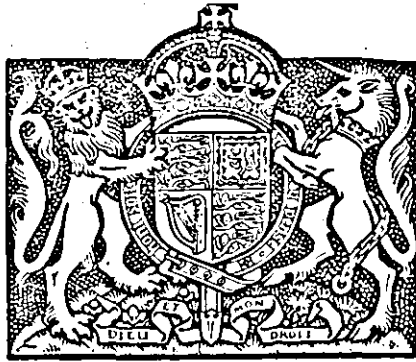


MEMORANDA ON COLONIAL
FERTILIZER EXPERIMENTS



COLONIAL OFFICE

MEMORANDA ON COLONIAL FERTILIZER EXPERIMENTS

I

PLANNING AND CONDUCT OF
FERTILIZER EXPERIMENTS

by A. B. Stewart, M.A., B.Sc., Ph.D.,
F.R.I.C.

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FERTILIZER EXPERIMENTS
IN COLONIAL AGRICULTURE

by E. M. Crowther, D.Sc., F.R.I.C.

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The Colonial Advisory Council of Agriculture, Animal Health and Forestry recently recommended that, in view of the very great importance of increasing the production of crops, especially food crops, steps should be taken to carry out in Colonial territories fertilizer experiments with different crops on all soil types and through the sequence of cropping, with the object of obtaining precise information on the response of crops to the different fertilizers and on the economics of application. The Council suggested that the experiments should include local fertilizer material where available and should take account of the recent advance in the knowledge of the rôle of trace elements.

This recommendation was considered by the Committee for Colonial Agricultural, Animal Health and Forestry Research, which drew attention to the fact that the economic difficulties of introducing artificial fertilizers into native farming had tended to deter authorities hitherto from undertaking extensive fertilizer trials. The Committee also pointed out that in some Colonial territories and, in particular, on plantation crops adequate fertilizer trials were in progress. The Committee endorsed the Council's recommendations for extensive programmes of fertilizer trials in such Colonial territories and on such crops as were at present inadequately served in this respect and referred the matter to its Soils Sub-Committee.

On the request of the Sub-Committee two of its members have prepared the memoranda which follow; Dr. E. M. Crowther on fertilizer experiments in Colonial agriculture and Dr. A. B. Stewart on the planning and conduct of fertilizer trials.

The Secretary of State is much indebted to Dr. Crowther and Dr. Stewart for the trouble they have taken in preparing these memoranda, which will provide valuable guidance for Colonial Agricultural Departments and Research Institutes on the conduct and technique of fertilizer trials.

COLONIAL OFFICE.

May, 1947.

MEMORANDUM ON THE PLANNING AND CONDUCT OF FERTILIZER EXPERIMENTS

by

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This memorandum is based on a "Report on Soil Fertility Investigations in India with Special Reference to Manuring," which was prepared recently by the writer for the Government of India. The report, which embodies a review of the position to date with suggestions for the planning and conduct of future experiments, is meantime being printed in India and should be available in the near future.

The planning and conduct of fertilizer experiments may be considered conveniently under the following heads:—

- (1) General observations, with particular reference to (a) factors affecting crop growth and (b) the need for field experiments.
- (2) Types of field experimentation:—
 - (a) simple experiments in cultivators' fields,
 - (b) detailed experiment work at carefully selected centres.
- (3) Experimental treatments.

(1) General Observations

The ultimate aim of research and experimentation is to provide a sound basis of information of practical value to agriculture. More specifically the objective of soil fertility investigations is to determine what treatments are likely to be needed if the fertility of poor soils is to be improved and that of good soils maintained. It should be clearly borne in mind that acquiring sound information on such points is an essential preliminary to any consideration of the economic implications of measures designed to raise the level of agricultural production.

(a) *Factors affecting crop growth.*—Although agricultural conditions vary enormously in different parts of the world, it is important to remember that the fundamental principles of agricultural science are the same the world over, and failure to realize and allow for the interdependence of numerous individual factors in crop growth may vitiate or, at the best, reduce the value of field experiment work. Such soil factors as depth, drainage, permeability, aeration, physical composition, structure and tilth, as well as the contents of the raw materials from which the plant builds or synthesizes its food, all have an important influence on crop yield. It must be remembered, therefore, in the planning of experimental work that manuring is but one of many factors which may determine final yield. Further, manuring is not something which depends entirely on the crop; simultaneous account must be taken of the functions of individual nutrients, the properties of possible fertilizer supplements, and the properties of the soil to which the supplement is given. The whole aim of manuring is to overcome deficiencies in the soil, and manuring can only be efficient when it is based on a knowledge of the manurial content and other properties of the soil and the factors limiting crop production in the area. In parts of India, for instance, it is found that inherently poor lateritic soils in certain regions of high rainfall have, at present, a much greater agricultural value than have their potentially richer neighbours, the black cotton soils, in rainfall deficient areas, and the interdependence of water and

manure supplies in dry farming areas generally has been well established. Experience also shows that such questions as water, nitrogen and humus requirements, variety and time of planting, and most suitable crop rotations are all intimately linked with soil type. To establish the main classes of soil and their approximate location in a given region is an obvious first step in supplementing experimental data on manuring. Generally it will be found, however, that soil variations within main and sub-types are also factors of major agricultural importance. It is highly important, therefore, that every endeavour should be made to supplement essential and primary field experiment data with observations on climatic, soil and other relevant environmental factors. It is only when such data are available for correlation with the field experiment results that the full value and significance of the latter can be realized.

(b) *Need for field experiments.*—In view of the fact that the manurial and other requirements of soils and crops are influenced to a large extent by natural environmental conditions, it is essential to have, as ultimate criteria, data which have been accurately determined in the field. Pot experiments and laboratory studies, which can be carried out far more cheaply, conveniently and speedily than field experiments, are of undoubted value in the diagnosis of soil deficiencies and should be developed wherever possible. Such methods, however, are arbitrary, and before they can be expected to yield results of direct practical value to agriculture, they must be checked or calibrated against an adequate background of field experiment data. The field experiment is, therefore, an indispensable means of acquiring knowledge of the manurial needs of soils and crops. If a field experiment is to provide reliable information it must, of course, be well planned, well conducted and truly interpreted. The objective of the experiment must be stated clearly, the treatments in the design should be arranged at random, allowance made for necessary replication and consideration given to the number of experiments needed to ensure reliable results. Careful attention must also be given to the choice of site, and relevant data on soil and other environmental conditions in the experimental area should be noted, together with observations on such factors as germination, appearance of the crop, incidence of disease, etc., during the period of the experiment. Finally, careful consideration must be given to the interpretation of the results in the light of possible variations in soil and other agricultural conditions in the area as a whole.

(2) Types of Field Experimentation

It is suggested that consideration should be given to two main types of experimentation, viz. :—

(a) Simple experiments in cultivators' fields ;

(b) research and detailed experiment work at carefully selected centres.

In the former the aim is to obtain as quickly as possible a general picture of manurial response in a major area or tract, whilst in the latter the main objective is to obtain a more detailed picture for individual centres chosen to represent specific soil types and agricultural conditions. Work of both types should be carried out simultaneously.

(a) Simple Experiments

Before any new finding or scientific discovery, which has been indicated either by laboratory work or by field experiments at selected centres, can be considered safe for translation into general agricultural practice, it must be submitted to trial under ordinary farming conditions. The simplest and indeed the only sure means of assessing the value to agriculture of a new discovery is to test it by the field experiment method in fields which are under

ordinary crop production. Experiments in the cultivators' fields are, therefore, essential links in the chain connecting discovery and its application in practice. Quite apart, however, from their importance as essential links between research and practice and their incidental value for purposes of demonstration and education, fertilizer experiments in cultivators' fields have an intrinsic value as a means of acquiring new knowledge, for instance, on response to treatment under a variety of conditions. It is suggested, therefore, that a large number of simple experiments should be undertaken in the fields of cultivators and that the principal objectives of such experiments should be :—

- i. to find the average response to a particular manurial supplement in a given tract ;
- ii. to study as far as possible the interaction of this response with local variations within the tract, and thereby obtain information as a basis for making practical recommendations which would be applicable to specific conditions.

As a "tract" in objective i. above, any suitably compact geographical unit could be selected. If, for instance, an up-to-date and sufficiently detailed soil map of an area were available, the unit might be a soil zone. In most cases, however, information on the soil will be lacking and will have to be obtained while the experiments are going on, and for this reason it will generally be simpler to base the selection of experimental sites on administrative units. By "local variations" in objective ii. above are meant variations in soil, climatic and general agricultural conditions within the tract, detailed information on all of which should be obtained for correlation with the field experiment results, if the final aim of making practical recommendations applicable to specific local conditions is to be achieved. The factors of principal importance in the planning and conduct of these experiments may be summarized as follows :—

Practical limitations.—No one will underestimate the difficulties of carrying out experiments in areas over which the experimenter does not have direct control. Such difficulties, however, are not insuperable and attainment of the stated objectives is worth considerable effort. Consideration may, for instance, have to be given to such questions as compensation payments or guaranteeing the cultivator against loss, making payment for whatever labour assistance is provided, and so on. The main point to be emphasized under this head, however, is that an experiment in the cultivator's field must, above all, be simple in its conception.

Statistical considerations.—The science of statistics can devise appropriate methods of approach to suit different agricultural circumstances, and the statistician and the agriculturist together should work out details of the plan of experimentation which will be most suitable for a given tract. If, for instance, because of practical agricultural conditions it is very difficult or doubtfully possible to carry out a fully replicated trial in individual fields where replication in different fields would be feasible, the latter should most certainly be undertaken, the statistician in this case being guided by the agriculturist. As against this, the agriculturist must realize that certain fundamental requirements must be met, if a series of experiments is to yield reliable and representative results from which valid conclusions can be drawn. Crop yield and response to manuring in a given tract are both determined by the simultaneous operation of a larger number of factors of which the experimenter is ignorant at the outset and, because of this ignorance, it is necessary that he should make use of the principle of randomization to ensure that the result is truly representative of the conditions under which the experiment is conducted. It is suggested that the scheme of experimentation in cultivators' fields should be

based on the carrying out of a statistically adequate number of extremely simple experiments, and that even duplication of the experimental treatments within one field should be sacrificed in favour of increasing the number of individual fields in which the experiments are conducted. In experiments of this type the results in any one field have by themselves little meaning. It is the total number of results which counts, and hence if an odd experiment does happen to be spoiled this will not seriously detract from the value of the results as a whole.

In the choice of a site for any experiment, consideration must be given to two possibilities, random selection and deliberate selection, and the method of selection to be adopted in any given instance must depend on the aims or objectives of the experiments, coupled with the knowledge already available. Deliberate selection implies a pre-knowledge of the effects of individual factors which influence crop yield and manurial response and, for detailed experimental work of the type referred to later, it is obviously necessary to make use of such knowledge where possible. To attain objective ii. above, a case for the deliberate selection of sites might also be argued. To attain objective i., however, where pre-knowledge of the numerous factors involved in crop growth is inadequate, it would be desirable, if possible, to adopt the principle of random selection of sites, e.g., according to the methods devised by Sukhatme, Panse and others in India.^{1,2} Failing this, an endeavour should be made to select sites which are as representative as possible of the tract as a whole, and final details of the plan of experimentation should be arranged in consultation with a competent statistician.

Design of experiments.— A procedure which has the great merit of simplicity and which undoubtedly makes a greater appeal to the average small scale farmer than does the laying down of numerous small plots in his fields is the following: Divide the field arbitrarily into three equal or otherwise suitable portions. In one portion, amounting to one third of small fields or a suitably larger fraction of large fields, the crop is grown according to the cultivator's normal practice and serves as the control treatment for experimental purposes. In the other two portions of the field or selected area, experimental treatments are simply superimposed on the cultivator's normal practice. Wherever possible, yield data should be obtained from a randomly selected plot within each portion of the field; in these simple experiments, however, the value of assessment by visual diagnosis should not be underestimated. Where it is not found possible to obtain exact yield data for all centres, assessment by visual scoring will serve to show whether the experimental treatments have materially affected the yield.

A possible objection to subdivision of a whole field on the above lines might be made on the grounds that it involves treating an unnecessarily large area, and thus increases the cost of the experiment. The dimensions of the areas to be treated must, of course, depend on local circumstances, but it should be remembered that the risk of an experiment being unwittingly spoiled by the cultivator will certainly be less if the design of the experiment is simple. For this reason it is suggested that experimental treatments be applied to whole strips or fractions of a field in which the cultivator can carry out readily his normal cultivation operations, and the aim should be to conduct these experiments in as large a number of fields as possible. To allow for seasonal

1. *Sukhatme*: Report on Crop-Cutting Experimental Survey for Estimating the Out-turn of Wheat. Punjab 1943-44. I.C.A.R. Bulletin.

Sukhatme: Progress Report of Scheme for Crop-Cutting Survey on Paddy in Tangore District, Madras. 1944-45. I.C.A.R. Bulletin.

2. *Panse, Kalamkar and Shaligram* (1945): A Large Scale Yield Survey of Cotton. Current Science, 14, 287.

variations, experiments in a given tract should be repeated annually over a period of at least three years, before any attempt to draw final conclusions is made. Under Indian conditions, for instance, it is calculated that, in a scheme of simple experiments on the above lines involving the principle of random selection of sites at all stages within a district (say 3,000 square miles) as a major unit, approximately 30 experiments per crop per annum over a period of three years will be necessary to detect a response of the order of 10 per cent. between the cultivator's normal and the experimental treatment.

(b) Research and Experimentation at Carefully Selected Centres

In general terms the main objective of work of this type is to build for the future on a basis of ever-increasing knowledge by obtaining more adequate information on the individual, aggregate and cumulative effects of numerous growth factors under different conditions of soil, climate and cropping. It is desirable, therefore, to make provision for the study of several factors simultaneously in both long- and short-term experiments of suitable design, *i.e.*, it is desirable to conduct general experimental and research work on the modern lines now adopted in most countries. The information so obtained must next be interpreted in terms of improvement measures suitable for translation into agricultural practice, either directly or through the medium of simple experiments in cultivators' fields.

The method of random selection of sites, which has the great advantage of eliminating personal predilections on the part of the experimenter and which has been suggested above for simple experiments, is obviously impracticable for limited numbers of detailed experiments. For the latter, deliberate selection of sites must be adopted, and great care must be exercised to ensure that the centre chosen is indeed a representative one. Availability of land and accessibility of a particular site are factors of undoubted importance in the selection of an experimental centre, but they are nevertheless factors of secondary importance to suitability of the centre for the end in view. The first need is to select centres which are typical of each major soil group and each major climatic belt. Next, an endeavour should be made to select centres which will represent varying conditions of depth and texture within each major group, *e.g.*, deep, medium deep and shallow soils of heavy, medium and light textures. Account should also be taken of such factors as: prevailing cropping practices; marked differences in existing fertility levels; water supply position as it affects the need for experiments under both irrigated and unirrigated conditions or, in rain-fed tracts, the need for the measurement of fertilizer response in banded and unbanded areas, etc.

(3) Experimental Treatments

The final choice of treatments to be studied in experiments either of the simple or of the detailed type must rest with the responsible local authorities, who have knowledge and experience of the agriculture of the district. Such general considerations as the following might, however, be borne in mind by the officers-in-charge :—

(i) *Form of fertilizer supplement.*—Whilst in any experimental programme priority should certainly be given to the study of the manurial value of those materials which are available locally, there is also a need for experimental work on ordinary commercial fertilizers, which could doubtless be made available if the evidence in their favour should prove sufficiently strong. In particular, in large countries where distances are enormous and transport costs are consequently high, the manurial value of concentrated forms of fertilizers, which can be transported at a relatively cheaper rate per unit of plant food, should be carefully investigated. Urea, triple superphosphate, ammonium nitrate and T.V.A. metaphosphate serve as examples of such products.

(ii) *Rates, times and methods of application.*—In addition to comparing fertilizers in different forms, attention should also be given to rates, times and methods of application of the ordinary fertilizers supplying the major plant food substances nitrogen, phosphorus and potassium. In acid soils the effects of liming may also have to be studied. As already indicated, the optimum rate of application of a fertilizer will depend not only on the crop to be grown, but also on the properties of the soil and the climatic conditions. Whereas phosphate and potash can usually be applied with greatest advantage before a crop is sown or planted, the optimum time of application of nitrogenous compounds may have to be determined. In India, for instance, general experience indicates that : (a) for crops of long-growing period such as sugarcane part of the nitrogenous dressing should be given at planting and the remainder in the form of top dressings, *e.g.*, at earthing-up times ; (b) for cotton grown under irrigated conditions a nitrogenous top dressing at the time of flowering is likely to be superior to application at other times ; (c) for crops of short-growing periods nitrogen in readily available form can usually be applied with greatest advantage as an early application about the time of sowing.

As regards methods of application of fertilizers, recent work in various parts of the world suggests that : (a) placement of phosphate in bands or rows at a few inches depth may be superior to ordinary broadcast application ; (b) for unirrigated crops in regions of deficient rainfall drilling or placement of certain nitrogenous fertilizers at the side of and slightly distant from the plant rows may be superior to broadcasting the fertilizer in the ordinary way. Account of these and similar possibilities should therefore be taken in drawing up experimental programmes.

(iii) *Trace elements.*—It is possible that, if crop yields are increased following improvements in irrigation and manuring, and the introduction of improved seeds and varieties and similar agricultural improvement measures on an extensive scale, problems of trace element deficiencies may become more acute through consequent increase in the rate of depletion of natural soil reserves. Provision, therefore, should also be made for a certain amount of work on trace elements. In a recent monograph, Wallace¹ has described in detail how test plots, combining indicator plants and fertilizer treatments, can be arranged to provide information of value in the diagnosis of mineral deficiencies in soils and crops. To facilitate cultural operations, the crops are grown in strips running the whole length of the experimental area, and the various fertilizer treatments, which include both major nutrients and trace elements, are randomized in blocks. Experiments of this type are of particular value in characterizing rapidly, by the method of visual diagnosis, the nutrient status of land of unknown cropping potentialities. It is suggested, therefore, that a considerable number of these observational experiments should be undertaken as a preliminary to more detailed work on both major and trace element deficiency problems. Such experiments are likely to be particularly valuable in regions where existing information on crop yields and soil properties is scanty.

(iv) *Use of fertilizers in the production of humus.*—A further general problem, of which account should also be taken in a fertilizer experiment programme, concerns the use of fertilizers in the production of humus and in the growing of legumes for the improvement and maintenance of soil fertility. In India the phosphate manuring of berseem (Egyptian clover) has given very promising results in long-term rotation experiments. Green manuring with both leguminous and non-leguminous crops has also given promising results in certain

1. Wallace (1943) : The Diagnosis of Mineral Deficiencies in Plants. A Colour Atlas and Guide. H.M. Stationery Office, London.

ropical soils, and may be worthy of more detailed investigation in long-term oil fertility work. Under Indian conditions the main requisite for the success of green manuring appears to be the availability in the soil of sufficient moisture, equivalent to at least 5 to 6 acre inches of water, to ensure the decomposition of the green crop after inversion.

(v) *Choice of treatments in individual experiments.*—It has already been suggested that the final choice of experimental treatments should rest with local officers-in-charge. Usually it will only be the latter who are able to decide, for instance, whether a particular treatment is suitable for inclusion in a scheme of simple experiments such as has been outlined, or whether it should first be subjected to further trial in a scheme of detailed experimentation at selected centres. In general, treatments in simple experiments in cultivators' fields should be based on what has shown most promise in past work, either in the tract in question or, failing this, in other countries with roughly comparable climatic conditions. Under tropical conditions, where nitrogen deficiency is likely to be widespread, it would appear reasonable, in simple experiments, to give high priority to experimental treatments involving nitrogenous manures or fertilizers. It should also be remembered, however, that in many areas where phosphate by itself may have little effect on yield, a combination of nitrogen and phosphate may be markedly superior to nitrogen alone. Again a mixture of nitrogen, phosphate and potash may be superior to either ingredient by itself.

In conclusion, it is to be emphasized that the foregoing are intended to serve merely as examples and not as a comprehensive list of points which should be borne in mind in the planning of fertilizer experiments in regions where little accurate information on crop yields and soil properties may, at present, be available.

November, 1946.

FERTILIZER EXPERIMENTS IN COLONIAL AGRICULTURE

by

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Introduction

1. In a memorandum on Fertilizer Experiments (15666/45 of 16th September, 1946), the Colonial Advisory Council of Agriculture, Animal Health and Forestry recommended that "steps should be taken to carry out in Colonial territories fertilizer experiments with different crops on all soil types and through the sequence of cropping with the object of obtaining precise information on the response of crops to the different fertilizers and on the economics of application. The experiments should include local fertilizer material where available and should take account of the recent advance in the knowledge of the rôle of trace elements." The Council also recommended that the advice of the Colonial Agricultural Research Committee and its Soils Sub-Committee should be obtained on the laying-out of the fertilizer experiments with a view to preparing recommendations for the use of Colonial Agricultural Departments.

Drs. A. B. Stewart and E. M. Crowther were invited by the Soils Sub-Committee to prepare memoranda on the subject and, by arrangement, they decided to deal independently with two separate aspects of the problem. Dr. A. B. Stewart based his memorandum on his recent and, as yet, unpublished report on soil fertility investigations in India. He gave especial attention to co-ordinated series of simple experiments in cultivators' fields, designed to see how far general recommendations based on results at a few experimental stations could be applied economically over wide areas.

The present memorandum is supplementary to Dr. Stewart's. It deals especially with some of the problems of design which arise in new investigations where there is little previous local experience on either the most appropriate technique or the kind of results to be expected.

Plantation crops

2. Field experimentation of the highest standard has been going on for over 20 years on some plantation crops. The Gezira Research Farm at Wad Medani, Sudan, took up Professor R. A. Fisher's new methods in 1926, the year in which they were first adopted systematically at Rothamsted. In 1943 the Gezira Research Farm harvested separately the crops on 2,792 plots, as compared with a total of about 1,700 on the Rothamsted farm. Further, the Gezira Research Farm has several series of long-term crop rotation experiments, some of patterns not hitherto attempted elsewhere. In collaboration with the Sudan Plantations Syndicate simple experiments on the manuring and cultivation of cotton have been conducted on thousands of individual native tenancies.

Excellent field experimental work on manuring has also been in progress for many years in the tea estates of Ceylon, the sugar-cane estates of the West Indies, the rubber estates of Malaya, and, more recently, the oil-palm estates of Nigeria. It may be anticipated that before long there will be good experimental work on groundnuts in Tanganyika.

It is in fact one of the great merits of the plantation system in tropical agriculture that it provides both the stimulus and the opportunity for good field experimental work. Some plantation crops lend themselves fairly readily to field experimentation because the cultivations are made and the crops grown in simpler geometrical patterns than are normal for food crops. Usually

there is only a single perennial crop or a dominant annual one. On the other hand, the recording of yields is often laborious. Thus, there may be many separate pickings of cotton or tea, repeated test-tappings of rubber, weighings of individual bunches of oil palm, etc. The main problem is one of organization, supervision and checking. The successes already achieved have demonstrated beyond any doubt that it is possible with native field staff to carry out the most complex and ambitious programme of field experiments. Indeed one may go further and say that just because land and labour are more easily obtained in the tropics than in Britain, field experimentation on plantation crops is more advanced than on most British crops.

Food crops and native farms

3. Comparatively little has yet been done to test the effects of fertilizers and manures on food crops, especially in native systems of farming. It has commonly been assumed that the economic conditions of peasant farming preclude outlay on fertilizers or other soil improvers. Experience elsewhere, notably in Belgium and Egypt, shows, however, that peasants are not slow to take up fertilizers once their merits have been clearly determined and widely demonstrated. Again, in some countries there was no possibility of developing successful farming systems until fertilizers were introduced. Thus, in parts of South and West Australia wheat growing is impossible without superphosphate. There may be parts of the Colonies in which acute deficiencies of single plant nutrients may be responsible for the current low levels of production. The correction of such deficiencies may transform farming and raise the whole level of nutrition and standard of living.

Where it is believed that no change in agricultural systems or standards of living during the next 50 years could render the use of fertilizers economic, there would still be strong grounds for starting at once both annual and long-term manurial experiments in order to discover the dominant factors in soil fertility. This is particularly urgent where progress has already been made in developing alternative systems of husbandry with either mixed farming and animal manure or resting under leys or long grass. Parallel with all the agronomic and social work involved in developing and testing the new systems, it is necessary to analyse by field experiments the ways in which the old and the new methods affect the fertility of the soil. Sometimes the general approach to these problems seems to be based on little more than the assumption that because farmyard manure and mixed farming are good in Britain, they must necessarily be good everywhere. It is often forgotten that the dressings of organic manure used on demonstration mixed farms are trivial by comparison with those used in Britain, and that added organic matter burns up very rapidly in tropical soils. These considerations suggest that one or other of the major plant foods may often be the principal cause of the benefits observed from farmyard manure. Thus, it has been shown in Northern Nigeria that the effect of farmyard manure on cotton could be matched by a small dressing of superphosphate supplying the same total amount of phosphorus. When the dominant manurial elements have been identified, it will be possible to see how far various systems of husbandry conserve these nutrients. The problem of improving soil fertility may then be formulated more clearly. For example, where supplies of nitrogen and phosphate prove to be of paramount importance ways should be sought for returning to the land rich products such as cotton seed, cotton cake or groundnut cake. At present the model mixed farm in Africa sells off plant foods in its crops and brings nothing back in exchange. Under these conditions mixed farming may "mine the land" even more drastically than native methods. A false antithesis has sometimes been implied between improving crops and

improving soil structure, and fears have been expressed that it might be dangerous to allow native farmers access to fertilizers, because improved crops would "exhaust the soil". Obviously no system of farming can be good if it endangers the soil, but there are many conditions in which it would be easier to improve the soil if there were bigger crops to provide more food for man and beast with more straw for manure.

In some colonies recent discoveries of phosphate rock have served to arouse interest in experiments on fertilizers. I wish to suggest that for some years to come experimental programmes on fertilizers should not be limited to materials available locally, and that all of the early experiments in any region should include recognized and well-tried fertilizers. The general problems involved are too important and urgent to allow any risk of failure through using unsatisfactory materials. It is known that mineral phosphates are quite unsuitable for most crops on neutral soils, and that many of the less common mineral phosphates are of little value for direct use on any kind of crop or soil. Some of the mineral phosphates recently discovered in Africa appear to be of these less active types. I would recommend that wherever possible the newer phosphates should be tested against imported superphosphate or against a well-tried mineral phosphate such as Gafsa. Where only a single kind of phosphate fertilizer can be tested in any experiment, I recommend that it should be superphosphate or Gafsa mineral phosphate and not one of the new local phosphates. The same phosphate should be used as standard for many years in long-term experiments and in series of annual experiments. There will be ample opportunity to work out the practical details for using local phosphates once their value has been established by comparative trials against standard materials over a range of crops and soils.

Experience in Northern Nigeria gives a good warning of the danger of using untried local phosphates. Much publicity was given to an early finding there that farmyard manure improved crops but fertilizers did not, and it is possible that this result affected agricultural programmes in several other colonies as well as in Nigeria. It transpired that the fertilizer which failed was an unusual and untried mineral phosphate from Southern Nigeria. As has already been mentioned, later experiments with equivalent amounts of superphosphate gave results closely similar to those from farmyard manure. It is doubtful whether this important revision of the earlier Northern Nigerian conclusion has permeated as far in tropical agricultural circles as the original finding in favour of the unique virtues of farmyard manure.

General principles of experimental design

4. It has not been found feasible at this stage to draw up a single programme of field experiments for general use in the tropical colonies. The conditions vary so widely that a whole series of separate programmes would be required initially, and these would need to be kept constantly under review in the light of results and facilities. It has seemed better to point out some of the practical applications of general principles set out in R. A. Fisher's "Design of Experiments" and F. Yates' "The Design and Analysis of Factorial Experiments",¹ to add some more recent developments and some practical considerations which have emerged from experience of series of field experiments in Britain and some of the colonies.

Randomization

5. Every experiment must have an element of randomness in the arrangement of the plots. Normally an independent act of randomization must

1. Technical Communications No. 35, Imperial Bureau of Soil Science, 1937.

be performed for each individual experiment, however simple the design. Otherwise, the cumulative results of many experiments may still have a systematic bias. In the extreme case of three observation plots A, B, C, the order of the plots should always be randomized afresh for every centre. Unless this is done A and C will tend to differ from each other more than they do from B, merely because they are always farther apart.

In more complicated experiments the possibility of applying statistical methods of analysis presupposes the element of randomness.

Replication

6. In order that the differences due to treatments may be compared with those differences not under control, it is necessary that similar treatments should be repeated on several plots. Replication within a single experiment is essential if it is desired to estimate the precision of the results of this one experiment. Sometimes it is sufficient to test the consistency of results over a whole series of experiments without aiming at detailed information for the individual centres. Dr. Stewart has discussed such series.

Local control

7. In replicated experiments the precision can be improved by arranging the plots in blocks in such a way that every treatment (or balanced group of related treatments) occurs in each block. This gives the so-called "randomized block" experiment. Soil and other irregularities are likely to be less within the individual blocks than over the whole experimental area, and differences between blocks are eliminated from the treatment contrasts. In laying-out experiments each block should be made as compact and as uniform as possible. The plots may either run across the length of the block or be arranged in window-pane pattern. Long narrow plots in square blocks generally give the most accurate results but the plots may have to be nearly square where "edge effects" are likely to be appreciable.

In the Latin Square arrangements each treatment occurs once in each row and once in each column. Latin Squares of 16, 25, 36 or 49 plots have proved very efficient in balancing out the effects of soil and other inevitable irregularities.

Many variants on these designs have been introduced to meet the special requirements of plant breeders who often have to test large numbers of strains or varieties likely to differ but little from each other. Some of these designs are complicated, and the analysis of the results may require considerable statistical skill. In several experimental farms modern statistical methods were first introduced by plant breeders, and the very complexity of the designs sometimes needed may have been responsible in part for the arbitrary contrast between a "statistical" and a "practical" test. Some of the more recent developments in experiment design are particularly well suited for dealing with the multitude of complicating factors which necessarily enter into "practical" experiments, but which may, if necessary, be omitted from the earlier stages of the plant breeder's trials. Although the principles of complex or factorial experiments were set out fully by Fisher and Yates over ten years ago, their practical value still does not appear to have been widely enough appreciated in colonial agriculture.

Factorial design

8. If all combinations of two or more contrasts are tested factorially all plots contribute some information on each of the factors tested. If the response to one treatment is but slightly affected by variations in other treatments then the finding may be expected to prove applicable over a wider range of conditions than if it had initially been tested under only one rather arbitrarily chosen set of conditions. If, however, the response to one

treatment is profoundly modified by some other treatment, then much will be learnt about the conditions under which response to the first treatment may be expected to recur in practice. Thus, if, as is general, liming reduces the response to mineral phosphate, one has fairly strong presumptive evidence that it would be unwise to recommend mineral phosphate for neutral soils. If, as is usual in Britain, on plots with dung the responses to phosphate are reduced to about one-half and those to potash are reduced to one-third of those on similar plots without dung, this is evidence that the benefit from dung must be in large measure due to the amounts of available phosphate and potash it supplies. Indeed, factorial experiments testing combinations of dung or compost with individual fertilizers provide one of the few ways in which the manifold possible effects of dung can be analysed into simpler terms. Quite often the effects of varietal or cultural treatments are profoundly modified by differential manuring. If two varieties give similar results over a wide range of manurial conditions, they are more likely to give similar results over a wide range of soils. If two cultural treatments give different results on manured and unmanured land, one has learnt something vital about the dominant factors in soil fertility and husbandry under local conditions.

Confounding interactions

9. If many factors were to be included in randomized block experiments of the early design, the experiments would become unwieldy. Many new developments have taken place in recent years to meet this difficulty by the device of identifying certain complex treatment contrasts with those between the blocks into which the plots are grouped. This device has been termed "confounding treatment interactions with blocks."

The general method may be illustrated from the simple 8-treatment test on N, P, K fertilizers. The 8 treatments

Nil, N, P, NP, K, NK, PK, NPK

may be arranged in two sets of four, each of which forms a block, thus

(a) nil, NP, NK, PK

(b) N, P, K, NPK.

Each block still tests each fertilizer in the presence and in the absence of each of the other two, but the interaction between all three fertilizers becomes identified with the differences between the two blocks and is sacrificed in the interest of obtaining smaller blocks and greater control of local irregularities.

Confounding has been very widely and successfully used. It is particularly well suited for preliminary orienting experiments and for testing the needs of a crop over a wide range of soil conditions. One example used on sugar beet at some 20 centres in Britain each year since 1940 tests all 32 combinations of the presence and absence of the elements N, P, K, Na and B in four blocks of eight plots. It generally happens that the effects from boron are small, but boron deficiency is largely seasonal, and, if and when a "boron season" should occur, the experiments will be ready to detect and measure the benefit from added boron. In the meantime they go on piling up evidence over a steadily mounting number of soils and seasons that boron is not a necessary fertilizer for sugar beet on normal soils in most seasons. This evidence is obtained with no additional effort beyond applying the boron with the other fertilizers on half the number of plots. Even if boron had been omitted from the experiment, it would have taken just as many plots to test the other questions at issue. (The methods of arranging such confounding so as to maintain the balance of the experiment are discussed in the Appendix.) There must be many cases in the colonies where some of the less common elements could be systematically tested in this way—either as soil treatment or as sprays—once the nucleus series of factorial experiments is established.

Estimating error from interactions

10. Where there is no absolute replication, as in the 32 plot experiment just mentioned, the error of the experiment is estimated from some of the high-order interactions between combinations of treatments and blocks which can be regarded as of no practical importance.

Fractional replication

11. With larger numbers of treatments it is possible to economize in plots by the device of fractional replication. Thus in the 32 plot experiment on sugar beet already mentioned it would be quite easy to work in another treatment to test something not closely related to the present treatments; e.g., magnesium might be given to half of the plots. The interactions to be confounded must be selected with great care, because, formally, a fractional replication implies that any given difference between sets of plots may be ascribed to two alternative treatment effects (including interactions). This may be illustrated by the example of the 8-treatment NPK test using only four treatments (nil, NP, NK, PK) as a half replicate. The contrast used for estimating the main effect of N is $(NP + NK) - (nil + PK)$, which is also the contrast that would be used for estimating the PK interaction $(PK - NP - NK + nil)$ if the N effect were negligible. Such an extreme example would not be used in practice, and there will normally be no difficulty in ascribing any given difference to the more obvious and simpler treatment effect.

Fractional replication provides a most useful development for pioneering experiments where there is little previous experience. A single comprehensive test on many factors may often give valuable information more quickly than a long series of single-factor experiments, because several of the conditions necessary for success may be lacking in many of the single-factor experiments. Thus, tests on minor elements alone would be a waste of time if nitrogen were the limiting factor.

Split-plots

12. In the designs already considered it has been assumed that all plots are of similar form and size and are arranged in randomized blocks. Sometimes this is not feasible because certain treatments cannot be applied to very small randomized plots. Thus cultivations, sowing dates, irrigations may have to be given to large plots, but each of these main plots may still be subdivided or "split" for two or more subsidiary treatments, such as treatment with fertilizers or time and method of harvesting. Where plots are split in this way it must always be remembered that the results from the main treatments on a few large plots are necessarily less accurate than those from the subsidiary treatments tested on a much larger total number of sub-plots. Split plot experiments have two (or more) standard errors and they often yield the disturbing result that an apparently large effect from a main treatment may not be significant when a small effect from a minor treatment on split plots may be highly significant. Caution should be exercised in introducing as split-plots treatments which could equally well be included factorially as main-plots. Splitting should never be used unless practical conditions dictate it, or unless the extra work in harvesting fractions of main plots is relatively small. It is often convenient to confound interactions for split plots. Thus half of the splits in an experiment might be A and B, and the other half nil and A + B, thus confounding the interaction between A and B.

Analyses of variance and statement of results

13. Analyses of variance are required to obtain the full information from a set of experiments, and especially to check the validity of some of the more complicated comparisons and to decide whether the design and amount of replication are adequate for the purpose in view. It is not, however, necessary

in most statements of the results of field experiments to set out the analysis in full. Indeed, it is often unnecessary to work out the analysis. No one should refrain from using modern designs merely because he lacks a calculating machine or clerical assistance for all the heavy computations which may sometimes be required. All designs recommended in this memorandum will give reliable and unbiased results by simple addition and subtraction, provided care is taken to tabulate results only for those treatments which are represented in every block of the experiment. No sets of results should be given for any contrasts confounded with blocks unless steps have first been taken to correct the individual plot yields for block differences. It is a useful precaution to record the confounded treatments on the plans of the experiment as a warning to other workers who may make observations or take yield data from the experiments.

It is far more important to understand the principles of experimental design than to be skilled in the actual work of computation. Properly planned experiments will provide unbiased and unambiguous results even without formal analyses of variance.

The need for experience or advice is particularly great for long-term experiments, which are notoriously difficult to plan well both on statistical and agricultural grounds. But experience or advice are just as necessary in planning preliminary orienting experiments where there is little previous local information. One may venture the almost paradoxical generalization that the less one knows about the conditions to be tested and the less elaborate the field technique, the more important it is to use the more advanced designs. All experiments require some selection of treatments and local conditions from a vast range of possible factors, known and unknown, and it is essential, if progress is to be at all rapid, to select most carefully the actual sample of conditions to be tested. This is obvious for perennial tree or bush crops, because it would take too long to do preliminary "look-see" trials, and in any event these would probably be too inaccurate to give any clear indications. The need for most careful planning is just as great in tests on annual crops. Obviously no long-term or elaborate series of short-term experiments should be embarked on until it is known that the cultural treatments are technically feasible, but once this stage has been reached the various "practical" tests should be balanced and randomized in such a way as to allow a reliable over-all picture to emerge as the series is extended.

Observational experiments

14. Very valuable information can often be obtained from series of purely observational experiments in which the results of treatments are estimated only visually—preferably by scores on a scale of 1 to 10. It is often possible in this way to extend the results of a few trials at experiment farms to a wide range of practical conditions. Observational experiments can even be carried out where, through labour and transport difficulties, it would be impossible to obtain full yield data. Thus useful information has been obtained in Britain in tests on alternative phosphate fertilizers for the establishment of leys by making purely visual judgments of the "take" of seeds, or on alternative methods of applying phosphates for cereals by visual estimates of yields. There must be many places in Africa where the effects of phosphate and other fertilizers could be judged sufficiently well by crop establishment. Such simple experiments should always be randomized and should generally have factorial combinations of treatments. Where quantitative estimates of the effects of treatments are required it will be necessary to calibrate the eye estimates by determining actual yields on a number of experiments.

Useful information can be obtained from a series of local trials using the simplest of all possible experimental units. Patches of a crop in native gardens

could be divided into differently treated halves to provide a series of two-plot blocks. Such comparisons need not be restricted to testing single factors. Two factors, e.g., N and P, can be tested in this way by arranging the experiments in sets of six, to compare all possible pairs of the treatments nil, N, P, N + P, thus :—

- (1) nil and N
- (2) P and NP
- (3) nil and P
- (4) N and NP
- (5) N and P
- (6) nil and NP

The first four pairs give direct tests of each fertilizer alone and in the presence of the other, and the last pair gives the contrasts needed to balance the series and allow both the average effects of N and P and their interaction NP to be determined. Testing combinations of two fertilizers in this way is particularly useful on very poor soils in which both nutriment may be so deficient that adding one alone cannot produce a full effect. Under such conditions simple tests on single fertilizers might give poor results and lead to the erroneous conclusion that the crops did not respond to fertilizers.

It will be shown in paragraph 20 that sets of two-plot blocks can be used to test combinations of three or more treatments, where possible interactions between pairs of factors may be ignored.

Series of annual experiments

15. The general objective for annual experiments should be to build up series to show

- (a) the average responses to fertilizers of the principal crops over a range of local soils ; and
- (b) the average fertilizer requirements of the principal classes of soils.

Such experiments should include factorial combinations of N, P, K treatments, though other factors such as a bulky organic manure and/or limestone may with advantage be included.

With such data it will be possible to work out a rational policy for practical manuring, to determine the economic conditions under which different kinds of manuring would be justified and to provide fundamental information on soil fertility problems for correlation with general agricultural investigations and soil surveys. The relative requirements of the individual crops over a wide range of soils and seasons would gradually be worked out and the visual symptoms of nutrient deficiencies recognized. The special requirements of different kinds of soils would be examined at the same time by correlating the results of the individual experiments with those of soil descriptions, soil surveys and soil analyses. It should, however, be noted that it is not necessary to postpone the experiments until the soil survey is well advanced or until there are full facilities for soil analyses. If the sites of the experiments are carefully recorded and the soil described as well as possible, useful data will be accumulated against the day when the soil surveyor or soil chemist can take a hand in interpreting the detailed results. It may often be useful to preserve soil and crop samples for future analysis.

The series of experiments serving purposes (a) and (b) above may well include additional tests on such questions as

- (c) Comparison of alternative forms of fertilizer (*see* paragraph 18 below).
- (d) Methods and times of applying fertilizer (*see* paragraph 21 below).
- (e) Observations on minor elements (*see* paragraph 20 below).

Some well-tried designs for annual experiments are discussed in paragraphs 16 to 20.

Factorial experiments with single rates

16. The simple NPK test on multiples of eight plots, often with confounding to blocks of four plots, should be used very widely. It has already been recommended that only the recognized standard fertilizers should be used in this sort of experiment.

It will often be possible to include one other treatment, *e.g.*, farmyard manure (D) or limestone (Ca) in all factorial combinations. Thus with a minimum of 16 plots in two blocks of eight, confounding the highest order interaction, the blocks would be

- (a) nil, NP, NK, PK, ND, PD, KD, NPKD ;
- (b) N, P, K, D, NPK, NPD, NKD, PKD.

All five factors N, P, K, Ca, D, can be tested in 32 plots which may go in two blocks of 16 or in four blocks of eight. In general it would probably be better not to confound highly in the earlier experiments where several closely related treatments such as farmyard manure and fertilizers may be expected to affect yields and show considerable interactions. Thus, until some experience had been gained, it would probably be better to use only two blocks of 16 in an experiment on N, P, K, Ca, D.

Factorial experiments at three rates

17. To determine the optimal rate of manuring it is necessary to obtain some estimate of the forms of the response curves. The simplest way is to use a given fertilizer at the rates 0, 1, 2. A design with three factors each at three rates requires 27 plots, and these may with advantage be arranged in three blocks of nine plots. This design on N, P, K fertilizers has been used successfully in many countries during the last 15 years, and it is to be recommended as soon as there is any local interest in the economics of manuring.

A fourth factor may be introduced to make 81 plots, but it is not necessary that the whole experiment should be laid down as a single unit. In a plantation sets of 27 plots in three blocks of nine plots each might well be put down on different soils or on plants of different ages. (A word of warning may be offered against using designs of the type $2 \times 3 \times 3 \dots$ in blocks of six. Although this design may be capable of high accuracy because it has many small blocks, it needs complicated calculations not only to evaluate the errors, but, what is much more serious, to arrive at a valid statement of yields in any of the 3×3 tables. The $3 \times 3 \times 3 \times 3$ design in 81 plots is much more convenient than the $2 \times 3 \times 3 \times 3$ in 54 or 108 plots.)

Comparison of alternative forms of fertilizers

18. In all comparisons of alternative materials the objective is to find what rates of the two materials give similar effects on crops. Many wrong conclusions have been drawn from experiments made with too high dressings of manures. Thus it has often been claimed that "low-soluble" basic slags are almost as good as "high-soluble" basic slags because they gave similar yields of crops when both were used at fairly heavy rates of dressing. The only justifiable conclusion was that both materials supplied sufficient available phosphate at the rates tested. The relative efficiencies of the two materials were not in fact determinable under the experimental conditions.

One or both materials should be used at two rates, the lower one being sufficiently small to allow the three yields from, say, OP, 1P, 2P to fall on a rising part of the yield curve. It would then be possible to see how much of one fertilizer would give the same yield as the other at a point where the yields are still rising. From the average of a number of such experiments one could derive a fairly reliable response curve and from this express the relative efficiencies of the two materials.

Where the size of the response to be expected is uncertain, it is necessary to have an adequate number of plots without the plant food in question, as otherwise the response curve may be ill-determined. A convenient design to compare two phosphate fertilizers is

OP, OP, 1PA, 2PA, 1PB, 2PB

where 1P and 2P might be, say 0.33 and 0.66 cwts. P_2O_5 per acre and PA and PB are different kinds of phosphate fertilizer. Such a design in a Latin Square requires 36 plots, which may be too many for local experiments. To meet this difficulty, especially where one or two less well-known materials are to be compared with a standard, the writer has often used the following treatments in a 4×4 (or 5×5) Latin Square:—

	<i>cwts P_2O_5 per acre</i>
1	nil
2	0.33 as standard
3	0.66 as standard
4	0.5 as first unknown
(5	0.5 as second unknown)

With 16 (or 25) plots it is possible to obtain some idea of the response curve, to compare one (or two) unknowns with about the same amount of standard (*i.e.*, half of 0.33 + 0.66) and to make a direct comparison of the unknowns with a little more and with a little less of the standard. Unless the three (or four) treatments with phosphate show appreciable differences among themselves they will not allow any clear differentiation between the phosphates.

There is, of course, no necessity to test only equivalent or nearly equivalent rates. If one expects one material to be much less effective than another one could test such rates as

Form A 0, 1, 2, 4
Form B 0, 4, 8, 16

Where an experiment on rates and kinds of phosphates is to have a second factor superimposed (*e.g.*, nitrogen and no nitrogen), it is, of course, essential that all treatments, including the "no phosphate," should occur both with and without nitrogen.

Experiments on bulky organic manures and fertilizers

19. It is often necessary in analysing the action of bulky organic manures to separate the effects of the major plant foods from those physical and biological effects more characteristic of the organic matter in the manure. It is rarely feasible to test exactly the same quantities of nutrients in the two contrasted forms, for this would require a heavy programme of rapid chemical analyses immediately before the bulky manure is applied, and, in any event, it is not to be expected that the nitrogen in the organic manure will be as active as that in fertilizers. A more useful method, especially where two or more bulky organic manures are of interest, is to use each at two rates and in conjunction with the presence and absence of fertilizers. A large series of experiments has been conducted successfully in England in recent years with three bulky organic manures (each at two rates) and no organic manure, in all combinations of the presence and absence of sulphate of ammonia, superphosphate and muriate of potash. This makes $4 \times 2 \times 2 \times 2 \times 2$ or 64 treatments, but half-replicate experiments with 32 plots in four blocks were used. An example of the confounding is given in the Appendix. It is believed that this type of experiment would be found useful in colonial agriculture to test such materials as farmyard manure, pen manure and compost.

Experiments on minor elements

20. It has already been explained how tests on one or two minor elements may be incorporated without requiring extra plots in experiments planned

primarily for other purposes. Where through crop failure or unusual symptoms there may be grounds for suspecting minor element deficiencies, observation plots can be laid out to test appropriate salt solutions applied to the foliage through a watering can. The solutions should be given during a period of active growth early in the season, and may be repeated once or twice at fortnightly intervals. Where only a few plots can be used there is much to be said for using factorial combinations of treatments. Thus all combinations of the presence and absence of B, Mn, Cu, Zn, Fe could be tested on 32 plots or even on 16 in a half replicate. Alternatively Mn, Cu, Zn and Fe might be tested as solutions on the foliage of 16 or 8 plots, and B, Na and Mg tested separately as borax, salt and magnesium sulphate (or dolomitic limestone) as applications to the soil before sowing. Until there is positive evidence of definite responses to some of the individual elements, there is little point in endeavouring to assess interactions between minor elements.

Tests on minor elements are particularly necessary where plants show obvious signs of malnutrition. Sometimes these occur patchily giving an irregular site unsuitable for the usual pattern of experiment. Where it is possible to find, say, eight such patches, not necessarily close together, each might be divided at random into halves and an experiment made on eight blocks of two plots. Confounding all first order interaction between four elements, a, b, c, d, the treatments to be given at random to the halves would be :—

<i>Block</i>				<i>Treatment 1</i>		<i>Treatment 2</i>	
(1)	nil	abcd	
(2)	a	bcd	
(3)	b	acd	
(4)	c	abd	
(5)	d	abc	
(6)	ab	cd	
(7)	ac	bd	
(8)	ad	bc	

Methods and time of applying fertilizers

21. Most investigations on annual crops will begin with applications in the seedbeds but there are many conditions in which other times and methods should be considered. Thus nitrates are readily lost from light soils by leaching and later applications may then be useful. The response of the crop may depend on the stage of growth at which the fertilizer is applied. Thus phosphates are particularly suitable for securing good early root growth; whereas early nitrogen may encourage vegetative growth at the expense of roots. Phosphates may be rapidly converted into very inert forms in some soils and it is therefore desirable to concentrate the phosphate near the seeds and avoid mixing it uniformly through the whole mass of soil. Combined fertilizer-seed drills have long been used for this purpose in Australia for wheat. Special machines have been developed in the United States for widely spaced crops, such as cotton and maize, the fertilizer being applied in bands about 2 ins. to the side of and a little below the seeds. Care must always be taken to prevent soluble salts from checking germination and early growth, and preliminary tests should be made before this technique of fertilizer placement is recommended under new conditions.

Where fertilizers are to be applied by hand it may be possible to place them in the same hole as the seed or, alternatively, in holes made with a sowing stick near the young plant. Where crops are grown on ridges the fertilizer may be tried within the ridge in much the same way as potatoes are manured and planted in split ridges in Britain.

On heavy soils in regions with seasonal drought there may be grounds for applying nitrogen and other fertilizers well before sowing. In an extreme case in the Sudan Gezira irrigated cotton after clean-weeded fallows responds as well to sulphate of ammonia given twelve months before sowing as to that given a few weeks before or after sowing. In this very heavy soil there is no leaching, and early applications of nitrogen fertilizers provide a means of getting a little more nitrate into the subsoil.

Long-term experiments

22. It is assumed that the annual experiments will be repeated for several years with new sites each season. It is unsafe to judge fertilizer responses from one or two years only. The writer recently had a series of sugar beet experiments which showed no response to sulphate of ammonia in two consecutive seasons, though they had given clear responses in the 10 preceding years. Quite misleading conclusions might have been drawn if the experiments had been limited to these abnormal seasons.

At experiment stations it would be sound policy to concentrate the field work on experiments planned to run over a term of years or else requiring operations and observations too difficult or detailed to be made elsewhere. Single year tests can generally be made at a series of commercial farms or out-stations.

Many of the designs already discussed for single year experiments are appropriate for long-term experiments. In particular the NPK test, perhaps with a bulky organic manure and/or limestone, should be continued year after year on an appropriate crop rotation. In this way it is possible to allow cumulative effects to build up and to have crops with different levels and kinds of nutrition growing each season and thus able to reveal their reactions to such seasonal factors as rainfall distribution, attack by pests, etc. The long-term effects of treatments with and without bulky organic manures are, of course, of particular importance, because effects on soil structure cannot be expected to show in the first few years. Even acute nutrient deficiencies may take several years to mitigate, and responses may build up steadily. Again, some effects may show only under unusual conditions. In one series of experiments on young budded Hevea there was no appreciable response to potash for many years, except in a single experiment after the tops of many trees had been torn off in a tornado.

Residual values of manures

23. Some idea of the residual effects of manures may be obtained by following a differentially manured crop by one which is uniformly treated, and either weighing or estimating yields. This would show the residual benefit of treatment under practical conditions, but there are some aspects of residual values, rate of action and fixation of nutrients which cannot properly be studied this way. The second crop may be much less responsive than the first, and the second-year test will be insensitive. Indeed, in British farming manuring is normally arranged to ensure that this is the case. Thus root crops receive phosphate and potash, but the following cereals do not. Where it is important to know how long a fertilizer remains active in a soil it is necessary to use a sensitive crop to compare the effects of the residues against those of fresh additions. This means combining a cropping rotation with a manurial rotation. One simple design is to have sets of plots receiving 0, 1, 2 units in odd years and 2, 1, 0 units in even years respectively, together with an appropriate number of plots permanently without fertilizer. Each season then gives immediate, cumulative and residual comparisons. Such experiments would show whether or not superphosphate is rapidly "fixed" in the

soil, and whether or not mineral phosphate makes up by more prolonged action for any initial inferiority.

If anything more ambitious is attempted it is necessary to have all phases of the crop rotations present each year. If there are three crops in the rotation, three separate experiments are laid out, one for each crop. The three need not necessarily be continuous though this will generally be convenient. Information is obtained each year from each crop. There is one other important condition—to have the manuring cycle of different length from the cropping cycle. Thus, Rothamsted has one experiment with a two-year manuring rotation on a three-year cropping rotation and another experiment with a five-year manuring rotation on a four-year cropping rotation. This trick of design spreads the effects of the more permanent characteristics of the individual plots evenly over the various combinations of crop and degree of exhaustion, and the results progressively become more accurate as the experiments are continued. An earlier Rothamsted residual value experiment was invalidated by laying out the treatments systematically. However long this experiment had continued, contrasts from one side of the field to another would have appeared as differences between materials and those at right angles as differences between rates of exhaustion.

Crop rotation and manuring

24. Experiments to compare alternative rotations are necessarily complicated and have rarely been attempted. Where anything of this sort is proposed, attempts should be made to see how far the observed differences in residual values from crops can be matched by fertilizer treatments to the test crops. In this way it may be possible to learn something of the mechanism by which remedial or exhaustive cropping systems affect the soil. At Woburn Experimental Station two three-year leys and two three-year arable rotations, one with a one-year ley, are followed by test crops of potatoes and barley; half the potato plots receive farmyard manure to see how far this can even out the possible benefits from the ley.

An urgent problem in Africa is to analyse the effects of bush fallow against frequent cropping with varying forms and durations of leys or resting under grass. Where such experiments are attempted it would be most useful to include tests on the major plant foods.

Sampling

25. The field work at harvest time may often be greatly eased by harvesting only a fraction of the total produce on each plot. It has been found convenient to have experiments on root crops on, say, 4 or 8 rows and to record the yields only of the central 2 or 4 rows on each plot. This is more accurate than carrying out the whole experiment on the smaller area because all the test rows have similarly treated "guard rows", and "edge effects" are minimized. Where it is possible to thresh small quantities of cereals by hand the fraction harvested and threshed can be quite small. The actual sampling unit should be a composite one chosen to include portions from each of a large number of sections into which the plot may be regarded as being subdivided. These representative portions may be bulked for weighing and threshing. In the early stages of such work it is useful to take two independent composite samples per plot and determine the error of sampling.

The value of field experiments can often be increased very greatly by measuring and sampling the crop at various stages during growth and again at harvest, and analysing various fractions of the plant for nitrogen or other nutrients likely to limit growth. Indeed, one of the most useful methods for discovering the dominant factors in soil fertility and crop production is to

sample, weigh and analyse crops grown in factorial experiments on sowing date, spacing and manuring with nitrogen (and possibly other plant foods in addition).

Units

26. Round figures are chosen for convenience at some stage in drawing up the plan of an experiment. Where practice is already well established it will be natural to use current local rates. Thus many experiments have been made in Britain with round numbers of cwt. of fertilizer per acre. There is, however, such a wide range of grades of fertilizer that it is preferable to use round numbers of cwts. of plant food per acre. Most Rothamsted experiments have rates in multiples of 0.2 cwts. of N, P_2O_5 or K_2O per acre and all yields except roots are expressed in cwts. of grain, hay, dry matter, etc., per acre. The suggestion is offered that in colonial experiments the native staff and students would be spared quite unnecessary mental arithmetic and bad chemistry if from the start all fertilizer and crop analyses and all rates of fertilizer dressings were given in terms of elements and not in the antiquated terms P_2O_5 and K_2O .

APPENDIX

1. Note on the statement of results of 2ⁿ experiments

In setting out the results of factorial experiments with all combinations of a number of treatments each at two levels, generally 0 and 1, it is convenient to give the general mean (G.M.), the "main effects" (single letters) and "interactions" (combinations of two or more letters). The "main effects" show the mean difference in yield or other data between plants with and those without a given treatment, averaging over all other conditions. Yields with and without a given treatment are obtained by adding to or subtracting from the general mean one-half of the main effect. Thus:—

$$\begin{aligned} \text{Mean yield without N} &= \text{GM} - \frac{1}{2} \text{N} \\ \text{Mean yield with N} &= \text{GM} + \frac{1}{2} \text{N} \\ \text{Difference} &= \text{main effect of N} = \text{N} \end{aligned}$$

The interaction between two treatments (*e.g.*, NP) shows how far the effect of one of the fertilizers is increased or decreased by the presence of the other. NP is defined as **one-half** of the difference between the effects of N on plots with and without P respectively. Thus:—

$$\begin{aligned} \text{Mean effect of N on plots without P} &= \text{N} - \text{NP} \\ \text{Mean effect of N on plots with P} &= \text{N} + \text{NP} \\ \text{Difference} &= 2 \text{NP} \end{aligned}$$

Alternatively the interaction may be regarded as **one-half** of the extra benefit from using two fertilizers together over the sum of their effects when used separately. This may be seen from the following statement of the way in which the yields for individual treatments are built up from main effects and interactions.

Mean yield for treatment with

$$\begin{aligned} \text{No N, no P} &= \frac{1}{8} (\text{GM} - \text{N} - \text{P} + \text{NP}) \\ \text{+ N, no P} &= \frac{1}{8} (\text{GM} + \text{N} - \text{P} - \text{NP}) \\ \text{No N, +P} &= \frac{1}{8} (\text{GM} - \text{N} + \text{P} - \text{NP}) \\ \text{+ N, +P} &= \frac{1}{8} (\text{GM} + \text{N} + \text{P} + \text{NP}) \end{aligned}$$

The above method of defining interactions has the advantage that all main effects and interactions are obtained by adding or subtracting all the individual plot yields in an experiment. They therefore have the same standard error. The calculation of main effects and interactions from the individual plot yields may be illustrated by the 8 treatment NPK experiment, using Fisher's conventions* by which small letters denote individual treatment or yields and (1) denotes the plots with none of the treatments considered. If each of the 8 treatments occurs 4 times (*i.e.*, 32 plots in all):—

$$\begin{aligned} \text{Main effect N} &= \left(\frac{1}{16} \text{npk} + \text{np} + \text{nk} + \text{n} - \text{pk} - \text{p} - \text{k} - (1) \right) \\ \text{Interaction NP} &= \left(\frac{1}{16} \text{npk} + \text{np} - \text{nk} - \text{n} - \text{pk} - \text{p} + \text{k} + (1) \right) \\ \text{Interaction NPK} &= \left(\frac{1}{16} \text{npk} - \text{np} - \text{nk} + \text{n} - \text{pk} + \text{p} + \text{k} - (1) \right) \end{aligned}$$

*Fisher's convention of using capital letters for effects and interactions, and small letters for treatments and plot yield has been followed here to facilitate reference to the books and papers of Fisher and Yates. For practical convenience in drawing up plans and in preparing labels and reports, the present writer much prefers to use the ordinary chemical symbols with any necessary suffixes for treatments and plot yields and, where necessary, to indicate effects and interactions by underlining the symbols in manuscript and typescript and by italics or heavy type capitals in print. Such treatment symbols as CaMg become very clumsy in small letters.

2. Note on the arrangement of 2^n experiments

Arrangements appropriate for 2^n experiments may be extended to cover four-level comparisons, *e.g.*, four blocks, four kinds of a given fertilizer, or three kinds and none of a given fertilizer, by the trick of writing the treatments pseudofactorially as (1) a, b, ab. Then, as usual

$$\begin{aligned} A &= \frac{1}{2} (ab + a - b - (1)) \\ B &= \frac{1}{2} (ab - a + b - (1)) \\ AB &= \frac{1}{2} (ab - a - b + (1)) \end{aligned}$$

The arrangement of 2^n factorial experiments in blocks with confounded interactions or fractional replication is facilitated by using a series of formal identities based on the special algebraic convention that the product of any letter by itself equals unity, *e.g.*, $A^2=1$, $ABC \times ABD = A^2B^2CD = CD$. By multiplying the identity $ABCD=1$ in turn by appropriate letters it will be seen that, if $ABCD=1$, then

$$\begin{aligned} A &= A^2BCD = BCD \\ AB &= A^2B^2CD = CD \\ ABC &= A^2B^2C^2D = D \end{aligned}$$

In other words, any letter or group of letters in such an identity may be regarded as being identical with the rest of the letters in the identity.

Confounding interactions with blocks means identifying certain treatment contrasts with certain block contrasts. Fractional replication means identifying certain treatment contrasts with certain other treatment contrasts. The identities show which identifications are to be made. Some may be chosen arbitrarily, but others then follow automatically. To write out the relevant identities provides a rapid and convenient means of seeing the implications of any restriction on complete replication.

It is convenient to denote treatment contrasts by early letters of the alphabet and block contrasts by late letters. Each independent identity written down halves the number of plots required to balance all treatments with the restriction covered by the identity. Thus in the 8 plot NPK test with two blocks—denoted by (1) and x—a full replication would require 16 plots but the number may be reduced to 8 by the condition $NPK.X=1$. Multiplying both sides by X gives $NPK=X$, which is equivalent to identifying the NPK interaction with the contrast between blocks. It will also be noted that any two-term interaction is identical with the interaction of the third treatment with blocks, *e.g.*, $NP=KX$.

When appropriate identities have been selected, the treatments within each of the blocks are chosen to satisfy the conditions that the letters representing each combination of treatments and blocks must have either no letters or an even number of letters in common with each of the identities. This may be expressed more concisely by saying that treatment and block letters must be "even" with the identities. Treatment and block letters must be considered together in choosing "even" combinations, but it is more convenient to tabulate them separately, setting out the block letters (*e.g.* (1), x, y, xy) at the heads of columns. The first block, denoted by (1), may be termed the principal block. It includes the plot (1) with none of the treatments. Two "even" combinations of treatment letters are then selected, together with their product. This gives four of the treatments in the principal block. Another set of four is obtained by picking another "even" combination and multiplying the first four by it. This process is continued until the principal block is completed. The treatments for the second block (x) are found by picking any "even" combination of X but not Y with treatment letters and using it to multiply all the treatment letters in the principal block, remembering the rule the $A^2=1$.

Thus, for the identity $NPK.X=1$, the principal block must contain the untreated plot (1), such a pair of combinations as np and nk and their product pk. The second block x must contain a pair of letters such as nx. Entering x as a heading for the block and multiplying the treatment letters of the principal block (1) by n, we have for block x the treatments n, p, k, npk.

Identity $NPK.X=1$

Block (1)	x
Treatments (1)	n
np	p
nk	k
pk	npk

The example given in paragraph 20 of arranging combinations of four treatments in 8 blocks of 2, confounding all two-factor interactions, requires 7 identities to cover the degrees of freedom for blocks.

$$\begin{aligned}
 AB.X &= 1 \\
 AC.Y &= 1 \\
 BC.XY &= 1 \\
 AD.Z &= 1 \\
 BD.XZ &= 1 \\
 CD.YZ &= 1 \\
 ABCD.XYZ &= 1
 \end{aligned}$$

Block (1)	x	y	xy	z	xz	yz	xyz
Treatments (1)	b	c	bc	d	bd	cd	bcd
abcd	acd	abd	ad	abc	ac	ab	a

A 2^5 experiment in four blocks of eight could be given by:—

$$\begin{aligned}
 ABC.X &= 1 \\
 ADE.Y &= 1 \\
 BCDE.XY &= 1
 \end{aligned}$$

Block (1)	x	y	xy
Treatments (1)	b	d	bd
bc	c	bcd	cd
de	bde	e	be
bcde	cde	bce	ce
abd	ad	ab	a
acd	abcd	ac	abc
abe	ae	abde	ade
ace	abce	acde	abcde

It will be found convenient to check the arrangement by setting out the treatments factorially and inserting the block letters, thus:—

	(1)	c	d	cd	e	ce	de	cde
(1)	(1)	x	y	xy	y	xy	(1)	x
a	xy	y	x	(1)	x	(1)	xy	y
b	x	(1)	xy	y	xy	y	x	(1)
ab	y	xy	(1)	x	(1)	x	y	xy

Half replicates are obtained by an identity involving only treatment letters, quarter replicates by two such identities and so on. Such identities may be used in conjunction with additional ones for blocks. It is convenient to write down all the identities or aliases involved and not merely the minimal number required to define the conditions. Thus a half replicate of a 2⁶ experiment in four blocks of eight would be given by:—

$$\begin{aligned} ABCDEF &= 1 \\ ABDE.X &= 1 = CF.X \\ ADF.Y &= 1 = BCE.Y \\ BEF.XY &= 1 = BEF.XY = ACD.XY \end{aligned}$$

Such designs have been used for Rothamsted experiments at many centres on three bulky organic manures at two rates and no organic manure, each tested with and without N, P, K.

The contrasts may be denoted: Blocks (1), x, y, xy.
 No organic manure (1) Rate 1 (1) No N (1) No P (1) No K (1)
 Sewage sludge a „ 2 d + N n + P p + K k
 Compost b
 Farmyard manure ab

The identities are: $ABDNPK = 1$
 $ABNP.X = 1 = DK.X$
 $ANK.Y = 1 = BDP.Y$
 $BPK.XY = 1 = ADN.Y$

Blocks	(1)	x	y	xy
Treatments	(1)	dp	np	dn
	adpk	ak	adnk	anpk
	dnpk	nk	dk	pk
	an	adnp	ap	ad
	bp	bd	bn	bdnp
	abdk	abpk	abdnpk	abnk
	bdnk	bnpk	bdpk	bk
	abnp	abdn	ab	abdp

The pattern of confounding may be seen and checked by writing the block letters (1), x, y, xy into a formal arrangement of the treatments.

Organic Manures		Fertilizers							
		(1)	n	p	np	k	nk	pk	npk
<i>Single rate</i>									
None	(1)	(1)	—	—	y	—	x	xy	—
Sludge	a	—	(1)	y	—	x	—	—	xy
Compost	b	—	y	(1)	—	xy	—	—	x
F.Y.M.	ab	y	—	—	(1)	—	xy	x	—
<i>Double rate</i>									
None	d	—	xy	x	—	y	—	—	(1)
Sludge	ad	xy	—	—	x	—	y	(1)	—
Compost	bd	x	—	—	xy	—	(1)	y	—
F.Y.M.	abd	—	x	xy	—	(1)	—	—	y

The confounding of the DK interaction with the X contrasts of blocks (*i.e.*, $DK.X = 1$) is seen by the circumstance that in the x and xy blocks the single rates of organic manures occur with potash and the double rates without potash.

It will be realized that very great care must be taken in the choice of interactions for confounding, especially with fractional replication. Where there are only one or two experiments it may pay to increase the size of the blocks to minimize confounding. In a series of closely related experiments different interactions may be confounded in different experiments.

In all these patterns the actual arrangement of plots within the blocks must, of course, be randomized, and it will generally be worth while randomizing the position of the blocks within the experiment.

For more detailed discussions of confounding and fractional replication in 2ⁿ and 3ⁿ experiments, reference should be made to:—

D. J. Finney, *Empire Journal of Experimental Agriculture*, 1947, Vol. 15, pp. 107–112, and O. Kempthorne, *Biometrika*, 1947 (in the Press).

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