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Scientists and Scientific Research

Fisher to J.R. Baker: 4 November 1940

Thanks for your letter on your new Group or Society on behalf of individual freedom in research.[1] I subscribe all too easily to your four propositions, too easily because I am sure they would receive adequate lip-service from the most dangerous opponents of your ideas. I want something more positive, and therefore more difficult. If a group could hammer out a set of consistent views, principles, or policy, answering the question, 'How should research be *organised*?', it would have far more practical influence. This, of course, would only be possible by frequent meetings over a period of a year or two.

The first answer, that research cannot, or should not, be organised at all, on the ground that all the outstanding workers in the past found the means to do their work, and, in fact, achieved their discoveries without organisation, is not a little unsatisfying to those who suppose

(a) that the great majority of whole-time research men now and in the future live on salaries from the State, Universities and Research Institutions, and have their expenses defrayed from the same sources;

(b) that the professional administration responsible for these funds will be largely ignorant of, and often indifferent or antagonistic to, the advance of knowledge.

(c) In most lines of research one relies intimately on past work of living contemporaries, and often consciously does a good deal with their problem specifically in view.

(d) Many workers hold strongly the belief that their work could be greatly aided by the technical co-operation of other specialists, e.g. in the production of new apparatus for their special requirements, and think, perhaps optimistically, that this need can be met by collaboration within large research institutions.

(e) Finally, I personally submit that when I have solved a problem to my own satisfaction, I have still made no contribution to Science until my ideas and methods have been grasped by at least some other minds, and that the contribution only really materialises when this number of minds approaches the hundreds.

It is a tragedy of the history of science that we have, I fancy, no good example of a great scientist using his penetration, knowledge, and prestige primarily to enable others to do more interesting, or more important, or more successful work.

All this implies that the word 'organisation' is not to be taken in the narrow sense of projects to be filed in triplicate with allocations approved by a series of remote pundits, but rather in the sense that we really do begin to appreciate the work of a great man when it throws new light on our own problems.

Organisation is merely a name for intention rather than inadvertence in relationships between human beings. The would-be research worker, inexperienced and fresh from a University course, has a number of quite definite needs, which anything worth calling organisation should attempt to supply. I think your group ought to survey these and discover how they can be met. Let me try to list a few here:

1. He needs experience, for his university training has certainly not introduced him to the actual difficulties of personal research, or to the attitude of mind of those who successfully make it their business. Contact with other research workers of various ages is valuable here.
2. Very frequently he needs moral support of the kind given when workers of standing think that his problems, or preliminary results, are of real importance. If he can be brought into contact as assistant with men whose work he already has reason to admire, his confidence may be very greatly strengthened.
3. Whatever the framework of his work, he must feel free to take up and give something like half his time to any problem which excites his interest, and which he feels he is fitted to grapple with. I think I have before compared the recognition by a man of his own problem with falling in love, as it seems to have almost as personal an appeal.
4. As University teaching is bound to have left him with great limitations, both on the methods and the subject-matter of research, it is often of great value to be constrained to face some problem of practical application, especially as this will confront him with a great deal of unscientific uncertainty, and with dealing with men interested only in practical results. The test is sometimes rather a tough one, but research gains immensely whenever correct principles or exact methods can be applied in new fields. Many of the best research men react most happily to a situation in which nothing can be supposed definitely known.

I think it is unfortunate, at least in mathematics, that the aristocratic prestige of the word *pure* should be applied often to ineffectual pottering of a kind which avoids the real difficulties which come in sight when applications are attempted. In your proposition 2 I should say the advancement of knowledge by scientific research is measured by the increase of power which it gives to other men to overcome their difficulties, theoretical or practical. I rather doubt if proposition 4 touches a practical issue. I know a good many scientists who do all their own work. Many of them do not want to have assistants, and would not know how to use them. They do not, in themselves, constitute any problem, except that it often seems disappointing that their earlier work of great apparent promise has not been more fully followed up.

[1] Baker's letter proposed the formation of what became known as the Society for Freedom in Science.

[The following Memorandum on Freedom in Scientific Research written by Fisher was sent to Baker in March 1941.]

Freedom in scientific research, like freedom in citizenship, is a relative term. The free man does not claim to live without prohibitions or restraints, but to be subject only to such restraints as are necessary to safeguard the just claims of others. A programme of anarchism, with the destruction of all forms of authority and organisation, is an emotional gesture, implying both impatience and laziness. It is an evasion of the problem, not an attempt to state or to solve it.

Who are the others to whom a professional research worker should pay regard, to the extent of curbing his own desires lest their just claims be infringed? First, I suppose, should be placed his obligation to civilization at large, to advance knowledge in his special field. This is, in particular, an obligation to those other scientific men having similar interests, who may be able to make use of his work. *Secondly*, one has obvious obligations to his employer, the State, a University, a Research Laboratory, or a Commercial Firm, whose salary he accepts upon a more or less clearly understood agreement as to the services which it is his duty to supply. *Thirdly*, the majority of workers have a scientific chief in whose assistance they are employed, and collateral workers with whom they may, and in most cases should, collaborate. To make claims for Liberty, without regard to such a framework of obligations, seems to be idle and unprofitable. To make claims for scientific freedom compatible and closely coherent with such a framework may well remove the grounds for legitimate grievances, and even be essential for maintaining in scientific work the intellectual integrity which it unquestionably requires. Minimum standards may be insisted on, with the whole weight of scientific opinion behind them, and used directly in aid of particular cases, provided they take account of the actual obligations of the scientific worker; whereas extravagant claims may win only a half-hearted verbal assent, since it manifestly appears that they cannot always be realized in practice.

I believe the most fruitful approach to the whole group of problems arising in this connection is to consider the requirements of the junior worker from the time he becomes engaged in scientific research, from the point of view of his personal progress and development. At this stage even those with the most brilliant academic careers will be, in certain important respects, definitely inexperienced. Confronted with differences in the apparatus, the material, and scale of the work around them, many of the less enterprising, if they had the option, and if they did not feel under an obligation to carry on the new work they had accepted, would certainly tend to gravitate to problems of a more academic nature, and more within the scope of their previous experience. I have frequently been impressed with the advantage that a worker has gained, especially in self-confidence and resourcefulness, by being confronted, *malgré lui*, with problems of the so-called applied or practical character, which in reality are problems requiring exploration and judgement rather than the application of a ready-made formula.

A second respect in which the new worker is in fact seriously handicapped by inexperience is in the practical conduct and design of experiments. In this case I believe much more could be done in post-graduate work at the Universities than is, in fact, done to prepare for the future scientific career. For the logical principles of experimental design and of reasoning from experimental results are of great interest to post-graduate students, who would appreciate definite courses in this subject. In fact, however, and at present, the majority of scientific workers enter their careers without this preparation, and learn as they go, by their own mistakes and those of their colleagues.

In these two respects it would be often far from helpful, and sometimes disastrous, if the scientific worker were to commence his career under the impression that it was his duty, thenceforth, to add to human knowledge by means of his own unaided and unguided efforts. Nevertheless, the conditions of his employment may be well or ill adapted towards fitting him step by step to exercise genuine independence of judgement; to develop the capacity for independent experimentation, and, finally, to plan a daring, original and fruitful research program. To give a basis for independence of judgement it is, I believe, of far more importance than is generally supposed that the worker should allot a considerable fraction of his working time to making himself acquainted with the published literature. This is, I suppose, in all subjects very large in volume, very diverse in scientific cogency, and varied in the ideas propounded. The student's reading may have been well directed, but it has covered almost certainly only a very small fraction of the published researches bearing on his problems. The literature is often not only very extensive, but intrinsically difficult, and requires time and deliberation, if it is to be properly assimilated. The junior worker should receive encouragement, and his duties should allow him to read, with adequate care, far beyond the limited series of papers which his chief may indicate to him as necessary for the understanding of the work of his Department. The object should be to familiarise the reader

with the stages whereby current opinions have been developed, and to train him, by scrutinising the results of past experimentation, to exercise his own judgement on the value of the experimental evidence available on different disputable points. There would be more confidence that real independence of judgement was expected from all, if it were true in all laboratories, as it certainly is in some, that many assistants are better acquainted with the current literature than is the Head of the Department. Since the study of the literature is time-consuming, and many junior workers feel that their other duties have a prior claim on their time, I am inclined to lay stress on the stipulation that extensive study of the literature, including participation in scientific discussion, should be a definite part of the duties of scientific assistants, recognised as such in the terms of their appointments.

A second share of time must be deducted from that earmarked for departmental duties for independent research, initiated spontaneously by scientific assistants, within the field for which the department is equipped. That a reasonable fraction of the time available should be made free for this purpose, not as a special favour, but as a normal right attaching to research appointments is a claim which, I believe, should be made universally. We must recognise that this right will often be used unwisely, and that the investigations proposed will often be ill-considered. I do not believe these facts should be thought to justify interference. Anyone employed in scientific research has a primary right to make his own reputation, at least among his immediate colleagues, and to disregard criticism at his own risk. In respect of publication, there will, I believe, be least embarrassment if the Head of the Department is not expected, *ex officio*, either to approve or to facilitate publication by his assistants. This suggestion, I know, differs considerably from the traditions and practice of many departments, but latent grievances in respect to publication are unfortunately rather common, and if we aim, as I think we ought to do, at the frankest cooperation of independent minds within our research departments, I believe it affords the only satisfactory basis.

A word might be said here on a not uncommon situation, sometimes felt to be oppressive, in which the Head of a Department is also the Editor of an important journal and may seem, to junior workers, thereby to control the only opening for effective publication. As the result of personal experience, I believe this fear to be so much exaggerated as to be in fact illusory. Other journals which at first sight may seem less suitable channels, may, in fact, bring the work published before a more valuable audience. In my own experience, for many years the leading English Journal in Mathematical Statistics was closed to my papers, through the disapproval felt for them by the Editor at the time. In consequence, my work was published in about 30 more or less appropriate journals, and thereby came under the notice of a far wider public. Being published in relation to its applications, moreover, it came before readers whose needs it met, and who wanted it for use; a much more important class than the self-conscious theoretical experts whose reaction is merely to criticise, or to ignore anything subversive in their own field. A paper may be more quickly appreciated, appearing in one journal rather than in another, but I do not think it ever happens that the obscurity of the journal is a reason for detracting from the importance which is attached to a paper.

To sum up:— The conditions of scientific research vary enormously, according to the employer and the size of the Department in which they are carried out. We may greatly aid the personal development of a young research worker by associating him in a Department with colleagues of varying experience, and by setting him problems in which some judgement and exploration is necessary. Two safeguards are suggested, both of which, unfortunately, detract from the amount of time he will be able to give to departmental work.

(I) That it shall be a definite part of his duties to read widely in the relevant literature, and to form a critical judgement of the value of the experimental work published.

(II) That he shall be free, in respect of a reasonable fraction of his time, to undertake independent personal research on his own responsibility.

Fisher to H. Corbière: 28 *May* 1947[1]

I have earned my own living most of my life by employment as a research worker or University teacher.

As a young man I did not find it really difficult to get scientific papers published, although anything of value, that is anything leading to ideas unfamiliar to the current authorities, was liable to rejection by one journal, though publishable in others.

I found my work rather quickly appreciated in the United States, where interest in statistical methods and genetics is more widespread, and there are probably more people on the alert to notice work of potential value. This was particularly so with the first edition (1925) of my *Statistical Methods for Research Workers*, which was unfavourably received (so far as reviews are concerned) in this country, but about which I had at once several most encouraging and appreciative letters from the United States, where it evidently was felt to meet a need.

Work that has reached what is nearing completion has, of course, lost its importance. Probably the most useful aspect of my early work was to develop accurate tests of significance and methods of statistical analysis clarifying and largely replacing the deeply involved entanglement in which the subject had been left by the biometrical school of Karl Pearson. I have found people also much interested in what can be done to improve technically the design of experiments and the logic of the inductive process by which experimental data

are interpreted. I have always been interested in Genetics, especially in relation to evolutionary theory.

[1] Corbière had sent Fisher a number of questions about his life as a scientist.

Fisher to H. Corbière: 2 *June* 1951[1]

I find it difficult to give an opinion on the excerpts from Jules Romains' article which seems to require some sort of semantic analysis before their meaning is quite clear. The first sentence seems to be true, merely by definition of 'major importance', for an historian of Science will pick out a few advances for emphasis merely because he judges their importance to be greater than that of numerous others, without, however, having any absolute scale on which importance may be judged.

If as a result of nuclear fission interplanetary travel became possible, it would be, I suppose, a prodigious and at the same time not confidently forseeable development, but scarcely a second rate result.

In the two fields with which I am most familiar, namely Statistics and Genetics, the scene has been so transformed during the present century that it is difficult to appreciate earlier work owing to the relatively childish nature of their language and concepts. An immense field has been opened out in each case for detailed exploration and this work might be judged by a literary man to be second rate although perhaps intellectually and economically fruitful, and in any case apt in the sense of being appropriate to our present state of intellectual advancement.

When science is, as it is tending to become, highly organised through large scale government agencies, every major advance may be regarded as an immediate set-back, since it is liable to disintegrate the basis of the existing organisation.

[1] Corbière had sought Fisher's opinion on the following excerpts which he said were taken from an article entitled, 'An epoch in science' by Jules Romains in the magazine *Science et Vie*.
'The exploration of the real knows no "pre-determined bounds"; it seems, however, that of all discoveries, those of major importance are of the same order numerically as the important discoveries of stars — "very few." The future should fall back "on prodigious and, at the present time, unforseeable developments of second-rate results and the application thereof."
Pure science has already probed into the inmost secrets of the Universe.
"Our time has little chance of being outclassed." '

Fisher to H. Corbière: 24(?) *March* 1957[1]

Morality is derived from the social tradition, including the religious tradition, interpreted in the light of experience. Such understanding of the real world as can be supplied by science, including biological science, is therefore quite relevant in the development of a morality.

[1] Corbière had asked Fisher if he thought one could draw a moral from Science and in particular from Biology.

Fisher to J.O. Irwin: 25 *June* 1934

I think you are Chairman of the study group which is responsible for getting Wishart to compile the Bibliography of Agricultural Statistics, published in the first number of the Society's supplement.[1]

I think you will agree with me that a bibliography ought to be rather carefully accurate, and that its value is seriously impaired if personal bias is admitted. The latter seems evident in entries 49[2] and 155[3] for Wishart does not, I understand, deny that his paper in the *Arch. f. Pflanz.* showed a complete misunderstanding of the limitations of the layout adopted and by ignoring the non-orthogonality of some of the interactions arrived at an estimate of error which was so much too low as to lead him to report a number of interactions as significant without any justification.

If the paper is still to be recommended to students as describing principles and arithmetical details of the modern layouts, the least a bibliographer should do is to point out that these principles and details have been later disputed.

Yates' paper (No. 155) reexamined this experiment in detail and showed exactly where Wishart had gone wrong. This was the main point of his paper, but Wishart's summary says nothing about it.

As regards general carelessness in descriptions, the next paper to Wishart's (No. 50)[4] affords an example of what I mean, for 'Alumnus', following Fisher is said to use multiple correlation, when in reality the method is a direct use of non-linear regressions, but there are a great number of examples of this kind of thing.

I think the idea of a descriptive or annotated bibliography is an excellent one and perhaps I might suggest to your committee the propriety of one or two safeguards which might avoid its abuse.

The bibliographer might submit his draft to a meeting of the committee in his absence, with a view to their selecting a number of papers regarded as controversial, the comments on this particular class of papers might: a) be left to the authors and have their initials appended or b) be drafted by the committee or c) by one or more experts in the subject nominated by the committee.[5]

[1] Wishart, J. (1934). Bibliography of agricultural statistics, 1931–33. *J.R. Statist. Soc., Suppl.* 1, 94–106.

[2] '49. Wishart, J. The analysis of variance illustrated in its application to a complex agricultural experiment in sugar beet. *Arch. f. Pflanz.*, 1931, *5*, 561–584.

A particular experiment with a complex lay-out is used for describing the principles and arithmetical details of the modern lay-outs.'

[3] '155. Yates, F. The principles of orthogonality and confounding in replicated experiments. *J. Agric. Sci.*, 1933, *23*, 108–145.

A theoretical study of the lay-out of the modern field experiment. Orthogonality is shown to be at the basis of the simple accepted methods, whereas more complex lay-outs may be deliberately non-orthogonal to secure additional information within the limits of size imposed.'

[4] '50. "Alumnus". A comparison of the effect of rainfall on spring- and autumn-dressed wheat at Rothamsted Experimental Station, Harpenden. *J. Agric. Sci.*, 1932, *22*, 101–114.

The method used following Fisher is that of multiple correlation of yield on constants representing the distribution of rainfall.'

[5] Wishart wrote to Fisher after receiving a copy of this letter, saying that Paper 49 was a description of an experiment that Fisher had designed and that the paper had been read and approved by Fisher before publication. For Fisher's reply to Wishart, see p. 355.

H. Jeffreys to Fisher: 23 *November* 1938

I expect you are snowed under with letters of congratulations,[1] but cannot restrain myself from writing another. You have done a great work in bringing sanity into biology, and I regard it as a tribute to biologists that they have recognized it.

I am having a scrap with the C.P.S.[2] editorial committee. On the advice of their referees they have cut out about half of a paper of mine, including the part that concerned Stevens's problem, and are taking the line that it is only as a personal favour that they can contemplate publishing any of it, on the ground that their referees object to the fundamental principles. I take the line that where there are different possible modes of treatment the only fair thing is to publish both, but as far as possible, usually by suggesting personal discussion, to avoid waste of space by emphasizing apparent differences that come from sheer misunderstanding. . . .

[1] Fisher had been awarded the Royal Medal of the Royal Society.

[2] Cambridge Philosophical Society.

Fisher to H. Jeffreys: 24 *November* 1938

I think it is time something were done with respect to the Secretary and Editorial Committee of the C.P.S. Two and a half years ago I was forced to resign a long-standing Fellowship of the Society[1] through the attitude of one, A.H. Wilson, whom I do not know apart from this correspondence.

The situation was that M.S. Bartlett thought he had detected an error in a paper of mine dealing with a test of significance originally put forward by W.U. Behrens. Bartlett's paper[2] purporting to expose this error was published by the Society, without notification by him or them to me as the author criticised; they thereby took the ordinary risk of receiving a reply. The Referee objected to my reply on the ground that I had chosen a degenerate case, although this case was not chosen by me, but by Bartlett, and reiterated a number of facts which I had demonstrated and emphasised in my note, as though I had overlooked them and as though they were opposed to my point of view. Nevertheless, Mr. Wilson and his Committee decided to refuse publication of my answer in which I gave my grounds for dissenting from Bartlett's conclusions. This course was, I think, particularly objectionable as I had been myself responsible for first putting forward the fiducial argument, and had, therefore, to continue to shoulder the responsibility of preventing its being misunderstood, as I think Bartlett had done.

The excuse was put forward that I had misrepresented Bartlett, but when Wilson was challenged to cite the particular passage which was thought to be a misrepresentation, with a view to my modifying it if it really seemed to be such, he failed to produce any case.

I do not know who is responsible for the management of the Philosophical Society, but think it must contain a sufficient number of reasonable men to conclude that this is not a reasonable or possible way to treat Fellows of the Society. I am enclosing the correspondence, in case it may be of service in getting the Society to conduct its business in a more tolerant way.

Many thanks for your kind congratulations.

[1] Fisher's explanation of his resignation shows the situation was not as simple as is perhaps suggested by Bartlett's statement that he had heard 'from Wishart that Fisher resigned from the Cambridge Philosophical Society when they published my paper'. (in *The making of statisticians*, ed. J. Gani, Springer, New York (1982)).
[2] See Bartlett's letter of 16 April 1937 (p. 51).

H. Jeffreys to Fisher: 27 November 1938

There are some reasonable people on the Council; I have passed your literature on to one of them, who was very outspoken. It does seem a bit thick that they should have published Bartlett's paper without consulting you and then made it a condition of accepting your paper that you should get his permission. . . .

I had heard vaguely that you had left the Society, but had no notion of what it was about, and rather thought it might have been over my 1936 significance tests paper — in which case I should have agreed with about 3/4 of what you said. The Editorial Committee consists of Hardy, Hodge and Hall (pure mathematicians), Wilson (quantist, knows about metals), Dee and Ratcliffe (Cavendish physicists, young and enthusiastic). Except Hardy I don't think any of them is over 35; the pure mathematicians all have experience of the L.M.S.[1] council, but I should doubt whether the others have had experience on any other society. The trouble about pure mathematicians is that their decisions can be so clear cut in their own job; a result follows from the postulates or it doesn't, it is new or it isn't, and it doesn't matter what the postulates are. A paper can be short and to the point, in which case it is trivial, or long and (to a Philistine like me) not worth doing anyhow, and then it is important. It's a bad training for statistics. A mathematical logician would be more to the point; they don't believe in universal agreement. Frank Ramsey called some of *Principia Mathematica* 'sloppy' in the L.M.S. and Russell was the referee. His report was 'quite right, it is'. . . .

[1] London Mathematical Society.

Fisher to H. Jeffreys: 28 November 1938

. . . Many thanks for your information about the Cambridge Philosophical Society. I will want to have my correspondence back when you have done with it; but I think it is necessary for you and your friends to have it all in order to see just how the Society's business has been conducted.

H. Jeffreys to Fisher: 1 February 1939

Here are your papers (by the way you didn't sign your letter but authorship was assignable by internal evidence). Appleton has had them and has been taking a keen interest in the matter, but he is leaving in a few weeks to become secretary of the D.S.I.R. and I don't expect that he will have time to do anything. I suggested a set of possible Council rulings that would meet both our problems, but I don't know whether anybody else can be found that would be both willing to take them up and have enough push to get them through. I am, meanwhile, making such efforts as a private member can.

Fisher to H. Jeffreys: 3 February 1939

Thanks for returning the correspondence. For the rest, more power to your elbow!

H. Jeffreys to Fisher: 13 February 1939

I wrote to Hodge as Editor of the C.P.S. *Proceedings*, suggesting that the Editorial Committee might climb down a bit over your row with them. The suggestion that you might rejoin if they did was my idea, as I told them. My motive is the selfish one that I like to have my mistakes pointed out before publication, and so far in the C.P.S. I have been able to get away with anything if it was wrong. I enclose Hodge's reply, which is as cordial as one could wish, though I should welcome something a bit more specific and am answering with a couple of suggestions that they might adopt. One of them has, I have been unofficially told, been adopted already, but I want to make it official. . . .

Fisher to H. Jeffreys: 15 February 1939

Thanks for sending me Hodge's letter, which I return herewith. It is good of you to suggest the possibility of my rejoining the Society, but I am glad you made it clear that this was your own suggestion, as I only mentioned the affair to you in corroboration of your own impression that the affairs of the Society were being pretty badly mismanaged.

So far as I am concerned, I resigned for an entirely specific reason, which I believe I made clear to the Secretary, namely, that the right of answering a misunderstanding and, as I think, a misrepresentation, of my own work in the Society's *Proceedings* was peremptorily denied. I suppose I ought, at the time, to have taken the matter up with the President, whoever he may be, but in the circumstances I was content to leave the matter as it was.

I might, indeed, sometime, like to rejoin the Society, especially if I ever came to Cambridge, but I should really hesitate to do so if the Secretary at the time thought the right way to treat Fellows whose work had been criticised in the *Proceedings* was the way in which I had been treated. . . .

Fisher to H. Jeffreys: 20 *February* 1939

Since writing to you last I find that Yates has been thinking about the problem of samples with unequal variances and has written a short paper about it. As he was one of the people who thought at first there was something in Bartlett's objection, it would afford a good opportunity for the Cambridge Phil. Soc. to put themselves right, if they cared to publish his paper. I think he would be willing, if that was their intention, to send it in for the *Proceedings*.

However, the prevailing attitude two years ago was so inexplicable that it may be that your efforts will reveal difficulties of a kind I have not suspected.

H. Jeffreys to Fisher: 16 *April* 1940

. . . By the way, now that Yates's paper[1] is out, are you ready to join the Camb. Phil. Soc. again?

[1] Yates, F. (1939). An apparent inconsistency arising from tests of significance based on fiducial distributions of unknown parameters. *Proc. Camb. Phil. Soc.* **35**, 579–91.

Fisher to H. Jeffreys: 18 *April* 1940

Thanks for your letter. Yes, I should be happy if you cared to put me up for the Camb. Phil. Soc., as, with how much reluctance I know not, they have now done what is reasonable towards correcting their previous mistaken attitude.

L.F. Richardson to Fisher: 30 *July* 1937

Thanks for two papers from the *Annals of Eugenics*. I have read the one about 'Karl Pearson and the Method of Moments' [*CP* 149] with considerable interest and a tinge of regret. With interest, because I look to you as the authority on how these things should be done. The agenda on pp. 316–317 interested me particularly. It is true that I seldom do statistics, but one never knows when one may have to. With a tinge of regret because it seems to me unnecessary to accuse K.P. of having deliberately falsified his figures. There might easily be other explanations. Anything like deception was entirely contrary to his character as I remember it. Fiercely jealous he was certainly, and I can well believe, without knowing the details, that he treated you abominably. That was in a way a compliment; for it meant that he regarded you as a formidable rival. But I think he had a relentless love of truth. Some months ago I read the obituary notice of K.P. which Udny Yule wrote for the Royal Society. I read it with delight! For Yule seemed to me to have drawn K.P.'s portrait according to the motto 'nothing extenuate nor aught set down in malice'.

Fisher to L.F. Richardson: 4 *August* 1937

Thanks for your letter of July 30. I shared your regret very heartily when I decided what I had to do about Karl Pearson's attack on Koshal. It is appalling that the very last paper that he wrote should have shown such vindictiveness, and should have required such criticism. I enjoyed Yule's obituary, as you did, and thought it most fair and well-balanced. The question whether Pearson's reputation will stand so high with a later generation as it did with Yule's is, I think, beside the point in an obituary, which should aim at being a contemporary appreciation.

As for accusing him of having deliberately falsified his figures, this I was obliged to do, as soon as I understood what he had really done. That he deliberately falsified them is a statement I base only on his own assertion. He adds the explanation that it was done in order to make his results more comparable with Koshal's, but he does not compare them with Koshal's, but with other material with which it makes his figures less comparable. Let who will accept his explanation. I, for one, cannot.

As for aspersions on the honesty of scientific colleagues in general, I do not think you can feel more strongly than I do that they are undesirable, and hitherto I have consistently avoided them. My reason for making an exception is that they seem to be less undesirable than the results of successful dishonesty. My immediate motive, of course, was to discredit Pearson's despicable attack on a comparatively helpless victim, that is on Koshal's competence, as a statistician, an attack which had already been used to threaten Koshal's scientific prospects. If such methods are allowed ever to be successful, it is certain that there are other vindictive persons who will not scruple to use them. The fact that the example was set by a name widely honoured made it particularly important to show that honest work can, with goodwill, always be defended against dishonest attack.

If you travel much, or hear much of the squabbles and disputes among foreigners, you will have realized, with much the same dismay as I have done, that what we, in this country, accept as ordinary standards of honour are not universally so accepted abroad.

Peoples are now engaged in scientific research without any long native tradition as to propriety in its conduct. In the eyes of any careful reader of Pearson's paper, once I had decided to answer it, I had to stand either as exposing a fraud, or as conniving at it.

N.M.V. Rothschild to Fisher: 6 *February* 1952

I mentioned to you that I had been thinking about the status of biometricians and statisticians in scientific publications. This problem affects the Agricultural Research Council when questions involving the grading or promotion of biometricians is under discussion. If a statistician is recommended for promotion or employment in a particular grade, it is inevitable that the A.R.C. should wish to scrutinize his publications. I don't see how this can be avoided in present circumstances, even though we may attach considerable importance to the views of Directors and referees.

Several cases have come to my notice where the candidate has published very little though his Director said he was first class and that, though he had not published much, he had often been acknowledged in other people's papers.

It seems to me that the questions at issue are:

1. What are the conditions in which a biometrician should be a co-author of a paper?
2. What are the conditions in which he should be acknowledged at the end of the paper?
3. What are the conditions in which the biometrician's work should be in an Appendix at the end of the paper?

I think one can examine these questions from at least two points of view. First, did the biometrician's contribution involve original statistical research? Secondly, did his contribution involve him in a lot of work?

In the field of agricultural research it quite often happens that the biologist does some experiments but the results are quite incomprehensible without statistical treatment; the biologist may not be able to do the necessary computation even with the advice of a statistician. In such a case the statistician, though not making much contribution from the pure statistical point of view, is involved in a lot of work. Should he be a co-author of the paper? In other cases the biologist may be able to do the computation provided he is given detailed instructions by the statistician. Should the statistician be acknowledged at the end of the paper on these occasions?

Would it be possible to make a brief classification on the above lines of the different types of contributions that statisticians can make to experiments; and to lay down general principles as to how these contributions should be acknowledged? There will always be borderline cases, I imagine.

I wonder whether you would consider submitting this letter to any of the biometrical societies that you may think fit, and let me know the results of their deliberations. I have in mind that the Agricultural Research Council might inform Directors of Institutes and other persons concerned of any 'findings' that may eventuate, and ask them to comply with these so far as is possible.

Fisher to N.M.V. Rothschild: 7 *February* 1952

Thanks for your letter of February 6th on the status of biometricians in joint work. I think it sets the ball rolling and I have sent it to Yates, now President of the Biometric Society, with an explanatory letter.[1]

[1] See p. 356.

Fisher to P.M. Sheppard: 11 *November* 1949

Thanks for your letter. I do not really see why my name should appear in this matter at all.[1] It has always been a pleasure to me to carry out any statistically detailed work which may suggest itself. If the author values this he can say so in about five gracious words, and, if he fears that his readers will not understand it, he can often add an appendix in the name of his associate. Joint papers are to be avoided for doctorate purposes, simply because university boards have such incompetent regulations with respect to them. I recently had a series of papers for a higher degree in which the author, the only interested and therefore least reliable party to the transaction, had provided a statement representing the importance of his own and the unimportance of his collaborators' contribution to each paper. As I knew some of these statements to be untrue I refused to act as referee. I do not think it is a necessity, but I do think it a fact, that university boards generally must be expected to be about as incompetent as this. So put your views before Ford, and whatever you agree to will be agreeable to me. . . .

[1] Sheppard had suggested that Fisher's name should be included with his on a paper reporting work on *Panaxia dominula*.

Fisher to W.H. Thorpe: 12 *May* 1953

Thank you for your letter and enclosure.[1] I do not know what to say about the proposed organisation, for anything like an agitation group, however well started, is almost bound to be parasitised by political agitators and to do more harm than good, as indeed I think is true of the many organisations aimed at preserving peace during the last half century. Most of us have paid very heavily for their folly and fanaticism. Yet surely one would want to help the aims you had in view.

[1] Thorpe had asked Fisher if he would be a signatory to an open letter to Cambridge scientists calling for the formation of 'a local organisation for social responsibility in science'.

Fisher to J. Wishart: 17 *September* 1934[1]

I think you know I have always held and often expressed the opinion that whenever a mistake has been made in scientific work, it is best for the author of it to be the first to point it out. If he cannot be first he usually has the opportunity of being a good second. My main criticism of your bibliography is that you recommend in it, as an example of method, a paper in which the statistical methods and the numerical conclusions had been shown to be seriously in error.

This error arose from your attempt to use the experiment to study certain possible interactions which the experiment was designed to ignore and which had previously been ignored in previous experiments. It was obvious that it must be theoretically possible to use the data to examine these interactions and when you said that you had done so, and found that many of them were significant, I certainly supposed that you had done the work right and did not imagine that the supposed significance was due to your having greatly under-estimated the residual error.

To Yates belongs the credit of having read your paper with sufficient care to see where you went wrong in particular and principle and to show how it should have been carried out. If your mistake was a natural one in the current state of statistical knowledge then you should have given Yates the credit for having made a material advance in the subject, in addition to having ascertained the correct conclusion to be drawn from this particular experiment.

I think if you will re-read the two entries referring to your own paper and to that of Yates you will see why they should have made an unfavourable impression upon all who were aware of the circumstances.

[1] For background to this letter, see Fisher's letter of 25 June 1934 to Irwin (p.348). See also Fisher's letter of 27 December 1933 to Wilks (p.299).

Fisher to F. Yates: 7 February 1952

. . . Rothschild has been consulting me on the ethics and proprieties of co-operation between biometricians and others in scientific work and is concerned that the biometricians do not receive such formal and explicit acknowledgment as should weigh with the A.R.C. in assigning grades, etc., or with universities for higher degrees.

Evidently, what would suit him best would be for the Biometric Society to draw up formal recommendations which should make clear to directors of research stations, and not least, to the biometricians themselves, exactly what forms of recognition or acknowledgment their colleagues think appropriate to different cases.

Of course, the cases are quite diverse, and it is not to be assumed that participators in joint work always form a just opinion as to the originality or scientific worth of their several contributions. In fact, I think we must proceed partly by options, e.g. if A does a job of statistics which meets B's requirements, but which B had supposed (rightly) to be of a comparatively routine nature within the known techniques of the statistician's craft, while A imagines (mistakenly) that he has done something exceptionally bright, it might possibly be held that none the less, A should have the option of adding an appendix explaining or demonstrating his method under his own name.

I shall be very much interested to hear how you react to all this and how you think the Biometric Society should take it up.

F. Yates to Fisher: 8 February 1952

. . . Rothschild did mention the question of giving due credit to statisticians when I saw him last.

To dispose of the least important category first, I have always found that acknowledgements at the end of a paper mean very little. Firstly, one cannot stop authors acknowledging one's help, when in fact all they have done is to have a conversation and then go away and ignore everything that has been said. Secondly, different authors seem to have very different standards as to what merits acknowledgment. If, therefore, acknowlegments are really to mean anything we shall have to lay down some form of wording which will differentiate them from the casual acknowledgments which are thrown around by some authors.

More important, I think, are the other two categories. My own opinion is that if the statistician has contributed to the interpretation of the results as a whole and is prepared to make himself jointly responsible for the whole substance of the paper, then he should be a co-author. If he has contributed by developing new statistical techniques but is either unwilling or unable to associate himself with the work as a whole then these statistical techniques should be made the subject of an appendix or a separate paper, depending on their importance and the space required for their presentation. If an appendix is added to a paper then the responsibility for this appendix rests squarely on the shoulders of the writer and not the author of the original paper.

Editors of journals do, of course, adopt some control over such appendices, but this control is at present not very critical, and I think it might be suggested to them that they should ask the question: 'Is your appendix really necessary?'

These, I am afraid, are my immediate and unconsidered reactions; I certainly think it would be a good plan to have the whole subject ventilated by the Biometric Society. I do find that at present I have to tell some people who ought to know better that it would be appropriate to include the name of one of my youngsters as a joint author in their work.

The problem is, of course, not entirely one-sided. We have a particular headache here in the reporting of survey work and co-operative field experiments in cases where the field work is done by a number of members of the National Agricultural Advisory Service, but in which most of the planning and the critical analysis and consideration of the results is carried out by our people. The difficulty here is that there are too many names of people involved in the N.A.A.S. for them to be placed at the head of the paper, but it does, quite rightly, I think, create bad feeling if the only names are those of the Rothamsted staff. It has been suggested that such papers should have no name at the head, but should state the names of the associated workers in the introduction. This, however, creates great difficulties in indexing. I have suggested that the N.A.A.S. should pick one or two of their people who are particularly interested in the work to be co-authors and make themselves jointly responsible with the people here. This, however, is liable to slow things down as it is difficult to get their agreement in any reasonable time. The alternative would be to put the N.A.A.S. names in the form of 'A.B. Smith *et al.*, National Agricultural Advisory Service' with a footnote giving the names of the others.

All these matters I think require thorough ventilation; there is considerable ill-feeling engendered from time to time and it is important that young scientists should get a reasonable number of papers to their name, as, rightly or wrongly, their prospects of promotion are greatly influenced by this. Actually, I am one of those who believe that a man cannot be a good, or at least a useful, scientist unless he publishes his work. Unfortunately, however, the selection boards tend to read the papers in title only and frequently do not give sufficient consideration to their real merit.

Fisher to F. Yates: 15 February 1952

On the enclosed two sheets I have attempted classifications, first of types of publication commonly used in joint work and secondly of types of cooperation.

I suppose any recommendations which we should like the A.R.C. to make to editors or to research directors should be in the form that certain classes of one category are appropriate to certain classes of the other. Anyway, this seems to be the kind of classification which may help us to clear our heads if we are to give useful advice. . . .

[Sheet 1]

The various types of outcome of participating in scientific work which is later to be published can be classified as follows:–

a) Joint authorship with indistinguishable responsibility.

b) Joint authorship in which the authors are prepared to make a joint statement, dividing their responsibility either in respect of the techniques employed, e.g. a physicist looks after the electron microscope, or specifically by sections or paragraphs of the paper. In this case (b), editors should ask the authors to make their statement at the time of publication for the

benefit of committees awarding higher degrees, increase of pay, changes of status, etc., who need to apportion credit.

c) A technical appendix for which one writer or group of writers is specifically responsible, and for which the other author or authors have no responsibility, even though it is published with and distributed in separates with the main paper.

d) The author or authors of the paper acknowledge specific aid from another. E.g. 'we are indebted to Professor Einstein for developing the relativity corrections of our formulation'.

e) Acknowledgment of a general character is usually given to the Head of Department or Professor for his kindness, encouragement, advice of an unspecific nature.

[Sheet 2]

Classification of types of co-operation.

1. B has been associated with A throughout the research period, perhaps doing work of a different kind, yet influencing the course of the research during its progress.
2. B, as a scientific employee in the same institution, has done certain work, e.g. chemical analyses required in A's research.
3. B has been consulted on A's initiative on a specific point which he has examined at his request, and the result of which is incorporated in the publication.

The intrinsic importance of the work done by B in these situations may vary very greatly, and its intrinsic importance may not be clearly and justly apprehended by the persons concerned.

F. Yates to Fisher: April 1952

I am enclosing a redraft of the authorship and acknowledgments paper. As you will see I am suggesting that we put this out under our joint names (an example of co-authorship!). When we have all approved of the text I propose, as indicated, to seek the approval of the other members of the Regional Committee.[1] This form of presentation will enable the document to be dealt with without further formality, but if you feel that some alternative form of presentation would be preferable please let me know. When we have approval of all the biometricians I propose to send it to Lord Rothschild to see if he agrees to its circulation and possible publication in its present form. I think we might then see if *Nature* would be willing to publish it as a matter of general scientific interest.

The chief modification in the revised draft is the inclusion of specimen forms of acknowledgment which you felt were desirable. It might be worth including a suggested form of a note for 'Instructions to contributors'. If you feel this is worth while perhaps you might like to try your hand at drafting one.

[1] The British Regional Committee of the Biometric Society.

Fisher to F. Yates: 28 *April* 1952

I am returning your suggested draft with no modification save on the first page a reference to University degrees, where shared responsibility in joint publications is often a matter of great difficulty.

I suppose any instruction to contributors by editors must be exceedingly brief and might be no than 'In cases of joint authorship, authors should, so far as is possible, specify their respective contributions if individual credit is to be reserved in regard to the conferment of Doctorates, etc.. Acknowledgments also should be, so far as possible, specific.'

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