The Statistician and the "Stats"—

R. A. Fisher and the RSS

John Aldrich University of Southampton

RSS February 2019

Obituary

SIR RONALD AYLMER FISHER, 1890–1962

INTRODUCTION

By J. O. IRWIN

President, Royal Statistical Society

Sir Ronald Fisher was the most famous statistician and mathematical biologist in the world. The Council of the Royal Statistical Society feel that his work covered such a wide field that an adequate appreciation could be given only by a cooperative effort.

An obituary of unprecedented magnificence

- Celebrates a life in science/statistics *not* a life in the Society

- The two were different for so much of the life was outside the Society

- Celebration due because the Society largely remade itself in his image, e.g. by creating an Industrial and Agricultural Research Section in the 1930s and Research Section + theory journal, *Series B*, in the 40s

Outline of talk

- Background
- Fisher's life with the RSS
  - 1920s joins, leaves, rejoins
  - 1930s uncomfortably present
  - 1940s usually absent
  - 1950s President & writing in the Society's journals
- Being in a Society

Sources for the story

❑ Published

■ *JRSS* Articles and reports of meetings

❑ Archives

■ *RSS* Archives

■ Fisher Archives in Adelaide—mainly letters ***to*** him

❑ Books

■ Henry Bennett (1983) *Natural Selection, Heredity, and Eugenics. Including Selected Correspondence of R. A. Fisher with Leonard Darwin and Others*.

■ Joan Fisher Box (1978) *R. A. Fisher: The Life of a Scientist.*

1920 Ronald Fisher and mentor Leonard Darwin

Ronald Fisher (1890-1962)

Cambridge maths graduate

Interests—genetics, eugenics, stat. theory

Ag stats at Rothamsted from 1919
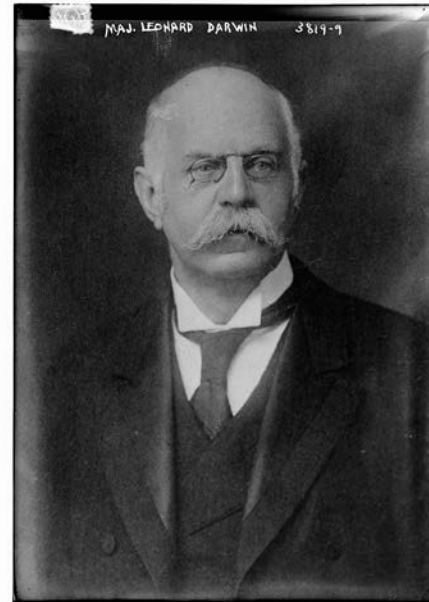
Leonard Darwin (1850-1943)

Son of Charles, President of Eugenics

Education Society + **connections galore**

Fisher's patron + friend

1920 One journal/one editor to please *Biometrika* and Karl Pearson

BIOMETRIKA

A JOURNAL FOR THE STATISTICAL STUDY OF BIOLOGICAL PROBLEMS

Fisher's success rate with KP

❑ 1 publication (1915)

FREQUENCY DISTRIBUTION OF THE VALUES OF THE CORRELATION COEFFICIENT IN SAMPLES FROM AN INDEFINITELY LARGE POPULATION.
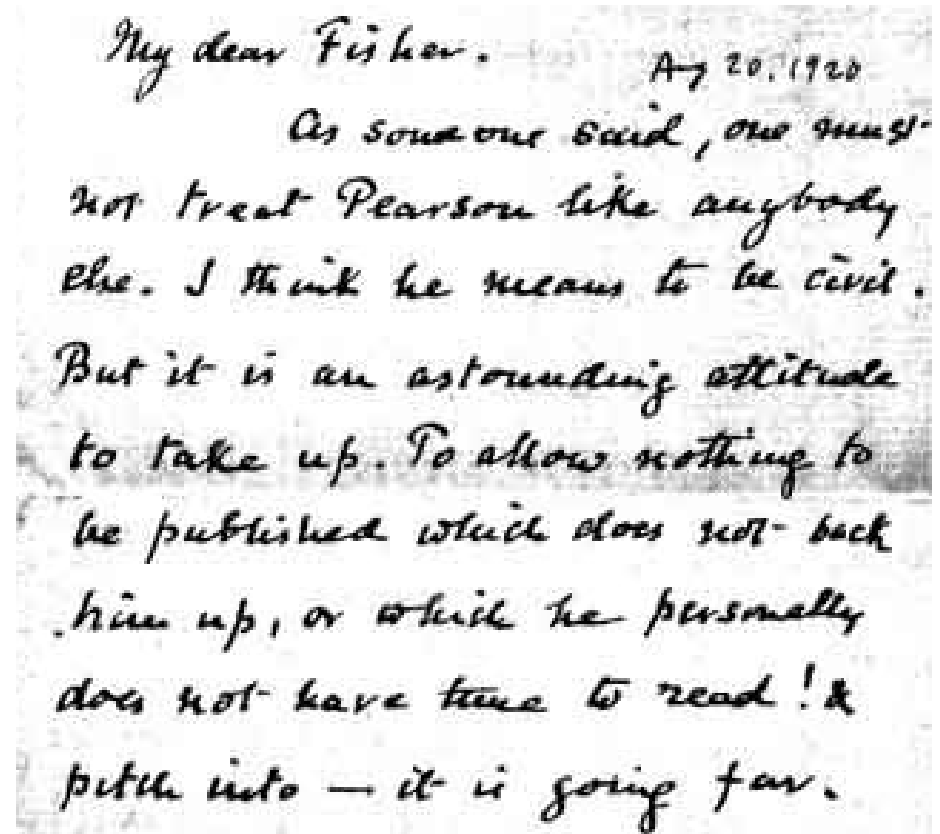
By R. A. FISHER.

❑ 2 or 3 rejections
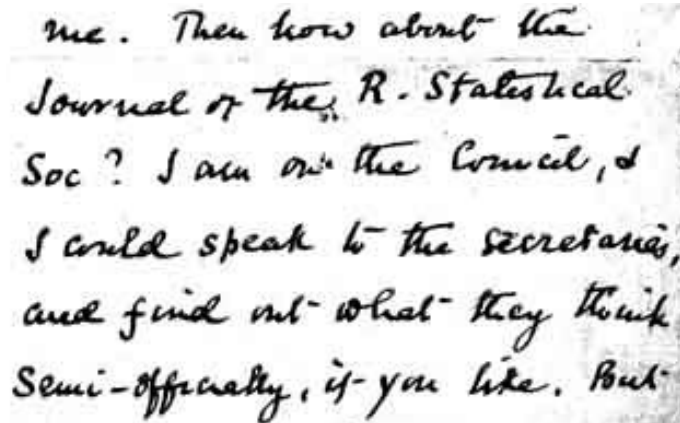
1920 Fisher's latest offering rejected—Darwin's reaction

"On the 'Probable Error' of a Coefficient of Correlation deduced from a Small Sample"

- Extended Fisher (1915)
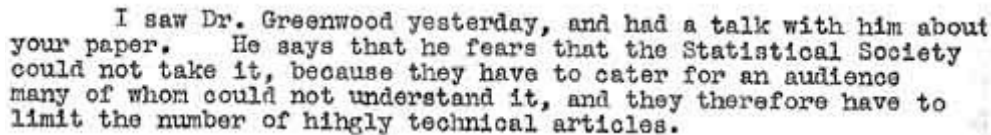- Criticised Pearson's "Appendix" to it for confusing ML with Bayes

My dear Fisher.   Aug 20. 1920
As someone said, one must not treat Pearson like anybody else. I think he means to be civil. But it is an astounding attitude to take up. To allow nothing to be published which does not back him up, or which he personally does not have time to read! & pitch into — it is going far.
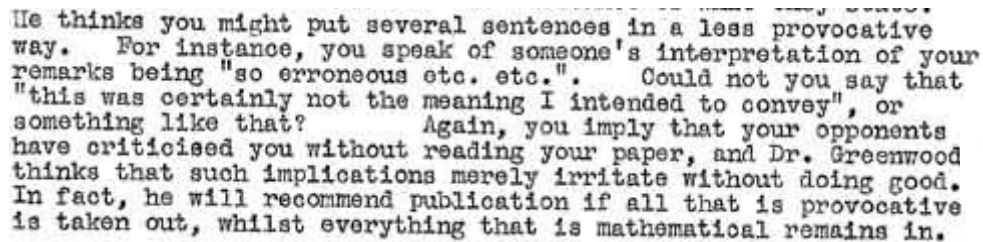
Darwin's connections

me. Then how about the Journal of the R. Statistical Soc? I am on the Council, & I could speak to the secretaries, and find out what they think Semi-officially, if you like. But

I saw Dr. Greenwood yesterday, and had a talk with him about your paper. He says that he fears that the Statistical Society could not take it, because they have to cater for an audience many of whom could not understand it, and they therefore have to limit the number of hihgly technical articles.

He thinks you might put several sentences in a less provocative way. For instance, you speak of someone's interpretation of your remarks being "so erroneous etc. etc.". Could not you say that "this was certainly not the meaning I intended to convey", or something like that? Again, you imply that your opponents have criticised you without reading your paper, and Dr. Greenwood thinks that such implications merely irritate without doing good. In fact, he will recommend publication if all that is provocative is taken out, whilst everything that is mathematical remains in.

- Try *JRSS*
- Major Greenwood (editor) says no
- But—subject to softening—recommends it for Gini's *Metron*
- Published as

ON THE "PROBABLE ERROR" OF A COEFFICIENT CORRELATION DEDUCED FROM A SMALL SAMPLE

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1921 Another line to the RSS: Arthur Bowley (1869-1957)

Professor of Stats at LSE and Fisher's neighbour in Harpenden

- ❑ Recommends to *JRSS* Fisher's piece on χ^2
- ❑ Reads Fisher's "Mathematical Foundations of Theoretical Statistics"
- ❑ Cool about its criticism of Bayes
- ❑ Pearson on χ^2 paper

1921 Fisher's best ally among the Stats: Udny Yule (1871-1951) "kindly, gentle and genial"

Lecturer in Stats (joint between Ag and Eco) in Cambridge. Like Fisher, interested in stat theory, genetics and ag

- ❑ Supports Fisher's fellowship application to RSS: "I have seen a good deal of Mr. Fisher's work and cordially recommend him."
- ❑ Favourably reviews Fisher's "Mathematical Foundations of Theoretical Statistics"
- ❑ Agrees with Fisher's criticism of Pearson on χ^2

JRSS publishes, *Biometrika* reacts

MISCELLANEA.

CONTENTS.

On the Interpretation of χ^2 from Contingency Tables, and the Calculation of P.   By R. A. FISHER, M.A.   ....

On the Application of the χ^2 Method to Association and Contingency Tables, with Experimental Illustrations.   By G. UDNY YULE, C.B.E., M.A., F.R.S. ....

MISCELLANEA.

THE GOODNESS OF FIT OF REGRESSION FORMULÆ, AND THE DISTRIBUTION OF REGRESSION COEFFICIENTS.

By R. A. FISHER, M.A.

- ❑ *JRSS* published the occasional technical piece in *Miscellanea*
- ❑ In 1922 publishes 2 by Fisher
- ❑ More *JRSS* friendly than first

❑ KP blasts χ^2 paper

The above re-description of what seem to me very elementary considerations would be unnecessary had not a recent writer in the *Journal of the Royal Statistical Society* † appeared to have wholly ignored them. He considers that I have made serious blunders in not limiting my degrees of freedom by the number of moments I have taken; for example he asserts (p. 93) that if a frequency curve be fitted by the use of four moments then the n' of the tables of goodness of fit should be reduced by 4. I hold that such a view is entirely erroneous, and that the writer has done no service to the science of statistics by giving it broad-cast circulation in the pages of the *Journal of the Royal Statistical Society*.

The Stats reject 3rd Fisher paper–"they have treated you badly … the dignified course …"

I think the Stats. have treated you badly. But I hope you will think twice before resigning. The fault lies with at most 2 or 3 individuals, even if more nominally consent. There men go in time and the affair is quite forgotten. If you now protest to the Council or resign, you will get the reputation, justly or unjustly,
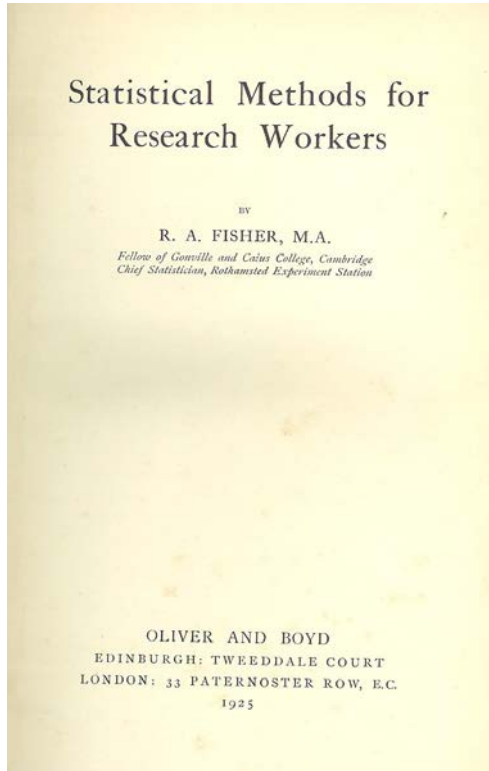
of being very touchy and easily put out. That reputation will not die out easily. Therefore you will lose by any action. The dignified course is that which makes you appear to say "I don't care a damn what you do or say". Forgive me writing thus plainly.

L.D

Fisher did *not* take the dignified course

- Argued with the RSS
- Darwin took up his case
- No joy
- Fisher resigns
- The reputation of "being very touchy and easily put out" stuck—Yates and Mather (1963) recalled his "notoriously contentious spirit"

1923+ Fisher finds other outlets

Statistical Methods for Research Workers

BY

R. A. FISHER, M.A.

Fellow of Gonville and Caius College, Cambridge
Chief Statistician, Rothamsted Experiment Station

OLIVER AND BOYD
EDINBURGH: TWEEDDALE COURT
LONDON: 33 PATERNOSTER ROW, E.C.
1925

- ❑ Book with new editions every few years
- ❑ Maths journals—Cambridge Philosophical Society and Royal Society
- ❑ Ag journals take work on Experiments

1929 Elected FRS

Fisher; Sir; Ronald Aylmer (1890 - 1962) — Elected 1929

EC/1929/04 — ©The Royal Society

With support from

- Stats — Yule but **not** by Pearson and Greenwood

- Cambridge maths— Eddington, Fowler

- Genetics —Punnett

- Ag science — Russell and Hall

Now you're FRS—why not FRSS?

Darwin mobilises

- Bernard Mallet (past President)
- Yule who wants assurance there will be no more resignations

Darwin locks Fisher in with a gift of a lifetime subscription

1933 All change

- ❑ People
- ■ Karl Pearson and Yule retire
- ■ At UC KP replaced by Fisher as Prof of Eugenics and Egon Pearson as Head of Stats
- ■ Yule replaced by John Wishart who teaches maths students—Bartlett, Daniels etc

- ❑ Society
- ■ Creates Industrial and Agricultural Section with its own journal—the *Supplement*.
- ■ Fisher not involved

Fisher on the Council April 1933/Off April 1936

Dear Miss Thorburn:

I am much obliged for your note of the 20th., informing me that the Council of the Royal Statistical Society wished to propose my name for election to that Council.
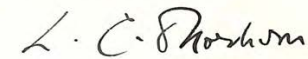
I should be glad to be of any assistance I can to the Society and, without pledging myself to be a regular attendant, I think I could manage to be present as often as there is any special business on which I could be of assistance. I have therefore much pleasure in accepting the kind offer of the Council.

Dear Professor Fisher,

At their last meeting the Council considered the changes in their membership which must take place, according to the byelaws, in June. They are obliged to retire six members, at least three on the ground of seniority, and it is their custom to use the criterion of attendance with respect to the others; and, as you were one of those with the fewest attendances during the last two sessions, they are reluctantly obliged to eliminate your name from this year's ~~next session's~~

list.

Yours very truly,

L. C. Thorburn

Assistant Secretary.

Fisher's main project in the Society was getting recognition for Student—a Guy medal in gold. He failed.

1934-5 Resisting Fisher and going beyond him

Bowley had followed Fisher's work from the start and disagreed with the attack on Bayes

Jerzy Neyman (1894-1981)

Egon Pearson (1895-1983)
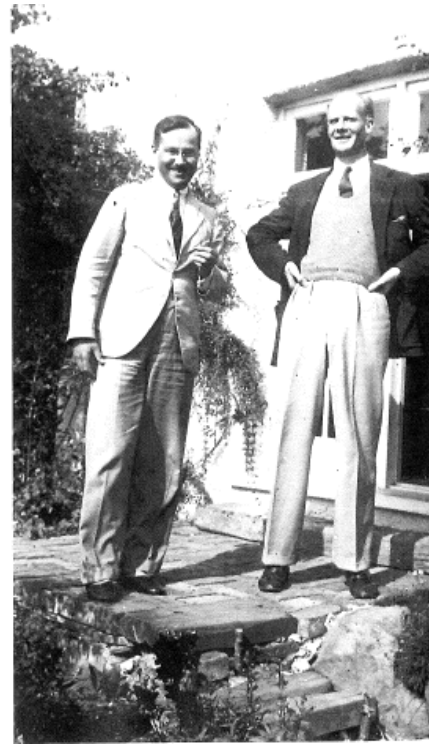
worked on a theory of testing from 1928

ON THE TWO DIFFERENT ASPECTS OF THE REPRESENTATIVE METHOD:
THE METHOD OF STRATIFIED SAMPLING AND THE METHOD
OF PURPOSIVE SELECTION.

By JERZY NEYMAN

(Biometric Laboratory, Nencki Institute, Soc. Sci. Lit.
Varsoviensis, Warsaw).

[Read before the Royal Statistical Society, June 19th, 1934, the PRESIDENT,
the RT. HON. LORD MESTON of Agra and Dunottar, K.C.S.I., LL.D.,
in the Chair.]

Bowley vote of thanks

After Dr. Neyman's very courteous references to my work on the subject, it is somewhat ungrateful that I feel it my duty to criticize the theory of probabilities in Section II, part 1, and I am very glad Professor Fisher is present, as it is his work that Dr. Neyman has accepted and incorporated. I am not certain whether to ask for an explanation or to cast a doubt. It is suggested in the paper that the work is difficult to follow and I may be one of those who

have been misled by it. I can only say I have read it at the time it appeared and since, and I read Dr. Neyman's elucidation of it yesterday with great care. I am referring to Dr. Neyman's confidence limits. I am not at all sure that the " confidence " is not a " confidence trick." Put in a simple form I think the method is as

"confidence trick"

Fisher discusses

It would be expected that he should comment on those applications of inductive logic which constituted so illuminating and refreshing an aspect of the evening's paper. All realized that problems of mathematical logic underlay all inferences from observational material. They were widely conscious, too, that more than 150 years

of disputation between the *pros* and *cons* of inverse probability had left the subject only more befogged by doubt and frustration. Recently, however, some research workers, working in the apparently abstract realms of the theory of estimation, and the logical bases of tests of significance, had become increasingly confident that, when properly stated, rigorously exact, though, of course, *uncertain* inferences might be drawn from observational or experimental data.

"illuminating and refreshing"

THE LOGIC OF INDUCTIVE INFERENCE.

By PROFESSOR R. A. FISHER, Sc.D., F.R.S.

[Read before the Royal Statistical Society on Tuesday, December 18th, 1934,

Fisher went in   came out

WHEN the invitation of your Council was extended to me to address this Society on some of the theoretical researches with which I have been associated, I took it as an indication that the time was now thought ripe for a discussion, in summary, of the net effect of these researches upon our conception of what statistical methods are capable of doing, and upon the outlook and ideas which may usefully be acquired in the course of mathematical training for a statistical career. I welcomed also the invitation, personally, as affording an opportunity of putting forward the opinion to which I find myself more and more strongly drawn, that the essential effect of the general body of researches in mathematical statistics during the last fifteen years is fundamentally a reconstruction of logical rather than mathematical ideas, although the solution of mathematical problems has contributed essentially to this reconstruction.

PROFESSOR FISHER replied in writing as follows :—
The acerbity, to use no stronger term, with which the customary vote of thanks has been moved and seconded, strange as it must seem to visitors not familiar with our Society, does not, I confess, surprise me. From the fact that thirteen years have elapsed between the publication, by the Royal Society, of my first rough outline of the developments, which are the subjects of to-day's discussion, and the occurrence of that discussion itself, it is a fair inference that some at least of the Society's authorities on matters theoretical viewed these developments with disfavour, and admitted them with reluctance. The choice of order in speaking, which

Fisher's first address to an RSS meeting—expecting what—hail the conquering hero!?

After another vote of no thanks from Bowley.

Neyman supportive of Fisher

Fisher tolerant of Neyman's different way

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STATISTICAL PROBLEMS IN AGRICULTURAL EXPERIMENTATION.

By J. NEYMAN with co-operation of K. IWASZKIEWICZ and ST. KOŁODZIEJCZYK.

[Read before the Industrial and Agricultural Research Section of the Royal Statistical Society, March 28th, 1935, Prof. F. L. Engledow in the Chair.]

Fisher vote of no thanks

PROFESSOR R. A. FISHER, in opening the discussion, said he had hoped that Dr. Neyman's paper would be on a subject with which the author was fully acquainted, and on which he could speak with authority, as in the case of his address to the Society delivered last summer. Since seeing the paper, he had come to the conclusion that Dr. Neyman had been somewhat unwise in his choice of topics.

Neyman's paper criticised Fisher and Fisher reacted

Egon Pearson comments

DR. PEARSON said that while he knew there was a widespread belief in Professor Fisher's infallibility, he must, in the first place, beg leave to question the wisdom of accusing a fellow-worker of incompetence without, at the same time, showing that he had succeeded in mastering his argument. He felt sure that Dr. Neyman would be able himself to answer the criticisms that had been raised, but there was a rather more general point touching on the whole field of statistical enquiry on which he would like to comment. Professor Fisher had on more than one occasion thrown out the suggestion that while some other statisticians were academic he and his collaborators were practical men; Dr. Pearson believed that attempts to dub as academic enquiries into the underlying principles upon which practical tests were based, showed a serious loss of perspective.

Fisher had portrayed Neyman and Pearson "academic statisticians"

1935-50—quiet times for Fisher and the Stats

- 1935-9 Fisher takes part in meetings—usually to support Rothamsted friends
- Industrial and Agricultural closes down in 39
- Fisher receives awards from Society, joins Council and is President in 52
- Does not *do* anything in Society

War brings new people

S.R.17 25 YEARS ON

Top row: P. Armitage, R. L. Plackett, P. J. Stanley, B. H. P. Rivett, J. R. Isaac, H. G. H. Carpenter, D. H. Smith, A. M. Walker, J. Taylor, D. H. Ward
Middle row: Miss V. Allinson, L. S. Vallance, J. P. Burman, G. V. Owen, R. Temple, W. T. Hale, D. V. Lindley, C. D. Bates, J. G. Day, W. A. Pridmore, H. J. Godwin
Bottom row: A. H. J. Baines, S. N. Collings, Miss F. Rigg, E. D. Van Rest, J. F. Holdworth, Mrs. C. M. Bandi

Some in 1968; a reunion of Ministry of Supply's big SR17 unit. Dennis Lindley in centre—George Barnard absent.

- A lot of them!
- Some Cambridge maths graduates
- The more senior e.g. Barnard got jobs as statisticians
- The junior e.g. Lindley went back to Cambridge to learn more
- RSS their natural home

1945: Research Section formed

Dear Professor Fisher,

    As you probably know, a Research Section of the Royal Statistical Society has been formed, which, together with an Industrial Application Group, constitutes the new form of the pre-war Industrial and Agricultural Research Section.

    At the inaugural meeting of the Research Section on December 4th, Mr. G.A. Barnard is reading a paper on Sequential Tests in Industrial Statistics. The committee feel that, in view of your interest in Mr. Barnard's work, it would be most valuable if you could contribute to the discussion on the paper, and very much hope that you will be able to accept this invitation to speak at the meeting.

Fisher didn't go!

- Fisher was not involved in reform of the Society
- Nor in such intellectual developments as stochastic processes
- From 45 to 54 his stats papers went to *Biometrics*

George Barnard (1915-2002)
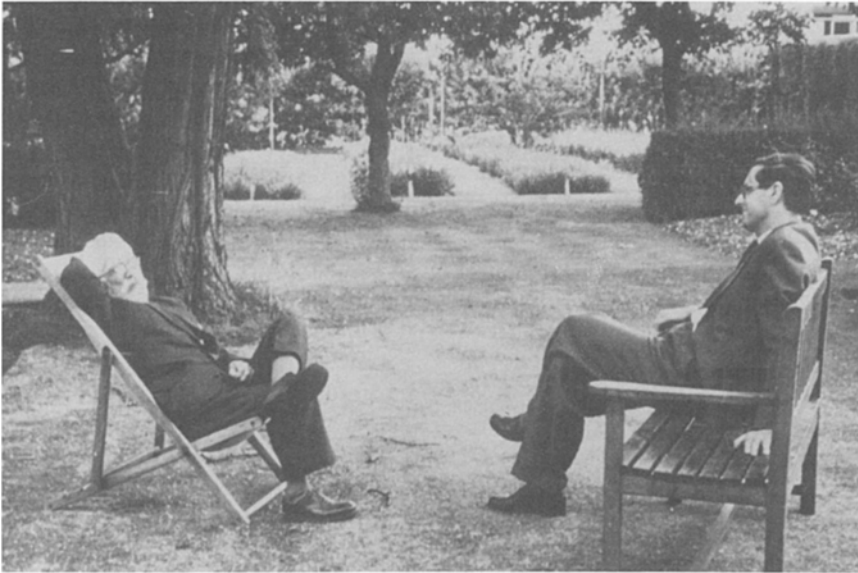
- Diverted by war from a career in mathematical logic
- Fisher's only intellectual companion from that generation
- When Fisher was President chose him as VP

Statistical Methods & Scientific Inference—Fisher's *Series B* book

Statistical Methods and Scientific Inference

Sir Ronald A. Fisher
Sc.D., F.R.S.
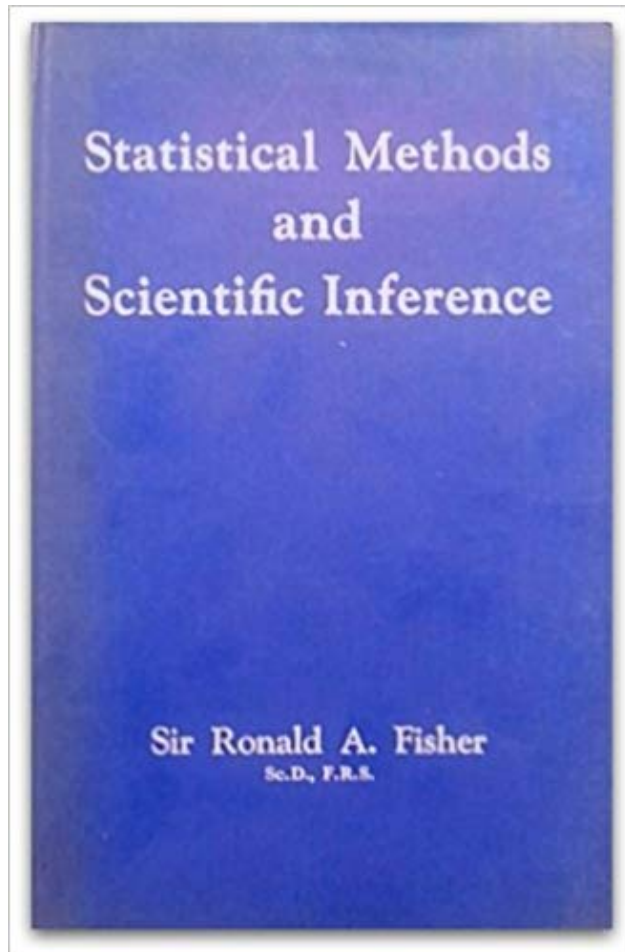
Looked 2 ways

- Back to controversies of 30s
- Forward to extensions of Fiducial inference
- Brought in a new critic D. V. Lindley

Series B referee on Fisher on Lindley

COMMENTS ON FISHER'S PAPER

Referee 1.

It seems to me that Fisher has made a valid point in emphasising that his section 6 on page 123 relates to observations of two kinds, and he has made what is perhaps a more important point in suggesting (his page 3, end of paragraph, 9 lines from the bottom) that perhaps some limitations may need to be applied to the fiducial argument.

The paper is marred I think by the suggestion of incompetence on the part of Lindley made at the end of paragraph 3, and I think Fisher should be asked to modify this sentence, for example, to read:

"He emphasises that many of his results are contradictory and ridiculous, but this is due in fact to his failure to recognise correctly the problems to which the method may be applied."

This would still leave some sharp criticism of Lindley, but this would not seem to me to be out of place in view of the criticisms Lindley has already made of Fisher.

- An echo of Greenwood inn 1920
- JRSS publishes anyway
- So at last RSS gives Fisher an outlet

Man and Society—quotes from Yates and Mather

his notoriously contentious spirit,

Fisher had a likeable but difficult character. He had many friends, and was a charming and stimulating man to work with, and excellent company.

He liked the company of other scientists and was a familiar figure at scientific meetings and international gatherings; the latter he attended more for the opportunity of meeting his friends than to listen to scientific communications. He was largely instrumental in setting up the Biometric Society, and played a leading part in the affairs of many other societies.

- The RSS was ***never** a* place for meeting friends

A great resigner
- in the 20s RSS
- In the 30s the *Proceedings of the Cambridge Phil Soc*
- in the 50s *Biometrics*