Econometrics and Psychometrics: Rivers out of Biometry

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In 1902 John Pease Norton of Yale University wrote the following in the preface to his study of the New York money market: “Although [Karl] Pearson’s writings have been largely in connection with the biological problems of evolution, his statistical methods have been found to apply satisfactorily to the problems presented under the theory of money and credit.” In 1904 British psychologist Charles Edward Spearman advocated a “correlational psychology” in which Pearson’s formula for correlation had a central place.

The period 1895–1925 saw the origins and establishment of the fields that came to be called econometrics and psychometrics. I consider what these fields owed to biometry—the statistical approach to the biological problems of evolution—and make some comparisons between all three. I emphasize developments in biology and psychology, for these are less familiar to historians of econometrics. These developments are interesting to contemplate, for the biometricians and psychometricians were already discussing issues associated with the respective roles of statistical analysis and of subject matter theory, issues that became prominent in econometrics only much later.

The early psychometricians and econometricians learned correlation from Pearson 1896, a technical paper written for the Royal Society, or

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from the more general account in the second edition of his philosophy of science book, *The Grammar of Science* (1900a). The paper showed how by the use of the multivariate normal distribution and correlation "such subjects as inheritance, regression, assortative mating and panmixia, are capable of perfectly direct quantitative treatment." Pearson's (1896, 318) belief was that such a treatment "alone can settle the chief problems of evolution." Pearson's project was studiedly descriptive, but the "rediscovery of Mendel" in 1900 posed the question of whether the biometricians' correlations had a structure grounded in Mendel's laws. Running in parallel to this debate was one in psychology about whether correlations between test scores reflected a particular structure in human abilities. At the time the parallel was not perceived, nor, of course, was the term *structure* used—it was not popularized until the 1940s by the Cowles econometricians who saw affinities between their work and that of the biometricians and the psychometricians.

1. Describing Inheritance

"Regression, heredity and panmixia" was the third of Pearson's "Mathematical Contributions to the Theory of Evolution"—a series that ran to nineteen parts. Karl Pearson (1857–1936) was professor of applied mathematics and mechanics at University College London, and he had been collaborating with the zoologist Raphael Weldon (1860–1906) since 1891.[1]

The 1896 regression paper was Pearson's attempt to summarize and extend Francis Galton's work on regression and correlation. *Natural Inheritance* (1889) represented the latest word of Galton (1822–1911), but his ambition to create a quantitative science of heredity was first evident in his work on humankind, *Hereditary Genius* (1869).[2] An early contribution, "Typical Laws of Heredity" (Galton 1877), formalized the notion of "reversion" as a stable first-order autoregressive process with a generation as the unit of "time." Reversion did not travel beyond the study of heredity, although Galton ([1877] 1977, 202) found William Stanley Jevons a parallel in political economy in "the successive stages by which overproduction of any commodity reverts to one of normal production." At first sight, it is

1. For a sketch of Pearson's life and a guide to the literature, see Aldrich [2003] 2010, and, for a thorough account of his very broad apprenticeship in physics, history, and literature, Porter 2004.

2. Galton's work has been discussed from the viewpoints of the history of statistics by Stephen M. Stigler (1986, pt. 3) and of the history of biology by Michael Bulmer (2003).

surprising that an evolution project should yield no methods for analyzing time series, but the typical data set for Galton and Pearson consisted of observations on a cross-section of relatives.

A decade after reversion (regression) came co-relation (correlation), which Galton (1888, 135) introduced thus:

> Two variable organs are said to be co-related when the variation of the one is accompanied on the average by more or less variation of the other, and in the same direction. . . . It is easy to see that co-relation must be the consequence of the variations of the two organs being partly due to common causes. If they were wholly due to common causes, the co-relation would be perfect, as is approximately the case with the symmetrically disposed parts of the body.

Galton soon drew others into correlation: Raphael Weldon (1893) correlated measurements on crustaceans, and the political economist F. Y. Edgeworth (1845–1926) generalized the theory to more than two variables.[3]

The variations considered in "Regression, heredity and panmixia" "obey the normal law of frequency." Pearson (1896, 261–63) considers a "complex" of measurable characteristics, $\eta_1, \eta_2, \ldots, \eta_{\mathrm{n}}$ for which the "law of frequency" is

$$P = Ce^{-\frac{1}{2}\chi^2}\delta\eta_1\delta\eta_2\cdot\ldots\delta\eta_{\mathrm{n}},$$

where χ^2 is a quadratic function of the η's. Pearson does not postulate the multivariate normal directly but derives it on the assumption that the observable η's depend on "an indefinite number of quite inappreciable and unascertainable contributory causes":

$$\eta_1 = \alpha_{11}\varepsilon_1 + \alpha_{12}\varepsilon_2 + \alpha_{13}\varepsilon_3 + \ldots + \alpha_{1m}\varepsilon_m$$

$$\eta_2 = \alpha_{21}\varepsilon_1 + \alpha_{22}\varepsilon_2 + \alpha_{23}\varepsilon_3 + \ldots + \alpha_{2m}\varepsilon_m$$

$$\ldots$$

$$\eta_n = \alpha_{n1}\varepsilon_1 + \alpha_{n2}\varepsilon_2 + \alpha_{n3}\varepsilon_3 + \ldots + \alpha_{nm}\varepsilon_m,$$

where the ε's are independent normal variables. Pearson's use of an indefinite number of contributory causes to establish the multivariate normal resembles the use of "elementary errors" to establish the univariate normal in the theory of errors, except that in the latter case a central limit theorem is applied to nonnormal quantities. Some of Pearson's mathematics

3. For details, see Stigler 1986, chaps. 9, 10.

came from a branch of error theory devised for positions in space, and Pearson referred to Bravais 1846.

Pearson (1896, 262) thought of the contributory causes as "magnitudes of other organs not in the complex, variations in environment, climate, nourishment, physical training, various ancestral influences and innumerable other causes which cannot be individually observed or their effects measured." Behind Pearson's treatment is Galton's idea that there is correlation between the observable η's because some of the ε's are common to parent and offspring, but Pearson does not work out the algebra relating the parameters of the η distribution to the α's and does not suggest that statistical analysis of the observables could make the magnitudes of the contributory causes or their efficacy accessible.

Pearson (1896, 292) actually made very limited claims for his analysis: "[The] formulae . . . make not the least pretence to explain the mechanism of inheritance. All they attempt is to provide a basis for the quantitative measure of inheritance—a schedule, as it were, for tabulating and appreciating statistics." In the terminology of Tjalling Koopmans and Olav Reiersøl (1950), Pearson was concerned with "population" characteristics, not "structural" ones.

Pearson's (1896, 259) population analysis rested on definitions like this: "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 parent and offspring." Thus biological concepts are expressed in terms of the parameters of the η distribution: the simplest case (268) is uniparental inheritance where, if the parent deviates by h from the population mean and x is the corresponding value in the offspring, the joint distribution is

$$z = \frac{N}{2\pi\sigma_1\sigma_2\sqrt{1-r^2}}\, e^{-\frac{1}{2}\left\{\frac{x^2}{\sigma_1^2(1-r^2)} - \frac{2rxh}{\sigma_1\sigma_2(1-r^2)} + \frac{h^2}{\sigma_2^2(1-r^2)}\right\}}.$$

The offspring have "variation following a normal distribution about the mean

$$x_0 = r\,\frac{\sigma_1}{\sigma_2}\,h$$

and . . . by our definition, the coefficient of regression $= x_0/h = r\sigma_1 = \sigma_2$."

The η/ε analysis was not adopted by other writers or developed by Pearson himself; there is a distant echo in Arthur L. Bowley's ([1901] 1920, 355–56) interpretation of correlation as the proportion of causes in common in the genesis of two variables. Pearson's paper settled the estimation issue for correlation by introducing the product-moment formula. This was generally adopted, as were the formulas for large-sample standard errors that Pearson developed. Pearson's justification of the "most probable" value was unclear and changed over time—see Aldrich 2008—but the formulas went unaltered into the era of maximum likelihood inaugurated by Ronald Aylmer Fisher (1922a). Fisher (1890–1962) completed the inference wing of the correlation edifice when he derived the exact distribution of the correlation coefficient in 1915, of the partial correlation coefficient in 1921, and of the multiple correlation in 1928. During the 1920s he replaced Pearson as leader of the biometric school and of English mathematical statistics.[4] (Fisher reappears in section 3.)

Pearson (1896) not only provided a theoretical scheme for tabulating the statistics of inheritance but, in works like Pearson and Lee 1903, presented the actual "laws of inheritance"—correlations between physical characters based on a sample of a thousand or so relatives. One biometric law of special importance was the "ancestral law"—the multiple regression expression of the idea that the character of the offspring can be calculated with more exactness, the more extensive our knowledge of the corresponding characters of the ancestry.

Pearson's formulas soon appeared in textbooks: Bowley ([1901] 1920) presented them to statisticians, that is, to economic and vital statisticians; Charles Davenport (1904) to biologists; W. Palin Elderton (1907) to actuaries; Charles Samuel Myers (1909) to psychologists; and G. Udny Yule (1911) to anybody. Generally the textbooks confined themselves to the level of describing populations, although Yule discussed breakdowns of interpretation, "illusory" correlations (their story is told in Aldrich 1995). None of the textbooks went into structural interpretations of correlations, although these soon became matters of intense debate in the journal literature of genetics and psychology (see sections 3 and 5).

2. Regression, Catenas of Causation, and Economics

While Pearson was writing about correlation and regression in biology, his student/collaborator/colleague, G. Udny Yule (1871–1951), was publishing in economics. Stephen M. Stigler (1986, chap. 10) describes the work, and

4. See the biography by Joan Box (1978) and the annotated bibliography by Aldrich ([2003] 2010).

John Aldrich (2010a, 2010b) examines his relationship with the economists. Yule was interested in biometry, economics, and psychology and, before becoming a very wide-ranging statistician, had a year as a student of Heinrich Hertz.[5]

Yule developed the Pearson scheme in three respects—by introducing the concept of partial correlation, by relaxing the multinormal assumption for the joint distribution of the observables, and by devising new causal interpretations. Pearson was very conscious that his scheme was restricted to jointly normal variables—his previous mathematical contribution (1895) was on skew curves, and he projected a scheme for analyzing skew correlation—but he never produced a satisfactory theory, and indeed continuous multivariate analysis still largely rests on the multinormal distribution. Stigler (1986, 345–58) describes how Yule got around the skew correlation impasse by using results on least squares. Yule's regression analysis proved a temporary compromise between Pearson and Gauss, but Fisher's fixed x regression analysis of the 1920s in which Gauss was extended to include t-tests and F-tests has endured.[6]

Yule's thinking about the interpretation of correlations can be seen evolving in his three treatments (1895, 1896, 1899) of the relationship between the incidence of pauperism in the unions (districts) of England and Wales and the way the Poor Law was administered in them. From the beginning Yule was interested in the possibility that the positive correlation reflected a causal connection, but he never used Pearson's scheme of dependence on common causes to rationalize it. At first he was doubtful:

> [The statement of a positive correlation] does not say either that the low mean proportion of out-relief is the cause of the lesser mean pauperism or vice versa: such terms seem best avoided where one is not dealing with a catena of causation at all. To use a simile, due I believe to Professor Marshall, the case is like that of a lot of balls—say half a dozen—resting in a bowl. Then you cannot say that the position of ball No. 3 is the cause of the position of No. 5 or the reverse. But the position of 3 is a function of the positions of all the others including 5; and the position of 5 is a function of the positions of all the others including 3: hence variations in the positions of the two balls will be correlated, and it is to this term that I prefer to adhere. To be quite clear, I do not mean simply that out-relief determines pauperism in one union and pauperism out-

5. His life is recalled in Kendall 1952, which has a bibliography.
6. The development is described in Aldrich 2005.

> relief in another, so that you cannot say which is which in the average: but I mean that out-relief and pauperism mutually react in one and the same union. (1895, 605 n. 2)

Alfred Marshall (1890, 534) had criticized Jevons for insisting on a "catena" of causation, instead of representing "supply price, demand price and amount produced as mutually determining one another." The simplest representation of mutual reaction is the simultaneous equations model

$$\eta_1 = \beta_{12}\eta_2 + \varepsilon_1$$
$$\eta_2 = \beta_{21}\eta_1 + \varepsilon_2$$

(cf. Haavelmo 1943, 2), but Yule did not develop such a model. He dropped the mutual reaction idea to consider whether the correlation might express the influence of a third variable, an observable common cause: "My two notes have shown distinctly that there is a connection, but do not show whether it is direct, or whether, e.g., I must simply attribute the result, that pauperism is positively correlated with out-relief, to the fact that pauperism and out-relief are both positively correlated with poverty" (Yule 1896, 620). To decide this question he introduced and used "net coefficients" (partial correlations).

Yule's (1899, 251) final piece on pauperism was a large multiple regression exercise that concentrated on the issue of direct versus indirect influence and attempted to answer the following questions:

1. Taking each of the two decades 1871–81, 1881–91, to find by discussion of the changes in all the unions of the country, whether the changes in administration had a direct influence on the changes in pauperism, and, if so, to what extent.
2. If the changes in administration had such a direct influence, to find what proportion of the total change in each decade might be ascribed to changes in administration, and what proportion to other changes.

Yule used the regression equation to decide the matter of direct influence as follows: "Any double interpretation is now—very largely at all events—excluded. It cannot be argued that the changes in pauperism and out-relief are both due to the changes in age distribution, for that has been separately allowed for in the third term on the right; $b \times$ (change in proportion of out-relief) gives the change due to this factor when all the others are kept constant."[7]

7. For an account of what was "in its way a masterpiece," see Stigler 1986, 355.

Correlation came to economics, but in the work of Bowley ([1901] 1920), Norton (1902), Persons (1908), Irving Fisher (1911), and Moore (1911) as bivariate analysis. Multiple regression is treated in the 1920 edition of Bowley's book, and it was adopted by American agricultural economists, the most energetic econometricians of the time. Sometimes the correlation signified a direct causal connection—as in Bowley's ([1901] 1920, 320) investigation of the correlation between the marriage rate and prosperity—and sometimes it simply measured covariation. Economists added nothing fundamental to the concepts of Pearson and Yule. An American zoologist, Sewall Wright, offered a new conceptual scheme, but it was not taken up (see section 4).

3. Mendel Rediscovered

In the 1860s when Galton was investigating genius in humans, Gregor Mendel (1822–1884) was propagating peas. When Mendel's results were "rediscovered" in 1900, the biometricians reacted in two ways—by investigating the relationship between Mendelian principles and biometrically established facts and by producing statistical techniques that could be used in Mendelian research. The reconciliation of Mendelism and biometry is one of the most famous passages in the history of biology—see, for example, Provine 1971, Bowler 1989, and Gayon 1998—but the development of statistical technique in genetics is less well known.

In England there was a collision between Mendel's champion, William Bateson (1861–1926), and Pearson and Weldon. Bateson had a low opinion of biometric work and did not undertake any detailed investigation of the relationship between Mendel's findings and the patterns obtained by the biometricians. The task was attempted by people—Pearson, Yule, and R. A. Fisher—who accepted the biometric results. At first sight, Mendelian principles seemed incompatible with the established facts about ancestry, or so Weldon (1902, 252) thought: "The fundamental mistake which vitiates all work based upon Mendel's method is the neglect of ancestry, and the attempt to regard the whole effect upon offspring, produced by a particular parent, as due to the existence in the parent of particular structural characters." Bateson (1902, 114) actually agreed—Mendel's laws are "absolutely at variance with all the laws of ancestral heredity however formulated."

When he reviewed Bateson's book Yule (1902) took great exception to the polemics, but he went on to produce the first examination of the consequences for population outcomes of Mendelian principles. He concluded by insisting, "It is, however, essential, if progress is to be made, that biologists—statistical or otherwise—should recognise that Mendel's Laws and the Law of Ancestral Heredity are not necessarily contradictory statements, one or other of which must be mythical in character, but are perfectly consistent the one with the other and may quite well form parts of one homogeneous theory of heredity" (236). Yule looked for an accommodation between the two systems of ideas. Pearson (1904) responded to Bateson with his own essay in theoretical population genetics. Pearson had insisted that the formulas developed in his big correlation paper were descriptive and not explanatory (see section 1), but, as he now explained, they could still have a bearing on explanatory theories:

> [The] biometric or statistical theory of heredity does not involve a denial of any physiological theory of heredity, but it serves in itself to confirm or refute such a theory. Mendelian formulae analytically developed for randomly mating populations are either consistent or not with the biometric observations on such populations. If they are consistent, it shows their possibility, but does not prove their necessity. If they are not, it shows they are inadequate. (85)

On examination the gross incompatibility that Weldon and Bateson saw disappeared, yet still Pearson's verdict was against Mendelian theory:

> The present investigation shows that in the theory of the pure gamete there is nothing in essential opposition to the broad features of linear regression, skew distribution, the geometric law of ancestral correlation, etc., of the biometric description of inheritance in populations. But it does show that the generalised theory here dealt with is not elastic enough to account for the numerical values of the constants of heredity hitherto observed. (85)

For Pearson the triumphant later developments involved investing the theory with enough elasticity for it to account for the observed numerical values—it was as though he were in the presence of a "degenerating research programme."[8]

Yule (1907) responded to the results in Pearson (1904) by arguing that the theory Pearson tested was unduly specialized. Yule (1907, 141) saw

8. Deeper explanations for Pearson's resistance are considered by Margaret Morrison (2002), and James Tabery (2004) considers Yule's position in more detail.

no difficulty in accounting for the troublesome correlation values: a value of 0.5 "probably indicates an absence of the somatic phenomenon of dominance. In the case of characters like stature, span, &c. in man this does not seem very improbable." There was a respect in which Yule's 1907 note widened the scope of the discussion: "A complete theory of heredity should take into account, besides germinal processes, the effect of the environment in modifying the soma obtained from any given type of germ-cell—an effect which is hardly likely to be negligible in the case of such a character as stature. This may be done without much difficulty for the limited case discussed." There were no environmental variables, for Yule treated the effect of the environment as an unobservable. From his analysis he concluded that "the common ratio of the ancestral coefficient remains, however, unaltered at its former value of ½. So far as the coefficients of correlation are concerned, it is accordingly impossible to distinguish between the effect of the heterozygote giving rise to forms that are not strictly intermediate, and the effect of the environment in causing somatic variations which are not heritable" (141).

A paper by Ronald Fisher brought the discussion to an end. "The Correlation between Relatives on the Supposition of Mendelian Inheritance" (1918b) was written in 1915, but already as an undergraduate Fisher (1911) had his own synthesis of Bateson and Pearson. "The Correlation between Relatives" derives a whole set of correlations between offspring, parents, and ancestry and compares them with those published by Pearson and Lee (1903), concluding, "In general, the hypothesis of cumulative Mendelian factors seems to fit the facts very accurately" (Fisher 1918b, 433). It was as though the η/ε analysis of Pearson 1896 had been realized with the ε's as binary Mendelian factors and by applying the central limit theorem. Fisher's paper is much more ambitious and much more difficult than the earlier work by Pearson and Yule.[9]

Fisher (1918b, 399) introduced the term *variance*, explaining that he preferred to work with this quantity rather than with the standard deviation because variances may be added, and "we may ascribe to the constituent causes fractions, or percentages, of the total variance which they together produce." Fisher's (1918a) nontechnical account for the *Eugenics Review* had the less hypothetical title "The Causes of Human Variability." There he explains what is meant by a "cause of variability": "In a popu-

9. William Provine (1971, 143–47) gives an overview, and Pat Moran and Cedric Smith (1966) a detailed commentary.

lation absolutely uniform in regard to other causes, such as breeding and exercise, existing differences of nutrition would produce a certain variability—in fact that a certain percentage of the variance must be ascribed to nutrition" (214).

In the main paper Fisher (1918b, 401) writes that

> [Yule] shows the similarity of the effects of dominance and of environment in reducing the correlations between relatives, but states that they are identical, an assertion to which, as I shall show, there is a remarkable exception, which enables us, as far as existing statistics allow, to separate them and to estimate how much of the total variance is due to dominance and how much to arbitrary outside causes.

Fisher presents his estimates of the relative importance of dominance and environment in height using the Pearson and Lee data, concluding, "These determinations are subject to considerable errors of random sampling, but our figures are sufficient to show that . . . it is very unlikely that as much as 5 per cent. of the total variance is due to causes not heritable, especially as every irregularity of inheritance would, in the above analysis, appear as such a cause" (423–24).

Fisher 1918b was refereed for the Royal Society by Pearson and the prominent Mendelian Reginald Punnett (1875–1967), and they were unenthusiastic. Pearson thought that the decision to publish "should depend on Mendelian opinion as to the correspondence of the author's hypotheses with observation, and the probability that Mendelians will accept in the near future a multiplicity of independent units not exhibiting dominance or coupling," while for Punnett, the exercise was "too much of the order of problem that deals with weightless elephants upon frictionless surfaces, where at the same time we are largely ignorant of the other properties of the said elephants and surfaces" (Norton and Pearson 1976, 154, 155). Fisher, a schoolmaster in 1918, had the satisfaction of succeeding Pearson as professor of eugenics at University College London in 1932 and Punnett as Arthur Balfour Professor of Genetics at Cambridge in 1943.

4. Statistical Methods in Genetics

For different reasons Pearson, Yule, and Fisher worked to derive the correlations associated with Mendelian models of reproducing populations. In their theoretical exercises, observed correlations were treated as known without error, and so there was no statistical inference in the technical sense. Other meetings of biometry and Mendelism did involve statistical inference. One stream runs from Weldon's (1902, 235) application of Pearson's (1900b) X^2 goodness of fit test to Mendel's results on alternative inheritance in peas. Weldon wrote as a critic, but biometricians more sympathetic to Mendelism adapted their methods to suit its requirements; the American botanist and *Biometrika* author J. Arthur Harris (1912) applied the goodness of fit test to Mendelian ratios associated with the phenomenon of coupling of, or linkage between, genes, and structural estimation soon followed. Frank Engledow and Yule ([1914] 1997) considered a structured multinomial population where the four theoretical probabilities (corresponding to two forms of two characters) are given by

AB	Ab	aB	ab
$p^2 + 0.5$	$0.25 - p^2$	$0.25 - p^2$	p^2,

where the three independent probabilities depend on a single parameter p measuring the degree of coupling. To estimate p, Engledow and Yule ([1914] 1997, 42) improvised a method later called minimum X^2: this was an ad hoc solution to a specific problem, but when R. A. Fisher (1922b) applied maximum likelihood to the same problem he had the general theory of Fisher 1922a behind him. Fisher used the estimation of linkage to illustrate the method of maximum likelihood in his manual, *Statistical Methods for Research Workers* (1925). The scheme is more elaborate than that of Engledow and Yule, for there are two parents and possibly different degrees of linkage. In Engledow and Yule the parameter p enters in the form of p^2, but in Fisher (1925, 24) p^2 is replaced by the product of the parental terms; in the second edition Fisher (1928, 241) explained that the male and female parameters are not separately identified—as the saying would later go.[10]

There was also a stream of correlation analysis informed by genetic theory to which the American zoologist Sewall Wright (1889–1988) was a major contributor. Wright had no connection with the English biometric community and was unaware of the Pearson-Yule-Fisher discussion, but he had a basic knowledge of correlation and could see how the technique could be applied in Mendelian research. Wright developed a scheme resembling Spearman's factor analysis (see below) in his "On the Nature of Size Factors" (1918), and his "Relative Importance of Heredity and Environment in Determining the Piebald Pattern of Guinea-Pigs" (1920)

10. The history of the estimation of linkage is treated by A. W. F. Edwards (1996, 2005).

introduced the method of path coefficients. There was some overlap between his 1920 paper and Fisher's "Correlation between Relatives"—Ching Chun Li (1968) also brings out the similarity in technique—but, while Fisher did not extract a general approach, Wright did and, beginning with his *Corn and Hog Correlations* (1925), tried to persuade economists to adopt it.[11]

The collision—or confluence—of biometry and Mendelism gave rise to two forms of structural inference: studies in theoretical population genetics that took the facts as described by the parameter values of a multinormal distribution as given and asked whether a Mendelian interpretation could be given; Mendelian research in which parameters based on overidentified multinomial distributions or—in the case of path analysis—exactly identified multinormals. There was parallel activity in psychology, although there the overidentified distributions were multinormal and not multinomial. Structural estimation with overidentified multinormals came to econometrics to settle only with Trygve Haavelmo (1943).

5. Factors and Their Reality

Charles Edward Spearman (1863–1945) was a career soldier who turned to psychology in his midthirties. The study "The Abilities of Man" (the title of his 1927 treatise) was his lifework, and his "General Intelligence" of 1904 was the first draft. In 1907 Spearman joined the University College London department of philosophy and psychology to become a colleague of sorts to Karl Pearson; in the beginning Yule was also present as Newmarch Lecturer in statistics.[12] Spearman appears to have taught himself statistics, and his two papers (1904a, 1904b) demonstrate a good command of current correlation techniques. Pearson was never satisfied with Spearman's work, but Yule (1911, 209) was impressed enough to include the results of the analysis of the effects of measurement error on correlation (Spearman 1904b) in the *Introduction to the Theory of Statistics*. The idea of latent variables measured with error underpinned Spearman's analysis of "general intelligence." David Bartholomew (1995, 212) gives this assessment of Spearman's factor analysis:

11. For Wright's general contribution to biology, see Provine 1986, and, for path analysis specifically, Wolfe 1999 and Steffes 2007; for his campaign in econometrics, see Goldberger 1972 and Stock and Trebbi 2003.

12. For a biography of Spearman, see Lovie and Lovie 1996 and, for broader perspectives, Cowles 2001 and Boring 1950.

Table 1 Spearman's Table of Correlations

	Classics	French	English	Mathematics	Discrimination	Musical Talent
Classics		0.83	0.78	0.70	0.66	0.63
French	0.83		0.67	0.67	0.65	0.57
English	0.78	0.67		0.64	0.54	0.51
Mathematics	0.70	0.67	0.64		0.45	0.51
Discrimination	0.66	0.65	0.54	0.45		0.40
Musical talent	0.63	0.57	0.51	0.51	0.40	

Spearman's great contribution lies in his recognition that one could introduce latent variables into scientific discourse through their relationship with manifest variables. The technical apparatus which he introduces to do this seems, in retrospect, crude and limited and his concern with a particular substantive issue made it difficult for him to see the full potential of what he had done.

"General Intelligence" (Spearman 1904a) is a long paper that includes a wide-ranging survey of previous attempts to characterize intelligence, but, while the drift of the statistical argument is clear, the details are obscure; Douglas Vincent (1954) attempts to reconstruct them. Spearman (1904a, 275) wished to understand what was behind a correlation table such as that in table 1, based on scores from tests of mental abilities. With few exceptions each correlation is greater than any to the right of it in the same row, or below it in the same column.

Spearman perceived a structure consisting of a general factor underlying all the skills and factors specific to the skills; the hierarchy reflected the degree to which the tests measured this general factor. Thus he drew these conclusions:

> IV. The above and other analogous observed facts indicate that *all branches of intellectual activity have in common one fundamental function (or group of functions), whereas the remaining or specific elements of the activity seem in every case to be wholly different from that in all the others*. The relative influence of the general to the specific function varies in the ten departments here investigated from 15:1 to 1:4.
>
> V. As an important practical consequence of this universal Unity of the Intellectual Function, the various actual forms of mental activity

> constitute a stably interconnected Hierarchy according to their different degrees of intellective saturation. (284)

Spearman did not present any algebra, but the modern way to present his scheme can be achieved by specializing the η/ε analysis of Pearson (1896)—see section 1—and expressing the performance on the *i*-th test by

$$\eta_i = \alpha_i \varepsilon_0 + \varepsilon_i.$$

Although Spearman (1904a, 225) gave Pearson (1896) as his reference for the product-moment formula, he did not pick up the η/ε analysis. Conceptually, of course, Pearson and Spearman were worlds apart, for Pearson held the ε's to be "unascertainable" and indefinite in number.

The variances and covariances (as R. A. Fisher would have called them) are given by

$$\mathrm{var}(\eta_i) = \alpha_i \sigma_0^2 + \sigma_i^2$$

$$\mathrm{cov}(\eta_i, \eta_j) = \alpha_i \alpha_j + \sigma_0^2.$$

Spearman expressed the implied restrictions in terms of correlations: for any four scores, the "tetrad difference" $r_{ik} r_{jl} - r_{il} r_{jk}$ is equal to 0; a condition Spearman and Hart (1912, 58) derived from the absence of correlation between scores when the general factor ε_0 is partialled out. The scheme and its extensions gave plenty of scope for devising estimation and testing routines.

Spearman's scheme could be challenged by finding data that exhibited no hierarchy, but Godfrey Hilton Thomson had an objection more theoretical than empirical. Thomson (1881–1955) was the second figure in English psychometrics. He originally studied mathematics and physics, and he taught himself statistics so well that Pearson offered him a job and published three of his papers in *Biometrika*. In a string of articles published in 1916–19, Thomson emphasized that the Spearman structure is not the only one that would generate such a hierarchy in the correlations: Spearman's statistical results were no "proof" of the "existence of general ability" (Thomson 1919), as it is possible to have a "hierarchy without a general factor" (Thomson 1916). With the benefit of the concepts and formalism introduced in the 1940s, the task may now seem an easy exercise in matrix algebra, but Thomson made the point with Monte Carlo experiments using dice.[13]

13. David Bartholomew, Ian Deary, and Martin Lawn (2009) have a full account of Thomson's career.

Pearson and Yule kept an eye on what others did with correlation; Fisher (1920–21 and 1923–24) also monitored activity in psychology. Reviewing a book by Thomson, Yule (1921, 105) gave his verdict on the Thomson-Spearman controversy: "The difficulty is that there may be more than one explanation. From the statistical standpoint Dr Spearman's explanation seems to me by far the simplest, but the judgement as to its validity must be based on other grounds." Pearson reviewed Spearman's *Abilities of Man*, and he was not impressed by the "new Copernican theory"—"It may possibly turn out to be true, but the proof will have to be more rigid than anything provided so far in 'The Abilities of Man'" (1927, 183). The chief deficiencies were in the sampling theory of the tetrad differences, and Pearson and Margaret Moul (1929) set out to repair them.

The econometricians do not appear to have noticed the psychometricians, although one economic project had a favor of factor analysis, the construction of an index of general business conditions, and much later psychological factor analysis could serve as a reference point for the subject of business barometers.[14] The project had roots in economics, in particular in index numbers, but there was a new idea—series belong together if they move together. The architect of the project, Warren Persons, had been an early proponent of correlation, and correlation figured in the new project. In his big inaugural paper, Persons (1919, 132) alluded to the η/ε analysis of Pearson 1896 but rejected it because the data sets of Galton and Pearson are too unlike the time series considered by economists. In his analysis Persons tried to accommodate the time-series patterns of movement and comovements and, unlike Spearman, did not come close to formalizing what he was doing.[15] Economists, unlike psychologists and biometricians, put great efforts into developing time-series correlation techniques.[16]

6. Futures: Divergences and Convergences

Biometry was not the only source of statistical economics or statistical psychology: in psychology there was the psychophysics of Wilhelm Fechner, and in economics there was work on business cycles and index numbers.[17] Indeed, the most discussed piece of statistical economics of the

14. See, for example, Rhodes 1937.

15. For an account of the project, see Morgan 1990, 56–63.

16. See Klein 1997.

17. On Fechner, see Stigler 1986, chap. 7; on business cycles and index numbers, see Morgan 1990 and Aldrich 1992.

period 1895–1925—Vilfredo Pareto's (1896) income distribution law—owed nothing to biometry. The rise of biometry had an effect, however, and a number of economists—Bowley, Norton, Moore, Persons, and Irving Fisher among them—adopted its methods, and a number of psychologists did the same. The economists did not have the same grasp of the theory of correlation and ability to reimagine the concept as the psychologists Spearman and Thomson. However, neither psychometrics nor econometrics stood comparison with biometry. The statistical scene was dominated by Pearson—and then R. A. Fisher—and their statistical work was so much a product of their interest in heredity and evolution that psychometrics and econometrics were completely overshadowed.

From the work of Pearson and Yule—and after 1925 from Fisher—a bundle of statistical ideas was extracted and circulated. This had no room for structural reasoning involving genes and factors that belonged to the substantive disciplines and was not easily accessible to outsiders. Factor analysis had a high barrier to entry, but it was low compared with that for biometry. There appear to have been few exchanges between biology, economics, and psychology: nearly all of the exchanges were with the statistical hub so that only people there—Pearson, Yule, Fisher, and later Jerzy Neyman—saw what was going on in more than one field.

The arrival on the scene of Ragnar Frisch, Harold Hotelling, and Henry Schultz around 1925 raised the level of statistical sophistication in economics.[18] By the 1930s econometrics matched psychometrics in statistical sophistication, and in Frisch's confluence analysis it had something as talismanic as factor analysis. There was still a lag behind biometry: Fisher used maximum likelihood to estimate linkage in 1922, but it arrived in econometrics and psychometrics only much later—in the work of Koopmans (1937) and Derek Lawley (1940).

In the 1930s the University of Chicago replaced University College London as the center for applied correlation, with Thurstone in psychology, Schultz in economics, and Wright in genetics. In the 1940s the Cowles Commission brought together identification in the simultaneous equations model and in factor analysis,[19] and there was nothing surprising in Theodore Anderson and Herman Rubin writing on both limited information maximum likelihood (1949) and inference in factor models (1956). Factor analysis and stochastic equations both appear in the concluding further topics chapter of Anderson's *Multivariate Statistical Analysis*

18. See Aldrich 2010a for references.
19. See Koopmans and Reiersøl 1950.

(1958). Arthur Goldberger looked out from econometrics to psychometrics (1971) and looked at path analysis (1972), in response to the interest sociologists were showing in the topic.[20]

In the long run the metaphor of rivers out of biometry lost any validity. In the English statistics of Pearson and R. A. Fisher (roughly 1890–1940) biometry was the central discipline, but in American mathematical statistics (after 1940) in the age of Samuel Wilks, Hotelling, Neyman, and Abraham Wald, biometry was just another applied field.[21]

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20. See Blalock 1961 and Duncan 1966.

21. There are references in Aldrich 2010a.

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