1
Statistical Inference

A.C. Aitken to Fisher: 22 *January* 1936

Very many thanks for the offprints, which are safely to hand. I shall return the ones you mention. I had of course read your alternation of papers with Dr. H. Jeffreys in the *Proc. R.S.*[1] . . .

The trimming up of the mathematics of probability, to which you allude, is of secondary importance compared with a correct grasp of the basis, and a recognition of what the variables are when encountered in practice. Several interesting books in the last three years have appeared, founding probability very prettily on the theory of measure and integration of sets of points; but refraining from mentioning what these sets are in such cases as the tossing of an inhomogeneous, irregular and biassed die, etc. No subject is so perennially interesting, or uselessly controversial.

[1] See *CP* 109 and Jeffreys, H. (1934). Probability and scientific method. *Proc. R. Soc.* **A 146**, 9–16.

Fisher to A.C. Aitken: 23 *January* 1936

I am very glad to have your letter, and agree with you entirely as to the position in mathematical probability. In some form or other the subject must sooner or later be introduced, or re-introduced, into the teaching of mathematics in Universities, and, in this respect, the question of the nomenclature and affiliation of the subject really deserves some consideration. In his great book it was clearly Laplace's intention to enlarge the meaning of the term 'theory of probability' so as to include a wide range of mathematical studies, equations of finite differences, for example, which are cognate but do not involve uncertain inference or its mathematical specification in any form. The same tendency extended to the present day would make the subject include all the various topics vaguely associated with statistics. It is now clear, however, and it was, I think, clear to Gauss, that this was bound to happen, that the old theory of probability would come to mean a group of rather academic topics, studied as a preliminary to statistics. If it had not any other association, therefore, the word 'statistics' might be used for the whole subject which is now opening up, and I used to think that this would be the appropriate modern position, but I now rather doubt it, and would be glad to know your opinion on the way in which these studies may best be furthered. To many, 'statistics' means Government publications, and in Universities it is often absurdly confused with economics. I was greatly attracted by Whittaker's term, 'the calculus of observations', which, if it were shorter, would cover the ground admirably. I suppose the term 'theory of errors' could properly be extended to cover the distributions of statistical estimates, and might have been used for the whole theory of estimation, had I not been concerned with the temporary necessity of making my own approach quite explicitly clear, and to avoid the assumption that I was accepting previous formulations of analogous problems. Even here, though, confusion has been introduced through Pearson and Neyman using the term 'theory of estimation' for the views they have developed on tests of significance, without reference to the results of the original theory.

From my point of view the important point is that the original concept of probability is not adequate to specify the nature of the uncertainty inherent in many forms of inference from observations. From this point of view it is almost unfortunate that a group of cases has been found in which inductive inference may properly be expressed in terms of probability, using the fiducial mode of argument; for this has tempted some mathematicians, and will, I fear, tempt more, to imagine that this type of argument is more widely applicable than is really the case, and to avoid enlarging their imaginations sufficiently to grasp the cases where no probability statement is adequate. This is, in my view, a decisive reason against enlarging the meaning of the theory of probability so as to cover all types of inductive inference, since the word 'probability' must be tied closely to one quite defined mathematical concept.

Pray excuse this long dissertation.

G.A. Barnard to Fisher: 14 *October* 1945

I am enclosing a reprint of my original letter to *Nature* on 2 × 2 tables,[1] together with a copy of a reply[2] to your remarks [*CP* 205] which I have sent in.

As one whose training prior to the war was exclusively mathematical, and who is only too conscious of the fact that his practical experience of experimental work is virtually restricted to development work during the war, I should like to say that there is no one with whom I should less like to disagree than yourself. I am therefore hoping that the apparent disagreement that now exists is principally due to the excessive brevity of my original letter to *Nature*.

May I add a few more points of explanation which I have omitted from the letter to *Nature* for the sake of brevity?

First, I think the first two paragraphs of my first letter are somewhat misleading, in so far as I did not draw a clear distinction between the test of association and the test of homogeneity. Your test is primarily, (though not exclusively) to my mind, a test of association—analogous to a test of

correlation—while my test is a test of homogeneity, analogous to a test of equality of means. If X and Y are two random variables representing the events in question (and taking only the values 0 or 1), as I see it the test of association is a test of the correlation coefficient

$$\rho_{XY} = \{M(XY) - M(X)M(Y)\}/(\sigma_X\sigma_Y) = 0? \tag{1}$$

while mine is simply a test of equality of means, $M(X) = M(Y)$? (2)

Ordinarily there would be no confusion between these two questions, but when X and Y represent events (and not measured quantities) and so take the values 0 and 1 only, we have $X^2 = X$ and $Y^2 = Y$, and then *provided neither* σ_X *nor* σ_Y *is zero*, we can transform (1) to make it look like (2).

Because, if neither variance vanishes, (1) reduces to the question, does

$$M(XY) = M(X)M(Y)?$$

which may be put

$$M(XY) - M(X)M(XY) = M(X)M(Y) - M(X)M(XY),$$

i.e.

$$M(1 - X)M(XY) = M(X)M(Y - XY),$$

or

$$M(XY)/M(X) = M\{(1 - X)Y\}/M(1 - X).$$

Now XY is a random variable representing the event Y, given that X has happened, and $(1 - X)Y$ is a random variable representing the event Y given that X has not happened. If we represent these two new events by A and B, then our last equation reduces to

$$M(A) = M(B)$$

which looks like (2). The division respectively by $M(X)$ and $M(1 - X)$ is done in order to make the number of experiments involved equal in the two cases.

Now if all the animals die, this is the case where the variances do vanish, and in this case (1) ceases to have a meaning, while (2) still makes sense.

The other point I should like to make is that my use of the adjective 'powerful' was simply for the sake of brevity, and it in no way means that I accept Neyman and Pearson's theory of testing hypotheses. In fact, what I tried to do with my test was to base it on what might be called 'common-sense' notions. Apart from the condition of 'validity', the condition of 'symmetry' arises directly from the fact that the table

$$\begin{Bmatrix} a & b \\ c & d \end{Bmatrix} \text{ is really the same as the table } \begin{Bmatrix} b & a \\ d & c \end{Bmatrix}$$

while the condition of convexity arises from the fact that, if we consider

$$\begin{Bmatrix} a & b \\ c & d \end{Bmatrix}$$

to be 'significant', then we must (on common-sense grounds) consider

$$\begin{Bmatrix} a-1 & b+1 \\ c & d \end{Bmatrix}$$

to be significant (provided $a<b$). The condition of 'maximum number of points' is perhaps the most doubtful one; I base the argument for this (a) on the requirement that we must have a unique test, and then (b) on the idea that we have no means of choosing, on the data given, one point of the lattice diagram rather than another—which amounts to saying we are testing the null hypothesis, but we have not specified any particular alternatives against which we are testing it. This last idea is certainly in flat contradiction with Neyman's ideas. (see P.S. below).

If I may say so, it has for a long time struck me that Neyman and Pearson's ideas have caught on widely, because they are based on an explicit theory of probability (the neo-classical theory), and they are therefore more easily put in 'clear' mathematical language than your own ideas. It is only too true, however, that 'clear' mathematical language often presents such an abstract picture of the true state of affairs, that 'clarity' is gained at the expense of truth. As far as I know (and I should be very grateful for correction), your own ideas on *probability*, as distinct from statistical testing, have not been set out explicitly in a complete form—I have found suggestions in your early papers in *Proc. Camb. Phil. Soc.*, but they do not seem to have been developed in full.

During this summer I have been trying to work out a theory of mathematical probability which goes beyond the usual 'additive set function' notions, and tries to account for the *origin* of the notion of distribution. The theory is algebraic in character, and it appears that the notion of invariance under the symmetric group, or under sub-groups of the symmetric group, plays an essential part. The theory is in a sense purely mathematical, and it seems to have purely mathematical points of interest, but in addition I venture to think that the role of the symmetric group in this theory parallels the role of 'randomisation' in your theory of significance testing; so that I speculate that this theory may make it possible to 'formalise' some parts of your own theory.

I have nearly finished writing out a preliminary account of this theory, and I should be very grateful for the privilege of sending you a copy for criticism.

P.S. If we make another 'common-sense' condition, that the 'significance' of the result does not depend on the unit of measurement chosen, we can derive certain cases of the t-test in a similar way.

[1] Barnard, G.A. (1945). A new test for 2 × 2 tables. *Nature* **156**, 177.
[2] See *Nature* **156**, 783.

Fisher to G.A. Barnard: 30 *October* 1945

I am sorry I have not answered before your letter with your entirely courteous reply to my comments in *Nature* [*CP* 205]. I think I now understand your position much better, though I do not altogether agree with it. You say[1] 'In case (1) we do not have in mind any proposal to do away with blue-eyed people in order to reduce colds', but we surely do have in mind proposals of this kind, e.g. with people bearing in one of their chromosomes the gene for Huntington's Chorea, in order to reduce the incidence of this distressing form of madness. In any case, if blue-eyed people did have colds more than brown-eyed people, the scientific fact would be that a generation with fewer blue-eyed would have fewer colds, supposing the conditions to remain otherwise unaltered, and the question always seems to be whether there is or is not such an association. The all blue-eyed population *is* a possible world, as doubtless every good Japanese would agree.

My quite general point is that even when we fix the total volume of data, it is sometimes more informative than at other times, and it is the general function of ancillary information to notify us of how good or informative our data actually are in all cases where they may be variable in this respect.

Expecting a ratio 1:1, I may have 14 plants, four of one kind and ten of another. The obvious convention is to compare this with a frequency distribution of the various ways in which 14 would be subdivided, given by the binomial $(\frac{1}{2}+\frac{1}{2})^{14}$; but repeated experimentation may not always give 14. Even if I fix the number of seedlings planted out, there may be variation due to some other factor, e.g. perhaps about half the plants are white-flowered and so not classifiable as between original classes pink and purple. I put out in all 25 plants, and I may get 14, but I may get only 11 to be classified. I believe the appropriate test of significance disregards the total of 25 plants which is constant from trial to trial of the kind I am making, and regards the actual randomly distributed number 14 of plants which supply information relevant to my problem.

How do you think this goes?

[1] *Nature* **156**, 783.

G.A. Barnard to Fisher: 12 *January* 1949

Two years ago you were kind enough to write to me about a letter I had sent to *Nature*, about 2 × 2 tables. I did not reply, because your letter gave me so much food for thought that I hesitated to commit myself to paper. I am now sending you the fruits of the thoughts suggested by your letter. I am afraid they are rather long; but I should be most grateful if you could find time to read them.

The duplicated paper is to be read before the Research Section of the Royal Statistical Society on March 10th,[1] but it does not have to go to the printer for another month or so. If you have any alterations to suggest—particularly to the passages in which I suggest that the theory put forward is essentially a development of your own—I should be glad to make them, with proper acknowledgement.

[1] See Barnard, G.A. (1949). Statistical inference. *J. R. Statist. Soc.* **B 11**, 115–39.

Fisher to G.A. Barnard: 21 *January* 1949

Thank you for your letter and enclosures. The typescript was of course of particular interest to me, for there you have, so to speak, developed a general calculus of likelihood applicable more widely than to those situations for which I first introduced the idea.

This seems to me particularly worth doing in view of the fact that, whereas exhaustive estimation leading to [a] probability statement may or may not be possible, the likelihood function becomes well defined as soon as distinct theoretical ideas are formed at all.

Consequently I congratulate you on your enterprise and on your interesting paper. . . .

G.A. Barnard to Fisher: 13 *October* 1953

First, may I thank you for the magnificent dinner on Thursday evening. I do hope your train was not so late back in Cambridge as to make you resolve never to take it again, for I hope you will do me the honour of allowing me to be the host to you next time there is a Council[1] meeting at that time of day.

I am afraid I shall always be a bad correspondent with you, because what you have to say sets up in me a train of thought which is liable to go on for months—even years, as with the remark, which you have probably forgotten, about what happens to the 'fixed' sample size when someone treads on one of the plants. . . .

Coming now to another point we discussed, the definition of 'sufficient statistic' which has now gained widest currency among *mathematicians* goes as follows:

(I) A statistic t is sufficient for a parameter θ if the distribution of any other statistic t', given t, does not involve θ.

This is the way, for example, the thing is put in Wilks.[2] I had assumed that it

was your definition, until I looked the matter up in your collected papers. There, the nearest to a definition seemed to be given in 'The mathematical foundations of theoretical statistics' [*CP* 18], (p. 316) where we have:

(II) 'That the statistic chosen should summarise the whole of the relevant information supplied by the sample.

This may be called the *Criterion of Sufficiency*'.

You elsewhere show that (I) is a *true statement* (not a definition) when there are no unknown parameters other than θ, but you do not (so far as I can find) give it as a definition.

I would not bother you with my confusions on this point if I did not know that such confusions are quite widespread. For example, I remember Fieller telling me that the sample correlation coefficient r was not sufficient for the population ρ, because the distribution of the sample means $(\bar{x},\bar{y})$ given r, also involved ρ. I replied that r was 'in a way' sufficient, because you could obviously not make use of the information in $(\bar{x},\bar{y})$ without knowing the population means (μ_1,μ_2). But I thought at the time that Fieller was strictly right according to the definition. Now it seems not. If we take definition (II), r is sufficient for ρ, in the absence of knowledge of μ_1,μ_2, and it only fails to be sufficient if we take (I) as a definition, rather than as a special consequence of the definition.

In fact, so far as I know, you have not given a mathematical definition (as 'opposed' to a logical definition) of the phrase 'sufficient statistic for a parameter θ'. May I now suggest the following:

(A) If $x_1,x_2, \ldots x_n$ are independent observations on X, whose probability function is $\phi(x \mid \theta,\theta')$, the statistic $t = t(x_1,x_2, \ldots x_n)$ is a *statistic for the parameter* θ if, for any $Y = f(X)$ which has a probability function of the same form $\phi(y,\xi,\xi')$, the condition $\theta = \xi$ implies that

$$t(f(x_1),f(x_2), \ldots, f(x_n)) = t(x_1,x_2, \ldots, x_n).$$

(B) A statistic t for a parameter θ is sufficient for that parameter if the conditional distribution, given t, of any other statistic t' which is a statistic for θ, is independent of θ.

The definition (A) differs from what I suggested the other night in that one does not need to have a *group* of transformations—one transformation may be enough. . . .

[1] Council of the Royal Statistical Society. Fisher was President and Barnard a Vice-President of the Society.
[2] Wilks, S.S. (1947). *Mathematical statistics*. Princeton University Press.

Fisher to G.A. Barnard: 17 *October* 1953

Thanks for your letter explaining those of your statements the other night that I did not understand. About sufficiency, at various times in the past I have tried out on classes and discussions a system of ideas much like the following:

(a) A set of functions of the observations, the joint distribution of which in whatever is to be called the population of random samples is independent of all parameters, is a set of ancillary statistics.

(b) A set of functions of the observations such that, given all members of this set, the distribution of any functionally independent function of the observations is independent of all parameters, is an exhaustive set of statistics.

Scholium: By throwing out unnecessary members there is, I suppose, a minimal exhaustive set, but the possibility of such a minimal set not being unique probably needs investigating.

(c) A set of functions of the observations and of the parameters, the simultaneous distribution of which is independent of all parameters, is termed a pivotal set of functions.

Scholium: One rather likes problems in which the maximum number in the pivotal set is equal to the number of parameters and in which only the minimal exhaustive set of statistics is involved.

Probably I have mis-stated some of this, as it is a long while since I have tried to expound it to anyone.

Fisher to G.A. Barnard: 21 *November* 1953

I have just received the enclosed from E.T. Williams of the *D.N.B.*[1] Perhaps you might forward it to [R.F.] George as R.S.S. business.

I am inclined to suggest that you might write an appropriate biographical notice calling attention to the omission in the *D.N.B.* and to their rather surprising policy of not making such omissions good. If we published in the Society's journal it would not only come within reach of the very large membership of the Society, but you would be able to secure offprints for your own distribution.

If you think well of this suggestion, you might enclose my letter also to George.

[1] Fisher had sent the following letter to the Editor, *Dictionary of national biography*, on 31 October 1953 and E.T. Williams had replied advising that earlier omissions are not made good.

re The Reverend Thomas Bayes, F.R.S.

I understand that it is your custom in making up supplementary volumes for the *Dictionary of National Biography* to include, not only those who by recent death have become eligible for inclusion, but also some others of earlier date whose distinction can now be more clearly appreciated.

The Council of the Royal Statistical Society, of which I am this year President, wishes to draw your attention to the case of the Reverend Thomas Bayes (1697–1762)(?) who in the twentieth century has become one of the best known figures in the history of the development of our understanding of inductive reasoning, a central theme in the study of statistical methodology. Biographies already exist in your *Dictionary* of his father and grandfather who were distinguished as dissenting Ministers, but not of their descendant who was Fellow of the Royal Society for twenty years, and has gained perhaps a more lasting celebrity.

Bayes was cited by Laplace in his *Theorie Analytique*, and obviously exercised a profound influence on this great French writer. In 1838 he is referred to by Augustus De Morgan[1] in the terms 'This was first used by the Rev. T. Bayes, in *Phil. Trans. liii.* 370.; and the author, though now almost forgotten, deserves the most honourable remembrance from all who treat the history of this science'. It is, however, only in the present century, in connection with the theory of experimental design, and of inductive inference in general, that widespread attention has been focussed on Bayes' important contribution.

It would not be difficult at the present time, although many particulars have doubtless been lost, to obtain a competent notice of this distinguished mathematician. To my knowledge, for example, Professor G. Barnard of The Imperial College of Science, and a Vice President of this Society, has been gathering particulars of his family and career. My Council would all be gratified if, through their mediation, it were possible to add to the national series a biographical notice worthy of so remarkable a subject.[2]

[1] De Morgan, A. (1838). *An essay on probabilities and on their application to life contingencies and insurance offices*. Longman, London.

[2] An omissions volume of the *Dictionary of national biography* is being prepared in 1989 and this will include Thomas Bayes.

Fisher to G.A. Barnard: 9 *February* 1954

. . . I find, looking up the old papers, that I can now understand, much better than before, the early work of Neyman, or Neyman and Pearson, in the light of what you said the other afternoon, for it now seems clear to me, as it did not before, that Neyman, thinking all the time of acceptance procedures, was under the misapprehension that my own work on estimation had only the same end in view.

A subsidiary trouble, which has emerged again and again since, is his non-recognition of the fact that rigorous inductive inference must include the totality of the available information. A good example of the latter I can quote from a paper of 1933, [*Proceedings of the*] *Cambridge Philosophical Society*, volume 29, page 492. In the middle of the first paragraph he says, 'In dealing with the problem of statistical estimation, R.A. Fisher has shown how, under certain conditions, what may be described as rules of behaviour can be employed which will lead to results independent of these probabilities; in this connection he has discussed the important conception of what he terms fiducial limits'.

The points to notice here are (i) the change from rules of inference to rules of *behaviour*, and (ii) the failure to distinguish between inferences valid when no Bayesian *a priori* knowledge is available, and inferences valid whatever may be the nature of such information *a priori*. He ignores completely my warning of three or four years earlier that the fiducial distribution would be invalid to any one possessing knowledge *a priori* in addition to the observed sample.

G.A. Barnard to Fisher: 14 *June* 1954

I enclose the notes on the 'Creasy–Fieller paradox'[1] which I promised. Of course, my elaborate argument is not intended to replace the simple one used by Bliss and Fieller, but to show that if we start by the roundabout route taken by Miss Creasy we still arrive at the same result, provided we proceed with proper care.

I would very much appreciate any critical comments you may have time to make.

On the other paradox, 'Mauldon's paradox',[2] while the Jacobian has a constant sign, one of the sub-Jacobians changes sign. I wonder whether one should require not only that $\partial(u,v,w)/\partial(x,y,z)$ should remain of constant sign, but also that the sub-Jacobians $\partial(u,v)/\partial(x,y)$, $\partial(u,v)/\partial(x,z)$, . . ., $\partial(u,w)/\partial(x,y)$, . . etc. should all remain of constant sign?

The paradox arises from the general Wishart distribution, but the simplest case can be put as follows:

Define $Q = Q(x,y,z|\alpha,\beta,\gamma)$ by

$$Q = \{(x/\alpha)^2 + (y/\beta)^2 + 2(x/\alpha)(y/\beta)\sin z \sin \gamma\}/2,$$

and consider the trivariate distribution

$$\mathrm{d}F = (\cos^2\gamma/\pi\, \alpha^2\beta^2)xy\, \mathrm{e}^{-Q}\, \mathrm{d}x\mathrm{d}y\mathrm{d}z,$$

with $0 < x < \infty$, $0 < y < \infty$, and $-\pi/2 < z < +\pi/2$, where the variates are x,y,z and the parameters are α,β,γ, with $\alpha > 0$, $\beta > 0$, and $-\pi/2 < \gamma < +\pi/2$.

If we make the transformation

$$u = (x/\alpha)\cos\gamma,\ v = (y/\beta)\cos z,\ w = (x/\alpha)\sin\gamma + (y/\beta)\sin z,$$

we find that

$$Q = (u^2 + v^2 + w^2)/2.$$

and the Jacobian is

$$\partial(u,v,w)/\partial(x,y,z) = \begin{vmatrix} (1/\alpha)\cos\gamma & 0 & 0 \\ 0 & (1/\beta)\cos z & -(y/\beta)\sin z \\ (1/\alpha)\sin\gamma & (1/\beta)\sin z & (y/\beta)\cos z \end{vmatrix}$$

$$= \{y/(\alpha\beta^2)\}\cos\gamma$$

giving the distribution

$$\mathrm{d}F = (1/\pi)u\, \mathrm{e}^{-(u^2+v^2+w^2)/2}\, \mathrm{d}u\mathrm{d}v\mathrm{d}w,$$

where $0 < u < \infty$, $0 < v < \infty$, $-\infty < w < +\infty$.

Since this is independent of all parameters, the set (u,v,w) are pivotal quantities, and given the observations (x,y,z) they therefore appear to determine the fiducial distribution of (α,β,γ) as

$$\mathrm{d}F = ((1/\pi)x^3y\cos z)\,\alpha^{-4}\beta^{-2}\cos\gamma\cdot \mathrm{e}^{-Q}\mathrm{d}\alpha\mathrm{d}\beta\mathrm{d}\gamma.$$

But if instead we had taken the transformation

$$u' = (x/\alpha)\cos z,\ v' = (y/\beta)\cos\gamma,\ w' = (x/\alpha)\sin z + (y/\beta)\sin\gamma,$$

we would have arrived at the apparent fiducial distribution

$$\mathrm{d}F = ((1/\pi)xy^3\cos z)\,\alpha^{-2}\beta^{-4}\cos\gamma\cdot \mathrm{e}^{-Q}\,\mathrm{d}\alpha\mathrm{d}\beta\mathrm{d}\gamma$$

which is different from the other one, in that α and β are interchanged.

There does seem to be an internal 'twist', associated with the angular variable z, in the distribution, and this may be connected in some way with the change of sign of the sub-Jacobians $\partial(u,w)/\partial(x,y)$, and $\partial(u,v)/\partial(x,z)$, as z passes through 0.

I hope when examinations and other distractions are finished to chew this over thoroughly.

[1] See Fieller's letter of 10 January 1954 (p. 86).
[2] See Mauldon, J.G. (1950). Pivotal quantities for Wishart's and related distributions, and a paradox in fiducial theory. *J. R. Statist. Soc.* **B 17**, 79–85.

[Enclosure]

THE CREASY–FIELLER PARADOX

1. For simplicity we consider the case of a pair (x,y) of independent observations from normal populations with unit variances and means (ξ,η). Given x, the fiducial distribution of ξ is normal, centred at x, and that of η is normal, centred at y, and these fiducial distributions are independent and each has variance 1. The joint fiducial distribution of (ξ,η) is therefore circular normal, centred at the point (x,y). We suppose, for definiteness, that both x and y are positive.

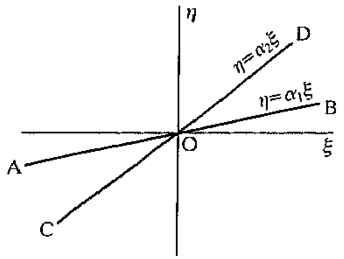
2. The Creasy–Fieller paradox is concerned with the fiducial distribution of the ratio $\xi/\eta = \alpha$. Fieller considers the pivotal quantity $(x - \alpha y)/(1 + \alpha^2)^{\frac{1}{2}}$, which has a standard normal distribution, and in effect says that the fiducial probability that

$$\alpha_1 < \alpha < \alpha_2 \text{ is } \phi\{(x - \alpha_2 y)/(1 + \alpha_2^2)^{\frac{1}{2}}\} - \phi\{(x - \alpha_1 y)/(1 + \alpha_1^2)^{\frac{1}{2}}\}, \tag{1}$$

where $\phi(u) = (1/\sqrt{2\pi}) \int_{-\infty}^{u} \mathrm{e}^{-t^2/2}\,\mathrm{d}t$. As α ranges over $(-\infty, +\infty)$,

$|(x - \alpha y)/(1 + \alpha^2)^{\frac{1}{2}}|$ cannot exceed $(x^2 + y^2)^{\frac{1}{2}}$, and this corresponds to the fact that, at the level of significance $2\phi\{-(x^2 + y^2)^{\frac{1}{2}}\}$, we have no evidence any linear combination of ξ and η differs from zero, i.e. we have no evidence at this level of significance that the ratio ξ/η exists at all.

3. Miss Creasy takes the joint, circular normal, distribution of (ξ,η) and to obtain the fiducial probability that $\alpha_1 < \alpha < \alpha_2$ she integrates this distribution over the sectors BOD, AOC, where AOB is the line $\eta = \alpha_1\xi$ and COD is the line $\eta = \alpha_2\xi$. Her result differs from Fieller's.



4. The fallacy in Miss Creasy's argument seems to lie in that when we transform from the pair of variables (ξ,η) to a pair (α,β), where $\alpha = \xi/\eta$ and β is any function of ξ and η, the origin is a singular point of the transformation. We may contrast this position with that arising, for example, in the Fisher–Behrens' problem, where we are concerned with the difference $\xi - \eta$ instead of the ratio ξ/η, and where a non-singular transformation is possible.

5. It may be objected that Miss Creasy's method is the same as that ordinarily used to find the distribution of the ratio of two normal deviates. Why should this method not work when we are dealing with fiducial probability?

6. The answer to this objection is that strictly speaking the ordinary method does not give us the distribution of the ratio of two normal deviates unconditionally. It does so only on the assumption that the ratio exists. We must assume that the case (0,0) can be excluded, on some ground or other. Now in most applications of the theory of probability we can interpret events of probability zero as being impossible, though a consistent application of this rule in all cases would lead to contradictions. Because in most cases we can thus ignore possibilities whose probability is zero, we argue here that the case (0,0) can be ignored, as having probability zero. This means that the origin can be removed from the plane we are considering, and then our transformation becomes non-singular.

7. The principle that propositions of probability zero can be ignored, while it is often, but not always, applicable when the propositions are concerned with observations, seems hardly ever to be applicable to propositions concerned with parameters. With observations, we are most often concerned with series of independent trials, and with events which occur in such series with finite relative frequencies. With parameters, the concept of a series of independent trials has not the same interpretation. We may say, roughly, that when we are dealing with a parameter, such as ξ, we have to reckon with the fact that, if by chance ξ were zero, it would forever retain this value, since it is a constant. While if an observation were found to have this value, we could be

sure the next observation would have a different value. The observation with direct probability zero could occur, if at all, only transiently; a parameter value with fiducial probability zero, if it occurred, would remain so for ever.

8. In transforming the joint fiducial distribution of (ξ,η), therefore, special attention must be paid to the possibilities $\xi = 0$, $\eta = 0$. Perhaps the most natural way of doing this is to interpret the fiducial distribution, in relation to the ratio of the means, in the light of the fact that we normally restrict our considerations to the ratios of quantities both of which have the same sign. A very general method of expressing a ratio is as a percentage, but one does not hear of one quantity being -50 per cent of another.

9. If we interpret the fiducial distribution of (ξ,η) in this way, we are led to interpret points lying to the left of the η-axis as corresponding to a non-significant departure of x, and for such cases we may regard the ratio ξ/η as having the 'improper value' 0 — a value improper in the sense that it does not, in conjunction with the value of ξ, determine that of η. Similarly, we may interpret points below the ξ-axis as corresponding to $\eta = 0$, and the improper value ∞ for the ratio ξ/η. But if we thus interpret points to the left of the vertical axis, and points below the horizontal axis, we shall have counted twice the points lying in the third quadrant. To restore the balance, in considering the points lying between the lines AOB, COD, corresponding to values of the ratio between α_1 and α_2, the sector AOB, lying in the third quadrant, must be taken negatively, while that in the first quadrant is taken positively. We are thus led to take, as the fiducial probability that the ratio ξ/η lies between α_1 and α_2 the difference of the probabilities associated with the areas underneath the lines AOB and COD. But the probability associated with the area under AOB is evidently that associated with a normal deviate equal to the distance from the point (x,y) to the line AOB, i.e. to a normal deviate of

$$x - \alpha_1 y/\sqrt{(1 + \alpha_1^2)},$$

and taking the difference of this probability and that corresponding to α_2, we evidently obtain Fieller's result.

Fisher to G.A. Barnard: *16 June* 1954

Thanks for your letter; I hope I may understand it!

I think there is a distinction of data which may be worth attending to.

If we have a number of observations (x,y) which we interpret as independent shots of some hypothetical pair (ξ,η) with generalised bivariate error of normal specification, I suppose one can calculate $\bar{x}$, $\bar{y}$ and the quadratics

$$A = S(x - \bar{x})^2/\{N(N - 1)\}, \text{ etc.},$$

and infer a bivariate fiducial distribution with density

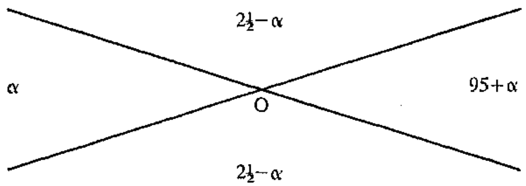
$$\{N/(2\pi)\}\ (1 + r^2)^{-\frac{1}{2}(N+2)}$$

where $r^2(AC - B^2) = C(\xi - \bar{x})^2 - 2B(\xi - \bar{x})(\eta - \bar{y}) + A(\eta - \bar{y})^2$; then the integrated total probability beyond any line will agree (will it not?) with the corresponding 'Student' test for e.g.

$$\lambda(\xi - \bar{x}) + \mu(\eta - \bar{y}) > \nu,$$
$$\nu^2 = (A\lambda^2 + 2B\lambda\mu + C\mu^2)\, t_{N-1}^2.$$

On such an interpretation of the data the integral over any chosen region seems to give the appropriate probability statement.

But, in the example in *Statistical Methods* § 26.2, I suppose the two drugs to have effects on any chosen patient in a fixed ratio. I am concerned to set limits to this ratio, not to estimate the properties of any population from which the patients might be supposed to be chosen. This hypothesis gives a series of points, one for each patient, distributed in some unknown manner, but all on one and the same straight line through the origin.



The t tests exclude lines outside the V on the right, and exclude also the V on the left, so that fiducially the probabilities are as above with α undetermined.

If one were to receive an accretion of data to the effect that the patients were normally distributed about some population mean, I suppose one could ease the V in so as to make it include exactly 95%. But this would answer a different question.

Fisher to G.A. Barnard: 15 *July* 1954

I am just writing to see whether, by any chance, a letter of mine of June 16th has not yet reached you.

If it has duly arrived, do not think that I am in a hurry for a reply, though, of course, I should be glad to clear up any point that might have been sufficiently puzzling to delay you in answering.

G.A. Barnard to Fisher: 18 *July* 1954

Thank you for your letter of June 16th and for your further note of July 15th. I must apologise for not having acknowledged earlier your first letter. The fact is, I did not understand it fully at first reading, and so kept it to meditate on. . . . .

I did not write to ask you about my difficulties because my past experience has been that such difficulties have usually been of my own making. It has so

often happened that I have found something you have written to be obscure at first reading, only to see later on that this was because I had not understood the problem properly; and I have usually ended by seeing that what you have said was not only correct, but also the simplest and most direct way of stating the truth. And the process of untying the knots in my own mind has been enjoyable as well as salutary.

However, since you encourage me, may I say how far I have progressed up to now?

If I understand you aright, you point out that to assume that

$$z = x - \alpha y$$

is normal, as you do in *Statistical Methods*, Section 26.2, in no way implies that either x or y is normal, nor that they have a joint bivariate normal distribution. For example, if x had a rectangular distribution with range $(-\delta, +\delta)$, and y had the density

$$(1/\sqrt{2\pi}) \int_{-\delta}^{+\delta} \exp\{-(y - u)^2/2\sigma^2\}\, du$$

then z could be normal. The situation is exactly as in regression theory, where the independent variable need not be distributed in any particular way.

If all we know is that z is normal, or can be taken so, for some unknown α, then the method of Section 26.2 gives the only way to proceed. No question of a bivariate fiducial distribution of the means (ξ,η) of x and y can arise in this case.

But if we are given the further information that x and y are each normally distributed, then we have a bivariate normal distribution from which we can infer the joint fiducial distribution of (ξ,η) as on the first page of your letter. The fact that, with this extra information, we can make statements qualitatively different from those we can make without it is not surprising, when we remember the peculiarities of inductive inference, as opposed to deductive inference, which you have so often stressed.

So far so good. But if we consider now the case where we know x and y separately to have independent normal distributions, then we can proceed to discuss the ratio $\alpha = \xi/\eta$ of means *either* by using the single pivotal quantity

$$(x - \alpha y)/(A - 2B\alpha + C\alpha^2)^{\frac{1}{2}}$$

or by using the two pivotal quantities

$$(x - \xi)/\sqrt{A} \text{ and } (y - \eta)/\sqrt{B}$$

and we do not get the same answer. Which method should be used? Here I feel inclined to think that my argument about the singularity at (0,0) shows that the first method should still be used. In other words, although there has been an accretion of data, we are unable to use it.

This last paragraph is where I am now stuck. Of course, in the light of your point about the distinction of the data, the question becomes largely theoretical, since the cases that occur in practice seem all to be covered by Section 26.2.

In all the discussion at the Research Section, no one made your point about the distinction of data, and it would, I think, be a great pity if the discussion went into print without mention of the point. I wonder if you would allow me to mention the matter to Fieller, so that he could refer to it (in terms to be approved by you) in his reply to the discussion? From the practical point of view, what you say certainly disposes of the question.[1]

P.S. I think what I am trying to say in the last paragraph but three above could be put in this way: If, *per impossibile*, we had information that the value (0,0) for (ξ,η) was excluded, but all other positive or negative values were possible, then Miss Creasy's argument would be correct. We could never, of course, have such information; but we might approximate to it in cases where x and y were confined to positive values (cf. the people with negative heights we once discussed)—if, for example, the ratio with which we were concerned were some anthropometric index—and then, to the extent that we were still justified in taking x and y normal, we would have something like this situation. But precisely in this case the means would have to be large compared with the standard deviations, and it is here that Miss Creasy's argument leads to the same answer as that of *Statistical Methods*, Section 26.2.

[1] Shortly after receiving this letter from Barnard, Fisher invited Barnard and Fieller to Cambridge for a discussion of the questions raised.

Fisher to G.A. Barnard: 20 *September* 1954

Thank you for thinking of sending me the little Dutch cigars with which I shall experiment with much pleasure. The container certainly is charming.

I am glad you are thinking of taking up again with me questions involving the use of fiducial probability. I suppose it is agreed that statements of fiducial probability are the only kinds of probability statements that we can make on empirical evidence about the real world.

G.A. Barnard to Fisher: 15 *November* 1954

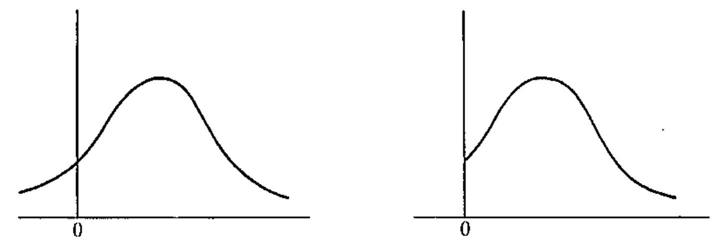
May I take up yet again the questions on fiducial distributions you were kind enough to discuss with Fieller and me in the summer?

First, you may remember saying that some Russian mathematician had undoubtedly proved a theorem, with many lemmas, to the effect that, if we know, for all α,β, the distribution of $U = \alpha X + \beta Y$, then we know the joint distribution of X and Y. The Russian in question is Romanovsky, who proved in 1928, in the bivariate case, that the characteristic function uniquely

determines the distribution function. For the c.f. of U is $\xi \exp itU = \xi \exp i(\alpha tX + \beta tY) = f(\alpha t,\beta t)$, say. The joint c.f. of X and Y is then $\xi \exp (iuX + ivY) = f(u,v)$. Since we know $f(\alpha t,\beta t)$ for all α,β, we know $f(u,v)$ for all u,v.

Now some questions. They are general ones, because I cannot expect you to recall the details of what we discussed so long ago, and also because I think I can see the points better in the more general cases.

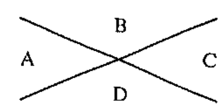
Question 1: If we have a sample of n, with mean $\bar{x}$ and s.d. s, and we find the fiducial distribution of μ in the usual way, it is a t-distribution centred at $\bar{x}$. Now suppose that for some reason we can rule out negative values of μ, as being impossible. Can we then take the fiducial distribution to be a 'mixed' one, having the same t-distribution form to the right of the origin, but having the probability which formerly was to the left of the origin all lumped at the origin? This seems to be reasonable. Am I wrong?



All values of μ possible   Only positive values of μ possible

A mixed type of fiducial distribution like this arises in your paper on 'Dispersion on a sphere' [*CP* 249], where there is a lump of fiducial probability corresponding to the possibility that the vectors are randomly oriented.

The bearing of this on the question of the ratio of two means is, that in the case where (x,y) are known to be jointly normal, and independent, we have, as you say, the option of choosing sectors so that the difference $A - C$ is α, or we may slightly alter them to make the sum $A + C = \alpha$. Now what should we do? It seems to me that when we can rule out negative values of the means, as impossible, we should lump as above, and then it is appropriate to make $A - C = \alpha$.



But if we cannot rule out negative values, and still wish to discuss the ratio of the two means, then we should make the *sum* $A + C = \alpha$. I cannot think of an example for this latter case, and am inclined to think that it could not arise in practice; but the theoretical possibility seems there.

Question 2. In deriving pivotal quantities u,v,w, from the sufficient statistics x,y,z, for the parameters α,β,γ,

$$u = f(x,y,z;\alpha,\beta,\gamma)$$
$$v = g(x,y,z;\alpha,\beta,\gamma)$$
$$w = h(x,y,z;\alpha,\beta,\gamma)$$

you have said that the Jacobian must remain of constant sign, and I note George Owen as saying (in his 1948 *Sankhyā* paper) that 'certain monotonicity conditions', e.g. constancy of sign of the Jacobian, are required.

My question is, should we not be cautious whenever f, g, and h are not monotonic in x,y, and z? In other words, should we not require, in general, that not only the Jacobian, but also the sub-Jacobians (e.g. of (u,v) with respect to (x,y)) should also be of constant sign? If this stronger condition is violated, funny things seem able to happen. The possibilities are more easily pictured than written about, I think.

This question relates to a point raised by Mauldon, of Oxford, who obtains two distinct 'pseudo-fiducial' distributions from the same data, by using a transformation in which the sub-Jacobians change sign, although the Jacobian itself stays positive.

Third (and last!) question: Can we not say, whenever we have a given body of data, relating to parameters θ,ϕ, etc., that the likelihood function represents an inference from the data? And can we not deduce, from the likelihood function, that e.g. this pair of values for θ,ϕ is ten times as likely as that pair of values? In this sense, can we not make inferences in terms of likelihood, even when fiducial probability statements are not possible? I would not be happy to say that fiducial probability statements are the only possible ones respecting unknown parameters which it is possible to infer from samples. Fiducial probability statements are the only possible *probability* statements which it is possible to infer; but likelihood statements are always there too. Is this right? . . .

Fisher to G.A. Barnard: 16 *November* 1954

I had been hoping to hear from you, and your letter is even a greater pleasure than I had expected.

i) Your treatment of the uniqueness of the two-way distribution function when the probability dichotomy is given for all lines on a plane is delightfully simple and direct.

ii) I should think, as you do, that there is a condensation of fiducial probabilities in the case you mention, and in a fair number of other cases, of which one of practical importance arises from the fact that we can assume an unknown variance to be non-negative. I have not, however, so much experience of cases of this kind arising in practice that I should be shaken if anyone could show that this type of argument leads to unreasonable conclusions, but then I feel this way about all self-evident axioms. For reflection, however, consider the case where negative values of zero measure, e.g. all numbers of the form $-N/2^n$, were admissible, but not other negative numbers.

iii) In applying this method to the case of (x,y), known to be jointly normal, you say also 'and independent' so are not discussing the case I took in my note [*CP* 257] on the general shemozzle between Fieller and Miss Creasy, but the one I mentioned later in a letter.[1] I agree with your first proposition, but would modify your second from 'still wish to discuss the ratio of the two means', by the addition, 'and to ignore whether for a positive ratio they are both positive or negative', for it seems that the data do also discriminate these cases.

iv) I am very glad that you are thinking of the question of pivotal uniqueness which I feel sure resembles, even if it is not strictly isomorphic with, the question of mapping one set on another, or rather an infinite series of sets. I have never got my head quite clear about this, but it would be exceedingly interesting to demonstrate that the cases are, or are not, strictly isomorphic.

v) I agree with you entirely that there are cases in which we have validly determined likelihood ratios for all theoretical possibilities, and no probability statements, and so far as I can see, this must be the general and prevailing case in respect of scientific data in general. From this point of view it might be thought an unfortunate coincidence of a mathematical nature that so many practically important questions should lead (trifles having been ignored) to statements of fiducial probability, since it has led the world of mathematical statistics to ignore the manifest fact that on some realistic types of data the condition of our rational uncertainty is not expressible in terms of mathematical probability at all. . . .

[1] See Fisher's letter of 16 June 1954 (p. 13).

Fisher to G.A. Barnard: 2 *December* 1954

It was good of you to turn up to the discussion on Braithwaite's talk,[1] which, indeed, had not much to do with the theory of testing hypotheses. I tried to catch you afterwards without success, so I am writing now to know whether you had ever accomplished the work I rather threw at you last year, of preparing a biographical notice of Thomas Bayes.[2] I believe you had sorted out his family better than had been done in the *D.N.B.*, at least, if I understood you aright, in the definitive edition first published, I believe, in

1900, though I suppose such things can always be corrected in supplementary volumes.

Anyway, I should very much like some time to see Bayes done justice to.

[1] On 22 November 1954, Professor R.B. Braithwaite gave a talk entitled 'Choosing between statistical hypotheses' to the Cambridge University Philosophy of Science Club.
[2] See Fisher's letter of 21 November 1953 (p. 8).

G.A. Barnard to Fisher: 14 *December* 1954

Thank you so much for your letter. I enjoyed the trip to Cambridge very much, particularly because you made quite clear to me the difference of principle between the random allocation of treatments to plots, and the randomisation of an inference. Of course, one feels instinctively that the one is sensible, while the other is, to say the least, suspect. And you had said to me before that the difference lay in the fact that in the second case one *knows* the value of the random component. But until you explained it again, at the meeting, I did not have it quite clear.

The point about randomisation has more importance, I think, than Prof. Braithwaite seemed inclined to admit. I enclose two quotations from Blackwell and Girshick, in the first of which they make such a virtue of randomisation that it becomes a reason for rejecting Bayes principles. (The fact that they virtually contradict this in the second quotation merely shows, I think, that the first passage was written by Girshick and the second by Blackwell). Actually, the kind of randomisation to which Bayes principles fail to lead is the second kind, which, as you have pointed out, is different in principle from randomisation in sampling.

The two quotations are interesting, I think, as indicating a schizophrenic attitude to Bayes principles. Girshick, the more practical and less mathematical, seems to feel instinctively that he must avoid subjective conclusions; while Blackwell, the better pure mathematician, sees quite clearly that the decision function approach leads to subjective conclusions, unless the prior distribution is given and he does not have the practical experience to make him appreciate that this is objectionable.

I shall make a point of coming on January 17th, and look forward to a still more pleasant and profitable evening.

About Thomas Bayes, there was little response to the notice in the Journal.[1] I had a note from the Equitable Life Assurance Company, to say that they believed they had, in their muniment room, a notebook of Bayes, and their man put me on to someone who had made a study of Bayes from a local antiquarian point of view. This man had looked up the copy of Bayes' will in Somerset House, but apart from confirming the impression that Bayes must have been a kindly man, it was of little interest. He had seen the notebook, and I gathered it contained little of importance. However, I must make time to go over to the Equitable offices and look at it myself.

Apart from this, there seems little hope that more information than I have already circulated will come to hand. I hope to be able, after Christmas, to visit the Equitable, after first looking at the Canton MS. in the Royal Society's Library, to check Bayes' handwriting, and then it seems we shall have to close the account. I am afraid the biographical information will hardly do Bayes justice. . . .

[1] See *J. R. Statist. Soc.* A **117**, 120 (1954).

[Enclosure: from Blackwell, D. and Girshick, M.A. (1954). *Theory of games and statistical decisions.* Wiley, New York]

p. 115: 'An objection often raised to the minimax and minimax loss principle . . . is that they do not take into account any information the decision maker may have about ω (the true state of affairs—G.B.). . . . If he can describe his information about ω by a probability distribution ξ over Ω (the set of all possible ω—G.B.), so that $\xi(\omega)$ represents the probability, based on his information, that ω is the true state of the system, then the utility of action (rule — G.B.) d is simply

$$U(\xi,d) = \sum_{\omega} \xi(\omega)u(\omega,d)$$

($u(\omega,d)$ is the expected gain if ω is the true state and decision rule d is used—G.B.).

Thus he maximises his utility by choosing d so as to maximise $U(\xi,d)$. This principle of choice, based on ξ, is called a *Bayes principle*. . . . Bayes principles have the objection that, in most statistical games, ξ is simply an expression of the personal judgement of the decision maker, so that two decision makers, facing the same decision problem and using Bayes principles, might well reach different conclusions from the same data. . . . A further objection to Bayes principles is that they never require randomisation; many statisticians consider that random sampling, which is a form of randomisation, is useful'.

p. 119: 'The above theorem . . . indicates rather clearly that, without assuming an *a priori* distribution on Ω, there is in terms of the present theory no adequate principle of choice for choosing a strategy in a statistical game'.

Fisher to G.A. Barnard: 13 *January* 1955

Thanks for your letter and quotations from Girshick and Blackwell. How remote from the original this talk about Bayes' principle is! . . .

When we meet you must tell me the exact date of Bayes' death, which I have not been able to check, with a view to an opening chapter on history to what may be a book on statistical logic. I suppose there is no longer any hope of your unearthing Bayes' original introduction from which Price quotes.

As news, I believe I now understand what Laplace was saying in his definition of probability. I suspect a definite semantic transference from instances (événemens) to theories, or hypotheses, or states of things, as Boole puts it. Anyway, I shall like to show you what I have written.

Fisher to G.A. Barnard: 19 *January* 1955

I had thought of putting my paper on scientific inference in Series B of the Royal Statistical Society, of which I suppose Irwin is still effectively editor.

I have just now, however, been approached by Linder of Geneva, who was given the tip by Mahalanobis, who would like to secure it for a newish journal called *Dialectica*, now in Volume 8, published jointly by the Neuchatel firm and 'Presses Universitaires de France'. It seems, however, to be mostly in German.

I wonder if you could let me know your own opinion as to whether it ought preferably to be published in England? . . .

G.A. Barnard to Fisher: 23 *January* 1955

. . . About *Dialectica*, I think it rather resembles *Mind*. Few statisticians, or even natural scientists of any sort, read it, I think—at any rate in this country or the U.S.A. So I would urge in favour of Series B of the *Journal*. It would then certainly be easily available to those who would profit most by it.

A thought does occur to me, that the last Ordinary meeting of the R.S.S. for this session is not yet fixed up, and I wonder if you would think fit to give it there. That would have the advantage of allowing a London audience to hear it; but presumably the disadvantage that in the ordinary way the paper would be printed in the *Journal* Series A, which is not quite so widely read as Series B. On the other hand, the written discussion and reply would help a good many, I think.

I think this meeting would be in May. If you like, I would be glad to get in touch with R.F. George about this. But if you prefer Series B, Irwin is still the Editor.

Fisher to G.A. Barnard: 25 *January* 1955

Thanks for your letter. I will see what Irwin thinks.

I do not much want to give it to a meeting of the Society, as it means a long and tiring afternoon with a very wearisome and unprofitable discussion usually to follow. The Society's method of persuading a number of weighty authorities to express themselves at length immediately following a communication is, I think, a bad one in regard to the Society's function of promoting mutual understanding.

However, I suppose there is nothing to prevent Series B printing it[1] without its presentation at a meeting.

[1] See *CP* 261.

Fisher to G.A. Barnard: 18 *February* 1955

As you know I have been putting together ideas on scientific inference, and I believe you have seen most or all of the chapter on the history of the subject in England, which is effectively the history of the subject. I have now completed a first draft of the chapter on the forms of quantitative inference, being particularly concerned to stress the primitive nature of simple tests of significance, and to distinguish them from inferences in terms of mathematical probability in the strict sense, and from inferences in terms of mathematical likelihood.

If you have time I should very much like you to see this chapter, of which I am sending you a copy, together with one of the first chapter, in case you wish to refer to it. The excuse for all this burden on your time is that I very much want personally to have your reaction to what I have said, and to *how* I have said it, with a view later to a little discussion among like minded people who share my belief that we need to develop means of self-expression in the natural sciences more appropriate, and for our purposes more accurate, than have been offered by Neyman, Wald and their followers.

To be successful we shall inevitably have to form firm opinions about a great deal of miscellaneous and inconclusive discussion on probability, logic, and what not. . . .

G.A. Barnard to Fisher: 25 *February* 1955

Thank you so much for your letter and enclosures . . . I shall write again after chewing over, but for the present would commend the enclosed extract, in connection with the historical chapter—you may like to quote it—which seems to me to embody the earliest published significance test, by Daniel Bernoulli, writing in 1734. It seems to help also to stress the primitive nature of tests of significance; though why such a mode of reasoning should remain in virtual eclipse for something approaching 200 years is a bit of a mystery.

One other remark. In connection with the fiducial argument, I have the feeling that many people have found this hard to grasp because they are misled by mathematical symbolism. In real scientific work, an observation is something real, and so is what we represent by a parameter. When we regard an observation as one of a series, or a parameter value as one of a series, we become able to speak of them as 'random variables'; but if we choose to regard them by themselves, alone, they will not be 'random variables', in the mathematical sense. Thus if T denotes a function of the observations, and θ the parameter, T, or θ, or both, may denote random variables, or not, according to the point of view we are, at a given moment, taking. What confuses people, among other things, I think, is that in pure mathematics, where we do not reason on things having objective existence, but on pure symbols, we must *fix* our attitude, so that once T is a random variable, it must always be so; and once θ is regarded as a fixed constant, not a random variable, so it must also remain.

I am afraid I do not make myself clear, and seem to be writing very loosely. I will think some more, to see if I can put things more precisely, in writing if possible, and then perhaps in discussion. I have in mind the distinction, in logic, between what is called the semantic approach and what is called the syntactical approach.

I look forward to seeing you.

[Enclosure]

'—Pièce de M. Daniel Bernoulli.

—Recherches physiques et astronomiques sur le problème proposé pour la seconde fois par l'Académie Royale des Sciences de Paris: Quelle est la cause physique de l'inclination des Plans des Orbites des Planètes par rapport au plan de l'Equateur de la révolution du Soleil autour de son axe; Et d'où vient que les inclinaisons de ces orbites sont différentes entre elles.—Paris, Academie Royale des Sciences—*Recueil des Pièces qui ont remporté les prix de l'Académie*. Tome troisième (1734–37), Paris 1752.

III. Avant que d'entreprendre ces deux points, il ne sera pas hors de propos d'examiner plus particulièrement ce que nous avons posé en fait; savoir, *que les Orbites célestes s'approchent de trop près pour ne point affecter quelque plan commun situé au milieu d'elles, et que ce n'est que par une circonstance particulière, que les mêmes Orbites ne sont pas entièrement unies dans un même plan.* Sans cet examen, on pourroit attribuer à un hasard le Phénomène qui fait le sujet de notre question, et regarder tout notre raisonnement comme superflu, ou peut-être même chimérique.

Voici comme je m'y prendrai: Je chercherai de toutes les Orbites planétaires les deux qui se coupent sous le plus grand angle; après quoi je calculerai quelle probabilité il y a, que toutes les autres Orbites soient renfermées par hasard dans les limites de ces deux Orbites. On verra par-là que cette probabilité est si petite, qu'elle doit passer pour une impossibilité morale.

IV. Après avoir comparé chaque Orbite avec chacune, et calculé les angles sous lesquels elles s'entre-coupent, j'ai trouvé se couper sous le plus grand angle l'Orbite de Mercure, et celle de la Terre ou l'écliptique: car leurs plans font un angle de 6° 45′: pendant que l'Orbite de Saturne ne fait, avec celle de Mercure, qu'un angle de 6° 24′; et l'Orbite de Jupiter, encore avec celle de Mercure, un angle de 6° 8′. Toutes les autres Orbites, de quelque manière qu'on les combine, se coupent sous des angles beaucoup plus petits. Je parle ici des Orbites des Planètes principales.

(Il est facile de voir qu'on peut trouver lesdites intersections par la simple Trigonométrie; . . .).

Je m'imagine donc toute la surface sphérique ceinte d'une zone, ou espèce de Zodiaque, de la largeur de 6° 54′. (Car telle est la plus grande inclinaison de l'Orbite de Mercure avec l'écliptique.) Cette zone contiendra à peu près la dix-septième partie de la surface sphérique. Si l'on considère donc les Orbites planétaires comme placées par un pur hasard, il sera question de déterminer quel degré de probabilité il y a, pour que toutes les Orbites tombent dans une zone donnée de position, faisant la dix-septième partie de toute la surface sphérique. Mais la position elle-même de la zone se détermine par une des Orbites, quelle qu'elle soit, puisqu'elles ne diffèrent guère entre-elles; ce qui fait qu'il n'y a plus que cinq Orbites qui entrent en ligne de compte: cela posé, on trouvera par les règles ordinaires, le nombre des cas, qui fassent tomber les 5 Orbites dans ladite zone, au nombre des cas contraires, comme 1 à $17^5 - 1$; c'est-à-dire, comme 1 à 1419856.

(Je ne donne pas à cette méthode toute la précision géométrique, ce que le Lecteur n'aura pas manqué de remarquer; mais je m'en suis contenté, parce qu'il ne s'agit ici que d'avoir quelque idée générale de la chose. Un nombre considérablement plus grand ou plus petit, ne nous feroit pas envisager autrement le point de la question. On voit pourtant assez que notre proportion ne peut-être fort éloignée de la véritable. Mais, me demandera-t-on, quelle est donc la véritable? Je réponds à cette demande, qu'on ne sauroit le déterminer, à cause du mouvement des noeuds qui changent à tout moment les limites des Orbites: j'ai donc simplement considéré une zone, hors de laquelle aucun point des Orbites, quoique changeantes de position, ne sorte jamais, et j'ai comparé cette zone avec la surface de la sphère dont elle fait à peu près la dix-septième partie, tantôt plus, tantôt moins, à cause de la variabilité des limites. Dans cette zone, il n'y a aucun point qui ne soit sujet à être touché par une des Orbites; et hors de la même zone, il n'y a aucun point qui puisse jamais l'être; d'où l'on voit assez le fondement de ma solution. Si tous les noeuds étoient constamment dans un même point commun, il auroit fallu avoir égard au plus grand angle d'intersection de 2 Orbites que nous avons vu être de 6° 54′: et comme cet angle auroit pu aller jusqu'à 90°, si le hasard l'avoit formé, il faudroit comparer ces deux angles, et dire que le premier fait environ la treizième partie du second; d'où l'on tireroit le degré de probabilité, (pour qu'aucune des Orbites ne fit avec une autre Orbite un angle plus grand que de 6° 54′) égal à 1: $(13^5 - 1)$ qui donne une proportion environ quatre fois plus grand, que dans la première solution; savoir, celle de 1 à 371292. Enfin, la meilleure manière de calculer le degré de probabilité seroit de considérer le plan au milieu des Orbites, (qui, selon toutes les apparences, est le plan même de l'Equateur solaire) avec lequel chaque Orbite, quoique mobile, fait sans doute un angle constant, ou presque constant. Si ce plan étoit donné de position, il faudroit calculer quelle Orbite fait le plus grand angle avec ce plan, et quelle est la grandeur de cet angle; et comme dans l'hypothèse des Orbites fortuitement placées, cet angle auroit pu monter jusqu'à 90 degrés, on auroit encore eu à considérer le rapport dudit angle avec celui de 90°, et, posé ce rapport être de 1 a m, le degré de probabilité cherché, seroit maintenant comme 1 à $m^6 - 1$. Je mets ici l'exposant 6 au lieu de 5, que j'ai mis dans les deux exemples précédents, parce que le terme fixé n'est pas ici une des Orbites, mais l'Equateur solaire. Cette méthode me paroîtroit la plus juste de toutes, si la détermination de l'Equateur solaire étoit un peu plus certaine; suivant ce que M. Cassini rapporte dans les Mémoires de l'Académie Royale des Sciences de Paris, de l'année 1701, c'est l'Orbite de la Terre qui fait le plus grand angle avec l'équateur solaire, et cet angle doit être de 7° 30′, cela donneroit $m = 12$, et $m^6 - 1 = 2985983$. Si donc toutes les Orbites étoient placeés fortuitement par rapport à l'équateur solaire, il y auroit à parier 2985983 contre 1, qu'elles n'en seroient pas toutes si proches. Toutes ces méthodes, quoique fort différentes, ne donnent pas des nombres extrêmement inégaux. Cependant je m'attacherai au nombre donné en premier lieu, et n'ai fait cette addition que dans le dessein de faire voir au Lecteur quel fond on y peut faire.)'

Fisher to G.A. Barnard: 26 *February* 1955

Thank you for your letter and its interesting enclosure. The latter I will cap with a quotation from de Moivre,[1] which I believe, but have not yet checked, to be from his first edition of 1718, and to have an error of 10000 in the calculation, which has been corrected in the third edition by adding four noughts after the first eight digits. My memory being bad, I might be mistaken on some of this, but it seems likely that had he done the thing right, he would have used six figures only and said 'millions' one time more.

You will notice that de Moivre assumes wrongly that he has arrived at a probability statement.

I found your example exceedingly interesting, especially the fact that the Academy had posed for a prize essay competition the problem of the angles between the planes of revolution of the planets, a bone that was still being worried over 100 years later in Boole's *Laws of Thought*. It is curious that Bernoulli does not simplify his problem by considering the poles of the respective planes of revolution, and the probability of them, or their antipoles, being grouped as closely as they are found to be. It seems to me irrelevant that his zone should contain only a 17th part of the sphere, for that seems not to mean the same as that the chance of a great circle chosen at random, lying entirely within the zone, should be one part in 17. However, I may be wronging the great Daniel. . . .

[1] There is no record of the quotation in Fisher's file, but Professor Barnard has kindly provided a copy of the relevant passage, shown below, from page *v* of the Preface to the third edition of A. de Moivre's *The doctrine of chances* (1756).

'Further, the same Arguments which explode the Notion of Luck, may, on the other side, be useful in some Cases to establish a due comparison between Chance and Design: We may imagine Chance and Design to be, as it were, in Competition with each other, for the production of some sorts of Events, and may calculate what Probability there is, that those Events should be rather owing to one than to the other. To give a familiar Instance of this, Let us suppose that two Packs of Piquet-Cards being sent for, it should be perceived that there is, from Top to Bottom, the same Disposition of the Cards in both Packs; let us likewise suppose that, some doubt arising about this Disposition of the Cards, it should be questioned whether it ought to be attributed to Chance, or to the Maker's Design: In this Case the Doctrine of Combinations decides the Question; since it may be proved by its Rules, that there are the Odds of above 263130830000 Millions of Millions of Millions of Millions to One, that the Cards were designedly set in the Order in which they were found.

From this last Consideration we may learn, in many Cases, how to distinguish the Events which are the effect of Chance, from those which are produced by Design: The very Doctrine that finds Chance where it really is, being able to prove by a gradual Increase of Probability, till it arrive at Demonstration, that where Uniformity, Order and Constancy reside, there also reside Choice and Design.'

G.A. Barnard to Fisher: 16 *April* 1955

I enclose the draft chapters, in reply to your letter[1] which I found on returning from Paris, where I have been giving some lectures about foundations. . . .

There are just a few detailed points on the drafts:

1. p.1: On Bayes' mathematical attainments, I would suggest adding to 'his mathematical contributions to the *Phil. Trans.*' 'and his (anonymous) tract on fluxions'.

Also, I wonder if you think fit to make the point that not only was the scientific atmosphere ripe (after Newton and Boyle), but also the philosophical atmosphere, after Locke and Hume. . . .

Perhaps it might be mentioned that Cournot, in France, expressed views similar to those of Boole in England; though Cournot suffered even more neglect from his countrymen than did Boole from his.

On the general point, I think the only one is that made by Frank Yates, about considering the sensitivity of a test. And on this, perhaps it might be pointed out that a good test of significance needs to be both sensitive and insensitive — sensitive to the kind of departures from the null hypothesis which are of interest, and insensitive to the kind of departures not of interest. Thus t is sensitive to departures of the mean, but not to departures from normality. But Bartlett's test for homogeneity of variances is sensitive to departures from normality almost as much as it is to departures from equality of variances.

I am still thinking hard about the question of axiomatising ignorance!

[1] Fisher's secretary had written on 1 April asking about the two draft chapters which Fisher had sent Barnard on 18 February.

Fisher to G.A. Barnard: 18 *April* 1955

Many thanks for your long letter. I am afraid I knew nothing about the 'anonymous tract on fluxions'. I should like to know more, but I do not think I can use this knowledge without making an unnecessary display of learning which I do not possess. This applies also to Locke and Hume.

By the way, have you completed the biographical notice which I think you were invited by the Statistical Society to give about Thomas Bayes? If that is published, I could meet your points by referring to such a notice.

Perhaps you could give a reference to the work of Cournot you refer to, and if you have any such things in your mind, to quotations showing the state of French opinion at the time. On the question of axiomatics, you might be interested in a little section which I have inserted somewhere in the second chapter. I enclose, also, a similar section from chapter four, as a further step in the asymptotic approach to intelligibility!

Fisher to G.A. Barnard: 21 *May* 1955

I was very glad to receive the invitation to your inaugural lecture with a title embodying one of my own special fads, namely that statistics should be taught as a technological subject.

I hope to be able to come up and shall look forward to seeing you, perhaps at tea.

I have written as much of my book now as was burning to find expression, though I suppose I could be flogged into writing some more if any yawning chasms are pointed out. If you want to know how it has developed, let me know and I will send or bring a copy of some part of it.

G.A. Barnard to Fisher: 11 *June* 1955

I am writing to thank you for going to so much trouble to come to my inaugural lecture last week. I only wish I could feel that what I had to say was of some interest. . . .

You wrote some time ago to say that your six chapters were in the hands of Frank Yates, and that I might see them later. I would be very glad to do this, if you would be so kind as to send them.

Fisher to G.A. Barnard: 15 *June* 1955

It is I that should have written to you to thank you for the most enjoyable lecture, and subsequent Dinner, at Imperial College. . . .

I have sent a complete typescript to the printers in Edinburgh, and of the second copy here, chapter five, 'Some simple examples of inferences involving probability and likelihood', is, I think, in Yates's hands. I should guess that it and number six are the only two you have not seen, and number six is chiefly concerned with connecting the ideas of the rest of the book with those of the theory of estimation, and with the difficulties that have been felt about fiducial distributions with many parameters.

Why not send Frank a line and tell him you are ready for chapter five, and I am ready for his comments on it, and let this office send you chapter six when you want it, probably while I am in Brazil?

Fisher to G.A. Barnard: 27 *July* 1955

I think I remember your wanting to see chapter five. I have now got this back from Frank, and could let you have it whenever it may be convenient.

Fisher to G.A. Barnard: 27 *September* 1955

I wonder if the long working hours of a family man on holiday have left you any time to look at my fifth chapter. Anyway, when you are through with it, if possible without hurrying, it would be nice to have it back.

On your instigation I have looked at the Pearson and Hartley Table 11, and for the case of the utmost simplicity ($n_1 = n_2$, and, in my sense, $s_1 = s_2$) I was quite taken aback to find that what Sukhatme and I have called the value of d (here called v) is, as given by Welch, or perhaps Mrs. Aspin, (for Pearson takes a lot of trouble to distribute the responsibility) actually less than t, at the level of significance chosen, for $n_1 + n_2$ degrees of freedom; so you get a sharper test if you can say 'I really am not quite sure that these variances are equal' than if you have reluctantly to admit that they must absolutely be exactly equal, in which case 'Student's' test would have *faute de mieux* to be applied!

Actually, it is not difficult to demonstrate that if d were less than t for the total number of degrees of freedom, then whatever might be the true variance-ratio of the populations sampled, the number of claims to significance, at any level, would be invariably greater than the fraction defining the level chosen; and this to a really rather large extent.

I enclose a short note[1] of a demonstration of this kind, though, of course, I have many years ago given effectively the same analysis; and wonder whether you think that in the atmosphere of concreteness which should be produced by the production of actual tabular values, it would do anything to prevent all but the wildest asses from deceiving themselves when they have data of this sort.

[1] See *CP* 264.

Fisher to G.A. Barnard: 7 *October* 1955

I have been hoping to hear from you about the miscellaneous material of mine in your hands, for which the paper on Welch's table will not take you long, and I hope you will send it soon to Frank Yates.

The draft of chapter five I should also be glad to have back, so as to revise it adequately before it is set up in print.

G.A. Barnard to Fisher: 12 *October* 1955

I have been a long time answering your letter about Welch's test, because I found myself disagreeing with you. And experience teaches me that when I do this I am wrong.

But it seems to me that what you have proved is that in the conditional set in which s_1^2/s_2^2 is fixed at unity, the relative frequency of judgements of significance, at the 10 per cent level, is greater than 10 per cent, when the null hypothesis is true. This does not exclude the possibility that in other conditional sets, where s_1^2/s_2^2 is fixed at some other value, the relative frequency of such judgements may be less than 10 per cent; so that overall, when s_1^2/s_2^2 is allowed to vary, the relative frequency may have the required value of 10 per cent.

I have looked up the literature and found a paper of Aspin and Welch (*Biometrika 36*, (1949) pp. 290–296) in which Welch gives the following figures for the probabilities of significant (according to his test) positive values of d, depending on $\gamma = \sigma_1^2/(\sigma_1^2 + \sigma_2^2)$:

γ:	0.1	0.2	0.3	0.4	0.5
P:	0.0501	0.0500	0.0500	0.0498	0.0498

These were obtained by coarse-meshed quadrature, so that the last digit is not reliable. (For two-tailed tests, the figures should be doubled.)

However, using Welch's test, we are left with the paradox that, with $s_1 = s_2$, we can judge that a given result significantly contradicts the hypothesis of equal means, while the same result fails to contradict the hypothesis of equal means and equal variances. We therefore seem to be in a situation where we can believe A, but not B, although B is a logical consequence of A.

If we require that a fixed level of significance should correspond to a fixed frequency in repeated samples, of judgements of significance when the null hypothesis is true, then such a paradox as this one inevitably arises whenever we have a test of a wider hypothesis (B), and of a narrower hypothesis (A), unless these two tests happen to be identical.

We might try to get round the difficulty by saying that B, being a wider hypothesis, should be tested at a higher level of significance than should A. But this does not seem to me at all convincing.

I am sending your Chapter V by separate, registered post. I found it *very* meaty. . . .

Fisher to G.A. Barnard: 13 *October* 1955

Thank you for looking at my note on Pearson's table, and for your comments thereon. Unless I misunderstand you, to a user who has observed $s_1 = s_2$, and has calculated his value of d, and later complains that the value tabulated by Mrs. Aspin on Welch's formula would make him claim significance with a frequency of more than 12%, the reply is, if I have you right, 'Don't you worry about that! If s_1/s_2 had been some other value, the table would have led you to reject the hypothesis in less than 10% of trials'.

To the experimenter who actually hopes to learn from his data, this is just as much as to say 'The value tabulated for v, or d, is too low in the case you have encountered, but in other parts of the table this is compensated by tabulating a value too high'.

What Welch and his colleagues have failed to realize is that the table is entered with the ratio s_1/s_2, so that each tabular value must be judged right, or wrong, according to the subset of possible experience specified by that tabular entry. The rider that it is not entered with the ratio σ_1/σ_2, this being unknown, might, of course, suggest that the subset of possible experience represented by a fixed ratio σ_1/σ_2 is irrelevant to a significance test.

I have tried to make simple points simply, because as between people who wish to understand each other it is important, so far as possible, to take such things one at a time.

I hope you will have another shot if you think I am missing anything. Of course, I do not believe there really is compensation in other parts of the table, but it might be nice to know in what parts to look for undue insensitiveness, which is supposed to counteract (though to the user it does not) the undue laxity in those parts of the table to which I have pointed.

[P.S.] I suppose you have now sent my note to Frank.

G.A. Barnard to Fisher: 13 *October* 1955

I enclose Chapter V, and would like to say I feel privileged to have been able to see it at this stage. I also hate to let if go, because I am finding so much food for thought in it. Would you please let me see a copy as soon as you can spare one for good?

One thing I found especially salutary was the interpretation of fiducial statements about parameters as limiting forms of fiducial statements about observations, serving to emphasise the lack of valid distinction between fiducial probability and other 'kinds' of probability.

I do just wonder whether the fiducial distribution of μ, given on p. 10, may mislead. You say (near the bottom) 'the equation

$$\mu = \bar{x} - st \tag{76}$$

thus specifies the fiducial distribution of μ. . . . . .'.
In inserting 'thus', it seems to me you suggest that the deduction is immediate; whereas, if I have rightly grasped p. 15, the rigorous step-by-step derivation here would require, first the distribution of σ, then the joint distribution of μ and σ, and thence the marginal distribution of μ. Am I right?

Other thoughts will take time to find a brief expression.

Fisher to G.A. Barnard: 14 *October* 1955

Thank you for your second letter, about chapter five. After equation (76), $\mu = \bar{x} - st$, I am inclined to say 'supplies a basis for a rigorous fiducial argument giving the distribution of μ'. In a few years time I hope this may sound over-formal, but at present, with people like Tukey ready to get deeply confused, I suppose one cannot be too careful.

I am inclined, also, to follow the three 'unsolicited testimonials' to Welch's solution by the additional comment:

I have no doubt that the error of these calculations consists in ignoring the fact that the Table is entered with s_1/s_2, or some equivalent function, and therefore that each tabular entry can come into use only in that selection of cases in which this ratio is realized. The frequency distributions of v (or d) at other ratios, or mixtures of them, are therefore misleading for each particular entry used. [*CP* 264, p. 59.]

Re-reading your letter I do not think the marginal distribution of μ requires to be based on the joint distribution of μ and σ, for the inequality

$$\mu > \bar{x} - st_p$$

has a definite probability of being realized in the set of all values of μ, $\bar{x}$ and s appropriate to any sample, and if it is admitted that no subset can be recognized having a different probability, and to which the observed sample certainly belongs, (as can scarcely be disputed since $\bar{x}$ and s are jointly sufficient and it is postulated that no information *a priori* is available), the distribution of μ follows from that of t, as indeed 'Student' recognized, though naturally enough he was not very easy about the mode of expressing this inference.

G.A. Barnard to Fisher: 17 *October* 1955

Thank you for you letter of the 14th, and that of the 13th, which make all clear, on Welch's test.

Now that you have brought it out, I am reminded that in my 1947 paper I quoted Yates' 1939 paper[1] on this very topic, indicating why one was concerned with fixed s_1/s_2! But, as you say,—and my mistake, I think, proves it—you can't be too careful.

There is a connection, isn't there?, with the 2×2 table, where we fix the marginal totals for much the same reason as we fix s_1/s_2.

But now I think that what you have proved proves more—in fact, we can surely get Wilks' result, that if we require fixed frequency of wrong judgements of significance, in the conditional sets s_1/s_2 fixed, then we cannot have this in repeated samples of the same size. For, from the form of the likelihood function, any admissible test criterion must be based on $\bar{x}_1$, $\bar{x}_2$, s_1, and s_2 only; that is, it must be a function of $d = (\bar{x}_1 - \bar{x}_2)/(s_1^2 + s_2^2)^{1/2}$, of s_1/s_2 and perhaps of $\bar{x}_1$ and s_1. (Since any function of the four quantities $\bar{x}_1, \bar{x}_2, s_1, s_2$ is expressible as a function of the four quantities d, s_1/s_2, $\bar{x}_1$, and s_1). But by a shift of common origin and change of scale we can make $\bar{x}_1$ and s_1 take any value we please, while d and s_1/s_2 are invariant. Hence, since our test must be unaffected by shift of origin and change of scale, it must depend on d and s_1/s_2 only. And if we fix s_1/s_2, you have shown that the distribution of d depends on σ_1/σ_2, in repeated samples of fixed size. Thus we must either give up the insistence on fixed sample size, or we must give up fixed s_1/s_2, or some other requirement of equal necessity.

I shall write again about the fiducial distribution of μ, given $\bar{x}$ and s, because I am still not quite happy about that. I am wondering whether we ought not to require, in the first place, that there should be a pairing of the sufficient statistics with the parameters, in such a way that one sufficient statistic only is involved for each parameter—like s with σ; then we may get further fiducial distributions by using those we already have to get rid of nuisance parameters. But I will try to formulate my difficulty more precisely before asking you to help me with it.

[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 G.A. Barnard: 21 *October* 1955

I am exceedingly glad that the extra paragraph made the point clear, though, of course, it is only a guess that the various people I quoted have actually overlooked the necessity, for the purpose of a test of significance, of calculating the frequency in *pari materia*.

Logically it seems to me, as to you, to be exactly the same criticism as should be made against the use with the 2×2 table of frequencies based on varying marginal totals, for judging the significance of a case having marginal totals known. It is rather like mixing in some data from rats in testing the significance of an infection, or prophylactic, applied to mice. . . .

Fisher to G.A. Barnard: 14 *March* 1956

. . . Have you sufficiently wrestled yet with that sixth chapter? The last example should be critical for the understanding of people like Tukey and Savage who have been writing as though any sort of Jacobian transformation would do. I imagine that reflection on the examples I have given of introducing parameters not only one-by-one, but sometimes two-by-two, may suggest to you conditions of the legitimacy of such an operation.

However, probably you are horribly busy.

G.A. Barnard to Fisher: 15 *March* 1956

. . . I have fixed to come to Cambridge for a day or two about the weekend of April 14th, and as you suggested I will ring your secretary a little before then. . . .

G.A. Barnard to Fisher: 24 *April* 1956

I am still unhappy about linear functions of the cell frequencies. In the case of a continuous variate, the sum of squared deviations from the sample mean is expressible as the difference between the sum of squares, and the squared sum divided by N. Both the sum of squares and the sum are linear functions of the cell frequencies, but squaring the sum introduces non-linearity. However, it does not introduce discontinuity. Of course, you allow non-linear functions of a single linear function of cell frequencies. But this example seems to show that you need functions involving more than one linear function.

If one allowed continuous functions of a finite number of linear functions of the cell frequencies, these difficulties would seem to be overcome. But just why one should make precisely this limitation is still not clear to me, though I can see one can't allow entirely arbitrary functions. . . .

Fisher to G.A. Barnard: 25 *April* 1956

So far as developing a mathematical theory is concerned, I have no objection to offer for the use of non-linear functions of the frequencies, though I think one does require that the functions of the frequencies used shall be so definable as to have a meaning when non-integral, or irrational, expectations are to be substituted for them. I do not suppose, however, that this offers any difficulty.

Of course you are right in saying one may have functions of many linear functions. The stipulation of linearity in the frequencies refers to that part of the statistical process in which the evidence of a *series of frequencies* is to be combined. Once they have been combined in one or several linear functions, I see no inconvenience in combining these in any chosen way. My point about linearity is merely that I see no prospect of any function that is not built up of such linear functions being of any use or interest. I might easily be wrong without having injured the development of the theory, which of course I did develop at first without considering that functions linear in the frequencies had any particular rationality.

For a student, I should say that the frequencies are peculiarly related to one another in that they are counts of mutually exclusive occurrences, and therefore that the whole sequence of frequencies must be considered together in relation, usually, to a fixed total. Measurements may be regarded as frequencies of unity in rather specially defined classes, and though the number of measurements is relevant, they are, as frequencies, a series of frequencies of one, which may, or may not, occupy the same class. I think you only get discontinuity if you were to apply a non-linear function of the frequencies (such as Pearson used long ago in demonstrating, with much sound and fury, that least squares was not the right way of fitting frequency curves, i.e. least squares of frequencies!) to such frequency classes defining measurements. The point is that a frequency distribution is an organic whole corresponding more or less with one or more measurements, but not particularly with N measurements if N is the size of the sample, but really it scarcely corresponds with exact measurements at all.

Fisher to G.A. Barnard: 20 *August* 1956

It is a pleasure that I have been looking forward to, to send you an early copy of this book, which I believe will be on sale in September. I have not noted, so far, a great many misprints, although there doubtless are many more than I have seen. . . .

By the way, I should rather like to know if any of the people, and they are doubtless many, with whom you have argued about fiducial probability, have been misled by the misapprehension that I tried to remove on page 120,[1] i.e. think that a fiducial probability statement about unknown parameters, because it has the same form as it would have if such parameters had been chosen from a super-population, asserts or implies that such a super-population is a historical reality, which would be nonsense seeing that we know nothing of the origin of the population sampled, but we do know something, and that something expressible in terms of probability, about its character. Casting my mind back to a period more than twenty years ago when, under Neyman's influence, Bartlett was attacking the theory, it seems to me that people like Bill Cochran may have been largely influenced by this misapprehension. It may, however, have been quite without influence in more recent discussions in which I have taken no part, for at a fairly early stage I formed the opinion that the number of tolerably good mathematicians who were then in the subject might safely be left to get their thoughts straight by themselves and without my continually yapping, for it seemed to me unavoidable that they would have much to withdraw, and such withdrawals are made most gracefully when made spontaneously. In consequence of this, I have not heard perhaps so much as I should have of the points which may now be thought logically influential. Arguing during the last year with John Tukey, I encountered a most impenetrable mass incorporating every sort of fallacy imaginable, and I cannot tell whether I have had any success in loosening it up.

Anyway, as these discussions are likely to become active, perhaps you will let me know what you can.

[1] *SMSI*, p. 124.

G.A. Barnard to Fisher: 7 *September* 1956

I am writing to thank you most warmly for the copy of your new book, and for the most kind letter enclosed. I have not had the chance to study it in peace as I would like, so far. . . .

I have had some discussion with John Tukey this summer, from which it seems to me you must have cleared his mind a good deal on the fiducial argument. . . .

G.A. Barnard to Fisher: 17 *January* 1957

May I say how glad I am to hear you are back, and to thank you for the very pleasant evening you gave me just before you went away. I hope you and your daughter enjoyed your stay in Calcutta.

A point has come up while you were away which has led me to feel that the notion of group invariance, or some other way of expressing ignorance about a parameter, is essential to the validity of the fiducial argument. You may remember my putting forward the idea some time ago that we could express our ignorance of μ, in relation to the estimation of σ, by requiring that our estimator should be invariant under translations (i.e. changing x to $x + a$).

To apply this idea in a wider context, we might say that, in order to be able to carry through the fiducial argument, we not only need to be in ignorance about the parameters in question, but also we need to be able to give this ignorance a precise mathematical expression. One such way is to be able to point to a group of transformations operating on the set of possible parameter values, under which our analysis of the situation must be invariant.

The specification of a continuous group of transformations on the set of possible parameter values would imply the unique specification of a measure function defined for sets of parameter values. And it would be natural to express the primary likelihood inference from the data by integrating this likelihood function with respect to the measure. In this way we would obtain a probability, which could be regarded as the fiducial probability distribution of the parameter.

The reason why I think some such requirement is necessary is that Lindley has shown that the following is possible; we have a sample S, made up of two sub-samples S_1,S_2. S gives a fiducial distribution $F(\theta)$ for a parameter θ. S_1 gives the fiducial distribution $F_1(\theta)$, and when we use F_1 as a prior distribution, in relation to S_2, we obtain the distribution *a posteriori* (i.e. given S_2) $G(\theta)$. Now we would expect that $F(\theta)$ should be the same as $G(\theta)$, but Lindley shows that this is not the case in general. I can show that it will be the case if we impose the requirements about group invariants.

I am afraid this is a long letter, which you may not find at all clear. I wonder if I might now come to Cambridge to see you about it? . . .

Fisher to G.A. Barnard: 18 *January* 1957

Thanks for your letter, which indeed deserves more consideration than I shall give it before my next lecture at 12 o'clock. I have seen a rather abusive review by Lindley, which is indeed, I think, the second he has written, in which he speaks with great confidence of detecting a mathematical error which overthrows the notion of fiducial probability. My work, like that of others, is liable to contain mathematical errors, and his criticism has led me to a revision of one example, but he is mistaken in thinking that it affects the

logical status of fiducial probability, which is, I believe, the only form of probability about the real world which can be inferred from quantitative observational material.

Of course, the overruling requirement of sufficient or exhaustive estimation imposes analytic restrictions on the problems for which fiducial probability can be calculated, and it may be that Lindley has been rediscovering these restrictions under other forms and expressing them in other language. The fallacy of Lindley's argument lies in the fact that the fiducial probability, derived from sample S_1, is only valid if there is no means of recognising subsets in the reference sets used for probability statements. It is certain that a second sample makes such recognition possible, just as information *a priori* might do, and it is in each particular case a matter for investigation whether the fiducial probability distribution is thereby altered. The example on the question of observations of two kinds (Section 6, page 123)[1] is a mistaken one just because there is no exhaustive method of estimation for the two kinds of observation concurrently. I do not think it will be difficult to think up an example free from this defect to exemplify the position (a) that different types of observation lead to statements at different levels of uncertainty, and (b) that the combination of data of these two kinds, when a rigorous small sample combination is possible, will yield statements of probability (i.e. of the higher quality) with the quantitative force of both sets of observations together.

I hope I have not expressed this too obscurely.

No, your letter is not either long or obscure. I do not understand what you say about 'group invariance', but I presume that it is a reappearance of the analytical requirements of exhaustive combination of data.

[1] *SMSI*, p. 127.

Fisher to G.A. Barnard: 22 *January* 1958

I believe some time ago you expressed the intention of answering Lindley's review in *Heredity*[1] of my new book. Some people seem to be expecting that I should do so myself, but I should prefer not, since the review is so full of mis-statements about it, e.g. on his first page:

> 'The fourth chapter is concerned essentially with the problem of what space we should integrate over in forming our significance levels . . .'

which is merely an evasion of the straight exposure in that chapter of the false criterion advocated by Neyman, Pearson, and Bartlett as a ground for doubting the exactitude of Behrens' test of significance.

And of course at the end of this paragraph also the strong definition of consistency and the proof of efficiency (why asymptotic?) of the maximum likelihood estimate are neither of them new.

Of course the rest of the article is equally chaotic, e.g. on page 282 all chromosomes are apparently known *a priori* to be equivalent. The important point, however, is that the second paragraph on page 281 is not impugned by the bad example I took in Section 6 of Chapter V, of which I believe you have already seen the replacement which proves the same point without the original fault. . . .

[1] *Heredity* 11, 280–3, (1957).

Fisher to G.A. Barnard: 8 *February* 1958

I have been reconsidering the example in Chapter V, Section 6, to which Lindley took so much exception, and I see that he has put on it an interpretation which the example as it stands will not at all bear, for it treats specifically of the combination of data of two distinct kinds, of which one *ex hypothesi* is incapable of supplying probability statements.

In fact the more I consider it, the more clearly it would appear that I have been doing almost exactly what Bayes had done in the 18th century. As Lindley purports to be a protagonist of Bayes, it would seem that his misunderstanding and confusion goes deeper than anyone could imagine.

In fact I am not inclined in the second edition to alter a word of the section, but to add to it a short further explanation of which I enclose a copy. Anyway, I should be glad to hear from you.

G.A. Barnard to Fisher: 27 *February* 1958

. . . About Lindley, and his review, I must say I had hoped that some editor would have given me an opportunity to review your book, so that an indirect reply to Lindley would have been possible. But this has not happened, so that one is left to choose between a letter to *Heredity*, and an article or series of articles, perhaps more suitably published elsewhere. My feeling is that the latter would be preferable, because the bases of Lindley's misunderstandings lie too deep for treatment in a short letter, or reasonably short correspondence. And the important thing is that others . . . have fundamentally similar misunderstandings of principle.

That this is so seems to emerge from a series of discussions we have held in our colloquium, in which a series of people, mainly the 'younger generation' have been invited to give their views on your book. Almost everyone agrees with your criticisms of Neyman and Jeffreys, and with at least a part of your general theory. But nearly all become highly convoluted because (I think) their mathematical training has followed the now fashionable trend towards axiomatics to such an extent as to make them sometimes incapable of following a semantic argument.

This makes me think that the next step, for me, is a series of articles, covering the general field as far as possible, in which I would try to go meet

these mathematicians, by using their language and notation as much as possible. I gave a course of lectures last term along these lines which Jenkins (who was trained by Pearson) said cleared up a good many things for him. And I hope things will ease a bit here after Easter, so that I could get some sections drafted. I would hope very much to have the benefit of your criticism of them.

Apart from exposition of your ideas, I have in mind a few new things. For one, I have made a detailed analysis of the concept of a 'long run'. According to Neyman's followers, all statistical procedures are to be considered in relation to 'the long run'; but the idea is not at all as simple as they seem to think. It is worth analysis, I think, because it is relevant to some technological applications. And incidentally it seems to throw some further light on Behrens' problem.

Another idea is that one can give a 'consistency proof' for the fiducial argument in a wide class of cases—rather as one could give a consistency proof for the law of excluded middle in mathematical logic, a la Hilbert.

With regard to your second letter, I agree there seems no need to change what is in the book, though your addendum will be illuminating, I think. I should perhaps suggest that after 'The percentile values' in line 10, 'of the distribution *a posteriori*' should be inserted, since this will serve to avoid confusion with the percentile values referred to a little before.

Fisher to G.A. Barnard: 3 *March* 1958

It is extremely nice to have your letter and to hear what you have been thinking. What might have been a storm in a teacup seems to be becoming a world war. . . .

In January of last year I received from Lindley a proposed review of *S.M.S.I.*, which was, I believe, materially different from that which later appeared in that journal (*Heredity*). If you have a copy of the original I should be immensely glad to have it, for in any future discussion it is important to know how his mind works (one can allow for mere brashness but not for all the convolutions of the cortex), and I think his change of attitude during last year, perhaps influenced by discussions with you, might help me a good deal.

I am discussing these problems, of course briefly, with the Kapitza Club here on Tuesday evening.

When are you coming to dine with me?

G.A. Barnard to Fisher: 6 *August* 1958

Several months ago you asked me for a copy of the first draft of Lindley's review of your book. I spent some time looking for it but could not find it till now. I expect it is much too late for your purpose, but I am enclosing it just in case you may have use for it. I marked in pencil some of the places where I hoped to persuade Lindley he was wrong fairly easily; but I was wrong, and he was more impenetrable that I expected. . . .

Fisher to G.A. Barnard: 16 *August* 1958

Thank you for sending me the script, which has been less altered than I had thought. . . .

You might like to see the address I have undertaken to give in Brussels in connection with, I believe, the Société Adolphe Quetelet, if not the International Statistical Institute, more or less entangled with an International Pharmaceutical Congress. You may be interested to see the new look which can be given to Bayes' celebrated theorem.

[P.S.] The *Centennial Review* of Michigan State College has a rather similar paper [*CP* 272] on The Nature of Probability being also, of course, a lecture. I will send an offprint when I have one.

G.A. Barnard to Fisher: 16 *October* 1958

Stuart Hunter, no doubt on the prompting of George Box, has sent me your paper about mathematical probability in the natural sciences,[1] for me to referee as a joint editor of *Technometrics*. I am glad of this because it gives me the chance to write to you and say—as I have had to say so many times before—that on reflection I think you are right. I would like to withdraw completely what I said in Brussels and to apologise for making myself a pest.

I tried to consult Bayes's paper in Brussels, but found that their files of the Royal Society *Transactions* go back only to 1831, so that it was not until I was able to read the printer's proofs of the forthcoming reprint in *Biometrika*[2] that I could confirm the accuracy of your account of Bayes, and the inaccuracy of my own recollections. I also looked up Gauss and here again found myself in the wrong. The only thing I could plead in connection with Gauss is that even in his early work of 1809 he is clearly not entirely satisfied with the use of prior probabilities, since he says later on that the principle of least squares should itself be taken as an axiom.

I wonder if, as assistant editor, however, I might suggest two small changes in your paper. On page 4 you say 'for it seems to be only in mathematical departments insulated from practical research in the natural sciences that confusion and misapprehensions abound'. I feel that this statement, while undoubtedly true, is open to misinterpretation and I wonder therefore if you feel it could be deleted without serious loss. Second, on page 15, line 9 the phrase 'of two types' might be misinterpreted by someone who felt he was not abreast of the latest news on fundamental particles. Perhaps 'by two methods' would be better than 'of two types'.

Hunter has suggested that we might institute the practice in *Technometrics* of printing some discussion along with the paper, rather as the read papers are printed in the R.S.S *Journal*, and he has asked me if I would care to draft something in connection with your paper. With certain sorts of paper I think this would be an excellent idea, but with yours my first reaction is that I could do nothing but gild the lily. If pressed I could I suppose expand the historical references a bit and I might make a few additional comments concerning the definition of probability. I would be very glad to know how you would feel about such a proposal.

[1] See *CP* 273.
[2] Bayes, T. An essay towards solving a problem in the doctrine of chances. (Reproduced from *Phil. Trans. R. Soc.* **53**, 370–418, (1763)). With a biographical note by G.A. Barnard. *Biometrika* **45**, 293–315, (1958).

Fisher to G.A. Barnard: 17 *October* 1958

Thank you for your letter, which was indeed very welcome to me as I did not really want a longish controversy in print, or even in correspondence, involving long quotations in German, or for my part preferably in Latin.

About Gauss, you probably have the paper of mine from the Cambridge Philosophical Society of 1930 in which I say a fair amount about what I think his argument was, though not of course right on the point now in discussion. I should be interested to know if you find yourself in agreement with these earlier thoughts.

Of course I agree with you that Gauss used the word 'probability' to cover what I later called 'likelihood', being influenced, I have no doubt, by the very wide extension of the meaning of the word current in the early nineteenth century on the strength of Laplace's comprehensive definition. This penumbral meaning of the word, though quite unsuited for exact or mathematical thought, has been accepted a good deal too easily by some mathematicians, as by those who argue that we do not lack knowledge *a priori* seeing that we usually have some vague information before we start arguing seriously. What a mathematician should emphasize is that the probability *a priori* required by Bayes' theorem, and without which Bayes' theorem cannot be applied, is an exact and complete specification of the unknown as a random variable, and this no-one thinks that he possesses unless he has found it experimentally.

About your changes, the second is easy, though hard to find since it is on page 15 and not page 12 as you say! I have altered 'types' to 'sorts'. The first one is considerably more difficult. I do not want to be argumentative, but I do regard the position as serious, and it will certainly remain serious unless the self-satisfaction of some mathematical departments is shaken, for, as nineteenth century experience shows, it is not easy to reform anything so highly traditional as mathematical teaching. In my paper the phrase, like that with which the paper ended, was an attempt to warn pharmacists in particular that they could easily be led up the garden path by confident young men apparently well-qualified as mathematicians. I do not like to think of the standardization of drugs, the issue of prophylactic sera, or the aiming of ballistic missiles, being influenced by the arguments of which *Biometrika Tables* No. 11 is a concrete product. I certainly owe a duty to my scientific friends to inform them of what they probably would find unimaginable, of the levity with which make-believe is accepted in some mathematical departments, even when they do not think they are joking.

I do not of course say, or, I hope, seem to imply, that in all mathematical departments the Neyman fog has settled in, but that it has settled in only in those departments which are insulated from practical research in the Natural Sciences, and that the remedy is that, at least in all departments teaching mathematical statistics (other branches of mathematics must look after themselves), people with *real* experience of the fields of application are needed. Remote control consultant work is only a first step to understanding the function of statistical methods and their applications. Hence I should like to think more and discuss further with you your first suggestion.

Of course I should welcome the kind of commentary you suggest to follow my paper. Reviewers of my book have rather curiously found nothing new in it to comment on. . . .

G.A. Barnard to Fisher: 28 *October* 1959

I have been very glad to hear, at third hand through George Box, that you are enjoying life in Australia.

A point has occurred to me, in connection with the fiducial argument, which so far as I know has not been touched on by you in published work. Suppose we are measuring (observing?) a length x which is restricted to discrete values $\pm\, nh$, $n = 0,1,2, \ldots$ and suppose we take the distribution to be the 'discrete normal' with probability function

$$P_r\{x = nh\} = \{hK(h)/(\sqrt{2\pi})\}\exp\{-(nh - \mu)^2/2\}$$

(with $K(h)$ very nearly 1.) And suppose the parameter μ is also restricted to discrete values $\pm\, hr$, $r = 0,1,2, \ldots$ then it seems to me the quantity $(x - \mu)$ is pivotal, with

$$P_r\{x - \mu = rh\} = \{hK(h)/\sqrt{2\pi}\}\exp(-r^2h^2/2)$$

and, if nothing is otherwise known of μ, we can infer, from an observation x, the (fiducial) probability function

$$\{hK(h)/\sqrt{2\pi}\}\exp\{-(rh - x)^2/2\}$$

for $\mu = rh$.

Am I wrong?

I came upon this point after trying to give a rigorous account, for the *continuous* case, of the idea of sufficiency, likelihood, quantity of information, etc. One gets very much tangled up with the continuum hypothesis, non-measurable sets, and so forth; and it seemed to me one way to cut the Gordian knot would be to argue that all observable distributions are discrete. But then one seems to come against difficulties with the fiducial argument, unless one takes the point of view I have indicated.

G.A. Barnard to Fisher: 12 *January* 1960

Thank you for your letter.[1] If ever there is a chance I shall try to get the misprint with 0 and o corrected.

It is most exciting to hear that you have derived the simultaneous distribution of a set of correlation coefficients. I first made the acquaintance of the variance co-variance distribution from Wilks's book and perhaps because I was wanting it at the time I realised very well that there was a good deal further to go before one had the correlation coefficient distribution. I had one or two goes at it myself, especially when prodded by psychologist friends, but made no headway. Is there any way of seeing how nearly jointly normal are the 3 z-transformed correlations between 3 variates? Presumably the approximate joint normality will cease long before the approximate marginal normality, so that we would here have a case where normality of marginal distributions went along with non-normality of the joint distribution. This would have a certain personal interest for me in that the only inaccurate statement I ever found in the later editions of Yule's book was to the effect that normality of marginal distributions implied normality of joint distribution; a theoretical counter-example is easy to find but it is not so easy to find such a distribution in practice.

[1] Fisher's letter (probably handwritten) from Australia has been lost.

G.A. Barnard to Fisher: 8 *May* 1961

I am enclosing the papers which you left with me and have kept copies for myself, for which many thanks.

One point that arises in connection with the case where the observations O are obtained by counting (case B)[1] is that it would appear that an alternative way of converting the observations O to the form required to give a fiducial distribution would be to include the actual times at which the particles arrived within the period of observation. It seems to me one could then treat the interval from the beginning of counting to the arrival of the first particle as corresponding to the detector W, and then the remainder of the observation interval as corresponding to detector O. If I am right it would seem that although the information about the times of arrival of the particles contributes no information about λ in the likelihood sense, it does enable us to convert the likelihood statement into a probability statement. I hope I make myself clear and that I have not misunderstood the situation. I would like to discuss this and the other question when I come to Cambridge.

[1] See *CP* 289.

G.A. Barnard to Fisher: 13 *July* 1961

David Sprott, whom I mentioned to you as having written two papers concerning the connection between the likelihood function and the necessary form for the fiducial distribution, has come over here and we have been discussing a number of points in connection with the fiducial argument. We have reached a stage where we feel very much in need of your help and I wonder if we could come to see you on Monday of next week (the 17th) to discuss it with you.

If I may briefly indicate the nature of the difficulty it is this. At the end of *Statistical Methods and Scientific Inference* you derive the fiducial distribution for the normal bi-variate [parameters] ρ, σ_1, σ_2. At the same time you point out that one could use another set of pivotal quantities, one of which is the variance about the regression line, which might conceivably be misused to derive another 'fiducial distribution', for the same set of parameters. The difference between David Sprott (and incidentally Quenouille) on the one hand, and myself on the other, is that they seem to think that the second set of pivotal quantities are appropriate for a fiducial distribution of the parameters corresponding to them; but that this fiducial distribution is not transformable to a distribution of ρ, σ_1 and σ_2. For my part it seems to me that this second 'fiducial distribution' is just wrong, since when it is transformed in the normal way to its expression in terms of ρ, σ_1 and σ_2 it contradicts the validity principle.

This is just to indicate the kind of issue involved and I hope we may explain it more fully when we see you. . . .

Fisher to G.A. Barnard: March (?) 1962[1]

Your letter Feb. 15 was forwarded from India, and I have just read it. I think we have arrived at nearly indistinguishable conclusions. Let me now formulate my own position.

A pivotal quantity is a function of parameters and statistics, the distribution of which is independent of all parameters. To be of any use in deducing probability statements about parameters, let me add

(a) it involves only one parameter,
(b) the statistics involved are jointly exhaustive for that parameter,
(c) it varies monotonically with the parameter.

As you have observed, and as the last example in *Statistical Inference* was

intended to make clear, parameters and their corresponding exhaustive statistics may arrange themselves in strata.

For the normal sample s alone is exhaustive for σ, consequently the marginal distribution of σ is expressible in terms of s, using the pivotal s/σ. Both s and $\bar{x}$ are needed for exhaustive estimation of μ using $(\mu - \bar{x})/s$. In this case it is noticeable and probably essential that the two pivotals together each involves one parameter. *Jointly* they involve a set of statistics exhaustive for both. Each is monotonic in its parameter *uniformly* for variations of the other. Also, I should have stipulated that their simultaneous distribution is independent of all parameters.

That I think is enough. In the bivariate case s_1/σ_1 and s_2/σ_2 have a joint distribution independent of σ_1, σ_2 but not of ρ. However, for arbitrarily assigned ρ, they suffice to give the simultaneous distribution of σ_1 and σ_2 in an array (with ρ constant) and this suffices for the trivariate distribution.

For sets of pivotals then I add

(d) the joint distribution is independent of parameters (of as high or higher stratum)
(e) all are monotonic, uniformly for variations of parameters of as high or higher stratum.

See if you can formulate a justification for ignoring parameters of lower strata once their simultaneous marginal distribution is determined.

Fraser wrote hopefully about the trivariate case, but he may not see all the difficulties, e.g.

$$\frac{r_{12}}{\sqrt{1-r_{12}^2}}\chi_{N-2} - \frac{\rho_{12}}{\sqrt{1-\rho_{12}^2}}\chi_{N-1}$$

is distributed in a standardized normal distribution, and is uniformly monotonic in ρ_{12} but is unlikely to be distributed independently of

$$\frac{r_{13}}{\sqrt{1-r_{13}^2}}\chi_{N-2} - \frac{\rho_{13}}{\sqrt{1-\rho_{13}^2}}\chi_{N-1}\,.$$

In fact the distribution of r_{13},r_{23} conditional on r_{12} is not at all alluring.

I expect I told you I had run out the simultaneous distribution of r_{ij} given the system ρ_{ij} for any number of variables.

[1] This letter was published in Barnard, G.A. (1963). Fisher's contributions to mathematical statistics. *J. R. Statist. Soc.* **A 126**, 165–6.

M.S. Bartlett to Fisher: 25 *September* 1933

Though no doubt you have seen my paper in the last Royal Society *Proceedings*,[1] may I take the liberty of sending you a reprint? Since this paper was accepted, Jeffreys' latest paper in the *Proceedings* was published;[2] and

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