need to repeat each treatment.

7. Decide on the number of replicates

How many times should you try it?

When conducting natural resource management trials it is hoped that any real differences between treatments will be detected so that management decisions can be made on the reliable results.

The detection of real differences between treatments relies on experimental units receiving one treatment being measurably different to experimental units receiving a different treatment.

Underlying differences between experimental units may not always be obvious (e.g. differences in soil depth or soil seedbank between experimental units) and may actually change throughout the trial period (e.g. amount of overhanging shade on experimental plots). To be sure that the effects observed in trials are due to treatments and not due to underlying variation between experimental units it is important to have a number of experimental units receiving each of the treatments. This will assist in separating the real effects from the background variation.

Experimental units that receive the same treatment are called 'replicates'. Individual replicates should be managed in the same way so that any differences are due to underlying variation and not due to different management. The number of replicates required will depend on:

- *The amount of underlying variation between experimental units*
- *The size of the difference expected as a result of different treatments*
- *The level of confidence that the difference is a real difference and not due to chance*

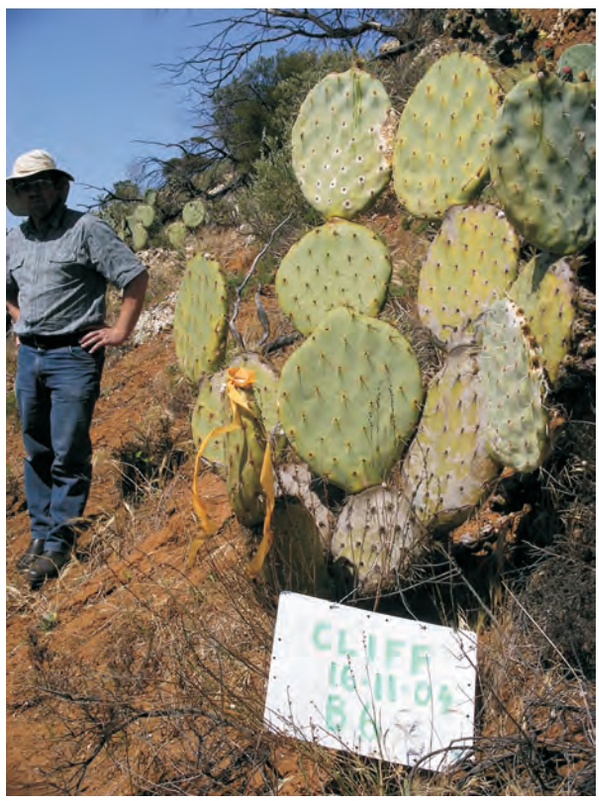
If there are not enough replicates there is a risk of observing a difference when there is no real difference or of failing to detect real differences when they are present.

There are ways of calculating the approximate number of replicates required to optimise a given trial design, however, these calculations are beyond the scope of these guidelines. If the trial is large, expensive and/or the results will have large consequences for management decisions, it is highly recommended that you seek assistance from an expert to determine the optimum number of replicates for your trial design.

As a rule of thumb, a trial should include at least 3 replicates of each treatment but including more than 10 replicates of each treatment is rarely necessary. To decide roughly how many replicates are needed for a given trial, consider the following:

Variability between experimental units

Variability between experimental units may come from a number of sources. Variability, as discussed previously,



By using experimental units of the same size, this trial reduced its need for a large number of replicates.

may be due to differences across the trial site (e.g. differences in soil properties) and/or the variability of experimental material (e.g. genetic differences between seedlings). The more variation between experimental units, the more replicates may be required.

Remember that blocking is one way to account for variability between experimental units (as discussed in the last section).

Question 7.1–What is the level of underlying variability between experimental units?

Expected size of differences between treatments

In some trials it is reasonable to expect quite large differences in effect between treatments (e.g. irrigating versus not irrigating in an arid climate). In other trials we would expect that the differences in effect might be relatively small (e.g. comparisons between slightly different fertiliser types). If the size of the effect from different treatments is expected to be large, fewer replicates will be needed. If it is important to detect even small differences, more replicates will be required.

Question 7.2–What is the expected size of any difference between treatments?

This trial was designed to detect small differences between treatments and therefore a greater number of replicates were used.

Confidence in the results

Although it is not always possible to have 100% certainty about the cause of differences between treatments, a larger number of replicates will give greater confidence that the observed differences (or similarities) between treatments are real.

A high number of replicates were used in this trial because a high level of confidence in the results was required.

Question 7.3–What is the level of confidence required in the results of the trial?

Deciding on the number of replicates

After considering the issues above it is time to decide on the number of replicates which gives confidence in detecting differences of interest without wasting resources unnecessarily. As a general rule this will be between 3 and 10 replicates.

A high number of replicates will be required for trials with either a high variability between experimental units, small expected differences between treatments or a need for high levels of confidence in the results. If unsure about the these issues, using more replicates is better than using too few.

Question 7.4–What is the number of replicates required for each treatment?

Total number of experimental units

The total number of experimental units required can now be worked out by multiplying the number of treatments by the number of replicates.

Question 7.5–What is the total number of experimental units required (number of treatments x number of replicates of each treatment)?

The next section discusses the random allocation of treatments to experimental units.

8. Random allocation of treatments

Where to try what?

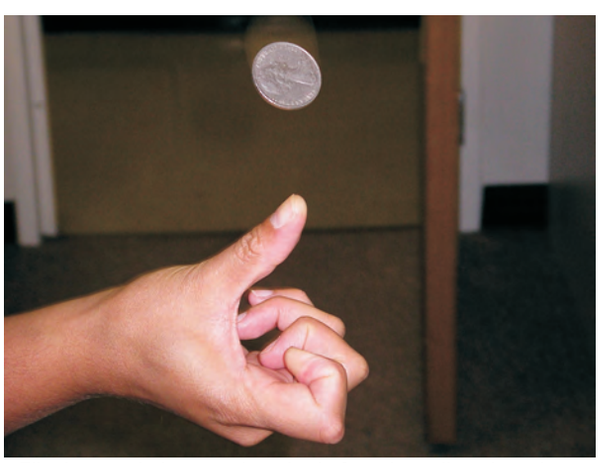
The previous sections have covered issues related to managing or accounting for variation between experimental units (i.e. replication and blocking). The next step involves the unbiased allocation of treatments to experimental units.

It is tempting to believe that managers will automatically allocate treatments to experimental units without bias. However, there is a risk that deliberately trying to avoid bias may lead to overcompensation and the introduction of bias from different sources.

Randomising the allocation of treatments to experimental units can be as simple as flipping a coin (if there are only two treatments) or numbering experimental units and drawing numbers from a hat to select individuals for each treatment. There are also numerous tools available free of charge on the internet to assist in the random allocation of treatments to experimental units.

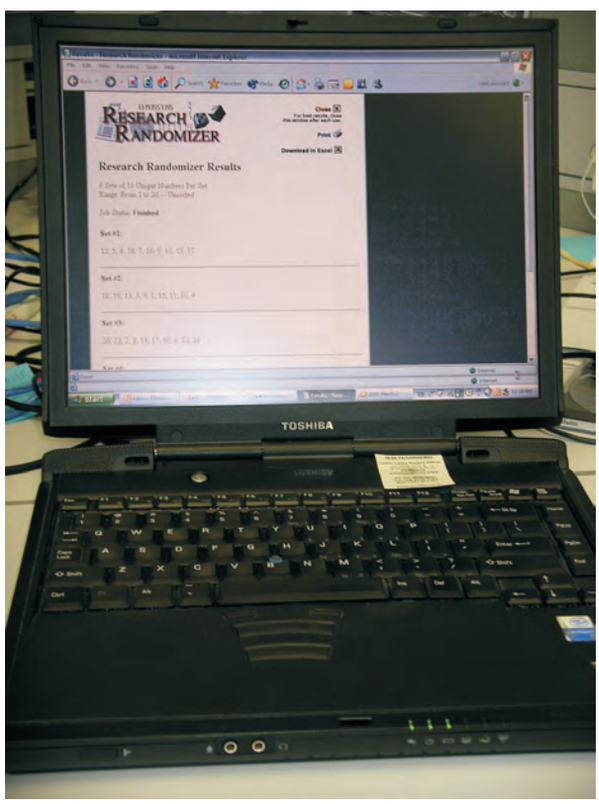
It is possible that after randomisation some pattern in the distribution of treatments is obvious (e.g. all replicates of treatment A are on one side of the paddock and replicates of treatment B are on the other). This is more likely to occur when a small number of replicates are being used for each treatment.

To avoid the results of the trial being compromised by an obvious underlying difference between experimental units selected for each treatment, it is advisable to re-randomise the allocation of treatments. This should be a limited practice, however, as to much 'shopping' for a desired pattern of treatment allocations (even apparently unbiased patterns) is a form of biasing in itself. It is recommended that a decision about how much pattern is acceptable is made before re-randomisation.



Flipping a coin is a good method of random allocation when only 2 treatments are being used.

Random allocation of treatments to experimental units can be as simple as pulling numbered pieces of paper out of a hat.



Many free tools for random allocation of treatments to experimental units are available on the internet.

Question 8.1–How will you randomly allocate the treatments to experimental units?

More assistance on assigning treatments to experimental units will be provided in Stage II.

The next section discusses developing a measurement program.

9. Develop a measurement program

What are you going to measure and how?

Developing a measurement program is an essential part of conducting a successful trial. A measurement program should include all aspects of trial measurements, including which measurements will be taken, who will take them, when and how they are to be taken, and how the data is to be recorded and analysed. It may be useful at this time to consider any anticipated problems and additional observations that could be valuable for later interpretation of results.

The measurement program should also include the collection of information about the site and the trial design. This includes recording the location and characteristics of the site, the dates of different activities associated with the trial, weather conditions throughout the trial and information on the history of the site and the origin of the experimental materials.

The following considerations should be made when developing your measurement program:

- *What is the timing of data collection?*
- *What will be measured to demonstrate the results of the trial?*
- *What type of variables (discussed later) are these measurements (different data types can be analysed different ways)?*
- *How will the results be measured (what methods)?*
- *Are there practical considerations or constraints to the measurement regime?*
- *How will data from the trial be stored and managed?*
- *Are other measurements necessary to interpret the results?*
- *Is there other information that should be recorded to keep a thorough record of the trial?*

What type of variables?

Variables are qualities or quantities of the experimental units which change as a result of the different treatments applied in the trial. It is useful to understand what kind of variables will be measured because different types of variables can be analysed using different techniques. It would be worth getting help from a statistical expert to determine if continuous or discrete variables should be used.

Measurement variables

Continuous variables

Can assume an infinite number of values between two points e.g. length, area, volume, weight, angle, temperature, time period, percentage and rate.

Discrete variables

Can have only certain fixed numerical values with no intermediate values possible e.g. number of seeds on a plant, number of plants in a quadrat, number of individuals in a population, number of branches on a tree.

Ranked variables

Cannot be measured but can be ranked or ordered with respect to some quality e.g. shade of grey, order of germination of seeds, order of flowering of plants.

Attributes

Cannot be measured but must be expressed qualitatively e.g. alive or dead, male or female, fertile or infertile.

Measuring the effect of your treatments

What will be measured to determine the effects of different treatments? For example, it may be the number of seedlings surviving, the number of weeds that have died, the size of plants, the amount of regeneration, the species diversity in a wetland, or any number of other things, depending on the trial. The trial objective should aim to specify the expected response and therefore the variables of interest. For example, nitrogen fertiliser will increase the *growth rate* of seedlings, therefore height would be used to measure the effect of nitrogen treatments.

Question 9.1–What is/are your response variable(s)?

The cacti in this trial were measured as either dead or alive 12 months after treatment.

Other measurements

As well as measuring the response variable at the end of the trial, measurements at the beginning and during the trial are often necessary.

Measurements of the condition of experimental units at the time of establishing the trial (baseline data) are often

needed, especially where the changes in the response variable will be interpreted with respect to the starting conditions. For example, in a trial to compare the effect of different fertilisers on the growth of trees, it would be necessary to know the size of the trees before the treatments were imposed. Photographs of experimental units from before the treatments are applied is another example of baseline information.

Question 9.2–What measurements will be taken as baselines?

Often measurements are needed before the treatments are applied.

Measurements during the intermediate stages of the trial can also be useful. This information may be helpful in determining the optimal time for measuring or interpreting the final response. For example, the number of flowers per plant may be useful in interpreting the effect of treatments on the final seed set in a seed orchard trial.

Question 9.3–What measurements will be taken at the intermediate stages?

Measuring seedling height and number of leaves in a revegetation trial.

Some measurement activities, such as the use of this fyke net, require permits.

Timing

It is important to determine the length of time the trial will be undertaken over and at what points measurements will be taken.

Question 9.4–When will the measurements be taken?

Specialist NRM officers may be available to assist with the design of your measurement program.

Other information to record during data collection

The measurement program should not only include measurements but also include the collection of information about the site and the trial. This includes recording:

WHO took the measurements (so that information can be checked later if necessary).

WHAT the field conditions were at the time of measurement (e.g. weather conditions immediately before and during measurement, site observations).

The next section discusses developing an implementation program for your trial.

10. Implementation program

How are you going to get it done?

A clear and simple plan is important for a successful trial. If steps or details of the implementation process have not been documented thoroughly, the trial may be incorrectly set up. This could lead to either having to set the trial up again, aborting the trial or using a poorly implemented trial which may lead to inconclusive results. The implementation plan must include the order of carrying out the tasks, costs, who is responsible for carrying out each of the tasks and the timing of those activities.

Question 10.1–What is the implementation plan for the trial? (Who is going to do what? When is it all going to happen?)

The next section discusses costing your trial.

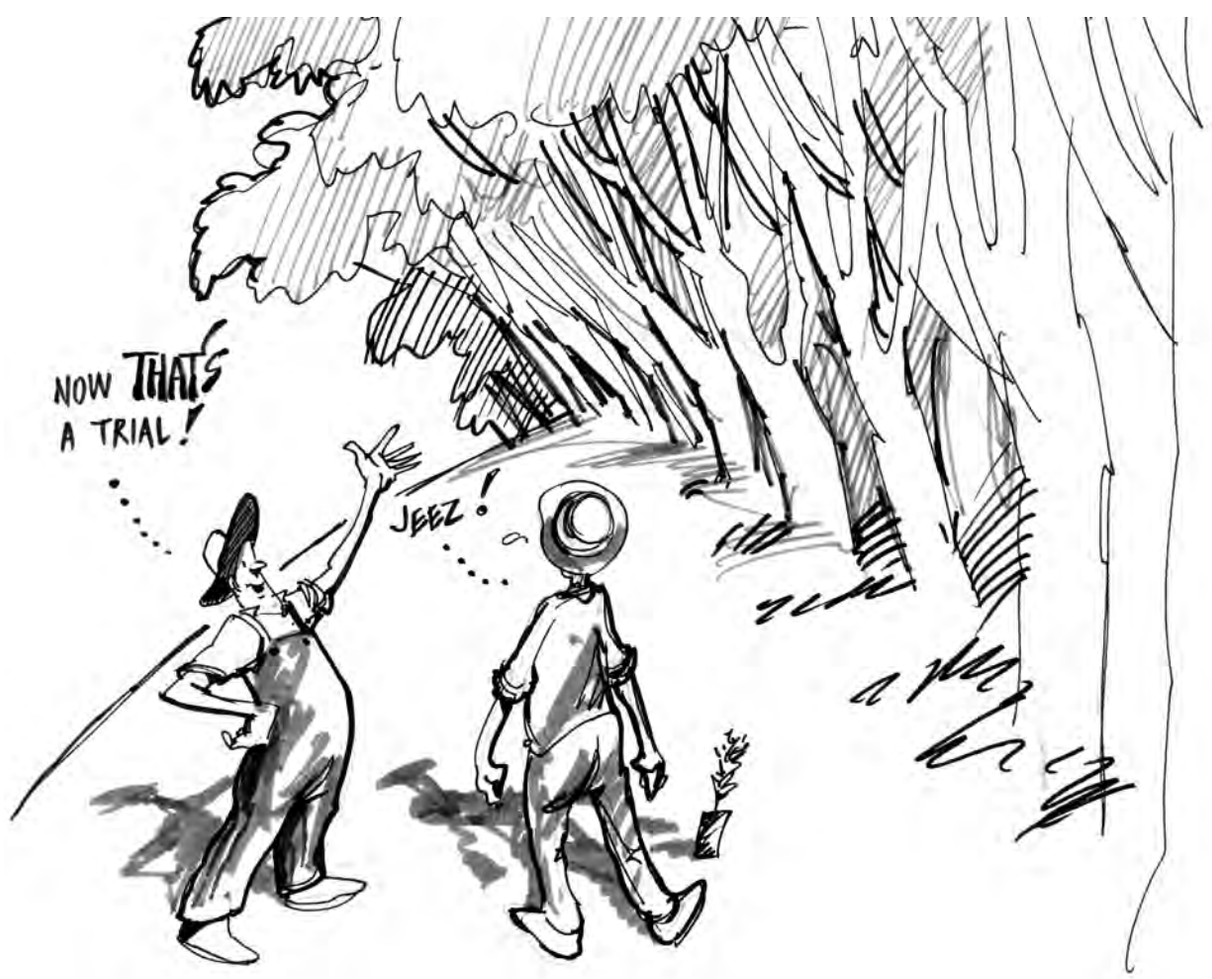
11. Costing the trial

How much is it going to cost?

Designing, implementing, measuring and analysing the trial will all have a cost associated with them. It is important to cost out these activities. If the costs are too high, it may be necessary to scale down the trial. Costs may be covered by in-kind contributions from landholders, community groups, etc. It is essential to make sure that there is available money for a biometrician to help in the planning and analysis of the trial.

Question 11.1–How much will the trial cost (capital, in-kind contribution)?

The next stage of the guidelines uses the information generated in this stage to finalise the trial design.



STAGE II

Finalising your design

Choosing a trial design type

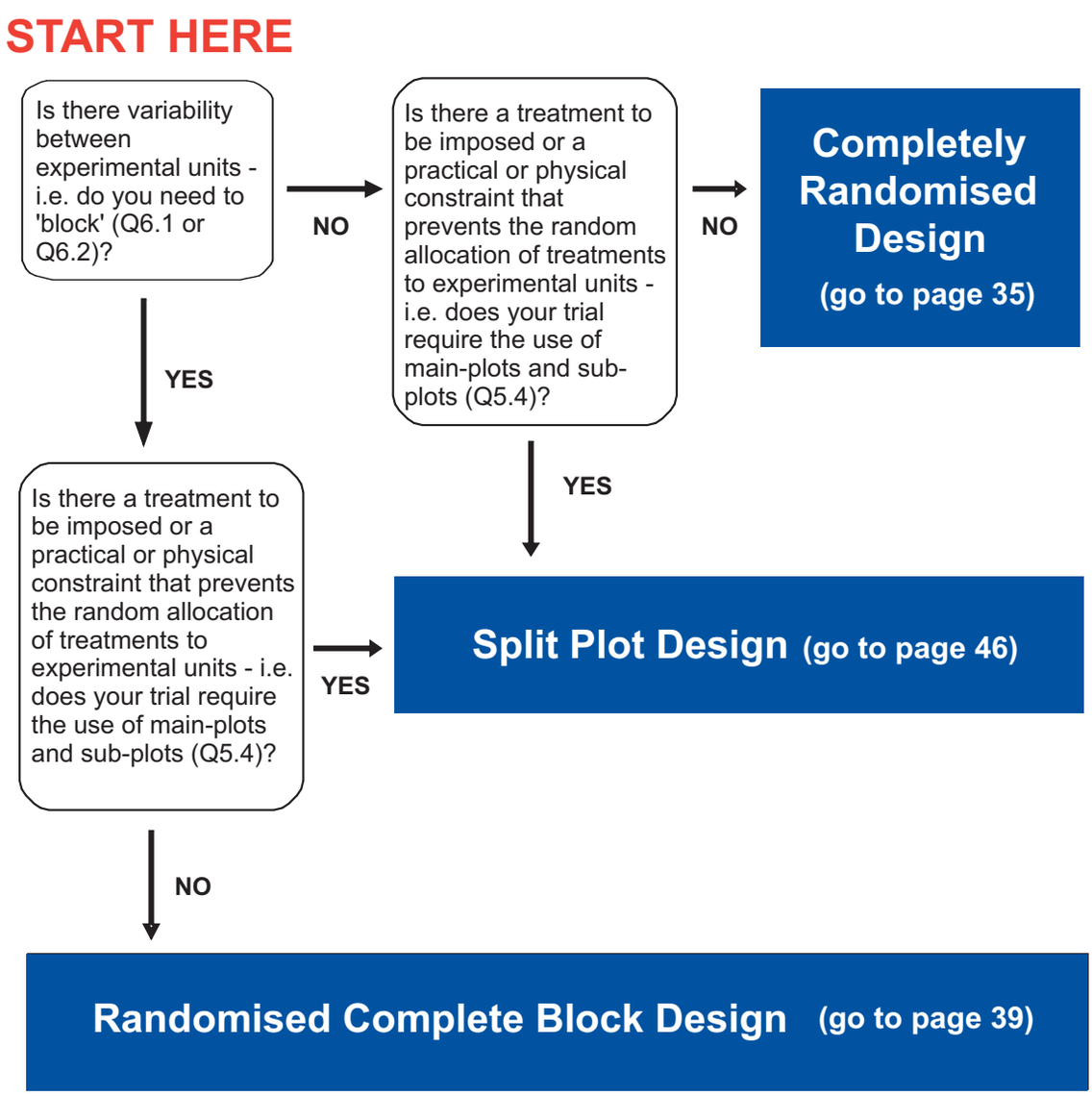
Which type of design is best for your trial?

Although there is an endless array of potential trials to undertake, most are catered for by a relatively limited number of trial designs.

This stage uses the answers generated in the last section to help you decide on the appropriate trial design type. For each design type a range of example layouts are presented as well as information on setting up the trial, collecting the data, and having it analysed.

Only the most common and basic designs are presented here. There are many other more advanced designs available to overcome specific issues. If the following do not seem to quite suit your trial, seek expert assistance, as there may be a better design out there for you.

Use the flow chart below and the answers from Stage I to identify the appropriate design type for your trial. Go to the section for that design type and use the specific information and examples to finalise your design.



Question 12.1 - Which design type is most appropriate for your trial?

Completely Randomised Designs

The simplest design type for uniform conditions

Trial layout: *Trial Area → Individual Experimental Units*

The **Completely Randomised Design** (CRD) is the simplest type of trial design. In a CRD design the only recognisable difference between experimental units is the treatment applied.

The CRD is most appropriate for trials where the experimental units are homogenous (i.e. there is little or no variability between experimental units). CRDs are also advocated where experimental units are widely spaced and influences on each unit are too individual (or unknown) to make blocking meaningful.

This section illustrates how CRDs can be used in a range of situations. It also provides a guide for developing a CRD for your trial.

Examples of plots for completely randomised designs include sections of ground, areas of vegetation and grids of planted vegetation in homogenous conditions.

Examples of Completely Randomised Designs

The illustrated examples in this section are included to assist the application of the standard CRD design type to individual trial situations. The examples cover trials involving experimental units that are plots, scattered individuals or lines.

Once you have identified the design that suits your need, proceed to the information on setting up a CRD trial on page 38.

Plots (page 36) are regular experimental units usually laid out in a regular pattern, for example:

- Sections of ground
- Areas of vegetation
- Grids of planted vegetation

Scattered experimental units (page 37) may be irregularly distributed, for example:

- Scattered trees
- Woody weeds
- Whole wetlands
- Animal burrows

Lines (page 38) involve application of treatments to experimental units arranged in lines, for example:

- Lines of planted tubestock
- Direct seeding lines
- Herbicide spray lines
- Rabbit trailing lines
- Orchard or vineyard rows

Examples of scattered experimental units for completely randomised designs include scattered plants, animal burrows and whole wetlands in homogenous conditions.

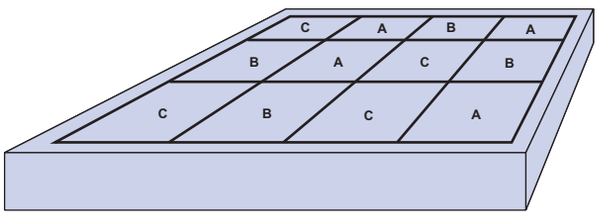
Examples of lines for completely randomised designs include lines of seeding or planting, herbicide spraying lines and orchard rows in homogenous conditions.

CRD examples using plots

Plots are regularly laid out experimental units, for example sections of ground, areas of vegetation or grids of planted vegetation. See below for examples of CRDs using plots in different circumstances.

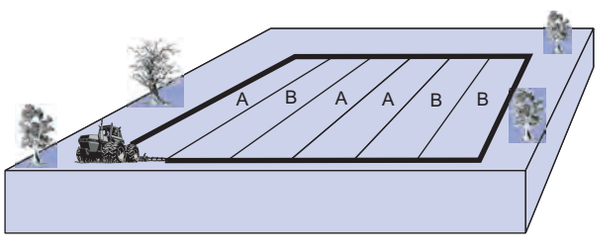
CRD plots in open uniform conditions

e.g. a flat open paddock with uniform soil conditions.



In this example, the four replicates of treatments A, B and C are randomly allocated among the 12 rectangular plots (experimental units).

Refer to page 38 for assistance in setting up this design.

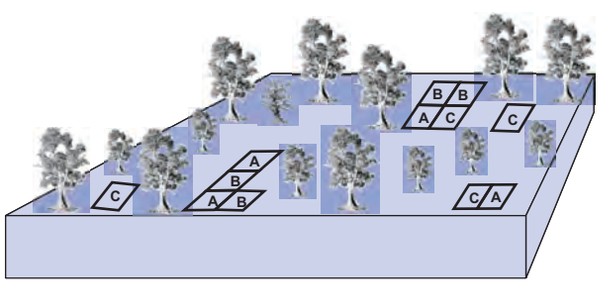


In this example, the plots are linear due to practical considerations (e.g. using large equipment). The three replicates of treatments A and B are randomly allocated among the six linear plots (experimental units).

Refer to page 38 for assistance in setting up this design.

CRD plots where only certain areas can be used

e.g. Among scattered trees, on patches of a certain type of soil / vegetation / elevation.

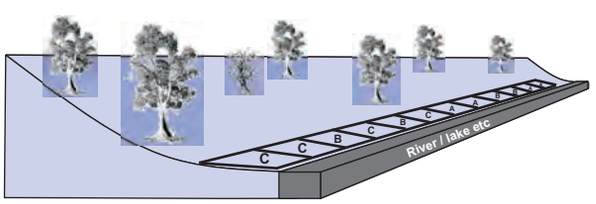


In this example, the four replicates of treatments A, B and C are randomly allocated among the 12 rectangular plots (experimental units) located in homogeneous areas across the larger site.

Refer to page 38 for assistance in setting up this design.

CRD plots along a linear feature

e.g. along a river, creek, lake edge, cliff, revegetation shelter-belt or boundary of remnant vegetation.



In this example, the four replicates of treatments A, B and C are randomly allocated among the 12 rectangular plots (experimental units) located in homogeneous conditions along the edge of the linear feature (in this case a river or lake edge).

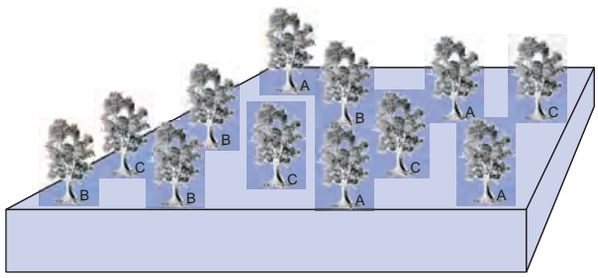
Refer to page 38 for assistance in setting up this design.

CRD examples using scattered experimental units

Scattered experimental units are often irregularly distributed, for example scattered trees, woody weeds, whole wetlands or animal burrows.

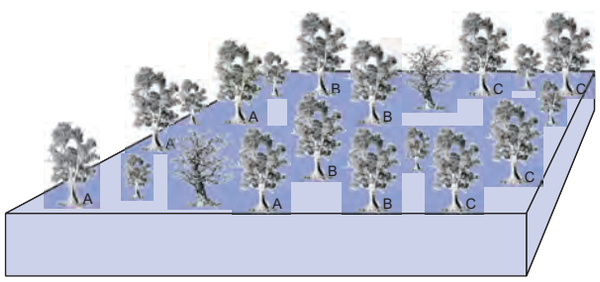
CRD scattered uniform experimental units in homogeneous conditions

e.g. trees, weeds, burrows, wetlands of similar size / age / condition within uniform environmental conditions.



In this example, all of the potential experimental units (trees) are of similar size, age and condition. The four replicates of treatments A, B and C are randomly allocated among 12 trees.

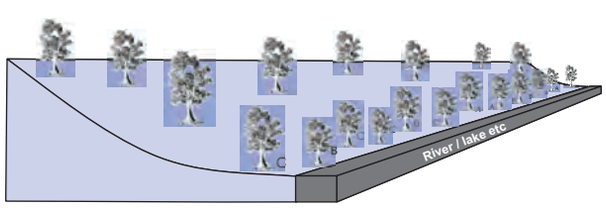
Refer to page 38 for assistance in setting up this design.



In this example, the potential experimental units (trees) vary in size, age and condition. The four replicates of treatments A, B and C are then randomly allocated to the 12 trees identified as relatively uniform experimental units (trees that are very old or very young are not included).

CRD scattered experimental units along a linear feature

e.g. among scattered trees, on patches of a certain type of soil / vegetation / elevation.



In this example, the four replicates of treatments A, B and C are randomly allocated among the 12 experimental units (trees) located in homogeneous conditions along the linear feature (in this case a river or lake edge).

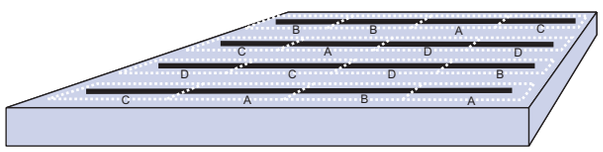
Refer to page 38 for assistance in setting up this design.

CRD examples using lines

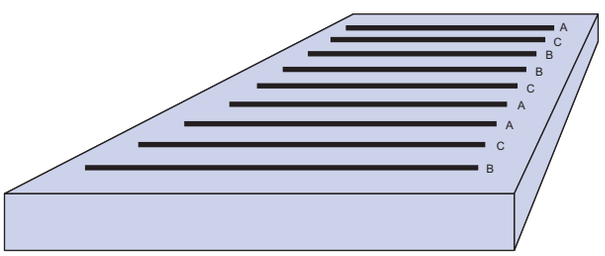
This section refers to trials that involve linear treatments or experimental units, for example lines of planted tubestock, direct seeding lines, herbicide spray lines, rabbit trailing lines or orchard or vineyard rows.

CRD lines with uniform experimental units in homogenous conditions

e.g. lines of planted vegetation under uniform environmental conditions.



In this example, none of the experimental units (sections of line) differ significantly from one another. The 4 replicates of treatments A, B, C and D are randomly allocated to the 16 sections of line identified as experimental units.



In this example, the treatments are applied to full lines or rows. Other than the treatments, the experimental units (the lines) do not differ from one another. The three replicates of treatments A, B and C are randomly allocated to the 9 lines.

Refer below for assistance in setting up this design.

Setting up a CRD trial

In the CRD, all experimental units have an equal chance of receiving randomly assigned treatments. The example below shows a trial using plots

The first step is to make a sketch of the trial site and the experimental units. This example is using three replicates of four treatments, therefore 12 experimental units are required.

C	C	B
A	D	C
B	A	D
D	A	B

Next randomly assign a treatment to each experimental unit. An easy way to do this is to write each treatment on separate slips of paper the number of times you intend to replicate it. The number of slips of paper should be equal to the total number of experimental units in the trial. Put the slips into a hat and pull them out one by one, assigning them in order to the experimental units.

The Completely Randomised Design is now ready to go! Proceed to Stage III of these guidelines on page 51.

Randomised Complete Block Designs

A very useful design for variable conditions

Trial layout: *Trial Area → Blocks → Individual Experimental Units*

The **Randomised Complete Block Design** (RCBD) accounts for the variability across a trial site or between groups of experimental units. This minimises the impact of underlying variability on the trial and improves the likelihood of detecting real differences between treatments. RCBDs involve using a single replicate of each treatment in each block, making the number of blocks the same as the number of replicates.

There are a number of more complicated block designs that may occasionally be needed. One of these is the Randomised Incomplete Block Design that is used when a single full set of all treatments cannot be neatly fitted within each block. Another is the Latin Square Design which is used when there are two strong gradients of variation to account for which run at right angles to each other at the trial site. These more advanced designs will rarely be needed for simple natural resource management trials, but further expert assistance should be sought if the RCBD presented here does not fit the trial situation.

This section illustrates how RCBDs can be used in a range of situations. It also provides a guide for developing a RCBD for your trial.

Examples of plots for randomised complete block designs include sections of ground, areas of vegetation and grids of planted vegetation in variable conditions.

Examples of scattered experimental units for randomised complete block designs include scattered plants, animal burrows and whole wetlands in variable conditions.

Examples of lines for randomised complete block designs include lines of seeding or planting, herbicide spraying lines and orchard rows in variable conditions.

Examples of Randomised Complete Block Designs

The illustrated examples in this section are included to assist the application of the standard design type to individual trial situations. The examples cover trials involving experimental units that are plots, scattered individuals or lines. Once you have identified the design that suits your need, proceed to the information on setting up the trial on page 45.

Plots (page 40) are regular experimental units usually laid out in a regular pattern, for example:

- Sections of ground or areas of vegetation
- Grids of planted vegetation

Scattered experimental units (page 42) may be irregularly distributed, for example:

- Scattered trees, woody weeds or animal burrows
- Whole wetlands

Lines (page 44) involve application of treatments to experimental units arranged in lines, for example:

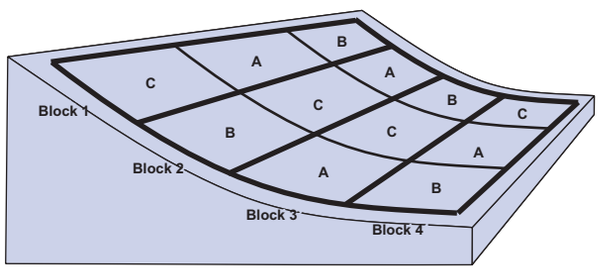
- Lines of direct seeding or planted tubestock
- Herbicide spray lines
- Rabbit trailing lines
- Orchard or vineyard rows

RCBD examples using plots

Plots are regularly laid out experimental units, for example sections of ground, areas of vegetation or grids of planted vegetation. See below for examples of RCBDs using plots in different situations.

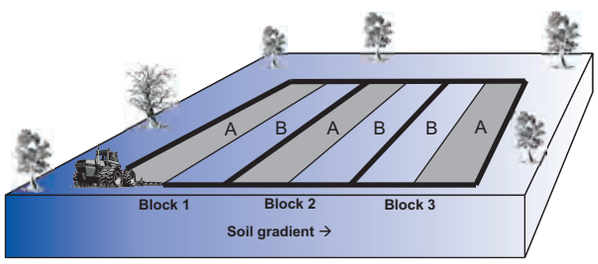
RCBD plots along an environmental gradient

e.g. a sloping site, a site with a changing soil type.



In this example, the blocks are spread along a steep gradient. Each treatment occurs once in each block.

Refer to page 45 for assistance in setting up this design.

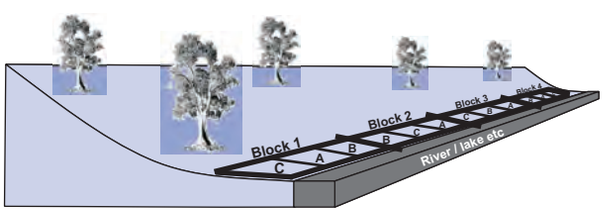


In this example, the plots are linear due to practical considerations (e.g. using large equipment). The site has been divided into three blocks along the soil gradient and the two treatments are randomly allocated once each within each block

Refer to page 45 for assistance in setting up this design.

RCBD plots along a linear feature

e.g. along a river, creek, lake edge, cliff, revegetation shelter-belt or boundary of remnant vegetation.

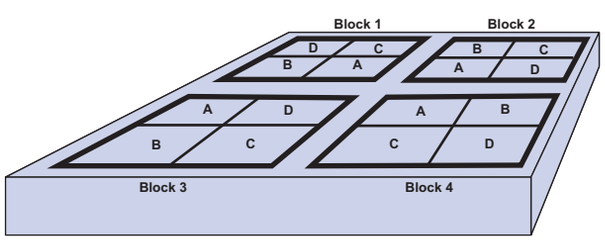


In this example, the trial area is broken into blocks because conditions are not homogenous along the linear features entire length.

Refer to page 45 for assistance in setting up this design.

RCBD plots where variability is assumed but unknown

e.g. for sites where there is likely to be underlying variability in soil type, shade, weed competition.



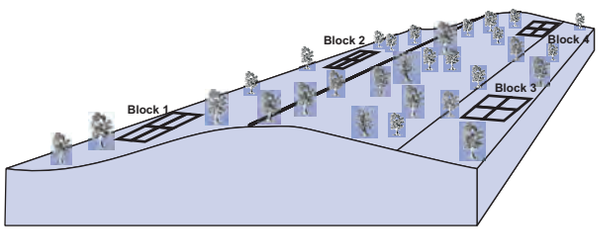
In this example, the treatments A, B, C and D are spread evenly across the site in four regular blocks. This design will reduce bias due to underlying variability that exists at the site.

Refer to page 45 for assistance in setting up this design.

RCBD examples using plots cont.

RCBD plots across multiple sites

e.g. trials that include a number of properties or a number of distant sites on a single property.

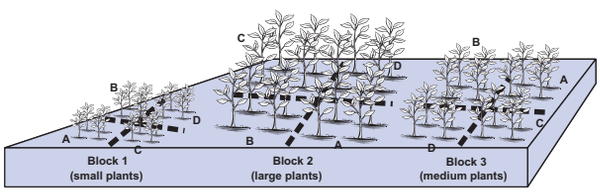


In this example, each of the four sites have been set up as a single complete block (containing one replicate of each of the four treatments). This design accounts for the variability that will exist between sites separated by large distances.

Refer to page 45 for assistance in setting up this design.

RCBD plots using variable material

e.g. trials where the size, age, condition of the experimental material is variable and not the subject of investigation.

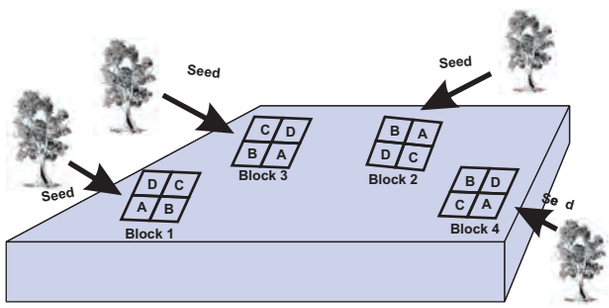


In this example, plant size is not part of a treatment, however the effect of having plants of different sizes could influence the results. To account for any effect that plant size has on the results, the seedlings have been sorted into three size categories and each has been used in a separate block.

Refer to page 45 for assistance in setting up this design.

RCBD plots using different parent material

e.g. trials where the material being used (eg seed, plants, animal offspring etc) are distinct because of their origin or parentage.

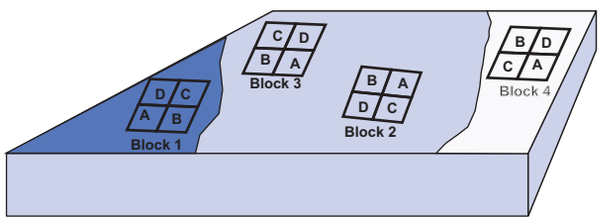


In this example, the seed of four different parent trees has been used in the trial. To ensure that the different genetics of the seed does not confuse the results (e.g. seed from one tree being more viable or vigorous), a separate block has been established from seed from each tree.

Refer to page 45 for assistance in setting up this design.

RCBD plots on sites with distinct variability

e.g. sites where areas of distinct variability (e.g. soil type, weed populations, shade, etc) are known.



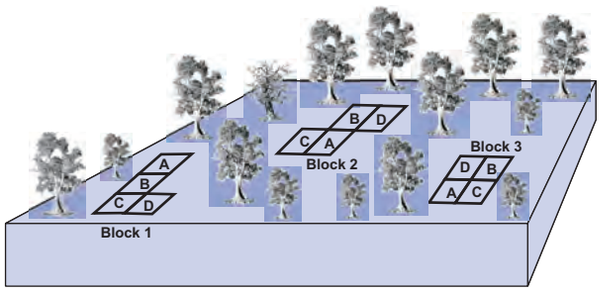
In this example, three distinct areas of soil type have been identified and the blocks have been placed wholly within a single soil type. This accounts for the variability across the site and ensures that the soil type within each block is as homogenous as possible.

Refer to page 45 for assistance in setting up this design.

RCBD examples using plots cont.

RCBD plots where only certain areas within a site can be used

e.g. trials within remnant vegetation, in rocky paddocks or where the areas of interest are patchy across the site.



In this example, the plots and blocks have been fitted in among scattered trees. The layout of the plots does not have to be consistent or regular.

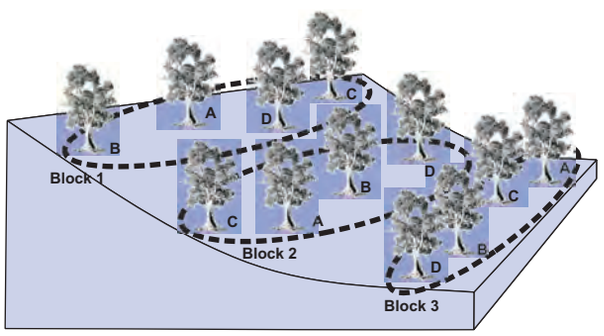
Refer to page 45 for assistance in setting up this design.

RCBD examples using scattered experimental units

Scattered experimental units are those that are irregularly distributed, for example scattered trees, woody weeds, whole wetlands and animal burrows. Blocks can be made by grouping experimental units that are similar or under similar influence but are different to those in other blocks.

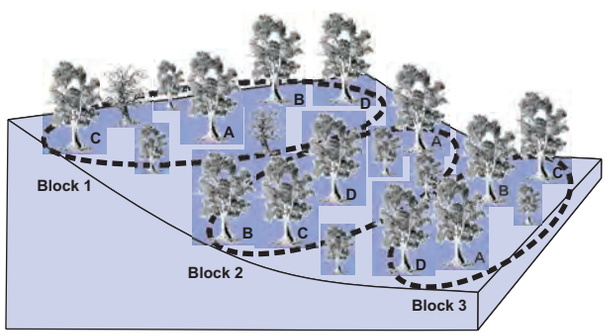
RCBD scattered experimental units on environmental gradient

e.g. trees, weeds, burrows or wetlands on a gradient such as elevation, soil type or salinity.



In this example, the experimental units (individual trees) have been grouped into blocks according to position on the slope. Here all of the trees are of similar size and condition and can therefore be used as experimental units.

Refer to page 45 for assistance in setting up this design.



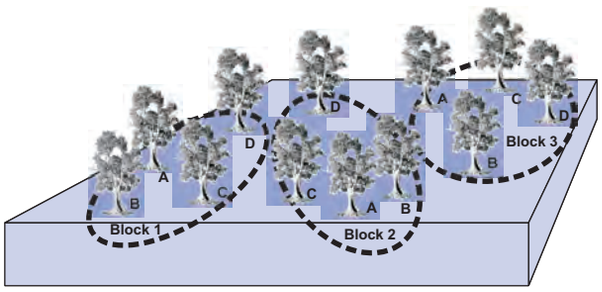
In this example, the experimental units (individual trees) have been grouped into blocks according to position on the slope. Here only some of the trees are suitable for use as experimental units (i.e. trees that are too old or too young are not included).

Refer to page 45 for assistance in setting up this design.

RCBD examples using scattered experimental units cont.

RCBD scattered experimental units where variability is unknown but assumed

e.g. trees, weeds, burrows, wetlands, etc where variability in soil type, moisture, salinity is assumed to underlie the site but is not clearly identifiable.

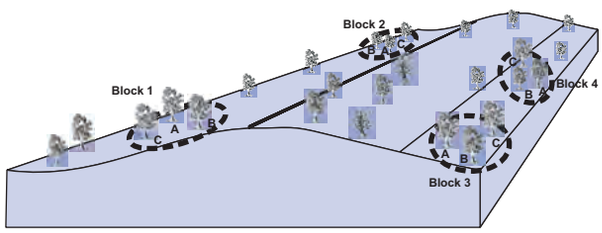


In this example neighbouring experimental units (individual trees) have been grouped into blocks because it is assumed that variability exists across the site.

Refer to page 45 for assistance in setting up this design.

RCBD scattered experimental units across multiple sites

e.g. trials that include a number of properties or distant sites on a single property.

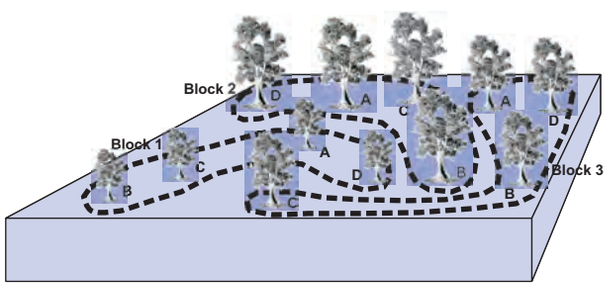


In this example, each of the four sites has been set up as a single complete block (containing one replicate of each of the three treatments). This design accounts for the variability that may exist between sites.

Refer to page 45 for assistance in setting up this design.

RCBD scattered using variable experimental units

e.g. trials where the experimental units being used vary in size, age, etc.

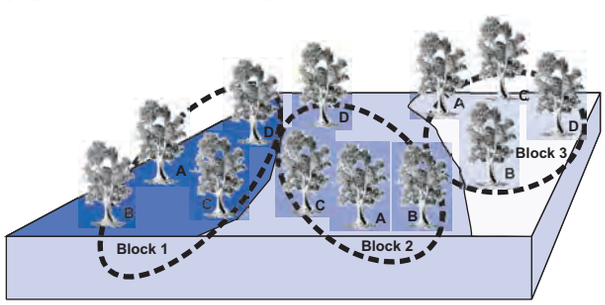


In this example, the number of trees is too limited to enable the use of only one size of tree. To overcome the problems of including trees of different sizes, each size class is being treated as a block (Block 1–small trees, Block 2–large trees and Block 3–medium trees).

Refer to page 45 for assistance in setting up this design.

RCBD scattered experimental units with distinct site variability

e.g. trees, weeds, burrows, wetlands that occur across sites where areas of distinct variability (e.g. soil type, weed populations, moisture regime) are known.



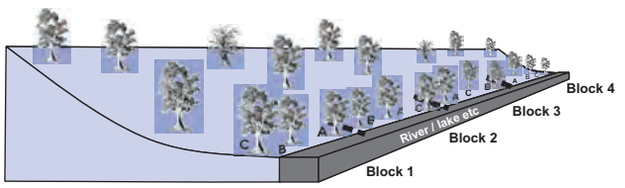
In this example, three distinct areas of soil type have been identified and the blocks are made up of trees which all grow within a single soil type. This accounts for the variability across the site and ensures that the conditions within each block is as homogenous as possible.

Refer to page 45 for assistance in setting up this design.

RCBD examples using scattered experimental units cont.

RCBD scattered experimental units along a linear feature

e.g. along a river, creek, lake edge, cliff, revegetation shelter-belt or boundary of remnant vegetation.



In this example, the blocks are spread along the linear feature because conditions are not homogenous along its entire length.

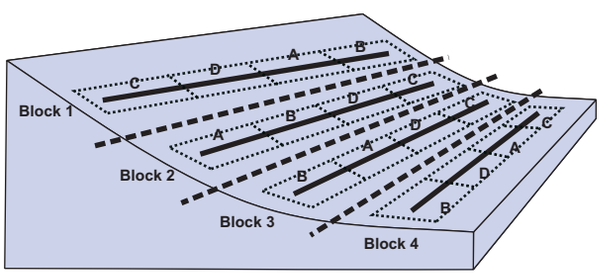
Refer to page 45 for assistance in setting up this design.

RCBD examples using lines

This section refers to trials that involve linear treatments or experimental units, for example lines of planted tubestock, direct seeding lines, herbicide spray lines, rabbit trailing lines or orchard or vineyard rows.

RCBD lines across an environmental gradient

e.g. lines of planted vegetation running on the contour of a slope.

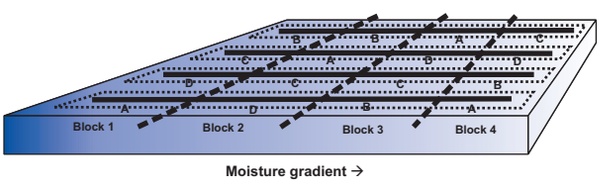


In this example, each of the lines is running along the contour on a different elevation so each has been treated as a block with a section of each line receiving a single replicate of each treatment.

Refer to page 45 for assistance in setting up this design.

RCBD lines along an environmental gradient

e.g. lines of planted vegetation running up a slope or along a moisture gradient.

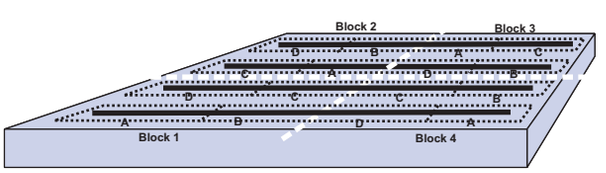


In this example, the lines are running in the same direction as a moisture gradient due to some practical consideration. The blocks are created at right angles to the gradient with each block containing a section of each line.

Refer to page 45 for assistance in setting up this design.

RCBD lines where variability is unknown but assumed

e.g. lines of planted vegetation where variability in soil type, moisture, salinity etc is assumed but is not clearly identifiable.

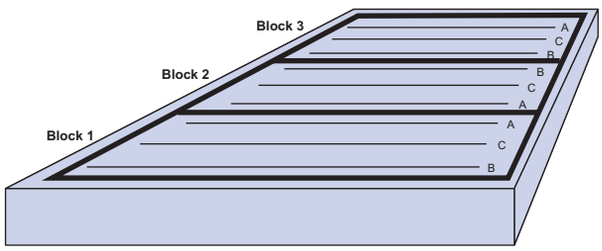


In this example, site variability is assumed although it has not been clearly identified. To account for potential variability the site has been split into four blocks with two sections of line in each block.

RCBD examples using lines cont.

RCBD lines where treatments require full lines

e.g. lines of planted vegetation where it is not practical to split the line into different treatments.

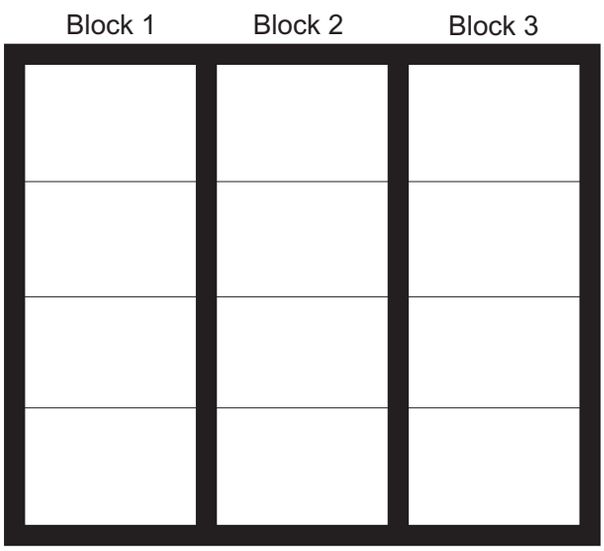


In this example, all of the experimental units (sections of line) do not differ significantly from one another. The three replicates of treatments A, B and C are randomly allocated to one of three lines within each block.

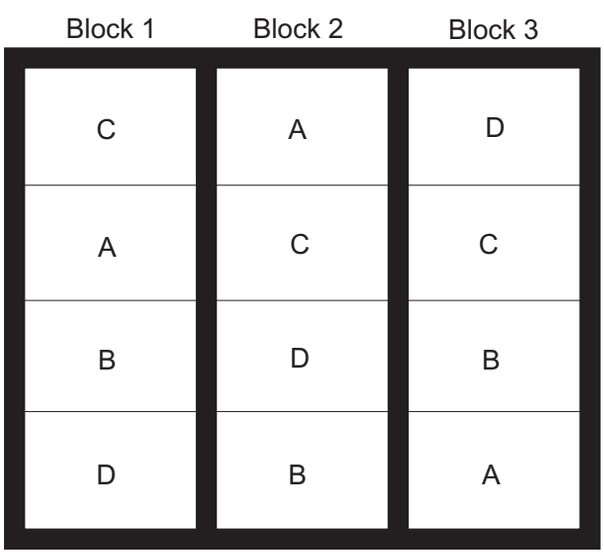
Refer below for assistance in setting up this design.

Setting up a RCBD trial

In the RCBD the number of replicates and the number of blocks is the same. The example below shows a trial using plots.



The first step is to make a sketch of the trial site and break it into blocks. Here the site has been broken into 3 blocks. Each block needs to include every treatment once so it must contain the same number of experimental units (plots of land, trees, etc) as the number of treatments to be applied. In this case there are four treatments and therefore four experimental units per block.



Treatments can then be assigned to one experimental unit in each block.

An easy way to do this is to write each treatment once on separate slips of paper and put them in a hat. For each block, pull the slips out one by one and assign them to the experimental units for that block. Put the slips back in the hat and do the same for the next block. Repeat this until all experimental units in each block have a treatment assigned to them.

The Randomised Complete Block Design is now ready to go! Proceed to Stage III of these guidelines on page 51.

Split Plot Designs

If practical or physical constraints prevent random allocation

Trial layout: *Trial Area → +/- Blocks →Main Plots → Sub Plots → Exp. Units*

Split plot designs (SPDs) are useful in situations where different treatments need to be applied at different scales (e.g. some treatments requiring large equipment) or there is a large potential for treatments to interfere with one another (e.g. irrigated and non-irrigated treatments).

Split plot designs can be set up with or without blocking. This section illustrates how SPDs can be used in a range of situations, both blocked and unblocked. It also provides a guide for developing a SPD for your trial.

Examples of plots for split plot designs include sections of ground, areas of vegetation and grids of planted vegetation.

Examples of Split Plot Designs

The illustrated examples in this section are included to assist the application of the standard design type to individual trial situations. The examples cover trials involving experimental units that are plots, scattered individuals or lines.

Once you have identified the design that suits your need, proceed to the information on setting up the trial on page 49 (no blocking) or 50 (blocking required).

Plots (page 47) are regular experimental units usually laid out in a regular pattern, for example:

- Sections of ground
- Areas of vegetation
- Grids of planted vegetation

Scattered experimental units (page 47) may be irregularly distributed, for example:

- Scattered trees
- Woody weeds
- Whole wetlands
- Animal burrows

Examples of scattered experimental units for split plot designs include scattered plants, animal burrows and whole wetlands.

Lines (page 48) involve application of treatments to experimental units arranged in lines, for example:

- Lines of planted tubestock
- Direct seeding lines
- Herbicide spray lines
- Rabbit trailing lines
- Orchard or vineyard rows

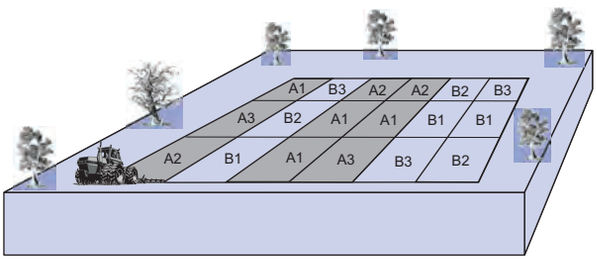
Examples of lines for split plot designs include lines of seeding or planting, herbicide spraying lines and orchard rows.

SPD examples using plots

Plots are regularly laid out experimental units (eg sections of ground, areas of vegetation or grids of planted vegetation). See below for examples of SPDs using plots in different situations.

SPD plots in homogenous conditions

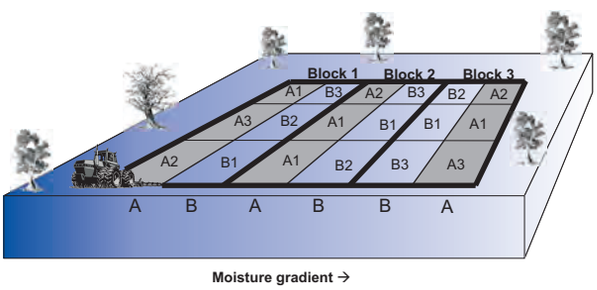
e.g. a flat open paddock with uniform soil conditions.



In this example the main treatments A (dark)and B (light) are randomly assigned to the main-plots. These main-plots are then broken into three sub-plots and the sub-treatemnts 1, 2 and 3 are randomly assigned once each within each main-plot. This results in the six treatments A1, A2, A3, B1, B2 and B3.

SPD plots in when blocking is required

e.g. variable site conditions, environmental gradients.



In this example, three blocks have been set up to cover different sections of a soil moisture gradient. Each block contains a single replicate of each of the treatments laid out in main-plots and sub-plots.

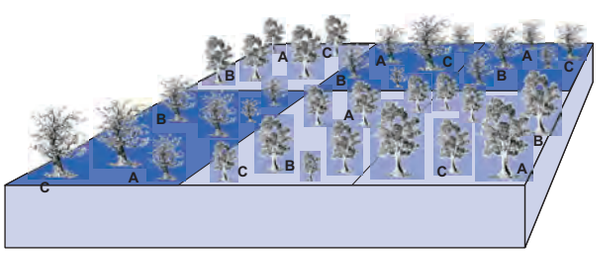
Refer to page 50 for assistance in setting up this design.

SPD example using scattered experimental units

Scattered experimental units are those that are irregularly distributed (eg scattered trees, woody weeds, whole wetlands and animal burrows). See below for an example of a SPD using scattered experimental units.

SPD scattered experimental units

e.g. trees, woody weeds, burrows, wetlands.



In this example, the main-plot treatment is fire and the sub-treatments are applied to individual trees. In variable sites blocking could be used as in the last example.

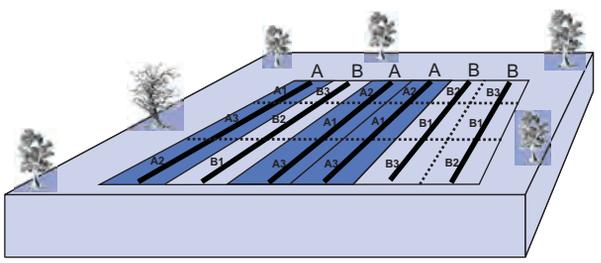
Refer to page 49 (no blocking) or 50 (blocking required) for assistance in setting up this design.

SPD examples using lines

This section refers to trials that involve linear treatments or experimental units, for example lines of planted tubestock, direct seeding lines, herbicide spray lines, rabbit trailing lines or orchard or vineyard rows.

SPD lines in homogenous conditions

e.g. lines of planted vegetation, vineyard rows.

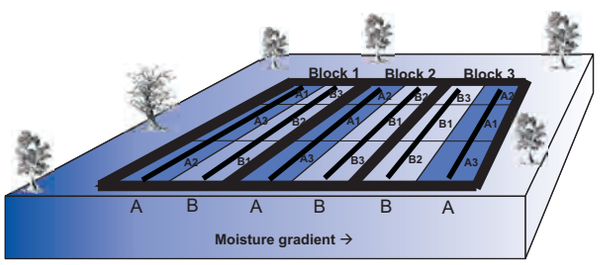


In this example, the main-plot treatments is applied to full lines/rows. These are then broken down into sections for the sub-treatments to be applied.

Refer to page 49 for assistance in setting up this design.

SPD lines when blocking is required

e.g. variable site conditions, environmental gradients etc



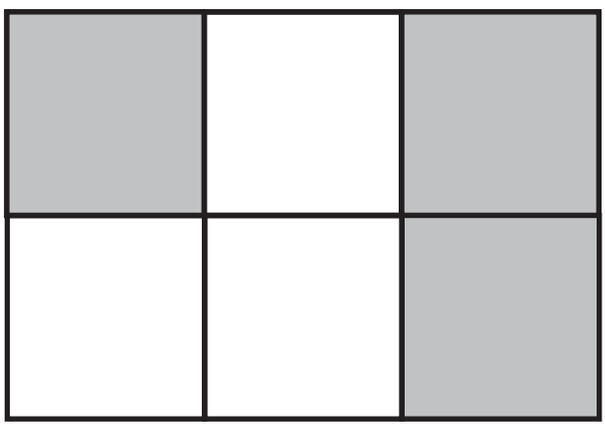
In this example, three blocks have been set up to cover different sections of the soil moisture gradient. Each block contains a single replicate of each of the treatments laid out in main-plots and sub-plots.

Refer to page 50 for assistance in setting up this design.

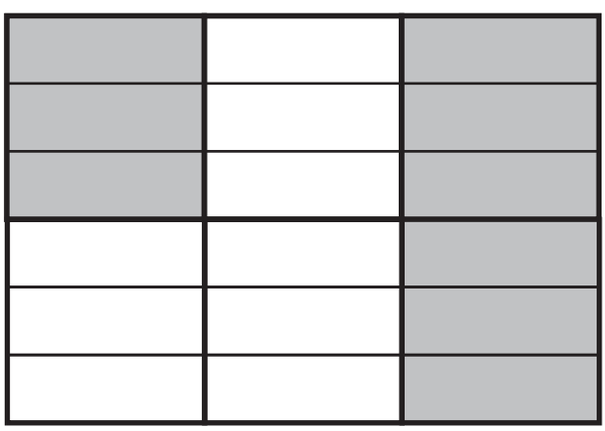
Setting up a SPD trial when blocking is not required

Setting up a SPD trial involves breaking the site up into main and sub plots. This example uses main-plots that are either irrigated or non-irrigated and sub-plots for three different fertiliser rates.

The first step in setting up a SPD trial when blocking **is not** required (Q6.1 or Q6.2), is to break your trial site into the required number of main-plots. In this example three replicates are to being so six main-plots are needed (three irrigated and three non-irrigated = six plots). Draw a diagram of this basic layout.



The next stage is to randomly assign the main-treatments to the main-plots. An easy way to do this is to write each main treatment on separate slips of paper the number of times you intend to replicate them. The number of slips of paper you end up with should be equal to the total number of main-plots in your trial. You then put the slips into a hat and pull them out one by one, assigning them in order to your main-plots (draw these on your diagram). In this example the shaded main-plots indicate those to be irrigated.



Next you need to split each main-plot into the required number of sub-plots. In this example this is for the three rates of fertiliser to be compared. Update your diagram again.

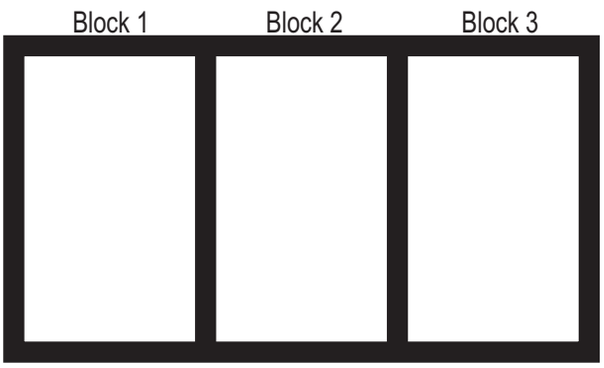
B	A	C
A	B	A
C	C	B
A	B	A
B	C	C
C	A	B

Once you have done this you have to randomly assign each of the sub-treatments to the sub-plots within each main-plot. To do this write each sub-plot treatment once on separate slips of paper and put them in a hat. For each main-plot, pull the slips of paper out one by one and fill in the sub-plots within it. Continue until all sub-plots within each main plot are filled out.

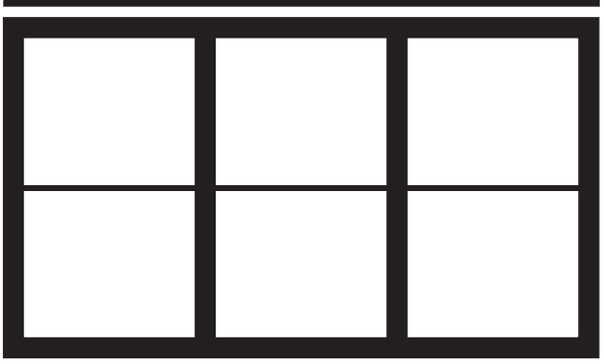
Your SPD is now ready to go! Proceed to Stage III of these guidelines on page 51.

Setting up a SPD trial when blocking is required

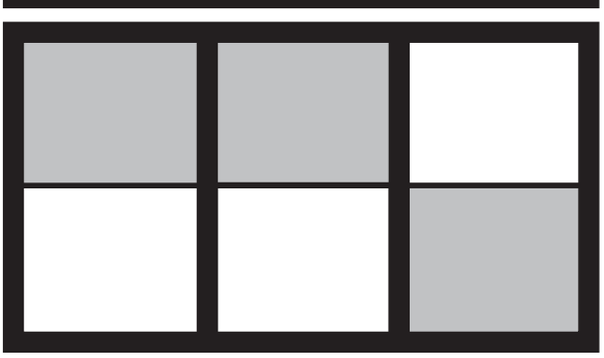
Setting up a blocked SPD trial involves breaking the site up into blocks and then main and sub plots. This example uses main-plots that are either irrigated or non-irrigated and sub-plots for three different fertiliser rates.



The first step to setting up a SPD trial when blocking **is** required (Q6.1 or Q6.2), is to is to break your trial site into the required number of blocks. The number of blocks equals the number of replicates to be used. Draw a diagram of this basic layout.

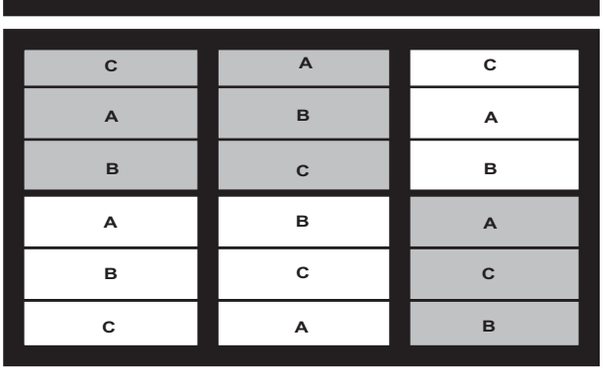


Next break each of these blocks into main-plots (a single replicate of each main plot treatment in each block). In this example there are two main-plot treatments, irrigated or non-irrigated. Draw this on your diagram.



The next stage is to randomly assign the main-plot treatments to the blank main-plots within each block. An easy way to do this is to write each main treatment on separate slips of paper only once. The number of slips of paper should be equal to the total number of main-plots treatments in your trial. Put the slips into a hat and pull them out one by one, assigning them in order to the main-plots within each block. Put the slips back in the hat and continue for all blocks. In this example the shaded plots represent those to be irrigated. Draw this on your diagram.

Next you need to split each main plot into the required number of sub-plots. Here three rates of fertiliser are to be compared within both the irrigated and non-irrigated main-plots. Update your diagram again



Once you have done this you have to randomly assign each of the sub-treatments (e.g. Fertiliser rates) to the sub-plots within each main-plot. To do this write each sub-plot treatment once on separate slips of paper and put them in a hat. For each main-plot, pull the slips of paper out one by one and fill in the sub-plots within it. Continue until all sub-plots within each main plot are filled out.

Your SPD is now ready to go! Proceed to Stage III of these guidelines on page 51.

STAGE III

Completing the cycle

Completing the cycle

This stage provides a brief overview of the steps involved in implementing a trial, collecting and analysing data, interpreting and evaluating the results and reviewing the outcomes in terms of the management decisions to be made. Having planned your trial well it is now important to implement it properly and use the results as part of adaptive management by completing the 8-step cycle of effective trials (see page 10).

Implement the trial

After completing Stage II, the trial design should be appropriate to the management objectives and adequate to ensure the dependability of the results. Care should be taken to implement the trial as planned and note any deviations or omissions from the plan. If unforseen practical obstacles arise, it may be better to redesign the trial than to continue with a sub-standard or poorly focussed trial.

Some tips when implementing your trial include:

- *Ensure the trial plan and layout has been well communicated with all those involved in setting it up;*
- *Simulate the set up of each treatment to check if there are any parts of the procedure which are open to interpretation;*
- *Ensure a standard method is being used if more than one person is involved;*
- *Laminate your trial layout information especially if there is the chance of rain, etc;*
- *Encourage checking and cross-checking of work where the design is complicated;*
- *Clearly label experimental units and treatments so that they can be identified without the plan;*
- *Record what you do and maintain good records of the actions;*
- *Take photos of the site and/or experimental units before and after setting up. This may assist to identify changes and relocate experimental units.*

The hard work is made easier in the knowledge that the trial is properly planned and designed.

Taking photographs and making good diagrams will assist later when it comes to collecting data and interpreting results.

Data Collection

As part of the trial planning process you will have already decided what you are going to measure and when

Collecting the data.

and how the measurements will be taken. It is good policy to develop a data collection plan to ensure everyone involved in the trial understands what is to be done. It may also be advisable to discuss the data collection with anyone involved in the collection to ensure that methods remain consistent between operators. Data collection sheets should be checked before use to ensure that they are appropriate to the trial. It may be helpful to test them on a few experimental units or on "dummy" data and refine them if necessary before collecting all data.

Quality control processes should be implemented to ensure consistency and accuracy in data collection and storage processes. Ensure that individuals taking measurements are checking and discussing their actions with others and that all necessary information is recorded on data sheets.

The data collection may involve measuring the response of a species to different management actions.

Entering data into spreadsheets, tables or statistical packages is another point at which errors can enter the data. Data should be verified after entry to check that typing errors or other mistakes are eliminated. In general, the larger the trial, the more care should be taken to coordinate and check processes that ensure the quality of the data.

Evaluate

Evaluating the results of a trial involve two processes. The first is analysis of the data to reduce the bulk of data to manageable units and to compare treatments and levels of treatments. The second is interpretation of the results with respect to the objectives of the trial and the management decisions to be made.

Without these two stages most of the work in undertaking the trial will be wasted. However, data analysis may require specialist help and data interpretation may be improved if a broad range of experience is consulted over the results. This expertise and experience may be difficult to access but is essential to a good trial.

The first step involved in evaluation is the analysis of the collected data.

Before proceeding to detailed analysis it is advisable to look at the raw data using summary statistics (e.g. mean, median) and graphs. This can help to determine the pattern of response to treatments and can give an indication of the variation between treatments and between experimental units. Data analysis should usually not stop at an inspection of the summary statistics but should continue with a thorough examination of the effects of different treatments and any influence of other factors associated with the design (e.g. Blocking).

The second process involved in evaluation is the interpretation of the results with respect to the trial's objectives.

A properly designed trial may be analysed using a number of methods depending on the experience and capability of the analysts (usually someone with a working knowledge of statistics, statistical models and computer analysis packages).

If the data is to be analysed by someone who was not involved in undertaking the trial or data collection, it is a good idea to provide them with the trial plan, including the factors, number of factor levels, use and type of randomisation or blocking, and the data itself.

It may also be necessary to provide notes on missing experimental units or treatments and it is a good idea to have a meeting with the person analysing the trial data to discuss any of the finer points of the trial. Always check the results after analysis to ensure they align with what was obvious or intuitively apparent from the raw data and summary statistics.

Here the results of a trial to establish reeds for erosion control are discussed.

After analysis the size of differences due to different treatments and the level of confidence in the wider application of the results will be known. These results should then be considered in the light of the original management questions / objectives that led to the trial.

Review

Once the data from a well planned and implemented trial has been collected and evaluated, the reliable results can be used to inform future management practices or further investigations.

The results will probably be of interest and use to others so make sure that they are promoted and available through extension staff, newsletters, reports, presentations and internet sites.

This is also a good time to reflect on the trial and its outcomes. What have been the benefits of undertaking the trial? What have been the costs? If the results suggest new management techniques, are there any issues that might emerge from these changes? Are further investigations needed?

Having completed the 8–step cycle you can be confident in your results and begin to apply this new knowledge and understanding to your on-going management activities and investigations.

A good trial will inform and improve on-going management activities.

It is important to involve others in your trials and share the results so that everyone can benefit from your work. Here a revegetation trial is being visited on a field day.

Further reading

Further reading

Trial Planning and Design

1. Montgomery, D.C. (2005) *Design and Analysis of Experiments.* John Wiley & Sons Inc., New York.

2. Quinn, G.P. and Keough, M.J. (2002) *Experimental Design and Data Analysis for Biologists.* Cambridge University Press, United Kingdom.

3. Lorenzen, T.J. and Anderson, V.L. (1993) *Design of Experiments: A No-name Approach.* Marcel Dekker Inc., New York.

4. Hoshmand A.R. (1994) *Experimental Research Design and Analysis: A Practical Approach for Agricultural and Natural Sciences.* CRC Press Inc., London.

5. Statistical Services Centre, (1998) *Statistical Guidelines for Natural Resources Projects.* The University of Reading. www.rdg.ac.uk/ssc/.

6. Le Clerg, E.L., Leonard, W.H. and Clark, A.G. (1966) *Field Plot Technique (2nd Edition)*, Burgess Publishing Company, United States.

7. Mead, R. (1988) *The Design of Experiments: Statistical Principles for Practical Application.* Cambridge University Press, United Kingdom.

Statistical Principles of Trial Design

8. Sokal, R.R. and Rohlf, F.J. (1995) *Biometry: the principles and practice of statistics in biological research.* (3rd Edition), W.H. Freeman and Co., New York.

9. Moore, D.S. and McCabe, G.P. (2003) *Introduction to the Practice of Statistics (4th Edition).* W.H. Freeman and Co., New York.

10. Kuehl, R.O. (1994) *Design of Experiments Statistical Principles of Research Design and Analysis (2nd Edition).* Duxbury Press, California, United States.

11. Pearce, S.C. (1983) *The Agricultural Field Experiment A Statistical Examination of Theory and Practice.* John Wiley and Sons, New York.

Appendices

Appendices

Appendix 1–Trial planning and design record sheet

Use photocopies of this record sheet to record your answers to the questions presented in these guidelines.

Appendix 2–Trial implementation record sheet

Use photocopies of this record sheet to record the details relating to the implementation of your trial.

- PHOTOCOPY -

Appendix 1 - Trial planning and design record sheet

Trial name: _____

Name: _____ **Date:** _____

Stage I Planning your trial

1. Assess the problem / opportunity

Question 1.1–What is the specific problem or opportunity you wish to address with this trial (page 15)? _____

_____

_____

Question 1.2–What is the scale and importance of the problem or opportunity (page 15)?_____

_____

_____

Question 1.3–Question 1.3What information / knowledge concerning the problem or opportunity already exists (page 15)?

_____

_____

_____

_____

Question 1.4–What are the gaps in existing knowledge that your trial aims to fill (page 15)? _____

_____

_____

_____

_____

2. Set the objectives

Question 2.1–What is your management objective (page 16)? _____

_____

_____

_____

Question 2.2–What is your specific trial objective (page 16)? _____

_____

_____

_____

Question 2.3–What is your trial hypothesis (page 17)? _____

_____

_____

Question 2.4–What is your null hypothesis (page 17)? _____

_____

_____

3. Select the treatments

Question 3.1–What have learnt from the literature and from discussing the trial with others regarding appropriate treatments (page 18)?

_____

_____

_____

_____

Question 3.2–What are the factors in your trial? What are the levels within each of your factors (page 19)?

_____

_____

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_____

_____

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_____

Question 3.3–What is your factorial treatment structure and total number of treatments (page 19)? _____

_____

_____

Question 3.4–Does your trial require a control treatment (page 19)?_____

_____

_____

Question 3.5–How many treatments and control treatments are to be used in your trial (page 19)? List them in full.

_____

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4. Choose the trial site

Question 4.1–Does your trial involve one or more than one site (page 20)? _____

_____

Question 4.2–What is / are the main source(s) of variability at your site (page 21)? _____

_____

_____

_____

_____

5. Decide on the experimental unit

Question 5.1–Will site variability and limited area or available experimental material impact on the size of experimental units (page 23)? Briefly explain your answer.

Question 5.2–What is the experimental unit? What are the characteristics of the experimental units (e.g. size, age) (page 23)?

Question 5.3–Is there the potential for interaction between experimental units? Can this be minimised by manipulating plot size, shape or orientation or by leaving a buffer zone around experimental units (page 24)?

Question 5.4–Is there a need to use main-plots and sub-plots? Which treatments will be the main plots and which will be the sub-plots (page 24)?

6. Account for variability

Question 6.1–Do you need to block to account for site variability? What source of variability do you need to block for (page 26)?

Question 6.2–Do you need to block to account for other sources of variability (page 26)? _____

_____

_____

_____

7. Decide on the number of replicates

Question 7.1–What is the level of underlying variability between experimental units (page 27)? _____

_____

_____

_____

_____

Question 7.2–What is the expected size of any difference between treatments (page 27)? _____

_____

_____

Question 7.3–What is the level of confidence required in the results of the trial (page 28)? _____

_____

_____

Question 7.4–What is the number of replicates required for each treatment (page 28)?

_____

_____

Question 7.5–What is the total number of experimental units required (number of treatments x number of replicates of each treatment) (page 28)?

_____

_____

8. Randomly allocate treatments

Question 8.1–How will you randomly allocate the treatments to experimental units (page 29)? _____

_____

_____

9. Develop a measurement program

Question 9.1--What is/are your response variable(s) (page 30)?_____

_____

Question 9.2--What measurements will be taken as baselines (page 31)? _____

_____

_____

Question 9.3--What measurements will be taken at the intermediate stages (page 31)? _____

_____

Question 9.4-- When will the measurements be taken (page 31)?_____

_____

10. Develop an implementation program

Question 10.1--What is the implementation plan for the trial? (Who is going to do what? When is it all going to happen?) (page 32)

_____

_____

_____

_____

_____

11. Calculate trial costs

Question 11.1--How much will the trial cost (capital, in-kind contribution) (page 32)? _____

_____

_____

Stage II Finalising your design

Question 12.1--Which design type is most appropriate for your trial (page 34)? _____

_____

- PHOTOCOPY -

Appendix 2 - Trial implementation record sheet

Trial name: _____

Trial objective: _____

_____

Date/s: _____

Weather: _____

Implemented by: _____

Site location (include coordinates): _____

Site location map:

Site description: _____

_____

_____

Site history: _____

_____

Design type: _____

Number of replicates: _____

Treatments & details:

Treatment number	**Treatment** (e.g. fertiliser – Single Super)	**Details** (e.g. equiv to 200kg / ha spread by hand)

Diagram of trial layout / design (include treatment locations):

Details of a standard experimental unit: _____

_____

_____

_____

_____

Method of marking experimental units: _____

_____

_____

Comments: _____

_____

_____

Follow up / maintenance: _____

_____