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BOOK REVIEW

Statistics in Britain (1865-1930) : The Social Construction of Scientific Knowledge by Donald A. MacKenzie, University of Edinburgh, U. K., Edinburgh University Press, 22, George Square, Edinburgh, 1981, pp. i-vii, 1-306

This book is directed to testify as to what extent and in what ways, scientific knowledge is socially constructed. The development of statistical theory in Britain from 1865-1930 has been taken to demonstrate this objective. Since the debate on Science and Society has been going on in several other biological and social sciences, the author has taken the example of a mathematical discipline like Statistics to take the debate a step further. However, the nature of the mathematical discipline has not made the debate inaccessible to those with little or no training in statistics. The subject-matter has been presented in a manner avoiding theoretical details in the text, providing mathematical appendices and a glossary of the technical terms used. In doing so, the author has incidentally done a good service to those interested in the historical development of statistics during the period 1865 to 1930. The reading is quite interesting and intelligible as it brings into focus several controversies, the various viewpoints as well as the different attitudes of the eminent statisticians existing during the period.

The author begins well by first defining statistical theory as the construction of a theoretical framework for the analysis of numerical data. He does not regard it as merely gathering data or simply the abstract study of mathematical probability. In Britain, the former aspect of gathering quantitative information was well established in the Victorian period and there was then no tradition of statistical theory. It was during the period covered in the book that an active group of researchers such as those at University College, London and at Rothamsted Agricultural Research Station worked on Statistical theory. It was also during this period that the famous Journal 'Biometrika', came into existence with articles on Statistical Theory. The personalities responsible for this development were mainly three: Francis Galton (1822-1911), Karl Pearson (1857-1936), and R. A. Fisher (1890-1962). The ideas on eugenics shared the development of statistical methods in the hands of these three personalities. All the three personalities claimed that the most important human characteristics such as mental ability were inherited from one generation to the next. The only secure long-term way to improve society, according to them, was to improve the characteristics of the individuals by ensuring that those in the present generation with good characteristics had more children than those with bad characteristics.

The author describes the eugenics movement in Britain, in Chapter 2, as a social structure responsible for the development of statistical theory in the hands of the trio Galton, Pearson and Fisher. Chapter 3 deals with Galton, a cousin of Charles Darwin, who used the finger-prints as a method of personal identification and gave concepts of regression and correlation, primarily to study the inheritance of quantitative characters in human beings. He invented the concept of mid-parents as a fictitious amalgam of characteristics of father and mother. The author draws upon Galton's work as a case study to show the thesis of social construction of scientific knowledge. Chapter 4 is devoted to Karl Pearson, who besides being a Mathematician, was also a political thinker of those times and wrote a famous work on 'The Grammar of Science (1892)' which was attacked by eminent persons like Lenin. Like Galton his thinking was a clear manifestation of the class to which he belonged. As Head of the Department of Applied Statistics at University College, London, he bore the title 'Professor of Eugenics'. The standard formulae for the correlation coefficient and the widely used 'chi-square' test of the goodness of fit between observations and theoretical predictions are both named after him. The Biometrical School set up by Karl Pearson and W.F.R. Weldon, a zoologist, has been discussed in Chapter 5, indicating that the need for such a move arose firstly because biological scientists wanted to quantify biology and secondly because Karl Pearson wanted to continue the tradition of eugenic research begun by Galton. Not much success, however, was achieved in the former case since it required a relatively unusual combination of training of both biology and statistics. The biometric school, therefore, gained momentum around eugenic researches. W. S. Gosset (1876-1937), writing under pen-name 'Student' joined the biometric school in 1906 but deviated from its research programme to invent statistical techniques valid for small samples as against Karl Pearson's emphasis to work with large samples from plant, animal and human populations. In particular, he derived 'the distribution of the distance of the mean of a sample from the mean of the population expressed in terms of the standard deviation of the sample for any normal population' making possible what is now called Students' t-test. Thus, Pearson had to involve individuals whose goals were different from his and those of Galton. Soon after his retirement in 1933 and much to his dismay, the connection between statistics and eugenics was severed by separating the department into two, one of Eugenics headed by R. A. Fisher and the other of Statistics headed by Egon Pearson, son of Karl Pearson. To the third chair of Biometry, J. B. S. Haldane was appointed.

In Chapter 6 the famous controversy between Biometrician and Mendelian protagonists is discussed. Pearson and his collaborators attacked William Batson (1861-1926) and the early Mendelian geneticists. The refusal of Pearson to accept Mendel's principles is argued by the author as motivated by social interests rather than simple technical issues of biology. With the rediscovery of Mendel's work in 1900, the Cambridge biologist Batson started operating with a model of heredity based on discrete, elementary genetic factors which pass unchanged from parents to offspring. He coined the term 'genetics' and went from 'genotype' to observable 'phenotype'. Karl Pearson and his group on the other hand followed Galton's 'laws of heredity' describing statistical regularity in the relationship between parental and offspring characteristics. The biometricians were thus, concerned with primarily the 'phenotypes' without regard to the underlying genotypes. The main factor which could account for the difference in the two approaches, according to the author of this book, is the different sorts of skills employed by the two sides. Bateson felt that Galton and Pearson were not trained biologists whereas Pearson felt that the biologists lacked mathematical training and said that '. . . mathematical knowledge will soon be as much a part of the biologist's equipment as today of the physicist's'. Thus the two sides adhered to the judgements based on their technical competences and to avoid the devaluation of their competences rejected the judgements based on the skills of the other side. This shows the social interests of groups of scientific practitioners with differing skills. Besides the controversy over the validity of Mendelism, there was the dispute over the nature of evolutionary change. Pearson's group believed, following Darwin (1859) that evolution was a process of gradual change, taking place by the selection of continuous differences. Bateson, on the other hand, emphasised the role of large discontinuous variation in the evolutionary change and published in 1894 a book entitled 'Materials for the study of Variation' to give a large number of examples of such variation. This view was compatible with Mendelism but anti-Darwinian.

In Chapter 7 is described another controversy between Karl Pearson and his former people George Udny Yule (1871-1951) on the methodology for the measurement of association between characters. The different approaches to measure association adopted by the two sides reflected different attitudes to eugenics. The biometric school's commitment to eugenic research and Yule's distaste for eugenics were important factors in the divergence of the two theories of association. Pearson's use of the tetrachoric coefficient of correlation to measure association of data arranged in contingency tables was criticised by Yule (1906). He described the assumption of the underlying bivariate normal distribution for the contingency tables as absurd stating 'all those who have died of small pox are equally dead; no one of them is more dead or less dead than another, and the dead are quite distinct from the survivors'. On the other hand, Pearson's work on this issue continued the link between the methods of regression and correlation and the eugenic problem of the hereditary relationship of successive generations which made him to measure association by a tetrachoric correlation coefficient.

In Chapter 8, the author takes up the central figure of the period viz. R. A. Fisher. He discusses his earlier involvement in eugenics, its relationship to social interests and its role in his decision to begin work on statistical theory and statistical biology. Like Galton and Pearson, Fisher also treded the path of eugenics. His first known scientific paper was read to the Cambridge University Eugenics Society and in his famous book 'The Genetic Theory of Natural Selection (1930)', biological science and its eugenics applications were explicitly related. Unlike Pearson, however, Fisher accepted Mendelism as a theory of heredity and reconciled Mendelism with Biometry in his famous work on correlations between relatives (1918). However, Fisher disagreed totally with the Mendelian protagonist's rejection of Darwinism. He sought a eugenics that would be applicable to whole human societies in their growth and decay. Natural selection was the key to Fisher's evolutionism. He was against the theory of mutation or Sewell Wright's random drift theory that denied the theory of natural selection. According to him, selection drafted a eugenist's conscious intervention in human population. His fundamental theorem of natural selection (1930) was probably an outcome of his treatment of evolution as a predictable mass process working on populations as aggregates of individuals and homologous to eugenic intervention in human populations.

Besides genetics and evolution, Fisher's main contributions were in the theory of statistical inference and design of experiments in the agricultural field. He constructed a general non-Bayesian theory of inference. He gave an 'absolute criterion' for the fitting of frequency curves to observed data, the concept of likelihood, the exact distribution of sample statistics by representing a sample of size n by a point in n-dimensional space, criteria of estimation viz. consistency, efficiency and sufficiency and the method of maximum likelihood. He distinguished likelihood from inverse probability and gave the concept of information contained in the data as the inverse of the variance of the estimate obtained by the method of maximum likelihood. At Rothamsted Experimental Station, where he was earlier appointed as Statistician in 1919, he worked on practical problems of agricultural research and invented the method of curve fitting using orthogonal polynomials, the technique of analysis of variance, factorial method of experimentation and principles of design of experiments viz. replication, randomisation and local control. His famous book entitled 'Statistical Methods for Research Workers' published in 1925 became the first classic on the foundations of statistical methods, particularly in relation to practical problems. It was this book which gave the message that the statistician should get involved in the practical business of experimentation and large-scale applied experiments should be designed by those with statistical expertise. With Fisher's influence in Britain, a new era in the development of statistical theory began in mid-1920s which had a tremendous impact in the twentieth century on scientific research in general.

In spite of the bitter personal controversy between Karl Pearson and R. A. Fisher, the work of Fisher was in fact in continuity with that of Karl Pearson and before him with that of Galton. Despite Fisher's different approaches to many statistical problems, despite his acceptance of Mendelism, despite his support for the Conservative Party and adherence to the Anglican religion, Fisher and Pearson shared many common goals. Both saw in the science and political programmes of eugenics, the path of national salvation and both saw in statistics not merely a technical adjunct to research but a new methodology of real philosophical importance. Though in different ways, the thinking process of both of them can be seen as reflecting the interests of the professional middle class of Britain of those times.

In the concluding Chapter 9, the author, who is incidentally a lecturer in Sociology at the University of Edinburgh, draws together some of his ideas on the social construction of scientific knowledge embodied in the statistical theory. In his view many of the statistical methodologies developed were of an invention rather than the discoveries. Science is goal oriented, and the pursuit of all goals is sustained by social interest either in the internal social structure of Science or in that of Society at large. Scientific knowledge is therefore a social construct in the sense that it is the product of interactive groups of scientists and in the sense that these interests affect it at the most basic level of the development and evaluation of theories and techniques. The author also concludes that the modern statistics is different from the statistics of the period discussed in this book in that it has evolved from it but like "their's" it is a social and historical product and can and should be analysed as such.

One of the points which come up while reading this book is that why did the author stop at the period 1930 when a lot of development of statistical theory took place after 1930. The author has tried to justify this on page 10 in the Introduction that he has chosen 1930 as the closing date in his title of the book because that was the year of the publication of the last work to be considered in any detail in his book viz. Fisher's 'The Genetical Theory of Natural Selection', but the argument is hardly convincing. Similarly, many of the important work of Jerzy Neyman and Egon Pearson with whom Fisher got into controversy were also developed around 1930. Their description and examination in the context of the social construction of scientific knowledge would have enhanced the value of this book and strengthened much more the thesis of the author on how social structure influences the development and content of statistical theory. Nevertheless, this book is a fascinating contribution to the history and sociology of statistical science in the British School. It can be a useful addition to the collection of books by a sociologist as well as a statistician interested in the historical development of statistical theory.

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