STATISTICS AND IDEOLOGY: THE BRITISH SCHOOL OF STATISTICS 1865 - 1925

Paper read to the Summer Meeting of the British Society for the History of Science at Southampton, July 1976; by Donald Mackenzie.

The period 1865 - 1925 is crucial in the development of modern statistical theory, and in that time Britain was perhaps the key centre of the subject. Galton's invention of the concepts of regression and correlation, Pearson's work on the chi-square test and his elaboration of Galton's insights into an extensive system of statistical theory, the work of 'student' (W. S. Gosset) on the t-test, the beginnings of Fisher's comprehensive reformulation of statistical theory: these all belong in this period. In 1865 there was only a scattered awareness that probability theory and the numerical data of the social researcher or scientist could productively be brought together: by 1925 there was a considerable bulk of theory and applications of the theory, there was at least one institution (the Department of Applied Statistics at University College, London) devoted to teaching and research in statistics, there was in Biometrika a well established journal of statistical theory. Although full institutionalisation of the discipline hadnot as yet taken place (that had to wait until after the Second World War), the crucial stage of a cluster of scientists involved had been reached.

This perhaps explains my interest in this particular country and period, and thus one side of my title. What of the other side - ideology? My aim in this paper is to discuss one crucial factor (an external factor if you wish, though I don't like the internal/external distinction too much) that was of importance in this development of statistics: eugenics. The eugenists sought to improve the genetic characteristics of the British nation by increasing the birth-rate of some groups (the 'fit') while decreasing that of others (the 'unfit'). Eugenics I argue (elsewhere) was not simply an example of the over-rash extension of uncertain knowledge: it was an ideology. It was a set of ideas that served the interests of aspecific social group (the group one might loosely call the professional middle class) in a specific historical situation, a crucial element in this situation being the problem of the urban slum-dwelling poor, the so-called 'residuum', that formed a particular problem of social control for pre-first World War capitalism.

What was the relationship between eugenics and British statistics? One level of approaching this question is to ask why people started doing statistical theory. Once a discipline has become institutionalised, there need be no too specific answer to this question: they may do it simply because it provides them with a job. However, in the period discussed here this was not the case. Doing statistical theory was not a ticket to a job. Instead I shall argue that at least some of the key individuals in the development of British statistics got involved in it primarily because of their prior interest in eugenics.

In the case of Francis Galton this is, I think, established. Galton was both the founding father of British statistical theory - all the debelopment in this period owes a tremendous lot to him - and the founder of the eugenics movement (he coined the word). The connection between Galton's statistics and his eugenics has been clearly shown, first by his biographer Karl Pearson and more recently by Ruth Schwartz Cowan. It was his passionate commitment to eugenics, and his belief that in the statistical analysis of inheritance lay the key to developing a scientific eugenics, that was Galton's motivation in his statistics. He was not a sophisticated mathematician, yet he pursued the thread of his investigations into heredity till they led him first to regression and then correlation. These concepts were the intellectual breakthrough that Pearson was to build on.

What then of Karl Pearson? His work in statistics did not really begin until 1892 when he was 35 and established as Professor of Applied Mathematics at University College, London. His previous mathematical work was mainly on the theory of elasticity. The reason he started to work on statistics lies more in his non-mathematical involvement. He was a radical, a free-thinker, a feminist, a socialist (of the Fabian rather than revolutionary variety) and a vigorous pro-imperialist Now eugenics we think of nowadays a straightforwardly right-wing belief system: at this time it had considerable attraction also to technocratic, meritocratic socialists such as Pearson. It fitted well into his imperialistic social Darwinism. He was not believer in the capacity for self-action of the working class: they had to be led into socialism by the middle class - and the socialist utopia must also be the eugenic utopia. In eugenics and a mathematicised Darwinism Pearson saw the key science of social reconstruction. This was the context of his work in mathematical statistics: the very title of his main series of papers indicates this - 'Mathematical Contributions to the Theory of Evolution'.

Galton and Pearson are the key figures of the pre-1914 development of British statistics. But is seems clear that similar motives also operated in the case of some less important figures. (For many people of course we just don't know - the historical evidence isn't there). One of these was a man called Arthur Black.

He has received no attention from historians as he died without publishing anything. However, some notebooks that have been discovered by him show him to have started work in mathematical statistics before Karl Pearson, and to have been a mathematician of considerable ability. He too was interested in problems of heredity and evolution: his _magnum opus_, now unfortunately lost, was entitled 'An Algebra of Animal Evolution'. We can only guess what that contained, but surviving notebooks show him to have been the first person to reach chi-square as the limit to a multinomial distribution, and also the first Briton to rediscover (independently) the Poisson distribution.

Another man is worth mentioning, even though he was to reach fame in a field very different from statistics - H. J. Laski, the distinguished political scientist and also supposed red menace behind the Labour Party. He was converted to eugenics by a travelling lecturer in the subject, and worked for a short time with Pearson on statistical studies of heredity. He was held by Galton to show great promise, but entering Oxford University he was won away from science and moved to history and politics.

Just before the First World War another young man, who was to prove of more importance to statistics than Laski, also became interested in the subject: R. A. Fisher. Once again, Fisher's interest in eugenics seems to have been a causal factor in his getting involved in statistical work. While an undergraduate at Cambridge he was one of the driving forces behind the Cambridge University Eugenics Society. Reading what remains of the records of this society makes it clear that his eugenic interests led him to a remarkable extra-curricular study programme in statistics (it had to be extra-curricular: error theory was the only statistical area you could get much teaching in at Cambridge until Yule got a job there in the Autumn of 1912). Most interesting of the papers of this society is a paper Fisher read to it in October 1911 (when he was just starting his third year as an undergraduate in mathematics and physics) on "Heredity - comparing the Methods of Biometry and Mendelism". This little paper shows the extent of his thought and reading in the subject, and prefigures many of the ideas of his famous paper of 1918 on the "Correlation of Relatives" that was so important in the development of population genetics.

Now I don't want to argue that all British statisticians of this period were motivated by an interest in eugenics - indeed it is quite clear that some, notably Edgeworth, Yule (whose work is discussed later) and Gosset were not. But there is a crucial difference between those who were and those who weren't. Galton and Pearson were key figures, central to the network of personal and intellectual links of the statistical community. They were the organisational leaders. Pearson in particular played the key role of the intellectual entrepreneur - guiding developments at University College from a few scattered workers with productive ideas to an established research institute and teaching department. The history of this department makes clear the connections between statistics and eugenics: thus from 1911, Pearson as head of the Department of Applied Statistics held the title Galton Professor of Eugenics.

We cannot, however, stop at this level of analysis. The connection between statistics and eugenics was not simply at the level of recruitment and institutionalisation: it was also at the level of ideas. Certain methodological problems, however, appear when we attempt to work at this level. How can we attempt to show that eugenics affected how statistical theory developed? Might it not have developed in the way it did anyway, even had the eugenics movement not existed? In the mathematical sciences, especially, concepts and theories are frequently thought of as developing by an immanent process of logical debelopment. As Wittgenstein puts it "so long as one thinks it can't be otherwise, one draws logical conclusions". Could statistical theory have been 'otherwise', could it have developed differently? If so, what is the cause of the difference, of the specific characteristics of the way the theory did develop?

One way of approaching this question is to take a comparative view. Thus, in this period a vigorous school of statistics developed in Italy, led latterly by Corrado Gini, which developed a radically different approach to statistical theory. Their approach was very different to that of Pearson and his pupils. At least part of the difference can be attributed to the differeint circumstances of development, in Italy, statistics developed in the Law Faculties of Universities and was primarily concerned with problems of administrative, social and economic statistics. It developed along more intuitive, less mathematical, more descriptive lines. Interestingly, Gini argued that the approach of what he called the 'English' school was too narrowly focussed on particular indices - notably the standard dviation (and related measures) and the coefficient of correlation. Now their particular prominence in the work of Galton and Pearson, can in part be attributed to their particular significance as measures of the variability of a generation and the relatedness of two generations.

But this is perhaps too speculative. It is also possible to look at theoretical differences within the British statistical community and attempt to account for these. The most bitter of these (at least prior to the 1920's) was the controversy between Pearson and Yule over the measurement of association of non-quantitative characters. (I exclude the biometrician-Mendelian debate, which, while involving somewhat similar political aspects, involved many non-statistical biologists). The basic issue of the Pearson/Yule controversy was this.

Suppose you have a fourfold table:

	X_1	X_2	
Y_1	a	b	a+b
Y_2	c	d	c+d
	a+c	b+d	N

Thus Y_1 could be 'survived an epidemic', Y_2 'dies in the epidemic', X_1 'vaccinated', X_2 'unvaccinated' - a is the number of those vaccinated who survived, etc. Now the problem is: what is the strength of association between the two attributes X and Y? Yule's approach was straightforward: he laid down three criteria that a coefficient of association must meet: that it should be zero if X and Y are independent (non-associated), that it should be +1 if X and Y show perfect positive association and that it should be -1 if they show perfect negative association. He put forward his coefficient $Q = \frac{ad-bc}{ad+bc}$ simply as empirically fulfilling these conditions. Clearly an unlimited number of coefficients also fulfil these conditions (for example, all the odd powers of Q), and Q has no special justification. Further, as Pearson was to point out, some of these other coefficients will rank order (according to strength of association) the same set of tables in different ways.

Pearson's approach was to produce, by a much tighter (but more precarious) theoretical argument, a uniquely justified coefficient of association: the tetrachoric coefficient of correlation. This coefficient was derived by positing a theoretical model of how the four-fold table had arisen - the assumptions of the model being that beneath the observed dicontinuous categories lay underlying variables which were continuous and which followed a bivariate normal distribution. Pearson fitted this model to the observed frequencies in the table, and deduced a value for the correlation of the underlying binormal surface. This correlation (which I repeat was a parameter of a theoretical model) he called the correlation of the table (and later the tetrachoric coefficient of correlation). Although Pearson was aware that this coefficient had been reached by a theoretical process and involved the positing of a model which could not generally be tested, this did not prevent him putting forward his method as the way to measure correlation. It was this approach that was to be criticised by Yule, who argued that Pearson was making unnecessary and contentious assumptions.

Time does not permit a full discussion of the course of the controversy between Pearson and his supporters on the one hand, and Yule on the other. I shall, however, try to explain why, in my view Pearson chose to measure association in the wya hé did, and not, for example in the way Yule did, even though his very theoretical and assumption-laden approach might well be hald to contradict his own philosophical views as advanced in his Grammar of Science.

The tetrachoric method was put forward as a general means of finding the correlation of characters of characters not quantitatively measureable. However one particular purpose dominated Pearson's approach - that was to have aparticular tool to do a particular job. That job was laid down by his eugenic concerns. He wanted the tetrachoric coefficient as a measure of the strength of inheritance for characters for which no scale of measurement was available. Now for measureable characters such as height Pearson had a measure of the strength of inheritance: he would take a group of families, measure the heights of parents and the heights of off spring, and work out an ordinary coefficient of correlation. This for him would be the strength of heredity for height. (It is interesting to note how this method builds hereditarianism into the very process of his science all parent-child correlation is ascribed to heredity). However many characteristics of eugenic importance were not measureable in the way height was. Most important of these were the mental characteristics of humans: their intelligance (this was before the invention of the Binet scale), their temperament etc. Now Pearson felt able to convince people of the inherited nature of most characteristics in animals, and of the physical characteristics in man: he wanted to complete the eugenic argument by a convincing proof of the inheritance of human mental characteristics. This was what the tetrachoric coefficient was designed to do. Pearson knew that the ordinary correlation of brothers for height was about 0.5: he could now get teachers to classify pairs of brothers (getting parent/child data in this area was difficult) as (in effect) bright-dull, bright-dull, work out the tetrachoric coefficient of correlation, and call this the strength of inheritance for human ability. In fact doing this for a wide range of characters, mental and physical, measuring correlation by both the ordinary and the tetrachoric methods, he found values of what was for him the strength of inheritance clustered closely round 0.5. He considered the eugenic argument conclusively proven.

To review my argument: I claim that Pearson's eugenic purposes - his ideological purposes - led to his measuring association in the particular way he did. The point is that a looser method of measuring association, such as Yule's, would not have worked for Pearson. A value of Yule's Q and a value of the ordinary coefficient of correlation cannot be validly compared. If you accept Pearson's model, however, a tetrachoric coefficient and an ordinary coefficient of correlation can be validly compared: because you are assuming that your observed data are generated by underlying variables for which the ordinary product-moment coefficient of correlation is meaningful. And this comparability of the coefficients is what is needed to make the eugenic argument work.

I have only examined one particular aspect of the development of statistical theory in Britain in this period - although it should be said that this problem was the one to which Pearson devoted the largest single part of his statistical work after 1900. But I think that this instance, together with other similar instances - notably Galton's work on regression - demonstrates the influence of eugenics on the development of statistical theory as a system of knowledge.

One final note of reservation - the end of the period I consider sees the beginnings of an approach to statistics in which the dominant interest was not the demands of eigemocs but rather the need for technical control and prediction in industry and agriculture. Notable was Gosset's work - as an industrial scientist employed by the Guiness Brewery - and that of Fisher at the Agricultural Research Station at Rothamsted. This is a very different development, and needs to be analysed separately. But now is not the time to attempt to do it.