

## Rothamsted Repository Download

### G - Articles in popular magazines and other technical publications

Fisher, R. A. and Wishart, J. 1930. *The arrangement of field experiments and the statistical reduction of the results*. London H.M.S.O..

doi:10.23637/rothamsted.8v20y

The publisher's version can be accessed at:

- <https://dx.doi.org/10.23637/rothamsted.8v20y>

The output can be accessed at: <https://repository.rothamsted.ac.uk/item/8v20y/the-arrangement-of-field-experiments-and-the-statistical-reduction-of-the-results>.

© 1 May 1930, Please contact [library@rothamsted.ac.uk](mailto:library@rothamsted.ac.uk) for copyright queries.

[Crown Copyright Reserved]



C.C. No. 46460

# IMPERIAL BUREAU OF SOIL SCIENCE

TECHNICAL COMMUNICATION No. 10

## THE ARRANGEMENT OF FIELD EXPERIMENTS AND THE STATISTICAL REDUCTION OF THE RESULTS

by

R. A. FISHER, Sc.D., F.R.S., and J. WISHART, D.Sc.

LONDON:

PUBLISHED BY HIS MAJESTY'S STATIONERY OFFICE,

To be purchased directly from H.M. STATIONERY OFFICE at the following addresses:  
Adastral House, Kingsway, London, W.C.2; 120, George Street, Edinburgh;  
York Street, Manchester; 1, St. Andrew's Crescent, Cardiff;  
15, Donegall Square West, Belfast;  
or through any bookseller.

1930.

Price 1s. Net.

### IMPERIAL AGRICULTURAL BUREAUX

EXECUTIVE COUNCIL,  
2, Queen Anne's Gate Buildings, London, S.W.1.

IMPERIAL BUREAU OF SOIL SCIENCE,  
Rothamsted Experimental Station, Harpenden, Herts.

IMPERIAL BUREAU OF ANIMAL NUTRITION,  
The Reid Library, Rowett Institute, Bucksburn, Aberdeen.

IMPERIAL BUREAU OF ANIMAL HEALTH,  
Veterinary Laboratory, New Haw, Weybridge, Surrey.

IMPERIAL BUREAU OF ANIMAL GENETICS,  
King's Buildings, University of Edinburgh, Scotland.

IMPERIAL BUREAU OF PLANT GENETICS (FOR CROPS OTHER THAN HERBAGE),  
School of Agriculture, Cambridge.

IMPERIAL BUREAU OF PLANT GENETICS (HERBAGE),  
Agricultural Buildings, Alexandra Road, Aberystwyth.

IMPERIAL BUREAU OF FRUIT PRODUCTION,  
East Malling Research Station, East Malling, Kent.

IMPERIAL BUREAU OF AGRICULTURAL PARASITOLOGY,  
Winches Farm, Hatfield Road, St. Albans, Herts.

### STAFF OF THE IMPERIAL BUREAU OF SOIL SCIENCE

Director ... ..	Sir E. J. RUSSELL, D.Sc., F.R.S.
Deputy Director ... ..	A. F. JOSEPH, D.Sc., F.I.C.
Assistants ... ..	A. J. LLOYD LAWRENCE, M.A. Miss H. SCHERBATOFF.
Private Secretary ... ..	Miss L. V. IVES.

631.421/FIS

## IMPERIAL BUREAU OF SOIL SCIENCE

### TECHNICAL COMMUNICATION No. 10

In much field experimental work, lay-out so as to economise space, and correct interpretation of data obtained from the experiment, necessarily cause the research worker much thought. No mathematical calculations can obviate the need for common sense or, so far as the results of particular years are concerned, eliminate seasonal effects. Space in lay-out can, however, be economised and the reliability of the deductions drawn increased by the use of sound statistical methods.

This memorandum aims at explaining the principles underlying the field experimental technique recently elaborated at Rothamsted, and gives, with appropriate illustrations from actual experiments, the full arithmetical working involved in the statistical reduction of the data. The memorandum stresses two points of importance: (1) the desirability of uniformity of procedure, and (2) the necessity for a field technique which shall minimize experimental errors and at the same time provide for an estimate of these errors by valid statistical methods.

Small experiments may sometimes have their value assessed by cursory examination of the results, provided the differences are sufficiently striking to be obvious. Very few experiments, however, are of this nature. In the majority of experiments the averages appear to indicate a response to some fertilizer, or a superiority of one particular variety over another, but an element of doubt remains as to how far the differences in those averages may be due to chance. No one would now dream of testing the response to a treatment by comparing two plots, one treated and the other untreated. The idea of replication has become firmly established. Even the crudest of examinations takes into account the variation of the replicates before assessing the value of the difference between the means of two sets, and if the experiment consisted of nothing more than a number of sets, distributed at random over the field without any attempt being made to allow for soil differences, then some sort of criterion could be arrived at on the basis of experience by the individual worker, whereby he could assert that differences of a certain magnitude were probably real. Without statistical analysis, however, two experimenters would not necessarily draw the same conclusions from the same experiment. In doubtful cases the significance of observed differences should be expressed precisely, i.e. in the shape of certain odds for or against the reality of the phenomenon detected.

The majority of field experiments can be made straightforward in character, and one of the objects of this memorandum is to set out the calculations involved in a proper statistical reduction of the results. This need not be difficult. It is unnecessary, for example, to use a calculating machine: all the examples given here were worked out without one. A handy table of squares saves arithmetic, and occasional use may be found for a book of logarithms, although this is obviated by possession of a slide rule having a logarithmic scale. The modern field methods—Fisher's Latin Square and Randomised Block arrangements—are elaborated in order that a valid "Analysis of Variance" may be possible. Anything less than this analysis, which, as the examples show, is simple and straightforward as long as the experiment is simple in design, will fail to get out of the data results of a precision warranted by the lay-out. To give a simple example, suppose there are two experiments, in each of which Treatment A

is compared with Treatment B, there being four replicates of each. Suppose the yields are as follows:—

	Experiment I.		Experiment II.	
	A	B	A	B
	6	11	8	12
	12	13	9	13
	8	9	11	11
	10	15	8	12
	—	—	—	—
Mean ...	9	12	9	12
	—	—	—	—

The difference between the average yields is 3 in both cases, but as the replicates in the second case are much closer to the average than in the first, we might say that the difference is significant in Experiment II but not significant in Experiment I, if we knew that there were four plots of A and four of B, scattered at random. But if in fact we know in Experiment I that the two plots whose yields are given in each horizontal line of the table occur together in one block, the actual order being determined by chance, then we see that the replicates tend to go up and down together, i.e. are correlated. In other words, the major portion of this variation is due to soil differences between blocks, which do not affect the comparison of treatments, and when this is eliminated by means of the "analysis of variance" we find that the difference of 3 between the means is now significant, as in the case of Experiment II.

This example shows the intimate connection between lay-out and interpretation, and also illustrates a case where an experimenter might well be in doubt as to the correct reading of his results. It is true, of course, that the statistician expresses his results in terms of probabilities, and not as certainties; that is, he finds that a certain difference between two means could have occurred by chance, not more than, say, once in 20 trials. But by fixing an appropriate standard of significance, as 1 in 20 or 1 in 100, and sticking to this throughout, he can distinguish between the results which should be ignored and those which should form the basis of his conclusions.

Of the technical problems with which the experimental agronomist is faced, the choice of statistical procedures in the reduction and analysis of the data yielded by experiments is one of the most insistent. It is improbable that, without special training, he would be expected to decide upon questions of such difficulty, but for the belief that all that is needed is the ability to do simple arithmetic, such as he will have learnt at school. Simple arithmetic is indeed all that is needed to carry through a chosen procedure, provided that the experiment has been so designed that a simple procedure is adequate to its interpretation, but agronomic literature even of a not very remote date is full of discussions of procedure which are either directly misleading or, in the circumstances in which they are used, inconclusive. That this is particularly the case in respect of statistical methods arises chiefly from the fact that it is only in recent years that the underlying theoretical problems have been resolved, and that technically correct and practically simple procedures have been made available.

The advantages to be sought from uniformity of procedure are the same as characterise competent work of all sorts, namely, intelligibility and accuracy. Work expressed in unfamiliar terms will generally be unintelligible, or intelligible only with great difficulty to the majority of those to whom it is addressed. It will be unread or read cursorily, and understood but vaguely. The weight of the evidence supplied by new experiments, and the value of the methods adopted, will only be appreciated if the results are set out in a familiar form which is widely understood, and conforms in all relevant details to the form of statement adopted elsewhere. Good experiments will necessarily be disregarded, or overlooked, if the meaning of the secondary data derived from them is obscure, or if the tests of significance applied to them are not, or do not seem to be, those on which reliance is habitually placed. Moreover, the advantage of uniformity in the form of presentation of the results is at least as valuable to the reader as to the writer. The specific results of comparable experiments carried

out elsewhere, and the general trend of experience in agronomic practice, can only be made available to those who wish to make use of them if writers adhere as strictly as is convenient, and much more strictly than is yet habitual, to well-understood conventions in the presentation of their results.

The second advantage to be gained at the present time by uniformity of procedure is that of accuracy. By adhering to authentic methods of calculation the computer may not only minimize the danger of arithmetical errors, but avoid those errors and discrepancies, due to faulty theory, which introduce an extravagant amount of labour into the examination of much which has been published in the past. An alternative procedure, such as substituting the median for the mean yield, or estimating deviations by the mean deviation instead of the mean square deviation, may seem to save labour, and in the special cases in which they are applied may in fact not be misleading, but the labour either to the reader or to the writer of ascertaining whether in fact the variation of procedure is or is not harmless, is certainly greater than that of adopting from the first a more reliable process. Moreover, when variations are applied in the initial processes, the whole structure of subsequent comparisons, either within one experiment or between experiments carried out at different times and places, is thrown into confusion.

The new field technique developed in recent years (*see* list of references appended) at Rothamsted has attracted much attention, and the Soil Bureau has therefore asked Dr. Fisher, who originated the method, to prepare the following memorandum: in doing this he has had the co-operation of Dr. Wishart.

ROTHAMSTED EXPERIMENTAL STATION,  
HARPENDEN, HERTS,  
ENGLAND.

MAY,  
1930.

THE ARRANGEMENT OF FIELD EXPERIMENTS AND THE STATISTICAL  
REDUCTION OF THE RESULTS.

By R. A. FISHER, Sc.D., F.R.S., and J. WISHART, D.Sc.

The four sections are devoted to :—

- A.—Preliminary illustrations of plot arrangement :  
 (1) Randomised Blocks.  
 (2) The Latin Square.
- B.—The arithmetical processes.
- C.—The structure of an experiment in relation to the analysis of variance.
- D.—Illustrative examples :—  
 (i) Randomised blocks.  
 (ii) The Latin square.  
 (iii) Example of a complex experiment.

A.—PRELIMINARY ILLUSTRATIONS OF PLOT ARRANGEMENT.

Of the randomised forms of replicated experiment in common use, two occur with such frequency that it will be convenient to give a brief statement of the methods of laying them out, before introducing the arithmetical processes used in their interpretation. These are (1) Randomised Blocks, and (2) the Latin Square. These are the simplest types of lay-out which fulfil the condition of supplying a valid estimate of error, and at the same time possess the advantage of eliminating a substantial fraction of the soil heterogeneity; they are not, therefore, to be regarded as the only types to be recommended, but as representative of a large class of useful arrangements.

(1) *Randomised Blocks.*

Let us suppose that there are five treatments, and that six-fold replication has been decided on (as in example on p. 8). The area is divided into six more or less compact blocks. If it can be divided in two directions at right angles, instead of into six strips lying side by side, so much the better. Within each block there are to be five plots, given over to the five treatments, the position of these being determined at random. To do this a set of random two-figure numbers is taken, i.e. a book may be opened at random, or a set of consecutively numbered cards may be thoroughly shuffled and the top one taken. Suppose the first number reached is 37. On dividing by 5 we find a remainder of 2; this means that treatment No. 2 goes into the first plot of the block. If a multiple of 5 was itself reached that would be taken to be treatment No. 5, while if the same number occurred twice in the same block the second occurrence would be disregarded. This procedure is repeated for every plot. With, say, six treatments it would be advisable to disregard numbers above 96 because we wish to give all remainders on dividing by 6 an equal chance of occurring.

The advantage of the randomised block arrangement is that replication is secured, and at the same time it is easy to distinguish the soil variation within the blocks, from that between different blocks. For the totals of each block are comparable in that each contains one plot of each of the treatments. The variation of the totals may, therefore, be ascribed to soil differences, and these gross differences can be eliminated from the comparisons of the different treatments in a manner to be described on the following pages.

This arrangement involves a restriction on the completely random location of plots, in that the blocks contain only one plot of each treatment. The object of this is to eliminate from the comparisons of treatments the major elements of the soil heterogeneity. The advantage in precision gained in this way is often so great that it is desirable to carry the process further, and introduce additional restrictions as in the Latin Square.

(2) *The Latin Square.*

Two restrictions are here made. There are as many replicates of each treatment as there are treatments, the plots being arranged in a square, with as many rows as columns, so that each treatment occurs once in each column and once in each row of the square. Thus both columns and rows may be regarded as randomized blocks. This provides for a double elimination, in two directions at right angles to one another, of soil differences, i.e. between columns and between rows. The area is only square in a conventional sense, for the actual size of the plots may be made to suit the available area. They may be square or rectangular in shape, but precision is lost if they are too long and narrow. Subject to the restrictions indicated, the treatments should be allocated at random. This is best done by having cards made with all the possible arrangements. Thus for a  $4 \times 4$  square there are the four types :—

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

which are alike in that each has A B C D in order along the top and down the left-hand side. For each there are, therefore, 6 ways of rearranging the rows other than the first, and cards might be written out for all these 24 arrangements. Having shuffled and chosen a card at random, the letters A B C D are allotted at random to the four treatments. This can be done in 24 different ways, with the result that we are choosing one square out of the 576 possible squares of this size. In the case of larger squares the number of possible squares becomes so large that it would scarcely be possible to write out cards for them all. In the case of  $5 \times 5$  one might write out the 56 possible squares with A B C D E in order along the top and down the left-hand side. Then choosing one of these at random one can randomize the four rows other than the first by allocating the rows beginning with B, C, D, E to the numbers 2, 3, 4 and 5 at random, i.e. by putting the letters in one box and the numbers in another and drawing a pair simultaneously. In the square thus obtained the letters A, B, C, D, E are then allotted at random to the five treatments to be tested. With  $6 \times 6$  squares the number of preparation of cards to those of certain types, randomizing later within the types. If larger squares are needed they should be found by filling in the first few rows in random fashion, but satisfying the condition that no treatment should occur more than once in each row and once in each column. It will then be found that the fixing of the first few rows largely determines the position of the remaining treatments. Such larger squares may then be randomized by random rearrangements of the letters and rows.

B.—THE ARITHMETICAL PROCESSES.

(i) *Totals and Means.*

The data from a field trial usually consist of one or more values from each plot harvested separately; they may, of course, refer to yields, or to qualitative determinations such as percentage moisture, sugar content, etc., or to compounds of these items, such as yield of dry weight, yield of sugar, and so on. The procedure applied is the same in all cases, and the first stage, of obtaining the necessary totals and means, is applied to each item of those to be analysed, independently. For any item, such as yield of grain, we then have a single value from each plot. The area of each plot will be the same, but may be any fraction of the standard unit (acre, hectare, etc.). It is needless to reduce the yields of individual plots to yields per acre, since this can be done much more rapidly to the mean values, when these have been found, and divergencies of practice would arise as to the number of decimal places to be retained in the plot values, upon which all other calculations are to be based.

Each mean will be obtained from the corresponding total by dividing by the number of plots which contribute to it; the totals which, with their corresponding means, will be required will be (i) a grand total of all the plots in the experiment, (ii) sub-totals for different treatments

and usually also (iii) sub-totals for divisions of land such as blocks, rows, columns, etc. The sub-totals are themselves sometimes further sub-divided into smaller groups, as would be the case if several different varieties had been subjected to several different manurial treatments, all with replication, in the same experiment. The method of computation is to form the totals first for the smallest sub-divisions to be used, and to compound these step by step until the grand total is formed; otherwise the labour of forming the grand total from the original items will be duplicated. The grand total should always be found in more than one way, as occurs naturally, for example, if 5 treatments are tested each in one plot of each of 6 blocks, for the grand total is then equal either to the sum of the 5 treatment totals, or to the sum of the 6 block totals. It is convenient to add up such data in a two-way table, with double margins for the totals and means.

## BARLEY, 1928.

Area : 1/40th acre. Yield of straw in  $\frac{1}{2}$  lb.

Blocks.	Treatments—					Total.	Mean.
	1	2	3	4	5		
A	214	273	303	372	350	1,512	302.4
B	235	278	320	304	377	1,514	302.8
C	222	285	345	347	370	1,569	313.8
D	317	389	387	398	417	1,908	381.6
E	238	372	389	426	376	1,801	360.2
F	324	323	362	398	428	1,835	367.0
Total ..	1,550	1,920	2,106	2,245	2,318	10,139	2,027.8
Mean ..	258.3	320.0	351.0	374.16	386.3	—	337.96 General mean

Treatment 1—No nitrogen.  
 Treatment 2—Cyanamide, single dressing.  
 Treatment 3—Sulphate of Ammonia, single dressing.  
 Treatment 4—Cyanamide, double dressing  
 Treatment 5—Sulphate of Ammonia, double dressing.

If the structure of the experiment is so simple that only one set of sub-totals is available to form the grand total, it should be found independently for artificially-chosen "blocks" in order to check the arithmetic.

(ii) *Crude sums of squares (or products).*

The second process, and the only one with which difficulty has been felt, consists of summing the squares of the yields, or of their deviations from the general mean, or from special means. The unnecessarily laborious process is sometimes adopted of subtracting from each plot yield in turn the exact value of the general mean, retaining sufficient decimal places, and squaring all the differences. The result obtained in this way is correct, but it may be arrived at in most cases much more rapidly by using not the exact general mean, but some other "working mean."

Often the working mean may be taken to be zero, in which case we merely add up the squares of the recorded yields, avoiding all labour of subtraction. This is usually quite a suitable procedure, using a machine or a table of squares, such as Barlow's, which gives the exact squares of all four-figure numbers; it does, however, involve usually an unnecessarily large number of figures. For example, if the yields all lie between 350 and 450 units, the squares of these numbers as they stand will be of 6 figures each, whereas if we had taken the differences from 400 before squaring we should never need more than 4 figures. Rapidity of

working is much increased if the numbers to be squared are small enough for their squares to be written down at sight. Any computer who has much of this work to do should memorize the squares of all numbers up to 40; an easy rule will then extend his range up to 60, for  $(50 + a)^2 = 2,500 + 100a + a^2$ , and if  $a$  is a positive or negative number less than 10,  $a^2$  itself provides the last two digits, while the first two are  $25 + a$ . If a working mean, choosing some round number near to the true mean, does not reduce all differences to numbers of not more than two digits, it is probable that the plot weighings have been carried out with an excessive precision. The computer, however, will probably waste his time if he proceeds to group the record in larger units, rather than to use the actual squares of a few three-figure numbers.

(iii) *Sums of squares of deviations.*

From the crude sum of squares of deviations from any working mean, the sum of squares which would have been obtained from the true mean is obtained by subtracting the product of the difference between these two means and the difference between the grand total and the product of the working mean and the number of plots. The correction is exact and does not depend on the working mean being near to the true mean, though if this is the case the correction will be small, and will be wanted perhaps to only 2 instead of 6 or 7 significant figures. If the working mean is zero the correction to be deducted is simply the product of the grand total and the general mean, a product which, if this process is used, will appear repeatedly in calculating the other sums of squares required. The principle of obtaining in the simplest way possible a sum of squares, and then deducting from it a portion or portions which are irrelevant to our purpose, is one that should especially be noted, for it is the essence of the practical procedure of the analysis of variance.

The other sums of squares required are those of the individual factors whose effect upon yield the experiment is competent to elucidate. If, for example, we have 5 treatment totals, and the corresponding means, we may find the sum of squares corresponding to treatments, by multiplying each mean by the corresponding total, adding the results, and deducting the product of the grand total and the general mean. Equally, and without a machine this is more convenient, we may find the sum of the squares of the treatment totals, divide by the number of plots (6 in our example) which contribute to each total, and then deduct the product of the grand total and the general mean. The sum of squares corresponding to blocks is then found by an identical procedure, and in more complex experiments a number of similar items will be calculated, which is far less laborious than the evaluation of the general sum of squares of all deviations.

(iv) *Degrees of freedom and mean squares.*

It will have been noticed that the arithmetical analysis has dealt only with sums of squares, and not, for example, with mean squares found by dividing the sums of squares by the number of plots. It is this feature which makes it materially simpler than the older forms of analysis, as well as bringing it into harmony with the sampling theory to which it is to be applied. The number of plots is, in fact, not the right divisor in any case. The divisors which are really appropriate are found by considering the number of independent comparisons, or "degrees of freedom," corresponding to each of the sums of squares calculated.

To begin with the total, if there are 30 plots, then there are 29 independent comparisons which can be made among the yields recorded from these plots; we have, therefore, in all 29 degrees of freedom. Equally, 5 treatments will involve 4 degrees of freedom for treatments, 6 blocks will involve 5 degrees of freedom for blocks; these groups have, however, already been included in the total of 29, and if each treatment appears once, and once only, in each block, they will be mutually independent, i.e. our 5 degrees of freedom for blocks do not include any of the 4 degrees of freedom for treatments. We have, therefore, accounted for 9 degrees out of the total of 29, and there must be 20 more to be considered. Now our treatment differences as judged by the plot yields will generally be different in different blocks; there are 4 independent

treatment differences, and for each of these the comparison of the six blocks will give 5 independent comparisons. There are, therefore, 20 degrees of freedom which represent the differences among the different blocks of the comparisons between treatments. These may be regarded as the discrepancies, or errors, of our experiment, and if the treatments have been assigned their places in the different blocks wholly at random, it may be shown that these 20 degrees of freedom do in fact supply a valid estimate of error for the treatment comparisons which the experiment was designed to make.

We may now set up a table showing the three parts into which the 29 degrees of freedom of the experiment have been divided, and, opposite to each, in a parallel column, set out the sum of squares obtained from the corresponding group of comparisons.

ANALYSIS OF VARIANCE.

Due to—	Degrees of freedom.	Sum of squares.	Mean square.	$\frac{1}{2} \log_e$ . (Mean square.)
Blocks .. .. .	5	31,634.17	6,326.83	2.0737
Treatments .. .. .	4	62,903.47	15,725.87	2.5290
Error .. .. .	20	13,143.33	657.17	0.9414
Total .. .. .	29	107,680.97	—	—

Standard error (of total of 6) is 62.79, or 3.10 per cent.

The sum of squares obtained from all the plot yields is set down opposite the total of 29 degrees of freedom; the sum of squares from the block totals opposite the 5 degrees of freedom for blocks; and the sum of squares from the treatment totals opposite the 4 degrees of freedom for treatments. To find the entry to set down opposite the 20 degrees of freedom for errors we might proceed in two ways. The long way would be to replace every plot yield by the corresponding deviation from the mean value of the block in which it lies, and then find for each treatment the sum of the squares of the deviations from the treatment mean. These deviations are evidently freed both from the differences between blocks as such, and the differences between treatments, so that the sum of squares obtained arises solely from the fact that the response to different treatments has been different on the different blocks; it therefore measures only the discrepancies or "errors" of the experiment. The shorter way is merely to subtract from the sum of squares for the whole 29 degrees of freedom the two portions already obtained for the 9 degrees of freedom for blocks and treatments. The removal of the portion due to blocks is mathematically equivalent to replacing each yield by the deviation from the block mean, and the further removal of the portion due to treatments to using the deviations of these values from the treatment means. The second method is evidently much the quicker, and will be found to agree, if the arithmetic is correct, with the value found by the longer method to as many significant figures as are used.

(v) *Analysis of variance.*

The process of analysis outlined above consists merely in finding the sum of squares of the deviations of all the plot yields from the general mean, and dividing this total into the portions about which the experiment is designed to supply information. To each of these portions a definite number of degrees of freedom corresponds, and the structure of the experiment is best understood from the manner in which the total number of degrees of freedom is divided into these portions. This partition is of the greatest aid in designing experiments, and should always be examined when the experiment is planned, and before the results are obtained.

The manner in which the sum of squares has been analysed is such that the mean squares for the different portions, found by dividing the sum of squares by the corresponding degrees of freedom, are comparable. The analysis of variance is therefore completed by making a third

column, of mean squares, as is done in the Table (p. 10). The mean square corresponding to error forms a basis for further direct tests of significance, and measures the precision of the experiment. It is in fact a valid estimate of the variance (mean square error) of the yield from a single plot. The corresponding variance for the total of 6 plots, such as we need for comparing treatments, is 6 times as great. In each case the square root of the variance gives the standard error, and this as well as the total yields may be reduced by the appropriate factor to the basis of yield per acre, or, by dividing by the mean of the treatment totals, to a standard error per cent. Thus, in our example the variance of a single plot is 657.17, the variance of the total of 6 plots is 3,943, the standard error of the total of 6 plots is 62.79, which is 3.10 per cent. of the mean yield for 6 plots, namely, 2,027.8.

The significance of factors other than experimental errors is tested by comparison of the mean square with that ascribable to error, and the work has been arranged so that this can be done directly from the table of the analysis of variance. Mean squares considerably greater than that ascribable to error are significant, for had the factor concerned exerted no influence on yield the mean square to be expected would have been equal to that due to field errors only. Of the mean squares, that due to the total, being a composite group of degrees of freedom, will usually be without interest, and need not be calculated. The primary interest of the experiment will lie in the mean square due to treatments; in addition, we shall see from the mean square due to blocks, whether the arrangement in blocks actually adopted has been an advantageous one, for this represents the components of variance which have been eliminated from our comparisons, and which with other forms of field arrangement might have appeared among the errors of our comparisons. The larger the fraction of the sum of squares eliminated by the field arrangement the smaller generally will the remainder be, and as this remainder consists of the two portions of (i) errors in our comparisons and (ii) differences between replicates upon which are based our estimate of errors, we shall seek for field arrangements which make these two items as small as possible.

Although, when the mean square for treatments is much greater than that for errors, the observed differences between treatments are clearly significant, while if the two mean squares are nearly equal, or if that for treatments is the smaller, they are clearly insignificant, yet it is important to have an exact test of significance by which to judge of the more doubtful cases. This is known as the *z*-test. If the treatments used had in fact no differential effects upon yields, the mean square for treatments would occasionally by chance bear a ratio materially exceeding 1 to the mean square for errors, although in fact the treatment differences would be wholly due to errors. It is necessary to know how frequently any given ratio will be exceeded by chance, or more conveniently what ratio will be exceeded in a given small proportion, such as 5 per cent., or 1 per cent., of the trials. These ratios will of course depend on the numbers of degrees of freedom on which the two mean squares are based, but for any such pair of numbers tables have been prepared by which it can be determined whether the observed ratio is greater or less than that which would by chance be exceeded in 5 per cent. or 1 per cent. of trials.

The procedure is to make a fourth column of the table of the analysis of variance by entering opposite to each mean square, half its natural logarithm; these are obtained from such tables of natural logarithms as those given in Bottomley's four-figure tables, four figures being amply accurate enough for our purpose. The differences between any two values in this column give the observed value of *z*, and this has only to be compared with the 5 per cent. or 1 per cent. value for the same numbers of degrees of freedom, of which tables are already available. In these tables  $n_1$  is the number of degrees of freedom corresponding to the larger mean square, which in practical use will usually be that for Treatments. (Ref. 1.)

Thus, in testing the significance of the effect of treatment in the example, we compare a value from 4 with one from 20 degrees of freedom. Chance will allow *z* to exceed .5265 in 5 per cent. of trials, and .7443 in 1 per cent. The observed value of *z*, namely, 1.5876, is clearly significant on the more severe test. Since only differences between the logarithms are used, it is convenient in taking logarithms to move the decimal point in the mean squares until the least of them lies between 1 and 10, and so to avoid as far as possible the addition of the natural logarithm of 10 (2.3026) in finding the logarithms.

## C.—THE STRUCTURE OF AN EXPERIMENT IN RELATION TO THE ANALYSIS OF VARIANCE.

(See Ref. 6.)

With the analysis of variance in view, it is possible to appreciate the main points in the structure of an experiment which contribute to its value. All judgments of significance being based on the estimate of error, it is first of all essential that this value shall be accurate. Assuming the arithmetic to be correct, the two further requirements are that it shall be sufficiently well determined, or that it shall be based upon a sufficient number of degrees of freedom, and that it shall really represent the sources of error to which our comparisons are exposed, and no others. The first point does not affect the validity of the tests, but it much affects their precision. A valid test could be made based on only one degree of freedom for errors, but as the table of  $z$  shows the values of  $z$  exceeded in 5 per cent. or 1 per cent. of trials are very large, and in consequence relatively large real effects must be judged insignificant, or, in other words, escape detection. Precision begins to be lost seriously when the number of degrees of freedom for error falls below 10, the usual numbers being from 20 to 100. The second point is still not very widely understood. The function of our estimate of error being to compare with observed differences between treatments, it must for validity arise from just those causes of error which are effective in disturbing our treatment comparisons. This condition may be satisfied by arranging the plots wholly at random, so that any pair of plots in the area have an equal chance of being treated alike, when their differences in fertility will contribute only to the estimate of error, or differently, when they will contribute to our comparisons. It may also be satisfied when restrictions are placed upon the arrangement of the plots, provided that these restrictions can be represented by separate items in the analysis of variance; as in our example is the restriction that each treatment shall occur once and once only in each block, so that differences in fertility between different blocks are eliminated from our comparisons by the field arrangement, and from our estimate of error by the statistical procedure, provided always that, apart from these deliberate restrictions, the arrangement of the plots is arrived at wholly by chance. It cannot be satisfied by any systematic arrangement, i.e. by one in which no element of chance enters, for in such an arrangement there will always be elements of soil fertility variation which affect our comparisons, but not our estimate of error, and *vice versa*. Since we cannot say in any particular case just how important these elements are, an estimate of error from such an experiment can claim no objective validity.

It is often felt, when first a randomized arrangement is compared with one in which the plots are arranged in a regular or systematic order, that the latter has an advantage in that, the sets of plots treated alike being more regularly spread over the whole area, the errors in the comparisons between treatments will for this reason be reduced. This may often be really the case; what should be noted, however, is that, as the arrangement of our calculations in the form of an analysis of variance makes clear, any "improvement" on the random arrangement which reduces the real errors of our comparisons will be accompanied automatically by an increase in our estimate of error, a definite portion of the variance being simply transferred from one item to the other. Consequently, the skill and judgment devoted to obtaining plot arrangements which involve errors between the comparisons less than those of random arrangements may indeed make the experiment really better, but at the expense of making it seem worse. In the contrary case, if the experimenter were so unlucky as to increase his real errors, his estimate of error would be diminished. In either case, the validity of his estimate of error is vitiated, and it is impossible to be sure whether an over-estimate or an under-estimate has been obtained.

Subject to the primary requirement satisfied by randomization, there is much to be gained by restricting the distribution of the plots so as to diminish the soil differences between different treatments, or, in other words, by eliminating from the comparisons, and from the estimate of error, certain major elements of the soil heterogeneity. A method of very wide application of doing this is that already described as Randomized Blocks. The land is divided into as many blocks as there are to be replicates, the blocks being as compact as possible in form. The analysis is then strictly similar to that of our example. The second method, the Latin

square, which is very reliable if the number of treatments is from 4 to 7, is particularly suitable when all the possible comparisons between pairs of treatments are of equal interest, as in variety trials. The arithmetical working for a  $5 \times 5$  square is given in Section D.

The more restrictions are made, the more fully can the major elements of soil heterogeneity be eliminated, but at each such step the number of degrees of freedom available for the estimation of error is diminished. Thus, with a  $3 \times 3$  Latin square only two degrees remain for error, and in consequence a single  $3 \times 3$  square is not usually sufficient. A set of 10 or 20 such squares would, however, compare three treatments or varieties with great precision. It is, of course, advantageous when possible to replicate also the larger squares.

The precision of the final comparisons depends not only on how small the residue can be made by elimination of the major elements of soil heterogeneity, but on the actual number of plot yields which can be utilized in each comparison. It is this circumstance which gives to large and complex experiments the very high precision which they can undoubtedly attain. A large experiment of, for example, 144 plots allows a number of degrees of freedom to be used up in eliminating soil heterogeneity, without unduly depleting the number available for the estimation of error. Further, by allowing every one of a set of differential treatments to be combined with every one of another set, such an experiment gives to each set of comparisons the same precision as if the whole experiment had been devoted to it alone. If, for example, 6 different varieties were in such an experiment subjected each to all of 6 different treatments, the varietal comparisons would all be based upon the totals of 24 plots, and even if the standard error for a single plot were as high as 10 per cent., that for the total would be scarcely greater than 2 per cent. The same is true of the treatment comparisons. Each plot is thus used twice and the whole experiment does double work. This greatly understates the real advantage, for, apart from the elimination of soil heterogeneity, which weighs lightly on the big experiments, they will also provide information on the same level of precision upon any differential responses that may exist among the different treatments. They therefore fulfil the desiderata of exploring a wide range of combinations, and of supplying results of high precision. An example of a fairly complex experiment is worked out in Section D.

The variety of possible types of arrangement for large and complex experiments is very great, and work designed to explore the possibilities here open to the experimenter is much to be desired. In the preliminary consideration of new types of arrangement, it is useful to utilize the material supplied by uniformity trials. A number of these have from time to time been published, and any projected arrangement may be superimposed on the uniformity trials, using the results of the latter to test the precision of the plan under consideration. By allowing the plots to be assigned by chance in several different ways, a fair idea can be obtained as to the potential advantages of any arrangement or of variations from it.

## D.—ILLUSTRATIVE EXAMPLES.

Other worked examples will be found in references 3, 8, 12 to 16, and 18.

(i) *Randomized Blocks.*

The data for the example of this type have already (p. 8) been prepared for subsequent analysis by being added up by blocks and by treatments. The detailed arithmetical working which leads to the results given on page 10 is then as follows. Sum the squares of the 30 plot yields, giving 3,534,325. Subtract from this the amount 3,426,644.03, which is arrived at either as the product of the grand total, 10,139, and the general mean, 337.96 (retaining as many decimals in the mean as will give the product correct to, say, two decimal places), or by finding the square of the grand total, and dividing by 30, the number of plots. The first method is appropriate to machine calculation, for then the process is continuous and the crude sum of squares need never be written down; the second is more convenient when working from a table of squares. We are left with 107,680.97, which is the total sum of squares of deviations of the 30 yields from the general mean.



Proceeding now to the margins of the table, the sums of squares of deviations due respectively to blocks and to treatments are obtained as follows:—

Blocks.			Treatments.		
Number.		Square.	Number.		Square.
1,512	..	2,286,144	1,550	..	2,402,500
1,514	..	2,292,196	1,920	..	3,686,400
1,569	..	2,461,761	2,106	..	4,435,236
1,908	..	3,640,464	2,245	..	5,040,025
1,801	..	3,243,601	2,318	..	5,373,124
1,835	..	3,367,225			
Total	..	17,291,391	Total	..	20,937,285
Divide by 5	..	3,458,278.2	Divide by 6	..	3,489,547.5
Subtract	..	3,426,644.03	Subtract	..	3,426,644.03
Remainder	..	31,634.17	Remainder	..	62,903.47

These last are the amounts which appear on page 10 as the sums of squares ascribable to blocks and treatments respectively. The sum of squares ascribable to error is now found as the difference  
 $107,680.97 - (31,634.17 + 62,903.47)$ .

The method of using a working mean somewhere near the true mean, which is illustrated in the next example, might also have been used here, and would have reduced the size of the numbers met with in the course of the calculation.

The items headed "Mean Square" on page 10 are then found by dividing the items of the previous column by the appropriate number of degrees of freedom, namely, 5, 4 and 20 respectively. The last column,  $\frac{1}{2} \log_e$  (Mean Square) is obtained by dividing the mean squares by 100 (purely a matter of convenience in entering the logarithm tables), and finding one-half the natural logarithm of the quotients. If natural logarithms are not available, the same result is secured by finding the logarithms to the base 10, and multiplying each by the constant factor 1.15129 (this being  $\frac{1}{2} \log_e 10$ ). Since the mean square due to treatments is significantly greater than that due to "error," a table is presented showing the average yields for each treatment, together with the general mean yield and a standard error appropriate to these average yields. This table should be drawn up in commonly accepted units, such as cwt. per acre, and should also show the various treatment means, and the standard error, as percentages of the general mean yield. It is therefore convenient to begin by writing down the total treatment yields from page 8, which are in  $\frac{1}{4}$  lb. per  $\frac{6}{40}$ th acre:—

Treatments—					Mean.	Standard error.
1	2	3	4	5		
1,550	1,920	2,106	2,245	2,318	2,027.8	62.79

The mean, 2027.8, is simply one-fifth of the grand total, 10,139, while the standard error, as explained on page 11, is obtained by the calculation

$$\sqrt{657.17 \times 6.}$$

To obtain the mean yields in cwt. per acre, each of the above yields must be multiplied by

$$\frac{40}{4 \times 6 \times 112}, \text{ i.e. by } \frac{1}{67.2}$$

If a machine is used, this factor, i.e. 0.0148809, should be set on the keys and multiplied successively by the seven quantities listed above. Otherwise the results may be obtained by use of a slide rule. A second line of results, showing the mean yields as a percentage of the general mean, is obtained by dividing the above seven numbers by 20.278, and this is again conveniently done by multiplying by the reciprocal of 20.278, i.e. 0.0493145.

Expressed in tabular form the final results are:—

Average yield.	No Nitrogen.	Single Cyanamide.	Single Sulphate of Ammonia.	Double Cyanamide.	Double Sulphate of Ammonia.	Mean.	Standard error.
Straw, cwt. per acre ..	23.1	28.6	31.3	33.4	34.5	30.2	0.93
Straw, per cent. ..	76.4	94.7	103.9	110.7	114.3	100.0	3.10

A statement should normally follow, summarizing in simple terms the statistically significant elements in the results obtained. In the present case, the following would suffice: "Significant response to both single and double dressings of nitrogenous manure. The average yield of the plots treated with cyanamide is significantly below that of the plots treated with sulphate of ammonia."

When once treatment has been shown to be significant by means of the *z* test, individual differences between mean yields may be examined in the light of their standard error. The standard error given above, namely, 0.93 cwt., is the standard error of any one of the mean treatment yields given in the table. Consequently, the standard error of a difference will be got by multiplying this by  $\sqrt{2}$  or 1.414. A difference exceeding twice the standard error of this difference may usually be judged significant, and therefore a good working rule is to treat differences of more than three times the standard error of a single mean as significant. It may well happen that, in an experiment which has not been proved significant by the *z* test, the difference between the highest and the lowest means is, nevertheless, greater than three times the standard error. The reader is warned against judging such a result as significant, for this is only one, and that the largest, of all the possible differences, and it will frequently happen through chance alone that the difference between highest and lowest should exceed three times the standard error. The test has, in fact, been vitiated by choosing the treatments to be contrasted by reason of their performance. It is valid for contrasts such as Sulphate *v.* Cyanamide, which the experiment was from the first designed to test.

(ii) *Latin Square.* BARLEY, 1929. PLAN.  
 Letter denotes treatment, while figures give yield of grain in  $\frac{1}{4}$  lb. per  $\frac{1}{40}$ th acre plot, less 275.

		Columns.					Total.	Mean.	
Rows.	N	U	M	S	C	-113	-22.6		
	27	-30	-24	-49	-37				
	S	C	N	M	U			15	3.0
	19	-2	19	-11	-10				
	M	S	U	C	N			62	12.4
41	-4	13	11	1					
C	N	S	U	M	45	9.0			
28	31	20	-23	-11					
U	M	C	N	S	91	18.2			
31	25	23	31	-19					
Total ..	146	20	51	-41	-76	100	—		
Mean ..	29.2	4.0	10.2	-8.2	-15.2	—	4.0		

		TREATMENTS.				
		U	S	M	N	C
Total ..	-19	-33	20	109	23	
Mean ..	-3.8	-6.6	4.0	21.8	4.6	

U—Urea. S—Sulphate of Ammonia. M—Muriate of Ammonia. N—Nitrate of Soda. C—Cyanamide.

In this example the method is followed of subtracting from all the yields a working mean of 275, this being near the true mean. Actually the true mean is 279 exactly, and if this were subtracted the work would be still further simplified, since no correction whatever would have to be applied. But it very seldom happens that the mean is a whole number in the chosen units, and 275 has been taken to illustrate the procedure in general. The plot yields, as adjusted, are first added by rows, by columns and by treatments. This last necessitates a further short table which is shown below the main table of yields. It is necessary to take account of the signs in preparing the totals and means, but in the calculation of the sums of squares they may be ignored. The sum of squares of deviations of the 25 plot yields from the general mean, which is  $275 + 4$ , is obtained by adding up the squares of the 25 numbers shown in the table, giving 15,142, and subtracting 400, which is the product of the grand total and general mean of our adjusted table. The difference is 14,742. Three amounts now fall to be deducted from this total, and the arithmetical working is shown below:—

Rows.		Columns.		Treatments.	
Number.	Square.	Number.	Square.	Number.	Square.
-113 .. ..	12,769	146 .. ..	21,316	-19 .. ..	361
15 .. ..	225	20 .. ..	400	-33 .. ..	1,089
62 .. ..	3,844	51 .. ..	2,601	20 .. ..	400
45 .. ..	2,025	-41 .. ..	1,681	109 .. ..	11,881
91 .. ..	8,281	-76 .. ..	5,776	23 .. ..	529
Total .. ..	27,144		31,774		14,260
Divide by 5 .. ..	5,428.8		6,354.8		2,852.0
Subtract .. ..	400.0		400.0		400.0
Remainder .. ..	5,028.8		5,954.8		2,452.0

The number of degrees of freedom for each of the components is 4, and by subtracting the total of these three results from 14,742.0, the total sum of squares of deviations, we are left with 1,306.4, the sum of squares due to error, with 12 degrees of freedom. The Analysis of Variance is therefore as follows:—

Due to—	Degrees of Freedom.	Sum of Squares.	Mean Square.	$\frac{1}{2}$ loge. (Mean Square).
Rows .. ..	4	5,028.8	1,257.20	1.2657
Columns .. ..	4	5,954.8	1,488.70	1.3503
Treatments .. ..	4	2,452.0	613.00	0.9066
Error .. ..	12	1,306.4	108.87	0.0426
Total .. ..	24	14,742.0	—	—

Standard error (total of 5 plots) =  $\sqrt{108.87 \times 5} = 23.33$ , or 1.67 per cent. of the mean yield  $5 \times (275 + 4)$ , or 1,395  $\frac{1}{4}$ -lb.

Results of Analysis.

Treatment is certainly significant in this experiment. For  $n_1 = 4$  and  $n_2 = 12$ , the 5 per cent. point in the z table is .5907, while the 1 per cent. point is .8443. The smallest difference in the above table is that due to treatment, and is .9066 — .0426, or .8640. A difference of this amount would occur by chance rather less than once in a hundred trials, and is therefore

undoubtedly significant. Rows and columns have accounted for a satisfactorily large proportion of the total variation, which justifies the arrangement of the experiment in the form of a Latin square. For final tabulation purposes we require first to adjust our treatment totals, which are about a working mean of  $5 \times 275$ , or 1,375, by adding 1,375 to each. We obtain the following results:—

U	S	Treatments—			Mean.	Standard Error.
		M	N	C		
1,356	1,342	1,395	1,484	1,398	1,395	23.33

These figures are yields in  $\frac{1}{4}$  lb. per  $\frac{5}{40}$ th acre, and must therefore be divided by 56 to bring them to cwt. per acre, and by 13.95 to express them as percentages of the mean yield. Our final results are therefore as follows:—

Average yield.	Urea.	Sulphate of Ammonia.	Muriate of Ammonia.	Nitrate of Soda.	Cyanamide.	Mean.	Standard Error.
Grain, cwt. per acre ..	24.2	24.0	24.9	26.5	25.0	24.9	0.42
Grain, per cent. ..	97.2	96.2	100.0	106.4	100.2	100.0	1.67

Yield of Nitrate of Soda plots significantly higher, and that of Sulphate of Ammonia plots significantly lower, than the mean yield of all plots.

(iii) Example of a Complex Experiment.

BARLEY, 1927. 48 plots of  $\frac{1}{40}$ th acre, in 4 randomised blocks.

A				B			
2 U P	2 M P	2 C	O (b)	O (a)	O (b) P	2 S P	1 S P
1 M P	1 C	2 S	1 S	1 U	2 C P	2 U	2 M
O (a) P	O (d) P	1 U P	O (c)	1 M	1 C P	O (c) P	O (d)
2 U	O (a)	O (d)	2 C P	O (a) P	2 C	2 S	O (d) P
O (b) P	O (c) P	1 S P	1 M	1 S	2 U P	O (b)	1 M P
1 U	1 C P	2 S P	2 M	2 M P	1 C	1 U P	O (c)
C				D			

O—No Nitrogen.  
U, C, S, M—Nitrogen in the form of Urea, Cyanamide, Sulphate and Muriate of Ammonia.  
1, 2—Single and double dressings at the rate of 1 and 2 cwt. Sulphate of Ammonia, or equivalent, per acre.  
P—Superphosphate at the rate of 3 cwt. per acre.

YIELDS IN  $\frac{1}{8}$  LB., LESS 370.

	O (a)	O (b)	O (c)	O (d)	1 U	1 C	1 S	1 M	2 U	2 C	2 S	2 M	Total.
A	-86	-184	-110	-57	-34	-5	-89	61	110	3	-77	172	-296
B	-102	-73	-120	-131	-27	42	101	-4	128	73	166	152	205
C	-95	-26	-93	-128	31	59	-11	-2	105	25	94	172	131
D	-126	-111	-103	-142	19	39	43	83	142	26	30	134	34
Total—													
P	-723				-15	101	90	144	252	98	260	306	513
O	-964				4	34	-46	-6	233	29	-47	324	-439

Grand Total, 74. General Mean, 1.5416.

The total number of combinations of treatments is 24, allowing 4 no-nitrogen plots without phosphate, and 4 with phosphate. There is thus complete replication only in duplicate—blocks A and B against blocks C and D. If the untreated plots are for the moment regarded as distinct treatments, the total 47 degrees of freedom will be divided into 1 for blocks, 23 for treatments, and 23 for error. But there are really only 18 distinct treatments, since no-urea is the same as no-cyanamide, etc., and the variation between the 16 untreated plots with  $4 \times (4 - 1)$ , or 12 degrees of freedom, is properly ascribed to error, since it is the variation, within blocks, of similarly treated plots. The analysis therefore becomes:—

	Blocks	...	...	...	...	...	...	...	1
	Treatments	...	...	...	...	...	...	...	17
Error	Interaction of Blocks and Treatments		...	...	...	...	...	17	29
	Between Parallel Plots		...	...	...	...	12		
								47	

Now the 17 degrees of freedom may be further subdivided into the effects due respectively to quantity of nitrogen, to kind of nitrogen supplied, to phosphate, and to the various interactions of these three. The details of this are given in the arithmetical working which follows. But one more point falls to be considered. The arrangement is such that half of the plots within each block receive phosphate, while half do not, while each of the blocks A, B, C, and D contain the 12 nitrogenous combinations. Furthermore, the sulphate and cyanamide plots go together: they are without phosphate in A and D and with phosphate in B and C. Also the muriate and urea plots go together in the reverse order. The experiment is thus designed in 4 blocks, but 1 degree out of those for the interaction of kind of nitrogen with phosphate is deducted from the treatment total: it is the one for sulphate and cyanamide against muriate and urea in their response to phosphate, and has been sacrificed for the sake of increasing the precision in the other comparisons by confounding it with blocks so that it cannot therefore be distinguished from the effects of soil differences. As will be seen from the calculation at the foot of the next page, any arbitrary addition to all the plot yields of any one block will affect the result, which cannot therefore be taken as indicative of true treatment effect. This degree of freedom and a corresponding one from error go to make up the new block component of 3, and our analysis is essentially as below:—

	Blocks	...	...	...	...	...	...	...	3
	Treatments	...	...	...	...	...	...	...	16
	Error	...	...	...	...	...	...	...	28
									47

The sum of the squares of the 48 plot yields (taken from a working mean of 370) is 448,080. Subtract 114.08, which is  $\frac{1}{48}$ th of the square of the grand total 74, and we are left with 447,965.92. Next for blocks we have:—

Number.										Square.
-296	..	..	..	..	..	..	..	..	..	87,616
205	..	..	..	..	..	..	..	..	..	42,025
131	..	..	..	..	..	..	..	..	..	17,161
34	..	..	..	..	..	..	..	..	..	1,156
Total ..	..	..	..	..	..	..	..	..	..	147,958
Divide by 12 ..	..	..	..	..	..	..	..	..	..	12,329.83
Subtract ..	..	..	..	..	..	..	..	..	..	114.08
Remainder ..	..	..	..	..	..	..	..	..	..	12,215.75

For the total sum of squares of deviations due to treatments (17 degrees of freedom) we make use of the 18 column totals in the table of yields. The first two numbers, for the untreated plots, are totals of 8 plots, while the others are totals of 2 plots. The calculation is therefore as follows:—

Number.					Square.	Number.				Square.
-723	..	..	..	..	522,729	144	..	..	..	20,736
-964	..	..	..	..	929,296	-6	..	..	..	36
Total ..	..	..	..	..	1,452,025	252	..	..	..	63,504
Divide by 4 ..	..	..	..	..	363,006.25	233	..	..	..	54,289
-15	..	..	..	..	225	98	..	..	..	9,604
4	..	..	..	..	16	29	..	..	..	841
101	..	..	..	..	10,201	260	..	..	..	67,600
34	..	..	..	..	1,156	-47	..	..	..	2,209
90	..	..	..	..	8,100	306	..	..	..	93,636
-46	..	..	..	..	2,116	324	..	..	..	104,976
Total ..	..	..	..	..	384,820.25	Total ..	..	..	..	417,431
					417,431					
Sum ..	..	..	..	..	802,251.25					
Divide by 2 ..	..	..	..	..	401,125.62					
Subtract ..	..	..	..	..	114.08					
Remainder ..	..	..	..	..	401,011.54					

From this result we must subtract the component of interaction of phosphate with the comparison sulphate and cyanamide *versus* urea and muriate. The calculation is: Subtract the total of the sulphate and cyanamide plots without phosphate from the corresponding total with phosphate, then from the remainder subtract the corresponding difference for the urea and muriate plots. Thus:—

S + C with phosphate	..	..	549	U + M with phosphate	..	..	687
S + C without phosphate	..	..	-30	U + M without phosphate	..	..	555
Difference	..	..	579	Difference	..	..	132
Subtract	..	..	132				
Remainder	..	..	447				
Square	..	..	199,809				
Divide by 32	..	..	6,244.03	(1 degree of freedom).			

The last stage in the calculation consists of squaring the number 447 and dividing by 32, the number of plots having a nitrogenous dressing and therefore coming in to this comparison. Subtracting the result from the total sum of squares due to treatments, we are left with 394,767.51.

The sum of squares due to error is now easily obtained by subtraction from the total, and up to this point our results are:—

Due to—	Degrees of Freedom.	Sum of Squares.
Blocks .. .. .	3	12,215.75
Treatments .. .. .	16	394,767.51
Error .. .. .	28	40,982.66
Total .. .. .	47	447,965.92

There are, however, many components into which the sum of squares due to treatment can be divided, in order to ascertain the separate effects due to the particular manures tested, or the interaction between them in combination. We shall take these in turn.

(a) Quantitative effect of nitrogen, of phosphate, and of interaction between them.

The following table is formed from the lower margin of the table of yields given above, each figure being a total of 8 plots:—

	No Nitrogen.	Single Nitrogen.	Double Nitrogen.	Total.
With Phosphate .. .. .	-723	320	916	513
Without Phosphate .. .. .	-964	-14	539	-439
Total .. .. .	-1,687	306	1,455	74

Number.	Square.	Number.	Square.
-723 ..	522,729	-1,687 ..	2,845,969
-964 ..	929,296	306 ..	93,636
320 ..	102,400	1,455 ..	2,117,025
-14 ..	196	Total ..	5,056,630
916 ..	839,056	Divide by 16 ..	316,039.37
539 ..	290,521	Subtract ..	114.08
Total ..	2,684,198	Remainder (a) ..	315,925.29 (2 degrees of freedom).
Divide by 8 ..	335,524.75	{ 513 - (-439) } <sup>2</sup>	906,304
Subtract ..	114.08	Divide by 48 (b)	18,881.33 (1 degree of freedom).
Remainder ..	335,410.67 (5 degrees of freedom).		
Subtract ..	315,925.29		
and	18,881.33		
Remainder (c) ..	604.05 (2 degrees of freedom).		

The amount marked (a) is the amount due to quantity of nitrogen, (b) is the amount due to phosphate, while (c) is that due to interaction between quantity of nitrogen and phosphate,

(b) Qualitative effect of nitrogen, and interaction with quantity.

From the margin of the yield table, leaving out the untreated plots, we can construct the following table:—

Quantity.	Quality.				Total.
	C	U	M	S	
1	135	-11	138	44	306
2	127	485	630	213	1,455
	262	474	768	257	1,761

Number.	Square.	Number.	Square.
135 ..	18,225	262 ..	68,644
127 ..	16,129	474 ..	224,676
-11 ..	121	768 ..	589,824
485 ..	235,225	257 ..	66,049
138 ..	19,044		
630 ..	396,900	Divide by 8 ..	949,193
44 ..	1,936	Subtract ..	118,649.12
213 ..	45,369		96,910.03
Total ..	732,949	Remainder (d) ..	21,739.09 (3 degrees of freedom).
Divide by 4 ..	183,237.25	(1,455 - 306) <sup>2</sup>	1,320,201
1761 <sup>2</sup> ÷ 32	96,910.03	Divide by 32 (e) ..	41,256.28 (1 degree of freedom).
Difference ..	86,327.22 (7 degrees of freedom).		
Subtract ..	21,739.09		
and	41,256.28		
Remainder (f) ..	23,331.85 (3 degrees of freedom).		

The result (d) is the sum of squares due to qualitative differences in the nitrogen supplied; (e) represents the difference between single and double nitrogen, and is part of the amount (a) already calculated, while (f) represents the interaction between quantity and quality of nitrogen.

(c) Interaction of quality of nitrogen and phosphate.

This consists of three parts—one, for the comparison sulphate and cyanamide *versus* urea and muriate, has been disposed of already, and the calculation for the others, *i.e.* sulphate and urea *versus* cyanamide and muriate, and sulphate and muriate *versus* cyanamide and urea, is similar, and is carried out below:—

S + U with phosphate .. .. .	587	C + M with phosphate .. .. .	649
S + U without phosphate .. .. .	144	C + M without phosphate .. .. .	381
Difference .. .. .	443	Difference .. .. .	268
Subtract .. .. .	268		
	175		
S + M with phosphate .. .. .	800	C + U with phosphate .. .. .	436
S + M without phosphate .. .. .	225	C + U without phosphate .. .. .	300
Difference .. .. .	575		
Subtract .. .. .	136		
Remainder .. .. .	439		

	Number.	Square.
	175	30,625
	439	192,721
Total	..	223,346
Divide by 32	..	6,979.56 (2 degrees of freedom).

(d) Second order interaction. Quantity and quality of nitrogen with phosphate.

This is similar to the last, but reversing the single and double quantities. The procedure is as follows:—

S + C (double N) with phosphate	..	358	(single N) with phosphate	..	..	191
without phosphate	..	-18	without phosphate	..	..	-12
Difference	..	..	Difference	..	..	203
Subtract	..	..	Subtract	..	..	203
Remainder	..	..	Remainder	..	..	173
U + M (double N) with phosphate	..	558	(single N) with phosphate	..	..	129
without phosphate	..	557	without phosphate	..	..	-2
Difference	..	..	Difference	..	..	131
Subtract	..	..	Subtract	..	..	131
Remainder	..	..	Remainder	..	..	-130

Difference of the results = 173 - (-130) = 303.

A similar procedure to the above for the other groupings, viz., sulphate and urea against cyanamide and muriate, and sulphate and muriate against urea and cyanamide, gives 375 and -37 respectively. We then have:—

	Number.	Square.
	303	91,809
	375	140,625
	-37	1,369
Sum	..	233,803
Divide by 32	..	7,306.34 (3 degrees of freedom).

This last term of the treatment sum of squares might have been got by subtraction from the total, since all the other items are known, but it is useful to work it out separately and thus check the whole of the working. In full, the analysis of variance is as follows:—

ANALYSIS OF VARIANCE.

Due to—	Degrees of Freedom.	Sum of Squares.	Mean Square.	$\frac{1}{2}$ loge. (Mean Square.)
Blocks	3	12,215.75	4,071.92	—
Quantity of Nitrogen (N)	2	315,925.29	157,962.64	2.5312
Quality of Nitrogen	3	21,739.09	7,246.36	0.9902
Phosphate (P)	1	18,881.33	18,881.33	1.4691
Interactions—				
Quantity and quality of N	3	23,331.85	7,777.28	1.0256
P and quantity of N	2	604.05	302.02	—
P and quality of N	2	6,979.56	3,489.78	0.6249
P with quantity and quality of N	3	7,306.34	2,435.45	—
Error	28	40,982.66	1,463.67	0.3810
Total	47	447,965.92	—	—

We are now able to assess the effects of the various treatments. Quantity of nitrogen is undoubtedly significant; this is, in fact, the biggest of all the observed changes. The effect of phosphate is also well marked. The values of  $z$  reached for quality of nitrogen and for interaction of quantity and quality are also significant, although they do not reach the 1 per cent. level of .7595 (for  $n_1 = 3$  and  $n_2 = 28$ ). The interactions of nitrogen and phosphate are not significant, and it therefore becomes clear how we are to present the results. A table showing the average of all plots without phosphate and all with phosphate is necessary, and also a two-way table showing the average values for the different kinds of nitrogen at the single and double dressings, and without nitrogen. The first of these tables is formed from the following material:—

Total of 24 phosphate plots	..	..	..	..	370 × 24 + 513 = 9,393
Total of 24 plots without phosphate	..	..	..	..	370 × 24 - 439 = 8,441
Average	..	..	..	..	= 8,917
Standard error (total of 24 plots)	..	..	..	..	$\sqrt{1,463.67 \times 24} = 187.4$
Factor for conversion to cwt. per acre	..	..	..	..	$\frac{40}{24 \times 8 \times 112} = \frac{1}{537.6}$

Average of all Nitrogenous treatments.	Without Phosphate.	With Phosphate.	Mean.	Standard Error.
Grain, cwt. per acre	..	..	15.7	0.35
Grain, per cent.	..	..	94.7	2.10
			17.5	105.3
			16.6	100.0

The second table is based on totals of four plots, except for the no-nitrogen total, which is of 16 plots. The standard error for a total of 4 plots is equal to  $\sqrt{1,463.67 \times 4} = 76.5$ , while the totals themselves are obtained by adding (4 × 370) to the totals already reached. We have, in units of  $\frac{1}{8}$  lb. per 4 plots:—

	S	M	C	U
No nitrogen	..	..	..	..
Single nitrogen	..	..	..	..
Double nitrogen	..	..	..	..
	1,524	1,618	1,058.25	1,469
	1,693	2,110	1,615	1,965
			1,607	1,965
Factor for conversion to cwt. per acre	$= \frac{1}{89.6}$			

Average of plots with and without phosphate.	Grain, cwt. per acre.				Grain, per cent.			
	Sulphate of Ammonia.	Muriate of Ammonia.	Cyana-mide.	Urea.	Sulphate of Ammonia.	Muriate of Ammonia.	Cyana-mide.	Urea.
Quantity of Nitrogen	..	..	..	..	..	..	..	..
	17.0	18.1	18.0	16.4	102.5	108.9	108.7	98.8
	18.9	23.5	17.9	21.9	113.9	142.0	108.1	132.2
Standard error	0.85				5.15			

Significant responses to superphosphate and to single and double nitrogenous dressings, No differences between the equivalent nitrogenous manures appear in the single dressing, but the double dressing gives no further increase with cyanamide and very little with sulphate.

REFERENCES AND PAPERS ON APPLIED STATISTICS PUBLISHED FROM  
THE ROTHAMSTED EXPERIMENTAL STATION.

- (1) R. A. FISHER. 1925. *Statistical Methods for Research Workers*. Edinburgh: Oliver & Boyd. 3rd Edition. 1930.
- (2) R. A. FISHER. 1921. Studies in Crop Variation. I. An Examination of the Yield of Dressed Grain from Broadbalk. *Journal of Agricultural Science*, Vol. XI, pp. 107-135.
- (3) R. A. FISHER and W. A. MACKENZIE. 1923. Studies in Crop Variation. II. The Manurial Response of Different Potato Varieties. *Journal of Agricultural Science*, Vol. XIII, pp. 311-320.
- (4) R. A. FISHER. 1923. The Influence of Rainfall on the Yield of Wheat at Rothamsted. *Phil. Trans. of the Royal Society*, Series B, Vol. 213, pp. 89-142.
- (5) W. A. MACKENZIE. 1924. Studies in Crop Variation. III. An Examination of the Yield of Dressed Grain from Hoos Field. *Journal of Agricultural Science*, Vol. XIV, pp. 434-460.
- (6) R. A. FISHER. 1926. The Arrangement of Field Experiments. *Journal of the Ministry of Agriculture*, Vol. XXXIII, pp. 503-513.
- (7) L. H. C. TIPPETT. 1926. On the Effect of Sunshine on Wheat Yield at Rothamsted. *Journal of Agricultural Science*, Vol. XVI, Part II, pp. 159-165.
- (8) T. EDEN and R. A. FISHER. 1927. Studies in Crop Variation. IV. The Experimental Determination of the Value of Top Dressings with Cereals. *Journal of Agricultural Science*, Vol. XVII, pp. 548-562.
- (9) T. N. HOBLYN. 1928. A Statistical Analysis of the Daily Observations of the Maximum and Minimum Thermometers at Rothamsted. *Quarterly Journal of the Royal Meteorological Society*, Vol. LIV, No. 227, pp. 183-202.
- (10) R. A. FISHER and T. N. HOBLYN. 1928. Maximum and Minimum Correlation Tables in Comparative Climatology. *Geografiska Annaler H*, 3, pp. 267-281.
- (11) BHAI BALMUKAND. 1928. Studies in Crop Variation. V. The Relation between Yield and Soil Nutrients. *Journal of Agricultural Science*, Vol. XVIII, pp. 602-627.
- (12) T. EDEN and R. A. FISHER. 1929. Studies in Crop Variation. VI. Experiments on the Response of the Potato to Potash and Nitrogen. *Journal of Agricultural Science*, Vol. XIX, pp. 201-213.
- (13) A. R. CLAPHAM. 1929. The Estimation of Yield in Cereal Crops by Sampling Methods. *Journal of Agricultural Science*, Vol. XIX, Part II, pp. 214-235.
- (14) J. WISHART and H. J. G. HINES. 1929. Fertiliser Trials on the Ordinary Farm. *Journal of the Ministry of Agriculture*, Vol. XXXVI, No. 6, pp. 524-532.
- (15) J. WISHART and A. R. CLAPHAM. 1929. A Study in Sampling Technique: The Effect of Artificial Fertilisers on the Yield of Potatoes. *Journal of Agricultural Science*, Vol. XIX, Part IV, pp. 600-618.
- (16) H. G. SANDERS. 1930. A Note on the Value of Uniformity Trials for Subsequent Experiments. *Journal of Agricultural Science*, Vol. XX, Part I, pp. 63-73.
- (17) J. WISHART and W. A. MACKENZIE. 1930. Studies in Crop Variation. VII. The Influence of Rainfall on the Yield of Barley at Rothamsted. *Journal of Agricultural Science*, Vol. XX, Part III, pp. 417-439.
- (18) F. E. ALLAN and J. WISHART. 1930. A Method of Estimating the Yield of a Missing Plot in Field Experimental Work. *Journal of Agricultural Science*, Vol. XX, Part III, pp. 399-406.
- (19) J. WISHART. 1930. On the Secular Variation of Rainfall at Rothamsted. *Memoirs of the Royal Meteorological Society*, Vol. III, No. 27, pp. 127-137.
- (20) R. J. KALAMKAR. 1930. Studies in Crop Variation. VIII. An Application of the Resistance Formula to Potato Data. *Journal of Agricultural Science*, Vol. XX, Part III, pp. 440-454.