

The Statistician and “the Stats”—

Ronald Fisher and the RSS

John Aldrich

Economics Department

University of Southampton

Southampton

SO17 1BJ

UK

e-mail: john.aldrich@soton.ac.uk

Abstract

R. A. Fisher was the inspiration behind the 20th century re-orientation of Statistics as Research Methodology. He died in 1962 acknowledged by the RSS as the “most famous statistician in the world.” Yet his four decades of transactions with the Society were generally troubled: in the 1920s he joined, left and re-joined, in the 30s he was uncomfortably present, in the 40s he was usually absent and in the 50s he was President and contributing to the Society’s journals. The conflicts were of personalities—one a “notoriously contentious spirit” —and institutions but the issues concerned statistical inference.

[1st draft incomplete: see p. 6]

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Introduction

When Sir Ronald Aylmer Fisher (1890-1962) died the Royal Statistical Society commemorated “the most famous statistician and mathematical biologist in the world” with an obituary of unsurpassed splendour—Irwin, Barnard, Mather, Yates & Healy (1963). Recognition was especially appropriate as the Society had remade itself in Fisher’s image: it never admitted mathematical biology but Fisher’s conception of statistics inspired younger Fellows to form an Industrial and Agricultural Research Section in the 1930s and a Research Section in the 40s.

The obituary celebrated a life in science rather than in the Society for most of it—even the statistical life—happened elsewhere and Fisher’s long relationship with the Society is best described as unhappy.¹ In the early 20s he joined and left. In 1929 his friend Leonard Darwin asked, “Do not you think you ought to rejoin the Stats.?” He did and stayed—to be uncomfortably present in the 30s, mainly absent in the 40s but apparently at ease in the 50s when he was President of the Society and writing in its journals.

This paper follows Fisher’s relationship with the Stats. The earliest phases are covered from his side in Joan Fisher Box’s *The Life of a Scientist* (1978) and Henry Bennett’s (1983) edition of the Darwin-Fisher correspondence but the Society’s archivist, Janet Foster, has pointed to documents from its side.² The present account adds the ‘new’ information and extends the story to later decades; it also looks across to Fisher’s

¹ A large literature details the life—see Aldrich (2003/18).

² In a talk on “Exploring the Royal Statistical Society Archives” given to the RSS’s History of Statistics Section in April 2017. Fisher’s correspondence is held in the University of Adelaide’s Special Collections where I have had help from Cheryl Hoskins and Marie Larsen.

relations with other societies and journals and amplifies the Fisher-dimension of the story of mathematical statistics in the Society's first century or so; see Aldrich (2010a).

1 Darwin connects, Fisher disconnects

R. A. Fisher's reputation was built on publications. Before joining Rothamsted Experimental Station in 1919 his work had appeared in mathematics and general science periodicals but his core interests had a perfect match in a journal for the "statistical analysis of biological problems" and to publish there had been his goal since he was an undergraduate. Fisher first made overtures to *Biometrika* in 1912 but he had only one success, on the distribution of the interclass correlation (1915). The journal was not the organ of a society but of an individual, Karl Pearson.³ In August 1920 Pearson rejected Fisher's latest offering, "On the 'probable error' of a coefficient of correlation deduced from a small sample."⁴ In content it was pure *Biometrika*, complementing Fisher (1915) by treating the intraclass correlation and criticising its *Biometrika* "Appendix" by Soper et al. (1917) for misrepresenting its "method of the optimum," the future maximum likelihood.⁵ With the paper rejected, Fisher dropped his *Biometrika* ambitions—quietly, for there was no higher authority to appeal to or society to resign from, but permanently.

Rothamsted was a leading centre with links to journals like the *Journal of Agricultural Science* and the *Annals of Applied Biology* but, while Fisher used them, they did not publish mathematical statistics.⁶ Fisher was a mathematician/geneticist doing agricultural science and he turned in an unlikely direction—to the Statistical Society. He had never

³ For *Biometrika*, its mission and how it treated Fisher see Aldrich (2013).

⁴ Box (ch. 3) recounts these incidents.

⁵ For the theoretical issues involved see Aldrich (1997: 168-170).

⁶ Aldrich (2019a) describes Fisher's contributions to these journals.

used any of the articles in its journal, though his great paper on population genetics (1918) noticed work (published elsewhere) by Brownlee, Snow and Yule, Pearsonians who had found their way to the Society.⁷ Fisher's way was found for him by his friend, mentor, benefactor and general father-figure, Leonard Darwin (1850-1943), son of Charles, President of the Eugenics Education Society and a man of substance and connections. Darwin was Fisher's intellectual companion on all matters eugenic and genetic: he was not a statistician but, as spokesman for eugenics, he was listened to by the Society and two of his papers had recently appeared in the *Journal*. Darwin had no influence with Pearson for he was the wrong kind of eugenicist.

Twenty years before a rejection led to the founding of *Biometrika* but Fisher and Darwin just looked for another journal.⁸ Fisher had found a new one and Darwin asked about it before making his own suggestion:

Is Gini on its staff? He would wish to be civil to me. Then how about the Journal of the R. Statistical Soc? I am on the Council, & I could speak to the secretaries [who edited the *Journal*] and find out what they think semi-officially, if you like.⁹

Corrado Gini (1884–1965) was known to Darwin as an important Italian eugenicist: for Fisher perhaps the journal, *Metron*, had all the promise of a *Biometrika* without Pearson.

⁷ For these and other Society figures of the period—including Bowley, Flux, Greenwood, and Isserlis—see Aldrich (2010a: §§3 & 4).

⁸ Darwin controlled a journal, the *Eugenics Review*, but Fisher only published one statistical article there, his (1926).

⁹ See Box (83) and Bennett (1983: 73); the sentences about Gini are omitted from Bennett's edition. All the letters are available in full on the University of Adelaide Fisher Digital Archive.

On September 1st Darwin sent news of Gini, “I have a note from Gini, asking me to write an article in *Metron*. I shall reply ‘no’ soon, in qualified terms. So I could easily open up about your paper.” Darwin continued, “My idea, however, is that R. S. J. would be best for you. If they don’t print mathematics they had better shut up.” Darwin remained attached to the idea that the *Journal* (and the Society) would be “best for you”—indeed more attached than Fisher.

Darwin may have thought that the *Journal* had better print mathematics but Major Greenwood, Secretary and old Pearsonian, explained the realities and Darwin put them to Fisher on October 18th (Bennett 73): Greenwood “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 highly technical papers.” Such papers, which had been appearing since the 1880s, went in the small *Miscellanea* section. Greenwood was friendly and “would be glad to send it on to Professor Gini for insertion in *Metron*.” He thought, though, that Fisher might put certain sentences in a “less provocative way.” With the endorsement of Greenwood and/or Darwin, the paper was published as (1921a); *Metron* took three more on *Biometrika* themes—one in 1924 on the distribution of the partial correlation coefficient and two in 1925 on Student’s distribution.

Fisher was not put off the Society but the next initiative was through Arthur Bowley (1869-1957), LSE professor and a neighbour in Harpenden.¹⁰ Bowley, an economic statistician and follower of Edgeworth in matters of inference, had long been associated with the Society and was a member of Council though not involved with the *Journal*. On May 22nd 1921 he recommended a paper by Fisher on χ^2 to the RSS—see Box (85). Two days later Fisher spoke at a meeting when G. Udny Yule (1871-1951) discussed the time-

¹⁰ See Aldrich (2008a: passim) for Fisher’s relations with Bowley.

correlation problem; meetings were the life of the Society and the main function of the *Journal* was to record them. Yule, elected FRS in 1921, a past Secretary and future President of the Society, had worked on population genetics as well as on statistical theory and was teaching at the Cambridge University School of Agriculture: to Fisher he must have seemed the most relevant person in the Society. However, Yule's feeling for the Society's statistics was much warmer than Fisher's for he embraced the Society's traditional 'state-istics' agenda and had introduced himself by applying Pearson's methods in a study of pauperism.¹¹

By June Darwin had orchestrated Fisher's entry into the Society—in its argot, becoming a Fellow. More was involved than applying and paying the subscription: “Candidates must be proposed and seconded by Fellows of the Society, who, either from personal or general knowledge, vouch for the Candidate's qualification and eligibility.” Fisher was proposed by Darwin and seconded by Yule who attested, “I have seen a good deal of Mr. Fisher's work and cordially recommend him.” Yule would be the most supportive of the established statisticians: his referee's report for the *Philosophical Transactions* on Fisher's “On the mathematical foundations of theoretical statistics” (1922c) declared, “A paper of such a basic kind is, I think, precisely the sort of paper which should be published by the Royal Society.”¹² Bowley had corresponded with Fisher over the paper in—the busy month of—May 1921, begging to disagree, “Nor am I

¹¹ See Kendall (1952) for Yule's life. For his entry into the Society and his relations with economists see Aldrich (2010a: 12-1; 2010b: 115-5).

¹² See Edwards (1997: Appendix 1). The other referee was the astronomer Arthur Eddington, an authority on the theory of errors. This paper was communicated by John Russell, director of Rothamsted. For the paper's significance see Aldrich (1997) and Stigler (2005).

at all certain that a priori probabilities have not a considerable use [...]—that the method can be abused does not differentiate it from many other methods.”

In 1922 two theoretical articles by Fisher, including the one recommended by Bowley, appeared in the *Journal's Miscellanea*. Like all his statistical articles of this time, they derived from Pearson but these had more connection with the Society than the 1920 offering: the regression paper (1922b) extended Slutsky (1913), a paper Yule took for the *Journal* when *Biometrika* declined it; the paper on contingency tables (1922a) was on a topic Bowley and Yule had treated in their textbooks.¹³ Yule had long sensed there was something wrong with Pearson's original work and Yule (1922). appeared alongside Fisher (1922a). Pearson did not attend to the regression paper but he (1922: 187) blasted the χ^2 contribution (without naming the author): “I hold that such a view [i.e. Fisher's] 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*.”

When Fisher sent the *Journal* a second paper on χ^2 it was rejected: according to Box (87), he suspected that the editors had “simply bolted for cover in the face of Person's anger.” The referee was Leon Isserlis.¹⁴ [As I write, the relevant RSS file cannot be found and I do not know the specifics of Isserlis's report or even what the paper was about.]

Darwin advised Fisher against precipitate action: “I think the Stats. have treated you badly. But I hope you will think twice before resigning.” Darwin feared “you will get the reputation, justly or unjustly, of being very touchy and easily put out. That reputation will not die out easily.” Forty years later Yates and Mather's (1963: 97) biographical memoir

¹³ For Pearson, Slutsky and the regression stream in Fisher's work, see Aldrich (2005). Fienberg (1979) and Lancaster (1969) discuss the χ^2 work.

¹⁴ For Isserlis see Aldrich (2006/18).

remembered Fisher's "notoriously contentious spirit." Although Darwin thought Fisher best drop the matter, he spent two months digging, putting Fisher's case to Sir Bernard Mallet, former Registrar General and President of the Society, and suggesting that Fisher contact Yule. Fisher himself wrote to Alfred Flux, an economic statistician, the editor identified by Darwin as the "man chiefly concerned"; see Bennett (76-7). Eventually Fisher resigned, ending two years of fellowship—see Box (87).¹⁵ However, in February, while he and Darwin were still discussing the problem, he was corresponding with Bowley about χ^2 and the new LSE journal *Economica* published Fisher (1923a); it is unclear what relation this published paper had to the piece rejected by the *Journal*. Fisher did not publish again in *Economica* but the *Journal* took one more of his papers, a comment (1924) on Brownlee's (1924) experiments testing Fisher's goodness of fit results.

By resigning, Fisher gave up the Society—not statistics in which he had much invested: at Rothamsted he was Head of *Statistics*, his most ambitious paper treated *theoretical statistics* and he was already speaking for the subject. The *Proceedings of the Cambridge Philosophical Society*, the journal for Cambridge mathematics and mathematical physics (Fisher's intellectual birthplace), had published a piece by William Burnside and in a subsequent note Fisher (1923b: 635) explained:

That branch of applied mathematics which is now known as Statistics has been gradually built up to meet very different needs among different classes of workers. Widely different notations have been employed to represent the same relations, and still more widely different methods of treatment have been designed for essentially the same statistical problem. It is therefore not surprising that Dr. Burnside writing on errors of observation in 1923 should

¹⁵ The resignation does not figure in any documents in the Society's archives.

have overlooked the brilliant work of “Student” in 1908 [in *Biometrika*], which largely anticipates his conclusion.

Evidently Fisher’s “Statistics” had sovereignty over the theory of errors and over biometry. The opening created by Burnside’s paper gave Fisher another outlet and the *CPS Proceedings* made a major contribution to Fisher’s publishing problem, taking seven of his papers in the 20s and 30s. The CPS, incidentally, was less of a society than the RSS: society existed for the journal not vice versa.¹⁶

Books also helped solve Fisher’s publishing problem. In 1925 *Statistical Methods for Research Workers* appeared in a series of Biological Monographs and Manuals. The book sold and there was a new edition every few years—14 in all. For the rest of Fisher’s life revising his books would be a form of journal writing—there would be 8 editions of *Design of Experiments*, 6 of *Statistical Tables* and 3 of *Statistical Methods and Scientific Inference*.

The publication of *Statistical Methods for Research Workers* gave Fisher and the statisticians an opportunity to re-assess each another and they disliked what they saw. Fisher (1925: 2) disparaged the statisticians’ statistics:

This particular dependence of social studies upon statistical methods has led to the painful misapprehension that statistics is to be regarded as a branch of economics, whereas in truth economists have much to learn from their scientific contemporaries, not only in general scientific method, but in particular in statistical practice.

¹⁶ See Aldrich (2009) for Fisher’s earliest dealings (1923-5) with the *PCPS*. Hall’s (1969) history of the Society focusses on mathematical physics and does not pick up the importance of the *Proceedings* as a mathematical statistics journal in the 20s and 30s.

The book's theoretical skeleton was the system of distributions based on the normal which included the t and χ^2 , topics on which he had published in the *Journal*. On the χ^2 controversy Fisher (17) stood his ground on the substantive issue and matched the rhetoric of Pearson's (1922) condemnation, "Pearson's paper of 1900 contained a serious error, which vitiated most of the tests of goodness of fit made by this method until 1921."

Isserlis reviewed the book for the *Journal* and there was an unsigned review in the *British Medical Journal*, probably written by Greenwood.¹⁷ The *BMJ* reviewer criticised the author: "an able and original mathematical statistician" with an objectionable manner— "the trained statistician [...] may resent the somewhat arrogant way in which the law is laid down upon points respecting which there is difference of opinion among persons possibly as well informed as Mr. Fisher." The reviewer went on to recall Macaulay's remark in a similar situation—"we have heard a baby, mounted on the shoulders of its father, cry out, "how much taller I am than Papa!" Isserlis did not mention the χ^2 controversy or the author's manner but concentrated on the design of the book and the difficulty of combining "an authoritative record of achievement in a particular branch of biological investigation" with a presentation of "the results of [the author's] own researches in a more extended form." He concluded, "The book will undoubtedly prove of great value to research workers whose statistical series necessarily consist of small samples, but will prove a hard nut to crack for biologists who attempt to use it as a first introduction to statistical method."

In the next few years Fisher made his reputation away from the statisticians with his book and publications in mathematics journals and agricultural journals. *JRSS* was not troubled by papers that went to the *Proceedings of the Cambridge Philosophical Society*

¹⁷ For the reviews—with notes—see Aldrich (2005 and -6/18). The latter suggests that Greenwood was the *BMJ* reviewer.

and the *Journal of Agricultural Science* (his main outlets) nor would they have fitted in: *JRSS* did not do mathematics or agricultural experiments. Fisher's (modest) needs for intellectual company were met by Darwin, Rothamsted colleagues and Gosset, the brewer of Guinness alias "Student".¹⁸

2 Darwin reconnects

In May 1929 Fisher joined Pearson, Yule and Greenwood as a Fellow of the Royal Society. He was proposed by Yule, seconded by Eddington and supported by leading geneticists and agricultural scientists: the award recognised a "long series of original contributions to the mathematical theory of statistics, in particular the theory of sampling, and applications to agriculture, biology and meteorology." Fisher valued the award and subsequent honours, the Royal Medal in 1938, the Darwin in 1948 and the Copley in 1955, and appears to have had a happy relationship with the Society's journals. He took the Society very seriously, formulating schemes for reform—see Box (237-8)—and over the years supporting several candidates, beginning with the occasional statisticians, J. B. S. Haldane (elected 1933) and A. C. Aitken (1936). He had talked of getting Wishart and Egon Pearson into the Society but the statistical full-timers came much later with Mahalanobis (1945), Yates (1948) and Finney (1955). He thought there should be more statisticians in the Royal but only of the right stuff—in April 1955 he wrote to Yates about Harold Jeffreys's suggestion that Henry Daniels be put up:

I do not know much of Daniels' work but I can well believe that it is more fruitful than Bartlett's. We have not put up George Barnard yet, and it may be that he and Daniels can be put up together this year. I notice that Irwin is still in,

¹⁸ Aldrich (2019a) describes the Rothamsted scene. Some of the Rothamsted colleagues were in the Society but that seems to have been coincidental.

but Pearson and Maurice Kendall have dropped out. I fancy David Kendall may be put up. I think we must try to keep the pot boiling.¹⁹

Fisher would also be involved in putting people up for the International Statistical Institute: in 1930 Bowley put him up, in May 1934 he was asking Bowley to support Jerzy Neyman “who is spreading the light of mathematical statistics in Poland” and in -47 he was encouraging Bartlett to stand. Fisher never published with the Institute.

Immediately Darwin had Fisher’s R.S. news he brought up “the Stats”: “I am sure now you are F.R.S. you should be F.R.S.S.” One can only speculate why Darwin pushed so hard: in 1921 it had been a matter of getting his protégé published and perhaps of bolstering eugenics in the Society but now perhaps it just seemed fitting that the national Society recognise its greatest exponent. By June 25th 1929 matters had progressed: Yule was nervous about the possibility of a second resignation and Darwin got around this by making Fisher the gift of a life-time subscription—see Bennett (103-4). So Darwin and Yule reprised their earlier roles with Mallet in support. After the re-entry Darwin had no further role in Fisher’s transactions with the Society and Yule did no further mediating between Fisher and the Society as illness soon forced him into virtual retirement. Yule had always appreciated Fisher’s importance and Kendall (1952: 157) recalls him reflecting on the death of Pearson in 1936, “I feel as though the Karlovingian era has come to an end, and the Piscatorial era which succeeds it is one in which I can play no part.” Kendall (158) further noted “the genuine regret of a man who lived to see his

¹⁹ A year later writing to Finney, Fisher was less sure about supporting Barnard: “I do not know what major work of his to point to.” Barnard, Irwin and Maurice Kendall were never Fellows. Pearson, Bartlett, David Kendall and Daniels were elected in 1966, -61, -64 and -80 respectively. Kingman (2009: 135) remarks of the belatedness of Bartlett’s election: it “had to run the gauntlet of the disapproval of R. A. Fisher.”

subject opening up new pathways along which he could not hope to tread; and most of all, perhaps, the revealing fact that he felt regret rather than resentment.”

There was little in the *Journal* to show that Fisher was missed in his years away—he was more noticed in *Biometrika*. 1929, however, saw an account of his multiple correlation work in the *RS Proceedings* from the old Karlovingian Herbert Soper (1929) and praise for his book—in its second edition—from Oscar Irwin (1929: 103): “one which neither the biological or the mathematical statistician can afford to neglect.” J. O. Irwin (elected 1925-6) was one of a new post-war generation to whom Fisher mattered. Another was John Wishart (elected 1928-9) and like Irwin, a Karlovingian who was reborn a Piscatorial after working for Fisher. Yet another was Egon Pearson, Karl’s son, though it was action at a distance that produced works like Neyman and Pearson (1928). Egon’s election was announced at the same time as Fisher’s.²⁰

In 1931/2 there were two significant retirements which led to the most important positions in mathematical statistics falling to the Society and, directly or indirectly, to Fisher.²¹ When the non-Fellow Karl Pearson retired at University College, he was succeeded by two Fellows: Fisher as professor of Eugenics and Egon Pearson as head of the Department of Applied Statistics. At the Cambridge School of Agriculture one Fellow—Yule—retired to be replaced by another—Wishart—but with the significant change that Wishart’s students included future statisticians like Maurice Bartlett and

²⁰ Fisher had a following in the American Statistical Association where the economists Mordecai Ezekiel and Henry Schultz noticed his work and Harold Hotelling championed it; see Aldrich (2010a: §3; 2007/10).

²¹ For the new players, J. O Irwin, J. Wishart, E. S. Pearson, J. Neyman, etc., see Aldrich (2010a: §6).

Henry Daniels who joined the Society. Fisher had left Rothamsted but his successor as Head of Statistics, Frank Yates, became a Fellow.

Having been manoeuvred into the Society, Fisher did not rush to participate: he was in touch with everybody he wanted to reach and his publication needs were being met by mathematics journals: the *Proceedings* of the RS and of the CPS provided a platform for fiducial inference and for controversies with Haldane and Jeffreys.²² Things began to change in 1933. On April 12th the Society made the biggest change in direction in its century of existence when the Council decided to form an Industrial and Agricultural Research Section—its own journal, the *Supplement*, followed in 1934. While the change brought the Society closer to Fisher's interests and was testament to his intellectual influence, he appears to have had no direct part in achieving it.²³ Soon after the critical meeting Fisher accepted an invitation to join the Council, though he limited his commitment—"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." He remained on the Council until 1936 when he was sacked for poor attendance.²⁴ From Fisher's correspondence with the Society it appears that his pet project was to have Gosset awarded its highest honour, the Guy medal in gold; living recipients included Flux, Greenwood and Yule. Fisher's bid was unsuccessful.

²² For fiducial inference and these controversies see Aldrich (2000; 2005).

²³ The Council meeting is described by Aldrich (2010a: §6).

²⁴ In April 1933 he accepted the invitation, in April 1936 he was told he was being stood down for poor attendance.

3 “The Society’s authorities on matters theoretical”

Fisher became a public presence in the Society in 1934 when he started speaking at its meetings. These had a set format: to give “plenty of time for discussion” the author had around 20 minutes to present a summary and discussion followed, based on copies of the paper available in galley proof.²⁵ A record of the discussion was printed along with the paper; the contributions and the author’s reply might be as long as short papers.

The meetings of 1934-5 on matters theoretical show how interested fellows divided into unreformed pre-Fisherians and Fisher off-spring. Most of the latter either thought his work needed tidying up or wanted to go beyond it and such tempered admiration brought Fisher small comfort, or so the record of the meetings seems to show. Things started amicably: in November 1933 the Council approved two new fellows, Gosset and Yates, both proposed by Fisher and seconded by Egon Pearson.

At the first meeting of the Industrial and Agricultural in January 1934 Fisher was in the chair when Wishart spoke on “Statistics in agricultural research.” The welcome turned icy when Fisher (1934: 52) criticised Wishart and others, including Irwin, for questioning his work though he was “glad to learn, since the meeting, that Dr. Wishart recognises the validity of the proof given in *Metron* [1925].”²⁶ Another statistician who displeased Fisher by expressing the view that his work had not been done properly was the American visitor Samuel Wilks—see Box (266).

Others inspired by Fisher wanted to go beyond him. The Karlovingian era had lasted for decades but the Piscatorial would have no such longevity. In June 1934 Neyman, a visitor working with Egon Pearson, spoke to a general meeting about sample survey

²⁵ When Fisher spoke he was told, “I think you can safely take 25 minutes.”

²⁶ See Box (267).

methodology.²⁷ No meeting of the Society had ever debated the fundamental principles of statistical inference and they were being reviewed because Neyman wanted to change the principles on which “the representative method” was based. Bowley was responsible for the established principles and Neyman (1934: 561-3) quoted him on the division between direct and inverse probabilities, noting Bowley’s willingness to proceed with inverse (Bayesian) inference conscious that it is “based on assumptions that are difficult to verify and which are not applicable in all cases.” However, Neyman (562) had good news:

an approach to problems of this type has [since] been suggested by Professor R. A. Fisher which removes the difficulties involved in the lack of knowledge of the *a priori* probability law. [...]

Fisher’s theory becomes, I think, the very basis of the theory of representative method.

Neyman appears to have believed that Bowley was unaware of the work and how it removed his difficulties when Bowley had followed Fisher’s work and rejected it.²⁸

Proposing the vote of thanks, Bowley (606) described how the paper “will be found to answer most of the questions which relate to the setting out of an investigation by sample.” However, he (608-9) felt obliged to comment on the inference machinery:

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 [*“The Theory of Probabilities a posteriori and the work of R. A. Fisher”*]

Understandably, Bowley perceived Neyman as speaking for Fisher but this was a misperception as Neyman saw himself as building on Fisher’s work—not expounding it.

²⁷ For Neyman’s paper see Reid (113-119) and Lehmann (2010).

²⁸ See Aldrich (2007) for Fisher and Bowley.

The inferential novelty was the confidence interval which Neyman presented as a development of Fisher's fiducial probability.²⁹ Bowley (609) attacked the concept and, by implication, Fisher's work:

I am not at all sure that the "confidence" is not a "confidence trick." [...] Does it really lead us towards what we need—the chance that in the universe which we are sampling the proportion is within these certain limits? I think it does not.

Bowley (608) was "very glad Professor Fisher is present, as it is his work that Dr. Neyman has accepted and incorporated."

Fisher had not come to discuss what was for him a settled matter and used his presence otherwise—first (614-6) to report on related sampling work in agriculture with which he had been associated and then (616-9) to "comment on those applications of inductive logic which constituted so illuminating and refreshing an aspect of the evening's paper." Fisher echoed Neyman's remarks on how inverse probability had been superseded but his (617) present concern was with the relationship between Neyman's confidence and his own fiducial probability:

Dr. Neyman claimed to have generalized the argument of fiducial probability, and he had every reason to be proud of the line of argument he had developed for its perfect clarity. The generalization was a wide and very handsome one, but it had been erected at considerable expense, and it was perhaps as well to count the cost [..]

Fisher treated Neyman as a colleague and the differences between them matters of advantages and disadvantages, not of truth and error or of competence and incompetence.

²⁹ Zabell (1992) discusses the relationship between the two concepts and Reid (1982) the relationship between Neyman and Fisher.

In the pipeline was another meeting that would produce a clash of duty with courtesy and of generation with generation. In December 1933 the Council with Fisher present arranged for him to read a paper. In May 1934 H. W. Macrosty reminded him that he had promised a paper and, wanting to start things off “with a jump,” confirmed the date in December. This was Fisher’s first address to the Society and he (1935: 39) presented himself as an outsider of recognised standing:

When the invitation of your Council was extended to me to address the 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.

When he next presented—in 1952—it was as President of “our” Society.

If Fisher expected a coronation or even a dutiful hearing, he had misjudged. The vote of thanks was moved and seconded by Bowley and Isserlis. The report shows that they had a peculiar interpretation of their duties. After compliments—Bowley’s (1935: 55), “I am glad to have this opportunity of thanking Professor Fisher, not so much for the paper that he has just read to us, as for his contributions to statistics in general”—came, “It is not the custom, when the Council invites a member to propose a vote of thanks on a paper, to instruct him to bless it” and Isserlis’s (1935: 57) reference to “the ordinary privilege of proposer or seconder on these occasions, of treating an author’s paper somewhat critically.” Their common approach may have seemed like—and even been—a case of ganging up but the critical treatment was more an accumulation of grumbles than

an attack. Bowley was a well-known supporter of inverse probability and, while Isserlis worked on distribution theory, his (1936) shows him a supporter.

Their attitude was the first thing Fisher (1935: 76) mentioned in his written reply:

The acerbity, to use no stronger term, with which the customary vote of thanks has been moved and seconded [...] 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 today'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.

This victim statement may describe how Fisher felt about his relationship with the Society's "authorities" but it is hard to see evidence for the implicit claim that people like Bowley and Isserlis blocked the admission of his ideas into the Society. Bowley had been sceptical about the Royal Society developments but Fisher had excluded himself from the Society—until 1929—over matters unconnected with those developments and he had not been banging on its door.

Neyman and Pearson were among the discussants: they too were critical for, while they recognised Fisher's achievement, they wanted to go beyond it. To them, however, Fisher (1935: 82) was condescendingly benign: it had been of "great interest to me to follow the attempts which Drs Neyman and Pearson have made to develop a theory of estimation independent of some of the concepts I have used." He made no objections to their new concepts such as power—nor had he when he refereed their 1933 paper.

Conflict next appeared in the *Industrial and Agricultural* in which Bowley and Isserlis had no part. In March 1935 Fisher's benign interest turned to horror when he spoke at the

presentation of Neyman, Iwazskiewicz and Kołodziejczyk's "Statistical problems in agricultural experimentation" (1935). According to the report (154),

[Fisher] 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.

Fisher attributed "the series of misunderstandings" in the paper to "the persistent efforts which Dr. Neyman and Dr. Pearson had made to treat what they speak of as problems of estimation, by means merely of tests of significance."

Neyman's reply was restrained but the reported words (170) of his partner E. S. Pearson were not:

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.

Fisher's relations with Neyman and Pearson never recovered; in *Design of Experiments* he was openly critical of the Neyman and Pearson approach.³⁰

Meetings could be sunshine, as in May 1935 when Yates spoke to the Industrial and Agricultural on "complex experiments": according to the report (230), Fisher

said he would like to add his congratulations to those that had gone before, to Mr. Yates for what he thought, as he listened to it, to be not only an extraordinarily comprehensive but a surprisingly lucid statement of this subject.

³⁰ From a biographical point of view the change in Fisher's attitude to Neyman's work over 1934-5 is fascinating but Box (262-4) does not consider it.

Although Fisher had left Rothamsted, he continued to live nearby and to spend a lot of time there. Yates collaborated with him on the *Statistical Tables* (1938) and he would be Fisher's most constant intellectual companion.

All should have been sunshine at Fisher's next public appearance in March 1936 when Gosset presented a paper to the Section. Like Fisher's own appearance in December 1934, this should have been a triumphant occasion for it was public recognition of Student's entry into the Society. The occasion was neither triumph nor disaster. Gosset spoke on "Co-operation in large-scale experiments" and Fisher expressed disappointment at the scope of the discourse but he also criticised the content of "an Appendix on a controversial side-issue of less importance" treating Beaven's half-drill strip design. Gosset (136) replied in his usual genial manner:

That is an old matter of controversy between Professor Fisher and myself. He says to me, "Your half-drill strips have no validity and conclusions cannot be drawn from them"; I say to him, "Your errors are so large that no conclusions are drawn." Neither of these criticisms is true, and the one is about as good as the other.

The disagreement escalated with publications on both sides: "the debate grew hot" comments Box (269) in her account of the episode.³¹

After this hectic period Fisher's participation in meetings fell off. In March 1939 he was discussant for another presentation from Rothamsted by W. G. Cochran: this Fisher (1939: 143) judged was "clearly the outcome of practical and competent experience in all the subjects with which it dealt." Societies keep their members informed of other research in their field. Fisher did not need a society for this purpose for the only 'other' that

³¹ For more see Picard (1980: 51-54).

interested him was Rothamsted with which he was in constant communication. He went to the Rothamsted presentations to support them.

Turning from appearance in the flesh to appearance in print, Fisher's name appeared more often in the *Journal* than it had in the 20s though curiously his *Design of Experiments* was not reviewed. He did not look to the Society to publish his own research. He had found other outlets in the 20s and his appointment to the chair of eugenics brought him his own journal, the *Annals of Eugenics*; Karl Pearson had founded it for the "scientific study of racial problems" but Fisher turned it into his own version of *Biometrika*.³² The statistical content of the *Annals* increased and, after a dispute with one of his old outlets, the Cambridge Philosophical Society *Proceedings*, it became the main outlet for his statistical papers. In 1936 Fisher resigned in protest at the way the *Proceedings* handled a criticism of his fiducial argument—Bartlett (1936).³³ Although Bartlett and Fisher had been corresponding on the matter, Fisher did not see Bartlett's paper until it was published. Fisher was offended by the discourtesy of the *Proceedings* and more offended by the limitations it placed on his reply—this appeared in the *Annals* as Fisher (1937). As in the earlier episode with the RSS, Fisher re-entered the Society. This time the tactful negotiator was Jeffreys who gave Fisher an inspired justification, "My motive is the selfish one that I like to have my mistakes pointed out to me before publication."³⁴ Fisher returned to the CPS in 1940 but published nothing more with them.

³² Box (280-1) describes Fisher's editorial style.

³³ Bartlett was a great synthesist and in the 30s he worked to bring together the ideas of Fisher, Jeffreys and Neyman-Pearson.

³⁴ The relevant letters (Bartlett-Fisher and Jeffreys-Fisher) are in Bennett (1990: 50 ff.; 349-52).

4 In absentia

The Second World War changed the Society: the traditional ways of training statisticians were disrupted but membership grew and among the entrants were Cambridge mathematics graduates—like Fisher but unlike him in that Cambridge mathematics had changed in the intervening 30 years.³⁵ With his move to the chair of genetics at Cambridge in 1943 he lost control of the *Annals of Eugenics* and subsequently published only one article there. His old books appeared in new editions but his new book of the 40s was the *Theory of Inbreeding*.

The war closed down the Industrial and Agricultural Section—Fisher’s chief link with the Society. In 1945 there was a major reorganisation of the Society’s sections and journals: the Industrial and Agricultural became the Research Section and the *Supplement*, renamed, *Series B*, accepted articles as theoretical as those Fisher used to send to the CPS or the RS. *JRSSB* and *Biometrika* now served as the home journals for theoretical statisticians. The RSS changes do not appear to have involved Fisher. “As you probably know, a Research Section has been formed” Daniels wrote in June 1945 when he invited Fisher to discuss Barnard’s paper to be presented at the inaugural meeting: George Barnard (1915-2002) was one of the Cambridge mathematicians recruited through the war. Fisher did not attend. He also stood aside from other Research Section developments like the nurturing of stochastic processes: he was invited to attend the famous 1949 symposium but did not.³⁶

Fisher’s business with the Society became honorific, contracted and one-sided: in 1946 he received the Guy medal in gold, as Box (418) records; he served on the Council in the sessions 1948-9, 49-50, 50-51 and became President in 1952. However, he rarely

³⁵ See Aldrich (2019b) for mathematical statistics at war.

³⁶ See Aldrich (2018) for the development of stochastic processes in Britain.

attended Council meetings and was present at only a few public meetings to discuss Rothamsted work by Yates and Anscombe; otherwise, he did not publish in the Journals.

There were better causes than the Society such as the post-war reconstruction of the International Statistical Institute; see Box (431-5). Yet for Fisher *the* institutional development of the decade was the founding of the International Biometric Society, for which he had been waiting all his life. The IBS was probably his ideal society: he identified with its mission, was involved in its running, was its first president and published in its journal.³⁷ He was proud to be part of biometry, “the active pursuit of biological knowledge by quantitative methods” (1948: 218), and valued (217) its place in history:

The rise of biometry in this 20th century, like that of geometry in the third century before Christ, seems to mark out one of the great ages or critical periods in the advance of human understanding.

This remark was elaborated in a historical sketch which focussed on Galton and did not mention Karl Pearson.

Fisher was at home in the IBS: Cox (2016: 752) recalls meetings of the British Region when

Fisher and Yates would enter [15 minutes late], having had a pub lunch, and would walk slowly to two seats in the front row [...]. The poor speaker often was unclear whether to stop, review the first portion of the lecture, or what. Yates’s deafness meant he spoke rather loudly and sometimes private comments about the talk from Yates to Fisher, which were by no means necessarily favourable, echoed around the room.

Cox remembers these as “hard times” but not presumably for Fisher.

³⁷ See Box (428), Billard (2014), Hall (2010) and Stigler (2007).

The IBS journal, *Biometrics*, replaced the *Annals of Eugenics* as outlet for Fisher's statistical papers; his biological papers went to *Heredity* a journal he founded with C. D. Darlington in 1947 (see Box (402)). The *Biometrics* era came to an end when he had a dispute with the editor, Gertrude Cox, over the refereeing of his (1954); see Box (430) and Hall (2010: 216-9). Fisher did not resign from the Society but the articles ceased. Yet he was remembered in the IBS and when he died *Biometrics* devoted an issue (June 1964) to "In Memoriam: Ronald Aylmer Fisher, 1890-1962."

5 In?

The RSS Presidency was one more accolade but coincidentally it inaugurated Fisher's final and probably most satisfying decade with the Society. He had what he had wanted in 1920—somewhere to publish his papers. There was again a need for he was boycotting *Biometrics* and his pre-war outlets, the CPS and the RS, were no longer so important for mathematical statistics. These years also saw his retirement from Cambridge in 1957 and his move to Australia in 1959—see Box (ch. 18).

Fisher's Presidential Address (1953: 1-2) celebrated "what we can now recognise as a unitary discipline", viz., Statistical Science. This had become a vital discipline for, as "members of the present audience" know, "it is to the statistician the present age turns for what is most essential in all its more important activities." Fisher (6) ended with the future of "our" Society:

our Society has taken an appropriately vigorous part in this tidal movement of our century, which I have ventured to call the Expansion of Statistics; and [...] there is every prospect that it will continue to exercise, in developments not yet to be foreseen, a wise and helpful understanding.

Yet the praise was less fulsome than that in “Biometry.” The “tidal movement” came from natural science and especially biology and “our Society” had no significant part in its origins. Thought of Darwin and Wallace took Fisher (4-5) to Malthus, whose “distinguished part in the foundation of this Society must predispose us in his favour,” but only to find that “it is scarcely his logic but his rhetoric, which has gone to their heads.”

In his time as President Fisher was not associated with any great initiatives though at a Council meeting in November 1953 he got unanimous approval for a letter he wished to send to the *Dictionary of National Biography* calling attention to the omission of Thomas Bayes.³⁸ Fisher did not return to participating in meetings though on one occasion he contributed (in writing) to the discussion of a paper by Monica Creasy to the Research Section. He (1954: 213) explained, “I did not take part in the discussion in the Research Section on some supposed paradoxes propounded by Miss Creasy, [...] principally because I could not understand the reasoning from which it was supposed the paradoxes arose.”³⁹ Incomprehensibility was rather a familiar barrier between Fisher and the new generation. They were not being taught properly as he told John Nelder (1924-2010)—who would later succeed Yates at Rothamsted—in February 1956:

Of course I have known for some time how poor an opportunity of mastering mathematical statistics is offered by this University. I suppose the misapprehension as to what Cochran and Cox said about my formula $(n+1)/(n+3)$ was due either to your misunderstanding one of the teachers here, or to his own misapprehension on this point.⁴⁰

³⁸ Nothing came of this Bayesian revival beyond a piece in *Biometrika*—Barnard (1958). For Fisher’s complicated attitude to Bayes see Aldrich (2008b).

³⁹ Wallace (1980) describes the issues.

⁴⁰ The “misapprehension” came to light in Nelder (1956); see Aldrich (2007: §10).

The only new generation statistician Fisher published with was Michael Healy (1923-2016). Healey later recalled (2003: 303), “I even did some computing for him and got my name on a joint publication [(1956c)] relating to perhaps the least useful task that he ever undertook.” The only new British statistician with whom Fisher had a productive collaborative relationship was George Barnard; Fisher chose him as a Vice-President when he became President and the two had a close intellectual relationship in the last decade of Fisher’s life.⁴¹

Fisher’s book of the 50s was *Statistical Methods and Scientific Inference* (1956). By subject-matter this was his *Series B* book and several *Series B* articles were associated with it. It was heralded by “Statistical methods and scientific induction” (1955): where twenty years before “The logic of inductive inference” had crystallised Fisher’s work in statistical inference, this one catalogued the misunderstandings of Neyman and Pearson, Bartlett—his 1930s adversaries—and Wald.⁴² Three *Series B* notes—Fisher (1956b, -c and -57)—were continuations of 1930s debates with 1930s opponents but one (1960) involved a new adversary that *Statistical Methods and Scientific Inference* had generated, Denis Lindley (1923-2013) author of (1957 and -8). A referee of Fisher (1960) echoed Greenwood in 1920: “the paper is marred I think by the suggestion of incompetence on the part of Lindley.”⁴³

⁴¹ Barnard gave his own account of his relations with Fisher in DeGroot (1988). Barnard was an RSS insider but Fisher also had productive relations with outsiders like the Canadian statistician David Sprott (1930-2013).

⁴² For a summary see Aldrich (2007: §10).

⁴³ See Smith (1995: 310-1) for Lindley’s recollections and also Aldrich (2008: §5).

6 Being *in* a society

Learned societies promote learning in various ways, including publishing journals, holding meetings and distributing honours, and members contribute to their societies in different capacities. Fisher belonged to several societies, including the Royal and the international IBS and ISI, in different ways enjoying the benefits of membership and contributing time and energy to the running of the society. Yates and Mather (1963: 97) recall

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.

These other societies played a bigger part in Fisher's life—and he in theirs—than the RSS.

Fisher's transactions with the Stats were spread over four decades and took different forms but one constant—despite the changing personnel—was a sense of being an outsider. In the Karlovingian era Pearson knew he had large territories and that he was their ruler but in the Piscatorial the emperor could feel like a fugitive. An impression of what being *in* the RSS could be like—and how an outsider might feel being excluded—is given by the career of Yule. From joining aged 24, the Society was *his* society, where he met friends, reported his research and discussed that of others; he edited the journal, served on the Council and received its honours. Reviewing the history of the Society and recalling his life in it, Yule (1934: 680) had “many memories: of older Fellows who were kind and encouraging in one's younger days, of colleagues on the Council and in the office of Honorary Secretary, of pleasant meetings of the old-established Dinner Club, founded in 1839.” Presumably that experience guided the actions of Yule—a person remembered by Kendall (1952: 159) as “kindly, gentle and genial”—towards Fisher. He,

though, would not have associated the words “kind and encouraging” and “pleasant” with the Society.

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