# Fisher: A Retrospective

## George Barnard

### Statistician and Geneticist

Future visitors to Cambridge, England who go into the hall of Caius College may be puzzled by a stained glass window made up of 49 square pieces of glass, 7 red (R), 7 blue (B), 7 green (G), 7 orange (O), 7 brown (Br), 7 yellow (Y), and 7 purple (P). The pieces are arranged in 7 rows of 7 pieces each:

| R | B | G | O | Br | Y | P |
|---|---|---|---|----|---|---|
| B | Y | R | P | G | Br | O |
| Y | Br | P | B | O | R | G |
| Br | O | Y | G | B | P | R |
| O | P | B | Y | R | G | Br |
| G | R | O | Br | P | B | Y |
| P | G | Br | R | Y | O | B |

so that each color appears once in each row and once in each column. The pieces form a "Latin square." The Master and Fellows of Caius are having the window put there in celebration of the centennial, February 17, 1990, of the birth of Sir Ronald Aylmer Fisher, who first came to their college as a scholar in 1909, who published his first scientific paper when still an undergraduate in 1912, and who was a Fellow from 1920 to 1926 and again from 1943, having returned to Cambridge as

Arthur Balfour Professor of Genetics. He served the college as its president from 1956 until 1959. After touring in the U.S. he settled in Adelaide, Australia, where he died in 1962.

The Latin square is one of the many types of experimental design systematically studied by Fisher. If the whole square represents a rectangular field, and each small square represents a plot in the field, and each color represents a particular strain of wheat, then the pattern represents a possible layout for an experiment intended to compare the yields of the various strains on a field in which there may be fertility gradients, due to drainage pipes or to ploughing patterns parallel to the sides of the field. Any such differences can be eliminated from the yield comparisons because of the balance properties in a Latin square. Although special types of Latin square designs had been used before, Fisher was the first one to develop a general theory and in particular to stress the importance of *randomization* to ensure balance with respect to the many unknown factors, apart from these two kinds of fertility gradient, that could affect the yields of particular plots.

The foundation of the general theory of design of experiments was one result of Fisher's only period of employment as a statistician, at Rothamsted Experimental Station, from 1919 until 1933. In the latter year he was invited to stand for the Galton Professorship of Eugenics at University College, London. He had hoped to succeed Karl Pearson as head of the Department of Applied Statistics, combined with directorship of the Galton Laboratory. But the university thought that Egon Pearson, who had worked in his father's department for 10 years, could not be simply passed over. So Egon was appointed as Reader (the equivalent of associate professor) to head a separate department of statistics. It thus came about that for the whole of Fisher's *academic* life he was employed as a geneticist, not as a statistician. Not that what he was paid to do had an overwhelming influence on what Fisher actually did. While he worked at statistics at Rothamsted during "working hours" he also, with the help of his patient wife and family, used his house and garden to breed mice, snails, and other animals for genetical experiments. (His own large family led to the quip that he was the only member of the Eugenic Education Society who had

the courage of his convictions.) And his appointment to the Galton Chair merely exchanged what he was paid to do with what he did unpaid from scientific enthusiasm.

In the 50 years from 1912 until 1962, he published 140 papers on genetics, 129 on statistics, and 16 on other topics, besides repeated editions of 4 books and numerous reviews.

For Fisher's current reputation as a biologist we may quote that remarkable essay in scientific popularization *The Blind Watchmaker* by the Oxford biologist Richard Dawkins:

> ... For me the peacock's fan has the unmistakable stamp of positive feedback. It is clearly the product of some kind of uncontrolled, unstable explosion that took place in evolutionary time. So thought Darwin in his theory of sexual selection and so, explicitly and in so many words, thought the greatest of his successors, R.A. Fisher. After a short piece of reasoning he concluded (in his book *The Genetical Theory of Natural Selection*):

> 'plumage development in the male, and sexual preference for such developments in the female, must thus advance together, and so long as the process is unchecked by severe counterselection, will advance with ever-increasing speed. In the total absence of such checks, it is easy to see that the speed of development will be proportional to the development already attained, which will therefore increase with time exponentially, or in geometric progression.'

> It is typical of Fisher that what he found 'easy to see' was not fully understood by others until half a century later. He did not bother to spell out his assertion that the evolution of sexually attractive plumage might advance with ever-increasing speed, exponentially, explosively. It took the rest of the biological world some 50 years to catch up and finally reconstruct in full the kind of mathematical argument that Fisher must have used, either on paper or in his head, to prove the point to himself."

The statistical world found understanding Fisher almost equally
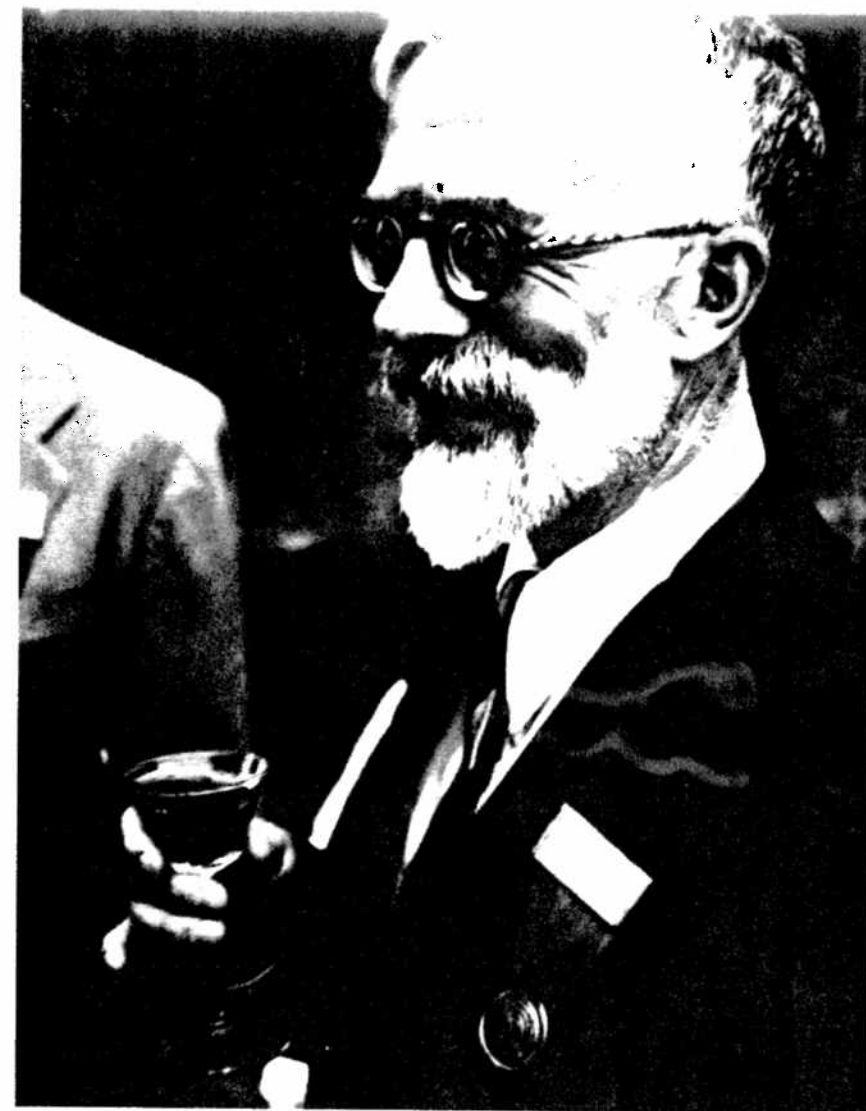


**R.A. Fisher at 1947 meeting.**

difficult. In his memoir of "Student" (W.S. Gosset), shortly to be published, Egon Pearson tells how in the late twenties, after reading Fisher, he had perceived the need for a "Mark II" statistics to develop and to some extent to replace his father's "Mark I." But all he had to go on was the non-mathematical account in Fisher's "Statistical Methods for Research Workers" (SMRW) together with some very densely written and scarcely accessible papers. Pearson also refers to my own account of having first met Fisher in 1933, in connection with the analysis of an opinion survey I had carried out at my school. When I told Fisher I had been looking for mathematical texts on statistics

without success he pointed out a copy of SMRW and said, "I believe you're a mathematician." I told him that I hoped to become one. He then said, "You'll find in this book a lot of statements given without proof. If you're a mathematician you should be able to prove these things for yourself. If you work through the book doing that, you'll learn mathematical statistics." I next met Fisher nearly twenty years later when he became president of the Royal Statistical Society. To my astonishment I found he had appointed me as one of his four vice-presidents. I was then able to tell him I had just completed the task he had set for me during our first meeting.

## Fisher, the Geneticist

As a child and young man, Fisher was something of a prodigy. He entered Harrow, 10 years after young Winston Churchill left it, with the top mathematical scholarship, and soon after won the first of his many gold medals. His early academic brilliance was specially fortunate because his father had recently been financially ruined. In 1908 Fisher won a major scholarship to Caius College, Cambridge, and there he was again most fortunate in having as his tutor F.J.M. Stratton, an astronomer of exceptionally wide interests outside his specialty, who encouraged so many young men in the course of a long life that the collection of papers written by former pupils to celebrate his 70th birthday in 1951 ran to several large volumes. It was Stratton who encouraged Fisher to publish his first scientific paper in 1912.

In that 1912 paper, advocating what later came to be called the method of maximum likelihood as an "absolute" method of fitting frequency curves, we find the first hint of Fisher's fundamental contribution to scientific method—his insight that there are at least two distinct types of *measurable* uncertainty: classical, or frequency probability, and what he later called "likelihood." It seems likely that both this and his greatest contribution to biology arose in his mind at that time because of his involvement with genetics.

The Mendelian theory is now the agreed basis of genetics and, along with natural selection, is the agreed basis of biological evolution, but when Fisher was an undergraduate Mendelism and evolution were hotly debated. After the early battles, Darwin's theory of the origin of species by natural selection had won wide acceptance by the time he died in 1882. But not long after that, doubts were raised as to whether in the apparently short time since the formation of the Earth natural selection could possibly have produced the widely differing species we see in the living world. A powerful advocate of the view that species must have originated in large, discontinuous "jumps" was William Bateson, and when Mendel's work was rediscovered Bateson seized on it as confirmation of his arguments. A debate, sometimes bitter, developed between Bateson and the Mendelians, on the one hand, and the "biometricians," led by Karl Pearson and his great friend Walter Weldon, on the other. The biometricians argued that the sharp, discontinuous differences dealt with by the Mendelians were the exception rather than the rule, and they tended to accept the current theory of "blending inheritance"; the Mendelians, impressed by discoveries such as that of Sir Archibald Garrod, who showed in 1908 that the distressing disease of phenylketonuria was inherited in strict accord with Mendel's laws, insisted on their universal validity.

Mendel's theory was the first in natural science to express its experimental predictions in terms of *exact frequency* probabilities—for example that the probability that a pea from a certain type of cross will be round rather than wrinkled is 3/4—not approximately 0.75, but 3/4. By "frequency probability" I mean one that relates to a repeatable procedure (in Mendel's case, repeated crossing of genetically similar plants or animals) and whose ultimate empirical justification lies in the agreement of the stated probability with a long run frequency. In the nineteenth century, science was dominated by deterministic theories—adding zinc to hydrochloric acid, under specified circumstances, *would* produce hydrogen and zinc chloride with certainty, not with any stated probability. True, in such fields as astronomy the observations themselves were subject to error, so that corresponding errors could arise in predictions. And in observational studies like those of Francis Galton, variations in body measurements were distributed *to sufficient approximation* in bivariate normal distributions. But the idea that nature itself could behave like a perfect gambling machine was revolutionary, and Mendel's work lay dormant for 35 years before being rediscovered in 1900.

## Fisher Invents "Likelihood"

Karl Pearson laid the foundations of modern mathematical statistics in the 1890s with his "Pearson family of distributions" aimed at *approximate* description of the natural variability of crab shells, fish lengths, etc. He produced his famous chi-squared test to check whether his fitted curves could be taken as sufficiently accurate. Years before, he had achieved worldwide fame as the author of *The Grammar of Science*, in which he had attacked the idea that *any* scientific prediction could be more than an approximation. Thus his mental cast made it difficult for him to accept Mendel's ideas.

Young Fisher, who shared Karl Pearson's enthusiasm for Darwinism, had a front-line view of the debate between the Mendelians and the biometricians because Bateson had been appointed to a Cambridge chair of biology the year before Fisher entered the university. In a talk entitled "Mendelism and Biometry" given on November 10, 1911, to the second undergraduate meeting of the Cambridge University Eugenics Society, Fisher made clear his view that, so far from there being a conflict between Darwinism and Mendelism, the one was a necessary complement to the other. Eight years later, on the biological

side he sustained this view with his paper "On the correlation between relatives on the supposition of Mendelian inheritance"—rejected by the Royal Society of London, but published by the Royal Society of Edinburgh, and since reprinted as a classic of genetics. On the statistical side, having been put in touch, by Stratton, with W.S. Gosset ("Student," of "Student's t") he gave a rigorous proof of Student's result on the probable error of a mean, and in 1915 he succeeded with a problem that had defeated the best efforts of Karl Pearson and a group of his assistants—the determination of the exact expression for the distribution of the estimated correlation coefficient $r$ in samples from a normal distribution with true correlation $\rho$. This was the beginning of a long series of papers, ending only in 1962, in which he derived almost all the standard distributional results associated with normal distributions.
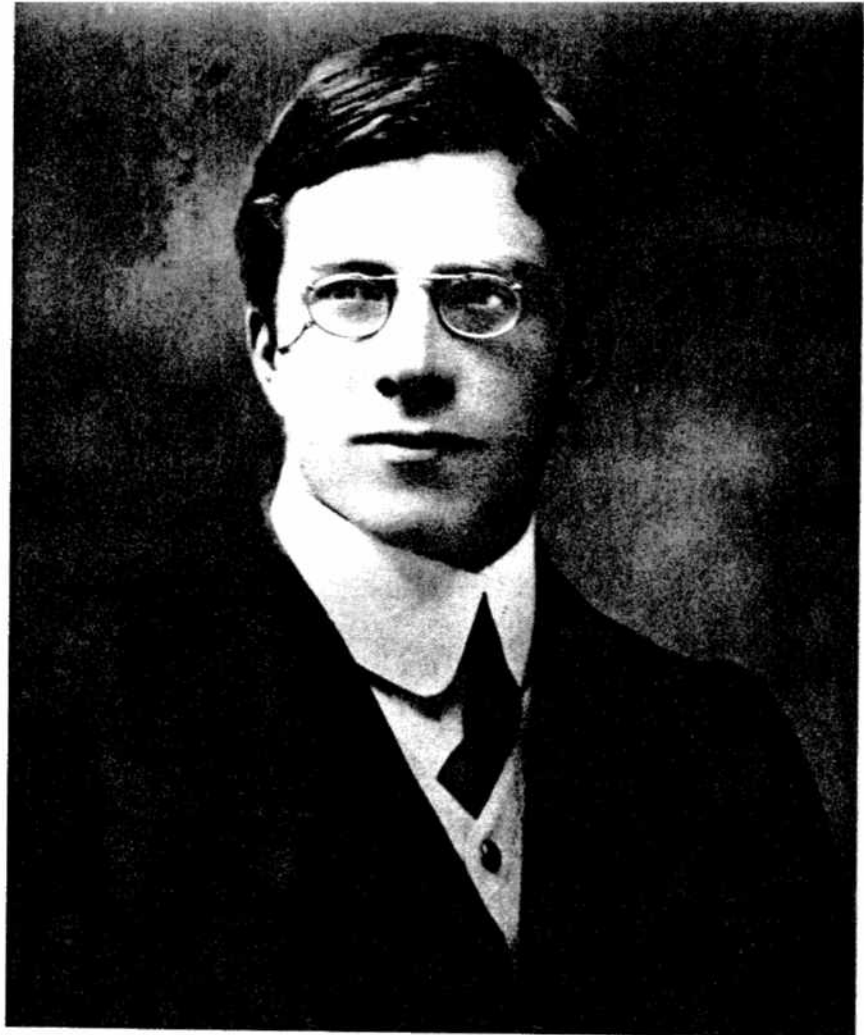
Fisher's concept of "likelihood," as the relative measure of credibility of one hypothesis as opposed to another, based on given data, took longer to evolve. In 1912 he had seen that what he later called likelihood was a relative measure only, fit to compare one hypothesis with another on the basis of given data, but differing from probability in having no absolute meaning. In 1915, discussing the correlation coefficient, he used the fact that likelihood differs from probability density in that the likelihood is unaffected by a scale transformation—but still without using the word. It was not until 1921 that he proposed the term "likelihood," saying that "probability and likelihood are quantities of an entirely different nature." Probabilities can meaningfully be added or, in the continuous case, integrated; likelihoods cannot. He stressed this later, saying "Whereas such a phrase as 'the probability of $A$ or



R.A. Fisher as steward at the First International Eugenics Congress in 1912.

Photo: Lecture Notes in Statistics, Volume 1, *R.A. Fisher: An Appreciation*, 2nd printing, Springer-Verlag

$B$' has a simple meaning, where $A$ and $B$ are mutually exclusive possibilities, the phrase 'the likelihood of $A$ or $B$' is more parallel with 'the income of Peter or Paul'—you cannot know what it is until you know which is meant."

The first lengthy exposition of the Method of Maximum Likelihood was published by the Royal Society in 1922. Karl Pearson had used various methods to estimate parameters—the method of moments, the method of minimum chi-squared and, on occasion, what would now be called an empirical Bayesian method. He applied the last to "correct" the maximum likelihood method used by Fisher in an example Fisher gave. Pearson's student Fröken Kirstine Smith (a pioneer in optimal experimental design) used minimum chi-squared. And in fitting curves of the Pearson

family to sets of measurements of a quantity $x$, he (Pearson) used the method of moments—equating the mean values of the first, second, third, and fourth powers of $x$ to their theoretical expressions. Fisher had continued to treat Pearson with the respect due from a younger man to an older pioneer for some years after 1915. But he devoted a large part of his 1922 paper to showing that unless the frequency curve in question was close to normal, the method of moments was highly inefficient compared with maximum likelihood.

This was the first of a series of open brushes with the 65-year-old Pearson that led to bitter animosity. Pearson wrote, in connection with his advocacy of his "empirical Bayes" method, "I know I have been preaching this doctrine vainly in the wilderness for many

years, and made a distinguished statistician a permanent enemy by suggesting it, but I believe it to be correct." His term "suggesting" hardly conveys the confident tone in which Pearson had made his "correction." More conflict was to come very soon when Fisher pointed out that Pearson's use of his chi-squared test failed to take account of the fitting of parameter values to those of the data. It may be that Fisher had learned that Pearson was one of the two referees who had rejected his paper on the correlation between relatives. Acrimony continued right into the 1930s, so that when Fisher asked Egon Pearson to approach his father with a view to their jointly proposing "Student" for Fellowship of the Royal Society, the son, who had his own problems in his family relationship, felt unable to do so. Even in 1956, 20 years after Karl Pearson died, much of Fisher's foreword to SMSI consisted of an unfair diatribe against his distinguished predecessor.

## Gains and Losses

Fisher's greatest direct service to humanity was perhaps his share in unraveling the genetics of the Rhesus factor, the understanding of which has saved the lives of hundreds of thousands of babies. A greater service almost certainly was his work on the design and analysis of agricultural experiments. This has played a large part in the agricultural revolution that has transformed the world's food problem from a global technical one into a local, essentially political one. The extension of his methods into the industrial and medical fields has also resulted in great advances. His greatest direct service to natural science was his demonstration that Mendelism is the necessary complement to Darwinism; but he did much else in genetics and he played an impor-

tant part at a critical time in the development of the geological theory of plate tectonics. In statistics the exact distributional theory of many tests of significance and the concomitant tightening up of their associated logic, the theory of likelihood in its application to the theory of estimation, and the theory and practice of experimental design are each very largely Fisher's creations. His work here was devoted to the development of natural science and its direct application to agriculture. He welcomed the work of Gosset, Shewhart, Egon Pearson, and their followers in the applications of statistics to industrial problems, but he did not himself participate.

Insofar as his criticisms of the work of Doll and Hill may have delayed the adoption of antismoking policies, Fisher could be said to have performed some disservice to humanity. And in statistics he displayed a curious unwillingness to give public acknowledgment of the value of the work of others unless their ideas chimed very closely with his own. Although as referee he approved for publication by the Royal Society the classic paper of Neyman and Pearson on the theory of hypothesis testing, and in reply to a letter from Neyman expressing thanks he indicated close interest in their result, his public approval was limited to a footnote in the introduction to SMRW indicating the connection between the Neyman-Pearson concept of "power" and his concept of likelihood. He repaid early encouragement from "Student" (W.S. Gosset) with enthusiastic propagation of Student's work in SMRW; but longstanding differences of opinion with "Student" concerning the relative importance of randomization as against other considerations in agricultural field trials led to a breach with his greatest friend that was never healed because of the latter's sudden death.

Worst of all, his labile temper,

developing to something close to paranoia, eventually led to the bitter breakdown of his marriage. This had begun as an idyll and in spite of occasional outbursts complaining of lack of love, it continued with his wife taking down by dictation, in longhand, the manuscript of the first edition of SMRW. She continued to assist with his genetic experiments, while faithfully bringing up his large family in financial straits, until the 1930s. It ended tragically in 1943 when, having finally left his family in Harpenden to occupy the residence of the Arthur Balfour Professor of Genetics at Cambridge, filling it with mice rather than with his wife and children, news came of the death on active service of their firstborn, George. Fisher returned for the last time to Harpenden, but the two parents who had each lost their dearest child could find no comfort in each other. Fisher was left a sad and lonely man for much of the rest of his life. Fisher's daughter, Joan Fisher Box, has written one of the most remarkable of all scientific biographies, describing expertly and in detail Fisher's biological and statistical work, in addition to his personal life. I recall her reading, in draft, parts of her chapter entitled "Losses of War" to an audience at the University of Waterloo. When she ended there was a period of silence—few in her audience were not close to tears.

## Scientific Interactions

Fisher's relations with other scientists were mixed. I was present when Fisher had a discussion on rock magnetism with Patrick Blackett in which the two strongly disagreed on the interpretation of the data. But after Fisher left, Blackett remarked to me, "You know, I like old Fisher." Fisher and Jeffreys enjoyed public arguments, and this vigorous jousting

misled many into thinking that Fisher wholly rejected Jeffreys's theories. In fact, Jeffreys, whose centennial we will be celebrating next year, got on extremely well with Fisher. When he found that Fisher had resigned from the Cambridge Philosophical Society because the Society had refused Fisher right of reply to a paper by Bartlett (not, as alleged by John Wishart, just because the Society had published Bartlett's paper), Jeffreys arranged for the Society to make amends to Fisher and so persuaded him to rejoin. Jeffreys was anxious to do this, he wrote to Fisher, "because you are the only man capable of sensibly ref-ereeing my papers." Fisher's se-lected biological correspondence has been published by the Oxford University Press, and the same press will shortly follow with his correspondence on statistical in-ference. The two volumes show him in a much softer light than in other accounts—particularly in his willingness to take pains when asked to explain his ideas to younger people. It also throws light on some otherwise puzzling incidents.

In 1934 he was belatedly invit-ed to read a paper to the Royal Statistical Society giving an ac-count of the developments in sta-tistical theory the implications of which had been expounded in SMRW and the practical value of which had been witnessed by the fact that five editions had been called for since the first publica-tion in 1925. In the resulting pa-per of 16 pages, 9 are devoted to a highly condensed account of the main results arrived at in his 61-page Royal Society paper of 1922 and his 28-page Cambridge Philo-sophical Society paper of 1925, with brief references to a 24-page Royal Society paper of 1934. These nine pages continue to be very well worth studying as a summary of Fisher's ideas, pro-vided the student has read the three earlier papers and a good

## Fisher on Likelihood and Inference

The concept of "likelihood" applies to *scientific* hypotheses—that is, hypoth-eses relating to *repeatable* experiments—which assert that observable events, $x$, $y$, $z$, have specified frequency probabilities. If $H$, $J$, $K$, are scientific hypotheses each of which specifies the probability of observable events $x$, $y$, $z$, and $H$ says the probabilities of $x$, $y$, $z$ are $p$, $q$, $r$, while $J$ replaces $p$, $q$, $r$ by $p'$, $q'$, $r'$ and $K$ replaces $p$, $q$, $r$, by $p''$, $q''$, $r''$ then if in fact $x$ occurs, the likelihood of $H$ relative to $K$ is $p/p''$ and the likelihood of $J$ relative to $K$ is $p'/p''$; if, instead, $y$ occurs, the likelihood of $H$ relative to $K$ is $q/q''$ and that of $J$ relative to $K$ is $q'/q''$. The disjunction "$H$ or $J$" does not specify the probability of $x$ beyond saying that it is either $p$ or $p'$; and "(either $p$ or $p'$)/$p'''$" is *either* $p/p''$ or it is $p'/p''$ and we can't know which it is until we know which of the two, $H$ or $J$, is meant. By contrast, if $x$ and $y$ are mutually exclusive, $H$, $J$ and $K$ specify the probability of "$x$ or $y$" as $p + q$, $p' + q'$, and $p'' + q''$; probabilities can be added, but likelihoods cannot. Likelihoods can be multiplied, howev-er: If in two independent experiments we observe first $x$, then $y$, the relative likelihoods of $H$, $J$, $K$ are in the ratio $xy$: $x'y'$: $x''y''$.

Hypotheses such as $H$, $J$, and $K$, occur frequently in applications of Mendelian theory. And, indeed, we can have situations in which $K$ could be regarded as equivalent to "$H$ or $J$"—as when a black mouse could be either homozygous ($H$) or heterozygous ($J$) and knowledge of the black mouse's parentage tells us, for instance, that $H$ and $J$ each have probability ½. In that case a full statement of $K$ would be "$H$ or $J$, each with probability ½." We would have $p'' = ½ (p + p')$, $q'' = ½ (q + q')$, etc., and these would be valid frequency probabilities in relation to repeated crossings of mice of similar parentage. But it was at one time popular to argue that if nothing was known of the parentage, mere ignorance as between $H$ and $J$ would allow us to assign equal probabilities to each of them. But such probabilities would not be valid frequency probabilities in relation to repeated crossings of mice of unknown parentage—for aught we knew, such mice might all be homozy-gous, and the true frequency probabilities would then be $p$, $q$, $r$, etc. As Fisher wrote in connection with a genetic example discussed in his book *Statistical Methods and Scientific Inference* (SMSI), "If knowledge of the mouse ... were lacking, no experimenter would feel he had warrant for arguing as if he knew that of which he was ignorant."

deal else besides. The remaining six pages were used, in Fisher's words, "to add a few novelties which might make the evening more interesting to those few among the audience who were al-ready familiar with the general ideas." Such wild overestimation of the capacity of his audience was typical of Fisher's lecturing style. The Royal Statistical Soci-ety always had, and it continues to cherish, a tradition of sharp criticism of papers read to it. But the reception of Fisher's paper represents a high point in sharp-ness, not to say rudeness in dis-cussion. Of the main part of the paper Professor Bowley, moving the "vote of thanks," said " ... I found the treatment to be very obscure. I took it as a weekend problem, and first tried it as an

acrostic, but I found I could not satisfy all the 'lights.' I tried it then as a crossword puzzle ... next as an anagram ... finally I thought it must be a cypher ... and Professor Fisher had hidden the key. ... " Bowley went on to suggest that Fisher had, without acknowledgment, taken over a re-sult proved by Edgeworth in 1908. Dr. Isserlis, the seconder of the vote of thanks, continued in the same vein.

Now Fisher was not given to "turning the other cheek" as his resignation from the Cambridge Philosphical Society testifies. I had long been puzzled to under-stand why Fisher had not also resigned from the Royal Statistical Society, until I learned that he had been tempted to resign from the Statistical Society some 10

years previously, because of their treatment of some of his submissions. He was dissuaded by his great friend and wise supporter Leonard Darwin (fourth surviving son of Charles). Knowing of Fisher's financial straits at the time, Darwin carefully and secretly asked Fisher if he would allow him to provide Fisher with a life subscription; and this he did.

Jeffreys and Fisher shared the view that statistical inference is *possible*. From 1935 onwards, Jerzy Neyman did not share that view. He asserted that statistics could be concerned only with inductive behavior, involving an act of will. The clash has proved fruitful in provoking developments from both camps. To enter into details would more than double the length of an already long article. Suffice it to mention the "2 × 2 table," such as one obtains as the result of many clinical trials. The most useful way of interpreting such a table is in terms of the ratio of the odds on "cure" with one treatment to the odds on "cure" with the other treatment. The best way of obtaining Neyman confidence limits for this ratio is given in one of Fisher's last papers. The best way of seeing the appropriateness of Fisher's "exact" test, strongly attacked by supporters of Neyman, is to look at the matter from the point of view of the Neyman-Pearson concept of power.

## Back to the Future

Statistics in general, and statistical inference in particular, continue to develop rapidly. The past 30 years have seen a growth in the "neo-Bayesian" doctrine according to which the likelihood from an experiment must be supplemented by a "prior" probability distribution expressing what we knew about the parameters of an experiment before we saw the experimental results; and much emphasis has been laid by some pro-ponents of this doctrine on the differences between these ideas and those that Fisher advocated. But Fisher's great friend W.S. Gosset regularly used a uniform prior, which gives results numerically equivalent to likelihoods, and Fisher did not bother to correct him. In our own time one of the two authors of the Box-Jenkins book on time series analysis is Bayesian while the other was Fisherian. I am one of those who share Fisher's view that there is a multiplicity of kinds of uncertainty, some quantifiable in a reasonably objective way, others not; and we do well to have different names for these different kinds. And whereas to make decisions we may need to add prior assumptions to the likelihoods resulting from our experiments, in forming our scientific view of the world decisions are often unnecessary—even undesirable—and likelihoods alone can tell us all we need.

I have indicated above that Fisher's ideas were not well received by the senior British "statistical establishment" of the 1920s and 1930s. The first really favorable review of SMRW to appear in any statistical journal was by Harold Hotelling, in the *Journal of the American Statistical Association*. Later, of course, Fisher was elected Fellow of the Royal Society and after receiving their Royal Medal, he was awarded the Copley Medal, the highest in the power of the Society to give. He was also knighted, in the same year as Gordon Richards, the champion jockey. Other institutions throughout the world awarded him honorary degrees and similar distinctions. Some 30 years ago, W. Allen Wallis was visiting London and we had a conversation in which Fisher's name came up. At the time I had the impression that schools of statistics in the United States were so strongly dominated by a heavily "pure mathematical" approach to statistical inference that what I saw as Fisher's "likelihood" approach seemed more or less in eclipse. I was somewhat surprised, therefore, to hear Allen Wallis say that he broadly categorized distinguished scientists into "once in a generation" persons, "once in a century" persons, and "once in five hundred years" persons such as Isaac Newton and Charles Darwin. He indicated that Fisher, in his opinion, was near enough to the top of the "once in a century" category to make it doubtful whether he might not in time be promoted.

## Additional Reading

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*We wrote to statisticians asking them for R.A. Fisher's greatest accomplishment, the impact his ideas have had upon their own work, or their most vivid personal recollection of Fisher. Here are responses from four countries.*

# Comments from Selected Statisticians on Fisher

My first encounter of R.A. Fisher in 1951 is also my most vivid. He was in the chair for an afternoon meeting of the British Region of the Biometric Society. The format consisted of the presentation of three short papers. To me, a graduate student at University College London, the first talk seemed fine. But Fisher in words indelibly in my mind, intoned: "I thought we were going to hear something new from Dr. . . . but everything he told us has been said elsewhere and better said elsewhere." Subsequently in 1953 in Sydney, Fisher was very kind to me in a half-hour conversation, even recommending that I should do graduate work abroad!

> Herbert A. David
> Iowa State University
> United States

To discover Fisher's book *Statistical Methods and Scientific Inference* was a personal, ever wondrous revelation. I quailed as the false prophets Pearson, Bayes, and Neyman were consigned to the flames. I banged my head against the age-old Problem of the Nile. I fell in love with the fragile beauty of Fiducial Probability and



Oscar Kempthorne (left in photo; from his personal collection) with R.A. Fisher during a short visit to Ames, Iowa in 1952.

*Property of the Statistical Laboratory, Iowa State University, Ames, IA.*
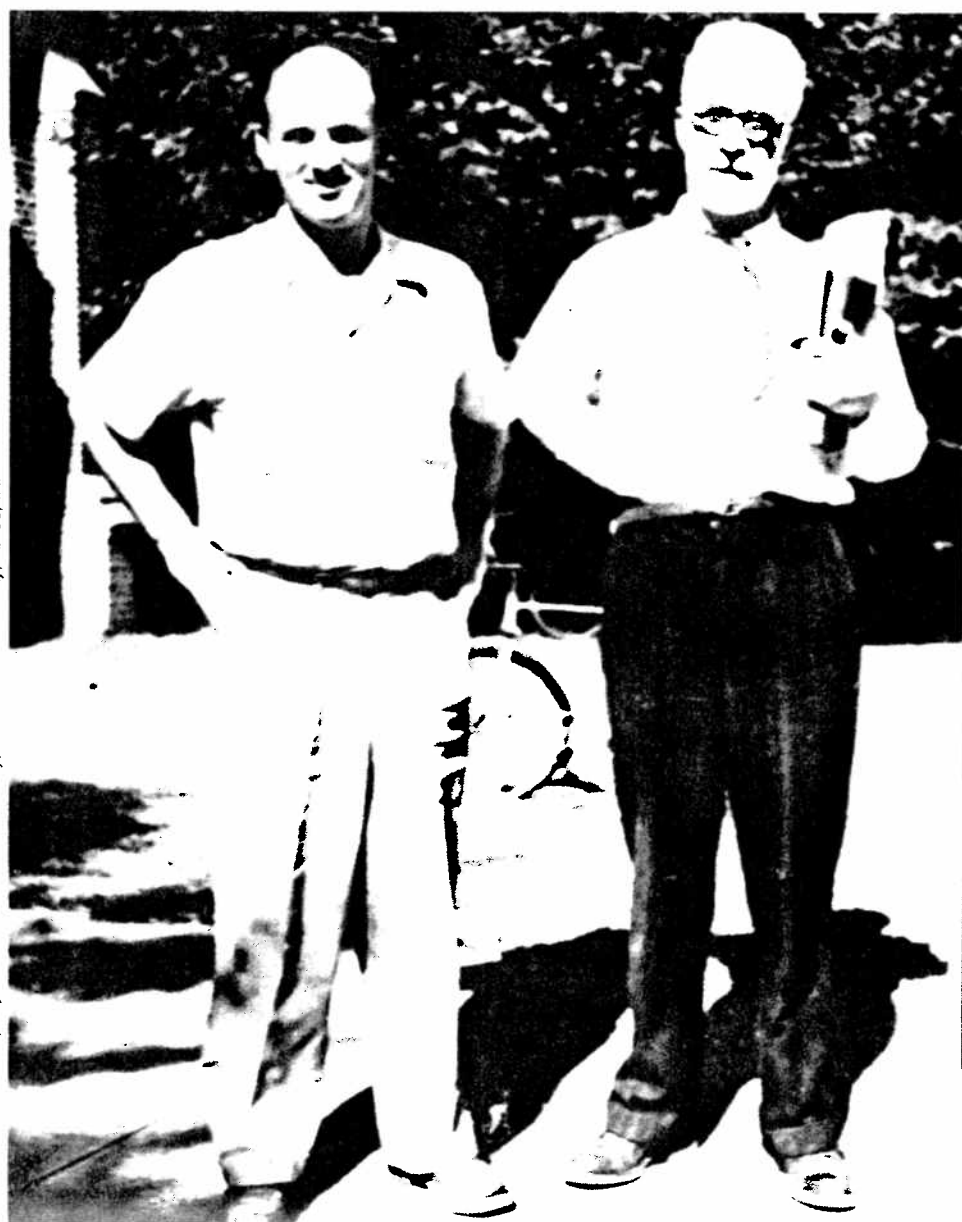
was perplexed by her association with likelihood. True, my further delvings into the mysteries of conditional inference have but hardened me in my Bayesian heresy, but still I journey on, thirstily, in an endless quest for the deeper meanings of Fisher's delphic utterances.

*A.P. Dawid*
*University College London*
*United Kingdom*

Time is slowly revealing the depth and substance of Fisher's work: A major portion of the core of present-day statistics is directly attributable to him. He combined deep understanding of the physical substance of problems with an intuitive feel for productive statistical concepts: sufficiency, likelihood, conditioning, randomization, experimental design, significance tests and p-values, . . . And he became impatient and irritable with those who didn't see the "obvious," which alienated him from many more conventional statisticians. For example, his visit to a famous American university at the invitation of the medical faculty was attended by only two representatives of the statistics department: a visitor and his student! Recent advances in statistics, and this commemoration, are bringing us closer to true appreciation of his encompassing contributions to statistics.

*D.A.S. Fraser*
*York University*
*Canada*

Fisher came to Rothamsted in 1919 to analyze the accumulated results of 75 years of agricultural experiments. He soon pointed out that better results would have been obtained if the experiments had been designed differently. The fundamental ideas of replication, randomization, blocking, and balance have influenced de-sign far outside agriculture, notably in clinical trials and in industrial R&D (where they now have their place in the Taguchi revolution). If only we could measure how much money has been saved and how many false trails avoided by using good design. I have no doubt that experimental design is Fisher's greatest legacy to applied statistics.

*J.C. Gower*
*Rothamsted Experimental Station*
*United Kingdom*

I met Fisher for the second time (the first time was when he visited the NIH) at the 1961 ISI meeting in Paris. The accompanying picture shows Fisher with Manny and Carol Parzen, an unidentified woman (the lady who tasted tea?), Ingram Olkin, and myself. At neither of these occasions did we have any lengthy conversation. Such an opportunity did arise, however, at the end of the meeting when Fisher and I were waiting for a bus to the Orly airport. We just exchanged greetings (I was sure he didn't know who I was), and I then led him to the line already boarding the bus. As Fisher started to climb the first step, he stumbled and I caught him. He thanked me and then proceeded to a seat for two and insisted that I take the window seat. He then asked my name, and when I responded he surprised me by asking, "Aren't you in that Health Research group near Washington?" When I said yes, he then asked if I worked with Cornfield and if I knew him well. I said: "Oh yes—quite well." Fisher then remarked "He's a contentious fellow, isn't he?" This coming from Fisher left me speechless for a moment. (Evidently Fisher was annoyed with Cornfield for questioning his view of the smoking-lung cancer controversy.)

We discussed several other topics on that bus ride. Fisher brought up the work in "your country" on decision theory that he characterized as nonscience. I brought up, for reasons that now escape me, the status of mathematics in Britain. I indicated that I was a great admirer of Bertrand Russell—the Russell of *Principia Mathematica* (the first volume of which I had studied in a logic class at City College of New York). Fisher responded quite vehemently that Russell couldn't sit at the feet of Hardy. His feeling was strong, and I suspect was based on more than mathematics.

*Samuel W. Greenhouse*
*George Washington University*
*United States*

Many years ago (I think in 1950 roughly) I attended a lecture given by Fisher at Oxford. In the question time at the end of the lecture, I asked whether fiducial probability satisfied Kolmogorov's axioms. He replied, "What are Kolmogorov's axioms?" I cited them; but he then refused to answer my query, and changed the subject.

*J.M. Hammersley*
*University of Oxford*
*United Kingdom*

The particular works by Fisher which have influenced my research most strongly are the theory papers of 1925 and 1934, having to do with likelihood and conditional inference. These inspired the work that Bradley Efron and I published in *Biometrika* (1978) on approximate conditioning. My own views about conditioning were indelibly set then and continue to affect both my research and my teaching.

*David Hinkley*
*University of Oxford*
*United Kingdom*

Surely the single idea usually associated with Fisher that is of greatest scientific importance is

randomization. I would not regard Fisher's advocacy and study of randomization as his greatest accomplishment, however, in part because the idea was apparent to others (see Stigler, *The History of Statistics*, p. 253), and in part because I would prefer to place Fisher's discussion of randomization within his broader analysis of experimental design. I would pick the systematic construction of principles of theoretical statistics as Fisher's greatest contribution. Fisher proposed solutions to many, many statistical problems, but it is his synthesis of fundamental concepts in inference and design that I consider his greatest achievement.

*Robert Kass*
*Carnegie Mellon University*
*United States*

What was Fisher's greatest accomplishment? The question is difficult because Fisher made great advances in distribution theory, he laid mathematical foundations of theoretical statistics, he founded the design of experiments, and he gave hugely stimulating ideas on statistical inference. Also, it is no exaggeration to assert that Fisher was the founder of the theory of population genetics and quantitative genetics. I do not name one of these accomplishments. Instead I name Fisher's development of statistical methods for research workers. The impact was huge; one need only compare his 1925 book with recent books. Fisher's ideas dominate statistical thinking in biology and other "noisy sciences" to present times.

*Oscar Kempthorne*
*Iowa State University*
*United States*

Few statisticians appreciate Fisher's standing as an applied probabilist. His book (1930) *The Genetical Theory of Natural Selection*,



**Fisher is second from left, with Samuel W. Greenhouse on his left in photo, and to his right an unidentified woman, Carol Parzen, Ingram Olkin, and Manny Parzen, at the 1961 ISI meeting in Paris.**

Oxford, was immensely influential in biology, but also contributed to stochastic process theory. Feller once remarked that, if Kolmogorov's basic paper (1931) "Über die analytischen Methoden der Wahrscheinlichkeitsrechnung," *Math. Annalen*, had never been written, the modern theory of stochastic diffusions would have developed in much the same way with Fisher's book instead of Kolmogorov's paper as the point of departure. Another Fisher-Kolmogorov doublet is "The wave of advance of advantageous genes," Fisher (1936), and " . . . diffusion avec croissance . . . ," Kolmogorov, Petrovsky, and Piscounov (1937).

*David Kendall*
*University of Cambridge*
*United Kingdom*

A great achievement of R.A. Fisher was to develop the theory of the analysis and design of agricultural

experiments. Beginning with the rather slender achievements of the theory of errors, he was able to develop the use of orthogonality, separating out the various components of the sums of squares which under the null hypothesis were mutually independent, but he did not clear up all the mathematical details. He was able to apply these methods to contingency tables, even of several dimensions. Fortunately, others have filled in the details, giving matrix derivations of the main results and broadening the fields of application.

*H.O. Lancaster*
*University of Sydney*
*Australia*

In 1956 I read Fisher's book *Statistical Methods and Scientific Inference* with the statement (p. 51) about fiducial probability that "the concept of probability involved is entirely identical with