R. A. Fisher (1890–1962): An Appreciation

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R. A. Fisher was a great scholar, typifying Cambridge, the wonderful melting pot of ideas, where astronomers rub shoulders with historians, neurophysiologists with lawyers, and mathematicians with geneticists, statisticians, and others. The appraisal of Fisher's scholarly activity should, then, be made from at least three points of view, the point of view of statistics, that of mathematics, and that of empirical science.

As a statistician, R. A. Fisher appears to me to be a direct descendant of Karl Pearson, with side influences of G. Udny Yule and of F. Y. Edgeworth. From the point of view of mathematics, the situation is more complicated. Fisher's early days at Cambridge were, roughly, on the dividing line between two epochs, the earlier epoch of manipulative skills and the subsequent period of conceptual developments. Fisher seems to have belonged to the epoch of manipulative skills, in which he was supreme. Also, I remember his declaring that the change symbolized by the names of Hardy and Littlewood was a disaster in English mathematics. Nevertheless, some of the most important writings of Fisher appear to be influenced by what was then the incipient era of conceptual mathematics.

The part of Fisher's work that I admire most is one that must have resulted from the general Cambridge atmosphere, from his contacts with representatives of empirical sciences, astronomers (in particular with A. S. Eddington), geologists, and biologists. Here I have in mind not only Fisher's direct contributions to science, especially to population genetics, but also Fisher's actual founding of an entirely novel discipline related to scientific research which I like to call the theory of experimentation. Even though Fisher's close contact with experimentation began after he left Cambridge and assumed a position at the Rothamsted Experimental Station, I rather think that the roots of experimental designs started growing while he was still at his alma mater.

My contacts with Fisher go back to 1932, the second of the two occasions known to me on which he and Karl Pearson agreed. The first occasion occurred in or slightly before 1915, when Pearson agreed to publish Fisher's paper giving the distribution of the coefficient of correlation (*1*). The occasion of 1932, memorable to me, was when Karl Pearson communicated to the Royal Society a joint paper on the theory of testing hypotheses by E. S. Pearson and myself and on which Fisher wrote a favorable review. Generally, the reviews are confidential. However, on this occasion Fisher deviated from the customary anonymity in order to call our attention to an oversight: moments do not always determine uniquely the corresponding distributions. Fisher's kindly suggestion is duly acknowledged on page 315 of our paper (*2*).

My subsequent, already personal, contacts with Fisher began in April 1934 when I came to London, first on a temporary and, later on, a tenure appointment at University College. Karl Pearson, the founder of the famous institute, had retired a year or so before and his institute was divided into two departments: the department of statistics with Egon S. Pearson as its head, and the department of eugenics under Fisher. My position was under E.S.P.

There was a sharp feud raging between Karl Pearson and R. A. Fisher with, seemingly, the population of the earth divided into two categories: K.P.'s school and R.A.F.'s school. Both E. S. Pearson and I did not like the situation and did our best to avoid becoming involved. To begin with, my own relations with Fisher were excellent and not infrequently I was a guest at Fisher's home in Harpenden. However, after a year or so, a break occurred between Fisher and me, an individual break, not a part of the K.P.-R.A.F. feud. While personalities were involved, the break had a scholarly background and was the start of a dispute lasting a quarter of a century (*3-6*). Except for the ready admission that Fisher's writings, including his polemical articles, influenced my own work, the dispute itself and the issues involved are not the subject of the present paper.

Four Principal Directions of Fisher's Work

Any classification of empirical material, be it plants, inhabitants of a country, stars, or works of a particular scholar must be somewhat fuzzy, involving borderline cases. Also, any such classification is unavoidably subjective, reflecting the background and the attitudes of the person attempting the classification. The classification of Fisher's works given below has both these characteristics: it is somewhat fuzzy and it is subjective.

As I see it, the many research papers published by Fisher fall under four principal headings: (i) conceptual papers dealing with foundations of theory; (ii) papers giving exact distributions of various statistics; (iii) papers and books dealing with the theory of experimentation; and (iv) papers on stochastic models of natural phenomena.

As mentioned earlier, in the history of mathematics Fisher belongs to the era of generations preceding his own. Contrary to this, in the history of statistics Fisher's role was that of a pioneer. In order that this role may be more easily understood, a brief historical background might be helpful.

Transition from "Kollektivmasslehre" to Analytical Statistics

The era in the history of statistics that preceded Fisher's may be labeled by the excellent German term *Kollektivmasslehre* invented, I think, by Bruns. Its development followed the realization, at the end of the 19th century, that the treatment of the then novel subjects of scientific investigations, namely studies of what we now call "populations," be it populations of stars or of molecules, of plants or of humans, require a new "collective" mathe-

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matical discipline. A population is characterized by the distribution of one or of several "individual" characteristics of this population's members. Thus, the mathematical problem was to devise flexible formulas which could be considered as idealizations of empirical distributions, such as the distribution of magnitudes of stars or of anthropological measurements of various kinds. As is well known, the efforts in this direction resulted in several systems of frequency curves connected with the names of Bruns, Charlier, Edgeworth, and Karl Pearson.

The next era in the history of mathematical statistics, the era that might be labeled analytical, began with a considerable temporal overlap with the *Kollektivmasslehre* period. The basic question typical of the analytical period is: what is the chance mechanism operating within a given population that generated the particular distribution we observe? For example: given that the number of industrial accidents per worker per year follows, approximately, the negative binomial distribution, what might be the chance mechanism underlying this phenomenon? (Actually, this particular question was asked, and answered, in the late 1920's and in the early 1930's. It is used here for its excellent illustrative quality.)

A very clear and compact definition of the problem typical of the analytical period has been given by Émile Borel in his book *Éléments de la Théorie des Probabilités* first published in 1909. As described by Borel, this problem is *the* problem of mathematical statistics (*7*):

> Le problème général de la statistique mathématique est le suivant: déterminer un système de tirages effectués, dans des urnes de composition fixe, de telle manière que les résultats d'une serie de tirages, interprétés à l'aide de coefficients fixes convenablement choisis, puissent avec une très grande vraisemblance conduire à un tableau identique au tableau des observations.

In modern terminology, and with an appropriate extension, this statement may be reformulated in the following ways:

1) The general problem of mathematical statistics is that of stochastic models: given a distribution—what is the chance mechanism (Borel's system of draws of balls) that generated it?

2) Admitting tentatively that an empirical distribution has been generated by a specified chance mechanism involving several unknown parameters, what are the values that one should ascribe to these parameters? (These are the Borel's "*coefficients, convenablement choisis.*")

These two questions are inevitably followed by a third:

3) Given a set S of observations and a tentatively proposed chance mechanism M, how to decide whether M is consistent with S?

To my knowledge, apart from the definition quoted and apart from a somewhat inconclusive discussion in *Le Hasard* (*8*) of the possibility of solving problem 3 (above) through the use of "*fonctions en quelque sorte remarquables*," Borel did not contribute much to the development of mathematical statistics as he understood the term. On the other hand, Fisher's contribution was tremendous. Even though the analytical period of the history of statistics began before Fisher, the relevant papers were few and far between and, apart from a few exceptions [for example, Karl Pearson's paper (*9*) on χ^2 published in 1900 and "Student's" paper (*10*) published a few years later], the analytical element in them was rather tentative and indistinct. From the point of view of generality of interest, the analytical era of statistics began with Fisher.

Fisher's Attack on Distribution Problems

Curiously, the first important series of Fisher's papers, clearly belonging to the analytical period, was concerned with certain technical details and conceptualizations came later. The first sequence of Fisher's papers, the sequence that shook the statistical community, was concerned with the distribution problems involved in the general problem 3 (above), which we now label the problem of testing statistical hypotheses. This series began with the paper on the distribution of the coefficient of correlation, published in *Biometrika* in 1915 when Fisher was 25 years of age (*1*). Compared with distribution problems that were treated earlier, the problem of the correlation coefficient was emphatically very much more difficult and Fisher deserved general applause. Yet, he seems to have had difficulties. In fact, his subsequent papers, giving the distributions of partial correlation, of multiple correlation, of the correlation ratio, and of the F statistic, were published not in the same journal, where they obviously belonged, but in many different journals where, occasionally, they appeared out of place.

This section of Fisher's activity exercised, and continues to exercise, a considerable influence on statistical literature, with such contributors as Harold Hotelling, S. S. Wilks, and J. W. Wishart. Later, Harald Cramér's book was published, popularizing matrix theory, and serving as predecessor of the works by T. W. Anderson, R. C. Bose, S. N. Roy, Henry Scheffé, and others.

Quite apart from the unusual manipulative skill exhibited by Fisher in his early distribution papers, they contain a very important conceptual contribution. As of now, this contribution may seem trivial. However, the mere fact that at the time it appeared to have been a novelty indicates the heavy weight of routine of thought that Fisher managed to shake off. The particular contribution consists in introducing a clear distinction between the value of a parameter characterizing a population (for example, of the population correlation coefficient ρ) on the one hand, and the value of the same parameter (which we now call "statistic," following Fisher), computed from the sample (for example, the sample correlation coefficient r). Distinct as ρ and r are, some of the earlier studies indicate a degree of most embarrassing confusion which Fisher helped to dispel.

Conceptual Papers by Fisher

The strictly conceptual papers of Fisher began to appear after a substantial interval since his first groundbreaking paper of 1915. Here I have in mind mainly the paper, "On the Mathematical Foundations of Theoretical Statistics," published in 1922, and the paper, "Theory of Statistical Estimation," that appeared in 1925, both reprinted in the collection of Fisher's papers published by Wiley (*11*).

It is here that Fisher's ideological descendance from Karl Pearson and, partly, from Edgeworth, is apparent. It is also here that I sense the influence on Fisher of the contemporary development of conceptual mathematics in Cambridge. At the time the first of the two papers was written, the Pearson system of frequency curves was fully developed, as well as the method of fitting them by moments, complete with a set of the necessary tables. Also in 1908–09, Edgeworth came up with two novel ideas. One was that the method of moments may not be the most advantageous method of fitting all the Pearson curves, and the other, that the most advantageous method of fitting is the one which we now call the method of maximum likelihood. Edgeworth formulated a conjecture (*12*) that the a priori asymptotic variance of the maximum likelihood estimate (which he termed the a posteriori most probable value) cannot be greater than that of any other estimate. Edgeworth admitted his inability to prove this proposition in its full generality, but provided proofs for several particular cases.

In his two papers just quoted, Fisher divested himself of all considerations of a posteriori probabilities and attacked the two problems with considerable vigor and ingenuity. In fact, a strong attempt was made to build up a general theory of statistics, at least the theory of point estimation, as a balanced architectural whole. Compared to what has been done for probability by Kolmogorov (*13*), this attempt cannot be considered successful, which must have been apparent to the editors of the two journals. In fact, the introductory sections to the two papers have the appearance of having been written as addenda to the rest of the texts, possibly in response to objections by the referees. In particular, the "Prefactory Note" to the second paper begins with the sentence (*11*, p. 11.700): "It has been pointed out to me that some of the statistical ideas employed in the following investigation have never received a strictly logical definition and analysis." Clearly the word "never" covers the earlier paper by Fisher, "On Mathematical Foundations. . . ."

Nevertheless, in spite of the lack of mathematical rigor, not only were the two papers published, as they should have been, but they also exercised a very considerable influence on the generations of statisticians that came on the scene after their publication.

Of the concepts formulated by Fisher, the following appear to be the most fruitful: consistency of an estimate, its efficiency, and the concept of sufficiency. The number of papers dealing with these concepts is tremendous and their enumeration is an impossibility. However, the authors who were inspired by Fisher's two papers are a good illustration of the importance of Fisher's ideas. These authors include Darmois, Dugué, and Fréchet in France, Harald Cramér in Sweden, Hotelling, Doob, Wald, and Wolfowitz in the United States, C. R. Rao and Mahalanobis in India, Pitman in Australia, Dynkin, Linnik, and Kolmogorov in the Soviet Union, not to mention many British authors.

A fair description of the situation seems to be as follows: Even though Fisher failed to construct a theory of statistics, or even a theory of estimation, as an internally consistent system of concepts, he did through persistent work on a great number of particular problems manage to bring out several recurring phenomena of prime importance which inspired generations of other research workers. In addition, Fisher was a fighter and, after reaching a result which satisfied him, he would struggle for the general acceptance of this result. One example of this is Fisher's series of papers on a subject brought out by Yule, concerned with the loss of degrees of freedom in χ^2 due to the use of estimates of unknown parameters. At times there were "no holds barred" in the disputes that developed, and in the process it was inevitable for Fisher to step heavily on the toes of some generally recognized contemporary authorities. This led to feuds and to several spectacular developments. One was a long series of lengthy articles in Italy, in which an offended authority and his students repeatedly claimed to have "annihilated" the Anglo-Saxon theory of statistics. Incidentally, after having dealt thus with Fisher, the same individuals dealt similarly with me. Another and perhaps even more spectacular occurrence was the December 1934 meeting of the Royal Statistical Society. This was the first and, so far as I know, the only meeting of the Society at which Fisher was invited to present a paper (*14*). The subsequent motion of the "vote of thanks" (*15*) (the quotes are intentional and fitting the situation) and the following discussion, all duly recorded in the Society's journal, have few parallels in the scholarly literature known to me. However, the violent attacks Fisher sustained on this occasion were harmless. At the time Fisher had easy access to the printing press, both through the *Annals of Eugenics*, of which he was editor, and by being a fellow of the Royal Society of London. With these advantages, Fisher could ignore the displeasure of the leaders of the Royal Statistical Society. Besides, the angry outbursts of several "oldsters," countered by manifestations of Fisher's high polemical talent, impressed the audience as evidence of his originality. His following grew and the problems he was concerned with attracted more attention.

Theory of Experimentation

As the research in science inexorably drifted to subjects exhibiting more and more variability from one experimental unit to the next, the problem of designs of experiments, taking this variability into account explicitly, became more and more urgent. However, the urgency of a problem in a given domain is not always recognized by the rank and file of workers in this domain. This is particularly true with problems that are interdisciplinary in character. Also, if an active research worker W in a domain A is requested to do something with reference to problems that are pressing in another domain B, the very frequent response of the worker W is unwillingness to cooperate. When the authorities of the Rothamsted Experimental Station sought the cooperation of Fisher, he not only agreed to cooperate, but put his heart and soul into this cooperation. Both sides deserved compliments—the authorities of Rothamsted for their foresightedness and Fisher for his ability to become genuinely and actively interested in the problems at Rothamsted.

Fisher's contributions to the theory of experimentation are many and his now famous books *Statistical Methods for Research Workers* (*16*) and *The Design of Experiments* (*17*) are really composed of items of his own finding. Also, the third book (*18*), of which Frank Yates is a coauthor, *Statistical Tables for Biological, Agricultural and Medical Research Workers* represents a compendium of a number of findings either by the two authors themselves or by other members of what may be called the Rothamsted School of Statistics.

These books were followed by a long series of other books by other authors. In this country alone the literature on experimentation is very extensive, from Snedecor, to Cochran and Cox, to Brownlee, to the manuals of Youden, each starting where Fisher left off and then extending the principles of scientific experimentation to further and further domains of scientific research. Since about 1950, I also joined this general trend with an effort to introduce Fisher's experimental principles into research on weather control.

Important as the books on experimentation are, my favorite publications are two little papers, one under the title "The Arrangement of Field Experiments" originally published in 1925, and the other, a joint paper by Fisher and S. Barbacki, "A Test of the Supposed Precision of Systematic Arrangements," published in 1936. Both are reprinted in the volume (*11*) of Fisher's *Contributions to Mathematical Statistics*. My particular preference for these papers is due to the emphasis they place on randomization as a prerequisite to soundness of an experiment. Fisher's own argument in favor of randomization is that it is necessary for a valid estimation of the error variance. While this is undoubtedly true, I prefer to formulate essentially the same statement somewhat differently: without randomization there is no guarantee that the experimental data will be free from a bias that no test of significance can detect.

Fisher must be credited not only with a clear statement of principles of experimentation, but also with the great success he achieved in propagandizing these principles so that now they are generally accepted and adhered to in many domains of science. Also, in other domains where experiments are going on without randomization, the particular experimenters feel compelled to present excuses for not randomizing. Thus it is likely that, in a generation or so, sound experimentation will spread to these domains also.

The development of sound principles of experimentation is a great achievement per se. However, Fisher did more. Here I have in mind his analysis of variance and the development of a system of relevant tests, both the parametric tests based on assumptions of normality and independence, and of nonparametric randomization tests. When speaking of a system of tests I have in mind prescriptions like that of first deciding (using the F test) whether there is any significant effect in an experiment at all. Further analysis, leading to decisions as to particular treatments tested is only justified if the F test indicates significance. As a further development of the same idea, Fisher introduced the familiar χ^2 procedure of combining the results of several independent trials; each trial taken separately may fail to indicate significance, but jointly these results may provide evidence that the treatment studied did, in fact, have some effects. Alternatively, it may be that the true effects of treatments are really zero and the occasional significance observed in a few independent experiments out of a substantial number of them is the result of the unavoidable random variation. Fisher's summary test, for which Egon S. Pearson proved a property of optimality (*19*), is a means of controlling these two sources of error.

The influence of Fisher's theory of experimentation on further developments in statistics seems to have no limits. Fisher's summary treatment of several already completed experiments appears to me as a predecessor of the achievements of Tukey and Scheffé, now nicely summarized by Rupert Miller, concerned with multiple decision problems. Fisher's experimental designs, particularly those involving incomplete blocks and confounding, brought to the fore delicate combinatorial problems and appear to have inspired R. C. Bose who, in his turn, generated a special branch of literature. Finally, Fisher's study of experiments influenced Abraham Wald and, thereby, a long series of outstanding scholars who follow Wald.

There is an interesting classification of problems of experimentation born out of a conversation I once had with M. G. Kendall. As we saw it, the original problems of Fisher, exemplified by single experiments to be conducted on given pieces of land, might be called problems of experimental tactics. Contrary to this, the problems of Wald visualized sequences of experiments and the possibility of a variety of decisions after each member of the sequence. One decision could be to discontinue the sequence with some sort of "terminal" substantive decision. Another possibility is to continue experimentation, perhaps with some novel design. Problems of this kind, obviously different from Fisher's, might be called problems of experimental strategy. Wald's ideas as introduced in his *Theory of Statistical Decision Functions* (*20*), are obvious predecessors of the more modern works of Blackwell, Girshick, and their followers.

It is now appropriate for me to mention a problem which obviously belongs to the category of experimental tactics, but is missing in Fisher's writings. This is the problem of evaluating the probability that an experiment with a tentatively fixed design will detect the effects that it is designed to detect, if such effects are real and have preassigned magnitudes. The problem is that of the power of the tests contemplated for the treatment of the given experiment. The consideration of power is occasionally implicit in Fisher's writings, but I would have liked to see it treated explicitly.

I do not believe that Fisher ever thought of his work on experimental designs with reference to Borel's definition of the typical problem of mathematical statistics. However, the correspondence is unambiguous. Consider, for example, the randomized blocks experiment designed to compare some s varieties. Let n be the number of blocks. Let u_{ijk}, for $i = 1,2, \ldots, n$ and $j,k = 1,2, \ldots, s$, denote the potential yield of the kth variety if grown on the jth plot of the ith block. The Borel type scheme of draws of balls visualizes n groups of urns of s urns each. The ith group corresponds to the ith block of plots. The kth urn of each group corresponds to the kth variety tested. It contains s numbered balls, the jth of them corresponding to the jth plot on the ith block and carrying the number u_{ijk} written on it. The s urns of the ith group have the magic property that, if the jth ball is removed from any one of them, the jth balls disappear from all the other urns of the same group. Under this system, the randomized n-blocks experiment with s varieties is equivalent to extracting just one ball out of each of the ns urns. The number written on the ball extracted from the kth urn of the ith group is a random variable, say x_{ik}, representing the yield of the kth variety on the randomly selected plot within the ith block.

The above schematization may be considered as the structural part of the stochastic model of the randomized blocks experiment; no numerical assumptions are involved. This structure may then be supplemented by other particularizing assumptions, such as the assumption of additivity of block and varietal effects and assumptions regarding the values of the u_{ijk}, and others. The final urn model to test against the observations is that described by all such assumptions and, in addition, by the assumption that the block means, say $u_{i\bullet k}$ do not depend upon k.

The peculiarity of this situation consists in the fact that the structural part of the phenomenon, namely the randomization of the n blocks of s plots each, is the result of a deliberate choice by the experimenter and is known for certain. The only freedom that Nature is allowed is the values of the numbers u_{ijk}. This is in contrast with the frequent situation where the statistician is confronted with a truly natural phenomenon, such as the phenomenon of inheritance, where Nature plays a game of chance constructed by herself and the problem is to guess the underlying mechanism, including its structural part.

Here again we are confronted with remarkable achievements of Fisher. While these concern several domains of science, including earth magnetism, Fisher's preference seems to have been genetics and evolution, both out of my usual bailiwick. The general impression I formed from occasional reading is that the modern discipline of population genetics, including such authors as S. Karlin, O. Kempthorne, M. Kimura, R. G. Lewontin, G. Malécot, K. Mather, P. A. P. Moran, and a number of others, is a development that grew out of the works of essentially only three scholars of the earlier generation: R. A. Fisher, J. B. S. Haldane, and Sewall Wright. The provenance of the ideas that underlay the population genetical studies of these three research workers is likely to be quite complex and, probably, very different. However, the relevant works of Karl Pearson (*21*) and, perhaps unexpectedly, a little note by G. H. Hardy (*22*), one of the purest of pure mathematicians, seem to have been a common inspiration.

In his very interesting book (*23*), Moran refers to 28 contributions to population genetics by R. A. Fisher, either alone or with some coauthors, extending from 1918 (*24*) to 1943, and probably this list is not complete. There is no doubt in my mind that in this domain also Fisher's role was that of the founder, at least that of one of the founders, of a fruitful novel domain of human thought and inquiry.

Concluding Remarks

As stated at the outset, the present appreciation of Fisher's scholarly work is subjective. Also it is one-sided. Both the strict subjectivity and one-sidedness are intentional.

The subjectivity of my account of Fisher's work depends on my personal scientific past and on my personal perspective. No doubt, other scholars will view the same developments differently. Also, I rather expect that Fisher himself would have disagreed with my views on a number of points. One example is the connection between Fisher's own work on experimental tactics, on the one hand, and Wald's work on experimental strategy, on the other. In fact, soon after the appearance of Wald's book, Fisher published an article emphasizing his view that Wald's theory of decision functions has no relation with Fisher's designs of experiments. In a sense, I agree. Wald's work was original work on his own, not on Fisher's problems. My point is that, if Fisher's theory of experimentation did not exist, then, probably, Wald's theory of statistical decision functions would not have been developed as it was developed. As stated by Wald himself, his thinking was stimulated by Fisher's.

Another point on which Fisher is likely to have disagreed with me is my calling him a "descendant" of Karl Pearson. Here a few comments might be useful. A "descendant" does not necessarily mean either a follower or even a student. What I mean here is that, in the early phase of his scholarly activities, Fisher was preoccupied with problems immediately suggested by Karl Pearson's writings. In fact, Fisher seems to have picked up where Karl Pearson left off, and for the history of human thought, it is this link that is significant, not the feelings that the two great scholars had for each other.

The one-sided character of the present article results from my opinion as to how an individual's scholarly activity should be judged. In several earlier writings I have pointed out that certain of Fisher's conceptual developments, not mentioned here, are erroneous. Lest there be a misunderstanding on this point, I emphasize that I continue to maintain this view. However, to err is a part of human nature and I feel that a scholar's activity should be judged by his positive achievements and, particularly, by the influence he exercised on subsequent generations. The purpose of the above outline of Fisher's work is to emphasize my personal views on his record, which is second to none.

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Footnote by William G. Cochran

In adding a few notes to Neyman's summary and appraisal of Fisher's contributions, I would like to present an impression of my own about Fisher's outlook, and to give some personal reminiscences of Fisher.

The subject matter of statistics has been defined in various ways. I believe that Fisher thought of statistics as essentially an important part of the mainstream of research in the experimental sciences. His major books, *Statistical Methods for Research Workers, Design of Experiments*, and the Fisher-Yates *Statistical Tables* (*1*) were addressed not to statisticians but to workers in the experimental sciences. The 1925 preface to the first edition of *Statistical Methods* opens as follows (*1*): "For several years the author has been working in somewhat intimate co-operation with a number of biological research departments; the present book is in every sense the product of

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