ABSTRACT

This paper examines the controversy that took place between 1900 and 1914 about how best to measure statistical association. The divergent views of the two sides are examined by means of a study of the work of the major participants in the controversy: Karl Pearson (1857-1936) and George Udny Yule (1871-1951). It is argued that the theorizing and scientific judgments of the two sides embodied different 'cognitive interests': that is to say, differing goals in the development of statistical theory resulted in approaches to the measurement of association that were structured differently. These different cognitive interests arose from the different problem situations of statisticians whose primary commitment was to eugenics research and those who lacked any such strong specific commitment. It is suggested that eugenics embodied the social interests of a specific sector of British society, and not those of other sectors. Thus differing social interests are seen as entering indirectly, through the 'mediation' of eugenics, into this episode in development of statistical theory in Britain.

Statistical Theory and Social Interests:

A Case-Study

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The esoteric knowledge to be found in the mathematical sciences is frequently held to develop according to its own laws, immune from social influence. The purpose of this paper is to cast doubt on this assumption by the presentation of a case-study drawn from the development of the mathematical theory of statistics.

The episode under consideration is a controversy which took place in Britain between 1900 and 1914. The emerging community of mathematical statisticians was split by a dispute over how best to measure statistical association. Karl Pearson, one of the founders of that community, and George Udny Yule, his best-known pupil, found themselves opposed to each other in an increasingly acrimonious debate. Analysis of this episode throws light on the 'social relations' of statistical theory by revealing connections between statistics and wider social and ideological issues.[1]

I begin by describing the two publications in 1900 by Yule and by Pearson in which their divergent views were first presented. In the

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second and third sections, I discuss the further development of their views and their evaluations of each other's position. I then argue that the theorizing and scientific judgments of Pearson and Yule have to be understood as embodying different 'cognitive interests': that is to say, differing goals in the development of statistical theory resulted in approaches to association that were structured differently. I identify these goals by examining published and unpublished writings of Pearson and Yule, and then extend the analysis to include the other members of the British statistical community who supported one or other of the two leading participants. I discuss possible alternative explanations of the controversy. The paper ends with a tentative suggestion as to how divergent goals in the development of statistical theory might be related to opposed social interests.

THE ISSUE

By 1900 British statisticians had reached apparent consensus on how to measure the correlation of those variables, such as height and weight, for which a measurement scale with a valid unit of measurement existed. In his concepts of regression and correlation Francis Galton had provided the basic technology for dealing with these 'interval' variables.[2] F.Y. Edgeworth, S.H. Burbury and Karl Pearson had extended the theory from two to any number of variables, and Pearson had provided the now standard product-moment formula for the coefficient of correlation.[3] Aside from some private disagreement[4] as to the extent to which Galton's theory, developed for normally-distributed variables, could be applied to non-normal variables, the problem seemed solved for interval-level variables. From 1900 onwards attention shifted to nominal variables — those in which no unit of measurement was available, and classification into different categories was all that was possible. The two main attempts to develop a theory of the association of nominal variables were by Karl Pearson (1857-1936) and George Udny Yule (1871-1951).

Let us consider Yule's work first. His approach was extremely direct.[5] Consider a set of N objects, classified according to two nominal variables A and B. Each object is classed as either A_1 or A_2, and either B_1 or B_2[6]. Thus A_1 might be 'survived an epidemic', A_2 'died in the epidemic'; B_1 'vaccinated', B_2 'non-vaccinated'. The data can be presented conveniently as follows:

	B_1 (vaccinated)	B_2 (unvaccinated)	Total
A_1 (survived)	a	b	a + b
A_2 (died)	c	d	c + d
Total	a + c	b + d	N

Thus 'a' is the number of those vaccinated who survived the epidemic, 'b' of those unvaccinated who survived the epidemic, and so on.

Yule argued that a coefficient of association for such a table must have three properties. Firstly, it should be zero if and only if A and B are non-associated or independent. In the above example, survival and vaccination (A and B) would be said to be independent if the proportion of survivors was the same amongst the vaccinated and the unvaccinated. This can be expressed symbolically as:

$$\frac{a}{a+c} = \frac{b}{b+d}$$

$$\text{or} \quad ab + ad = ab + bc$$

$$\text{or} \quad ad - bc = 0.$$

Working backwards through this chain of thought, it can be shown that $ad - bc = 0$ implies that A and B are non-associated. Thus the first desideratum will be satisfied by a coefficient which has the value zero if and only if $ad - bc = 0$.

The second property is that the coefficient should be $+1$ when, and only when, A and B are completely associated. There are two possible senses of complete association here. The first is the strong sense in which A and B are said to be completely associated only when all A_1's are B_1's and all A_2's are B_2's (i.e. $b = c = 0$). In the above example, this would mean that all those who were vaccinated survived and all those who were not vaccinated died. There is also a weaker sense of complete association, according to which A and B are completely associated if either all A_1's are B_1's *or* all A_2's are B_2's. Either of the following two tables thus displays complete association in this sense:

	B_1 (vaccinated)	B_2 (unvaccinated)
A_1 (survived)	a	0
A_2 (died)	c	d

	B_1 (vaccinated)	B_2 (unvaccinated)
A_1 (survived)	a	b
A_2 (died)	0	d

In the first table none of the unvaccinated survive (even though some of the vaccinated die). In the second none of the vaccinated die (even though some of the unvaccinated live). Yule chose to use this weaker definition of complete association; thus his second criterion was that the coefficient should be $+1$ if and only if either $b = 0$ or $c = 0$.

The third property is that the coefficient should be -1 when A and B are completely associated in a negative sense. Again there is a strong and a weak meaning of complete negative association, and Yule chose the weak meaning. A and B are complete associated in the negative sense when either all A_1's are B_2's or all A_2's are B_1's.

	B_1	B_2
A_1	0	b
A_2	c	d

OR

	B_1	B_2
A_1	a	b
A_2	c	0

Thus the coefficient should be -1 if and only if either $a = 0$ or $b = 0$.

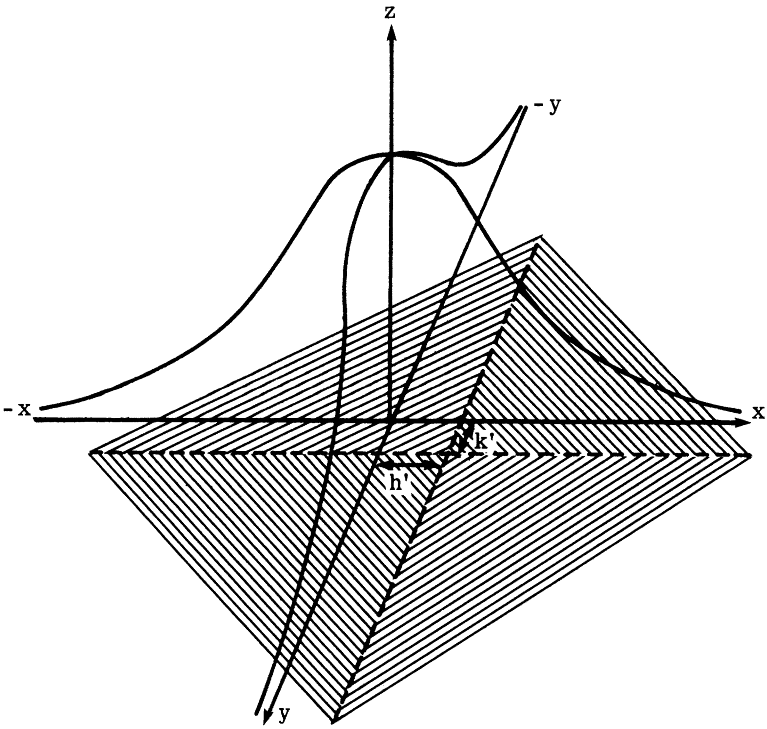
Yule then examined the coefficient $Q = \frac{ad - bc}{ad + bc}$. Clearly, if $ad - bc = 0$, then $Q = 0$. Conversely, $Q = 0$ implies $ad - bc = 0$. So Q satisfies the first condition. If either $b = 0$ or $c = 0$, then $bc = 0$, and $Q = ad/ad = +1$. Also if $Q = +1$, then $ad - bc = ad + bc$, hence $bc = 0$, and so either $b = 0$ or $c = 0$. So Q satisfies the second condition. Finally, if either $a = 0$ or $d = 0$, then $ad = 0$, and $Q = -bc/bc = -1$; conversely $Q = -1$ implies $ad - bc = -ad - bc$, hence $ad = 0$, and so either $a = 0$ or $d = 0$. Q thus satisfies all three conditions, and Yule put it forward as a measure of association in two-by-two tables. However, as Yule was aware, Q has no *special* justification. There are an unlimited number of functions which satisfy Yule's three conditions — for example Q^3, Q^5, and so on. Further, as Pearson was later to show, two different tables could be ranked in one order as regards strength of association by one of these functions, and in a different order by another.

Pearson's approach was to produce, by a much tighter but more precarious theoretical argument, a coefficient of association which he called the 'tetrachoric coefficient of correlation'. I shall denote it by r_T. The crucial assumption at the base of the derivation of r_T is that the observed four-fold table can be regarded as having arisen in the following fashion. The observed categories A_1, A_2 and B_1, B_2 are taken to correspond to ranges of more basic interval variables y and x: A_1 corresponding, for example, to $y \leqslant k'$, A_2 to $y > k'$, B_1 to $x \leqslant h'$, B_2 to $x > h'$. It is further assumed that y and x jointly follow a bivariate normal distribution, with x having zero mean and standard deviation σ_1, y zero mean and standard deviation σ_2. Geometrically this can be shown as in Figure 1.

In Figure 1 we see the bivariate normal frequency surface (which is shaped like a bell with elliptical cross-sections) rising above the plane of x and y. This plane is divided into 4 quadrants by lines through the point (h', k') — the four quadrants corresponding to the cells of the four-fold table. The volume above the top left of these quadrants corresponds to the frequency with which $x < h'$ and $y < k'$ and thus corresponds to the frequency a in the original table.[7]

Pearson had thus provided a model of a statistical distribution assumed to underly the given two-by-two table. The model has three parameters, h'/σ_1, k'/σ_2, and r, the correlation of x and y. There are three independent parameters in the given table (not four, as the total, N, is regarded as fixed and $a + b + c + d = N$). The model can be fitted to any four-fold table, as the equations relating the model and the observations are always soluble, although the solution requires the use of numerical methods (see Appendix). A value for r, the correlation of the underlying variables, can thus be found.

Figure 1. Pearson's Model of Underlying Variables



For explanation, see text.

This correlation of the underlying variables was what Pearson called the 'tetrachoric coefficient of correlation'. While Pearson was clearly aware that the mathematical derivation of this coefficient involved the assumption of an underlying bivariate normal distribution, and was also aware that this assumption could not usually be tested, he referred to it as *the* correlation in the title of his memoir and in other places. He did consider other, empirical, coefficients of association, including Yule's Q, but treated them only as approximations to r_T, with the advantage of much greater ease of calculation, but the disadvantage of deviating by a greater or lesser extent from r_T.

One last point has to be made before the further developments of the different approaches are considered. Yule's and Pearson's coefficients have been presented as if the data to which they were applied were always entire populations. In this I am remaining faithful to the work of Yule and Pearson, who did not systematically distinguish between sample statistics and population parameters. Such systematic distinctions only became widespread with the work of Fisher in the 1920s. Yule and Pearson were of course aware that the data to which they applied Q and r_T were often drawn from samples, but, apart from calculating the 'probable errors' of their coefficients, they did not address themselves generally to the problems posed by this.

FURTHER DEVELOPMENTS IN PEARSON'S AND YULE'S APPROACHES

The invention of the tetrachoric coefficient by no means concluded Pearson's theoretical work on the measurement of association. Indeed this area was a major focus of his work in mathematical statistics from 1900 to 1922. Pearson was fully aware of the shortcomings of r_T — in particular, its restriction to two-by-two tables. While continuing to champion the use of r_T, he attempted to find an approach to the problem of the measurement of association that would allow the direct analysis of larger tables (those in which objects are classed as $A_1, A_2, \ldots., A_p$ and $B_1, B_2, \ldots, B_q$) and would, if possible, avoid the assumptions involved in the derivation of r_T.

The most important of these attempts was his development of the theory of contingency. This derived from the application of his own X^2 (chi squared) test to two-way tables.[8] For any such table it is possible to work out the expected frequencies in each cell on the assumption that the two variables are independent, and then to measure the divergence between observed and expected frequencies by means of X^2. Reference to the distribution of X^2 then gives a measure of the probability of such a divergence from the expected frequencies, on the assumption of independence. The value of X^2 itself was of little direct interest to Pearson. He wanted not simply to reject the hypothesis of no association, but to measure the strength of association. The value of X^2 cannot serve as such a measure, because multiplying the frequencies in each cell of a table by a constant (which presumably does not alter the strength of association) multiples the value of X^2 by that constant. This problem is, however, easily avoided. If the value of X^2 is divided by

N, the total number of cases in the table, then the resultant coefficient clearly remains unaltered by multiplication of each cell in the table by a constant. This coefficient $\phi^2 = X^2/N$, Pearson referred to as the mean square contingency.[9]

A measure based on X^2 has clear attraction. It is free from any need to assume underlying variables, and it can be applied to any size of table. It is even independent of the ordering of the categories of each variable. The problem is, which particular measure based on X^2 should be used? Once again Pearson solved this problem by reference back to the correlation of normally-distributed interval variables. He supposed any given table to have arisen by splitting these continuous variables into categories. He then found a relationship between the mean square contingency for such a table and the coefficient of correlation of the underlying variables, r. In the limiting case that the number of cells in the table tends to infinity, he showed that:[10]

$$r = \pm\sqrt{\frac{\phi^2}{1+\phi^2}} \cdot$$

He then proposed the coefficient:

$$C_1 = \sqrt{\frac{\phi^2}{1+\phi^2}}$$

which he called the 'first coefficient of contingency'.[11] If the two-way table had arisen by categorization of an underlying bivariate normal distribution, and if the number of cells in the table was large, then C_1 approximated to the coefficient of correlation of the underlying variables. Because C_1 is a monotonic function of the value of X^2 for the table from which it is calculated, it has also a certain justification quite apart from the validity of these assumptions.

C_1 did not displace r_T in Pearson's affection. Pearson felt that C_1 was best used only in larger tables (of about 25 cells), because for small tables the limit relationship between C_1 and r did not hold, and thus C_1 was a bad estimate of the correlation of underlying variables.

> Hence the new conception of contingency, while illuminating the whole subject . . . does not do away with the older method of fourfold division.[12]

Pearson's fundamental criterion was still the relationship between a coefficient of association and the correlation of underlying variables: he still sought a coefficient of association directly comparable with the correlation coefficient of interval variables.

Other developments of the theory of association by Pearson and his co-workers follow broadly on the same lines. The desire for comparability with the interval-level coefficient of correlation can be seen in such comments as 'in order that our results shall agree fairly closely with the results for Gaussian distribution we select . . . our scale . . .'.[13] One major aim of this work was to 'improve' C_1 by various corrections, the most important being the class-index correction, described in 1913.[14] Again, the basis of the correction is the assumption of underlying continuous variables, and the purpose of the correction is to improve the estimate of the correlation of these variables by taking account of the fact that C_1 is calculated from a finite number of cells rather than the infinite number presupposed by the limit relationship between C_1 and r. Uncorrected, C_1 has thus a tendency to underestimate the 'true' correlation. The typical effect of a class index correction on a five-by-five table is to boost C_1 by about 0.05.

The final attempt Pearson made to find a 'perfect' solution to the problem of the measurement of association was to derive an iterative method for fitting a bivariate normal distribution to a two-way table (in effect, to find a counterpart to r_T for tables larger than two-by-two). A solution to this problem was published in a joint paper with his son Egon Pearson in 1922.[15] But the resultant 'polychoric coefficient', while representing in a sense the logical conclusion of Karl Pearson's approach to the problem, was in that pre-computer age defeated by the sheer laboriousness of its mode of calculation.

Yule developed two further coefficients, the 'product-sum coefficient', r_{PS}, and the 'coefficient of colligation', w. These two coefficients did not represent any fundamental break with the approach lying behind his earlier work. Both satisfy his three criteria for a coefficient of association, with the only difference being that while Q and w take the value 1 for perfect association in the weak sense (either b or c zero), r_{PS} takes this value only for positive association in the strong sense (both b and c zero). The product-sum coefficient is the ordinary interval-variable co-effficient of correlation applied to a two-by-two table, not on Pearson's sophisticated model, but 'naively', by making the assumption that the two categories correspond to the values 0 and 1 of a discrete variable. It can be shown that this yields the value

$$r_{PS} = \frac{ad - bc}{\sqrt{(a+c).(b+d).(a+b).(c+d)}}.$$

Yule referred to r_{PS} as 'the correlation-coefficient for a [two-by-two] table' although he did not suggest it displaced Q.[16] The coefficient of colligation[17] links Q and r_{PS}. The formula for it is

$$w = \frac{\sqrt{ad} - \sqrt{bc}}{\sqrt{ad} + \sqrt{bc}}$$

and Q and w are related by a simple equation:

$$Q = \frac{2w}{1+w^2}.$$

When the given two-by-two table is reduced to a standardized symmetrical form by multiplication and division of the rows and columns by constants until each marginal total equals ½N, w for the original table equal r_{PS} for the standardized table. So w and r_{PS} are also related. But the inter-relatedness of Q, w and r_{PS} is much weaker than the inter-relatedness of Pearson's coefficients, all of which bear some reference to the single theoretical standard of the interval-variable coefficient of correlation. Q, w, and r_{PS} give different values when applied to the same table, and Yule gave no general rules as to which to use in a given case.

THE CONTROVERSY

The fundamental issues at stake in the controversy were implicit in the two original papers Pearson and Yule published in 1900. Neither openly attacked the other, however, and personal relations between the two men seem to have remained good. Open conflict began only in late 1905. On 7 December, Yule read to the Royal Society of London two papers critical of some aspects of Pearson's work, in particular throwing doubt on the validity of the asssumptions underlying Pearson's use of the tetrachoric coefficient.[18] Pearson replied to these criticisms in an article in *Biometrika*.[19] At this stage, the controversy was still not generalized to all aspects of the competing approaches to the measurement of association. This happened only when Yule published his textbook *An Introduction to the Theory of Statistics,*[20] in which he gave an account of his measures Q and r_{PS}. Pearson's collaborator David Heron wrote a sharply-worded warning to the readers of *Biometrika* on the 'danger' of Yule's formulae.[21] Yule in his turn read to the Royal Statistical Society a long paper defending his position and attacking Pearson's.[22] Pearson and Heron replied in a paper covering 157 of the large pages of *Biometrika.*[23] This paper, published in 1913, effectively marked the end of the overt phase of the controversy.[24] It was, however, unresolved. Pearson and Yule no doubt felt they had fully stated their positions, but neither had succeeded even partially in convincing the other. Yule's obituary notice of Pearson, written in 1936, refers to the controversy and comments, 'Time will settle the question in due course'.[25]

The main focus of Yule's attack on the tetrachoric coefficient was on the assumptions involved in its derivation and use. He wrote:

> The introduction of needless and unverifiable hypotheses does not appear to me a desirable proceeding in scientific work.[26]

When dealing, for example, with vaccination statistics (an area where biometricians had applied the tetrachoric method), Yule argued that 'vaccinated', 'unvaccinated', 'survived' and 'died' constitute naturally discrete classes.

> . . . all those who have died of small-pox are all equally dead: no one of them is more dead or less dead than another, and the dead are quite distinct from the survivors.[27]

To apply here a coefficient that had as its basis an assumption of underlying continuous variables was absurd:

> At the best the normal coefficient can only be said to give us in cases like these a hypothetical correlation between supposititious variables.[28]

There were cases, Yule conceded, where the assumption of underlying continuity was 'less unreasonable'. In these cases, however, the hypothesis that the underlying distribution is bivariate normal was frequently doubtful. Pearson had often used the tetrachoric coefficient in two-by-two tables which had been obtained from larger tables by the amalgamation of adjacent classes. Indeed until his invention of the coefficient of contingency he was forced to do this, as he had no method of analyzing larger tables. In these larger tables, unlike two-by-two tables, it was possible to test the validity of the hypothesis of an underlying bivariate normal distribution.

This could be done in two ways. First, if the hypothesis is true, then it should not matter from the point of view of the calculation of r_T which precise way one chose to amalgamate classes: the value of r_T should be at least approximately independent of the boundary line chosen between the two final classes. Yule was thus able to test Pearson's hypothesis by calculating r_T in several different ways for the same large table. He showed that, at least in certain cases given by Pearson, the values obtained varied considerably, ranging for example from 0.27 to 0.58 in a table on the resemblance between fathers and sons in eye-colour.[29] Secondly, if a large table has in fact arisen according to Pearson's hypothesis, then it should display the property Yule termed 'isotropy'. Consider any 4 adjacent frequencies, n_1, n_2, n_3 and n_4, extracted from a larger table.

		n_1	n_2	
		n_3	n_4	

The table is called 'isotropic' if the sign of $n_1n_4 - n_2n_3$ is the same for all similar 'sub-squares' of the table. In his first published criticism of Pearson's work, Yule tested for 'isotropy' tables on which Pearson had, after amalgamation of classes, used r_T. He found that many were not 'isotropic'.[30]

Pearson defended himself by arguing that Yule's isotropy criterion was invalid because he had failed to evaluate the probable error of $n_1n_4 - n_2n_3$. Because a given table is only a sample from a larger population, a failure of isotropy may occur through random fluctuation alone. Pearson accepted that the variation in values of r_T obtained in different ways from the same table showed that in certain cases the assumption of underlying normality did not appear to be tenable. But he had been aware of this, he said, and the method of contingency had been developed to deal precisely with those cases. When coefficients of contingency were worked out for the tables in question, they were found to agree 'sensibly' with the tetrachoric coefficients, and Pearson claimed that his conclusions thus held, despite the flaws in the method by which they had been obtained.[31]

The basis of the attack on Yule's approach mounted by Pearson and Heron was that for the same table, Yule's various coefficients did not agree in value, and further that for tables formed from genuine bivariate normal data none agreed with the ordinary correlation coefficient. For one table given by Yule, Heron found that $Q = 0.91$ while $r_{PS} = 0.02$. For bivariate normal data Q did not differ very much from the correlation coefficient so long as divisions were taken near the medians, but for more extreme divisions the divergence could be large (e.g. $r = 0.5$, $Q = 0.97$). For such data Q varied in value according to exactly where the divisions were taken: the same is true of r_{PS} (and, indeed, of w).

Pearson and Heron felt that Yule was reifying his categories. Only in rare cases — such as that of Mendelian theory, where the categories of a two-by-two table correspond to the presence or absence of a Mendelian unit and thus the two variables genuinely are discrete (factor present = 1; factor absent = 0) — was the use of such methods justified. In these cases r_{PS} was the correct way to extend the ordinary theory of correlation, as it assumed just such discrete variables. In general, however, treating categories in this way was mere empty formalism.

> And here we will at once emphasise the fundamental difference between Mr Yule and ourselves. Mr Yule, as we will indicate later, does not stop to discuss whether his attributes are really continuous or are discrete, or hide under discrete terminology true continuous variates. We see under such class-indices as 'death' or 'recovery', 'employment' or 'non-employment' of mother, only measures of continuous variates — which of course are not *a priori* and necessarily Gaussian . . .
>
> The controversy between us is much more important than an idle reader will at once comprehend. It is the old controversy of nominalism against realism. Mr Yule is juggling with class-names as if they represented real entities, and his statistics are only a form of symbolic logic. No knowledge of a practical kind ever came out of these logical theories. As exercises for students of logic they may be of educational value, but great harm will arise to modern statistical practice, if Mr Yule's methods of treating all individuals under a class-index as identities become widespread, and there is grave danger of such a result, for his path is easy to follow and most men shirk the arduous.[32]

Pearson and Heron justified the biometric position by arguing that it was necessary to make *some* hypothesis about the nature of the continuous frequency distribution of which the observed classes were groupings. The only distribution which had been adequately studied mathematically was the normal. In practice, they argued, methods based on the normal distribution almost always gave adequate results. The unique advantage of these methods seemed to them to outweigh the difficulties involved:

> The coefficient of correlation has such valuable and definite physical meanings that if it can be obtained for any material, even approximately, it is worth immensely more than any arbitrary coefficients of 'association' and 'colligation'.[33]

COGNITIVE INTERESTS

It would be naive to assume, as is sometimes done, that the objections raised by one party in a scientific controversy to the position of the other can be taken as the explanation of the controversy. It is necessary, rather, to begin explanation by seeking factors which adequately describe the differing ways in which theories are developed and the differing criteria of evaluation employed by the two sides.

As a tentative hypothesis, I suggest that 'cognitive interests' may be among these factors. While the term is drawn from the work of Jürgen Habermas,[34] in using it I do not wish to imply the full applicability here of Habermas' stimulating but contentious epistemology. 'Cognitive interests' will be used here to refer to those aspects of the actual or potential scientific applications of theories which 'feed back' into theoretical development by structuring scientists' construction and judgment of theories. Clearly not all applications of a theory, nor all aspects of even a limited set of applications, affect theoretical development: theory is not the same as practice. Nor does 'applications' refer simply to 'uses' in the normal, technological sense. Scientists can and do use theories for purposes entirely internal to science. The point of using the term 'cognitive interests' is to focus on what might be called the 'goal orientation' of scientific sub-cultures, on the fact that theory construction and evaluation have to be seen as construction for particular ends and evaluation according to particular criteria.[35]

In a very general sense the work of Pearson and Yule can be seen as manifesting the same cognitive interests. As Habermas points out, the natural sciences typically embody cognitive interests in technical prediction and control. Statistical theory, in for example its provision of techniques of inference for use in situations of uncertainty, can in a general sense be taken as enhancing the scope of prediction. In providing measures of association, both Pearson and Yule were attempting to extend the scope of statistical analysis into a field where no reliable techniques of inference were available. To frame matters like this is, however, insufficiently specific. There was no single 'natural' way to extend the scope of statistical analysis into this new area; the different ways in which Pearson and Yule did it can perhaps be accounted for by the differing concrete forms in which general interests in prediction and control were manifested.

Pearson's work was dominated by its reference to an existing achievement of statistical theory, the interval-level theory of correlation and regression. For Pearson, this theory was an exemplary instance of the way statistics enhanced the scope of prediction. Thus regression was the theory of how best to predict the value of one variable from that of another, in situations where there was no one-to-one correspondence. The correlation of two variables was, for Pearson, that constant, or set of constants, that was sufficient to describe how the expected value of one variable depended on the value of another.[36] In one case only had the correlation in this sense been fully specified — that of two variables that followed a bivariate normal distribution. Given the correlation coefficient for two such variables, it was possible to state immediately the expected value of one variable associated with any value of the other.

Pearson's approach to the association of nominal variables was evidently structured by an interest in maximizing the analogy between the association of such variables and the correlation of interval-level variables with a joint normal distribution. This correlation had a clear meaning in terms of prediction, and this meaning made it uniquely suitable as the criterion for judging the strength of association. Use of this basic reference point was the foundation of Pearson's attempt to construct a unitary theory of association and correlation, and of his negative evaluation of the work of Yule.

The derivation of r_T shows that Pearson intially defined association as the correlation of the hypothetical underlying bivariate normal distribution. In the later work on contingency this literal superposition of the two cases was partially discarded:

Pearson accepted that the assumption of an underlying bivariate normal distribution might not be factually correct. But the analogy still operated, as can be seen in the way that the bivariate normal model was used to choose the particular functions of X^2 that were selected to be the coefficients of contingency. Measures of association were thus seen by Pearson as ways of estimating the correlation of an actual or notional underlying distribution. This was, in effect, simply what Pearson meant by 'measuring association', and the way in which he described r_T as the 'the coefficient of correlation' indicates the taken-for-granted nature of the metaphor. For Pearson, the basic criterion of the validity of coefficients of association was their usefulness in the estimation of this underlying correlation.

This criterion of validity was typically operationalized in the following way. Interval data that followed a bivariate normal distribution would be taken and from this data a two-by-two or larger table would be constructed. Thus if the data referred to the height and weight of individuals, a two-by-two table could be constructed by classifying those individuals over six feet as 'tall', those under as 'short', those over 150 lb. as 'heavy', those under as 'light'. A coefficient of association would then be applied to this table. If the value of the coefficient approximated well to the interval-level correlation of height and weight, this was a point in its favour. If the values of a coefficient did not tally with the coefficient of correlation, then this was an argument for its rejection.

The tetrachoric coefficient passed this test; its ability to do so was of course guaranteed by its method of construction. So did the coefficient of contingency, at least for sufficiently large tables. Yule's coefficients, on the other hand, all failed abysmally. Not only were they on the whole poor approximations to the coefficient of correlation, but the values they took depended on where the arbitrary divisions between 'tall' and 'short' and 'heavy' and 'light' were taken.[37]

Given the basic interest in maximizing the nominal/interval analogy, Pearson's use of the bivariate normal model makes sense. It was not that he was obsessed by the normal distribution. Quite the opposite: he was one of the first statisticians to point to the non-normal nature of many empirical distributions, and had sought, albeit unsuccessfully, to develop a theory of correlation for non-normal variables which would fully take into account their non-normality.[38] Pearson's position was pragmatic. If correlation is taken, as Pearson took it, to depend upon the specification of the function which best predicts the value of one variable from that of another, then something about the joint distribution of the two variables must be assumed. Only one joint distribution was, Pearson felt, sufficiently well known for this kind of analysis to be possible — the bivariate normal. Experience with the normal distribution had, he argued, shown that even if the assumption of normality was not strictly correct, inferences based on that assumption were unlikely to be seriously mistaken.[39] Thus if one had to use a model, Pearson felt that the bivariate normal was best. Further, some model was necessary if the nominal/interval analogy was to have any validity. For consider Yule's Q as an example of a coefficient not based on an explicit model. Values of Q are not comparable with those of the coefficient of correlation. Nor can comparability of the nominal and interval cases be achieved by reducing the interval data to two-by-two tables and applying Q, for the value of Q depends on the process by which this is done. Indeed, comparison of the values of Q from one two-by-two nominal table to another becomes, on this perspective, a process which is very difficult to justify. Without some model of the situation to give a meaning to coefficients of association, their comparative use appeared to Pearson dangerously arbitrary.

Pearson's approach to the theory of association was thus fairly tightly structured by the analogy between the association of nominal variables and correlation employed as a tool for interval-level prediction. Yule's approach was much looser. A coefficient of association in the nominal case (or indeed a coefficient of correlation in the interval case) was for him a measure of statistical dependence that need satisfy only general formal criteria (be zero for independence, one for complete dependence, and so on). Just to know that two variables are associated (that vaccination and survival, for example, are not independent) is obviously of some use in solving problems of prediction and control. Yule was not primarily concerned to be able to draw tighter inferences than this. *Specific* problems of prediction and control in *specific* contexts of application did enter into Yule's choice of *particular* coefficients (for example, between Q, w and r_{PS} in any particular instance) but did not structure Yule's overall formulation of the problem of association.[40] Yule can thus be seen as putting forward a general, formal, theory of association which left a great deal of room for elaboration in specific instances. He did not seek a single best measure of association. Just as there are different measures of central tendency (mean, median, mode, and so on), there were, Yule felt, different ways of measuring association, which would yield different values for the same table. The superiority of one to the other could not be guaranteed in advance of the consideration of particular applications. Attempts to do so on the basis of contentious assumptions (such as that of underlying distributions) were, Yule felt, simply dangerous and misleading. Yule felt that when working with nominal data one had to accept the limitation implied by the level of measurement: one was dealing with cases classed into categories, and nothing more. The statistician had to accept the data as given. Yule's methods were thus structured by a cognitive interest in prediction using nominal data as phenomena in their own right; the nominal/interval analogy had for him no direct force.

The differing cognitive interests of Pearson and Yule led to their two positions being incommensurable.[41] Logic and mathematical demonstration alone were insufficient to decide between the two positions. Their concepts of 'measuring association' were different: for Pearson it meant seeking to estimate an underlying correlation; for Yule, seeking in a looser sense to measure the dependence of the given nominal data. The same mathematical result would be interpreted differently by the two sides, in the light of their different cognitive interests.

Thus both sides knew that for any given table Yule's three coefficients, Q, r_{PS} and w, would normally not agree, and sometimes would differ wildly in their values. For Pearson this was sufficient to damn Yule's system utterly, for how could there be three different values for the association of one table? For Yule, on the other hand, this was fully to be expected, for Q, r_{PS} and w were simply different ways of summing up the observed data. Similarly, both sides accepted that the value of the coefficient of contingency was affected by the size of the table to which it was applied. For Yule this was a severe weakness of the coefficient of contingency. Under certain circumstances its value reflected the number of cells in the table as much as the association of the data. For Pearson, on the other hand, this property was only to be expected. The coefficient of contingency was equal to the coefficient of correlation only in the limit case where the number of cells in the table became infinite. Therefore it was not surprising that the value of the coefficient of contingency should be affected by table size: on the assumption of an underlying normal distribution this could be corrected for. To take another instance, it was not disputed by either side that when applied to genuinely continuous, binormal data, the value of Yule's Q differed considerably according to where the division (for example, between tall and short) was taken. For Pearson this invalidated Q. For Yule any property that Q had when artificially applied to interval data did not affect its use for normal data, because he rejected Pearson's basic model of an underlying distribution.[42]

COGNITIVE INTERESTS AND GOAL ORIENTATION

The differing cognitive interests manifested in the work of Pearson and of Yule were not accidental. They can be related to their differing objectives in the development of statistical theory, and perhaps ultimately to differing social interests.

As Norton has shown in his paper in this issue,[43] Pearson's commitment to eugenics played a vital part in motivating his work in statistical theory. Pearson's eugenically-oriented research programme was one in which the theories of regression, correlation and association played an important part. The connection between these theories and eugenics had been first forged by their founder, Francis Galton, who had developed the theory of regression and the bivariate normal distribution while studying the relationship between two populations connected by heredity. Regression was originally a means of summing up how the expected characteristics of an offspring depended on those of its parents; the bivariate normal distribution was first constructed by Galton in an investigation of the joint distribution of parental and offspring characteristics.[44] Pearson's work in statistical theory continued this link between the mathematics of regression and correlation and the eugenic problem of the hereditary relationship of successive generations.

In his first fully general discussion of the statistical approach to the theory of evolution, Pearson gave the following operational definition of heredity:

> Given any organ in a parent and the same or any other organ in its offspring, the mathematical measure of heredity is the correlation of these organs for pairs of parents and offspring . . . The word organ here must be taken to include any characteristic which can be quantitatively measured.[45]

Two pages earlier Pearson had explained that the correlation of two variables (he used the term 'organs') was what defined the function allowing the prediction of the value of one from that of the other.[46] Put together, these notions of heredity and of correlation indicate what Pearson was doing. He was constructing a predictive mathematical theory of descent, in order to be able to predict from the knowledge of an individual's ancestry the characteristics of that individual. Galton had solved the problem for the individual's parentage; Pearson wished to go further back and consider grandparents, greatgrandparents, and so on.

Pearson's paper reveals two aspects of his attitude to correlation and its measurement. His notion of correlation, as a function allowing direct prediction from one variable to another, is shown to have its roots in the task that correlation was supposed to perform in evolutionary and eugenic prediction. It was not adequate simply to know that offspring characteristics were dependent on ancestral characteristics: this dependence had to be measured in such a way as to allow the prediction of the effects of natural selection, or of conscious intervention in reproduction. Pearson's goal was to establish conclusions such as the following:

> Accordingly on this hypothesis, with the correlation coefficients of inheritance anything like their value in man, five generations of selections of the type required in *both* parents would suffice to establish a breed.[47]

To move in the direction indicated here, from prediction to potential control over evolutionary processes, required powerful and accurate predictive tools: mere statements of dependence would be inadequate. Secondly, the prominence of correlation in his statistical thought can be seen to be related to the role of correlation as measuring the 'strength of heredity'. To define heredity as the correlation of parents and offspring indicates the a priori nature of Pearson's hereditarianism; that the correlation could be due to the similarity of parental and offspring environments was not even considered in this paper.[48] It also indicates the possibility that the direct linking of correlation and heredity could well be the motor behind Pearson's work on the theory of correlation. If the study of heredity was to be increased in its scope, the theory of correlation had to undergo parallel development. In this paper of 1896, the move from consideration of parentage to entire ancestry was clearly associated with the development of the theory of correlation from Galton's two variable case to an indefinite number of variables.

The major restriction on Pearson's studies of heredity in the late 1890s was their limitation to measurable characteristics. Many characteristics, such as the colouration of animals and plants and the eugenically crucial mental characteristics of man, were not immediately susceptible to quantification (this period of course predates the invention of the Binet-Simon scale of 'intelligence'). All that was possible for these characteristics was classification of individuals into categories, and as the resulting data could not be analyzed by an interval-level theory of correlation, there was no direct way of estimating the 'strength of heredity' for these characteristics. To extend research in heredity from interval to nominal characteristics required, given Pearson's operational definition of heredity, the extension of the theory of correlation from interval to nominal variables.

That this is the correct interpretation of the origins of Pearson's work on the theory of association is suggested by Pearson's own description of his problem situation:

> Many characters are such that it is very difficult if not impossible to form either a discrete or a continuous numerical scale of their intensity. Such, for example, are skin, coat, or eye-colour in animals, or colour in flowers . . . Now these characters are some of those which are commonest, and of which it is generally possible for the eye at once to form an appreciation. A horse-breeder will classify a horse as brown, bay or chestnut; a mother classify her child's eyes as blue, grey, or brown without hesitation and within certain broad limits correctly. It is clear that if the theory of correlation can be extended so as to readily apply to such cases, we shall have much widened the field within which we can make numerical investigations into the intensity of heredity, as well as much lessened the labour of collecting data and forming records.[49]

Pearson's research on heredity did not simply provide the motivation for the development of his theory of association. It also conditioned the nature of that theory. In his problem situation can be seen the connection between his social Darwinian and eugenic goals and the cognitive interests manifest in his work on association. Pearson already had what he felt to be a satisfactory means for the investigation of the inheritance of interval characteristics, by the use of which he had accumulated a considerable body of 'coefficients of heredity'. In order to maximize the value of information on the inheritance of nominal characteristics, it was necessary to devise a 'coefficient of heredity' for them that paralleled that for interval characteristics. Therefore the direction of development of the theory of association was, in the case of

Pearson, determined by the need to maximize the analogy between the association of nominal variables and the correlation of interval variables. Pearson wanted to be able to say 'the coefficient of heredity for human mental ability is r', and to compare that with the already calculated 'coefficients of heredity' for height, and other similar characteristics. A coeffficient of association such as Yule's Q would not have enabled him to do this. As explained above, values of Q cannot be compared with that of the coefficient of correlation; nor can height and mental ability data both be analyzed by the use of Q, because of Q's dependence on the arbitrary boundary between 'tall' and 'short'. For interval/nominal comparison to be plausible, Pearson needed a coefficient which, when applied to dichotomized height data, would yield a value as close as possible to that of the coefficient of correlation: hence Pearson's construction of r_T, and hence also his fundamental criterion of evaluation of coefficients of association.[50]

Pearson had in fact begun collecting a set of primarily nominal data of great relevance to eugenics even before he had devised, in r_T, the necessary means of analyzing it. Parent-child correlations were difficult to collect; Pearson however reasoned that the correlation of siblings (a term he introduced for pairs of brothers or sisters irrespective of sex[51]) were of equal theoretical value as measures of the strength of heredity.[52] By circulating teachers, he obtained information on nearly 4000 pairs of siblings, including interval physical characteristics such as the cephalic index, nominal physical characteristics such as eye-colour, and a range of nominal mental characteristics such as 'ability' and 'conscientiousness'. The study was begun in 1898; by 1903 Pearson felt able to give a comprehensive survey of the results obtained in his Huxley Lecture to the Anthropological Institute. This was Pearson's major contribution to the hereditarian theory of mental characteristics, and the forerunner of many later more sophisticated attempts to prove the dominance of nature over nurture.[53] It is also his most central attempt to use r_T, and the one which most strongly drew Yule's criticism.

Pearson's analysis of mental ability can be taken as an example of his procedure. He had asked teachers to classify each of a pair of siblings into one of the following classes: quick intelligent, intelligent, slow intelligent, slow, slow dull, very dull and inaccurate-erratic. 'Very dull', for example, was defined as 'capable of holding in their minds only the simplest facts, and incapable of perceiving or reasoning about the relationship between facts'.[54] To permit the use of r_T, these seven categories were reduced to two, 'quick intelligent' and 'intelligent' forming one category, and the rest the other. Two-by-two tables were then constructed, such as the following for pairs of brothers.[55]

Second Brother	First Brother		
	'Intelligent' and 'Quick intelligent'	Other	Totals
'Intelligent' and 'Quick intelligent'	526	324	850
Other	324	694	1018
Totals	850	1018	1868

From these tables, values of r_T were then calculated (in this case $r_T = 0.46$).

Pearson found from these data measures of the 'strength of inheritance' for nine mental and nine physical characteristics, and was also able to bring into the comparison other previously produced estimates of the correlation of physical characteristics in pairs of siblings. Central to his argument were two assumptions, only partly explicit: the comparability of the coefficients of correlation for interval data and the value of r_T for nominal data; and the interpretation of these coefficients as measures of the 'strength of heredity'. On the basis of these assumptions, he was able to claim a remarkable finding: the strength of inheritance for a wide range of human mental and physical characteristics was virtually identical at around 0.5. Further, he claimed that environment played no significant part, and thus presumably assumed that residual effects (the fact that the correlation was only 0.5 and not 1.0) were simply the result of chance variations. Environment could, Pearson felt, be discounted because his series of characteristics included eye-colour. It was accepted that environment played no part in determining eye-colour, and yet the strength of inheritance for eye-colour was very close to the common 0.5. If environment played no part in the case of eye-colour, Pearson deduced that it therefore played no part in the other cases. Pearson's conclusion was a strong affirmation of rigorous hereditarianism:

> We are forced, I think literally forced, to the general conclusion that the physical and psychical characters in man are inherited within broad lines in the same manner, and with the same intensity . . . We inherit our parents' tempers, our parents' conscientiousness, shyness and ability, even as we inherit their stature, forearm and span.[56]

At the end of the Huxley Lecture Pearson drew out the political conclusions which followed from his analysis. He talked of Britain's failure in imperialist competition with Germany and the United States, and the lack of intelligence and leadership that was the cause of it. His work, he argued, showed that the only solution was 'to alter the relative fertility of the good and the bad stocks in the community'.

> That remedy lies first in getting the intellectual section of our nation to realize that intelligence can be aided and be trained, but no training or education can *create* it. You must breed it, that is the broad result for statecraft which flows from the equality in inheritance of the psychical and the physical characters in man.[57]

Given the contemporary concern for 'national efficiency', these were words in season, and were not without impact outside the scientific community. Pearson's lecture was quoted at some length by the Inter-Departmental Committee on Physical Deterioration, which had been set up by the Conservative Government as a result of the scare following early defeats of the British by the Boers in the South African War.[58] Few of his contemporaries would have fully understood the mathematics of the tetrachoric coefficient, and few seem to have subjected his argument to close scrutiny, but the conclusion he was able to draw struck home.

Yule, on the other hand, had no commitment to eugenics. There is no record of his ever having made a public statement of his attitude to eugenics, nor do his letters to Karl Pearson, for example, reveal his opinions. In correspondence with the man who was perhaps his closest friend, Major Greenwood, it is however possible to discover evidence of Yule's private views. These appear to have been a mixture of indifference and hostility, as the following quotations indicate:

> . . . votes for women is to me nearly as loathworthy [sic] as eugenics.

> The Eugenics Congress is rather a joke . . .

> I've just got the letter from the Eugenics Ed[ucatio]n Soc[iety] asking me to lecture. I do not altogether like it . . .

> I am not a eugenist, and I am not the least keenly interested in eugenics.[59]

When Yule's academic work touched on subjects of eugenic importance, a certain distance from the standard eugenic positions is apparent. On the issue of heredity versus environment he was cautious:

> To take an example from the inheritance of disease, the chances of an individual dying of phthisis depends not only on the phthisical character of his ancestry, but also very largely on his habits, nurture and occupation.[60]

A major topic of Yule's early statistical work was pauperism, which the eugenists claimed to be a symptom of hereditary degeneracy. Yule, however, eschewed such arguments, and concentrated on the way administrative reforms, notably the abolition of out relief, reduced the observed rate of pauperism.[61]

Even while he was a student of Pearson, Yule gave signs that he was to develop in an independent direction from his teacher.[62] In 1893, aged 22, he became Pearson's demonstrator, assisting in the teaching of mathematics to engineering students — meanwhile forming, along with Alice Lee, the audience for Pearson's first advanced course in mathematical statistics.[63] In 1895 he was elected to, and became an active member of, the Royal Statistical Society, a body which Pearson, although Britain's foremost statistician, never joined. The concerns of this august but rather conservative body, rather than Pearson's social Darwinism, form the context of application for much of Yule's statistical work. Founded in 1834, the Statistical Society had shown little concern for the development of statistical method, focussing instead on administrative and official statistics, on the facts of such topics as finance, trade, wages, pauperism, crime, vaccination and epidemics. Abrams describes the Society:

> In its early years the Council of the Society often looked like a subcommittee of a Whig Cabinet. A significant element of its leading members, those who had most experience of intensive statistical research, was always made up of government officials. And serving, advising, and seeking to influence and rationalize government all encouraged the style of work to which the Society was already predisposed — accumulations of facts systematically detached from fundamental speculation about the meaning of facts.[64]

While Yule's work was technically far in advance of what the Royal Statistical Society was accustomed to, in subject, style and, indeed, in political assumptions it would have been familiar. Thus the Fellows were accustomed to an ameliorative orientation towards pauperism, and to Yule's focus on administration rather than the economy or social structure, even if the technical apparatus he employed was new.

It is possible that Yule may have come to realise the need for a measure of association while studying another favourite topic of Royal Statistical Society, vaccination statistics. In 1897, during a discussion of an anti-vaccinationist paper at the Society, he made a long and highly critical comment on the author's use of statistical technique.[65] Consideration of the frequently dubious use of statistics in the vaccination debates then raging[66] might well have prompted him to seek a standardized measure of the association between vaccination and survival during an epidemic. Although cognitive interests associated with an ameliorative orientation to vaccination statistics may have played some role in structuring Yule's work on association,[67] they did not generate a search for a single measure of association as a unique property of the data. At most, the requirements of the vaccination question placed but loose constraints upon the evaluation of measures of association. For example, a shared convention was needed which would distinguish between intervention being totally without effect (no association) and intervention being totally effective (complete association). But no more general inductive inferences needed to be drawn. Yule's use of formal rather than substantive criteria in the construction of coefficients of association, his development of an empirical rather than a unitary theoretical approach, and his preference for dealing with nominal data as it was given, would all make sense in the light of this situation.

It was not, however, that Yule was developing a general theory of association while Pearson was developing one with only a limited sphere of application. Pearson strongly felt that his was a general theory, and applied it even to Yule's favourite cases such as vaccination statistics; Yule most strongly criticized the application of Pearson's theory to inheritance data.[68] Both sides felt the theory of the other was *wrong,* and not merely *misapplied.* It was rather that Pearson's specific goal orientation led to a sophisticated and elaborate theory embodying specific cognitive interests, while Yule's more diffuse goal orientation led to a looser and more empirical approach which embodied cognitive interests of a more general nature.

FURTHER ASPECTS OF THE CONTROVERSY

Up to this point I have treated the controversy as if it were simply a dispute between two individuals, Pearson and Yule. While these two were overwhelmingly the most active participants, it is important to look at the involvement of others in the British statistical community. The group of scientists contributing to the development of statistical theory in Britain in the period 1900 to 1914 was small. A list produced from Kendall and Doig's *Bibliography of Statistical Literature,* together with examination of journals, correspondence, and so on as a check, consists of 26 individuals who can be seen as having in some sense an active ongoing interest in the development of statistical theory.[69] Of these, twelve can be regarded as members of Pearson's biometric school, since they had close institutional or personal ties to the Biometric and Eugenic Laboratories at University College London, and their preferred medium for publication seems to have been *Biometrika.* The other fourteen had a wide variety of affiliations, and included civil servants, administrators and one industrial scientist, as well as university staff.[70]

Ten of the twelve biometric school members either took part in attacks on Yule on this topic (Pearson, Heron), contributed to the theoretical discussion or development of the Pearsonian approach (J. Blakeman, W.P. Elderton, Everitt, Heron, Pearson, Snow, Soper) or used the tetrachoric coefficient in empirical work (E.M. Elderton, A. Lee, E.H.J. Schuster and all above except Blakeman and Soper). In the remaining two cases (Galton and Isserlis), I have not been able to find evidence of attitudes. Galton died in 1911, before the controversy came to a head; the work of Isserlis on the theory of statistics was just beginning at the end of this period. This overall pattern is as one would expect. The tetrachoric method and the related later developments were part of the distinctive approach of the biometric school, were widely applied to empirical data, primarily in the eugenic field, and were the focus of theoretical attention.

It is not that all individual members of the biometric school were convinced eugenists. Some were. Thus David Heron (1881-1969), Pearson's main collaborator in the attacks on Yule, seems to have retained eugenic convictions even after leaving the biometric group.[71] But the case of Yule shows that it was possible to be for quite a long period an active member of the biometric group without sharing the dominant attitude towards eugenics. For over

30 years this group was the major locus of teaching and research in statistics in Britain. It could therefore attract individuals who wished to receive training in statistics, but who did not necessarily share Pearson's beliefs. However, if one considers the biometric school as a social group, rather than as a mere aggregate of individuals, the focus must shift from individual motives to the institutionalized research activities of the group and to the group interests generated therefrom. The biometric school was a tightly-knit, coherent group,[72] a large part of whose funding came from its activities in eugenic research.[73] This research was a team activity in which data collection, the development of the necessary mathematical theory, computation, and so on, were closely integrated under the personal supervision of Karl Pearson.[74] So a relationship between the needs of eugenic research and the cognitive interests manifested in the development of the theory of association by the biometric school can reasonably be held to exist, irrespective of the particular motives of individual members of the school. I have not been able to discover whether P.F. Everitt, say, who drew up the tables of tetrachoric functions to permit easier calculation of r_T, shared Pearson's beliefs. The point is, however, that he was working to overcome a difficulty which had arisen within the context of an integrated research programme in which the demands of eugenic research generated, and conditioned the solution of, particular technical problems.

One important British statistician can be seen as leaving the biometric school in this period: Major Greenwood (1880-1949). In his case three parallel processes can be observed in the period 1910 to 1914. He left the immediate group of researchers round Karl Pearson at University College to take up a post of statistician at the Lister Institute of Preventive Medicine;[75] he seems, perhaps as a result of his move into a new academic field which traditionally stressed environmental causes of disease, to have become critical of eugenic doctrines;[76] and he changed from being an enthusiastic user of the tetrachoric coefficient (which in a 1909 paper arguing the importance of the hereditary factor in tuberculosis he described as the 'exact' and 'true' four-fold method) to being first a private and then an open critic of r_T.[77] While it would be impossible on the available evidence to demonstrate a causal relationship between these processes, the case of Greenwood adds weight to the association between membership of the biometric school, scientific work in the eugenic field, and use of the tetrachoric and other Pearsonian methods.

What of those statisticians who were not members of the biometric school? Of these only one, John Brownlee, seems to have been an enthusiast for the tetrachoric method. He was a member of the Glasgow Branch of the Eugenics Education Society.[78] Yule, Greenwood and Brownlee apart, only two 'non-biometric' statisticians seem to have publicly committed themselves on the measurement of association: F.Y. Edgeworth and R.H. Hooker. Neither, as far as I am able to tell, was a eugenist.[79] Both were members of the Royal Statistical Society, and it was at a meeting of it that they gave at least qualified support to Yule:[80] the Society seems, in fact, to have been the closest Yule came to having an 'institutional base'. Clearly it was in no way comparable to Pearson's Biometric and Eugenic Laboratories, with their own publications and journal, but at least the Society provided Yule with a sympathetic hearing and a place to publish his major attack on Pearson as well as other more minor writings on association.

Thus consideration of British statisticians other than Pearson and Yule seems to confirm in broad terms the association of Pearson's approach with the needs of eugenic research and that of Yule with the broader and less specific needs of general applied statistics. However, before moving to the final stage of the argument, it is necessary to consider other possible explanations of the controversy, and to examine briefly the history of the measurement of association after 1914.

It might be argued that Pearson's philosophical views account for his attitude to the measurement of association. However, it would seem that his approach, with its use of hypothetical underlying variables, violates rather than exemplifies the positivist and phenomenalist programme of *The Grammar of Science*.[81] The practical demands of his research proved stronger than his formal philosophy of science. His characterization of the dispute as between his 'nominalism' and Yule's 'realism' can indeed be turned on its head.[82] In their concepts of correlation Pearson was the 'realist' and Yule the 'nominalist'. Pearson's Huxley Lecture argument, for example, rests on the interpretation of a correlation as the measure of a real entity, as a strength of heredity, and largely collapses if a correlation is seen as merely the name for an observed pattern of data.[83] Pearson's general cosmological bent towards continuity and variation rather than homogeneity and discrete entities[84] may in part account for his rejection of methods such as r_{PS} (which involved treating individuals in a given category as in a certain sense identical), but cannot, it seems to me, account for the specific features of Pearson's methods of measuring association. In any case, general cosmological attitudes such as this are perhaps better seen as generalizations from scientific practice (in this case Pearson's practice as a statistical biologist and statistical eugenist) rather than as independent determinants of that practice.

Psychological explanations (such as a clash of personalities) also seem inadequate. Personal relations between Pearson and Yule seem to have been soured as a result of disagreement, rather than disagreement being caused by personal antagonism.[85] The divergence of views was already present in the perfectly amicable papers of 1900. Even if Pearson and Yule had remained the best of friends they would still have measured association differently, and this difference would still have to be explained.

A third possible alternative explanation might be that non-eugenic biometrical concerns were of equal or greater importance in leading to Pearson's development of the tetrachoric method. It is certainly true that Pearson used r_T to measure the 'strength of inheritance' in organisms other than man. But to separate a 'neutral' biometry from an 'ideological' eugenics would be ahistorical and would fail to capture the integral nature of Pearson's thought. The results of the biometric studies of heredity in plants and animals were used in eugenic arguments (comparing the strength of inheritance of human and animal characteristics, for example). Pearson's biometric work was in any case an endeavour to quantify the theory of evolution and to make it both rigorous and applicable to man. It was a social and political enterprise.

> The theory of evolution is not merely a passive intellectual view of nature; it applies to man in his communities as it applies to all forms of life. It teaches us the art of living, of building up stable and dominant nations, and it is as important for statesmen and philanthropists in council as for the scientist in his laboratory or the naturalist in the field.[86]

How did the controversy end? Debate virtually ceased at the time of the First World War. Two factors may have been involved in this. After 1918 the huge amount of data on inheritance of human and animal characteristics flowing into the Biometric and Eugenic Laboratories was much reduced. 'The post-war years were not favourable to the spread of Galton's eugenic creed' and in Pearson's work 'eugenics was for the moment set aside'.[87] Thus the immediate importance of the problem for Pearson was reduced, and much less theoretical and practical work on the measurement of association was done at the Biometric and Eugenic Laboratories. Secondly, a new approach to the theory of statistics was developing, most notably in the work of R.A. Fisher, which focussed attention on different sets of problems. Like Pearson, Fisher was a convinced eugenist, but there was a disjunction between his eugenics and his statistics, his crucial innovations in statistical theory being motivated by the demands of experimentation, particularly in agriculture, rather than by his eugenics. As a Mendelian his approach to heredity was different from that of Pearson, and he sought to measure the strength of heredity by an analysis of variance scheme based on a theoretical Mendelian model rather than by the direct comparison of correlation coefficients.[88] While Fisher did not reject Pearson's work on the inheritance of mental characteristics, his own research programme led him beyond it in a way that did not require the use of coefficients of association.

The controversy was not, however, resolved. Contemporary statistical opinion takes a pluralistic view of the measurement of association, denying that any one coefficient has unique validity. The influential work of Goodman and Kruskal argues that measures 'should be carefully constructed in a manner appropriate to the problem in hand'[89] in such a way as to have operational interpretations. The general approach of modern statisticians is thus closer to that of Yule than that of Pearson. Yule's Q remains a popular coefficient, especially amongst sociologists.[90] Pearson's tetrachoric coefficient, on the other hand, has almost disappeared from use except in psychometric work.[91] It is interesting to speculate whether this situation can be explained in terms of, on the one hand, the sharing by most modern statisticians of Yule's lack of an overall, specific goal-orientation and, on the other, the continuing influence of hereditarianism in psychometrics — but this point could be established only by an analysis of the contemporary literature, which is outside the scope of this study.

THE CONTROVERSY AND SOCIAL INTERESTS

The preceding analysis has shown that Pearson's and Yule's theories of association were structured by different cognitive interests, and that these different interests can be accounted for in terms of the relationship between Pearson's statistical theory and his eugenics research, and the lack of any similar relationship in the case of Yule. Pearson drew his support almost exclusively from the tightly-knit group of researchers, under his leadership, which was pursuing a unified research programme in statistics, biometry and eugenics. Yule gathered what support he could from individuals who were not enthusiastic about eugenics, and whose chief organizational link appears to have been the Royal Statistical Society.

The final stage of the analysis is necessarily very tentative. In this section I shall examine the social interests underlying eugenics in Britain, in order to suggest a possible grounding of the controversy in social interests arising from the changing social structure of Britain. Eugenics will be analyzed as an ideology expressing the interests of a particular section of British society but not those of other sections. In arguing this, I am not making any claim to provide a causal explanation of the beliefs of particular individuals. To take an analogy from the sociology of politics, to say that political party P expresses the interests of group G is not to imply that all members, or even most members, of G vote for P. It is rather to assert that P's policies, if put into effect, would enhance the wealth, status, power, security and so on of G. Differential support for P between members and non-members of G might then be anticipated, but the point is that the core of the argument is structural and not individual. Thus in examining possible connections between eugenically relevant research and social interests, I am certainly not claiming that these interests are necessary and sufficient to explain the scientific work and beliefs of particular individuals. Pearson, Yule and the other statisticians discussed here were individuals who followed frequently complex career patterns and developed often idiosyncratic commitments. Yet their choices of belief and affiliation were not taken in a vacuum, but in a given social and historical situation. Study of this situation can hopefully illuminate their choices, even if it cannot provide a causal account of them.

In Britain, the period at the end of the nineteenth and the beginning of the twentieth century was one of transition from early, laissez-faire, competitive capitalism to monopoly capitalism, with its extensive private and public bureaucracies. One of the consequences of this transition was a considerable expansion in the numbers, role and importance of what Poulantzas calls 'the new petty bourgeoisie', of the non-manual workers who were not owners of capital but who were differentiated from the manual working class by their favourable position in a division of labour with a rigid and hierarchical mental/manual split.[92] Of particular interest is the upper fraction of this class, the 'professionals', the doctors, scientists, engineers, social workers, and so on, whose activity was legitimated with reference to their accredited possession of a body of knowledge, which was held to be of unique importance and access to which was strictly controlled. This group can be seen as intermediate between the manual working class, on the one hand, and the bourgeoisie and aristocracy on the other. This situation would, one would expect, lead it to develop and adhere to ideologies that, on the one hand, emphasized its difference from, and superiority to, the manual working class, and, on the other, pointed to the social value of professional knowledge and skill as against the mere ownership of capital or land.

Eugenics was just such an ideology. According to eugenics, the difference between the professional and the manual worker was due to inherited differences in ability. Thus the division of mental and manual labour was given the force of a natural division between different types of people. At the same time the eugenists had an analysis of the particular problems of Victorian and Edwardian capitalist society, most importantly the situation of chronic deprivation and unrest of the urban lumpenproletariat, the 'residuum'. This group was claimed to be the 'unfit' in extreme form, and was the prime target for elimination by a eugenic programme. The eugenists' social policy was 'scientific', at least in its emphasis on giving full play to the skills and knowledge of the scientific professionals. Where the politician, philanthropist and priest had failed, the statistician, biologist, doctor and social worker could succeed.[93]

While some groups (such as the new professionals) increased in importance as a result of the development of British capitalism, others suffered a relative decline in their fortunes. The accommodation between, and intermixing of, landed property and industrial capital made the phenomenon of an aristocratic anti-bourgeois reaction a very muted affair in Britain. However the position of traditional élites was at least partly eroded. While many members of these élites did succeed in coming to terms with social change, others did not. As a consequence of their situation, one would expect them to deploy anti-industrialist, conservative ideas, emphasizing the value of the country as against the city, culture as against technology, the organic as against the atomistic, the aesthetic as against the utilitarian. And several cases of this can be at least tentatively identified: for example the 'culture and society' tradition discussed by Raymond Williams; Perkin's 'aristocratic ideal'; 'Christian socialism' as discussed by Levitas; the absolute idealist philosophers; and the Cambridge physicists as discussed by Wynne.[94]

What attitude could be predicted from such groups on the subject of eugenics? They had no reason to defend the urban residuum, nor to call into question the hierarchical division of labour, and certainly were unlikely to be attracted to assertion of the inherent equality of all. However, the scientistic, interventionist, middle class nature of eugenics was not likely to attract them. Although they had no quarrel with many of the basic premises of eugenics, as a cultural form it was alien to them. William Bateson, the pioneer geneticist and in many ways an archetypal conservative thinker, expressed this distaste for the petty bourgeois values of the eugenists, and for their self-confident reformism, when he wrote:

> Broadcloth, Bank balances and the other appurtenances of the bay-tree of righteousness are not really essentials of the eugenic ideal.
>
> The kind of thing I say on such occasions [talks about eugenics] is what no reformer wants to hear, and the Eugenic ravens are croaking for Reform . . .[95]

Thus I would anticipate a tendency for support for eugenics amongst rising professionals and indifference to it or distaste for it from opponents of bourgeois progress. (Although, as explained above, this remains only an expected correlation, not a deterministic prediction. The argument is structural, not individual.) Such indeed is roughly the observed pattern. The membership of the Eugenics Education Society was almost exclusively professional, with university teachers and doctors particularly prominent. Opposition to it came from such diverse groups as right-wing conservatives, the Catholic Church and defenders of traditional individual liberties.[96]

Let us now return to our two main protagonists. Pearson, the son of an upwardly-mobile lawyer who became a professor, the meritocratic, élitist socialist, the positivist free-thinker, can be taken as an archetype of the rising professionals. His early essays show a clear consciousness of himself as a member of 'the class which labours with its head' with interests distinct from both the manual working class and traditional élites. The former, for Pearson, were ignorant and politically volatile; the latter lacked the disinterested competence for the efficient running of a modern society.[97] His socialism was closest to that of the Fabians. He favoured the politics of gradual evolution towards a post-capitalist society controlled by an administrative and scientific élite, though he felt that the Fabian proposals for the extension of political democracy were dangerously illogical.[98] He called for 'the enthusiasm of the study' rather than that of 'the market-place'.[99] His view of morality was evolutionist and scientistic: his epistemology based on the search for sure knowledge on which to base social action. In his eugenics he was very much the Fabian, opposing those who wanted to go too far too fast and concerned to build up the scientific respectability of eugenics before engaging in rash political action. But that is not to say that his eugenics was socially neutral. It was explicitly used to justify, for example, educational differentiation. 'We need a system of education for the bulk of men, who follow, entirely independent of the system requisite for the minority, who organize and lead'.[100] That minority would have to be drawn predominantly from the ranks of head workers, not manual workers, for

> . . . the middle class in England, which stands there for intellectual culture and brain-work, is the product of generations of selection from other classes and of in-marriage.[101]

In discussing Yule, I have up to now dealt primarily with the 'public man', the statistical 'consultant' without strong specific commitments. There was however another side to Yule. Unlike Pearson, Yule was reticent about his social, political and philosophical attitudes, so there is a poverty of definite information on which to draw. What does, I think, emerge from his letters, from comments on him by those who knew him well and from occasional passages in his writings, is a personally genial but at the same time fundamentally detached, sceptical and conservative man. Major Greenwood wrote of Yule that 'politically, even in university politics, he is a stern, unbending Tory'.[102] In later life, he turned to religion.[103] On several politically-relevant scientific issues, his position was radically different from that of Pearson. As against Pearson's orthodox Darwinism, Yule advocated the anti-

Darwinian and mutationist views of J.C. Willis.[104] As against Pearson's 'entrepreneurial' and 'socially-relevant' science, Yule's ideal of the scientific researcher was of a 'loafer of the world', free from ties, grants and commitments.[105] As against Pearson's positivism, Yule was suspicious of the cult of measurement.[106] In his social position, Yule can, at least in his early life, be seen as downwardly mobile. He came from an old-established élite family of army officers, Indian civil servants and orientalists. Both his father and his uncle had been knighted.[107] The family's wealth does not, however, seem to have been transmitted to Yule. In the absence of a sufficiently well paying statistical job, he was forced, during most of the period discussed here, to take an administrative position in a board examining apprentice craftsmen and technicians, and to lecture in the evenings to clerks. While Yule's social situation cannot be seen as predetermining his attitude (there was nothing to stop him deciding, say, to throw in his hand with the eugenists or Fabians rather than to remain aloof), his career and beliefs, taken as a whole, can perhaps be seen as instancing possible connections between a declining élite, general conservativism and distaste for eugenics.

What of the others involved in the dispute? In the light of the institutionalized nature of the connection between statistics and eugenics in the biometric school, there would be little point in examining the social situation of individual biometricians other than Pearson. Although it would appear that in fact those of Pearson's students for whom information is available can in general be seen as 'rising professionals' rather than 'members of a declining élite' (for example, David Heron, who came from a Scottish village school up through the education system to be a leading figure in government and academic circles),[108] this sort of information is not of central importance. Yule's supporters are of somewhat greater interest, in that Yule was not the head of a research institution nor in any position of power, and thus we can be rather more certain that those who supported him did so out of conviction. Both Hooker and Edgeworth were similar to Yule in background. R.H. Hooker was the son of Sir Joseph Dalton Hooker and grandson of Sir William Hooker, both Directors of the Royal Gardens at Kew: he himself had a humbler career as a civil servant in the Board of Agriculture.[109] Francis Ysidro Edgeworth came from an old distinguished family of Anglo-Irish gentry (Edgeworthstown, County Longford was their family seat), but one that was in particularly sharp decline. Although Edgeworth was the fifth son of a sixth son, he was the last in the male line of the Edgeworth's, and by the time he had inherited it the family estate had sunk into neglect.[110] On the other hand Greenwood, although listed in Burke's *Landed Gentry*, can, as the son and grandson of doctors, be better placed in the body of the professional middle class. His case (in which it can be hypothesized that his later occupational position in public health led him away from his early eugenic commitment) indicates the complexities of the relationship between class position and eugenic belief.

The tentative hypothesis put forward in this section can be summarized as follows. It is suggested that two distinct constellations of interests can be seen in the British intelligentsia in the Victorian and Edwardian period. One was grounded in the situation of those professional occupations that were growing in importance with modernization: it found expression in technocratic ideologies such as Fabianism,[111] and in the eugenics movement. The other was grounded in the situation of those disparate members of the traditional élite (for example, downwardly-mobile offspring),[112] to whom modernization posed a threat: this constellation of interests found expression in various forms of conservatism, but not in scientistic ideologies such as eugenics. Eugenics can thus be seen as an ideology expressing the interests of some, but not other, sectors of British society.

CONCLUSION

I have argued that the two divergent approaches to the measurement of association to be found in the work of Pearson and Yule can be seen as expressing different cognitive interests; that these different cognitive interests arose from the different problem situations of a statistician whose primary commitment was to a research programme in eugenics and a statistician who lacked any such strong specific commitment; and finally, that eugenics itself embodied the social interests of a specific sector of British society, and not those of other sectors. Thus differing social interests can be seen as entering indirectly, through the 'mediation' of eugenics, into the development of statistical theory in Britain.

In the absence of a great deal of further research, particularly on the hypothetical constellation of interests suggested in the previous section, this conclusion must remain tentative. I hope, however, that this paper has shown that 'hard sciences', such as the mathematical theory of statistics, should not be excluded a priori from analysis in terms of social interests.

• *APPENDIX*

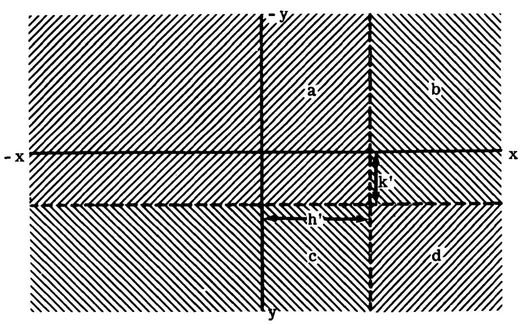
The Tetrachoric Expansion of the Bivariate Normal Distribution

In this account I have stayed as close as possible to Pearson's original presentation, while removing some of the more detailed steps of the argument. The modern statistician would of course want to improve this account by systematically distinguishing between sample statistics and population parameters. The derivation of the tetrachoric expansion can also be made neater by the use of characteristic functions and Hermite polynomials.[113]

Consider a bivariate normal frequence surface

$$z = \frac{N}{2\pi\sqrt{(1-r^2)}\sigma_1\sigma_2} \cdot \exp\left\{-\frac{1}{2}\cdot\frac{1}{(1-r^2)}\left(\frac{x^2}{\sigma_1{}^2}+\frac{y^2}{\sigma_2{}^2}-\frac{2rxy}{\sigma_1\sigma_2}\right)\right\}$$

where N is the total number of observations, σ_1 and σ_2 are the standard deviations of variables x and y (both of which are measured in terms of deviations from their respective means) and r is the correlation of x and y. Let this surface be divided into four parts by planes at right angles to the axes of x and y, at distances h' and k' from the origin:-



(This figure corresponds to the horizontal plane of Figure 1)

Let $h = \frac{h'}{\sigma_1}$, $k = \frac{k'}{\sigma_2}$. Then h and k can easily be evaluated in terms of the frequencies in the four quadrants formed by these two planes. Let these frequencies be a, b, c, d. Then

$$b+d = \int_{h'}^{\infty}\int_{-\infty}^{\infty} z \,.\, dx \,.\, dy = \int_{h'}^{\infty}\left[\int_{-\infty}^{\infty} z \,.\, dy\right] dx.$$

Now $$\int_{-\infty}^{\infty} z \,.\, dy = \frac{N}{\sqrt{2\pi}\,.\sigma_1} \,.\, \exp\left\{-\frac{1}{2}.\frac{x}{\sigma_1^2}\right\},$$

as this is the unconditional distribution of x.

So $$b+d = \frac{N}{\sqrt{2\pi}.\sigma_1}.\int_{h'}^{\infty} \exp\left\{-\frac{1}{2}.\frac{x^2}{\sigma_1^2}\right\} dx$$

$$= \frac{N}{\sqrt{2\pi}}.\int_{h}^{\infty} \exp\left\{-\frac{1}{2}.x^2\right\} dx.$$

Thus h can be evaluated in terms of b + d by use of tables of the normal distribution.

Similarly $$c+d = \frac{N}{\sqrt{2\pi}}.\int_{k}^{\infty} \exp\left\{-\frac{1}{2}.y^2\right\} dy$$

and k can be evaluated in terms of c + d.

Now

$$d = \int_{h'}^{\infty}\int_{k'}^{\infty} z \,.\, dx \,.\, dy$$

$$= \frac{N}{2\pi\sqrt{(1-r^2)}\sigma_1\sigma_2}.\int_{h'}^{\infty}\int_{k'}^{\infty} \exp\left\{-\frac{1}{2}\frac{1}{(1-r^2)}\left(\frac{x^2}{\sigma_1^2}+\frac{y^2}{\sigma_2^2}-\frac{2rxy}{\sigma_1\sigma_2}\right)\right\} dx \,.\, dy$$

$$= \frac{N}{2\pi\sqrt{(1-r^2)}}.\int_{h}^{\infty}\int_{k}^{\infty} \exp\left\{-\frac{1}{2}\frac{1}{(1-r^2)}(x^2+y^2-2rxy)\right\} dx \,.\, dy.$$

This equation relates r to d, N, h, and k (the last two of which we have already evaluated in terms of a, b, c, d), and can be solved for r. If the right-hand side is expanded in a series in r, after some manipulation the following result is obtained:

$$\frac{ad-bc}{N^2HK} = r + \frac{r^2}{2!} . hk + \frac{r^3}{3!} . (h^2-1)(k^2-1)$$

$$+ \frac{r^4}{4!} . h(h^2-3)k(k^2-3)$$

$$+ \frac{r^5}{5!} . (h^4-6h^2+3)(k^4-6k^2+3)$$

$$+ \frac{r^6}{6!} . h(h^4-10h^2+15)k(k^4-10k^2+15)$$

$$+ \frac{r^7}{7!} . (h^6-15h^4+45h^2-15)(k^6-15k^4+45k^2-15)$$

$$+ \frac{r^8}{8!} . h(h^6-21h^4+105h^2-105)k(k^6-21k^4+105k^2-105)$$

$$+ \text{etc.}$$

where $$H = \frac{1}{\sqrt{2\pi}} . \exp\left\{-\frac{1}{2}h^2\right\}; \quad K = \frac{1}{\sqrt{2\pi}} . \exp\left\{-\frac{1}{2}k^2\right\}$$

With $|r|<1$, the series converges rapidly, and terms of order higher than r^8 can normally be neglected, leaving a polynomial equation for r that can be solved numerically. Thus given observed frequencies a, b, c, d it is always possible to fit the model of an underlying bivvariate normal distribution to the observations, and to deduce a value for its correlation.

• *NOTES*

I am grateful to Mr George B. Greenwood, to Professor E.S. Pearson, to the Secretary of the Royal Statistical Society, and to the Librarians of University College London and the American Philosophical Society for allowing me to consult unpublished documents in their possession. I should also like to thank those who commented on earlier drafts of this paper, in particular Barry Barnes who suggested the usefulness of the notion of 'cognitive interests' in illuminating this episode. An earlier summarized version of this paper was read to the meeting of Project PAREX at Regensburg, July 1976.

1. There has been no full historical analysis of this controversy. However, Helen Walker gives a useful annotated bibliography of the major papers in her *Studies in the History of Statistical Method* (Baltimore, Md.: Williams and Wilkins,

1929), 130-41, and the important theoretical papers by L.A. Goodman and W.H. Kruskal on 'Measures of Association for Cross Classifications' contain a most thorough review of previous work in the area. See the *Journal of the American Statistical Association,* Vol. 49 (1954), 732-64 and Vol. 54 (1959), 123-63.

2. The use of terms such as 'interval' and 'nominal' here is anachronistic, but their use clarifies the issue at stake. For these terms see S.S. Stevens, 'On the Theory of Scales of Measurement', *Science,* Vol. 103 (1946), 677-80.

3. The crucial papers were F. Galton, 'Typical Laws of Heredity', *Proceedings of the Royal Institution,* Vol. 8 (1877), 282-301; F. Galton, 'Family Likeness in Stature', *Proceedings of the Royal Society,* Vol. 40 (1886), 42-73; F. Galton, 'Correlations and their Measurement, chiefly from Anthropometric Data', ibid., Vol. 45 (1888), 135-45; F.Y. Edgeworth, 'Correlated Averages', *Philosophical Magazine,* Series 5, Vol. 34 (1892), 190-204; S.H. Burbury, 'On the Law of Distribution of Energy', *Philosophical Magazine,* Series 5, Vol. 37 (1894), 143-58; K. Pearson, 'Mathematical Contributions to the Theory of Evolution III: Regression, Heredity and Panmixia', *Philosophical Transactions of the Royal Society, Series A,* Vol. 187 (1896), 253-318. This work is discussed in more detail in my forthcoming dissertation, to be presented to the University of Edinburgh.

4. This disagreement is discussed below in note 38.

5. G.U. Yule, 'On the Association of Attributes in Statistics', *Philosophical Transactions of the Royal Society, Series A,* Vol. 194 (1900), 257-319. Reprinted in A. Stuart and M.G. Kendall (eds), *The Statistical Papers of George Udny Yule* (London: Griffin, 1971), 7-69. Further references will be to the latter version. In the following I have been forced, for the sake of clarity, to use a standard form of notation. This is to be regretted, as Yule's and Pearson's notations did to some extent reflect their differing purposes. See below, notes 6 and 7.

6. Yule in fact used a slightly different notation, drawn from symbolic logic. For A_1 and A_2 he wrote A and α, where α signified not-A, and for B_1 and B_2 he wrote B and β, with β signifying not – B. His notation for the frequency I label 'a' was (AB), for 'b', (A β), etc.

7. Pearson, 'Mathematical Contributions to the Theory of Evolution VII: On the Correlation of Characters not Quantitatively Measurable', *Philosophical Transactions of the Royal Society, Series A,* Vol. 195 (1900), 1-47. Pearson, who wished to emphasize the analogy between r_T and the ordinary coefficient of correlation, referred to it simply as 'r'.

8. The X^2 test was first presented in K. Pearson, 'On the Criterion that a Given System of Deviations from the Probable in the Case of a Correlated System of Variables is Such that it can be Reasonably Supposed to have Arisen from Random Sampling', *Philosophical Magazine,* Series 5, Vol. 50 (1900), 157-75.

9. K. Pearson, 'Mathematical Contributions to the Theory of Evolution XIII: On the Theory of Contingency and its Relation to Association and Normal Correlation', *Draper's Company Research Memoirs: Biometric Series, I* (London: Dulau, 1904), 6. ϕ^2 is sometimes used today as a measure of association. For two-by-two tables

$$\phi^2 = \frac{(ad - bc)^2}{(a+b).(c+d).(a+c).(b+d)}$$

and has an upper limit of 1.

10. Ibid., 7-8.

11. Ibid., 9. Pearson also proposed a second coefficient of contingency, based on a different function of the divergence between observed and expected frequencies. This was easier to calculate but did not have any similar clear relationship to r, and was less used.

12. Ibid.

13. K. Pearson, 'Mathematical Contributions to the Theory of Evolution XVIII: On a Novel Method of regarding the Association of two Variates classed solely in Alternate Categories', *Draper's Company Research Memoirs: Biometric Series, VII* (London: Dulau, 1912), 24. For a general review of this work, see K. Pearson (ed.), *Tables for Statisticians and Biometricians* (Cambridge: Cambridge University Press, 1914), xxxvi-xlii, lvii-lx.

14. K. Pearson, 'On the Measurement of the Influence of "Broad Categories" on Correlation', *Biometrika,* Vol. 9 (1913), 116-39.

15. K. Pearson and E.S. Pearson, 'On Polychoric Coefficients of Correlation', *Biometrika,* Vol. 14 (1922-23), 127-56.

16. The product-sum coefficient was first introduced by Yule in his textbook, *An Introduction to the Theory of Statistics* (London: Griffin, 1911), 212-13. This coefficient had previously and independently been suggested by the geneticist W. Johannsen in his *Elemente der exakten Erblichkeitslehre* (Jena: Fischer, 1909), 272-79, and by the anthropologist F. Boas, 'Determination of the Coefficient of Correlation', *Science,* new series, Vol. 29 (1909), 823-24. It had even been used by Pearson, quite without comment, in 1904, but in a very different situation, that of theoretical Mendelian inheritance (for which see below). K. Pearson, 'Mathematical Contributions to the Theory of Evolution XII: On a Generalised Theory of Alternative Inheritance, with special reference to Mendel's Laws', *Philosophical Transactions of the Royal Society of London, Series A,* Vol. 203 (1904), 53-86.

17. The coefficient of colligation is introduced and discussed in Yule, 'On the Methods of Measuring Association between two Attributes', *Journal of the Royal Statistical Society,* Vol. 75 (1911-12), 579-642, as reprinted in Kendall and Stuart (eds), op. cit. note 5, 107-70.

18. Yule, 'On a Property which holds good for all Groupings of a Normal Distribution of Frequency for Two Variables, with Applications to the Study of Contingency-Tables for the Inheritance of Unmeasured Qualities', *Proceedings of the Royal Society, Series A,* Vol. 77 (1906), 324-36; 'On the Influence of Bias and of Personal Equation in Statistics of ill-defined Qualities', *Journal of the Anthropological Institute,* Vol. 36 (1906), 325-81 (abstract in *Proceedings of the Royal Society, Series A,* Vol. 77 [1906], 337-39).

19. Pearson, 'Reply to Certain Criticisms of Mr G.U. Yule', *Biometrika,* Vol. 5 (1907), 470-76.

20. Yule, op. cit. note 16.

21. D. Heron, 'The Danger of Certain Formulae suggested as Substitutes for the Correlation Coefficient', *Biometrika,* Vol. 8 (1911-12), 109-22.

22. Yule, op. cit. note 17.

23. K. Pearson and D. Heron, 'On Theories of Association', *Biometrika,* Vol. 9 (1913), 159-315. K. Pearson, 'Note on the Surface of Constant Association', ibid., 534-37, is essentially a supplement to this paper.

24. One partial exception is a paper by Major Greenwood and Yule in which they consider and reject as implausible the bivariate normal model for the case of

vaccination statistics: 'The Statistics of Anti-typhoid and Anti-cholera Inoculations, and the Interpretation of such Statistics in general', *Proceedings of the Royal Society of Medicine (Epidemiology),* Vol. 8 (1915), 113-90, reprinted in Stuart and Kendall (eds), op. cit. note 5, 171-248.

25. Yule, 'Karl Pearson, 1857-1936', *Obituary Notices of the Royal Society of London,* Vol. 2 (1936-38), 84.

26. Yule, op. cit. note 17, 140.

27. Ibid., 139-40.

28. Ibid., 140.

29. Ibid., 144.

30. Yule, 'On a Property . . .', op. cit. note 18.

31. Pearson, op. cit. note 19.

32. Pearson and Heron, op. cit. note 23, 161, 302.

33. Ibid., 300.

34. J. Habermas, *Knowledge and Human Interests* (London: Heinemann, 1972).

35. For a fuller discussion of this notion of 'cognitive interests', see S.B. Barnes and D. MacKenzie, 'On the Role of Interests in Scientific Change', in R. Wallis (ed.), *Rejected Knowledge, Sociological Review Monograph* (1978, in press).

36. Pearson, op. cit. note 3, 256-57.

37. For examples of this process of evaluation see Pearson, op. cit. note 7, 15-18, and Pearson and Heron, op. cit. note 23, 193-202. Pearson's use of it can be found from the very beginning of his work on association. Thus, on 6 May 1899, before the appearance of the first published papers on the topic, he wrote to Yule pointing out to him that Q failed this text (Pearson papers, University College London, Cl D6).

38. K. Pearson, 'Contributions to the Mathematical Theory of Evolution II: Skew Variations in Homogeneous Material', *Philosophical Transactions of the Royal Society, Series A,* Vol. 186 (1895), 343-414; K. Pearson, 'Notes on the History of Correlation', *Biometrika,* Vol. 13 (1920), 25-45, reprinted in E.S. Pearson and M.G. Kendall (eds), *Studies in the History of Statistics and Probablility* (London: Griffin, 1970), 185-205.

Pearson felt that an approach to the correlation of non-normal variables must be built on knowledge of the particular form of their joint distribution, for only if this was known would it be possible to know how best to predict values of one variable from that of the other. Yule, by comparison, claimed that the ordinary product-moment coefficient could be used for these non-normal variables as it had an interpretation as the slope of best-fitting line (in the least-squares sense) through their joint distribution irrespective of the particular form of this distribution. See Yule, 'On the Significance of Bravais' Formulae for Regression, etc., in the Case of Skew Correlation', *Proceedings of the Royal Society, Series A,* Vol. 60 (1897), 477-89; Pearson, op. cit. note 3, 274; Pearson, 'Notes on the History of Correlation', op. cit.; and the letters of 1896 between Pearson and Yule in the Pearson Papers, University College London, Cl D6.

39. Pearson and Heron, op. cit. note 23, 300.

40. Indeed Yule was to come to doubt whether a coefficient of association was always what was needed. He wrote to Major Greenwood on 2 March 1915 (the letter is one of a collection in the possession of George B. Greenwood, Esq.):

> Here are the cholera arithmetic and diagrams. I have also enclosed a couple of sheets of lucubration on the measure of the advantage, and efficiency or effectiveness, of immunisation or similar processes. I cannot see my way to a measure of association, for I cannot get clear in my mind to begin with what we want to measure by the association coefficient: I seem to get more muddle headed whenever I try to think it out. In fact I don't seem really to want a measure of association at all. The 'advantage' or 'effectiveness' give what I want and neither is of the nature of an association coefficient, but the first is a regression and the second GOD knows what.

41. For the concept of incommensurability, see P.K. Feyerabend, 'Explanation, Reduction and Empiricism', in H. Feigl and G. Maxwell (eds), *Scientific Explanation, Space and Time, Minnesota Studies in the Philosophy of Science,* Vol. 3 (Minneapolis: University of Minnesota Press, 1962), 28-97; T.S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: The University of Chicago Press, second edition, 1970), especially 148-50.

42. For the above see Yule, op. cit. note 17, especially 145-46 and 159-63; Pearson and Heron, op. cit. note 23, especially 171-83, 193-202; Pearson, op. cit. note 9, especially 8-9; Pearson, op. cit. note 14.

43. Bernard Norton, 'Karl Pearson and Statistics: The Social Origins of Scientific Innovation', *Social Studies of Science,* Vol. 8 (1978), 3-34.

44. See the papers of Galton cited in note 3, and Ruth Schwartz Cowan, 'Francis Galton's Statistical Ideas: The Influence of Eugenics', *Isis,* Vol. 63 (1972), 509-28.

45. Pearson, op. cit. note 3, 259.

46. Ibid., 256-57.

47. Ibid., 317 (Pearson's emphasis).

48. Later Pearson attempted to demonstrate the small role of environment by comparing 'coefficients of heredity' with correlations between the characteristics of children and particular aspects of their home environment; however, this was for him a subsidiary problem, as he believed that home environment was in any case largely a reflection of the innate characteristics of a child's parents.

49. K. Pearson (with the assistance of Alice Lee), 'Mathematical Contributions to the Theory of Evolution VII: On the Application of Certain Formulae in the Theory of Correlation to the Inheritance of Characters not Capable of Quantitative Measurement', *Proceedings of the Royal Society, Series A,* Vol. 66 (1900), 324-25.

50. See above, pp.49-50.

51. K. Pearson, *Life, Letters and Labours of Francis Galton* (Cambridge: Cambridge University Press, 1914-30), Vol 3A, 332.

52. Sibling correlations and parent-child correlation were of course connected by the famous Galton/Pearson 'Law of Ancestral Heredity'. See K. Pearson, 'Mathematical Contributions to the Theory of Evolution: On the Law of Ancestral Heredity', *Proceedings of the Royal Society, Series A,* Vol. 62 (1898), 404-07.

53. Three crucial differences between Pearson's work and later studies are the introduction of a numerical scale of 'intelligence', the use of twins as well as siblings in general, and the application of multi-factorial Mendelian models (in addition to simple measures of resemblance) to gain estimates of 'heritability'. Important though these differences are, this later work can be seen as elaborating Pearson's basic approach, rather than diverging radically from it. Pearson's lecture, entitled 'On the Inheritance of the Mental and Moral Characteristics in Man, and its

Comparison with the Inheritance of the Physical Characteristics', was published in the *Journal of the Anthropological Institute,* Vol. 33 (1903), 179-237. For an interesting point of view on this lecture see B.L. Welch, 'Statistics — a Vocational or a Cultural Study?', *Journal of the Royal Statistical Society, Series A,* Vol. 133 (1970), 531-43, but see also the comments by E.S. Pearson, ibid., Vol. 135 (1972), 143-46.

54. K. Pearson, op. cit. note 53, 209.

55. Reconstructed from Pearson's full data, ibid., 236.

56. Ibid., 204.

57. Ibid., 207 (Pearson's emphasis).

58. *Report of the Inter-Departmental Committee on Physical Deterioration* (London: HMSO, 1904, Cd. 2175), 38-39. For the general background see G.R. Searle, *The Quest for National Efficiency* (Oxford: Blackwell, 1971).

59. Yule to Greenwood, 3 April 1912, 8 August 1912, 8 November 1912, 17 August 1920. Letters in the possession of George B. Greenwood.

60. G.U. Yule, 'Mendel's Laws and their Probable Relations to Intra-Racial Heredity', *New Phytologist,* Vol. 1 (1902), 228.

61. G.U. Yule, 'On the Correlation of Total Pauperism with Proportion of Out-Relief' *Economic Journal,* Vol. 5 (1895), 603-11, Vol. 6 (1896), 613-23; 'Notes on the History of Pauperism in England and Wales from 1850, treated by the method of Frequency Curves; with an Introduction on the Method', *Journal of the Royal Statistical Society,* Vol. 59 (1896), 318-49; 'An Investigation into the Course of Pauperism in England, chiefly during the last two Intercensal Decades', ibid., Vol. 62 (1899), 249-86.

62. Biographical details for Yule are to be found in F. Yates, 'George Udny Yule, 1871-1951', *Obituary Notices of Fellows of the Royal Society,* Vol. 8 (1952-53), 309-23 and M.G. Kendall, 'George Udny Yule, CBE, FRS', *Journal of the Royal Statistical Society, Series A,* Vol. 115 (1952), 156-61, reprinted in Kendall and Stuart (eds), op. cit. note 5, 1-5.

63. I am grateful to Professor E.S. Pearson for allowing me to examine Yule's lecture notes for this course.

64. P. Abrams, *The Origins of British Sociology, 1834-1914* (Chicago: The University of Chicago Press, 1968), 15.

65. *Journal of the Royal Statistical Society,* Vol. 60 (1897), 608-12. The paper Yule was criticizing was A. Milnes, 'Statistics of Small-Pox and Vaccination, with Special Reference to Age-Incidence, Sex-Incidence and Sanitation', ibid., 552-603.

66. For the vaccination debates see R.M. MacLeod, 'Law, Medicine and Public Opinion: the Resistance to Compulsory Health Legislation, 1870-1907', *Public Law* (Summer 1967), 107-28; (Autumn 1967), 189-211.

67. In measuring the association of vaccination and survival it is obviously desirable for comparative purposes to have a measure which is independent of both the virulence of the epidemic (of the overall proportion of cases falling into the 'survived' and 'died' columns) and of the degree of activity of the medical authorities (proportions vaccinated and unvaccinated). Yule thus sought to construct coefficients which were unaltered by multiplication of any row or column by a constant. See Yule, op. cit. note 17, 113-23.

68. Pearson, op. cit. note 7, 43-45; Yule, op. cit. note 17, and 'On a Property . . .', op. cit. note 18.

69. The list omits those who wrote only one paper in the field and who did not,

therefore, seem to have had an ongoing active interest in it. The most obvious problem of inclusion/exclusion is the decision as to whether a piece of work contains a development of statistical theory and method or simply an application of existing methods. Thus, for example, Spearman is included but Burt excluded, and while this does indicate real differences in the type of work they did, it shows that there is no absolute division between those included and those excluded.

70. I would class the following as members of the biometric school: J. Blakeman, E.M. Elderton, W.P. Elderton, P.F. Everitt, F. Galton, D. Heron, L. Isserlis, A. Lee, K. Pearson, E.H.J. Schuster, E.C. Snow, H.E. Soper. The 'others' are A.L. Bowley, J. Brownlee, F.Y. Edgeworth, R.A. Fisher, W.S. Gosset, M. Greenwood, R.H. Hooker, J.M. Keynes, G.J. Lidstone, A.G. McKendrick, W.F. Sheppard, C. Spearman, G.H. Thomson, G.U. Yule.

71. See his remarks in discussion of a paper by Leonard Darwin, *Journal of the Royal Statistical Society,* Vol. 82 (1919), 27-29. For Heron's career see the obituary notice by E.S. Pearson, ibid., Vol 133A (1970), 287-90.

72. That the opponents of r_T felt themselves faced with a coherent group can be seen from the following skit:

> Extracts from the *Times,* 1 April 1925.
>
> G. Udny Yule, who had been convicted of high treason on the 7th ult. was executed this morning on a scaffold outside Gower St. Station. A short but painful scene occurred on the scaffold. As the rope was being adjusted, the criminal made some observation, imperfectly heard in the press enclosure, the only audible words being 'the normal coefficient is —'. Yule was immediately siezed by the Imperial guard and gagged. The coroner's jury subsequently received evidence that death had been instantaneous. Snow was the executioner and among others present were the Sheriff, Viscount Heron of Borkham and the Hon W. Palin Elderton.
>
> Up to the time of going to press the warrant for the apprehension of Greenwood had not been executed, but the police have what they regard to be an important clue. During the usual morning service at St Paul's Cathedral, which was well attended, the carlovigian creed was, in accordance with an imperial rescript chanted by the choir. When the solemn words, 'I believe in one holy and absolute coefficient of four-fold correlation' were uttered a shabbily dressed man near the North door shouted 'balls'. Amid a scene of indescribable excitement, the vergers armed with several volumes of *Biometrika* made their way to the spot, but one of them was savagely bitten in the calf by a small mongrel and in the confusion the criminal escaped.
>
> (Greenwood to Yule, 8 November 1913. Yule archive, Royal Statistical Society, box 1.)

73. L.A. Farrall, 'The Origin and Growth of the English Eugenics Movement, 1865-1925' (unpublished PhD Thesis, Indiana University, Bloomington, 1970) gives a breakdown of the funding and personnel of the Biometric and Eugenic Laboratories.

74. See E.S. Pearson, 'Karl Pearson. An Appreciation of some Aspects of his

Life and Work', Part 1, *Biometrika,* Vol. 28 (1936), 193-257 and Part 2, ibid., Vol. 29 (1937-38), 161-248 for this (especially Part 2, 182-83).

75. Greenwood's career is discussed in L. Hogben, 'Major Greenwood, 1880-1949', *Obituary Notices of Fellows of the Royal Society,* Vol. 7 (1950-51), 139-54.

76. The letters from Greenwood to Yule in the Yule papers, Royal Statistical Society, box 1, reveal Greenwood's doubts (see, for example, Greenwood to Yule, 30 June 1913).

77. M. Greenwood, 'The Problem of Marital Infection in Pulmonary Tuberculosis', *Proceedings of the Royal Society of Medicine (Epidemiology),* Vol. 2 (1909), 259-68. Greenwood's growing criticisms can be seen in his letters to Yule, and in Greenwood and Yule, op. cit. note 24.

78. Eugenics Education Society, *Annual Reports* (1910-11, 1911-12). For Brownlee's life see M. G[reenwood], 'Obituary: John Brownlee, MD, DSc', *Journal of the Royal Statistical Society,* Vol. 90 (1927), 405-07. Brownlee even used r_T in the case of theoretical Mendelism, where the biometricians denied its applicability. See J. Brownlee, 'The Significance of the Correlation Coefficient when applied to Mendelian Distributions', *Proceedings of the Royal Society of Edinburgh,* Vol. 30 (1909-10), 473-507; E.C. Snow, 'The Application of the Correlation Coefficient to Mendelian Distributions', *Biometrika,* Vol. 8 (1911-12), 420-24.

79. Edgeworth, in his *Mathematical Psychics* (London: Kegan Paul, 1881), did use hereditarian ideas against the egalitarian environmentalism of radical utilitarianism, but, this book apart, does not seem to have shown sympathy for eugenics. I have not been able to find any writings of Hooker on eugenics.

80. See their remarks following Yule's paper 'On the Methods of Measuring Association Between Two Attributes' (op. cit. note 17), as quoted in the *Journal of the Royal Statistical Society,* Vol. 75 (1911-12), 643-44 and 646-47.

81. London: Walter Scott, 1892.

82. See above, p. 47.

83. See above, pp. 56-58.

84. B. Norton, 'Biology and Philosophy: the Methodological Foundations of Biometry', *Journal of the History of Biology,* Vol. 8 (1975), 85-93.

85. This was the account given by Yule to Greenwood, letters of 18 May and 26 May 1936 in the Yule archive, Royal Statistical Society, box 2. The Yule-Pearson correspondence bears this out, as it continues on an amicable basis up to Yule's first criticism of Pearson in 1905 and is then abruptly terminated (apart from three letters of 1910, dealing with a personal matter). This correspondence is in the Pearson papers, University College London, C1 D3 and C1 D6.

86. K. Pearson, *The Grammar of Science* (London: Adam and Charles Black, second edition, 1900), 468.

87. E.S. Pearson, op. cit. note 74, Part 2, 205, 206.

88. See B. Norton, 'Fisher and the neo-Darwinian Synthesis', paper read to International Congress of the History of Science, Edinburgh, 1977.

89. L.A. Goodman and W.H. Kruskal, 'Measures of Association for Cross Classifications', *Journal of the American Statistical Association,* Vol. 49 (1954), 763.

90. For example J.A. Davis, *Elementary Survey Analysis* (Englewood Cliffs, NJ: Prentice Hall, 1971).

91. For example N.J. Castellon, Jr, 'On the Estimation of the Tetrachoric

Correlation Coefficient', *Psychometrika,* Vol. 31 (1966), 67-73.

92. N. Poulantzas, *Classes in Contemporary Capitalism* (London: New Left Books, 1975).

93. This argument is a much condensed version of D. MacKenzie, 'Eugenics in Britain', *Social Studies of Science,* Vol. 6 (1976), 499-532.

94. Raymond Williams, *Culture and Society 1780-1950* (London: Chatto and Windus, 1958); Harold Perkin, *The Origins of Modern English Society 1780-1880* (London: Routledge and Kegan Paul, 1972), 237-52; R.A. Levitas, 'The Social Location of Ideas', *Sociological Review,* Vol. 24 (1976), 545-57; David Bloor, unpublished paper; B. Wynne, 'C.G. Barkla and the J Phenomenon — A Case Study in the Sociology of Physics' (unpublished M Phil thesis, University of Edinburgh, 1977).

95. W. Bateson, 'Common-sense in Racial Problems', reprinted in B. Bateson, *William Bateson* (Cambridge: Cambridge University Press, 1928), 375; letter to M. Pease, 28 January 1925, reprinted, ibid., 388. For Bateson's conservatism see W. Coleman, 'Bateson and Chromosomes: Conservative Thought in Science', *Centaurus,* Vol. 15 (1970), 228-314.

96. See Farrall, op. cit. note 73; MacKenzie, op. cit. note 93; G.R. Searle, *Eugenics and Politics in Britain, 1900-1914* (Leyden: Noordhoff, 1976).

97. See K. Pearson, 'Anarchy', *The Cambridge Review,* Vol. 2 (1881), 268-70, and the essays on socialism in 'The Ethic of Freethought' (London: Unwin, 1888).

98. See his review of *Fabian Essays in Socialism* in *The Academy,* Vol. 37 (1890), 197-99.

99. See the essay of that name in *The Ethic of Freethought* op. cit. note 97.

100. K. Pearson, 'Prefatory Essay: The Function of Science in the Modern State', *Encyclopedia Britannica,* 10th Edition (London: Adam and Charles Black, 1902), Vol. 8 of new volumes, xvi.

101. Ibid., x.

102. Greenwood to Raymond Pearl, 19 August 1926, Pearl Papers, American Philosophical Society, Philadelphia.

103. See Yule to Greenwood, 2 February 1936. In the possession of George B. Greenwood.

104. Yule, 'A Mathematical Theory of Evolution based on the Conclusions of Dr J.C. Willis, FRS', *Philosophical Transactions of the Royal Society, Series B,* Vol. 213 (1924-25), 21-87. For Willis' views, see his *Age and Area* (Cambridge: Cambridge University Press, 1922).

105. George Udny Yule, 'The Wind bloweth where it Listeth', *Cambridge Review,* Vol. 41 (1920), 184-86.

106. Yule, 'Critical Notice' [of Brown and Thomson's *Essentials of Mental Measurement*], *British Journal of Psychology,* General Section, Vol. 12 (1921-22), 106-07.

107. For Yule's family, see the *Dictionary of National Biography* entry for Sir Henry Yule.

108. E.S. Pearson op. cit. note 71.

109. G.U. Yule, 'Reginald Hawthorn Hooker, MA', *Journal of the Royal Statistical Society,* Vol. 107 (1944), 74-77.

110. See J.M. Keynes, 'Francis Ysidro Edgeworth, 1845-1926', *Economic Journal,* Vol. 36 (1926), 140-53, and A.L. Bowley, 'Francis Ysidro Edgeworth',

Econometrica, Vol. 2 (1934), 113-24.

111. E.J. Hobsbawm, *Labouring Men* (London: Weidenfeld and Nicolson, 1968), 250-71; H. Perkin, op. cit. note 94, 261-62.

112. On this point see also R.A. Levitas, op. cit. note 94.

113. See M.G. Kendall, *The Advanced Theory of Statistics* (London: Griffin, 1943), Vol. 1, 354-56. Note that Kendall's 'Tchebycheff-Hermite Polynomials' (ibid., 145-47) are somewhat differently defined from the Hermite polynomials commonly used in applied mathematics (e.g. G. Arfken, *Mathematical Methods for Physicists* [New York: Academic Press, 1968], 477-81).

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