

[6 March 1931]

Dr Ragnar Frisch,
100 Howe Street,
New Haven, Conn., U.S.A.

Dear Dr Frisch,

I am much obliged for the copy you send me of your letter to Prof. Hotelling on the question of the terminology you advocate for the coefficients of the logarithm of the characteristic function. It is perhaps a pity that Laplace did not once and for all give a name to these coefficients, as well as to the function in the expansion of which they occur, which function is of course analytically of much the greater importance. Perhaps we may take it that he left it to the good sense of subsequent mathematicians to say explicitly what they meant, without quarreling about points of terminology.

The ^{concurrent} ~~commonest~~ use of alternative terms for the same objects does not in practice give rise to any inconvenience, and has the great advantage that each person may suit his own convenience; by this process moreover, the more generally useful term, in respect of brevity, of its descriptive quality and of the facility with which it associates itself with a wider terminological system, will in time inevitably come

into the more general use.

This, of course, is what we should all desire, in a philosophical spirit, even if our own special associations have given us a strong preference for one particular usage; and it is perhaps all to the good, that willy-nilly this is the process which slowly perhaps, but inevitably, determines the use of words in the English language.

Apart from its associations, which perhaps ought to weigh with a Scandinavian, I have, personally, always felt a certain awkwardness, both logical and linguistic, about the use of the term half-invariants and semi-invariants, and should be glad to see them replaced by more suitable terms, which should of course include a term for the function in the expansion of which they appear. Mathematically these coefficients are not in any sense invariant; and it is not very clear by what process of calculation the fraction half is arrived at! Linguistically "half-invariant", the use of which by Thiele gives it the strongest^r claim to consideration of the two, is what is called a bastard word, and for this reason will not be readily admitted into good usage. The term "semi-invariant" is an attempt to repair this defect, but is scarcely so euphoni^{ous} as to make up for its other deficiencies.

There is an added inconvenience if one is led to speak of the semi-invariantive function, which would seem to be the proper cognate term.

You do not develop in your letter any objections to the

terms cumulant and cumulative function, though one is led to feel that you have some strong objections which you do not mention. I must conclude, therefore, that your objection is based solely upon the undesirability of making a change in a usage with which you are already familiar. I should advise that whenever such a change of usage seems inconvenient, it should not be made, and then the newer terms will be used only by those who feel that they have real advantages. You will not, however, and I am glad to see that your letter does not, question the right of mathematicians to make use of whatever terms they find most suitable for the expression of their meaning.

Yours sincerely,

RAGNAR FRISCH

Minneapolis, Minnesota,
April 7, 1931.

Dr. R. A. Fisher,
Rothamsted Experimental Station,
Harpenden, Herts.
England.

Dear Dr. Fisher:

Thank you very much for your letter of March 6. Of course, I do not question the right of mathematicians to make use of whatever terms they find most suitable for the expression of their meanings. I have only expressed my personal opinion in the matter of the term "semi-invariants". I believe that no pressing need exists for introducing another term than this and that the mere fact that the term "semi-invariants" has been used for a long time is an asset with this term which weighs very heavily.

As to the term "cumulants", the term is not self-explanatory and I was not present at the meeting when Hotelling explained it at Cleveland, so I probably do not understand quite what it is meant to cognate. Does it refer to some properties of semi-invariants of a function of observations? Or, does it refer to the fact that these parameters are defined ~~by~~ integrals, that is, by "cumulation"? In the latter case it seems that the term cumulation is just as proper in connection with the original movements as with anything else.

Whatever the situation may be, in this respect, I must admit that I have still some difficulty in seeing the necessity of a change in the present case.

I have recently received a letter from Dr. Craig who is of the same opinion as me in this matter.

Sincerely yours,

Ragnar Frisch
Ragnar Frisch
Visiting Professor pro tem
of Economics at the
University of Minnesota

School of Business Administration
University of Minnesota
Minneapolis, Minnesota

Copies of this letter are being sent to Professor Hotelling and Dr. Craig.

CORA HOTEL,
A.A. RECOMMENDED.
UPPER WOBURN PLACE.
LONDON, W. C. I.

TELEGRAMS:
AQUACORA, LONDON

TELEPHONE:
MUSEUM 4472

March 9, 1934

Dr. R. A. Fisher
Rothamsted Experimental Station
Harpenden, Herts.

Dear Dr. Fisher:

I have been lecturing at the London School of Economics and will visit Cambridge and Oxford next week. During this trip I should also very much like to have an opportunity of meeting you and seeing on the spot ~~how~~ your ingenious statistical methods work in practice. There is also a paper that have been presented to Econometrica criticising your methods, and which I would like to talk over with you before accepting.

If convenient to you I could come to Rothamsted either Thursday March 15 or Friday March 16. The latter day would suit me best. I do not expect to take much of your time, but I should much appreciate it if you could arrange it so that one of your assistants could devote some time to going through with me a series of examples of actual data exhibiting the use of your various methods (including the use of your special tables and graphs for judging significance). I am prepared to spend the most part of a day on this. Even so the time will of course be very short, I there-

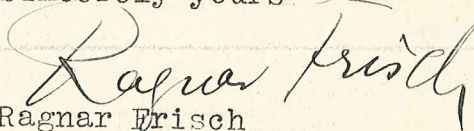
fore do not expect to go into proofs of the methods (I have read a number of your papers previously and will later check up on what I find necessary). At the moment I am only ~~interested in your~~ trying to get an idea of the successive computational steps needed when your various methods are employed.

If you are interested we may also have a chat on some methods of computation which I have lately been developing in my laboratory, for instance certain methods of handling determinants.

Will you drop me a word at the above address.

With best personal regards

sincerely yours


Ragnar Frisch

12th March 1934.

Dear Professor Frisch,

Last year I was asked to succeed Professor Pearson at the Galton Laboratory, and I have been here since the autumn. I hope my letter may catch you in time to prevent an unnecessary visit to Rothamsted, though Mr. Yates, who is now in charge of the Statistical department there, is excellently fitted to show you the methods of computation used in practice.

I have no assistant statistician yet, who knows much of methods, but I should be very happy to see you when you are again in London, and to discuss with you points of theoretical or practical interest.

Yours sincerely,

19 March 1934.

Prof. R. Frisch,
Cora Hotel,
Upper Weburn Place,
W.C. 1.

Dear Professor Frisch:

I have looked through Arne Fisher's paper and, as you asked me, am sending you my comments. The paper seems to have two objects, which might perhaps with advantage be kept distinct; one is to establish the merit and priority of certain work by Thiele and subsequent work as influenced by him, especially in recent times Bertelsen and Arne Fisher, and the other is to deplore the influence in contemporary America of some recent English work with which the critic is much less familiar. The confusion of the two subjects weakens the paper through the author's anxiety to say at the same time (1) The formulae of which modern writers are so proud are to be found identically in Thiele's writings and (ii) The modern work is erroneous in comparison to Thiele's. The confusion is also, I suggest, generally undesirable in making it difficult to refer, with ordinary generosity to a distinguished earlier writer, from whom one may occasionally differ, if, in such references one is forced to dissociate himself from the kind of well-meaning but indefensible claims which a modern

writer like Arne Fisher has put forward on his behalf. For example it seems proper to give Thiele credit for first giving the formulae corresponding to the partitions (2^2) , (2^3) , (3^2) and (4^2) but rather childish to make a song about the formulae for l^x which must have been familiar at least since Laplace introduced the characteristic function, or about the mean values of μ_2, \dots, μ_6 , i.e. of the primary series of statistics with which he chose to work. But the set of results given by Thiele can scarcely be claimed as giving the analytical expression in general for partitions of the type r^1 , still less for the more general partitions in which the parts are unequal. When I wrote my paper on the solution of the general problem by partitional methods I was surprised in looking through the works of Tschouproff and Church to find how few seemed to have been given by previous writers, and, had I known of it, should have been glad to cite Thiele amongst the others as the earliest writer who had realised that exact formulae of this kind could be derived by direct algebra.

Again I think it is now clear that Thiele's system of presumptive values is not the same as my series of k statistics and the implication that it is the same does no credit to Thiele, for his intention was clearly different from

mine. His principle in making presumptive values was to choose those values of the parameter for which the observed values of his h statistics would be equal to their expectations and this is not the same as the definition of a presumptive value given as Tschouproff's on p. 6. I imagine that Thiele knew what he was about and did what he wanted to do, whereas Bertelsen seems to have done as I did and chosen statistics having K as their mean values. Why Bertelsen did so is not explained, and should be of interest, but the implication seems to be that he was aiming at an improved method of estimation, and in consequence fell into the greivous error, of which I alone am accused in the numerical example in the attached sheet. Bertelsen's aim must evidently have been different from mine since he was a good boy to do in 1927 what was very naughty of me in 1929.

As far as I am concerned anyone who troubled to read my papers would know that I was not concerned with estimation at all, first because, in the paper on the theory of estimation, I am particular to dismiss the question of bias at an early stage as of no interest to the theory and in the paper on moments of moments I go out of my way to point out that moment functions only provide statistical estimates of high efficiency for a special type of distribution (p. 200) with a reference to the paper in which I first demonstrated their great inefficiency for all of the Pearsonian types of

curve which depart at all widely from the normal. My interest in the formulae arose solely from the facts that they can be made exact and general for all distributions, that very few of them had been previously given, and these in very complicated forms, and that no general notions seem to have been developed as to their classification, method of derivation in the more complicated cases, or extension to multivariate distributions. In fact what I had done was just what Craig in a paper published nearly contemporaneously suggested needed to be done if the formulae were to be brought within manageable compass.

The quotation given by Arne Fisher in translation from Thiele's paper 1889, bottom of p. 5, is of interest as showing that Thiele also was only partly concerned with estimation and that he was quite aware that in the current state of the subject he was unable to give any satisfactory account of the principles on which methods of estimation should be based. It seems to me much more to the credit of a man of science that he should be aware of the limitations of his own knowledge and of that of his age than that he should be represented as opposed to the very advances of which, as an active thinker, he must ^{have} felt the need.

I hope your voyage to Oslo was not so rough as seemed likely.

Yours sincerely,

RAGNAR FRISCH

SLEMDALSVEIEN 98
OSLO, NORWAY

March 23, 1934

Dear professor B.A.Fisher

Thank you very much for your prompt and full consideration of the Arne Fisher Ms. I am going away on a vacation tomorrow, as soon as I am back I shall again communicate with you in this matter.

Best regards

Yours sincerely

Ragnar Frisch

30 April 1934.

Professor R. Frisch,
University of Norway,
Oslo, Norway.

Dear Sir,

Professor Fisher would be obliged if you would return to him as soon as possible the type-script of his paper on "Z Distribution". He hopes that you have quite finished with the paper by now.

Yours truly,

(Secretary.)

18 May 1934.

Professor R. Frisch,
University of Norway,
Oslo, Norway.

Dear Sir,

On 30 April I wrote to you asking if you would kindly return to Professor Fisher the type-script of his paper on "Z Distribution". As I have received no reply, I am wondering if you received the letter. Professor Fisher would be much obliged if you would return this paper to him as soon as possible.

Yours truly,

(Secretary).

RAGNAR FRISCH

SIEMDALSVÆIEN 88
OSLO, NORWAY

May 231 1934

Professor R.A.Fisher:

Dear professor Fisher,

I have to apologize for not having answered before your secretary's letter of April 30. It slipped into the wrong pile and was left without being attended to. Your typescript is enclosed.

I wonder if this material has been published. If it has not and you think it would be interesting to present it in this form I should be very glad to have it appear in *Econometrica*. Possibly you could write a short *introduction* with this particular *group* of readers in view. The part of your typescript which I have found the most interesting was the last part (from the subheading "The probability integral of the distribution of z "). Possibly that part could be published separately. I was particularly interested in this because I see that it leads to the incomplete sum of the point binomial, on which I spent considerable time and effort some eight years ago, arriving amongst others at limits between which the integral must lie. I think I told you this when we had our conversation in London.

If you do not wish to publish in *Econometrica* anything along the lines of this typescript, possibly you have something else that may be suitable for our columns. I should be very pleased to have a paper from you appear in our journal.

With best regards
Sincerely yours

Ragnar Frisch

July 23, 1934

Dear professor R. A. Fisher.

It does not seem likely that I shall ever receive from R. A. Fisher any contribution on the work of Thiele and yourself which is held in an academic form that will make it suitable for *Econometrica*. Your kind and elaborate letter to me of March 19, indicates however that there are many interesting things that could be said about this subject and which would be a very interesting contribution to the columns of *Econometrica*.

I am therefore writing you today to ask whether you would be willing to write up for *Econometrica* a survey, half analytical and half historical, pointing out what are the chief contributions of Thiele and of yourself, taking stock so to speak of the results now available.

Such a clarification would be it seems very useful. and would - if published in *Econometrica*, help making your theory and methods known to a public that is now not ~~fully~~ familiar with it. I hope you agree to writing such a survey. Sincerely yours Ragnar Frisch

14 August 1934.

Professor R. Frisch,
Slendalsveien, 98,
Oslo, Norway.

Dear Professor Frisch,

I am enclosing now the notes on "Mathematical distributions used in the common tests of significance." I hope to goodness the algebra has escaped errors in transcription, especially the complex expression on page 21, which I have not, at least lately, used numerically. Otherwise, I feel confident that it is correct. I hope you will adhere to your intention to follow it with a discussion of the asymptotic approximation available when neither h_1 or h_2 are small. The second approximation given in Section 41 of my book, page 213 of the third edition, though good enough for practical tests, does not suffice for the completion of the table as a table of a mathematical function in the way that it ought to be completed.

As to the further paper you suggest, on moments, cumulants and semi-variants, I will think about it and commence a methodical statement of the present state of the subject as I understand it, but have doubts as to

whether anything like a just historical survey could be done within a reasonable compass.

Yours sincerely,

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Warsaw, Poland

21st September 1934.

Professor R. A. Fisher, F.R.S.,
University College,
Glover Street,
LONDON, W.1.

Dear Professor Fisher,

Your letter of August 14th I found on my recent return to Oslo from a trip to the United States.

With your letter was enclosed MS. of your paper on "The Mathematical Distributions used in the Common Tests of Significance." Thank you ever so much for this MS. It will be a very valuable contribution to our columns. You will receive galley direct from the printer. I do not know exactly in which issue it will appear as this will depend partly on routine matters which are attended to by the Assistant Editor in Colorado Springs.

I am sorry that at the moment I have no time to work out a paper on the completion of the two corners of your table where my point ~~binomial consumption~~ limit would probably give a more accurate result than your method. But I hope to revert to this question some time in the future.

I am glad you are thinking of the further paper giving a methodological statement of the present stage of the subject on moments, semi-invariants, etc. I wish you would let us have a chance of considering it for "Econometrica", even though it might be somewhat larger than originally contemplated.

With best regards.

Sincerely Yours,


Ragnar Frisch.