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

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The four sections are devoted to :—

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B.—The arithmetical processes.

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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 × 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 × 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 × 6 squares the number of cards, even for restricted squares, would be enormous, and one would have to confine the 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 ¼ lb.

Blocks.	Treatments— 1	2	3	4	5	Total.	Mean.
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	—
Mean.. ..	258·$\dot{3}$	320·0	351·0	374·1$\dot{6}$	386·$\dot{3}$	—	337·9$\dot{6}$ 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 $(50 + a)^2 = 2{,}500 + 100\,a + a^2$, 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 plot 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 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 a 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 is 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 × 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 × 3 Latin square only two degrees remain for error, and in consequence a single 3 × 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. Number.	Blocks. Square.	Treatments. Number.	Treatments. 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

$$107{,}680{\cdot}97 - (31{,}634{\cdot}17 + 62{,}903{\cdot}47).$$

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 6/40th acre :—

Treatments— 1	2	3	4	5	Mean.	Standard error.
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

$$\sqrt{657{\cdot}17 \times 6.}$$

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

$$\frac{40}{4 \times 6 \times 112}, \text{ i.e. by } \frac{1}{67{\cdot}2}.$$

If a machine is used, the reciprocal of this number, i.e. ·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.* ·0493145.

Expressed in tabular form the final results are :—

—	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, 0·934 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 ¼ lb. per 1/40th acre plot, less 275.

	Columns.					Total.	Mean.
Rows.	N 27	U −30	M −24	S −49	C −37	−113	−22·6
	S 19	C − 2	N 19	M −11	U −10	15	3·0
	M 41	S − 4	U 13	C 11	N 1	62	12·4
	C 28	N 31	S 20	U −23	M −11	45	9·0
	U 31	M 25	C 23	N 31	S −19	91	18·2
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 :—

ANALYSIS OF VARIANCE.

Due to—	Degrees of Freedom.	Sum of Squares.	Mean Square.	$\frac{1}{2}\log_e$ (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) $= \sqrt{108 \cdot 87 \times 5} = 23 \cdot 33$, or 1·67 per cent. of the mean yield 5 × (275 + 4), or 1,395 ¼-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 × 275, or 1,375, by adding 1,375 to each. We obtain the following results :—

Treatments— U	S	M	N	C	Mean.	Standard Error.
1,356	1,342	1,395	1,484	1,398	1,395	23·33

These figures are yields in ¼ lb. per 5/40th 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 :—

BARLEY, 1929.

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 1/40th acre, in 4 randomised blocks.

PLAN.

N.E.

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			

TREATMENTS.

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, $1 \cdot 541\dot{6}$.

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. 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 1/48th 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.
−723	522,729
−964	929,296
Total	1,452,025
Divide by 4	363,006·25
−15	225
4	16
101	10,201
34	1,156
90	8,100
−46	2,116
Total	384,820·25
	417,431
Sum	802,251·25
Divide by 2	401,125·62
Subtract	114·08
Remainder	401,011·54

Number.	Square.
144	20,736
−6	36
252	63,504
233	54,289
98	9,604
29	841
260	67,600
−47	2,209
306	93,636
324	104,976
Total	417,431

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
S + C without phosphate	−30
Difference	579
Subtract	132
Remainder	447
Square	199,809
Divide by 32	6,244·03 (1 degree of freedom).

U + M with phosphate	687
U + M without phosphate	555
Difference	132

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.	
−723	..	522,729	
−964	..	929,296	
320	..	102,400	
−14	..	196	
916	..	839,056	
539	..	290,521	
Total	..	2,684,198	
Divide by 8	..	335,524·75	
Subtract	..	114·08	
Remainder	..	335,410·67	(5 degrees of freedom).
Subtract and	..	315,925·29 18,881·33	
Remainder (*c*)	..	604·05	(2 degrees of freedom).

Number.		Square.	
−1,687	..	2 845,969	
306	..	93,636	
1,455	..	2,117,025	
Total ..	..	5,056,630	
Divide by 16	..	316,039·37	
Subtract	..	114·08	
Remainder (*a*)	..	315,925·29	(2 degrees of freedom).
$\{513-(-439)\}^2$		906,304	
Divide by 48 (*b*)		18,881·33	(1 degree 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. C	U	M	S	Total.
1	135	−11	138	44	306
2	127	485	630	213	1,455
	262	474	768	257	1,761

Number.	Square.	
135 ..	18,225	
127 ..	16,129	
−11 ..	121	
485 ..	235,225	
138 ..	19,044	
630 ..	396,900	
44 ..	1,936	
213 ..	45,369	
Total	732,949	
Divide by 4 ..	183,237·25	
$1761^2 \div 32$	96,910·03	
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).

Number.	Square.	
262 ..	68,644	
474 ..	224,676	
768 ..	589,824	
257 ..	66,049	
	949,193	
Divide by 8 ..	118,649·12	
Subtract ..	96,910·03	
Remainder (*d*) ..	21,739·09	(3 degrees of freedom).
$(1{,}455 - 306)^2$	1,320,201	
Divide by 32 (*e*)..	41,256·28	(1 degree 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
S + U without phosphate	144
Difference	443
Subtract	268
	175

C + M with phosphate	649
C + M without phosphate	381
Difference	268

S + M with phosphate	800
S + M without phosphate	225
Difference	575
Subtract	136
Remainder	439

C + U with phosphate	436
C + U without phosphate	300
	136

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	376	Difference	203
Subtract	203		
Remainder	173		
U + M (double N) with phosphate ..	558	(single N) with phosphate	129
without phosphate	557	without phosphate	−2
Difference	1	Difference	131
Subtract	131		
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.	½ log$_e$. (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	—	—

* See also The Design of Experiments, Section 52.

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	$370 \times 24 + 513$	$= 9{,}393$
Total of 24 plots without phosphate	$370 \times 24 - 439$	$= 8{,}441$
Average		$= 8{,}917$
Standard error (total of 24 plots) $\sqrt{1{,}463 \cdot 67 \times 24}$		$= 187 \cdot 4$
Factor for conversion to cwt. per acre	$\frac{40}{24 \times 8 \times 112}$	$= \frac{1}{537 \cdot 6}$

Average of all Nitrogenous treatments.	Without Phosphate.	With Phosphate.	Mean.	Standard Error.
Grain, cwt. per acre	15·7	17·5	16·6	0·35
Grain, per cent.	94·7	105·3	100·0	2·10

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 $\sqrt{1{,}463 \cdot 67 \times 4} = 76 \cdot 5$, 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		1,058·25		
Single nitrogen	1,524	1,618	1,615	1,469
Double nitrogen	1,693	2,110	1,607	1,965

Factor for conversion to cwt. per acre $= \frac{1}{89 \cdot 6}$.

Average of plots with and without phosphate.	Grain, cwt. per acre.				Grain, per cent.			
	Sulphate of Ammonia.	Muriate of Ammonia.	Cyanamide.	Urea.	Sulphate of Ammonia.	Muriate of Ammonia.	Cyanamide.	Urea.
Quantity of Nitrogen 0		11·8				71·2		
Quantity of Nitrogen 1	17·0	18·1	18·0	16·4	102·5	108·9	108·7	98·8
Quantity of Nitrogen 2	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.

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