institution from their experimental use, owing to the legitimacy in much physical material of the assumption that the calibration remains valid, whereas for biological material this assumption is often so palpably untenable that a good biological experiment has to contain within itself the controls needed to give confidence in the conclusions to be drawn. It is, I imagine, this common simplification of physical research which has led to the problem of experimentation being much more thoroughly elaborated in biological and psychological than in physical research. A good biological experiment, for example in agriculture, is in fact from the logical standpoint a far more perfect whole than a physical experiment need ordinarily be.

Fisher to G.W. Snedecor: 25 *April* 1935

I was glad to see a paper of yours in the *Journal of the [American]* Statistical Association,[1] and that you have been trying out the analysis of covariance in its application to the educational material at Ames. I am, however, entirely puzzled by your references to my work. You say, at the end, that in my fifth edition [*SMRW*] I do not 'accept all Bartlett's conclusions.' Perhaps not, if the phrase is taken quite literally, but it seems to refer to the method you had just given of isolating, in the analysis of variance, the components due to differences between regressions, either the regressions between different lots or that among lots. It would be strange if I did not agree with this, as I taught him the method while he was at University College, and communicated his paper[2] to the Cambridge Philosophical Society. The secretary, Whyte, must have forgotten that I am a Fellow, for he put the paper in as communicated by himself. Bartlett does not suggest that his procedure is other than mine, though the acknowledgement of its origin (p.333) is a bit ambiguous.

The fact is that Wishart seems to have jumped to the conclusion, some time, I should think, about 1933, that because the sum of squares of deviations of the adjusted means of groups may be derived simply from the sum of squares and products in the covariance table, in the way I set out in my fourth edition, that no allowance for the sampling error of the estimated regression coefficient was necessary in making an exact z test. As an approximation this is harmless enough in experimental data where the material has been properly randomised, for in that case, the independent variate will have been randomised also. So that the differences between the means of the different groups will be small compared with the differences within the groups. Anyhow, in the summer of 1933, several people working with Wishart, Wilks, Bartlett and Cochran, among others, were all at work applying the method wrongly to material in which the approximation was very bad indeed; and it was not until their papers began to come in to me, as referee, in the autumn and winter of that year, that I knew what they had been doing. Indeed, by that time some of them certainly had a suspicion that

the method was wrong, though without seeing how to put it right.

In Wilks' case the effect was most unfortunate. He is by nature extremely academic, and his training has not made him familiar with the research aspect of statistical problems. If his time had been better spent in this country, he might really have widened his grasp of what mathematics are good for in this connection. As it was, he got hold of the idea that the whole question of analysis of variance needed a rigorous overhauling, and no one seems to have taken the trouble to refer him to the original papers in which the method was built up. At one time, I understand, he thought he had proved that the Latin square was invalid, but I understand he gave that up when Cochran showed him the simple algebra of the kind which Irwin has from time to time published, by which the unbiassed character of the test is demonstrable merely from the use of randomisation. Finally Wishart persuaded Yule to submit a paper of his to the Royal Society, but owing to his lack of background, it was so pointless that it had to be turned down.[3]

The mistake was evidently partly my fault, for it had happened that neither in my first paper on the analysis of covariance, with Eden, in 1927 (*J.A.S.* 17. 548) [*CP* 57] which Tippett had reproduced in his book,[4] nor in the section which I had added to the fourth edition of my book, had I had occasion to apply the z test, and Tippett had not done so, either. I do think, however, that Wishart would have shown better sense if he had consulted me as soon as he had found there was any difficulty, instead of stolidly teaching the wrong method, at least up to the time of his address to the Statistical Society (Jan. 1934)[5] when I happened to be Chairman. If you look up the *J.R.S.S. Supplement*, Vol.1 p.43, you will see his statement, and his bewildered footnote, added later. I took the next opportunity, at a meeting of the Section, to give the exact test (Vol.1. p. 198),[6] as Pearson and Miss James had produced a wildly erroneous application.

It may interest you that an exactly similar situation arises when correction is made for a missing observation. If you put x for such a missing observation, all the entries for sums of squares will be simple quadratic functions of x, and, if that for error is $(a + 2bx + cx^2)$, then obviously its minimum value is $a - b^2/c$.

The value of x which gives this minimum value is the best estimate that can be made of the missing observations, but if this value is inserted in the analysis, or, as Wishart says (*J.A.S.* Vol. 20 p.406),[7] we 'proceed as usual with the analysis of variance, using the estimated figure for the missing yield,' we shall ignore the fact that the precision of our comparison is lowered whenever an observation is lost. To make an exact z test, it is only necessary to minimise equally the sum of treatments plus error, and to ascribe the difference between this minimum and that for error to the treatment effects. The sum of squares for treatment, obtained in this way, will always be less than that using the best estimated treatment means, for it allows for the greater inaccuracy with which they were estimated. In fact, the particular component of which the error is enhanced has been scaled down until it has the same precision as the others. In Wishart's example, p.405, the sum of squares for treatment should be reduced from 22148 to 20892, if I have done the arithmetic right.

The analysis of covariance is useful for such a variety of purposes that I took a great deal of trouble, the winter before last, to demonstrate its proper use and to prove its exactitude when properly used. Both points needed a good deal of hammering at. I believe the exactitude of the test is no longer disputed, but I think you may be glad to have the facts before you for any future reference you may make to the subject.

[1] Snedecor, G.W. (1935). Analysis of covariance of statistically controlled grades. *J. Am. Statist. Ass.* **30**, 263–8.
[2] Bartlett, M.S. (1934). The vector representation of a sample. *Proc. Camb. Phil. Soc.* **30**, 327–40.
[3] See Fisher's letter of 27 December 1933 to Wilks (p.299).
[4] Tippett, L.H.C. (1931). *The methods of statistics*. Williams and Norgate, London.
[5] Wishart, J. (1934). Statistics in agricultural research. *J. R. Statist. Soc., Suppl.* **1**, 26–51.
[6] See Discussion following Wilsdon, B.H. (1934). Discrimination by specification statistically considered and illustrated by the standard specification for Portland cement. (with Appendices by E.S. Pearson and F.E. James) *J. R. Statist. Soc., Suppl.* **1**, 152–92.
[7] Allan, F.E. and Wishart, J. (1930). A method of estimating the yield of a missing plot in field experimental work. *J. Agric. Sci.* **20**, 399–406.

Fisher to R. Summerby: 25 *April* 1932

It is a great pleasure to hear from you, and to discuss again some of the problems of experimental design and interpretation. . . .

As to when you should use the P tabled in my book, and when the half value, is a point which I might well have discussed more fully.[1] Let me take an example. A man comes and declares that if we would only use a rotary cultivator we should get much better germination with our mangolds. We might try an unreplicated experiment, with single areas under the two treatments, then if the germination really does better we shall think there is something in the assertion, but if not we will think no more about it. In circumstances like these when we come to replication and estimates of error, it is clear that the new method scores a significant success only if it differs from the old in the positive direction; consequently if we decide to use the 5 per cent point we ought in fairness to give him 5 per cent in the right hand side of the curve, i.e. everything beyond $t = +1.645$, if n is large, because he is not going to claim any success if the difference is negative, however large it is.

On the other hand with a pair of varieties the fact that one beats the other in a single trial would not contribute anything to our decision, unless we already had prior evidence that one was better than the other, and are just testing it further. One or other is bound to win; and so in the test of significance we shall count both tails as significant, and for the 5 per cent point allot 2.5 per cent to each, so counting anything outside the range $t = \pm 1.960$. . . .

[1] Summerby had asked Fisher to elaborate on his discussion of this point in Section 12 (p.45) of *SMRW*.

D.S. Villars to Fisher: 26 *September* 1938

. . . I have recently been interested in using a new test for homogeneity of a set of variances and I wonder if I could again presume upon your kindness to clear up a point which I am not sure I fully understand. . . . Thus, suppose we have the set of variance estimates $s_1^2, s_2^2, \ldots s_n^2$. Compute $z_1' = \frac{1}{2} \ln s_1^2$, $z_2' = \frac{1}{2} \ln s_2^2, \ldots z_n' = \frac{1}{2} \ln s_n^2$ (where ln means *natural* logarithm). Next compute the sum of squares of deviations of the z''s from their mean. This sum of squares of deviations must equal the sum of squares of deviations of legitimate z''s which might have been computed by forming ratios of each s_i^2 to any new independent variance estimate, s_{n+1}^2. So, form the ratio of this sum of squares of deviations to the true variance of z which you state in your book (*Statistical Methods*, 6 [edn.], 231)[1] to be

$$\sigma^2 = \tfrac{1}{2}\left\{\frac{1}{f_1} + \frac{1}{f_{n+1}}\right\}$$

(where the f's are the number of degrees of freedom) and look up in the χ^2 table for $n - 1$ degrees of freedom. This test is valid for the case where we have a set of variances of equal numbers of degrees of freedom.

Now what I am not quite certain about is this. What should one use for f_{n+1} in the formula for the variance of z? . . . Also I would like to ask if you see any fundamental objection to this method. . . .

[1] *SMRW*, p. 228.

Fisher to D.S. Villars: 4 *October* 1938

The method you propose for testing for significant variation among a group of estimated variances is quite a reliable one, which I have sometimes used, though I do not know that it has anywhere been published.

As you know, if σ^2 is the true variance and s^2 an estimate of it based on n degrees of freedom, then ns^2/σ^2 is distributed as is χ^2; consequently the sampling variance of half the natural logarithm of s^2 is the sampling variance of $\log \chi$.

This may be expressed exactly as the sum of an infinite series

$$\frac{1}{n^2} + \frac{1}{(n+2)^2} + \frac{1}{(n+4)^2} + \ldots$$

This is a function which has been tabulated under the name of the Trigamma Function (B.A. *Mathematical Tables*, Vol. I), actually what you want being 1/4 trigamma in $(n-2)/2$. For the first few values of n the actual values are as shown below, where I have put the approximate value $1/2(n-1)$ for comparison:

1	1.2337055	
2	0.4112335	0.5

3	0.2337006	0.25
4	0.1612335	0.167
5	0.1225944	0.125
6	0.0987335	0.1000
7	0.0825944	0.0833
8	0.0709557	0.0714

You will see that, except for very small samples, you would need an enormous number of them for the difference between the exact and approximate variance to matter appreciably.

Knowing the true variance of the natural logarithm of s, which, of course, is half the natural logarithm of s^2, you can, as you suggest, make a very precise test of heterogeneity. Using the method of Section 21.03, Ex. 14, in my book [*SMRW*], making a comparison between the observed and the expected sum of squares of deviations from the mean of the logarithm, you will see that there is no need to introduce the total number of degrees of freedom, as the sum of squares you use is itself an empirical measure of the variance of the quantities among which significant variation is being tested. . . .

H.M. Walker to Fisher: 15 March 1940

. . . A problem which arises often in educational research, with which I do not know how to deal, is this. Suppose that the means of two samples have been compared on k different traits, and suppose that p is the probability that on a single trait a given value of t, say t_0, would occur by chance if in the population the two means are equal. Then we should expect pk differences to be, by chance, as large as t. If more than pk differences are of this size, how do we interpret the significance of the several differences? This is not a problem for multivariate analysis. We do not want to know whether the two groups can be reliably differentiated on a group of traits, but we want to know on *which individual* traits they can be differentiated. This search for traits which will differentiate two criterion groups occurs in *many* of our studies. Recently I read a study in which the author studied 2500 items on a group of 700 persons to see which ones were reliably related to a criterion. On how many of these items would he be justified in drawing conclusions? If this has been discussed in any paper to which you can give me a reference, I shall be most grateful, for I can find nothing on the subject.

Fisher to H.M. Walker: 22 April 1940

. . . I have rather a dread of the procrustean process of forcing a problem to

fit a favourite method; for, quite early in life, I was impressed by the fatal effects of this in Karl Pearson's work; but I am not sure that your problem is not one for multivariate analysis. To begin with, unless the best multivariate discriminant is significant, I imagine no one would wish to go further, or to draw conclusions about individual traits; but, if it is significant, this means that it is significantly different from a kind of null criterion having all its coefficients zero. Now, one can equally test significance from less severely restricted criteria, e.g. from criteria having all coefficients of variables with suffix greater than r [equal to] zero. An insignificant difference here would surely allow one to say that only x_1 to x_r taken jointly are needed, and failure to obtain an insignificant difference when any of these is omitted would indicate that all were necessary. . . .

Fisher to S.S. Wilks: 27 December 1933

My wife and I were very glad to have your Christmas card. I hope you have had the best of good times this winter.

I have been looking through the paper submitted for you to the Royal, on the analysis of variance, and am much puzzled as to why you (and apparently Wishart also, if he suggested the problem) should feel that such an elementary point as that you discuss should need a new and very elaborate discussion.[1]

As regards the proofs I have given, I think from 1923–25, I was content with the partly intuitive treatment of linear forms as restrictions in multiple space, but in 1925 I gave an explicit analytical treatment, very simple in character, which enables you to prove all the cases you deal with in a page or two, and what is more important to perceive exactly what other cases it must apply to. In addition, cases in which Blocks (say) are not homogeneous are readily seen to present no difficulty, such as you suggest in your summary.

As the method seems not to have come to your notice, and as considerable harm is being done by the idea that the analysis of variance contains unproved assumptions, instead of being merely a convenient arithmetical arrangement arrived at some years after the sampling problems had been solved, I may as well outline it now.

If $x_1, \ldots, x_a$ are independent variates each normally distributed about zero with unit variance, then

$$\xi_r = p_{1r}x_1 + p_{2r}x_2 + \ldots + p_{ar}x_a$$

is normally distributed about zero, and its variance is unity if

$$\sum_{\alpha=1}^{a} (p_{\alpha r}^2) = 1. \qquad \text{(A)}$$

Also ξ_r and ξ_s are independent if

$$\sum_{\alpha=1}^{a} (p_{\alpha r} p_{\alpha s}) = 0 \tag{B}$$

If r variates, ξ, have already been constructed, there are r linear homogeneous equations for $p_{\alpha s}$ to satisfy, and this can be done up to $r = a - 1$, but no further. The condition (B) at once excludes the possibility that any set of coefficients can be linear functions of any selection of the other sets. Thus a new variates, ξ, may be constructed, in an infinite variety of ways, such that they are all normally and independently distributed with unit variance.

Note further that the rows of the p-matrix are the coefficients of ξ expressed in terms of x, and the columns are the coefficients of x expressed in terms of ξ. Also that

$$S(x^2) = S(\xi^2).$$

Hence

$$S(x^2) - \overset{r}{\underset{1}{S}}(\xi^2) = \overset{a}{\underset{r+1}{S}}(\xi^2)$$

which is distributed in samples as the sum of the squares of $a - r$ independent variates, wholly independent of each of $\xi_1, \ldots, \xi_r$, or any combination of them.

The validity of the z-distribution for any analysis of variance thus depends only on satisfying the condition (B) of orthogonality. It was the ignoring of this condition which spoilt Wishart's paper[2] in the German journal, where he failed to see that the treatments did not occur in all rows and columns, and in consequence obtained much too low a sum of squares in the residue. This is the only case I know of the analysis being applied wrongly.

For example, take deviations from the mean, let

$$\xi_1 = (x_1 + \ldots + x_a)/\sqrt{a}\,;$$

this satisfies (A), consequently

$$S(x^2) - S^2(x)/a = S(x - \bar{x})^2$$

is distributed as is the sum of $(a - 1)$ squares of independent variates having the same variance. Had Helmert taken this step he would have anticipated 'Student's' result by 32 years. These $(a - 1)$ variates may be specified in a number of ways, such as

$$\begin{aligned}
\xi_2 &= (x_1 - x_2)/\sqrt{2} \\
\xi_3 &= (x_1 + x_2 - 2x_3)/\sqrt{6} \\
\xi_4 &= (x_1 + x_2 + x_3 - 3x_4)/\sqrt{12} \\
\xi_a &= \{x_1 + \ldots + x_{a-1} - (a - 1)x_a\}/\sqrt{a(a-1)}
\end{aligned}$$

which are easily seen to be orthogonal.

Now suppose you have ab variates, x, arranged in a columns and b rows. From each row you can put down $\xi_p (p = 2, \ldots, a)$, and from the b values of ξ_p from the different rows you can similarly calculate $\xi_{pq} (q = 2, \ldots, b)$, giving $(a - 1)(b - 1)$ independent variates, all mutually orthogonal, and all independent of the sums of the rows and columns. There is, of course, no need to specify the $(a - 1)(b - 1)$ particular linear functions in this way; all that is needed is the general proof that it can always be done; and that the sum of their squares may be obtained by subtraction from the total.

For a 4 × 4 Latin square

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

you can take coefficients

																	Grand
	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	Total
	1	1	1	1	−1	−1	−1	−1	0	0	0	0	0	0	0	0	
	1	1	1	1	1	1	1	1	−2	−2	−2	−2	0	0	0	0	Rows 3
	1	1	1	1	1	1	1	1	1	1	1	1	−3	−3	−3	−3	
	1	−1	0	0	1	−1	0	0	1	−1	0	0	1	−1	0	0	
	1	1	−2	0	1	1	−2	0	1	1	−2	0	1	1	−2	0	Columns
	1	1	1	−3	1	1	1	−3	1	1	1	−3	1	1	1	−3	3
A−B	1	−1	0	0	−1	0	1	0	0	1	0	−1	0	0	−1	1	
A+B−2C	1	1	−2	0	1	0	1	−2	−2	1	0	1	0	−2	1	1	Treat-
A+B+C−3D	1	1	1	−3	1	−3	1	1	1	1	−3	1	−3	1	1	1	ments 3

All necessarily orthogonal if each treatment comes once in each row and once in each column. Whence it follows that the remaining 6 degrees of freedom are distributed as required independently of these 9.

It is possible that your attention has not been called to the 1925 paper, 'Applications of 'Student's' distribution' [*CP* 43], where this method is set out with especial reference to its application to regression, to which all the other applications can be reduced as special cases.

E.g. If u is a variate having values 1 in the first column, −1 in the second, and zero in the others, v has 1 in the first two, and −2 in the third, w has 1 in the first three and −3 in the fourth, then u, v and w are uncorrelated independent variates, and their elimination jointly is the same as the elimination of variation between columns. Since all that is assumed in the general case is normality and homogeneity of deviations from the regression surface, the differences among the columns may be as great as you please without affecting the validity of the comparisons among the other components. The limitation of the proof of the validity of the test of significance of treatments to the case when rows and columns have no significant effect is quite unnecessary. The general case is established by the same argument as the special case you have chosen for discussion.

If you find on consideration that you agree that the whole idea that complete and adequate proofs have not been given of the validity of the z-test in the analysis of variance is a mare's nest, and that cases to which such analysis can be applied are already satisfactorily defined, the question arises as to whether your paper can be amended so as to avoid the criticisms to which in its present form it is exposed. Probably you will think it wisest to withdraw it for the present, on the ground that it was written under a misapprehension as to what had already been demonstrated (for which you can in no way be held to blame).[3]

The ground would then be clear for any new paper you cared to publish on the characteristic functions of the distribution of the quadratic forms occurring in the analysis.

[1] Wilks's paper was entitled, 'On the independence of sums of squares in the analysis of variance'.
[2] Wishart, J. (1931). The analysis of variance illustrated in its application to a complex agricultural experiment in sugar beet. *Arch. f. Pflanz.* 5, 561–84.
[3] See also Fisher's letter of 25 April 1935 to Snedecor (p. 294).

Fisher to S.S. Wilks: 6 *February* 1934

Thanks for your letter of January the 20th, which has just reached me.[1] It has done much to clear up your attitude, and I see, at least, that though you say you were aware of the existence of the 1925 *Metron* paper, you have evidently, even now, not grasped the simplicity and generality of the proof there given, of the distributional problem needed for tests of significance in the analysis of variance. I say this (i) because you speak of it as containing an analytical method which *could* be applied, followed with 'but in as much as it had not been applied in the literature', as if something more needed to be done. Now, of course, a method of proof constitutes the proof in all cases to which the method is shown to be applicable, just as Euclid's fifth proposition is a method of proof shown by him to be applicable to any given isosceles triangle, and thereby constitutes a proof of this property for all such triangles. The class of cases to which I showed the method to be applicable was that of equivariant variation about any regression line or surface. And this includes, as no doubt you will readily appreciate, the experimental treatments which you have discussed.

In his paper of, I think, 1931, Irwin expressly disclaimed any novelty, in the result to be proved, and referred the reader to my paper of 1925 for a complete discussion.[2] In your letter you say 'The paper is definitely one of technique and does not purport to be a presentation of new discoveries'. I think, however, that every reader would judge that it did purport to supply

proofs on points on which no adequate proof had yet been given. My second reason for thinking that you have not apprehended the process of proof given in 1925, lies in your discussion on page 2 of your letter, where, after 'it is necessary, as I understand it to show' you discuss a procedure of most superfluous complexity.[3]

The example I had given in my letter of the natural method of applying the general process to any particular case was a Latin square, which I chose because that arrangement was dealt with in your paper as though it were one involving particular difficulty. Your comment 'A general proof, of course, would be one that holds for any Latin square whatever, without having to consider any particular one' seems to overlook the fact that what I illustrated was a method of writing down the coefficients of the linear components, applicable to any Latin square whatever. I cannot suppose that you want me to think that you do not perceive that it is so applicable, since its applicability rests merely on the mutual orthogonality of all differences among rows, all differences among columns, and all differences among treatments. If you were in doubt, perhaps the best method of satisfying yourself as to its general applicability would be to try and set up a Latin square to which it could not be immediately applied. This should lead you at least to perceive very clearly the simple reasons that make it generally applicable. You will perceive, too, that, apart from the arbitrary subdivision of the sets of 3 degrees of freedom into unitary parts, the sets of components are those picked out in the ordinary arithmetical procedure of the analysis, as is quite generally the case in the analysis of other experimental arrangements. That you have not grasped the point of the method, perhaps because it is too easy for you to start thinking about it, is shown by your statement 'The case of proving the mutual independence of sums of squares of deviations from means of rows, columns and treatments and residuals . . . is a little more difficult.' Perhaps you have not noticed that the sum of the squares of the three components assigned to rows *is* the sum of squares of deviations of rows from their mean, and that its independence of other sums of squares is assured by the mutual orthogonality of the linear components. With regard to residuals the simplest general treatment is that given in *Metron*.

I had hoped that on reading my previous letter you would have perceived that, in view of your unawareness of the very direct and complete proof already available, your paper, submitted to the Royal Society, is not one that would add to your reputation as a mathematician, however interesting you yourself may have found it, to develop the characteristic functions of the quadratic forms involved. You refer to a number of novel points, which come up as by-products, but you do not say what these are. Any such should, I suppose, receive publication in a short paper, in an appropriate technical journal. The Royal Society confines itself, as far as possible, to papers that are not only good of their kind, but also of more than specialist interest, and I should judge that I should be doing both the Society and yourself an ill service if I were to recommend your paper, as it now stands, for publication.[4]

If you are in any doubt it might be wise to ask the opinion of Professor Hotelling or some other American friend of standing. I need hardly say that I shall always be willing to assist you, or any other mathematician of merit, to gain adequate recognition for your contributions to knowledge. In the present case I judge that the whole work was undertaken under a most regrettable misapprehension, and that you may need some little time to familiarise yourself with methods of reasoning and of demonstration other than that in which you have so far specialised.

[1] Wilks asked Fisher to reconsider his paper, the purpose of which was, he said, to give by means of characteristic functions the proofs of independence of the sums of squares entering into the analysis of variance.

[2] In his letter to Fisher, Wilks had referred to Irwin's paper (Irwin, J.O. (1931). Mathematical theorems involved in the analysis of variance. *J.R. Statist. Soc.* **94**, 285–300) saying that it 'did not go into the question of independence — thus omitting a very important part of the theory'. Cf. Irwin, J.O. (1934). On the independence of the constituent items in the analysis of variance. *J. R. Statist. Soc., Suppl.* **1**, 236–51.

[3] The complete sentence in Wilks's letter is as follows:
'In order to carry out an analytical proof of the independence of a number of sums of squares by the method you give, it is necessary, as I understand it, to show that you can reduce each sum of squares to a sum of independently distributed squares diminished in number by the number of linear restrictions, and further, that the squares entering into each sum are independent of those entering any other.'

[4] Fisher's report on Wilks's paper, which he sent to the Royal Society on 26 March 1934, concludes as follows:
'I cannot help feeling, of the paper as a whole, that its aim has been so much misdirected from the first that the author would, in a few years' time, be glad that it should not be now published, since, as far as it goes, the proofs it provides amount to no more than that certain widely used experimental arrangements possess the properties that they were originally designed to possess, and which have never been, save by temporary and accidental confusion, in the least doubt. However, if other Fellows of the Society greatly wish for its publication, in spite of this opinion, I would raise no further objection, provided it is immediately followed by a demonstration that the cases here considered fall into the general class of simple regressions, and are demonstrable individually by a very simple application of the general proof.'
Wilks's paper was not, in fact, published by the Royal Society.

Fisher to E.B. Wilson: 25 January 1940

I am glad to have your nice long letter of January 3rd, which I have only just received. It is really a long while since I heard from you. I have heard from Fréchet, and hope to see him when he visits England, as I understand he has some such mission in view. So far, I am afraid, the Galton Laboratory has not contributed towards winning the war. We have been reluctantly evicted from our former premises, on the ground that the College was 'closed, and no infringement of its state of evacuation could be allowed'. Consequently our former habitation stands empty. The Serological section of my Department was taken over by the Medical Research Council, who, thank Goodness, have had the sense to maintain them in existence as a unit, and, after a struggle, the rest of us are permitted to continue our researches in rooms which Rothamsted has been good enough to lend us.

I have been doing nothing more useful than tabulating blood group frequencies, which show a very pleasing gradient from the North to the South of Great Britain. However, the *Annals of Eugenics* is not at present threatened, and we have had so far a good supply of first class material. . . .

I followed Shewhart's earlier work on Quality Control with great interest, but have not seen much of it later, and so do not know whether it has at all developed. It seemed to me that what he had to say he said very well. The best sense, I think, that can be made of Frisch's notions[1] was made by a Dutchman, Koopmans, in his book called *Linear Regression Analysis of Economic Time Series*, which might interest you if you have not seen it.

One of the central confusions in this subject seems to be that regression coefficients are thought of, or used, by economists in two discrepant senses: 1) as coefficients in abstract economic laws connecting a number of variables either exactly or with random errors superimposed, and 2) as the best coefficients to use for predicting one quantity not to be observed from others of which we have the values. The second which, I fancy, is the more frequent, in practical use, is straightforward regression work without modification. Even if we knew the values in the first type of problem, we should prefer to use those of the second type for its own purpose of making predictions. However, when the first type is wanted, or is under discussion, then there is no avoiding the disturbance caused by errors of measurement of the independent variates, which seem to be inevitably indeterminate in economics, and those not indeterminate, never actually determined in experimental physics.

[1] Wilson had asked if Fisher had made much of Ragnar Frisch's monograph on Confluence Analysis.

Fisher to E.C. Wood: 9 December 1947

Thanks for you letter.[1] For many years before the table was computed I had come to the conclusion that the only proper treatment of ranked data, looking at the matter from the point of view of estimation theory and on the assumption that the ranked series was based on a sample from a bivariate normal distribution, lay in using the mean deviate corresponding with each observed rank. The immense simplification in the scoring of ties was an accidental by-product. I had studied the integrals obtained and in outline the appropriate method of numerical computing, having calculated some of the simpler values.

It was the stimulus of getting out a definitive table collection with Yates which led me to get the table completed, partly with the help of Stevens at the Galton Laboratory. . . .

[1] Wood had asked about the origin of Table XX in *Statistical tables* for converting ranked data into normally distributed scores.

Fisher to A. Zeller: 2 February 1957

Thanks for your letter of 1st February. . . . With respect to your question on the analysis of variance,[1] I have from time to time over the last thirty years expressed the opinion, arising out of the earlier practice of agronomists, that to calculate a standard error for an individual variety, based on say three degrees of freedom, is less accurate, even for that variety, than to use thirty degrees of freedom found by pooling ten different varieties, even though these may not have accurately exactly the same variability. In such cases the judgement may be confident that the true variability between varieties is rather trifling compared with the sampling errors of their determination.

There are, of course, numerous cases in experimentation in which degrees of freedom for high order interactions may reasonably be judged on much the same grounds to be effectively pure error. I think it rather a dangerous tendency among some mathematical writers on the design of experiments to carry this assumption so far as to dispense with replication altogether, or to use so-called fractional replication, except in cases where the factors in question have been rather fully explored in previous experiments. In fact I always advise, personally, that it is well worth while in a design to have a number of degrees of freedom providing *nothing but confirmation* of the general ideas on which an experiment is planned. This, I think, is important in real research, but of course much less important in routine technological determinations, though even there, if I were directing such work, I should like to insert checks on the technological perfection of the processes actually employed. . . .

[1] Zeller asked Fisher to say to what extent one may pool with the error sum of squares other sums of squares (e.g. for blocks or treatments) having non-significant variance ratios.

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