CONDUCT OF FIELD EXPERIMENTS

Field experiments are needed to study the effect of treatments under the many interacting conditions of soil, climate, aspect, and types of farming. No pot or glasshouse trial can adequately replace the field trial for this purpose.

Bulletin No. 399

N.Z. Department of Agriculture

Revised in 1966

Conduct of Field Experiments

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FOREWORD

ENTHUSIASM for the job is essential for the proper conduct of field experiments. Unless the experimentalist is keen and interested and takes a pleasure in doing research, the quality and quantity of his work must suffer.

This state of affairs applies with more force in research than in most other fields of human endeavour. To the dedicated man experimental work is a pleasure. In earlier days scientific endeavour was largely in the hands of the amateur, who willingly gave to his "hobby" the enthusiasm it demanded. Today science is largely the province of those who make it a profession, but the need for the enthusiast still exists.

In 1937 Sir David Rivett gave a presidential address to the Australian and New Zealand Association for the Advancement of Science entitled "The Scientific Estate". In this survey of the role of science and the scientist he said, "The scientist knows well that his prime object is to ascertain the rules in accordance with which Nature works. The mere discovery of them affords a joy and pleasure of successful adventure". And again, "To observe and interpret the results is a task to fire the enthusiasm of any man with a lust for inquiry".

The experimentalist must be a man with a "lust for inquiry".

The rewards of his endeavour will come from the joy and pleasure that he finds in a well conducted piece of research brought to a successful conclusion.

Conduct of Field Experiments

Part 1 — Introduction

WHY ARE FIELD EXPERIMENTS NECESSARY?

Advances in laboratory technique and refinements of pot experimentation have not altered the fact that most practical problems in field research must at some stage or another be solved in the field where the results are to be applied. Only a minor proportion of the multitude of interacting factors of soil and climate that affect plant growth can be studied individually and in combination in the laboratory or greenhouse. This does not mean that such "inside" work is of no use. Though these interacting factors operate in the field, there is no guarantee that they will operate in a reproducible fashion. In field experiments we try to **measure** the variation that results and we try to **standardise** conditions of measurements as far as possible. But we cannot hope to **control** these factors in the way that is possible, to some extent, in the "controlled climate chamber," or in the glasshouse and the laboratory. Both methods of approach are necessary.

Another reason for field experiments, especially those of the more extensive kind, is to provide answers to questions in a form which can be readily translated into practical advice to the farmer. It is, for example, most dangerous to advise on fertiliser use on the basis of evidence from pot experiments or soil tests only.

Once it is necessary to introduce the grazing animal it is, of course, necessary to do field trials. Unfortunately, bringing in the animal also brings in a host of other variable factors and makes the conduct and interpretation of experiments with animals so much the more difficult.

A publication by the United States Department of Agriculture

("A Guide to Extensive Testing on Farms," 1954) lists the following broad problems that are solved by extensive tests on farms and not by experiments at a research area.

1. **To determine conditions in an area** (for example, the value of subterranean clover in Canterbury). The validity of the answer depends on getting an adequate number of tests over the area.

2. **To find responses for different regions** (for example, the response to fertiliser on a soil type).

3. **To find responses under actual farming conditions.** Practical considerations may make impossible the application of a research finding that demands very precise conditions of soil or stage of growth of a crop.

4. **To measure the profitableness of a practice.**

5. **To measure the variability of benefit** to farmers of different ability, with different types of farm management, on different soil types, and on different sized farms.

6. **To assess a practice when there is no single check** (control treatment). Where the control is "farmer's practice" and where this varies widely over an area it is simpler to try the new treatments against the farmer's method on his farm than to attempt to copy the many different methods in a trial or trials on a research area.

All of these points are additional reasons why field experiments are necessary. For the advisory officer a final most important reason is that by doing experiments he "learns by doing" and his advice concerning new practices is immeasurably improved as a result.

THE SCOPE OF THIS BOOKLET

A few remarks applicable to all experimental work are made in Part 2. These give some basic concepts and general rules to follow. A field experimenter cannot take a set of instructions for any operation on any particular crop into the field and apply them blindly. If he does, he will soon be in trouble. The most carefully prepared notes will not deal with all the eventualities that may occur. Because the need for improvisation will always arise, it is essential that the field man should have some general principles on which to work.

These general principles rest on three basic requirements:

1. Common sense.
2. Integrity, lack of bias; in other words, “scientific ethics”.
3. Statistical considerations.

Part 3 outlines the statistical considerations. If we are to use statistical analyses to help us in the conduct and interpretation of field experiments, we must follow the rules laid down by the statistician. It is no use playing a game of football and disregarding the rules of the game, and it is no use asking for the aid of the statistician unless you have satisfied his requirements regarding trial design and conduct. This leads to the major section of Designs of Experiments.

All statistical requirements must be leavened by a good measure of common sense. Sometimes it is possible to use another design or another method if common sense says that the first scheme of the statistician is unworkable. In some cases practical considerations make it impossible to comply with statistical requirements—but the decision to do work which cannot be statistically analysed should always be made with the full knowledge of the limitations of such work.

Parts 4 to 8 inclusive set out field technique and details of sowing, fertilising, managing, and harvesting trials on pastures and crops. These details are primarily for reference purposes. Some suggestions are given for improving efficiency and for saving time and effort.

After a satisfactory experiment has been conducted the next stage is to write-up the results. As this depends on accurate recording, the section on experimental records is not out of place in Part 10. To record work properly it is necessary to be able to read and interpret statistical tables, and terms and some notes are given on this subject in Part 3.

Finally we come to the preparation of the material for publication. A book could be written on this topic alone, but a few pointers and suggestions for layout of papers might be helpful. This is the final stage in experimental work. What follows is its application, whether to farming practice or as a basis for further work. Whatever happens, this is the critical phase of all investigations, but it is one which too frequently gets too little attention.

Some useful tables for drill settings and other calculations are given in the appendices.

Only passing reference is made to problems of experiments with animals. Though the general remarks on experimental method apply with equal force in this field, this booklet has been prepared primarily for those concerned with experimental work with pastures and crops.

Part 2 — Basic Requirements for a Satisfactory Experiment

It is convenient to consider these requirements in the following sequence:

1. Ask the right question.
2. Put the question in the right way.
3. Record the results adequately and accurately.
4. Summarise and analyse the results to get the most out of them.
5. Write up the results for publication in one way or another.

Part 2 considers 1 and 2 above, and Part 10 deals with recording, summarising, and writing up the results of experiments.

ASK THE RIGHT QUESTION

We can consider an experiment as a question put to nature. Obviously to get the answer we require, the right question must be put and it must be put clearly and at the right time.

To decide what is the right question may not be as simple as it seems. It demands:

(a) **A study of relevant published information:** This will help to ensure that the question is not one that has been put before. It will also help in framing the right sort of question.

(b) **Doing any necessary preliminary work:** Such work may take the form of "pilot" trials or surveys. It will probably include arranging preliminary soil, herbage, or other chemical analyses.

This type of work aids in defining the problem more exactly and assists in the selection of treatments for more precise evaluation in more precise trials later. It should also help to define the rates of application over which the trial treatments should range.

(c) **Having a clearly defined objective:** Woolly thinking has no place in experimental work. You should be able to put down on paper a clear statement of the aims and objects of the proposed investigation. You may need to study published work and do some preliminary investigation before this is possible. There is an important difference between a proposal entitled "fertiliser responses on Lismore soil" and "a study of the phosphorus and sulphur requirements of pasture on Lismore soil". In the latter a knowledge of the requirements of that soil and a logical selection of treatments are implied.

(d) **Selecting the right treatments:** To do this it is necessary to have a clearly defined objective and a full knowledge of the practical limitations of site, climate, type of farming, and ecological factors affecting the crop or pasture with which you are experimenting. Treatments and trial design are closely connected. The following are some of the points to consider when selecting treatments.

1. **Always have a "control" or basic treatment:** In fertiliser research this is usually (but not always) a "nil" treatment: in some circumstances it is a basic level of application. In variety trials the control is usually the most commonly grown variety in the district. In management trials control plots may be "farmer's practice". In a standard series of trials control plots may be selected for some special reason not related to district practice: for example, a lodging-resistant and uniform but not widely grown cereal variety. Statistical considerations require that "control" treatments be selected as such before the trial is laid down, and not selected, when results come in, as the lowest-yielding treatment of the trials.

2. **See that all nutrients other than those in the trial treatments are in adequate supply:** "Basal applications" of some of these other materials may be necessary. With variety trials the nutrients supplied should be adequate for the expression of maximum yields of all varieties. In many cases it is highly desirable to study varietal differences at two or more levels of nutrient supply.

3. **Select complete range of treatments whenever possible:** With most trials factorial combinations are the best (see Part 3). These give all possible combinations (for example, for L, P, and K comparisons at two levels (nil and "some") treatments would be O, L, P, LP, K, LK, PK, LPK). If only some treatments are selected from this range, statistical efficiency is reduced and the possible in-

formation from the trial is also reduced.

4. **Use a logical sequence of treatments:** The factorial trial is usually the best logical sequence. Another might be, for example,

Superphosphate	2 cwt and 4 cwt
Double superphosphate	1 cwt and 2 cwt
Control	

Here the rates of phosphate comparisons are of the same order (approximately) with each fertiliser. The difference between them is, essentially, one of sulphur (gypsum), so that this trial is really 0, P1, P2, P1S1, P2S2, which is a logical sequence of treatments but not a complete factorial series. The factorial series would be 2 rates of double superphosphate (1 cwt and 2 cwt) by 2 rates of gypsum (1 cwt and 2 cwt) + control; that is, 0, P1, P2, S1, S2, P1S1, P1S2, P2S1, P2S2. This is the better experiment.

5. **See that the basis of comparison between treatments is right.** Different forms of materials should usually be applied at equivalent rates of the active ingredient. For example, 4 cwt of serpentine superphosphate has about the same phosphate content as 3 cwt of superphosphate. Occasionally, however, it may be better to apply such materials at the same rate per acre if it is desired to test some other factor such as "phosphate saving power" or some economic consideration. If this is done, it should be additional to the basic comparison of equivalent rates of active ingredients.

6. **Make comparisons between materials at appropriate rates of application.** If two phosphatic fertilisers are compared only at a high rate of application, there is a distinct possibility that this is a "luxury" level and that the less efficient material may still supply enough phosphate for near maximum growth. Such comparisons are best made at a low rate or, better still, at both a low and a high rate of application.

7. In rates of application trials (fertilisers, spacings, etc.) **include an adequate number of rates** and see that the range of difference between rates is sufficiently great.

At least three rates of application are necessary to establish a "response curve" and more than three are preferred. From a study of previous work and experience in the field it is usually possible to guess what the optimum rate of application is likely to be. If this rate of application is X, with three levels the rates should be 0, 1¼X, and 2½X. With four levels the rates should be 0, 0.8X, 1.6X, and 2.4X. With five levels the rates should be 0, 0.6X, 1.2X, 1.8X, and 2.4X.

The optimum rate of application is usually taken as that which gives the greatest profit, not necessarily the maximum yield.

Having selected the treatments, and with a clear idea of what information we are seeking, we are now ready to start the field work.

PUT THE QUESTION IN THE RIGHT WAY

Step 1: Decide on the Regions of Application

The first step is to decide on the regions or conditions to which your experiments are going to apply. Practically all districts will have variable conditions of soil, climate, topography, and types of farming.

Most work nowadays is related to the soil type as a unit. Defining a soil type automatically defines to a considerable degree the climate and topography as well. But you must **know** your soils and their relative importance and farming use to place the field experiments to best advantage.

Let us see how this works. Suppose you desire to test a new variety of chou moellier. The information required is (a) the soil types on which chou moellier is grown and their relative importance as soils, (b) the importance of chou moellier as a farm crop in the farm management programme, (c) how and when chou moellier is utilised and by what classes of stock, (d) growing practices (time of sowing, fertiliser requirements, seed rates, etc.), (e) varieties commonly grown now and their advantages and disadvantages, and (f) diseases and insect pests attacking the crops.

With this background of knowledge you can select soil types on which the trials can be placed to best advantage. You must decide whether you are going to consider other factors as well—for example, dairy and sheep farms. In other words you will have to define the regions and the conditions to which the results of your trials will apply. This planning is particularly important to get the most out of the limited amount of time and money that can be devoted to the work.

Step 2: Decide on Type and Number of Trials Required

There are at least five factors that will influence the type and number of trials needed.

1. Nature of the Problem

Very simple types of observational trials may be adequate for survey types of investigation such as the defining of molybdenum-responsive soil types. More complex trials are necessary when we wish to study rates of application of molybdenum and the residual effects of molybdenum applications. Here we may need some measurement trials. If we wish to study the interactions of molybdenum with other applied plant nutrients, we shall probably need complex mowing trials supported by many chemical analyses. If we wish to study the effect on the grazing animal of molybdenum applied to pasture, we must of course use the animal in question and may well need some large-scale and complex experiments. Thus the investigation into the place of molybdenum in agriculture requires all types of trials.

Some weed control trials may be simple observational studies of the effect of weedkillers on a certain species; others may require replicated measurement trials to study the effect on pasture and crop production, and some may need to use the grazing animal in conjunction with weedkillers and require large-scale grazing management trials. If you have a clearly defined objective, the type of trial required is easily decided: if you have not sorted this objective out clearly, much work may be wasted on trials that are not suitable for the purpose.

2. Facilities Available and Number of Trials Required

These factors will, of course, affect the type of trial decided on, but if it is essential to do a measurement trial, you should be quite sure that you are not wasting your time doing observational trials to solve the same problem. It may be better to tackle another type of problem the solution of which is within your resources.

Considerations of available time and facilities will always set a maximum limit on the number of trials you can handle, but it is important to know what are minimum numbers necessary to get the information required. If you had defined your “regions of application” so that they were perfectly uniform in all respects and if one season was exactly like the next, one trial in one season per “region” would be enough. But because of the operation of many variable factors of soil, farm, and climate many more trials than one are required.

Generally the number required will depend on the uniformity of the “regions of application”. For a rough guide about four to six trials per year over a minimum of three years is necessary for most agricultural experiments. More simple than complex experiments are usually required, particularly if the complex trials try to estimate some of the variables within the region. For instance, 12 simple variety trials with six trials on each of (a) well fertilised and (b) poorly fertilised soils migh be replaced by six variety x fertiliser trials which incorporate the difference between (a) and (b) above. (It is not really as simple as this, but this gives the general idea.) Of course, if your neighbour joins forces and he has a similar “region of application,” the work can be shared with him. Most investigations on a national scale are planned to get the required number of trials per region and to extend the “regions” to which the results apply.

We have, therefore, two factors affecting the number of trials required: (1) variability of the region, and (2) the type of trial. A third factor is the size of the expected difference.

If differences between treatments are expected to be large, not only will simpler types of trials be sufficient, but also fewer numbers of them will be required. As the size of the expected differences between treatments gets smaller both the number of trials required and the degree of replication within each trial must be increased. In addition the precision of measurement must also be improved.

3. Size and Nature of the Expected Differences

As indicated previously this will affect both the number of trials and the type of trial required.

(a) **Size of differences:** If differences are expected to be large, simple trials with relatively few replications are sufficient. To detect small differences, accurate, well replicated experiments are essential.

(b) **Nature of differences:** Obviously the type of trial to adopt will depend on what sort of differences you are measuring. But there are also less obvious considerations to bear in mind. One of these, for example, is the probability that the nature of the differences between the production of pasture species will depend on the type of grazing management given. You may decide that a type of trial that incorporates different grazing management treatments may be essential to test adequately differences between pasture species.

Similarly differences among treatments may be measured in a number of different ways. With cereal trials, whether you are measuring just yield of grain or whether you also wish to measure yield of straw, composition of grain and straw, germination and establishment of plants, and so forth, will determine the type of trial to adopt. Large plots may be needed as samples have to be taken from them at different stages of growth.

The nature of the expected differences due to treatment might be such that observations will give a better measure of them than will yield data. This may arise, for example, where the disturbance of the crop or pasture that is caused by the measurement technique is such as to interfere with the comparisons being made in the trial. Cutting certain plant associations to obtain yield data may destroy or seriously weaken desirable species. In some cases it may be possible to use large plots and by sampling methods destroy only a small portion of them when taking measurements.

4. Variability and Nature of the Plant being Tested

Generally, plots should be of such a size that they each contain a minimum number (say 30) of plants of the crop under test. Thus crops where each plant occupies much room (for example, maize) usually need large plots. With weed control trials on small, annual weeds micro-plots, say 1 yard square, may each contain an adequate number of plants. Within limits, the smaller the plot is the better. Usually a greater number of replications is possible and the soil variation between adjacent plots is reduced the smaller the plot is.

Some plants are naturally much more variable than others and bigger plots or more replications are needed to secure the same degree of accuracy that is possible with smaller plots and trials on more uniform plant material. Maize and potatoes, for example, are two extremely variable crops. With maize we have large plants, some bearing one cob, some two or more cobs, and a very great variation among individual plants. To obtain accurate comparisons among treatments relatively large plots and many replications are usually necessary. With wheat, on the other hand, we have uniform plant material and each plant takes up a small area. Accurate trials showing little variation due to factors other than treatment are, therefore, relatively easy to obtain in wheat experiments.

The nature of the plant affects the type of trial in many other ways. There is an obvious difference in the type of trial required for annuals compared with that for perennials. Weed control trials on spreading perennials may need designs with buffer strips around plots to prevent re-infestation from untreated plots and ineffective treatments.

5. Variability of the Soil under Test

Natural soil variation may be of many types. If the trial site is carefully chosen, soil variation may be small and fewer replications and small plots will give accurate comparisons among treatments. The reverse is true with trial areas showing much soil variability.

The type of variation due to soil will affect the choice of trial design and the shape of plots. If variation is patchy, long, narrow plots may be better than more square shapes which are more likely to coincide with the shape of these “patches”. If the trial site is very variable, the number of treatments and/or the block size (see Parts 3 and 4) should be kept small so that comparisons are made only between adjacent or nearly adjacent plots. Where the trial area is more nearly uniform comparisons may be made more safely between plots a reasonable distance apart. This matter is discussed again in the section on statistical considerations beginning on the next page.

Part 3 — Some Statistical Considerations

No profound mathematical knowledge is necessary to understand the reasons why experiments are designed and analysed in various ways and to get some idea of how to do this. The mathematical proof of these matters may be safely left in the hands of the mathematician.

The old saying that "there are three kinds of lies—lies, damned lies, and statistics"—has an element of truth in it, for statistics can be applied to unsound data and make them look impressive without in any way altering the fact that they are unsound. If a technician consistently adds a pound or so to the weight of produce from plots of a certain treatment, statistical analysis may well show that treatment to be "statistically significantly" higher yielding than the others. Don't blame statistics for this! The value of experimental data presented with statistical trimmings depends **wholly and solely on the nature of the material analysed. If it is biased in any way, the results are not valid.** Sound field technique is essential before statistical analysis is applied to experimental data.

Everyone who conducts field experiments must have at least a little statistical knowledge to enable him to conduct his trials efficiently so that the least amount of field work produces the maximum amount of reliable information. He needs statistical knowledge so that he can read intelligently the results of his and of other trials and draw the correct conclusions from them. An appreciation of the reasons why certain things are necessary leads to increased interest in the work and therefore to better work.

Experimental statistics is a special branch of mathematical theory quite distinct from the popular idea of "statistics" as applied to the work done by the Department of Statistics. The term "biometrics" is possibly better in agricultural work. This is statistics as applied to biological problems.

1. SOME BASIC IDEAS OF EXPERIMENTAL STATISTICS

Experimental statistics has two main aims:

1. The summarising of data into simple and readily understandable forms.
2. Assessing the reliability of the calculations based on the data. This, of course, assumes that the data are sound and completely unbiased. Statistical analysis sorts out the **variation** due to treatment from the variation due to soil, climate, and the multitude of other factors affecting the results, and assesses the value of these treatment effects in relation to those of the other factors affecting the experiment.

The idea of statistics leading to simplification may at first seem absurd, but imagine trying to interpret the results of a trial merely by a study of individual plot yields. Obviously the first thing to do is to find **means** or **averages** for each treatment. But these means vary greatly in reliability. Consider the following example:

	Individual values					Mean
A	0	5	10	15	20	10
B	8	9	10	11	12	10

Both A and B have a mean of 10, but the mean of B is a much more “reliable” figure than the mean of A.

It can be seen, therefore, that some way must be found to assess this reliability of means by showing the **variation** of the figures from which they were calculated. When means are compared this measure of variation can be used to assess the **reliability of the differences** between them. Let us take two examples:

Example 1

	Individual values					Mean
A	0	5	10	15	20	10
B	5	10	15	20	25	15

Example 2

A	8	9	10	11	12	10

B	13	14	15	16	17	15

The difference between A and B in Example 2 is much more "reliable" than that between A and B in Example 1. This illustrates a basic idea of experimental statistics—the study of variation.

Statistical analyses are done to test the reliability of such differences. The mathematical theory on which the analyses are based enables one to calculate the "probability" of obtaining any given difference between the means of two sets of values taken at random from the same very large population (a term for any series of data such as measurements of yield) of possible values. This probability will, of course, differ according to the variability of the particular population with which we are concerned. The range of values of the actual samples we have taken is an indication of the variability of the total population.

A series of experimental plots of one treatment in a random layout can be considered as one sample out of a very large number of possible samples that could have been obtained by varying the order of the plots. For eight treatments and six replications this possible number of arrangements is a number with 28 figures. Now if we have a certain difference between two treatment means, and calculations based on the theory mentioned above showed that there was a 1 in 2 chance that such a difference would have turned up between two samples drawn from the **same** population, it is reasonable to say that the two treatment means may well have been drawn from the same population. In other words, we have not proved that they differ and the two means are said to be **not significantly different.**

On the other hand, if the calculations had shown that there was only a 1 in 30 chance that such a difference would have turned up between the means of two samples drawn from the same population, it is reasonable to say that the sets of results did **not** belong to the same population. In other words, the treatment results showed a **significant** difference.

To avoid confusion certain **standards of significance** have been set, but it is important to realise that these are quite arbitrary and there is nothing to stop them being altered for different sets of circumstances. The standards usually taken are the **5 per cent** and **1 per cent** levels of significance. At the 5 per cent level there is a 1 in

20 chance (and at the 1 per cent level a 1 in 100 chance) that the difference at this level has arisen by chance. Differences greater than these standards are **significant** at the 5 per cent (or 1 per cent) level; those less than the standards are **not significant** at these levels. Where no level is stated, significance is assumed to be at the 5 per cent level. The 1 per cent level is sometimes indicated by the term "highly significant".

2. VARIANCE AND STANDARD ERROR (S.E.)

The way we measure variation in experiments is by calculating either the **variance** or the **standard error.** These two measures are simply related:

$$\text{Standard error} = \sqrt{\text{Variance}}$$

If we consider our first example again:

	Individual values					Mean
A	0	5	10	15	20	10
B	8	9	10	11	12	10

and we write down the deviations of each set of numbers from its mean we get:

A	—10	—5	0	5	10
B	—2	—1	0	1	2

Now we square each of these deviations and add them to give:

						Total
A	100	25	0	25	100	250
B	4	1	0	1	4	10

We now divide each of these totals by 4 (the number of **degrees of freedom***) and we have the **variance** of each set of numbers:

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* **The number of degrees of freedom which we use as the divisor is in simple cases like this just one less than the number of plots or figures. We know that the deviations themselves must add to zero, and therefore when we have written down 4 figures the fifth one is determined.**

A	62½
B	2½

The **standard errors** will be correspondingly

$$A = \sqrt{62\tfrac{1}{2}} = 7.9 \text{ approx.}$$

$$B = \sqrt{2\tfrac{1}{2}} = 1.6 \text{ approx.}$$

These standard errors (and variances) are each measures of the variability of their respective sets of figures. There are other possible measures of this variability, but these are by far the most useful.

Standard Error of the Mean

What we have just calculated is the standard error of 1 figure. The mean of 5 figures will be more reliable than 1 figure alone, and the mean of 10 figures will be more reliable still. This **increased** reliability will be indicated by a **decreased** variance and standard error; the way in which the variance decreases can be shown to be:

$$\text{Variance of mean of 5 figures} = \frac{\text{Variance of 1 figure}}{5}$$

$$\text{and consequently S.E. of mean of 5 figures} = \sqrt{\frac{\text{Variance of 1 figure}}{5}}$$

$$= \frac{\text{S.E. of 1 figure}}{\sqrt{5}}$$

In the examples we have just quoted, therefore, we have the following:

$$\text{A: S.E. per plot} = 7.9$$

$$\text{S.E. of mean} = \frac{7.9}{\sqrt{5}} = \frac{7.9}{2.2}$$

$$= 3.6 \text{ (approximately)}$$

$$\text{B: S.E. per plot} = 1.6$$

$$\text{S.E. of mean} = \frac{1.6}{\sqrt{5}} = \frac{1.6}{2.2}$$

$$= 0.7 \text{ (approximately)}$$

This shows that the second mean is much more **reliable:** it has been derived from individual figures which varied much less about the mean.

Statistical analysis of experiments is basically the study of such variations.

3. COEFFICIENT OF VARIATION (C.V.)

This is also known as the **“Standard Error per Plot as a Percentage of the Mean Plot Yield”** (S.E. as per cent M.P.Y.)

The standard error per plot was dealt with in Section 2 above. It will be clear that the size of this will be affected by the units we work in. The S.E. if we work in grammes will be 453.6 times the S.E. worked in pounds. There is a need, therefore, for a measure of the **variation of the trial** that is independent of the units in which we work. This is obtained by dividing the S.E. per plot by the overall trial mean (and multiplying by 100 to get a percentage).

$$\text{Coefficient of variation} = \frac{\text{S.E. per plot}}{\text{Overall trial mean}} \times 100$$

If this figure exceeds about 10 per cent in a cereal trial, for instance, it usually indicates that the trial is more variable than is desired. With some crops such as potatoes the C.V. is commonly about 15 to 20 per cent. If we want to increase the precision of potato trials we can **either** try to reduce the C.V. by improving the trial technique **or** increase the number of replications (or do both).

One drawback to the C.V. is that low-yielding trials tend to have higher values for C.V. than high-yielding trials, and this measure of trial accuracy must be interpreted with this factor in mind.

4. CAUSES OF VARIATION IN EXPERIMENTS

Some of the chief factors causing variation in experiments may be listed as follows. Most of these can be minimised by efficient field technique and by the application of sound experimental designs.

1. **Soil fertility variations:** These are assessed or minimised by adequate replication of treatments in good designs: by care in selecting trial sites and in laying down plots so that each receives a more or less equal share of known fertility variations: by replication of experiments, and by the use of methods of measuring or evaluating soil fertility, such as surveys, analyses, and uniformity data.

2. **Seasonal and year to year variations:** These can be overcome by conducting trials in as many seasons and years as possible and in as many different climatic districts as possible.

3. Failure to sow compared treatments under uniform conditions.

4. Lack of uniformity in seeds and fertilisers and in their application.

5. Lack of uniformity in after-treatment of plots.

6. Effect of some factor uniformly applied to all plots but affecting them differently (such as effect of palatability differences when plots are grazed).

7. Border and competition effects (such as "outside rows" in cereal trials).

8. Lack of care and interest on the part of the experimenter. This is, of course, the vital factor. Most of these matters are considered more fully in succeeding sections of this bulletin.

5. ANALYSIS OF VARIANCE

This is the most common method of treating experimental data. We shall consider a very small and simple trial with three rates of nitrogen replicated four times in a "randomised block" design (see "Designs of Experiments", pages 25 and 26). This is the most commonly used design in which treatments are arranged in blocks, each block having one plot of each treatment, as the following plan illustrates.

Block	**1.**			**2.**			**3.**			**4.**		
Treatments	**B**	**A**	**C**	**A**	**C**	**B**	**C**	**B**	**A**	**B**	**C**	**A**
Plot	1	2	3	4	5	6	7	8	9	10	11	12
Yield	11	8	14	6	12	10	11	12	7	11	11	7

Treatments: A = No N
B = 1 cwt of sulphate of ammonia per acre
C = 3 cwt of sulphate of ammonia per acre

Now we can say that the yield of plot 1 is due to four things:

1. General mean of the trial as a whole;
2. Block 1;
3. Treatment B;
4. Individual factors associated with plot 1,

and we can make similar statements for each of the other plots.

The first of these terms is, by definition, constant over the whole trial, and the **other three factors account for the variation** that is found among the plot yields. It is the technique known as "analysis of variance" that separates the variation between plots into its three components of

Blocks
Treatments
and Error.

Now let us continue with the analysis of variance of the simple trial we began with. If the yields are re-arranged in a table, we can take out block and treatment totals:

	Block	**A**	**B**	**C**	**Block**
	1	8	11	14	33
	2	6	10	12	28
	3	7	12	11	30
	4	7	11	11	29
Treatment	totals	28	44	48	120

and write down deviations from the means of treatments and blocks. The overall mean is 10, and so the deviation of individual treatments will be

Deviations Block totals Squares of devi- Block

					ations			**totals**
	— 2	1	4	3	4	1	16	9
	— 4	0	2	—2	16	0	4	4
	— 3	2	1	0	9	4	1	0
	— 3	1	1	—1	9	1	1	1
Treatment totals	—12	4	8	Treatment totals	144	16	64	

The sum of squares of deviations of blocks has to be divided by the number of plots in a block (3) and similarly the treatments sum of squares needs dividing by 4.

Blocks sum of squares = 9 + 4 + 0 + 1 = 14 ÷ 3 = 4⅔
Treatments sum of squares = 144 + 16 + 64 = 224 ÷ 4 = 56
Total sum of squares = 4 + 16 + …… 1 = 66 ÷ 1 = 66

The sums of squares of deviations can now be written down in one column of an analysis of variance table:—

Variance due to	**Degrees of freedom**	**Sum of squares**	**Mean square**
Blocks	3	4⅔	$^{15}/_{9}$
Treatments	2	56	28
Error	6	5⅓	$^{8}/_{9}$
Total	11	66	

The error term is obtained by subtraction of blocks plus treatments from total. The degrees of freedom column is written down for blocks, treatments, and total and the error figure again obtained by subtraction. The mean square column is obtained by dividing the sum of squares by its degrees of freedom.

It can be seen that we have now analysed the variation into its three components, and now we are able to say that the treatments effect is or is not statistically significant according to the ratio of treatment mean squar $\frac{\text{treatment mean square}}{\text{error mean square}}$, in this case 28 ÷ 8/9 = 32 (approx.). We error mean square now proceed as follows: We suppose that the treatments have had no effect and we then consider the probability of getting a ratio of $\frac{\text{treatment}}{\text{error}}$ treatment as big as this. When we find that the probability is less error than 1 in 20, we say that the effect of treatments is significant; when we find this probability is less than 1 in 100 we say that the effect of treatments is highly significant, or significant at the 1 per cent level). In the present case this ratio 28 ÷ 8/9 = 32 (approx.).

It is now necessary to look up tables of this ratio (which is known as "F"). These tables show values of F according to the number of degrees of freedom for treatments and error for each level of significance. F for 2 and 6 degrees of freedom is 10.9 at the 1 per cent level of significance. As our value of 32 for F exceeds this figure, the treatment differences are significant at the 1 per cent level.

6. DECIDING THE SIGNIFICANCE OF TREATMENT DIFFERENCES

We now need to decide **which** of our treatment differences are significant. The F test merely tells us that significant differences do exist among the treatments. Two of the methods in common use are as follows:

(a) "Least Significant Difference" (L.S.D.) Test

On page 17 it was explained how means could differ in reliability. This difference in reliability of a single mean similarly affects **differences between means**. In the examples 1 and 2 given on page 18 the differences between the means of A and B is 5 in both cases, but the difference of 5 in Example 2 is much more "reliable" than the difference of 5 in Example 1.

The **least significant difference** (L.S.D.) is the smallest difference between the means of treatments which is statistically significant at the particular level of significance chosen.

In the analysis of variance of the simple type we have been considering, the S.E. of treatment means is the same for all treatments. The S.E. per plot is the square root of the error mean square and S.E. per plot. the S.E. of the mean is $\frac{\text{S.E. per plot}}{\sqrt{P}}$ where P = the number of plots ? P of each treatment (see page 19).

In our present example (see pages 20 and 21), therefore, the

S.E. 0.9 0.9 per plot is $\sqrt{8/9} =$ (approx.) and the S.E. mean $= \frac{0.9}{\sqrt{4}} = \frac{0.9}{2} = 0.45.$

It can be shown that the least significant difference at the 5 per cent level is approximately 3 times the standard error of the mean or, in our example, (3 × 0.45) = 1.35 (1.4 rounded off). At the 1 per cent level the multiplier is approximately 4.

Thus we have the following result:

	Mean yield
Treatment A	7
Treatment B	11
Treatment C	12
L.S.D. (5 per cent)	1.4
L.S.D. (1 per cent)	1.8

Treatment A differs from B and C at the 1 per cent level of significance, but B and C do not differ significantly.

(Note: The figures of 3 or 4 times the S.E. mean are an approximation. The actual multipliers vary according to the number of degrees of freedom for error and are found from tables known as t-tables.)

(b) Duncan’s Multiple Range Test

It has been recognised for many years that there are certain inconsistencies in the working of the Least Significant Difference (L.S.D.) Test as described above, particularly where a large number of treatments are being compared. For this reason Duncan’s Multiple Range Test has come into more general use in recent years.

Duncan’s test operates as follows:

(a) The treatment means are arranged in order from the highest to the lowest.

(b) In this order the method tests the difference between any two of the means and **makes allowance for the number of other treatments lying between the two being tested.** Such allowance is not made in the L.S.D. test. This is done by multiplying the S.E. per treatment mean by the appropriate figure from a set of tables produced by Duncan. (Duncan: Biometrics (1955), vol. 11, No. 1, p. 1.)

(c) Each treatment mean is allotted one or more letters (a, b, c, etc.). **Groups of treatments which have a letter in common do not differ significantly, whereas any two treatments which do not have a letter in common do differ significantly.** Small letters denote significance levels of 5 per cent, and capital letters significance levels of 1 per cent. Thus, in the example given on page 23 the results would be written as follows:

	Mean yield	**5 per cent**	**1 per cent**
Treatment A	7	b	B
Treatment B	11	a	A
Treatment C	12	a	A

An example with a greater number of treatments is given by the results of a wheat variety trial.

	Treatments (in order of yield in bushels per acre)						
	1.	2.	3.	4.	5.	6.	7.
Yields	64.2	57.8	55.8	54.2	53.4	52.1	51.1
5 per cent level	a	b	bc	bcd	bcd	cd	d
1 per cent level	A	B	BC	BC	BC	BC	C

At the 5 per cent level treatment 1 differs significantly from the rest; treatments 2, 3, 4, and 5 each have b in common and do not differ significantly; however, 2 differs significantly from 6 and 7, and 3 differs significantly from 7.

At the 1 per cent level treatment 1 differs significantly from the rest, and treatment 2 from 7, but there are no other significant differences.

With Duncan's test there is a different "significant difference" for each comparison rather than a single one as with the L.S.D. test. It is therefore not possible to quote a single "significant difference" figure.

Duncan's test attempts to overcome difficulties associated with multiple comparisons. Where there are only two treatments the

L.S.D. test is the most efficient, and Duncan's test in this case is the same as the L.S.D. test.

7. OTHER METHODS OF ANALYSIS

Some results of experiments are not suitable for treatment by analysis of variance methods such as those described above, because the data do not conform to the assumptions on which these methods depend. Other methods of analysis must be used. Two examples are as follows:

1. Where the results are **rankings,** such as in tasting experiments, ranking analysis methods must be used (see appendix, pages 148 and 149).
2. Where the results are **counts** of insects, diseased fruits, weeds, and so forth it may be necessary to transform the data (for example, into logarithms) before analysis of variance is possible or an entirely different method of analysis may be required.

In particular, where each individual result is in one of two categories such as dead or alive, diseased or healthy, another test known as the chi-square test may be more appropriate.

It is not intended to discuss such methods in detail, but it is necessary to appreciate that the common methods of analysis as described in this bulletin are not appropriate to all types of data.

Part 4 — Designs of Experiments

The purpose of this discussion is not to teach you how to design experiments: you should consult a biometrician when you want an experiment designed. Some idea of the types of design available will, however, give you an idea of what can and what cannot be done in experimental design, so that you can more readily discuss your proposals with the biometrician.

Books have been written on this subject and it is impossible in these notes to do more than briefly describe some common designs and the reasons for them.

A good design must satisfy two requirements:

1. Enable valid comparisons to be made between treatments.
2. Obtain the greatest amount of information from the smallest expenditure of labour, time, and space in the field.

Many simple designs will satisfy the first requirement and complex designs should only be considered if they increase efficiency (point 2 above). Practical considerations must always be taken into account: in remote areas it may be quite out of the question to attempt complex experiments. There is also a greater risk with these trials—they “go wrong” more easily and each trial costs much time and labour. Where only non-technical labour is available and where there is no assurance against outside interference in the trial, it may be better to spread the risk and put down several simple trials rather than one or two complex trials. In such cases, however, there is probably a loss of efficiency and a loss of information.

As statistical analysis rests on certain theories which presuppose certain conditions, experimental designs from which data are intended to be analysed statistically must satisfy these conditions. The most important of these conditions is that there must be random layouts of plots so that chance enters into the siting of plots of each treatment within the trial.

Nevertheless a certain restriction of this random principle is adopted in experimental designs to give greater efficiency in the comparisons between treatments. The basic idea is the grouping of one plot of each treatment in blocks. With this arrangement it is possible to eliminate differences between blocks and thereby reduce the size of the standard error and increase the precision of comparisons of treatments.

Some of the commonly used designs are as follows:

(A) RANDOMISED-BLOCK DESIGNS

Randomised-block designs are fundamental to all designs and are the most widely used. Where the treatment numbers are not large (say 9 or less) randomised blocks are usually as efficient as any other design. With large numbers of treatments, however, the block size gets too big and as there is no way of removing the effect of soil variation **within** the block, comparisons among treatments cannot usually be made with the necessary degree of accuracy. In such cases various “incomplete block” experiments may be better.

This also explains why plots can be too big. There is a most efficient size of plot which is big enough not be be unduly influenced by the “patchy” type of soil variation, but not so big that the block size is made too big. With big blocks major soil variations are likely to occur within the block and cannot be eliminated. The question of the ideal plot and block size is usually one that requires preliminary technique and uniformity investigations for each crop in question, but has still to be assessed by the experimenter in relation to his knowledge of the type and size of the soil variation of the trial area.

In a randomised-block design each block contains one plot of each treatment. The position of each treatment within the block is determined **at random.** Thus with four replications of four treatments we may have the following:

Block	1	2	3	4
Treatments	BACD	CBDA	ACDB	CABD

These plots may be laid down in a continuous row. This is not essential, however. As long as the blocks are not split the trial may be laid down to suit practical convenience. Plots within blocks, on the other hand, should be as close to one another as possible. Blocks should be compact in shape. Long, narrow plots are usually best for this purpose.

Blocks should be treated as **units** in respect of sowing, after-treatment of plots, and harvesting or other measuring of yield, unless practical considerations require otherwise. Thus varieties maturing at different times might require harvesting at different times. This should, however, be avoided where possible in the planning of experiments.

How the Design Works

A very simple example of a randomised block was used in Section 5 of Part 3—"Some Statistical Considerations" (page 21). There we had 4 blocks of 3 treatments,

	BAC	**ACB**	**CBA**	**BCA**
Plot yield	11 8 14	6 12 10	11 12 7	11 11 7
Block yield	33	28	30	29

and we saw the way in which the "between blocks" effects can be separated from the other sources cf variation. In many cases these block differences may be quite large, and they would reduce the efficiency of the treatment comparisons if they were not separated out in this way.

(B) LATIN SQUARES

Latin squares are arranged so that one plot of each treatment falls once in each row and once in each column of a square as in this example:

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

Soil fertility variations both "between rows" and "between columns" can be eliminated and this means a very efficient design. With small squares more replications may be required. These can be provided by having more than one square or by adding extra rows or columns to the existing square. This latter method is not, however, as efficient as having a series of complete squares and the analysis is rather more complex.

The main drawbacks to the Latin square are:

1. The practical difficulty that sometimes exists in laying out the trial in the field and in harvesting, especially with machines.

2. The fact that the number of replicates must be a multiple of the number of treatments. This makes the design of limited use when the number of treatments is large, as a trial with eight treatments needs eight replications (64 plots), a trial with nine treatments needs nine replications (81 plots), and so on.

The second difficulty can be overcome by the use of one of the Incomplete Latin Squares or Youden Square designs, as they are called. These designs were devised specifically for glasshouse experiments. They are more efficient than randomised blocks, but less so than complete Latin Squares.

(C) GRAECO-LATIN SQUARES

Graeco-Latin square designs are used where there are three sources of variation to be eliminated. They are particularly useful for experiments with horticultural crops involving treatments at the seedling stage and further treatments at or after planting out. The sources of variation would be "between rows," "between columns," and, for example, "between seedling treatments". The main treatments are assigned one to each row and column. The seedling treatments (indicated by figures in the example below) are then assigned one to each row and column and one to each main treatment. Designs are available for from 3 to 12 treatments, except 6 and 10.

A_1	B_3	C_2
B_2	C_1	A_3
C_3	A_2	B_1

(D) "SPLIT-PLOT" DESIGNS

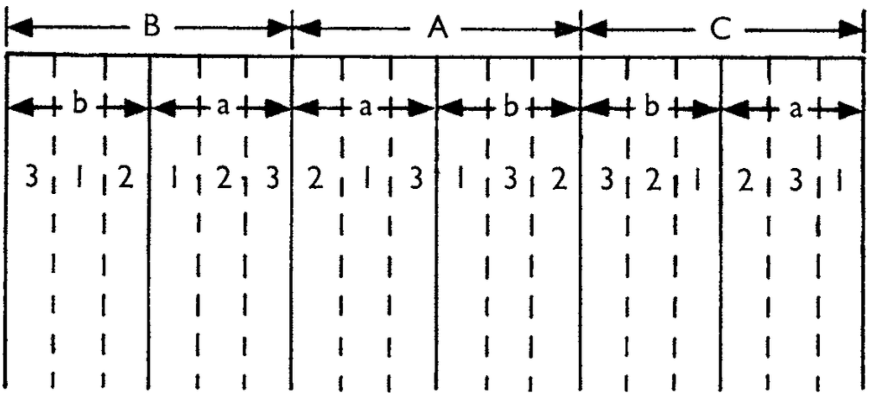
The principle of "split-plot" designs can be adapted to other layouts. They are particularly useful where practical considerations require that one type of treatment must have a large plot, such as irrigation by border-dike or cultivation with tractor equipment. They may also be used where the effect of one type of treatment is already well known. Thus the effect of lime may be well known, but we are interested in the effects of different rates of phosphate in the presence and in the absence of lime. A split-plot design might be very useful in such cases.

Splitting can proceed past the "sub-plot" stage, though rarely are more than "sub-sub-plots" worth while. The sub-plots are compared with greater precision than the main plot, partly because there are more of them, and partly because they are closer together.

Example

Main plots	A, B, C	(cultivation treatments)
Sub-plots	a, b	(varieties)
Sub-sub-plots	1, 2, 3	(fertilisers)

A layout of one replication is shown here.



The fertiliser treatments have the most plots and the lowest errors and the cultivation treatments the least number of plots and the highest errors. The interactions (see section (f) page 30) are efficiently measured in these trials. The (variety x fertiliser) interaction in the above example is tested against the fertiliser (lowest) error.

Note that each "splitting" is allocated at random.

(E) LATTICE DESIGNS

From this point we will consider the designs where large numbers of treatment comparisons make simple designs like randomised blocks less efficient. Lattice designs are used where the treatments do not have a "factorial" relationship (see section (f) page 30). They are most commonly used for preliminary trials on selections or crosses of varieties where large numbers are being compared. There are many types of lattice designs, such as complete lattice, cubic lattice, etc., and most are adapted to numbers of treatments that are perfect squares or cubes such as 16, 25, 27, and one or two other numbers.

The following example will give the idea. Consider nine treatments, a, b, c, d, e, f, g, h, i, and arrange them in a square, thus:

a	b	c
d	e	f
g	h	i

Now make blocks of three.

	(1)	(2)	(3)	
from rows	a b c	d e f	g h i	(1st replication)
from columns	a d g	b e h	c f i	(2nd replication)
from diagonals	a e i	b f g	d h c	(3rd replication)

giving nine blocks and three replications. (Note that the use of another set of diagonals, afh, bdi, ceg, will give a balanced lattice, but this only applies to a 3 × 3 square.

It will be seen that varieties a, b, and c occur together in one block in the first replication, but in separate blocks in the second and third replications. Now if the mean yield of a, b, and c in the second and third replications is compared with the mean yield of a, b, and c in the first replication, we get a measure of how much of the yield of block 1 is due to the varieties a, b, and c and how much is due to soil fertility at that place.

This is the way in which some of the variation due to soil fertility can be eliminated among the nine blocks as well as among the three replications. Random placing of plots within each block is still essential.

(F) MULTIPLE-FACTOR EXPERIMENTS

A mutiple-factor experiment combines in the one trial two or more **factors.** These might be, for example, different varieties and different fertilisers. A **factor** is more fundamental than a treatment. Thus the **factor** of P might include P at **three levels,** say 0, 1, and 2 cwt of double superphosphate per acre.

Factorial experiments may be of a wide variety of types with two or more “factors” at two or more “levels”. **All possible combinations** of these levels and factors are included. Thus with two levels (nil and some) of the factors L, P, and K, the treatments are O, L, P, LP, K, LK, PK, LPK (eight in all). The advantages of using such combinations are increased efficiency and the fact that they give additional information to that available from “single-factor” experiments. Information is obtained not only on the effects of each factor included but also on the effects of these factors in the presence or absence of the others.

Note that the term “factorial” refers to a relationship between the **treatments** and not to the layout of the plots.

Main Effects

These are the **average responses** to factors in a multiple-factor experiment. Thus in the LPK factorial at two levels the “main effect” of L is the average of the following:

L	minus	No manure
PL	minus	P
KL	minus	K
PKL	minus	PK

The increased replication given by these comparisons is the main reason for the increased efficiency of the multiple-factor experiments.

Interactions

"Interactions" may be considered as correcting terms adjusting the values of the "main effects". Thus if L gave a response only in the presence of P, the "main effect" of L (in which two of the four comparisons are made without P) is not a satisfactory measure of the L response. In this case the response to the **treatment** LP (over control) would be greater than the sum of the response to the L treatment plus that of the P treatment, and there would be a **positive** LP interaction. Negative interactions will also occur where (in the above example) the response to LP treatment is **less** than the sum of the responses to L and to P separately.

Many other situations will give rise to interactions and it is possible to determine whether these interactions are or are not "significant" at any desired level of significance. "High order" interactions such as "LPKN" are more difficult to interpret and are rarely of importance. In many cases they are allowed to enter into the "error" of the experiment or may be found with less precision by the use of experimental designs where "confounding" is used.

An LPK factorial trial has overwhelming advantages over the alternative of putting down three separate trials, one on lime, one on phosphate, and one on potash. If there is any interaction between fertiliser responses (for instance, if there is a P response in the presence of L but not in its absence), the three separate trials can tell you nothing about it; and where there is not an interaction the one trial gives exactly as much information about each fertiliser response as would three trials each the same size as this one.

If there is no interaction, the lime "main effect" is obtained by comparing all the plots which receive lime with all the plots which do not receive it. If there are four replications of the eight treatments (32 plots), we have **16 replicates** of the lime comparison.

"Confounding"

This has a meaning in experimental statistics quite apart from its more common meaning. Perhaps the term is not as inappropriate as it might seem to be!

Confounding is a device to reduce the block size. In a confounded experiment each block does not contain all the treatments in the trial. Each **replication** now consists of two or more blocks, but the treatments in these blocks are so arranged that it is possible to analyse the trial, eliminating block differences but also losing certain other information. This information is usually one or more of the "high order" interactions.

Suppose an LPK factorial trial was arranged so that all the P plots (P, PK, LP, LPK) fell into one block and all the plots without P (O, L, K, LK) in another. Now the "main effect" of P would be completely "confounded" with blocks, and in removing differences due to blocks we would also eliminate the effect of P. In practice, of course, this would not be done unless we were not interested in the P response. It is essential, in these trials, that the trial plan be strictly followed in the field.

Confounding aims at reducing block size with the minimum loss of desired information. Small blocks give a better elimination of soil fertility differences and therefore experiments of greater precision. With multiple-factor experiments there are usually large numbers of treatments and if a device such as this is not adopted, it may be difficult to get trials of the necessary accuracy

Single Replication and Fractional Replication Factorial Experiments

Another very useful aspect cf the factorial design is the use of single replication trials. A device similar to confounding is used in this case. High order interactions very rarely reach significance in experiments in which they are estimated, and it is reasonable in many cases that they should be disregarded. In a single replicate trial it is assumed that the higher order interactions are negligible, and their apparent size is therefore used to give an estimate of experimental error. The single replicate 25 design (32 plots) has proved very valuable in field experiments.

A similar principle allows the use of half or quarter replication. A half replicate of a 27 design can give a very efficient experiment in which seven factors are tested at 2 levels in 64 plots. All two-factor and most three-factor interactions can be tested in this design. A quarter replicate 28 (again 64 plots) may also be useful, but information on three-factor interactions is much more restricted.

Factorial designs in which factors have more than two levels present additional complications, particularly “mixed” factorials where different factors have a different number of levels. However, designs of these types which have proved useful include the 4 x 4 and 5 x 5, each with a minimum of two replications (32 and 50 plots respectively), the 4 x 2 x 2, 4 x 4 x 2, and 4 x 4 x 2 x 2. The 33 (three factors each of three levels) is a good design with two or more replicates, but would not normally be recommended with only one replicate. The use of a one-third replicate of a 35 (81 plots) has been recorded.

(G) RESPONSE SURFACE DESIGNS

Factorial designs become unwieldy when it is necessary to examine several factors, each at four or more levels. A different approach to this problem led G. E. P. Box to a set of designs he described as composite. In these designs we do not test the significance of the difference between two levels of a treatment factor, but instead look at the response curve that best fits the data.

The striking advantage of this type of design is the relatively small number of plots required: for instance, a four-factor trial with each factor at five levels needs only 30 plots per replicate.

This type of design was originally developed for industrial experimentation, but it has been used with some success in agriculture. In practice it has distinct limitations and is not the easy answer to all design problems. One such limitation is the absence of a “control” or “nil level of each factor” treatment; another lies in the fact that the second degree curve or surface that is fitted is a somewhat unconvincing approximation to agricultural data at least. These designs have their uses, but it is especially important to consult a biometrician before using them.

(H) DESIGNS FOR OBSERVATIONAL TRIALS

By “observational trials” are meant those experiments where, normally, measurement data are not secured. Such trials may, however, be "scored" by one system or another and Part 7 gives some of the various types of scoring systems that have been devised. Occasionally some type of count or measurement may be done on trials planned for observational data only. In weed control experiments counts of weed populations may be made; in some cases heights of pasture may be measured; in others some measures of the severity of attack by diseases or insect pests may be taken.

Some of these scoring systems and most of the other types of measurement that may be made on "observational" trials give data that can be statistically analysed. Scores, if done without knowledge of treatments applied to each plot and on trials of a random design, may be analysed to allow an estimate to be made of the accuracy or consistency of the scorer and of the value of his observations. It allows us to identify those cases where an observer has truly observed an effect of treatment. Where observations are not examined by statistical analysis we can never be quite sure that personal bias has not influenced the judgment of responses.

If, therefore, we wish to analyse observational trials statistically, we must use designs that allow such analysis. Any of the designs for measurement trials, as outlined in the preceding pages of Part 4—"Designs of Experiments", are quite suitable for observational trials from this point of view. In practice, however, it is unlikely that the more complex designs would be used for such experiments, and the great majority of trials will be laid down on randomised-block layouts.

Factorial series of treatments (multiple-factor experiments) are to be preferred where these are possible. The great advantages of having all possible treatment combinations in the one experiment still apply. Nevertheless, many trials are made observational mainly to allow of large numbers of them being laid down in "survey" types of investigation. Such experiments usually have few treatments and the minimum number of plots. Complete factorial series may, in some cases, be difficult to put down in the field.

In hill country and on difficult terrain it may be very difficult to secure a sufficiently large piece of ground uniform in soil type, aspect, and pasture composition to allow of an experiment with more than a few plots. Though small plots (and more of them) may help in some circumstances, it is not always efficient to make the plots very small in such conditions.

We have really two types of observational trials to consider. The first is where we hope to apply statistical analysis to suitable scoring systems and occasional measurement data. Here we will use one of the designs suitable for such treatment and most probably a randomised-block layout. In the second case we are limited by practical considerations to a few plots and must accept the fact at the outset that statistical analysis of the data is not going to be done. Cases where this would apply are as follows:

1. In survey types of investigation where very large numbers of simple trials are laid down to identify, for example, soil types likely to show responses to a certain trace element. When these soil types have been found, more complex experiments, either observational or measurement, but of a design suitable for statistical analysis, would be laid down.

2. Where it is confidently expected that the response to a treatment is going to be obvious and unmistakable. Such trials are more demonstrations than experiments.

3. Where conditions of the area on which the trial is to be laid down are such that only (say) less than five plots of an acceptable size can be laid down in a block. Note, however, that by laying down several “blocks” of treatments, not necessarily adjacent, a trial of a size suitable for statistical analysis may sometimes be laid down. About 24 plots is the minimum number for such purpose. In some cases, however, access to the trial site may be so difficult that it is unreasonable to lay down more than a few plots.

4. Where the type of treatment demands large plots and it is impracticable to replicate these adequately. This would apply to many grazing management experiments. It would also apply to certain insect and disease control trials where large plots and buffer areas are necessary to prevent contamination from untreated and ineffective treatments.

5. Where observers who are capable of doing suitable scoring systems are not available, it may be better to accept simple trials and simple methods of describing responses to treatments.

(I) DESIGNS FOR SIMPLE NON-RANDOM OBSERVATIONAL TRIALS

Where it is not intended to apply statistical analysis to the data the following general considerations apply:

1. Treatments must always be at least in duplicate. To see the same response in both series of a duplicate trial means a great deal more than seeing it in one replicate only.

2. The **size of plots** should be kept to a minimum consistent with practical requirements. Provided common-sense limits are used, it is better to have small trial areas. These limit soil variation and improve ease of observation.

3. The **shape of plots** should preferably be long and narrow. Much paddock variation is of the “spot” or patchy type, and plots that tend to be square are more likely to correspond with the patchy type of variation. However, this is a matter of opinion in many cases and when cross treatments are used plots tend to become square.

With weed trials the best size and shape of plots depend on the type of weed, size of weed, area of uniform infestation, and spray equipment. With small annual weeds plots can be very much smaller than with large weeds, especially if these are perennial or spreading in nature.

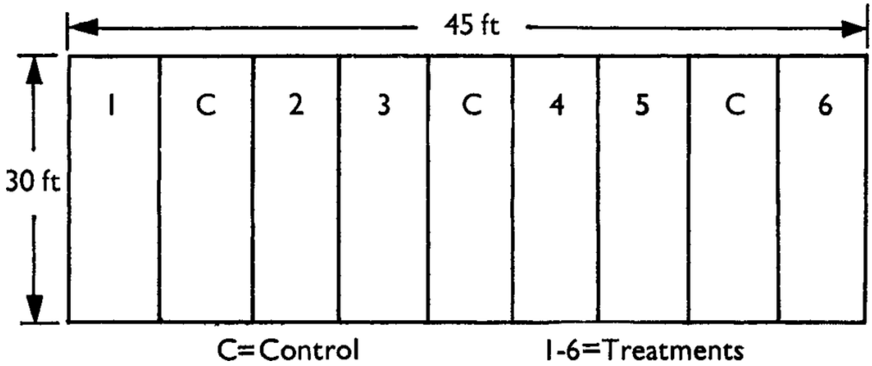
4. There must be an **adequate number** of **control plots.**

Too often a control is located in one corner only of a big trial. For preference all plots should be adjacent to (or not more than one remove from) a control. A control surround is, of course, essential.

5. Where **basal treatments** are applied (in fertiliser trials) the control surround and control plots must always be treated. Sometimes an additional “no basal” control might be considered as an additional treatment—for example, as a second “control surround”.

Now I will discuss some designs that might fit in with these requirements. **All** of these will have a control surround (not shown) and **all** must be in duplicate at least.

(1) A Simple String of Plots



Layout I.

Plots say 30 ft x 5 ft for fertiliser trials. If a cross dressing is applied, perhaps 40 ft x 5 ft might be suitable.

(2) The 3x3 Square

(Layouts 2, 3, 4, 4a, and 5 refer to fertiliser trials.)

	A	Nil	B
1	1A	1	1B
Nil	A	Control	B
2	2A	2	2B

Layout 2.

1 and 2 might be two rates of one treatment, and A and B two rates of another.

Square designs are however difficult to arrange with long, narrow plots. A square design is a good one for economy of control plots, however.

(3) The 2 x 2 Square

Nil	A
B	AB

← Layout 3.

This is a very simple and very useful design.

(4) Overlapping Blocks

The LPK design is a good example.

O	P	PK	K
L	PL	PKL	KL

Layout 4.

This gives a factorial series of treatments and good “block” comparisons (a block of 4 P and of 4 K plots and a strip of 4 L plots), which assist ease of observation. If no L effect operates, plots are long and narrow. The main drawback is the control plot in the corner; it could be placed more in the centre as in the design at the top of the next page, but this would break up the block effects.

P	O	PK	K
PL	L	PKL	KL

Layout 4a.

(5) Decreasing Treatment Combinations

With pilot trials where many treatments are examined complete factorial arrangements are impossible. One design used in such cases is the "decreasing series" of combinations like—

Plot	1.	A B C D E
	2.	A B C D
	3.	A B C
	4.	A B
	5.	A
	6.	B
	7.	C
	8.	D
	9.	E
	10.	Control

Where A is the element most likely to respond and E is that least likely.

This principle can be used on occasions. An Australian design is somewhat different, but may be of use in some cases.

Plot	1.	A	B	C	D	E
	2.	A	B	C	D	
	3.	A	B	C	D	E
	4.	A	B	C		E
	5.	A	B	C	D	E
	6.	A	B		D	E
	7.	A	B	C	D	E
	8.	A		C	D	E
	9.	A	B	C	D	E
	10.		B	C	D	E

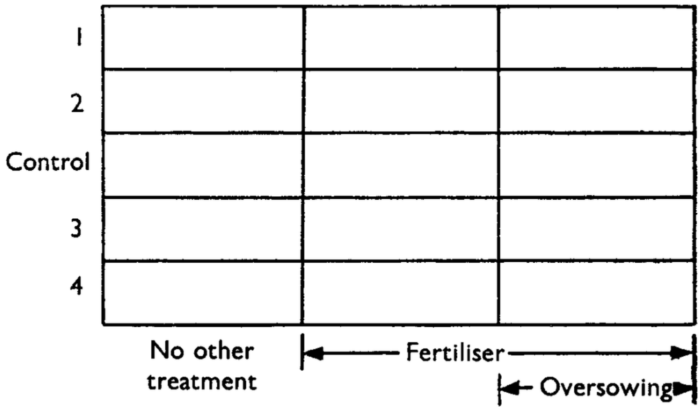
It is important that none of these treatments is likely to have a serious **depressing** effect or the whole trial may be affected.

Though it is a good design for observation, it has many weaknesses. The "control" plots are the "complete treatment" plots.

(6) Layout for Weed Control Trials

Plot sizes for small annual weeds can be kept small in many cases. In most cases where chemicals are but one part of the weed control programme other operations will decide the size and shape of plots In the first instance plots may need to be large to allow sub-division for other treatments. For example, weeds in pasture:

Weed control chemicals



This basic design may be adapted to a wide variety of types of trial.

Part 5 — Laying Down of Field Experiments

This subject may be considered in four main sections:

1. Selecting the sites for the trial.
2. Setting out the trial.
3. Taking uniformity data.
4. Applying the treatments.

1. SELECTING THE SITES FOR THE TRIALS

The first consideration in selecting trial sites is to make sure that they are each truly representative of the “region of application” you are testing. This means, for example, making certain that the soil type is the one you want, that the management of the trial area will be of the required type, and that the previous history of the area is satisfactory. Some general points to bear in mind are as follows:

(a) See that the areas are uniform in slope. If they are flat, avoid small ridges and hollows, that is, any conformation which will cause differences in moisture supply. On soils underlain by gravel moisture-holding capacity depends on the depth to the underlying gravel. Depth to the underlying gravel can vary considerably over a short distance. Sites with gravel subsoil are best selected during the drier part of the year, when differences in moisture levels show up most.

(b) Avoid concave surfaces and use convex surfaces. Concave surfaces are found at the bottoms of hills where slopes ease off. Such places frequently receive seepage and seepage usually means higher fertility. Similarly if the trial is to go on a narrow flat at the bottom of a hill, keep it as far away from the hill as possible.

(c) Keep the trials away from gates, watering troughs, and fences. The fertility is usually much higher near gates and watering troughs.

It can be higher near fences if stock wander along the fence. If the fence abounds on to a busy road, fertility is usually much lower near the fence, as stock, particularly sheep, dislike traffic and spend more time away from the fence than near it. This causes transfer of fertility away from the fence.

(d) Choose your farmers and fields carefully. With pasture trials you will usually want some guarantee that the fields are rotationally grazed, not topdressed by the farmer, and not spoilt in any way. An interested farmer is usually more cooperative.

(e) Select sites convenient for yourself and for visitors. Convenience to a roadway should be sought but not given too much consideration. If a crop trial has to be sown and harvested by machinery, for example, suitable access for such equipment must not be overlooked. With trials on farms the sites must be convenient for the farmer and must not be difficult for him to work round with implements.

(f) With crop trials make sure the plots will run at right angles to the direction of last ploughing so that “finishes” run across all plots and affect them equally. With grain trials avoid areas such as those near trees where bird damage is likely.

The soil over the trial areas finally selected should be checked at various points with a soil auger to see that it is reasonably uniform in respect of colour, depth to subsoil, and other profile characteristics and, as far as can be judged, in texture. If in doubt about texture, groups of, say, five soil samples should be taken from various points over the trial area, usually to depths of 0 to 6 in. Each of these groups should be kept separate and forwarded to the appropriate laboratory for mechanical analysis.

If everything about the sites is satisfactory to this stage, soil samples representative of the whole trial area should be taken for chemical analysis. About 15 cores should be bulked for the area sample. Depth of sampling on ploughed land is normally 0 to 6 in., and on grassland 0 to 3 in. Results from these tests should be available before proceeding further. This soil analysis, for instance, should be the means of avoiding the placement of trials comparing forms of phosphatic fertiliser on soils already well supplied with available phosphorus. In other cases the soil test will show the need for certain basic treatments to the trial area.

If all is in order, you should now be ready to peg out the trials.

2. SETTING OUT THE TRIAL

It is quite common for a trial to be carefully conducted in all respects save that of being clearly and adequately pegged out. In pasture trials substantial pegs well driven in are essential to avoid damage by stock. Painted pegs that stand out against the green background are a great advantage. Do not skimp on this apparently trivial but really quite important point.

The techniques of measuring out, getting right-angles, and so forth are best demonstrated in the field. Check and recheck all measurements to make sure that plots are of the right size and shape or serious errors in the application of treatments are likely. Special techniques have been devised for drilled trials with crops and these are described on pages 41 to 43.

Make a good plan of the trial and fix its position and orientation in the field with reference to and measurement from fixed objects. A surprising number of field experiments get "lost" because of inadequate plans and measurements. The plan must be sufficiently clear so that it can be used by another officer to locate the trial and identify each plot.

It should be possible to repeg trials with confidence if the pegs have been removed by the cooperating farmer for some purpose such as mowing. This means that at least one and preferably two corner pegs should each be "fixed" by two measurements to "fixed objects" such as marked posts in a fenceline. Clear, well drawn plans showing all such features are essential.

Questions of size and shape of plots to use have been considered in Part 4—"Designs of Experiments" (pages 25 to 36).

(a) Locating Permanent Trials

Where trials are to be carried on for several years special attention must be given to the above points. This particularly applies to trials which involve a rotation of crops and where each trial crop has to be freshly pegged out.

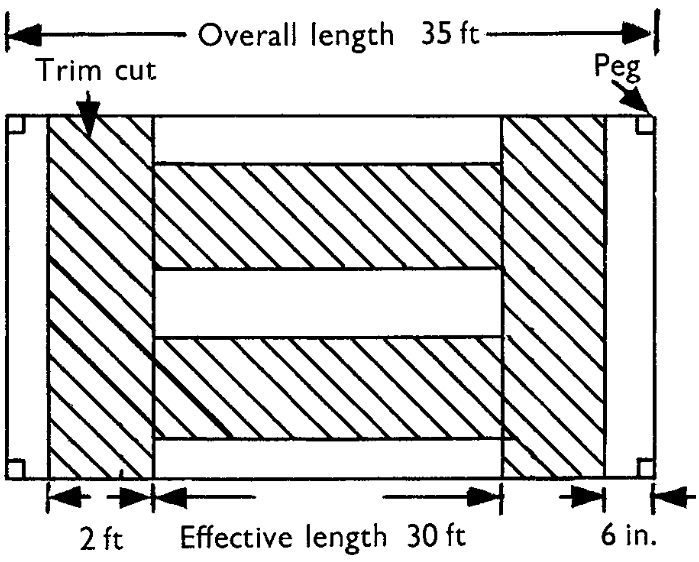
There are two ways of marking such plots. One is to sink substantial pegs (say 6 in. x 6 in. cross-section) of durable timber below plough depth at the corner pegs and to fix the position of these pegs in relation to fenceline markers. These pegs are probed for when each crop is sown. Another method is to have plots extending from one fenceline to another and to put all pegs along the fence where they are out of the way of cultivating implements.

(b) Marking out Pasture Trials (Especially Mowing Trials)

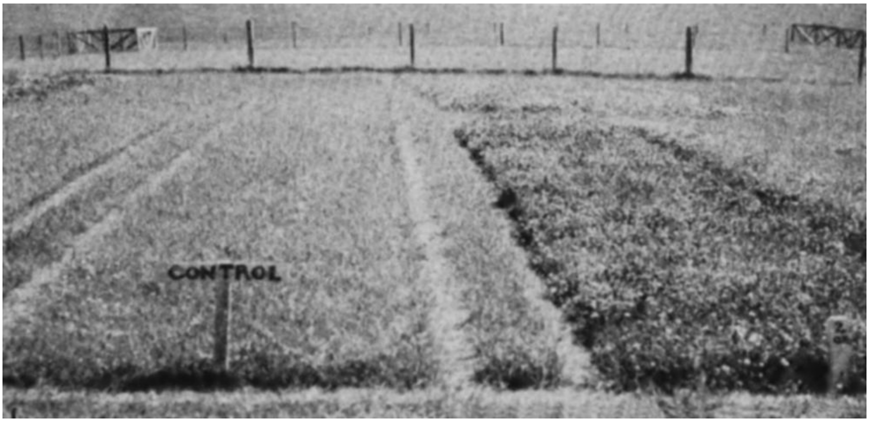
In most trials adjacent plots have common boundaries, but buffer areas should be provided in certain cases—such as molybdenum trials—where a treatment effect is inclined to "spread" outside a plot, or where one treatment may affect the neighbouring plots as in weed-spraying trials; 3 ft is a useful width for a buffer.

When laying down plots make necessary allowances for trimming ends of plots before cutting, as in the following diagram (for use with a 2 ft mower).

(i) Using Pegs



The shaded portion shows the cut track.

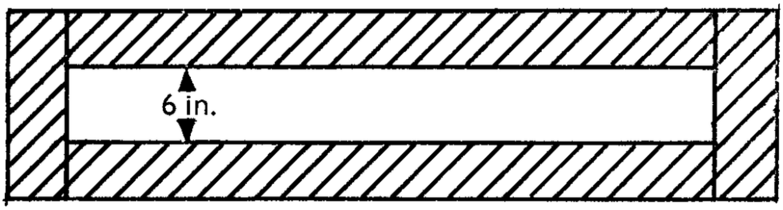


A minor element mowing trial with plots marked out by weedkilling chemicals.

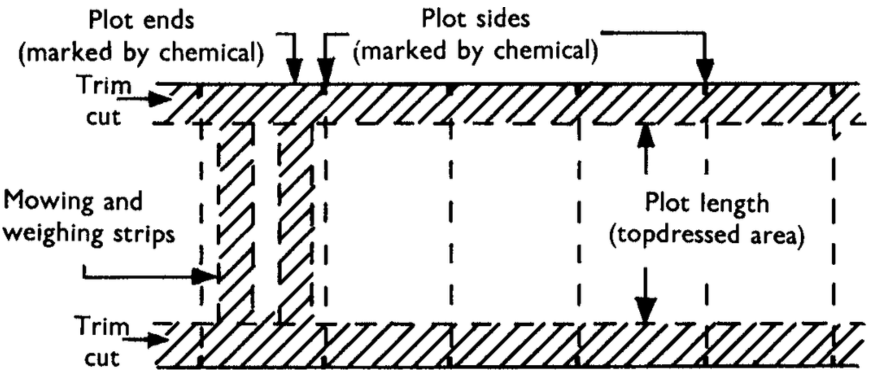
(ii) Using Grass-killing Chemicals to Mark out Plots

The following materials have been used: waste oil; creosote; arsenic; diesel fuel oil.

To get straight lines it is suggested that two boards be nailed together, say 6 in. apart (see diagram below) and used to leave exposed only that portion of grass requiring treatment.



It is recommended that when chemical markers are used plots be marked out at ends, with only 6 in. to 1 ft of sides marked at the ends of the plots, as shown in the following diagram.



A series of plots is marked out with chemicals as shown. The trim cuts are taken inside the end marks in the position shown.

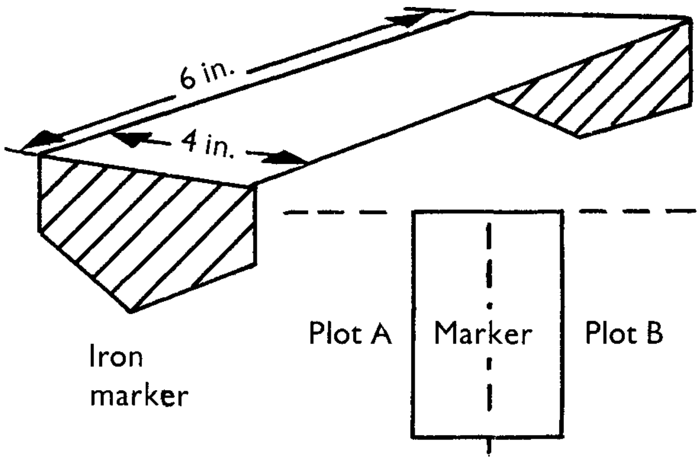
The use of chemical markers should speed up trial mowing; no pegs are required, apart possibly from a few reference pegs, which can be driven into ground level; such reference pegs will ensure accurate remarking of plots when the effects of the chemical fade.

(iii) Marking out when Plots are in One Line

To avoid shifting pegs another method in trials of this nature is to put large, permanent pegs in the fenceline, where they need not be removed for mowing.

(iv) Using Flat Galvanised Iron Markers

A suitably sized marker (say 6 in. by 4 in.) of galvanised iron (see upper diagram below) may be used to replace pegs at the corners of plots. Such markers may be run over with the mower. Lines may be painted on them to indicate boundaries. The markers are best placed inside plot areas as shown in the lower diagram below.



The shaded portions in the diagram are pushed into the soil to hold the marker in position.

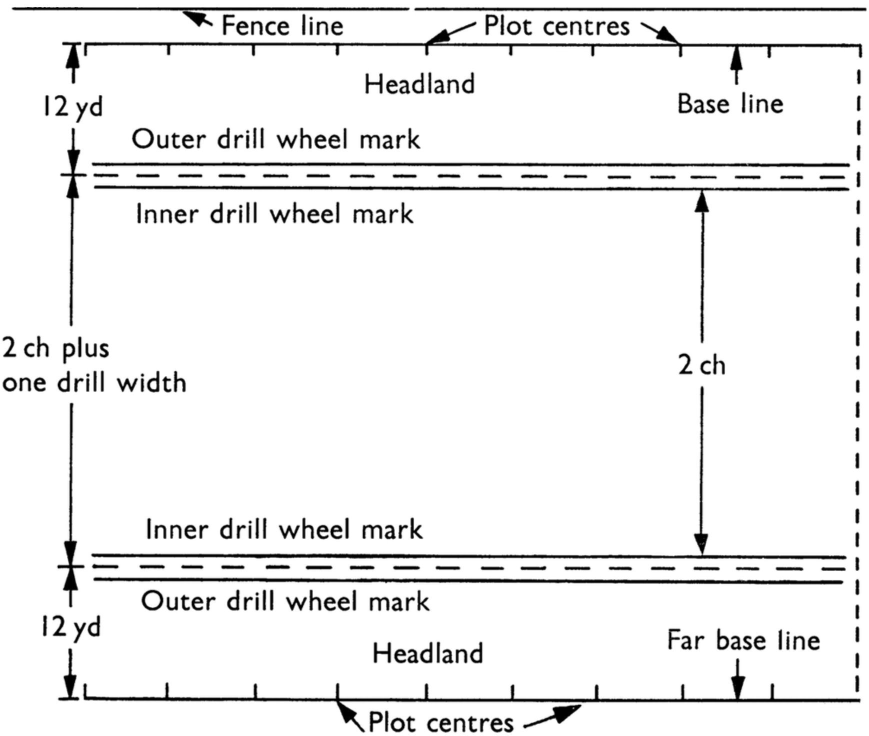
(c) Marking out Areas for Drilled Trials

Details of a suitable method are as follows:

(i) Fix on a base line parallel to a fence. For convenience this should be about a yard from such a fence (but see note on page 42). From one end of this line mark out **plot centres** at intervals depending on the size of the drill, using a measuring stick, and, following the random numbers in the field plan, place a small peg with the appropriate number or letter at these centres. Mark out also a buffer plot at either end of the trial.

(ii) Now erect a right-angle at each end of the base line, using for example the "30, 40, 50 ft" method.

(iii) Measure on the lines at right-angles to the two ends of the base line the following points. (See diagram and text on page 42.)



For any-sized drill and for plots 2 chains long measurements from the base line are:

(a) 12 yd to mark the centre point of the first "in and out".

(b) 2 chains 12 yd plus one drill width to mark the centre point of the second "in and out".

(c) 2 chains 24 yd plus one drill width to mark the far base line.

(See diagram above).

Put a sighting pole at each of these points.

NOTE: 12 yd is a convenient distance from a fence to avoid headland effect, but this may be any distance according to the necessity to select a uniform site. If, however, the trial is further into the paddock than 12 yd, the base line (which is 12 yd from the plots) must be measured in from and parallel to the fence as the first step.

(iv) Mark out the plot centres along the far base line at 4 ft 8 in. centres (for a 7-coulter drill). The correspondence of the end plot with the line erected at right-angles from the first base line will check the accuracy of marking out.

(v) **Check back from the field plan each number on the plot pegs**. This is most important.

(vi) Erect sighting poles for drilling the ins and outs on (a) and (b) and the corresponding points at the other end of the trial. Using a third "back-sighter" pole in each case, drive the drill along each of these lines.

The distance between the inner wheel marks will be 2 chains.

The first treatment may now be drilled.

To do this one pole is placed at the correct peg at either end of the trial. Now place the third pole at the end of the plot toward which the drill will travel (that is, alternately at one end or the other). Alternatively, where space will allow, the third pole may be placed well back from the trial; this gives a more easily followed line.

The effect of a mistake in drilling a cereal trial caused by the wrong sighting poles being followed. Someone must be available at each end of the trial to check that the correct plot is being drilled.

Explanatory Notes

Ins and outs: These are the names given to marks which are made by a drill which is run across the ends of the trial to show the place to let the drill in at the commencement of sowing of each plot and the place to take it out at the end of each plot. Because there is some time lag before the seed and fertiliser reach the ground after the gears are engaged on the drill, the drill should be let "in" at the near side of the "in and out" drill mark (see diagram and procedure below it on page 42, and illustration on page 49).

Sighting poles: Long poles (at least 6 ft) about 1 in. x 1 in. and painted white for easy visibility.

3. TAKING UNIFORMITY DATA

For many important trials, and for long-term experiments, uniformity data may be well worth securing. Of course these cannot always be obtained—you cannot secure uniformity data on production from a pasture species trial, though soil sampling for uniformity study is possible even in this type of trial. Where an experiment cannot be adequately replicated owing to practical considerations uniformity data may be almost essential. This applies with particular force in some experiments on soil conservation where a plot may be a "catchment" and where considerable variation among catchments is probable, as they are rarely uniform in slope, aspect, or size.

Measurements of productivity, water run-off, and soil loss before treatments are applied, provided such measurements are made for a sufficiently long period, are essential in these cases, particularly as duplication of treatments is frequently all the replication that may be practicable.

Uniformity measurements have certain limitations, particularly when they are made over a short period only. Thus in a pasture production measurement trial a three months' uniformity period may have reasonable application to the following year's results, but after, say, five years the general conditions of the area may have changed markedly. Proper statistical treatment of the data should safeguard the position, however. In well replicated trials good uniformity measurements may enable a much more precise measure of treatment differences to be made later.

(a) Uniformity Soil Samples

These are taken on a **per plot** basis before the application of treatments and to the depths at which samples will be taken later (usually 0 to 3 in.). About five cores per plot are sufficient. In many trials, therefore, these are the second series of soil samples, the first being preliminary samples taken to assist in securing good sites for the trial (see page 38).

(b) Uniformity of Pasture Composition or Plant Cover

In many pasture experiments it may be very useful to have data on the uniformity of the sward over the trial area. The degree of uniformity may be assessed by “point quadrat” methods, by dissection of mown herbage into species, or by other recognised methods of estimating botanical composition. The method decided on should preferably be one that will be used later during the trial period proper.

In weed control trials counts of weeds may be necessary to assess the effectiveness of chemical weedkillers or other treatments applied later. The infestation of such plants is usually very variable.

(c) Uniformity Data on Pasture Production

Generally a trial site is decided on as being as uniform as possible by judgment and observation. A further test can be applied, however, by carrying out “uniformity cuts”.

Uniformity cuts consist of actual plot weights obtained for a period before trial treatments are applied. If cuts are made over a sufficiently long period, the results can be subjected to statistical analysis, just as with actual treatments, and the trial can thus be examined for uniformity. Reasonable uniformity may be shown or, on the other hand, significant differences might be established (showing that a bias existed before the trial was commenced).

In the taking of uniformity cuts or any other uniformity measurements each plot is identified, and when several cuts have been made the layout proposed for the trial is theoretically superimposed. The statistical analysis of the plot weights might show that the plots to which treatment 1 will be applied later, for instance, is significantly higher or lower yielding than the others.

If this type of bias has been shown to exist, there are several courses open: (1) If it is extreme, obviously the site is unsuitable, and the trial on that particular site should not proceed. Another site should be examined. (2) It might be possible that a different randomised layout would rectify matters and the layout should be reorganised and re-examined. (3) The trial might proceed, but reference would be made to the results of the uniformity cuts. The results for trial treatments, when available for a period (say for a season of three months), would be compared with the uniformity results, and if a significant relation could be established, the treatment results can be adjusted on the basis of the uniformity cuts. If there is no significant relation, no adjustment can be made.

It is quite possible that production data for the first period after topdressing will have to be adjusted, but that for succeeding periods this need not be done, because no significant relation with the uniformity cut was established. This is reasonable, because by then the treatment effects might be exerting a large influence—large in relation to the variation shown in the uniformity cuts.

In trials which are not extensively replicated, because of the technique or because of the large number of treatments or any other reason, uniformity cuts are more important than in well replicated trials. In "frame" technique trials involving large paddocks and in complicated trials reliable data on uniformity obtained before treatment might be of great value.

The number of cuts which should be taken to constitute a series which can be confidently analysed for uniformity depends on the layout and technique employed in the trial. In frame or cage trials the area enclosed by the frame constitutes only a very small sample of the whole paddock; in replicated strip trials most of the trial plot is cut. Hence, with the frame technique a greater number of cuts needs to be taken to be sufficient for examination of uniformity than with replicated strip trials. About six or eight cuts in the frame technique would probably be considered necessary for a "uniformity cuts" period.

To summarise, for examination of uniformity a number of cuts should be made. These would be of value (a) in determining whether the trial site is satisfactory; (b) for making adjustments, either by reorganising the layout if it can be made satisfactory, or by adjustments in treatment yields when significant relations are established.

Special Points

(a) Whenever the application of trial treatments is delayed for some reason the opportunity to take uniformity cuts should not be missed. The effect of taking these cuts often reduces variation due to uneven pasture establishment or other causes and increases the precision of the experiment quite apart from the value of the uniformity data.

(b) Long-term investigations (say more than three years), especially if under frame technique, should as a general rule have a period of about three months under uniformity cuts. In some cases, such as pasture species trials, such data cannot, of course, be secured.

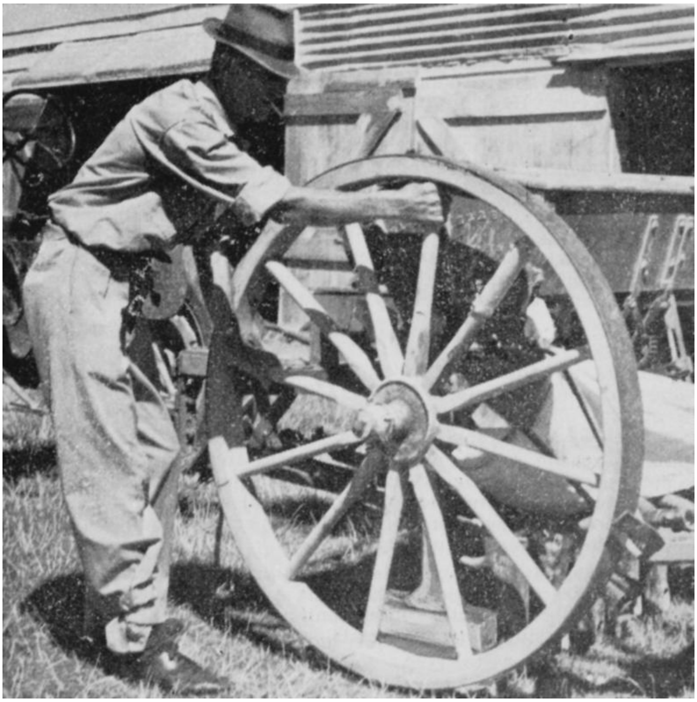
(c) Experiments not adequately replicated should be considered as requiring uniformity measurements unless these trials are “pilot” experiments.

(d) When the uniformity period is complete, an urgent analysis of the data should be completed before the application of trial treatments.

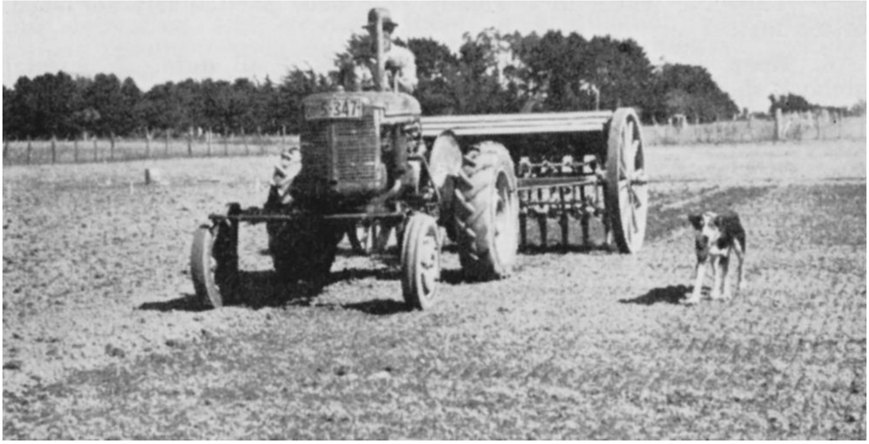
4. APPLYING TREATMENTS TO THE TRIAL

We have now reached a critical stage in the experimental programme. Mistakes due to application of treatments to wrong plots, carelessness in applying treatments, uneven application of treatments, and other types of misjudgment, such as sowing when soil conditions are unsuitable, can wreck all the preparatory work. At best it may mean some last-minute adjustment to the trial layout or some “missing plots” in the design.

If mistakes do occur they **must** be recorded with complete honesty and in detail in order that allowances for them may be made later. But all possible care must be taken to avoid mistakes, and some ideas are given in this section that may help you in so doing. Carelessness and shoddy work such as uneven application of treatments is nowhere more glaringly exposed later than in field experimental work. Conversely, there are few things that show better the hand of the careful experimenter than a well sown, neatly pegged, and well looked after field trial.



Drilling technique—Conducting standing drill "try-outs" with one wheel of the drill jacked up to obtain preliminary estimates of the correct cogs and settings.



Drilling technique—Doing a 10-chain "field try-out" at the side of a trial.

(a) Crop Experiments Sown with Drills

In Section 2 (pages 38 to 43) we dealt with setting out the trial area for sowing. The following notes give details of sowing following this operation.

(i) Preliminary Procedure

1. See that the drill is in good working order. Drills for sowing experiments should have had the delivery of seed and fertiliser tested for individual coulters.
2. See that adequate fertiliser and seed are available to sow the whole of the trial.
3. Mix, if necessary screen, and rebag fertiliser and lime so that it will run freely and evenly in the drill. Granular materials should be used whenever they are available and suitable.
4. Conduct "drill try-outs" with the appropriate wheel jacked up to obtain preliminary estimates of the cogs and settings for the various rates of the trial sowings. To save time in the field these are best done in a shed beforehand.

(ii) Data for Drill Try-outs

Useful tables are given in the appendix on pages 140 and 141.

(iii) Precautions when Doing Drill Try-outs

1. Weather: For fertiliser try-outs particularly do not choose a humid day (if possible), as conditions are likely to be different when the trial is actually sown.

2. Material: See that this is prepared properly (for example, no lumps in fertiliser) for drilling.

3. Make sure every coulter is running freely before beginning to measure delivery. This is particularly important with seed delivery in spoon-fed drills.

4. Maintain an even height of seed and fertiliser in the boxes.

5. Turn the wheel at a steady rate and, particularly for seed, not too fast.

6. **Keep a careful and legible record** of all data. A special notebook should be kept for recording all such tests, so that reference can be made. It will be found that a lot of time can be saved in future experiments.

(iv) Apparatus Required for Drilling

Drill, complete with all cogs and tools.

Adequate seed and fertiliser.

Drill notebook (containing try-out data, etc.).

Notebook and pencil for entering laying-down report data and plan.

Sighting poles: 35 to 40. Small pegs: sets of 8 to 10 of each numeral 1 to 9 for marking out plots.

Large pegs (4) for marking corners of trial. Tape (measuring). Wooden mall.

Measuring stick 9 ft 4 in. long with centre mark at 4 ft 8 in. (for 7-coulter drill).

Binder twine.

Spring balance (in good order).

Two or three buckets or kerosene tins (with handles).

Hand shovel for fertiliser box.

Canvas catcher for drill try-outs.

Harrows for use after drilling.

Field plan of trial showing layout of plots (make sure this is accurate).

Check this list before leaving headquarters.

(v) Field Procedure

The field try-outs of the drill and marking out of the plots can proceed simultaneously if sufficient men are available: two are required for each job.

(a) **Field try-outs: These must always be made before drilling the trial**. They consist of running the drill for 10 chains with the coulters down, tubes off, canvas catcher in place, and appropriate cog wheel removed.

The appropriate precautions listed under (iii) above for standing try-outs apply, and the data listed in the appendix on pages 140 and 141 may also be used here.

(Note: **All** seed and fertiliser must be carefully "weighed in" the drill, and any delivered into the canvas catcher during try-outs must be returned to the drill after weighing. A **most careful check** must be maintained so that the amount of seed and fertiliser in the drill is known before drilling the treatments begins. With fertiliser first put a small quantity in the drill, turn the wheel so that all coulters are running, level off and remove the surplus, and then weigh in the fertiliser.)

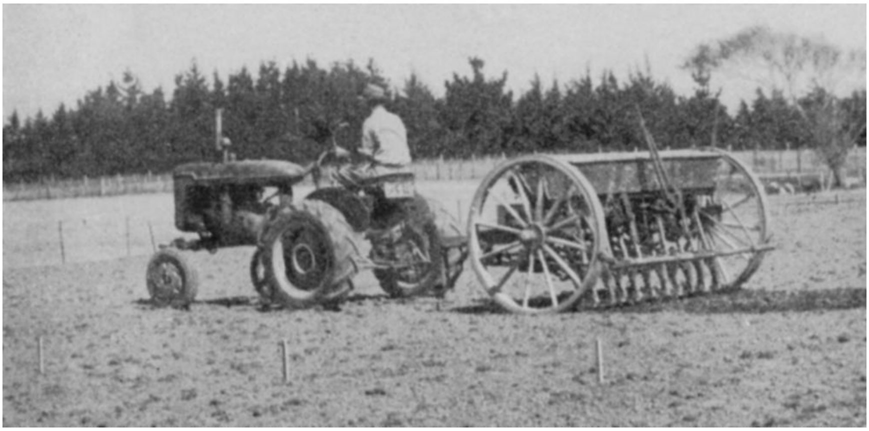
When doing the 10-chain field try-out mark out a 5-chain length along the headlands so that the drill returns to the starting point where weighing and filling are done.

Always keep full notes in the drill notebook of cogs, settings, and deliveries, and examine these in conjunction with standing try-out results. Notes on the weather and condition of fertiliser, size of seed, etc., are also helpful.

Field try-outs should be continued until results within 0.3 lb of the required quantity are obtained (that is, 5 lb per acre for a 7-coulter drill) for rates of about 100 lb per acre.

(b) **Drilling procedure—**

(i) Mark out the "ins and outs". This may be done with the drill sowing or not: if the former, the distance must be accurately known, as it is usual to carry on drilling the first treatment of the trial.



Drilling technique—Marking out the "ins and outs" with the drill to mark the ends of the plots.

(ii) The **inside** wheel marks of the drilled ins and outs marking the **ends of the plots** should be the point from which the plot length is measured. To do this the centre line marked with sighting poles for drilling ins and outs should be 2 ft 6 in. (3 ft 6 in. for a 9-coulter drill) on the outside of the line marking the ends of the plots (see before under Section 2 (c), pages 41 to 43).

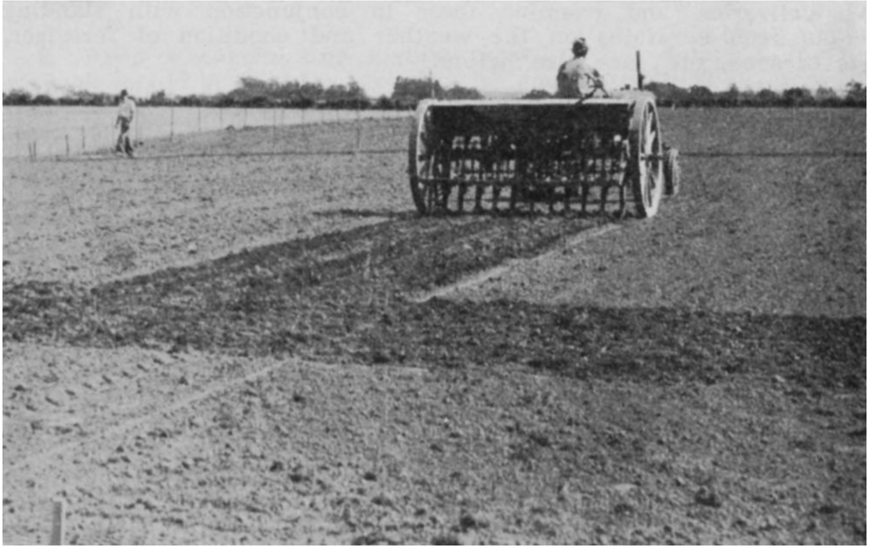
(iii) At the commencement of sowing the coulters are dropped (not violently) at the **outside** wheel mark to ensure that seed and fertiliser will be sown at the inside wheel mark (from which point the plot actually begins), while at the other end of the trial the coulters should be lifted **promptly** on the **inside** wheel mark.

(iv) **Precautions in drilling—**

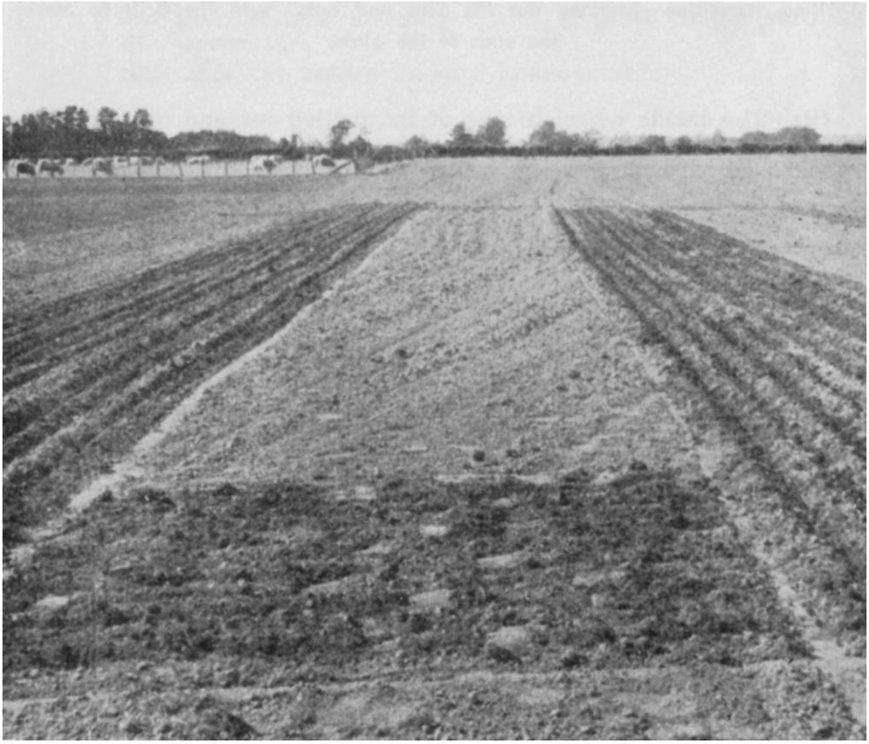
(a) Check with the person in charge of the field plan that the drill is on the right plot. The pegs in the base lines which identify the treatments of each plot should have been checked and rechecked against the field plan.

(b) Drill accurately: see that you are sighting on the correct poles.

(c) Make certain at all times that seed and fertiliser are running freely through every coulter. This should be done after every plot is drilled, the guide being the depressions made by the grain and fertiliser running out. When this has been checked level out before beginning the next plot.



Drilling technique—Drilling the first plot. The "in and out" marks can be seen at both ends of the trial.



Drilling technique-—A “fill-in” plot awaiting drilling. No sighting poles are needed for this.

Drilling technique—A trial completely drilled and awaiting final pegging.

(d) Keep up the level of seed and fertiliser in the boxes within reasonable limits.

(e) Never forget to weigh in all material put into the drill, and weigh out after sowing each treatment.

(f) Check rates per acre sown before proceeding to the next treatment. Check cogs and settings.

(g) Keep full notes of everything you do.

(v) **Drilling subsequent treatments:** All sighting poles need not be erected for every treatment sown, because drill-wheel marks will be guides as drilling proceeds, but a sighting pole must always be placed on the correct peg number at either end of the plots and the drill guided into place on the correct plot before drilling of a plot begins.

Drill buffer plots and ins and outs at the conclusion of the trial.

(vi) **Mistakes:** When a mistake is made the fact must be carefully and fully noted.

If possible, sow another complete replication at one end of the trial to replace that in which the mistake is made. If this is not possible, proceed as if no mistake had been made and drill the remainder of the plots.

Do not sow an odd plot of a treatment at one end of the trial to replace one which is useless (unless the adjoining replication is the one affected). This plot cannot be used in the statistical analysis of the results.

(vii) **Final details—**

Before leaving the field make sure you have:

(a) Put in corner pegs (painted) in the centre of the buffer plots at the four corners of the trial.

(b) Harrowed (where desirable) the trial, running the harrows **along** the plots.

(c) Obtained all the information required in respect of previous history, condition of the soil, and all other relevant details, and taken all necessary measurements to fix the trial location.

(d) **Numbering plots**: Always call Plot 1 the left-hand' plot, standing on the base line nearest the fence and facing the trial. If this is always done, inspection of the trial will be greatly facilitated. Make sure that the trial plan shows these plot numbers as well as the treatment numbers.

(b) Observational Topdressing Trials

It is essential to have all materials spread as uniformly as possible. Do not put an especially heavy dressing along the boundary. Such a practice may ruin a plot for observation. A good method is to spread one half of the dressing in one direction and the other half at right angles. **All** materials (fertilisers, seeds, etc.) should be applied on a "per plot" basis rather than in blocks or strips of plots. Each plot must receive its correct treatment whether this be a basal dressing or a trial treatment.

Avoid drift of materials. Even with a slight breeze the drift of finely ground material can be so large as to spoil the experiment. This was shown in a trial at the Marton Experimental Area. The average loss by drift of basic slag on a fairly calm day amounted to 30 per cent. It was applied with maximum care, yet much of it never reached the plot. Moistening of fertilisers is the best way to avoid drift. Basic slag and other water-insoluble materials can be easily moistened to the right consistency. Even double superphosphate or superphosphate should be moistened on a windy day. Very little experience is needed to get the right degree of moistening.

Drift can be even more serious with trace elements, as minute amounts may stimulate growth. If at all possible, trace elements should not be applied by fine spray but with a watering can. They may also be applied dry if mixed with a suitable inert spreader.

(c) Applying Treatments to Measurement Trials on Pastures

The comments made in (b) above apply here with equal force. Some additional points may be considered, however.

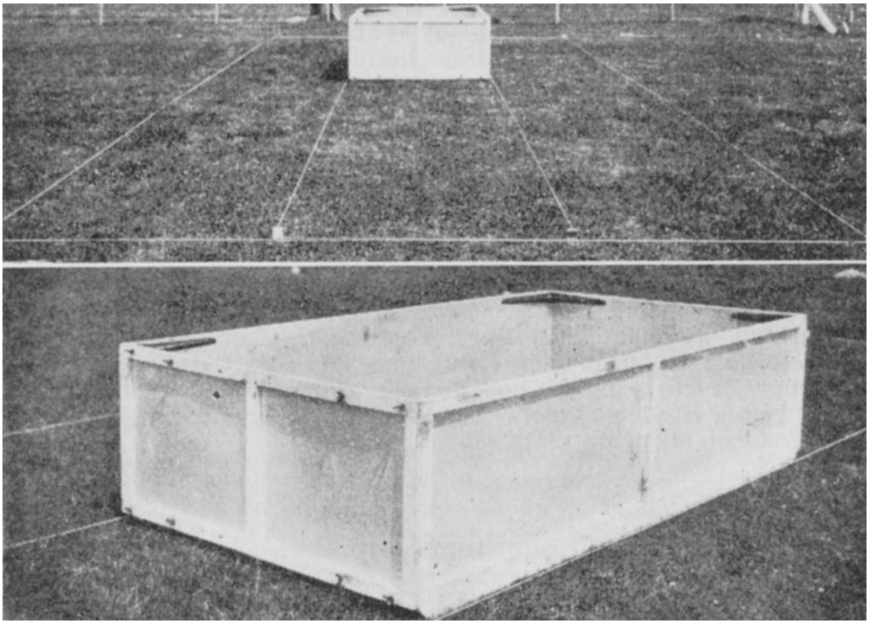
(i) Where the amount to be applied is rather small increase the bulk by using an inert material such as sand for a filler or carrier.

Where a basal fertiliser dressing is being given to the whole trial some of the basal dressing material can be used for a filler, provided the amount applied is taken into account. In some cases seed and fertiliser can be mixed and sown together (provided they are not left mixed for any lengthy period before sowing, as this may impair germination).

(ii) It is a good idea to divide the amount to be sown, and sow or topdress in two directions, as distinct operations.

(iii) Topdressers have been developed for the topdressing of trial plots. Most designs work on a moving-belt type of delivery and may be adjusted to deliver fertiliser evenly for a certain distance. Quantities and distances are adjustable. These machines can be of considerable assistance in improving evenness of distribution.

(iv) **General:** Evenness of application of fertilisers or seeds is most important in mowing trials and **all care must be taken** to see that materials are applied evenly to the correct plots. If application must be done in windy weather, some form of windbreak around plots to be treated is highly desirable. Mistakes and poor work at this point can ruin a trial for the taking of mowing weights on which much labour and expense have been required. Extra time and care taken for this work should never be considered unnecessary.



Applying fertilisers to pasture mowing trials. Plots are lined out and a canvas windshield is in place for use in windy weather. This shield covers one-quarter of the plot area.

(d) Applying Sprays to Plots

With trials involving spray applications of treatments such as weedkillers and insecticides accurate and even distribution is essential and spray drift must be minimised. In many cases the range of rates of materials that crops will tolerate is small. As a result overlapping applications and strips applied at double rates may cause serious damage to crop or pasture, while areas missed or with too light applications may likewise ruin small plots.

The first essential is good equipment. Spray nozzles should be tested for even spray distribution, correct pressures of operation should be maintained, and correct height of spraying kept constant to obtain even spray coverage. Hand-applied treatments should be made in a similar manner to machine or boom spraying; the spray wand should be held steady at a fixed height above the ground and an even pace maintained across the plot.

The ideal width for plots is one spray-boom width so that difficulties of coverage at the edges of runs do not occur. Where several runs down a plot are required the area should be marked out in lanes with string (for hand application), each lane being one spray-boom width.

Much preliminary work is needed before going on to the plots. Nozzle delivery must be tested beforehand, dummy plots should be marked out for testing equipment, and the correct speed of travel, nozzle spacings, and so on determined to give the best coverage. With tractor-drawn equipment "field try-outs" of spray delivery are essential. Once the required data for applying a set gallonage per acre are determined different rates of material are applied simply by varying the proportion of spray liquid to diluent. Water has no weedkilling properties and, within limits imposed by evenness of distribution and avoidance of run-off, variations in the amount of water applied are not important. Amounts of liquid applied per acre should normally be determined by commercial practice except for certain practices such as "drenching" sprays known to be inefficient. For most purposes sprays should be well atomised and the application of excessive quantities of water should be avoided.

All precautions should be taken to avoid excessive spray drift. Spray booms should be kept as close to the ground as practicable and windy weather should be avoided. In certain cases it may be preferable to avoid excessively fine spray particles, provided the efficiency of spraying is not thereby reduced. Adequate "buffer" strips between plots should be provided to avoid interference of spray on neighbouring plots.

Logarithmic Sprayer

A recent promising development from England is the "logarithmic sprayer". This machine sprays at a decreasing rate, beginning with the undiluted weedkiller as formulated and finishing with the pure water, the strength of the solution being halved every 6 yd. The machine works on the "closed tank" principle, spray liquid being replaced, as it is used, by water. At any point along the run, selected as giving the most desirable effect, a measurement from the starting point gives, from reference to a graph, the rate of application at that point.

A logarithmic spraying attachment for a small-plot sprayer has been devised in New Zealand and is being used.

It may be possible to adapt the principle to other types of “rates” trials, and, if successful, this should be a means of simplifying the application of treatments in these trials.

Part 6 — How to Measure Pasture Trials

SOME GENERAL THOUGHTS

Experiments are conducted so that treatments may be compared and a fair and unbiased judgment made on their value. There are a great many ways of arriving at this judgment, but the more objective the method is the better. This is where measurements, whether they be weights or counts or any other type, have a great advantage over observations and are to be preferred to observations for most critical work. The observational method, however, has other advantages no less real. Some of these are as follows:

1. Observational trials have a much lower time and labour demand and therefore it is possible to conduct many more observational than measurement trials with the same labour force.
2. There is little or no restriction as to slope and topography such as often applies with measurement trials. Observational trials are possible in remote areas: most measurement trials require relatively easy access and nearness to an experimental centre.
3. Observations do not interfere with the natural conditions, whereas most measurements do. Some pasture swards have been ruined by mowing cuts taken for yield measurements.

Both trial methods have their place and their limitations. The measurement trial, however, must always be supplemented by observations or the data may be meaningless or misleading.

Measurement Trials on Pasture

The perfect pasture measurement technique has yet to be designed. Each has its drawbacks. Each investigation, however, has a mowing technique best suited to it, and the first essential is to use the most efficient technique for each problem. As a guide the following are suggested, but there will always be special cases requiring special consideration.

Technique	Problems
1. Mowing and clippings returned	(a) Most fertiliser trials, unless stock data are also required. (b) Trials of some pasture species which do well under mowing. (c) Some simple types of tolerance to weedkiller trials, but not where weed control in pastures is being studied.
2. Frame, cage, or enclosure	(a) Fertiliser trials where stock data are required or where stock: fertiliser interactions are important. (b) Pasture species trials (most). (c) Weed control in pastures where stock management effects are important.
3. Rate of growth (regular intervals between cuttings)	(a) Pasture growth studies generally. (b) Pasture species trials where the time of making growth rather than total production is important. (c) Special types of fertiliser trials where it is important to know the **time** when responses are occurring (for example, nitrogen trials).
4. Occasional cuts	Various trials, but care must be taken in interpretation of results.

These four techniques are the more important ones and will be considered in greatest detail.

Mowing Trials and the Grazing Animal

Primarily, the aim of a mowing technique is to measure the feed available to the grazing animal under conditions of “normal” grazing management. That is, it must imitate the grazing animal as closely as possible in respect of height and frequency of defoliation and the technique must interfere with normal grazing management as little as possible. In some cases we also want to measure what the animal actually eats—but this is a different matter.

The mower is non-selective, but it is unbiased and efficient. It does seem to give results in line with animal production measurements. The correlation can never be perfect owing to the many variables in grass production and particularly in animal production measurements.

In some techniques stock never touch the trial. These are acceptable because they have been found to give results comparable with techniques using the grazing animal. In most cases the "mowing only" techniques are used in conjunction with other trials or as preliminary trials to sort out a few treatments for more extensive trial under more "natural" conditions. In any case such techniques are usually confined to a comparison of fertiliser treatments where it is likely that differences in sward production will be a good guide to the effect of such treatments on carrying capacity.

With pasture species trials, on the other hand, the interaction of the sward with the grazing animal is more important and mowing only techniques are of limited value for these classes of trial.

Type of Grazing Animal

Where we use the animal primarily as a mower, the dry sheep Is the most useful type of grazing animal. On the other hand such animals remove the minimum amount of nutrients and if we wish to tie the results more closely to farming practice, it may be desirable to run ewes and lambs or the class of stock usually found in the district concerned. The type of investigation and the data required will be the factors determining the type of grazing animal to use. Unless there is good reason to do otherwise, however, the use of dry sheep will be found to have many practical advantages over grazing with other classes of stock.

OPERATION OF VARIOUS PASTURE

MEASUREMENT TECHNIQUES

(a) Small-scale Plot Trials

Where they can be carried out successfully small-plot trials are preferred because of the smaller demands on area and labour and the greater degree of replication and number of treatment comparisons that are possible. Where stock measurements are also required, however, large-scale paddock trials are needed. For background reading to this work see "Methods of Measuring the Production from Grasslands", P. B. Lynch, New Zealand Journal of Science and Technology, Vol. 28 (Sec. A), No. 6, 1947.

All these small-plot trials may be laid down in the various designs given in Part 4. Typical plot sizes have been given in Part 5. Let us consider some of these methods in detail.

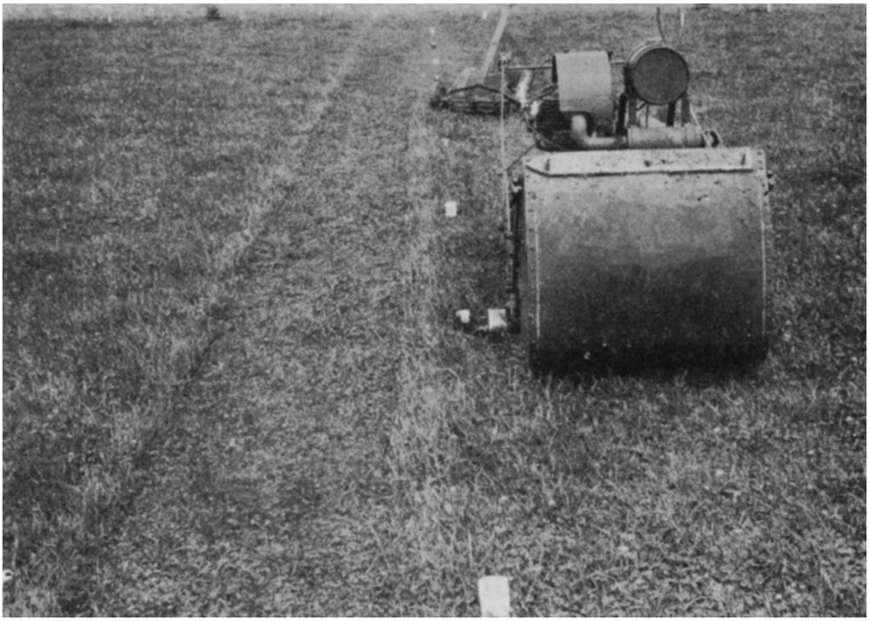
(1) Mowing and Clippings Returned Technique

This is the most convenient small-plot technique and is one that gives continuous production records. All plots are enclosed in a single enclosure and no stock are allowed to graze the trial area.

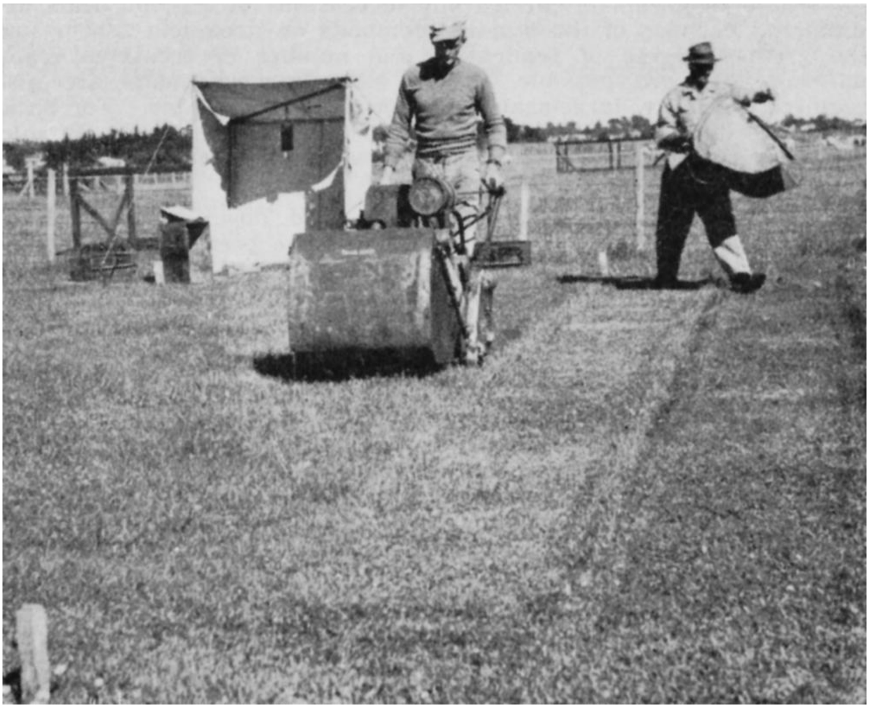
Details of Technique

1. Assume area ready for mowing and weighing.
2. Trim off ends of plots. Put clippings in heaps at side of area.
3. Mow one or more strips from each plot; weigh, take dry matter and herbage samples.
4. Put clippings back on cut areas on each plot in heaps so as not to interfere with the trimming of discard strips.
5. Trim up discard strips. These should be cut lengthwise to avoid transferring clippings from plot to plot. First mow discard strips on plot sides with catcher on; these clippings may be **discarded** to avoid transference from plot to plot. Then cut centre discard strip (or strips), making sure the herbage on these is returned to the plot from which it came.
6. Spread clippings from weighed area and centre discard strip **evenly** over appropriate plots.

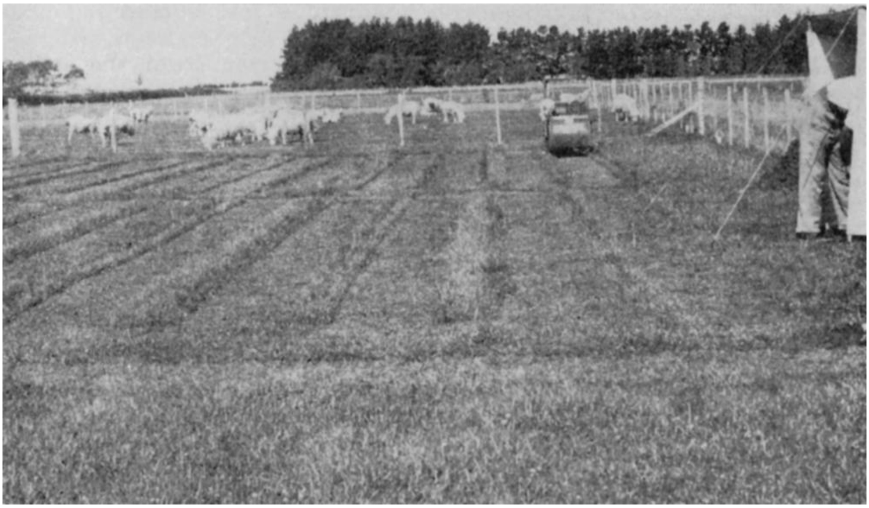
7. Clippings from headlands and trim strips are spread evenly over appropriate areas—or, if desired, may be discarded.



Mowing and clippings returned technique—Ends of plots being "trimmed off" before trial plots are mowed and weighed.



Mowing and weighing in progress. The weighing tent is in the background.



The mowing and weighing are completed and the discard strips are ready for trimming.

Precautions

1. **Never** let growth get too long before mowing.

2. Spread the herbage returned to the plots evenly.

3. Check that the herbage gets back to plots from whence it came.

4. Avoid mowing in windy weather (if possible).

5. Mow same strips for weighing each time on plots.

6. In cases where trimmings remain from previous cut,

   (a) rake off where possible;

   (b) where not possible, secure dry matter samples of fresh material only.

7. If herbage gets too long before cutting, discard clippings.

8. In very dry spells discard clippings.

9. If area gets very weedy, use weedkillers.

If area gets infested with insect pests use appropriate insecticides.

10. Take all possible care to get clean cutting of discard strips.

Drawbacks

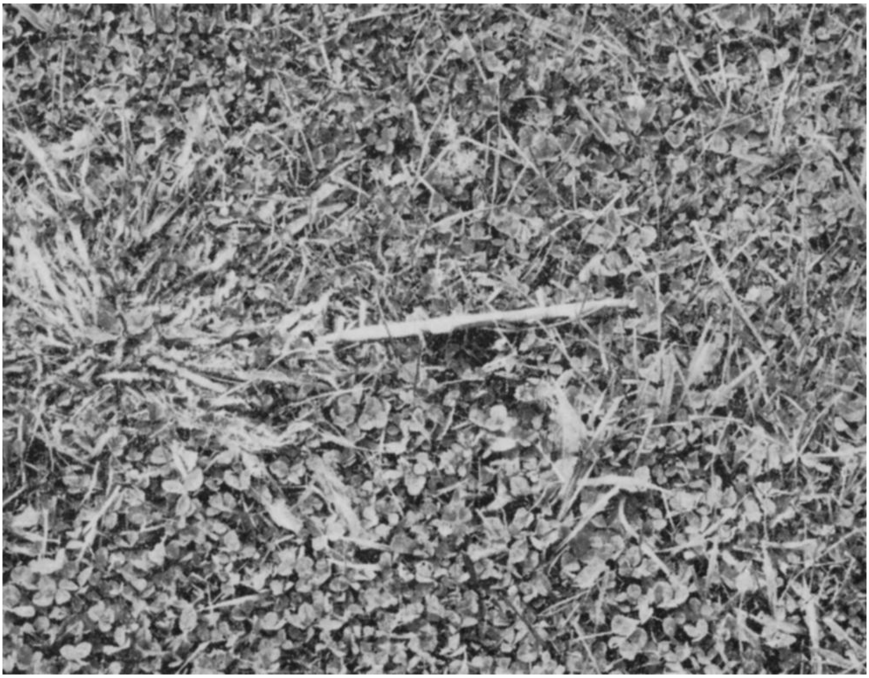
The main limitations of this technique arise from the lack of stock grazing which gives

1. A somewhat artificial type of sward.
2. A nutrient return different from that via the animal.
3. No stock production figures.
4. An underestimate of total pasture production.
5. Encouragement to some weeds and weed grasses.
6. Efficiency of method depends considerably on earthworm activity.
7. Non-decomposition of clippings in certain circumstances.

Nevertheless the absence of the grazing animal makes possible small trials yet good treatment replication and it means no transference of fertility. This is easily the most generally useful of the available techniques, and its limitations are worth careful study.

1. The Artificial Sward

The sward tends to become more clovery than does a grazed sward and it is even and lacks the typical patchy nature of a grazed sward. These are mostly advantages statistically, as they lead to less variation: they do not matter much in lime and phosphate fertiliser trials, but must affect potash and nitrogen comparisons in some degree. The position with regard to sulphur and minor elements is not well known.



Swards of "mowing only" trials tend to become clover dominant and weedy.

Where ecological considerations are important, as in many species and weed control trials, this evening-up of the sward is a most serious defect and may mean that another technique must be sought.

2. Different Nutrient Return

This is most important in fertiliser trials. The main difficulties are:

(a) **Return of Total Amounts of Nutrients:** If the herbage has to be discarded at any time, the loss of potassium (and sulphur) may be serious.

(b) **Different "Speed" of Return:** It is possible that decomposition of clippings and incorporation into the soil is a slower process than, for instance, the return of potassium to soil reserves via urine. This is probably not a very important factor, but it has not been properly investigated, though some work is in progress on urine effects.

(c) **"Non-patchy" Return:** Stock return of nutrients is in patches—clippings return is (or should be) even. This has effects on nutrient distribution in the soil and on sward variability. There doesn't seem much that can be done about this. Even return has its advantages from sampling and plot variability considerations.

3. No Stock Production Data

This is self-explanatory.

4. Total Pasture Production is Under-estimated

This matters only with some pasture species trials and rate of growth trials. From work done to date the effect seems to be evenly spread over fertiliser treatments, so that relative yields are not greatly affected.

5. Weed Ingress

This may be serious in some cases. Rosette weeds are encouraged, but can be controlled with weedkillers, but grass species like *Poa trivialis* can be a real nuisance. Weed ingress often shortens the useful life of trials under this technique.

Other limitations of the mowing and clippings returned technique are:

6. Earthworm Activity

The efficiency of the method is probably dependent partly at least on worm activity. It is possible on plots where this is low (for example, no lime) that incorporation of clippings into soil may be poorer than on plots with high worm activity.

7. Non-decomposition of Clippings

Clippings may remain undecomposed at the time of next cutting, particularly in dry weather. Their removal is difficult, but should be attempted by raking before cutting.

Advantages

1. Small plots can be adequately replicated and a considerable number of treatments compared.
2. Labour and land requirement is at a minimum.
3. No transference of fertility through stock is possible.
4. With manurial trials, treatment differences of the same order as those measured by the frame technique have been secured
5. The method gives clean herbage and dry matter samples, not fouled with stock droppings.

Modifications of the Mowing and Clippings Returned Technique

Method 1. Alternate Year Technique

The alternate year method is useful for topdressing trials. It works as follows:

(a) Have duplicate areas A and B with identical treatments and the same number of replications in each.

(b) After the first topdressing, area A goes under mowing and clippings returned and area B goes under normal grazing.

(c) At the next annual topdressing, area A is opened to stock; area B (which has been trimmed up for a few weeks before topdressing) goes under mowing and clippings returned technique.

(d) In alternate years, therefore, area A is mown and area B grazed, and area B mown and area A grazed.

Reasons for the Modification

(a) The year under grazing allows the sward to recover from the mowing only year and the sward is maintained for a longer period nearer in composition to a grazed pasture.

(b) No transference of fertility of each year's topdressing is possible, though transference through stock of "residual" fertility is possible in the year under grazing.

This is a useful technique for areas where it is difficult to maintain the sward satisfactorily under mowing only technique.

Method 2. Modified Mowing and Grazing (Intermittent Cuts)

The modified mowing and grazing method uses the usual mowing and clippings returned layout and technique, but after clippings have been distributed back on to the plots, the area is given a **quick** grazing by sheep sufficient to **clean up clippings only**. After this the area is trimmed up finally for another cut.

It may be used when for some reason it is necessary to cut relatively long herbage at some time or where the clippings do not decompose satisfactorily for some reason. It does not give continuous production records and introduces some transference of fertility (limited by the short time sheep are on the trial).

Sheep have to be watched closely while grazing and removed when clippings (which they normally eat first) have been eaten. The stocking rate should be heavy to keep the time the sheep are on the plots as short as possible.

(2) Occasional or Seasonal Cuts

The design of occasional or seasonal cut trials may vary greatly, but essentially they are small-plot trials, usually fenced off from stock. In such cases they are similar to the mowing and clippings returned trial, but are intermittently cut and grazed. In some cases the plots and trials may be rather larger and not fenced off from stock, but these are usually short-term trials and examine treatments such as nitrogen for out-of-season growth or treatments for hay or silage production.

This technique has been devised primarily for trials on farms which it is impossible to visit at short intervals. While in some circumstances the mowing only and clippings returned technique may be used in cooperative trials on farms, the necessity for frequent cutting sometimes makes it impossible to handle this method satisfactorily. However, if there are circumstances where that technique can be handled it is usually better to use it than the seasonal cuts or the strip technique.

In general terms mowing trials on farms should be so designed

(a) that they cause little interference with the farmer's management;

(b) that they measure growth under conditions approximating the farmer's grazing management (assuming this is good management for the particular circumstances and that we are not considering "rate of growth" trials).

(c) that they will not be ruined if, for some reason, growth gets away. This usually means they have to be designed for cutting with a sickle or rotary type mower;

(d) that treatments are adequately replicated. Soil variation is usually greater than on a research area.

The frame technique may be used, though it is difficult to get satisfactory replication in this case. The use of the frame technique in farmers' trials is mainly confined to cases where it is desired to place a figure on a response (for example, size of potash response) and for rate of growth trials or where the total production of special pasture mixtures is required.

In the seasonal cuts technique the aim is to get a cut representative of each season's growth. In areas with good winter production this should provide a sample of

(a) winter and early spring growth;

(b) spring growth ("silage" cut);

(c) summer-grown grass,

and possibly (d) autumn-grown grass; after this the area is topdressed.

The "sampling" aspect of these trials must be stressed. Continuous production records are not obtained, but valid treatment comparisons are possible if all seasons' growth is sampled. With fewer cuts this may not be possible—a cut shortly after topdressing will favour quick-acting fertilisers, and one shortly before the next topdressing may favour slow-acting fertilisers. Cuts should therefore be made under **both** circumstances.

Where this technique is used it is most necessary to realise the limitations of the data. Responses to fertilisers change in degree and sometimes even in sign with the seasons, while pasture species have their different best seasons of growth. Production data which do not give continuous records over the year are liable to give misleading results.

Thus lime responses are usually most marked in late summer and autumn and may be absent or lime may even be depressing in spring. A single cut during the year will not, under these circumstances, be a fair measure of the lime response over the year. Some lucerne varieties are noted for earliness of growth in spring. A cut in midsummer will not show this desirable feature.

On the other hand, where treatments are applied for special purposes the occasional cuts technique may be sufficient. Thus a hay cut to measure a treatment specifically applied to stimulate growth for hay is, of course, adequate. In general, however, when the occasional cuts technique is used one cut should be taken each season at a time when it is representative of that season's growth.

Details of "Seasonal Cuts" Technique

A small-plot trial is laid out and enclosed by a fence.

Consider the trial ready for cutting and weighing.

(a) Strips are cut and weighed out of each plot and necessary herbage samples taken. Herbage is put back on the plots from which it came.

(b) Discard strips are trimmed and area is opened up to normal grazing. (This trimming is necessary so that the area may be grazed off evenly.)

(c) After a period under grazing the area is closed for the next production cut.

(d) About one to two weeks after the area is closed it is trimmed over evenly with the mower, clippings being discarded.

(e) The area remains closed from grazing until ready for mowing and weighing again.

Precautions

1. A close watch should be kept on the sward. If it shows signs of deterioration, it may be desirable to give the trial a year's rest from mowing. This deterioration is hastened by taking too many cuts in a year, particularly if cuts are always made of long herbage (say above 6 in.). This will rapidly lead to smothering of clover and ingress of species such as Yorkshire fog. If circumstances are such that cutting of long herbage is usual, it is all the more important to give the trial a grazing spell between mowings.

2. In certain low-production trials it may not be possible to get more than one or two cuts a year, and those only by closing the trial from grazing all that time. In such trials, if the sward is being adversely affected by such treatment, measurements should be made every second year.

3. If the area has had a heavy production cut taken off and it is not possible to get the stock on the area shortly after trimming up, these clippings should be discarded rather than returned to the plots, where they would damage the sward.

4. The trial must always be trimmed up **after** mowing and weighing (and before grazing) and before it is finally closed in preparation for a production cut.

Drawbacks

1. Total yearly production figures are not obtained unless stock are completely excluded, when the technique becomes, in effect, mowing and clippings returned.

2. Some swards will rapidly deteriorate from too frequent cutting of long herbage. In these cases it is necessary to have more frequent grazing spells or try to cut growth at a shorter height.

3. There is some transference of fertility through stock during the grazing period.

Advantages

The technique meets the requirements for cooperative trials:

(a) It requires the least amount of supervision for a mowing trial.

(b) Long growth can be handled satisfactorily if a sickle-type or rotary mower is used.

(c) The trial can be adequately replicated.

(d) The technique can be fitted into normal grazing management. Cuts can be made, for example, which are representative of “autumn-saved pasture” or a “silage cut” or “winterspelled pasture”.

(e) The technique can be used under most types of pastoral farming and operated independently of the farmers’ management.

(f) It causes little interference with the normal running of the farm.

For these reasons the seasonal cuts technique is the one usually recommended for farmers’ mowing trials. Under certain circumstances, however, the strip technique is very useful, particularly on dairy farms under rotational grazing.

Strip Technique

A technique widely used overseas is to take strips out of a paddock before grazing. This is a type of occasional cuts technique which may have application under some circumstances. It means, however, that rotational grazing must be practised and the experimentalist must be closely in touch with the grazing management. The use of frames gives much greater latitude in this regard as well as allowing continuous production records.

A modification of this strip method may have some use on areas being break-fed with an electric fence. Such areas usually carry a considerable bulk of feed. The cutting of relatively long pasture is preferred with this technique, as the relative size of the errors introduced (because of the absence of a trimming cut) is less with long growth. Further, quick defoliation in break-feeding is getting quite close to the type of defoliation done by a mower.

The idea is simply to put a replicated small-plot trial in the field in a position where it will be fed off in one “break” after strips have been cut for production estimates. Alternatively one or more replicates can be placed in a break. In such trials the different replicates are cut at different times, just before grazing. The stock concentration on the break, after mowing and weighing, effectively cleans up the trial and there is the minimum of time lost in the grazing period.

It may be necessary to trim the trial after grazing in preparation for the next cut and possibly to harrow to spread droppings.

Cutting times fitted in with grazing times have the great virtue that they most closely imitate grazing management.

In the strip technique, where grazing pressure is not high, the stock tend to concentrate on and to punish the newly mown strips. In such cases it is most unwise to attempt to remow these strips before the next grazing, as growth will be markedly depressed thereon. This can be overcome by having plots sufficiently big so that a different strip can be mown for production estimates before each grazing. About four such strips per plot would probably be adequate.

The following notes give details of the more usual procedure with the strip technique.

Details of Strip Technique

A small-plot trial is laid out in the farmer's paddock, preferably not too far away from a fence. A sickle-type or rotary mower is usually essential, as the ground will in most cases be too rough for a cylinder-type mower.

Consider the trial ready for cutting. The field (or section of it if the paddock is strip-grazed by an electric fence) is ready for grazing.

(a) Mow and weigh the strips out of each plot and take the necessary herbage samples. Put the herbage back on the plots from whence it came.

(b) Trim up the discard strips.

(c) The trial is then grazed with the rest of the paddock in the normal rotation.

(d) About a week after the stock go out the trial area should be trimmed up again.

(e) A few days before the stock are due to graze the paddock (or strip) mow and weigh and repeat the above procedure.

Precautions

1. An occasional cut can be missed if the area is grazed unex-

pectedly, without much loss of information. If this does happen, do not forget to trim up the area after this grazing.

2. Occasionally the mown trial area may be selectively grazed and punished while the rest of the paddock is relatively neglected. This is least likely to occur with strip grazing with dairy cows and most likely to occur with rotational grazing with sheep. If it is affecting the trial seriously, it may be necessary to fence the trial off and use the seasonal cuts technique. This preferential grazing will rarely occur in well managed pasture.

3. Close liaison with the farmer is necessary because mowings must be adjusted to grazings. Ask him to avoid plots when feeding out.

4. If the trial area is excessively fouled after grazing, it may be necessary to delay the trimming cut until the area is in a reasonable state for mowing.

5. If a trial is in a paddock strip-grazed by an electric fence, it is best placed in the last strip or strips to be grazed. This avoids undue trampling of the trial area.

Drawbacks

1. The most serious drawback is the fact that mowings are dependent on the way the farmer grazes the field, and the technique cannot be operated satisfactorily if strict rotational grazing or strip grazing is not practised throughout the year.

2. There is always danger of stock punishing the plots by selective grazing, as the trial is only a small portion of the field. This has been discussed earlier.

3. Transference of fertility through stock is possible.

4. Production records are not continuous and no estimate is made of total sward production.

5. At certain times of the year plots may become fouled, especially under dairy cow grazing.

6. Reasonably close supervision is essential and the trial cannot be neglected at busy times without serious loss of information.

Advantages

1. The trial is cheap and easy to put down. No fencing is required and adequate replication is possible.

2. The pasture is as near under normal farming grazing management as can be secured without frames.

3. The technique is particularly well adapted for use on dairy farms and where strip grazing with an electric fence is practised. It is also possible to use it under rotational grazing with sheep.

4. Sward deterioration is unlikely, as the area is frequently grazed.

5. Records are automatically secured at all seasons of the year. If the field is closed for hay or silage, cuts representative of this growth can usefully be secured.

(3) Other Small-plot Techniques

These methods are not described in detail, as they are not considered to have general application. For further details reference should be made to the following:

Mowing and grazing technique and mowing only technique: (a) Hudson, Doak and McPherson: New Zealand Department of Scientific and Industrial Research, Bulletin 31. (b) A. W. Hudson: Measurement of Pasture Production, I.A.B. Herbage Publication Series, Bulletin 11.

Sears technique: P. D. Sears, “Pasture Plot Measurement Technique”, New Zealand Journal of Science and Technology, Vol. 25 (Sec. A), No. 5, 1944.

(a) Mowing Only and Discarding Clippings

The continuous use of the mower and discarding of clippings soon cause serious sward deterioration. This arises partly because of the encouragement of close-growing species which are defoliated less severely by the mower than are plants of more upright habit, but more especially because of the loss of nutrients in the discarded herbage. Of these losses that of potassium is probably the most serious, but the loss of other elements such as sulphur, phosphorus, and especially nitrogen may also be important.

The return of nutrients in the herbage may be made in ways other than the direct return in the mowing and clippings returned technique. Sears used a proportional return of dung and urine as described on page 69, and various methods of composting or otherwise treating the herbage before return have been tried. McNeur (Proceedings of New Zealand Grassland Association, 1953) used a return of nutrients as artificial fertilisers. His calculations are as follows:

Calculation of Fertilisers Required to Return Nutrients Removed in Herbage

(Weight in lb for pasture yielding 14,000 lb of dry matter)

Blood and bone	1,000
Dried blood	2,100
Superphosphate	200
Muriate of potash	975
Sulphate of ammonia	1,430
Limestone	2,004
Total	7,709

As a result of further work the rate of return finally adopted was 240 gm of this fertiliser mixture per pound dry matter of herbage removed. McNeur stated that the effect of this on pasture yield was very similar to that of equivalent dung and urine. He admits, however, that the technique is most suitable for preliminary sorting out of strains and species of pasture plants. It would seem to have limited application to fertiliser trials.

(b) Alternate Mowing and Grazing Technique (Hudson)

In this method two duplicate series of plots (A and B) are provided. Two types of mowing are made, (a) “M and W” when herbage is mown and weighed, and (b) “M and C” when herbage is trimmed up to an even height with the mower. The method operates as follows:

Day	Section A	Section B
1	Closed	Closed
6	—	Grazed

12	M and W (1): Herbage transferred to B.	Grazed
24	M and W (2): Herbage left on plots from which cut; section grazed.	M and C: Clippings transferred to A
25	Grazed	—
30	Grazed	—
36	Grazed	M and W (1): Herbage to A
37	Grazed	—
48	M and C: Clippings to B	M and W (2): Herbage left on plots; section grazed

and so on.

The method gives continuous production records and maintains the sward in good condition by frequent grazing. However, the most serious drawback does appear to be the transference of fertility from plot to plot, which is possible because all plots are grazed in a common enclosure. Though this transference is probably of little importance with some elements, including phosphorus, it may be serious with others. No trials under this technique have been laid down for several years.

(c) Sears Technique: Return of Stock Droppings in Proportion to Treatment Production

Briefly, the following procedure is followed:

(a) The plots are mown and the herbage is weighed and replaced on the plots from which it was cut, after necessary samples have been taken.

(b) Sufficient sheep are grazed on the plots to clean the area to mower height in a day or two. These sheep are fitted with bottles and bags to collect urine and dung so that none falls on the trial. The total collection is then mixed in a barrel, sieved, and measured before being watered back on to the plots in proportion to the amount of dry matter of herbage yielded by each plot and headland.

There are some minor defects in the method such as a possible irrigation effect from the watering on of dung and urine and certain difficulties of securing the type of patchy application that would occur with natural return, but the main problem is the high requirement of time and labour. It is only possible to conduct trials under this method on well staffed research stations. Recent work would indicate that for fertiliser trials at least the return of clippings gives a comparable effect to the return of dung and urine and is infinitely less laborious.

(b) Large-scale Grassland Production Trials

Investigations of this type require large areas and are costly to conduct. Adequate replication of treatments is difficult and few treatment comparisons in each trial are possible. Nevertheless, these methods are essential for certain types of problems, particularly those where pasture species are being studied. They are necessary where stock production data are required, and in all cases where the animal: pasture interaction is expected to be important and it is desired to compare treatments under as natural conditions as possible.

Where small-plot trials will give the answer just as well, they are to be preferred to large-scale trials. For many problems it will be found possible to do preliminary investigations using small-plot techniques and to use the large-scale trials in the final stages. By this time the probable useful treatments will have been reduced to two or three. It will be possible to arrange that the data produced by the large trials will be in a form which has direct application in advisory work.

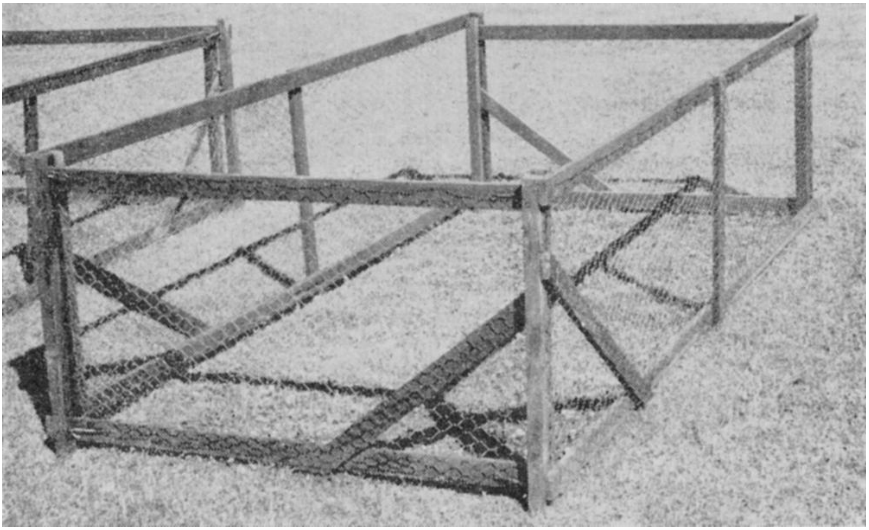
Frame or cage methods would nearly always be used were it not for the cost and size of trials under this technique and the difficulty of securing adequate replication of treatments. Generally the **precision** of treatment comparisons is relatively low under this technique, and this is its most serious defect. Another less important drawback is the possible shelter effect of the cage on the pasture. This does not seem very marked, especially with open-at-the-top cages with metal frames.

Stock grazing management of cage trials is not easy. Though it is desirable to have one flock for each treatment in all cases (and essential where stock production is being measured), this may be hard to arrange in practice. With “on and off” grazing the chance of fertility transfer to and from the trial may be considerable. If the farm paddocks have been well treated, control plots are likely to benefit unduly by such grazing management.

On the other hand, with, say, four replications of treatment it may be difficult to get satisfactory rotational grazing. Set stocking for periods at least may be preferable, unless this is going to affect the swards adversely. The best compromise has to be worked out for each trial according to the particular circumstances at the time.

(1) Frame, Cage, or Enclosure Technique

This technique is undoubtedly one of the most useful for measuring pasture production. Its great advantages are that it causes no interference with normal management and that it can be adapted to a variety of different types of trial.



Frame technique—A wooden open-at-the-top frame or cage in position on a trial.

Details of Operation

One or more movable cages are set up in the experimental fields and used to sample production in those fields. The sampling aspect needs to be stressed. Because the frames move over the fields (or, in some cases, the plots) in the course of a year, the percentage of the total area sampled is quite high. This is one of the main reasons for the efficiency of the technique, provided production is considered over fairly long periods (for example, seasonal or yearly). The error involved in a single cut is relatively high.

Because the technique cannot operate on other than large plots, the number of replications must be kept down to a minimum and we frequently find it difficult to establish statistical significance among treatments. (Large plots mean great opportunity for soil variation, and this is coupled with few replications and a sampling error.) It is the practical rather than the statistical advantages that make this a valuable technique.

Measurement of Pasture Production by Frames

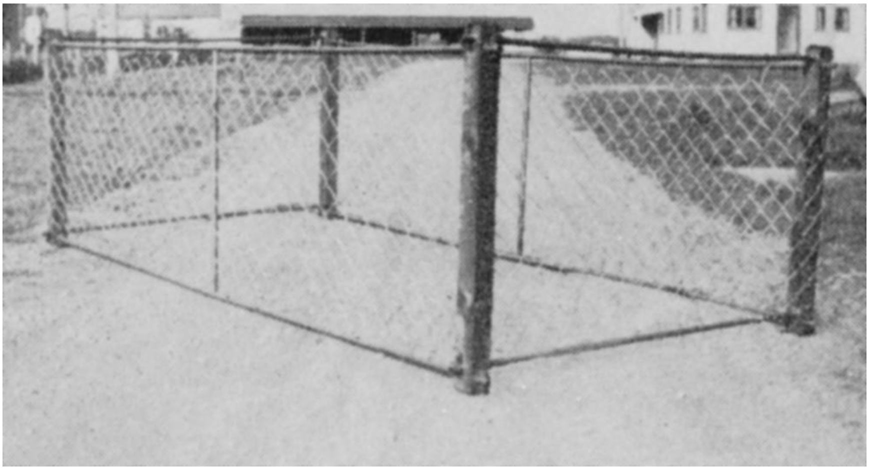
Method (a)

In the first case all frames are treated in the same way.

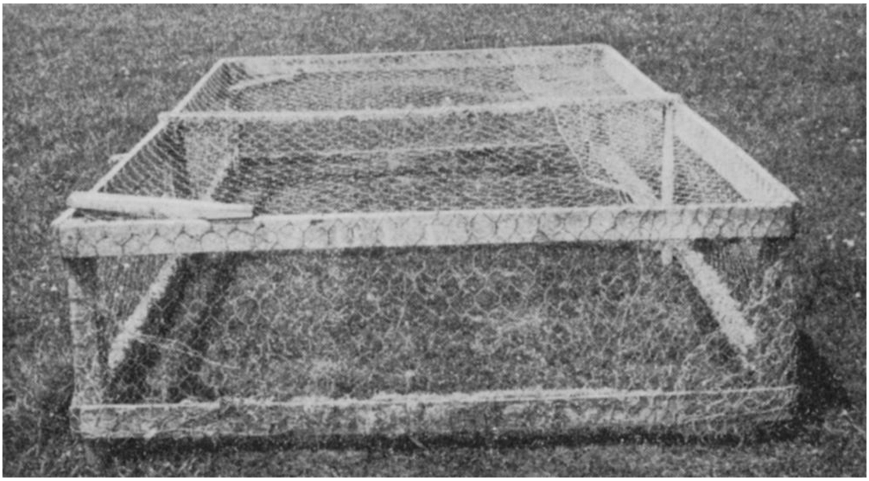
Start: Grazing of field is completed.

About four days later: Areas in the paddock are trimmed and frames placed thereon.

Subsequently: Paddocks are grazed as required—the last grazing to be three or four days before mowing and weighing herbage in frames, and trimming fresh areas in the paddock and placing frames thereon.



Frame technique — An open-at-the-top metal cage which gives the least shelter to pasture



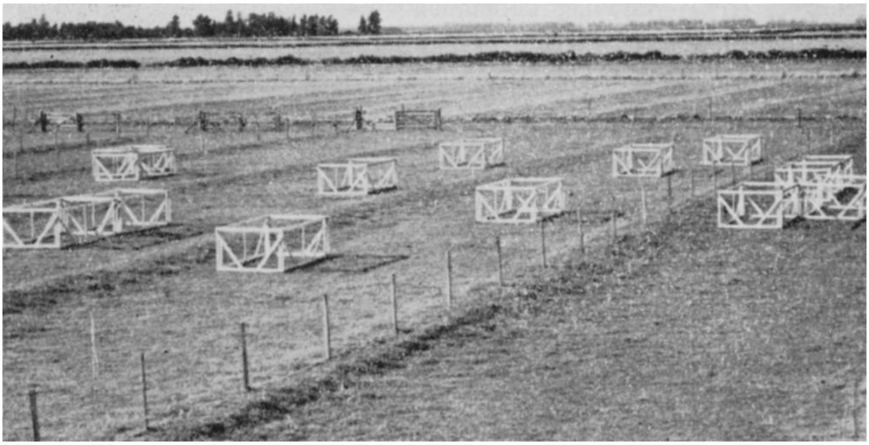
Frame technique—A low, wooden, closed-at-the-top frame. This type of frame gives the greatest "shelter effect". Open-at-the-top frames are preferred.

This is the simplest case, the only important point being the necessity to wait for sufficiently long after grazing until **all** the pasture in the field **has recovered above mowing height** so that it may be trimmed off evenly for placing frames in preparation for the next production cut.

Method (b)

In the second case the experimentalist has no strict control over grazing (as is essential with method (a)), or he is operating a **rate of growth** technique which requires uniform intervals between cuts, irrespective of grazing, or some special types of trial such as trying to measure production as it would be under set stocking. In this method **two** frames are required to get **one** production cut. The grazing operates independently of mowing.

Day 1:	Frame A is "placed" in the paddock on existing sward.
Day 7 (say):	Herbage in Frame A is trimmed and Frame B is "placed".
Day 21 (say):	Herbage in A is mown and weighed and A is placed in a new position. Herbage in B is trimmed.
Day 35 (say):	Herbage in B is mown and weighed and B is placed in a new position. Herbage in A is trimmed, and so on.



A typical trial being measured by the cage technique.

The value of this method is clear. It is especially useful in farmers' trials if the grazing management is good, because no control over grazing is needed. On the other hand two frames are needed to give the same result as one frame in Method (a).

One special point: if growth in the field is long at time of mowing and weighing (when the technique is in full swing), placing a frame over this long growth and leaving it for a further period will give an excessive amount to trim off, to the detriment of production subsequently. In such a case it is better to trim off the growth when the frame is placed (giving two trimmings before mowing and weighing).

Alternatively, if the herbage in the "placed" frame has grown excessively long, whereas the field (which has been grazed) is a reasonable height, it is better to trim an area in the field for the placing of the frame for mowing and weighing cut, and ignore the growth in the "placed" frame. **It is useless to try and operate the technique on a badly managed field**. Modifications will be necessary from time to time as indicated, provided that

(a) production is measured from an area previously trimmed to an even height;

(b) trimming is always done on an area that has wholly recovered above mowing height.

Method (c)

With the "full" enclosure technique where stock are maintained on the trial, one flock to each treatment, all plots cannot be cut on the one day (as it is desirable to tie in cutting with grazing to enable Method (a) to be operated). In this case **replications** are cut on the one day, usually operating Method (a).

Method (d)—"Grazing Estimation"

No trimming cut is required in the grazing estimation method. Herbage in frames for weighing is "plucked" or removed by shears to the height of the grass on the grazed pasture. Frames can then be placed in a new position without prior trimming. This method has the advantage of simplicity, as no mower is required and it most closely approximates grazing. However, the factor of personal bias is so great as to make suspect any figures produced unless the technique is operated under the most rigid supervision and the data are checked in every way possible. "Plucking" is also much more laborious than mowing.

Method (e)—"Australian Difference"

In the "Australian difference" method the herbage must be removed to ground level. Shears are usually used and only small areas can be sampled. The method operates as follows (considering one frame only):

1. Frame is placed without prior trimming. The herbage in that frame is estimated by taking the growth off at ground level from an adjacent "open cut" area of the same size as the frame.
2. Herbage in frame is cut to ground level (after a "growth" period) and weighed. Frame is placed in a new position and an adjacent open cut taken. Production is the growth in the frame (as above), less that of the open cut taken **previously.**

This is a particularly useful method on areas that cannot be mown and does not require a previous trimming cut. It is, however, more laborious to operate than mowing, and this means that the sample areas are smaller.

Comparison of Methods

An investigation was made into the size of the errors and the estimates of production secured by these methods (P. B. Lynch and N. S. Mountier: "Cutting Techniques in Grassland Experiments", New Zealand Journal of Science and Technology, Vol. 36 (Sec. A), No. 4, 1954). The conclusion reached was that the "standard method (Method (a) above) was to be preferred for most types of investigation.

(2) Rate of Growth Technique

The rate of growth technique is usually run in association with the cage technique. The essential feature is **regular** intervals between cuts so that pasture production changes throughout the year can be closely followed.

With these trials comparisons between treatments in respect of total production are regarded as less important than the time of making production. The fact that cutting at the usual interval of two weeks means lower pasture production than cutting at, say, 4 to 6 in. height of growth is therefore less important. Whatever interval is used the use of a **standard** method is essential if we wish to compare pasture production in different districts. That this is so is shown by a trial at the Marton Experimental Area, where the following data were obtained:

	Relative yields
Cutting at 1-week intervals	100
Cutting at 2-week intervals	139
Cutting at 3-week intervals	169
Cutting at 6-week intervals	227

Plot techniques to measure rate of growth have not been very successful and the simple cage method to get fortnightly measurements seems to be the most generally useful. Smaller intervals between cuts could be used in certain high-producing districts, but the two-weekly interval seems to be the best compromise.

In some trials, such as with nitrogen fertilisers, it may be worth adopting a rate of growth interval at critical times, say in early spring or for the first three months after the nitrogen application.

Most species comparisons are best made under the cage and rate of growth technique, the field being managed as is best known for the species sown therein and the cages cut at regular intervals to study in greater detail the changes in species production over the years. It is possible that some species which are slow to recover after defoliation may be penalised by such a technique. This will have to be watched. It could be overcome by cutting two pairs of cages in each field, say as follows:

One pair cut at fortnightly intervals.

One pair cut at four-weekly intervals.

This would greatly improve the value of the comparisons made.

Pasture species comparisons can also be made by cutting (and grazing) at some predetermined factor such as height of growth or, possibly, light intensity at ground level. This may have to vary at different seasons, but the important thing is to remove the decision as to the time of cutting each species from the unsupported judgment of the experimentalist.

Special Points in Operating Rate of Growth Trials

1. If there is no growth to cut at any time, nil production **must** be entered against the appropriate date. With the frame method, the frames are left in position for a further fortnight. Mowings need not be made if growth is estimated to yield less than ½ lb green herbage per frame or plot.

2. **Adapt a standard interval between mowings for all rate of growth trials (usually two weeks):** It is most important that this interval be maintained as closely as possible.

3. In operating the frame rate of growth technique in a large paddock restrict the movement of frames to a small, uniform representative area—about 1 acre is adequate. By such means the effect of paddock variability, which might interfere seriously with measurements of changes in growth rate, is reduced considerably.

General Comments

The frame technique has the following advantages:

1. Production is estimated with the least disturbance of normal grazing management.

2. The method is adaptable to a wide range of managements and types of trial.

3. Small frames give good production estimates over a period because the shifting of frames over the field means a high percentage of area sampled.

4. The method can be used in conjunction with trials giving stock production data.

5. Several cutting techniques can be used according to circumstances.

6. In pasture species trials requiring different managements for each treatment the frame technique is particularly useful.

7. Swards which rapidly deteriorate under mowing only may have their production estimated by the frame technique without harm.

On the other hand, the drawbacks must be realised.

1. Large areas are required for trials; this means few replications and difficulty in many cases of getting statistically significant dif-

ferences among treatments.

2. A possible effect of frames on pasture production may operate (see below).

3. The technique needs reasonably close supervision for best results.

4. It can be used with only a few treatment comparisons.

Effect of Frames on Pasture Production

The main types of cages used in this country for protecting herbage from animals are

1. Wooden frame, netting covered, open top, sides about 3 ft high.
2. Low wooden frame, netting covered, with netting top.
3. Metal frame, netting covered, open top, sides about 3 ft high.
4. Metal cage in sections, netting covered, with netting top.
5. “Electric cage”; metal frame, two or three wires on sides charged from an electric fence unit.

A useful size (ground area) is 10.9 ft x 5 ft. Two strips each 2 ft wide cut from a frame of this size give an area of 1/1000 acre.

The efficiency of cages in regard to their effect on the herbage weights obtained has been studied and the types may differ markedly. With open cages the main effect on grass growth appears to be through a temperature difference inside and outside the cages and through protection from wind. In closed cages the effect of reduced sunlight and of different humidity might also be present.

Some English work has been done on closed cages of a type with a semi-circular cross-section and a metal frame, and an average yield increase of 11 per cent dry matter was obtained inside the cages compared with the growth in the field. In this experiment the grass temperature within the cages was on the average more than 1 degree higher and the relative humidity 9 per cent higher than outside. These cages probably offer more protection than any open cages, but would resemble low, covered types.

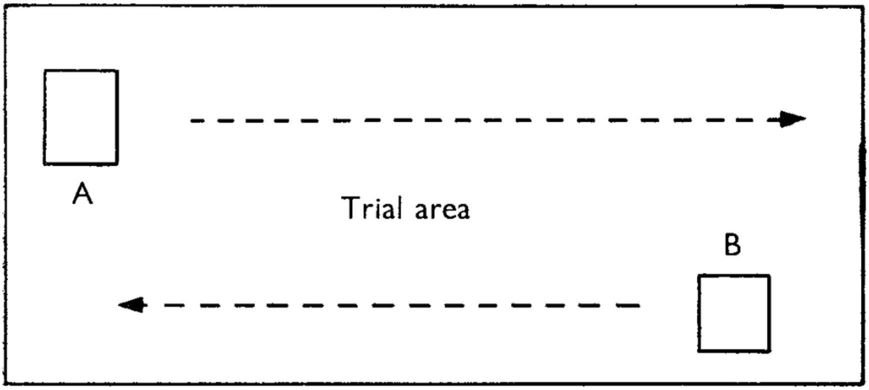
Work on the temperature aspect has been done in this country with open cages. In two experiments the average differences in grass minimum temperatures inside wooden frames (type 1 on page 76) with a 3 in. baseboard and those outside were 1 degree and 2 degrees F respectively, both results being highly significant (that is, they were reasonably constant over all the days recorded). It was also shown that the differences were greater at low temperatures but disappeared above about 56 degrees F. There was no trend in temperature as the thermometers were moved further from the base-board into the middle of the frames.

Temperatures within the metal frame were no different from those outside. Thus the metal types appear to be best, and their advantages should be realised for open, windy areas or areas of low production, where the frame is likely to remain in one place for a considerable period. In one exposed area the protection from wind offered by the base board and corner pieces of a wooden frame had caused the grass at the edges to be more than 6 in. higher than that in the middle of the frame. Of course, if such a thing happens, mowing strips must be taken from the centre of the frame area only.

Placing of frames: In all replicated trials frames must be placed by a random method. The plot area is divided (by stakes in the boundary) into a number of possible frame positions and random numbers are then used to fix the position at each shift.

For simple trials and farmers' trials where "random number" methods are inconvenient placings may be made in a more or less regular fashion so that the whole area is covered in about a year. The distance between each frame position should be chosen at random (for example, between 1 and 10 yd by the use of sets of random numbers of 1 to 10).

In all plans of frame placings omit headlands and obviously atypical areas from the area to be sampled.



With two frames in this field, placement is first made as shown and then the frames are moved (roughly) in the directions indicated.

(3) Stock Production Measurements

One of the great advantages of the frame technique is that it can be used to give supplementary pasture production information on trials where stock carrying capacity and production are also measured. The remarks in this section, however, apply mainly where the stock data are subsidiary to pasture production data. The conduct of stock experiments includes many aspects outside the scope of this bulletin.

With the "full" technique stock are maintained on the trial area, one flock for each treatment, so that no transference of fertility among treatments is possible. However, if (as usually happens) the stock graze each replication in rotation, transference of fertility does operate to even up differences among replications. If this effect is considerable, it invalidates the statistical analysis usually made. It is not usually something we need to bother about overmuch, provided no effort is deliberately made to even out replication differences.

Stock Grazing Days

The calculation of these is straightforward, certain conventions being accepted. Over long periods sources of error, such as time spent grazing in relation to feed consumed, are evened out, though if one treatment fattens stock while another merely maintains them, stock grazing days are obviously an imperfect measure. Such things are usually, but not always, obvious. Differences in stock grazing days should not be considered unless at least a year's records are available.

In trials where sheep graze rotationally all fields in one treatment and are maintained on the treatment, flocks are small, and one sheep added or taken away materially affects the computed grazing days. If the flock contains six sheep, the addition of one sheep gives a 16 per cent increase in grazing days. Over a long period the effect of the overgrazing would show in the pasture and cause sheep to be removed.

CONVENTIONS USED IN RECORDING GRAZING DAYS

Kind of stock	Abbreviated description	Equivalent to
Milking cow	M.C.	1 cow
Dry cow	D.C.	1 cow
Bull	B.	1 cow
Yearling heifer (12 to 18 months)	Y1.	$\frac{1}{2}$ cow
Yearling heifer (18 months to 2 years)	Y2.	2/3 cow
Calf (6 to 12 months)	C.	1/3 cow
Horse	H.	1 cow
Ewe	E.	1/6 cow
Wether	W.	1 ewe
Hogget (from 6 months)	Hgt.	1 ewe
Ram	R.	1 ewe
Lamb under 1 month	L1 }	$\frac{1}{4}$ ewe
Lamb 1 to 2 months	L2 }	
Lamb 2 to 3 months	L3 }	$\frac{1}{2}$ ewe
Lamb 3 to 4 months	L4 }	
Lamb 4 to 5 months	L5 }	2/3 ewe
Lamb 5 to 6 months	L6 }	

In calculating cow-days on dairy farms a part-day grazing during hours of daylight between morning and evening is reckoned as two-thirds of a day and a part-day grazing during night as one-third of a day. Sheep day-time grazing and night-time grazing periods each equal one half-day.

Stock Production Data

If flocks are maintained on each treatment, their progress can be checked by measurements such as live-weight changes, wool weights, lamb production and fattening, milk production, etc. All these measurements are subject to high errors because of the small numbers of stock on each treatment and one must not expect perfect correlation with pasture production data. The main values of these data are:

(a) To check on the pasture records in case some gross effects on stock are occurring as a result of some differences in pasture quality.

(b) To provide some means of interpreting pasture production records into stock data when the evidence indicates that the latter are sufficiently reliable.

Live-weight changes can be used to some extent to check on the efficiency of grazing management, and they do provide a more objective study of treatment effects. But animals vary individually. Cows may vary up to 100 lb in a week on a plentiful diet and when weighed under similar conditions. For reliable weight studies at least 20 sheep per treatment are necessary.

The first controlled live-weight study at Rukuhia showed that on a near-starvation ration, with very strict control over numbers and frequent weighings and within stable growth-rate periods, the average standard error per sheep for each weighing was about 3 lb. At times, when the growth rate of the grass was changing quickly, the average weight of all sheep in the group changed by up to 10 lb, with large variations for individual sheep.

Animals should be weighed at regular intervals, which should not be greater than one month for growing stock and less frequently for mature animals. They must always be in as nearly similar condition as possible in respect of feed and water intake at the time of weighing. If possible, they should be held in a yard without feed for a period (at least two to four hours) to stabilise them in this respect; with some classes of stock, of course, such treatment is impossible.

Observations on stock thrift (for example, worm infestation) and pasture management are essential. With sheep, wool weights should always be secured; with lambs, percentages drafted fat and percentages primes, seconds, and culls. Whenever a definite change in nutrition or treatment (for example, tailing, weaning, etc.) occurs animals should be weighed just before it occurs and also shortly after.

Estimates; of Consumption of Herbage by Stock

Modifications of the frame technique have been tried by various workers, such modifications being based on measuring what is left after the stock have grazed the paddock compared with the growth protected from grazing by the frames. The difficulty with all these methods is that no distinction is made between consumption and wastage and the figures obtained often indicate 100 per cent utilisation of herbage by animals. **Chemical marker methods** seem a much more satisfactory approach. That developed by Ruakura workers is as follows:

(a) Chromium sesquioxide fed in capsules twice daily is recovered quantitatively in the faeces. To estimate the amount of faeces excreted in 14 days the chromium content of a bulk sample of faeces taken twice daily from the rectum is estimated.

(b) The intake of the animal can be calculated from a knowledge of the amount of faeces excreted if the digestibility of the pasture is known. This digestibility can be estimated from the percentage of nitrogen in the faeces organic matter.

(c) Thus, chemical analyses of the faeces of animals fed with chemical markers can be used to estimate herbage consumption.

The method, which is still being improved, has been used successfully for many investigations. It can, of course, only be operated on a research station with full field and laboratory facilities.

(R. J. Lancaster, M. R. Coup, and J. C. Percival, New Zealand Journal of Science and Technology, Vol. 35 (Sec. A), No. 2, 1953; R. J. Lancaster, Vol. 36 (Sec. A), No. 1, 1954.)

SAMPLING OF HERBAGE FOR DRY MATTER AND SPECIES COMPONENTS BY DISSECTION OF CUT MATERIAL

1. Herbage Samples for Dry Matter

Errors in the estimation of dry matter may arise from several sources. Some of these are as follows:

(i) Drying out of the green sample after cutting and before weighing.

(ii) Errors in weighing the green sample.

(iii) Variations in the percentage dry matter between the sample actually taken for analysis and other samples which could be taken.

(iv) Errors in the process of drying.

Good technique will reduce these errors to a minimum.

Waterproof bags or tins must always be used to hold samples until such time as they are accurately weighed. The procedure is usually as follows:

(a) **When dry matter samples are taken from each plot:** As soon as each plot is mown the herbage cut must be weighed and a sample taken **immediately** into the bag or tin. This sample must be representative of the herbage cut. To do this the weighed grass must be thoroughly mixed, and then a handful taken from four or five places to get the required amount. All this must be done in the shortest time possible to avoid drying out.

(b) **When a bulk sample is secured from each treatment:** The procedure is the same as for plot sampling except that one waterproof bag is kept for each treatment and this is filled with samples from each plot of **that treatment** as it is mown and weighed.

Weighing the Green Sample

This is best done with a good balance after the trial is mown and weighed and in sheltered conditions. With a balance weighing to an accuracy of 1 gm or better, a 200 gm sample is adequate. Accurate weighing is essential; inaccuracies at this stage are one of the largest sources of error.

With complicated trial designs such as lattice designs that require adjustment of plot weights according to their location, dry matter samples **must be secured from each plot.**

Any obvious contamination of samples must be removed before dry matter samples are weighed out.

2. Herbage Samples for Dissection Analyses

The accuracy of figures obtained by this method is often questioned. The main sources of error are:

1. Error in sampling from the grass cut from the plot.
2. Error in the sample of 10 gm or 20 gm drawn in the laboratory from the grass received.
3. Wrong identification.

There are obviously a few species which are difficult to identify from small cut leaves and stems, such as red clover from subterranean clover, and ryegrass from crested dogstail. But check runs done at the laboratory indicate that most operators are accurate in identification. Most sampling trials we have conducted indicate a higher accuracy than many observers feel is obtained under field conditions. Since the results are quoted in percentages of dry matter, however, the relative density of the species may cause unexpected results. The dry weight of 1 gm of each of several species (estimated only once, at Hamilton) was

	gm
Perennial ryegrass	23
Cocksfoot	30
Timothy	22
Chewings fescue	27
Browntop	25
Yorkshire fog	16
Poa annua	14
White clover	16
Subterranean clover	14

All grasses and clovers were in the same state of dampness.

The accuracy obtained with different sample amounts will vary according to the length of the herbage, but probably in a well mixed sample it is directly proportional to the number of separate pieces. With short herbage 10 gm samples will be adequate, with medium herbage 20 gm, and with long herbage 40 gm.

The sampling error in taking the sample from the mower is probably much more serious. In one trial at Winchmore the difference in ryegrass and in dead matter between two heaps of grass mown from the same plot was 10 per cent for each, and one suspects that anomalous results are often due to poor mixing before the sample is drawn in the field. Good mixing is more important than taking a large sample.

Special Points

Herbage dissection samples are taken in the field in a similar manner to samples for dry matter estimation. Weighing is not required. Samples should be about ½ lb to 1 lb and must be forwarded **fresh** to the dissection laboratory by the most rapid transport available. Plastic bags should be used to hold samples and these are best loosely packed in boxes for transport. It is probably desirable to have small holes punched in the bags so that air circulation around the grass is not impeded.

Samples should, if at all possible, not be sent in a finely chopped "salad" condition, as this makes the analysis much more difficult. In some cases it may be desirable to clip (by hand) special samples for herbage dissection from the discard strips, but if this is done, the greatest care must be taken to see that sampling is done at random and that enough is clipped to get a representative sample.

MOWERS AND THEIR OPERATION

The Ordinary Hand Mower (Lawnmower)

If properly used, this can be a useful machine in pasture measurement trials. It is suitable for small-scale trials and where herbage is not long. It is particularly valuable for small trials under mowing and clippings returned technique. It is also useful when used with the motor mower for minor trimmings of plots, such as ends. The capacity of the lawnmower is limited, and where there is considerable mowing to do it must be replaced with the motor mower. Hand mowers chosen must be of a design that will throw all the grass **backward** and, under reasonable conditions, catch all the grass mown.

Motor Mowers

Motor mowers are of various types which may be classified as follows:

(a) Cutters in the form of rotary cylinders (as in the lawn-mower).

(b) Sickle-type, that is, straight knife and ledger plates.

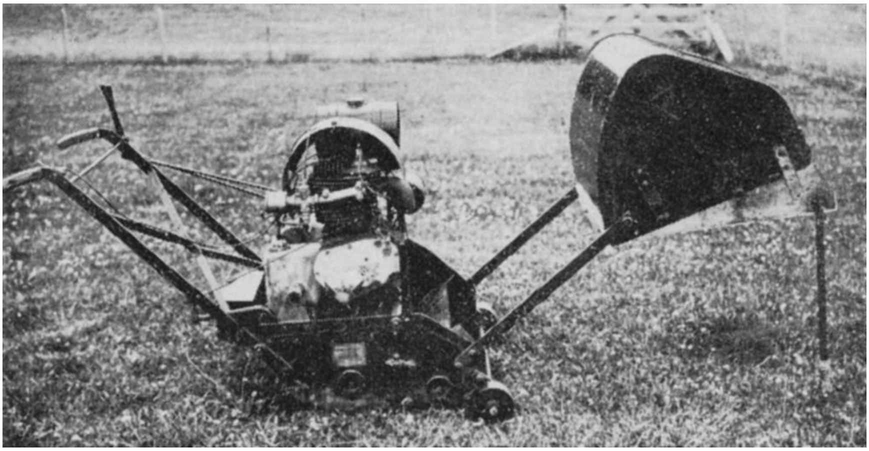
(c) Cutters rotating in the horizontal plane.

(a) Rotary-cylinder Types

The rotary-cylinder mower commonly used has an effective cutting width of 24 in. When this type is used for pasture trials the roller in front of the cylindrical cutter is removed and replaced by wheels at the sides. A special high-cutting attachment is available.

Too many cutting bars in the cylinder may chop up the herbage too finely. Some cutter bars may have to be removed to reduce to a satisfactory number, usually three or four. The radius of the cutting cylinder is also important. With a small radius comparatively long herbage cannot be cut satisfactorily.

In the preferred makes there are separate drives for the cutting cylinders and the wheels, that is, the cylinders can rotate while the machine is stationary. This is quite important—particularly at the beginnings and ends of plots. This type of machine, properly adjusted, is very suitable for mowing trials under sheep-grazing conditions.



A rotary-cylinder mower modified for use on experiments.

(b) Sickle-knife Types

The sickle-knife types are more suitable for trial work where pasture grows long, for dairy-grazing conditions, for hay and silage cuts, and for lucerne cuts. In addition, they can be used on rougher ground than can the rotary-cylinder type.

These machines are frequently not very manoeuvrable, as they are long overall and a lot of space is required for turning.

In some models there is not much clearance behind the cutting knife and cut herbage can be caught up and thrown aside. Where there is sufficient space behind the cutting knife it is sometimes possible to fit a catcher to gather cut herbage. If this is not possible, the cut herbage has to be raked up later.

(c) Rotary-cut Mowers

Rotary-cut mowers can handle both long and short growth and recent improved models are well worth close study. Their main disadvantage is the maceration of the clippings so that these are quite unfit for herbage dissection. Separate hand-cut samples are needed. In wet conditions or with damp grass some models tend to clog. Grass collects underneath the cover and is not thrown efficiently into the catcher. These defects will probably be overcome, and the machines, being light in weight and very versatile, will probably prove very useful, particularly for experiments on farms. As they can cut close to obstructions, these mowers are also useful for cleaning up around trials.

(d) Flail-type Harvester

A new pasture sampling machine, based on a flail-type harvester, is described by R. I. Johnson in "The Agricultural Gazette of New South Wales" (Vol. 73, part II, page 607: Nov. 1962). This mower is particularly useful in long herbage, which it cuts and delivers into a collection bag at the rear of the machine. Preliminary trials in New Zealand have been very promising.

(e) Cutting Small Areas

Mechanised equipment is now available which uses cutting devices similar to sheep-shearing handpieces. These may be driven by small, portable, petrol-driven engines or by batteries, some models of which may be carried on the back of the technician. The use of motor mowers is impracticable for most hill country pastures, and they are being used to sample small areas for production measurements or botanical assessments. This equipment would not usually be used where a mower can be operated, because of the time taken to cut sample areas sufficiently large to be considered adequate for most types of experiment.

Points in Cutting Technique

All mowers must be sharp and in good working condition to be efficient. They should cut level and at the same height along the effective cutting width. They should be adjusted for correct height.

When mowing for weighing the whole plot should **not** be cut. The size of plot cut should be governed by the actual cutting width of the mower, or multiples of this width, and the plot length. One, two, or even three cleanly mowed strips should be taken along the effective plot length. These strips are separated by unmowed ridges of herbage, which are discard strips later to be mowed and discarded. If the whole plot were cut, inaccuracies are almost certain to occur along the edges. In addition, wheel tracks tend to flatten grass so that it is not cut cleanly by the mower on the next cutting trip.

The actual cutting width of the mower must be checked by measurement. This could be an important source of error when later calculations are made. In a 24 in. cut, a divergence of 1 in. means a 4 per cent error. The length of plot cut is also important and should be measured. Ends of plots must be trimmed off evenly to give the same length of plot mown in each plot. The same strip should be cut at each mowing if possible. If the tracks of the previous mowing are still visible, they should be followed.

Points to Avoid

1. Loss of herbage through faulty technique; mowing too quickly or in too windy weather.
2. Wet weather mowing. There will be error in the green weights, owing to water content, but dry matter can be correctly determined if care is taken in the securing of samples. Do not mow in circumstances where the pasture will be seriously injured by the mower, such as on wet, soft ground.
3. Do not let growth get too long before cutting, or repeat mower journeys may be necessary to deal with the herbage not cut on the first journey.
4. The weight of the mower may be important. Heavy mowers may bruise herbage or side-slip on sloping ground.
5. Avoid cutting long herbage at one time and short herbage at another, but as far as possible cut at the same **stage of growth.** Rate of growth trials are exceptions to this rule.

Special Points

Make frequent checks on height of cutting. On wet, soft ground mower wheels are apt to sink and to give more close cutting than when the ground is hard. A standard cutting height **must** be adhered to: under no avoidable circumstance should the height of cutting vary from one mowing to the next.

Use **the same mower** for each cutting of the trial. Mowers for experiments should never be used for general-purpose work, but should be kept sharp and adjusted and set to the standard height of cutting.

Both frequency of cutting and height of cutting affect total pasture production and the species composition of the sward. Overseas work and trials at the Marton Experimental Area have shown this clearly. The main thing to avoid is cutting too closely. Try to copy the type of defoliation of good grazing management and aim-to measure a grazing sward rather than a lawn.

HOW TO MEASURE THE BOTANICAL COMPOSITION OF PASTURES

In many pasture trials we need a measure or estimate of the species composition to understand fully the effects of fertilisers, seeding rates, weedkillers, or other treatments on the pasture. Yields alone may not be sufficient, as we are usually interested in the quality of the herbage as well as the quantity. In pasture surveys of districts the species composition has to be determined to find what pasture types occur under the different conditions of climate, management, aspect, and fertility, and to see whether there is a predominance of one kind of species under a certain set of conditions.

To measure the species composition we use a method of botanical analysis to find out what species are present and the proportions of each in the sward; that is, a list of species and some quantitative data. There are a number of methods of analysis, and the choice of method depends primarily on the type of pasture (open or dense) and secondly on whether we want information on **frequency of occurrence** of the different species; **number** of different species; **area covered by** or cover of the different species; or **weight** or bulk of the different species, or perhaps a combination of two or more of these standards of measurement.

Whatever method we use, reliability and rapidity in identifying plants are essential and some process of sampling has to be used, as it is not possible to examine every plant in a plot or paddock.

Sampling

We have a **frame**, a **line**, or a **point** to assist us in sampling.

1. **Frame:** The area enclosed by the frame is analysed. The frame may be rectangular, circular, or square. When it is square it is called a **quadrat.** It can be large or small to suit the circumstances; usually the denser the pasture is the smaller is the frame. The frame can be subdivided into a number of small areas by cross wires to form a grid or mesh. By analysing only some of the small squares out of the frame more positions of the frame can be taken instead of spending the time on examining the whole area enclosed within the frame and thus taking fewer placings. Generally a large number of small samples is preferred to a small number of large samples.
2. **Point:** If the size of the frame or quadrat is further and further reduced, it will ultimately become a point, which is the smallest plot or area possible. Dr. L. Cockayne recorded the plant present at the point of his shoe on tussock country; the point of a meat skewer was used to record weeds in a tea plantation in India, and knots in string have been used as points.

The point quadrat apparatus (usually called the point analyser) consists of a frame containing a row of equally spaced points. The spacing of the points at 2-in. intervals and the number of points (10) in the point analyser have been largely determined by convenience. The placing of every one of 100 points separately would take much longer than recording 10 points at each position.

3. **Line:** A transect is another name for a line. The point analyser represents a very small transect. A line may be placed diagonally across a paddock or down the slope of a hill over hollows and humps. It is a useful sampling unit for covering a large area of ground which varies in topography, aspect, fertility gradient, and so on. Records can be made at equally spaced intervals along the line or can be taken at random. We can sample with a quadrat or a point analyser along a line.

Some Methods of Botanical Analysis Applicable to Agricultural Experiments

Method	Type of in-	Uses

	formation	
1. Eye estimations and refinements of eye estimations with a quadrat.	Cover Space occupied = cover x bulk. Weight.	More suitable for recording considerable differences for observational unreplicated trials, and for notes accompanying mowing yields. Preliminary surveys.
2. Counts of plants	Number	For the establishing stage of pasture or for a tussock sward where vegetation is relatively sparse so that the identification of individual plants is easy. Useful in trials studying seeds mixtures, strains, and control of weeds.
3. Tiller counts	Number	Useful for comparing strains, rates of seeding, and seed mixtures where interested in effect of different mixtures on one species. Very time-consuming method.
4. Point analysis	Cover	Particularly useful for dense swards. Unsuitable for tall grasses such as those in some tussock swards. Records changes over the years and can measure small differences in species composition.
5. Loop or ring method	Cover	Open tussock swards to record changes over the years.
6. Line intercept method (basal area of plants along a line or narrow belt)	Cover	Useful in open, sparse pastures such as tussock country where identity of plants is easily distinguished.
7. Photographs	Cover and bulk	Useful for recording big differences in species composition and for tracing major changes in composition over the

		years.
8. Herbage dissection (= persantage productivity)	Weight	For dense pastures; most suitable for estimating small differences in species composition.
9. Specific frequency	Frequency	For surveys and for comparing pastures of different types.

When making a botanical analysis of a sward consisting of plants of very different sizes, the same method need not be used for both the small and large plants. In a ryegrass-clover pasture containing thistles the point analysis method could be used to estimate the effect of weedkillers on the pasture species and the number of thistles could be counted. Similarly in tall tussock country the *Danthonia* tussocks could be counted and the ring method could be used to estimate the cover of the herbs.

Specific Frequency

Specific frequency, which may be defined as the number of times a species is present per 100 readings of a frame, is of limited use in agricultural studies, as it gives us a list of species and information on how they are distributed, but no quantitative information. A species occurring as a trace only in each frame would be recorded as many times as a species occupying practically the entire area of each frame. This method approaches a quantitative one the smaller the frame is and the more placings that are taken, and then becomes more applicable to agricultural experiments.

As different methods of botanical analyses define species composition in different ways, the results may not necessarily be comparable. A good example is point analyses and herbage dissection analyses. The first gives the cover and often records more low-growing, prostrate species that are mainly below mower height and missed from the mown herbage used for dissection analyses. Flat-bladed species are usually more prominent in point analysis data, as they cover a greater area of the ground than erect-growing, fine-leaved species.

When we want to obtain a good picture of the differences in species composition in a trial we quite often employ more than one method. Tiller counts, point analyses, and herbage dissection analyses may all be used in a pasture establishment trial comparing seeds mixtures, and basal cover measurements, counts, and photographs in a management trial on a tussock sward.

Eye Estimations

These are considered in Part 7, page 97.

Counts

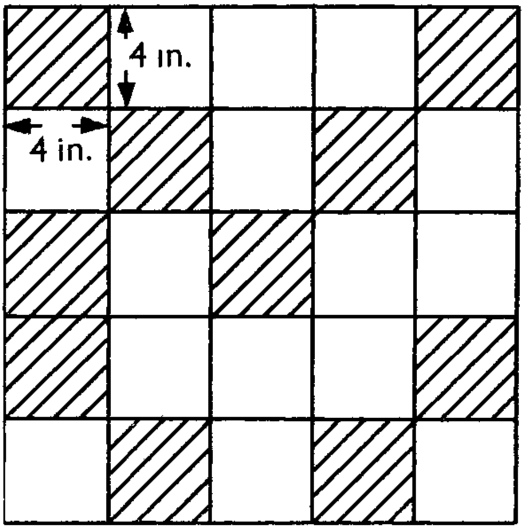
If the trial area has been sown broadcast, a small quadrat is the most convenient sampling unit.

When uniformity data such as in weed control trials are being obtained the sample areas may be permanently marked and later recounted. The effects of the treatments can then be expressed as the percentage survival of plants.

In the establishing stage of a pasture the strike of sown species is sometimes patchy and weeds often occur in groups. In this case the more sampling positions the better, and the use of a smaller sampling area is necessary. The quadrat can be subdivided and only certain of the small squares within the quadrat used. The small squares can be chosen according to a pattern such as is shown in the diagram at right.

Whether all squares within the quadrat are counted or not the subdivision assists in counting, especially if two species are being counted.

For a drilled pasture a certain portion of the row can be counted, preferably several short lengths of a row. Where the interest is in weeds both the row and the area between have to be sampled, and again the quadrat can be used and a selection of small squares made within it according to a pattern.



A 20 in frame. Only the shaded areas are counted.

Tiller Counts

These usually have to be confined to either end of a plot, as it is easiest to remove a turf and dissect it into tillers in the laboratory. A plug sampler, say of 3-in. diameter, is a useful means of taking cores of turf for tiller counts.

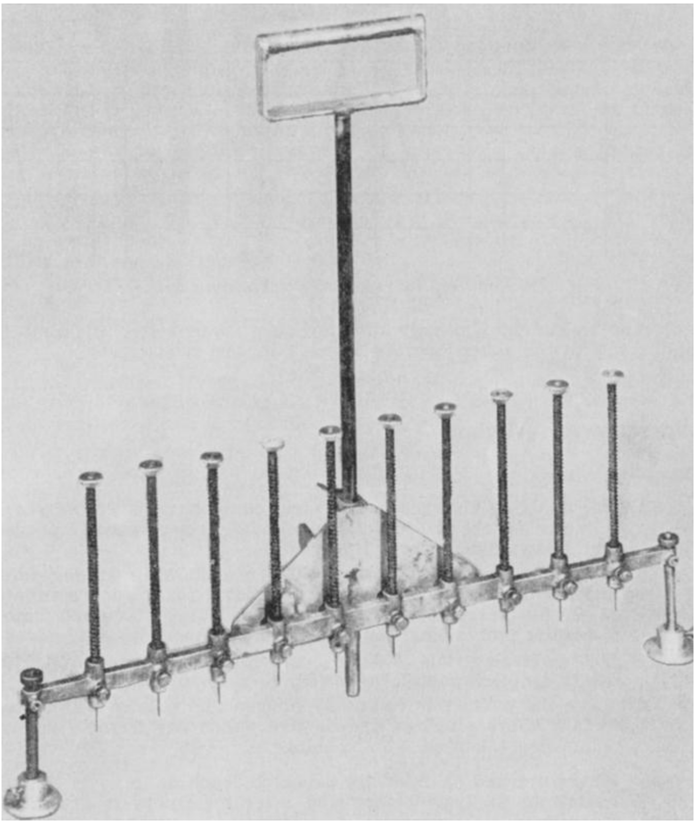
Point Analysis (=Point Quadrat Method)

This method estimates the amount of ground surface covered by the leaf spread of the different species. The equipment used is illustrated on the opposite page. Each needle is pushed into the sward and as it descends all species touched by the point are recorded on specially prepared sheets. The needle may first touch a white clover leaf, then a perennial ryegrass leaf, then another white clover leaf, and finally a *Poa trivialis* leaf. These three species are recorded, the white clover twice, as covering that point of ground. If the needle misses all vegetation and touches only bare ground, one hit of bare ground is recorded.

Method of Recording

Sheets are drawn up (such as the one illustrated on page 90) and these save much time in the field. The recordings can be made so that information on the number of hits per needle, number of hits per frame, or only number of hits per plot is obtained. For most trials, unless a sampling investigation is being carried out, hits per plot are sufficient. The "strike" system has proved a convenient method of recording data where totals per plot or frame are sufficient, and it is easy to read later when summarising results. Instead of recording the fifth hit we cross out the preceding four to indicate five hits.

Two lines are used to record the hits for each species. On the top line the **total hits** are recorded—that is, the number of times the species was touched by the needle. On the lower line the **cover hits** are recorded—here the presence of species is recorded, but not the number of layers.

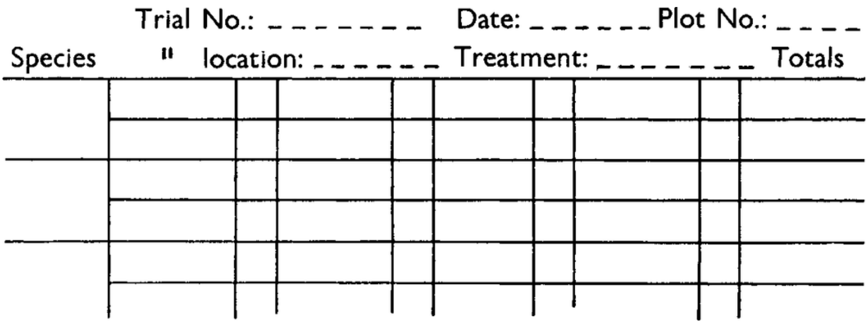


A pasture analyser in the working position.

For example, results may be recorded as:

White clover	**11** **1** —	**If a further three hits were recorded of white clover on the next needle, the totals of white clover would change to—**
Perennial ryegrass	**1** **1** —	~~1 1 1 1~~ 11
Poa trivialis	**1** **1**	

From both cover and total hits a variety of information on the different species is obtained from the one analysis. This method of analysis is most useful when we want to examine the effects of different treatments on individual species, irrespective of what is happening to the other pasture species in the sward.


Trial No.: _______ Date: ______ Plot No.: ____
Species " location: ______ Treatment: ________ Totals

The form used for recording point analysis data.

Operation of Method

Sampling

(a) For studying changes in botanical composition over the years a line or an area 20 in. x 20 in. should be **permanently** pegged and re-examined each year.

(b) If mainly interested in the difference showing at one time and not in changes, 100 points per plot gives a good estimate of the species composition, the frame being placed 10 times along the plot, with about 2 to 3 ft between placings.

(c) For paddock trials 400 or more points are required, 100 points for each sample area (20 in. x 20 in.).

(d) When the pasture is relatively uneven and patchy it is better to take more placings and do five points per frame—that is, points are taken at 4-in. spacings.

(e) Where marked changes are expected (such as in weed control trials) an analysis **before** and **after** treatment is desirable.

(f) The same number of points should not necessarily be taken on all treatments. For example, in a weed control trial there may be a big difference between the control and treated plots, which can be accurately measured with 100 points, but an extra 50 points may be necessary to determine the difference between the weedkillers.

(g) Each observer should point half of each plot or sample area. In this way any observer difference does not affect treatment differences.

(h) When pointing a new trial check the adequacy of the number of points by doing a second set of pointings on, say, two treatments, and compare the two assessments. Aim to have agreement within 5 to 10 per cent for the main species.

(i) A sketch map of permanent sample areas should be made, and the distances of two pegs from the fence or end of plot recorded to assist in relocating the area.

(j) If possible, point all plots of a replicated trial so that the point analysis data can be statistically examined.

Time of Analysis

Where changes in composition are being measured over the years (for example, in trials studying the persistence of various species and strains under different grazing treatments) the analysis should preferably be carried out in early spring after the onset of clover growth. The least variation in composition due to differences in seasonal growth conditions occurs at this time of the year. If this is not done in spring, wait until late autumn.

Where the effects of weedkillers are being measured an analysis should be made within four to six weeks after spraying. This can be followed up by a later analysis to obtain information on recovery of the species. In pasture renovation trials, where chemicals are used to destroy the old sward, and one assessment only of the results of oversowing is to be made by point analysis, the analysis should preferably be carried out a year after sowing. For other trials time of analysis will depend mainly on the time of year differences in species composition are most noticeable by eye. Pasture establishment trials on ploughed land with various mixtures and rates of seeding should be pointed if possible within six months of sowing and again a year later.

The pasture should be mown or grazed short to 2 to 3 in. Sample areas in a paddock can be mown a few days before analyses are made.

Other Notes

Record only the species touched by the **point** of the needle.

Species touched by the side of the needle should not be recorded. Needles should be sharp and the analysis done preferably on a calm day.

A folded leaf is counted as one hit. A dried, dead-looking leaf (such as Yorkshire fog) is counted as alive provided its base is green. If much dead litter occurs in the sward, it must be recorded as a separate category.

Herbage Dissection

This method of analysis is based on the dry weight of each species. A sample of fresh herbage is separated into the various species. Each separation is weighed and dried in the oven. The dry weight of each species is recorded next day, and later these weights are totalled and the percentage amount of each species in the sample is calculated. Thus we get an estimate of the relative proportion of the dry weight of each species growing above cutting height. The seasonal and total production of the various species can be calculated from the total dry matter yields.

This method of analysis measures small differences in the species composition of the sward, and as the size and vigour of the different species is taken into account, it evaluates the agricultural importance of the pasture species.

Selection of Field Samples

Approximately ½ lb of herbage is sufficient for a field sample.

Mown Samples

Take several handfuls of herbage from **different** positions in the catcher.

When preparing a bulked sample per treatment make sure that all plots or frames share equally in the sample.

Hand-cut Samples

Some method of sampling is necessary, as it is easy to be biased when selecting herbage for hand-cut samples. A low growing patch will be avoided and a clumpy patch of herbage chosen. Either use a line with knots in it stretched from end to end of the plot and cut the herbage below, say, 5 knots, which are spaced 2 to 3 ft apart, or throw down a small quadrat three to four times along the discard strip (at every two to three paces) and cut the herbage within the quadrat.

Hand-cut samples are often required during slow-growing periods of the year, when the very short herbage cut with the mower is difficult to separate. With trials under the frame technique it may be necessary to take the hand-cut samples from just outside the frame (one from each side) during periods of very slow growth, as the area outside the frame has not been trimmed the previous fortnight and the herbage is longer. Samples from outside the frame can only be taken at periods of the year when growth is very slow, otherwise the true picture of rate of growth of the different species would not be obtained.

All herbage cut by the shears should be carefully collected and care taken not to cut lower than the mower. Duplicate field samples should be taken occasionally to check on the adequacy of the hand-cut sample. This also applies to mown herbage.

Dispatch

When samples have to be sent by post, rail, or air to a dissection centre perforated plastic bags should be used and samples packed loosely in a carton. The label for each sample should either be inserted after the first fold of the bag or tied on to the bag to avoid its becoming sodden and illegible.

General

The main source of errors is in sampling whether in the field or in the laboratory, and care should be taken to select a sample that is as representative as possible of the treatment. The size of sample in the laboratory is chosen according to the complexity, length, and coarseness of the herbage. Variability in the dryness of material should also be taken into account. A few dry, stalky pieces can cause quite an appreciable difference in the percentage amount of one species. Results should show a 5 per cent difference in a major species to be statistically significant.

The species composition as estimated by herbage dissection is not likely to agree with eye estimates. This is because the results are given as percentages of dry matter, and species differ in their dry matter content. Flat weeds, which are mainly below mower height, are easily seen, yet form a small proportion of the cut herbage.

Part 7 — How to Observe Pasture Trials

Both observations and measurements may be used to assess the effects of treatments. Where measurements are possible they are usually to be preferred to observations, though the economic use of time, labour, and facilities always has to be considered. However, not only are measurements unsupported by observations of very doubtful value, but in many cases observations are either the most practical means or they may be the only means of assessing treatment effects.

It is, therefore, essential to consider methods of taking observations and to evaluate their usefulness and limitations. In all cases the basic weakness of any observation is that it depends on the observer's judgment, and human judgment is not infallible. Bias is a constant danger and the eye can be deceived by many things. Observational methods must, therefore, aim to reduce observer bias to a minimum and where possible be designed so that the reliability of observations may be evaluated. Bias is largely avoided if plots are observed without knowledge of treatments applied, and to assess the reliability of observations both the trial design and the method of observation must allow of valid statistical analysis of observational data.

LIMITATIONS OF THE OBSERVATIONAL METHOD

Some of these limitations are listed below:

1. Inability to recognise fine differences: it is more difficult to see differences on high-producing swards. It is useless, therefore, to compare treatments by observation when small differences are likely; for example, rates of phosphate trials on highly productive

land.

2. What may be obvious to the eye may have little or no effect on production; such as colour differences, though in many cases such effects are also reflected in production differences at some future date.

3. Difficulties of scoring on different types of pasture: generally it is easier to see differences if the control plots are poor. However, no responses may occur if no clover is present.

4. Difficulties of judgment as to what constitutes a response: relative weight to give to different factors, such as changes in sward composition and in amount of growth and freedom from weeds.

5. State of pasture unsuitable for observation: overgrazed pasture may never show a response. Observations at low-production periods may reveal no differences from treatments which show responses at other times of the year.

6. Differences between the ability of observers to see and to evaluate treatment effects. This is a fundamental weakness of all observational methods.

OVERCOMING THESE LIMITATIONS

In many cases it will be noted that we are **comparing** the observational method with production measurements, but it cannot be taken for granted that production measurements will necessarily give the complete answer. Small differences may be equally impossible to measure with precision as to observe. Furthermore, production measurements cannot evaluate many factors, such as colour differences. The two methods are in many respects complementary and that is why measurements without observations are often of restricted value and may easily be misleading.

1. Direct Estimates of Production

In the first instance we will confine our attention, therefore, to observational methods which enable us to assess **production**. Some preliminary work indicates that, with training, direct estimates of pasture production are possible and give data reasonably comparable with production measurements on the same trials. It is probably more difficult to assess absolute values of production for treatments, but in several pasture trials treatments have been placed in the same order as far as production is concerned by both observation and measurement.

Direct estimates of production may be useful to observe pasture trials of various types, but this method has not been sufficiently well tried to be advocated generally with confidence. Production may be estimated either as total annual production (say in thousands of pounds of dry matter per acre) or in terms of the herbage present at the time the observation is made. It is probably necessary for the persons making the observations to be taking measurements on some mowing trials as well so that a constant check may be kept on their ability to do production observations.

One of the real advantages of this method is that, provided the experiment is of satisfactory design, these production estimates may be analysed statistically and their reliability measured. Bias can be avoided if each plot is assessed without knowledge of the treatments applied to that plot. This should be done with all methods of observation.

2. Fertility Index

The Department of Agriculture has for some years been using a scale of "fertility index" as a measure of pasture quality and production for use with advisory soil testing data. The scale has also been used for taking observations on pasture experiments and appears to be a useful observational method.

"Fertility index" is designed to indicate the general soil fertility conditions as reflected in the thrift of the pasture.

The range of figures is from 0, representing an extremely poor, unthrifty pasture, to 20, which indicates an extremely vigorous, high-producing pasture. Within each class a range of fertility index numbers is available. This range varies from class to class. For example, in class 0 the range is very limited, being 0 to 2. On the other hand in class 8 the range is wider, being 6 to 20.

This allows for the complete range of 0 to 20 being used as a general indication of the type of pasture apart from the class. For example, any pasture with a fertility index figure of, say, 7 will be regarded as comparatively poor, and similarly, any pasture with a figure of 15 will be regarded as good. The overlapping that occurs is desirable. Thus it is possible to have a thrifty class 7 pasture with a fertility index figure of 12 and an unthrifty class 8 pasture with a fertility index figure of 10.

All types of pasture are not provided for. For example, pastures with strawberry clover or alsike are not classified. Similarly, a pasture that is dominant and almost pure ryegrass is not allowed for. It obviously could not be placed into class 1, but could still be given a high rating for fertility index.

The following gives the details of the classes of pasture and the range of fertility index figures allocated to each class:

Class	Main clover species	Main grass species	Fertility index
0	Haresfoot trefoil or only insignificant amounts of annual clover. (No perennial clover.)	Tussock, danthonia, browntop, chewings fescue, hairgrasses, microlaena.	0 - 2
1	Suckling, striated, clustered, *Lotus hispidus*, trefoil	Tussock, danthonia, browntop, chewings fescue, vernal, fog, ratstail, hairgrasses, microlaena, and sod-bound paspalum.	1 - 6
2	Subterranean	Tussock, danthonia, browntop, chewings fescue, fog, ratstail, hairgrasses, microlaena.	2 - 8
3	Subterranean	Ryegrass, dogstail, paspalum, timothy, phalaris, barley grass.	6 - 15
4	*Lotus penduncululatus* (*major*).	Danthonia, browntop, chewings fescue.	3 - 6
5	*Lotus penduncululatus* (*major*).	Ryegrass, dogstail, cocksfoot, paspalum, timothy, fog.	5 - 15
6	Red	Goosegrass, hairgrasses, fog.	5 - 10
		Ryegrass, cocksfoot, paspalum, timothy.	6 - 20
7	White	Tussock, danthonia, browntop, hairgrasses, fog, ratstail, microlaena, dogstail, paspalum (sod-bound), barley grass.	3 - 13
8	White	Ryegrass, cocksfoot, paspalum, timothy, prairie, fog (vigorous).	6 - 20
9	Lucerne		5 - 20

Fertility index assessments thus reflect both sward composition and production (or “thrift”). In practice it has been found that these two are closely related and it is therefore considered reasonable to apply statistical analysis to such data. The method can be applied to a variety of types of trial, including both pasture top-dressing trials and pasture species trials.

3. "Response Pointing" on Pasture Fertiliser Trials

Until recently the usual method of assessing responses to fertilisers on observational pasture trials by the Department of Agriculture has been by the following scale of differences from the control or "no treatment" plot. The control plot may, of course, have received some basal treatment common to the whole trial.

Response point	Nature of difference observed from control
0	None
½ or ?	Questionable
1	Questionable
2	Fair
3	Good
4	Very good
5	Excellent

Half-points are used; thus 3½ is a "good to very good" response.

There are several points of interest in this method of making observations.

1. The "difference" which is "pointed" may be an expression of one or more of many things such as production, sward composition, or general appearance. The aim is to observe **responses** and to try and evaluate them in terms of the manner in which they are seen. Thus a marked increase in the more productive sward components, particularly clover species, receives more weight than colour changes, which are, at times, particularly difficult to interpret. It is obvious that **standardisation** of observers and their training in pointing is essential if the scheme is to operate successfully. The successful use of this pointing system over some 25 or more years has resulted in sufficient numbers of trained observers being available to operate the scheme and to train new observers in the method. Constant watch must be kept on the standardisation and efficiency of observers.

Because the response point may reflect a difference in one or more of several things, it is essential to note in words just what feature has been pointed.

2. The control plot is always given “0” regardless of its condition. It may be a weak, unthrifty pasture or a vigorous, productive sward. It must, therefore, be described in some detail. With fertility index and production index pointings the control plot is marked as one of the treatments.

3. Because treatments are each compared with the control, the position of the control plots must be known and there is a danger of biased observations because of this factor. Attempts to overcome this defect by pointing differences from the poorest plot (which is given 0), or from some fixed plot, are worth further consideration.

4. Mainly because the linearity of the scale is not known (that is, whether a difference of one between 1 and 2 is the same as one between 4 and 5, for example), it is most doubtful whether response points should be analysed statistically, even if care has been taken to avoid bias by some means such as those suggested in the preceding paragraph. The reliability of the observations is, therefore, difficult to assess.

5. Differences between plots are often judged along their boundaries and some observers pay special attention to these edge effects rather than the whole plot. A clear line of demarcation between neighbouring plots is certainly a good sign that differences exist between those plots, but there is a danger that, owing to inaccuracies in applying materials which often result in heavier application along the borders of plots, such edge effects may be misleading at times. Judging the whole plot rather than the edge only is to be preferred in most cases.

Despite these limitations, this pointing system has been used successfully to assess treatment effects in many thousands of observational topdressing trials. It is important, however, to appreciate its limitations and defects. It should be regarded as a simple and abbreviated method of describing a **response to fertilisers.** For the more complex types of experiments where statistical analyses of observations are to be undertaken, other methods of assessment, such as “Fertility Index” or direct estimates of production, are to be preferred.

4. Methods of Observing the Botanical Composition of Swards

In many pasture trials estimations of sward composition are required. The production value of the swards of different treatments may be assessed visually by the direct estimate of production, or fertility index observations may be made. Systems have been used whereby a 0-10 scale for various factors is used, and in one of these systems points for different important features are given as follows:

Sward value	0, useless sward	10, highly productive, dense, palatable sward
Sward density	0, practically bare ground	10, excellent cover
Density of sown species	0, nil	10, excellent cover
Vigour of growth	0, dormant, lifeless	10, exceptionally vigorous
Freedom from weeds	0, extremely weedy	10, weed free
Palatability	0, neglected	10, exceptionally palatable

This system works reasonably well and is quite descriptive of most of the various factors that are important practical features of a pasture. Additional factors can be pointed as occasion arises.

Estimating Botanical Composition by Eye

In many trials, and particularly those comparing different pasture species, it is necessary to describe the botanical composition more closely than can be given by a generalised pointing system such as the one just described. In Part 6 we have discussed methods of measuring botanical composition, but often less precise and more rapid observational assessments are adequate.

When we estimate the species composition by eye we are really assessing the **space occupied** by the different species. We not only notice the area of ground covered by the different species, but also take into account the volume or bulk of leafage, and the estimate becomes a three-dimensional one.

We can assess the major classes of species, that is, grasses, clovers, weeds, bare ground, and give a percentage value to each group or class, but if we attempt to give a percentage value to each grass species, each clover species, etc., and check each time that the sum of the percentages equals 100, it becomes a very time consuming and fatiguing process. Inconsistencies of judgment soon occur. Instead we resort to a descriptive system whereby species are described as dominant, sub-dominant, much, some, trace.

How to Estimate the Species Composition by Eye

1. Head up a field notebook with the following headings:

	Grasses	**Clovers**	**Weeds**	**Bare ground**
Dominant				
Sub-dominant				
Much				
Some				
Trace				

* B/A/W/B.G. Ratio:

Note: Dominant or Sub-dominant—Species comprises over 20 per cent of the sward

Much —Species comprises 10 to 20 per cent of the sward

Some —Species comprises 5 to 10 per cent of the sward

Trace —Species comprises less than 5 per cent of the sward

2. Walk over the plots or paddocks of one block to obtain a general impression of the main species present and the major differences in composition between plots or paddocks.

3. Then **look into the sward** of the first plot or paddock. This is best done by examining several small areas closely; that is, bend down and move the upper layers of herbage apart to notice what species are present in the bottom of the sward. It must be appreciated that for estimating the species composition a much closer

* B/A/W/B.G. refers to grasses/clovers/weeds/bare ground.

examination of the sward is required than for pointing the top-dressing response, when we are mainly interested in differences in colour, amount of growth, vigour of the herbage, and differences in the proportion of clover in each plot. Observation on the top canopy of growth is usually sufficient for fertiliser trials, but the species in the bottom of the sward are also important when the species composition is being determined. Otherwise such species as *Poa trivialis* and goosegrass may be overlooked.

4. Make your recordings of the species present and their relative dominance.

For a paddock trial sample the area by walking across one diagonal of the paddock, and at every ten paces or so place a frame, examine the sward closely, and record the species present and their dominance. Later these separate readings can be averaged for the paddock.

5. When all plots or paddocks of one block have been assessed for species composition check the plots of the second block against your field notes. If there is good agreement, no further assessments are necessary; if not, continue determining the species composition of the other blocks.

Points to Note

1. Use symbols for grasses and clovers and abbreviations for weeds, as these are quick to record.

2. The grass/clover/weed/bare ground ratio should also be given for the control or standard treatment and for the treatment judged the best. Weed grasses are entered under grass; dead litter, if present in any noticeable quantity, is given a separate entry. It is usually easiest to assess the percentage of the class providing the majority of cover first. The bare ground is usually underestimated. It occupies a greater area of the ground than is suspected by most observers.

3. If one is particularly interested in assessing the differences in species of one group—for example, red and white clovers of the clover group, or weed grasses and useful grasses—the percentage of two or more species can be estimated for all plots of a trial without one becoming too fatigued. First, estimate the B/A/W/B.G. ratio of

the control and best treatment, so that the percentage amounts of the two species are within reasonable limits. Don't try to point a difference of less than 5 per cent between plots. As standards change over the trial, it is worth going back to the first plot at the finish of the last plot and making another independent estimate. This gives you an idea of how consistent you are and helps to improve your judgment.

4. One of the best training courses for eye estimations is to do a series of point analyses. Estimate by eye the percentage of one species of several plots; then after point analysing the plots compare results and observe the plots again.

5. The advantage of **an eye estimation** is that it is rapid. It cannot be expected to reach the same standard of accuracy as much slower but more exact measurements such as herbage dissection.

5. Observing Weed Control in Pasture Trials

There are two main types of observations to make on these trials:

(a) The effect of the weed control treatment on each weed species.

(b) The general effect of the treatment on the sward composition and production.

For the latter observation one or more of the methods already discussed may be used. Thus estimates of production or "fertility index" assessments may be worth while, while frequently estimates of botanical composition are necessary. In such cases the weed elements would receive more detailed assessment than in other types of trial.

Observations of the effect of weedkillers on the various weed species need to be done carefully, and the following classification has been found to be useful:

1. Killed (examination shows roots are dead. This is especially important for perennials).
2. Apparently killed (vegetative growth to ground level killed)
3. Severely affected (foliage and stems damaged; sets no flowers or seeds).
4. Slightly affected (foliage and stems slightly damaged, but

flowered normally; seeds apparently viable).

5. Affected but recovered later.
6. Unaffected (no sign of any damage; flowered and seeded normally).

TIME OF MAKING OBSERVATIONS

With weed control trials, particularly, frequent observations are necessary following treatment. The first should be within two weeks of application, and, usually, monthly observations should follow for a season at least. In most other trials at least one observation should be taken each season, so that an interval of three months between visits is usual. There are many exceptions to this, however. Newly sown pastures should be inspected frequently in the critical early stages of establishment and special visits are required for special types of trial.

In production trials regular visits are necessary because regular measurements of the trial are required. It is all the more important in observational trials that observations are made at the time when the trial best shows the feature being studied. Every effort should be made to ensure that the trial is in a good condition for observation when required. This may entail fencing or the use of cages to exclude stock from those portions of the trial being studied intensively.

6. Observing Insect Control Trials on Pastures and Crops

Before successful trials can be undertaken it is necessary to have a thorough knowledge of the biology and ecology of the insects concerned. Unfortunately, this knowledge is not always adequate for some of the major pests of pastures and field crops. This information is of greater importance when less persistent short-term material such as organo-phosphates and carbamates are used than when the long-lasting organochlorine type insecticides are used.

Site Selection

A knowledge of previous insect infestations on the site is often helpful. If materials of short residual life are used, the insects must be present at the time of application. When the use of organochlorine insecticides is permitted prophylactic treatments are desirable. Population density of insects often varies considerably over paddocks. With mobile insects it may be impossible to estimate numbers by sampling before the trial is laid down. Even with relatively static populations such as soil-inhabiting grubs, large numbers of samples are required to estimate numbers. Too few samples may give misleading results.

The more mobile the insect the larger the plot size needs to be. This usually implies that fewer replications are possible. With less mobile insects plot sizes may be smaller (1/160th of an acre is satisfactory in many cases). Because of the high variability of insect populations, more replications of treatments are required than in most other types of field experiments. Even the most carefully chosen sites may later prove to be unsatisfactory, and it is sound to lay down a sufficient number of trials, which will enable unsatisfactory sites to be discarded.

Trial Evaluation

The two main types of observations made on insect control trials are:

(a) The effect of treatments on the control of the insect pest.

(b) The effect of treatments on the botanical composition and production of the pasture or on the quality and production of the field crop.

Observations made under (a) mainly depend on the type of insect treated and the type of material used. For organochlorine insecticides used on soil insects, sampling before and after treatments is satisfactory provided it is done adequately and at the right time.

When materials of short residual life are used or when surface feeding insects are treated, especially with wettable powders and emulsions, the effects of the insecticides are usually seen immediately. Counts may, therefore, be taken shortly after application. The sooner counts can be taken the better, especially if birds are liable to consume affected insects. For example, trials on some insects may be laid down in the late afternoon during a warm period and the dead caterpillars counted very early next morning before birds are active.

With slower-acting materials the insect may reach cover before death; for example, some soil-inhabiting caterpillars could return to their burrows. In this case soil sampling should be done within 48 hours, as decay of dead insects is rapid.

Where assessments are made of the pasture or crop treated it is desirable to measure yields wherever possible. For pasture trials botanical analysis of the sward is often necessary, as some insects are more severe in their action on particular pasture species. Any side effects such as death of earth-worms and thriftiness of turf should be observed. The time of making assessments is less critical and may extend over several months.

Part 8 — How to Measure the Yields of Crops

With most crop experiments it is reasonably easy to secure a measurement of yield, and observational data, though important, find their main use not in estimates of production but in qualifying the yield data and in describing important agronomic features. Apart from some forage and horticultural crops, only one harvest is involved and we are not concerned with the effect of the harvesting method on the subsequent growth of the crop. This contrasts with the position in grassland experiments, where the measurement data secured depend very largely on the method of measurement and where a continuing growth is being harvested.

It is most convenient to deal with groups of crops according to the method of harvesting. Horticultural crops fit into some of these categories, but are not considered in detail in this bulletin.

The following will be considered:

1. Cereals, peas, linseed, and linen flax
2. Maize
3. Potatoes
4. Root crops
5. Forage crops (rape and kales)
6. Cereal green feeds
7. Lucerne
8. Vegetables and berry crops

BORDER AND COMPETITION EFFECTS

All experimental plots, whether on crop or pasture, are subject to interference from border and competition effects along their edges. The border effect is most clearly seen in crop trials where plots are separated from each other by a gap. In such cases the plants growing on the edges of the plots often are more vigorous and productive because they benefit from the freedom from competition in the gap. Occasionally the reverse occurs: weed growth in the gap may adversely affect the growth of the plants on the outside of the plots.

Other types of interference along the borders of plots may arise from the movement of fertilisers or the spread of plants from neighbouring plots. Competition effects are usually considered to be interference from plants in adjacent plots. Thus a tall-growing variety may adversely affect the growth of a shorter variety in the next plot. Border and competition effects have been studied in some detail in wheat crops (Miss J. G. Miller and N. S. Mountier, New Zealand Journal of Science and Technology, Vol. 37 (Sec. A), No. 4, 1955). Where possible, and where these effects are substantial, the measurements of yield and other data should be restricted to the central rows of plots and the plants along the edges omitted from the harvested material.

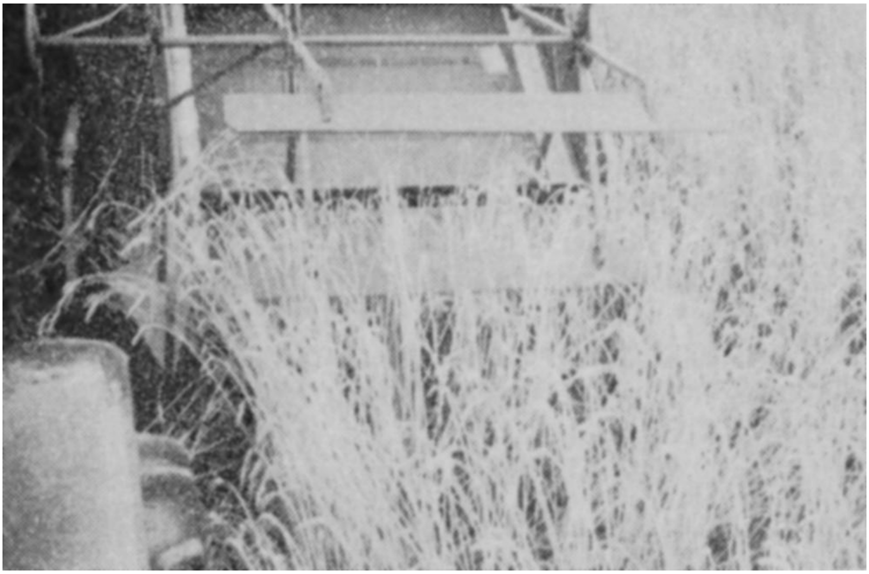
MEASURING CROP YIELDS

1. Cereals, Peas, Linseed, and Linen Flax

Trials with cereals, peas, linseed, and linen flax can be considered together, as they are usually sown in drill-strip layouts and harvested with the header harvester or with a reaper and binder (or with a pulling machine for linen flax).

(a) Harvesting with the Header Harvester

It is essential in all trials, where possible, to eliminate the outside drill rows from the plot harvested for yields. Trials have shown that the outside-row effect can be very great and variable, particularly in responses to differences in the width of the inter-plot gaps. With a poorly drilled trial, therefore, harvesting the outside rows with the plot is likely to result in a big increase in the experimental error and a trial of much reduced value in consequence. With a well drilled trial, on the other hand, the outside-row effect, though still great, is less variable and the precision of the comparisons is less affected.



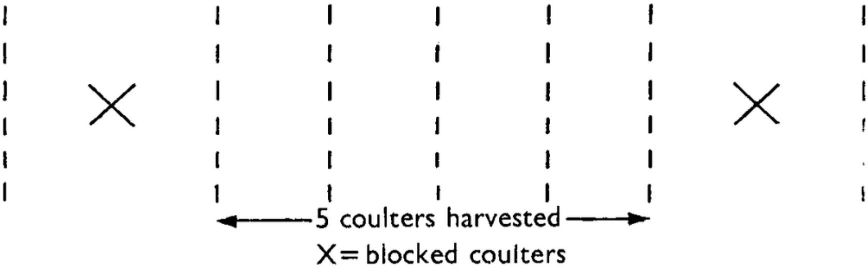
Header harvesting cereal trials—Close-up of a header in a plot.

This has led to the development of the “blocked coulter” technique for sowing. With 9-coulter drills and the use of the blocked coulter method of sowing, the 5 centre coulters may be harvested, as shown in the diagram at top of page 104.

By such means the same quantity of seed and fertiliser will be required as for 7-coulter drills. Nine-coulter drills are not too difficult to handle on trailers or trucks.

Header harvesting a lodged and tangled plot. Here all drill rows must be harvested.

The blocked coulter technique merely standardises the outside-row effect. The gap between plots is constant and the same variety is on both sides of the gap, so that differential competition effects should be removed. It will give an overestimate of the paddock yield which may, perhaps, best be compensated for by considering the plot size to be (say) 6 coulters wide (or "plot plus gap") when 5 coulters are harvested.



It is essential to maintain a constant time interval between the threshing of each plot. When the plot length is 3 chains this is automatically obtained in returning down the plot to start the next (which should be a discard). If the plot length is less than 3 chains, a check should be made that the circuit time is a minimum of 75 seconds, 20 seconds of which are normally taken in the discard (see plan below).

A B = 20 seconds and at some point after B the “main flow” of grain from Plot 1 begins

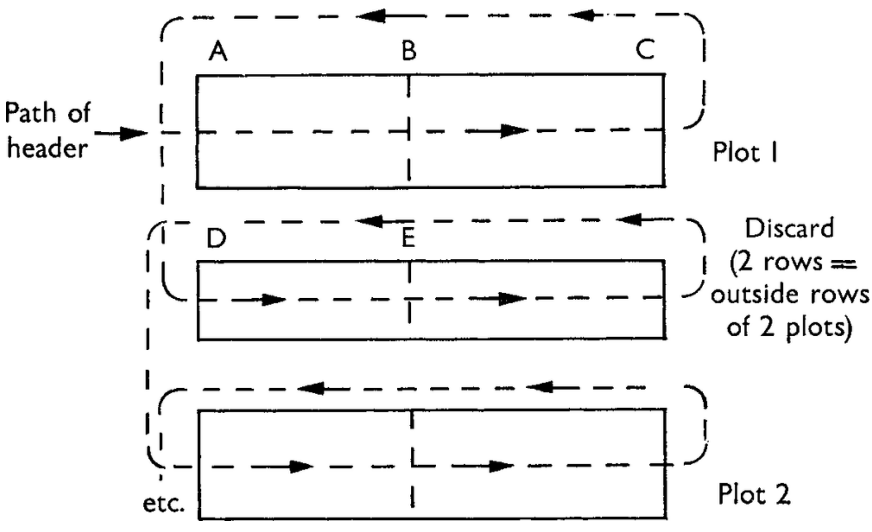
A C = 45 seconds for a **3-chain plot**

C D = circuit of 50 seconds

C E = 70 seconds. At E the main flow from Plot 1 has ceased and that from the discard has not yet commenced

B E = the complete circuit of 95 seconds. **It should never be less than 75 seconds**

The change-over should never be made **later than 20 seconds** after threshing of the plot begins.



Grain Samples

(a) Quality Tests

Samples are usually required for the baking test for wheat, malting test for barley, milling quality test for oats, and quality test for linseed. Samples should be relatively free from contamination (maximum permissible = 5 per cent), and they should therefore be taken from the grain as it is threshed from each plot. A composite sample of about 6 lb is usually required, which can readily be obtained as follows:

(i) Obtain a tin which holds about 1 lb of grain for use as a measure. Weigh accurately the grain it will hold when full for threshing each trial.

(ii) While the grain is running freely from the machine when it is threshing the plot fill the tin with grain and empty the grain into the appropriate sample bag.

(iii) **Add the weight** of grain taken to the plot weight when recording this.

(b) Samples for Moisture Test

Samples for moisture test should be taken (in suitable airtight containers such as tobacco tins or polythene bags made airtight by secure tying) from the grain which has been secured for quality tests in the case of wheat, oats, barley, and linseed, and from the appropriate treatments after weighing in other crops.

General Notes

Before the trial is harvested each plot should be inspected carefully and notes taken of any important feature such as lodging, straw break, bird damage, and so on. Any plots suffering from some factor external to the trial such as bird damage or plots with coulters missed in sowing should be noted and the weights sheet marked accordingly.

Before harvesting of each plot is started the trial should be "squared up" by running the header across the ends of the plot and then the plot length measured at both ends of the trial to record the actual length of plot harvested. The weights sheet should also show the number of coulters harvested in each plot and all relevant details.

The equipment required is:

Several small bags (about the size of meat-meal bags).

One kerosene bucket.

One spring balance to weigh 60 lb.

One chain tape.

One set of tripods to hold the balance.

Twelve sighting poles.

Sample bags for grain samples (one for each treatment).

Tins or polythene bags for samples for moisture test (one for each treatment).

Sacks for wheat.

Weighing sheets and experimental plan.

Tin for securing grain sample (see under "Grain Samples" on page 104).

Labels for the sample bags, etc. (preferably already written out)

Header Harvester for Trials

The "Journal of Agriculture" for November 1948 (vol. 77, page 486) gives details of a movable point which may be attached to the divider point of a header to assist in cereal trials in the separation of outside rows of plots from the central rows which are used to obtain plot yields. Apart from this addition, no special modification to commercial machines is required for the threshing of experiments.

(b) Sampling Methods of Harvesting

The first thing to realise is that sampling must introduce a further source of error, which we call **sampling error.** We are estimating the yields of whole plots and that estimate is subject to error. **No amount of sampling will make up for insufficient replication of treatments.** This being so, too much time should not be spent on sampling poorly replicated treatments.

The second point follows from the first, namely, that in a replicated trial it is better to get **some samples from all plots** rather than to get many samples from only some of the plots.

These points may be illustrated by the following examples:

(a) A poorly replicated trial with, say, only duplicate plots of 3 treatments = 6 plots. Because you take 8 samples from each of these plots you cannot say that you have 8 x 6 = 48 plots and 16 replications for your 48 samples. All you have is a poorer measure of 6 plots than would have been the case had the whole of the plots been harvested.

(b) A well replicated trial of 8 replications of 6 treatments = 48 plots. One sample (if properly taken) from each plot (= 48 samples) will now give you a poor measure of **48 plots,** but statistical analysis is valid, though the "error" of the trial is likely to be high, as you have introduced a big sampling error.

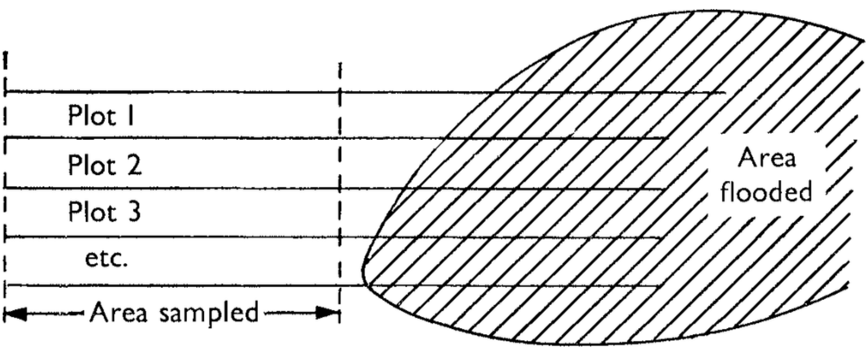
Nevertheless, if you can take a maximum of 48 samples only, it is better to take one per plot of 48 plots rather than two samples of 24 plots, or four samples of 12 plots.

Let us now consider some of the basic principles of sampling.

1. **Sampling must be at random.** That is, there must be no deliberate picking of sampling positions. It is very easy to introduce bias unless you are strict about sampling positions. Some restrictions on this random placing are in order, however, namely:

(a) **Restriction of the area sampled** over all plots of a trial or a replication: If one end of the trial has suffered from flooding during winter, you may decide to harvest only the higher end of the trial as shown above.

Similarly a plough "finish" running across all plots may not be included in the area to be sampled.



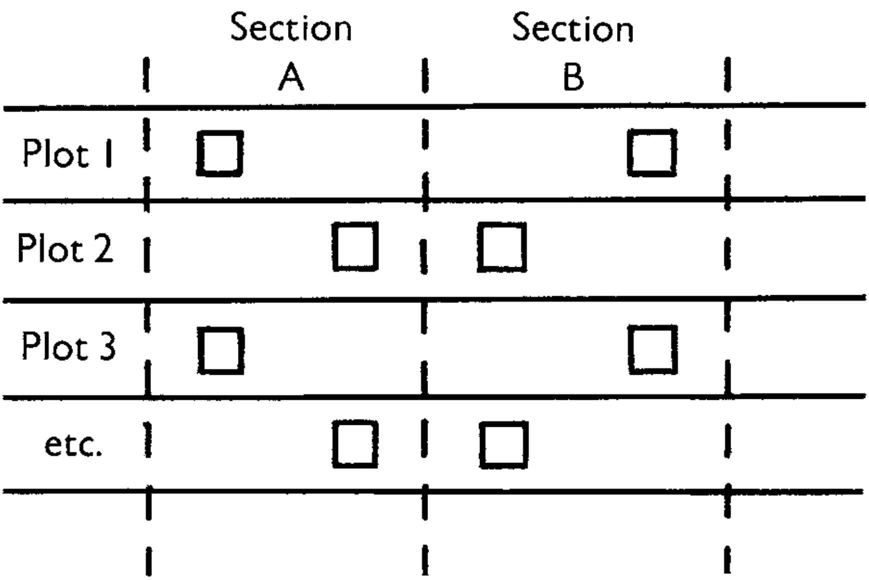
This selection must not be done on units smaller than one replication. It should be done only in those trials where you have confidence that some factor external to the trial proper is operating.

(b) **"Stratified sampling":** This is a device to get more representative sampling without losing randomness. Just as we use experimental designs to get some measure and some elimination of effects due to soil variation, so we can use sampling designs to ensure that soil and other sources of variation are fairly equally distributed among the samples taken in each plot.

This method operates by dividing the plots into sections and taking, at random, one or more samples in each section. Thus in a cereal trial we usually divide the plot length into four sections per chain and take one sample of 2 ft at random within each section (see diagram below).

If each plot was sampled without stratification, by chance we would find many plots with samples grouped in various positions down the plot length. Stratification ensures that, within limits, all parts of the plot are sampled. The result is a better measure of the "whole plot" yield.

2. Sampling must be adequate: To be of value a reasonable proportion of the plot area must be sampled. What this proportion is will depend on the type of crop and soil variability and usually has to be determined by prior investigation. Generally some thing like one-eighth or one-tenth of the plot area should be sampled for most crops.



Sampling sections. The squares indicate the sampling position in each section.

3. Sampling must be representative of the whole plot. Stratified sampling is one means of ensuring this. Samples must, however, be taken in such a way that they represent a fair sample of the plot.

4. In most cases at least two samples should be taken per plot. This enables a measure of “sampling error” should that be required. Where it is convenient to do so the yields of each sample should be given separately; however, if the sampling method has been thoroughly examined in earlier trials, bulking of samples on a per plot basis may be adequate. This is the case in cereal trials.

5. The number and size of samples and the method of sampling must be clearly reported.

Sampling of Cereal Trials

Technique experiments have shown that four sub-samples per chain of plot length, each sub-sample being five drills of 2 ft, constitute reasonable sampling for cereal trials. Details of one method used are as follows:

Apparatus required:

(a) Two sickles.

(b) Binder twine and labels for tying and labelling sheaves At least 100 labels will be required per trial.

(c) Sighting poles.

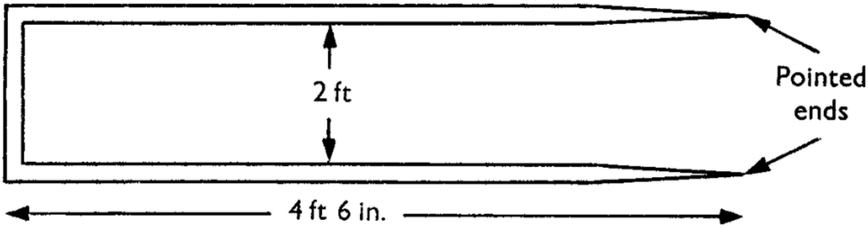
(d) Tape measure.

(e) Hessian bags for bagging sheaves. The number required will be **twice** the number of plots in each experiment.

(f) Metal “samplers” for measuring off the sampling unit.

Sampling of cereal trials—Deciding on sample positions, using random numbers.

A useful sampler is one made of piping, 2 ft wide and about 4 ft 6in. long (see illustration below):



Preliminary Work

(a) Prepare a **clear plan** of each trial area, number each plot (for example, 1 to 48), indicating the end of the trial from which the numbers commence, and make a key relating each number to the variety or treatment concerned.

(b) Each trial should be allotted a letter (as A), and a set of random numbers from 0 to 7 should be prepared for each plot.

Time of Cutting

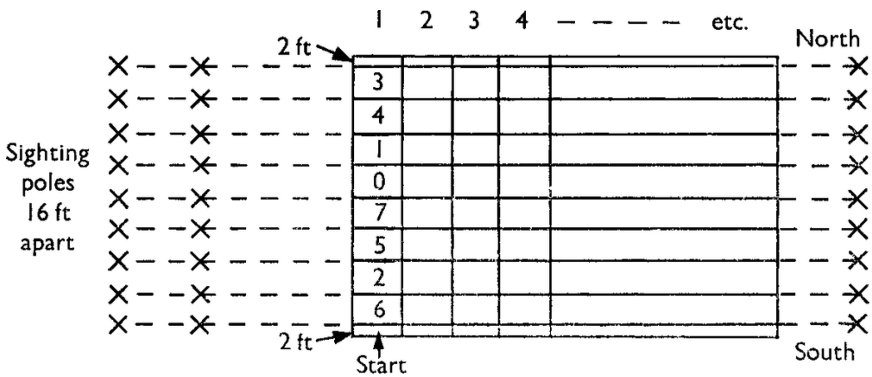
If possible, cutting should be delayed a day or two past the binder-ripe stage so that the grain shall contain as low a percentage of moisture as possible. Early-maturing varieties and treatments should be sampled **when ready** and thus reduce loss of grain by shaking or by birds. The sampling of each such treatment should be completed on the one day.

Method of Cutting

Erect sighting poles as in the plan below.

The plot length is divided into sections each of 16 ft, 2 ft being allowed at either end for irregular finishes in drilling.

The random numbers from 0 to 7 allotted to each plot (for example, as in Plot 1 in the plan below) are used as follows:



Plots

Starting at the point indicated (random number 6), pace six **short** steps of 2 ft each toward the far end of the plot. A metal sampler 2 ft wide is then inserted across the drills at the point reached. Thus the sample takes in 2 ft of the plot length from the twelfth foot to the fourteenth foot from the line of the first sighting poles. At this point the **5 centre coulters only** (in a 7-coulter plot) are cut with a sickle and the sheaf is left in position. In the next section (random number 2) two paces are taken and the procedure is repeated for all the sampling sections and for all plots in the trial. The sheaves are gathered to one end of the trial, tied, and labelled. In heavy crops it may be necessary to make two sheaves.

Sampling of cereal trials—Cutting the sample sheaf, with the metal "sampler" in position.

Labelling: Each label will bear the trial letter (for example, A), plot number (for example, 1), and the treatment (for example, Cross 7).

Labelling the sample sheaf.

Bagging and forwarding: To safeguard from loss of grain the sheaves are inserted head first into hessian bags, which are then tied on. Woolpacks are suitable containers in which to pack the bagged sheaves for forwarding to the threshing mill. Sample sheaves are best threshed in small mills specially built for handling individual sheaves.

Sampling of cereal trials—Bagging the sample sheaf.

(c) "Sample-sheaf" Method of Harvesting

The sample-sheaf harvesting method is particularly suitable to crops harvested with the reaper and binder (or the linen flax "puller"). The technique operates as follows:

(a) Cut each whole plot with the binder. Weigh and record the total weight of all sheaves on each plot.

(b) Select at random two sheaves from each plot and obtain their weights. These are the samples. Label each sheaf appropriately **a** and **b**, with the plot number and trial letter also indicated. On each label also put the words "sample sheaf method".

Example:

Sample sheaf method

M21a
Milford

Note: M is the trial letter
21 is the plot number (sheaf a)
Milford is the variety or treatment.

(It is most important that the sample sheaves be weighed on the same day as the whole plot yields are weighed.)

(c) Bag each sample sheaf in the normal way and forward to the appropriate centre for threshing.

How the Method Works

Consider Plot 16 of Trial C.

		lb
Weight of total sheaves at cutting on Plot 16	=	100
Weight of sample sheaf 16a	=	11
Weight of sample sheaf 16b	=	9
Weight of grain from sheaf 16a	=	1.2
Weight of grain from sheaf 16b	=	0.8
Hence total grain weight from 20 lb sheaves	=	2
Hence total grain weight from whole plot of		

$$100 \text{ lb} = 2 \times \frac{100}{20} = 10.0$$

If it is possible to thresh from the stook, whole-plot yields may be secured. The sheaves from each plot are stooked on the plot and later threshed for grain yields. Stooks should be secured as described below for linen flax.

Linen Flax

The sample-sheaf method is used with slight modifications. Whole plots are harvested with the pulling machine. Plots should first be “squared off” and the effective length measured. After pulling, the sheaves from each plot should be stooked on the plot to be left to condition. The stooks should be securely tied with binder twine, the twine being looped around each component sheaf, and a label with the plot number should be tied to the stook.

Yields

When the stooked material has dried the total produce from each plot should be weighed and recorded. In addition, at weighing, take **two** sheaves at random from each plot. Weigh each sheaf and then bag each separately, with appropriate labelling.

Forwarding of Samples

Send sample sheaves to the appropriate processing station for determination of fibre yields, fibre quality, and seed yields. Send a covering letter stating the weight of each sample sheaf.

2. Maize

For maize hand harvesting is the rule and the only satisfactory method. The question of getting yields of shelled grain, however, is more complicated. The best method appears to be:

1. Weigh the total yield of green cobs from each plot.
2. Weigh two sample bags of about 30 green cobs each, selected at random.
3. Either: (a) store these bags in a crib for drying, then shell and get the weight of shelled grain per sample, or (b) send samples to a laboratory for drying and dry weight of grain estimations.

Calculations are as follows:

Weight of green cobs per plot $= X$

Weight of two samples of green cobs $= Y$

Weight of dried shelled grain $= Z$

Percentage of shelled grain in sample $= \frac{Z}{Y} \times 100$

Weight of shelled grain per plot $= Z \frac{X}{Y}$

With relatively small plots it may be possible to store all the produce from each plot for drying and shelling. This should be done wherever possible. Maize is a difficult crop to experiment with, however, and it is unlikely that a plot would have fewer than 30 plants which are harvested for yield.

3. Potatoes

Mos t potato trials these days are harvested with the potato digger and picked up by hand. There are some points to watch here:

1. Be more than usually careful to pick up all potatoes. A recent English study of potatoes left in the field after harvesting revealed a surprisingly large proportion left behind—often well over a ton per acre.

Potato manuring experiment—General view of a trial after picking up. Bags are being labelled before being carted to the grader.

2. Be careful regarding separation of plots. This can be done in two ways: (a) dig out two plants at the ends of the rows before the digger commences in the trial, or (b) sow, say, two tubers of a distinctively coloured variety at the end of each plot.

3. Be consistent about grading. One person should be responsible for grading the trial, or if any change-over is made, it should be done at the end of a replication. All potatoes should be picked up together and grading done separately from picking. Grading should

be done according to recognised standards and plot yields given of table, seed, and table plus seed grades. For some trials yields of pig potatoes will also be required. Experienced graders are desirable and from time to time their efficiency should be checked by weighing various samples of, say, 10 tubers each. Grading errors are probably the cause of much variation in potato experiments.

4. If the trial is at all gappy, count the number of plants harvested per plot and make a special note of plots you consider have suffered unduly in yield because of poor stand.

4. Root Crops

Most root crop trials are sown in relatively large plots which will be sample-harvested for yield. The remarks on sampling made in Section 1 (cereal, etc., trials) should be read in this connection. A representative sampling fraction is two 10 ft lengths per chain of plot, each length consisting of two rows if the plot size will allow. "Stratified sampling" with two sections per 60 ft of plot length is desirable. With 30 ft per section random numbers of 0 to 10 can be used as described before for cereal trials.

Whether a drill or ridger is used for sowing or whether the trials are hand sown will depend on circumstances. In many trials farmer's equipment is used and in all relatively large trials it is, of course, desirable to have the trial sown in the same way as the paddock so that inter-cultivation with the paddock crop can be done. Accurate sowing rates are difficult to achieve and in all critical work it is highly desirable to thin to an approximately equal number of plants per acre on all plots.

It is worth while to consider the possibility of hand-sown trials and "micro-plots" with root crop trials. I suggest that a minimum number of about 30 bulbs per plot would suffice. These small plots need more replications, but may in the long run be more efficient than large plots. Sowing could, perhaps, be done by precision sowers (which are quite efficient) or by "spot" sowing. Subsequent work on the trial is reduced because there is a smaller total area to handle.

Of course, this design will not suit all problems, particularly those involving disease resistance where it may be important to have blocks sufficiently big to imitate field conditions more closely and to lessen problems of spread of disease and pests from adjacent plots. However, for some manurial trials small plots are well worth considering.

Placement of fertiliser is a critical factor in many root crop trials, not only with regard to germination injury but also growth and yield effects. For all trials other than fertiliser trials local practice is probably the soundest method to adopt. For fertiliser trials we must use some standard method of placement. The simplest standard placement is broadcasting after sowing, but this is not the most efficient method.

Counting: Establishment counts are straightforward, but it should be a rule always to count the number of bulbs weighed. In deciding whether a bulb does or does not come within the sample area, take in those bulbs with more than half inside the area. If there are too many bulbs half in and half out, it might be best to cut these bulbs in halves.

Weighing and sampling: Bulbs should be clean when weighed; if much dirt adheres to the bulbs when lifted, it should be washed or wiped off. Whether or not the tops are weighed will depend on the use being made of the crop. If the tops are a significant item of feed, they should be weighed separately from the roots.

Sampling for dry matter: Tops should be roughly chopped up and a 2 lb sample weighed out for dry matter estimation. Bulbs may be sent intact to the laboratory or sampled for dry matter in the field. Sample bulbs sent intact may be identified by the plot number painted on each bulb. The number required will vary according to the size of plot and the variability of the trial, but 10 bulbs per plot is usually an adequate sample.

When samples are prepared for dry matter estimation either in the laboratory or in the field a sector of each bulb is the best sample. If cores are taken, they should not pass directly through the centre of the bulb, as this gives an unduly high proportion of sampled material from the centre. Analyses of turnips have shown that the centres have a lower dry matter percentage than the more external layers.

With field sampling a 2 lb sample of bulb sectors or core samples taken from each plot should be weighed out accurately before the sample is sent to the laboratory. This should be done before any drying out takes place. If it is impracticable to weigh samples in the field, unweighed samples of about 2 lb per plot may each be put into a waterproof and airtight plastic bag or tin for dispatch to the laboratory. Samples should be clean: avoid contamination with soil.

Selection of bulbs for samples should be done at random from all bulbs weighed. It might be helpful to separate bulbs into, say, large, medium, and small, and from each grade take bulbs at random according to the number in each category This should help to get a more representative sample. A less precise method is to select bulbs of average size; that is, average by the standard of that particular plot. Small bulbs have a higher dry matter percentage than large bulbs, other things being equal.

5. Forage Crops

Harvesting: The time to harvest forage crops can be an important consideration. Should it be done when the farmer would normally use the crop or when the maximum growth has occurred? I suggest that in most cases the time to harvest is when the crop would normally be used, though there may be circumstances which would cause a different procedure. Thus with manurial trials on chou moellier the best time is at maximum leaf development before the lower leaves are shed. A crop like rape must, of course, be harvested when considered “ripe”.

In taking yields the crop should be cut off at ground level; 2 lb is a convenient size for dry matter samples. Where the crop is stemmy, like chou moellier, leaves should be weighed separately from stems and dry matter samples taken from each. To secure representative samples it will be necessary in most cases to chop up the material before weighing out the sample.

Because of the bulk of material to be handled it is usual to cut samples for yield estimates rather than whole plots, unless the plots are reasonably small. In some cases an estimate of the efficiency of utilisation of the crop is useful information, particularly when varieties of different growth habits are being compared. In these trials it may be sufficiently accurate to take samples before and after grazing, but if the grazing period is likely to be prolonged, it is preferable to protect sample areas with cages and, after grazing, cut the areas protected by the cages and in addition equivalent areas that have been grazed.

Sampling methods should be in accordance with the principles given in Section 1 (Cereal, etc., trials) on page 103. With drill-strip trials stratified sampling should be used. A useful sampling unit is the cereal sampler, and the procedure for sampling given for cereal trials may be used in forage crop trials.

6. Cereal Green Feeds

In cereal green-feed trials total production is often not as important as the time at which growth is made. Where this applies the trials are best treated as "rate of growth" trials. Replication of treatments in such experiments need not be as great as would otherwise be the case. Duplicate or triplicate block sowings might be adequate.

Various techniques of measurement have been used. Three of these are as follows:

A. On each block set up, say, eight cages as for pasture trials. When the green feed is considered ready for first grazing cut the herbage in one pair of these cages, and then at fortnightly intervals cut herbage in the remaining four pairs of frames. Frames are **replaced on the same area** after cutting and recovery growth is cut six weeks later.

Thus, if the first pair of cages is cut six weeks after sowing, subsequent pairs are cut at eight, 10, and 12 weeks after sowing. By 12 weeks after sowing the first pair of cages has six weeks' recovery growth, and growth from the twelfth to the fourteenth week is measured by the difference in recovery growth in the first and second pairs of frames. Thus:

Cages	**Cutting time after sowing Weeks**	**Production measured from Weeks**	**Recovery growth (time cut after sowing) Weeks**	**Production measured from Weeks**
Pair 1	6	0-6	12	6-12
„ 2	8	6-8	14	12-14
„ 3	10	8-10	16	14-16

„	4	12	10-12	18	16-18

and so on.

B. A simpler method which does not give as complete a picture of rate of growth uses only two cages per plot, which are cut about the time the area is grazed. In this case it is desirable to replace the cages on a new and freshly trimmed site after each cutting and weighing. This method can be run in conjunction with paddock trials where stock grazing data are being obtained.

C. With a replicated small plot trial all plots (and the whole trial area) can be cut as for a mowings and clippings returned trial. If growth is not too heavy, it can with advantage be returned to the plots from which it came. These trials will often be required where manurial comparisons are being made.

Samples for dry matter and, where appropriate, herbage dissection should be secured as described in Part 6, "How to Measure Pasture Trials".

7. Lucerne

Where lucerne is grown for silage and hay the taking of yields is straightforward. Trials will in many cases be in small plots similar to those of pasture measurement trials, and the lucerne may be mown (usually with a sickle mower) and sampled for dry matter and herbage dissection analyses as described for pasture trials. Cutting times are governed by the amount of lucerne growth, and there is no need to return cut material to the plots, as it is removed from the field in farming practice.

Where plots are larger a useful method is to cut with, say, a tractor mower a strip for weighing from the centre of each plot. It is usually more convenient and more accurate to secure samples of green herbage for dry matter analysis than to make hay of the produce of the plots. There may, however, be circumstances where hay weights are desired. In such cases the best method is to put the material up into cocks (separately on each plot) at the appropriate time, and weigh the hay cocks when drying is complete.

Where lucerne is under grazing the best technique will usually be the frame technique as described in Part 6, pages 70 to 74. Cutting times should coincide with grazing dates in such trials. Trials under grazing should have plots large enough to permit the operation of the frame technique of measurement.

8. Vegetable and Berry Crops

Most horticultural vegetable crops are either hand sown or transplanted and in addition have a large number of plants per unit area. This means that small plots, adequate replications, and complex designs are possible. As vegetables are often hand harvested, square designs are usually possible. With small plots it is desirable to get standard plant populations, and where thinning is the rule sowing rates need not be so critical.

Where plants are transplanted into the trial plots some precautions are necessary. In fertiliser trials, for instance, seed boxes or beds should be standardised for soil and fertiliser and should preferably not be fertilised to a high level. As little soil as possible should be transferred to the trial location. Seedlings of uniform height and vigour should be used.

Placement of fertiliser requires more adequate study in many crops. Placement can be very important, particularly with crops which are in the ground for only a short time from seeding to harvest. Trials need careful planning to separate the effects of each fertilising element and its placement and to study interactions between these factors.

Though “standard” fertiliser mixtures may of necessity be included in the trials, I suggest that a more critical look at each element of these mixtures would be worth while in many instances.

Harvesting: Most agricultural crops are either harvested when ripe (such as wheat) or when feed is required by the farmer (such as green-feed cereals). Vegetables, however, usually come to maturity over a period, and with some crops successive pickings are made, as with strawberries. The questions that arise are, therefore, when to harvest and what value to place on successive harvests.

Market values of out-of-season crops make them highly desirable and a low total yielding but early maturing variety may be preferred to a later, high yielding variety. A fertiliser may be economic to use solely because it hastens maturity and’ not because it increases total yield.

It is obviously important, therefore, that the **time** factor be considered. Another important consideration is the yield of **marketable** produce. In this regard the market requirements and what will satisfy the home gardener are not necessarily the same thing. One cannot lay down specific rules for all these conditions, but general rules would seem to be the following:

1. A picking or harvest should be made as early as possible whenever advantage can be taken of out-of-season prices. In some cases a late picking at the end of the season would be necessary. Such pickings should be statistically analysed separately.

2. In some crops four analyses would be required:

(a) An early picking (or possibly two).
(b) Main-crop yields.
(c) Late pickings.
(d) Total yield.

3. For some crops two measurements are necessary:

(a) Marketable produce.
(b) Total crop.

Total crop yields might be valuable, for instance, in fertiliser trials.

4. The evaluation of treatments may be done by consideration of their behaviour under the conditions of 2 and 3 above. If a single figure is required, it may be worth using a "market value" to estimate the total value of a crop. Thus the early picking would in many cases be weighted to allow for its increased value.

		s.
For example:	(a) 10 lb picked early at 5s. per lb	50
	(b) 40 lb picked main crop at 2s. 6d. per lb	100
	(c) 10 lb picked late at 3s. per lb	30
	Total value of treatment	180

The real value of a treatment is usually assessed from a consideration of many things, of which yield is only one. Quality factors come into the picture and are especially important in horticultural crops. Quality measurements are often difficult and a simple substitute such as market value of the produce might be as good an estimate of quality as anything else. Market values fail to give a good estimate in some cases, however, such as where anything new or different is being introduced. In this case relative market values at the time of conducting the trial might have no relation to those operating in subsequent years. All those matters and many others will have to be given due consideration by the experimentalist in the evaluation of treatments.

Wherever possible objective methods should be used to decide on time of or fitness for harvest; for example, maturometer measurements on green peas. If this is not possible, it is most desirable for one person to harvest a trial or each replication where decisions have to be made as to maturity or fitness to harvest.

GERMINATION COUNTS AND OTHER MEASUREMENTS OF CROPS

Plant counts often give useful additional data and in some cases may explain the reasons for differential yield effects. It may be desirable to adjust yields according to plant populations where the differences in population are due to some factor not connected with treatment, such as when the seed of different varieties sown in the trial has different percentages of viable seed. Statistical procedures are available whereby, provided a significant relationship is established between (say) plant population and yield, yields may be so adjusted that comparisons may be made on the basis of equal plant populations in all treatments.

Such adjustments to yield data must always be made with care, for it is easy to eliminate treatment differences where these are associated with different plant populations. One variety of root crop, for example, may normally have a smaller bulb than another, but in the field this is compensated for by a greater number of bulbs. Such a variety would suffer in comparison with a larger-bulbed variety if yields were adjusted on the basis of equal numbers of bulbs per unit area.

Most plant counts are made by sampling methods. Where counts of plants harvested are made, such as with root crops, it is simply a matter of counting the number of bulbs weighed. With maize it might be useful to count both the number of plants on each plot from which cobs are taken and the number of cobs. Cobs, plants, roots, etc., may be classed in various ways for disease, size, or other factors, according to circumstances which can be decided on when harvesting.

With some crops, particularly cereals, it is impracticable to count all plants germinating, or all heads, from the areas measured for yield. Sampling fractions must be smaller than those adopted for yield estimations. Thus it is common with cereal trials to count plants in only two drills per sample area, whereas five drills are cut when sampling for yield. The methods described above for defining the sample areas for harvesting should be followed when counts are taken.

Care must be taken in the selection of drills when counts are made on some only of the drills. Several studies of the sowing efficiency of drills have shown that gross variations in seeding and fertilising between the individual drills are frequent. To remove this source of error it may be preferable to count the same drills throughout the trial or at least each replication. However, if it is desired to correlate counts with yields, it will be necessary to select the drills to be counted at random in each sampling position or, alternatively, to count a shorter length of all drills to be taken later for yield estimations.

A variety of types of counts and measurements may be made in different circumstances. Germination counts, establishment counts, tiller counts, disease counts, measurements of height, lodging, bulb size, head size, and so forth may be taken. In all cases the selection of areas for taking such measurements should be made in accordance with the sampling procedures already described. The sampling fraction to take should be decided on with reference to the available labour and the importance of the measurement, and some preliminary studies are well worth while before starting work on the experiment.

The variability of the material is an important factor and it may be useful to calculate the errors attached to your data after (say) half the measurements have been taken. This might show that more intensive or less intensive sampling is required to secure a desired level of accuracy.

Germination Counts

Germination counts are necessary in the following circumstances:

1. Where the treatments in the trial may affect germination, such as comparing different fertilisers sown with the seed, fertiliser placement studies, and seed treatments, such as dusting, pelleting, and other treatments which might affect germination.

2. In rates of seeding trials. In such trials the quantity of seed sown is not as important as a knowledge of the resultant differences in plant population.

3. Where differences in plants establishing from the lines of seeds sown are likely to occur owing, for example, to seed of different sizes being sown at the same weight per acre.

Germination counts should usually be taken when all seed has germinated but before plants become so crowded that counting is difficult. In some cases where studies of emergence speed are desired several counts may be necessary at intervals after sowing.

Part 9 — How to Observe Crop Trials

By themselves yield measurements from crop trials give only part of the information. They may even be misleading if not supported by observations on agronomic characters of the variety or treatment and on factors that have influenced the trial from the time of sowing onward. To take an extreme case, if one variety in a cereal trial has been selectively attacked by birds so that half the grain is lost, yield measurements are useless. In some investigations observations of factors such as resistance to disease may be much more important than yield data. In all cases, therefore, yield figures must be supported by sound and adequate observations.

There are two main types of observations to be made:

(a) Factors operating during the course of the trial, and

(b) Agronomic features of the crop.

FACTORS OPERATING DURING THE TRIAL

Factors operating during the trial have to be considered to decide to what extent they influenced the expression of agronomic features and yields. In a nutshell, did these factors operate in such a way that a fair comparison of all the treatments in the trial was obtained? If some factor did upset this comparison, its effect should, if possible, be taken into account in the final assessment of results.

Factors may be controllable or uncontrollable. Controllable factors include those which are controlled by good experimental technique. Seeding and fertiliser rates, variation of soil, and siting of the trial are among these factors. On the other hand, more or less uncontrollable factors include weather and the onset of disease, pest, and weed infestations. Some of these factors can be controlled by timely action, such as the use of insecticides and weedkillers.

Details of some of the factors concerned are:

1. Establishment

If greatly different densities of plant population result from variations in germination, all future comparisons may be affected. For instance, different fertiliser treatments may affect germination differently. As an example of this, one may quote borax as significantly increasing germination of linen flax, or of several phosphatic fertilisers significantly depressing germination of linseed. Again, different fertiliser placements may differently affect “strike”. Generally, where fertiliser comparisons are made, germination counts should be taken. It is possible, if the data are available, to determine statistically to what extent germination counts and yield are related. If desirable, yields can be adjusted for variations in stand if germination counts are taken.

In some instances differences are so marked that counts are hardly necessary. Several years ago in barley trials in the South Island, one variety was such a poor-germinating line that yields were obviously affected and had to be discounted.

Again, in a trial at the Marton Experimental Area comparing various lines of lupins, cold, windy conditions considerably reduced the plant population of certain varieties, while not seriously affecting others, and this was reflected in the yields which were obtained from the trial.

There are numerous other examples of differences in establishment from sowings, but the point to be stressed is that these differences were observed and recorded, and were of great importance in the interpretation of results.

2. Factors Operating during Growth

Any factors which operate during growth should be observed and reported. In wheat, for example, some varieties may be affected more than others by diseases such as mildew or stem rust, or by insects such as Hessian fly. Obviously these should be reported as harmful factors which might cause reductions in yields. They may be worse in some seasons than others, and may account for a certain variety experiencing a poor year.

Weather conditions may produce differential effects on treatments. In cereal green-feed trials hard frosts may “burn” the plants and reduce the rate of growth of certain varieties while others are not affected. Soil variations not noticeable when the trial site was selected may at some stage begin to exert obvious influences on the trial. Subsoil variations such as shingle ridges may show up in the crop in dry weather. Excessive weed infestation may seriously affect a trial, and where this occurs the extent of the infestation and how it affects the crop should be reported.

Many factors other than those mentioned may operate, but enough has been stated to indicate their importance.

3. Appearance at Harvest

In many trials at harvest time factors have operated which have caused some treatments to be at a disadvantage compared with others. Differing times to mature may cause differences in losses due to shaking or bird damage in cereals, or different degrees of straw strength may have resulted in different degrees of lodging or straw break.

4. Grain Quality

In all cereal and other seed crops notes should be given on the quality of the grain; for example, pinched, full, bright, discoloured, sprouted, etc. Bushel weights are desirable wherever they can be obtained.

Perhaps it will now be realised to what extent these factors can influence a trial, and equally it may be realised how important is the recording of observations as an aid to the correct interpretation of results.

AGRONOMIC FEATURES

Certain agronomic features of a variety may be as important as yield, perhaps even more important. Resistance to clubroot in brassicas, standing ability in cereals, rust resistance in linseed, and palatability in forage crops are examples of these. The importance of these things must not be overlooked when observations are recorded. Some of the more important agronomic features to be looked for in crop trials are now considered under the headings of individual crops:

Wheat

Time to maturity: This is important, particularly in spring-sown wheat. A variety must be ready for harvest at the time weather is favourable. If it ripens too late, when harvesting conditions have deteriorated, it is not suitable for the particular local conditions.

Resistance to lodging: A variety which has strong straw and stands up in adverse weather conditions is obviously a better proposition than one which has not this ability.

Shaking: This refers to a loss of grain at or near maturity, mainly through disturbance of the crop by winds. As wheat is mainly direct headed, the grain must be mature at harvest. Many older wheat varieties are unable to retain their grain up to this stage, and are unsuitable for heading.

Some heading varieties can stand for a considerable period after the grain has reached maturity, without shaking. This is obviously a desirable feature.

Straw break: This occurs through weakness of straw, though it can also be due to insect attack or disease. Inherited proneness to straw break is most undesirable.

Disease resistance: Some varieties show more susceptibility to some diseases than others. Some varieties are more affected by mildew attack than are others, for example, and where mildew conditions are likely this is an important feature. It is, of course, essential to be able to identify the disease in the field.

Insect attack: Varieties differ in susceptibility to Hessian fly and stem weevil attack. Recognition of insect damage is important.

Barley

Remarks applied to wheat may equally be applied to barley, but some additional aspects need to be considered.

Neck break: In barley neck break is an important feature. As the crop approaches maturity and the filling heads droop the stem just below the head becomes weakened in some varieties and the head is apt to drop off on the ground, becoming a complete loss. This can be a serious cause of loss in yield.

Evenness of ripening: In barley a special feature is evenness or otherwise of ripening. Grain of differing degrees of ripeness germinates unevenly, and this affects the process of malting in both quality and quantity. Evenly ripened grain produces the best and most malt.

Oats

Shaking: Some varieties when grown for grain are liable to shake severely if left to stand to complete maturity. Because of this they are usually cut at the binder-ripe stage and left to mature in stooks or in windrows.

Resistance to lodging is a desirable feature in oats. Recently developed varieties show considerable resistance to lodging.

Straw break is an undesirable feature frequently encountered in oats and one that often affects yields.

Grain quality is of particular importance, millers accepting only grain which is plump, bright, white, and without undue amounts of husk. The standard varieties usually have these desirable features.

Cereal Forage Crops

With oats, barley, ryecorn, and wheat grown for green feed, the following agronomic features are important:

Speed of growth: Some varieties are superior in providing quick, early growth, though producing little feed later. It is the period at which cereal green feed makes its growth as well as the amount of growth it makes that is important.

Habit of growth: Upright or prostrate. Usually the upright varieties will not stand severe grazing as well as the more prostrate ones.

Recovery from grazing: This is self-explanatory.

Persistency: Some varieties will stand many grazings; others are set back severely by one or two grazings.

Winter hardiness: In frosty weather some varieties become frost burnt, and are not so attractive to stock as those which still have fresh, unspoilt growth.

Palatability: Stock preferences should be noted.

Linseed

Disease resistance: Rust plays such an important part in linseed that rust resistance of varieties is among the most important aspects of this crop. Varieties may be otherwise excellent agronomically but are not entirely satisfactory because they are liable to severe rust infection. Present research heavily emphasises rust resistance.

The diseases pasmo and browning are also important and should be recognised.

Time to maturity: Varieties very suitable for warmer districts may be unsuitable for colder regions, because they are not sufficiently early maturing to be harvested in good weather. Evenness of maturity is important.

Resistance to lodging is a desirable feature in linseed.

Other features are toughness of stem (that is, for cutting), size of seed bolls, and amount of secondary growth.

Linen Flax

Disease resistance: This crop, like the related linseed, may be greatly affected by rust infection. Stem rust causes deterioration of fibre quality. Thus, in linen flax rust-resistant varieties are of great importance and the search for these is a main feature of present experimentation. Older varieties are sometimes grossly infested with rust, but more recently introduced varieties show promising resistance. The diseases pasmo and browning are also important and should be recognised.

Straw length: All linen flax crops must reach a certain length of stem so that they may be processed economically in the factory. Tall varieties are therefore preferable to shorter varieties.

Resistance to lodging: This is an important factor, as lodged crops are very difficult to harvest, which is done by a special pulling machine.

Maize

Current experimentation with maize consists mainly of evaluating the merits of imported lines of double-hybrid type. The use of these lines in recent years has resulted in a considerable increase in yield in New Zealand maize-growing areas.

Time to maturity: This is a special consideration in maize, lines showing wide differences in this aspect. Many imported lines of maize grown in experiments have been shown to be much too late in maturing for New Zealand conditions. Obviously maize should ripen in time for advantage to be taken of suitable harvesting conditions.

A feature to be noted is that both silks and tassels should mature together, thus ensuring complete fertilisation. Some lines have not shown this desirable feature in all districts.

Cob height: Cobs should be produced at a reasonable height so that harvesting is facilitated. Cobs produced too high or too low are therefore undesirable.

Number of cobs per plant: This obviously affects yields. Plants producing only one cob per plant will most likely be lower yielding than if they had produced two.

Angle of cob: A cob which matures pointing downward is capable of shedding moisure falling on it, thus preventing weathering and deterioration of grain.

Cob sheath: A sheath should fully protect the grain of the cob. The sheath should not be short, thus exposing the grain at the tip end of the cob. This is important for protection against weathering.

Suitability for green feed or silage: Maize is a useful crop for late summer green feed and for silage. Observations on growth habit with this possible use in view should be made.

Lupins

Palatability: Apart from the comparison of sweet (non-alkaloid) versus bitter lupins, other considerations are succulence of stem and woodiness of stem. The latter would be unattractive, in comparison, to grazing stock.

Recovery after grazing: Some varieties, because of ability to produce recovery growth after grazing, may offer the advantage of a second grazing.

Suitability for harvesting for seed: Some lines of lupins, notably Sweet Yellow lupins, while very desirable for certain purposes such as growing lamb feed on sandy country, have a very serious defect in that the shattering of the seed pods makes harvesting very difficult and uneconomic. Some recently introduced lines, however, are better in this respect.

Rate of growth: Pink bitter lupins because of their rapid early growth have a particular value as green feed.

Habit of growth: Note whether upright or semi-prostrate.

Peas

Agronomic features of importance are: Disease resistance (for example, fusarium wilt), density of foliage (in relation to smothering of weeds), number of pods per plant, time to maturity, and (where appropriate) adaptability for the special purpose of the canning industry. Suitable canning or quick-freeze varieties mature their pods more or less at the same time.

Potatoes

Growth habit may be important if weeds threaten, as a low-growing, sparsely foliaged variety is far more liable to weed invasion than are tall, dense, spreading varieties. This is a point worthy of consideration when the commercial possibilities of varieties are assessed, for a low-growing and sparsely foliaged variety would be unsuitable in a weed-infested district.

Time to maturity: This is established by the approximate date when the foliage withers. It is a vital factor in the growing of early potatoes. Some varieties can be dug before they are fully mature;

others make much of the tuber growth in the weeks just before the tops die off.

Disease: The two most important things to look for are (a) resistance to late blight infection and (b) freedom from the virus diseases, of which the main ones are leaf roll and mosaic. There are also some tuber-carried diseases, such as scab and powdery scab. In all potato trials it is important to record the amount of recognisable virus infection. Both leaf roll and mosaic are shown in the foliage.

Quality: Without considering the aspect of cooking, quality relates to the appearance of the harvested tuber. Points of quality are good shape (with absence of irregular shapes and growths), shallow eyes, and attractive skin colour.

The proportion of table and seed tubers may also come within this section. There are some varieties which normally produce large numbers of small tubers, many so small as to be useless for culinary purposes.

Swedes and Turnips

Time to maturity: This should be considered in relation to the purpose for which the variety is grown. White-fleshed turnips are much quicker maturing than the harder-fleshed swedes, but the latter are superior in keeping quality and provide feed at a different period of the year. The yellow-fleshed turnip tends to be intermediate.

This principle, however, applies also to varieties of the same crop. Thus there are varieties of white-fleshed turnips of differing maturities, as also there are in swedes and in yellow-fleshed turnips. Certain white-fleshed turnips are so quick growing that they can be sown in autumn to produce late autumn food. Thus though these varieties may not bear comparison with others for sowing in late spring, they are superior for the special purpose of autumn sowing.

Again, in swedes a variety may be particularly valuable because it reaches maturity at a late stage and can be utilised when other varieties have deteriorated.

Quality: In this category can be considered bulb characteristics. Bulbs with good shape, with a large proportion protruding from the soil, and without fangy roots, are desirable. Where the crop is fed off by animals wastage will occur if a considerable portion of the bulb is below soil surface; when the crop is to be hand pulled easy drawing from the ground is required.

Disease resistance: The main diseases are clubroot, dry rot, and turnip mosaic, a virus disease. Varieties exhibit differing degrees of susceptibility to these diseases.

Insect attack: Aphids, especially in dry weather, sometimes severely affect crop growth. Some varieties are able to withstand and recover from attacks; some seem to be resistant to attack. A variety of other insects attack brassica crops and it is important to be able to recognise the cause of damage and, where appropriate, to apply corrective treatment such as insecticides.

Palatability: Stock preferences with regard to varieties should be noted if possible.

Rape, Kale, and Chou Moellier

Time to maturity: This should be considered in relation to requirements for different purposes, for example, as lamb feed in autumn or as winter forage.

Disease resistance: Clubroot resistance is a desirable feature which should be reported when observed.

Palatability: Stock preferences should be reported.

Insect attack: Aphids are a serious pest of these leafy crops, especially in dry weather. Any differences in severity of attack should be observed and reported. Other types of insect damage should be recognised.

Suitability for grazing: The height to which the crop grows is often important. The crop must not be too tall if it is to be grazed, for instance, by sheep or lambs. Tall crops carrying much moisture can wet grazing sheep unduly.

Ratio of leaf to stem: This relates also to the purpose for which the crop is grown. For lamb feed succulent, leafy material is required; for winter foraging of cattle chou moellier of the Giant type is grown principally for its stem.

Fodder Beet, Sugar Beet, and Mangels

Where those crops are grown for animal food the important agronomic features are time to maturity and ease of harvest. Roots which protrude markedly out of the soil surface are easier to harvest than those more deeply embedded in the soil. The shape of the roots and resistance or susceptibility to disease and insect attack should be noted.

SOME CONCLUDING REMARKS

It is clear that the experimentalist observing crop trials should be something of a specialist in plant pathology and entomology as well as a good agronomist. In many cases when special problems arise it will be necessary to call in the expert. Advice from growers is often of special value. It is practically impossible for one man doing experiments with field crops to get the most out of his trials without assistance from workers in specialised fields.

Part 10 — Recording, Summarising, and Writing up Experiments

The most carefully conducted experiment is of little value unless it is properly recorded, summarised, and written up in a manner appropriate to the type of investigation. It is unfortunate that many research workers pay little attention to proper reporting. As a result they have little to show for a lifetime of hard work. Conversely, some workers, by making the most of their data, sometimes achieve a reputation in excess of the value of their work. If a little can go a long way, a lot can go much further.

The three main stages in the preparation of the results of research for publication are:

1. Adequate reporting of observations during the progress of the experiment and accurate recording of data produced.
2. Good summarising of data and statistical treatment of results to get the most out of them.
3. Writing-up the data to best advantage.

RECORDING AND SUMMARISING OF DATA

The type of data to record has been discussed in various sections of this bulletin, but it is the **method** of recording that will be discussed here.

The first essential is a good system of reporting. Provision should be made for reports at the laying down of experiments, progress reports during the progress of the trials, data sheets to record measurements and scoring systems, and final reports to summarise the available information. These final reports should, where appropriate, include the statistical analyses of the data and comments on what the analyses show in relation to observations made. These types of reports are worth considering in more detail.

1. Laying down of Experiments

Reports at laying down should be a record not only of treatments applied and the method of application but also of the conditions of soil, previous history of the area, and climate before and at the time of applying treatments. This report is the base on which the trial is built. An inadequate report on the previous history of an area may, for example, make all subsequent work on a fertiliser experiment of very restricted value. The type of information required in a laying-down report will vary with the type of experiment. Previous fertiliser applications are especially important in fertiliser experiments, whereas a knowledge of cropping history, soil type, and condition of soil at sowing may be more important in a crop variety trial.

Treatments in the trial must be specified clearly and in detail. A sketch plan showing the position of the plots in the field must accompany the laying-down report. This plan must show a good “key” to treatments and the position of each plot in the field. Each plot should have a plot and a treatment number. The plan should show the dimensions of plots.

A good plan is vital to a good experiment. Unless plots can be correctly identified in the field the experiment is useless. Plans should be correctly orientated and should show sufficient measurements from fixed and easily recognisable objects to enable a stranger to find the experiment readily and to identify each plot correctly. A hastily scribbled drawing in a field notebook is not good enough.

It is particularly important that any mistakes or departures from original proposals should be clearly stated and clearly shown on the experiment plan. Details should be given of the method of sowing or applying treatments and comments made on the efficiency of application and accuracy of doing the work.

2. Progress Reports

The type of observations to make and the methods of making observations have been discussed earlier. The type of progress report will vary with the type of trial and the observations made. In brief, annual crop trials should, at a minimum, have a report covering establishment, one made during growth, and one made near harvest. Experiments on pasture and lucerne should at least have a report representative of each season's growth plus special reports to cover particular features, such as an early-spring report to examine early-spring growth of lucerne and an early-summer report to cover the flowering of subterranean clover strains.

Particular care should be taken to note factors which have influenced the growth of the plant and the comparisons among treatments. Where appropriate it does no harm to draw tentative conclusions as to the value of treatments. Such comments will be found of great value later when the trial is summarised.

Any measurement data should be plainly presented in a table with at least three columns:

1. The plot number.
2. The treatment number.
3. The measurement for this plot and treatment.

The exact size of each plot measured or sampled must be indicated and the units of measurement or the scale of "pointings" indicated. The technique employed in taking the measurement or making the "score" should be described. All these data are necessary for the compilation of the results.

3. Final Report and Trial Summary

There are really two reports required where measurement data are available. The final report is a summary of the history of the trial from its beginning. This report should pay particular attention to factors that have influenced the trial and should try to evaluate their effect. Care must be taken to point out to the biometrician who is analysing the measurement data any factors that he should take into account. For example, if a particular plot in a cereal trial has been attacked by birds, this fact should be plainly indicated. The experimentalist, not the biometrician, should decide whether certain data should or should not be included in the analysis. Where some factor external to the trial has operated on some plots it may be proper to reject data from those plots. On the other hand, a somewhat lower-yielding plot, because of soil variation, in an otherwise high-yielding treatment must be included in the analysis.

Where data are forwarded for analysis this final report should accompany the data.

The second "final" report is the **trial summary.** It is written on receipt of the statistical analyses and is written "around" those analyses. It is, in effect, an evaluation of the treatments in the light of the analyses and of the other factors reported on in the final report. If properly done, such a final summary makes easy the writing-up of the experiment for publication.

The final summary should, among other things, include reference to the following:

(a) The reasons for doing the trial and some background information concerning the trial.

(b) Measurement data as analysed and an evaluation of that data in the light of observations made or factors operating during the course of the trial.

(c) An evaluation of treatments and the reasons for any favourable or unfavourable comments.

(d) An assessment of the value of the trial as a true indication of the worth of the treatments included. Any limitations of the data or qualifications to the figures should be stressed.

(e) Recommendations as to future work based on the results of the experiment and other available information.

Where no measurement data are available one report can cover both the "final report" and the "trial summary". It should deal with all the matters discussed above, where appropriate.

It is not easy to make good reports on experiments. It is, perhaps, better to be too verbose rather than to omit vital information, but the best report is not necessarily a lengthy one. The ability to pick out the important facts and to make sound comment on these facts is not easily learnt. Nevertheless, it is necessary to be able to write a good experimental report before attempting to write up the results of work for publication.

WRITING UP THE RESULTS

(a) The Scientific Article

If the final summary of the trial or trials in question has been well prepared, the writer should be able to set out in skeleton form the essential features of an article. These are as follows:

(a) The introduction, which sets out the reasons for doing the work and the previous work published and unpublished to which the writer has access.

(b) The techniques used and the reasons for using them and details of the experiment from the laying-down report.

(c) The results secured and observations on the factors that have affected the results.

(d) A discussion of the results in relation to results of other workers and the circumstances under which the trial was conducted, including limitations of the data and suggestions for further work.

(e) A conclusion which summarises what new information the trial has produced.

(f) A short summary of the whole investigation.

This is the usual form of a scientific article. At this stage it is appropriate to consider whether there is enough "meat" for a good article in a single trial, or whether a group of trials should be written up together. If you have a group of more or less identical trials, it is usually possible to have a statistical analysis of the group. If all members of the group of trials show reasonably similar results, the region of application of the results is greatly widened and it may be possible to draw conclusions that apply to a district or a soil type.

The difficulty arises, however, when the series of experiments is only more or less similar. They may vary as to soil type, for instance. It may be possible in such cases to form sub-groups of more homogeneous trials. As a general rule such sub-groups should consist of at least four experiments. Trials finally selected should be reliable and have reasonably low errors.

When the groups or sub-groups have been selected the consistency of the results should be examined. If one treatment is sometimes better and sometimes worse than another, a composite analysis will not help, but a reconsideration of the method of grouping the experiments may improve things. Differences which are small and not statistically significant but which are consistent may prove to be statistically significant in a group analysis.

Consultation with a biometrician may well be advisable when you have reached this stage, particularly in rates of application trials where special procedures may help you to determine such matters as the maximum economic rate of application under certain circumstances. No general rules can be given about group summaries of trials except to say that a conclusion soundly based on the results of a group of trials is much more generally applicable than one based on a single experiment. In the latter case the results really have application only to the particular circumstances under which the experiment was conducted.

The results of all treatments should be given and there should be no selection of data. Accurate statements should be made and precise terms should be used; for example, give the botanical names of plants correctly in addition to the common names. Be especially careful of loose phrasing and ambiguous statements. There is no need to go into great detail regarding trial design and statistical analysis unless there are some unique features about these.

Similarly, field techniques should be briefly described unless these are of special interest or have had some unusual effect on the results secured. Technical difficulties encountered and overcome are, however, of special interest. Be careful when using terms which have a precise scientific meaning but a more general popular meaning (for example, "significance") and see that it is the scientific meaning that you are considering.

Preparation of Tables

1. Make tables which can stand alone, with suitable heading and legend, which should be as brief as is consistent with clarity. It is still necessary to discuss these tables in the text.

2. Work in reasonable units. For instance, if you are dealing with figures of millions of pounds, do not head your table "£s" and then quote entries of 4,600,000. Rather head the table "million £s" and have entries 4.6. If quoting yields, do not quote hundreds of thousands of pounds when these could be converted to tons, or quote

yields in tons when other yields are of the order of .01 ton.

3. Round off figures to give the correct accuracy. If a plot is 1/100th acre and the yield is measured to the nearest 1/10th lb, the yield per acre can only be accurate to 10 lb, and should be rounded off to the nearest 10 lb when being quoted.

4. Have a base and relate all other figures to it. This will vary with the type of data. For crops the base should probably be the yield of the standard, and differences of all other treatments from the standard should be quoted. For pasture results it is useful to make the yield of the standard equal to 100 and quote other yields as relative to the standard at 100. The form of this base rather depends on the type of article.

5. For technical articles make a table even if the results are not significant.

6. If it is desired to show a trend, say over a number of years, it is bad to choose a year at the beginning, middle, and end of the period and quote results for these years. A graph of all years is better, or else a trend line can be fitted which will show the average increase per year.

7. Use graphs to supplement or highlight points in the tables. Graphs should never have many lines, particularly if the lines are close together or overlapping.

(b) The Popular Article

The approach to popular articles and the method of writing them up differ greatly from the scientific article. Strive for simplicity and clarity of presentation. This does not mean that the quality of the work on which the article is based may be lower—far from it. In fact, because details of the experiments are not given, it is assumed that they are sound, and as the results may be put in practice, it is all the more important to be convinced of the accuracy of the work in question. The farmer has sufficient confidence in the basic soundness of official research and experiment not to need many of the details that would be required in a technical report. It is important that this confidence be maintained.

Form of Presentation

The introduction for an article on experimental results should present the significance of the findings briefly as it affects farm economics or management. A following paragraph could give the background of the experiment or the circumstances that called for the work to be done.

The main body of the article should then be an orderly presentation of the progress of the work, but in this care should be taken to give only those details that are necessary to develop the argument. There is no need for a mass of technical detail of importance only to other technical workers. Complete documentation of all results in tabulated form is unnecessary. Brief textual coverage of results is frequently more effective than a bewildering array of columns of figures that do not vary significantly.

A long article is better with a summary in which the principal points, including the overall result included in the introduction, are recapitulated. The summary should be very brief and should not be a meticulous precis of every section or paragraph in the article. A short article is better without a formal summary.

In general, only definite results should be quoted. If a trial is inconclusive through lack of replication, or a number of trials show contradictory results, these should not be mentioned. The exception is where the purpose of the article is to discourage the general application of some new treatment which may be damaging and which has failed to give results better than the standard in any of the trials. Then the failure of the trials to show results is important. Usually, however, the quoting of inconclusive results is confusing. Simplify the results before including them in the article. It is better to quote few rather than many figures. It is better to say that the average increase due to lime was 3½ bushels, rather than that the yield without lime was 42½ bushels and the yield with lime was 46 bushels. The second way gives more information but is less easily grasped at a quick reading.

Tables

Tables should be reduced to a minimum in the popular article and only included to lend point to matters discussed in the text. They should be simplified to show only the important results from the practical viewpoint, and the figures should be rounded off and the headings constructed so that as few numbers as possible are given. If possible, not more than 10 to 15 figures should be included in each table. Several small, simple tables are better than one complex table. Graphs should be avoided where possible and are better replaced by other types of pictorial presentation.

Where possible avoid the use of scientific terms which have a different popular meaning. In tables of results it is, however, frequently necessary to use the term "statistically significant". This is all right, but if you say that A was significantly better than B, the non-scientific reader might misunderstand the phrase. To say that the difference between A and B is statistically significant may or may not be comprehensible to the general reader, but at least it is less likely to be misunderstood. In general, however, such terms should be avoided even at the expense of a more lengthy explanation.

With all articles it is sound practice to have them read over before publication by the type of person who will be reading the article in its published form. Take the scientific article to the scientist for comment, and the popular article to the farmer friend.

Illustrations

As a general rule the scientific article requires only those illustrations which explain matters in the text more fully. Good examples might be pictures illustrating deficiency symptoms in pastures, pictures of apparatus or machinery to show particular points of construction, and pictures of a soil profile. The general type of illustration is not required. On the other hand, the popular article needs illustrating. Further, it needs pictures that are interesting in themselves; "action" photographs, general scenes of the district to which reference is made, and pictures to drive home points in the article. A scientific article might have a picture showing the heads of a new wheat variety in some detail; a popular article might have a picture of the heads but also a general view of a paddock of the variety, preferably being harvested or with some other action to give the picture “life”. There is little scientific value in a general picture of a paddock of wheat (unless it illustrates some feature such as straw weakness), but this may still be a good illustration for a popular article.

Part 11 — Some Concluding Thoughts

We have followed the story of an experiment from the first conception of the idea that requires testing, through the preliminary stages before laying down, through the laying down of the trial and its progress to the trial summary, and finally to a consideration of writing up the results of the experiment for publication. It might be appropriate at this stage to consider something of the reasoning and the philosophy behind the experimental approach and what practical value we can hope to achieve from our work.

The experimental method is not something that arose by chance. It represents the continuing effort of man to find out more about his environment and by so doing make it better in some respect to live in. If an experiment is a question put to nature, at the same time it provides the means of finding the answer to that question. This dual function is dangerous, for it is very easy to get the answer we want rather than the correct answer. This is where we see the need for a high standard of scientific ethics and for taking all possible care to avoid the bogy of bias. Much experimental method is an endeavour to remove personal judgment from research. We measure yields where possible because the balance is less liable to bias than the eye; we adopt random systems of sampling because any system that allows the element of personal selection also allows the element of personal bias; we adopt statistical procedures in an endeavour to apply the impersonal laws of chance and probability to make our decisions rather than make decisions based on personal choice. We try to make our reasoning objective rather than subjective.

But despite all these precautions, in the final analysis the decisions as to the preferred treatments are made by the experimentalist himself. He has to weigh all sorts of factors in the balance of his personal judgment. It may be that variety A yields significantly more than variety B over a number of years, but the experimentalist may still prefer variety B because it is resistant to a disease that attacks about one year in five on the average. As long as his reasons for preferring B are plainly stated and due weight is given to the yield data also, this is a perfectly valid conclusion. But the more difficult cases arise when there is not much to pick between A and B in yield or disease resistance, but the experimentalist "likes the look" of B and this unconsciously affects his decision.

In some types of experiment the experimentalist by his conduct of the trial greatly influences the results. He "gets what he wanted to get". In grazing management trials where the definition of a management treatment is a very vague and loose thing experimentalist X can get one result and experimentalist Y the opposite merely by the way each interprets the treatment. Where new ideas are being tried it is very easy to find they will not work, simply because **not enough is known** about the details of the new treatment whereas the old one is tried and tested. The percentage of successful results from a new technique tends to rise as the years pass and knowledge of how to operate the new technique improves.

The experimentalist has, therefore, a big influence on the results of the experiment despite all precautions of modern experimental technique. We must be most careful, therefore, not to assume too much; not to assume that finality is reached and that it is not worth testing someone else's findings. The best safeguard is to have the same investigation done by a number of different experimentalists and hope that they do not all have the same preferences and prejudices. If six trials of a similar type give a similar result, it is much more likely to be a true result if a different experimentalist has been responsible for each one of the trials. Some types of investigations are more prone to this "experimentalist effect" than others, but it is necessary to be aware that such a thing exists, and to watch for cases where it might be operating.

PRACTICAL APPLICATION OF RESULTS

The normal sequence of events in an experimental programme is of the following type:

1. Preliminary “survey” type of trials, usually simple but widespread over the country.

2. More complex experiments to answer specific problems concerned with the new treatment and to get measurement data to evaluate it more precisely.

3. Another series of trials over the country of a more “practical” nature than 1 and 2 to see how the new treatment fits into farming practice.

Trials as in 3 above will not be required in many cases because of the nature of the investigation. If the new treatment is going to change farm management to any considerable extent, these trials are essential. On the other hand, if it is, say, simply the introduction of a new variety of wheat, series 1 and 2 trials should have provided sufficient information, though it will be desirable to study in some detail the first few paddocks of the new variety grown on a commercial scale.

In many cases we may leave it to the farmer to adapt the new treatment to his farming practice and follow his difficulties and successes while this process is going on. The practical application of the results often gives rise to additional problems requiring more research before the new treatment is suitable for general use.

Investigations of the more “basic” type will not have any immediate practical application, but other types of trial may be concerned primarily with farming technique. In such cases, and in all instances where an experimental programme aims at devising something that can be put into practice, use should be made of farmers’ experiences which are, perhaps, a more searching test of new methods than the best series of experiments. It is well worth while following up the practical application of the results of research. This can be disconcerting, or it can be a rewarding experience.

Acknowledgments

Free use has been made of lecture notes and instructional material prepared by officers of the Field Research Section, Department of Agriculture. Their assistance in this regard and in the general preparation of the bulletin is gratefully acknowledged.

Officers of the Horticulture Division made valuable comments on

sections concerned with horticultural experiments.

Appendices

APPENDIX 1 DRILLING TABLES

For most drills 49 turns of the wheel are the equivalent of a 10-chain run. This means that an area equivalent to 0.062 acre is covered with a 7-coulter drill (0.080 acre with a 9-coulter drill), coulters being 7 in. apart.

The following table shows the amount of seed or fertiliser which should run through the drill on the standing try-out (49 turns of jacked-up wheel) or 10-chain run in the field.

Desired seeding or manuring rate (lb/acre)	**Weight required to 7 coulter**	**pass through drill 9 coulter**
	lb	lb
50	3.1	4.0
60	3.7	4.8
70	4.3	5.6
80	5.0	6.4
90	5.6	7.2
100	6.2	8.0
110	6.8	8.8
112	6.9	8.9
120	7.4	9.5

Another useful figure to remember is that a 7-coulter drill running 162 chains sows 1 acre.

Tables 1 and 2 (on pages 140 and 141 respectively) give data for the conversion into pounds per acre of amounts sown for certain sizes of trials from a 7-and 9-coulter drill respectively.

APPENDIX 2 — CALCULATION OF FERTILISER AND SEED WEIGHTS

Use of the Tables

The tables are in three sections—

(a) The main table (on page 142) giving weights in grammes and pounds.

(b) Conversion numbers for multiple plots in links, feet, chains, and square links.

(c) Useful conversion factors (on page 143).

In using the main table the plot size is selected from the top lines (plot size), for example, for a 20 x 25 link plot see column L. Then if a dressing of 28 lb/acre is required for this plot, the horizontal line of figures corresponding to 28 lb in the margin is traced until column L is reached. This gives the figures 0.14 lb or 63.5 gm.

For all plots not included in the main table, reference is made to the smaller tables on page 143, for example, 5 ft x 43 ft 7 in. This is found under **feet,** where 5 ft x 43 ft 7in. = L, and, as before, for the 28 lb rate would require 63.5 gm per plot. When a particular-sized plot is not included in any table, work out the area in square links (using 1 sq. ft. = 2.30 sq. lks. if necessary—see conversion factor table), and then refer to the “square links” table. As an example, take 60 x 62.5 links = 3,750 sq. lks. = (3000 = P + Q) + (500 = L) + (250 = L/2 = (P + Q + L + L/2), and at the 28 lb rate = (127 + 254 + 63.5 + 31.7 gm) = 476 gm. If it is desired to convert from one system of weights to another, the appropriate factor is given in the conversion table; for example, to convert 0.14 lb to ounces note that 1/10 lb = 1.6 oz, therefore 0.14 lb = 1.4 × 1.6 oz = 2.24 (2¼) oz. The limits of experimental error must be taken into account when calculating per plot weights; for instance, 7 coulters × 1 chain = 618.7 sq. lks., but the nearest figure in the main table under column N is 625 sq. lks. Does this mean that the table cannot be used for those figures which are slightly different? An examination of the following values will show this is not the case.

	Area	
Rate per acre	**sq. lks. 618.7**	**sq. lks. 625**

8 oz	1.403 gm	1.44 gm
28 lb	78.6 gm	79.4 gm
3 cwt	943.0 gm	952.5 gm
1 ton	13.9 lb	14.0 lb

While these amounts could be weighed accurately, the limitations in applying these amounts evenly to a trial area would far exceed these small differences. In the above case the difference is about 1 per cent, and only when differences are more than 5 per cent must the weights be calculated anew. It is suggested that standard plot sizes should be used whenever possible.

All applications should be made on a **per plot basis, including basal dressings.** This will overcome some of the irregularities of application, which have important effects on basal dressing responses. Furthermore, the basal dressing may often be conveniently used as a suitable vehicle for the application of smaller amounts of other materials. This per plot application also allows an extra check of fertilisers to be made at topdressing time.

Multiples and submultiples of the rates can be calculated when desired; for example, 2½ oz = ¼ × 10 oz, and 7 lb = 5 + 2 lb. Generally, however, the smaller weights refer to minor elements which are frequently applied in solution.

TABLE 1: 7-coulter Drill—Equivalent in Pounds per Acre for Amounts Actually Sown (Coulters at 7 in. spacing)

12 chains				16 chains			
6 reps. of 2-ch or 4 reps. of 3-ch plots				8 reps. of 2-ch plots			
Wt. sown	Lb/acre	Wt. sown	Lb/acre	Wt. sown	Lb/acre	Wt. sown	Lb/acre
Lb		Lb		Lb		Lb	
6.0	81	8.4	113	8.0	81	10.4	105
6.1	82	8.5	114	8.1	82	10.5	106
6.2	84	8.6	116	8.2	83	10.6	107
6.3	85	8.7	117	8.3	84	10.7	108
6.4	86	8.8	119	8.4	85	10.8	109
6.5	88	8.9	120	8.5	86	10.9	110
6.6	89	9.0	121	8.6	87	11.0	111
6.7	90	9.1	123	8.7	88	11.1	112
6.8	92	9.2	124	8.8	89	11.2	113
6.9	93	9.3	125	8.9	90	11.3	114
7.0	94	9.4	127	9.0	91	11.4	115
7.1	96	9.5	128	9.1	92	11.5	116
7.2	97	9.6	129	9.2	93	11.6	117
7.3	98	9.7	131	9.3	94	11.7	118
7.4	100	9.8	132	9.4	95	11.8	119
7.5	101	9.9	133	9.5	96	11.9	120
7.6	102	10.0	135	9.6	97	12.0	121
7.7	104	10.1	136	9.7	98	12.1	122
7.8	105	10.2	137	9.8	99	12.2	123
7.9	106	10.3	139	9.9	100	12.3	124
8.0	108	10.4	140	10.0	101	12.4	125
8.1	109	10.5	141	10.1	102	12.5	126
8.2	110	10.6	143	10.2	103	12.6	127
8.3	112	10.7	144	10.3	104	12.7	128
						12.8	129

18 chains				24 chains			
6 reps. of 3-ch plots				12 reps. of 2-ch or 8 reps. of 3-ch plots			
Wt. sown	Lb/acre	Wt. sown	Lb/acre	Wt. sown	Lb/acre	Wt. sown	Lb/acre
Lb		Lb		Lb		Lb	
9.4	85	11.7	105	11.0	74	15.6	105
9.5	86	11.8	106	11.2	75	15.8	106
9.6	86	11.9	107	11.4	77	16.0	108
9.7	87	12.0	107	11.6	78	16.2	109
9.8	88	12.1	108	11.8	79	16.4	110
9.9	89			12.0	81	16.6	112
10.0	90	12.2	110	12.2	82	16.8	113
10.1	91	12.3	111	12.4	84	17.0	114
10.2	91	12.4	112	12.6	85	17.2	116
10.3	92	12.5	112	12.8	86	17.4	117
10.4	93	12.6	113	13.0	88	17.6	119
		12.7	114	13.2	89	17.8	120
10.5	94	12.8	115	13.4	90	18.0	121
10.6	95	12.9	116	13.6	92	18.2	123
10.7	96			13.8	93	18.4	124
10.8	97	13.0	117	14.0	94	18.6	125
10.9	98	13.1	118	14.2	96	18.8	127
11.0	99	13.2	119	14.4	97	19.0	128
11.1	100	13.3	120	14.6	98	19.2	129
		13.4	120	14.8	100	19.4	131
11.2	101	13.5	121	15.0	101	19.6	132
11.3	102	13.6	122	15.2	102	19.8	133
11.4	103	13.7	123	15.4	104		
11.5	103	13.8	124				
11.6	104						

TABLE 2: 9-coulter Drill—Equivalent in Pounds per Acre of Amounts Actually Sown (Coulters at 7 in. spacing)

12 chains

6 reps. of 2-ch or 4 reps. of 3-ch plots

Wt. sown	Lb/acre	Wt. sown	Lb/acre
Lb		Lb	
7.6	80	10.0	105
7.7	81	10.1	106
7.8	82	10.2	107
7.9	83	10.3	108
8.0	84	10.4	109
8.1	85	10.5	110
8.2	86	10.6	111
8.3	87	10.7	112
8.4	88	10.8	113
8.5	89	10.9	114
8.6	90	11.0	115
8.7	91	11.1	116
8.8	92	11.2	117
8.9	93	11.3	118
9.0	94	11.4	119
9.1	95	11.5	120
9.2	96	11.6	122
9.3	97	11.7	123
9.4	98	11.8	124
9.5	100	11.9	125
9.6	101	12.0	126
9.7	102	12.1	127
9.8	103	12.2	128
9.9	104	12.3	129
		12.4	130

18 chains

6 reps. of 3-ch plots

Wt. sown	Lb/acre	Wt. sown	Lb/acre
Lb		Lb	
11.3	79	15.1	105
11.4	80	15.2	106
11.5	80	15.3	107
11.6	81	15.4	108
11.7	82	15.5	108
11.8	82	15.6	109
11.9	83	15.7	110
12.0	84	15.8	110
12.1	85	15.9	111
12.2	85	16.0	112
12.3	86	16.1	112
12.4	87	16.2	113
12.5	87	16.3	114
12.6	88	16.4	115
12.7	89	16.5	115
12.8	89	16.6	116
12.9	90	16.7	117
13.0	91	16.8	117
13.1	91	16.9	118
13.2	92	17.0	119
13.3	93	17.1	119
13.4	94	17.2	120
13.5	94	17.3	121
13.6	95	17.4	122
13.7	96	17.5	122
13.8	96	17.6	123
13.9	97	17.7	124
14.0	98	17.8	124
14.1	98	17.9	125
14.2	99	18.0	126
14.3	100	18.1	126
14.4	101	18.2	127
14.5	101	18.3	128
14.6	102	18.4	129
14.7	103	18.5	129
14.8	103	18.6	130
14.9	104	18.7	131
15.0	105		

16 chains

8 reps. of 2-ch plots

Wt. sown	Lb/acre	Wt. sown	Lb/acre
Lb		Lb	
10.2	80	13.5	106
10.3	81	13.6	107
10.4	82	13.7	108
10.5	82	13.8	108
10.6	83	13.9	109
10.7	84	14.0	110
10.8	85	14.1	111
10.9	86	14.2	112
11.0	86	14.3	112
11.1	87	14.4	113
11.2	88	14.5	114
11.3	89	14.6	115
11.4	90	14.7	115
11.5	90	14.8	116
11.6	91	14.9	117
11.7	92	15.0	118
11.8	93	15.1	119
11.9	93	15.2	119
12.0	94	15.3	120
12.1	95	15.4	121
12.2	96	15.5	122
12.3	97	15.6	123
12.4	97	15.7	123
12.5	98	15.8	124
12.6	99	15.9	125
12.7	100	16.0	126
12.8	101	16.1	126
12.9	101	16.2	127
13.0	102	16.3	128
13.1	103	16.4	129
13.2	104	16.5	130
13.3	104	16.6	130
13.4	105		

24 chains

12 reps. of 2-ch or 8 reps. of 3-ch plots

Wt. sown	Lb/acre	Wt. sown	Lb/acre
Lb		Lb	
15.2	80	20.2	106
15.4	81	20.4	107
15.6	82	20.6	108
15.8	83	20.8	109
16.0	84	21.0	110
16.2	85	21.2	111
16.4	86	21.4	112
16.6	87	21.6	113
16.8	88	21.8	114
17.0	89	22.0	115
17.2	90	22.2	116
17.4	91	22.4	117
17.6	92	22.6	118
17.8	93	22.8	119
18.0	94	23.0	120
18.2	95	23.2	122
18.4	96	23.4	123
18.6	97	23.6	124
18.8	98	23.8	125
19.0	100	24.0	126
19.2	101	24.2	127
19.4	102	24.4	128
19.6	103	24.6	129
19.8	104	24.8	130
20.0	105	25.0	131

FERTILISER WEIGHT TABLE

(See Conversion Factors on page 143)

		A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R
Plot size	Acre	1/1089	1/1000	1/666.7	1/484	1/444.4	1/434.8	1/348.4	1/311.1	1/290.4	1/266.7	1/250	1/200	1/166.7	1/160	1/139.7	1/100	1/50	1/20
Rates	Feet	5 x 8			3 x 30		5 x 20	5 x 25	7 x 20	5 x 30						8 x 39			
per	Links			10 x 15		15 x 15					15 x 25	20 x 20	20 x 25	20 x 30	25 x 25		20 x 50	20x100	25x200
acre	Sq. lks.	91.8	100.0	150.0	206.6	225.0	230.0	287.0	321.4	344.3	375.0	400.0	500.0	600.0	625.0	716.0	1000	2000	5000
8 oz	gm	0.21	0.23	0.34	0.47	0.52	0.53	0.66	0.74	0.79	0.86	0.92	1.15	1.32	1.44	1.64	2.27	4.54	11.34
10 oz	gm	0.26	0.28	0.42	0.59	0.64	0.65	0.81	0.91	0.98	1.06	1.13	1.42	1.70	1.77	2.03	2.84	5.67	14.17
1 lb	gm	0.42	0.45	0.68	0.94	1.02	1.04	1.30	1.46	1.56	1.70	1.81	2.27	2.72	2.84	3.25	4.54	9.07	22.68
2 lb	gm	0.83	0.91	1.36	1.87	2.04	2.09	2.60	2.92	3.12	3.40	3.63	4.54	5.44	5.67	6.49	9.07	18.14	45.40
3 lb	gm	1.25	1.36	2.04	2.81	3.06	3.13	3.91	4.37	4.69	5.10	5.44	6.80	8.16	8.51	9.74	13.61	27.21	68.08
5 lb	gm	2.08	2.27	3.40	4.69	5.10	5.22	6.51	7.29	7.81	8.50	9.07	11.34	13.60	14.17	16.24	22.68	45.35	113.48
8 lb	gm	3.33	3.63	5.44	7.50	8.16	8.35	10.42	11.66	12.50	13.61	14.52	18.14	21.76	22.68	25.98	36.28	72.56	181.56
10 lb	gm	4.16	4.54	6.80	9.37	10.21	10.43	13.02	14.58	15.62	17.01	18.14	22.68	27.20	28.35	32.47	45.36	90.72	226.80
20 lb	lb												0.10	0.12	0.12	0.14	0.20	0.40	1.0
20 lb	gm	8.3	9.1	13.6	18.7	20.4	20.9	26.0	29.2	31.2	34.0	36.3	45.4	54.4	56.7	64.9	90.7	181.4	453.6
25 lb	lb											0.10	0.13	0.15	0.16	0.18	0.25	0.50	1.25
25 lb	gm	10.4	11.3	17.0	23.4	25.5	26.1	32.5	36.4	39.0	42.5	45.4	56.7	68.0	70.9	81.2	113.4	226.8	567.0
28 lb	lb									0.10	0.11	0.11	0.14	0.17	0.18	0.20	0.28	0.56	1.40
28 lb	gm	11.6	12.7	19.0	26.2	28.6	29.2	36.4	40.8	43.7	47.6	50.8	63.5	76.2	79.4	90.9	127.0	254.0	635.0
56 lb	lb				0.12	0.13	0.13	0.16	0.18	0.19	0.21	0.22	0.28	0.34	0.35	0.40	0.56	1.12	2.80
56 lb	gm	23.3	25.4	38.1	52.5	57.2	58.4	72.9	81.6	87.5	95.2	101.6	127.0	152.3	158.8	181.8	254.0	508.0	
1 cwt	lb	0.1	0.1	0.2	0.2	0.2	0.2	0.3	0.4	0.4	0.4	0.4	0.6	0.7	0.7	0.8	1.1	2.2	5.6
1 cwt	gm	46.6	50.8	76.2	105.0	114.3	116.8	145.8	163.2	174.9	190.5	203.2	254.0	304.6	317.5	363.5	508.0		
2 cwt	lb	0.2	0.2	0.3	0.5	0.5	0.5	0.6	0.7	0.8	0.8	0.9	1.1	1.3	1.4	1.6	2.2	4.5	11.2
2 cwt	gm	93.3	101.6	152.4	209.9	228.6	233.7	291.6	326.6	349.9	381.0	406.4	508.0	609.3	635.2	727.3			
300 lb	lb	0.3	0.3	0.5	0.6	0.7	0.7	0.9	0.9	1.0	1.1	1.2	1.5	1.8	1.9	2.2	3.0	6.0	15.0
300 lb	gm	125.0	136.1	204.1	281.2	306.2	313.0	390.6	437.4	468.6	510.2	544.3	680.4	816.0	850.5	974.1			
3 cwt	lb	0.3	0.3	0.5	0.7	0.8	0.8	1.0	1.1	1.2	1.3	1.3	1.7	2.0	2.1	2.4	3.4	6.7	16.8
3 cwt	gm	140.0	152.4	228.6	314.9	342.9	350.5	437.4	489.9	524.8	571.5	609.6	762.0	914.0	952.5	1091.0			
1 ton	lb	2.1	2.2	3.4	4.6	5.0	5.2	6.4	7.2	7.7	8.4	9.0	11.2	13.4	14.0	16.0	22.4	44.8	112.0

Conversion Factors

(For use with table on page 142)

0.10 lb = 45.4 gm = 1.6 oz
1 oz = 28.35 gm
1 ton = 1,016 kg
1 acre = 100,000 sq. lks. = 4,840 sq. yd. = 43,560 sq. ft.
1 lk = 0.66 ft = 7.92 in.
1 ft = 1.515 lks
1 sq. ft. = 2.296 sq. lks.

1 lb = 453.6 gm
2 lb = 907.2 gm
3 lb = 1,360.8 gm
1 cwt/acre approx. = 10.5 gm/sq. yd.
300 lb/acre approx. = 1 oz/sq. yd.

Chains		Sq. lks.
⅛ x ½	=	625
¼ x ½	=	1,250
¼ x ⅝	=	1,562
1/5 x 1¼	=	2,500
½ x ½	=	2,500
½ x 2	=	10,000
6 ft x ½ ch	=	455
6 ft x 1 ch	=	911
7 ft x 1 ch	=	1,063
120 ft x 3 ch	=	54,550
5 coulters x 3 ch	=	1,330
7 coulters x 2 ch	=	1,240
7 coulters x 3 ch	=	1,860

This is based on coulters at 7 in. spacing.

Square Links		
20	=	B/5
25	=	B/4
50	=	B/2
250	=	L/2
300	=	3B
625	=	N
3,000	=	P + Q
4,000	=	2Q
4,500	=	2Q + L
6,000	=	P + R

Feet		
3 ft x 120 ft	=	4D
5 ft x 5 ft	=	G/5
5 ft x 43 ft 7 in.	=	L
3 ft 4 in. x 40 ft	=	2C
30 ft x 37 ft	=	2,549 sq. lks
40 ft x 60 ft	=	16 I
39 ft x 81 ft	=	7,250 sq. lks
10 ft x 40 ft	at	1 cwt = 465.4 gm

Links				
12½	x	12½	=	N/4
12½	x	25	=	N/2
15	x	30	=	2E
15	x	40	=	4C
15	x	60	=	K + L
20	x	62½	=	2N
20	x	75	=	L + P
20	x	125	=	Q + L
20	x	150	=	P + Q
20	x	160	=	8K
25	x	50	=	2N
25	x	60	=	3L
25	x	75	=	3N
25	x	100	=	Q + L
25	x	120	=	P + Q
25	x	150	=	6N
25	x	160	=	2Q
30	x	15	=	2E
30	x	100	=	P + Q
30	x	120	=	6M
33⅓	x	120	=	4,000 sq. lks. (approx.)
40	x	75	=	P + Q
40	x	100	=	2Q
45	x	100	=	2Q + L
50	x	50	=	4N
50	x	80	=	2Q
50	x	120	=	3Q
50	x	175	=	14N
55	x	70	=	3,850 sq. lks.
55	x	250	=	13,750 sq. lks.
60	x	75	=	9L
60	x	80	=	8M

APPENDIX 3 — FERTILISERS USED IN TRIALS

The following list gives details of typical analyses of fertilisers commonly used in experiments. It is in alphabetical order.

It should be realised that these materials are liable to vary greatly in purity and that for trials material of known composition is desirable.

Solubility

In the table below the following abbreviations have been used: insol., insoluble; sol., soluble; v. sli. sol., very slightly soluble; v. sol., very soluble; sli. sol., slightly soluble; dec., decomposes.

Element	Formula	Molecular weight	Solubility in water	Percentage element present	Other nutrients
BORON	B	10.8	Insol.	—	—
Borax	$Na_2B_4O_7.10H_2O$	381	Sol.	11.3	—
Sod. metaborate	$Na_2B_2O_4.4H_2O$	204	Sol.	10.6	—
Fertiliser borate	$Na_2B_4O_7.4H_2O$	—	Sol.	13-14	—
Borated superphosphate	—	—	Sol.	0.4-0.8	—
Colemanite	$Ca_2B_6O_{11}.5H_2O$	—	Sli. sol.	10.1	—
Boron frit	—	—	Insol.	4-5	—
CALCIUM	Ca	40	Dec.	—	—
-carbonate	$CaCO_3$	100	V. sli. sol.	40	—
-hydroxide	$Ca(OH)_2$	74	Sli. sol.	54	Hydrated lime may contain 10 lb S/ton
-oxide	CaO	56	Sli. sol.	71	—
-magnesium carbonate (dolomite)	$CaMg(CO_3)_2$	184	V. sli. sol.	22	10-13% Mg
-sulphate (gypsum)	$CaSO_4.2H_2O$	172	Sli. sol.	23	14-19% S
CARBON	C	12	Insol.	—	—
CHLORINE	Cl	35.5	Sol.	—	—
CHROMIUM	Cr	52	Insol.	—	—
Sod. dichromate	$Na_2Cr_2O_7.2H_2O$	298	V. sol.	35	—
Pot. dichromate	$K_2Cr_2O_7$	294	Sli. sol.	35	26.5% K
COBALT	Co	59	Insol.	—	—
-sulphate	$CoSO_4.7H_2O$	281	Sol.	21	11% S
-ised superphosphate				South Is. 1.3 North Is. 0.6	Ca,P,S
COPPER	Cu	63.6	—	—	—
-carbonate (basic)	approx. formula $CuCO_3Cu(OH)_2$	221	Insol.	About 55	—
-sulphate (bluestone)	$CuSO_4.5H_2O$	250	Sol.	25	12.8% S
-oxychloride	Oxychloride base	—	Insol.	50	—
-cuprous oxide	Cu_2O	143	Insol.	89	—
Copperised superphosphate	—	—	—	0.6	Ca,P,S
HYDROGEN	H	1	Sol.	—	—

IRON	Fe	56	Insol.	—	—
-sulphate	$FeSO_4.7H_2O$	278	Sol.	20	11.5% S
-chelates	(for spray application)		—	6-8	—
MAGNESIUM	Mg	24.3	Insol.	—	—
-carbonate (amorphous)	$MgCO_3$	84.3	V. sli. sol.	29	—
-sulphate (Epsom-salt)	$MgSO_4.7H_2O$	246.5	V. sol.	10	13% S
Magnesite	$MgCO_3$	—	—	20-29	Occurs in natural deposits
Talc. magnesite (approx.)	$H_2SiO_33MgSiO_3$ $+MgCO_3$	—	good samples	20	—
		—	poor samples	2-10	—
(New Zealand samples appear to contain large proportion of impurities.)					
Dolomite	$MgCO_3CaCO_3$	184	V. sli. sol.	10-13	22% Ca
Kieserite	$MgSO_4.H_2O$	138	Sol.	17	23% S
Serpentine approx.	$3MgO2SiO_2.2H_2O$	—	—	18-21	—
Dunite	—	—	—	20-24	—
(Serpentine or Dunite if used alone would need to be very finely ground, as these materials are not readily soluble.)					
Serpentine superphosphate	—	—	—	5	Ca,P,S
MANGANESE	Mn	55	Dec.	—	—
-acetate	$Mn(C_2H_3O_2)_2.4H_2O$	245	Sol.	22.5	—
-sulphate	$MnSO_4.4H_2O$	223	Sol.	25	14% S
-manganite	$Mn_2O_3.H_2O$	166	—	62	—
MOLYBDENUM	Mo	96	Insol.	—	—
trioxide	MoO_3	144	Insol.	66.7	—
sod. molybdate	$Na_2MoO_4.2H_2O$	242	Sol.	40	—
Molybdenised superphosphate	North Island ($0.04\%Na_2MoO_4$*)		—	0.016	Ca,P,S
	South Island ($0.06\%Na_2MoO_4$*)		—	0.024	
NICKEL	Ni	59	Insol.	—	—
-carbonate	$NiCO_3$	119	V. sli. sol.	50	—
-sulphate	$NiSO_4.6H_2O$	263	Sol.	22	12% S
OXYGEN	O	16	Sli. sol.	—	—
PHOSPHORUS	P	31	Insol.	—	—
-pentoxide	P_2O_5	142	V. sol.	44	—
Superphosphate	—	—	Sol.	9 (20-22% as P_2O_5)	10-12% S 20% Ca

* Standard levels.

Superphosphate is often contaminated with Mo.

Good lines have 0.5 p.p.m. Mo; lines with more than 2 p.p.m. are not very suitable for experiments which study molybdenum.

Superphosphate may contain up to 100 p.p.m. of Cu (basic slag about 3 p.p.m.), 20 p.p.m. of cobalt (slag about 4 p.p.m.), up to 300 p.p.m. of zinc (slag about 40 p.p.m.). Zinc figures, however, may not be correct. For trace element mowing trials pure calcium monophosphate is available at a price which makes its use for trials practicable. Commercially it is used in the manufacture of baking powders.

Basic slag: High grade basic slag contains up to 18 per cent P_2O_5 citric soluble, 21 per cent P_2O_5 total (lower grade 15.5 per cent and 18 per cent respectively). There are much lower grade slags than these, but they are not being imported. Basic slag contains neutralising constituents equivalent to about 15 cwt $CaCO_3$ per ton; in other words, basic slag may have about the same liming value as 75 per cent pure limestone. It also contains about 8 lb S per ton, 80 lb manganese dioxide, and iron and chromium in appreciable quantities. It contains an average of about½ oz sodium molybdate equivalent per ton (but Mo content varies quite widely) and an average of about 70 lb sodium orthovanadate equivalent per ton. For other trace elements see under superphosphate (page 144).

	P_2O_5	**P**	**S**	**Ca**	**Etc.**
Serpentine superphosphate	15-17	7-7.5	7.5-9	15	Mg 5 per cent
Double superphosphate	45-50	20-22		40	Mo 5-10 p.p.m.
North African rock phosphate	25-28	11-12			Mo 3-15 p.p.m.
Nauru-Ocean Island rock phosphate	36-38	16-17		25-26	Mo 2-5 p.p.m.
Bonedust	15-25	7-11			N 3-5 per cent
Blood and bone (tankage)	8-15	5-7			N 4-8 per cent
Monocalcium phosphate	56	25		30	*Mo 0.3 p.p.m. *Cu 5.4 p.p.m.
Fused magnesium phosphate ex Japan	20-21	9			Ca 21 per cent Mg 11 per cent Mo 5-15 p.p.m. V 3000-5000

p.p.m.

(*Mo and Cu figures refer to analysis of one batch of material.)

The experimental batches of New Zealand made Cal-Mag fertilisers would have similar concentrations of P, Mg, and Ca. Their Mo and V content is not known.

Element	Formula	Molecular weight	Solubility in water	Percentage element present	Other nutrients
POTASSIUM	K	39	Dec.	—	—
-chloride (muriate of potash)	KCL	74.6	Sol.	52 ($60K_2O$)	—
-sulphate	K_2SO_4	174	Sol.	45 ($48K_2O$)	18% S
-nitrate	KNO_3	101	Sol.	39	14% N
NITROGEN	N	14	Sol.	—	—
Ammonium nitrate	NH_4NO_3	80	V. sol.	33	—
Ammonium sulphate	$(NH_4)_2SO_4$	132	Sol.	20	24% S
Monoammonium phosphate	$NH_4H_2PO_4$	—	Sol.	11	21% P
Diammonium phosphate	$(NH_4)_2HPO_4$	—	Sol.	21	23% P
Nitrolime	35% precipitated chalk	—	Sol.	20-21	13% Ca
Nitrochalk	44% nitrate, 48% precipitated chalk		—	15.5	20% Ca
Potassium nitrate	KNO_3	101	Sol.	14	39% K
Sodium nitrate	$NaNO_3$	85	Sol.	16	—
Urea	NH_2CONH_2	60	V. sol.	46	—
Ureaform	formalin + urea	—	—	20-40	—
Dried blood	—	—	Insol.	10-14	—
Blood and bone	—	—	Insol.	4-8	5% P
SODIUM	Na	23	—	—	—
SULPHUR	S	32	Insol.	—	—
Superphosphate	—	—	Sol.	10-12 S	20% Ca, 9% P
Gypsum	$CaSO_4.2H_2O$	172	Sli. sol.	14-19	23% Ca
(See also sulphates of ammonia, copper, magnesium, potassium.)					
TUNGSTEN	W	184	Insol.	—	—
Sod. tungstate	$Na_2WO_4.2H_2O$	330	Sol.	56	—
Scheelite	$CaWO_4$	288	Insol.	50-65	8% Mo
VANADIUM	V	51	Insol.	—	—
(see also basic slag)					
Ammonium metavanadate	NH_4VO_3	117	Sol.	43.5	12% N
Sod. orthovanadate	$Na_3VO_4.16H_2O$	472	V. sol.	10.8	—
ZINC	Zn	65.4	Insol.	—	—
-carbonate	$ZnCO_3$	125.4	V. sli. sol.	52	—
-sulphate	$ZnSO_4.7H_2O$	287.5	V. sol.	23	11% S

Zinc powder can also be used for pastures and crops.

APPENDIX 4 — PESTICIDES USED IN TRIALS

The types and commercial formulations of weedkillers and insecticides change very rapidly, and any published list would soon be out-of-date. Persons undertaking trials should see that they are supplied with up-to-date information. In New Zealand the Agricultural Chemicals Board publishes lists of available registered products and the Department of Agriculture has prepared a notebook giving recognised common names for pesticides.

Calculation of Quantities

For liquids

20 fl. oz. in 1 pint or 160 fl. oz. in 1 gallon

28.4 c.c. in 1 fl. oz.

For solids

16 oz in 1 lb

28.4 gm in 1 oz or 453.6 gm in 1 lb.

To assist in calculating the quantity of pesticides on a per plot basis it is a good idea to standardise the plots to 25 links × 25 links or 50 links × 12½ links. These areas are 1/160 of an acre, so that every fluid ounce of material applied equals 1 gallon per acre.

Example: Treatment, KMCP (4 lb acid equivalent per gallon) at 1 lb acid equivalent per acre. Plot size, 25 × 25 links.

Amount of material required per plot:

$$\frac{(25 \times 25)}{(100{,}000)} \quad \frac{(\text{or } 1)}{(160)} \times \frac{1}{4} \times 160 \times 28.4 = 7.1 \text{ c.c.}$$

In this formula

$$\frac{1}{160}$$

equals plot size.

¼ equals fraction of 1 gallon to give 1 lb acid equivalent.

160 equals the number of fluid ounces in 1 gallon.

1 fl. oz. = 28.4 c.c.

APPENDIX 5 — STATISTICAL ANALYSIS OF TRIALS BY RANKING

This is a quick method of finding whether or not an experiment shows significant differences between treatments (or varieties). It is not quite as sensitive as the method normally used, and therefore it is possible that there may be a slight difference between the results given by this method and the results computed by the more usual analyses. These differences will not, however, be great. The method gives a quick preliminary indication of the results of trials.

It is most valuable for finding whether there are real differences between treatments (or varieties); it is much less suitable for finding whether one particular variety is significantly better than another particular variety. It may be applied only to trials in simple randomised blocks. It may not be used for factorial trials or split-plot designs.

The method is set out below in seven simple steps.

1. In each replication, rank the varieties (or treatments), that is, the variety with the highest yield is called 1, the next highest 2, and so on. (In Example 1 (see page 149) this means obtaining table (b) from table (a).) If two or more varieties have the same yield, they are each given the mean of the positions they fill. For instance, if the two highest are equal, they each take rank 1½. Or if the third, fourth, and fifth are equal, they each take rank 4.

2. Then add the ranking figures for each variety. This will give the row of figures called “Total” in Example 1.

3. Find the mean of these totals. This may be done by adding across the row and dividing by the number of varieties (in Example 1, $84 \div 6 = 14$). This figure may be checked by use of the formula $\frac{1}{2}m(n + 1)$, where m is the number of replications and n is the number of varieties. (In the example $\frac{1}{2} \times 4 \times 7 = 14$.)

4. Find the differences of each variety total from the mean to get row d in the example. This row should always add to zero.

5. Square each term in d row to give row d^2. (Remember that whether d is + or —, d^2 will be +. For example, the square of —2 is +4.)

6. Add this row across to get the number that we want, and which we call S. (In the example $S = 64$.)

7. Calling number of varieties n and number of replications m, look up the table on page 150 to see whether S is significant. (In the example we see that the 5 per cent level of significance for S is 144 and the value we have is only 64. Therefore we say that we have not found a significant difference between the varieties.)

EXAMPLE 1

(a) Weights in lb per Plot

Varieties

Replications	**A**	**B**	**C**	**D**	**E**	**F**
P	61	62	72	55	68	69
Q	71	68	75	63	58	65
R	60	71	52	64	69	57
S	65	71	73	62	78	59

(b) Varieties Ranked

Varieties

Replications	**A**	**B**	**C**	**D**	**E**	**F**	
P	5	4	1	6	3	2	
Q	2	3	1	5	6	4	
R	4	1	6	3	2	5	
S	4	3	2	5	1	6	
Total	15	11	10	19	12	17	84
d	1	—3	—4	5	—2	3	0
d^2	1	9	16	25	4	9	64

If you have found significance and want to compare two particular varieties, the ranking method is not a very powerful one. Unless there are six or more replications it is not possible to show significance between two varieties. If the number of replications is 6, 7, or 8, it is necessary for one variety to be better than the other in every case for significance to be attained; if there are 9 or 10 replications, one variety must be better than the other in all or in all except one case for significance.

EXAMPLE 2

Varieties Ranked

Varieties

Replication	**1**	**2**	**3**	**4**	**5**	**6**	**7**
1	7	5	4	2	1	3	6

2	7	1	4½	6	2	3	4½
3	7	2½	1	4	2½	6	5
4	7	1	4	6	3	2	5
5	7	1	3½	5	2	3½	6
6	7	1	3	5	4	2	6

In Example 2, variety 1 is significantly worse than each of the others; variety 5 is significantly better than variety 4 and variety 7; but variety 5 is **not** significantly better than variety 3 because in the third and sixth replications 3 is better than 5.

TABLE OF "S" FOR RANKING METHOD
(Calculated according to the method of Friedman, Annals of Math. Stats., 1940)

m = No. of replication	**n = No. of treatments**					
	3	**4**	**5**	**6**	**7**	**8**
3			64	104	157	227
4		50	89	144	217	313
5		63	113	183	277	397
6		76	137	222	336	483
7		90	161	261	395	568
8	49	102	184	300	454	652
9	55	116	209	339	513	737
10	61	129	232	377	572	822

APPENDIX 6 — BIBLIOGRAPHY

(Suggestions for further statistical reading.)

D. J. Finney: Experimental Design and Its Statistical Basis (University of Chicago, 1955).

* G. W. Snedecor: Statistical Methods (5th edit., 1956) (Iowa State College Press).

W. G. Cochran and G. M. Cox: Experimental Designs (2nd edit., 1957) (Wiley).

* A. R. Saunders and A. A. Rayner: Statistical Methods with Special Reference to Field Experiments. (Science Bulletin N.200, Government Printer, Pretoria, South Africa, 3rd edit., 1951.)

* Tables of F and t may be found in these publications.

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