ABSTRACT

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KARL PEARSON: EVOLUTIONARY BIOLOGY AND THE EMERGENCE OF A MODERN THEORY OF STATISTICS (1884-1936)

This thesis examines the development of modern statistical theory and its emergence as a highly specialised mathematical discipline at the end of the nineteenth century. The statistical work of the mathematician and statistician Karl Pearson (1857-1936), who almost singularly created the modern theory of statistics, is the focus of the thesis. The impact of the statistical and experimental work of the Darwinian zoologist W.F.R. Weldon (1860-1906), on the emergence and construction of Pearsonian statistical innovation, is central to the arguments developed in this thesis. Contributions to the Pearsonian corpus from such statisticians as Francis Ysidro Edgeworth (1845-1926), Francis Galton (1822-1911), and George Udny Yule (1871-1951) are also addressed.

The scope of the thesis does not involve a detailed account of every technical contribution that Pearson made to statistics. Instead, it provides a unifying assessment of Pearson's most seminal and innovative contributions to modern statistical theory devised in the Biometric School, at University College London, from 1892 to 1903. An assessment of Pearson's statistical contributions also entails a comprehensive examination of the two separate methodologies he developed in the Drapers' Biometric Laboratory (from 1903 to 1933) and in the Galton Eugenics Laboratory (from 1907 to 1933).

This thesis arises, in part, from a desire to reassess the state of the historiography of Pearsonian statistics over the course of the last half century. Some of the earliest work on Pearson came from his former students who emphasised his achievements as

a statistician—usually from the perspective of the state of the discipline in their time. The conventional view has presumed that Pearson's relationship with Galton and thus to Galton's work on *simple correlation*, *simple regression*, inheritance and eugenics provided the impetus to Pearson's own statistical work. This approach, which focuses on a part of Pearson's statistical work, has provided minimal insight into the complexity of the totality of Pearsonian statistics.

Another approach, derived from the sociology of knowledge in the 1970s, espoused this conventional view and linked Pearson's statistical work to eugenics by placing his work in a wider context of social and political ideologies. This has usually entailed frequent recourse to Pearson's social and political views *vis-à-vis* his popular writings on eugenics. This approach, whilst indicating the political and social dimensions of science, has produced a rather mono-causal or uni-dimensional view of history. The crucial question of the relation between his technical contributions and his ideology in the construction of his statistical methods has not yet been adequately considered.

This thesis argues that the impetus to Pearson's earliest statistical work was given by his efforts to tackle the problems of asymmetrical biological distributions (arising from Weldon's dimorphic distribution of the female shore crab in the Bay of Naples). Furthermore, it argues that the fundamental developments and construction of Pearsonian statistics arose from the Darwinian biological concepts at the centre of Weldon's statistical and experimental work on marine organisms in Naples and in Plymouth. Charles Darwin's recognition that species comprised different sets of 'statistical' populations (rather than consisting of 'types' or 'essences') led to a reconceptualisation of statistical populations by Pearson and Weldon which, in turn, led to their attempts to find a statistical resolution of the pre-Darwinian Aristotelian essentialistic concept of species. Pearson's statistical developments thus involved a greater consideration of speciation and of Darwin's theory of natural selection than hitherto considered. This has, therefore, entailed a reconstruction of the totality of Pearsonian statistics to identify the mathematical and biological developments that

underpinned his work and to determine other sources of influence in this development.

Pearson's writings are voluminous: as principal author he published more than 540 papers and books of which 361 are statistical. The other publications include 67 literary and historical writings, 49 eugenics publications, 36 pure mathematics and physics papers and 27 reports on university matters. He also published at least 111 letters, notes and book reviews. His collected papers and letters at University College London consist of 235 boxes of family papers, scientific manuscripts and 14,000 letters. One of the most extensive sets of letters in the collection are those of W.F.R. Weldon and his wife, Florence Joy Weldon, which consists of nearly 1,000 pieces of correspondence. No published work on Pearson to date has properly utilised the correspondence between Pearson and the Weldons. Particular emphasis has been given to this collection as these letters indicate (in tandem with Pearson's Gresham lectures and the seminal statistical published papers) that Pearson's earliest statistical work started in 1892 (rather than 1895-1896) and that Weldon's influence and work during these years was decisive in the development and advancement of Pearsonian statistics.

The approach adopted in this thesis is essentially that of an intellectual biography which is thematic and is broadly chronological. This approach has been adopted to make greater use of primary sources in an attempt to provide a more historically sensitive interpretation of Pearson's work than has been used previously. It has thus been possible to examine these three (as yet unexamined) key Pearsonian developments: (1) his earliest statistical work (from 1892 to 1895), (2) his joint biometrical projects with Weldon (from 1898-1906) and a shift in the focus of research in the Drapers' Biometric Laboratory following Weldon's death in 1906 and (3) the later work in the twentieth century when he established the two laboratories which were underpinned by two separate methodologies.

The arguments, which follow a chronological progression, have been built around Darwin's ideas of biological variation, 'statistical' populations, his theory of

natural selection and Galton's law of ancestral inheritance. The first two chapters provide background material to the arguments developed in the thesis. Weldon's use of correlation (for the identification of species) in 1889 is examined in Chaper III. It is argued, that Pearson's analysis of Weldon's dimorphic distribution led to their work on speciation which led on to Pearson's earliest innovative statistical work. Weldon's most productive research with Pearson, discussed in Chapter IV, came to fruition when he showed empirical evidence of natural selection by detecting disturbances (or deviations) in the distribution from normality as a consequence of differential mortality rates. This research enabled Pearson to further develop his theory of frequency distributions.

The central part of the thesis broadens out to examine further issues not adequately examined. Galton's statistical approach to heredity is addressed in Chapter V, and it is shown that Galton adumbrated Pearson's work on *multiple correlation* and *multiple regression* with his law of ancestral heredity. This work, in conjunction with Weldon's work on natural selection, led to Pearson's introduction of the use of determinantal matrix algebra into statistical theory in 1896: this (much neglected) development was pivotal in the professionalisation of the emerging discipline of mathematical statistics.

Pearson's work on goodness of fit testing provided the machinery for reconstructing his most comprehensive statistical work which spanned four decades and encompassed his entire working life as a statistician. Thus, a greater part of Pearsonian statistics has been examined than in previous studies. This work, which is assessed in Chapter VI, began in 1892 when he used the sixth-moment (devised from his *method of moments*) for a measure of goodness of fit for problems of speciation, continued throughout the 1890s in his attempt to find an empirical measure of a goodness of fit test for problems of natural selection, culminated in 1900 when he devised the chi-square (χ^2, P) goodness of fit test (his single most significant contribution to modern statistical theory) and ended with the last paper he wrote six

weeks before his death on 27 April 1936. In 1904 Pearson devised the 'square contingency coefficient' (i.e., the *chi-square statistic*).

The penultimate chapter examines the methodologies underpinning the infrastructure of the Drapers' Biometric Laboratory (which was built upon the statistical methods devised in the Biometric School) and the Galton Eugenics Laboratory which involved the use of a complex interconnecting set of family pedigrees and actuarial death rates. It is concluded that in an effort to redress the balance in the historiography of Pearsonian statistics, these three key developments need careful consideration: Pearson's earliest statistical work on goodness of fit testing, the two separate Pearsonian methodologies used in his laboratories and his working relationship with his colleague and 'closest friend' W.F.R. Weldon.

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List of Abbreviations and Short Titles

FG:UCL	Papers and Correspondence of Francis Galton, Manuscript Room in University College London Library.
JRSS	*Journal of the Royal Statistical Society*
JSSL	*Journal of the Statistical Society of London*
KP:UCL	Papers and Correspondence of Karl Pearson, Manuscript Room in University College London Library.
MCTE	Mathematical Contributions to the Theory of Evolution. This refers to Karl Pearson's series of 23 memoirs published mainly in *Philosophical Transactions of the Royal Society* from 1894-1917 (Abstracts of these papers were published in *Proceedings of the Royal Society*). Pearson entitled his first two papers in the series as 'Contributions to the Theory of Mathematical Evolution'.
RP:APS	Papers of Raymond Pearl, American Philosophical Society Library, Philadelphia.

Chapter I

PRELUDE — WHAT IS STATISTICS?

By statistical, [it] is meant in Germany [by Gottfried Achenwell], an inquiry for the purpose of ascertaining the political strength of country or questions respecting matters of state; whereas, the idea, I annex to the term, is an inquiry into the state of a country, *for the purpose of ascertaining the* quantum *of happiness enjoyed by its inhabitants, and the means of future improvement*, as I thought that a new word, might attract more public attention, I resolved on adopting it, and I hope that it is now completely naturalised and incorporated with our language. (Sir John Sinclair, 1798)[1]

Statistics therefore may be said, in the words of the Prospectus [of the Statistical Society of London] , to be the ascertaining and bringing together of those 'facts which are calculated to illustrate the condition and prospects of society', and the object of Statistical Science is to consider the results which they produce, with the view to determine those principles upon which the well-being of society depends. (1834).[2]

Statistics—that is the science of the state—the science of men living in political committees, was never in such demand as it is in the present day... for the art of government can only be practised with success when it is grounded on a knowledge of the people governed, derived from facts observed. (William Farr, 1872)[3]

Statistics, according to the defintion of its professors, is 'the science of social facts expressed in numerical terms'. Its object is to collect and group methodically all moral or social phenomena which are susceptible of numerical valuation. Its method consists in exposition and induction. The method of exposition..., consists in the calculation of averages, and is based on this undoubted truth, that 'in an indefinitely protracted series of events, the action of regular and constant causes must in the long run outweigh that of irregular causes'. (LaPlace). The inductive method, which is less certain, consists in obtaining numerical expressions for social facts, by means of arithmetical or algebraic processes applied to a small number of observations, and in admitting, on the ground of analogy or probability...; Mr. [Francis] Galton employs both methods, but chiefly the second. He feels, therefore, confident in regard to his method. (Theodule Ribot, 1875)[4]

The object of statistical science is to discover methods of condensing information concerning large groups of allied facts into brief and compendious expressions suitable for discussion. The possibility of doing this is based on the constancy and continuity with which objects of the same species are found to vary. (Francis Galton, 1883)[5]

Statistics: The name is used in a double sense for a science and the material of a science... The theory or science to which we give the name of statistics is really a calculus of the interrelationship of variable quantities or qualities, and is essentially a branch of applied mathematics.... [and] a tool of logical inquiry in all fields. (Karl Pearson, 1916).[6]

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1 Sir John Sinclair, *The Statistical Account of Scotland Drawn Up from the Communications of the Ministers of Different Parishes*, vol. XX (Edinburgh, 1798).

2 Anon, 'Introduction' *Journal of the Statistical Society of London* [hereinafter referred to as *JSSL*], 1 (May 1838), 1.

3 William Farr, 'Inaugural Address', *JSSL*, 34 (1864), 151.

4 Th/eodule Armand/ Ribot, *Heredity: A Psychological Study of its Phenomena, Laws, Causes, and Consequences* (Henry S. King: London, 1875), p.191.

5 Francis Galton, *Inquires into Human Faculty and its Development* (Macmillan: London, 1883), p.33.

6 Karl Pearson, 'Statistics' (Lecture notes/77, 1916), Papers and Correspondence of Karl Pearson, University College London. [Hereinafter referred to as KP:UCL].

As it can be seen, the rubric of statistics does not lend itself to one simple definition—whether it is in reaching to the eighteenth century to find its development historically or whether an early twentieth-century definition is desired. At one end of the spectrum, 'statistics' may designate a systematic form of *numeracy* and at the other end, it may indicate a formal mathematical theory underpinned by determinantal matrix algebra and probability. The range of statistics that will be addressed in this chapter lie on the spectrum of 'numeracy' and includes the following: political arithmetic, vital statistics and life tables, official statistics (particularly from the work of William Farr) and the earliest statistical work of Francis Galton.

At the other end of the spectrum lies the statistical work of Karl Pearson who initiated and created, almost singularly, a modern theory of statistics at the end of the nineteenth century. The developments that underpinned this achievement are the focus of this thesis. Pearson's statistical work has been of considerable interest for a number of historians. Historiographical interests surrounding the Biometrician-Mendelian debate of 1900 and of the Galton Eugenics Laboratory, in particular, have fuelled a series of debates that have led to a greater consideration of Pearson's work in the twentieth century than of his work in the nineteenth century (except for his efforts in the development of *simple correlation* and *simple regression*). Much of this debate stems from the claim that the fundamental, if not exclusive, impetus to Pearson's work arose from his relationship with Franics Galton and thus from Galton's statistical work on inheritance and eugenics. It will, however, be shown in the next six chapters that those who have argued in these terms have not addressed the following: (1) Pearson's relationship with other key people who made substantial contributions to the corpus of Pearson's statistical work, (2) the totality rather than a part of Pearsonian statistics and (3) the two different Pearsonian methodologies that underpinned the infrastructure of the Drapers' Biometric Laboratory (from 1903 to 1933) and the Galton Eugenics Laboratory (from 1907 to 1933).

Pearson's link to Galton's work has been the *foci* of much of the research into Pearson's activities by historians of science, in particular, for more than half a century.

This emphasis seems to have led to the tendency to focus almost exclusively on *simple correlation* and *simple regression* to the neglect of the more crucial components in the corpus of Pearsonian statistics and also seems to have engendered the belief that this work was Pearson's principal contribution to the development of statistics. Consequently, correlation has come to be regarded as the underlying statistical method used in the Eugenics Laboratory, whilst virtually no consideration has been given to the quantitative tools that Pearson devised specifically and exclusively for the Eugenics Laboratory. Moreover, there has been a limited use of archival sources related to these developments. These approaches have thus led to a partial assessment of Pearson's contributions in the overall historiography of Pearsonian statistics.

A reconstruction of the totality of the corpus of Pearsonian statistical theory and techniques has, therefore, been undertaken to identify the conceptual and mathematical developments that led to the creation of a modern theory of statistics. Considerable attention has been given to Pearson's most innovative and seminal statistical work which occurred from 1892 to 1904 and especially to his much neglected work from 1892 to 1895. This neglect has been one of the two fundamental lacunae in the historiography of Pearsonian statistics; the other lacuna includes Pearson's relationship with his colleague and 'closest friend' W.F.R. Weldon and the joint biometrical projects they undertook from 1899 to 1906.

The principal argument that underlies this thesis is twofold: it is argued firstly, that the Darwinian biological concepts at the centre of much of Weldon's work, arising from his statistical and experimental investigations of marine organisms, provided the impetus to Pearson's earliest statistical work on goodness of fit testing (in 1892). These interests provided the wider basis of a programme that underpinned Pearson's work in the long term—and this programme may be distinguished in Pearson's corpus from specifically Galtonian influences. It is also argued that these developments were underpinned by a resolution of the Aristotelian essentialistic concept of species and of deterministic notions of the ideal (statistical) mean

surrounding Charles Darwin's theory of natural selection at the end of the nineteeth century. It will be shown that Galton's fundamental contribution to the corpus of Pearsonian statistics arose from his statistical work on heredity which led to Pearson's development of *multiple correlation*, *multiple regression* and the *coefficient of variation*. Contributions from such individuals as Francis Ysidro Edgeworth, George Udny Yule and from at least 20 biometricians will also be addressed. It is thus argued that consideration must be given to the totality of Pearson's statistical work and to all the individuals, *especially* to W.F.R. Weldon, in an effort to redress the balance in the historiography of Pearsonian statistics.

SIR JOHN SINCLAIR AND THE *STATISTICAL ACCOUNTS OF SCOTLAND* (1791-1798)

Although the use of quantitative thinking may be traced to, at least, the seventeenth century, the term 'statistics' did not emerge in the English language until the end of the eighteenth century. The origin of the word appears to have been the Italian word *statista* which was first used in the sixteenth century and referred to the statist or the statesman who was concerned with the reasoning in describing affairs of the state.[7] The adjective *statisticus* first came into fashion in the seventeenth century and was 'probably introduced by Gabriel Naude (1600-59) in 1639'.[8] This word soon became used in Germany where it was adopted by Hermann Corning (1607-1681), Casper Thurman (1641-1705) and then by Gottfried Achenwell (1719-1772).[9] The counsel of the Statistical Society of London would write in 1834 that

> the word statistics [was] of German origin, and is derived from the word *staat*, signifiying the same as our English word *state*, or a body of men existing in a social union.[10]

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7 Karl Pearson, *The History of Statistics in the Seventeenth and Eighteenth Centuries against the Changing Background of Intellectual, Scientific and Religious Thought* (Lectures by Karl Pearson given at University College London during the academic sessions 1921-1933), ed. E.S. Pearson (Charles Griffin: London, 1978), p.4. [Hereinafter referred to as Pearson, *History of Statistics*.]

8 *Ibid.*

9 *Ibid.*

10 Anon, 'Introduction', *JSSL*, 1 (1838), p.1.

Broadly speaking, it may be said that over the course of two centuries three different definitions of the word 'statistics' have evolved. The first definition of statistics arose from a quantitative system for describing matters of the state with the principal aim of building up a political edifice. Near the end of the eighteenth century, Gottfried Achnewell, then Professor of Law and Politics at Göttingen, introduced the term *Statistik* as a separate and distinct branch of knowledge to designate a 'mixture of constitutional history, the elements of political economy and statescraft'.[11] Like the statist or the statesman of the sixteenth century, Achenwell was concerned with 'inquiries respecting the Population, the Political Circumstances, the Production of a Country, and other Matters of State'[12] .

Achenwell's *Statistik* indicated a branch of learning that

> occupie[d] itself with the extent, limits, subdivisions, and natural relations of States, their advantages, their history, and their origin; as the description of the political constitution of one or more States;... by statistics (*die statistik*) we attain a knowledge of the States and their constitution.[13]

From this new 'branch of learning', Achenwell wanted to describe the actual condition of a State under all aspects—territorial, political, educational, religious, industrial, commerical and financial. In his eight treatises on Spain, Portugal, France, Great Britain, the Netherlands, Denmark, Russia and Sweden, he provided a short history of the origin and growth of the country, an account of the boundaries, climate, mountains and streams, the population and their fundamental laws.[14] 'Statistics' was thus applied to everything that pertained to the state.

In the Prelude, it may be seen that Sir John Sinclair describes this early Germanic definition (which would become later subsumed under 'political economy'),

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11 William A. Guy, 'On the Original and Acquired Meaning of the term "Statistics," and on the Proper Functions of a Statistical Society: Also on the Question whether there be a Science of Statistics; and, if so, What are its Nature and Objects, and What is its Relation to Political Economy and "Social Science"', *JSSL*, 28 (21 November 1865), 479 [hereinafter referred to as Guy, 'The Original Meaning of "Statistics"']; Pearson, 'Introduction of the Early History of Statistics' in Pearson, *History of Statistics*, pp.2 and 6.

12 Sir John Sinclair, 'First Circular Letter to the Clergy of the Church of Scotland' (25 May 1790), in Yule, '"Statistics"', *JSSL* (1905), 392

13 Guy, 'The Original Meaning of "Statistics"', *JSSL* (1865), 479.

14 *Ibid.*, 480.

and he also indicates how the word 'statistics' was to take on a different meaning once he introduced it into the English language (when he used the adjective "statistical" in 'The First Circular Letter to the Clergy and Church of Scotland' on 25 May 1790.)[15] One year later, Sinclair published the first volume (out of 21) of his *Statistical Accounts of Scotland.* In a period of eight and a half years, he had assembled information derived from the ministers of 938 parishes in Scotland *'without a single one being omitted'.*[16] Sinclair asked 160 questions concerning matters of geographic and natural history of the Parishes, Population of Parish, Production of the Parish and various Miscellaneous questions.[17] The material was subsequently presented to the public.

The second definition of the word statistics consists of vital statistics, actuarial methods and census taking. This approach involved the recording of a more extensive and systematic collection of aggregate data than that used by Achenwell and his followers. The earliest use of what may be regarded as 'vital statistics' occurred a century before Achenwell started his work with the publication of Captain John Graunt's *Natural and Political Observations upon the Bills of Mortality* in 1662 (which will be discussed in greater detail in the next section). Though this use of 'statistics' was still concerned primarily with matters of the state, a shift in the meaning of the word has been detectable in Sinclair's emphasis on the building of a social edifice rather than that of an Achenwellian political edifice. Sinclair's use would become adopted by the founders of the Statistical Society of London in 1834 as they thought that the statistical 'facts were to be applied to the building up of a social rather than a political edifice'.[18] Two years after the Statistical Society of London had become established, one of its founders, Adolphe Quetelet (1796-1884), expressed his

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15 See G. Udny Yule, 'The Introduction of the Words "Statistics", and "Statistical" into the English Language—Newmarch Lecture in Statistics, University College London', *JSSL*, 68 (1905), 392. [Hereinafter referred to as Yule, '"Statistics"'.]

16 Sir John Sinclair, *Statistical Account of Scotland*, XX (1798), p.vi.

17 *Ibid.*, vols I-XXI (1791-1798).

18 Guy, 'The Original Meaning of "Statistics"' *JSSL* (1865), 479.

view that 'in statistical research our object is generally to discover the causes which influences social facts, and to determine the degree of their energy'.[19]

Ribot's definition in the Prelude is quintessentially characteristisic of practice during the greater part of the nineteenth century. The statistical work and innovations of the archetypal statistician of this time, William Farr (1807-1883), fulfills aptly Ribot's definition. From this tradition, Farr created a national system of vital statistics in his capacity as Compiler of Abstracts to the Registrar General.[20] By the time Ribot published his view of statistics in 1875, Francis Galton had just read his first statistical paper 'On a Proposed Statistical Scale' at the Royal Institution, and a divergence in methods from those of the early statists and particularly from those of William Farr, had not only become detectable, but it had become increasingly conspicuous.[21] Though Ribot attempted, in his definition, to encompass Galton's statistical method, he neglected to identify Galton's use of *variation*: the linchpin of Galton's statistical work. Nonetheless, Ribot's description did, indeed, capture the statistical style of Galton's predecessors whose statistical thinking followed a Baconian inductive approach such as was reflected in the work of William Farr.

A third definition can be traced back to the eighteenth century with the introduction of a formal mathematical theory of probability when Abraham De Moivre (1667-1754) reached an approximation of the binomial distribution in 1733 which was followed several decades later, by Pierre Simon LaPlace's (1749-1827) work on the mathematical equation for the 'error curve' (i.e., the normal distribution) in 1777.[22] These ideas became further developed in the statistical work of Francis Ysidro Edgeworth, Francis Galton, Adolphe Quetelet and John Venn which came to

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19 Adolphe Quetelet, *Letters addressed to the H.R.H. the Grand Duke of Saxe Cobury and Gotha in the Theory of Probabilities as applied to the Moral and Political Sciences*, Letter XXXI, Trans. O. Linthus Downe (London, 1846), p.147.

20 John M. Eyler, *Victorian Social Medicine. The Ideas and Methods of William Farr* (Johns Hopkins University Press: Baltimore, 1979), p.45. [Hereinafter referred to as Eyler, *Methods of William Farr*.]

21 Francis Galton, 'On a Proposed Statistical Scale', *Nature*, 9 (5 March 1874), 342-3.

22 See Steve Stigler, *The History of Statistics. The Measurment of Uncertainty before 1900.* (Belknap Press of Harvard: Cambridge, Mass., 1986), pp.70-88 and 99-108. [Hereinafter referred to as Stigler, *History of Statistics*.]

fruition primarily with the statistical innovations of Karl Pearson. During the last decade of the nineteenth century, Pearson was to devise a rigorous mathematical system that would lead to the creation of a modern theory of 'mathematical-statistics' when he devised and incorporated the following:

(1) a set of mathematical tools (consisting of the *method of moments* used in tandem with the relative frequency approach to probability) for a systematic examination of frequency distributions for symmetrical and asymmetrical curves in 1892,

(2) an empirical criterion for a goodness of fit test derived originally from the *sixth moment* as a means of fitting a theoretical curve to observational data in 1892. This work was to lead to his single most important and original contribution to modern statistical theory when he devised the chi-square (χ^2, P) goodness of fit test in 1900 to determine how well an empirical distribution 'fit' a theoretical (or 'expected') distribution.

(3) the introduction of the use of determinantal matrix algebra in 1896,

(4) a range of statistical methods that emerged from this theoretical structure which included the following: the standard deviation in 1892; standard error of the mean $\frac{\sigma}{\sqrt{N}}$ and standard error of the standard deviation $\frac{\sigma}{\sqrt{2N}}$ for sampling distributions in 1894; the product-moment correlation coefficient, simple regression; multiple correlation, partial correlation, multiple regression, the standard error of estimate plus the coefficient of variation in 1896; the phi-coefficient and the tetrachoric correlation in 1900, the contingency coefficient *C* and the square contingency coefficient χ^2 (i.e., the chi-square statistic or test of independence for contingency tables) in 1904; the biserial correlation and the correlation ratio (for curvilinear relationships) in 1909 and finally,

(5) a set of *Tables for Statisticians and Biometricians* in 1914 (Part I) and in 1931 (Part II).[23]

The work that emerged from Pearson's Biometric School in the 1890s and from the Drapers' Biometric Laboratory in the twentieth century may be seen as an expansion of the previously existing statistical methods on the spectrum of statistics (thus, Pearsonian statistics represents the other end of the spectrum to 'numeracy' and vital statistics). The outcome of this emerging discipline of mathematical-statistics may also be seen as representing a different statistical paradigm (or 'model') from the statistical paradigms that preceded Pearson's work. The central tenets of the Pearsonian paradigm will be discussed more fully in the next chapter.

A continuity of approach that links the spectrum of the statistical work of John Graunt, William Farr and Karl Pearson can be found in their development of quantitative methods for a systematic presentation of aggregate mortality data. Graunt based his approach upon an 'arithmetical method derived from merchant bookkeeping' as well as following the Baconian tradition of using tables as a method for the presentation of observations.[24] Farr incorporated various actuarial methods to analyse data derived from the *Annual Reports of the Registrar General* and Pearson devised a statistical system to help W.F.R. Weldon measure selective death rates in plants and animals to show empirical evidence of natural selection. (Francis Galton's statistical work, however, came to fruition from his use of *fertility* data for his studies of inheritance and eugenics).

Some of the changes in the presentation of statistical data have been depicted in Figures 1.1 to 1.6. Figure 1.1 is an example of Graunt's London Bills of Mortality from 1662 and Figure 1.2 reflects a change in Mortality tables with one of Farr's Life Tables in 1841. Figure 1.3 is an example of one of the diagrams Farr used to show the results of his elevation law from the 1849 cholera epidemic. Though Farr's diagram

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23 Karl Pearson (ed.), *Tables for Statisticians and Biometricians* (Cambridge University Press, Part I (1st edn, 1914, 2nd edn, 1924, and 3rd edn, 1930); Part II (1st edn, 1931).

24 Philip Kreager, 'New Light on Graunt', *Population Studies*, 42 (1988), 130.

Figure 1.1
ANNUAL BILL OF MORTALITY FOR LONDON (1663)

A generall Bill for this preſent year, ending the 19 of *December* 1665. according to the Report made to the KINGS moſt Excellent Majeſty.

By the Company of Pariſh Clerks of *London*, &c.

	Buried	Plag.		Buried	Plag.		Buried	Plag.		Buried	Plag.
S' Albans Woodſtreet	100	121	S' Clements Eaſtcheap	38	20	S' Margaret Moſes	38	25	S' Michael Cornhill	104	52
S' Alhallowes Barking	514	130	S' Dionis Back-church	78	27	S' Margaret Newfiſh	114	66	S' Michael Crookedla	179	123
S' Alhallowes Breadſt	15	10	S' Dunſtans Eaſt	165	150	S' Margaret Pattens	49	24	S' Michael Queenhi	203	122
S' Alhallowes Great	455	420	S' Edmunds Lumbard	70	30	S' Mary Abchurch	99	54	S' Michael Quene	44	18
S' Alhallowes Honila	10	5	S' Ethelborough	195	106	S' Mary Aldermanbury	181	109	S' Michael Royall	152	118
S' Alhallowes Leſſe	139	173	S' Faiths	104	70	S' Mary Aldermary	105	75	S' Michael Woodſtreet	122	62
S' Alkall. Lumbardſtr.	90	52	S' Foſters	144	105	S' Mary le Bow	64	36	S' Mildred Breadſtreet	52	26
S' Alhallowes Staining	185	122	S' Gabriel Fen-church	62	29	S' Mary Bothaw	55	30	S' Mildred Poultrey	68	46
S' Alhallowes the Wall	500	156	S' George Botolphlane	41	27	S' Mary Colechurch	17	8	S' Nicholas Acons	36	28
S' Alphage	171	113	S' Gregories by Pauls	175	131	S' Mary Hill	94	64	S' Nicholas Colesabby	125	91
S' Andrew Hubbard	71	15	S' Hextens	108	75	S' Mary Mounthaw	36	37	S' Nicholas Olaues	90	62
S' Andrew Vndershaft	174	189	S' James Dukes place	262	190	S' Mary Summerſet	342	262	S' Olaves Hartſtreet	237	160
S' Andrew Wardrobe	476	301	S' James Garlickhithe	189	116	S' Mary Stayninge	47	27	S' Olaves Iewry	44	33
S' Anne Aldersgate	282	197	S' John Baptiſt	138	83	S' Mary Woolchurch	65	33	S' Olaves Silverſtreet	350	132
S' Anne Blacke-Friers	652	467	S' John Euangeliſt	9		S' Mary Woolnoth	75	38	S' Pancras Soperlane	30	15
S' Antholins Pariſh	58	33	S' John Zacharie	85	54	S' Martins Iremonger.	21	11	S' Peters Cheape	61	35
S' Auſtins Pariſh	43	10	S' Katherine Coleman	199	111	S' Martins Ludgate	196	128	S' Peters Cornehill	136	76
S' Barthol. Exchange	73	11	S' Katherine Creechu.	135	231	S' Martins Organs	110	71	S' Peters Paule Wharfe	114	86
S' Bennet Fynch	47	12	S' Lawrence Iewry	94	48	S' Martins Outwich	60	34	S' Peters Poore	79	47
S' Ben.t. Grace-church	57	41	S' Lawrence Pountney	214	140	S' Martins Vintry	417	349	S' Stevens Colmanſt	60	391
S' Bennet Paules Wharf	355	173	S' Leonard Eaſtcheap	42	27	S' Matthew Fridayſtr.	24	6	S' Stevens Walbrooke	14	17
S' Bennet Sherehog	11	1	S' Leonard Foſterlane	335	255	S' Maudlins Milkſtreet	44	22	S' Swithins	93	56
S' Botolph Billingſgate	83	50	S' Magnus Pariſh	103	60	S' Maudlins Oldfiſhſtr.	176	121	S' Thomas Apoſtle	63	110
Chriſts Church	653	167	S' Margaret Lothbury	100	66	S' Michael Baſſiſhaw	153	164	Trinitie Pariſh	115	79

Buried in the 97 Pariſhes within the walls — 15207 Whereof, of the Plague — 9887

S' Andrew Holborne	3938	3103	Bridewell Precinct	130	170	S' Dunſtans Weſt	958	665	S' Saviours Southwark	3225	1446
S' Bartholomew Great	491	344	S' Botolph Aldersga	997	715	S' George Southwark	1813	1260	S' Sepulchres Pariſh	4509	1746
S' Bartholomew Leſſe	193	119	S' Botolph Alyate	4916	4051	S' Giles Cripplegate	8069	4818	S' Thomas Southwark	478	371
S' Bridget	2811	1427	S' Botolph Biſhopſg	3454	2300	S' Olaves Southwark	4793	2785	Trinity Minories	168	133
									At the Peſthouſe	159	156

Buried in the 16 Pariſhes without the walls — 41351 Whereof, of the Plague — 28888

S' Giles in the Fields	1457	716	S' Katherines Tower	956	601	S' Magdalen Bermon	1943	1363	S' Mary Whitechappel	4766	1855
Hackney Pariſh	532	132	Lambeth Pariſh	798	337	S' Mary Newington	1272	1004	Reteiffe Pariſh	304	110
S' James Clarkenwell	1863	1377	S' Leonard Shoredich	2669	1949	S' Mary Iſlington	695	593	Stepney Pariſh	8598	5583

Buried in the 12 out-Pariſhes, in Middleſex and Surrey — 18554 Whereof, of the Plague — 11420

S' Clement Danes	1969	1339	S' Mary Sauoy	303	198	
S' Paul Covent Garden	408	261	S' Margarets Weſtminſt.	4710	3743	
S' Martins in the Fields	4304	2883				

Buried in the 5 Pariſhes in the City and Liberties of Weſtminſter — 12193
Whereof, of the Plague — 8403

The Total of all the Chriſtnings — 9967
The Total of all the Burials this year — 97306
Whereof, of the Plague — 68596

The Diſeaſes and Caſualties this year.

Abortive and Stilborne	617	Executed	21	Palſie	30	
Aged	1545	Flox and Small Pox	655	Plague	68596	
Ague and Feaver	5257	Found dead in ſtreets, fields, &c.	20	Planet	6	
Appoplex and Suddenly	116	French Pox	86	Pliriſie	15	
Bedrid	10	Frighted	23	Poyſoned	1	
Blaſted	5	Gout and Sciatica	27	Quinſie	35	
Bleeding	16	Griefe	46	Rickets	557	
Bloody Flux, Scowring & Flux	185	Griping in the Guts	1288	Riſing of the Lights	397	
Burnt and Scalded	8	Hangd & made away themſelves	7	Rupture	34	
Calenture	3	Headmouldſhot & Mouldſallen	14	Scurvy	105	
Cancer, Gangrene and Fiſtula	56	Jaundies	110	Shingles and Swine pox	2	
Canker, and Thruſh	111	Impoſtume	227	Sores, Ulcers, broken and bruiſed		
Childbed	625	Kild by ſeverall accidents	41	Limbs	82	
Chriſomes and Infants	1258	Kings Evill	86	Spleen	19	
Cold and Cough	68	Leproſie	2	Spotted Feaver and Purples	1929	
Collick and Winde	134	Lethargy	14	Stopping of the ſtomack	338	
Conſumption and Tiſſick	4808	Livergrown	21	Stone and Strangury	98	
Convulſion and Mother	2036	Meagrom and Headach	12	Surfet	1251	
Diſtracted	5	Meaſles	7	Teeth and Worms	2614	
Dropſie and Timpany	1478	Murthered and Shot	9	Vomiting	51	
Drowned	50	Overlaid & Starved	45	VVenn	8	

Chriſtned	Males 5114	Buried	Males 48569	Of the Plague	68590
	Females 4853		Females 48737		
	In all 9967		In all 97306		

Increaſed in the Burials in the 130 Pariſhes and at the Peſt-houſe this year — 79009
Increaſed of the Plague in the 130 Pariſhes and at the Peſt-houſe this year — 68590

Figure 1.2
ANNUAL LIFE TABLE FROM THE REGISTRAR-GENERAL'S OFFICE (1841)

(*n* 4) LIFE TABLES,
Deduced from the Enumerations of the Living and the Deaths in 1841.

Age.	Living.			Dying, in the next Interval of Age.		
	Surrey.	Liverpool.	Metropolis.	Surrey.	Liverpool.	Metropolis.
0	100,000	100,000	100,000	12,229	25,290	16,266
1	87,771	74,710	83,734	3,699	11,770	7,961
2	84,072	62,934	75,770	2,190	5,029	3,630
3	81,882	57,855	72,140	1,439	3,210	2,286
4	80,443	54,645	69,854	1,244	2,447	1,525
5	79,199	52,198	68,329	3,776	3,987	3,408
10	75,423	48,211	64,921	2,159	1,618	1,381
15	73,264	46,563	63,540	2,379	1,755	1,856
20	70,885	44,808	61,684	2,616	2,096	2,350
25	68,269	42,712	59,334	2,710	2,363	2,666
30	65,559	40,340	56,668	2,678	2,772	2,875
35	62,881	37,577	53,793	3,466	3,829	3,941
40	59,415	33,748	49,852	2,936	3,120	3,540
45	56,479	30,628	46,312	4,419	4,750	5,003
50	52,060	25,878	41,309	3,674	3,657	4,640
55	48,386	22,221	36,669	5,752	4,760	6,830
60	42,634	17,461	29,839	5,937	4,068	6,011
65	36,697	13,393	23,798	8,659	5,020	7,454
70	28,038	8,373	16,344	8,003	3,114	5,857
75	20,035	5,259	10,487	8,790	2,980	5,985
80	11,245	2,279	4,502	6,799	1,437	2,697
85	4,446	842	1,805	3,653	581	1,445
90	793	261	360	693	172	293
95	100	89	67	85	56	58
100	15	33	9	13	33	8
105	2	..	1	2		1

(*o* 5) EXPECTATION OF LIFE (in Years).

Age.	PERSONS.			MALES.			FEMALES.		
	Surrey.	Liverpool.	Metropolis	Surrey.	Liverpool.	Metropolis.	Surrey.	Liverpool.	Metropolis.
0	45	26	37	44	25	35	46	27	38
1	50	33	43	50	33	41	50	34	44
2	51	36	46	51	37	45	52	39	48
3	52	41	47	52	40	46	52	42	49
4	52	42	48	52	41	46	52	43	50
5	52	43	48	51	42	46	52	43	50
10	49	41	45	49	41	44	49	42	47
15	45	37	41	45	37	40	45	38	43
20	42	34	38	42	33	36	42	34	39
25	38	30	34	38	30	32	38	31	35
30	35	27	30	35	27	29	35	27	32
35	31	24	27	31	23	25	31	24	28
40	28	21	24	28	21	22	28	22	25
45	24	18	20	24	18	19	24	18	22
50	21	16	18	21	16	17	21	17	18

Figure 1.3
WILLIAM FARR'S DIAGRAM OF ELEVATION FOR THE 1849 CHOLERA EPIDEMIC

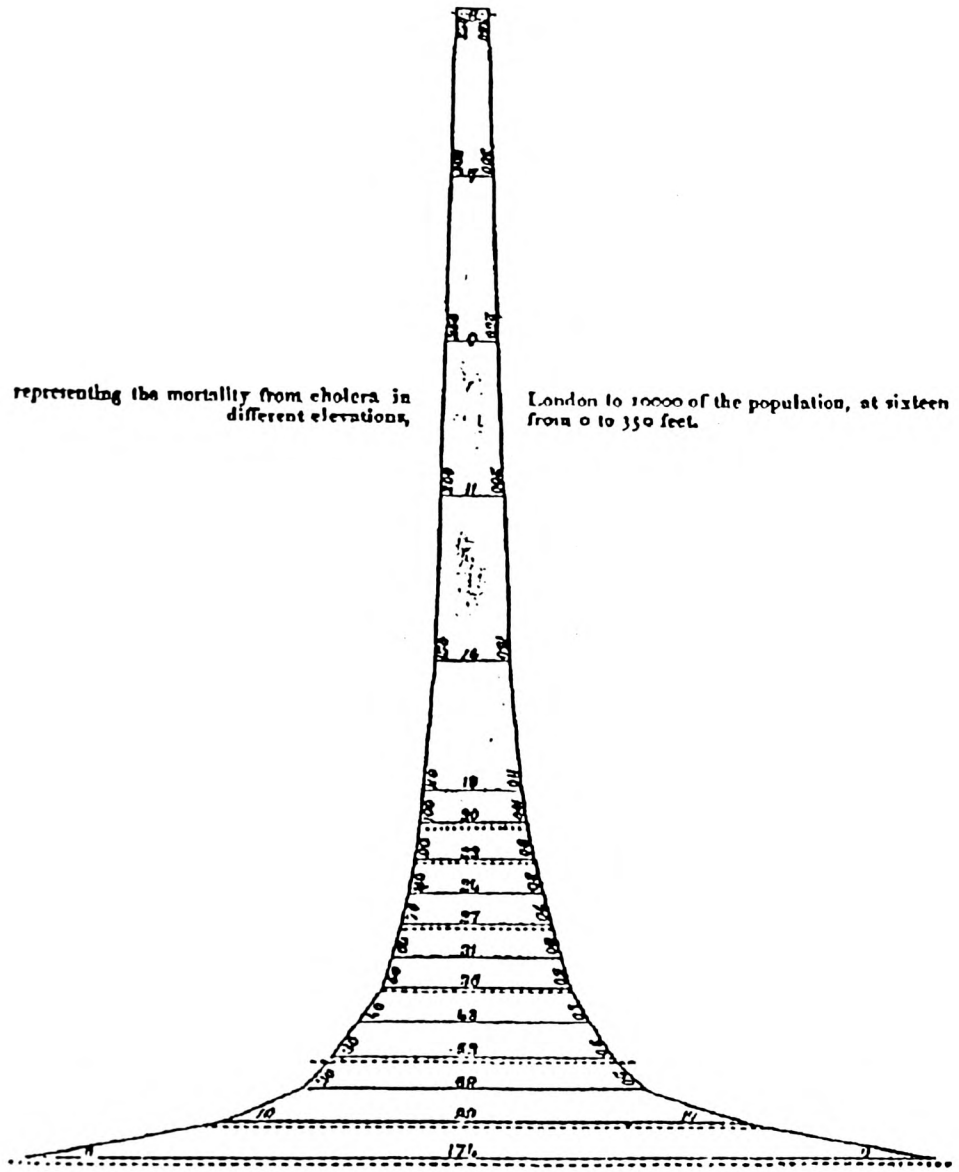


Figure 3. Farr's diagram for the elevation law for the 1849 cholera epidemic. William Farr, *Report on the Mortality of Cholera in England, 1848–49* (London, 1852), p. lxv.

William Farr, *Report on the Mortality of Cholera in England. 1848-49* (London, 1852), p. lxv.

Figure 1.4
ALEXANDER LANGMUIR'S GRAPH ILLUSTRATING FARR'S ELEVATION LAW FOR THE 1849 CHOLERA EPIDEMIC

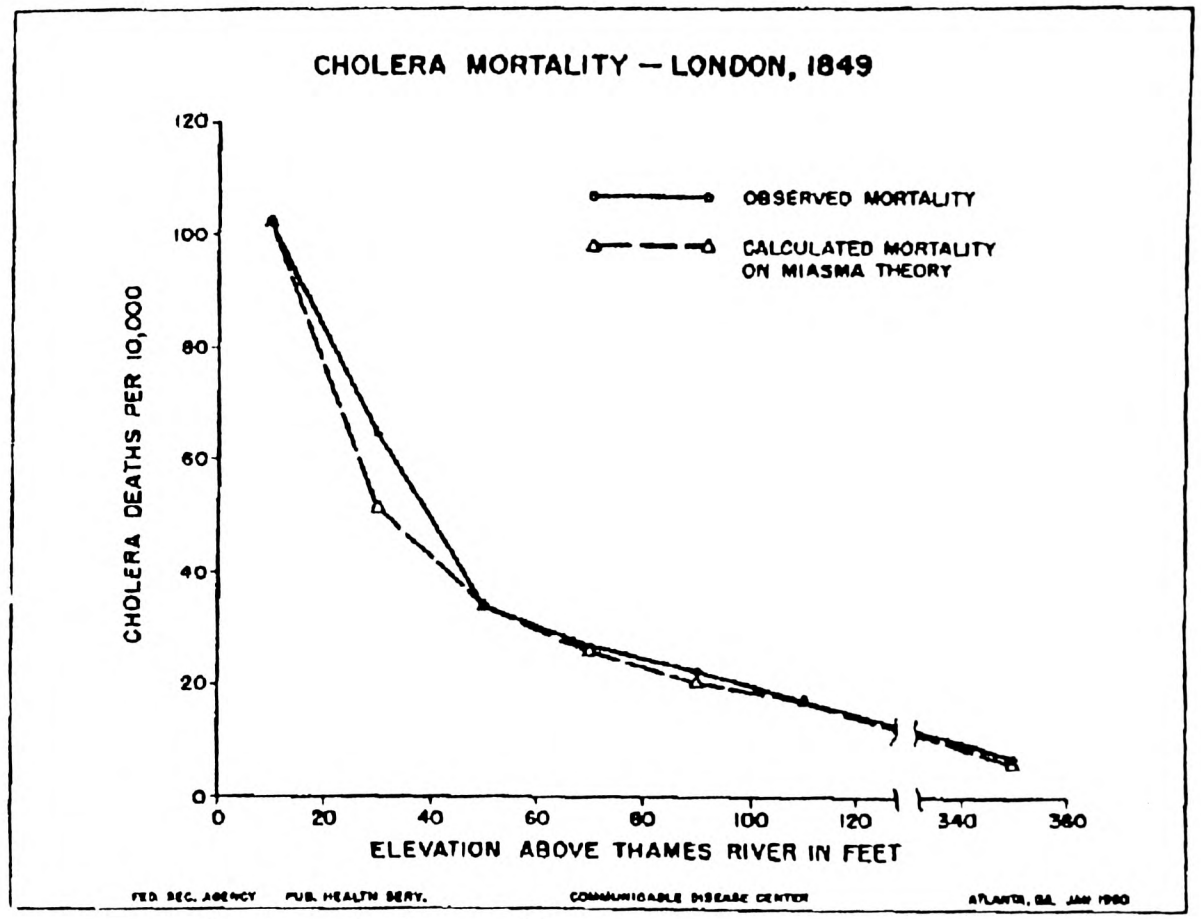


Figure 4. Alexander Langmuir's graph illustrating Farr's elevation law for the 1849 cholera epidemic. Alexander D. Langmuir, "Epidemiology of Airborne Infection," *Bact. Rev.* 25 (1961): 174.

Alexander D. Langmuir, 'Epidemiology of Airborne Infection', *Bact. Rev.*, 25 (1961), 174

looks as if it is a one-dimensional graph (as the data seems to have been plotted on the Y axis), his diagram was a rather ingenious method to cope with the data that would be regarded later as forming a bivariate distribution with distinct Y and X axes (which is the type of frequency distribution that Galton introduced and Pearson would further develop and popularise). An example of a bivariate distribution using Farr's data, may be seen in Figure 1.4. Figure 1.5 displays a diagram that Farr used in the *Seventh Annual Report of the Registrar-General* in 1843 which shows the number of deaths of an English generation through life whereas Figure 1.6 depicts the curves that Pearson calculated when trying to describe the shapes of the distributions for English mortality from 1871-1880. In this figure, he shows that four curves (i.e. childhood, youth, middle life and old age) represent the varying distributions of mortality at different stages in life which may be used rather than the one curve used in Figure 1.5.

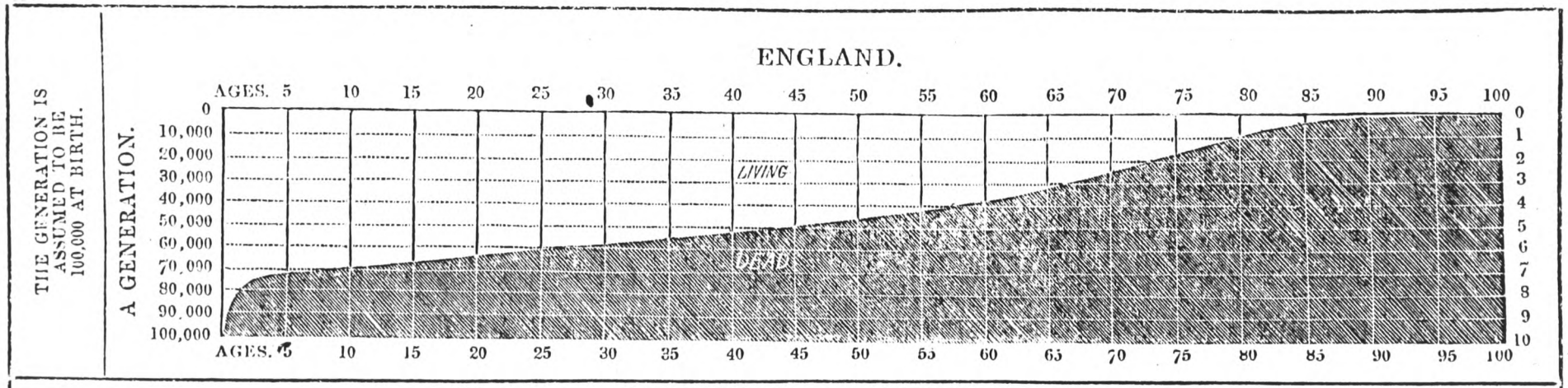
FROM POLITICAL OBSERVATIONS TO LIFE TABLES

Karl Pearson attempted to trace, what may be considered the earliest use of 'statistical thinking' to a fourteenth century chronicle of a certain parish in Florence where the priest 'recorded the births in his parish by beans, black for boys and white for girls, and so determined the sex ratio'.[25] Though he could not find the name of the priest and realised that this was a single incident only, he believed the unnamed priest may have been, nevertheless, 'the first to grasp the value of church records for statistical purposes'.[26] Indeed, the parish registers and the church records would provide a rich source of information for some of the earliest statistical ideas. John Graunt used such information, and provided an exemplary model of the earliest use of vital statistics in his *Natural and Political Observations upon the Bills of Mortality.*

Graunt's observations were both political and natural as they involved matters concerning 'Trade and Government... Air, Countries, Season and the proportion

25 Pearson, 'Introduction of the Early History of Statistics', in Pearson, *History of Statistics*, p.8
26 *Ibid.*

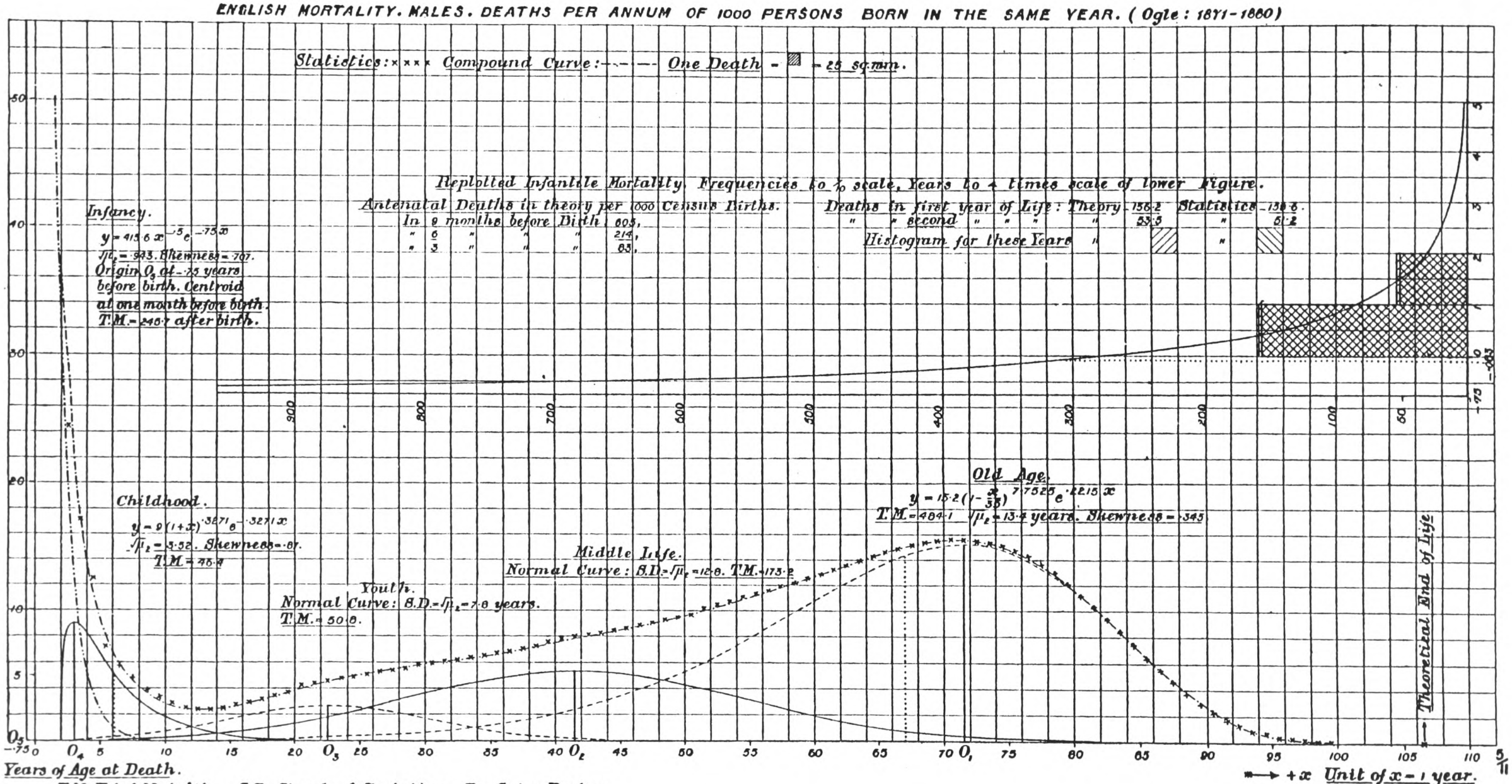
Table 1.5
FARR'S DISTRIBUTION OF ENGLISH DEATHS (1843)



"The first diagram is intended to represent the progress of an English generation through life the light part indicating the living, the dark the dead, at each age out of a given number (100,000) born alive. The vertical lines divided into ten degrees serve to measure, at every fifth year, the number alive and dead at the respective ages. The areas of the enclosed light spaces serve also to show the relative numbers living. The second, third, and fourth, diagrams exhibit the same facts for Surrey, Liverpool, and the Metropolis. The extent of light space upon each diagram gives a general notion of the relative population which would be maintained in the different circumstances by an *equal number* of births; but this is more clearly seen in the second set of diagrams (p. 52), in which the light space representing the living is thrown into the form of parallelograms. These parallelograms are divided by vertical lines, so as to show the relative numbers that would be living at the different ages (if the births were not more than the deaths) in Surrey, Liverpool, and the Metropolis. At the four very different rates of mortality, such would be the relative population of the kingdom—such the relative numbers of children, adults, and old people, to the same number of births. Observation has, however, shown that the births increase generally as the deaths increase; and the parallelogram of Liverpool, to represent the population of an unhealthy place (into which there was no immigration), must gain in breadth, particularly at the end on the left hand, representing the births, the space which it loses in length."—p. 38.

It will be seen by these Diagrams, that of the 100,000 born alive in Surrey, more than the half (50,000) are alive at the age of 50; while out of the same number born alive, 41,000 live to the age of 50 in the Metropolis, and 26,000 in Liverpool.

Table 1.6
PEARSON'S FOUR CURVES OF MORTALITY (1895)



T.M. = Total Mortality. S.D. = Standard Deviation. $\sqrt{\mu_2}$ = Swing-Radius.
Centroid Verticals: ·····; Axes of y: ———, Origins = O_1, O_2, O_3, etc.

between the Sex and Age of Mankind'.[27] The observations he made upon the *Bills of Mortality* consisted of 10,000 parishes in England and Wales which contained information of sex, age, and causes of death by disease (e.g. jaundice, palsy and sciatica) or casualties (e.g. burnt, drowned, murdered, 'poysoned', shot, and starved).[28] From Graunt's work emerged one of the earliest systematic uses of quantitative reasoning. Graunt used the term 'political arithmetick' to describe his work—a term coined for him by his friend William Petty.[29] Petty explained that

> instead of using only comparative and superlative Words and intellectual Argument, I have taken the course (as a Specimen of the Political Arithmetick I have long aimed at) to express myself in Terms of *Number, Weight, or Measure*.[30]

Karl Pearson considered Graunt to have been 'the father of statistics' as it was 'not possible for a real statistician to exist unless he ha[d] material to work upon ... [and] nothing of that sort existed until the middle of the 17th century with Graunt's *Bills of Mortality*'.[31] This was not only one of the first attempts to provide a systematic account of the births and deaths in England, but it could, so Pearson suggested, be regarded as 'one of the first treastise on vital statistics'.[32] Moreover, it also showed 'the earliest attempts to apply theory, if only "shop arithmetic" to real statistical problems'.[33]

One of Pearson's students, Major Greenwood who was to teach vital statistics in the University of London, came to regard Graunt as the 'founder of vital statistics' since

> Graunt was not only the first man to *measure* the difference between urban and rural mortality and to devise methods of estimating populations by sampling, he was the first constructive critic of medical-statistical data.... With one important exception,

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27 Captain John Graunt, 'The Epistle Dedicatory of *Natural and Political Observations upon the Bills of Mortality* [1662] in Charles Henry Hull, *Economic Writings of Sir William Petty with the Natural and Political Observations upon the Bills of Mortality (more probably by John Graunt)*, vol. II (Cambridge University Press, 1899), p.323.

28 *Idem*, *Observations upon The Bills of Mortality* (1662) in Hull, *Economic Writings of Petty*, *passim*.

29 Pearson, 'The Founding of the English School of Political Arithmetic' in Pearson, *History of Statistics*, p.10.

30 Sir William Petty, *Political Arithmetick* (St Paul's Churchyard: London, 1650), Preface.

31 Pearson, *History of Statistics*, pp.8 and 10.

32 *Ibid.*

33 Pearson, 'Introduction of the Early History of Statistics', in Pearson, *History of Statistics* p.2.

> no English writer added anything to Graunt's methodology for more than a century after his death. The exception is, of course, the technique of constructing what we call a life-table.[34]

The demographer, David Glass, would later consider Graunt to be the 'founder of demography'.[35]

The *Bills of Mortality* were drawn up primarily from records of baptisms and burials of members of the established church and published by the Company of Parish Clerks.[36] From this material Graunt extracted

> the excess of male births over female births, the approximate numerical equality of the sexes, the high rate of mortality in the early years of life, and the excess of urban over rural deaths.[37]

Graunt thought that 'there should be inherent quantitative regularities in so many [number of] deaths'.[38] The recordings of baptisms, marriages and burials had been the responsibility of the local clergy of the Church of England since Thomas Cromwell's injunction of 1538.[39] With the exception of two relatively short periods of time, it was the responsibility of the local minister of the Church of England to record the baptisms, burials and marriages of members of his congregation. Dissenters and members of other faiths were thus very largely excluded from the records as were many adherents of the established Church who did not wish, or could not afford to pay the fees associated with ecclesiastical registration.[40]

From the mortality rates derived from the *Bills of Mortality*, James Dobson, former pupil of Abraham DeMoivre and Master at the Royal Mathematical School, Christ Church Hospital, calculated a set of risks which led to the development of a

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34 Major Greenwood, *Some British Pioneers of Social Medicine* (Oxford University Press: Oxford, 1948), p.22.

35 David V. Glass, *Numbering the People: The Eighteenth-century Population Controversy and the Development of Census and Vital Statistics in Britain* (Gordon and Cemonesi: London, 1978; first published in 1973) p.130. [Hereinafter referred to as Glass, *Numbering the People.*]

36 David V. Glass, 'The Population Controversy in Eighteenth-century England', in Glass, *Numbering the People*, p.16; Eyler, *Methods of William Farr* (1979), p.37.

37 Michael Cullen, *The Statistical Movement in Early Victorian Britain: The Foundations of Empirical Social Research* (Barnes and Noble: New York, 1975), p.5. [Hereinafter referred to as Cullen, *Statistical Movement.*]

38 Kreager, 'New Light on Graunt', 129.

39 Glass, *Numbering the People*, p.1

40 *Ibid.*, p.15.

mutual system of whole life assurance.[41] Though Dobson's scheme was never accepted 'it formed the basis of the Equitable Society, established in 1762 under a Deed of Settlement drawn up by Edward Mores'.[42] Moreover, it was Dobson who introduced the term 'actuary' into the assurance world.[43]

The exceptional work on mortality data during the eighteenth century was the construction of the Life Table (sometimes referred to as the 'Table of Vitality' or the 'Table of Mortality').[44] The Life Table was first suggested by John Graunt and invented by Edmund Halley who constructed a life table in 1693 using the records of 5,869 deaths that occured during 1687-1691 in Breslau.[45] Two years later, Gregory King made one of the first systematic attempts to estimate the population in England and Wales. He estimated first the number of houses 'as 1.3 million and [then] the number of people as 5.5 million'.[46]

Joshua Milne's construction of the Carlisle Life Table, which came from the 'initiative of Dr. John Heysham', was an exemplary model of 'vital statistics' during this time.[47] Milne, who was an actuary for the Sun Life Assurance Company, used two sets of Heysham's observations from the parish records in Carlisle. The first set was made in 1780 and the second 'corrected version' was made in 1787.[48] David Glass considerd this to have been 'the first table, based on English data, to give a realistic view of mortality'.[49]

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41 *Ibid.*, p.121

42 *Ibid.*

43 William Farr, 'Construction of Life Tables', *Fifth Annual Report of the Registrar General* (1841), p.16.

44 *Ibid.*, p.15.

45 *Ibid.* Halley's table was published soon afterwards in his paper on 'An Estimate of the Degrees of the Mortality of Mankind drawn from Curious Tables of the Births and Funerals at the City of Breslaw; with an Attempt to Ascertain the Price of Annuities upon Lives', *Philosophical Transactions of the Royal Society*, 17 (1693), 596-612. Richard Price computed the Northampton Life Table in 1783 at the request of the Equitable Soceity from information taken from the parish registers.

46 Muriel Nissel, *People Count: A History of the General Register Office* (HMSO: London, 1987), p.49.

47 Cullen, *Statistical Movement*, p.15; Glass, *Numbering the People* (1973), p.16.

48 Eyler, *Methods of William Farr*, p.27.

49 *Ibid.* p.16. Glass also remarked that though 'the Carlisle Life Table found no real emulators and related only to data from the end of the eighteenth century,... the most striking fact was that the Carlisle table was the only one of its kind for Britain at the time'. (p.126)

THE STATISTICAL SOCIETY OF LONDON: At the end of the eighteenth century, attempts were being made to create a statistical office as an official body of the government. Two years after Sir John Sinclair produced his first *Statistical Account of Scotland* in 1791, the Government established a Board of Agriculture in England which was to collect and publish 'some useful statements relating to the state of Agriculture in each country'.[50] Nothing much seemed to come of these first attempts until 1832 when Lord Auckland and Mr. Poulett Thompson, who presided over the Board of Trade, established a Statistical Office in that department to 'collect, arrange, and publish statements relating to the condition, and bearing upon the various interest of the British Empire.'[51]

In the following summer, in 1833, a Statistical Section was created at the Third Meeting for the British Association of the Advancement of Science held at Cambridge. The object of the new section was to 'promote *statistical* inquiries... [and] to advance materially the greatly neglected subject of British statistics'.[52] Before the close of that year, the Manchester Statistical Society had been established.[53] Three months later, on 15 March 1834, the Statistical Society of London was founded by T.R. Malthus, Charles Babbage and Adolphe Quetelet.[54]

Though Adam Sedgwick, then President of the British Association, understood the word 'statistics' to 'represent mere facts to be used as the raw material of political economy and philosophy', the founders of the Statistical Society of London wanted to

> discourage the improper use of the word statistics as a mere synonym for the collection of facts [as well as to] discourage and repress all encroachments on the arena of politics as objects of party strife.[55]

The Statistical Society of London was established for the purpose of procuring, arranging, and publishing 'facts calculated to illustrate the condition and prospects of

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50 Anon, 'Introduction', *JSSL*, 1 (May 1838), p.4
51 *Ibid.*
52 Guy, 'The Original Meaning of "Statistics"', 482.
53 Anon, 'Introduction', *JSSL*, 1 (May 1838), 4.
54 Following the founding of the Statistical Society in London in 1834, other similar instititutions sprung up in Bristol, Liverpool and Ulster. Anon, 'Fourth Annual Report of the Council of the Statistical Society of London', *JSSL*, 1 (1838), 8.
55 Anon, 'Introduction', *JSSL*, 1 (May 1838), 4.

society'.[56] The first and most essential rule of its conduct was to 'exclude carefully all *opinions* from its transactions and publications—to confine its attention rigorously to facts' which could be 'stated numerically, and aranged in tables'.[57]

The statist did not 'discuss causes nor reason upon probable effects'; instead, he was to collect and compare a set of facts which alone could form the basis of sound conclusions 'to benefit a social and political government'.[58] Thus his primary job was to collect facts, though he might be expected to use a few arithmetical functions such as calculating averages and, in some cases, to 'mark their extreme values'.[59] Though he would gain an appreciation of 'the knowledge of the groups or classes to which the facts relate, the right and power of action was to rest with the State and not with the individual statist'.[60] Statistics was then defined to be

> the observations necessary to the social or moral sciences, to the sciences of the *statist*, to whom the statesman and the legislator must resort for the principles on which to legislate and govern. These sciences were equally distinct from the purely physical, the purely mathematical, and the purely metaphysical, though the mathematical must lend aid to their pursuit.[61]

In the spirit of this newly developed method, William Guy wrote that 'we largely use the true Baconian method of induction, and Lord Bacon's own favourite instrument the *Tabula inveniendi*'.[62] This tabular format, used in their presentation of statistical data, remained the primary method for British statistical publications for nearly half a century.[63] Though William Playfair had introduced graphical methods for the presentation of statistical data in 1801, his methods went largely unnoticed in England until the middle of the nineteenth century.[64] William Farr was one of the

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56 *Ibid.*, 1.
57 Guy, 'The Original Meaning of "Statistics"', 482
58 *Ibid.*, 491
59 *Ibid.*
60 *Ibid.*
61 Anon, 'Sixth Annual Report of the Council of the Statistical Society of London: Session 1839-1840', *JSSL*, 3 (April 1840), 7.
62 Guy, 'The Original Meaning of "Statistics"', 482.
63 H.G. Funkhouser, 'Historical Developments of the Graphical Representation of Statistical Data', *Osiris*, 2 (1937), 293
64 *Ibid.*, 292-4; see also William Playfair, *Statistical Breviary showing on a Principle Entirely New, the Resources of Every State and Kingdom of Europe* (London, 1901). Victor Hilts remarked that Playfair's work influenced Alexander von Humboldt and a French translation was made of

[Footnote continued on next page]

first to make extensive use of graphs, diagrams and charts in the *Journal of the Statistical Society of London*. By the end of the century, other types of graphs had been introduced by Francis Galton and Francis Ysidro Edgeworth.

THE CIVIL REGISTRATION ACT OF 1837

At the next annual meeting for the Statistical Section of the British Association in 1834, suggestions for a civil registration of deaths were gaining momentum. At this meeting Charles Babbage, who was Chairman of the Section, and Eliot Drinkwater, the Secretary, were both interested in the provision of accurate data on mortality. The committee of the medical section set up two sub-committees (one in London and the other in Edinburgh) to communicate with the statistical section 'relative to the registration of deaths, comprising particulars of a medical nature, with the view that if any legislative matter should hereafter be adopted as to registration, such suggestions may be offered by the desirable object'.[65]

During the meeting, Adolphe Quetelet described the system of civil registration in Belgium which preceded the religious ceremonies of marriage, baptism and burial. He wondered why such a system did not yet exist in England and thought that it was

> indeed a subject of wonder to every intelligent stranger, that in a country so intelligent as England, with so many illustrious persons occupied in statistical inquiries, and where the state of the population is the constant subject of public interest, the very basis on which all legislation must be grounded has never been prepared; [and] foreigners can hardly believe that such a state of things could exist in a country so wealthy, wise and great.[66]

The committee then recommended a national civil registration sytstem with a central office in London which would house duplicates of all records. The question of who should be the registrars in the new system would be left to Parliament to decide.[67]

[Footnote continued from previous page]

Playfair's work. Victor Hilts, '*Aliis exterendum*, or, the Origins of the Statistical Society of London', *Isis*, 69 (1978), 26.

65 'Report of the Fourth Meeting of the British Association, London (1835)', p.xxxix, in Glass, *Numbering the People* (1973), p.138.

66 *Ibid.*, p.127.

67 David Glass, 'Vital Registration in Britain during the Nineteenth Century', in Glass, *Numbering the People*, p.181.

Following this meeting, the Government brought in two Bills which were introduced by Lord John Russell on 12 February 1836. The first one was for amending the laws of regulating the marriages of Dissenters and the second, for a civil registration of births, marriages and deaths.[68] The Act for Civil Registration went into effect in the following year on 1 July 1837.[69] Under this act, 'the name, sex, age and occupation of every person who die[d] in England—as well as the time, place, and cause of death [were] registered'.[70] Russell then appointed his brother-in-law, T.H. Lister, as the first General Registrar who served for the first three years; George Graham, who was appointed as the second General Registrar, served for the next 37 years.[71] They were to publish 'the tabulated facts and present a report on the mortality of all the districts of England'.[72]

Edwin Chadwick urged Russell to appoint William Farr as the Compiler of Abstracts to the Registrar-General in 1839 as he 'was the only medical man who paid any attention to vital statistics'.[73] Farr thought that the most important use of the Registration Act was in collecting facts that threw

> light on the causes that affect the health of the people, and will ultimately lead to the diminution of sickness and to the extension of life. This [was] perhaps the most important use. Next to it, in interest, [was] their applications to the purposes of Life Insurance: one of the most valuable discoveries of modern times, and one of the highest branches of commerce, which may have originated in England, and has flourished to the same extent in no other country.[74]

With the Civil Registration Act of 1837, Farr could then organise tables of vital statistics on the numbers of births, deaths and marriages in England. This registration of deaths was to provide Farr with the material to develop a comprehensive nosology

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68 Marriage statistics still came from the parish thus the quality of information recorded depended upon the clergy; marriage statistics were still far less satisfactory than statistics of births and deaths. Glass, *Numbering the People*, p.129.

69 Cullen, *The Statistical Movement*, p.29

70 William Farr, 'Influence of Elevation on the Fatality of Cholera', *JSSL*, 25 (1852), 153.

71 [T.H. Lister], *First to Third Annual Reports of the Registrar General* (HMSO: London, 1838-1840); [George Graham] *Fourth to Thirty-First Annual Reports of the Registrar General* (HMSO: London, 1840-1868).

72 [Lister], *First Annual Report of the Registrar General*, p.155.

73 David V. Glass, 'Two Letters from the Chadwick Collection and a Correspondence between Chadwick and Farr', in Glass, *Numbering the People*, p.149.

74 William Farr, 'Letter to the Registrar-General', *Eighth Annual Report of the Regisrar-General* (1845), p.277.

listing secondary and tertiary causes of death which was to be beneficial to the medical profession and the assurance societies.

His *British Annals of Medicine* was the first medical journal to use vital statistics, and its purpose was to extend to 'all the laws of vitality capable of being observed in masses of men, expressible in numbers' which included state medicine, public health, insurance, the census and civil registration'.[75] Much of Farr's work at the General Register's Office involved using various applications of the National Life Table as 'without an intimate knowledge of their properties it [was] impossible to determine the laws of population, which are the basis of statistics'.[76]

In addition to this work on vital statistics, Farr also developed a series of 'statistical laws' used especially for his work on cholera (where he introduced such laws as his 'Law of Recovery', 'Law of Mortality' and the 'Law of Epidemics'.) Farr was probably the first person to describe an epidemic numerically by extending the actuary's technique of discovering the law of mortality in a life table.[77]

By the time of the cholera epidemic of 1848, the Act for the Registration of Births, Deaths and Marriages had been established. The quarterly abstracts and the London weekly tables of deaths, printed the rise, progress and decline of death from cholera in various districts. Farr wanted to present an account of what was for him 'one of the most important practical results of inquiry [on mortality from] cholera of all of the districts in England'.[78]

He was interested in 'the influence of locality, particularly of elevation of habituation on the diseases and characters of men'; he wanted to determine if there was a relationship between the influence of locality and the incidence of cholera in London. Through various calculations, Farr discovered that the rate of cholera was more prevalent on the coast than it was inland. He calculated that it was three times

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75 William Farr, 'The State of Medical Science', *British Annals of Medicine*, 1 (1837), 25.
76 William Farr, 'Sixth Letter to the Registrar General', *Annual Report to the Registrar General* (1842), p.524.
77 Eyler, *Methods of William Farr*, p.14.
78 Farr, 'Influence of Elevation on the Fatality of Cholera', *JSSL*, 25 (1852), 155.

more fatal on the coast where 50 in 10,000 died and inland the ratio was 17 per 10,000. He found that

> Cholera reigned wherever it found a dense population on the low alluvial soils of rivers, round the estuaries of the Thames, the Humber, the Mersey, the Severn, the Tamar and their tributary waters [and that] cholera attacked the [the] greater part of the districts on the coal-fields of England and was most fatal in the denser masses of the mining population.[79]

From his findings, he decided that the elevation of soil in London had a more constant relation with the mortality from cholera than any other known element as

> the mortality from cholera [was] the inverse ratio of the elevation; [moreover], cholera was excessively fatal for all the four districts which lie[d] on the level with or below the Trinity high water marks.[80]

After he arranged the districts 'by groups in order of their altitude', he concluded that the elevation of soil had 'certain constant relations' to mortality from cholera in London. He found that the rate of mortality at 100 feet was 17 in 10,000 whilst at 50 feet, it was 34 in 10,000. Farr then constructed one of his post-hoc axiomatic ratios to determine if the mortality from cholera were inversely related to the level of elevation. Farr thought that the relation of elevation of soil to mortality from cholera should produce this proportion:

$$e : e' :: c' : c = \frac{e'}{e}(c') \qquad (1.1)$$

			Farr's results
e	=	the elevation within the observed limits of 0 and 350 feet	= 50
c	=	the average rate of mortality from cholera at that elevation	= 34
e	=	any higher elevation	= 100
c	=	mortality at that elevation	= 68

Farr's data would work out to:

$$50 : 100 :: 34 : 68 = \frac{100}{50}(34) = \qquad (1.2)$$

From these levels of mortaltity, Farr went on to say that 'at half the elevation the mortality is doubled'. Farr concluded that the:

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79 *Ibid.*, 156-158.
80 *Ibid.*, 160.

> houses necessarily raise the people of London above the ground; and if their *habitat*, day and night, is on average 13 feet above the ground level,it is evident that the mortality *within the limits observed, is in the inverse ratio of the elevation at which the people live.*[81]

To put it more simply, a decrease in the elevation of houses should have been followed by an increase in mortality rate.

Farr's statistical laws tended to be formulated by using post-hoc axiomatic ratios . Though Farr seems to imply that this statistical law could not be generalised to other parts of London or England when he says that, 'the relation may not be expressed by the same figures in other places, or in London at other times', he suggests notions of causation when he goes on to say, 'but it will always be the general rule that the mortality of cholera is inversely related as the elevation of the people assailed above the sea level'.[82] Thus, however the figures may differ, he thought that his results would indicate this inverse relationship. From this, he went on to deduce a law of mortality that '*towns of some extent and density* is inversely [related to] the elevation'.[83] In other words, in densely populated towns, mortality from cholera was also related inversely to elevation of soil.

The results of Farr's statistical laws could have changed, however, if he had a different set of data (i.e., from other parts of the country). With a change in population, mortality might have 'tripled' rather than doubled at half the elevation or, indeed, could have been identical to the level of elevation. If this were the case, he might have found higher or lower rates of mortality. There may have also been other contributory factors that could indicate mathematical relationships between mortality from cholera and elevation of soil.

Nonetheless, there is, indeed, a relationship between water and mortality from cholera as John Snow demonstrated soon after Farr did this work. Snow found that

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81 *Ibid.*

82 *Ibid.*, 161. Near the end of the paper, Farr wrote that the four great pestilential diseases—cholera, yellow fever, remittent fever, and plague—have this in common; that they begin and are most fatal on low ground; that their fatality diminishes in ascending the rivers, and is inconsiderable around the river sources... Safety is found in flights to the hills and to the desert, or in the removal of ships from infected ports to the open sea. (p.170).

83 *Ibid.*, 162.

there was a high correlation between mortality from cholera with the *pollution* of water supplied.[84] The problem lay with contaminated waters containing the bacteria *vibrio cholera* in the water pumps (affected by water supply), rather than with the elevation of soil. In the *Fourteenth Annual Report of the Registrar General*, George Graham not only reported Farr's results from the rate of mortality by cholera, but he also urged that

> the precautions to take against cholera, in regard to *water*, are well stated by Dr. Snow; and they are of so simple a nature that, considering all the facts, no person can prudently neglect them. *Water into which sewers flow, or from which is navigated by persons living in boats or which is in any other way contaminated by the contents of drains or cesspools should be entirely disued.*[85]

Though Farr acknowledged Snow's work, he never relinquished his own belief that the rate of mortality from cholera was inversely related to elevation of soil. Two years after he wrote his paper on the 'Influence of Elevation on the Fatality of Cholera', he acknowledged Snow's work in his Seventeenth Letter to the Registrar-General, when he wrote:

> It is right to state that Dr. Snow by his hypothesis, by his researches, and by his personal inquiries; that the Registrar General, by procuring information and by prompting inquiry; as well as the Board of Health by their Report; have all contributed in various ways to establish the fact, that the cholera matter or *cholerine*, where it is not fatal, is largely diffused through *water*, as well as through other channels.[86]

In light of Snow's findings, Farr revised his law to indicate that that elevation did not act directly on individuals, rather, elevation was to have helped determine the

> distribution of cholera flux, fecal matter of cholera victims containing cholera. This material, being heavier than water, distributed itself in water suspension most densely at the bottom of a test tube and less densely at the top. It would therefore be found most concentrated in water pipes at low elevation and least concentrated at higher elevation.[87]

Farr, however, did not seem to think that water was the primary cause of the propagation of cholera, and in 1868 he reintroduced his law on the elevation of soil.[88]

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84 Eyler, *Methods of William Farr* (1979), p.116.
85 William Farr, *Sixteenth Annual Report to the Registrar-General* (1853), p.38
86 William Farr, *Fourteenth Report to the Registrar-General* (1853), pp.95-6.
87 *Ibid.*, pp.288-9.
88 William Farr, *The Lancet* (1868, II), 223, as quoted in John M. Eyler, 'William Farr: An Intellectual Biography' (Ph.D. thesis, University of Wisconsin, 1971), p.287.

Six years before Farr's retirement in 1880, a newer form of statistics was beginning to emerge. Francis Galton had introduced some of his statistical ideas at a lecture he gave to the Royal Institution on 27 February 1874.[89] Galton wanted to create a standardised scale that would be based on the *law of frequency error* (i.e., the normal distribution) as it would then be possible to make statistical *intercomparisons*. This newer method was considered necessary because

> at present, we are accustomed to deal with averages and the like, which can only be obtained by measuring *every* individual by a *detached standard scale*, and going through an arithmetical process afterwards.[90]

After Galton established that standardised comparisons could be made by using the law of frequency, he then wanted to find a way to measure statistical variation. He devised a statistical system that would give an approximate measure of variation at some points in the distribution. Galton proposed a series of percentile measures and determined that the graduations of +2°, +1°, 0°, -1°, -2° in a row of one hundred individuals, the percentages would be -2, 9, 25, 50, 75, 91. His method did not require 'measuring [subjects] individually as this [was] out of the question'.[91] Galton chose to measure stature which enabled him to arrange the person by height, from tallest to shortest, and then pick out

> two men and two only—the one as near to the middle as may be, and the other near the quarter point and I measure them at leisure. The height of the first man is the average [the median] of the whole series, and the difference between him and the other man gives the probable error.[92]

Several months later, he used these methods to measure the heights of boys at Marlborough School for 'the purpose of intercomparison, on the growth of Englishmen living under different conditions of town and country and, belonging to different ranks of society'.[93] At the end of the year, he determined that q_1 = the 25th

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89 Galton, 'On a Proposed Statistical Scale', *Nature*, 9 (5 March 1874), 342
90 *Ibid.*
91 *Ibid.*, 343.
92 *Ibid.*
93 Idem, 'Note on the Marlborough School Statistics", *Journal of the Anthropological Institute of Great Britain and Ireland*, 4 (1874), 130.

percentile, q_2 = the 50th percentile or the median and q_3 = the 75th percentile: these percentiles would be used later for his semi-interquartile range.[94]

Two years later, at the summer meeting of the British Association for the Advancement of Science in 1877, Galton was criticising their use of statistics for lacking rigour and for showing little concern for mathematical theory. Differences between Farr and Galton were reflected in the Reports for the Anthropometric Committee when the Chairmanship changed from Farr to Galton in 1880. Under Farr's direction, the committee had collected and presented physical measurments of various occupational and social groups by using averages. In Galton's first report, he introduced the median, probable error and his 'law of frequency error curve'. Though Farr never spoke explicitly about using distributions to examine his data, his Life Tables were probably his 'most valuable single instrument of social-medical research'.[95]

By the end of the nineteenth century, with the machinery of civil registration at hand, Karl Pearson had begun to use these mortality tables. He was to discuss their importance for studies of evolution in his lecture on the 'Chances of Death' delivered to the Leeds Philosophical Society in 1894 and this was to become one of his first treastise on probability theory.[96] Actuarial death rates would become one of the two principal tools employed in the Eugenics Laboratory in the early twentieth century (the other would be the use of family pedigrees).

When a new set of statistical problems occurred with Weldon's work on the identification of species and on 'divergence' of species (what would be termed today speciation) in 1892, arising from his concerns of the statistical interpretation of his 'double-humped curve' (i.e., a bimodal distribution), he found that Galton's statistical

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94 Francis Galton, 'Statistics by Intercomparison, with Remark on the Law of Frequency Error', *Philosophical Magazine*, 49, No. 322 (January 1875), 33-46.

95 For a discussion of the role of statistical distributions in Farr's work see John M. Eyler, 'William Farr: An Intellectual Biography' (Ph.D. thesis, University of Wisconsin, 1971), p.25; For a discussion of Farr's 'Life Tables', see Major Greenwood, *Some British Pioneers*, p.79.

96 Karl Pearson, 'Chances of Death' in Karl Pearson, *Chances of Death and other Studies of Evolution* (Edward Arnold: London, 1897), pp.1-4.

methods, which he first used in 1889, could not be used to analyse his data. Galton had devised his statistical methods on the assumption that all biological data would conform to the normal distribution whereas Weldon found that his distribution on the frontal breadth of carapace of the female shore crab in Naples was not normally distributed. Pearson's resolution of Weldon's asymmetrical distributions will be examined in Chapters III, IV and VI. It is argued that this work was to lead to the emergence of the Pearsonian paradigm of statistics and to the development of a modern theory of statistics.

Chapter II

THE PEARSONIAN PARADIGM

> The book of the universe is written in mathematical language, without which one wanders in vain through a dark labyrinth. (Galileo Galilei)

Over a period of eight years, Karl Pearson was to develop and construct a statistical system underpinned by his *method of moments* for curve fitting of symmetrical and asymmetrical distributions. This would also provide the basis for the development of an empirical measure of a goodness-of-fit test (which came to fruition in 1900 when he devised his chi-square (χ^2, P) goodness-of-fit test). The method of moments was devised in 1892 initially to help W.F.R. Weldon, with the analysis of his 'double-humped' data on the frontal breadth of the carapace of the female crab in Naples. Pearson incorporated the relative frequency approach to probability (in 1893) and introduced determinantal matrix algebra (in 1896) into statistical theory. During this time he also introduced new mathematical notations and a new vernacular of statistics in his development of a modern theory of statistics. His most innovative, creative and seminal statistical work occurred from 1892 to 1900.

It will be shown that Weldon's influence, which exceeded that of any other person in the emergence and development of Pearsonian statistics, can be detected in the following ways: (1) by providing the stimulus for new statistical methods by asking Pearson biological and statistical questions that could be answered only by devising a new statistical approach, (2) offering continual moral support as well as providing encouragement for Pearson and (3) by promulgating the Pearsonian statistical methodology (to 'serious students of animal evolution') throughout the 1890s and until his death in 1906.

Pearson regarded Weldon as 'one of the closest friends he ever had': their relationship could be characterised by an emotional and intellectual intimacy. After Weldon's death in 1906, Pearson seemed to have lost his creative drive. In the

remaining thirty years of his life, he would continue to refine his statistical methods and, with Mrs. Weldon's assistance, publish Weldon's work on Mendelian inheritance.

Whilst it will be shown in Chapters III and IV how Weldon assumed this priority in the emergence, development, and construction of Pearsonian statistics, it will also be shown that he was not the only influence. Indirectly, a fundamental influence is derived from Charles Darwin's evolutionary ideas of biological variation, statistical populations of species and from his theory of natural selection as this was to provide the context within which Pearson's statistical theory emerged. His early theoretical work on probability which was influenced by John Venn and Francis Ysidro Edgeworth will be discussed in Chapter VI. His mathematical tutor at Cambridge, William Herrick Macaulay and one of Pearson's first students of mathematics and mechanics, the mathematician Louis Napoleon George Filon, provided Pearson with much assistance with theoretical mathematics.[1]

A number of his students and co-workers in the Biometric Laboratory would supply him with a considerable range of data and calculations which extended the scope of application for his statistical methods. Some of his students who would offer the greatest assistance include the following: Julia Bell, F. Howard Collins, William Palin and Ethel Elderton, L.N.G. Filon, Major Greenwood, David Heron and Alice Lee. George Udny Yule would help to create an interest in contingency tables with his measures of association and quite separately he was to introduce the method of least squares to interpret regression.

Francis Galton, who was undoubtedly important to Pearson, took on a different role from that of Weldon. Though he was enthusiastic about Pearson's work and would recommend Pearson's papers to the Royal Society, he never felt 'competent enough to give a useful opinion on the soundness of Karl Pearson's paper'.[2] In

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1 Macaulay helped Pearson with his first set of Gresham lectures and with material in the first edition of *The Grammar of Science*. The correspondence between Pearson and Macaulay is fairly extensive with approximately 500 letters extant in Pearson's archive.

2 Francis Galton, 'Referee Report to the Royal Society: (RR.12.22), the Library of the Royal Society (22 October 1893).

Pearson's first extant letter to Galton, he asked him if he could give a lecture on probability at Gresham College (where Pearson held the chair of Geometry). On 28 February 1893 he wrote to Galton:

> Dear Sir,
>
> I fear you may think me rather unjustified in writing to ask a favour of you. I am unable owing to the pressing need of a real holiday to give my Easter lectures at Gresham College and shall have to give them by deputy. This session's lectures have been entirely on the Laws of Chance and their application, and the four Easter lectures were to have been on the geometrical aspect of chance, i.e. on probability curves and their applications. The audience is a popular, but appreciative one and amounts to some three hundred souls—as I cannot lecture myself, and I am very desirous that the lectures should be more or less continuous, it has struck me that lectures by specialists on the applications of chance to various branches of science would be of great interest and value to my audience. My colleague Professor Weldon has consented to give one of the four lectures, on some topic illustrating the laws of chance from biology. Is it possible that you would be induced to give another illustrating its relations to anthropology? The honorarium (£7.7) can only be looked upon as help to illustrating the lecture, should you care to do so... I can promise you an appreciative, if popular audience. The lectures have at any rate the historical interest of having gone on continuously since the days when Robert Hooke and Christopher Wren gave a real lustre to Gresham College. I am, dear sir,
>
> Yours faithfully,
>
> Karl Pearson[3]

Galton did not give the lecture. Whilst Weldon gave the first lecture, the three other lectures were given by John Venn, the Reverend W.A. Whitworth and Sir Robert S. Ball.[4] In the twentieth century, when Galton set up the Eugenics Record office and wanted to employ research workers 'trained in the Biometric Laboratory or in its statistical methods', he became a benefactor to Pearson and would endow him with the Galton Chair of Eugenics in 1911.

In Chapter V, it will be shown that Galton's contribution to the corpus of Pearsonian statistics arose from his work on his Law of Ancestral Inheritance. Pearson's statistical resolution of Galton's law led to his work on multiple correlation and multiple regression. These developments, in tandem with Weldon's interests in natural selection, were instrumental in Pearson's introduction of determinantal matrix

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3 Karl Pearson, Letter to Francis Galton, 28 February 1893, Papers and Correspondence of Francis Galton, University College London [hereinafter referred to as FG:UCL].

4 [Karl Pearson], 'A Course of Lectures on Special Applications of the Laws of Chance' (Gresham College, Basinghall Street, London, E.C.), Easter Term 1893, KP:UCL.

algebra. Pearson would then attempt to provide a synthetic mathematical model of evolutionary biology. Pearson's most comprehensive and seminal work, which involved the determination of a goodness of fit test, will be examined in Chapter VI. It will be shown that this work spanned the whole of Pearson's statistical career—beginning with one of his first Gresham lectures in November 1892 and ending with the last statistical paper he wrote in 1936 (published posthumously two months after his death).

STATISTICAL POPULATIONS

At the end of the nineteenth century, statistical theory was to undergo a series of transitions that would lead to its emergence as a highly specialised discipline. These transitions were brought about by a mathematical-statistical translation of the Darwinian change in ideas about what kind of natural processes occur in the world. It will be argued that these transitions were engendered by three problems within evolutionary biology: the first involved looking for a criterion for the identification of species (which I have termed the 'biometrical species criterion'); the second, in searching for an empirical method to explain the process of speciation and the third, in developing a quantitative system to detect empirical evidence of Charles Darwin's theory of natural selection.

Though Charles Darwin was one of the first to break away from essentialism, Pearson's statistical ideas would help to bring about an epistemological shift from the pre-Darwinian essentialistic view of species to what Ernst Mayr has termed Darwinian 'population thinking'.[5] This outlook differed from essentialism as 'population thinking stresses the uniqueness of everything in the organic world... there is no "typical" individual and mean values are abstractions'.[6] Essentialists had great difficulty in understanding that selection is a statistical process rather than an all-or-nothing

5 Ernst Mayr, *The Growth of Biological Thought: Diversity, Evolution and Inheritance* (Belknap Press of Harvard University: Cambridge, Mass., 1982), pp. 45-7.

6 *Ibid.*, p.46.

phenomenon.[7] With this Darwinian shift, species could be thought of as different individuals rather than as 'types' which implied the necessity for a detailed study of variation: this was to lead to a re-conceptualisation of statistical populations and would require the use of new statistical methods. Thus, Sewall Wright would remark in 1934 that 'a statistical process was indeed necessary to bring the new species into predominance'.[8]

Aristotelian essentialism, which was the dominant thinking of the taxonomists, typologists and morphologists until the end of the nineteenth century, gave rise to the morphological species concept.[9] Species were thought to have been based on 'essences' of an ideal type. This type of thinking underpinned the deterministic outlook of Adolphe Quetelet whose *I'homme moyen* symbolised the ideal statistical type (or arithmetic mean). For Quetelet, deviations from the ideal mean were flawed and thus seen as a source of 'error'. Such an outlook was to render problematic the search for the ideal type in the use of statistics and in the identification of species.

Though Francis Galton treated Quetelet's unwanted deviations as *variation,* and devised statistical methods to measure this variation, it may be suggested that his use of statistics retained a component of essentialism. Galton's regression to the mean may be seen as an attempt to normalise distributions that would sustain the ideal mean (or type). When considering Galton's use of regression, Peter Bowler remarked that

> Galton himself believed that the mean value of the characteristic for the sample would regress back towards the species as a whole... Regression provides a limit to variation, always pulling a characteristic back towards the mean, despite the effects of selection. Selection can improve the health of a population, but cannot permanently improve the breed. Galton believed that species normally remain static for long periods of time and evolve by sudden mutations only when confronted by a major environmental challenge.[10]

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7 *Ibid.*, p.568.

8 Sewall Wright, 'Evolution and Mendelian Populations', *Genetics*, 16 (1934), 98.

9 Carl Linnaeus, who introduced the binomial system of nomenclature for the naming of species of plants and animals, also embraced the typological concept of species. See William Thomas Stearn 'Carl Linneaus', *A Biographical Dictionary of Scientists* (Adam and Charles Black: London, 1969), pp.331-2.

10 Peter Bowler, *Evolution: The History of an Idea* (University of California Press: Berkeley, 1982), p.240.

Thus Galton believed that his regression would prevent selection from having a permanent effect on a species.

W.F.R. Weldon was to encourage a move away from the essentialistic concept of species when he found that the morphological and embryological methods for which he had been trained at Cambridge, could not resolve some of the problems he encountered when he tried to interpret some of the data he collected on the carapace of female crabs at Naples. He began first by using some of Galton's statistical methods, and then enlisted the mathematical assistance of Karl Pearson when Galton's methods did not produce any resolution to his problems.

Both Pearson and Weldon were adaptationists and committed selectionists; thus, they understood the mechanism of evolution as Darwin himself had (and were among the very few who had achieved this understanding at the end of the nineteenth century). It may thus be argued that Pearson and Weldon not only understood Darwin's theory of natural selection, but they also came close to creating a truly professional study of the mechanism of evolutionary biology in the late nineteenth century. Pearson achieved this, to some degree, by providing a mathematical system that would incorporate Darwinian population thinking and could cope with the non-normality of distributions that arose when attempts were made to determine how divergence in species (i.e. speciation) occurred, and also when trying to detect empirical evidence of natural selection by detecting disturbances (or deviations) in distributions from normality as a consequence of differential mortality rates.

The biometrical species criterion and speciation dominated much of their statistical work from 1889-1903. After some attempts had been made to deal with these problems, they became concerned with finding empirical evidence of natural selection (from 1894-1904). Statistical issues relating to studies of inheritance became of interest at a later date beginning in 1896. After 1900 much of their work was directed towards an understanding of Mendelism which led to a shift from the analysis of continuous variables to one that would incorporate the statistical analysis of discrete variables. Hence, although Pearson and Weldon viewed natural selection as

acting upon continuous variation, they considered that both continuous *and* discontinuous variation were features of inheritance.

After Pearson and Weldon had been engaged in eight years of intensely close collaborative and fruitful work, that spanned the last decade of the nineteenth century, Weldon was to leave University College London to take up his new post at Merton College, Oxford: he had been appointed to the Linacre Chair of Comparative Anatomy (succeeding E. Ray Lankester).[11] Whilst their collaborative efforts began in 1892, by 1900, with the introduction of the chi-square goodness of fit test, their mutual interests had led to the creation and development of the Pearsonian corpus of a modern theory of mathematical statistics.

Pearson read about Weldon's appointment in the newspaper, learning of the news on 28 February 1899, he sent this letter to him:

> My dear Weldon
>
> I must write and congratulate you, for I know the news I see in the paper will be a great source of pleasure to your life and you. My own personal loss is a small matter (even if you could grasp it) compared with what the loss will be to the College and as to the future of higher university work in London. I cannot say what it is. There was no definition of purpose or clear headed views of action in the bulk of our colleagues before in regard to future changes. Now I despair of the state.
>
> What vexes me just now most is that this break down leaves me little chance of grasping the few lunch hours which remain, and when you reach Oxford—Amen!
>
> I am afraid our friendship at Univ. Coll. has been a very one-sided one, I have received all and given little. Perhaps that is the reason I shall be punished more at your removal. *At any rate, you have changed the whole drift of my work and left a far deeper impression on my life than I on yours.* So that the change is greater to me than you can realise from your side. Well, old man, best luck to you in your new environment, and may it give you as great a leisure for new development as Australia has given the British rabbit. What a blessing to have no faculties, no senate, no council, to have fresh air and freedom to work out the best in you! (Italics mine.)
>
> Affectionately yours,
>
> Karl Pearson
>
> Don't answer this.[12]

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11 Weldon was also Lankester's successor in the Jodrell Chair of Zoology at University College London in 1891.

12 Karl Pearson, Letter to W.F.R. Weldon, 28 February 1899, KP:UCL.

Weldon honoured Pearson's request by not answering the letter. He did, however, have the chance to express some of his feelings some six months before when they had both discussed the possibility of Weldon leaving for Oxford. The matter had arisen soon after E. Ray Lankester left Oxford to take up his new post as Director of the British Museum in South Kensington in August of 1898.[13] Pearson first considered the possibility of losing Weldon to Oxford when he wrote to Weldon on a matter 'that weight[ed] at present most in [his] mind' as he supposed that with

> Lankester in London, you will go to Oxford. That is the worst aspect to me of his appointment. It will be chaos at U.C. and a personal loss to me that I won't describe for fear of its effect on your vanity. You may point your finger at my own attempt, but then I was seeking flight from the influenza and engineering students![14]

Four days later, Weldon wrote to say,

> The kindness of your letter has touched me very deeply. —I shall be sorry to lose you, if I have the chance of going to Oxford, but for every other reason I shall be so glad!
>
> To be in the country, with clean air to breathe, live animals to help one to ideas, and students who will listen to what one says without referring to our examination schedule! —But these things are not yet, and may never be.[15]

In the following year, Weldon left London to begin a new life at Oxford.

A decade after Weldon had first begun to use statistics to look for empirical evidence of natural selection, he expressed this view of the older school of morphologists:

> The starting point of Darwin's theory of evolution is precisely the existence of those differences between individual members of a race or a species which morphologists for the most part rightly neglect. The first condition necessary, in order that any process of Natural Selection may begin among a race, or species, is the existence of differences among its members; and the first step in an enquiry into the possible effect of a selective process upon any character of a race must be an estimate of the frequency with which individuals, exhibiting any degree of abnormality with respect to that character, occur. The unit, with which such an enquiry must deal, is not an

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13 E.S. Goodrich, 'Sir Edwin Ray Lankester', *Dictionary of National Biography* (1922-1930).

14 Karl Pearson, Letter to W.F.R. Weldon, 4 August 1898, KP:UCL.

15 W.F.R. Weldon, Letter to Karl Pearson, 7 August 1898, KP:UCL. For all of these advantages that Weldon perceived Oxford to have over UCL, he found later that UCL had certain advantages over Oxford—at least UCL seemed to be running things more efficiently than Oxford. On 3 October 1899, Weldon wrote to Pearson on the 'solemn frivolity of Oxford' that 'it takes Oxford one week to do what would have been done in an hour in Gower Street. A committee met three times last term, for no other reason than because the chairman had promised to take a lady down the river at the hour of the first meeting, and had called a garden party or something of the kind at the hour of the second meeting'.

> individual but a race, or a statistically representative sample of a race; and the result must take the form of a numerical statement, showing the relative frequency with which various kinds of individuals composing the race occur.[16]

Though Galton had wanted to devise an empirical measure for the identification of species, he was, nonetheless, looking for the ideal type (much as the morphologists had been looking for an underlying essence). Weldon, who was enthusiastic about using Galton's method, did not expect to find an underlying essence that would represent the ideal type. Pearson, however, not only shared Weldon's views, but he would go on to create a new statistical methodology to assist Weldon in his pursuits in looking for the biometrical species criterion and in attempting to understand the process of speciation.

PEARSON'S EARLY MATHEMATICAL WORK (1873-1891)

Karl Pearson went up to Cambridge in 1873 when he was seventeen years old and received private tuition from a Senior Wrangler (at Trinity College who had been his private tutor when he withdrew from University College London School at the age of 15 for health reasons). In the following year, whilst preparing for the degree of mathematics, he read with Edward John Routh who was considered to have been, perhaps, 'the greatest mathematical coach that Cambridge ever had'.[17] Pearson received an open scholarship from King's College in 1875.

When Pearson took the Smith Prize three years later, his examiners included Professor Stokes, James Clerk Maxwell, Sir Arthur Cayley and Sir Isaac Todhunter. Though he did not become a Prizeman, one of his answers was to make a 'turning point in [his] career' as some six years later, the Syndics of Cambridge University Press asked Pearson to finish Todhunter's work on *The History and Theory of*

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16 W.F.R. Weldon, 'Editorial (1.) The Scope of *Biometrika*', *Biometrika*, 1 (1901), 1-2.

17 Edward J. Routh, 'Testimonial for Karl Pearson' in Karl Pearson, 'To the members of the Gresham Committee (City Side)', (11 November 1890), KP:UCL, p.13. From 1861-1882, Routh had 700 pupils of which 500 became wranglers and of this group, 22 became Senior Wranglers. Raymond Pearl, 'Karl Pearson', *American Statistical Association Journal* (1936), 365.

Elasticity and Strength of Materials.[18] He took his degree in 1879 with Mathematical Honours being the Third Wrangler in the Tripos that year. After finishing he left immediately for Germany and studied in the Universities of Heidelberg and Berlin.[19] In Heidelberg, he studied Roman Law under Bruns and Mommsen and attended the lectures of Du Bois Reymond on Darwinism.[20]

On returning to Cambridge from Heidelberg, he was elected to a Fellowship at King's College; he declined a College mathematical lectureship as he 'wished to keep [his] time free for more genial work'.[21] Soon afterwards, he took rooms at the Inner Temple and read law at Lincoln's Inn where he took the degrees of LL.B. and M.A. During this time, he gave popular lectures to the *London Society for the Extension of University Teaching* at its Blackheath, Barnes and Hampstead Centres and in 1883 he 'undertook for two terms the senior mathematical teaching at King's College, London as deputy for...Professor Drew'.[22] In the following year he was elected to his post in the chair of 'Mechanism and Applied Mathematics' succeeding Professor Olaus Henrici (whose previous successor was William Kingdom Clifford).[23]

From 1884 to 1890 he taught mathematics to engineering students. (George Udny Yule and Louis Napoleon George Filon, his successor to the chair in 1911, were among Pearson's first students.) Pearson's work included annual courses on Modern Geometry and Geometrical Drawings; he lectured on statics, dynamics and mechanics

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18 Karl Pearson, 'Old Tripos Days at Cambridge, as seen from another Viewpoint', *Mathematical Gazette*, 20 (1936), 31. Isaac Todhunter, *A History of the Theory of Elasticity and of the Strength of Materials from Galilei to the Present Time*, ed. and completed by Karl Pearson (Cambridge University Press, 1886).

19 Karl Pearson, 'To the members of the Gresham Committee (City Side)', 18 November 1890, KP:UCL, p.5.

20 [Karl Pearson] *Speeches delivered at a dinner held in University College London in Honour of Professor Karl Pearson 23 April 1934* (Cambridge University Press, 1934), p.20.

21 Karl Pearson, '[Application for] Professorship of Applied Mathematics at the Owen College, Manchester', 12 May 1888, KP:UCL.

22 Pearson, 'To the members of the Gresham Committee (City Side)', 18 November 1890, KP:UCL, p.5.

23 *Ibid.*; Upon Pearson's appointment to University College London, he was asked by Messrs Kegan Paul, Trench and Co. 'to take up the task of editing' the incomplete work of Clifford who died in 1879 (which Pearson completed in 1885). William Kingdom Clifford, *The Common Sense of Exact Sciences*, ed. and completed by Karl Pearson, 3rd edn. (Kegan Paul, Trench, Trübner and Co.: London, 1892), p.vi.

which had been based on geometrical and graphical methods.[24] Nearly all of his work up to this time dealt with pure and applied mathematics.[25] One of these papers was to provide the foundation for Pearson's statistical theory when, in 1890, he published a 'Note on Clapeyron's Theorem of the Three Moments' in the *Messenger of Mathematics*.[26] Pearson introduced Clapeyron's Theorem in his Gresham lecture in February 1891 which he would use in the following year in his development of the *method of moments*.[27]

Pearson's earliest interest in some form of statistics can be seen in his application for the Gresham Lectureship in Geometry when, on 18 November 1890, he proposed that

> Should I have the honour to be elected Gresham Lecturer, I think this experience would be of service in appealing to the class of audience which is likely to be attracted to Gresham College. I believe that, by legitimate interpretation of the wide meaning of the word *Geometry* as used in Sir Thomas Gresham's time for one of the seven branches of knowledge, courses of lectures on the elements of exact science, on the geometry of motion, on graphical statistics, on the theory of probability and insurance might be given, in addition to purely geometrical courses, which would supply a want felt by clerks and others engaged during the day in the City.[28]

By the beginning of 1891, Pearson began to teach graphical statistics and the mathematical theory of probability with a particular interest in actuarial methods. Pearson had thus developed a general interest in statistics before meeting Weldon later that year. In February of the following year, he published the first of his writings on statistics from a series of his Gresham lectures in a chapter on 'Cause and Effect—Probability' in the *Grammar of Science*.[29] Although Pearson read Francis Galton's *Natural Inheritance* in 1889, Galton's statistical ideas on correlation were not used in

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24 *Ibid.*, p.6.

25 By 1891 Pearson had published 17 papers and three books on matters relating to pure and applied mathematics and physical sciences. G.M. Morant, *A Bibliography of the Statistical and other Writings of Karl Pearson* (Cambridge University Press: Cambridge, 1939).

26 Karl Pearson, 'Note on Clapeyron's Theorem of the Three Moments', *The Messenger of Mathematics*, 19 (1890), 129-35.

27 Karl Pearson, 'Syllabus of a Course of Lectures in Higher Graphics. Part II. The Graphical Theory of Elasticity' (February 1891), KP:UCL, p.3.

28 Egon Pearson, 'Karl Pearson: An Appreciation of some Aspects of his Life and Work. Part I 1857-1906', *Biometrika*, 28 (1936), 213.

29 Pearson, *The Grammar of Science*, 1st edn. (Walter Scott: London, 1892).

the first edition of the *Grammar of Science;* this edition was based primarily on Pearson's Gresham lectures on theoretical mechanics and physics.

In the second edition of the *Grammar of Science,* Pearson included a section on 'The Classification of the Sciences' which included the 'abstract sciences' that dealt with quantity which could be

> either *discrete* and definite, like the numbers of arithmetic 8,100, $^1/_3$, $^{17}/_{41}$, etc., the number of inhabitants of a town, the number of cubic feet in a room; or it may be *continuous* and changing with other quantities—for example, like the height of the barometer with the hour of the day, the stature or weight of a man with his age, the position or speed of a body with the time.[30]

After making distinctions between 'discrete' and 'continuous' levels of measurement, the amount of error which could arise in measurement was considered next. This was followed by a 'discussion of probable and average quantities'. From this arose, 'the *Theory of Measurement, Theory of Errors, Theory of Probability, Theory of Statistics, etc.*'[31] In this edition, he incorporated a very lengthy section on evolutionary biology and emphasised his work with Weldon on selective death rates and natural selection. It is in this section on Evolution that Pearson first introduced correlation into the *Grammar of Science* when he wrote that 'correlation [made it possible] to reconstruct from one organ the probable value of the second, or... from any number of known organs'. In the third edition of the *Grammar* in 1911, Pearson examined correlation in relation to contingency and its coefficient C as a measure of association. As regards sampling theory in the role of cause and effect, he wrote that the

> theory of samples—their probable errors and their legitimate use—is the chief topic of modern scientific statistics... it is at the basis of the wider conception of association, which is surely replacing the old limited idea of cause and effect.[32]

Pearson does not seem to have incorporated Galton's statistical ideas until 5 June 1895, six years after he read *Natural Inheritance,* when he gave a lecture to some of his students at UCL on 'Notes on Progression and Inheritance in the case of two

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30 Karl Pearson, *The Grammar of Science*, 2nd edn (Charles Black: London, 1900), p.515.

31 *Ibid.*, p. 516. See chapters IX-XI in the second edition of *The Grammar.*

32 Pearson, *The Grammar of Science*, 3rd edn (Adam and Charles Black: London, 1911), p.161. Pearson then went on to say that 'whether phenomena are qualitative or quantitative, a classification leads to a contingency table, and from such a table we can measure the degree of dependence between any two phenomena', p.177.

Parents'.[33] On 28 September, he lectured on 'Regression, Heredity and Panmixia, which came to fruition in the third of Pearson's statistical papers on the 'Mathematical Contributions to the theory of Evolution' in 1896.[34] Thus, five years after Pearson's first Gresham Lecture on 'Graphical Statistics' and three years after his first statistical paper in his series on 'The Mathematical Contributions to the Theory of Evolution', Galton's ideas on correlation and regression become a part of the Pearsonian corpus of statistics. Thus, there exists a relatively limited place for Galton's statistical ideas in Pearson's early statistical papers.

Pearson's response *to Natural Inheritance* was initially somewhat cautious. He discussed Galton's book in a lecture he gave to the Men's and Women's Club on 11 March 1889:

> Personally I ought to say that there is, in my own opinion, considerable danger in applying the methods of exact science to problems in descriptive science, whether they be of problems of heredity or of political economy; the grace and logical accuracy of the mathematical processes are apt to so fascinate the descriptive scientist that he seeks for sociological hypotheses which fit his mathematical reasoning and this without first ascertaining whether the basis of his hypotheses is as broad as that human life to which the theory is to be applied.[35]

Galton' s influence in the emergence and development of Pearson's statistical thinking does not thus seem to have been immediate, whereas Weldon's influence, as will be shown, was more direct and rapid in its impact.

THE HISTORIOGRAPHY OF PEARSONIAN STATISTICS

The historiography of Pearsonian statistics has been dominated by discussions of Pearson's relationship with Francis Galton and thus to Galton's work on correlation, inheritance and eugenics. As will be shown in this section, this emphasis has resulted in the emergence of the following conclusions: (1) Pearson was merely a 'disciple' of Galton and, therefore, they shared similar outlooks, (2) Pearson's principal

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33 George Udny Yule, 'Notes of Karl Pearson's Lectures', Vol. III. 'Correlation', 1895, KP:UCL.
34 Yule commented that 'ill health had delayed the completion of the full memoir in 1896'. 'Notes of Karl Pearson's Lectures on the Theory of Statistics, 1884-1896', *Biometrika*, 28 (1936), 198.
35 Karl Pearson, Lecture of 11 March 1889, KP:UCL.

contribution to statistical theory was the development of the mathematical theory for Galton's method of correlation and regression, and hence (3) correlation was not only the dominant methodology in the Pearsonian corpus of statistics, but was thus the dominant tool in the Galton Eugenics Laboratory.

Though four of Pearson's coefficients of correlation were, indeed, used in the Eugenics Laboratory (i.e., the product-moment, biserial and tetrachoric correlation coefficients plus the phi-coefficient), it will be shown in chapter VII that the dominant methodology in the Laboratory, from its establishment in 1907 until Pearson's retirement in 1933, involved the use of family pedigrees and actuarial death rates. It will then be shown that the work in the Biometric School from 1892 to 1903 and the Drapers' Biometric laboratory from 1903-1936, was instead underpinned by Pearson's goodness of fit testing, the use of a higher form of algebra (i.e. determinantal matrix algebra), statistical and experimental studies of natural selection, craniometry and physical anthropology. This historiographical tendency to link *in toto* Pearson's work in the Galton Eugenics Laboratory to his work in the Drapers' Biometric Laboratory, is probably the most problematic aspect in the historiography of Pearsonian statistics.

Peter Bowler has, however, detected differences between Pearson's and Galton's statistical outlook which he thought arose from their approach to evolutionary biology. For Galton, populations were 'naturally stable around a mean value for a particular value', selection could skew the normal distribution, but if selection were removed, the

> normal distribution around the mean would reassert itself. Only a saltation could set up a new mean for the value... Pearson adopt[ed] a very different view [for him] there was nothing tending to restore the old norm and if selection skews the curve that will have a permanent effect.[36]

Thus, it may be seen that

> if all [of] Galton's statistics was built around the assumption that every naturally [occurring] variable has a fixed mean and will fall into a normal curve around the mean, then there is a quantum leap from this to Pearson's more open-ended view. For

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36 Peter Bowler (Personal Correspondence, 16 May 1991).

> Galton, statistical processes by definition could not generate permanent change; they could only produce a scattering around a mean value.[37]

Sixteen years ago, Karl Pearson's son, Egon, wrote to Bernard Norton that 'Galton had, for some time, long been considered to have played the principal role' in Pearsonian statistics.[38] This was a view that was endorsed by Jerzey Neyman (Egon's collaborator on inferential statistics) who wrote a paper in 1974 entitled 'A view of Biometry', where he gave a 'rather stereotypical account of the early Biometry [by] attributing everything to Galton and K.P., with not a mention of the fundamental part played by Weldon'.[39]

A year after he expressed his concerns, Egon Pearson began to re-examine some letters from the 'biometric period' to further consider Weldon's influence on Karl Pearson. He wrote to Bernard Norton again that

> from the atmosphere brought out in [his] mother's holiday diary, the two of them P + W [Pearson and Weldon] were in this together, Yet subsequent writers always tend to ascribe the biometric views to Pearson and omit references to Weldon.[40]

The full extent of Weldon's role in the development of Pearsonian statistics has thus received very little consideration. Churchill Eisenhart, Pearson's biographer in the *Dictionary of Scientific Biography,* acknowledged Weldon's role in a way that would have incurred Egon's approval when he wrote (in 1974) that after reading Francis Galton's *Natural Inheritance* and after meeting Weldon in 1890, Pearson

> began to lay the foundation of the new science of statistics that he was to develop almost single-handed during the next decade and a half, But it is doubtful whether much of this would have come to pass had it not been for Weldon, who posed the questions that impelled Pearson to make his most significant contributions to statistical theory and methodology.[41]

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37 *Ibid.*

38 Egon Pearson, Letter to Bernard Norton, Bernard Norton Papers: University of Leicester, 15 August 1977.

39 *Ibid.*

40 Egon Pearson, Letter to Bernard Norton, University of Leicester, 22 February 1978. In this letter Egon also wrote that 'to help the future historian, [he was] planning to get typed some 90 letters exchanged between 'Student' [William Sealy Gosset] and myself in the years between 1926 and his death in 1937. This will be inevitably rather autobiographical (of course the stuff will not be for publication, but perhaps a few copies from a master-typescript might be made) p. 2.' Egon's collection was to be published posthumously in R.L. Plackett and G.A. Barnard (eds.), *'Student'. A Statistical Biography of William Sealy Gosset Based on Writings by E.S. Pearson* (Clarendon Press: Oxford, 1990).

41 Churchill Eisenhart, 'Karl Pearson', *Dictionary of Scientific Biography*, 10 (Charles Scribner's: New York, 1974), p. 449.

When examining Pearson's statistical work in 1958, J.B.S. Haldane had 'no doubt' that Pearson's Gresham lectures on statistics and probability (from 1892-1895)

> were written partly as a result of the questions that Weldon began putting to him soon after his arrival in University College [and that] the full answers to these questions [may] be found in the great series of memoirs on the Mathematical Theory of Evolution... published between 1893 and 1900.[42]

More recently, Weldon's influence on Pearson's early statistical work has been considered by Steve Stigler and A.W.F. Edwards. When addressing this early work, Steve Stigler noted that Pearson 'frequently cited interaction with [his] friend Weldon as a major stimulus in his early work'; A.W.F. Edwards thought Pearson's first paper of 1893 'descended from Weldon and Edgeworth's interests... [and] the link to Galton's work was indirect'.[43]

Outside the historiography of Pearsonian statistics, Weldon's role has been endorsed by Peter Bowler, who has written extensively on the history of evolution and natural selection, and by Robert Olby who has been especially interested in the history of Mendelism. Both have agreed that Weldon's role was considerable in attempting to assemble and present empirical evidence of Darwin's theory of natural selection at the end of the nineteenth century.[44]

Victor Hilts, whose interpretation of Pearson's statistical work matches most closely many of the arguments in this thesis, thought that most of Pearson's original contributions occured between 1893 to 1900 and that his most important contributions to statistics were the

> method-of-moments as a tool for the analysis of general distributional functions, his algebraic development of the correlation coefficient and the generalisations to n

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42 J.B.S. Haldane, 'Karl Pearson', in *Speeches Delivered at a Dinner held in University College, London on the Occasion of the Karl Pearson Centennary Celebration, 13 May 1957* (privately issued by the *Biometrika* Trustees, 1958), p.8.

43 Stigler, *History of Statistics*, p. 305; A.W.F. Edwards, 'Galton, Karl Pearson and Modern Statistical Theory' in Milo Keynes (ed.), *Sir Francis Galton, FRS: The Legacy of His Ideas* (Macmillan: London, 1993), pp.97-8.

44 See Peter Bowler, *Evolution: The History of an Idea* (University of California Press: Berkeley and Los Angeles, 1984). Robert Olby has especially helped to bring Weldon's role into prominence in 'The Dimensions of Scientific Controversy: The Biometrician-Mendelian Debate', *British Journal for the History of Science Commemorative Issue for Bernard Norton*, 74 (September 1989), 299-320.

> variables and the chi-square test for goodness-of-fit between a theoretical distribution and observed data.[45]

He further argued that 'with Pearson finally came the divorce of statistical theory from direct connection with theories of social science' and that for Pearson biometry was 'a field primarily defined by its attempts to apply quantitative methods to demonstrate the existence of natural selection'.[46]

In a manner that is most uncharacteristic of the vast bulk of Pearsonian historiography, Hilts also linked up Pearson's statistical developments to Weldon's measurements of the frontal breadth of crabs rather than to 'a further development of statistical methods by the earlier quantitative study of man'. Regarding Galton's influence, it was

> true, of course, that the idea of mathematical correlation itself was developed by Francis Galton at a time when he was intensely concerned with the study of man. Nevertheless, it was not Pearson's great concern with social reform and social statistics which led him to develop the new mathematical methods, but his contact at University College with the biologist William [*sic*] F.R. Weldon.[47]

That Weldon should occupy such a position in Pearson's life was attributed to an 'unexpectedly indirect way of scientific progress' rather than to, his quantitative interests in evolutionary biology.[48] Though Pearson's link to Weldon is fully endorsed by Hilts, his views diverge from the argument developed in this thesis when he argues

> that Biometry became all the more a crusade after the beginning of the twentieth century, because...it was carried out in opposition to Mendelism, The second, and more important, cause was 'eugenics'... [and] it was in 'eugenics' that statistics was to be associated with a new science of man itself.[49]

When Joseph Oscar Irwin wrote an article on 'Some Aspects of the Development of Modern Statistical Method' in 1935, the year before Pearson's death, Weldon was mentioned only in the context of the founding of *Biometrika* whereas

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45 Victor Hilts, *Statist and Statistician* (Arno Press: New York, 1981; reprint of his Ph.D. thesis, Harvard University, 1967), p.554.
46 *Ibid.*, p.505.
47 *Ibid.*, p.571.
48 *Ibid.*
49 *Ibid.*, p.581.

> Galton's work [was seen to have] inspired an outstanding mathematician 'one of the greatest scientific imaginations of our age'—and [he] refer[red] to Professor Karl Pearson.[50]

Thus it may be seen that even before Pearson died, Galton's influence had come to assume greater importance than that of Weldon.

Lyndsay Farrall attempted to link Pearson's work on correlation to Galtonian eugenics. His assessment of Pearson' work is, however, based on three misconceptions: the first stems from Pearson's positivism and the other two relate to Farrall's definition of 'correlation' and of 'biometry'. Farrall thought that Pearson's positivism, expressed in the *Grammar of Science,* was a 'philosophical analysis of causation'.[51] According to Farrell this was to have led to Pearson's construction of statistical methods, particularly for correlation, as Farrall thought that

> causation was, for Pearson, merely complete correlation. Smaller correlations were indicators of partial correlation and thus could be used to elucidate cause and effect. In situations where many factors (or potential causes) were operating on a large variety of objects (as was the case of natural selection) the use of partial correlations provided a tool of analysis for what actually happened.[52]

Yet in his third edition of the *Grammar of Science,* Pearson wrote that 'the universe was seen as a complex of contingent and not causally linked phenomena'.[53] In what is, perhaps, the most vivid anti-causalist statement Pearson made, he went on to say that

> the universe could be seen as a universe of variation rather than a universe controlled by the law of causation in its narrowest sense, *No phenomena are causal; all phenomena are contingent,* and the problem before us is to measure the degree of this contingency, which we have seen lies between the zero of independence and the unity of causation .[54] (Italics mine.)

Pearson defined *partial correlation* as 'the correlation between C and D when fixed values are given to A and B' (i.e. the correlation between two variables C and D when

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50 J.O. Irwin, 'Some Aspects of the Development of Modern Statistical Method', *Mathematical Gazette* (1934), 29.

51 Lyndsay Farrall, 'The Origins and Growth of the English Eugenics Movement, 1865-1925' (Ph.D. thesis, Indiana University, 1970), p.262.

52 *Ibid.*, pp.262-3.

53 Pearson, *Grammar of Science*, 3rd edn, p.173.

54 *Ibid.*, p.174.

a third A or fourth B variable is held constant or is partialled out).[55] Moreover, Pearson stressed continually that 'complete correlation' (i.e., when $r = \pm 1$) could never be interpreted as an indicator of causation. Pearson considered correlation to have been the 'ratio of common causes to the non-common contributory factors'.[56] Thus it would not seem that Pearson's positivism was a 'philosophical analysis of causation' nor that partial correlations 'could be used to elucidate cause and effect'.

Galton's method of correlation was misidentified by Farrall as one of 'Weldon's statistical techniques'. Farrall seemed to think that Pearson was critical of Weldon's use of correlation. Moreover, his interpretation of Pearson's reaction to Weldon's use of correlation contradicted his comments on Pearson's view of correlation and causation. Though Farrall referred to Pearson's statistical work as 'a philosophical analysis of causation', he also believed that 'Pearson condemned Weldon's interpretation of correlation [because] it could not be equated with causation as Weldon had appeared to do.[57] Thus, although Farrall claimed that 'causation was for Pearson merely complete correlation', he also claimed that Pearson would not equate correlation with causation.

Pearson had, however, cautioned Weldon on the reporting of *spurious correlations* as it should have been ascertained that the 'correlation between organ and death rate must be shown in itself not to be fortuitous'.[58] Indeed, Pearson wrote that 'all causality is of course correlation [though he went on to say that] the converse is surely a dangerous doctrine'.[59] Hence, whilst causality may be reflected by correlation, the opposite is not true (i.e., it cannot be said that correlation can be used to 'elucidate cause and effect'),

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55 Karl Pearson, 'Mathematical Contributions to the Theory of Evolution, XI. On the Influence of Natural Selection on the Variability and Correlation of Organs', *Philosophical Transactions* CCA (1902), 31.

56 Karl Pearson, 'Nature and Nurture Problems of the Future', *Eugenics Laboratory Lecture Series* III (Dulau: London, 1910), 26.

57 Farrall, 'English Eugenics Movement', p.81.

58 Karl Pearson, 'The Utility of Specific Characters', *Nature* (17 September 1896), p.450.

59 *Ibid.*

Farrall also attempted to analyse the emerging discipline of Pearsonian statistics within a sociological framework. Farrall viewed Pearsonian biometry as having no relation to Pearsonian statistics and he argued that 'biometry became a methodology of causation for eugenics'. According to Farrall, Pearson and his students were to have 'defined statistics outside the context of the discipline of mathematics'.[60] He then linked Pearson's and Weldon's work on selective death rates of animals and plants to eugenics as he thought that 'eugenics, like biometry, first emerged as a speciality within zoology'.[61] Eugenics was

> in reality applied biology based on the central biological theory of the day, namely the Darwinian theory of evolution. Hence, the study of eugenics demanded the application of methods similar to those used by biologists studying evolution.[62]

In concluding, he wrote that 'neither eugenics nor biometry have passed into the canons of twentieth century science... nor did they become recognised scientific specialties'.[63] If biometry is defined, however, in Pearson's terms, as 'the application of exact statistical methods to the problems of biology [which was] no more pledged to one hypothesis of heredity than to another' these claims would seem problematic.[64]

Hilts' and Farrall's ideas would later find much support in the work of Bernard Norton and of Donald Mackenzie. Norton thought that Farrall provided 'an excellent account of some of the stages involved in setting up the department...of Applied Statistics [as it emerged from the Biometric Laboratory at University College London]'.[65] Following Farrall's analysis of Pearson's statistical work, Mackenzie

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60 Farrall, 'English Eugenics Movement', p.193.
61 *Ibid.*, p.189.
62 *Ibid.*, p.55.
63 *Ibid.*, pp.179-80.
64 Pearson, 'Mendel's Law', *Nature, 70* (27 October 1904), pp.626-7. Other reasons why Farrall's view is seen as being problematic include the following: (1) the journal *Biometrika* has been in print since 1901 when it was first founded, (2) R.A. Fisher founded later the journal *Biometrics*, (3) the Biometrics Society today regards itself as 'devoted to the mathematical and statistical aspects of biology'. (4) J.B.S. Haldane was to become the first Weldon Professor of Biometry in 1936, and (5) Dr. A.W.F. Edwards is presently the Reader of *Biometry* at Cambridge.
65 Bernard Norton, 'Karl Pearson and Statistics: The Social Origin of Scientific Innovation', *Social Studies of Science*, 8 (1978), 30.

argued later that 'biometry as a speciality within professional biology must be judged a failure'.[66]

Norton argued that Pearson's positivism was the key factor in the development of his statistical work and that biometry was to have been formulated and constructed 'without theory'. Weldon's role was explicitly denied as being a contributory factor'. Pearson thus

> entered willingly into biometry when presented with the opportunity by Weldon, not because of Weldon's exceptional charm or because Pearson was short of problems of his own, but because by the time that he met Weldon, Pearson had independently developed a pattern of social, philosphical and political thought which disposed him to find Weldon's programme of mathematical biology one of the greatest possible significance. *Before meeting Weldon,* [Norton] argue[d], Pearson had grown into a social Darwinist anxious to provide his particular form of Darwinism with a proper scientific basis.[67] (Italics mine)

Social Darwinism gets linked inevitably to eugenics so that for Pearson his

> Statistics, thus formed, embodied the central tenets of [his] philosophy of science, and, as such, was to be universally recommended. It was to be applied to eugenics in particular, for eugenic thought was a component of Pearson's social Darwinism *before his meeting with Weldon.*[68] (Italics mine)

Whilst Pearson's epistemological outlook was, undoubtedly, a contributory factor in the development of his statistics, his 'meeting with Weldon' could account for a greater influence than Norton has suggested.

Donald Mackenzie shares similar views to Norton, though he placed greater emphasis on eugenics and on influences connected with the growth of a professional middle class. Mackenzie viewed Weldon as someone who

> was also interested in what Galton was doing. [Though] unlike Pearson, it was Galton's statistical methods rather than eugenic conclusions that attracted Weldon... [who] needed the help of a professional mathematician, and approached his colleague Pearson. In their collaboration *Pearson's methodological doubts about Galton's approach were overcome:* he realised that statistical analyses could be seen as exemplifying, rather than contradicting, the positivist and phenomenalist criteria of valid knowledge of the *Grammar.* Using statistics, the biologist could (apparently) measure without theorising, summarise facts without going beyond them, describe without explaining.[69] (Italics mine).

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66 Donald A. Mackenzie, *Statistics in Britain 1865-1930: The Social Construction of Scientific Knowledge* (Edinburgh University Press: Edinburgh, 1981), p. 103.
67 Norton, 'Karl Pearson and Statistics', 5-6.
68 *Ibid.*, 6.
69 Mackenzie, *Statistics in Britain*, pp. 88-9.

That Galton should have adhered to his idiosyncratic use of the normal distributions throughout his life and that Pearson came to move away from such an approach, would not seem to suggest that 'Pearson's methodological doubts about Galton's approach were overcome'.

Though Mackenzie wrote that Pearson 'began work on mathematical problems suggested by Weldon's work', this suggested to Mackenzie that 'Pearson was simply interested in applying his mathematical skills to other scientists' problems, irrespective of any intrinsic concern for these problems'.[70] Pearson's 'mathematisation' of Darwinism was seen,

> in part as an attempt to defend the theory of natural selection from its reactionary opponents [e.g. Lord Salisbury]. He sought to develop an evolutionary science that was philosophically impeccable, according to his own phenomenalism and positivism. This he did not do simply for its own sake, but in order to legitimate its application to the human species, its uses as social Darwinism.[71]

Theodore Porter, who espoused Mackenzie's social-constructivist analysis, argued that 'Pearson's eugenic convictions provided the principal explanation for the enthusiasm with which he took up the study of statistics in 1893'.[72] However, this view is problematic because by 1893, Pearson had already devised the standard deviation and his *method of moments*. When it was suggested that Porter's biometrical arguments did not take into account Pearson's series on the 'Mathematical Contributions to the Theory of Evolution'. Porter argued later that

> Pearson's eugenic commitments are readily apparent throughout his series of 'Mathematical Contributions to the theory of Evolution', and even before. His keen interest in evolutionary biology was largely (though) not entirely due to its obvious bearning [sic] on eugenics.[73]

Steve Stigler has been interested in the development of statistical theory throughout the greater part of nineteenth century. In looking at some of the charges

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70 *Ibid.*, p.89.

71 *Ibid.*, pp.89-90.

72 Theodore Porter, *The Rise of Statistical Thinking, 1820-1900* (Princeton University Press: Princeton, 1986), p.77.

73 M. Eileen Magnello, 'Statistical Regularity and Determinism', *Journal of the History of the Behavioral Sciences*, 25 (January 1989), p. 76. Theodore Porter, 'Dr Porter Responds' in Magnello, 'Statistical Regularity and Determinism', p. 77.

that statistical theory was undergoing towards the end of the nineteenth century, he argued that

> the three principal contributors...were Francis Galton, Francis Ysidro Edgeworth, and Karl Pearson...together they helped to create a statistical revolution... [and] without the work of any one of these men it is doubtful that the effect would have been so great.[74]

Edgeworth is given this status for the following reasons: 'because Pearson had encountered Edgeworth's work on *a priori* probabilities by 1888' (i.e. some four years before he met Weldon); Pearson gave his first Gresham lectureship on probability five months after Edgeworth gave his Newmarch lecture 'On the Uses and Methods of Statistics'; and also because Pearson made reference to Edgeworth, John Venn and Harold Westergaard in the *Grammar of Science*.[75] From this Stigler concluded that

> Pearson's manuscript notes on works of Venn, Edgeworth, and Westergaard make it clear at this period that Pearson was greatly expanding his intellectual horizons and that Edgeworth was his major intellectual colleague in the endeavor.[76]

Though it may be said that the statistical ideas of both Edgeworth and Venn influenced Pearson's statistical thinking on probability, that he encountered their work before he met Weldon, and that he did, indeed, make reference to their work on several occasions, Edgeworth may not have been as influential as Stigler suggests. Whilst in February of 1899, Pearson claimed that Weldon 'had changed the whole drift of his work', some seven years later he

> felt sure that his earliest contributions to biometry were the direct result of Weldon's suggestions and would never have been carried out without his inspiration and enthusiasm.[77]

As it may be seen, much of the historiography of Pearsonian statistics has, for more than half a century, placed greater emphasis on Galton's role than that allocated to Weldon's role. To gain a fuller impression of how Galton's role has assumed such prominence, the following five potential contributory factors may also be considered:

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74 Stigler, *History of Statistics*, p.266.

75 *Ibid.*, p.327.

76 *Ibid.*, p.328.

77 Karl Pearson, 'Walter Frank Raphael Weldon, 1860-1906', *Biometrika*, 5 (1906), 1-52. Also reprinted in E.S. Pearson and Sir Maurice Kendall, *Studies in the History of Statistics and Probability. Volume 1* (Charles Griffin: London, 1970). Page references are to the reprint in Pearson and Kendall.

1. Galton may be seen as having exerted greater influence on Pearson as he occupied a position which was more visible to the public, whereas Pearson's relationship with Weldon was more private in comparison. Galton's role was made public by the Anthropometric Laboratory he set up at the International Health Exhibition in South Kensington from 1884 to 1887, by the funding he provided for the Biometric Laboratory in 1904 and finally through his endowment of the Galton Chair of Eugenics in 1911.

2. Pearson's monumental three-volume biography of Francis Galton may be seen as further evidence of Galton's influence and this will be discussed in Chapter VII.

3. Pearson's less than cautious, if not fully enthusiastic, reaction to Galton's *Natural Inheritance* some 45 years after he first expressed his guarded concern to the Men's and Women's Club in 1889. On the second occasion in 1934 (two years before he died), Pearson was speaking about Galton at a dinner held in his honour at University College London. He began by quoting a passage from the Introduction where Galton wrote:

> This part of the inquiry may be said to run along a road on a high level, that affords wide views in unexpected directions, and from which easy descents may be made to totally different goals to those we have now to reach.[78]

On this occasion, he gave considerable attention to Galton's correlation where

> 'Road on a high level', 'wide views in unexpected directions', 'easy descents to totally different goals—here was a field for an adventurous roamer! I felt like a buccaneer of Drake's days—one of the order of men 'not quite pirates, but with decidely piratical tendencies', as the dictionary has it! I interpreted that sentence of Galton to mean that there was a category broader than causation, namely correlation, of which causation was only the limit, and that this new conception of correlation brought psychology, anthropology, medicine and sociology in large parts into the field of mathematical treatment. It was Galton who first freed me from the prejudice that sound mathematics could only be applied to natural phenomena under the category of causation. Here for the first time was a possibility—I will not say a certainty of reaching knowledge—as valid as physical knowledge was then thought to be—in the field of living forms and above all in the field of human conduct.[79]

This passage, though it is very different from his views expressed 45 years earlier, would seem to suggest very strongly that Galton's influence had been very powerful.

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78 Karl Pearson, *Speeches Delivered at a Dinner held in University College London in Honour of Professor Karl Pearson* (Cambridge University Press, 23 April 1934), p. 22.

79 *Ibid.*, pp.22-3.

It is noteworthy that Steve Stigler observed 'the change of direction' Pearson adopted in 1934; he thought that Pearson's later view which did 'not accord' with his earlier view, seemed to have indicated that 'as often is the case, later recollections can be unreliable'.[80]

4. Pearson's first two papers on the 'Mathematical Contributions to the Theory of Evolution' have been given little consideration whereas his third paper of 1896, on 'Regression, Heredity and Panmixia'—which was perhaps inspired partly by Galton's work, tends to be seen as the prime, if not the leading, plan in the emergence of Pearsonian statistics. Thus Mackenzie and Barnes have regarded Pearson's third paper as

> the most comprehensive statement of the biometricians' attempt to quantify evolutionary biology... This memoir makes clear the extent to which man was the 'paradigm organism' for Pearsonian biometry: the mathematical formulation proposed outran contemporary possibilities for measurement in any other species.[81]

5. Perhaps the fullest extent of Weldon's influence on Pearson's statistical thinking can be garnered from their correspondence which has not yet been examined in any thorough or systematic fashion. The letters remained in the hands of Egon Pearson until the latter part of the 1970's, and Bernard Norton was the first person to have received permission from Egon to examine the letters. Following the work of Norton, reference has been made to various letters from the Pearson collection by others who had written on Pearson, but little attention has been placed on the extensive correspondence between Pearson and Weldon. (A handlist was complied by Maxine Merrington in 1974 and in 1984 an annotated handlist was made available.[82])

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80 Stigler, *History of Statistics*, p.305.

81 D. Mackenzie and S.B. Barnes, 'Biometriker versus Mendelianer. Eine Kontroverse und ihre Erklarung', *Kölner Zeitschrift für Sozialpsychologie* (1975), Sonderheft 13, section iv, pp.165-96. This is the German translation of the unpublished English typescript: 'Historical and Sociological Analyses of Scientific Change: the Case of the Mendelian-Biometrician Controversy in England', pp. 1-66, which the authors duplicated and distributed in 1974.

82 M. Merrington, B. Blundell,S. Burrough, J. Golden and J. Hogarth (eds), *A List of the Papers and Correspondence of Karl Pearson (1857-1936) Held in the Manuscript Room, University College London Library* (University College London, 1983).

CORRESPONDENCE FROM THE PEARSON-WELDON COLLECTION

The earliest correspondence extant in the Pearson-Weldon collection is dated 13 February 1892 and relates to administrative affairs concerning the creation of a Professorial University of London. A reform movement for the University of London had begun sometime before 1884.[83] This resulted in a series of 12 letters beginning on 13 February 1892 and ending on 22 December 1894. The central idea was to unite all of the London teachers so that the separate colleges would then become absorbed. Pearson aspired to found a university in which teaching would be done by university professors who alone would largely control the University. Their aim, as expressed by Karl Pearson, was to be found in:

> The creation of a homogeneous academic body with power to *absorb,* not to federate existing institutions of academic rank... Such a corporation may be conveniently spoken of as a professorial university to distinguish it from a collegiate or federal university.[84]

When Weldon took up his post at UCL in 1891, he joined the association almost immediately. He showed himself to be a tireless campaigner and even then seemed to go out of his way to help Pearson. three weeks after he wrote his first letter to Pearson, he addressed 320 envelopes 'from the previous night' to gain support in setting up a charter for the University of London.[85] By the end of the month Weldon had circulated 50 copies of the charter to various people in London. Weldon also made suggestions to modify various parts of Pearson's original draft charter in 1893. It was this sort of enthusiasm and commitment which so much characterised Weldon's working relationship with Pearson.

There is a disproportionately greater number of letters from Weldon to Pearson than vice-versa: of those that survive, there are 491 pieces of correspondence from

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83 In the first letter, Weldon wrote to Pearson to say that there was some opposition (by Ramsay and Schapen) to Pearson's plan of setting up a Professorial University of London. A couple of weeks later, Weldon wrote to say that Max Muller, T.H. Huxley and Playfair joined the association 'on the grounds that London University ought to be the greatest in the world'; shortly afterwards, Huxley became President of the Association. From this point onwards, Weldon became engaged in various administrative tasks assisting Pearson with his goal.

84 Karl Pearson, 'Walter Frank Raphael, 1860-1906', *Biometrika*, 5 (1906), 286.

85 W.F.R. Weldon, Letter to Pearson, 5 March 1892, KP:UCL.

Weldon and 28 from Pearson. Clearly not all of his letters have survived as he quite often makes reference to letters that are not in the collection. However, it seems likely that Weldon did, indeed, write more often. Some of Pearson's replies were sent by telegrams and none of these have survived; when he writes his letters tend to be very lengthy whereas Weldon's are much shorter in comparison. Pearson also refers to a number of Weldon's letter in his lectures at Gresham College, University College London, his guest lectures and in most of Pearson's published statistical papers. The bulk of Weldon's letters were written after 1899; he wrote less than 100 letters between 1892-1898, the remaining 391 were written between 1899-1906 (when Weldon was at Oxford). Pearson did, however, write 269 letter to Mrs. Weldon who wrote 117 letters to Pearson) and the vast majority of these were written after Weldon's death in 1906.

Shortly after Weldon's death, Galton sent some material to Pearson in preparation for Weldon's obituary in *Biometrika.* Having been through all of Weldon's letters, Galton wrote to Pearson that

> the extraordinary fullness of his letters astonished [him] as [Weldon] would write almost a treatise, and insert long tables with apparent ease and as a work of supererogation, which would be a large labour to most men. I suppose too that a certain *pertinacity*, in the favourable sense of the word, was one of his most marked pecularities. The extraordinarily wide range of his accurate, not superficial, knowledge, was another feature.[86]

In the next chapter, it will be shown that when Weldon found that the female *Carcinus mœnas* in Naples was 'slightly dimorphic with respect to its frontal breadth', Pearson's resolution of the asymmetry which resulted from the dimorphism 'led to [his] first paper in the *Contributions of the Mathematical Theory of Evolution*'.[87]

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86 Letter from Galton to Pearson, 7 May 1906 in Karl Pearson, *Life, Letter and Labours of Francis Galton*, vol. IIIa (Cambridge University Press: Cambridge, 1930), p.282.

87 W.F.R. Weldon, 'On Certain Correlated Variations in *Carcinus mœnas*', *Proceedings of the Royal Society* (1893), 324. Karl Pearson, 'Walter Frank Raphael Weldon, 1860-1906', *Biometrika*, 5 (1906), 285.

Chapter III

THE BIOMETRICAL SPECIES CRITERION

The first attempt to provide an empirical measure for the identification of species was considered initially by Francis Galton in 1889 and then tested empirically by W.F.R. Weldon three years later when he used Galton's method of correlation.[1] Weldon's use of correlation in this context, which has been termed the 'biometrical species criterion', will be the focus of this chapter. It will be shown that Pearson's earliest statistical work arose from Weldon's attempts to find an empirical criterion for the identification of species and a quantitative method to explain the process of speciation.

Though Weldon pursued Galton's idea, their work displayed two distinguishing features which was to affect their working relationship with Karl Pearson. Firstly, Weldon was ready to break away from the essentialistic thinking derived from his morphological training whereas Galton's belief, that all frequency distribution should conform to the normal distribution, would suggest that he retained a strong essentialistic commitment; and secondly, Weldon was considerably more interested in natural selection than in inheritance. He thus embraced Darwin's theory of natural selection (in a way that Galton never did) and, could suggest ideas for an empirical measure of natural selection using differential mortality rates. Weldon's interests would thus demand a statistical treatment that differed from that which Galton had offered.

Problems associated with the identification of species and of speciation, rather than problems of inheritance may have been more amenable to the anti-realist epistemological stance in Pearson's positivistic outlook. At a time when so little was

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1 Francis Galton, 'Co-relations and their measurement, chiefly from anthropometric data', *Proceedings of the Royal Society*, 45 (1888), 135. Also in *Nature*, 39 (1889), 238; W.F.R. Weldon, 'Certain Correlated Variations in *Crangon vulgaris*', *Proceedings of the Royal Society*, 51 (1892), 2-19.

known about the mechanism of heredity, Galton's theories of inheritance may not have taken on the same 'reality' (i.e., a gene had not yet been identified) such that Pearson doubted the reality of postulated genes.[2] Species, however, consist of populations and have a 'reality' rather than an abstract 'essence'. Thus, when Pearson was attempting to work out some mathematical solutions for speciation in 1893, he thought that this could be determined by examining both roots of the quadratic solution from the population of species which would be either 'real and positive' or 'real and negative'.[3]

Plato's concept of the *eidos* (type) is the formal philosophical codification of essentialism. For Plato the vast observed variability of the world had no more reality than the shadows of an object on a cave wall. Fixed, unchangeable 'ideas' underlying the observed reality were the only things that are permanent and real.[4] Plato's philosophical belief in essences (or essentialism) and the modifications made by Aristotle dominated the natural sciences until the end of the nineteenth century.[5] The presence of some underlying 'essence' was to be inferred from some sort of similarity; this morphological similiarity then became the species criterion for the essentialist who thought that

> Individuals do not stand in any special relation to one another, being merely expressions of the same type—variation is the result of imperfect manifestations of the idea implicit in each species.[6]

Genuine change, according to essentialism, was possible only through the saltational origin of new essences. Because Darwin's theory of evolution depended

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2 Ian Hacking made the same observation about Lysenko who rejected Mendelism because he too doubted the reality of a gene. *Representing and Intervening. Introductory Topics in the Philosophy of Natural Science* (Cambridge University Press: Cambridge, 1983), p.24. Pearson's anti-realism is a feature of Bernard Norton's epistemological argument that Pearson's positivism was a major obstacle to his acceptance of Mendelism.

3 Karl Pearson, 'Contributions to the Mathematical Theory of Evolution', *Proceedings of the Royal Society*, 54 (Abstract), (16 November 1893), 332 (also in the *Journal of the Royal Statistical Society*, 56 (1893), 675-9).

4 Ernst Mayr, *Animal Species and Evolution* (Belknap Press of Harvard University: Cambridge, Mass., 1963), p.5.

5. Ernst Mayr, *Population, Species and Evolution* (Belknap Press of Harvard University: Cambridge, Mass., 1963), p.4.

6 *Ibid.*

upon 'gradual' changes, it was incompatible with essentialism.[7] Among the museum taxonomists, the essentialist interpretation of species continued to dominate and became known as the 'morphological species concept' because even similar species showed systematic morphological differences thus allowing individual organisms to be duly assigned to one species or another.[8]

Essentialism led to an increased idealisation in preserving the normal distribution (particularly for biological distributions) which helped to sustain a somewhat limited use of statistical methods. Pearson recognised some of the methodological problems that could occur when using the more conventional methods as

> naturalists, whether botanists or zoologists, are in the habit of preserving very infrequent normal variations; they get collected into museums, and labelled as abnormal variations, because the connecting links have either not been sought for or are too infrequently found; thus the prevalence of abnormal variations becomes a rather widespread idea, Further, another class of variations due to accidents of growth, to injury by insects or by environment, should be excluded from abnormal variation.[9]

That such a view was maintained by many of Weldon's biological friends, led Pearson to say later that this was a serious blow to Weldon as they

> could not understand how the Museum 'specimen' was in the future to be replaced by the 'sample' of 500 to 1,000 individuals, not to be looked through a glass, but to be handled, used, and if necessary *used up,* They warned his pupils to solemnly give up this sort of fooling and take to the real business of the 'biologer', if they wished for success.[10]

Certainly this was the view of E. Ray Lankester who disapproved of Weldon's activities if it meant that Weldon could spend as much time as seven hours just measuring his snails and he wrote to Weldon in 1900 to express his disdain. Weldon then wrote to Pearson to tell him that Lankester had urged him

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7 Ernst Mayr, *The Growth of Biological Thought: Diversity, Evolution and Inheritance* (Belknap Press of Harvard University: Cambridge, Mass., 1982), pp.38-9.

8 Both Ernst Mayr and Peter Bowler have shown that museum taxonomists during the nineteenth century retained this essentialistic view. Mayr, *Growth of Biological Thought*, p.270; Peter Bowler (personal correspondence, 6 May 1991).

9 Karl Pearson, *Grammar of Science*, 2nd ed. (Adam and Charles Black: London, 1900), p.391.

10 Karl Pearson, 'Walter Frank Raphael Weldon, 1860-1906', *Biometrika*, 5 (1906), 1-52. Also reprinted in E.S. Pearson and Sir Maurice Kendall, *Studies in the History of Statistics and Probability*, volume 1 (Charles Griffin: London, 1970), 265-321. Page references are to the reprint in Pearson and Kendall, p.304.

> to return to the pleasant way of describing beautiful beasts for the delight of the faithful to 'go to sea, and have a good time, and bring back a jelly fish which is bright blue'.[11]

W.F.R. WELDON AND THE MARINE BIOLOGICAL STATION AT PLYMOUTH

Walter Frank Raphael Weldon began university life in London in 1877 with the intention of pursuing a medical career and in the following year he entered St. John's College, Cambridge as a bye-term student.[12] Once at Cambridge, he met Francis Maitland Balfour and subsequently gave up his medical studies for zoology.[13] In 1881, he gained a first-class in the natural science tripos and in the autumn, he left for the Naples Zoological Station to begin the first of his studies in marine biological organisms. Upon returning to Cambridge in 1882, he was appointed university lecturer in Invertebrate Morphology.[14] Two years later, he became a founding member of the Marine Biological Station in Plymouth and resided there until 1887.[15] From 1884 until his death in 1906, Weldon's work was to be centered around this development of a fuller understanding of marine biological phenomena and particularly in the examinination of the relationships between various organs of crabs and shrimps to determine selective death rates in relation to the laws of growth.

The Marine Biological Station grew out of interests in the rising trade in fresh fish, transported to town by rail or by fish boats, in the 1870's.[16] This led to a subsequent increase in the numbers of registered fishing vessels which provoked widespread concern about the scarcity of fish and especially sole.[17] In response to these concerns, a committee on fisheries was set up which led to the eventual founding of the Marine Biological Station in Plymouth—the first of its kind in

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11 W.F.R. Weldon, Letter to Karl Pearson, 11 October 1900, KP:UCL.

12 Gilbert Charles Bourne, 'Walter Frank Raphael Weldon (1860-1906)', *Dictionary of National Biography* (1901-1911), p.629. Pearson, 'Walter Frank Raphael Weldon, 1860-1906', 272. A bye-term student is someone 'who for exceptional reason began residence for his degree in the Lent Term [Spring] of an academical year instead of the Michaelmas Term [Autumn]. F. Stubbings, *Bedders, Bulldogs and Bedells* (privately published, Cambridge, 1991).

13 Pearson, 'Walter Frank Raphael Weldon, 1860-1906', 273.

14 *Ibid.*

15 A.J. Southward and E.K. Roberts, 'One Hundred Years of Marine Research at Plymouth', *Journal of the Marine Biological Association*, 67 (1987), 471.

16 *Ibid.*, 465.

17 *Ibid.*

England. This was to become of some interest to T.H. Huxley initially and particularly to E. Ray Lankester, who wanted to pursue some of his ecological ideas with a view to undertaking research 'to foster the study of marine life'.[18] Weldon was one of the original founders along with Lankester, William Turner Thistleton-Dyer, Michael Foster, Adam Sedgwick and J. Burdon Sanderson. Though it was greatly owing to Lankester

> that the necessary funds were raised for erecting its laboratory at Plymouth, and that the undertaking became an institution of national importance to which the government contributed its support... Lankester [also] took an active interest in the welfare of the Association and helped raise its laboratory to the position which it now holds as a centre of zoological research.[19]

The Marine Biological Association was subsequently formed at a meeting held in the rooms of the Royal Society on 31 March 1884.[20] From their opening days, these two main aims were pursued by staff and visitors alike involved (1) the study of living marine animals and their physiology and (2) investigations on food fish and shell fish.[21] .

During his first five years at the Marine Biological Station, Weldon's early investigations were 'directed to the study of classification, morphology and the development of the *Decapod crustacea*.[22] Weldon's 'only work on invertebrate morphology' contained an account of the early stage of segmentation and the building of the layers of the shrimp.[23] E.J. Allen (then Director of the Station) thought that

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18 The appeal was answered by the following individuals: Sir John Lubbock, P.L. Sclater, E. Jeffrey Bell, Michael Foster, J. Burdon Sanderson, W.H. Flower, G.J. Romanes, Adam Sedgwick, H.N. Moseley, A. Milnes Marshall, W.T. Thistelton Dyer, W.B. Carpenter, G.J. Allman and John Murray, *ibid.*, 466.

19 E.S. Goodrich, 'Edwin Ray Lankester (1847-1929)', *Dictionary of National Biography*. 'Today, the MBA works alongside the institute for Marine Environmental Research, the Environmental Science Department and the Institute of Marine Studies at Plymouth Polytechnic, the Laboratory [thus] provides a body of knowledge, skill and expertise which makes Plymouth and Devon a centre of exellence for the study of the sea.' A.J. Southward and E.K. Roberts, 'One Hundred Years of Marine Research at Plymouth', *Journal of the Marine Biological Association*, 67 (1987), 503.

20 *Ibid.*, 467.

21 *Ibid.*, 476.

22 E.J. Allen, 'Professor W.F.R. Weldon, F.R.S.', *Journal of the Marine Biological Association*, 7(3); 331 (1906), 331. Allen commented that Weldon's researches 'was shown in the special courses of lectures upon it, which he subsequently delivered at University College.

23 Pearson, 'Walter Frank Raphael Weldon, 1860-1906', 279.

this paper exemplified 'Weldon's skill both as a master of histological techniques and as a powerful and accurate draughtsman'.[24]

Some of his early work at the Marine Biological Association also involved the breeding of the common lobster and the rock lobster or craw fish (*Palinurum*).[25] In 1887 Weldon started a notebook, in what seems to have been an attempt, to list systematically various species of *Decapod crustacea* at Plymouth Sound.[26] He identified crabs, lobsters, shrimp, prawns and leaches by indicating the genus and species in addition to the date and location of each observation. He recorded in all months except April, May and June and most of his recordings were made from July to October 1888. No recordings were made after October 1889; by then, he had filled 63 pages in his notebook. Nothing much seems to have developed from this notebook (which appears to have been unexamined since it was last used by Weldon).[27]

In 1889 Weldon read Galton's *Natural Inheritance.* In this book, Galton had shown that the frequency with which deviations from the average size of certain organs in man, in certain plants and in moths followed the curve of error (i.e. were normally distributed). Galton's material had been drawn from investigations of civilised man in addition to domesticated plants and animals; moreover, 'Mr Galton has himself pointed out that in the majority of cases studied, the effect of natural selection is probably insignificant'.[28] Similar investigations by Quetelet had also been confined to civilised man. The restrictions to man and to domesticated plants and animals meant that there was much material left unexplored for Weldon and he thus pursued his work as

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24 Allen, 'Professor W.F.R. Weldon, F.R.S.', 331.

25 Anon, 'The Opening of the Marine Biological Laboratory at Plymouth', *Nature*, 38 (June 28, 1888), 200.

26 W.F.R. Weldon, '*Decapod Crustacea* of Plymouth Sound' (Notebook: 1887-1889), Library of the Marine Biological Association of the United Kingdom at Plymouth.

27 Allen Varley, Former Head of Library and Information Services, remarked that there are no archival records of this notebook being examined at the Marine Biological Association; he also thought that the notebook had not been unwrapped since Weldon last used it. (Personal communication, 27 April 1993).

28 W.F.R. Weldon, 'The Variations occurring in certain *Decapod crustacea*: I. *Crangon vulgaris*', *Proceedings of the Royal Society*, 47 (March 1890), p.446.

> It [had], therefore, seemed worth while to attempt an investigation of the variations in the size of certain organs which occur in a species living in a wild state upon which natural selection and the other destructive or plastic influences from which domesticated animals and civilised man are alike protected may be supposed to act with full effect.[29]

In Galton's work on heredity in 1889, he had predicted that selection would not have the affect of altering the normal distribution. He expected that his frequency distributions to 'obey the law of error [normal distribution] in all cases whether the animals observed were under the action of selection or not.[30] In Weldon's investigation of 1890, he found that his data conformed to the normal distribution—as Galton had predicted.

DIMORPHISM AND SPECIATION

Two months after Weldon submitted this paper to the Royal Society, he wrote to Galton on 14 May and asked if he would

> Forgive the long delay in the preparation of the correlation curves, The measures [of various organs in the common shrimp] are practically finished-1000 Plymouth, 400 Southport. 390 Sheerness, Unless you feel anxious to see the results very quickly I will not do the arithmetic by daylight, because I want very much to do some anatomical work. By the help of Crelle [Multiplication Tables of 1000 x 1000], the arithmetic will not take many evenings.[31]

Galton had suggested to Weldon that the relation between the two organs indicated by his method of correlation was of such a kind that might be expected 'to have the same value in all races of the same species, and in some cases in groups of species'.[32] With this view, Weldon thought that a measure of correlation could have served as a diagnostic technique to identify possible membership in a particular species. Thus, the values obtained from calculating correlations should have been constant within a species, but different between species.

Weldon decided to investigate the biometrical species criterion by using Galton's method of correlation, or what he termed 'Galton's Functions', when examining relationships which could be described as 'functional' (i.e., when the size of one organ

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29 *Ibid.*
30 *Ibid.*
31 Letter from Weldon to Galton (4 May 1889), in Pearson, *Life of Galton*, vol. IIIb., p.483.
32 *Ibid.*

is a function of the size of another organ). Weldon's renaming of Galton's 'index of co-relation' to 'Galton's functions' may have derived from this pursuit.

Weldon collected data on some 2,980 adult female shrimps to test the constancy of the correlation between various organs in five samples of adult female shrimps.[33] The shrimp came from Helder (North Holland), Southport, Sheerness and Roscoff—all places fairly distant from Plymouth and 'differing from Plymouth and from each other in climatic conditions, in salinity and in other characters of the sea water and in the nature of the sea bottom'.[34]

A determination of this constancy was made from five pairs of organs in the shrimp. The constants were denoted by Galton's *r* and these organs were measured (see Figure 3.1):

(1.) The total carapace length, measured in a straight line;

(2.) The length of that portion of the carapace which lies behind the single gastric spine;

(3.) The length of the sixth abdominal torgum;

(4.) The length of the telson'.[35]

Weldon had first shown in 1890 that the variation of these organs were normally distributed.[36] The results from this second investigation confirmed Galton's idea, again, that the data should have been normally distributed (see Figures 3.2 and 3.3). With the confirmation of these results, Weldon was now prepared to test the proposition that correlation could be used as a diagnostic technique to identify membership in a species (i.e., as an empirical measure for the identification of species).

To test this constancy of correlation, the total length of carapace and length of sinuous portion were measured in samples of adult female shrimp. Each of the races

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33 W.F.R. Weldon, 'Certain Correlated Variations in *Crangon vulgaris*', *Proceedings of the Royal Society*, LI (1892), 9.
34 *Ibid.*, 9.
35 *Ibid.*, 3.
36 Weldon, 'The Variations occurring in certain Decapod Crustacea: I. *Crangon vulgaris*', 445-53.

Figure 3.1
WELDON'S CARAPACE OF THE COMMON SHRIMP (1892)

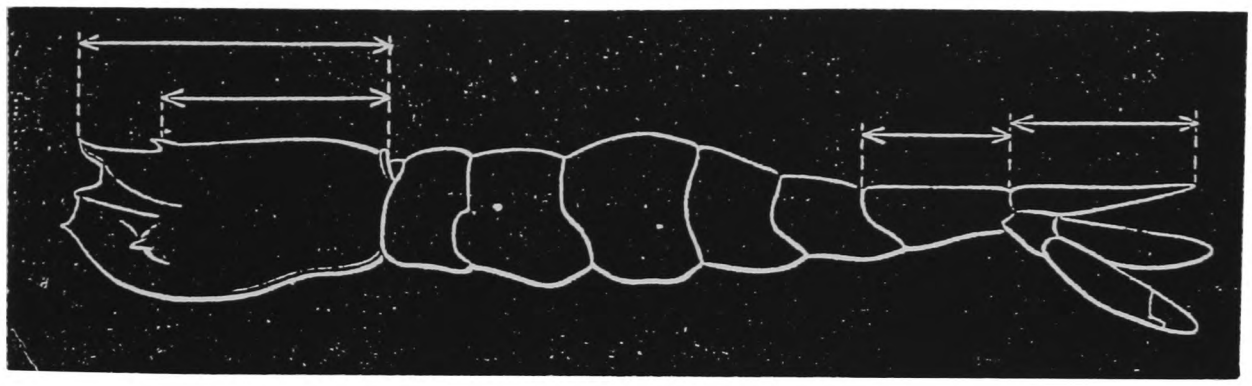
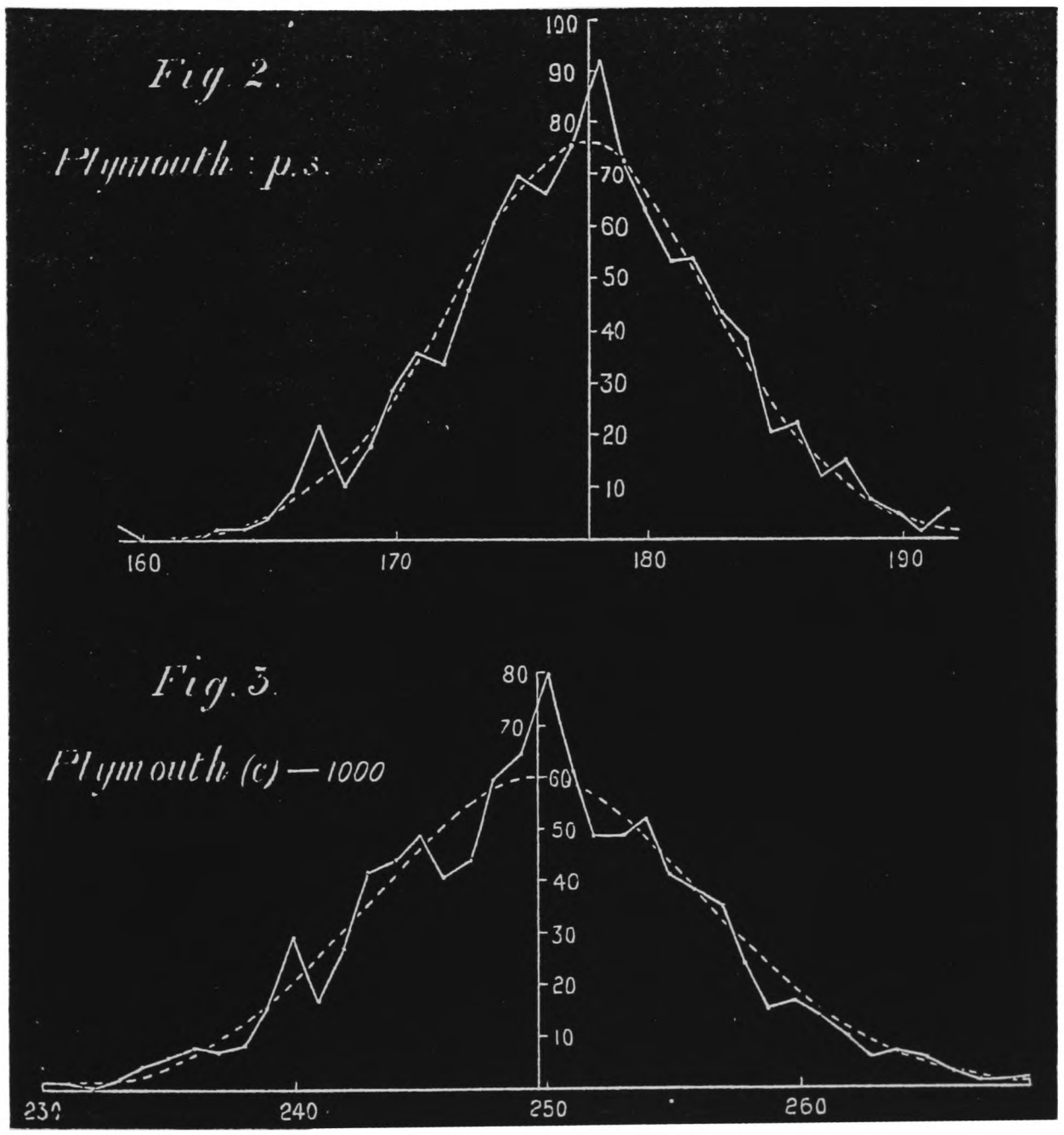


Figure 3.2 and 3.3
CURVES OF FREQUENCY OF ALL OBSERVED MAGNITUDES OF POST-SPINOUS LENGTHS (Fig. 2) AND TOTAL CARAPACE LENGTHS (Fig. 3) IN 1000 ADULT FEMALE SHRIMPS FROM PLYMOUTH



The dotted curve is a probability curve of the calculated probable error: the continuous cure shows the results of observation. The horizontal scale represents thousandths of the body length: the vertical scale represents tens of individuals.

was found to differ from the other in the average length of the organs measured; however, the relation between the two organs, as measured by *r,* was fairly constant throughout.[37] From these results Weldon thought that

> It may, therefore, be said that the values of r obtained by examining five races of shrimps are not inconsistent with the existence of a constant value in all the races examined—a value lying somewhere between 0.80 and 0.85.[38]

When Weldon found that the correlation values he obtained by using Galton's functions had nearly the same value for each race (see Table 3.1), he thought he had found a criterion for the identification of species. A numerical measure to use for any species of animals seemed forthcoming as

> the results recorded lead to the hope that, by expressing the deviation of every organ from its average in Mr. Galton's system of units, a series of constants [i.e., correlation] may be determined for any species of animal which will give a numerical measure of the average condition of any number of organs which is associated with a known condition of any one of them, A large series of such specific constants would give an altogether new kind of knowledge of the physiological connexion between the various organs of animals; while a study of those relations which remain constant through large groups of species would give an idea, attainable at present in no other way, of the *functional correlations* between various organs which have led to the establishment of the great sub-divisions of the animal kingdom.[39] (Italics mine.)

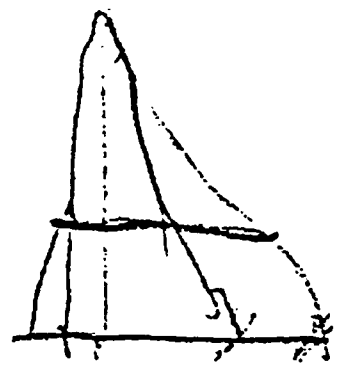
Table 3.1
Correlations between Carapace length and Post-Spinuous portion from five races of adult Female Shrimps

Area	Galton's functions (r)	number
Plymouth	0.81	1,000
Southport	0.85	800
Roscoff	0.80	500
Sheerness	0.85	380
Helder	0.83	300

During the Easter Vacation of 1892, Weldon and his wife went to Malta and Naples. Over the course of the summer, they calculated crab measurements at the Zoological Station in Naples. His sample of shore crabs from the Bay of Naples consisted of 1000 adult females and this was to be compared with 1000 adult female shore crabs from Plymouth Sound. He now wanted to further examine the propositions he had set out in his papers of 1890 and 1892, but this time he wanted to

37 Weldon, 'Certain Correlated Variations in *Crangon vulgaris*', 9.
38 *Ibid.*
39 *Ibid.*, 11.

use the shore crab rather than the common shrimp.[40] After Weldon had done some preliminary calculations of the correlations, with the assistance of his wife in the summer of 1893, he wrote this letter to Pearson on 27 November 1892:

> Out of the mouths of babes and suckling hath he perfected praise! In the last few evenings I have wrested with a double humped curve, and have overthrown it.
> Enclosed is a diagram. It represents 1000 measures of an organ in Naples crabs, Body length was taken as 1000, But as observations were only good to 0.1 mm., and as body length was about 25mm., the results, recorded to 001 body length, were thrown into groups of 4 and plotted, Now the resulting curve was clearly asymmetrical (see inked curve of diagram):
>
> I therefore took a table of the Probability curve, and drew curves whose sum seemed to fit the observations. The curves chosen are drawn on the diagram, together with their sums. These curves were drawn without any attempt to ascertain their meaning. Then the maximum ordinate and modulus were used to determine the population represented by the curve.
>
> 
>
> Result: 691.9 individuals in the larger, 309.7 individuals in the smaller. Sum, 1001.6 instead of 1000. As you can see, the arithmetic mean lengths of the organ in the curves are 653.5 and 625.5 respectively. Mean length in whole population from these data = 644.8. The A.M. [arithmetical mean] of all the measurements gives 644.7 as the mean lenghth in the population.
>
> Please observe the irregularity in observed curve as greatest where P.E. [probable error] is greatest—where each ordinate is the sum of two curves. Each of which has its own P.E.
>
> If you scoff at this, I shall never forgive you.[41]

By the end of the summer of 1893, Weldon submitted his paper to the Royal Society. He found that the results from the shore crab confirmed his results from his 1892 paper when he showed that the distributions of a number of characters in the common shrimp were normally distributed. When Weldon was looking for variations in the shore crab in Plymouth and Naples, he made a total of 11 measurements (see Table 3.2) which are listed along with a diagram of the carapace (see Figure 3.4)—the right half represents an average of Plymouth crabs and the left side represents an average crab from Naples.

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40 W.F.R. Weldon, 'On Certain Correlated Variations in *Carcinus mœnas*', *Proceedings of the Royal Society*, 54 (1893). Karl Pearson, 'Walter Frank Raphael Weldon, 1860-1906', 28.

41 W.F.R. Weldon, Letter to Karl Pearson, 27 November 1892, KP:UCL.

Table 3.2
WELDON'S 11 MEASUREMENTS OF NAPLES CRABS (1892)

1. *The total length of the carapace* (fig. 1, AB), in a straight line from the tip of the median inter-orbital tooth to the middle of the posterior margin.

2. *The total breadth* of the carapace, in a straight line from tip to tip of the posterior lateral teeth (fig. 1, EF).

3. *The frontal breadth*, from tip to tip of the anterior lateral teeth (fig. 1, CD).

4. *The right antero-lateral margin*, from the tip of the median inter-orbital tooth to the tip of the postero-lateral tooth (fig. 1, AF).

5. *The right dentary margin*, measured in a straight line from the tip of the antero-lateral to the tip of the postero-lateral tooth (fig 1, DF).

6. *The left antero-lateral margin*, measured in the same way as the right.

7. *The left dentary margin.*

8. *The sternal breadth*, measured between the articulations of the great chelæ.

9. *The meropodite of the right chela*, measured in a straight line between the inner articulations.

10. *The carpopodite of the right chela*, from the inner articulation, in a straight line to the tip.

11. *The proximal portion of the same carpopodite*, in a straight line from the inner articulation to the tip of the inner spine, at the base of the dactylopodite.

Figure 3.4
DIAGRAM OF CARAPACE: PLYMOUTH CRAB ON RIGHT HALF AND NAPLES CRAB ON LEFT HALF (1893)

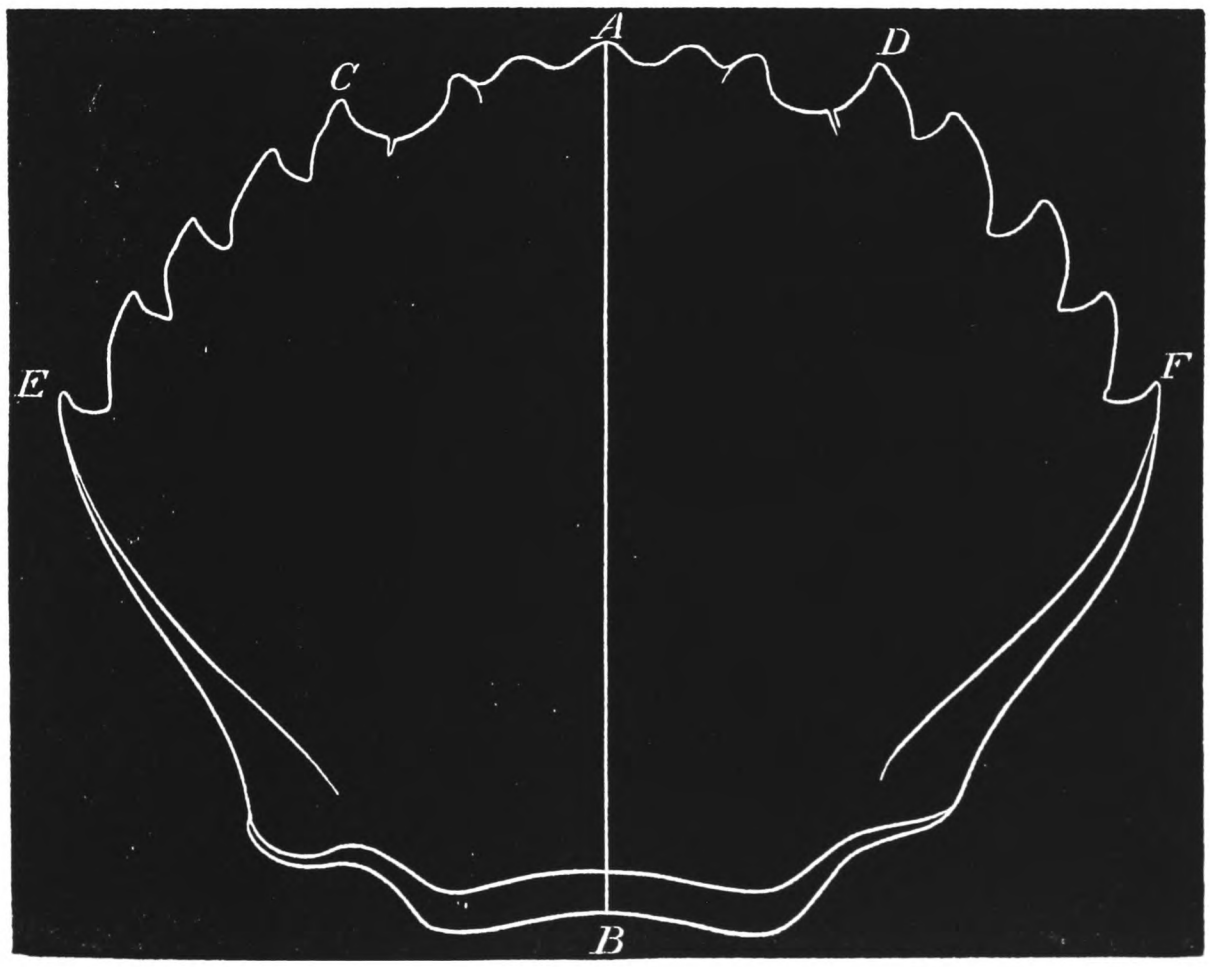


FIG. 1.—Diagram to show the parts of the carapace measured. The diagram is drawn to scale, the right half representing a perfectly average Plymouth crab, the left an average crab from Naples.

The only case where an asymmetric distribution appeared, rather than a normal distribution, was that of the frontal breadth (Weldon's #3) of the Naples specimen. Weldon had 'hoped that the results obtained might [have arisen] from the presence, in the sample measured, of two races of individuals clustered symmetrically about [two] separate mean [values]'[42] To test this supposition, Weldon wrote that

> Professor Karl Pearson has been kind enough to test this supposition for me; he finds that the observed distribution corresponds fairly well with that resulting from two series of individuals, one with a mean frontal breadth of 630.02 thousandths, and a probable error of 12.06 thousandths; the other with a mean breadth of 654.66 thousandths, and a probable error of 8.44 thousandths. Of the first race, Professor Pearson's calculation gives 414.5 individuals, of the second, 585.5. The degree of accuracy with which this hypothesis fits the observations may be gathered from Figure [3.5].[43]

Weldon then concluded, 'We may, therefore, assume that the female *Carcinus mœnas* is slightly dimorphic in Naples with respect to its frontal breadth'.[44]

Figure 3.5
Distribution of observed frontal breadth in Naples Specimen (1893)

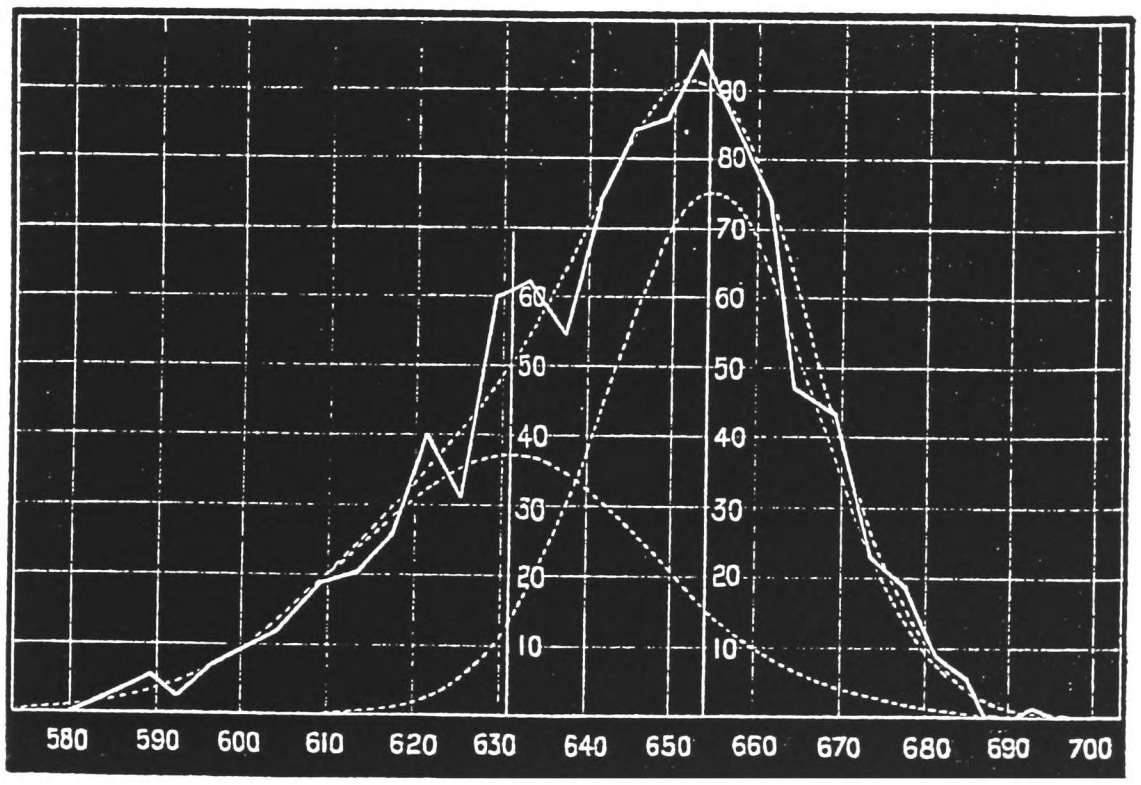


FIG. 3.—Diagram to show the distribution of all observed frontal breadths in the Naples specimens. The horizontal scale represents thousandths of the carapace length, the vertical scale numbers of individuals. Each ordinate of the upper dotted curve is the sum of the corresponding ordinates of the two component curves.

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42 Weldon, 'On Certain Correlated Variations in *Carcinus mœnas*', 324.
43 *Ibid.*
44 *Ibid.*, 328.

When Weldon began to tackle the calculations of the correlation coefficients he would use in this paper, to further test the biometrical species criterion, he wrote to Pearson on 1 June 1893:

> I enclose a table of Plymouth foreheads. Could you send me the results of your analysis of the Naples curves on a postcard soon? I should like to spend Saturday and Sunday working out the correlation coefficients based upon them! This is the the sort of agreement between corresponding coefficents. [The pairs of numbers listed under 'organs', are the same numbers that correspond to the diagram above for the measurements that Weldon made.]

[Table 3.3]

ORGANS	NAPLES	PLYMOUTH
2 & 3	0.11	0.13
2 & 4	0.66	0.65
2 & 8	0.15	0.15
4 & 6	0.76	0.78
5 & 7	0.82	0.83

> and so on: the worst being 0.63 in one case and 0.69 in the other. With the pairs made with organ 3 (the double-humped curve of Naples), the coefficients are:

[Table 3.4]

NAPLES	PLYMOUTH
0.41	0.18
0.42	0.20

> and so on—a remarkably greater divergence; and I am anxious to know whether this is a real thing or a consequence of my bad shot at an anlysis of the double-curve.
>
> If the coefficient involving the double-humped curve can be brought by your analysis as near together as others, it does seem to me that for purposes of empirically forming an idea of the distribution of variations in crabs, the identity of the correlation in the 2 races may be assumed: and it will become worth while to measure a much larger number of individuals in two races to see whether the thing is really and properly true?[45]

At the end of the month, Weldon wrote to Pearson reminding him that:

> I thought I had told you of my change of plan, I have long ago substituted arithmetic mean and mean error for median and quartile. The modulus of 4 from Mean Error comes out 3.84. What is the theoretical advantage of E.M.S. [Error Mean Square] over this?
>
> If you will show me, I will certainly work out the necessary arithmetic. But I cannot see why one value is preferable to the other!" [46]

Though there are no letters extant from Pearson to Weldon during this time, the next letter to Pearson would seem to indicate that he wrote a letter to answer Weldon's letter of 1 June. In this letter of 6 August, Weldon wrote:

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45 Weldon, Letter to Karl Pearson, 1 June 1893, KP:UCL.

46 Weldon, Letter to Karl Pearson, 21 June 1893, KP:UCL.

> You are a most charming person! All that weary analysis had to be gone through by some one some day; and I am very grateful to you for having done it! I have lots more puzzles for you next term!—In the first place, I have measured 2,000 little crabs from 6-10mm long so that I get about 400 of each group (group = individuals less than 1mm)—The 'standard deviation' of these little wretches increases as they grow older with repect to the two organs measured. But 400 are not enough because the mean value is changing so fast with growth that the curve is evidently flat on the top—I must measure more.—What becomes of the doctrine of Natural Selection, if I am not hopelessly wrong I cannot conceive.[47]

In the second part of the paper, Weldon then wanted to examine the degree of the correlation between two organs using the method that Galton had first introduced 'which may fitly be known as "Galton's functions"'.[48] He obtained a measure of correlation between the right antero-margin (Weldon's #4) from the list at the beginning) with the left antero-margin (Weldon's #6) from both sets of shore crabs. These produced strong positive correlations with an *r* of 0.76 for Naples and an *r* of 0.78 for Plymouth (just as he had indicated to Pearson in the above-mentioned letter). Table 3.5 lists the 23 sets of correlation coefficients he calculated from 23 pairs of organs from both Plymouth and Naples. In concluding, Weldon wrote that

> It may, therefore, be asserted that the investigation which has been described *does not demonstrate a difference* between the value of Galton's function for a given pair of organs in Naples and the corresponding value in Plymouth.[49]

As the values of the correlations for Weldon's crabs in Naples and in Plymouth (see Table 3.3) were quite similar, his results suggested that correlation could be used as a criterion for the constancy of relation that Galton had first suggested in 1889. Moreover, since these findings were similar to his shrimp data of 1892, this seemed to have provided further evidence for Weldon that 'Galton's functions' could be used to determine the constancy of correlation within local races. A procedure for a potential 'biometrical species criterion' looked promising as 'it may be assumed as at least an empirical working rule that Galton's function has the same value in all local races'.[50] To what extent this rule was 'rigidly true' would need to be determined by further studies using larger samples. Weldon, thus, concluded that

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47 Weldon, Letter to Karl Pearson, 6 August 1893, KP:UCL.
48 Weldon, 'On Certain Correlated Variations in *Carcinus mœnas*', 325.
49 *Ibid.*, 328.
50 *Ibid.*, 329.

> it cannot be too strongly urged that the problem of animal evolution is essentially a statistical problem: that before we can properly estimate the changes at present going on in a race or species we must know accurately (a) the percentage of animals which exhibit a given amount of abnormality with regard to a particular character; (b) the degree of abnormality of other organs which accompanies a given abnormality of one; (c) the differences between the death rate per cent. in animals of given degrees of abnormality with respect to any organ; (d) the abnormality of offspring in terms of the abnormality of parents and *vice versa*. These are all questions for a number of species we shall know the deviation and the rate of change in these species at the present day - a knowledge which is the only legitimate basis for speculations as to their past history, and future fate.[51]

In his letter to Pearson, it can be seen that the values in Table 3.4 do not indicate the same kind of constancy of relation for organ 3 (the frontal breadth of the carapace). The correlation of 0.41 and 0.42 in the Naples crabs compared to 0.18 and 0.20 in the Plymouth crabs, showed a greater discrepancy than the correlations of the other organs measured in Table 3.3. Weldon referred to the results from Table 3.4 as the 'double humped curve of Naples' as he found that the distribution of the frontal breadth for the Naples specimens was 'undoubtedly asymmetrical'. The statistical interpretation of the double-humped curve was to lead to Pearson's first statistical paper in the corpus of Pearsonian statistics and to his seminal work on goodness-of-fit testing.

It would not be for another two years before Pearson established the mathematical properties of Galton's correlation. Pearson would then show that the correlation coefficient could not be used as a diagnostic procedure for the identification of species. By then he would become convinced that the coefficient of correlation between any two organs, 'is just as peculiar and characteristic of a local race as the means and variation of those organs, In fact, if local races be the outcome of natural selection, then their coefficients of correlation must in general differ'.[52]

By 1902, Pearson concluded that it was very unlikely that 'Galton's functions' could be used as a biometrical species criterion. He claimed that 'if local races [had] been produced by selection from a common stock, it [was] impossible to look upon

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51 *Ibid.*

52 Karl Pearson, 'MCTE. III. Regression, Heredity and Panmixia', *Philosophical Transactions of the Royal Society*, A 187 (1896), 255.

correlation as a criterion for species.'[53] Correlation could be seen, instead, as being 'Just as much the outcome of selection as the coefficient of variation, standard deviation or even the mean size of organ.[54]

Of Weldon's attempts to find a biometrical criterion for the identification of species, Pearson was to write later that

> the credit of making the vast system of measurements, of carrying the necessary calculations (with the assistance of his wife), of seeing *a priori* the bearing of his result on the great problems of evolution, must be given to Weldon [who] *was on the look-out for a numerical measureof species.*[55] (Italics mine.)

ASYMMETRICAL FREQUENCY CURVES

In his 1893 Abstract and full paper of 1894 on the 'Contributions to the Mathematical Theory of Evolution', Pearson published his first measure of a goodness of fit test. This was derived from the sixth moment from his *method of moments* which he developed after examining Weldon's data from the carapace of female crabs in Naples. Pearson had introduced his method of moments in his second Gresham lecture on the 'Normal Curve' in November 1892 as a means of fitting a theoretical curve to observational data.[56] When Pearson was discussing Weldon's data in his third lecture on the 'Normal Curve' the following day, he compared the normal curve with 'double humped and asymmetrical frequency curves' from Weldon's crabs (see Figure 3.6.1).[57] He considered that 'selection could produce such a double-humped curve'; on 'the question of species', he then dissected this curve into two normal curves (see Figure 3.6.2.). His treatment of distributions might have been able to offer an 'exact measure of species or dimorphism'.[58] Thus, if two normal distributions could have been extracted from one double humped curve, this seemed to have indicated to Pearson that two separate species had arisen.

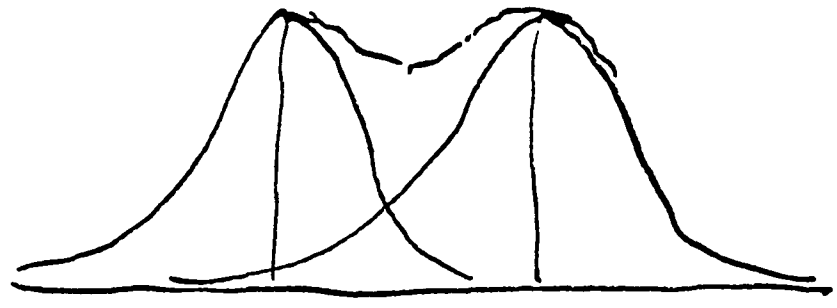
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53 Karl Pearson, 'MCTE. XI. On the Influence of Natural Selection on the Variability and Correlation of Inheritance', *Philosophical Transactions of the Royal Society*, A 200 (1902), 29.
54 *Ibid.*, 18.
55 Pearson, 'Walter Frank Raphael Weldon, 1860-1906', 283.
56 Karl Pearson, 'Normal Curves' (Part 2), Gresham Lecture, November 1892, KP:UCL, pp.11-14.
57 Karl Pearson, 'Conclusions to the Normal Curve', Gresham Lecture, November 1892, KP:UCL, p.7.
58 *Ibid.*

Figure 3.6.1
WELDON'S DOUBLE HUMPED CURVE
from Pearson's Lecture on 'Normal Curve' (1892)



Figure 3.6.2
PEARSON'S DISSECTION OF TWO NORMAL CURVES
using Weldon's data from his Gresham Lecture (1892)



In this abstract, he also introduced the standard deviation (which could measure variation at all data points), he defined a standard frequency curve, the normal (or symmetrical) frequency curve as well as

> curves which do not take the normal and are generally, but not necessarily, asymmetrical, Such abnormal curves arise particularly in biological measurements; they have been found by Professor Weldon, for the measurements of a certain organ in crabs, by Mr. Thompson, for prawns, and by Mr. Bateson, for earwigs, they occur, however in physics, e.g. Dr. Venn's barometric and thermometric frequency curves... and in fever mortality statistics; in economics, Mr. Edgeworth's curves of prices, and curves I have drawn for rates of interest.[59]

Asymmetrical frequency curves were of three types: (1) curves which were best represented by a point-binomial, or by its limit a continuous curve; (2) curves which had two or more normal curves with different positions of axes and different areas and different standard deviations; and (3) curves which were a compound of two or more normal curves having coincident axes, but different areas and standard deviations, or with the same areas and standard deviations but different axes.[60] The first curve was based on the binomial distribution (see Figure 3.7.1), the second type was a result of skewness (which is shown in Figure 3.7.2) and the third, is due to a form 'kurtosis' a term Pearson introduced as a measure of the peakedness or flatness of the distribution (see Figure 3.7.3).[61]

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59 Karl Pearson, 'Contributions to the Mathematical Theory of Evolution', *Proceedings of the Royal Society* (Abstract), (18 October 1893), 330.
60 *Ibid.*
61 *Ibid.*, 331.

For the first type of curve, Pearson recommended using the binomial distribution $\alpha(p+q)^n$ where p = the probability in favour of an isolated event, q = the probability against and n = the number of contributory 'causes' in a single trial. The importance of this curve resided 'in the fact that it measures the theoretical frequency number of *n* of contributory "causes" and the odds p:q that an element of deviation will be positive'.[62]

The first five moments about the arithmetic mean were used 'in the same manner as the determination of the normal curve by fitting any series of observation by aid of the area and the first two moments'.[63] The first moment gave the mean and the second, a measure of variation which for Pearson was the mean-square. The first moment about the mean is 0 and the second moment is (n - 1)/n times the unbiased sample error; the third moment can be used to measure the amount of skewness and the fourth, the degree of kurtosis. When working out the solutions for the second curve, the method led to what was

> termed the *fundamental nonic,* every root of which [gave] a real or imaginary solution of the problem. The best solution is selected by the criterion that it gives the closest approach to the given frequency curve in the value of the sixth moment. From the nonic is deduced a quadratic for the areas of the components corresponding to each solution.[64]

If both roots of the quadratic solution were 'real and positive' the frequency curve was then the sum of two normal curves. This meant that either there was a mixture of two heterogeneous species, or this was to have indicated that

> an evolution was breaking the homogeneous material up into families of different magnitudes, different means, and different standard deviations from the mean. If one root of the quadratic be real and positive and the other real and negative, we have evolution destroying a certain percentage round a certain mean out of an initially homogeneous group.[65]

When one of the roots was positive and the other negative, the given frequency curve was the difference of two probability curves.

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62 *Ibid.*
63 *Ibid.*, 332.
64 *Ibid.*
65 *Ibid.*

The last type of curve 'depended upon the equality of the areas for all six moments'. Rules were given for detecting whether there was a mixture of two heterogeneous groups or whether a 'differentiation into species of a homogeneous material is going on'. The most asymmetrical curve was to be selected and this would then be dissected by the method described for the second type of curve. The most symmetrical curve 'out of the curves for the organs measured' would be dissected into two groups by using all six moments. If

> the first dissection is possible and the second not, a real evolution is going on; ... if the first dissection is possible and the second is possible, and both groups give sensibly the same percentages, we have a mixture of two heterogeneous materials and no true evolution, unless the organs must vary directly with the other (e.g., length of right and left legs);... [finally] if both dissections are possible, but give groups with different percentages; we have *both* organs evolving differently at the same time.'[66]

When Pearson uses the word 'evolution', he quite often seems to be referring to speciation. Charles Darwin himself considered that the 'origin of species by natural selection' was the same as evolution.[67]

In the complete paper of 1894, Pearson gave a fuller account of the measurement of this process. He then showed that the theory could be applied to Professor Weldon's Naples crabs as

> his material is absolutely homogeneous, all roots of the nonic for No. 4 organ leading to the imaginary solutions, even its real root. On the other had, it is shown that the Naples crabs are breaking up into two different sized families owing to evolution in their foreheads. The theory was further applied to Mr. Thompson's measurements of the carapace of prawns.[68]

A week before Pearson sent his abstract to the Royal Society, he sent a note on 'Asymmetrical Frequency Curves' to *Nature*. (This paper, which was published on 26 October 1893, could be considered the first publication in the Pearsonian corpus of statistics.) Although he was not the first person to attempt to devise a method for curve-fitting asymmetrical frequency distributions, he does seem to have been one of

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66 *Ibid.*

67 Ernst Mayr, 'Isolation as an Evolutionary Factor', *Proceedings of the American Philosophical Society*, 103 (1959), 221-30.

68 Karl Pearson, 'Contributions to the Mathematical Theory of Evolution', *Proceedings of the Royal Society* (Abstract), (16 November 1893), 330.

Figure 3.7.1
BINOMIAL DISTRIBUTION

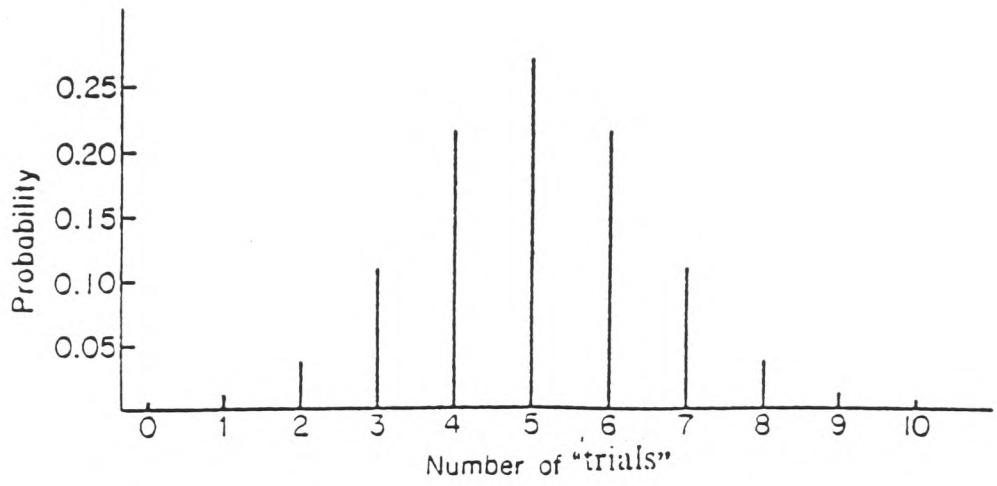


Figure 3.7.2
DISTRIBUTION WITH SKEWNESS

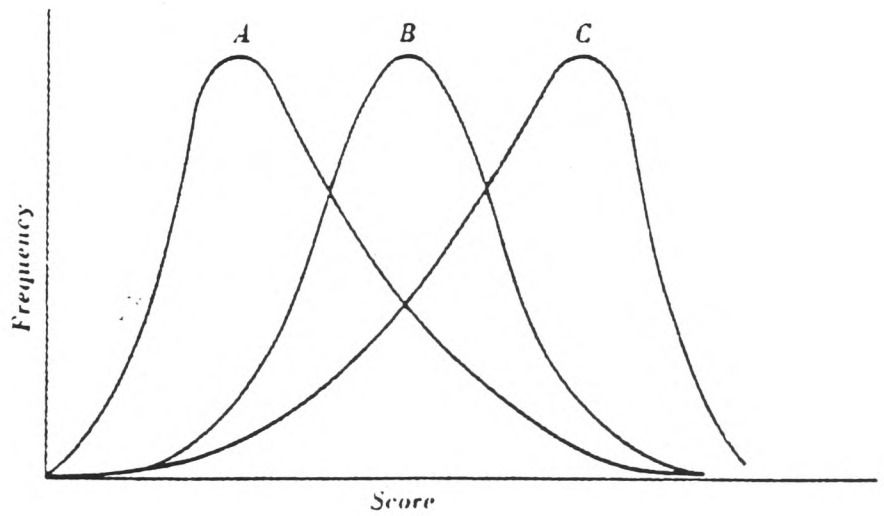
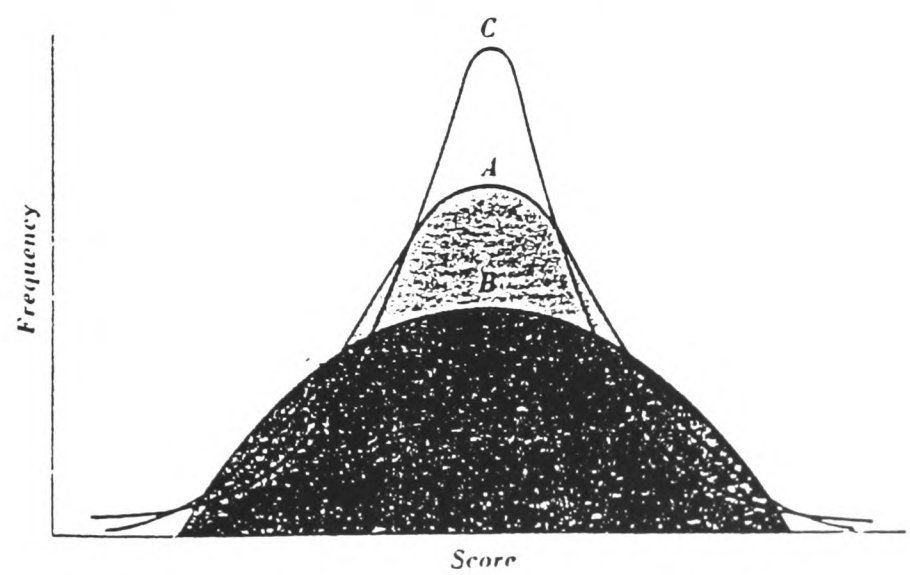


Figure 3.7.3
DISTRIBUTION WITH KURTOSIS



the first to have provided a systematic treatment for the examination of such frequency distributions with his *method of moments.*

John Venn had raised the problem of curve-fitting asymmetrical distributions some six years before. Venn had written to *Nature* on 1 September 1887, to challenge the assumption made by Adolphe Quetelet that all distributions should conform to the 'law of error' (i.e., the normal distribution). Venn was concerned that everyone who was

> interested in the theory of statistics [was] aware how strongly Quetelet was under the conviction that there is only one law of error (or curve of facility, to use the corresponding expression for the graphical representation of the law) prevalent for the departure from the mean of a number of magnitudes or measurement of any natural phenomenon. I have done what I can to protest against this doctrine as a theoretic assumption; and recently Mr. F. Galton and Mr. F.Y. Edgeworth have shown... how untenable it is, and how great is the importance of studying the properties of other laws of error rather than the symmetrical binomial, and its limiting form the exponential.[69]

Venn had been making calculations from metereological data, and presented a graph of 4,857 barometric heights taken in 1852 (which was slightly skewed—see Figure 3.8) so that he could ask the readers of *Nature:*

> (1) Do the magnitudes, when arranged in order of their departure from the mean, display a symmetrical arrangement?
>
> (2) If so, is this arrangement in accordance with the binomial or exponential law?

Venn presented his second graph from 4380 thermometric observations that were 'tolerably symmetrical, but they depart[ed] widely from anything approaching to Quetelet's fixed type' (see Figure 3.9). He considered the possibility that there might be some 'blunder' in his calculations, but he had done everything he could to guard himself from this situation occuring,

From this he hoped that such results would bring forth some discussion by

> practised experts in this branch of statistical inquiry which may serve to confirm or correct [his] results... but the statistics of metereology are so vastly extensive that it is impossible for any but a professional student of the subject to be acquainted with what goes on in it.[70]

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69 John Venn, 'Law of Error', *Nature* (1 September 1887), 411.
70 *Ibid.*, 412.

Figure 3.8
JOHN VENN'S DISTRIBUTION OF 4,857 BAROMETRIC HEIGHTS

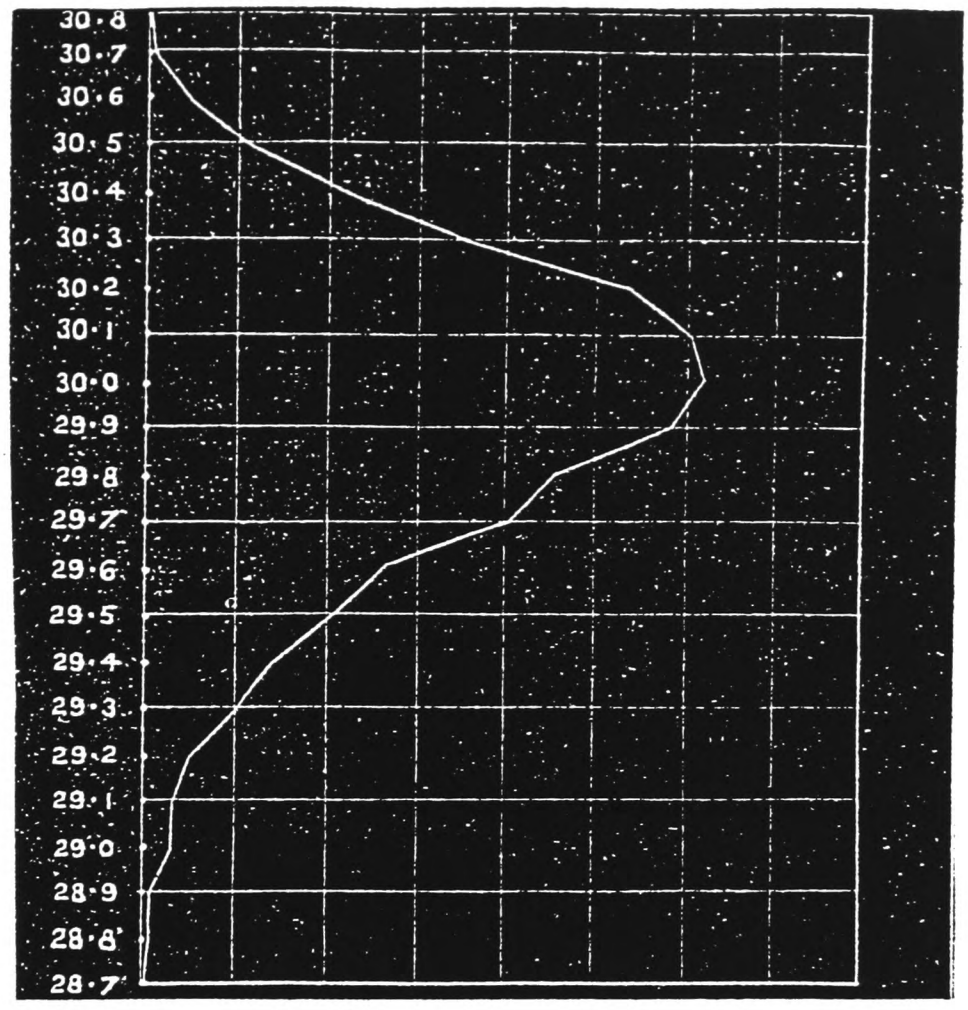
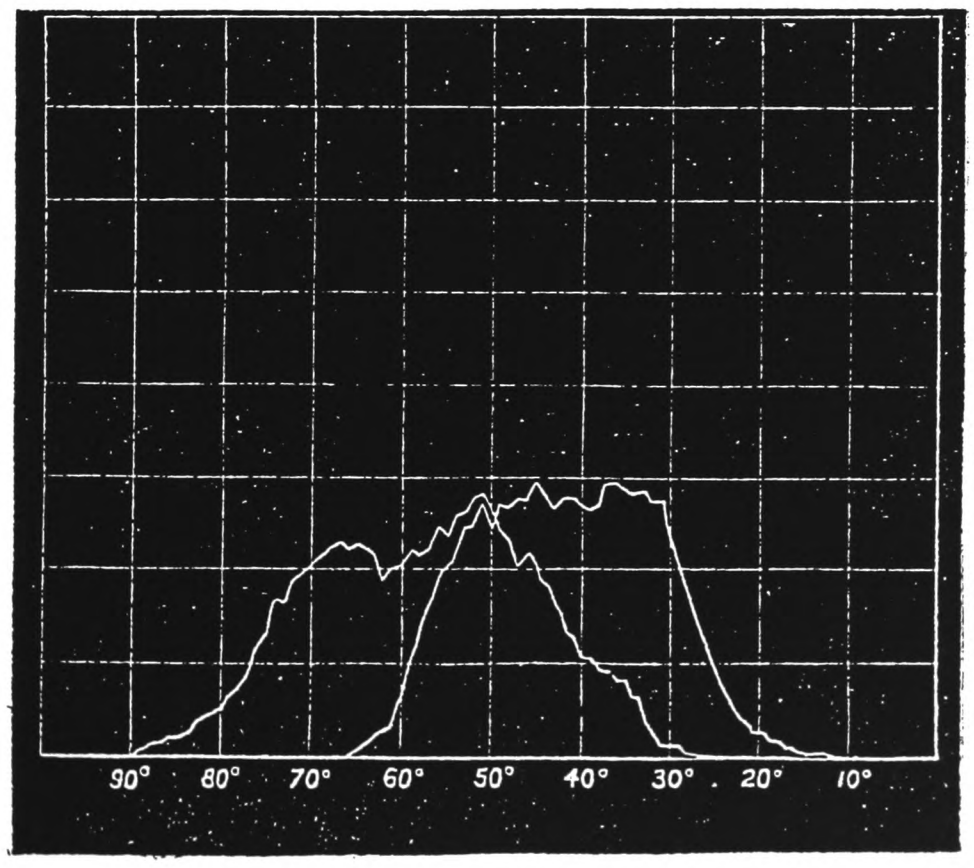


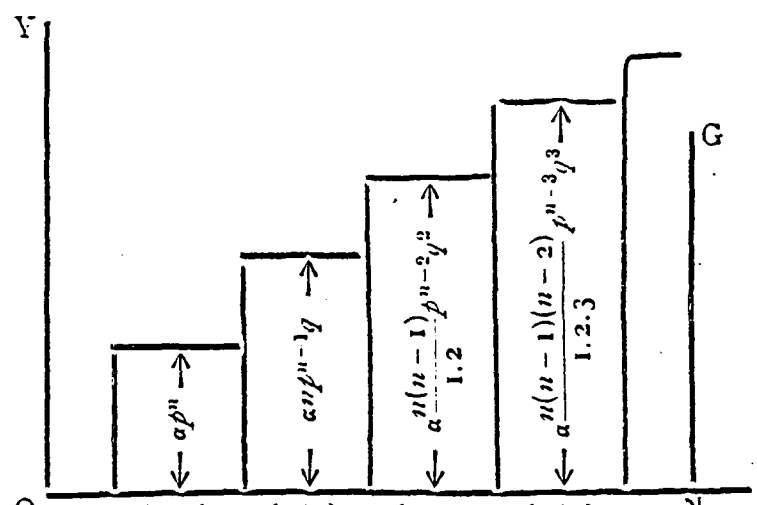
Figure 3.9
JOHN VENN'S DISTRIBUTION OF 4,830 THERMOMETRIC OBSERVATIONS



In his letter to *Nature* in 1893, Pearson went on to say, 'I have recently obtained a generalised form of the probability curve which fits with a great degree of accuracy such curves, and propose to discuss it at length shortly.'[71]

Pearson wanted to show that the asymmetrical point binomial (see Figure 3.10) could be used to fit such curves as the ones Venn had shown and the type that Weldon had encountered, though the curve-fitting would not be as 'complete' as that of the continuous (i.e., Venn's exponential) curve. He proceeded not by the method

Figure 3.10
PEARSON'S ASYMMETRICAL POINT BINOMIAL



where the successive heights are the terms in

$$a\left(p^n + np^{n-1}q + \frac{n(n-1)}{1.2}p^{n-2}q^2 + \ldots\right).$$

suggested by Professor Edgeworth in 1886 in his 'Law of Error and the Elimination of Chance', instead, he proceeded by a method of higher moments.

When Edgeworth wrote his paper on the 'Law of Error and the Elimination of Chance', he had wanted to work out the probability for a set of observations unsymmetrically grouped such as those that might arise in a Life Table between the ages of 50 to 100. He then asked, 'How shall we perform the analogue of the operation which Mr. Galton performs for symmetrical binomials?' Edgeworth thought that the 'Poisson higher order approximation for the distribution of sums could be useful for this purpose'.[72] Steve Stigler has shown that

> Edgeworth's aim was to apply the tools developed in the previous century for *observations* in astronomy and geodesy, where a more or less objectively defined

71 Karl Pearson, 'Asymmetrical Frequency Curves', *Nature* (26 October 1893), 615.
72 Stigler, *History of Statistics*, p.331.

> goal made it possible to quantify and perhaps remove nonmeasurement error and meaningful to talk of the remaining variation as random error, to social and economic *statistics* where the goal was defined in terms of the measurements themselves and the size and character of the variation depended upon the classifications and subdivisions employed.[73]

Pearson offered a more generalised form of treatment for asymmetrical curves which could be applied to a variety of situations where asymmetrical curves would occur.

PEARSON'S FREQUENCY CURVES

From a utilitarian view, 'good results for a simple normal curve [were] obtained by finding the mean from the first moment, and the error of the mean square' (which Pearson had termed the 'standard deviation') from the second moment. It then seemed likely that his 'investigations based on the first five or six moments of the frequency-curve may also lead to good results'.[74] If a method of equating chosen ordinates of the given curve with those of the components was used instead, this would mean that each equation was based only on the measurement of one organ of one size. The advantage of using his system was that the 'method of moments use[d] all the given data in the case of each equation for the unknown, and errors in measurement will, thus, individually have less influence'.[75]

Pearson was nonetheless aware of certain disadvantages under which his method suffered such as 'the labourious character of the arithmetic involved and the question of what may be the probable error of the solution obtained by the method of the higher moments'.[76] Though a better solution might have been found and though other 'mathematically trained statisticians' had recognised the problem, 'no solution had, so far as [he] was aware, has hitherto been forthcoming'.[77]

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73 Steven Stigler, 'Francis Ysidro Edgeworth, Statistician', *Journal of the Royal Statistical Society*, Series A, 141 (1978), 296.

74 Karl Pearson, 'Contributions to the Mathematical Theory of Evolution', *Philosophical Transactions of the Royal Society*, A, 185 (1894), 75.

75 *Ibid.*

76 *Ibid.*, 108.

77 *Ibid.*

In this paper, he described the frequency curve as being constructed from the measurements of the same organ in several hundred or thousand specimens where the abscissa (X) represent the size of the organ and the ordinate (Y) the number of specimens falling within a specified range. Such curves, he argued, played a large part in the mathematical theory of evolution especially as this may have been used as an empirical method to determine the process of speciation which 'had been dealt with by Mr. Francis Galton, Professor Weldon and others'. In this paper, he plotted four frequency distributions all of which were based on Weldon's data from his crabs and prawns (see Plates 1 to 4).

The bulk of the remaining 25 pages dealt mainly with Weldon's crab data from Naples and Plymouth. To illustrate that these statistical methods weren't restricted to crabs, he also included measurements of prawns from Mr. Thompson. To determine if a frequency curve was a component of two normal curves was 'vital to the interpretation to be put on Professor Weldon's results.[78]

When a series of measurements approached the normal curve, Pearson argued that one could assume that the population was approaching a stable condition—one of neither production or destruction. He stressed that as these biological (as well as social, and economic) measurements did not often conform to the normal curve, a dissection of the frequency curve, to determine the direction (skewness) and the amount of deviations (homogeneity), was essential. Thus, if a frequency curve was assymetrical, and not symmetrical (or normal), and further assuming that the data was not homogeneous: the question then became: 'What is the best way to treat these data?' For Pearson, the solution was an equation for a dissection of a frequency curve into n normal curves, and for his first paper $n = 2$ which required the calculation of higher moments. His reasoning for a resolution into *two* curves, he believed, was of particular importance to evolution because:

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78 *Ibid.*, 95.

Plate 1

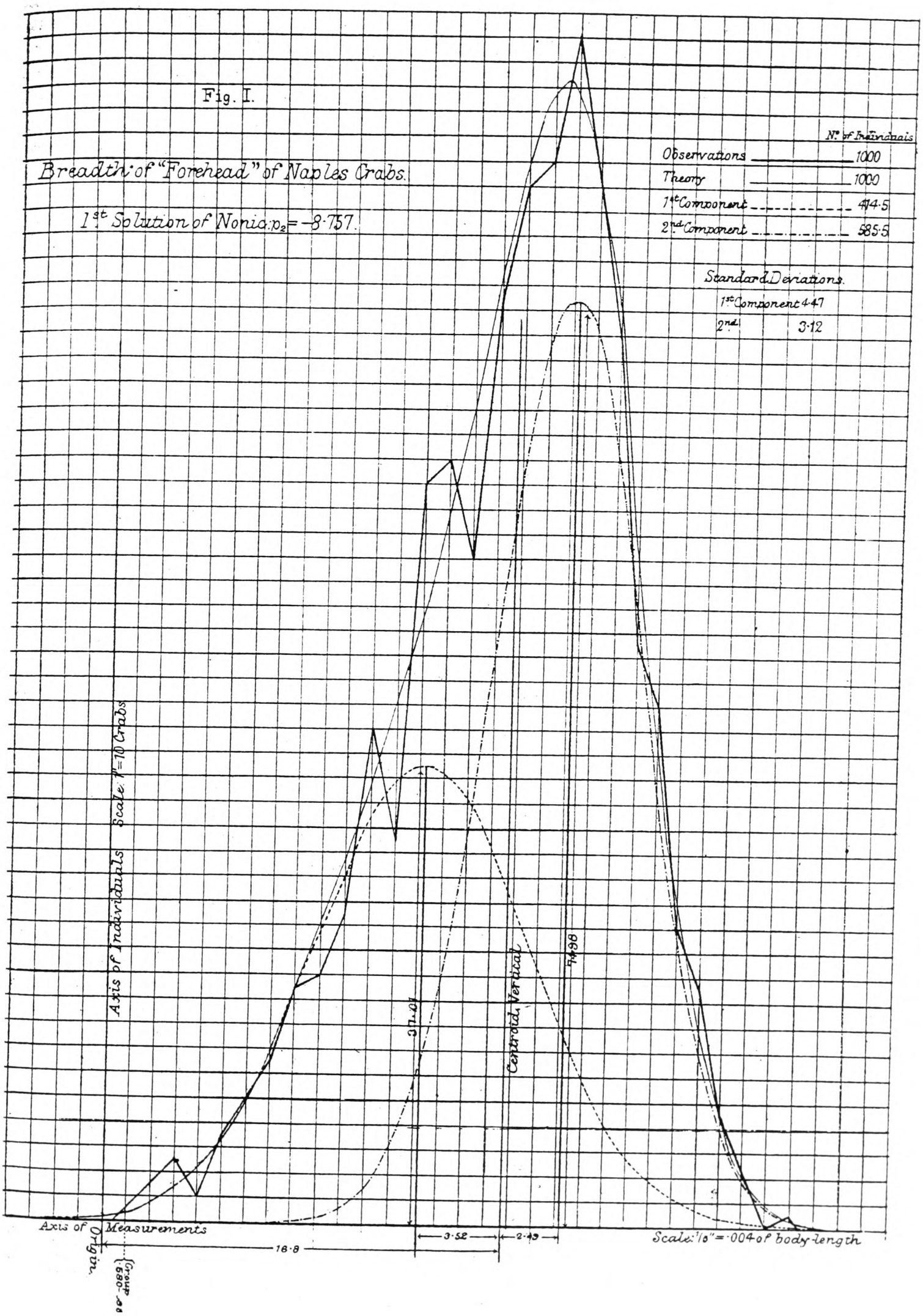

Fig. I.
Breadth of "Forehead" of Naples Crabs.
1st Solution of Nonia: p2 = -8·737.
No. of Individuals
Observations 1000
Theory 1000
1st Component 414·5
2nd Component 585·5
Standard Deviations.
1st Component 4·47
2nd 3·12
Axis of Individuals Scale: 1" = 10 Crabs
37·01
Centroid Vertical
74·98
Axis of Measurements
Origin
(Group 680-.68)
18·8
3·52
2·49
Scale: 1/8" = ·004 of body-length

Plate 2

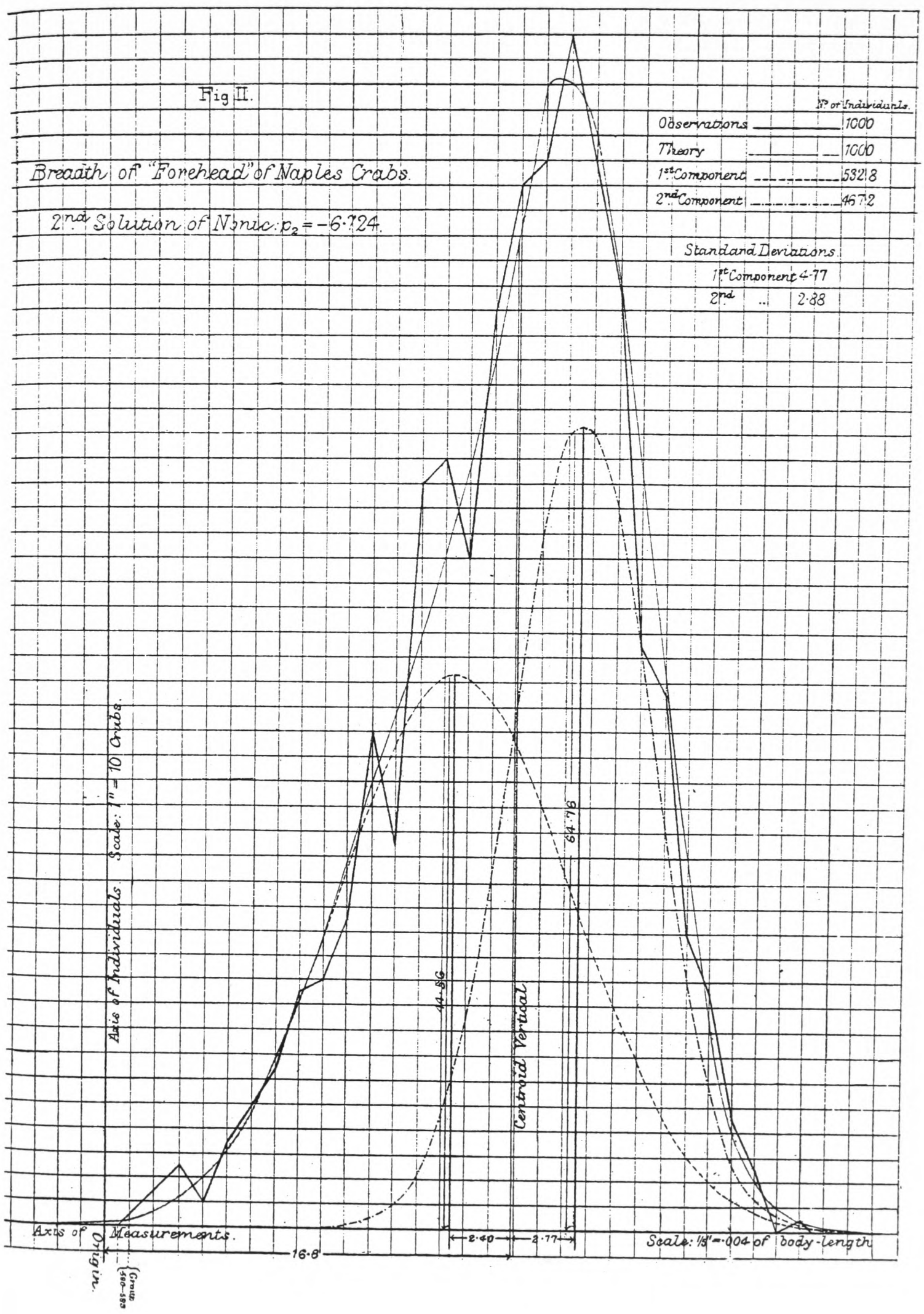

Fig II.
Breadth of "Forehead" of Naples Crabs.
2nd Solution of Nonic: $p_2 = -6{\cdot}724$.
No of Individuals.
Observations 1000
Theory 1000
1st Component 532·8
2nd Component 467·2
Standard Deviations
1st Component 4·77
2nd .. 2·88
Axis of Individuals. Scale: 1" = 10 Crabs.
64·16
44·86
Centroid Vertical
Origin
Axis of Measurements.
16·8
2·40
2·77
Scale: 1/5" = ·004 of body-length

Plate 3

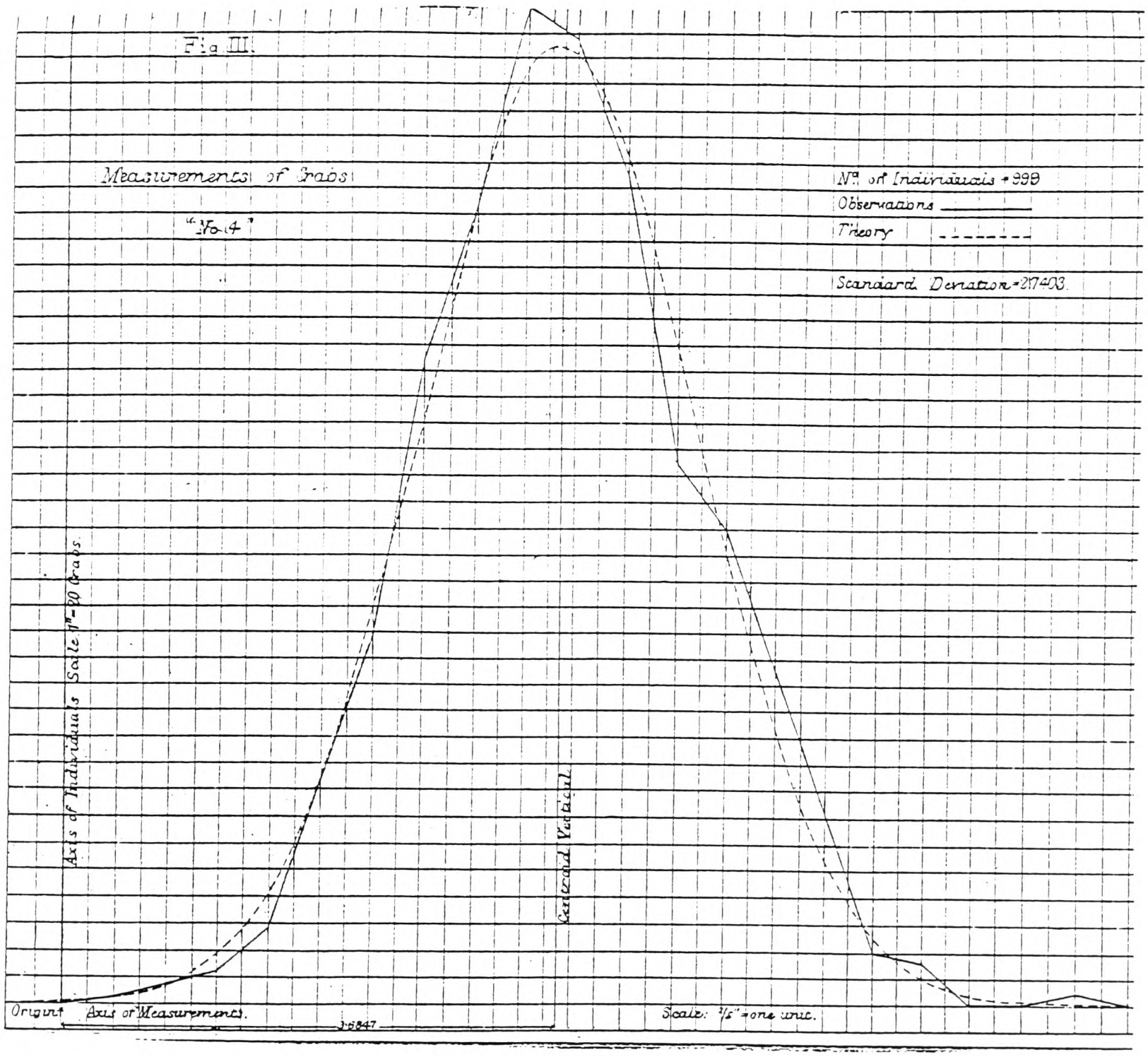
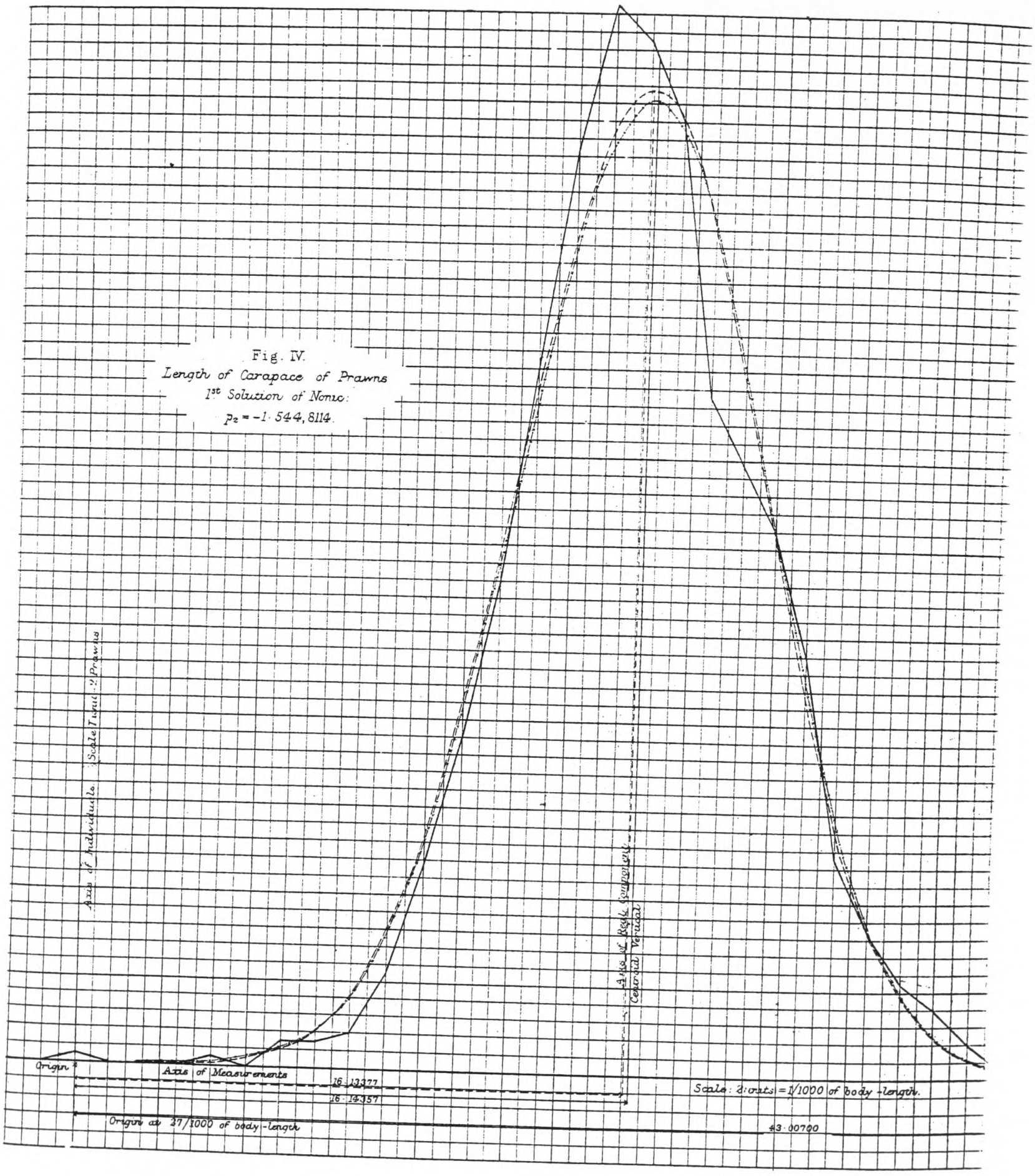

Fig III.
Measurements of Crabs
"Nola"
No. of Individuals = 999
Observations
Theory
Standard Deviation = 217403.
Axis of Individuals Scale 1" = 20 Crabs.
Centroid Vertical
Origin
Axis of Measurements.
3.6847
Scale: 3/5" = one unit.

Plate 4


Fig. IV.
Length of Carapace of Prawns
1st Solution of Nonic:
$p_2 = -1 \cdot 544,8114$
Axis of Individuals. Scale Twice = Prawns
Axis of Rock Superior Cervical Vertical
Origin
Axis of Measurements
16·13277
16·14357
Origin at 27/1000 of body-length
43·00700
Scale: 2 units = 1/1000 of body length.

> A family probably breaks up first into two species, rather than three or more, owing to the pressure at a given time of some particular form of natural selection; in attempting to procure an absolutely homogeneous material, we are less likely to have got a mixture of three or more heterogeneous groups than of two only.'[79]

SYMPATRIC SPECIATION

Speciation (i.e., the splitting of a single species into several species or the multiplication of species) is a normal and continuous process; it governs the greater part of the origin of species. Mutation is an abnormal and irregular mode of origin, which though not occurring frequently in nature is not essentially an adaptive process.[80] Though Pearson and Weldon seemed to have been interested in trying to find a way to determine speciation, the process was not to be understood fully until the middle of this century after the nature of geographic variation had been clarified.[81] Geographic speciation is not only one of the key theories of evolutionary biology today, but it is considered to be by far the most important mode of speciation among animals and usually the prevailing mode in plants.[82]

The prevailing concept of speciation among early workers 19th century was 'instantaneous speciation':

> the production of a single individual (or the offspring of a single mating) that [was] reproductively isolated from the species to which the parental stock belongs and that [was] reproductively and ecologically capable of establishing a new species population.[83]

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79 *Ibid.*, 72.

80 H.F. Osborn, 'The Origin of Species. V. Speciation and Mutation', *Amer. Nat.*, 61 (1927), 5-42. Cited in Mayr, *Animal Species and Evolution*, p.517. When Mayr defined speciation in his *Population, Species, and Evolution*, he remarked that 'more recently, it has become customary to restrict the term "speciation" to the process that leads to the multiplication of species through phyletic lines', p.248.

81 Mayr, *Animal Species and Evolution*, p.3.

82 *Ibid.*, p.481. In a recent article on 'Genetics and Speciation' in *Nature* (16 February 1992), Jerry A. Coyne commented that there 'are persistent claims, however, that geographic isolation is not in fact necessary, and that "sympatric speciation" may occur within a single interbreeding population. The likelihood of this process remains one of the most contested aspects of evolution' (511).

D.F. Roberts has argued that geography is just one of 'the factors promoting or maintaining isolation through geographical distance or geographical barriers that prevent or reduce gene flow. Others include ecological isolation... developmental factors... [and] behavioural factors... Mayr's classification of isolating mechanisms included ecological, ethological, mechanical factors and reduction of mating success as principal categories'. D.F. Roberts, 'The Galton Lecture for 1990: the Price of Isolation', in A.H. Bittles and D.F. Roberts (eds.), *Minority Populations: Genetics, Demography and Health* (Macmillan: London, 1992), p.35.

83 Mayr, *Population, Species and Evolution*, p.250.

Darwin, who referred to speciation as 'specification' or 'divergence' implied that geographical isolation and natural selection were 'alternate mechanisms in the production of species'.[84] He was rather uncertain as to the actual role of isolation during the speciation process. Though he never abandoned the concept of geographic speciation, Mayr noted that it was less emphasised in the 6th edition of the *Origin* and he began treating speciation as a process of adaptation.[85]

During much of the 19th century when typological concepts prevailed, no other mode of the origin of species seemed possible except sudden jumps or macromutations: the mode that many of the pre-Mendelians, particularly William Bateson, adopted.[86] The only apparent alternative was to go to the other extreme, to deny the existence of discontinuities altogether and to believe in the gradual origin of new species populations without geographical isolation by a process called sympatric speciation.[87] It is this process which Pearson and Weldon would most likely have considered. Evolutionary transformation of a species would not, however, necessarily lead to the multiplication of species: that is, an isolated population or an island might change in the course of time from species *a* through *b* and *c* into species *d* without ever splitting into several species.[88]

Ernst Mayr has argued that typological thinking was one of the major misconceptions imbued in nineteenth century evolutionary thinking. These ideas needed to be dismantled before a sound theory of evolution could be proposed as the

> assumptions of population thinking are diametrically opposed to those of the typologist. The population stresses the uniqueness of everything in the organic world. What is true for the human species, that no two individuals are alike, is equally true for all other species of animals and plants... All organisms and organic phenomena are composed of unique features and can be described collectively in statistical

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84 *Ibid.*, p.412; Peter Bowler, *The Eclipse of Darwinism* (Johns Hopkins University Press: Baltimore, 1992; original edition published 1983), p.31.

85 Mayr identified three reasons for Darwin's failure to appreciate the role of geographic isolation: 1) his confusion of evolutionary change with the multiplication of species; 2) his ambiguous use of the term 'variety' for individuals and for populations 3) the typologial species definitions made it impossible for him to appreciate the key aspects of the problem of speciation (p.484).

86 Mayr, *Animal Species and Evolution* (1963), p.482.

87 *Ibid.*

88 Mayr has described this type of evolutionary transformation in *Population, Species, and Evolution*, p.248.

> terms. Individuals, or any kind of organic entities, form populations of which we can determine the arithmetic mean and the statistics of variation. Averages are merely statistical abstractions; only the individuals of which the populations are composed have reality. The ultimate conclusions of the population thinker and of the typologist are precisely the opposite. For the typologist the type *(eidos)* is real and the variation is an illusion, while for the populationist the type (average) is an abstraction and only the variation is real. No two ways of looking at nature could be more different.[89]

This was a distinction Pearson made as he recognised that

> natural selection is not forced to choose an individual differing by a minute amount from the type. To hold this view is to think only in terms of the type, and not in terms of the whole population.[90]

Thus natural selection would have been meaningless to the essentialist as it could

> never touch the underlying essence; it [could] only eliminate deviations from the type. For him natural selection is by and large only a negative process, able to eliminate but unable to play a constructive role.[91]

Mayr also argued that nineteenth century evolutionists failed to appreciate the importance of populations and thought that they had to make a choice between selection and isolation.[92] The early Mendelians thought of speciation merely as a problem of individuals, of mutations, and of the acquisition of sterility: Bateson 'completely missed the point that speciation is the origin of discontinuity *not* between individuals but between populations'.[93] Thus, the existence of geographic variation was a source of considerable annoyance to Bateson and deVries as they attributed evolution to spectacular mutations.[94] That populations

> from different portions of the range of a species should differ from each other by 'gradual' characters, and that these differences should be greater the distance, was so completely in conflict with the mutationist interpretation of speciation that it forced the mutationists to deny the genetic nature of this variation.[95]

Mayr's concluded that speciation in sexually reproducing higher animals is not possible without geographic speciation.[96]

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89 Mayr, 'Isolation as an evolutionary factor', p.223.
90 Pearson, *Life of Galton*, vol. IIIb, p.123.
91 Mayr, *Animal Species and Evolution*, p.517.
92 *Ibid.*, p.485.
93 *Ibid.*, p.517.
94 Mayr, *Population, Species and Evolution*, p.186.
95 *Ibid.*
96 Mayr, *Animal Species and Evolution*, p.516.

THE ROYAL SOCIETY EVOLUTION COMMITTEE

By the end of 1893 (after Weldon's paper on the double-humped curve of the frontal breadth of female crabs in Naples and Pearson's Abstract of his first paper on the 'Contributions to the Mathematical Theory of Evolution' had been published), discussions were underway regarding the formation of the Evolution Committee. On 27 November 1893, Weldon wrote to Galton:

> I do not know whether you quite realise the ease with which one obtains a record of the changes in the crab, from the fact that he changes his shell, as he grows at frequent intervals. Now I carefully chose my measurements of fully grown crabs dimensions which could be measured: a moulted shell—that is, dimensions of parts which are not required during the process of moulting. I am making arrangements with the Laboratory at Plymouth, so that during the whole of next summer I shall keep at least 100 little crabs, in a separate vessel and feed them and let them grow and moult. It is surely worth while to take a great deal of trouble in order to see what is the value of your Function between the dimensions of the same organ in the same individual at different ages?[97]

One week later (on 4 December), Weldon wrote to Galton again that he thought that 'herring is the very beast of which you are in search'. It was said to be divided in Keil into two races; these two races were found in 'varying proportions through the Baltic, the North Sea and the Channel'.[98] He then informed Galton that he was 'writing to ask people to meet on Saturday at 3 at the *Savile Club*, 107 Piccadilly rather than at the Royal Society'. Weldon had already asked the Royal Society for the use of a room on Saturdays, but he was told that as 'it deprived the attendants of their half holiday', a room could not be made available.[99] Weldon suggested that they meet at a room in the Savile Club which was 'quite quiet [and] they could talk without distraction'.[100]

On 9 December Weldon, Galton and Raphael Meldola met informally, at the Savile Club, to discuss the possibility of forming such a committee to pursue the types of projects Weldon had suggested in his letter.[101] About two weeks after this meeting, Galton wrote a couple of drafts to express some of his own ideas about the

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97 W.F.R. Weldon, Letter to Francis Galton, *Miscellaneous Manuscripts*. Vol. 15, The Library of the Royal Society, #85, (27 November 1893).

98 W.F.R. Weldon, Letter to Francis Galton, 4 December 1893, FG:UCL.

99 *Ibid.*

100 *Ibid.*

101 Francis Galton, Minutes from the Evolution Committee Meeting of the Royal Society: RS CMM 65,492, from 18 Jan 1894 discussing the meeting of 9 December 1893.

purposes of the Committee. Parts of the draft appear to have been read at their first meeting on 18 January 1894. In his draft he wrote:

> To this end it has been agreed that the undermentioned [i.e., F. Darwin, F. Galton, A. Macalister, R. Medola, E.B. Poulton and W.F.R. Weldon] apply to the council of the Royal Society to form them into a committee 'for the purpose of conducting statistical inquires into the variability of organisms.'[102]

In both drafts he mentioned that 'the importance of statistical inquiry into the measurable characteristics of plants and animals, of a more minute and searching kind' had been discussed recently by Professor Weldon at the close of his paper 'On Certain Correlated Variation in *Carcinus mœnas'* which had been read at the Royal Society on 16 November 1893.[103]

In his first draft, Galton indicated why dimorphism was considered to be such an important phenomena to the Evolution Committee when he wrote that 'dimorphism is the first sign of progress in [the] evolution of species'.[104] This searching for dimorphism could be subject to statistical analysis as the degree of variability under different conditions, and the exact status of any species at any given time all require large amounts of statistical analysis before they can be numerically defined.[105] Though Galton mentions dimorphism in this first draft, he crossed out this passage in this draft; hence, no mention is made of dimorphism in the second draft—even though dimorphism was to be the central feature of Weldon's first project for the Evolution Commitee. This omission of dimorphism from the final report may suggest that Galton retained his saltational view of evolution.

Around this time, Weldon wrote a memorandum for the Committee on the 'Suggested Functions of the Committee'.[106] Though Weldon's name did not appear on the memorandum, it is thought to be his for the following reasons: (1) Galton's method of correlation is expressed as 'Galton's Functions' (i.e., the nomenclature used

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102 Francis Galton, *Miscellaneous Manuscripts*, vol 15, the Library of the Royal Society, #87 and #88 (26 December 1893).

103 *Ibid.*

104 Galton, *Miscellaneous Manuscripts*, vol 15.

105 *Ibid.*

106 [W.F.R. Weldon], "Suggested Functions of the Committee", *Miscellaneous Manuscripts*. Vol 15, The Library of the Royal Society, #72 [Written sometime between 18-25 January 1894].

only by Weldon); (2) the ideas put forth are characteristic of Weldon; (3) the animals suggested for the study included the common shrimp, the shore crab and herrings (i.e., the very same material Weldon used) and (4) it appears to be his handwriting. Though it is undated, it was most likely to have been written sometime after 18 January 1894 and probably before 25 January. The following seven 'suggested functions' were considered:

> 1. To organise a means for the collection of statistically adequate samples of measurable animals, eggs, bones, shells, plants, seeds etc. under conditions which shall ensure as nearly as may be the homogeneity of each sample.
>
> 2. To decide upon the dimensions which may be most profitably measured in each sample.
>
> 3. To organise the means for the measurement of each sample obtained, and for the reduction of the results of measurement curves of frequency and other suitable forms.
>
> 4. To determine the value of *Galton's function* in numerous cases; and especially to determine the corresponding values of the *function* (a) in several local races of one species and (b) in several closely allied species.
>
> 5. Considering that heredity is a special case of correlation, to organise experiments in a considerable number of plants and animals with a view to the determination of Galton's functions as between the parents severally [sic] their offspring.
>
> 6. To determine, in one species at least, the distribution of some one or more dimensions about their respective means (a) in a wild state and (b) in captivity.
>
> 7. To determine (a) the change in the error distribution of some one or more dimensions about their respective means during the growth of a generation in a wild state; (b) the change (if any) in the abnormality of various individuals in the same species during growth under various conditions of captivity.[107]

He then listed the types of animals that would be appropriate for each of the seven functions. For functions 1 through 4, he suggested *Crangon vulgaris* (the common shrimp), *Palnom serratus* (the common prawn) and other variants which were

107 *Ibid.*

'especially valuable, because of the various conditions of salinity under which it lives in the same area'. Herring, mackerel and sprat were also suggested. Function 4 (b) could be answered with *Carcinus mœnas* (the shore crab), *Portunus depurator* (the cleanser swimming crab), and *Portunus puber* (the velvet swimming crab).[108] For 5 and 6, he suggested *Leptopodia* and *Daphindae* (especially for sexual heredity.) *Carcinus mœnas* was considered appropriate for 7 because the shells moulted during growth leave a record of many dimensions which can be measured at leisure. Weldon concluded by saying he had already begun a 'determination of 7(a) for two dimensions of *Carcinus* beginning with individuals 6 millimetres long and proposing to go up to 25 mm long at least'.[109] He considered that 7(b) 'might be attacked by means of the same species as 7(a)'.

The committee was appointed at Galton's suggestion on 18 January 1894. It was then decided that a proposal should be made to the Royal Society 'for the very definite purposes of conducting statistical inquiry into the variability of organisms'.[110] It was also proposed that a grant of £50, from funds of the Royal Society be made to help with expenses incurred. The first formal meeting was to be held in one of the committee rooms at the Royal Society on 25 January 1894. The committee then consisted of Francis Darwin, Francis Galton, A. Macalister, Raphael Meldola, E.B. Poulton and W.F.R. Weldon, to which 'it may afterwards be desirable to add a statistician.'[111]

On 1 February, when Pearson was lecturing on 'Problems of Evolution' at Gresham College, he discussed a potential statistical resolution of dimorphism. Following an examination of biological frequency curves, the next stage was to determine whether the results from the 'statistics refer to a mixture of populations or

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108 For a comprehensive catalogue of British crustacea in the nineteenth century, see Thomas Bell, *A History of British Stalk-eyed Crustacea.* (John Van Voorst: London, 1834)

109 [W.F.R. Weldon], "Suggested Functions of the Committee", *Miscellaneous Manuscripts*. Vol 15, The Library of the Royal Society, #72 [Written sometime between 18-25 January 1894].

110 Francis Galton, Minutes from the Evolution Committee meeting of the Royal Society: January 25, 1894 CMB. 65.492; also in Pearson, *Life of Galton*, vol.IIIa (1930), p.126.

111 *Ibid.*

not?'[112] Bateson's measurements on earwigs seemed to suggest in itself 'all the problems that needed to be answered'. Pearson wanted to know if Bateson's data indicated any of the following:

> two different species in earwigs in every organ measured?—or a mixture of diseased earwigs in a general population? or are earwigs only double-humped curves for their prongs [i.e. pincers]?—Are earwigs like butterflies and many other things dimorphic.[113]

As Bateson had not given his data to Pearson, these questions could not be answered. Pearson, however, had Weldon's data on his measurements on some 20 to 30 organs of crabs. With this material it was then possible to make 'scientific distinctions between a pure mixture and between a pure species breaking up into two [groups] so far as one organ is concerned'.[114] He then explained that 'dimorphism [was] the first step in evolution'.[115] Moreover, the 'importance to the problem of evolution [was] the breaking up into species [as] distinguished from a simple mixture.[116]

DIMORPHISM IN HERRINGS: At the first meeting of the newly formed Evolution Committee, Weldon explained the reason why herrings appeared to be a suitable subject for a first investigation of dimorphism and the suggestion was approved. Professor Meldola suggested that 'beetles would be an easy subject and especially the cockshafer because of its extreme abundance'.[117] Sometime before the meeting, Weldon wrote a four page note on herrings which accompanied his application; this was very probably presented to the committee members before their first meeting of 25 January 1894. In this note he wrote:

> In attempting to study the process of animal modification, it is important to choose, as a subject of investigation, some species in which a farly rapid process of

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112 Karl Pearson, 'Lecture III. Problem in Evolution', (Delivered 1 February 1894) Gresham Lectures, KP:UCL, p.15.
113 *Ibid.*
114 *Ibid.*, p.16.
115 *Ibid.*
116 Karl Pearson, Syllabus of 'A Course of Lectures on the *Special Application of the Laws of Chance*, (Hilary Term, 1894), KP:UCL, p.3.
117 Galton, Minutes from the Evolution Committee Meeting: RS CMM 65,492, from Minutes of 25 January 1894 (Signed 1 February 1894).

> modification is in operation at the present time. The following reasons are offered in support of the belief that the common herring is such a species.[118]

The variation of the herring had been studied by officers of the Fisheries Board for Scotland, and by Dr. Heincke of Kiel whose observations were considered to be important to Weldon's project. It was found that the herring in the Gulf of Kiel varied in these six characters: length of head; length of ventral fin; the distance from snout to root of (i) the dorsal fin, (ii) the pelvic fin and (iii) the anus in addition to time and place of breeding. The herring were found to have bred 'chiefly either in Spring when they spawn in shallow, more brackish water, or in Autumn, when they spawn in deeper, salter water'.[119] The characters were considered to be 'imperfectly correlated' and the herring was 'dimorphic with respect to each of them'.[120] Heincke attempted to separate the Kiel herring into three structurally distinct groups which he labelled 'group A, group B and an intermediate group M'.

Weldon then outlined a 'type' of A and of B which can be seen in Figure 3.11. He thought that the correlations between the characters measured was so loose that 'typical' individuals could be considered rare; moreover, 'Heincke's method of classifying individuals into one or another group [was] quite artificial.'[121] Heicke reported his results by using percentages, but as they 'were based on a sample size of less than 300 individuals, [it] therefore need[ed] revision'.[122] Heincke made the following observations which Weldon summarised in Table 3.5.

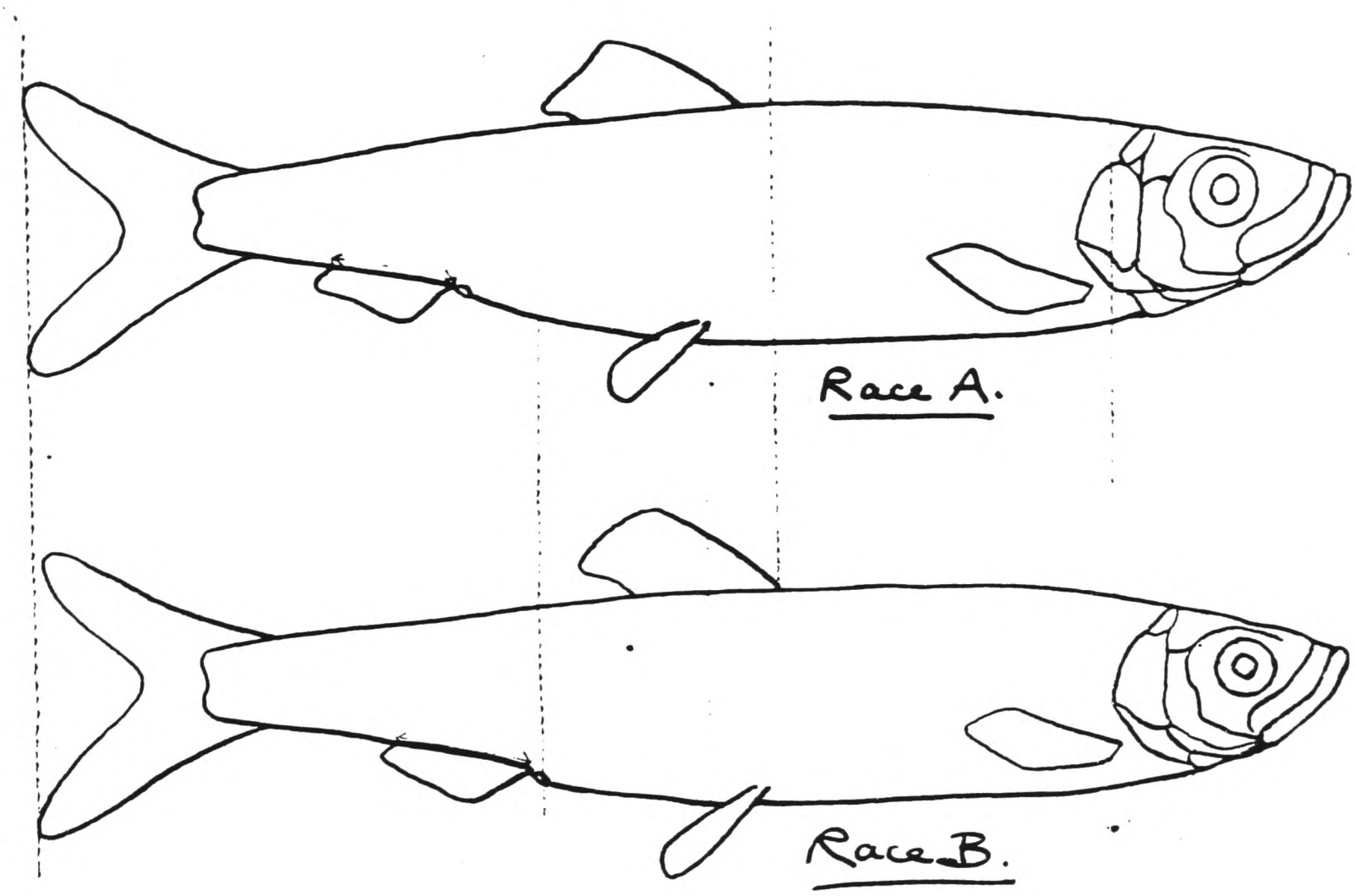
Table 3.5
GROUP BREEDERS PERCENTAGE OF POPULATION

A	Autumn	13.0%
	Spring	13.4%
	Intermediate	11.0%
B	Autumn	3.0%
	Spring	36.0%
	Intermediate	7.4%
C	Autumn	0.7%
	Spring	8.0%
	Intermediate	3.5%
		100.0%

Source: Weldon, 'A Note on Variation in Herring', Miscellaneous Manuscripts, #76, p.1

118 W.F.R. Weldon, 'A Note on Variation in Herring', *Miscellaneous Manuscripts*, Vol 15, The Royal Society, #76 p.1.
119 *Ibid.*
120 Ibid, p.2.
121 *Ibid.*

Figure 3.11
WELDON'S OUTLINE OF HENCKE'S FIGURE



Outlines traced from Heincke's figures of two typical Kiel Herring, one belonging to his Race A, the other to his Race B.

[Footnote continued from previous page]
122 Ibid, pp.2-3.

The percentage of A and B herring differed in different places. Weldon noted that in certain parts of Scotland the herring belonged almost entirely to the A type, whilst in the Channel both A and B types were nearly more equal in number than in Keil. The differences in characters were 'probably affected by Natural Selection'. Though Heincke made an attempt to estimate the variability throughout life, this was based on 'very insufficient data'. The evidence he brought forward created a strong presumption that variability, with respect to the characters referred to, [was] greater in the larva than in the adult. [Thus] the foregoing statements [were] considered to serve as a basis for extended statistical investigation. Their importance [was] increased by Heincke's statement that a precisely similar series of variations is found in the Sprat, and by the fact that the Pilchard is known to be variable.[123] In conclusion, Weldon wrote

> We have therefore three common, cheap and easily obtainable species of one genus, all living in the same area, and all exhibiting variations of considerable extent in characters which can be easily measured. The parallelism between the variation of two, at least of these species, besides its interest on other grounds, has this advantage, that any general conclusion which may be drawn from observations on one species can be checked readily by reference to the other.[124]

Following Weldon's suggestions, the Committee then decided upon three projects. The first project recommended to be taken up in 1893 was to test whether the method of resolution of two Gaussian curves, which suggested dimorphism in the Naples crabs (that Weldon had examined earlier in the year), could help to confirm this dimorphism that Weldon thought should exist in herring. (The next two projects, which involved measuring selective death rates during growth in the frontal breadths of the carapace in shore crabs from Plymouth Sound, will be examined in greater detail in the next chapter).

Weldon began his project on herring at the beginning of 1894 when 'several thousand herring' arrived at University College where a measurer was trained to deal with them with the intention of determining the 'variability of a wide series of

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123 Ibid, pp.3-4.
124 Ibid, p.6.

characters'. At the end of June, Weldon reported that 'about 1,000 herrings had been measured by Rip Jeffrey and he promised to arrange for further measures to work out a preliminary result.[125] When Weldon began this project, he had '1000 cards, of which about 700 were sorted with respect to another organ, and about 300 were sorted with respect to body length'.[126] Also at this meeting, Weldon submitted a preliminary result 'based on measures of about 5,200 young *Carcinus mœnas*'.

On 15 November, Weldon reported to the Committee that 1,900 herrings had been measured. After he communicated his results to the Committee, it was resolved that the measurements be stopped partly because of the time and costs involved.[127]. Moreover, the results were not what he nor the committee had expected. At this meeting Weldon 'presented a fuller account of the measurements of young crabs' and had also given 'an account of the effect of selection upon parthogenetic generations of *Daphnia magna*'. He was, however, requested to continue with these experiments.

Weldon wrote to Pearson on 3 January 1895 that he had not worked out the *moments* for the various series of herring, but he was 'planning to do so in a week's time'.[128] Five days later, Weldon asked Pearson '*not* [to] publish anything about the herrings as they belong[ed] to the Committee'.[129] He went on to say that he

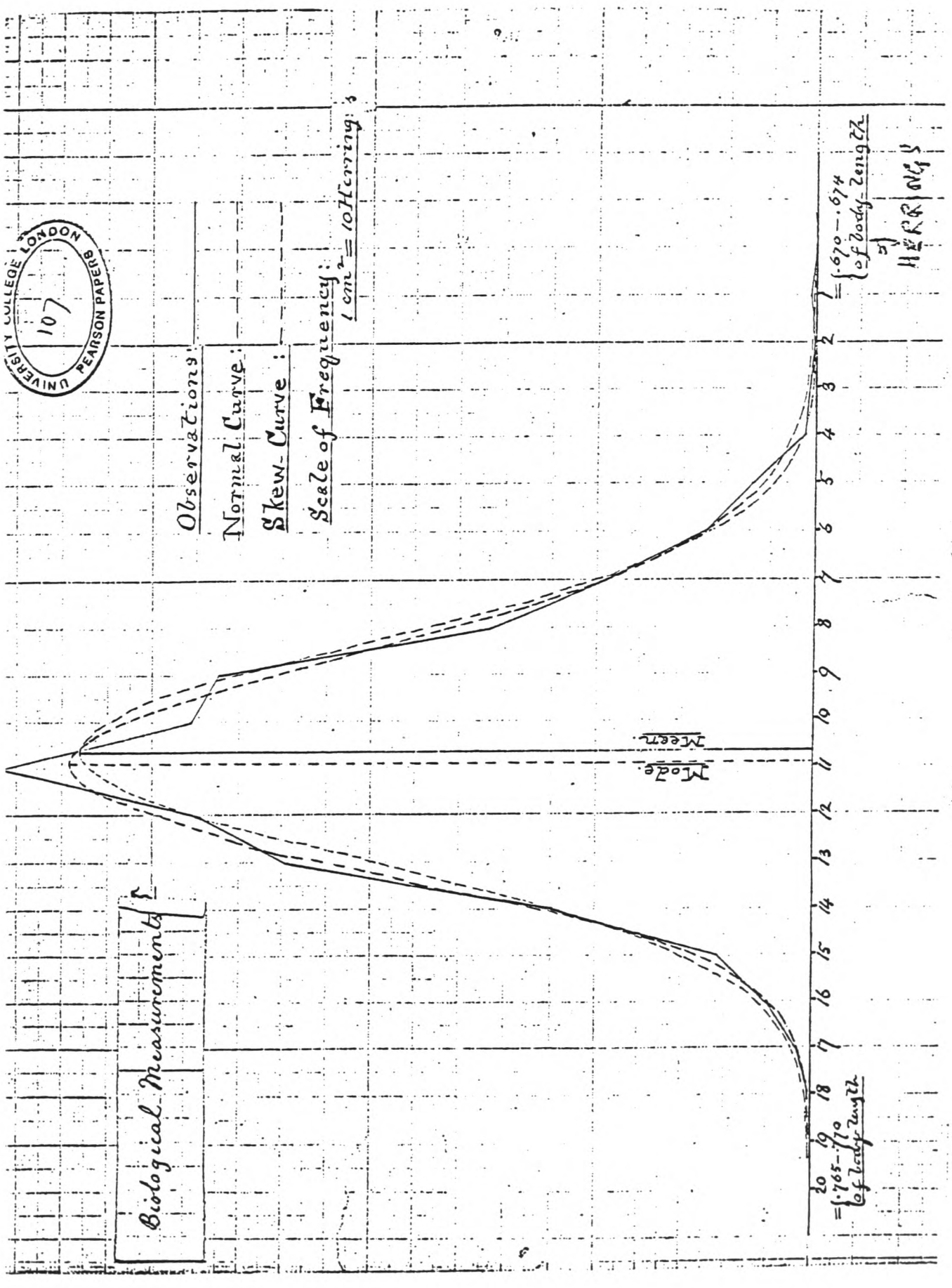
> never expected that the herring would yield normal curves. They were measured in the hope that all the characters would prove dimorphic. They are, however, to be treated as one species, because, so far as I at present see, the asymmetry of two organs will not lead to one way of breaking them up.[130]

The distributions from the herrings turned out to be skewed rather than 'double-humped' (see figure 3.12), as it had been for his crab data. Weldon 'was intensely hopeful that statistical evidence of dimorphism would be forthcoming, [however,] the analysis showed dimorphic Gaussian components to be impossible'.[131] Weldon put

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125 Galton, Minutes from the Evolution Committee Meeting: RS CMM 65,492, from Minutes read at the meeting of 6 July 1894.
126 Weldon, Letter to Karl Pearson, 3 January 1895, KP:UCL.
127 Galton, Minutes from the Evolution Committee Meeting: RS CMM 65,492, from Minutes read at the meeting of 15 November 1894.
128 *Ibid.*
129 Weldon, Letter to Karl Pearson, 8 January 1895, KP:UCL.
130 Ibid, pp.1-2.
131 Pearson, 'Walter Frank Raphael Weldon, 1860-1906', 290.

Figure 3.12
WELDON'S SKEWED DISTRIBUTION OF HERRING (1894)



the herring data aside and the results were never published. Pearson commented later that the result was a great disappointment to Weldon and to the Committee, though he could never understand why. Pearson thought that

> a set of very extensive and a valuable series of measurements had been made, which in themselves were well worth publishing. The data had reflected that simple dimorphism of a Gaussian kind did not hold for these herrings; in all probability it was a typical case of skew frequency which would have been valuable as adding to the known instances, and aiding statisticians eventually to classify such occurrences.[132]

From the limited amount of information available on Pearson's and Weldon's views of species divergence (or speciation), it is not clear what they expected to find. The overwhelming impression suggests that they were looking for dimorphism as a means of speciation. Dimorphism seems to have been regarded as the process involving one species leading up to the fragmentation into two species at the same time and in the same place—rather than something that represented two qualitative characters within the species. (It would thus seem that they thought that it was possible for an ancestral species to have occurred at the same time with the new species.)

The type of variation in most of the characters that Weldon examined, such as fin position in herrings or length and breadth of the carapace in crabs, was of a continuous form. As such this would have produced a range of different sizes rather than differentiating into two distinct groups. Thus these characters would not have shown the dimorphism that Weldon or the Evolution Committee was hoping to have found. Nevertheless, their unsuccessful attempt to find an empirical measure of speciation provided the impetus for Pearson's seminal statistical work on curve-fitting for asymmetrical distributions which, in turn, led to his first seminal statistical paper of 1893. Moreover, this was to establish the momentum for the development of Pearson's statistical innovations in the remaining years of the nineteenth century.

At a meeting held on 25 March 1895, Galton reported to the committee that the 'measurements of herrings were apparently not based upon homogeneous material:

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132 *Ibid.*

because when sorted in order of measurement, different samples gave different means'.[133] Weldon was then instructed, presumably from Galton, to report on his results 'by sorting the individuals in order of body length'.[134] At this meeting, Galton proposed that the council request to add the name of Mr. H.S. Burbury to the list of members of the Committee explaining that Mr. Burbury 'was willing to give the Cmtt. the advantage of his assistance in mathematical difficulties'.[135] This was carried out unanimously. Though Weldon sought Pearson's help from time to time, Pearson rarely attended any of the meetings. The only piece of correspondence from Pearson, extant in the Royal Society Evolution Committee collection, is his letter of resignation some five years later.

SKEWNESS AND THE PEARSONIAN FAMILY OF CURVES

About a week before Pearson submitted his second paper on the 'Mathematical Contributions to the Theory of Evolution: Skew Variation in Homogeneous Material', he wrote a book review for *Nature* on William Townsend Porter's *The Growth of St. Louis School Children.*[136] Pearson criticised Porter for simply

> tabulating results... [and] neglecting to publish the details which alone would enable us to test any development of statistical theory [as] the ignorance of the elements of mathematics, to say nothing of the theory of chance, is, indeed, singularly characteristic of many investigators, who think statistics can be handled, and conclusions drawn from them, without the least preliminary training.[137]

Galton's method of percentiles, 'useful as it [was] in the right place' was seen to be acting as 'a distinct check to statistical theory in a somewhat unexpected manner'.[138] Whilst some of Galton's innovations had led to a greater use of reporting data, his method of percentiles had become

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133 Galton, Minutes from the Evolution Committee Meeting: RS CMM 65.492, from Minutes read at the meeting of 25 March 1895.
134 *Ibid.*
135 *Ibid.*
136 Karl Pearson, 'Dilettantism and Statistics', *Nature* (13 December 1894), 145.
137 *Ibid.*
138 *Ibid.*

> so intelligible, that the non-mathematical anthropologist or physiologist grasps it at once, reserves his raw material, and publishes tables of percentages, It is hard to conceive anything more disastrous for true statistical progress.[139]

Statistical progress was seen as being encumbered when asymmetrical or skewed distributions of statistical data were forced to fit into the normal distribution. As Galton advocated the treatment of such data, this was seen by Pearson as being 'disastrous'. Pearson then explained that it

> is well known, any normal curve of errors could be reduced to any other by uniform stretches (or squeezes) parallel to its base and axis, though innumerable frequency curves in the biological, just as in economical statistics are *not* of the normal type... owing to it asymmetry or skewness, its limited range and other features, it neither possesses the stretch property of the normal curve, nor can its areas be expressed by tables of one entry. [Whilst] the use of percentiles may suffice to demonstrate the skewness of statistics, [it] at once precludes the use of any theory higher than that of the normal curve, by which the skewness could be accounted for... all means of scientifically treating this skewness disappear, because we have percentile results.[140]

Though Porter's data were skewed, he fitted them 'not very accurately' into the normal distribution. Porter's treatment of his data was seen as being problematic because this style was 'characteristic of much work of a statistical kind which is now being turned out in both Europe and America'.[141] Moreover,

> There has been up to the present time—with the honourable exception of courses of lectures by Dr. Venn in Cambridge—no teaching of statistical theory in England. There is no chair of statistics in any English university, and the Newmarch lectureship at University College, except for the year of its tenancy by Prof. Edgeworth, has been held by economists and not by mathematically trained statisticians. We want a centre, which shall not only contain a statistical museum, but embraces as well a statistical laboratory and workshop... where students receive mathematical training and where biologists and anthropometrists could send their data to be 'fitted', the physicists his observations to be dealt with, and the economist or sociologist his price or labour statistics to be analysed. At the same time, absolute measurements might be made bearing on the problems of evolution, disease and national economy.[142]

For the time had come when statistics,

> must be taken out of the hands of the dilettanti, and when a man, if he takes it upon himself to publish statistical researches, must, like the physicist, show his credentials in the form of a fair mathematical training.[143]

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139 *Ibid.*
140 *Ibid.*
141 *Ibid.*
142 *Ibid.*, 146.
143 *Ibid.*

When Pearson introduced his method of moments in 1893, he had offered a system for coping with asymmetrical data. In his second paper on the 'Mathematical Contributions to the Theory of Evolution', he emphasised skewed curves. In this paper he introduced the family of Pearsonian curves (which was followed with two supplements, the first in 1901 and the second in 1916).[144] The importance of his skewed curves, was expressed as a belief which was

> most suggestive and important [as] results might be obtained from the theory of evolution if we only had the series of skew curves for a biological case of progressive variation in the same sense as we have for the pauper percentage.[145]

In this paper he applied his curves to a much broader range of examples than he had in his first paper. In addition to using Weldon's crab measurements, he also included distributions of variation in heights of recruits and of school girls age 8, variation in number of petals of buttercups, the number of projecting blossoms in clover, the number of dorsal teeth on rostrum of prawns. As skew curves were seen as an absolute necessity 'in all questions of vital statistics' especially in infant mortality curves, he looked at variations in pauper percentages in England and Wales and also attempted to find a resolution of mortality curves for English males into five components.

Asymmetrical frequency curves could have arisen from two sources. The first type of curve arose from material that was 'heterogeneous which may have consisted of a mixture of two or more homogeneous materials'.[146] If this were the case, Pearson suggested that the treatment of this frequency curve was to 'break up the original curve into component parts, or simple frequency curves' as he had first suggested the year before.[147]

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144 Karl Pearson, 'MCTE. X. Supplement to a Memoir on Skew Variation', *Philosophical Transactions of the Royal Society*, Series A, 197 (1901), 443-59. Pearson, 'MCTE. XIX. Second Supplement to a Memoir on Skew Variation', *Philosophical Transactions of the Royal Society*, Series A, 216 (1916), 429-57.

145 Karl Pearson, 'MCTE. II. Skew Variation in Homogeneous Material', *Philosophical Transactions of the Royal Society*, Series A, 186 (1895), 406.

146 *Ibid.*, 344.

147 *Ibid.*

The second set of frequency curves were of a skewed nature and could arise 'in the case of homogeneous material when the tendency to deviation on one side of the mean is unequal to the tendency to deviation on the other side'.[148] In this paper Pearson dealt with the second set. He then explained that 'any theory which is to cover the whole series of these curves must give a curve capable of varying from one to another of these types'.[149] He then suggested using the point-binomial as 'the points given by the binomial $(^1/_2 + ^1/_2)^n$ coincide very closely with the contour of a normal frequency curve when n is only moderately large'.[150]

Pearson devised a probability machine whereby he continually bisected streams of sand and of rape seed for 20 successive falls and found that this gave a good normal frequency curve. When p : q was set to any other ratio than the bisection or any less power than 20, this produced a 'wide range of asymmetrical point-binomials' which was illustrated with a 14-power binomial machine which can be seen in Figure 3.13. Just as the normal frequency curve may be obtained by running a continuous curve throughout the point-binomial $(^1/_2 + ^1/_2)^n$ when n is fairly large, a more general form of the probability curve can also be found by running a continuous curve through the general binomial $(p + q)^n$.

The normal distribution is a theoretical construct used to express what might be true of the relation between the observed values and the probability of those values occurring by chance. The normal curve has the following mathematical properties:

(1) the distribution is unimodal, continuous and ranges from negative infinity to positive infinity (see Figure 3.14).

(2) the population mean $\mu = 0$ and the standard deviation $\sigma = 1$ (or unity),

(3) the skewness of the normal curve is zero and will always be symmetric around the mean μ. (If the distribution were skewed to the left side, a measure of skewness would produce a negative value—if skewed to the right side, this would result in a positive value).

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148 *Ibid.*, 345.
149 *Ibid.*
150 *Ibid.*

Figure 3.13
ILLUSTRATION OF PEARSON'S 14-POWER BINOMIAL PROBABILITY MACHINE (1895)

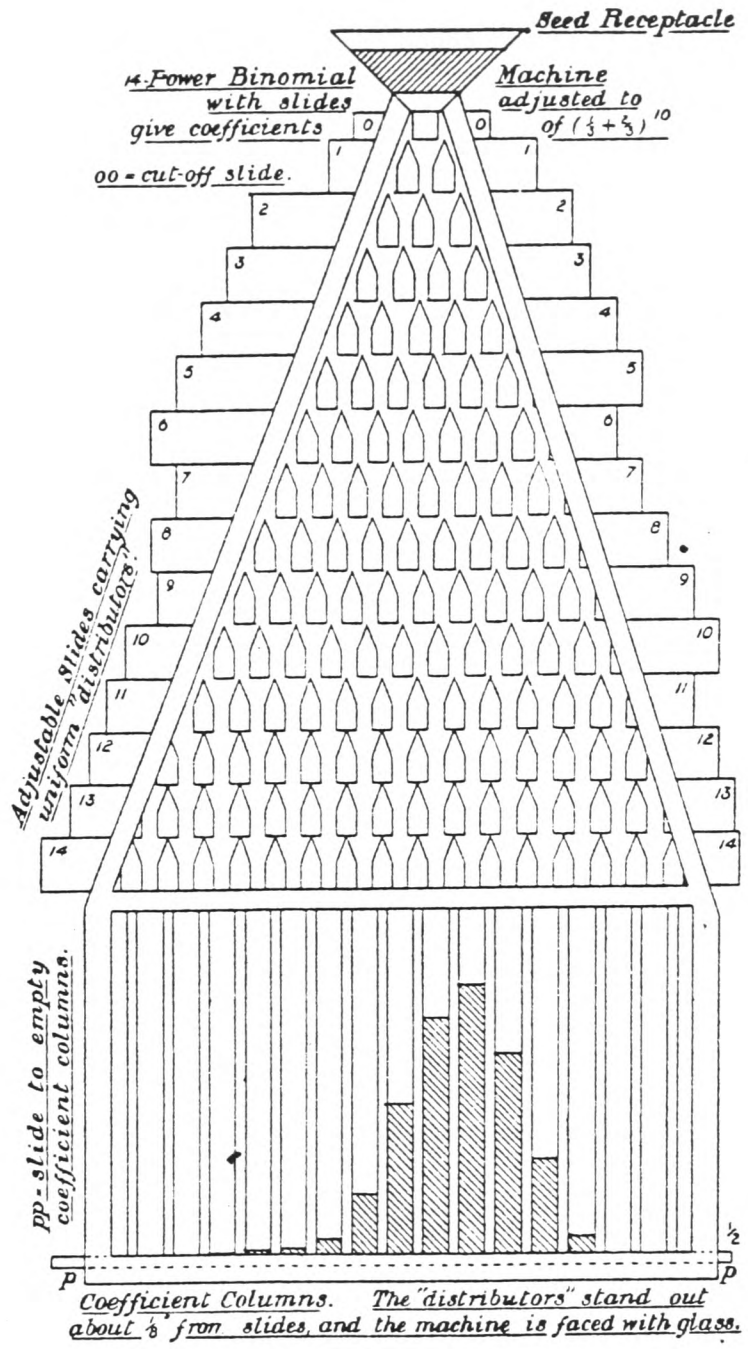
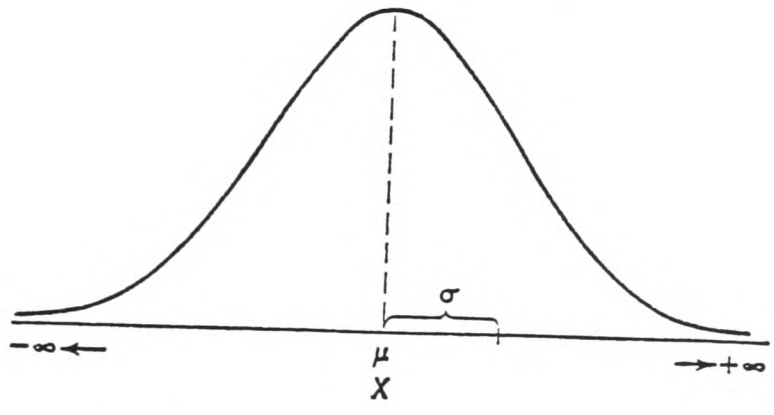


Figure 3.14
THE NORMAL CURVE



For any series of measures to fit the theoretical normal distribution, the values should range from negative infinity to positive infinity. In more practical terms, however, the range for a normal distribution should, at least, extend to both sides of the distribution (e.g. from -1 to +1). Thus, if a frequency curve was constructed of a given ratio of carapace length to body length, 'the range of values must lie between 0 and 1' as it would not be possible to have values less than 0 (i.e. 'for an adult prawn to have a carapace $^2/_3$ or of a body length would be regarded as impossibilities... [as] such an abnormality could hardly survive to adult condition').[151] Hence, the distribution could only approximate the *normal curve*.

This problem was not confined to 'biological statistics'. This could also apply to barometric curves as there are

> excessively low and excessively high barometric heights which would be not only inconsistent with the survival of any meterological observer, but also with the existing features of physical nature on this earth.[152]

Thus it may be seen that the problem of *range* is a very important one as it '*theoretically* excludes the use of the normal curve in many classes of statistics'.[153] From this basis, Pearson argued that to deal with statistics effectively, what was needed was a 'generalised probablity curve which includes factors of skewness and range'.[154] In his 1902 paper 'On the influence of natural selection on the variability and correlation of organs', Pearson examined the effect of the range on univariate selection more fully for the problem of correlation; this problem became referred to later as 'the restriction of range on the correlation coefficient'.[155]

In his paper of 1895, Pearson developed a general formula to use for subsets of various types of frequency curves (including the normal) and defined the following types:

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151 *Ibid.*, 359.
152 *Ibid.*
153 *Ibid.*
154 *Ibid.*, 360.
155 Pearson, 'MCTE. XI. On the influence of Natural Selection on the Variability and Correlation of Organs', *Philosophical Transactions of the Royal Society*, Series A, 200 (1902), 443-59.

TYPE I: Limited range in both directions and skewness (Asymmetric Beta Density Curve)

TYPE II: Limited range and symmetry (Symmetric Beta Density Curve)

TYPE III: Limited range in one direction only and skewness (Gamma Curve)

TYPE IV: Unlimited range in both directions and skewness (Family of Asymmetric Curves)

TYPE V: Unlimited range in both directions and symmetry (Normal Curves)

In his first supplement to his family of curves in 1901, he defined Type VI and VII and then types VIII and IX in his second supplement of 1916. Many of his curves were J-shaped, U-shaped and skewed as can be seen in Figures 3.15.1-6.

From Pearson's experience with anthropometric and biological data, whether relative or absolute measurements of organs were considered,

> the frequency curves all deviate from the normal—however slight the deviation... So far as this goes, it is evidence against the usual hypothesis that in biological matters the chances of deviation on either side of the mean are equal, and the 'contributory causes' independent and indefinitely great in number.[156]

Eisenhart would write later that Pearson's family of curves

> did much to dispel the almost religious acceptance of the normal distribution as the mathematical model of variation of biological, physical and social phenomena.[157]

CONCLUSIONS

Though Francis Galton's ideas of correlation found expression in problems of heredity, it has been shown that the earliest use of Galton's correlation was made by Weldon when he was trying to determine the constancy of relation between two organs of an animal for local races (i.e., the biometrical species criterion) as was suggested first by Galton himself. Weldon's primary interest in using correlation was as a diagnostic procedure for the identification of species when examining relationships between various organs in marine organisms.

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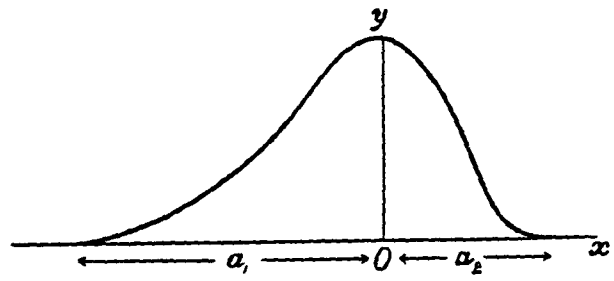
156 Pearson, 'MCTE. II. Skew Variation in Homogeneous Material', *Philosophical Transactions of the Royal Society*, Series A, 186 (1895), 389.

157 Churchill Eisenhart, 'Karl Pearson', *Dictionary of Scientific Biography*, 10 (Charles Scribner's Sons: New York, 1974), 461.

Figures 3.15.1-3.15.3
EXAMPLES OF PEARSON'S ASYMMETRICAL CURVES

Assuming that y_0, ν, α_1 and α_2 can take any sign whatever, we see that there are three fundamental subtypes of this frequency curve,

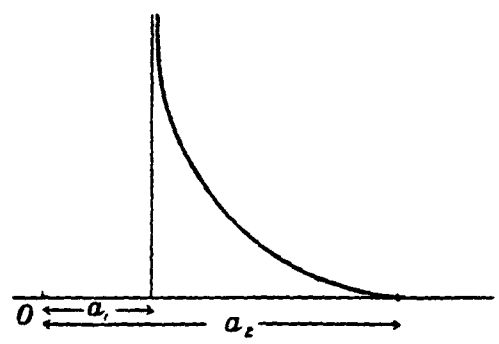
(i.) $y = y_0 (1 + x/\alpha_1)^{\nu\alpha_1} (1 - x/\alpha_2)^{\nu\alpha_2}$.



This is an asymmetrical curve with limited range and maximum towards mediocrity.

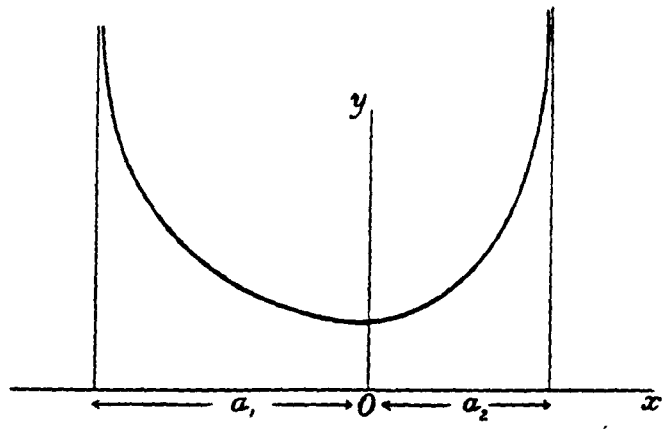
As a rule $\nu\alpha_1$ and $\nu\alpha_2$ are fractional and the curve becomes imaginary beyond the limits $x = -\alpha_1$ and $x = \alpha_2$.

(ii.) $y = y_0 (x/\alpha_1 - 1)^{-\nu\alpha_1} (1 - x/\alpha_2)^{\nu\alpha_2}$.



Here the ordinate between $x = \alpha_1$ and $x = \alpha_2$ varies from infinity to zero, and resembles the frequency curves given by "wealth" distribution or infant mortality.

(iii.) $y = y_0 (1 - x/\alpha_1)^{-\nu\alpha_1} (1 + x/\alpha_2)^{-\nu\alpha_2}$.

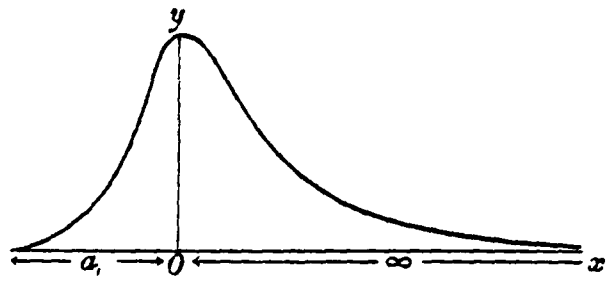


This is an asymmetrical curve with limited range, mediocrity being in a minimum. The disappearance of mediocrity is not a very uncommon feature of statistics; the

Figures 3.15.4-6
Examples of Pearson's Asymmetrical Curves

(iv.) Make $a_2 = \infty$ in (i.),

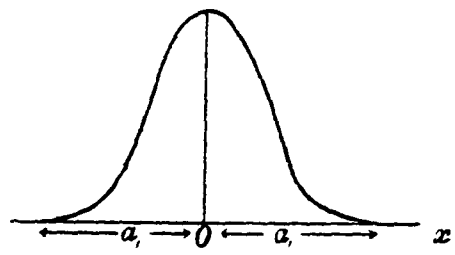
$$y = y_0 (1 + x/a_1)^{\nu a_1} e^{-\nu x}.$$



This is the limit to the asymmetrical binomial, which has been already referred to in § 8.

(v.) Make $a_1 = a_2$,

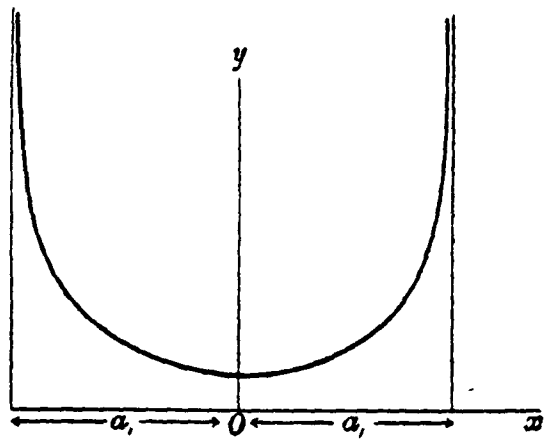
$$y = y_0 (1 - x^2/a_1^2)^{\nu a_1}.$$



This is the symmetrical frequency curve of limited range.

(vi.) Make ν negative in (v.),

$$y = \frac{y_0}{(1 - x^2/a_1^2)^{\nu a_1}}.$$



This is a symmetrical frequency curve, with limited range, and minimum of mediocrity.

(vii.) Put $\nu = p a_1$ in (v.) and make $a_1 = \infty$,

$$y = y_0 e^{-px^2}.$$

This is the normal curve.

The technique of correlation can indeed be expressed in various ways when investigating evolutionary relationships. Weldon's approach could be seen as a determination of a '*Functional Correlation*' (e.g., when the size of one organ is a function of the size of a second organ or, for example, when measuring the relationship between the lower jaw which must be correlated with the upper jaw—these are two 'functional' units that have to work together.) Charles Darwin used correlation when specifying relationships which could be described as *Mathematical Correlations* as, 'a case of correlation is that with the increased or decreased dimensions of the whole body, or of any particular part, certain organs are increased or diminished in number' and as *Developmental Correlations*, which Darwin thought was 'the simplest case of correlation [and occurs when] a modification which arises during an early stage of growth tends to influence the subsequent development of the same part, as well as of other and intimately connected parts'.[158] (A developmental correlation may be regarded today as a measure of enzymes and protein produced in the same character measured at different times.) A fourth measure of correlation used today refers to *Ecological Correlations* (e.g., the relationship between toes and teeth in the horse). In evolutionary terms, there was a reduction of toes, from five to one, which enabled the horse to run faster while the teeth were becoming longer due to a change of feeding habits from browsing to grazing.[159] This is to suggest that the technique of correlation was neither developed nor employed specifically with reference to heredity and thus needs to be viewed within a broader perspective (which includes the biometrical species criterion.)

In the next chapter, Pearson's theory of frequency distributions will be examined. It will be shown that this development arose from Weldon's and Pearson's work in looking for empirical evidence of natural selection when they were looking

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158 Charles Darwin, *The Variation of Plants and Animals under Domestication*, vol. 2 (John Murray: London, 1875), 313.

159 A.J. Cain and G.A. Harrison, 'Phyletic Weighting', *Proceedings of the Zoological Society*, 135 (1959), I-30.

for disturbances in the distribution from normality as a consequence of differential mortality rates.

Chapter IV

THE DEBATES ON THE QUANTIFICATION OF NATURAL SELECTION

> More individuals are born than can possibly survive. As the individuals of the same species come in all respects into the closest competition with each other, the struggle will generally be most severe between them. Of the many individuals that are periodically born, but a small number can survive. I have called this principle by which each slight variation, if useful, is preserved, by the term 'Natural Selection'.[1]
>
> [Charles Darwin]

In his 'Sketch of 1842', Darwin recognised two types of variation. The first type were 'sports' or large discontinuous variation which were said to be relatively rare and the second type were minor or continuous variations, which were pervasive and plentiful and occurred in every character of every organism. Three years before he published his *Origin of Species* in 1859, Darwin was convinced that sports were too rare to be considered the primary source of variation, especially since he thought that sports could be swamped easily by blending inheritance. Instead, he 'believed that individual differences were the significant source of variation, that natural selection was the motive agent, and that the movement of evolution was gradual and continuous'.[2] More than thirty years after the publication of the *Origin*, Darwin's theory of natural selection was to become endorsed whole-heartedly by W.F.R. Weldon and by Karl Pearson.

From the start of their working relationship in 1892, Weldon and Pearson were involved in a series of heated debates with William Bateson and other biologists who were sceptical of Darwin's theory of natural selection. The debates which centred around the use of statistical methods for problems of evolutionary biology became fuelled in 1895 when Weldon (with Pearson's assistance) demonstrated empirical evidence of natural selection. Although they would later find themselves being drawn

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1 Charles Darwin, *Origin of Species* as quoted in Karl Pearson, 'Socialism and Natural Selection', *Fortnightly Review*, 56 (July 1894).

2 William Provine, *Origins of Theoretical Population Genetics* (University of Chicago Press: Chicago, 1971), p.11.

into some of the debates on issues of inheritance at the very end of the nineteenth century, their work on natural selection was to become one of the more controversial phases of their working lives. At the height of these controversies in 1895, W.F.R. Weldon and William Bateson, who were on friendly terms with each other when they were up at Cambridge in the early 1880s, would come into conflict with each other over issues associated with the role of variation in speciation and in natural selection.[3]

In the last chapter, it was shown how problems of sympatric speciation, first expressed as dimorphic distributions by Karl Pearson, precipitated novel statistical developments for curve-fitting of various shaped distributions. An innovative statistical system was devised by Pearson in 1892 when he introduced his method-of-moments, in one of his November Gresham Lectures, as a system of fitting observational data to a theoretical curve (which was used first to analyse Weldon's 'double humped curve'). The next phase in their pursuit of the mathematisation of Darwinian ideas, involved Weldon's work on finding empirical and experimental evidence of natural selection when examining selective death rates: this was to be Weldon's most successful work and his most fruitful venture with Pearson.

Weldon was to introduce the basic framework of the corpus of Pearsonian statistics to a group of zoologists, in his Presidential Address to the Zoological Section of the British Association, in 1898. He would demonstrate two statistical innovations: the first involved using the standard deviation as a statistical method, to express the full range of organic variation, for a variety of biological characteristics; the second, was to emphasise the necessity of using the relative frequency approach to probability by showing how probability could be used for curve fitting, and this was followed with the subsequent construction of frequency distributions. Weldon then urged that Pearson's statistical machinery should be used by all those zoologists who

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3 W.F.R. Weldon, 'The Study of Animal Variation' (book review of William Bateson's *Materials for the Study of Variation Treated with Especial Regard to Discontinuity in the Origin of Species*), *Nature* (10 May 1894), 25-26.

professed to have a genuine interest in animal evolution. Whereas Pearson's statistics made it possible for Weldon to handle systematically large aggregate quantities of data in an unprecedented manner, Weldon's championed use of such statistical methods would enable Pearson to achieve near complete fruition in his development of a modern theory of statistics, by the beginning of the twentieth century.

In this chapter, it will now be shown how Pearsonian curve-fitting continued to play an important role for problems of evolutionary biology, and how, in particular, this work was to lead to the subsequent unfolding of a statistical system that would be used to detect empirical evidence of Darwin's theory of natural selection by looking for disturbances (or deviations) in the distribution from normality, as a consequence of differential mortality rates. It will also be shown that correlation continued to be an important feature of their work on evolutionary problems. Pearson 'thought that correlation was determined by natural selection'.[4]

As a prologue to the work that Weldon and Pearson undertook in the quantification of natural selection, two of the earliest attempts to mathematise Charles Darwin's theory of natural selection will be discussed. This will then be followed by a consideration of the extent to which Darwinian ideas could be said to have influenced Karl Pearson's statistical thinking.

ALFRED BENNETT'S THEORY OF NATURAL SELECTION FROM A MATHEMATICAL POINT OF VIEW'

In 1870, eleven years after Darwin published the *Origin*, Alfred W. Bennett wrote a paper for *Nature* on 'The Theory of Natural Selection from a Mathematical Point of View'.[5] His object was to show that the hypothesis of natural selection was unsatisfactory as he was unconvinced that 'Natural Selection had been the main, but not the exclusive means of modification' as Darwin himself claimed. To Bennett,

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4 Pearson, 'MCTE. III. Regression, Heredity and Panmixia', *Philosophical Transactions of the Royal Society*, series A, 187 (1896), 257; *idem*. 'MCTE. XI; 'On the Influence of Natural Selection on the Variability and Correlation of Organs', *Philosophical Transactions of the Royal Society*, series A, 200 (1903), 29.

5 He first read the paper before Section D of the British Association at Liverpool on 20 September 1870. Alfred W. Bennett, 'The Theory of Natural Selection from a Mathematical Point of View', *Nature* (10 November 1870), 30-3.

> there [was] no phenomenon in Natural History which [was] more thoroughly relied on by the advocates of Natural Selection as furnishing a decisive favour of their theory, than the one which forms the subject of the longest of the essays in Mr. Wallace's volume, that of Mimicry or Mimetism.[6]

Bennett then described two of Darwin's principles: the first was that the differences which ultimately become 'specific' are brought about by exceedingly slow graduations, and the second was that 'no change can possibly take place by the process of natural selection which is not directly of advantage to the individual'.[7] To test these hypotheses, Bennett wanted to apply 'the rigid test of mathematics to the problem' using Alfred Russel Wallace's examples of butterflies.[8] Wallace and Henry Walter Bates had shown that one particular species of *Leptalis* came to resemble a species of *Ithomia* through the agency of natural selection.

For some time, the primary source of evidence of natural selection acting upon biological variation was demonstrated by Bates with his work on mimicry.[9] After Weldon's work on his crabs had been established, Herman C. Bumpus provided an example of 'the action of selection against the more extreme members of a population' when he measured the wing length and other bodily dimensions of sparrows killed in a storm.[10] It was found that the survivors were more variable and included 'an excess of birds with wings markedly shorter or longer than the average'.[11] Weldon was to demonstrate later the same for the shapes of snail shells.

Contrary to the views of Wallace and Bates, Bennett, however, believed that on

> careful examination, the line of argument will be found to break down... [and] the early stages of the transformation [of colours] will be perfectly useless for the protection of the species.[12]

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6 *Ibid.*, 30.
7 *Ibid.*
8 *Ibid.*
9 Henry Walter Bates, 'Contributions of an insect fauna of the Amazon Valley', *Transactions of the Linnean Society*, 23 (1862), 495-566.
10 H.C. Bumpus, 'The Elimination of the Unfit as Illustrated by the Individual Sparrow *passer domesticus*', *Woods Hole Marine Biological Laboratory Lectures* (1898), pp.209-26, cited in John Maynard Smith, *The Theory of Evolution* (Chaucer Press: Bungay, Suffolk, 1958), p.156.
11 Maynard Smith, *The Theory of Evolution*, p.156.
12 Alfred W. Bennett, 'The Theory of Natural Selection from a Mathematical Point of View', *Nature* (10 November 1870), 31.

From this, he argued that the smallest change in the direction of the *Ithonia* which could have been beneficial to the *Leptalis*, 'is at the very lowest one-fiftieth' of the change necessary to create a 'perfect resemblance'. Bennett thus maintained that 'a change of one-fiftieth is beneficial to that small extent *after which* natural selection may begin to come into play'. With this reasoning, Bennett argued that 1,000 steps were necessary to create a change in the external appearance of the butterfly and that one-fiftieth of the whole change (or 20 steps) is the smallest amount that could be beneficial to the butterfly. He then wanted to examine the 'chance' (i.e., the probability) of this event occurring.

Bennett thought that a chance that a descendant would vary in the required direction should be $^1/_{20}$; to be repeated in the second generation the chance is thus $^1/_{20}$ or $^1/_{400}$; and for up to the tenth generation, the chance is $^1/_{20}$ or 'about one in ten billions'. From these calculations, Bennett thought that he had 'endeavoured to throw every advantage into the scale of the natural selectionist'.[13] He considered 'mere protective resemblances of colour' to be far less important than form or habit. He concluded that it was:

> impossible to conceive, therefore, that this absence of covering was ever directly beneficial to the race or to the individual; and hence it cannot have been produced by the operation of natural selection; but must have been connected with those reasoning powers which lead to the construction of clothing and dwellings on which his civilisation so largely depends.[14]

T.H. Huxley was among the few who had responded favourably to Bennett's paper commenting that it was the first paper

> he could recollect having heard at Section D [of the British Association], which, taking up the side against Mr. Darwin, still did so in a proper and philosophic manner... he thought it probable that when Mr. Darwin read Mr. Bennett's paper, he would have a few facts ready which might alter his view of the case.[15]

Alfred Russel Wallace did not, as might be expected, respond favourably partly because he was the subject of criticism in Bennett's paper, but largely because he

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13 *Ibid.*

14 *Ibid.*, 33.

15 Dr. Bastian, 'Reporting on Events at the Royal Society', *Nature* (10 November 1870), 38.

thought that Bennett had made several errors and misinterpretations of mimicry and of natural selection. Wallace's first comments was that Bennett

> ignore[d] the fact that the word 'species' denotes more than 'variety' or 'individual'. A species is an organic form which, for periods of great and indefinite length as compared with the duration of human life, fluctuates only within narrow limits. But the 'spontaneous tendency to variation' is altogether antagonistic to such comparative stability and would, if unchecked entirely destroy all 'species'.[16]

Wallace then examined Bennett's ideas on the phenomena of 'mimicry' when he tried to show how the effects could not be produced by natural selection through his mathematical calculations. From this, Wallace comments that Bennett made an 'important error' in his thinking of mimicry as

> he [led] his readers to understand that there is only one completely mimicking species of *Leptalis*, while the majority are of the normal white-butterfly type. The fact is, however, that but few species of *Leptalis* retain [their] simple colouring... while the great majority show a considerable amount of colour.[17]

The demonstration of his figures was even more problematic especially as Bennett supposed that a *Leptalis* would vary in twenty different ways and only one in the direction required. Wallace, however, never said that 'only one variation out of twenty would be in the right direction'. He thought it would be more likely that 'one-fourth or one-third at least would help to approximate to some of them, and thus be useful'. Apart from all of this, Bennett had made

> three great oversights... The first is, that each *Leptalis* produces, not one only, but perhaps twenty or fifty offspring; the second is, that the right variation has, by the hypothesis, a greater chance of surviving than the rest; and the third, that at each succeeding generation the influence of heredity becomes more and more powerful, causing the chance of the right variation being reproduced to become greater and greater. Now with these three modifications the weight of the argument is destroyed.[18]

He then concluded that

> in the case of 'mimicry' [Bennett] is not sufficiently careful in his statements of the facts, and this combined with his imperfect grasp of the facts of the theory, entirely

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16 Alfred Russel Wallace, 'Natural Selection: Mr Wallace's reply to Mr. Bennett', *Nature* (17 November 1870), 49.

17 *Ibid.*

18 *Ibid.* Wallace then restated his position that for natural selection to act either in producing 'mimicry' or strucutral changes, no large or special variations are required, because the usual amount of variability which occurs in every part of every organism is sufficient (50).

> neutralises the elaborate numerical proofs which at first sight appear so overwhelming.[19]

Another criticism came from Thomas R.R. Stebbing who noted that whilst the theory of natural selection incorporates numerous minute useful variations, Bennett could

> not allow for the combination of terms. Let them be numerous and minute, if you will, [Bennett] says, but if small they cannot be useful, if useful they cannot be small.[20]

When Darwin said that natural selection acts with 'extreme slowness', he did not imply that its steps must be so numerous as to be too small to allow any advantage. Stebbing realised this 'would be a contradiction in terms'.

Bennett was 'disappointed' with Wallace's reply as he had expected something more conclusive than simply having been accused of making errors he had not made.[21] He ended his diatribe, by appealing to authority, with a quote from one of Huxley's lay sermons:

> After much consideration, and with assuredly no bias against Mr. Darwin's views, it is our clear conviction that, as the evidence stands, it is not absolutely proven that a group of animals, having all the characters exhibited by a species in Nature, has ever been originated by selection, whether artificial or natural.[22]

Letters to the Editor of *Nature* continued until the very end of 1870. Most of the letters were critical, but there were a couple of letters which supported Bennett's ideas.[23] This scepticism of natural selection was not, however, entirely unfounded as 'direct proof for the occurrence of natural selection in nature and even in the laboratory was very scanty almost up to the middle of the twentieth century'.[24] No further attempts would be made to quantify natural selection for another twenty years

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19 *Ibid.*

20 Thomas R.R. Stebbing, 'Letter to the Editor', *Nature* (24 November 1870), 66.

21 Bennett, 'The Difficulties of Natural Selection', 65.

22 *Ibid.*

23 Additional letters were printed in *Nature* in the 24 November 1870 issue and more could be found in the 22 December 1870 issue.

24 Mayr, *Growth of Biological Thought*, p.586. Mayr cites E.B. Ford's work and that of Kettlewell on industrial melanism in 1964 as a 'particularly convincing' demonstration of the effectiveness in selection; Ford's work with the Oxford Group showed that 'many of the characters that had previously been called "neutral" have a selective value when carefully investigated'. Additionally, there were the 'calculations of Norton, Haldane, Fisher and others showing that even very slight selective advantages are important when continued over many generations'. pp.586-7.

until Weldon and Pearson began to look for a systematic way of finding empirical evidence of natural selection.

THE ALGEBRA OF ANIMAL EVOLUTION: About two years after Pearson and Weldon had begun their statistical work for problems of evolution (and a year after they published their results), Pearson was given a manuscript on *The Algebra of Animal Evolution* by the sister of the mathematician Arthur Black. Pearson let Weldon look at it immediately and he wrote to Galton that a

> manuscript has recently been put into my hands dealing with the mathematical theory of evolution. The writer was, I believe, a student of [William Kingdom] Clifford and he died under very sad circumstances about two years ago. He appears to have worked entirely alone [and] deals with parental influence, atavism, etc. etc. What his work is worth I cannot say for I have read only the introduction and table of contents. I think it is undesirable to go into his mathematical treatment so long as I am busy on the subject myself. I have seen enough to know that the manuscript is worth *reading*. I strongly suspect that it may well be worth publishing. I should like very much to have your opinion on the manuscript, much of it deals apparently with points of heredity with which I am quite unfamiliar and which you have made peculiarly your own.[25]

After Galton had looked at the manuscript, he wrote to Pearson that Arthur Black was 'a very able man', though his mathematics were 'above my power'. Galton had 'just a little misgivings about his *premises* here and there [as] in the case of atavism he omit[ted] important [points].'[26] With respect to his use of mathematics, Galton was 'in great doubt as what to suggest'.

On the same day that Pearson sent this letter, Weldon also sent a letter to Galton explaining that Arthur Black

> begins with a statement of a need for statistical treatment of all problems in inheritance and evolution [and this was] followed by a chapter on the chance function which includes a full development of [Galton's] surface of frequency of two variables, including atavism.[27]

An incomplete notebook of Black's work was found by Donald Mackenzie which he thought was 'probably part of the introduction to his larger manuscript'.[28] This notebook was

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25 Karl Pearson, Letter to Galton, 4 June 1894, KP:UCL.
26 Francis Galton to Karl Pearson, 17 June 1994, FG:UCL.
27 W.F.R. Weldon, Letter to Francis Galton, 4 June 1894, FG:UCL.
28 Donald Mackenzie, *Statistics in Britain, 1865-1930. The Social Construction of Scientific Knowledge* (Edinburgh, 1981), p.100.

> an essay on the quantitative mathematical treatment of rate of change of specific types, as affected by severity of competition, extent of deviation from the average, longevity, fecundity, tendency to deteriorate, and pure chance.[29]

In another notebook Black wrote that 'the aim is to put the theory of variation of specific characters in course of time by natural selection upon a mathematical footing'.[30]

At the end of 1894, Pearson returned the *Algebra of Animal Evolution* to the brother of 'Mr. Black with a strong recommendation that it should be sent to Cambridge University Press for consideration'.[31] Though the book was never published, a portion of his work was published in 1898 in the *Cambridge Philosophical Transactions* by Micaiah J.M. Hill, who was one of Pearson's fellow students from Cambridge and Professor of Mathematics at University College London from 1889-1924.[32]

Hill remarked that the 'Multiple Integral' evaluated by Arthur Black was

$$\int_n V(\exp - U)dx_1 \ldots dx_n, \quad \text{where}$$

> U and V are homogeneous quadractic functions of the n variables $x_1 \ldots x_n$ and a constant x_v, and all the integrations are from $-\infty$ to $+\infty$ [was] 'useful in the applications of the Theory of Probabilities to Statistics... on the application of Mathematics to the Theory of Evolution'.[33]

He presented the work 'not exactly in the form', used by Black and 'the notation [was also] slightly different'. Hill re-evaluated Black's Multiple Integral by using determinantal matrix algebra. Though Pearson had introduced matrix algebra into statistical theory two years before Hill assembled Black's paper, Hill's approach (like Pearson's) was most likely influenced by their former teacher Sir Arthur Cayley (who had devised this higher form of algebra).

As Hill gave virtually no indication of the evolutionary concepts that Black wanted to measure with his multiple integral, it is difficult to determine the extent of

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29 *Ibid.*
30 *Ibid.*
31 Karl Pearson, Letter to Francis Galton, 27 December 1894,.
32 Arthur Black, 'Reduction of a Certain Multiple Integral', communicated by Professor M.J.M. Hill, *Cambridge Philosophical Transactions*, 16 (1898), 219-25.
33 *Ibid.*

overlap between Black and Pearson. Black seemed to emphasise inheritance whereas Pearson's earliest statistical work (which started two years before he received Black's manuscript) was underpinned by problems of asymmetric (or dimorphic) distributions for speciation and of measuring differential mortality rates by looking for disturbances in the distribution from normality to demonstrate empirical evidence of natural selection.

THE IMPACT OF DARWINIAN IDEAS ON PEARSONIAN STATISTICS

If it can be argued that many of the evolutionary ideas of Charles Darwin provided such a rich source for the statistical developments that emerged from Weldon's and Pearson's work, to what extent could Darwinian ideas be attributed with helping to bring about a paradigmatic shift in statistical thinking at the end of the nineteenth century (and a shift that would lead to the creation of a modern theory of statistics)? If such a view can be considered, could this also suggest that Darwin's ideas played a greater role in the creation of Pearsonian statistical theory than did the statistical ideas of Francis Galton?

Although, Darwin did not approach his work in a 'mathematical-statistical' fashion, *per se*, these four fundamental statistical ideas underpinned some of his evolutionary thinking: (1) *variation*, the quintessential component to his theory of natural selection, (2) *statistical populations* as it emerged from his ideas of species and speciation, (3) *correlation* used to help explain natural selection and (4) a recognition of biological *frequency distributions* - in addition to his recognition that natural selection can change the shape of the normal distribution. Pearson recognised these key statistical concepts in Darwin's work as 'every idea of Darwin - variation, natural selection, sexual selection, inheritance, prepotency, reversion - seem[ed] at once to fit itself into mathematical definition and to demand statistical analysis'.[34] Pearson thus wanted to know if Darwin appreciated the value of a statistical approach. In searching for Darwin's use of statistics, he wrote to Galton on 3 July 1901 as he had

34 Pearson, 'Editorial. (II) The Spirit of *Biometrika*', *Biometrika*, 1 (October 1901), 4.

> been looking at one or two of Darwin's books to see if he anywhere emphasises *the value* of statistical inquiry. I can find nothing; and yet I feel quite certain he realised that value by undertaking, as he did, the long series of experiments in Cross- and Self-Fertilisation of Plants. In his book he states that he has appealed to *you* for an examination of his data from the statistical standpoint and for a report.[35]

Pearson then asked Galton if he still had any letters from Darwin requesting his assistance as Pearson was unable to find any such letters in the *Life and Letters of Charles Darwin*. He wondered if Galton could find 'any apt remark as to the need of statistical method in solving such evolution problems?'[36] Darwin's letter had not survived, but Galton wrote that he could recollect the terms well. He remembered that Darwin

> began in his usual kindly and appealing way, apologising for the trouble, and implying that he had not confidence in his own power of making the best of the few 'ipomaea' statistics, and then asked me to try what I could do with them. I doubt if he ever thought very much or depended much on statistical inquiry in his own work, in the sense that most members of the Statistical Society would have given to it; though, as you know, he quotes statistical results that others had arrived at, not infrequently.[37]

Galton then spoke to Frank and Leonard Darwin as he considered them to be better authorities on this matter though they were certain that their father had a 'non-statistical mind rather than a statistical one'.[38]

If Galton, and perhaps Frank and Leonard Darwin, viewed Charles Darwin's thinking 'in the sense that most members of the Statistical Society would have given to it', then it would seem quite likely that Darwin did not engage in that type of statistical thinking. For the Statistical Society of London, statistics was a secular science of the state that was not only chiefly enumerative and tabular, but was a form that was concerned primarily with averages (i.e., the 'ideal' type) where the variation that interested Darwin would have been viewed as a source of 'error'.

Three months after Pearson asked Galton about Darwin's statistical thinking, he was preparing the Editorial of the first issue of *Biometrika*: This would give him the

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35 Pearson, 'Letter to Francis Galton', in *Life of Galton*, IIIA, p.246.
36 *Ibid.*
37 *Ibid.*
38 *Ibid.*.

opportunity to address the statistical nature of Darwin's evolutionary ideas.[39] In this editorial, Pearson emphasised that 'all [of] Darwin's ideas fit themselves to algebraic definition... [which] leads us to new points of views and it directly suggests... fresh points for observation and novel directions for experimental research'.[40] This novel approach arose as

> evolution must depend upon substantial changes in considerable numbers and its theory therefore belongs to a class of phenomena which statisticians have grown accustomed to refer to as *mass-phenomena*... [Thus] the problem of evolution is a problem of statistics, in the vital statistics of populations.[41]

Just as one could not follow the growth of nations without the statistics of births, deaths, duration of life, marriage and fertility, it was 'impossible to follow the changes in any type of life without its vital statistics'. The evolutionist was to become 'in the widest sense of the words, a registrar-general for life'.

Darwin's comments about frequency distributions would seem to suggest that he very probably engaged in the sort of statistical thinking which Pearson probably had in mind, rather than that of Galton. In a letter to *Nature* in 1873, Darwin wrote, it

> is known from the researches of Quetelet on the height of man, that the number of individuals who exceed the average height by a given quantity is the same as the number of those who are shorter than the average by the same quantity: so that men may be grouped symmetrically about the average with reference to their height. I may add, to make this clearer, that there exists the same number of men between three and four inches above the average height as there are below it... we may presume that this is the usual variation in all the parts of ever species under ordinary conditions of life. That almost every part of the body is capable of independent variation we have good reason to believe, for it is this which gives rise to the individual differences characteristic of all species... it does not seem improbable that with a species under unfavourable conditions... that all or most of its parts should tend to vary in a great number of individuals towards diminution than towards increment of size; so that the groupings would no longer be symmetrical with reference to the average size of any organ under consideration.[42]

Darwin thus postulated that the shape of a distribution before selection would be 'symmetrical about the mean' and with selection, the distribution would lose its

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39 Pearson's decision to use a photograph of Charles Darwin for the frontispiece of *Biometrika*, seems highly suggestive of the prominence of Darwin's role in the development of Pearsonian statistics.

40 Pearson, 'The Spirit of *Biometrika*', 4.

41 *Ibid.*, 4.

42 Charles Darwin, 'On the Males and Complemental Males of Certain Cirripedes, and on Rudimentary Structure', *Nature* (25 September 1873), 432.

symmetry. Though Weldon and Pearson shared Darwin's view, Galton was convinced that natural selection would not change the shape of the distribution (i.e. distributions would remain normally distributed even with natural selection). Galton illustrated this belief in 1877 with his 'quincunx' - an apparatus he devised in 1873 to illustrate the phenomenon of reversion and a word he also used to describe the arrangement of the pins. To explain his idea of reversion, he used the quincunx to show that there would always be a 'continual maintenance of the law of deviations [i.e. the normal distribution] when looking at inheritance in a a stable population' (see Figure 4.1).[43] Thus, his regression to the mean could sustain the ideal type and preclude much of the variation that was of interest to Darwin, Weldon and Pearson. Stigler described Galton's quincunx as an apparatus that:

> had a glass face and a funnel at the top. Shot was poured through the funnel and cascaded through an array of pins, each shot striking one pin at each level and in principle falling left or right with equal probabilities every time a pin was hit. The device was an analogue for a binomial experiment; each row of pins subjecting a shot to an independent disturbance, equally likely to be left or right. The total displacement from the point of origin was the sum of as many independent disturbances as there were rows of pins... The resulting outline after many shot were dropped should resemble a normal curve.[44]

In his paper on 'Typical Laws of Heredity' in 1877, Galton used the quincunx to demonstrate the same phenomenon when natural selection acted upon a stable population (see Figure 4.2). In the upper portion of this figure is a 'typical' population before selection which is stable and normally distributed; the next generation, also normally distributed, is represented in the second portion, and in the third generation, the selected pellets form a distribution that shows a high degree of 'peakedness'. Considering the emphasis that Galton gave to his regression to the mean, his use would have indicated that the next generation would have returned presumably to the normal distribution. Hence, natural selection would not have had any *permanent* effects for plants, animals or men. To Galton, this procedure showed

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43 Francis Galton, 'Typical Laws of Heredity I' and 'Typical Laws of Heredity III', *Nature* (5 April 1877 and 17 April 1877), 493 and 532.

44 Stigler, *History of Statistics*, p.276.

Figure 4.1
GALTON'S QUINCUNX DEPICTING REGRESSION (1873)

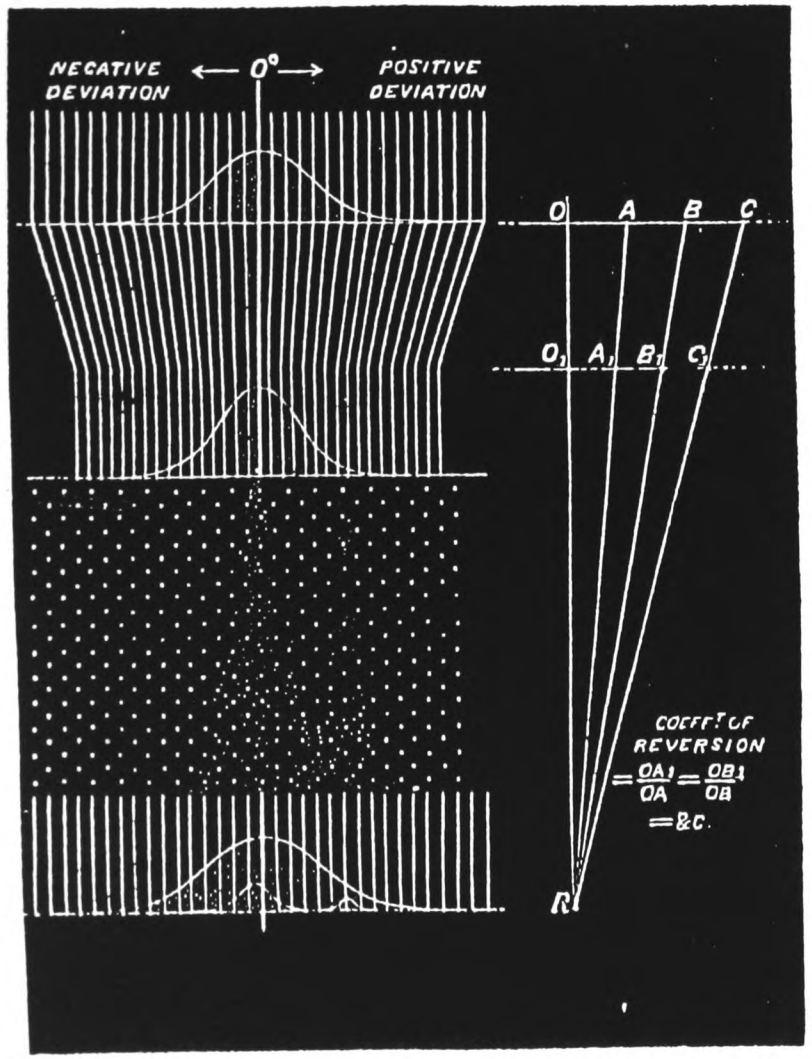
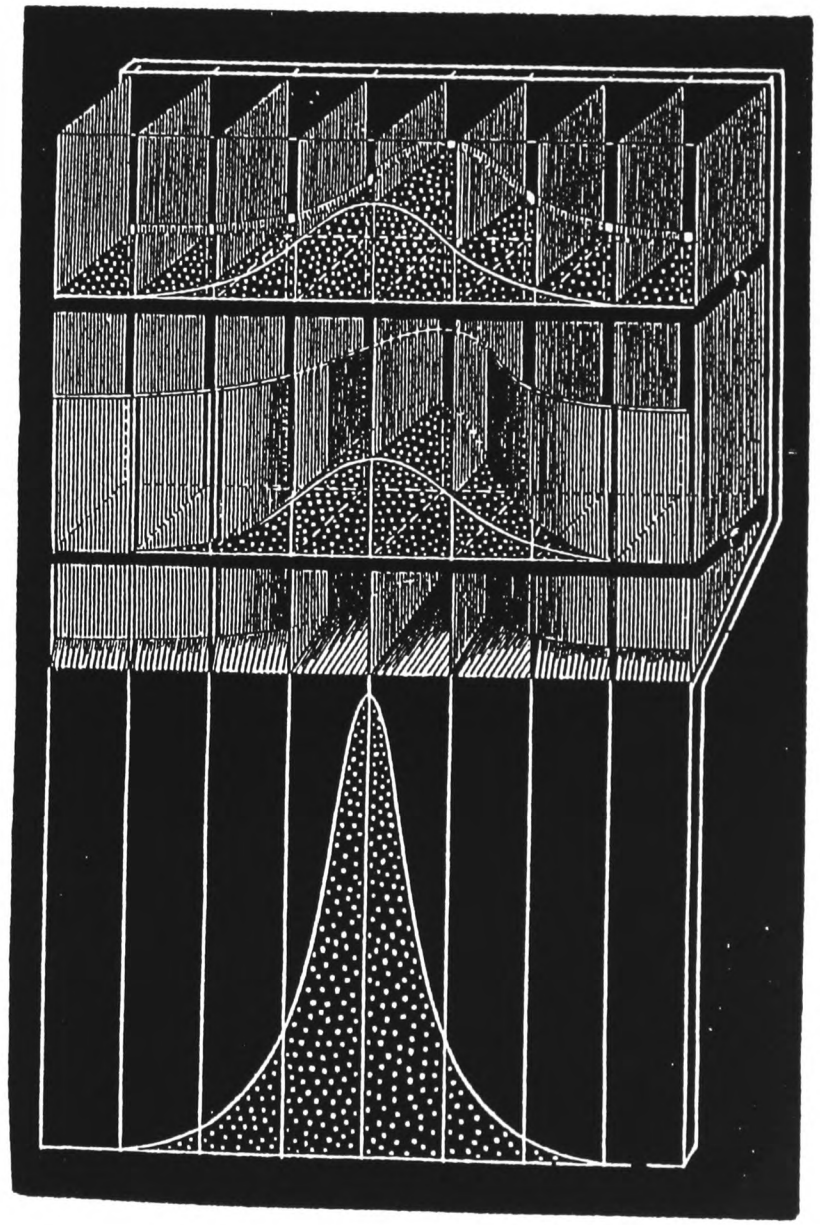


Figure 4.2
GALTON'S QUINCUNX DEPICTING NATURAL SELECTION (1873)



> the way in which the resemblance of a population is maintained; [moreover, the curves] show us that natural selection does not act by carving out each new generation according to a definite pattern on a Procrustean bed, irrespective of waste... We see by them that the ordinary genealogical course of a race consists in a constant outgrowth from its centre, a constant dying away at its margins, and a tendency of the scanty remnants of all exceptional stock to revert to that mediocrity, whence the majority of their ancestors originally sprang.[45]

In this respect, it might seem that Galton would not have thought that Darwin had a statistical mind, whereas Pearson would have seen such an inclination on the part of Darwin.

Darwin's views on correlation also found considerable support from Weldon. When Darwin considered the potential uses of correlation for evolutionary purposes in Chapter 25 of *The Variation of Animals and Plants under Domestication*, he wrote that 'in large groups of animals certain structures always co-exist: for instance, a peculiar form of stomach with teeth of peculiar form, and such structures may in one sense be said to be correlated'. This was a subject which he gave serious consideration as

> correlated variation is an important subject for us; for when one part is modified through continued selection, either by man or under nature, other parts of the organisation will be unavoidably modified. From this correlation it apparently follows that with our domesticated animals and plants, varieties rarely or never differ from one another by a single character alone.
>
> One of the simplest cases of correlation is that a modification which arises during an early stage of growth tends to influence the subsequent development of the same part, as well as other and intimately connected parts.

Another case of correlation could be found

> with the increased or decreased dimensions of the whole body, or of any particular part, certain organs are increased or diminished in number, or are otherwise modified.[46]

Darwin's primary use of correlation could be described as developmental correlation (one of the four types of evolutionary correlations mentioned at the end of the previous chapter). As such, Weldon's conceptual use of correlation was more Darwinian than it was Galtonian (as for Galton, his primary usage of correlation was for studies of inheritance). Pearson's statistics of correlated variation applied to large

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45 Galton, 'Typical Laws of Heredity III', 532-3.

46 Charles Darwin, *The Variation of Animals and Plants under Domestication* (John Murray: London, 1875), pp.311-13.

populations for the systematic examination of frequency distributions, would have required a different type of 'statistical' approach from Galton (and from the Statistical Society of London) and an outlook closer to Darwin's.

If Darwin's ideas were to play such a central role in the emergence of a modern theory of statistics and if statisticians had to deal with a mass-phenomena, Pearson thus queried 'may we not ask how it came about that the founder of our modern theory of descent made so little appeal to statistics?'[47] Whilst Pearson realised that Darwin could 'establish his theory of descent without mathematical conception', he considered that 'Darwin's mind did not operate easily in mathematical lines' - as was suggested by a comment Darwin made to Sir John Lubbock in 1857, when he wrote that he 'was muzzy on issues of proportion and chance [i.e. probability]'. Though, 'perhaps the strongest evidence of [Darwin's] consciousness of biometry' could be found in his empirical conviction that he had 'no faith in anything short of actual measurement and the Rule of Three... which would [have made] an apt motto for *Biometrika* and for all biometricians'.[48]

Hence, how could evolution be measured or more specifically, what was 'the essential part of the Darwinian theory' to the biometricians? Weldon identified 'two physiological propositions which [did] admit of direct experimental verification or disproof... which [had] been left almost untouched since Darwin himself wrote [the *Origin*]'.[49] The first was that the

> differences in structure between individual animals of the same race or species are associated with differences in the death-rate and power of offspring, so that the number of descendents left by an individual is a function of structure; and *secondly*, that the effect of differential fertility, associated with structural differences is often sufficient to change the character of the race or species in the course of successive generations.[50]

Weldon, therefore, needed a mathematical-statistical system that could measure firstly, organic variation and secondly, differential mortality rates. He surmised that

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47 Pearson, 'The Spirit of *Biometrika*', 3-4.
48 *Ibid.*
49 W.F.R. Weldon, 'Dr. Dreyer on Darwinism', *Nature*, 59 (16 February 1899), 365.
50 *Ibid.*

such work was not undertaken by 'Darwin [as he] was so fully occupied in forcing men to recognise the broad fact of structural variation, that he had little time to demonstrate the relation between variation and death-rate'.[51] Thirty-five years later, Sewall Wright would remark that

> Darwin was the first to present effectively the view of evolution as primarily a statistical process in which random hereditary variation merely furnishes the raw material. He emphasised differential survival and fecundity as the major statistical factors in evolution.[52]

During the summers of 1892 to 1898, Weldon was to undertake his most comprehensive studies of differential mortality rates at the Marine Biological Station in Plymouth. At the end of his fourth summer, in 1895, he was able to show empirical evidence of natural selection in the relative frontal breadth ratio of the shore crab at Plymouth Sound. He would then find that the relative frontal breadth ratio of crabs who survived had become narrower in size each year. This diminution was linked to the way the crabs filtered the water entering their gill chambers as a consequence of the water that had become polluted from the sewage that spilled into the sea from Devonport, and also from an increase in the amount of fine china clay deposited along the shores of Plymouth.

The work he undertook during his first three summers at Plymouth fulfilled two aims: the first was to establish the feasibility of conducting additional experiments to determine if different environmental conditions could be found to act selectively upon the death rate in crabs, and the second was to provide Karl Pearson the opportunity to develop the foundation of his mathematical-statistical theory. From 1895 onwards, Weldon was to use Pearson's statistical methods to measure the variation and the death-rate of shore crabs.

FRONTAL BREADTHS OF CRABS

Six months after Weldon had begun his first project on dimorphism in herrings for the Royal Society Evolution Committee in November of 1893, he wrote a letter to the

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51 *Ibid.*

52 Sewall Wright, 'Evolution in Mendelian Populations', *Genetics*, 16 (1931), 98.

Editor of *Nature* on 3 May 1894 urging for the need 'of a *quantitative* measure of the efficiency of selection'.[53] After Alfred Russel Wallace read Weldon's letter, he wrote to *Nature* some six weeks later that:

> As it is very important to obtain some direct evidence of the action of natural selection, I wish to suggest a mode of doing so which might probably be successful.[54]

He went on to suggest that since there seemed to be much evidence to indicate that the birds which migrated to England in the summer were mainly older birds who had lived through two or more migrations and of the very large number of young birds which migrated in the autumn for the first time, only a very small proportion will return to England. If this were the case, then the 'extreme severity of selection during migration could provide the opportunity to determine which physical characters influenced those birds that survived'.[55]

He then suggested that two or three common species of migrants should be chosen. Of these birds, he would then need a number of observers who should carefully record certain characters for the young and old birds separately, such as 'the weight, the total length and the length of the wing, would be sufficient'.[56] After collecting this information, he would look for empirical evidence of natural selection by

> laying down the dimensions of some hundreds of specimens in curves of variation, whatever differences existed between the young and the old birds would be easily detected; and this difference would presumably be the difference between the birth-mean and the survival mean, so far as the selective influence of migration is concerned. In the following spring [the process would be repeated]; and we should then perhaps be able to determine the characters which has led to the selection of the young birds which had survived the double migration.[57]

Given this perspective, Pearson thought that 'Wallace had a due appreciation of the necessity of large numbers; [as] he recognised that the true method of approaching the problem was *statistical*'.[58]

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53 W.F.R. Weldon, 'Panmixia', *Nature* (3 May 1894), 5.
54 Alfred R. Wallace, 'Panmixia and Natural Selection', *Nature* (28 June 1894), 197.
55 *Ibid.*
56 *Ibid.*
57 *Ibid.*
58 Pearson, *Life of Galton*, IIIA, p.133.

Weldon, of course, had begun to 'lay down the dimensions of some [thousands] of specimens in curves of variation' for purposes of looking for empirical evidence of natural selection in 1892 when he began measuring various lengths and the breadth of the carapace of the common shrimp and then the shore crab in Naples and in Plymouth. By the time Wallace had written his letter, the work on the statistical inquiries of evolution for the Committee on Evolution was well underway. They had already begun their work on herrings to test whether Pearson's method of the resolution of two Gaussian curves would confirm the alleged existence of dimorphism. In July 1893, Weldon began to pursue his second project on differential mortality rates in the shore crab at Plymouth Sound.

This second portion of the Committee's work was to result in Weldon's 1895 paper on 'An Attempt to Measure the Death-rate due to Selective Destruction of *Carcinus mœnas* with Respect to a Particular Dimension'.[59] The material available for the committee was Weldon's samples of *Carcinus mœnas* from Plymouth Sound. He had already reported the various distributions of these crabs in his paper in 1893.[60] It thus seemed worthwhile to compare the frequency of abnormalities in young individuals at various stages of growth with those frequencies of the same abnormality in adult life 'so as to determine whether any evidence of selective destruction during growth could be discovered or not'.[61] Pearson viewed this particular project as having been

> absolutely novel at the time, and embrace[d]... the best manner still of testing the truth of the Darwinian theory. It consist[ed] in determining whether the death-rate is correlated with measurable characteristics of the organism or as Weldon put it 'in comparing the frequency of abnormalities in adult life, so as to determine whether any evidence of selective destruction during growth could be discovered or not'.[62]

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59 W.F.R. Weldon, 'Report of the Committee consisting of Mr. Galton (Chairman), Mr. F. Darwin, Professor Macalister, Professor Meldola, Professor Poulton and Professor Weldon... for Conducting Statistical Inquiries into the Measurable Characteristics of Plants and Animals. Part 1. "An Attempt to Measure the Death-rate due to the Selective Destruction of *Carcinus mœnas* with respect to a Particular Dimension"', *Proceedings of the Royal Society*, 57 (28 February 1895), 360. [Hereinafter referred to as 'Report of the Committee... Part 1'.]

60 W.F.R. Weldon, 'On Certain Correlated Variations in *Carcinus mœnas*', *Proceedings of the Royal Society* (1893), 325.

61 Weldon, 'Report of the Committee...Part 1', 360.

62 Pearson, 'W.F.R. Weldon', *Biometrika* (1906), 291.

Crabs were seen as better material for measuring selective death rates than, say, human beings who lived in civilised countries as civilisation offered protection of the physically unfit such that any influences that might produce selective destruction would be reduced to a minimum, whereas 'in the case of crabs such selective influences are active'.[63]

The total sample consisted of 7000 female shore crabs which were collected randomly 'except as regards their size' and varied in length from 7.00 mm. to 13.95mm. The results from these crabs would then be compared to the results from Weldon's paper of 1893. The two characters measured were the '*frontal breadth*' - the distance between both tips of the extra-orbital teeth of the carapace (points A' to A in Figure 4.3) to the opposite side - and the second characteristic was the '*right dentary margin*' which was measured in a straight line (from A to B in the figure). This may be compared to the plotted frequency distribution shown in Figure 4.4. The length of each crab was taken as the length of the carapace 'from the tip of the middle inter-orbital tooth to the posterior margin' (from point C to point D). To picture the crabs as one standard size, Weldon expressed his measures 'in terms of the carapace-length of the crab to which it belonged, taken as 1000'.

Weldon had noted that one of the first problems in determining the error of distribution of the frontal breadths about their means in young crabs arose 'from the great rapidity with which the mean itself changes during growth'.[64] He divided his 7,000 crabs into 35 groups with approximately 200 crabs per group. The main disadvantage in creating these groups, with 200 in a group rather than using the entire group of 7,000 was that the sample size became so small that 'no satisfactory demonstration of the law of variation at any given moment of growth could be obtained'.[65] Weldon then needed to determine what the law of variation should be. Thus, the 'belief' in which Weldon undertook his work was that 'the law of variation

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63 Weldon, 'Report of the Committee...Part 1', 368.
64 *Ibid.*, 362.
65 *Ibid.*, 362-3.

Figure 4.3
CARAPACE OF PLYMOUTH SHORE CRAB (1895)

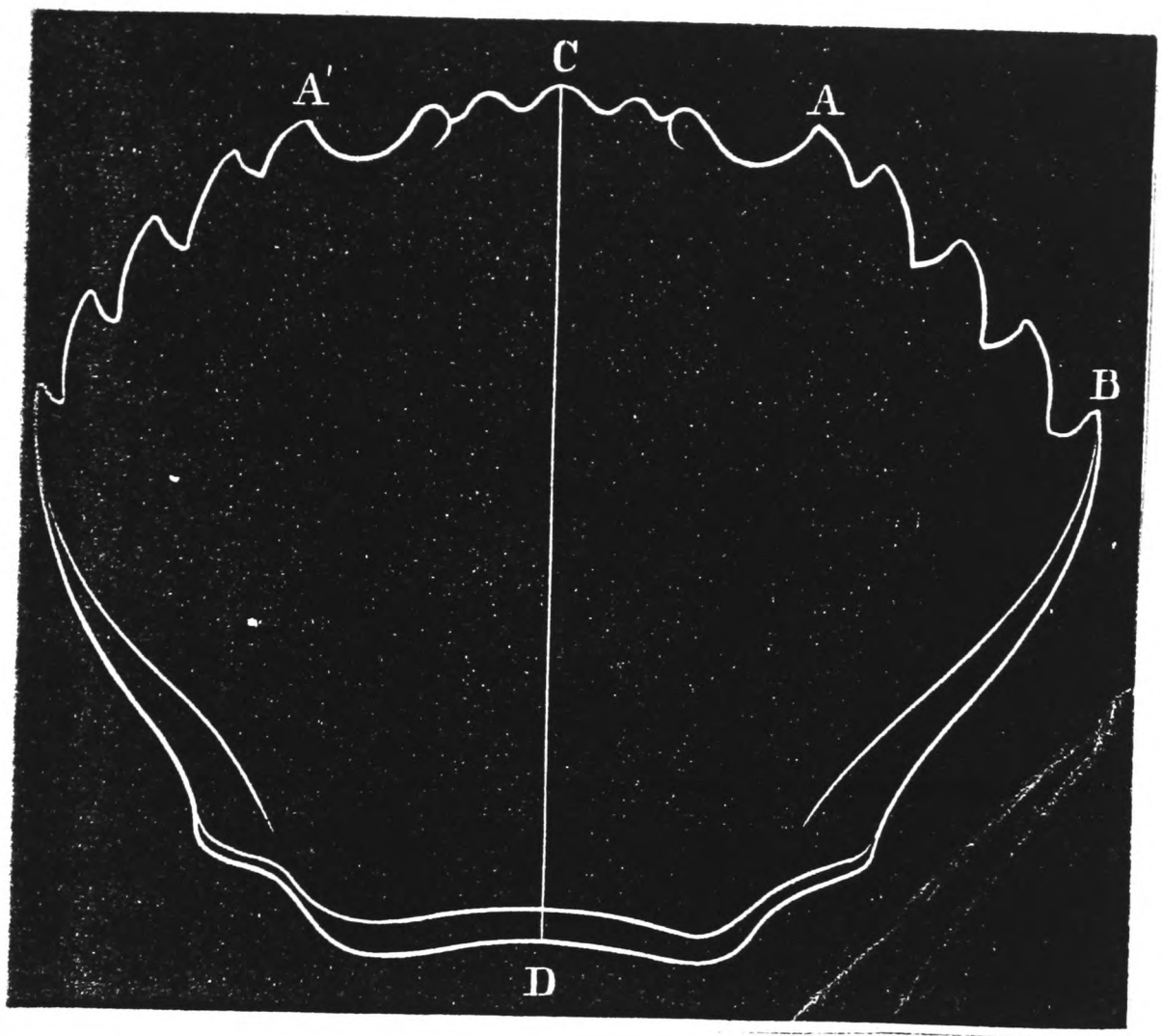
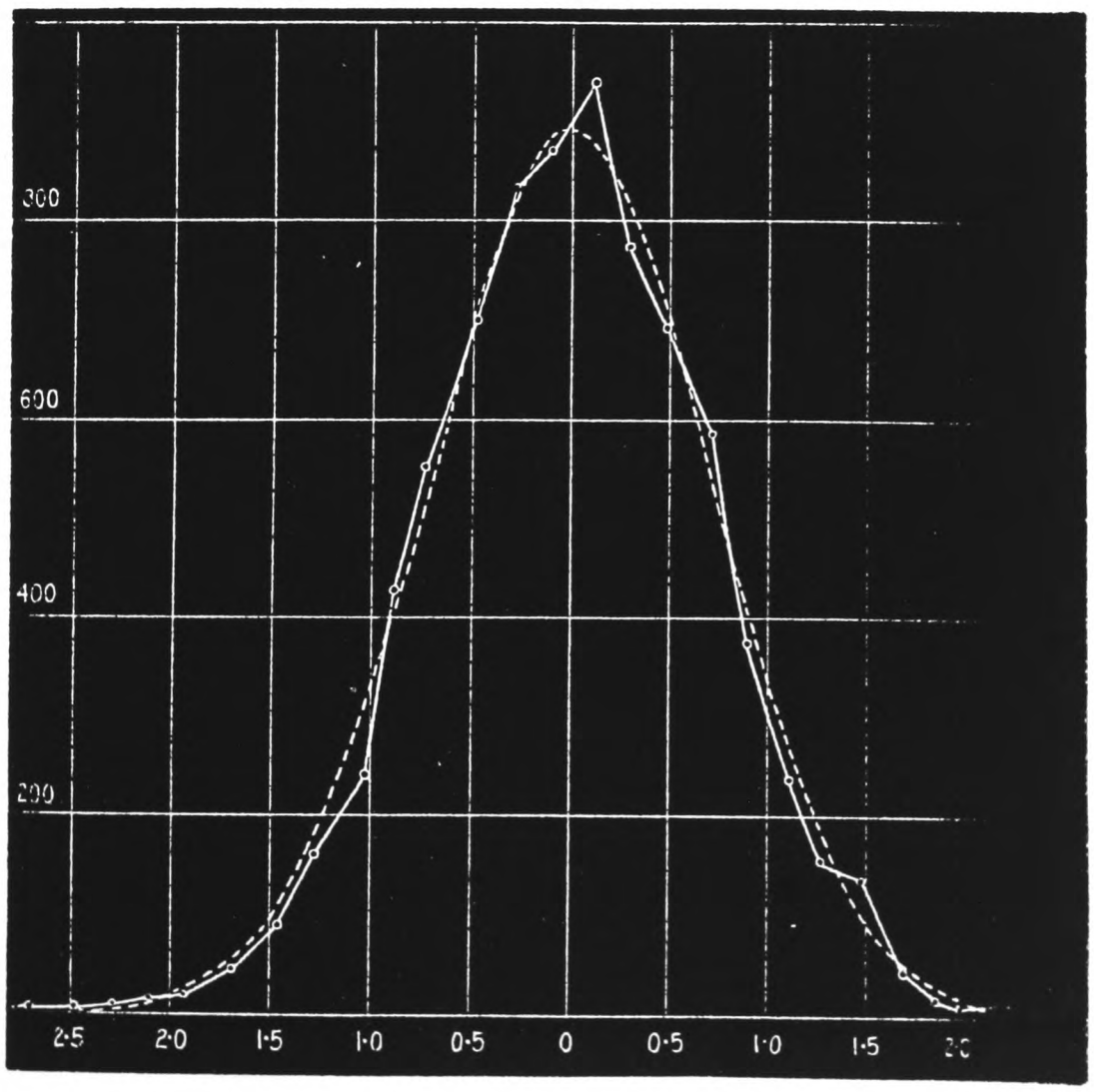


Figure 4.4
FREQUENCY DISTRIBUTION OF PLYMOUTH SHORE CRAB (1895)



would be found throughout to be of the ordinary probability equation' (i.e., his data should have been normally distributed).[66] He expected that if the 'chance function' (probability) really expressed the law of variation, the resulting curve would be a symmetrical probability curve. His results confirmed his thinking as he found that his frequency curve was normally distributed. One of his initial results also confirmed Darwin's statement that 'many variations appear at a late period of development'. In the absence of any experiment, Weldon's hypothesis was that 'the diminution in the frequency of individuals of given deviations is due to a selective destruction and the consequence of this hypothesis will be examined'.[67] The hypothesis of selective destructive was to have involved

> a death-rate of about 77 per thousand between the ages corresponding to 12.5 mm. in carapace length and maturity, as a consequence of deviation in frontal breadths, and in the group of structures... which are directly correlated with it.[68]

In this paper, Weldon emphasised his own view that whilst 'sports' in certain exceptional cases may contribute to evolution, ordinary 'continuous' variations were a more probable source of change and further stated that what was considered almost self-evident, that 'the questions raised by the Darwinian hypothesis are purely statistical'.[69] Though Weldon relied on the power of statistical analyses, he also realised that any set of numbers generated from any such analysis was not meaningful

> by purely statistical methods, without making any assumption as to the functional importance of the frontal breadth, the time of life at which natural selection must be assumed to act, if it acts at all, has been determined, and the selective death-rate has been exhibited as a function of the abnormality, while a numerical estimate which is at least of the same order as the amount of the selective destruction has been obtained.[70]

Weldon expected that the distributions should be symmetrical before selective destruction occurs, though distributions may very well not be symmetric in cases that arise when investigating material undergoing rapid change (that would occur when the

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66 *Ibid.*, 363.
67 *Ibid.*, 368.
68 *Ibid.*, 370.
69 W.F.R. Weldon, 'Remarks on Variation in Animal and Plants. To accompany the first Report of the Committee for conducting Statistical Inquiries into the Measurable Characteristics of Plants and Animals. Part 2', *Proceedings of the Royal Society*, 57 (28 February 1895), 381.
70 Pearson, 'W.F.R. Weldon', 371.

change is induced artificially by selection under domestication or naturally by rapid migration). For investigations of such rapid change, in which the chances of the deviations of opposite sign [inverse correlation] are not sensibly symmetrical, and in which the mean is not necessarily the position of minimum destruction, 'a professional mathematician may be called in to assist with data'.[71] When there is a sporadic occurrence of considerable deviations, which does not obey the normal distribution of variation, this was considered 'to be of great importance in evolution'.[72] Weldon's expectations followed that of Charles Darwin who thought that distributions of biological variation should be normally distributed 'under ordinary conditions of life' and may lose symmetry with selection.[73]

At this stage, Weldon had begun to put into practice Pearson's statistical methods for constructing frequency distributions and for finding an empirical measure of the goodness of fit. Weldon had, indeed, become Pearson's most allied advocate—though Francis Galton was, in principle, supportive of Pearson's statistical work, he very rarely used Pearsonian statistics in his own work.

In this paper Weldon set up a frequency distribution (see Table 4.1) for the plotting of frequency distributions incorporating the scheme that Pearson described in his abstract of 1893. It may be seen here that Weldon attempted to fit his empirical distribution (i.e., the solid line) to a theoretical distribution by calculating the probability values from the frequency of the observed values using Pearson's method of moments. When Table 4.1 is compared to the Galtonian frequency table that Weldon used in his first statistical paper of 1890 (see Table 4.2) and then to one of the tables he used in this 1893 paper (see Table 4.3), it may be seen that although both tables are well organised, they did not provide as much statistical information as can be found in Table 4.1.

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71 *Ibid.*
72 *Ibid.*, p.376.
73 Darwin, 'On the Males and Complemental Males', p.432.

Table 4.1

Frequency of all observed Deviations from the Mean Length of the Right Dentary Margin in 8020 Female Crabs, Young and Adult, from Plymouth (Deviations in terms of the Modulus)

Limits of deviations.				Mean deviation.	Observed frequency.	Theoretical frequency.
				+5.30	1	
Over	+3.20			+3.760	1	
				+3.340	1	
From	+3.00	to	3.19	+3.120	1	2.76
„	+2.80	„	2.99	+2.945	2	
„	+2.60	„	2.79	+2.677	3	
„	+2.40	„	2.59	+2.493	3	
„	+2.20	„	2.39	+2.253	12	4.71
„	+2.00	„	2.19	+2.050	9	11.29
„	+1.80	„	1.99	+1.888	16	24.99
„	+1.60	„	1.79	+1.705	45	51.14
„	+1.40	„	1.59	+1.492	132	96.49
„	+1.20	„	1.39	+1.276	155	168.30
„	+1.00	„	1.19	+1.108	238	271.13
„	+0.80	„	0.99	+0.897	375	403.41
„	0.60	„	0.79	+0.700	587	554.36
„	0.40	„	0.59	+0.493	735	703.61
„	0.20	„	0.39	+0.305	775	824.82
„	+0.00	„	0.19	+0.112	939	893.04
„	-0.00	„	0.19	-0.088	871	893.04
„	-0.20	„	0.39	-0.270	833	824.82
„	-0.40	„	0.59	-0.495	698	703.61
„	-0.60	„	0.79	-0.739	553	554.36
„	-0.80	„	0.99	-0.896	446	403.41
„	-1.00	„	1.19	-1.093	240	271.13
„	-1.20	„	1.39	-1.279	162	168.30
„	-1.40	„	1.59	-1.479	89	96.49
„	-1.60	„	1.79	-1.691	45	51.14
„	-1.80	„	1.99	-1.932	18	24.99
„	-2.00	„	2.19	-2.108	15	11.29
„	-2.20	„	2.39	-2.295	6	4.71
„	-2.90	„	2.59	-2.487	3	
„	-2.60	„	2.79	-2.710	2	
„	-2.80	„	2.99	-2.903	3	
				-3.330	2	2.76
				-4.180	1	
Over 3.00		..		-4.830	1	
				-5.960	1	
				-7.030	1	

1895. Report of the Committee... Part I

Table 4.2

Distribution of Lengths of Antero-lateral Margin of Carapace in 999 Female *Carcinus* from Plymouth

Length in thousandths of carapace length.	Number of individuals observed.	Numbers indicated by probability integral.
796-799	6	
792-795	3	21.5
788-791	7	
784-787	19	23.2
780-783	44	45.2
776-779	70	61.6
772-775	94	87.5
768-771	101	110.5
764-767	140	125.6
760-763	128	134.0
756-759	105	112.7
752-755	100	97.2
744-747	47	53.4
740-743	25	29.2
736-739	20	15.8
732-735	4	
728-731	6	13.2
692-695	1	

1893. Correlated Variations in *Carcinus mœnas*.

Table 4.3
1890: Curve of Distribution of Carapace Lengths—Plymouth.

Grade.	Ordinate.	Ord.–M	$\frac{\text{Ord.}-M}{Q.}$	Normal curve.
5°	261.50	+ 10.98	+2.42	+2.44
10	258.95	+ 8.43	+1.86	+1.90
20	256.05	+ 5.53	+1.22	+1.25
25	255.07	+ 4.55	+1.00	+1.00
30	254.10	+ 3.58	+0.79	+0.78
40	252.27	+ 1.75	+0.39	+0.38
50	250.52	0.00	0.00	0.00
60	249.09	- 1.43	-0.32	-0.38
70	247.29	- 3.23	-0.71	-0.78
75	246.00	- 4.52	-1.00	-1.00
80	244.00	- 5.78	-1.28	-1.25
90	241.39	- 9.13	-2.10	-1.90
95	238.60	- 11.92	-2.63	-2.44

1890. The Variations occurring in certain *Decapod crustacea.*

In Weldon's first statistical paper of 1890, he attempted to fit his data to a normal curve (following the method that Galton used) as he reported the probability values for the normal curve in the last column of Table 4.3. This procedure was appropriate at that time as Weldon's data (on the length and breadth of the carapace of the common shrimp) were normally distributed. By 1892, when Weldon had encountered his 'double-humped curve' (from the carapace of female crabs at Naples), he realised that Galton's method was not sensitive enough to cope with the non-normality of his distribution. Pearson's method of curve fitting (and of reporting data) represented a radical change from Galton's interpretation of observational data: in a Galtonian system, much statistical information would be lost by normalising asymmetrical distributions, whereas in a Pearsonian system, all information would be used. Though Galton's method of percentiles could be seen as 'a convenient method of representing observations', Pearson cautioned that

> it offered no theoretical law of distribution or legitimate means of smoothing observations. Its wide adoption to give the results of statistical researches is even to be deprecated, for it frequently cause[d] the omission of the very data upon which a real statistical theory could be developed.[74]

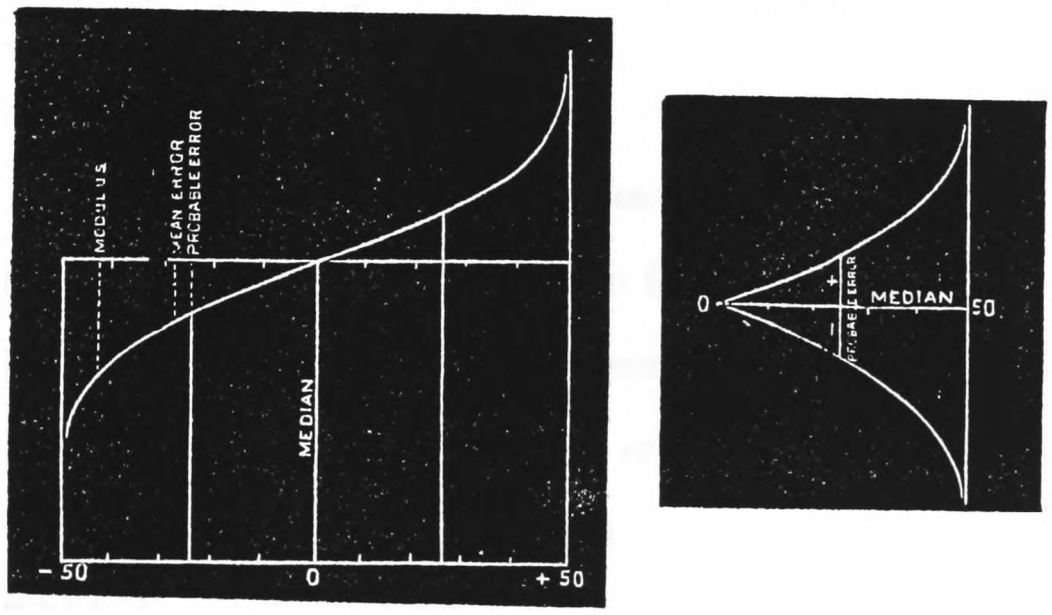
The format used by Weldon is, in fact, very similar to the way a frequency table would be plotted today if it were undertaken manually (rather than by relying on a

74 Karl Pearson, 'Contributions to the Mathematical Theory of Evolution. Note on Reproductive Selection', *Journal of the Royal Statistical Society*, 59 (June 1896), 398.

computer generated print-out). For example, Weldon's 'limit of deviations' and 'mean deviation' could be designated today as 'intervals' and 'median' respectively. The 'observed frequencies' would be unchanged, but the use of 'theoretical frequency' (which was to be instrumental in the development of the chi-square goodness-of-fit test) would be replaced with 'cumulative frequency'.

Weldon's presentation of frequency distributions shows a fairly marked change from Galton's graphical representation of distributions, which he first presented in 1875 (see Figures 4.5 and 4.6).[75] Galton thought that 'numerically, it [was] sufficient to measure and record the values of a very few of its ordinates corresponding to a fractional length of its base'.[76] Galton termed his curve in Figure 4.6 an 'ogive curve' (as 'such a curve [was] called in the phraseology of architects').[77] Pearson referred to Galton's ogive as the 'cumulative frequency distribution' (as it indeed has come to be known).

Figure 4.5-6
Galton's Ogive Distribution (1885)



Weldon's results indicated that among female crabs in Plymouth Sound, during the period of life which he examined, there was no destructive agency which acted selectively upon the dentary margin. Whether such variation should have occurred in

75 Francis Galton, 'Statistics by Intercomparison, with Remarks on the Law of Frequency of Error', *Philosophical Magazine*, 49 (January 1875), 35.

76 Francis Galton, 'The Application of a Graphic Method to Fallible Measures', *Statistical Society of London Journal*, Jubilee Volume (1885), 262.

77 Galton, 'Statistics by Intercomparisons', p.85.

males or females at a later period in life, was open for investigation. Variation in frontal breadth was considered to be 'of more importance in the economy of female crabs than variation in the length of dentary margin'.[78] He had also shown that 'the change of frontal breadth is correlated with change in several other dimensions of the ekoskeleton.[79] In concluding, Weldon wanted to emphasise the importance of determining:

> increase of death-rate, associated with a given abnormality of frontal breadth, [which] has here been roughly determined; ...these are all the data which are necessary, in order to determine the direction and rate of evolution; and they may be obtained without introducing any theory of physiological function of the organs investigated. The advantage of eliminating from the problem of evolution ideas which must often, from the nature of the case, rest chiefly upon guesswork, need hardly be insisted upon.[80]

In the following year, Weldon further described his results in a letter to *Nature* which showed that

> a diminution in the frequency of abnormal frontal breadth, with increasing size of crabs, might be due either to selective destruction of abnormal crabs, during growth, or to a modification off these crabs by which abnormal individuals lose their abnormality as they grow... Further, assuming the law of growth to yield evidence of selective destruction, so that change in frontal breadth is correlated with change in death-rate.[81]

THE EFFECT OF CLAY ON CRABS FROM PLYMOUTH: This was the most extensive of the three projects undertaken by Weldon for the Evolution Committee and would be carried out for a period of four summers. Weldon wrote up his procedures and findings in three separate unpublished reports for the Evolution Committee; these results were highlighted later in his Presidential Address to the Zoological Section of the British Association in 1898 in Bristol.[82] The first report, on the growth of moult of crabs was to be made to the Committee in 1896; this was accompanied with

78 Weldon, 'Report of the Committee...Part 1', 379.
79 W.F.R. Weldon, 'Utility of Specific Characters', *Nature*, 54 (3 September 1896), 413.
80 Weldon, 'Report of the Committee...Part 1', 379.
81 Weldon, 'Utility of Specific Characters', p.413.
82 Pearson, 'W.F.R. Weldon', 27.

another unpublished report that Weldon entitled 'Growth Experiments (Crabs), 1895'.[83] The third report was made to the Committee in 1897.[84]

By the time Weldon had begun his experiments, Plymouth Sound had been

> largely blocked up, and its communication with the sea [was] moved by a huge artificial breakwater, about a mile long, so that the tidal currents enter[ed] it and [left] it only by two openings. The huge modern barrier [had] largely changed the physical conditions of the Sound.[85]

Weldon explained that on either side of Plymouth (see map), a considerable estuary opened up into the Sound, and 'each of these estuaries [brought] down water from the high granite moorlands, where there [were] rich deposits of china clay'.[86] In rainy weather at Dartmoor, a great deal of china clay was washed into the brooks and rivers such that the water appeared 'white and opaque like milk'. Consequently, much

> of this finely divided china clay [was] carried down into the sea; and one effect of the breakwater [had] been to increase the quantity of this fine silt which settle[d] in the Sound itself, instead of being swept out by the scour of the tide and the waves of the severe storms.[87]

In addition to the changes brought about since the breakwater had been completed, Plymouth had undergone other changes due to the increase in population and to the expansion of the great dockyard at Devonport. Thus,

> the sewage and other refuse from these ships, [was] thrown into the Sound; so that while it [was] more difficult than it used to be for fine silt to be washed out of the Sound, the quantity thrown into it [had become] much greater than it [once] was and [was] becoming greater every day.
>
> [Moreover,] it was well known that these changes in the physical conditions of the Sound [had] been accompanied by the disappearance of animals which used to live in it, but which were now found only outside the area affected by the break-water.[88]

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83 Weldon's first unpublished report for this set of experiments, read by the Evolution Committee, was entitled: 'A Report by Weldon on Crab work, referring to [the] summers of 1895 & 1896', KP:UCL, 260/B. His Growth Paper was entitled: 'The Investigation of a Female *Carcinus mœnas*, taken from the Shores of Plymouth Sound, between the tide-marks, immediately below the Marine Biological Laboratory. Growth Experiments (Crabs) 1895', KP:UCL, 260/1A.

84 Weldon's third report was thus entitled: 'Experiments on the Selective Destruction of Crabs, 1897-1898', KP:UCL, 260/1A.

85 W.F.R. Weldon, 'Section D. Zoology. Opening Address by Prof. W.F.R. Weldon, M.A., F.R.S., President of the Section', reprinted in *Nature* 58 (22 September 1898), 505.

86 *Ibid.*

87 *Ibid.*

88 *Ibid.*

MAP OF PLYMOUTH SOUND



[Map of Plymouth Sound in 1904.] The Marine Biological Laboratory is at the tip of the Sound and is situated between the Citadel and the Hoe. The break-water, which is a mile long and 2½ miles from the Hoe, was started in 1812, used five million tons of stone and took some thirty years to complete.

These environmental changes, that had been occuring over the course of the past half century, led to subsequent changes in the marine life on the shores of Plymouth. Thus, Weldon was persuaded 'to try the experiment of keeping crabs in water containing fine mud in suspension, in order to see whether a selective destruction occurred under these circumstances or not'.[89]

Weldon began this work in the summer of 1895 and worked daily from 2 July to 30 August. He collected two samples consisting of approximately 600 female crabs 'taken from the shore of Plymouth Sound, between tide-marks, immediately below the Marine Biological Laboratory'.[90] When he presented the results from his findings in his first report to the Evolution Committee, he explained that

> an attempt was made to observe the frontal-breadth ratio in certain captive *Carcinus mœnas*, so as to compare the frontal breadth, in a series of individuals which had been protected from the effects of Natural Selection during a part of their lives, with wild crabs which had not been protected.[91]

As it would have been

> impossible for [Weldon] to remain at Plymouth long enough to observe captive crabs through more than a small portion of their lives:... the observations made relate only to crabs from 9 to 13mm. in length at the time of capture.[92]

The crabs were collected from a small stretch of shore below Plymouth Laboratory and were divided into two sets.[93] The first set were preserved for measurement and their frontal ratio was taken as a measure of the condition of wild crabs in Plymouth in the summer of 1895.[94] The size of these crabs ranged from 9.2mm. to 17.0mm. in carapace length and were 'measured after some six months in spirit'.[95] The second set

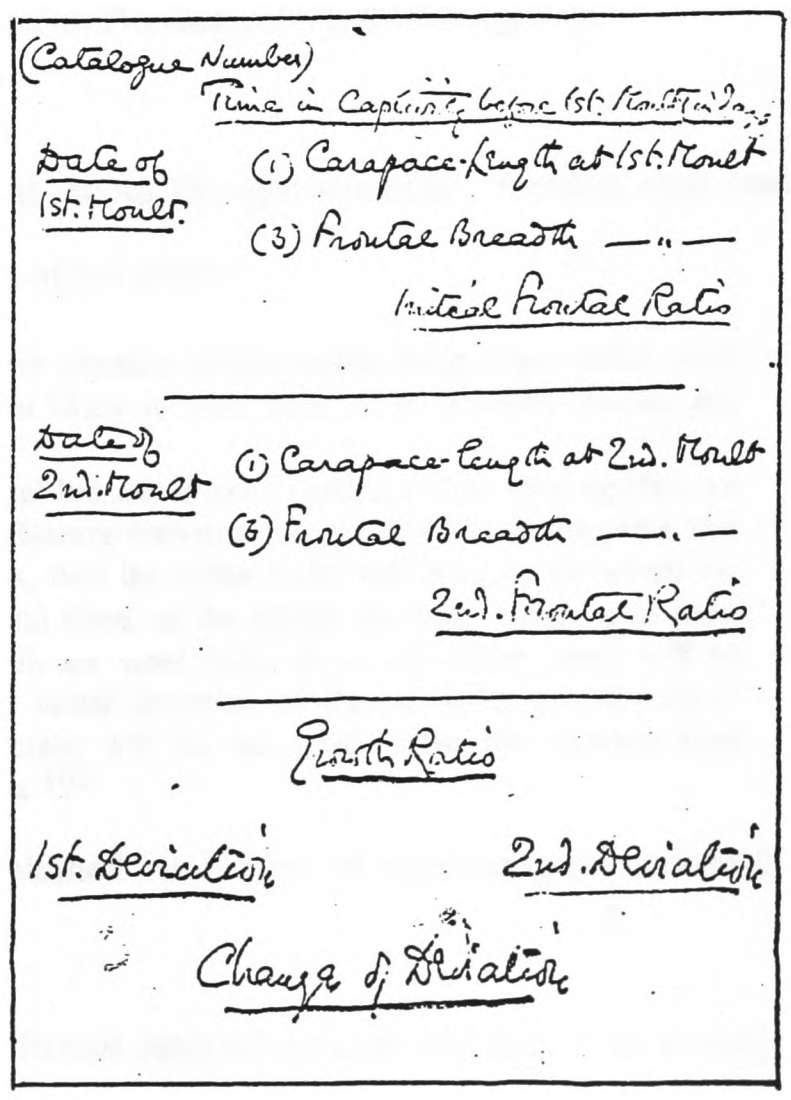
> were placed each in a separate glass jar, of capacity of nearly a litre, through which clean sea-water flowed continuously. The bottles used were numbered, and each crab was fed daily until it moulted. After a moult, the cast shell of each crab was measured while still wet with sea-water.[96]

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89 *Ibid.*
90 Weldon, 'The Investigation of... Growth Experiments (Crabs) 1895', KP:UCL.
91 Weldon, 'A Report... on Crab work, referring to [the] summers of 1895 & 1896', KP:UCL, p.1.
92 *Ibid.*
93 *Ibid.*
94 *Ibid.*
95 Weldon, 'The Investigation of... Growth Experiments (Crabs) 1895', KP:UCL, p.1.
96 Weldon, 'A Report... on Crab work, referring to [the] summers of 1895 & 1896', KP:UCL, p.1.

A measure of the frontal ratio was taken once before moulting and then two or three days later after the moult; when the crab had hardened a new shell, the 'crab was killed and measured, so that its frontal breadth ratio was again determined'.[97] Weldon was thus able to obtain two measures of the frontal ratio of each crab: the first measure gave an approximation to the ratio at the time of capture whereas the second gave the ratio after one moult in captivity. These measures were then compared with the measures from the wild crabs of corresponding length of carapace. During the time that Weldon was conducting his experiments, he recorded the measurements of each individual crab on cards as shown in Figure 4.7. The first seven lines are measurements for the captive crabs and the last three lines are measurements for the wild crabs.[98]

Figure 4.7
Example of Weldon's Card for crab measurements (Growth Report 1895)



97 *Ibid.*
98 Weldon, 'The Investigation of... Growth Experiments (Crabs) 1895', KP:UCL, p.3.

The details of this work were summarised for Pearson in a series of letters he wrote in the following year. In the first of these letters, Weldon explained that 'the time from entrance into bottle to the first moult was recorded in days, because the bottle could only be examined once daily'.[99] After the crabs in sample 2 moulted in captivity, Weldon then re-measured each sample and 'its deviation, in respect to frontal ratio from the mean crab of its own size'.[100] The mean deviation after the moult was +0.3975332 and the distribution around that mean was his second curve. The curve of the differences from these two samples was the first curve Weldon considered and its Arithmetic Mean was -0.224877. To measure the change of deviation at the moult, he computed a third curve by subtracting the mean values of curve 1 (which he referred to as: '$\triangle_1$') from the mean values of curve 2 ('$\triangle_2$'). He then determined a series of correlations between various values from $\triangle_1$ and $\triangle_2$ and he would 'joyfully show the whole thing' to Pearson on the following day.[101]

To answer Pearson's questions 'about the age business', Weldon then outlined these ideas for determining the ages of the crabs:

> (1) Among crabs of the same carapace length, surely those crabs which grow most rapidly at a moult are most likely to have done so at previous moults, and therefore to be the youngest.
> (2) Remembering that frontal breadth index diminishes with size rapidly, we may fairly assume that if the difference between the age of crabs of the same size affects their frontal breadth index, then the oldest crabs will have on the whole the smallest frontal breadth index. And since, on the whole, the crabs which grow most slowly at the one observed moult are most likely to be the oldest, there will be correlations between $\triangle_1$ — the initial deviation of frontal index growth rate.— Namely, the slowest growing crabs will on the whole have the smallest (and therefore the 'oldest') frontal index.[102]

In another letter, Weldon explained his system of measuring deviations for his two samples of crabs:

(1) The difference between the frontal ratio of the crab and that of an average crab of the same size was his '*first deviation*'.

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99 Weldon, 'Letter to Karl Pearson', 19 December 1896, KP:UCL.
100 Weldon, 'Letter to Karl Pearson', 22 November 1896, KP:UCL, p.1.
101 *Ibid.*, pp.2-3.
102 *Ibid.*, pp.4-5.

(2) The crab measured after moulting had another frontal ratio: the difference between this and the mean from the wild crab of the corresponding length was the '*second deviation*'.

(3) For the captive crabs, the ratio $\frac{\text{carapace - length after moult}}{\text{carapace - length before moult}}$ was the '*growth ratio*';

(4) The quantity '*2nd deviation* ***minus*** *1st deviation*' was the '*change of deviation*' of the crab of a moult.[103] As one of the first conditions ensured by growth in captivity was the abundance of food, Weldon recognised that 'the growth-ratio may be increased by a longer period of captivity.[104]

He brought the wild crabs into the laboratory and 'placed them on one table in a large basin containing sea water where they remained for some hours while he attended to the crabs already in bottles'. He was 'struck by the fact that a considerable number of these crabs moulted while on [his] table'; and from that time there seemed to have been 'a rough coincidence between the frequency of moults among the freshly caught crabs and the frequency observed in the bottles'.[105]

In his report to the Evolution Committee, Weldon remarked that the mean value of the growth ratio was 'remarkably constant', the range of variation was 'very considerable' (the values ranged from 1.05 to 1.38) and that the distribution of the observed values was symmetrical.[106] When Weldon compared his measurements to a similar set that his friend Herbert Thompson, had taken from samples gathered from 1892 to 1893, he was able to show that the frontal ratio of the wild crabs from 1895 was always less than it was in 1892 and 1893; moreover, Weldon was to show that this would continue to hold true for his crabs measurements taken from 1896 to 1898.

After Weldon collected his crab data that summer, he wrote to Pearson at the end of September telling him that he had

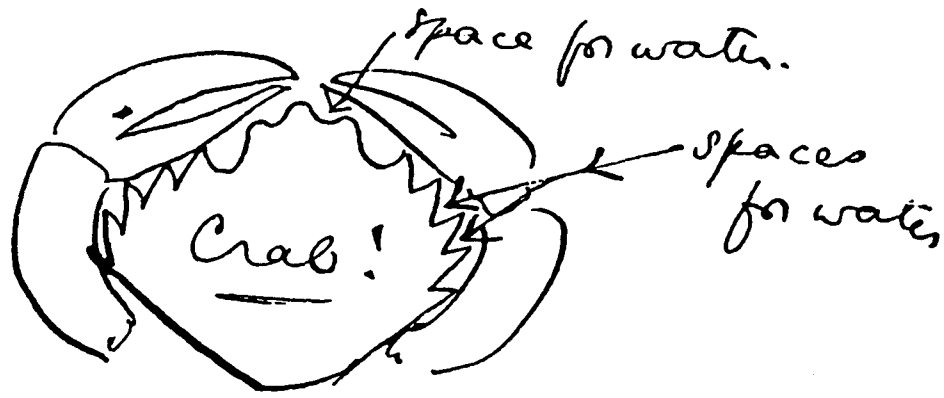
> worked some 10 hours a day at a wretched machine (of which I send you a photograph) until nearly the end of August! — I got measures of about 600 crabs as its successive rate of growth, but was too dead to work the results out decently — they will be awfully complicated! So I went away to Florence, carefully leaving no address. Your great big paper [the 1896 'Mathematical Contributions to the Theory

103 Weldon, 'Letter to Karl Pearson', 19 December 1896, KP:UCL.
104 Weldon, 'A Report... on Crab work, referring to [the] summers of 1895 & 1896', KP:UCL, p.5.
105 *Ibid.*, pp. 8-9.
106 *Ibid.*, p.5.

> of Evolution'], I have not yet tried to read. At Plymouth I simply had no energy after the daily crabs; and I will not read probability in Florence, even to please you.[107]

At this stage, Weldon was beginning to show signs of physical strain and fatigue. Whilst he was 'too dead to work out the results' and had 'no energy after the daily crabs' in September of 1895, by the end off the following summer he was 'too dead to consider anything properly'.[108] In need of some respite, he wrote to Pearson on 25 August 1896 to let him know that he would be going to Bayeux to 'bicycle about and look at the normal curves of the French gothic: which is better than Type IV' (the theoretical normal curve in the Pearsonian family of curves). Before he left, he summarised his findings on the selective death rates of these crabs at Plymouth telling him he had found that:

> (1.) Crabs which live in sand or mud hold their big claws so as to filter water through the sand. — The water passing through the gaps between the teeth. [Weldon drew the crab, pictured below, to illustrate this phenomenon]



> (2.) Crabs of species which bury themselves in sand or which live in very muddy water very often have *narrow* frontal breadths.
>
> (3.) Plymouth Sound is everywhere, and especially near here, becoming yearly muddier, so that the number of dredges necessary becomes yearly greater, the faunal yearly more scanty.[109]

Weldon wanted to determine if the difference in frontal breadth of the survivors was 'due to some treatment undergone by the crabs during captivity'. He calculated the correlation between

> the 2nd deviation (difference from an average wild crab after moulting),—This showed that no important effect was produced by a longer or shorter time in captivity. But [he] found that the increase of size at a moult was dependent on the

107 Weldon, 'Letter to Karl Pearson, 30 September 1895, KP:UCL, pp.1-2.
108 Weldon, 'Letters to Karl Pearson', 30 September 1895, KP:UCL, pp.1-2; 26 August 1896, p.1.
109 Weldon, 'Letter to Karl Pearson', 25 August 1896, KP:UCL.

> length of time spent in captivity... and [he] therefore found the correlation between the growth-ratio (as the best measure of growth [he] could think of) and 2nd deviation. This also seemed unimportant.[110]

He found, however, that the correlation between the first and second deviations was 'considerable'. He thought that the first deviation was 'a thing unaffected by the conditions of the experiment'.[111] From these findings, he 'concluded that the moult observed was not to any important extent due to special conditions involved in captivity, but [could instead be considered] a natural phenomenon'.[112] Unless there was a

> fallacy somewhere in what goes before, then at any of the moults occurring among wild crabs between the sizes [he] produced like those in the bottles and a selective destruction must, as it seems to me, occur *very quickly* after each moult, so that the excessive frontal breadths disappear very quickly.[113]

If this were not so, then Weldon could not see how the 'constant difference between crabs which have moulted in captivity, and wild crabs of corresponding size, [could] arise'. In responding again to Pearson's concern with age, Weldon did not

> see how a knowledge of the age of *captive animals alone* [would] help at all. — It will only enable you to use age as a rough measure of structure or size, just as I use a rough measure of age. — To know the age of the wild crabs is impossible, as it is to know the age of any wild animal: and the probable error of the assumption that wild crabs of a given age have the same size as domesticated crabs of the same age will be pretty big - bigger than that of the only assumption I can see about age in my own work, that the mean age of the two samples of crabs of the same size, collected at the same time, is the same.[114]

From the work involved with measuring the moults of his crabs, Weldon thought that he had determined the minimum selection necessary to effect the adjustment. While he was '*not* trying to find the whole selective death-rate among crabs through their life', with his information, he wanted to show that there was 'a selective destruction during this little bit of the life history of the crab'.[115]

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110 *Ibid.*, pp.2-3.
111 *Ibid.*, p.3.
112 *Ibid.*
113 *Ibid.*, p.4.
114 *Ibid.*, pp.5-6.
115 *Ibid.*, pp.4-5.

EXPERIMENTS ON THE SELECTIVE DESTRUCTION OF CRABS, 1897-1898

In the summer of 1897, Weldon began to carry out experiments on the selective death-rate of crabs kept in water and suspended china clay as well as on other types of foul water. In his 'Diary of Experiments on Crabs: July-August 1897', Weldon explained that 'the object [was] to submit *Carcinus mœnas* to varous ordeals, and to determine whether the death-rate, resulting from each, [was] selective with respect to frontal breadth'.[116]

The experiments were undertaken to determine whether or not agencies could be found 'similar in kind to those which may be supposed to act in Plymouth Sound, and capable of destroying small female *Carcinus mœnas* selectively, so far as its frontal breadth ratio [was] concerned'.[117] The agents that Weldon used included (1) putrescent animal matter, (2) variation in the salinity of the water and (3) solid matter (china clay) in suspension in the water. In this experiment he wanted to grow female crabs, between 10 and 12 mm. long in the glass bottles used in 1895, but *without cleaning the bottles*. The death-rate would therefore give a measure of the

> power of resistance to bacterial poisons of various kinds. Each crab being isolated, power of fighting etc., power of escaping from enemies, and similar properties, are not involved.[118]

The details of this experiment were not only kept on cards, but were also included in separate tables in his 'Diary of Crabs' from the summer of 1897.

The action of putrescent animals was tried by keeping a number of crabs in the glass bottles, and allowing the food given them to putrefy. The variation in salinity was produced either by the addition of fresh water or by the addition of a saturated solution of 'Tidnan's Sea Salt'. The china clay experiments were not uniform, and were therefore not considered to be strictly comparable

> because different samples of clay were used, in different states of fineness; so that neither the quality of the mud at the bottom of the vessel employed, [which] was the amount of material suspended in the water, was the same in any two experiments.[119]

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116 W.F.R. Weldon, 'Diary of Experiments on Crabs: July-August 1897', KP:UCL, p.1.
117 W.F.R. Weldon, 'Experiments on the Selective Destruction of Crabs, 1897-1898', KP:UCL, p.1.
118 Weldon, 'Diary of Experiments', p.1.
119 Weldon, 'Experiments on the Selective Destruction of Crabs, 1897-1898'.

The clay was prevented from settling by 'a slowly moving automatic agitator'; and the crabs were kept under these conditions for various periods of time. At the end of each experiment the dead were separated from the living and both were measured.[120] Weldon found that in

> every case in which this experiment was performed with china clay as fine as that brought down by the rivers, or nearly so, the crabs which died were on the whole distinctly broader than the crabs which lived through the experiment, so that a crab's chance of survival could be measured by its frontal breadth.[121]

The distribution of the differential mortality rate from 248 male crabs treated in one of the china clay experiments can be seen in Figure 4.8; of these crabs, 154 died during the experiment and 94 survived. Weldon also found that when the experiment was performed with coarser clay, the death rate was smaller and was not selective.

In his presidential address and in a letter to Pearson, Weldon concluded that his experiments had shown that

> very finely divided china clay does kill crabs in such a way that those in which the frontal breadth is greater die first, those in which it is greatest less live longer. The destruction is selective, and tends to lower the mean frontal breadth of the crabs subjected to its action. It seemed to [him] that the finer the particles used in the experiments, that is to say, the more nearly they approached the fineness of silt on the beach, the more selective their action was.[122]

Weldon thus concluded that he could 'see no escape from the conclusion that we have here a case of Natural Selection acting with great rapidity because of the rapidity with which the conditions of life are changing'.[123] The process by which selection was affected was thought to be

> largely associated with the way in which crabs filter the water entering their gill chambers. The gills of a crab which [had] died during the experiment with china clay were covered with fine white mud, which was not found in the gills of the survivors... In a least 90% of the cases the difference was 'very striking'.[124]

In his experiments with mud, the same difference in the death rate was detected. Weldon thought that it could, therefore, be shown that 'a narrow frontal breadth

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120 Weldon, 'Opening Address', 505.
121 *Ibid.*
122 *Ibid.*
123 *Ibid.*
124 *Ibid.*, 506.

renders one part of the process of filtration of water more efficient than it is in crabs of greater frontal breadth'.[125]

CRABS IN CLEAN SEA WATER: To determine the death rate of crabs that were protected 'through a certain period of growth from the action of this selective mud,' Weldon

> established an apparatus consisting of some hundreds of numbered glass bottles, each bottle being provided with a constant supply of clean sea water by means of a system of [two] glass syphons. Into each of these bottles [he] placed a crab from the beach... until it had cast its shell. The process of moulting involve[d] no distortion of the carapace, which could affect the measurements concerned, and therefore each cast shell was carefully measured.[126]

After the moulting, the crabs were killed and then measured; these crabs were then compared to the wild crabs. This time the captive crabs were unmistakably *broader* than wild crabs of their own size, and there were a few of these protected crabs which were 'very remarkably broad' (Figure 4.9 shows the distribution of the abnormalities before and after the measures). By protecting the crabs through a period of growth, Weldon was able to 'raise the mean frontal breadth'. Though he realised his experiment was open to objection, as 'confinement to a bottle may directly modify a crab during the critical period of moulting', his results seemed to have suggested that there was 'a selective agent on the shore which [was] absent from the bottles'.[127] Once Weldon had removed the selective action of the mud, it could then be seen that the differential death rate had been removed as well.

By the end of this sixth consecutive summer, of growing, feeding, cleaning and measuring hundreds of crabs at Plymouth Sound, Weldon was now showing greater signs of fatigue: 'in spite of unlimited tea and quinces, [he] had no energy to do more than grow crabs just now': and he doubted if he would 'manage much longer!'.[128] The labour was excessive. One crabbery

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125 *Ibid.*
126 *Ibid.*, 505-6.
127 *Ibid.*, 506.
128 Weldon, 'Letter to Karl Pearson', 22 July 1896, KP:UCL.

Figure 4.8
Differential Mortality Rate of 248 Male Crabs from China Clay Experiment

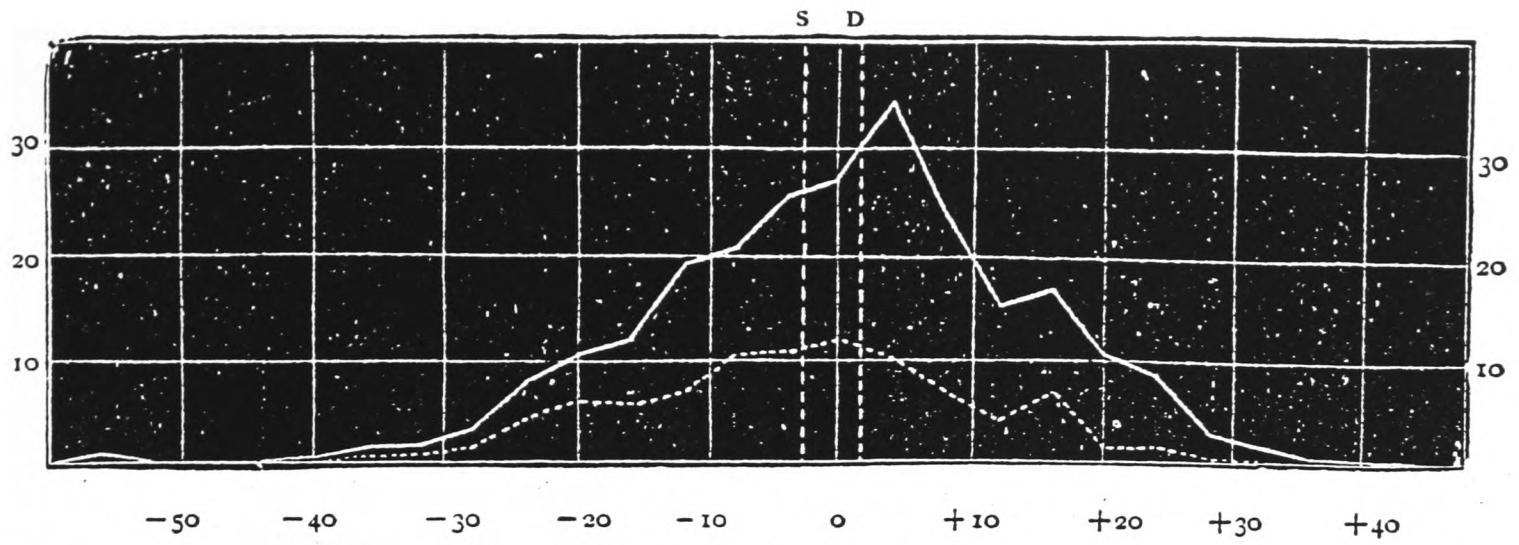


FIG. 5.—Diagram showing the effect of china clay upon 248 male crabs. The upper curve shows the distribution of frontal breadths in all these crabs; the dotted curve the distribution of frontal breadths in the survivors. The dotted line S shows the mean of the survivors; the line D the mean of the dead.

Figure 4.9
Distribution of 527 Female Crabs Before and After Moult in Clear Sea Water (1896)

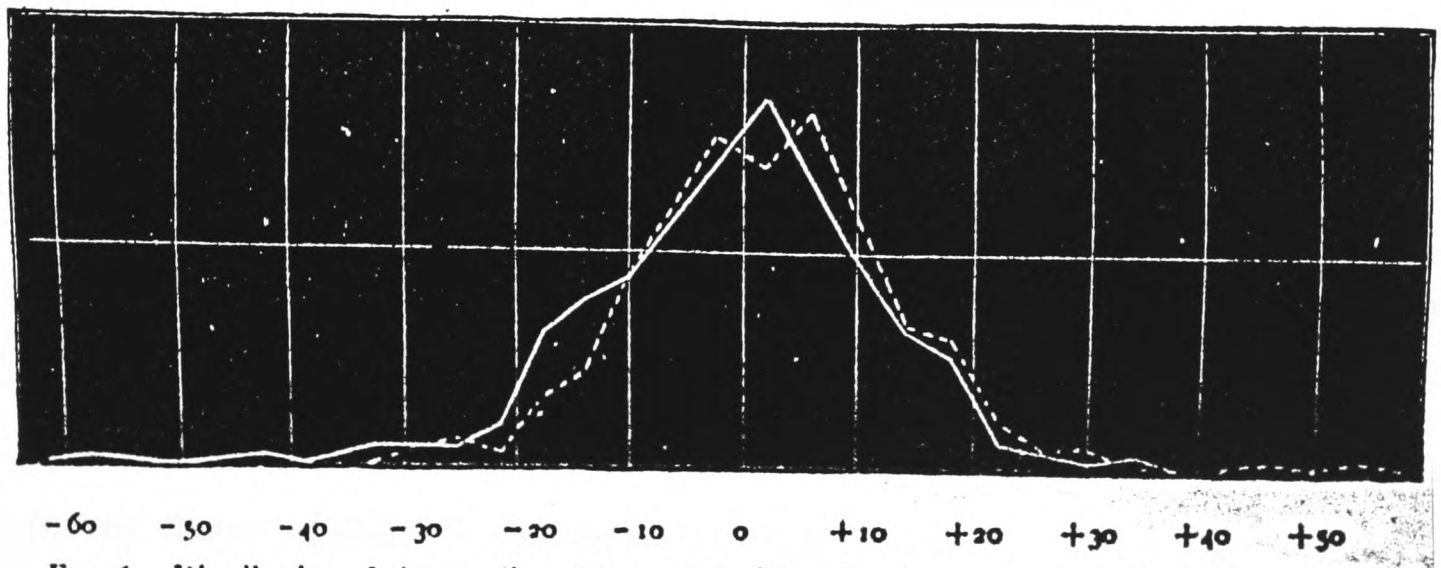


FIG. 6.—Distribution of abnormality of frontal breadth ratios in 527 female crabs before and after moulting in captivity. The continuous line shows the distribution before, the dotted line after moulting.

> consisted of 500 wide-mouthed bottles, each with two syphons for a constant flow of sea water, and each crab had to be fed daily and its bottle cleaned. During the 'summer of 1897 Weldon spent the whole of his days at the aquarium, and his wife hardly left him except to fetch the needful chop'.[129]

On 22 July, Weldon wrote to Pearson summarising the results of his experiment with crabs in putrescent animal matter (which he would also mention briefly, in a footnote, in his Presidential address to the Zoological Section of the British Association in the following year). He wrote:

> I have grown 341 crabs in putrid water and after 5½ days 226 died. The frontal breadth in the survivors is I think distinctly less than in the dead. I have put crabs in fresh water. After four hours only some 20% remain alive and they have always a very high frontal breadth index... Crabs grown in water with *clear* and (freshly precipitated $CaCo_3$, Kaolin, etc.) die slowly and so far as very few measures go, the crabs with large frontal breadths die first - the curve is not humpy in the same regular way.[130]

In all cases the survivors were found to have a smaller frontal breadth relative to their carapace length.

Weldon enclosed the distributions for these two sets of crabs (see Figure 4.10) which displayed a sizeable discrepancy 'which was proportional to a death-rate'; he wanted to know if Pearson thought 'this difference was significant'. Ending his letter, he remarked that he would be doing the same thing again, of course, and 'you have no idea how disgusting it is to do. One gets covered with putrid abominations'.[131]

WELDON'S ZOOLOGICAL ADDRESS OF 1898 VIS-À-VIS PEARSONIAN STATISTICS

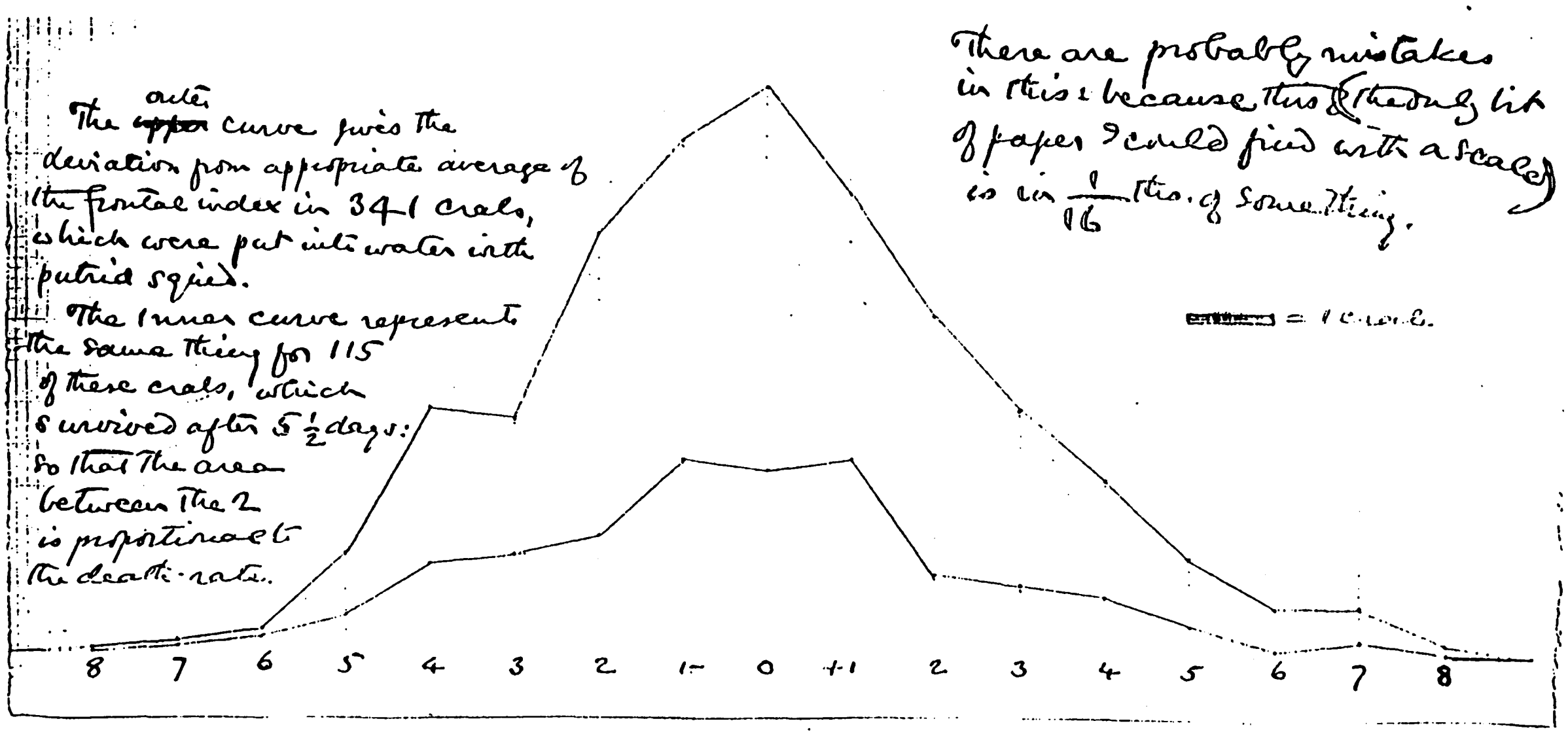
In his presidential address to the Zoological Section of the British Association for the Advancement of Science in 1898, Weldon discussed 'three principal objections which [were] urged against the theory of natural selection', and he also introduced Pearson's statistical methods as a means for dealing with some of these objections. Probability (or the 'theory of chance') was shown as being central to Darwin's theory of natural selection which asserted that

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129 Pearson, 'W.F.R. Weldon', 294.
130 Weldon, 'Letter to Karl Pearson', 22 July 1896, KP:UCL.
131 *Ibid.*

Figure 4.10
The Distribution of the Deviations of the Frontal Index in 341 Crabs placed in Water with putrid Squid (Outer Curve) with the Distribution of the 115 Crabs who Survived after 5½ Days (Inner Curve)

Enclosed in Weldon's letter to Pearson on 22 July 1897



> the smallest observable variation may affect an animal's chance of survival, and it further assert[ed] that the magnitude of such variation with which they occur, is governed by the law of chance.[132]

The first of these objections, which was underpinned by an essentialistic view of species, claimed that if species of animals fell into an orderly series, then purely fortuitous variations could not have provided the opportunity for the selection of such an orderly series, as the variation was thought to be too small and useless. The second was that it did not seem probable that minute structural variation could affect the death rate. The third, was that the process was so slow that 'the time required for its operation is longer than the extreme limits of the time given by estimates of the age of the earth'.[133]

The objection to fortuitous variation, as the source of material on which natural selection can act, was thought to be a misunderstanding of the meaning of the word 'chance'. The use of such epithets as *blind* and *lawless*, which were 'constantly [being] applied to chance', was thought to have contributed to this misunderstanding of statistical chance. As Weldon thought that ideas of chance were 'so thoroughly misunderstood by a number of writers on evolution', he devoted nearly the whole of his Presidential Address to the application of Pearsonian statistics to problems of animal evolution. He began with an illustration using the dice-throwing frequencies Pearson had used in one of his Gresham Lectures in 1892 (which Weldon had supplied for Pearson), and then used dice frequencies provided by his wife who 'had spent some time during the last two months in tossing dice for [his audience]'.[134]

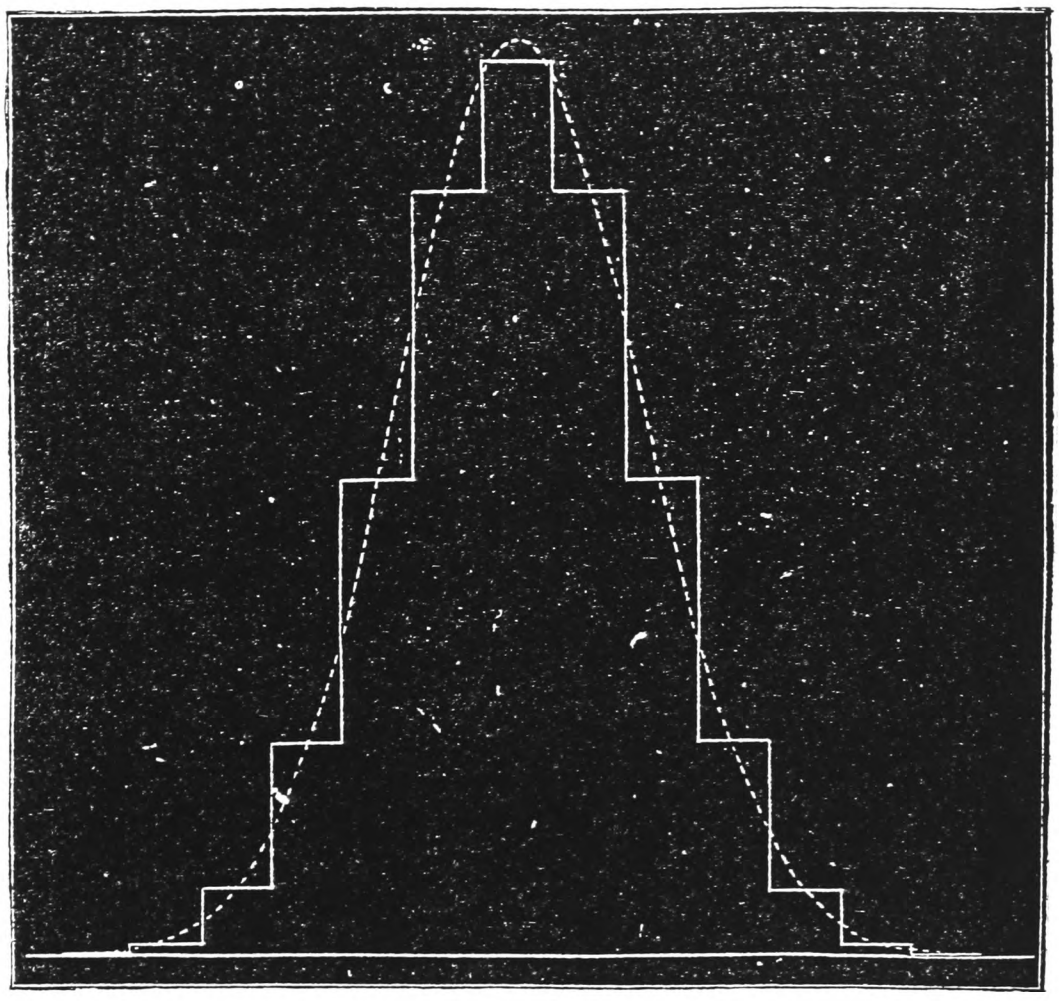
To express the law of chance so that it could be applied to animal variation, Weldon explained that it would be necessary to make use of the binomial expansion $(p+q)^n$. Thus, to determine the number of dice with six points, in a trial of 12 dice, Weldon then showed that it would be necessary to expand the binomial $(^1/_6 + ^5/_6)^{12}$. However, in situations where the exact number of outcomes was very large, the expansion of the binomial would have required a considerable amount of time.

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132 Weldon, 'Opening Address', 499.
133 *Ibid.*
134 *Ibid.*, 500.

Therefore, another statistical procedure would need to be undertaken: and this could be accomplished by setting up a frequency distribution. To illustrate this procedure, Weldon showed the frequency distribution for the binomial $(^1/_2 + ^1/_2)^{12}$ which resulted in the symmetrical distribution shown in Figure 4.11.

Figure 4.11
Weldon's Symmetrical Binomial Distribution (1898)



In situations where the variation in either direction was equally likely to occur, a symmetrical curve could be used. Weldon then mentioned that it was Francis Galton who first applied this law of chance to variations in human stature, and found that his distributions were symmetrical; Weldon found later that this held true 'in a good many characters of animals'.[135] There was, however, a

> great difficulty in applying the law of chance to the treatment of asymmetrical distributions by a similar curve, [which] until quite lately... was unknown; [hence,] there was no obvious way of determining whether these asymmetrical distributions obeyed the law of chance or not.[136]

135 *Ibid.*, 502.
136 *Ibid.*, 501.

Weldon went on to say that the curve of an asymmetrical distribution of chances 'was first investigated by my friend and colleague Prof. Karl Pearson'. It was in his 1895 memoir on skewed variation when Pearson showed how

> these curves might be applied to practical statistics... by showing that several cases of organic variation could be easily formulated by the method he described: and in this way he made it possible to apply the theory of chance to an enormous mass of material, which no one had previously been able to reduce to an orderly and intelligent form.[137]

Weldon considered that Pearson's statistical system was 'experimentally justified by the fact that most statistics relating to organic variation [were] most accurately represented by the curve[s] of frequency which Prof. Pearson deduce[d]'.[138]

For his first example of asymmetrical distributions, Weldon used his 1893 data from the carapace of 999 female crabs in Naples, and showed that 'in every generation a series of deviations from the mean is regularly produced'.[139] When showing examples of distributions which were more skewed than his Naples crabs, he remarked that 'again you can see the beautiful way in which Prof. Pearson's curve expresses it'.[140]

The determination of the law of variability and the law of chance (i.e., fitting an empirical curve to a theoretical curve calculated from the probability values generated by the empirical curve), was seen as being 'of fundamental importance to the theory of natural selection' when examining differential mortality rates in plants and animals.[141] Skewed distributions, for example, might be used to show directional selection — when 'the range of variation [was] greater on one side of the mean than on the other'.[142]

To show examples of directional selection, Weldon used data from the Müllerian gland in the right fore leg from 2000 female swine (examined in Chicago) as well as data from the variation in the number of petals from a race of buttercups

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137 *Ibid.*
138 *Ibid.*, 502.
139 *Ibid.*
140 *Ibid.*
141 *Ibid.*, 504.
142 *Ibid.*, 503.

collected by Hugo deVries in 1877. As both distributions showed a positive skew, it was thought that

> the amount of selective destruction required in order to change either the mean or the modal character of these pigs in one direction, would [have been] greater than the amount required, in order to produce a change of equal magnitude in the opposite direction, and the amount of change is greater in one direction than in the other.[143]

Therefore, it could be seen that the number of Müller's glands in swine, offered

> distinctly greater facility for selective modification in one direction than in the opposite; and [for buttercups] the number of petals in a race of buttercups appears to offer scope for modification in one direction only, at least by selection in one generation.[144]

A new method had been given by Pearson which 'afforded a means of expressing such knowledge in a simple and intelligible form'. Weldon thus felt very strongly that it was 'the duty of students of animal evolution to use the new and powerful engine which Prof. Pearson has provided, and to accumulate this knowledge in a large number of cases'.

In concluding, Weldon hoped that he had convinced his audience of two points - one statistical and the other experimental. The first point was that the law of chance made it possible to express easily and simply the frequency of variation among animals. This involved constructing frequency distributions and then using the method of moments as an empirical measure of the goodness of fit. Weldon then urged that it

> ought to be known numerically how much variation is occuring now in animals... the effect that variation has upon the death rate... [and] how such variation is inherited from generation to generation.

The second point was that the 'action of fortuitous variation can be experimentally measured, at least in the only case in which anyone has attempted to measure it'. From this, he also hoped that he had convinced his audience 'that the process of evolution is sometimes so rapid that it can be observed in the space of a very few years'.[145]

After Weldon had spent seven summers measuring crabs at Plymouth Sound, Pearson thought that Weldon had provided a reasonable explanation of the selective

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143 *Ibid.*, 506.
144 *Ibid.*
145 *Ibid.*

action in an aspect of the crab's breathing apparatus. Thus, 'after years of discouragement and hard labour, Weldon succeeded in demonstrating that natural selection was really at work, and further that it was at work at a very sensible rate': he was able to show disturbances (or deviations) in the distribution from normality as a consequence of differential mortality rates.[146]

THE RESPONSE TO WELDON'S FINDINGS

With the exception of the response from the biologist, William Turner Thistleton-Dyer, the reaction to Weldon's Report of 1895 'produced a mass of criticisms' which resulted in

> folios [being] written to the Chairman of the Committee, showing how this, that or the other vitiated entirely the results. The very notion that the Darwinian theory might after all [have been] capable of statistical demonstration seemed to excite all sorts and conditions of men to hostility. Weldon, instead of being allowed to do his own work his own way, had to be constantly replying to letters, some even eighteen sheets along... [which] were not sympathetic and suggestive, but mostly controversial.[147]

The reaction to Weldon's results is not altogether surprising when the reaction to Charles Darwin's theory of natural selection (some thirty-six years earlier) is taken into consideration. David Hull found that natural selection was not stressed in a single one of the favourable reviews of the *Origin* that were published in 1859.[148] Darwin found little support even from those who championed his work:

> Lyell never accepted natural selection and when he finally accepted evolution, he most often referred to it as 'Lamark's theory' to the great annoyance of Darwin. T.H. Huxley, Darwin's bulldog, was a staunch defender of natural selection throughout Darwin's life and yet Poulton provided good evidence that he 'was at no time a convinced believer in the theory he protected'... Huxley made no reference at all to

146 Pearson, 'W.F.R. Weldon', 294.

147 *Ibid.*, 292.

148 David L. Hull, *Darwin and His Critics* (Harvard University Press: Cambridge, Mass., 1973), cited in Mayr, *Growth of Biological Thought*. William Provine commented that whilst natural selection 'aroused a very strong reation' in the scientific community Darwin's 'ideas of the evolution of species was not strongly adverse to most biologists, especially after the initial impact of the Origin'. Provine further argued that most of the 'attacks were initiated by thinkers who found natural selection abhorrent for nonscientific reasons', in *Origins of Theoretical Population Genetics* (Chicago University Press: Chicago, 1971), pp.10-11.

> natural selection. When using the word 'Darwinism', Huxley applied it as often as not to the theory of evolution by common descent.[149]

The only solid support Darwin received for natural selection came from the naturalists, Thistelton-Dyer and Darwin's friend J.D. Hooker. E. Ray Lankester became convinced after '[August] Weismann had refuted the inheritance of acquired characters'.[150] Wallace remarked in 1894 that

> neither Bateson nor Galton appears to have an adequate conception of what Natural Selection is or how impossible it is to escape it. They seem to think that give a stable variation, and Natural Selection must hide its diminished head![151]

Indeed, Galton wrote in a paper on fingerprints in 1891 that, '*natural selection is wholly inoperative in respect to individual varieties of patterns and unable to exercise the slightest check upon their vagaries*'.[152] Pearson thought that Bateson had 'failed to realise that Evolution by natural selection depends upon mass-changes, i.e., on selective death-rates which demand actuarial method'.[153]

It was to be Thistleton-Dyer who would express the most optimistic, albeit cautious, assessment of Weldon's and Pearson's work as it was reflected in the 1895 Report to the Royal Society. He was particularly interested in their work on the stability of species as he thought the conception of the species most frequently used was the species as an

> abstraction which we endeavour to represent in our museums by a specimen which would be placed as near as possible to the centre of greatest density [i.e., the mean]. Such an abstraction we may call the *mean specific form*.[154]

Quoting from Darwin's letter of 1873 to *Nature* that the law of variation should be grouped symmetrically about the average, he went on to explain how natural selection

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149 Mayr, *Growth of Biological Thought*, p.511. 'Huxley was a morphologist, physiologist and embryologist and the evolution of the organic world was to him equivalent ot the evolution (as he called it) of the chick embryo in the egg'. (Mayr, p.510).

150 *Ibid.*, p.512. The botanists, on the whole, were opposed to selection. Natural selection was the component of Darwinism that offended Darwin's opponents most deeply ('it dethrones God'). Asa Gray was one of the few people who succeeded in reconciling natural selection with a personal belief in God. And it was not only theologians, philosophers, and lay people who opposed this thesis, but, up to the evolutionary synthesis of the 1930s and 1940s, even the majority of biologists (Mayr and Provine, 1980, cited in Mayr, p.510).

151 James Marchant, *Letters and Reminiscences of Alfred Russel Wallace* (New York, 1916), p.313.

152 Pearson, *Life of Galton*, IIIB, p.169.

153 *Ibid.*, p.260.

154 W.T. Thistleton-Dyer, 'Variation and Specific Stability', *Nature* (14 March 1895), 459.

could change the shape of the distribution which would have shifted the centre of density or its 'mean-specific form'. Moreover, he argued this principle of stability had scarcely received the attention it deserved until Weldon began his investigations. It then appeared that

> an important light [was] thrown on what [he] termed the *stability problem* by the remarkable investigation recently presented to the Royal Society by Prof. Weldon. It is one which I am persuaded would have given Mr. Darwin peculiar pleasure.[155]

He then went on to explain how Weldon set up his procedures by using a *frequency curve* and a discussion

> of the general principle and of the interpretation of such curves [and that] must be made to the able investigations for which biologists are indebted to Prof. Karl Pearson in *Phil. Trans.* 1894, A., pp. 71-110... By applying the method to a crab from Plymouth Sound, Prof. Weldon has been able to throw light, it seems to me for the first time, on a most important factor in the stability problem.[156]

When the frequency curve is asymmetrical, Thistleton-Dyer agreed with Pearson when he inferred that the pressure at a given time [indicated] some particular form of natural selection'.[157] He also agreed with Weldon in seeing no reason to believe that there was any 'innate progressive tendency to organisms'. Thus it seemed that Weldon had shown 'a most important proof of establishing the "selective destruction" of variations aberrant from the mean specific form'.[158] Moreover, Thistleton-Dyer thought that Weldon's 'actual statistical demonstration' which showed that

> selective destruction took place in early life amongst [crabs]... serve[d] to rank among the most remarkable achievement in connection with the theory of evolution experiments.[159]

E. Ray Lankester did not respond as favourably as Thistleton-Dyer had. He could not understand why it was not possible to say 'which of two or more correlated growths is the cause of survival' as Weldon had said to Lankester earlier, that it was 'impossible logically to separate... two correlated phenomena'.[160] When Weldon wrote about 'the "effect" of small variations upon the chance of survival', Lankester

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155 *Ibid.*, 460.
156 *Ibid.*
157 *Ibid.*
158 *Ibid.*, 461.
159 *Ibid.*, 460.
160 E. Ray Lankester, 'Are Specific Characters Useful?', *Nature* (16 July 1896), 245.

attacked Weldon as he thought it was 'unwarranted' to speak of the 'effect' of the frontal proportion of the crab. Moreover, it was unwarranted

> to estimate the 'advantage' and 'disadvantage' of this proportion, and finally to maintain that its 'importance' may be estimated without troubling ourselves to inquire how it operates, or *whether indeed it is operative at all.*[161]

A couple of weeks later Weldon wrote to Pearson to ask him:

> Have you read Lankester's letter about 'operative' and 'inoperative' correlations? It is a great joke; but it is really rather solemn that such doctrine can be tolerated by so many folks. All good biologers down here [Plymouth] bother me daily by explaining what a fool I must be to have no conception of 'operative' variations. They say that the idea of a cause is 'essentially dynamic' and they feel proud. But all things work together for good to them that love the Lord and the little row has made me read the *Kritik reiner Vernmirg* in earnest.[162]

Two weeks after Weldon wrote this letter, Lankester wrote to *Nature* again partly in response to Thistleton-Dyer and also to Weldon. Continuing the line of thought he developed in his previous letter, Lankester wrote:

> Surely [with] the case of Prof. Weldon's crabs, most naturalists would take the view that the frontal measurements may *possibly* be operative in saving the life of a crab, or *may* be only a correlative of some other life-preserving structure;... this should be inquired into by means of hypothesis and experiment; and that, until this is done, it is *premature* to speak of a particular frontal proportion as having for its *effect* the survival of those crabs distinguished by its possession.[163]

He continued to attack the methods that Weldon used to determine selective death rates for his crabs, as he thought that

> such methods of attempting to penetrate the obscurity which veils the interactions of the immensely complex bundle of phenomena which we call a crab and its environment, appear to me not merely inadequate, but in so far as they involve perversion of the meaning of the accepted terms and a deliberate rejection of the method of inquiry by hypothesis and verification, injurious to the progress of knowledge.[164]

Ending his letter somewhat derisively, Lankester wrote:

> You can not (it seems to me) reduce natural history, as Prof. Weldon proposes, to an unimaginative statistical form, without either ignoring or abandoning its most interesting problems, and at the same time refusing to employ the universal method by which mankind has gained new knowledge of the phenomena of nature - that, namely of imaginative hypothesis and consequent experiment.[165]

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161 *Ibid.*, 246.
162 Weldon, 'Letter to Karl Pearson', 8 August 1896, KP:UCL.
163 E. Ray Lankester, 'The Utility of Specific Characters', *Nature* 54 (20 August 1896), 366.
164
165 *Ibid.*, 413.

In defence of his work, Weldon wrote:

> I am far from wishing to reject the method of imaginative hypotheses and subsequent experiment or observations. I respect the method as sincerely as Prof. Lankester himself, and although I cannot pretend to his measure of skill in using it, yet, so far as I can see, I have, in my work on frontal breadth of crabs, employed this very method to the best of my ability. The hypothesis which I started was, that if natural selection acted upon the frontal breadth of crabs at all, there ought to be a demonstrable difference between the percentage of abnormal frontal breadth in young crabs, and the percentage of the same abnormalities in older crabs; and I proceeded to test this hypothesis by measurement of crabs of different sizes. The result showed that a change in the frequency of abnormal frontal breadth could, in fact, be observed. The effort of imagination was here small enough but, such as it was, it served to guide my first step.[166]

Pearson came to Weldon's defence in a letter to *Nature*. He began with a certain degree of caution by saying that it was

> dangerous for a mere mortal to take part in the strife of the gods, or for 'gyrating'[167] mathematicians to join battle with biologists. But as the enthusiasm of Prof. Weldon for his subject has so largely perturbed my normal gyrations, that I have devoted many months to the statistical theory of evolution, perhaps I may be permitted a word or two on the subject of the present controversy.
>
> To demonstrate that natural selection, whether secular or periodic, is actually taking place in any species, and to measure its amount, is in the present state of our knowledge one of the grandest pieces of work that could be done. It would achieve for the Darwinian theory what Hertz achieved for the Maxwellian theory of light. At present, no one has gone further in the direction of this demonstration than Prof. Weldon, and I am inclined to think with Mr. Thistleton-Dyer - and I take it is opposition to Prof. Lankester - that such a demonstration can only be achieved by the statistical method.[168]

William Bateson was not able to attend as he was in the Bahamas where he had been studying *Balanoglossus*. In a letter to *Nature* on 23 April 1895, Bateson mentioned that he could not attend the meeting when Weldon talked about finding empirical evidence of natural selection, but he makes no mention of Weldon's results. Instead he comments on Thistleton-Dyer's letter concerning the *Cineraria* controversy.[169]

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166 W.F.R. Weldon, 'The Utility of Specific Character', *Nature*, 54 (3 September 1896), 413.

167 In a footnote, Pearson explained that 'the term is due to Prof. Lankeser, who thus described us'. He thought the term 'gyrating' was first used by William Turner Thistleton-Dyer which he used in the early days of the Teaching University Movement.

168 Karl Pearson, 'The Utility of Specific Characters', *Nature*, 54 (17 September 1896), 460.

169 William Bateson, 'The Origins of the Cultivated Cineraria', *Nature* (25 April 1895), 605. Bateson begins the letter by writing:

> Returning from abroad, I have just seen Mr. Dyer's letter in *Nature*, March 14. Of the matters there treated I ask leave now to deal with one only, that numbered (18) [in Thistleton-Dyer's letter]. This is a point of fact—the origin of the cultivated Cineraria.

Nevertheless, examination of Weldon's extant copy of the 1895 Evolution Committee Report, which described the results that appeared in *Nature*, seems to suggest that Bateson read this copy as he seems to have written comments on it. That is to say, each time Weldon refers to Bateson's name or his work in the extant copy, comments such as, 'to me?' or 'This should be "greater". W.B.', have been written in the margins.[170] Apart from these comments, Bateson had very little else to say regarding this paper - perhaps this was indicative of Bateson's lack of interest in natural selection in general.

The nature and the extent of the controversy surrounding Weldon's attempts to show evidence of natural selection would seem to have inhibited the potential for other biologists to have pursued this type of work in the 1890's. Though many of their views received some support from Alfred Russel Wallace, William Turner Thistleton-Dyer was, perhaps, the most responsive and supportive biologist of their time.

THE REACTION TO WELDON'S PRESIDENTIAL ADDRESS

Six weeks after Weldon gave his Presidential Address, J.T. Cunningham and George Henslow both wrote letters to *Nature* expressing their doubts concerning Weldon's methods and interpretations for determining empirical evidence of natural selection. Cunnigham thought that Weldon's 'mathematical framework' of the crabs selective death rate, was 'no answer whatever to the reasoning of those who oppose the theory of natural selection'.[171] Though he did not dispute the measurements, he thought that Weldon's 'interpretations of them [were] obviously and demonstrably unsound'.[172] He was of the opinion that the dimunition of size of frontal breadths was not due to natural selection, but was instead due to the age of the crab. Henslow presumed that all of the crabs in 1893 were younger than those in 1895 who were, in turn, younger than those of 1898. Therefore, he did 'not suppose that the [selective action] of mud

170 Weldon, 'A Report... on Crab work, referring to [the] summers of 1895 & 1896', KP:UCL, *passim*.
171 T. Cunningham, 'Organic Variations and their Interpretations', *Nature*, 58 (20 October 1898), 593.
172 *Ibid.*

had anything to do with it'.[173] The survivors were thus regarded as being older than non-survivors (as their frontal breadth would have been narrower than the younger crabs).

Weldon, of course, did not agree with Cunningham that the theory of natural selection attempts to answer 'whether a given modification... originated accidentally, or as the results of a definite ascertainable cause'.[174] He also did not think that natural selection 'involve[d] a theory of the origin of variation' as all that it claimed was that 'the variation which is known to occur does affect the death-rate'.[175] He explained that his primary interest was to understand 'how much of observed change is due to a process of selective destruction, [and] how much to other causes'. He then pointed out that even though a change in varying conditions produced a change in the character of a race by methods other than selective destruction, this did not 'disprove the co-existence of selective destruction'. Moreover, he heartily agreed with the view that it was

> not possible for selection, under fixed conditions, to modify a species in every direction. It is only possible for natural selection to act so as to produce a race with a minimum death-rate.[176]

As regards age of crabs, Weldon had worked out the relation between growth-rate and frontal breadth abnormalities in more than 500 cases; he had determined that

> the frontal breadth of a crab depends on its age, while the length of a crab not only depends upon age, but upon temperature and other circumstances affecting it during growth. From this it [was] deduced that in a group of crabs of the same length, those with narrower fronts are older, those with broader fronts are younger.[177]

George Henslow's evaluation was based on his 'contention that *individual difference* - with which Professor Weldon [was] solely concerned - [*did*] *not afford*

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173 *Ibid.*
174 *Ibid.*
175 W.F.R. Weldon, 'Organic Variations and their Interpretations [Weldon's response to Cunningham], *Nature* 58 (20 October 1898), 595.
176 *Ibid.*
177 *Ibid.*

the materials for new varieties or species'.[178] Henslow had understood Darwin's theory to mean that

> an individual has some slight variation or *new* feature, which is beneficial to it in the struggle for life among other organisms, more especially of the same kind... then, such an individual may prove itself best fitted to survive and ultimately establish a new variety, the others dying out in the struggle.[179]

Thus, he had an Aristotelian essentialistic view of species as he thought that differences of 'form' were much more important than 'size' or 'number'. He also thought that whilst 'Darwin requires "variation", there [was] none in any of Prof. Weldon's four examples'. He thus concluded that Weldon's 'numerical' analysis 'tested natural selection, and [had] shown that nothing of the nature of a *true variety* [had] been established. There [was] no *evolution* in the process described at all'.[180]

Weldon had 'altogether fail[ed] to understand Mr. Henslow's letter'. He thought that it must have been his 'imperfect exposition' that led Henslow to 'misunderstand one of the clearest passages in the *Origin of the Species*'.[181] Henslow thought that a variation

> fit to afford material for natural selection, must be a *new* character, differing in some mysterious and undefined way from those individual differences which he refuse[d] to call variations; and he further attributes the same view to Mr. Darwin.[182]

Weldon then explained that from 1894 to 1895, the crabs at Plymouth had not exhibited any changes in the magnitude of their frontal breadth which Mr. Henslow would have ranked as variation, but they have exhibited individual differences. During this time

> the mean frontal breadth had changed 2 per cent so that the change now going on would produce, if it were to continue at the same rate for fifty years, a change big enough to constitute a difference which most men would rank as specific.[183]

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178 George Henslow, 'Organic Variations and their Interpretations', *Nature* 58 (20 October 1898), 594.
179 *Ibid.*
180 *Ibid.*, 595.
181 Weldon, 'Organic Variations and their Interpretations' [Weldon's response to Henslow], *Nature*, 58 (20 October 1898), 595.
182 *Ibid.*
183 *Ibid.*

He had attempted to show that this change was accompanied by a destruction which had acted selectively upon individual differences, Mr. Henslow had 'not seriously discussed [this] idea, but ridicule[d] the idea that so small a chance can be of importance in relation to evolution'.[184]

Writing in 1917 on 'The Selection Problem' of the late-nineteenth century, Raymond Pearl regarded Weldon's work on the shore crabs to be 'pioneering' as Weldon was the first to demonstrate numerically that the 'selective elimination of individuals [was] different physically from the survivors... [whilst] the eliminating environmental factor [was] the silt in the water'.[185] Pearl also remarked that Weldon's work was followed by a number of investigations 'of a more or less similar character'; this included the work of E. Poulton and Saunders as well as H.C. Bumpus in 1899; H.E. Crampton in 1904, C. Davenport in 1908 and J.A. Harris in 1910 and in 1912.[186] Whilst most of these investigations showed that

> on the whole, the survivors [were] not physically different from those naturally eliminated. Again the studies of Weldon on *Clausillia* [in 1904] come first... [in providing] experimental and quantitative evidence regarding selective elimination.[187]

Soon after Bumpus published his results on the American and English house sparrows, Pearson had found the 'occasion for a special purpose to measure 180 clutches, containing 687 eggs of the [English] house sparrow'.[188] Bumpus had concluded that the 'American egg was more variable than the English egg'. Pearson, however, thought that the method Bumpus used that led to this conclusion, 'was open to criticism'. To compare his results to the measurements from Bumpus' data, Pearson had to calculate first the means and standard deviations from Bumpus' measurements

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184 *Ibid.*, 596.

185 Raymond Pearl, 'Natural Selection', *The American Naturalist*, 51 (1917), 69.

186 Edward B. Poulton and Saunders, *Report of the British Association* (1899), pp.906-9; Herman C. Bumpus, *Biological Letters*, Woods Hole (1908), pp.209-26. Charles B. Davenort, *Nature* 78 (1908), 101; J.A. Harris, *Science*, N.S. 32 (1910), 519-28; *idem.*, *Science*, N.S. 36 (1912), 713-15, cited in Raymond Pearl, 'Natural Selection'.

187 Pearl, 'Natural Selection', 70.

188 Karl Pearson, 'Variation of the Egg of the Sparrow (*Passer domesticus*)', *Biometrika*, 1 (1901), 256-7. Pearson thought that the eggs were 'a fairly reprsentative English series' as they came from Essex, Hertfordshire, Berkshire, Surrey, Cambridgeshire, Nottinghamshire and Yorkshire.

(as he had not published these values). When Pearson re-analysed the data, by using the coefficient of variation, he found that there was 'no evidence that the American egg was more variable than the English egg'.[189] Nonetheless, Pearson thought that 'Professor Bumpus [had] opened a most interesting field of inquiry' and suggested that annual measurements be made in both countries to 'ascertain whether there are persistent sources of differentiation at work'.[190]

DIFFERENTIAL MORTALITY AND DIFFERENTIAL FERTILITY

In Pearson's third paper on 'The Mathematical Contributions to the Theory of Evolution: Regression, Heredity and Panmixia' in 1896, he began to look at certain mathematical properties of natural selection, sexual selection and reproductive selection.In this paper Pearson explained that natural selection itself merely signified the selection of variation useful to the individual, or gave it a more stable relation to its environment. Whilst it was perfectly compatible with a complete absence of heredity, Pearson emphasised that 'natural selection without heredity is, of course, not "Darwinism"'.[191] The word evolution would not only cover natural selection, 'but physical selection, and sexual selection, or any *vera causa* of progressive change in organic or inorganic types'.[192]

Pearson distinguished between two aspects of natural selection, both of which could be measured by using differential mortality rates. The first aspect was secular (or directional) natural selection which was 'measured by changes due to mortality, in the mean and standard deviation of the variation-curve as we pass from one adult generation to the next'.[193] He explained that for statistical observations on man, it was not easy to differentiate it from 'the effects of sexual selection, and of altered sanitary conditions'. The second aspect of natural selection was periodic (or balanced) natural selection, which was to have left

189 *Ibid.*, 256.
190 *Ibid.*, 257.
191 Pearson, 'MCTE, III: Heredity, Panmixia and Regression', 253-318.
192 *Ibid.*, 262-3.
193 *Ibid.*, 257.

> no trace of itself in the adult [mortality] variation-curves of successive generations; it is measured by the changes due solely to mortality in the mean and standard deviation of the variation curves at successive stages of the *same* generation - *due allowances being made for the changes of the variation-constants due to growth.* In other words, if we watched a generation from birth to adult stage, carefully preserving it from any form of selective morbidity, such arises from the struggle for existence, we should find changes in the variations constants due to the law of growth. If now the same generation be subjected to the struggle for existence, i.e., placed in its natural surroundings the variations-constants [i.e. the coefficient of variation] will differ from their values at the corresponding stages of unselected growth. This difference is due to the selective mortality; i.e., to natural selection. But this selective mortality may go on and still leave the variation-constants of the adult stage of each generation the same. In this case we speak of it as periodic natural selection. It repeats itself in each generation, but produces no secular change. It maintains an adult standard, but is not a factor of progressive evolution.No estimate of periodic natural selection can be formed until the law of growth has been accurately ascertained by a series of observations on individuals. The influence of natural selection will be allowed for in our investigations by supposing the mean and standard deviations of successive adult variation-curves to be not necessarily the same.[194]

Sexual selection was also of two types which could be distinguished as 'individual and tribal tastes'. Tribal taste was seen as the preference or the rejection of one sex mating with the other sex having special characteristics. This was also referred to as *preferential mating* and was measured 'by the difference in mean and standard deviations between the variation-curves for the whole adult population of one sex, and for the mated portion of it'.[195] Pearson suggested that the mean height and the mean variation (i.e., the standard deviation) in height of women which might not have been necessarily

> identical with the mean height and mean variation in height of wives. Preferential mating may [thus] have reference to any organ or measurable characteristic of either sex.[196]

Though natural selection most usually indicated differential mortality rates for Pearson, in this paper he introduced the term 'reproductive selection' which was the differential fertility rate. The quantitative measure was 'the correlation between the size of any selected organ in either male or female and their reproductivity, the reproductivity being measured by the number of offspring in either sex or both

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194 *Ibid.*
195 *Ibid.*, 258.
196 *Ibid.*

sexes'.[197] Nonetheless, most of his work with Weldon would continue to involve a determination of selective death rates.

Some two years later, when the Royal Society asked Galton to referee one of Pearson's papers, Galton, who referred to, 'sexual selection' as 'productiveness', commented that Pearson's term 'Reproductive Selection' was confusing, if not ambiguous.[198] Pearson then wrote to Galton on 30 November 1898, to explain his position that he did

> quite realise the difficulty about the term *Reproductive Selection*, but I thought that in vain three years ago for a better, and failing to find one have used it ever since in my papers... Evolution takes place by taking out of the community A,B,C,D,E,F...X,Y,Z, certain members L,M,N, and putting them into a position of advantage for propagating their kind. Anything which contributes to this advantage is *selection*, a differential death-rate is Darwin's *natural selection*, it should be *Selection of the Fitter* as all selection in wild life is 'natural'. Selection by a differential birth-rate is by reproductive selection; it is selection of the most fertile.[199]

Nonetheless, Galton thought that Pearson's idea, as expressed by the words, 'Reproductive Selection'

> will hereafter become so important an element in all questions of Evolution, that before they are too firmly established I really think it most advisable to change them. It will otherwise be a continual stumbling block to new students. Pray forgive me for all of this.[200]

THE DISSOLUTION OF THE EVOLUTION COMMITTEE

Pearson and Weldon had hoped to have been able to set up a type of laboratory where they could pursue more elaborate experiments with plants and animals to further investigate natural selection at work. This possibility arose when Charles Darwin's house went up for sale shortly after his wife's death. Weldon had learnt the news from Galton and he wrote to Pearson in the latter part of 1896 to tell him that

> Galton had exciting news. Darwin's house is for sale. An attempt is to be made to use it as a place where experiments in selective breeding, such as we both want to see performed may go on. It is quite well enough fitted for the purpose. There is a chance that the whole management may be given to an enlarged version of the Plants and Animals Measurement Committee of the Royal Society revised for this purpose. If

197 *Ibid.*
198 Galton, 'Typical Laws of Heredity', *Philosophical Magazine* (1877), 17.
199 *Ibid.*
200 Pearson, 'Letter to Francis Galton', 30 November (1) 1898, and Galton, 'Letter to Karl Pearson', 4 December 1898 in Pearson, *Life of Galton*, IIIB, p.505.

> that occurs, you must join the committee - you ought to join now, of course: but at present you see the only work done with less trouble to yourself by talking to me - Galton wants you as much as I do; and if the Counsel has the organisation of extensive experiments it will be most important that these should be devised with direct regard to your work. So don't say you have work, or influenza, or anything else; but get a bicycle and prepare to ride to Down on Sunday mornings several times a term.[201]

Alfred Russel Wallace had written to Galton more than five years before Darwin's house went up for sale 'urging [him] that the time was ripe for an experimental farm or institute to undertake researches which might decide disputed points in organic evolution'.[202] In his postscript to Galton, he emphasised the importance of collecting biological data as

> it would of course [have been] better still if a fund could be raised sufficient to establish an *Institute for Experimental Enquiry into the fundamental Data of biology.* This [was] surely of far higher importance than the anatomical embryological, and other work for which the Plymouth Biological Station was founded.[203]

On several occasions Galton had offered tentatively 'a big sum' of money to help in the founding of a Darwinian establishment for plants and animals. This was offered on the condition

> that the large balance needed for such an institution could be raised elsewhere. [He] repeated it more or less formally during the existence of the Cttee, but the response was quite inadequate. The offer of Charles Darwin's house in Down at a moderate (nominal) rent was made by the Darwin family to the Cttee, but the double event of cost of maintenance and the practical impossibility of visiting it from London on Sundays owing to the awkward hours of the trains, made it impossible to accept the offer.[204]

In January of 1897, Bateson, Lankester and others were elected members to the Evolution Committee of the Royal Society and Bateson attended.[205] In the following month, the Evolution Committee received a large increase in membership; it ceased henceforth to 'conduct statistical inquiries into the measurable characteristics of plants and animals'. It became transformed into the Evolution (Plants and Animals) Committee and was resolved to ask that the objects of the Committee should include

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201 Weldon, 'Letter to Karl Pearson', 17 November 1898, KP:UCL.

202 Alfred Russel Wallace, 'Letter to Francis Galton', 3 February 1891, in Pearson, *Life of Galton,*, IIIA, p.128.

203 *Ibid.*

204 Galton, 'Letter to Karl Pearson', 6 July 1906, in Pearson, *Life of Galton*, IIIA, p.287.

205 This included Godman, Heape, Maxwell, Masters and Selvin: taken from Galton's diaries 1891-1897 on the Royal Society Evolution Committee, in Pearson, *Life of Galton*, IIIA, p.127.

'accurate investigation of Variation, Heredity, Selection and other phenomena relating to Evolution'.[206] When this change went into effect, Pearson wrote to Galton that he felt

> sadly out of place in such gatherings of biologists, and little capable of expressing opinions, which would only hurt their feelings and not have been productive of real good. I always succeed in creating hostility without getting others to see my views; infelicity of expression is I expect to blame... All the problems laid down by [Galton and Weldon]... seem to me capable of solution and nearly all of them *in one way only*, by statistical methods and calculations of a more or less delicate kind.[207]

By the summer of 1899, the question was raised 'whether the [Committee] ought not to cease to exist'.[208] Galton had been disappointed with the work of the committee. He had hoped that it would have been

> sufficiently authoritative [and] that its weight would suffice to weld numerous bodies that have gardens of menageries into common action, to allow some plots or cages, &c. for research. The Clifton Zool[ogical] were prepared to do this, but Thistelton-Dyer said that even he could not depend on the gardeners at Kew to carry out any experiment accurately, so that plan fell through... On the whole, the Cttee seemed to have done so little and working with so much friction that I did not care to be longer connected with it.[209]

Galton wrote to Michael Foster on 14 December 1899 and requested that

> Please when the Committee of the Royal Society are appointed do not include my name in the Evolution Committee as its chairman; or even as a member of it. My deafness and prolonged absence in England incapacitate me from properly performing the duties of chairman and I can be of little service as an ordinary member.
>
> The prevalent opinion of the majority of the Committee which I am bound to respect is I hear in favour of the continuance of the Cttee, but the view I do not myself share being doubtful of is usefulness. The conditions of corporate research have proved different to what was anticipated when the Cttee was formed, the number of trustworthy workers being very small, and well able to look after their own interest.[210]

In a postscript, Galton wrote 'I sent this through Weldon's hands for him to read, but [his] views as to its future of the Cttee correspond [to mine], I believe'.[211] A month later, Weldon wrote to Michael Foster that

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206 Galton, 'Letter to Karl Pearson', Date 1898, in Pearson, *Life of Galton*, IIIB, p.506.
207 Pearson, 'Letter to Francis Galton', 5 February 1897, in Pearson, *Life of Galton*, IIIA, pp. 127-8.
208 *Ibid.*
209 Pearson, *Life of Galton*, IIIA, p. 287.
210 Francis Galton, 'Letter to Michael Foster', 14 December 1899, Miscellaneous Manuscripts, vol. 15, The Library of the Royal Society, no. 96.
211 *Ibid.*

> As secretary of the committee, I am (I imagine) bound to remain a member of it until the end of this month, because I have undertaken the responsibility of receiving and reporting upon the grant money. But I heartily concur in Francis Galton's opinion that the methods adopted by a majority of members of the committee are not such as to make further existence of the Committees views likely to serve any useful purpose and I shall therefore be glad if you will allow me to resign my membership on January 31st.[212]

This was followed two days later with Pearson's letter of resignation as he had

> only just heard that Mr Galton and Professor Weldon have resigned their membership of the Evolution Committee. Will you [thus] kindly take the proper steps to remove my name from it also and notify the committee that it is removed? This letter is not to be taken as a criticism of the work of the committee, of which I know nothing, but simply that its meetings have always taken place at a time I cannot attend, and that my only justification in so long remaining a member was the conviction that, if I ever could be of any aid, Mr. Galton or Professor Weldon being acquainted with the proceedings of the committee would let me know. With their resignation the *raison d'être* of my membership has ceased to be.[213]

On the following day, the final letter of resignation was sent to Michael Foster from William Turner Thistelton-Dyer as he had just heard 'that [as] Francis Galton and W.F.R. Weldon have resigned, it would be clearly wrong to go on with it'.[214] At a meeting on February 15, 1900 Michael Foster then announced that the

> Evolution Committee beg leave to report the following gentlemen have resigned their membership of the committee: Francis Galton, Professor Medola, Professor Pearson, Mr. Thistleton-Dyer and Professor Weldon and Mr. Bateson has been appointed to succeed Professor Weldon as secretary.[215]

Following Pearson's resignation, he wrote to Galton that

> Bateson had absolutely no sympathy with the statistical treatment of biological problems, the very work for which the committee had been appointed. Mr. Goodman then became Chairman and the Reports of the Committee were devoted entirely to the publications of Bateson and his school.[216]

Bateson's first memorandum as secretary of the committee, extant in the archives of The Royal Society Evolution Committee, reveals the change of locus that was to occur under his direction in July 1909. Although 'the Committee were in the

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212 W.F.R. Weldon, 'Letter to Michael Foster', 12 and 20 January 1900, Miscellaneous Manuscripts, vol. 15, The Library of the Royal Society, No. 97.

213 Karl Pearson, 'Letter to Michael Foster', 22 January 1900, Miscellaneous Manuscripts, vol. 15, The Library of the Royal Society, no.100.

214 W.T. Thistleton-Dyer, 'Letter to Michael Foster', 23 January 1900, Miscellaneous Manuscripts, vol. 15, The Library of the Royal Society, no. 101.

215 Michael Foster, *Minutes from the Evolution Committee Meeting of the Royal Society*, RS CMM 65.492, from minutes read at the meeting of 15 February 1900.

216 Pearson, *Life of Galton*, IIIA, p.127.

first instance appointed to promote researches in the group of studies now denoted by the term genetics', the full extent of the research in genetics did not materialise as

> owing to the conditions under which such researches are carried on, it ha[d] not been possible for the committee to organise or undertake experimental work. In effect the services of the committee [were] limited to the promotion of publications dealing with genetics. In this capacity they... issued four reports and... a fifth report containing three independent papers.[217]

Even without the support of the Evolution Committee, Weldon and Pearson would continue their work on natural selection into the twentieth century. Weldon now had the statistical machinery he needed to examine selective and continuous biological characters when looking for empirical evidence of natural selection. Moreover, his interests had been instrumental in bringing about Pearson's main statistical innovations during this time. Pearson's statistical resolution of Weldon's problems led to his work on curve fitting of various shaped distributions and in finding an empirical measure for a goodness of fit test. It was this work which would lead to further distributional testing of selective death rates and was instrumental in Pearson's introduction of the chi-square distribution and his chi-square goodness-of-fit test. This next phase of Pearson's statistical innovations, which can be regarded as the pinnacle of his work towards a modern theory of statistics, will be examined in Chapter 6.

Francis Galton's work on inheritance did, however, provide the basis for two significant innovations for Pearsonian statistical theory. The first came from his regression to the mean and the second, from his Law of Ancestral Inheritance which led Pearson to develop a theory of multiple correlation and multiple regression. Galton's contributions to Pearsonian statistics will be examined more fully in the next chapter.

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217 William Bateson, 'Report of the Committee', 6 July 1909, Miscellaneous Manuscripts, vol. 15, The Library of the Royal Society.

Chapter V

MULTIPLE REGRESSION AND FRANCIS GALTON'S LAW OF ANCESTRAL INHERITANCE

When Charles Darwin provided the mechanism for evolutionary biology, he was able to show how favourable variation was conserved and how unfavourable variation was eliminated; though it seemed evident that some of this variation was inherited, he was not able to show how it was inherited. Nine years after he wrote the *Origin*, he attempted to solve the problem of inheritance with his Provisional Hypothesis of Pangenesis.[1] Pangenesis was a blending theory of inheritance and implied that

> every separate part of the whole organisation reproduces itself. So that ovules, spermatazoa, and pollen-grains—the fertilised egg or seed, as well as buds,—include and consist of a multitude of germs thrown off from each separate part of the unit.[2]

Each cell of an organism threw off minute particles of their contents or gemmules, not only during the adult stage, but during each stage of development of every organism.[3] After the gemmules multiplied and aggregated in the reproductive apparatus, this material was to be passed on to the next generation.

To explain how useful variation could survive from one generation to the next without being swamped by the effects of blending inheritance, he argued that it was

> generally necessary that an organism should be exposed during several generations to changed conditions of habits, in order that any modifications thus acquired should appear in the offspring. [Moreover,] the direct action of changed conditions on the organisation of, and of the increased use or disuse of parts; will be themselves modified, and, when sufficiently multiplied, will supplant the old gemmules and be developed into new structure.[4]

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1 Charles Darwin, 'Provisional Hypthesis of Pangenesis', *The Variation of Plants and Animals under Domestication*, vol. 2 (John Murray: London, 1868; 2nd edn. 1875), pp.349-99. Adrian Desmond and James Moore remarked that

> the 'pan' was supposed to convey the idea that these bits—or gemmules—came from the whole body and congregated in the reproductive system, but [his] 'wife said it sounded wicked, like pantheism'.

Adrian Desmond and James Moore, *Darwin* (Penguin Books: London, 1991), p.531.

2 Darwin, *Variation of Plants and Animals*, p.350.

3 *Ibid.*, p.370.

4 *Ibid.*, pp.389-90.

Darwin thus adopted a Lamarkian view in an attempt to resolve the problem of blending inheritance.

Francis Galton, who considered himself to be 'a student of heredity', thought that his cousin's hypothesis 'seemed of [such] pressing importance that [the] postulates should be tested'.[5] Galton had assumed that Darwin's gemmules could be found in blood and he thus 'endeavoured to mongrolize' a breed of silver-grey rabbits 'by infusing blood into their circulation which [he] had previously drawn from other sorts of rabbits'.[6] When he found that there was no evidence of alteration of breed in a single case of his 88 rabbits in 13 litters, he concluded that 'the doctrine of Pangensis, pure and simple, as [he had] interpreted it, [was] incorrect'.[7]

A month after Galton reported his results, Darwin wrote to *Nature* that he 'had not said one word about the blood or any other fluid proper to any circulating system'. If anything, he thought that it was, 'indeed, obvious that the presence of gemmules in the blood can form no necessary part of my hypothesis'.[8] Nonetheless, Galton deserved 'the highest credit for his ingenuity and perseverance... but it [did] not appear that Pangenesis ha[d], as yet, received its death blow'.[9]

In the following year, Galton wrote his first paper on inheritance and had 'oberved the fact of reversion'.[10] Darwin had, by then, attributed reversion to the development of gemmules which had merely lain dormant.[11] Though Galton had objected initially to Darwin's Lamarkian view, some five years after his experiment, he accepted Pangenesis with considerable modification, as 'a supplementary and subordinate part of a complete theory of heredity, but by no means for the primary and

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5 Francis Galton, 'Experiments in Pangenesis, by Breeding from Rabbits of a pure variety, into whose circulation blood taken from other varieties had previously been largely transfused', *Proceedings of the Royal Society*, 19 (30 March 1871), 394.

6 *Ibid.*, 396.

7 *Ibid.*, 404.

8 Charles Darwin, 'Pangenesis', *Nature*, 3 (27 April 1871), 502.

9 *Ibid.*, 503.

10 Francis Galton, 'On Blood Relationships', *Proceedings of the Royal Society*, 20 (13 June 1872), 395.

11 Gerald Geison, 'Darwin, Heredity and the Evolution of his Hyothesis of Pangenesis', *Journal of the History of Medicine*, 24, 4 (1969), 377.

more important part'.[12] Nearly two decades later, Galton was to form a theory of 'particulate inheritance' derived from the 'particles' in Darwin's Hypothesis of Pangenesis. In his 1890 manuscript on 'Sexual Generation and Cross-Fertilisation', he thus wrote that the

> supposition on which the calculations are based, is that the process of heredity is essentially *particulate*. For in as much as the pecularities in minute portions of bodily (somatic) structure admit of being transmitted independently by inheritance, we are justified in looking upon the body (soma) as an organised structure formed through the orderly development of a large number of germinal particles of diverse qualities. It is almost needless to point out that the hypothesis of particulate descent is the basis of Darwin's provisional theory of pangenesis.[13]

In addition to Darwin's theory of Pangenesis, physiological theories of inheritance had also been suggested by Auguste Weismann, Herbert Spencer, Ernst Haeckle, Karl Nägeli and Hugo De Vries.[14] Their ideas

> produced theories of the physical mechanism of heredity—how the units controlling the development of the organism were produced, assembled in the germ-cells and handed on to offspring.[15]

These theories, which emphasised rare or discontinuous variation, attempted to explain what had happened in the reproductive organs to produce that 'most familiar phenomenon that offspring resemble parents in many respects although not completely'.[16] Galton, who was interested in measuring the ordinary and gradual (or continuous) variation that underpinned Darwin's theory of natural selection, added a mathematical component to theories of inheritance when he began to look for statistical laws of heredity. This work would lead to the development of Galton's ancestral and 'alternative' theories of inheritance which, in turn, provided the catalyst

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12 Francis Galton, 'Theory of Heredity', *Journal of the Anthropological Institute of Great Britain and Ireland*, 5 (1876), 330.

13 Francis Galton, 'Sexual Generation and Cross Fertilisation' (unpublished paper, 1890), FG:UCL, p.2.

14 R.G. Swinburne, 'Galton's Law—Formulation and Development', *Annals of Science*, 21 (1965), 15. For a further account of theories of inheritance in the 19th century see Robert Olby, *Origins of Mendelism*, 2nd edn (Chicago University Press: Chicago, 1985).

15 *Ibid.* Weismann's theory appeared in various articles in the 1880s and in his *Germ Plasm* in 1893. Weismann taught that 'the germ plasm of an organism, was developed entirely from that of its parents, and that there was no influence by bodily parts on the germ-plasm.'

16 *Ibid.*

to his work on correlation and especially for regression: two of his most important innovative contributions to statistics.

When Galton wanted to measure the relationship between stature of father with stature of son, he used graphical methods in an attempt to obtain a measure of *simple correlation* (termed by Pearson when measuring the relationship between *two* continuous variables only—for Galton this tended to be one characteristic usually in two generations). However, when Galton became interested in measuring the relationships between characters in *more than two* generations, he needed a different set of statistical procedures. In 1896 and 1898, Karl Pearson offered a partial resolution to Galton's concerns when he devised a set of mathematical-statistical procedures, known as *multiple correlation* (followed subsequently by *multiple regression*). Whilst simple correlation measures the linear relationship between two continuous variables (where one variable is termed the 'independent variable' and the other is the 'dependent variable'), multiple correlation is a measure of the relationship of three or more variables (i.e., between one dependent variable and two or more independent variables combined with optimal weights).

The historiography of Galton's work on inheritance has tended to focus primarily on simple correlation and simple regression; thus, less consideration has been given to Galton's contributions of multiple correlation and multiple regression in the corpus of Pearsonian statistics.[17] In this chapter, the totality of Galton's use of correlation and regression for studies of inheritance and its impact on Pearsonian statistics will be examined.

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17 See, for example, William Ernest Castle, 'The Laws of Heredity of Galton and Mendel, and some laws governing race improvement by Selection', *Proceedings of the American Academy of Arts and Sciences*, 39, No. 8 (November 1903), 221; Ruth Swartz Cowan, 'Nature and Nurture: The Interplay of Biology and the Politics in the work of Francis Galton', *Studies in the History of Biology*, 1 (1977), 197; Peter Froggatt and N.C. Nevin, 'Galton's "Law of Ancestral Heredity": Its influence on the Early Development of Human Genetics', *History of Science*, 10 (1971), *passim*; Donald Mackenzie, *Statistics in Britain, 1865-1930* (Edinburgh University Press: 1981), pp.67-8; Reginald C. Punnett, 'Early days of Genetics', *Heredity,* 4 (April 1950), 2 and 9; R.G. Swinburn, 'Galton's Law', 21-2, 30.

Whilst R.G. Swinburne has, for example, acknowledged that the 'fundamental triumph of Galton's correlation helped scientists to look for statistical relationships rather than explaining phenomena with rigid causal laws by causation', he thought that 'Pearson did a lot of detailed statistical work on a multiplicity of data [in his paper of 1896], and introduced enormous mathematical complications into Galton's primitive theory.'[18] He then concluded that

> all that Pearson and Weldon and those who worked with them at University College London and contributed to *Biometrika* did was to discover more accurate correlations and more complicated laws of the same pattern.[19]

Peter Froggatt and N.C. Nevin expressed a very similar view to Swinburne some six years later, as they thought that it was 'Pearson [who] introduced enormous mathematical complexity into Galton's primitive hypothesis'.[20] However, it will be argued that this 'mathematical complexity' enabled Pearson to achieve a mathematical resolution of multiple correlation and of multiple regression in 1896, adumbrated in Galton's Law of Ancestral Heredity in 1885. Moreover, this led to one of the most important mathematical developments in statistical theory at the end of the nineteenth century: this was the introduction of determinantal matrix algebra.

The implications of this development were twofold: firstly, it will be argued that Pearson's initial use of matrix algebra represented his attempt to provide a synthetic mathematical model of evolutionary biology for problems of Darwin's Theory of Natural Selection in tandem with Galton's Law of Ancestral Inheritance; and secondly, the adoption of this form of higher algebra signified one of the most striking departures from the earlier usage of statistics as it not only helped to bring about an ever increasing specialisation of 'mathematical statistics', but it became pivotal in the professionalisation of this emerging discipline. This key development, which has not yet been addressed in the historiography of Pearsonian statistics, will also be examined in this chapter.

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18 *Ibid.*, 20-1.
19 *Ibid.*, 30.
20 Froggatt and Nevin, 'Galton's "Law of Ancestral Heredity"', 8.

GALTON AND THE STATISTICS OF SWEET PEA SEEDS

In 1875, Darwin asked Galton for assistance with the statistical calculations of cross- and self-fertilised peas. By then, Darwin had decided that 'reversion is not a rare event, depending on some unusual or favourable combination of circumstances, but occurs so regularly with crossed animals and plants and so frequently with uncrossed breeds, that it is evidently an essential part of the principle of inheritance'.[21] When Galton began his work on sweet pea seeds later that year, Darwin and the botanist Sir Joseph Hooker, both suggested that Galton should experiment with sweet pea seeds because this plant rarely cross-fertilised, all of the seeds in the pods were of the same size and they were considered to be very hardy and prolific. Galton then wanted to know how was it

> possible for a population to remain alike in its features, as a whole, during many successive generations, if the *average* product of each couple resemble their parents? Their children are not alike, but vary: therefore some would be taller, some shorter than their average height; so among the issue of a gigantic couple there would be usually some children more gigantic still.[22]

He then measured the diameter and weight of thousands of mother and daughter sweet pea seeds and he made five groupings of each size of seed. He constructed graphically various procedures to determine relationships between the mother and daughter sweet peas. When Pearson re-examined this work more than forty years later, he considered this to be the first 'regression line ever to have been computed' (see Figure 5.1).[23]

Four years after Galton's first paper on inheritance and a year after Galton undertook his work on sweet pea seeds, he published his first set of theories of heredity. At this time, he also introduced the word 'stirp' into his theory of heredity which he had

> derived from the Latin *stirpes*, a root, to express the sum-total of the germs, gemmules, or whatever they may be called, which are found, according to every theory of organic units, in the newly fertilised ovum—that is, in the earliest pre-

21 Darwin, *Variation of Plants and Animals*, p.368.
22 Francis Galton, *Memories of my Life* (Methuen: London, 1908), p.300.
23 Karl Pearson, 'Note on the History of Correlation. Being a paper read to the Society of Biometricians and Mathematical Statisticians, 14 June 1920', *Biometrika*, 13 (1920), 35.

Figure 5.1
GALTON'S FIRST REGRESSION LINE (1875)

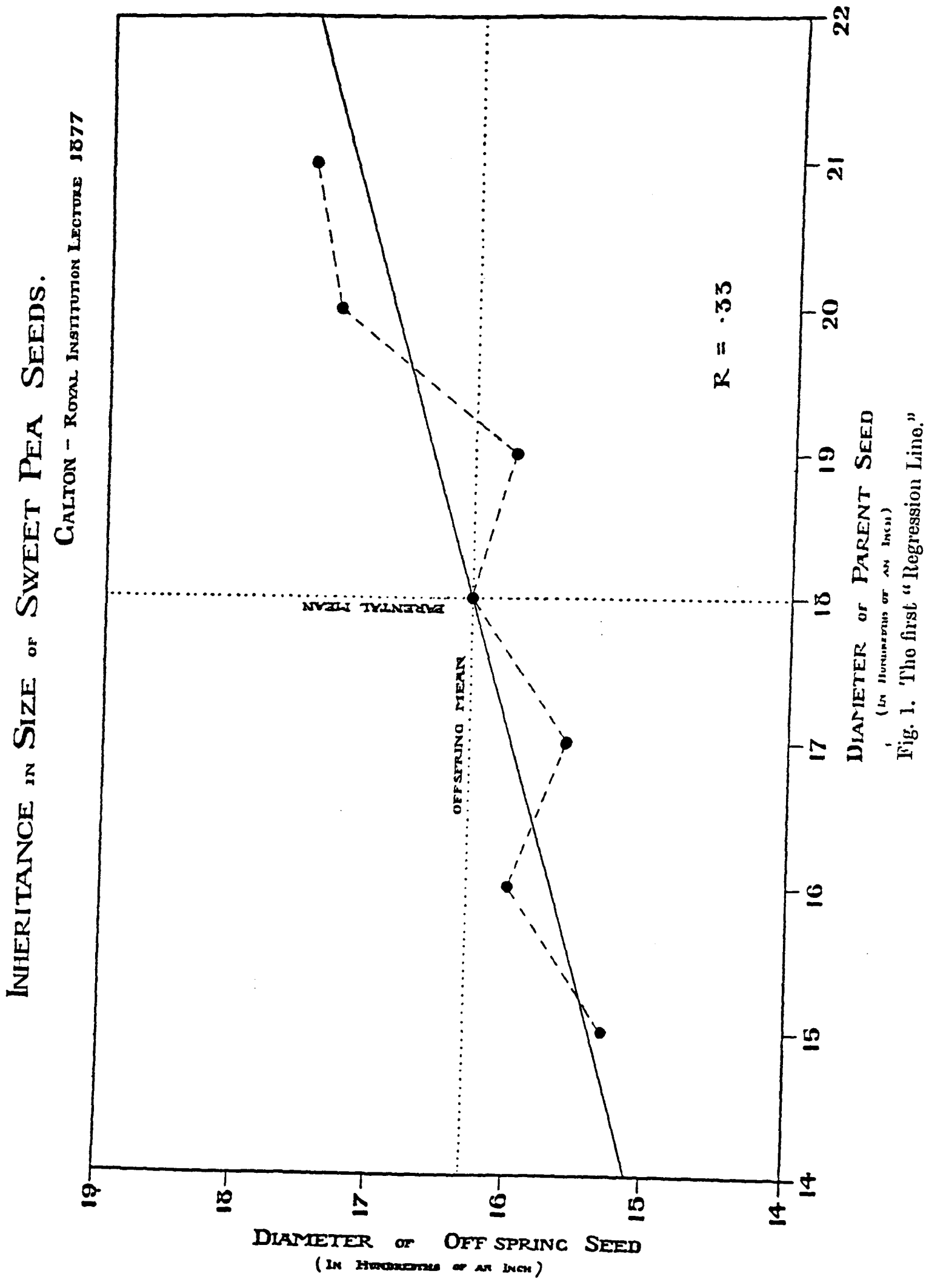


Fig. 1. The first "Regression Line."

> embryonic stage—from which it receives nothing further from its parents, not, even from its mother, than mere nutrients.[24]

The *stirp* was conceived as 'an organised aggregate of a host of germs' and sexual elements were 'generated by a residuum of the stirp.'[25] Galton used the results from the weight and diameter of his sweet pea seeds for his first lecture on reversion at the Royal Institute in 1877.[26] Reversion was expressed 'by a traditional coefficient of the deviation which [was written as] *r*.'[27] The population of each unit was considered a 'reverted parentage' which followed the 'law of deviation' (i.e., the normal distribution).[28] After studying several generations of sweet pea seeds, Galton had reached his idea of regression to the mean. He observed that as the size of the mother pea seed increases so does the size of the daughter pea seed, but whether in excess or defect of the mean, 'the daughter pea does not reach the deviation of the mother's diameter from the mean value: the offspring is less a giant or dwarf than is the mother pea'.[29]

Galton would use simple regression to help him explain what he perceived to be discontinuous variation (i.e., the skipping of generations or reverting to the ancestral type).[30] He defined regression as a phenomenon where the

> child inherits partly from his parents, partly from his ancestry. Speaking generally the further his genealogy goes back, the more numerous and varied will his ancestry become, until they cease to differ from any equally numerous sample taken at haphazard from the race at large. Their mean stature will then be the same as that of the race; in other words, it will be mediocre. Or to put the same fact [statistically], the most probable value of the midancestral deviates in any remote generation is zero.[31]

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24 Galton, 'A Theory of Heredity', 330. Derek Forest has traced Galton's interest in heredity to 1864 when Galton began writing his papers on 'Heredity Talent' which appeared in *Macmillan Magazine* in the following year. D.W. Forest, *Francis Galton: The Life and Work of a Victorian Genius* (Taplinger Publishing: New York, 1974) p.85. See also Francis Galton, 'Heredity Talent and Character' *Macmillan Magazine*, 12 (1865), 157-66.

25 Francis Galton, 'Typical Laws of Heredity Part I', *Nature* (5 April 1877), 492-5.

26 *Ibid.*

27 Galton, 'Typical Laws of Heredity, Part III'., *Nature* (19 April 1877), 532.

28 *Ibid.*, 532-3.

29 See Pearson, *Life of Galton*, IIIA, p.3.

30 Mendel's recessive factors was to later provide the key explanation to this reversion.

31 Francis Galton, 'Regression towards Mediocrity in Herditary Stature', *Journal of the Anthropological Instititute of Great Britain and Ireland*, XV (1885), 252-3.

If the complete ancestry was known, the whole ancestry could be replaced by a single midancestor. The midparent thus represented the parentage of the offspring. Pearson termed Galton's midancestor, a 'generant' and understood Galton's idea of regression to indicate the result of the influence of parental heredity pulling the offspring so to speak towards the parental value and to the mediocrity of the more distant ancestry pulling towards its own character.[32] Hence, the midparent was to be looked upon as only 'an approximation of a rough kind'.[33]

CO-RELATION AND REGRESSION: In Galton's first statistical paper, in 1874, he introduced the idea of ranking data which was to form the basis of his procedure for determining a measure of correlation.[34] When Galton wanted to measure the stature of fathers and sons some twelve years later, he would use some of the ranking procedures he introduced in 1874. He chose stature as his subject for inquiry for its 'ease and frequency of its measurement, its constancy during adult life [and] its inconsiderable influence on the death rate'.[35] He also thought that 'the blending of paternal and maternal qualities in the stature of the offspring [was the] one great advantage in selecting stature as a subject for [his] inquiry'.[36] Galton depicted his results in a bivariate distribution (see Figure 5.2) by plotting the 'frequency of cases in which the various deviations in stature of the adult person, measured along the top were associated with deviations of stature in his mid-parent, measured along the side'.[37]

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32 Pearson, *Life of Galton*, IIIA, p.20.

33 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896), 288.

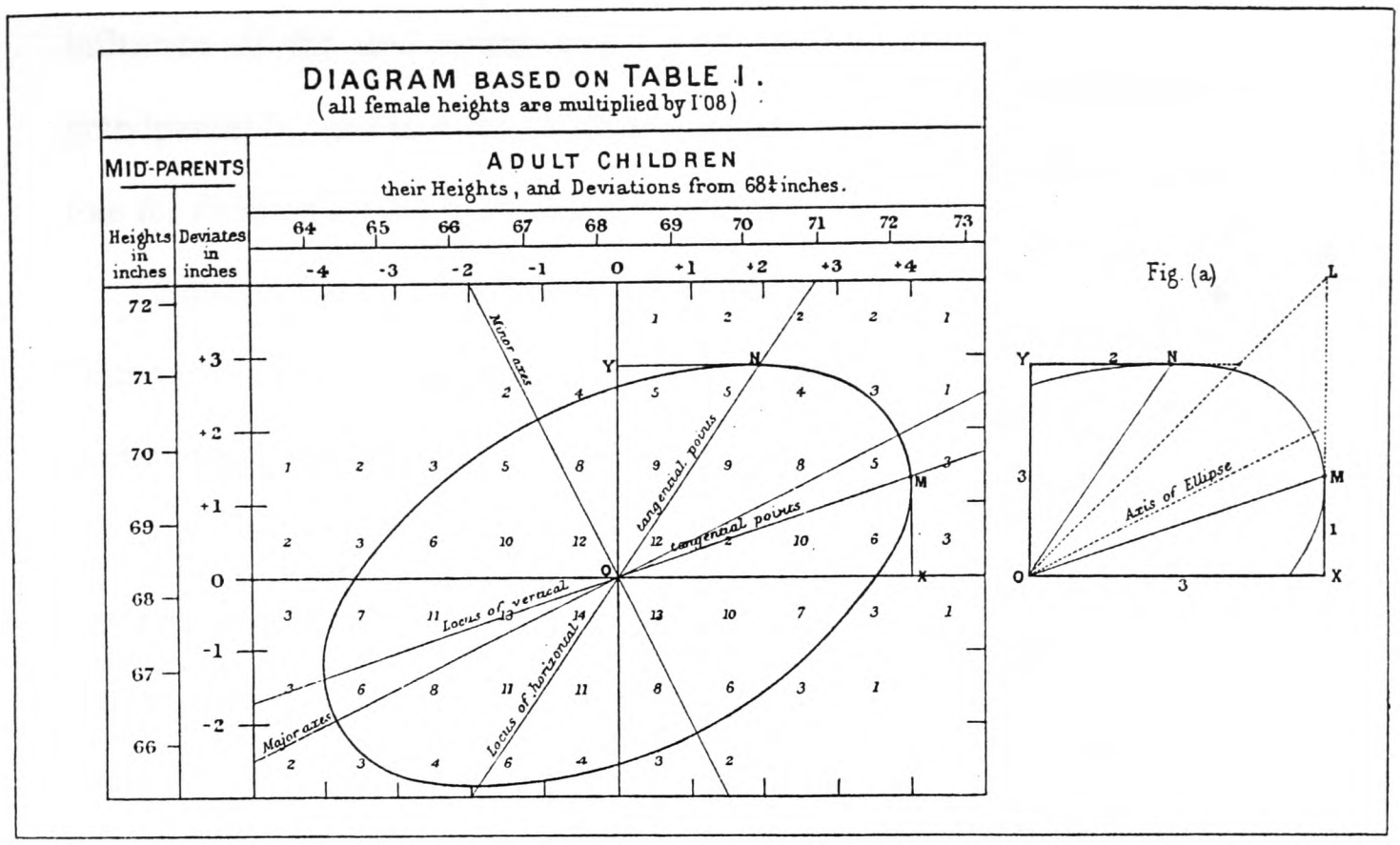
34 In this paper, which he read at the Royal Institute on 27 February 1874, he rank ordered the stature of boys at Marlborough College into 'quartiles': the first quartile, q_1, was at the 25th percentile, the second, q_2, was the *median* which was at the 50th percentile and the third, q_3, was at the 75th percentile. From these quartiles, he devised what he termed the semi-interquartile range; this was found by taking the difference between q_1 and q_3. Francis Galton, 'Proposed Statistical Scale', *Nature* (5 March 1874), 343.

35 Francis Galton, 'Family Likeness in Stature with Appendix by J.D. Hamilton Dickson', *Proceedings of the Royal Society* (21 January 1886), 43.

36 *Ibid.*, 53.

37 Galton, *Memories*, p.302.

Figure 5.2
Galton's Elliptic Contour Drawn from his Observations (1886)



After Galton studied this table and realised that his lines formed concentric ellipses, he wanted to find a single formula to express the results of this table from the ellipses. As he could not remember all the formulae of Conic Sections, he went up to Cambridge to meet with J.D. Hamilton Dickson, mathematician and tutor of St. Peter's College.[38] Hamilton-Dickson then examined mathematically the system of concentric ellipses that would correspond to the ellipses deduced by Galton in his 1885 paper of 'Regression towards Mediocrity in Heredity Sature'. His calculations confirmed Galton's theory that the value of the midparental regression was very nearly one-third.

Having determined that the mid-ancestral contribution was one-third, Galton then determined the separate contribution of each ancestor to the heritage of the offspring. The deviations of a midparent was 'a', the implied contribution of the deviation of the mid-grandparent was $^1/_3$a, and the midancestor of the next generation was $^1/_9$a and so on. Thus, 'the sum of the deviates of all mid-generations that contribute to the heritage of the offspring [was] $(1 + {}^1/_3 + {}^1/_9 + a) = a^3/_2$'.[39] From this,

38 *Ibid.*, pp.302-3.
39 Galton, 'Regression towards Mediocrity in Hereditary Stature', 260.

Galton would later derive the second supposition of his geometric progression that the influence of the mid-parent was $^1/_2$, of the mid-grandparent $^1/_4$, of the mid-great grandparent $^1/_{16}$ and of a single great grandparent $^1/_{64}$.[40] This work was to play a vital role for Pearson's work on multiple correlation and multiple regression.

Galton's theory of ancestral inheritance incorporated blending and non-blending inheritance. For characters that were thought to 'blend' (such as height or other continuous variables), the inherited characteristics were represented by each term in his series $^1/_2 + (^1/_2)^2 + (^1/_2)^3 + (^1/_2)^4$ and was furnished by *all* the ancestry of a generation; hence the mid-parent. For characters that didn't blend (such as eye-colour), Galton used the same series, but used information from *one parent only*: Galton termed this 'alternative inheritance' which Pearson and Weldon termed 'exclusive' inheritance (as one parent was excluded).

Two years after he published his results with Hamilton-Dickson, Galton determined that the 'index of co-relation is what I call "regression"'.[41] He wanted to show that the stature of kins-men were considered to be co-related variables; thus two variables were, said to be co-related when the 'variation of one is accompanied on the average by more or less variation of the other'.[42] The stature of the father was said to be correlated to that of the adult son, and the stature of the adult son to that of the father.[43] Galton thought that there would be an equal increase of variation in stature for every pair of father and son. Hence, his 'co-relation' was based on the assumption that the

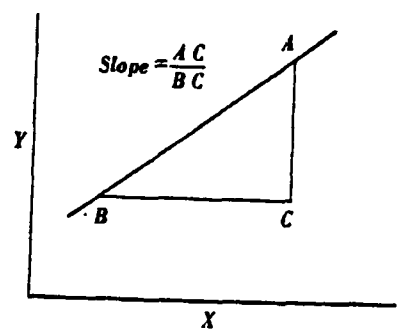


Figure 5.3: The Slope of a Line

40 *Ibid.*
41 Francis Galton, 'Co-relation and their Measurement Chiefly from Anthropometric Data', *Proceedings of the Royal Society*, 45 (20 December 1888), 143.
42 *Ibid.*, 135.
43 *Ibid.*, 143.

variability (i.e. the standard deviations) for the offspring and the parent were identical. His procedure involved measuring the slope of the regression line (see Figure 5.3) which was a measure of the regression coefficient instead.

The word 'correlation' had already been in use for some time. In 1846, the physicist, William Robert Grove (1811-1896) published his *Correlation of Physical Forces*.[44] Grove viewed correlation as a 'mutual convertibility' of forces that would enable him to establish (a non-mathematical) correlation between vital and physical forces. The physiologist, William Benjamin Carpenter (1813-1885) used Grove's theory of correlation of forces four years later to determine relationships between electricity and magnetism.[45] As mentioned in Chapter III, Darwin had discussed correlation in *The Variation of Plants and Animals Under Domestication* in 1875, and considered 'correlation to be an important subject'.[46] Though Galton later uses the spelling 'correlation', regression seems to have played a much larger role in his work. Steve Stigler has suggested that Galton's spelling of co-relation may have been 'a conscious attempt to distinguish [regression] from correlation'.[47]

Galton supposed that his populations were stable; he further considered that the medians of successive generations were the same. Since Galton thought that the two distributions should have been normally distributed, the median (or the mean) should have been the same from generation to generation.[48] Pearson, however, wondered how was it

> possible to pick out any particular stage of a general population as the 'focus of regression' and assert that regression of the offspring of parents now selected take place toward that stage of evolution?[49]

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44 William Robert Grove, *Correlation of Physical Forces* (London, 1846).

45 W.B. Carpenter, 'On the Mutual Relations of the Vital and Physical Forces', *Philosophical Transactions of the Royal Society* (1850), 744 as cited in Vance M.D. Hall, 'The Contribution of the Physiologist William Benjamin Carpenter (1813-1885), to the Development of the Principles of the correlation of forces and the conservation of Energy', *Medical History*, 23 (1979).

46 Darwin, *Variation of Plants and Animals*, p.347; see also pp.313-317.

47 Stigler, *History of Statistics*, p.268.

48 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896).

49 *Ibid.*, 307.

When Pearson examined Galton's work in 1896, he showed that if it is assumed 'that the standard deviations of the two marginal columns [are] the same, the regression coefficient is the coefficient of correlation'.[50] That is, if the standard deviation of the character being measured in the first generation had the same numerical value as the standard deviation in the second generation, then the value obtained from the correlation coefficient would be identical to the value from the regression coefficient. Pearson then showed that the amount of variation in one group is not necessarily the same amount of variation in another group (i.e, the standard deviation of Y does not always equal the standard deviation of X). Hence, Galton's notion of equal variability (when the values of the two standard deviations are identical) was indeed a rare occurrence.

Although a measure of correlation would be the same for pairs of organs between parent and child and between child and parent, a measure of regression would not produce the same two values.[51] Pearson recognised that it was possible to have a regression of parent on offspring as well as the regression of offspring on parent (as can be seen in Table 5.1). It also followed that the regression of husband on wife was not the same as the regression of wife on husband.[52]

Table 5.1
Pearson's Regression between Different Sets of Relatives

Class.	Coefficient of Regression
Assortative Mating:-	
Husbands on wives	·1062
Wives on husbands	·0816
Inheritance:-	
Fathers on sons	·3517
Sons on fathers	·4456
Fathers on daughters	·4192
Daughters on fathers	·3096
Mothers on sons	2692
Sons on mothers	·3384
Mothers on Daughters	·2753
Daughters on mothers	·2932

50 *Ibid.*
51 *Ibid.*, 260.
52 *Ibid.*

From Galton's idea of regression, Pearson would develop a statistical system that would be used for the linear prediction between two continuous variables. Though Pearson had shown that Galton's correlation formula was a measure of regression instead, he retained Galton's *r* to symbolise the correlation coefficient. The regression coefficient was instead symbolised by the letter b which designated a constant in the equation for a straight line $Y = a + bX$ (where a = the intercept constant). It then followed that $Y' = a + bX$ was the equation for the regression (or predicted) line. Pearson (who used the symbols σ for S and σ^2 for S^2) determined that the regression coefficient

$$b = \frac{\Sigma xy}{S^2_x} = \frac{\text{covariance}}{\text{variance of x}}$$

where X is the independent variable. The constant $a = \overline{Y} - b\overline{X}$.

A measure of the product-moment correlation coefficient was

$$r = \frac{\Sigma xy}{(S_X)(S_Y)} = \frac{\text{covariance}}{(\text{standard deviation of x})(\text{standard deviation of y})}$$

In these formulas, it may be seen that the covariance (i.e., the sum of the cross-products of X and Y) is used for the numerators; hence, the value will be the same for both. If, for example, the standard deviation of X = 2.50 and if the standard deviation of Y also = 2.50, then $(S_X)(S_y)$ or 2.50 x 2.50 = 6.25. It may then be seen that the variance, S^2_X or 2.50^2 is also 6.25 (as the value obtained from S x S is identical mathematically to S^2). In this situation, the value for the correlation coefficient will be identical to the regression coefficient: thus, $b = r\,(S_X)(S_y)$. However, if the standard deviation of X = 4.20 instead and if the standard deviation of Y = 2.50 (which Galton did not consider to be tenable), then the denominator (4.20 x 2.50) is 10.50 and the variance of X, $4.20^2 = 17.64$. Here, the correlation coefficent would *not* be identical to the regression coefficient.

George Udny Yule (1871-1951), who was one of Pearson's first students and his demonstrator at University College from 1893 to 1896, pointed out to Pearson that if the coefficient of regression was constant for all types of frequency distributions (symmetrical or asymmetrical), it followed that 'whatever be the law of frequency, the

coefficient of regression must = $r\sigma_1/\sigma_2$ [the correlation coefficient] when $r = (\Sigma xy)/(r\sigma_1 r\sigma_2)$.[53] Pearson did not think that this had resolved the problem of skew correlation because he thought that linear regression could be maintained in non-normal correlation.[54]

Galton's primary concern seems to have been to find a mathematical expression for regression rather than correlation *per se*. Given the importance he attached to his regression to the mean, which gave him an hereditarian framework for his law of ancestral heredity, this focus is not surprising. Galton's emphasis has led Steve Stigler to remark that

> while there is justice in associating correlation with Francis Galton, the concept in its name and interpretation as a single number measuring a bivariate relationship played only a minor role in Francis Galton's work.[55]

Galton's focus was to give Pearson plenty of scope to develop his work on correlation.

PEARSON'S PRODUCT-MOMENT CORRELATION COEFFICIENT

Once Pearson had developed a system of curve fitting that could cope with asymmetrical distributions, he could then address mathematical problems of correlation. Though Galton had worked out ideas of correlation for the normal distribution, the extension of correlation for asymmetrical distributions needed modification. By 1892, Edgeworth abolished Galton's median and quartile for computing the correlation coefficient as too inexact (thereby improving the accuracy of computation).[56]

Although Pearson had begun to lecture on variation, probability and curve-fitting at Gresham College in November of 1892, he did not begin to lecture on

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53 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896), 128, 147. From 1893-1896, Yule was responsible for helping Pearson with the teaching of Engineering students as well as with the 'teaching and research in the theory of statistics and applications of the same to research in social statistics'. He was made 'Assistant Professor' in the Spring of 1896, for two years, by the Council of the College. In Yule, 'Application of G. Udny Yule for Post of Principal of West Ham Technical College', January 1898, KP:UCL.

54 See Stigler, *History of Statistics* pp.344-5.

55 *Ibid.*, p.268.

56 F.Y. Edgeworth, 'Correlated Averages', *Philosophical Magazine*, 5th Series, 34 (1892), 429-38.

correlation until 1895 in one of his lectures on statistics at University College.[57] He published the Abstract of his Mathematical Contributions to the Theory of Evolution on 'Regression, Heredity and Panmixia' in 1895 and the full paper in the following year. In this paper he dealt with some of Galton's work on correlation and inheritance, and examined more carefully Weldon's notion of using correlation for the biometrical species criterion (i.e., the constancy of correlation for local races.)

In the summer of 1895, Pearson wrote to Galton to let him know that he had

> been able to show that your coefficient of correlation has a very important meaning, even if the distributions be not of the normal type. If it be calculated from the formula $\frac{\text{Sum of } x \times y}{n \times \sigma_1 \times \sigma_2}$ (where $x \times y$ is the product of a pair of correlated quantities, n is the total number of pairs, and σ_1 & σ_2 are the standard deviations (errors of mean square) of the quantities x and y respectively), it is a significant quantity whatever the law of frequency.[58]

Pearson first used his product-moment correlation coefficient for the calculation of the 'sexual selection with regard to height' from the data that Galton had sent him.[59] He also thought that he could now show Galton that not all biological data was normally distributed. He thus explained that the

> fact that r as measured above is independent of the law of frequency being normal, enables me I think to surmount your difficulty of the want of close conformity to the normal law....I hope I do not weary you with these details.[60]

Though one of Galton's most important statistical innovations was, indeed, the discovery that 'a normal mixture of normal distributions was itself normal', Pearson's method did not seem to change Galton's view that biological data could take on other shaped distributions apart from the normal distribution.[61] Thus, he would never use Pearson's product-moment correlation coefficient to find his 'index of correlation'.[62]

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57 G. Udny Yule, 'Notes of Karl Pearson's Lectures: 1894-1896 ("Correlation")', Vol. III, 1895. Yule's lecture notes have been bound in five volumes; he presented this set to the Department of Applied Statistics, University College London shortly after Pearson's death in 1936, KP:UCL.

58 Karl Pearson, Letter to Francis Galton, 18 June 1895, FG:UCL, pp.4-5.

59 *Ibid.*, p.6.

60 *Ibid.*, pp.7-8.

61 See Stigler, *History of Statistics*, p.281, who considered Galton's conceptual use of the normal distribution as 'perhaps, the single major breakthrough in statistics in the last half of the nineteenth century'.

62 Karl Pearson and David Heron, 'On Theories of Association', *Biometrika*, 9 (1913), 164.

Pearson gave Weldon a copy of the paper that he sent to the Royal Society on 28 September 1896. After Weldon read this paper, he wrote to Pearson on 21 November to ask if his 'formula [gave] the sign of r'.[63] With this suggestion, Weldon had introduced the idea of a 'negative' or 'inverse' correlation. Hence, the values of the correlation coefficient could range from -1.00 to +1.00 rather than the range 0.00 to +1.00 as first suggested by Galton. When Pearson revised his copy of 28 November 1896, he included a section on 'positive' (or direct) correlations and 'negative' (or inverse) correlations.[64] In this seminal paper of 1896, he determined the mathematical properties for simple correlation, provided new formulae for regression and correlation, devised the standard deviation of the correlation coefficient and also for regression (or what is known now as the standard error of estimate) in addition to the coefficient of variation. The terms 'independent variable' and 'dependent variable' were used for the first time: a distinction that is essential for regression (though such terms were not used by Galton).

The definitions that Pearson used in this paper 'agree[d] in part with those already adopted by Galton and Weldon'.[65] . The quantities considered capable of quantitative measurement included variation, correlation, natural, sexual and reproductive selection, heredity, regression, and panmixia.[66] It was not necessary to give correlation or the standard deviation 'meanings relating directly to the intensity of the disease; instead they referred to the size of organs or intensity of [the] characteristic on which the liability of disease or its intensity directly or indirectly depends.'[67] He emphasised that the formulae he had discussed made

> *not* the least pretense 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.[68]

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63 Weldon, Letter to Karl Pearson, 21 November 1896, KP:UCL, p.1.
64 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896). For details of Pearson's examples, see 296.
65 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' Abstract (1895), 70.
66 *Ibid.* 69.
67 *Ibid.*, Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896), 292.
68 *Ibid.*

Pearson proceeded to re-examine two sets of Weldon's data. The first set came from his 1892 paper when he found the correlation between total length of carapace with length of post-spinuous portion for the common shrimp at Plymouth; the second, came from his 1893 paper on the correlation of the frontal breadth and right antereo-lateral margin from the shore crabs at Plymouth. After Pearson found the probable error of the correlation coefficient and calculated the product-moment correlation coefficient, he then concluded that

> the evidence of these observations is against the constancy of Galton's function for local races of the same species... Professor Weldon's papers, illustrate not the constancy of correlation in species, but the equally interesting point of the extent and manner of its variation in local races.[69]

He then wanted to show that the 'whole theory of [simple] correlation as applied to [Galton's] uniparental inheritance may be at once applied to correlation between brothers, sisters and brothers and sisters.' Thus a simple correlation could be found between one pair of siblings (i.e. two sisters, two brothers or one sister and one brother); in his sample of data, Pearson found moderate correlations with values for r ranging from .38 to .44. (Pearson coined the word 'siblings' to 'cover a group of brothers and sisters regardless of sex and equivalent to the sense lost in modern English of *"Geschwister"*'.)[70] A fuller development of Galton's ideas of regression and heredity was offered 'particularly their application to bi-parental inheritance'. It had not appeared that

> mathematicians [had] hitherto developed his treatment or that biologists and medical men [had] yet fully appreciated that he [had] really shown how many of the problems which perplex them may receive a partial answer.[71]

Pearson wanted to 'develop the methods and generalise the conclusions of ... Galton's work on *Natural Inheritance'*. He endeavoured to show how a purely statistical (as distinguished from a mechanical or physiological) theory of heredity may be used to express Galton's Law of Ancestral Heredity.[72]

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69 *Ibid.*, 255-6.

70 Pearson, *Life of Galton*, IIIA, p.332.

71 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896), 240-1.

72 Karl Pearson, 'Note on Regression and Inheritance in the Case of Two Parents', *Proceedings of the Royal Society* 58 (1895) 240-1.

When Pearson defined variation and correlation in this paper, he placed particular emphasis on the organ of animals in populations where selection may have occured. Variation was defined as

> a curve constructed, of which the ordinate 'Y' is such that Yx measures the frequency with which an organ lying in size between x and x + x occurs in a considerable population (500 to 1000 or more) as the constants which, for any particular organ for any particular animal determine the form of this curve, are termed the *constants of variation*, or more briefly , the variation of the given organ.[73]

Correlation was defined initially within the context of evolutionary biology. A measure of correlation was used to describe relationships which might be considered 'functional' whereby

> two organs in the same individual, or in a connected pair of individuals, are said to be correlated, when a series of the first organ of a definite size being selected, the mean of the sizes of the corresponding second organ is found to be a *function* of the size of the selected organ. If the mean is independent of this size, the organs are said to be non-correlated. Correlation is defined mathematically by any constant, or series of constants, which determine the above function.[74]

Pearson's formulation of the product-moment correlation coefficient may be summarised as follows:

1. Let $\eta_1, \eta_2, \eta_3 \ldots \eta_n$ be the deviations from their respective means of a complex or organs or measurable characteristics (the organs may be in the same or different individuals).

2. Assume that the size of the organs are determined by great and variety of *independent contributory causes*

3. Let these causes be *m* in number (*m* being generally greater than *n*)

4. Let the deviations from the mean be $\varepsilon_1, \varepsilon_2, \varepsilon_3 \ldots \varepsilon_m$ then $\eta_1, \eta_2, \eta_3 \ldots \eta_n$ will be the functions of $\varepsilon_1, \varepsilon_2, \varepsilon_3 \ldots \varepsilon_m$.

5. The median was substituted with the arithmetic mean (as Weldon had suggested in 1892) and Galton's probable error with Pearson's standard deviation.

6. He then proposed $r = S(xy)/(n/\sigma_1\sigma_2)$ as the best formula for the product-moment correlation which was 'reduced to parallel axes (x', y') through the centroid of the system'.[75] The covariance, $S(xy)$ corresponded to the 'product

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73 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896), 256.
74 *Ibid.*, 258.
75 *Ibid.*, 272.

moment of dynamics' and $S(x^2)$, to 'the moment of inertia.'[76] Using contemporary notation, the product-moment correlation coefficient would be expressed as:

$r = \Sigma(xy) / SD_x SD_y$ where

r = the product - moment correlation coefficien
S or Σ = summation
σ or SD = standard deviation
xy = covariance

The standard deviation of the correlation coefficient was $\frac{1-r^2}{\sqrt{n(1+r)^2}}$

and the standard deviation of the regression (i.e., the standard error of estimate) was $\sigma\sqrt{(1-r)^2}$.

MULTIPLE CORRELATION: For problems of heredity, when an 'indefinite number of quite inappreciable and unascertainable independent contributory causes should be considered', it was essential to consider the relationships of more than two variables. When Pearson first considered a special case of three correlated variables, he devised an analytical form for two, three and four correlated variables. He also determined the standard deviation of a group of *x*-organs selected with regard to values of *y* and *z* then

$$\Sigma_1 = \frac{\sigma_1 x}{\sqrt{\{X(1-r_1)^2\}}} = \sigma_1\sqrt{\frac{1-r_1^2-r_2^2-r_3^2+2r_1r_2r_3}{1-r_1^2}}$$

$\frac{r_3-r_1r_2}{1-r^2_1}$ = coefficient of double correlation

$\frac{r_3-r_1r_2}{1-r_1^2}\ \frac{\sigma_1}{\sigma_2}$ = coefficient of double regression[77]

Edgeworth had dealt with statistical correlations of three variables in 1892 which he expressed as 'Galton's functions' (as named by Weldon in 1889).[78] A year after Pearson's paper on correlation and regression was published, Yule introduced the 'coefficient of double correlation'.[79]

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76 *Ibid.*, 265.
77 *Ibid.*, 287.
78 Edgeworth, 'Correlated Averages', 194-204.
79 G.U. Yule, 'On the Theory of Correlation'. *Journal of the Royal Statistical Society*, 60 (1897), 812.

After Pearson had established correlations for three variables when looking at three selected organs for problems of natural selection, he then considered the application to Galton's theory of mid-parental inheritance where: r_1 = the coefficient of correlation for the same or different organs in two parents which was Pearson's statistical measure of assortative mating; r_2 = the coefficient of correlation for the same or different organs of offspring and female parent or Pearson's statistical measure of maternal inheritance whereas r_3 = the coefficient of correlation for the same or different organs of offspring and male parent or Pearson's statistical measure of fraternal inheritance.[80]

THE HERITAGE OF OFFSPRING

Galton's Law of Ancestral Heredity was published in 1897 in his memoir on 'The Average Contribution of each several Ancestor to the total Heritage of the Offspring'. This law of had been stated briefly and 'with hesitation' in *Natural Inheritance* in 1889.[81] In this paper, he developed his law more fully and then explained that

> two parents contribute between them on average ½ (or .5) of the total heritage of offspring; four grandparents contribute ¼ or $(.5)^2$ and so on generating the occupier of each ancestral place in the nth degree, whatever be the value of n, contributes $(.5)^2_n$ of the heritage: the sum of the ancestral contribution is expressed as $(0.5)+(0.5)^2+(0.5)^2$ &c. which being = to 1 accounts for the whole heritage.[82]

Galton decided that 'the influence, pure and simple, of the midparent may be taken as $^1/_2$ the midparent, $^1/_4$ the mid-grandparent $^1/_8$ and the mid-great-grandparent. He then found the separate contributions of each ancestor to the heritage of the child where $^1/_4$ is individual parent; $^1/_{16}$ is individual grandparent; and $^1/_{64}$ is individual great-grandparent: this was Galton's 'Law of Ancestal Heredity'. Moreover, it was an 'essential condition that their total amount should be = to 1 in order to account for the whole of the heritage.'[83]

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80 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896), 288

81 Francis Galton, *Natural Inheritance* (Macmillan: London, 1889).

82 Francis Galton, 'The Average Contribution of Each Several Ancestor to the Total Heritage of the Offspring', *Proceedings of the Royal Society*, 61 (3 June 1897) 402. Reprinted in *Nature*, 56 (8 July 1897)

83 *Ibid.*, 403.

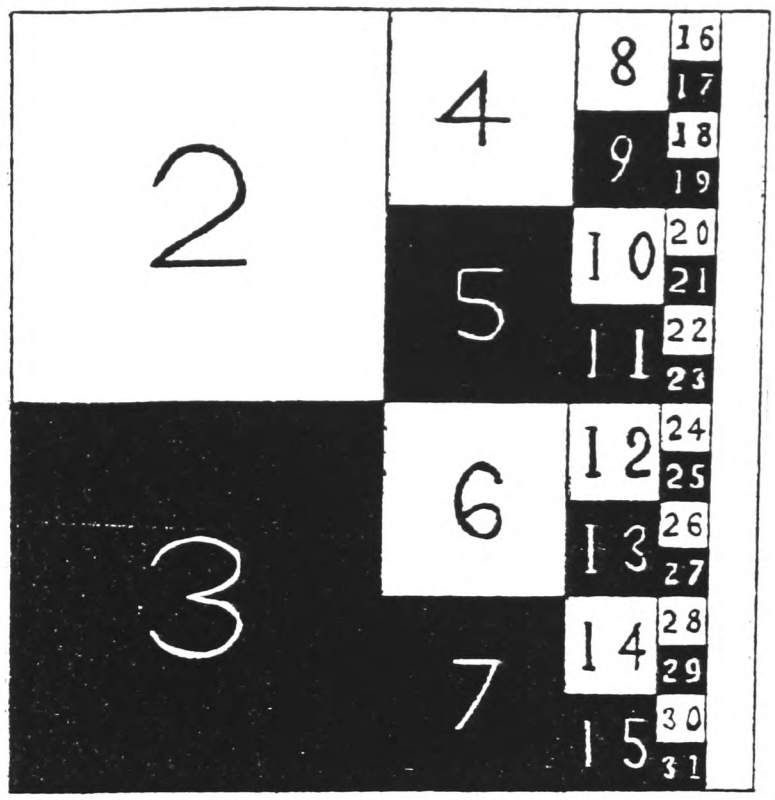


Figure 5.4
Mr. Meston's Diagram of Heredity (1897)

Six months later, he wrote a letter to *Nature* that his law of ancestral heredity had been 'strongly corroborated by an independent investigation' and had been 'illustrated by a useful diagram [see Figure 5.4] ... devised by Mr. A. J. Meston of Allen Farm, Pittsburg, Massachusetts'. Galton thought that Meston's diagram deserved a place in *Nature* as it 'conveyed in a very intelligble form the chief features of the law'. It showed that

> the total *heritage* of the offspring is derived as follows. The two parents between them contribute *on the average* one half of each inherited faculty, each of them contributing one quarter of it. The four grandparents contribute between them one quarter, or each of them one sixteenth; and so on, the sum of the series $1/2 = 1/4 + 1/8 + 1/16+$ &c. being equal to 1, as it should be. The prepotencies or sub-potencies of particular ancestors, in any given pedigree, are eliminated by a law that deals only with *average* contributions, and the varying prepotencies of sex in respect to different qualities, are also presumably eliminated.[84]

The area of the square diagram represents the total heritage of any particular form or faculty that was bequeathed to any particular individual. It is divided into subsidiary squares which referred to different ancestors. The size of these subsidiary squares shows the average proportion of the total heritage derived from the corresponding ancestors.[85]

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84 Francis Galton, 'A Diagram of Heredity', *Nature*, 57 (8 July 1897), 293.
85 *Ibid.*

When Galton wrote his series to determine the ancestral contributions, he used the following types of mathematical series which were expressed either as (a) exponential series, (b) percentages or (c) fractions:

$$^1/_2 + (^1/_2)^2 + (^1/_2)^3 + (^1/_2)^4 \qquad \text{(a)}$$

$$50\% + 25\% + 12.5\% + 6.25\% \qquad \text{(b)}$$

Thus $^1/_2 + ^1/_4 + ^1/_8 + ^1/_{16}$ (c)

Assuming that the contribution to parents was always 50% of that trait, then the values in all three series will be equivalent. Though Galton ostensibly achieved mathematical simplicity with this formula, as it seemed as though he could determine the ancestral contribution by using any one of the series, these three series are interchangable *only* when the contribution from parents = .50 (if the contribution is any other number, the results will not be different for each series). Galton, however, did not seem to realise that his series were not, in fact, interchangable. In one of his examples in *Natural Inheritance*, he found that the correlation of mid-parental contribution to stature was .65. To show the average ancestral contribution, Galton used the fractional series[86] where

$$^1/_2 + (^1/_2)^2 + (^1/_2)^3 + (^1/_2)^4.$$

Galton's calcuations were thus

$$.65 + (.65)^2 + (.65)^3 + (.65)^4$$

which equals

$$.65 + .42 + .27 + .18$$

When Pearson re-examined this data, he found, however, that 'Galton's law of ancestral inheritance contradtict[ed] his views on regression'.[87] Pearson had understood that the quantities $^1/_2 + ^1/_4 + ^1/_8 + ^1/_{16}$ were the amounts used to determine the

86 Francis Galton, *Natural Inheritance*, p.133.
87 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896), 396-7.

ancestral contribution (as Galton indicated in his 'Diagram of Heredity'). Pearson then showed that if the correlation of mid-parental contribution was .65, then Galton's law of ancestral heredity should have been:[88]

$$^1/_2 + ^1/_4 + ^1/_8 + ^1/_{16} \text{ or}$$

$$.65 + .32 + .16 + .08.$$

That is to say, the contribution for the individual grand-parental regression (the second number in the series) should be *half* the contribution of the parental regression (.32) rather than the *square* of (.42). Hence, the values Galton used for his ancestral correlations did not match the values he first used in his Law of Ancestral Inheritance.[89]

Galton attached considerably more importance to ancestry than did Pearson who thought that the 'more remote stages of ancestry contribute considerably less than the nearer one'.[90] Galton wanted instead to trace ancestry as far back as possible. A year after Pearson suggested this, he was discussing the need of 'reducing the *absolute* variability of a species', and maintained that it was 'idle to select beyond the grandparents, and hardly profitable to select beyond parents'.[91] Yet again, in 1898, he wrote that 'we do no need to know the characters of parents, grand-parents, great grand-parents to test Mr. Galton's law; any single relationship, near or far, direct or collateral... will bring its quota of evidence for or against the law'.[92] Pearson thus thought that most of the inherited material needed could be found in the parents without reverting to further ancestry.[93] Pearson's views, which remained constant

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88 *Ibid.*, 397.

89 Pearson, *Life of Galton*, IIIA, p.23.

90 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896), 400.

91 Karl Pearson, 'Variation in Man and Woman' in *Chances of Death and Other Studies in Evolution* (Edward Arnold: London, 1897), p.297.

92 Karl Pearson, 'Mathematical Contributions to the Theory of Evolution. On the Law of Ancestral Heredity (A New Year's Greeting to Francis Galton)'. *Proceedings of the Royal Society*, 62 (1898), 396 and 400. [Hereinafter referred to as Pearson, 'MCTE...Law of Ancestral Heredity'.] Collatoral Inheritance refers to indirect relationships such as grandparent and grandchild or aunt and niece.

93 It would be shown in the twentieth century, shortly after the rediscovery of Mendel's work, that by using a Mendelian theory of inheritance, most of the information could, indeed, be found in the parents. That is, if both alleles are 'dominant' or if at least one of the alleles is 'dominant', then

[Footnote continued on next page]

throughout the greater part of his life, may have helped to make him partially receptive to Mendelism.

Though Pearson's views on Galton's Law of Ancestral Inheritance differed from Galton, historiographical perspectives suggest that Pearson fully endorsed Galton's view. When the American biologist, William Ernest Castle (1867-1962) discussed the laws of inheritance by Galton and Mendel in 1903, he wrote that 'Pearson assigns a much greater influence to the more remote ancestors than does Galton'.[94] Castle came to this conclusion when he examined the differences in values that Pearson and Galton used for the Law of Ancestral Heredity. In one of Galton's series, he suggested the ancestral contribution should be $^1/_3$, $^1/_9$, $^1/_{27}$, $^1/_{81}$; whereas Pearson suggested the contribution should be $^1/_2$, $^1/_3$, $^2/_9$, $^4/_{27}$. As Pearson assigned a slightly greater numerical value to his series, Castle thought this meant that Pearson placed greater weight on ancestry than did Galton.[95]

Castle took the Galton set from Galton's work on Bassett Hounds for contributions from both parents and he used one of Pearson's series that Pearson had worked out for Galton for contributions from one parent only. Pearson responded to Castle's criticisms in a short note in *Biometrika*: he argued that it was not possible to

> deduce what number of the offspring any generation will be like this or that ancestor. It is not a law of types, but of the distribution of deviations from type, and this is a very different thing indeed. The Law of Ancestral Heredity as I have repeatedly stated is this: the means of determining the probable deviation of any individual from the *type of his own generation*, when we know the deviations of some or all of his ancestry from the types of their respective generations... [Moreover,] my types may alter and do alter in any way from one generation to a second... Galton supposed that we may apply his series not only to the deviations of an individual from type, but to the 'whole heritage'.[96]

More recently, Peter Froggatt and N.C. Nevin examined Pearson's 1898 paper 'On the Law of Ancestral Heredity (A New Year's Greeting to Francis Galton)'. They argued that

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[Footnote continued from previous page]

knowledge of the parents genetic structure is sufficient; however, if *both* alleles are 'recessive', then knowledge of more distant ancestry could be of help.

94 Castle, 'Laws of Heredity of Galton and Mendel', 221.

95 Karl Pearson, 'A Mendelian view of the Law of Ancestral Inheritance'. *Biometrika*, 3 (1904), 110.

96 *Ibid.*, 110-11

> Pearson sweepingly concluded 'If Darwinian evolution be natural selection with *heredity*, then the ancestral law must prove almost as epoch-making to the biologist as the law of gravitation to the astronomer'.[97]

Yet, the concluding statement that Pearson *actually* used differs from the one used by Froggatt and Nevin. Instead, Pearson wrote:

> If Darwinian evolution be natural selection with *heredity*, then the single statement which embraces the whole field of heredity must prove almost as epoch-making to the biologist as the law of gravitation to the astronomer.[98]

Whilst Pearson did say that 'At present [1898] I would merely state my opinion ... that the law of ancestral heredity is likely to prove one of the most brillant of Mr. Galton's discoveries', he did not state expicitly that 'the single statement which expresses the whole field of *heredity* was Galton's ancestral law'.[99]

It seems likely that Pearson may have considered Galton's law as one theory of inheritance and thus incomplete, and that he was still giving consideration to other explanatory models of inheritance. At the same time that Pearson wrote his 'New Year's Greetings to Francis Galton', he had just begun to undertake his work on 'Homotyposis' in his search for an explanatory model of inheritance.[100] The search would continue in the next century, and the rediscovery of Mendel's work in 1900, was to prompt further investigations by Pearson and Weldon on the mechanism of inheritance.[101] Nonetheless, Castle's as well as Froggatt and Nevin's views have found much support among a group of historians who have argued that Pearson was merely a disciple of Galton to the extent that there were no differences in their approaches or their outlook on inheritance.[102]

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97 Froggatt and Nevin, 'Galton's "Law of Ancestral Heredity"', pp.8-9.

98 Pearson, 'MCTE...Law of Ancestral Heredity' (1898), 412.

99 *Ibid.*

100 Karl Pearson, Alice Lee, Ernest Warren, Agnes Fry, Cicely D. Fawcett and others, 'MCTE. IX. On the principle of Homotyposis and its Relation to Heredity, to the variability of the inidividual and to that of the Race. Part I. Homotyposis in the Vegetable Kingdom.' *Philosophical Transactions of the Royal Society*, 197 (1901), 285-379.

101 W.F.R. Weldon, 'Mendel's Laws of Inheritance in Peas', *Biometrika*, 1 (1902), 228-54.

102 See also in Castle, 'Laws of Heredity of Galton and Mendel', 222-3 as well as Froggatt and Nevin, 'Galton's "Law of Ancestral Heredity"', *passim*; Ruth Swartz Cowan, 'Nature and Nurture', p.197; Mackenzie, *Statistics in Britain, 1865-1930*, pp.1-10; Bernard Norton, 'Karl Pearson and the Galtonian Tradition: Studies in the rise of Quantitative Biology' (Ph.D. Thesis, London University, 1978); Reginald C. Punnett, 'Early Days of Genetics', *Heredity,* 4 (April 1950) and Swinburn, 'Galton's Law', 20.

THE COEFFICIENT OF VARIATION: When dealing with the absolute measurements of man, Galton altered the average 'in the ratio of 13 to 12' to obtain those of woman. Galton coped with this problem by treating the mean stature of woman with an equivalent mean stature in man and then compared the deviations in men and women. Galton's midparent was

> the imaginary mean of the two parents, after the female measurements [had] been transmuted to their male equivalent, so that a mid-parent of 70 inches in height refers to a couple whose mean stature under the above reservations is 70.[103]

He adjusted or 'transmuted' all female stature to male by multiplying them by the constant 1.08. Pearson introduced another method to cope with this problem as in

> dealing with comparative variation of men and women (or indeed two sexes of any animal), we have constantly to bear in mind that the relative size influences not only the means but the deviation from the mean. When dealing with absolute measurements, it is, of course, idle to compare the variation of the larger male organ directly with the variation of the smaller female organ. The same remark applies also to the comparison of large and small built races.[104]

Though Galton had made the first step and 'had gone so far as to replace any woman by an equivalent man' on this basis, the best way to compare deviations in man and woman was to alter the deviations in the same ratio. Pearson then explained that it would be useful to

> take as a measure of variation the ratio of standard deviation to mean, or what is more convenient, this quantity multiplied by 100. We shall accordingly, define **V**, the coefficient of variation, as the percentage variation in the mean, the standard variation being treated as the total variation in the mean... Of course, it does not follow because we have defined in this manner our 'coefficient of variation', that this coefficient is really a significant quantity in comparison of various races; it may only be a convenient mathematical expression.[105]

Thus, C of V = /SD x 100 (i.e., the coefficient of variation = mean/ standard deviation 100.

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103 Galton, 'Family Likeness', p.53.
104 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896), 276.
105 *Ibid.*, 277.

THE LAW OF HEREDITY AND NON-BLENDING INHERITANCE

Shortly after Weldon's move to Oxford in 1899, Pearson wrote to Weldon seeking clarification on Galton's thories of inheritance.[106] In Weldon's response of 12 April, he wrote this letter to explain Galton's ideas of 'blending' and 'non-blending' inheritance:

> My dear Pearson, If your letters were easier to answer, I would answer them more quickly—As it is, my *Natural Inheritance* is in a packing case, in charge of a furniture man. I have been trying to evolve a conception of Galton's meaning out of my inner consciousness. I believe he means this: he distinguishes characters into those which 'blend' and those which do not; and he now at least explicitly, and I think implicitly in the last part of *Natural Inheritance*, regards the every character of an organism as the sum of a series of ancestral quantities, represented by his series $^1/_2 + {}^1/_2{}^2 + \ldots$ Now in characters which 'blend', the quantity represented by each item in the series is furnished by *all* the ancestors of a generation together,—probably each ancestor contributing a sensibly equal amount.—Hence the midparent, and the story we know.
>
> But, if a character be of the kind which does *not* 'blend' in inheritance, I think Galton means that its quantity is still represented by the same series, but that the whole amount, represented by one term of the series, is contributed by *one parent only*, though it is not certain *which* parent will contribute in a given case.[107]

(In his closing statment, Weldon wrote, 'I wish you had come up here too! We would have made animal evolution hum'.)

Galton looked at the inheritance of characters that did *not* blend when he was attempting to determine the inheritance of coat-colour in bassett hounds. He used Everett Millais's *Bassett Hound Club Rules and Stud-Book* (compiled from 1874-1896) to examine 'a total of 817 hounds of known colour'; they had been classified either as tricolour (a form of melanism) or non-tricolour of which 568 had all four grandparents.[108] He wanted to know if one sex was so 'markedly prepotent over the others in transmitting colour that a disregard of sex would introduce statistical error'.[109] As Galton thought that discrete characters were inherited by one parent

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106 Though Pearson's letter no longer exists, Weldon's response indicates that he was answering a letter Pearson sent to him.

107 Weldon, Letter to Karl Pearson, 12 April 1899, KP:UCL.

108 Francis Galton, 'The Average Contribution of each several Ancestor to the total Heritage of the Offspring', *Proceedings of the Royal Society*, 61 (1898), 403.

109 *Ibid.*, 401.

only, he did not think there was any statistical advantage of grouping the sexes of both parents together.[110]

Using his law of ancestral inheritance where $½ + (½)^2 + (½)^3 + (½)^4$, he found that 79% of parents of tricolour hounds were tricolour and 56% of parents of non-tricolour hounds were non-tricolour. After fitting his data to the normal distribution, he found from the closeness of calculated and observed numbers, an 'extraordinary close coincidence' which therefore 'proved that the [ancestral] law is correct in the present instance'.[111] Thus Galton placed considerable importance on the normal distribution for his laws of heredity (as he had for natural selection as well). Unlike Galton, Pearson and Weldon were not concerned that the distribution follow the normal law: when they would examine Mendelian distributions at the beginning of 1901, they used Pearson's chi-square goodness of fit test of 1900 (which could be used for various shaped distributions).

A year after Galton published his Law of Ancestral Heredity, he wrote a letter to *Nature* (on 14 July 1898) expressing his belief that 'high-prepotency' (i.e, variations which are inherited from one parent only) does not arise from normal variations but through sports'.[112] He concluded that 'Sports [from high prepotency] are more highly inherited than normal variations' and that 'no real step forward can be made by the selection of normal variations'.[113]

Two weeks later, Pearson wrote to *Nature* that Galton's conclusions was 'entirely opposed to Mr. Galton's own law of ancestral heredity'.[114] Pearson maintained 'that normal variation really affords material for stable change'. (In a footnote, Pearson remarked that he preferred the term *continuous* variation to Galton's *normal* variation as not all continuous variation was normally distributed.) Six days after *Nature* printed Pearson's letter, he wrote to Weldon as he was very much concerned about

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110 *Ibid.*, 405.
111 *Ibid.*, 407.
112 Francis Galton, 'The Distribution of Prepotency', *Nature*, 58 (14 July 1898), 246-7.
113 *Ibid.*
114 Karl Pearson, 'The Distribution of Prepotency', *Nature*, 58 (28 July 1898), 293.

Galton's views on regression. By the time Weldon received the letter, he had already read the letters in *Nature*. Pearson's letter indicates the extent to which Weldon's opinion was valued by Pearson when he wrote:

> My dear Weldon,
>
> You have been a good deal in my thoughts last week!
>
> I was a good deal drawn by Galton's letter [in *Nature*] for it seemed to me that he was still hopelessly at sea with regard to the theory of regression, and if *he* did not follow the bearing of my January paper on the law of ancestral heredity, who in the world can I expect to? I had tried to put the stability which arises even from *one* selection of parents as clearly as possible in that, and I see it was all no use. However I cling to the straw that you at least will have realised my point. That is why I want to get rid of the word *regression*, it simply misleads those who do not understand the theory, Galton, I fear, included. There is a permanent *progression* of the offspring of exceptional parents provided there is no degeneration by mating. I am glad you cared for the *Nature* letter, otherwise I don't know anybody else's opinion that I care about.[115]

Four days later Weldon replied that

> Galton's fixed idea, I believe, is that regression should be reckoned from the *parental* mean in children.—I can't see any other explanation of his attitude.—I know that Bateson's talk of *'organic stability'* is based on this idea.—You can see it in the preface of his book where he says that 'of course, where the parental race is dimorphic, there is *no* regression'.[116]

Thus may be seen that Pearson's views on inheritance thus differed from Galton's in two respects: firstly, Pearson did not attach the same amount of importance to ancestry as did Galton and secondly, he did not think that inherited variation should be normally distributed. Pearson would look to Weldon for clarification when developing his views of inheritance in the twentieth century.

THE LAW OF ANCESTRAL INHERITANCE

MULTIPLE REGRESSION: The mathematical procedures Pearson used to define Galton's Law of Ancestral Heredity provided the basis for the development of *multiple regression.* (Like simple regression, it involves a linear prediction, but rather than using only one variable to be 'predicted', a collection of variables can be used instead.

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115 Karl Pearson, Letter to W.F.R. Weldon, 3 August 1898, KP: UCL, pp.1-2.
116 W.F.R. Weldon, Letter to Karl Pearson, 7 August 1898, KP: UCL, pp. 1-2.

When Pearson re-examined Galton's theory of the 'Contribution of an Ancestor', he used the expression ***U*** to symbolise the deviate of Galton's mid-parent (or the generant) which defined the stirp: he termed this Galton's *Law of Ancestral Inheritance*. This was not a biological hypothesis, but 'the mathematical expression of statistical variates [i.e., multiple regression], which obey, as many measurable characters in man, certain forms of frequency distribution'. Pearson thought that Galton's theory could be used to test 'what on the *average* can be predicted of somatic characters of the offspring from a knowledge of what the germ-plasms of the "stirp" have produced in the past': this could then be interpreted as, or replaced by 'Contribution of the Ancestor to the Prediction Formula'.[117]

Thus, 'if ***U*** for any character be the deviate of the generant from the mean of the race, then the individual endowed with any such ***U***s for all characters would represent the stirp of any family.'[118] To throw light on Galton's idea, Pearson took the character ***U*** of that generant to be that linear function of the characters of all the ancestry which gave the highest correlation ***R*** with the character in the offspring. Hence ***U*** was simply proportional to the multiple regression equation where the correlations of the offspring with individual parents with indiviudal grandparents, with individual great-grandparents formed the series r_1, r_1a, r_1a^2, and where a is less than unity.[119] It could then be demonstrated that the deviate ***U*** will be given by the formula

$$U = \gamma (h_1 + \beta h_2 + \beta h_3 \ldots)$$

U = the Generant,
h = the deviates of midparental characters in successive grades of ancestry
$\gamma\beta$ = constants found in terms of r and a.

Further, the fraternity of which ***U*** defines the stirp will vary around ***U*** with variability $\sigma\sqrt{1-R^2}$ where R (the coefficient of multiple correlation) is known in terms of r_1 and a or γ and β.

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117 Pearson, *Life of Galton*, IIIA (1930), p.21.
118 *Ibid.*
119 *Ibid.*

Galton did not give any method for determining the ***U*** of the generant, instead, he attempted to solve contributions of ancestry through his generational taxation. If the midparent contributes $^2/_3$ of height to offspring then the grandparent will contribute $^1/_3$ of height and the great-grandparent will contribute $^1/_9$; hence, the sum of the deviates of all of the midparents that constitute the heritage of the offspring was:

$$= x\,(1 + {}^1/_3 + {}^1/_9 + \text{etc}) = x^3/_2$$

Pearson showed this was erroneous because it assumed that the quantities γ, γa, γa^2 ... of the generant could be found from the simple regression formula of parent on offspring, but this could 'not be as multiple regression coefficients have *no* simple relation to parental regression'.[120] Pearson suggested that the 'somewhat complicated mathematics of multiple correlation with its repeated appeals to the geometrical notions of hyperspace remained a closed chamber to [Galton]'.[121]

DETERMINANTAL MATRIX ALGEBRA: Whilst the mathematical development of correlation and regression seem to dominate Pearson's paper of 1896, Pearson's most seminal contribution in this paper was, undoubtedly, his introduction of determinantal matrix algebra into statistical theory (which was to become a *sine qua non* for multivariate statistical theory). Pearson's use of determinantal matrix algebra (which still corresponds to matrix notations used today) very probably stemmed from Arthur Cayley (1821-1895), one of his tutors at Cambridge, who created determinantal matrix algebra by his discovery of the theory of invariants during the middle part of the nineteenth century.[122] This form of mathematics, which underpinned Pearson's

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120 *Ibid.*

121 *Ibid.*, p.21. See also Karl Pearson, 'Further remarks on the Law of Ancestral Heredity', *Biometrika*, 8 (1911), 239-43; *idem*, 'On the Multiple Correlation of Brothers, being a Note on Mr. J.O. Irwin's Memoir ['The Further Theory of Francis Galton's Individual Difference Problem'] and on my statement of the Application of Galton's Difference Problem to the Determination of the Degree of Relationship of Brothers, made in August 1902', *Biometrika*, 17 (1925), 129-41.

122 At the time of Cayley's death, he was considered to have been 'the greatest living master of algebra' who would be remembered for 'his creation of an entirely new branch of mathematics'. Anon, 'Professor Arthur Cayley', *Nature*, 51 (31 January 1895), 323. John Michael Dubby considered Cayley to have been 'the inventor of the theory of matrices', in Arthur Cayley (ed.), Trevor Williams, *A Bibliographical Dictionary of Scientists* (Adam and Charles Black: London, 1969), p.101.

multiple correlation and multiple regression, not only provided the most striking departure from all other earlier forms and uses of statistics, but it would lead to an increasing specialisation of a discipline that was to become known as 'mathematical statistics'.

FUNDAMENTAL THEOREM IN SELECTION: When Pearson used multiple regression to address problems of natural selection, he needed a distribution that could handle more than two variables simultaneously. The general theory of correlation had shown that by taking $p + 1$ correlated organs, and selecting p of them of definite dimensions, the remaining organ [would] follow a normal law of distributions, of which the standard deviation and mean [could] be determined'.[123] In problems of natural selection, 'the p organs selected may be specially correlated together in selection, in a manner totally different from their natural correlation or correlation of birth'.[124] Thus, the problem was to find the distribution of the $(p + 1)^{th}$ organ related to this arbitrary surface and to determine the natural surface. Matrix algebra made it possible to consider a p-dimensional (or multivariate) system when a bivariate system was insufficient. This problem is highlighted in Figure 5.5.1 which depicts a three-dimenensional representation for problems of multiple regression which may be compared to the two-dimensional diagram in Figure 5.5.2 used for simple regression.

A generalised investigation of the following kind was required; 'each organ was selected normally *round a given mean*', and the p-selected organs, pair and pair, were to be 'correlated in any arbitrary manner'.[125] Having done this, the distribution of the remaining $(p + 1)^{th}$ needed to be determined.Pearson then constructed geometrically, a correlation surface in p-dimensional space and by using matrix algebra, he cut an arbitrary correlation surface of the p^{th} order. When 'p organs [were] organs of ancestry and the $(p +1)^{th}$ organ that of a descendent', Pearson considered this to be a

123 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896), 298.
124 *Ibid.*
125 *Ibid.*

Figure 5.5.1
GEOMETRICAL REPRESENTATION OF A MULTIPLE REGRESSION PLANE

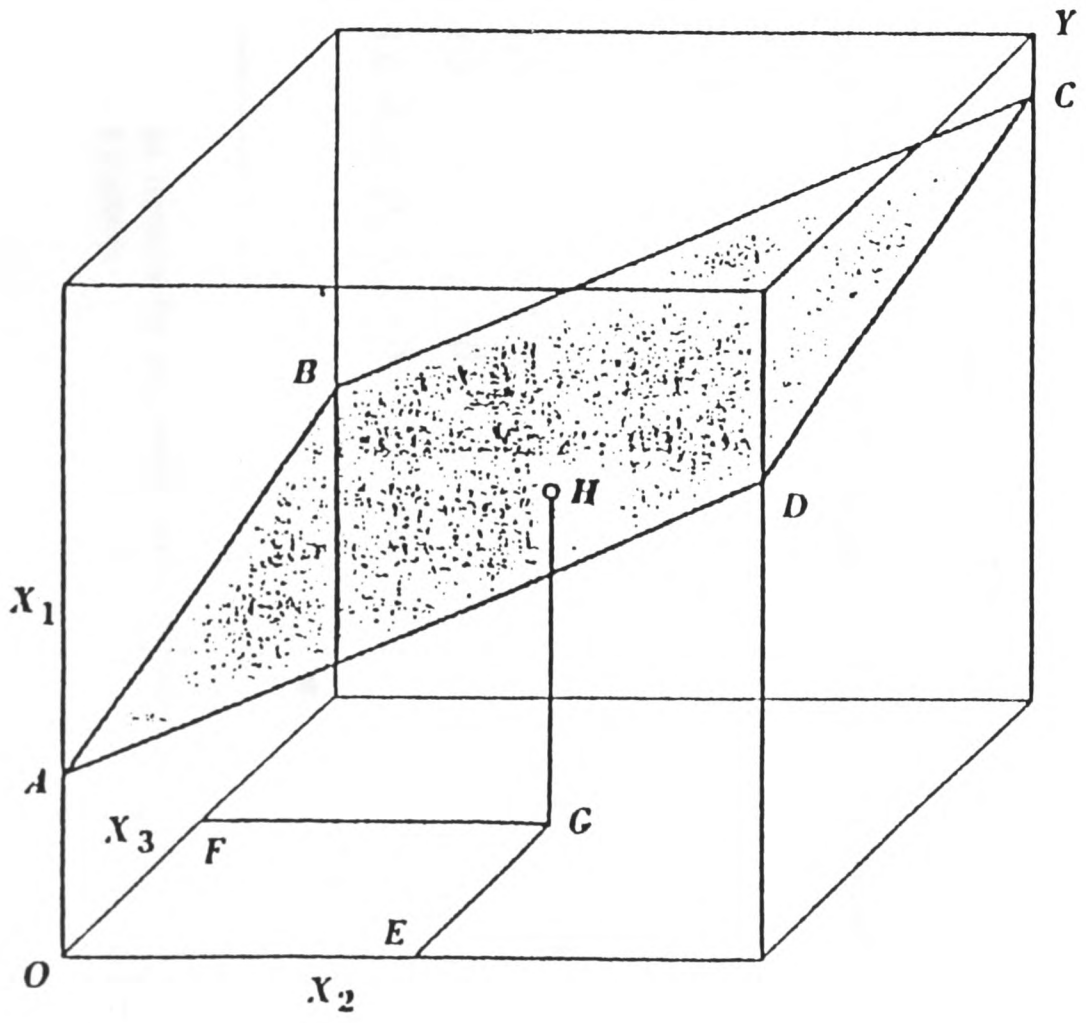
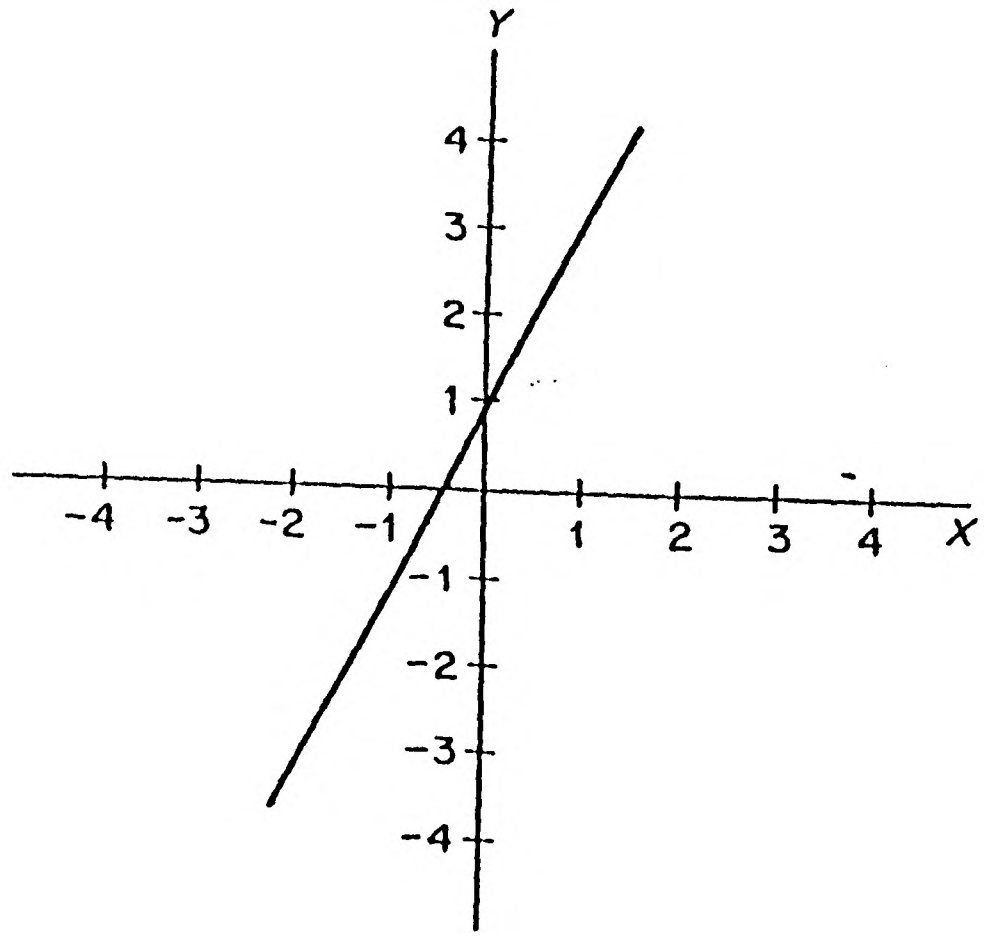


Figure 5.5.2
STRAIGHT REGRESSION LINE



'problem of natural selection modified by inheritance.'[126] A matrix is a rectangular table of numbers with p rows and n columns and may take on this form:

$$\begin{matrix} \mathbf{a}_{11} & \mathbf{a}_{12} \cdots \mathbf{a}_{1n} \\ \mathbf{a}_{21} & \mathbf{a}_{22} \cdots \mathbf{a}_{2n} \\ \mathbf{a}_{31} & \mathbf{a}_{32} \cdots \mathbf{a}_{1n} \\ \vdots & \\ \mathbf{ap}_{1} & \mathbf{a}_{p2} \cdots \mathbf{a}_{pn} \end{matrix}$$

A determinant is is a scalar function of a square matrix; the determinant A is denoted by $|A|$: λ symbolises the mean of the matrix and θ is the variance of the matrix.

THE MULTIPLE REGRESSION COEFFICIENT β: Pearson distinguished between two correlation surfaces for the selected and the unselected organs: β_1, β_2, β_3 were the regression coefficients of the $(p + 1)^{th}$ organ on the p organs for unselected correlation; the values of the p organs their respective means, h_1, h_2, h_3...h_n from the $(p + 1)^{th}$ organ had a distribution centering round $\beta_1 h_1 + \beta_2 h_2 + \beta_3 h_3 + ... \beta_n h_n$ and a standard deviation σ given by the general theory of correlation (i.e., the standard deviation of the array).[127] Similarly, for values h_1+x_1, h_2+x_2, h_3+x_3... of the p organs, the $(p + 1)^{th}$ will have a distribution with standard deviation and centre:

$$\beta_1(h_1 + x_1) + \beta_2(h_2 + x_2) + \beta_3(h_3 + x_3) + \zeta + \Sigma(\beta_1 x_1).$$

The selected correlation surface centering around h_1, h_2, h_3 was given by

$$z = \text{constant}\, e^{-\frac{1}{2}(a_{11}x_1^2 + a_{22}x_1^2 + 2a_{12}x_1x_2 + \ldots)}.$$

Hence:

$$\Delta x._1 = (\beta_1 A_{11} + \beta_2 A_{12} + \beta_3 A_{13}) \quad (\upsilon - \Sigma(\beta_1 x_1')) / \sigma^2,$$
$$\Delta x._2 = (\beta_1 A_{21} + \beta_2 A_{22} + \beta_3 A_{23}) \quad (\upsilon - \Sigma(\beta_1 x_1')) / \sigma^2,$$
$$\Delta x._3 = (\beta_1 A_{31} + \beta_2 A_{32} + \beta_3 A_{33}) \quad (\upsilon - \Sigma(\beta_1 x_1')) / \sigma^2,$$

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126 *Ibid.*

127 Pearson uses σ in his formula for the sample and population standard deviation and Σ for the standard deviation of a matrix.

where Δ symbolises a matrix and is the determinant of the *a's,* and the Ç's were its minors, clearly $a_{ij} = a_{ji}$ and $A_{ij} = A_{ji}$.

Edgeworth had tried to use a similar mathematical procedure to express his theorem of the correlation surface. When Steve Stigler examined Edgeworth's theorem, he observed that it was expressed 'in the determinantal notation and terminology of the time'.[128] Pearson, nonetheless, showed how Edgeworth's theorem of the correlation surface could be expressed by using matrix algebra (see Figure 5.6).[129] He used the theory of minors, in Salmon's *Higher Algebra.*

Figure 5.6
Pearson's expression of Edgeworth's Theorem of the Correlation Surface in Determinantal Matrix Algebra

$$\Delta'^{p-1} = \begin{vmatrix} A_{11}, & A_{12}, & A_{13}\cdots \\ A_{21}, & A_{22}, & A_{23}\cdots \\ A_{31}, & A_{32}, & A_{33}\cdots \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \end{vmatrix}$$

$$= \Delta^{p} S_1^{\,2} S_2^{\,2} S_3^{\,2} \cdots \begin{vmatrix} 1, & \rho_{12}, & \rho_{13}\cdots \\ \rho_{21}, & 1, & \rho_{23}\cdots \\ \rho_{31}, & \rho_{32}, & 1\cdots \\ \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot \end{vmatrix}$$

Hence $1/\Delta = S_1^2 S_2^2 S_3^2 \ldots$, where R is the determinant formed by the correlation coefficients with a diagonal of units.

Further, if B_{11}, $B_{22} \ldots B_{12} \ldots$ be minors of the A-determinant, and R_u, $R_{22}, \ldots R_{12} \ldots$ of the R-determinant, we have (SALMON, *loc. cit.*)

$$a_{11} = B_{11} / \Delta^{p-2} = \Delta R_{11} s_1^{\,2} s_2^{\,2} s_3^{\,2} = \ldots / s_1^{\,2} = R_{11} / (R s_1^{\,2}),$$

$$a_{22} = B_{22} / \Delta^{p-2} = \Delta R_{22} s_1^{\,2} s_2^{\,2} s_3^{\,2} = \ldots / s_1^{\,2} = R_{22} / (R s_2^{\,2}),$$

$$a_{12} = B_{12} / \Delta^{p-2} = \Delta R_{12} s_1^{\,2} s_2^{\,2} s_3^{\,2} = \ldots / s_1 s_2 = R_{12} / (R s_1 s_2).$$

. . . . . .

128 Stigler, *History of Statistics*, p.323.
129 Pearson, 'MCTE. III. Regression, Heredity and Panmixia' (1896), 301.

Stigler also interpreted Edgeworth's ideas using modern matrix algebra (see Figure 5.7). It may be seen that Pearson's correlation matrix (the second matrix) is very nearly identical to the one Stigler presented.

Figure 5.7

$$\Sigma = \begin{vmatrix} 1 & \rho_{12} & \rho_{13} & \cdot & \cdot & \cdot & \rho_{1r} \\ \rho_{12} & 1 & \rho_{23} & \cdot & \cdot & \cdot & \rho_{2r} \\ \cdot & & & & & & \\ \cdot & & & & & & \\ \cdot & & & & & & \\ \rho_{1r} & \rho_{2r} & \rho_{3r} & \cdot & \cdot & \cdot & 1 \end{vmatrix}$$

Pearson then determined that $^1/_\Delta = s_1^2\ s_2^2\ s_3^2 ... R$ which was the determinant formed by the correlation coefficient with a diagonal of units (i.e., ones).

R_{pq} is the pq minor of the determinant

$$R = \begin{matrix} 1 & r_{01} & & r_{02} & \cdot & \cdot & \cdot & r_{on} \\ r_{10} & 1 & \cdot & \cdot & \cdot & \cdot & r_{12} & r_{1n} \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & 1 & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ r_{n0} & r_{n1} & & r_{n2} & & & & 1 \end{matrix}$$

When Pearson wanted to work out the mathematical expression of ancestral inheritance *and* natural selection by looking at selected parents, he calculated the β's for *p* selected ancestor and obtained the regression ξ in the descendants by putting in the values of h_1, h_2, h_3 of the means of the selected ancestors. When *p* correlated organs were arbitrarily selected about *p* mean, using matrix algebra, Pearson found regression ξ and standard deviation Σ. Thus, matrix algebra could be used to express the chief features of natural selection.[130] Pearson thought that although Galton endeavoured to reach this step by a short cut, and thus evolved his law of ancestral heredity which 'was a brilliant and suggestive step, he was not able to state the conditions under which it is theoretically correct or bring forward data at that time to confirm its observational accuracy'[131]

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130 *Ibid.*, 302

131 Pearson, *Life of Galton*, IIIA (1930), p.21.

Pearson pursued this work with matrix algebra and natural selection for another seven years when in 1903 he stated the fundamental theorem of multiple correlation and gave the algebraic investigation of the new proof for natural selection.[132]

CONCLUSIONS

It has been shown that Francis Galton's work on correlation provided a stimulus for Weldon, Edgeworth, Pearson and Yule. Each of them made different contributions which led to the advancement of a modern theory of statistics. Weldon was the first to use Galton's method of correlation in 1889 (in his attempt to find a biometrical species criterion), and he was also the first to suggest the idea of a negative (or inverse) correlation which Pearson developed more fully. Edgeworth replaced Galton's median and quartile with the mean and probable error in 1892 (thereby improving the accuracy of computation). Once it had been established that a linear relationship could be made between pairs of observed (i.e., empirical) values, Pearson introduced the product terms into the equation.

In addition to working out the mathematical properties of the product-moment correlation coefficient, Pearson also devised the standard deviation for the product-moment correlation coefficient, the standard error of estimate (for regression) and the coefficient of variation. Pearson probably did more to promulgate all aspects of correlation at the end of the nineteenth century and at the beginning of the twentieth century than anyone else during that time.

Galton's 'Law of Ancestral Heredity' provided the framework in which Pearson could develop multiple correlation and multiple regression. Edgeworth and Yule also made various contributions to multiple correlation; in 1897, Yule introduced the method of least squares to interpret regression.[133] By the time Pearson had introduced determinantal matrix algebra in 1896, he had not only introduced

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132 Pearson, 'The Law of Ancestral Heredity', *Biometrika*, 2 (1903), 211-29.
133 Yule, 'On the Theory of Correlation', *Journal of the Royal Statistical Society*, 60 (1897), 812-54.

mathematical rigour into multiple correlation and multiple regression, but he had provided the mathematical underpinnings for multivariate statistics.

The continual specialisation of Pearsonian statistics was making it increasingly inaccessible to biologists. This was a development which was to create enmity between the biologists and the biometricians. Rather than unifying biology and mathematics as Pearson and Weldon might have hoped, the two groups became polarised. The different approaches to understanding inheritance would lead to a series of debates at the end of the nineteenth century, culminating in the Biometrician-Mendelian debate of 1900 and would continue fervently in the early years of the twentieth century.

The historiography of this debate has focused on two issues concerning Pearson and Weldon: the first is the belief that they were measuring continuous variation only and the second, is that Pearson rejected Mendelism. In this respect, Pearson's use of continuous variation has been considered to have been the key element in the development of Pearsonian statistics.

It will, however, be argued that this debate involves a number of wider issues that need to be considered for a fuller understanding of the biometrical issues entailed. Some of these issues, concerning Pearson's role in this debate, include the following: (1) discontinuous variation also formed a part of the corpus of Pearsonian statistics, and was measured before the turn of the century with appropriate statistical methods devised by Pearson; (2) Mendelism was accepted partially by Pearson (as a theory of inheritance for discontinuous variation) and (3) it will be suggested that the intensity of Pearson's sense of bereavement following Weldon's death, remained poignant for the remaining thirty years of his life and also led to an emotional and intellectual reconstruction of his life in the absence of his colleague. This personal loss needs careful consideration—insofar as it affected the content and quality of Pearson's biological papers.

The role of measurement for discontinuous variation (including the statistical analysis of Mendel's 'factors') in addition to the determination of the goodness of fit

for a Mendelian distribution, will be examined in the next chapter. Pearson's views of Mendelism, his joint biometric investigations with Weldon for problems of inheritance (from 1899 to 1906), and the effect of Weldon's death on Pearson and Galton will be discussed Chapter VII.

Whilst it has been shown that Pearson had established the mathematical underpinnings of the corpus of a modern theory of statistics by the end of the nineteenth century, the publication of his most fundamental and original contribution to statistics was yet to come. This work was the most comprehensive and encompassing of all of Pearson's statistical work as it spans nearly the whole of his working life: it begins with the first of his statistical lectures at Gresham College in November 1892 (when he devised a statistical system for the analysis of some of Weldon's data), reaches its apex in 1900, when he published his *chi-square* (χ^2, P) *goodness of fit test* and ends with the last paper he wrote three months before he died (published posthumously in June of 1936). In the next chapter, the chi-square goodness of fit test and the chi square test of independence for contingency tables of 1904, which underpinned the construction and development of Pearsonian statistics, will be examined.

Chapter VI

DIFFERENTIAL MORTALITY RATES AND PROBABILITY: THE CHI-SQUARE GOODNESS OF FIT TEST

At the turn of the century, Karl Pearson reached a fundamental breakthrough in his development of a modern theory of statistics when he found the exact chi-square distribution from the family of Gamma distributions and devised the *chi-square* (χ^2,P) *goodness of fit test*.[1] The test was constructed to compare observed frequencies in an empirical distribution with expected frequencies in a theoretical distribution to determine 'whether a reasonable *graduation* had been achieved' (i.e., one with an acceptable probability).[2] Pearson considered distributions which could be known *a priori* (such as the distribution from a set of dice tossed for a specific number of times) or for distributions that could not be known *a priori* (such as the frequency of petals in buttercups).[3] It will be argued that this landmark achievement was the outcome of the previous eight years of curve fitting for asymmetrical or 'abnormally symmetric' distributions (i.e., skewed and kurtotic) and, in particular, of Pearson's attempts to find an empirical measure of a goodness-of-fit test. It will be further argued that this work emerged in tandem with his theoretical work on probability.

This development may be seen as Pearson's reaction to the status accorded to the normal distribution at the end of the nineteenth century. The idea that empirical data should conform to the normal distribution was borne out of Adolphe Quetelet's use of the arithmetic mean for his *l'homme moyen*: Quetelet's deterministic outlook led him to believe that deviations from the ideal type were flawed and thus a product of error. This interpretation of error arose from the theistic argument that order and perfection

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1 Karl 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*, 50 (July 1900), 157-175 [hereinafter referred to as Pearson, 'System of Deviations' (1900)].

2 Pearson, *op. cit.* A year before his death, Pearson restated his position in 'Statistical Tests', *Nature* (24 August 1935), 296.

3 These are two of the examples Pearson used in his 1900 paper.

of nature in the universe were due to the existence of a divine Creator (i.e., 'the argument from design'). Thus, any 'error' which arose in nature could only be viewed as flawed if it would have interfered with the Deity's plan and purpose of the universe.

The nineteenth century belief in the Aristotelian essentialistic ideal type of species paralleled Quetelet's use of *l'homme moyen*. Darwin's recognition that species comprised different sets of populations, underpinned by different ranges of variation, not only shattered the essentialistic concept of species, but shattered the deterministic notion of the ideal (statistical) mean as well. With this Darwinian view, Pearson and Weldon recognised that empirical distributions of biological variation could produce a variety of asymmetrical and symmetrical curves which included the normal distribution (expressed as Type V in 1895 in the Pearsonian family of curves).

Thus from the beginning, Pearson's statistical work was directed towards finding statistical resolutions of variously shaped distributions and in particular, in looking for an empirical measure of a goodness of fit test. In his 1900 paper on the chi-square goodness of fit test, Pearson addressed the importance of examining empirical distributions that did not conform to the normal distribution. He argued that whilst it may be possible to find material within the probable limits of the normal law

> this law is not a universal law of nature. We must hunt for [these] cases... We can only conclude from the investigations here considered that the normal curve possesses no special fitness for describing errors of deviations such as [those that] arise either in observing practice or in nature. We want a more general theoretical frequency, and the fitness of any such to describe a given series.[4]

The statistical framework of Pearson's chi-square is a tripartite system as it not only incorporates a probability distribution and a goodness-of-fit test (from his 1900 paper), but it also includes a statistical technique which Pearson introduced in 1904 when he extended his goodness-of-fit test to manifold classifications for the analysis of contingency tables. Pearson termed this technique the 'square contingency coefficient' to test differences between observed cell frequencies and theoretically expected cell frequencies: this would become termed later by R.A. Fisher as the *chi-*

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4 Pearson, 'System of Deviations' (1900), 174.

square statistic.[5] Pearson was to 'object very strongly to [Fisher's] use of the word "statistic" for a statistical parameter'.[6] He objected as 'other branches of science' did not use 'a mathematic, a physic, an electric and etc.' for parameters or constants and he thus preferred not to use the term 'statistic'.[7] Nonetheless, what was to become known as the *chi-square* statistic for contingency tables would become the dominant statistical method in this century for a measure of association for discrete variables. This extensive use seems to have contributed to the emphasis of Pearson's chi-square contingency coefficient to the virtual neglect of his chi-square goodness of fit test (and to his goodness of fit testing in the 1890s) in the historiography of Pearsonian statistics. The test and the statistic are, however, built on two different hypotheses: the hypothesis of the goodness-of-fit test of 1900 seeks to determine if the observed distribution conforms to the theoretical distribution, whereas the statistics of 1904, seeks to determine if an association exists between two discrete variables in a contingency table.

Whilst Pearson's work on the chi-square is generally regarded as his greatest single contribution to a modern theory of statistics, this tripartite distinction (of a probability distribution, goodness of fit test and the test of independence) is rarely made in the historiography of Pearson's work on the chi-square.[8] When his work has been assessed, the goodness-of-fit test has been given little consideration whereas his test of independence on the chi-square statistic for contingency tables tends to be

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5 Karl Pearson, 'On the Theory of Contingency and its Relation to Association and Normal Correlation', *Drapers' Company Research Memoirs*, Biometric Series, I (Dulau: London, 1904), 1-37. Fisher introduced the word 'statistic' in his paper 'On the Mathematical Foundations of Theoretical Statistics', *Philosophical Transactions of the Royal Society*, 222, A (1922), 323.

6 Karl Pearson, 'Methods of Moments and Method of Maximum Likelihood', *Biometrika*, 28 (1936), 49.

7 *Ibid.*

8 J.B.S.Haldane thought that Pearson's 'most valuable single achievement was the invention of a statistic called the chi-square'. J.B.S. Haldane, 'Karl Pearson', *University College London Magazine* (June 1936), 292; Churchill Eisenhart considered this work to be 'one of Pearson's greatest single contributions to statistical theory'. Churchill Eisenhart, 'Karl Pearson', *Dictionary of Scientific Biography*, 10 (Charles Scribner's Sons: New York, 1974), p.468. Egon Pearson regarded this as 'One of Pearson's greatest single contributions to statistical methodology'. Egon Pearson, 'Karl Pearson. An Appreciation of some Aspects of his Life and Work'. Part 1, *Biometrika*, 28 (1936), 22.

overemphasised; occasionally, the two are treated as one entity. In Donald Mackenzie's interpretation of Pearson's 1900 paper, for example, he misidentified the goodness-of-fit test for the chi-square statistic. When he examined Pearson's statistical work on contingency tables, he thought that

> the most important of these attempts [on developments of contingency tables] was his development of the coefficients of contingency. This derived from the application of his own χ^2 (chi-square) test to two-way tables (Pearson 1900)... He wanted not simply to reject the hypothesis of no association, but to measure the strength of association.[9]

Hence, Pearson's goodness of fit test of 1900 was described as the chi-square statistic for contingency tables of 1904. MacKenzie's interpretation of the chi-square statistic and of Pearson's other tests for contingency table will be examined in greater detail in Part II in the section on contingency tables. Theodore Porter shares Mackenzie's view as he thought that Pearson 'defined mathematically... contingency analysis and the chi-square test in 1900'.[10] Writing on Pearson's 1900 paper, Ian Hacking attached a contemporary usage of the chi-square when he remarked that the 'chi-square test can be used for hypotheses and data where observations fall into discrete categories that statisticians call cells'.[11]

In *The Empire of Chance*, Gerd Gigerenzer *et al*, purport a view of Pearson's use of the goodness-of-fit that seems to runs counter to Pearson's intention when they wrote that

> Pearson published his chi-square test of 'goodness of fit' of a set of data to a curve to which they [were] supposed to conform. (He developed this test to prove the inadequacy of the normal curve and the superiority of his own generalized family of distributions.)[12]

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9 Donald A. Mackenzie, *Statistics in Britain. 1865-1930: The Social Construction of Scientific Knowledge* (Edinburgh University Press, 1981), p. 158. Pearson's goodness-of-fit test is mentioned briefly when Mackenzie examined the work of Arthur Black to whom he attributed the 'deriviation of... the chi-square approximation to the multinomial distribution' p. 101.

10 Theodore Porter, *The Rise of Statistical Thinking, 1820-1900* (Princeton University Press: Princeton, 1986), p. 315.

11 Ian Hacking, 'Trial by Number', *Science* (November 1984), 69.

12 Gerd Gigerenzer, Zeno Swijtink, Theodore Porter, Lorraine Daston, John Beatty, and Lorenz Krüger, *The Empire of Chance* (Cambridge University Press: Cambridge, 1989), p.79. Some three years earlier, in his book on *The Rise of Statistical Thinking*, Theodore Porter wrote that 'in every case the skew was superior' as regards Pearson's family of distribution. p.308.

Yet Pearson stressed that the normal curve was *one* of a number of distributions that could be used to describe empirical distributions. This view was expressed in his second paper on the Mathematical Contributions to the Theory of Evolution in 1895 when he wrote that

> I do not see why we have any right to select as our sole frequency curve the normal type $y = y_0 c^{-px2}$ in preference to the more general $y = y_0(1 + x/a_1)^{va1}(1-x/a_2)$, which not only includes the former, but supplies the element of skewness which is undoubtedly present in many statistical frequency distributions.[13]

Thus it would seem that Pearson made no attempt to show how curves were 'supposed to conform' to any particular distribution especially as he incorporated the normal curve in his Pearsonian family of curves. Moreover, it will be shown later that the goodness of fit test was not used to indicate how curves 'were supposed to conform', but rather it was devised to legitimate empirical distributions of all shapes which may or may not 'fit' the corresponding theoretical distribution. If statistical data could only assume the normal distribution, this would, indeed, have had a limited use for statistical analyses as non-parametric statistics revolve around the non-normality of distributions. Pearson's work thus provided the foundations for a major branch of non-parametric or 'distribution-free' statistics.

Though Robin Plackett recognised the tripartite framework of the chi-square as he wrote that the 'chi square is not only a distribution, but also a statistic and a test procedure', he also thought that 'all [three were] arrived *simultaneously*' (italics mine).[14] Ronald A. Fisher seems to have appreciated the development of Pearson's chi-square, perhaps, more fully than anyone else when he wrote that

> The first of the distributions characteristic of modern tests of significance, though originating with Helmert, was 'rediscovered'[15] by K. Pearson in 1900, for the

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13 Karl Pearson, 'Contributions to the Mathematical Theory of Evolution. II. Skew Variation in Homogeneous Material', *Philosophical Transactions of the Royal Society*, Series A, 186 (1895), 367 [hereinafter referred to as Pearson, 'CMTE II. Skew Variation in Homogeneous Material' (1895)].

14 Robin Plackett, 'Karl Pearson and the Chi-Squared Test', *International Statistical Review*, 51 (1983), 68.

15 R.A. Fisher attributed the chi-square distribution to F.R. Helmert who used Abbe's distribution in 1875 (which he referred to as a 'modified Abbe's Criterion'). R.A. Fisher, *Statistical Methods for Research Workers* (Hafner Press: New York, 1922). Harold Lancaster remarked soon afterwards that Helmert gave the frequency distribution of the sum of squares $\Sigma\varepsilon^2 i = n\sigma_2$ when ε

[Footnote continued on next page]

> measure of discrepancy between observation and hypothesis known as χ^2. This, I believe, is the great contribution to statistical methods by which the unsurpassed energy of Prof. Pearson's work will be remembered. It supplies an exact and objective measure of the joint discrepancy from their expectations of a number of normally distributed, and mutually correlated, variates. In its primary application to frequencies, which are discontinuous variates, the distribution is necessarily only an approximate one, but when small frequencies are excluded the approximation is satisfactory. The distribution is exact for other problems.[16]

Pearson's continual emphasis of the need for determining empirical distributions without assuming that these distributions should conform to the theoretical normal distribution has led to much criticism over the past fifteen years. Bernard Norton thought that Pearson placed too much emphasis on 'observations' and too little on 'theory' such that this was to have led to the creation of a mathematical system that was 'theory-free'. Norton thus argued Pearson's correlational work 'stemmed from a desire to find theory free connections between different sets of data' (i.e., between empirical and theoretical distributions) and Pearson's aim 'seems to have been to find ways of accurately describing any given set of data—notably by fitting a curve to it'.[17] A more historically sensitive explanation of Pearson's emphasis of 'observations' and 'theory' may be that this represented his attempt to cope with the asymmetry of empirical distributions which, in turn, required a mathematical reconceptualisation of curve fitting. Thus it would seem that *both* 'theory' and 'observations' were critical in the construction of the Pearsonian corpus of statistical theory particularly in those developments that led to the chi-square goodness-of-fit test.

To gain a fuller understanding of the developments over a period of eight years that led to the chi-square goodness of fit test, an overview of Pearson's earliest statistical work related to this development will be examined in Part 1. This will be

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[Footnote continued from previous page]

$_1$f are mutually independent normal variables with expected values equal to zero which has the distribution of χ^2 with n degrees of freedom. Harold Lancaster, 'Forerunner of the Pearson χ^2', *Australian Journal of Statistics*, 8 (November 1966). Oscar Sheynin noted that Helmert used a modification of Abbe's criterion for this distribution, which resembled Pearson's chi-square distribution. O.B. Sheynin, 'Origin of the Theory of Errors', *Nature* (27 August 1966), 1004. Fisher then attributed the 'rediscovery' of the chi-square to Pearson.

16 R.A. Fisher, *Statistical Methods for Research Workers* (Collier Macmillan: London, 1925.), p. 23.

17 Bernard Norton, 'Karl Pearson and Statistics', *Social Studies of Science*, 8 (February 1978), 13.

followed with an examination of his work on mortality curves and subsequent developments in probability (especially of his early attempts to determine a goodness of fit test for differential mortality rates as well as the subsequent 'significance tests'). A fuller discussion of the 1900 chi-square paper will then be presented and, finally, in Part 2, his work on contingency tables and the chi-square statistic will be examined.

1
GOODNESS OF FIT TESTS

Adolphe Quetelet made one of the earliest attempts to fit a set of observational data to a theoretical curve around 1840.[18] His procedures were graphical and he used a table based on the binomial distribution rather than using an approximation of the normal distribution. Such was Quetelet's conviction that observational data could be fitted to the normal curve only, a 'doctrine had been nicknamed 'Quetelismus' on the ground that Quetelet exaggerated the prevalence of the normal curve'.[19] Though Quetelet was aware that many distributions were skewed, he considered that this was 'due to curious accidental causes acting unequally in two directions'.[20]

When Louis-Adolphe Bertillon was fitting distributions of the stature of men from Southern France in 1863, he found that the distribution of height did not conform to the normal distribution; instead, his distribution was bi-modal (i.e. a curve with two modes rather than the unimodal normal type).[21] This finding suggested to Bertillon that the population in Doubs consisted of two human types (one short and one tall). These results were, in fact, quite similar to the dimorphic distribution of the forehead of the carapace of female crabs that Weldon and Pearson found in 1892.[22]

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18 Stigler, *History of Statistics*, p. 205.

19 Francis Ysidro Edgeworth, 'The Philosophy of Chance', *Mind*, 31 (1922), 257-83, cited in Stigler, *History of Statistics*, p. 203.

20 Adolphe Quetelet, *Lettres à S.A.R. le Duc Regnant de Saxe-Cobourget et Gotha, sur la théorie des probabilités, appliquée aux sciences morales et politiques* (Brussels: Hayes, 1846), cited in Stigler, *History of Statistics*, p.220.

21 *Ibid.*, p.215.

22 *Ibid.*, p.218. When Bertillon's data were re-examined three decades later, however, it appeared that the results were an artifact of the analysis; Bertillon had converted the measurements from centimetres to inches which altered the groupings of the original set of data.

Galton had learnt of Quetelet's method by 1863.[23] Much of Galton's work did not involve curve-fitting, *per se*, instead he compared his calculated values, to a normal probability table.[24]

In 1877, Wilhelm Lexis devised the *Lexican Ratio L*, as a goodness-of-fit test to determine if an empirical distribution conformed to the normal distribution.[25] Some of Lexis's statistical work was influenced by Quetelet's use of the normal cuve. When Lexis fitted the normal curve to an empirical distribution of age at death, he noted that there was an observed surplus of early deaths which he classified as 'premature deaths': deaths that fit the normal curve were classified as 'normal deaths'.[26] In the following year, Luigi Perozzo made one of the first attempts to analyse graphically a skew bivariate distributions and to give a formula from marriages in Italy from 1878-9. Perozzo, however, gave no underlying theoretical basis for his formulae nor did he fit them to his observations.[27] In the same year, a measure of a goodness of fit was also suggested by the American statistician, Erastus Lyman de Forest (1834-88). De Forest undertook a project with the Knickerbocker Life Insurance Company in New York where he encountered 'the problem of smoothing mortuary or life tables'.[28] Stigler remarked that when De Forest suggested a

> test of fit based on the number of permanences of signs in the residuals, De Forest provided no exact distribution theory; he did not attempt a combinatorial theory of runs. Rather he provided approximates to the mean and probable error of his statistic, based on an unproved assumption that asymptotically the number of permanences among the residual's sign behaved as would a like statistic based on tosses of a fair coin.[29]

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23 Galton, *Memories*, p. 304; Stigler, *History of Statistics*, p.205.

24 See, for example, *idem*, 'Co-relations and Their Measurement, Chiefly from Anthropological Data', *Proceedings of the Royal Society*, 45 (1888), 135-45.

25 Wilhelm Lexis, *Zur Theorie der Massenerscheinungen in den menschlichen Gesellschaft, Freiburg*: Wagner, as cited in C.B. Davenport and Merle P. Ekas, *Statistical Methods in Biology, Medicine and Psychology* (John Wiley: New York, 1936), pp.46-7.

26 Stigler, *History of Statistics*, p.223.

27 S.J. Pretorious, 'Skew Bi-variate Frequency Surfaces, examined in the Light of Numerical Illustrations', *Biometrika*, 22 (1930), 121.

28 Erastus de Forest, 'On the Grouping of Signs of Residuals', *Analyst*, 5 (1878), 65-72. *Idem.*, 'On an Unsymmetrical Probability Curve', *Analyst*, 9 (1882), 135-42, 161-8: 10 (1883), 1-7, 67-74, cited in Steven M. Stigler, 'Mathematical Statistics in the Early States' (Special Invited Paper), *The Annals of Statistics*, 6(2), 254-55.

29 *Ibid..*, 258.

De Forest began by using the normal curve; however, when he considered the limiting properties of unsymmetric distributions, he was led to a new distribution which he called the 'gamma curve'.[30]

Pearson first learned of the work of De Forest in 1895 when his mathematical tutor from Cambridge, William Herrick Macaulay, sent him some of De Forest's papers. Pearson then sent a letter to *Nature* on 1 August 1895 acknowledging De Forest's work and on 18 August he returned the papers to Macaulay. In his letter to Macaulay, Pearson wrote that

> I do not think I should have altered my work had I known of De Forest's, but should of course have acknowledged his priority as far as skew curve of Type III is concerned. I have cited a useful table he gives, which I have copied out. I hope I have not kept the books too long, but they have been a little difficult to sit down to, the last week. I am very much obliged for them.[31]

Francis Ysidro Edgeworth encountered the work of Lexis sometime in mid-1885 and dealt with his work in 'Methods of Statistics'. Two years later he published an article on testing the goodness of fit in 'The Empirical Proof of the Law of Errors'.[32] In this paper, Edgeworth described 'how empirical distributions, such as those of bank reserves or price changes could be examined to see if they supported the hypotheses of normality'.[33] Stigler noted that Edgeworth

> proposed first testing symmetry, then determining whether or not the normal distribution provided the best fit among a limited class of symmetric curves.. As a test of goodness of fit Edgeworth recommended breaking the range of the data into two classes (the points of division depending upon the particular alternative interest), and hypothesis of normality. The test, based on a normal approximation to a binomial distribution, is formally equivalent to a χ^2 test for this simple situation, although later correspondence with Karl Pearson indicates that Edgeworth did not know how to extend the test to counts of more than two classes.[34]

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30 *Ibid.*, 259.

31 Karl Pearson, Letter to William Herrick Macaulay, 18 August 1895, KP:UCL, pp.1-2; *idem,* 'On Skew Probability Curves', *Nature*, 52 (1 August 1895), 317.

32 F.Y. Edgeworth, 'Methods of Statistics', *Jubilee Volume of the Statistical Society* (1885), pp. 181-217. *Idem..*, 'The Empirical Proof of the Law of Error', *Philosophical Magazine* (Fifth Series), 24, 330-42, cited in Steven M. Stigler, 'Francis Ysidro Edgeworth, Statistician' (With Discussion), *Journal of the Royal Statistical Society*, Series A (General), 141, Part 3 (1978), 287-322.

33 *Ibid.*, 298.

34 *Ibid.*.

When Pearson first confronted asymmetrical distributions in 1893, he recognised that an 'objective method of measuring the goodness-of-fit had to be found' for distributions that did not conform to the theoretical normal curve.[35] Such distributions were suggested to Pearson initially by Weldon's biological data from measurements of crabs and subsequently, by Edgeworth's economical data.

Weldon's data, in particular, would be used extensively in all of Pearson's papers leading to the development of the chi-square goodness-of-fit test and in the 1900 paper itself—whether this came from his extensive collection of measurements from the carapace of crabs and prawns or from his exhaustive collection of dice-tossing and coin-throwing data. Weldon's dice were exemplary for Pearson when he was working on theoretical issues of probability in his series of Gresham lectures on 'The Geometry of Chance' in November of 1892 (and in particular for his lecture on 'The Normal Curve').[36] The dice calculations were used in his four lectures of January 1893 on 'The Logic of Chance I', 'The Logic of Chance II', 'Roulettes' and 'Teetotums'. The data were used again in 'The Chances of Death' and 'Science and Monte Carlo' published in the *Fortnightly Review* in 1894.[37]

Pearson's earliest consideration of determining a measure for the goodness-of-fit came out of his lecture on 'The Normal Curve' in November 1892 when he asked his students, 'Can you always fit a normal curve to fit a set of data?'[38] In his first lecture on 'The Normal Curve', he remarked that

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35 Egon Pearson, 'Some incidents in the early history of biometry and statistics. 1890-1894.' *Nature*, 52 (1965), 3-18. Also reprinted in E.S. Pearson and Sir Maurice Kendall, *Studies in the History of Statistics and Probability*, Vol. 1 (Charles Griffin: London, 1970), 329. All page references here and subsequently are to the reprint in Pearson and Kendall.

36 The first two sets of Pearson's Gresham lectures on the 'Geometry of Statistics' in 1891 and 'The Geometry of Chance' in 1892, were published later in the Grammar of Science. The extant collection of these lectures (from November 1891 to May 1894), are among the papers and correspondence of Karl Pearson at University College London.

37 Pearson's *Fortnightly Review* papers were reprinted later in his two volume *Chances of Death and other Studies in Evolution* in 1896.

38 Pearson, 'The Normal Curve' (Part 1), Gresham Lecture, November 1892, KP:UCL, p.7 and 'Conclusions to the Normal Curve', Gresham Lecture, November 1892, KP:UCL, p.3.

> a number of frequency curves arise from which [the] asymmetrical character does not hold. Sometimes ... the want of fitting the normal curve is quite obvious on inspection. In other cases we want a refined investigation to detect it.[39]

In his first paper on the 'Contributions to the Mathematical Theory of Evolution', he illustrated Weldon's data with four different plates—each plate represented an attempt to find the best fit between observed values and theoretical values using the first five moments and the fundamental nonic. Figure 6.1 depicts one of Weldon's four plates that Pearson used in this paper which reflects a very close fit between the theoretical curve and the empirical curve (the entire set was first shown in Chapter III). Pearson then noted that a frequency curve calculated from low moments fits best near the centre and curves calculated from higher moments fits best near the tails of the observation curve. After Pearson fitted the four distributions he concluded that

> it would [have been] difficult to say which of these results [gave] the best fitting theoretical curve. For statistics of this kind they are sensibly the same. Thus till another method of attacking the problem of the resolution of asymmetrical curves is propounded. I think there is not sufficient evidence against the use of the higher moments to lead us to discard a method based upon them as essentially likely to lead to large errors.[40]

Later in the year, Pearson produced the forerunner to his chi-square goodness-of-fit test. In one of Pearson's first statistics lectures at UCL in 1894, he discussed cuve-fitting and showed how data could be fitted to the normal curve by using the method of moments. Though Galton attempted to fit data to a normal curve by using his median and quartile measures, it seemed to Pearson that a 'rough test of a goodness of fit of a normal curve to an observation-polygon was a desideratum'.[41] He then demonstrated how to find Σ s/y where s equals the difference between the observation polygon and the theoretically expected curve and y equals its corresponding ordinate. As this measure was 'awkward to get', he thought it would be preferable to measure the ratio of the whole area between the theoretically expected curve and the polygon (of observational data); he counted all the values as positive

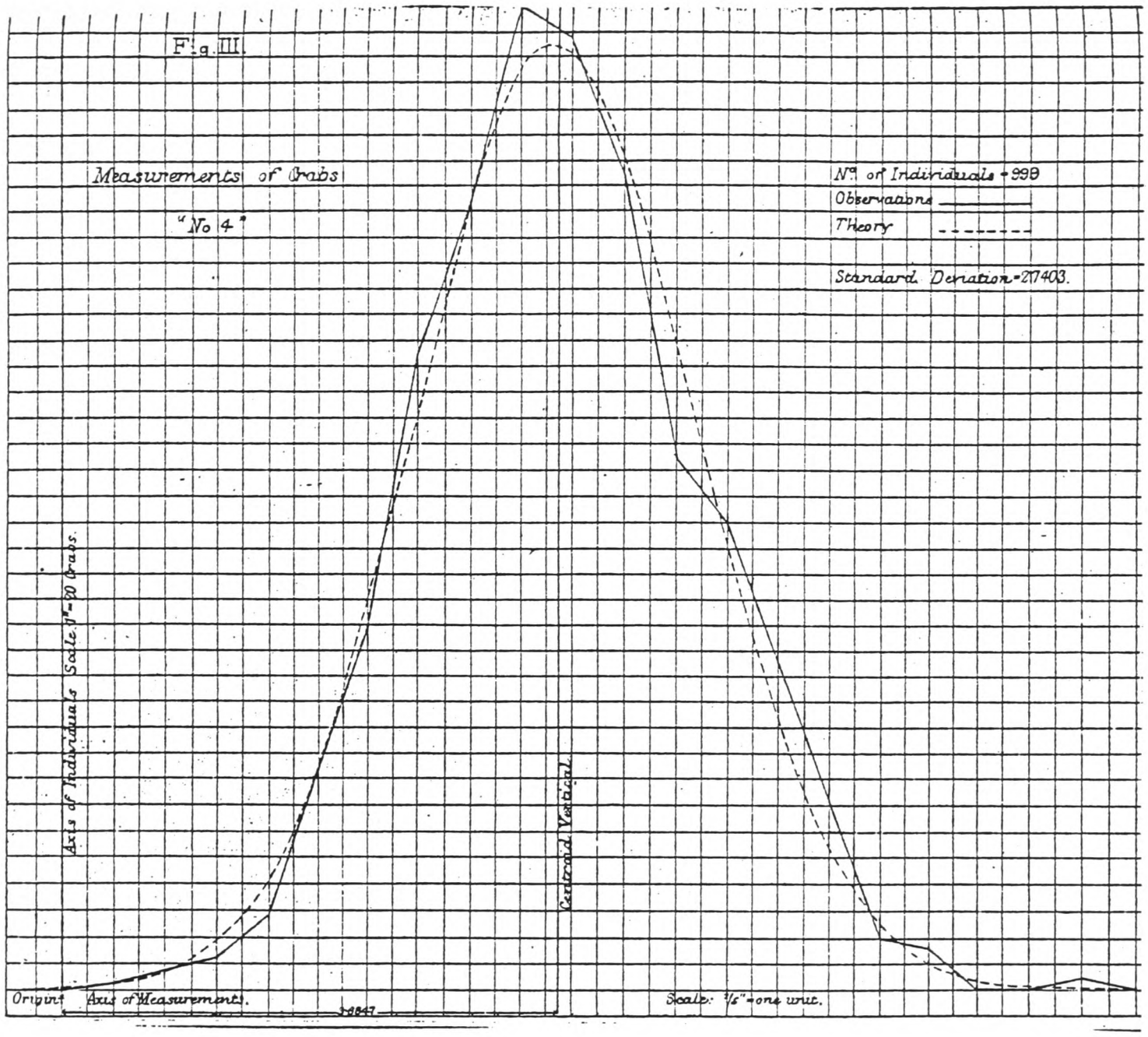
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39 *Ibid.*, p.2.

40 Karl Pearson, 'Contributions to the Mathematical Theory of Evolution', *Philosophical Transactions of the Royal Society*, A, 185 (1894), 120.

41 G. Udny Yule, 'Notes of Karl Pearson's Lectures: Mathematical Contributions to the Theory of Evolution, 1894-1895', vol. 1, KP:UCL, pp.81-2.

Figure 6.1
MEASUREMENTS OF WELDON'S CRAB DATA TO FIT PEARSON'S THEORETICAL CURVE (1894)



which equals W and then measured the total area under the curve which equals A. Hence,

$$W / A = \frac{\Sigma \text{ errors of fit}}{\Sigma \text{ ordinates}} = \frac{\Sigma s}{\Sigma y}$$

which was a 'fairly reasonable measure of a "goodness of fit"'.[42] Egon Pearson expressed this equation as: $R = \Sigma [O-T]/T$ as an empirical measure for the goodness-of-fit where Σ = Summation, O = observed frequencies, T = theoretically expected frequencies for empirical and theoretical distributions respectively.[43] The computational process in this formula (6.1) is quite similar to his χ^2 goodness-of-fit test in 1900 (6.2), and to the subsequent 1904 test of independence for contingency tables (6.3). R.A. Fisher's modification by degrees of freedom of what he termed the 'chi-square statistic' to test the 'discrepancy' between observed and expected values in 1925 is shown in (6.4) and the contemporary formula is shown in (6.5).

PEARSON'S FIRST MEASURE OF A GOODNESS-OF-FIT TEST:

1894: $$W / A = \frac{\Sigma \text{ errors of fit}}{\Sigma \text{ ordinates}} = \frac{\Sigma s}{\Sigma y} \qquad (6.1)$$

S = the difference between the observed frequencies and the theoretical curve
y = its corresponding ordinate

THE CHI-SQUARE GOODNESS-OF-FIT TEST:

1900: $$\chi^2 = S\frac{(m-m')^2}{m}$$ 'correction' = n-1* (6.2)

S = Summation
m' = observed frequencies from a distribution
m = theoretical frequencies from a distribution known *a priori*
m+μ = theoretical frequencies from a distribution not known *a priori*
*'correction' is one of the terms Pearson used for 'degrees of freedom'.

THE CHI-SQUARE TEST OF INDEPENDENCE FOR CONTIGENCY TABLES:

1904: $$\chi^2 = S\frac{(n_{uv} - v_{uv})^2}{V_{uv}}$$ 'correction' = $(x-1)(\lambda-1)/N-\phi^2$ (6.3)

S = Summation

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42 *Ibid.*, p.84.
43 Egon Pearson, 'Early History of Biometry and Statistics, 1890-1894', p.341.

n_{uv} = observed cell frequencies 'which corresponds to m'
v_{uv} = theoretical cell frequencies 'which corresponds to m'
x = number of rows
λ = number of columns

R.A. FISHER'S CHI-SQUARE STATISTIC MODIFIED BY DEGREES OF FREEDOM:

1922: $$\chi^2 = \sum \frac{(f-y)^2}{y} \qquad df = (r-1)(c-1)+1 \qquad (6.4)$$

Σ = Summation
f = observed cell frequency
y = corresponding theoretical cell frequency
r = number of rows
c = number of columns

CONTEMPORARY:

$$\chi^2 = \sum \frac{(O-E)^2}{E} \qquad df = (r-1)(c-1) \qquad (6.5)$$

Σ = Summation
O = observed cell frequencies
E = theoretically expected cell frequencies

Pearson broadened the scope of his examples for testing the goodness-of-fit in his 1895 paper on skewness when he introduced the Pearsonian Family of Curves as the next step in finding a measure of the goodness-of-fit for skewed distributions. He not only looked at Weldon's data (see Figure 6.2 which shows the right antero-lateral margin of 999 female Naples crabs and Figure 6.3 for number of dorsal teeth-rostrum of 915 prawns), but he also looked at economic data, vital statistics and botanical statistics such as the 'frequency of variations in petals or other characteristics of flowers... found in Herr H. DeVries buttercups'.[44] Pearson would use the observations from DeVries buttercups data again for his second example in his 1900 chi-square paper for distributions that could not be determined *a priori.*

In 1887, DeVries transferred several plants of *Ranunculus bulbosus* to his flower garden and counted the petals of 222 of their flowers in the following year. DeVries established these frequencies:

44 Pearson, 'CMTE. II. Skew Variation on Homogeneous Material' (1895)', 401.

Figure 6.2
SKEWED DISTRIBUTION OF RIGHT ANTERO-LATERAL MARGIN OF 999 FEMALE CRABS FROM WELDON (1895)

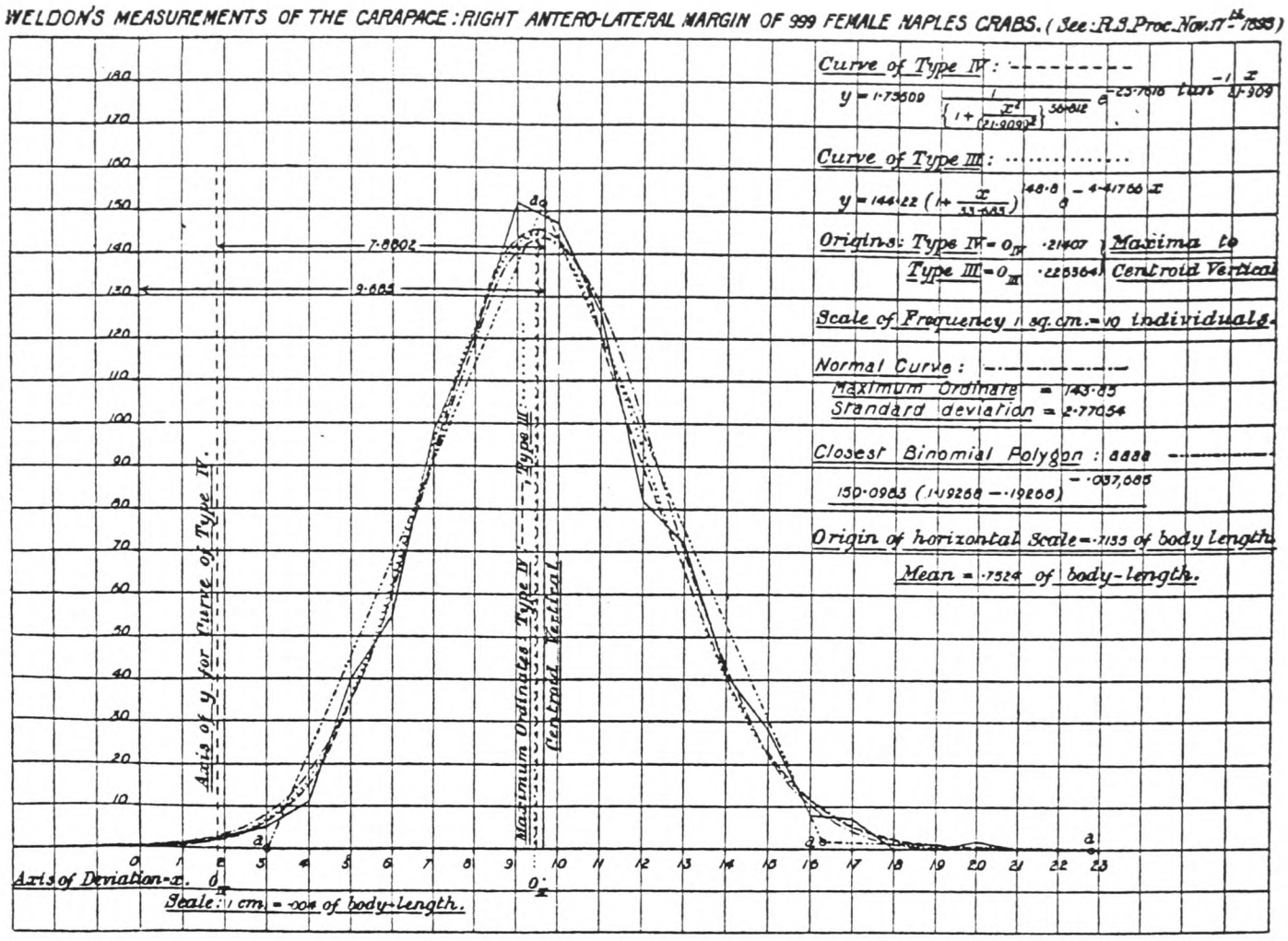
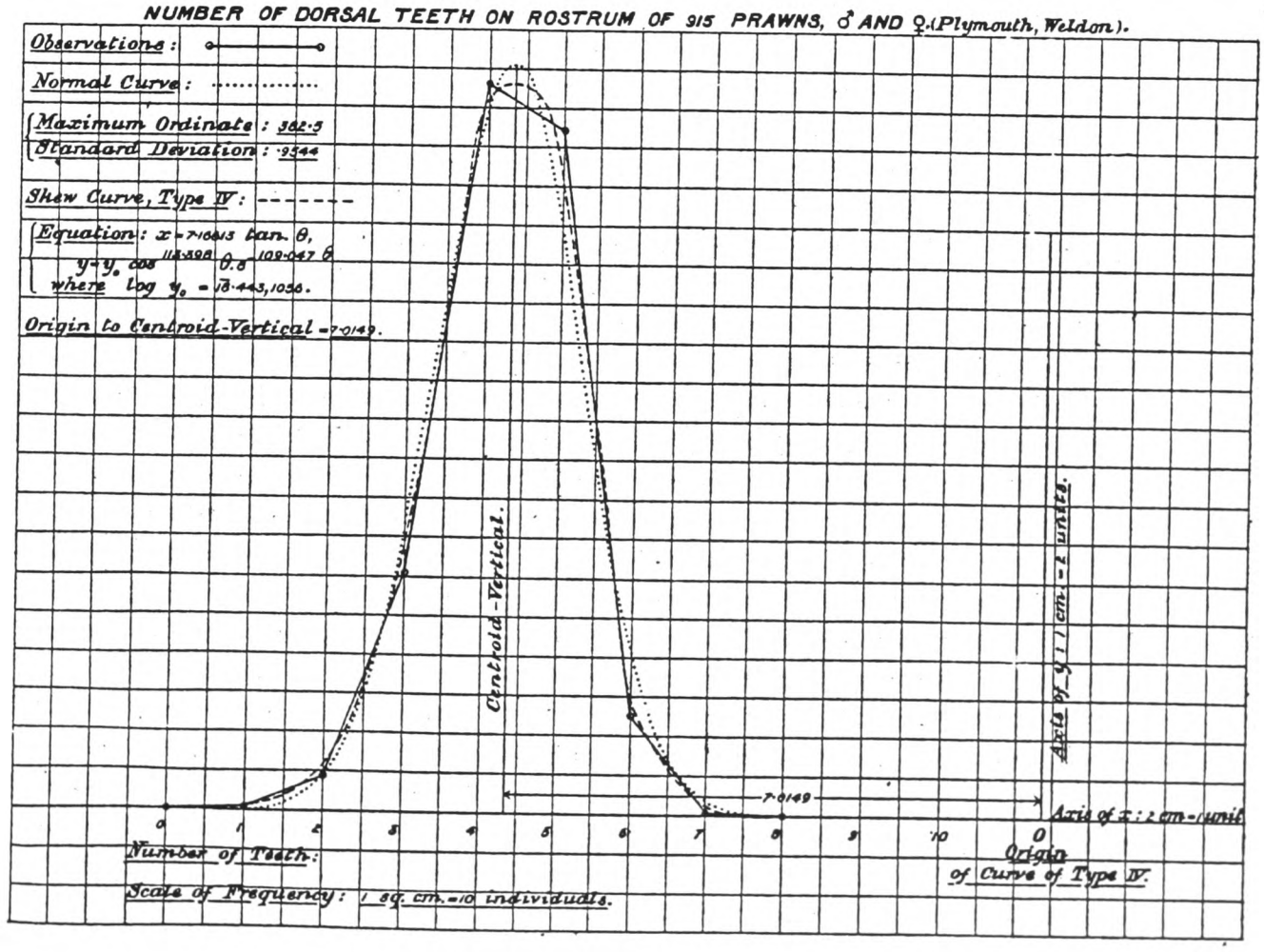


Figure 6.3
SKEWED DISTRIBUTION OF DORSAL TEETH-ROSTRUM OF 95 PRAWNS FROM WELDON (1895)



Number of Petals	5	6	7	8	9	10
Frequency	133	55	23	7	2	2

Pearson noted that this series proceeded 'by discrete units, and correspond[ed] probably to a hypergeometrical series'. With the disproportionate number of flowers that had five petals, he thus thought that a skew frequency curve could best represent this set of data. The range of the buttercups started with 5 petals and ended with 10; Pearson took the origin to be 4.5 and 'determined the constants to a second approximation' by the method of moments. This resulted in:

$$y = .211225x^{-.322}\,(7.3253-x)^{3.142}$$

a curve of Type 1 with a limited range, the asymptotic ordinate being 4.5 petals. He then calculated the areas and found these results:

Number of Petals		5	6	7	8	9	10	11
Frequency	Theory	136.9	48.5	22.6	9.6	3.4	.8	.2
	Observation	133	55	23	7	2	2	0

The results were shown by using a continuous curve and also with a histogram (see Figure 6.4). Hence, the agreement was found to be satisfactory 'considering the paucity of the observations'. The two points on the observation curve corresponding to five and six petals were 'deduced from the areas given by the statistics by the same percentage reduction as converts the theoretical areas into the ordinates of the theoretical curve'.[45] Other skew frequency curves were found in house evaluations (see Figure 6.5), pauperism (Figure 6.6) and barometric heights (Figure 6.7) and subsequent theoretical curves were fitted to the empirical data.

The 'absolute necessity of skew curves [were] all questions of vital statistics'. This was sufficiently evidenced in his resolution of the general mortality curve,

45 *Ibid.*.

Figure 6.4

CONTINUOUS CURVE AND HISTOGRAM FROM DEVRIES' BUTTERCUPS (1895)

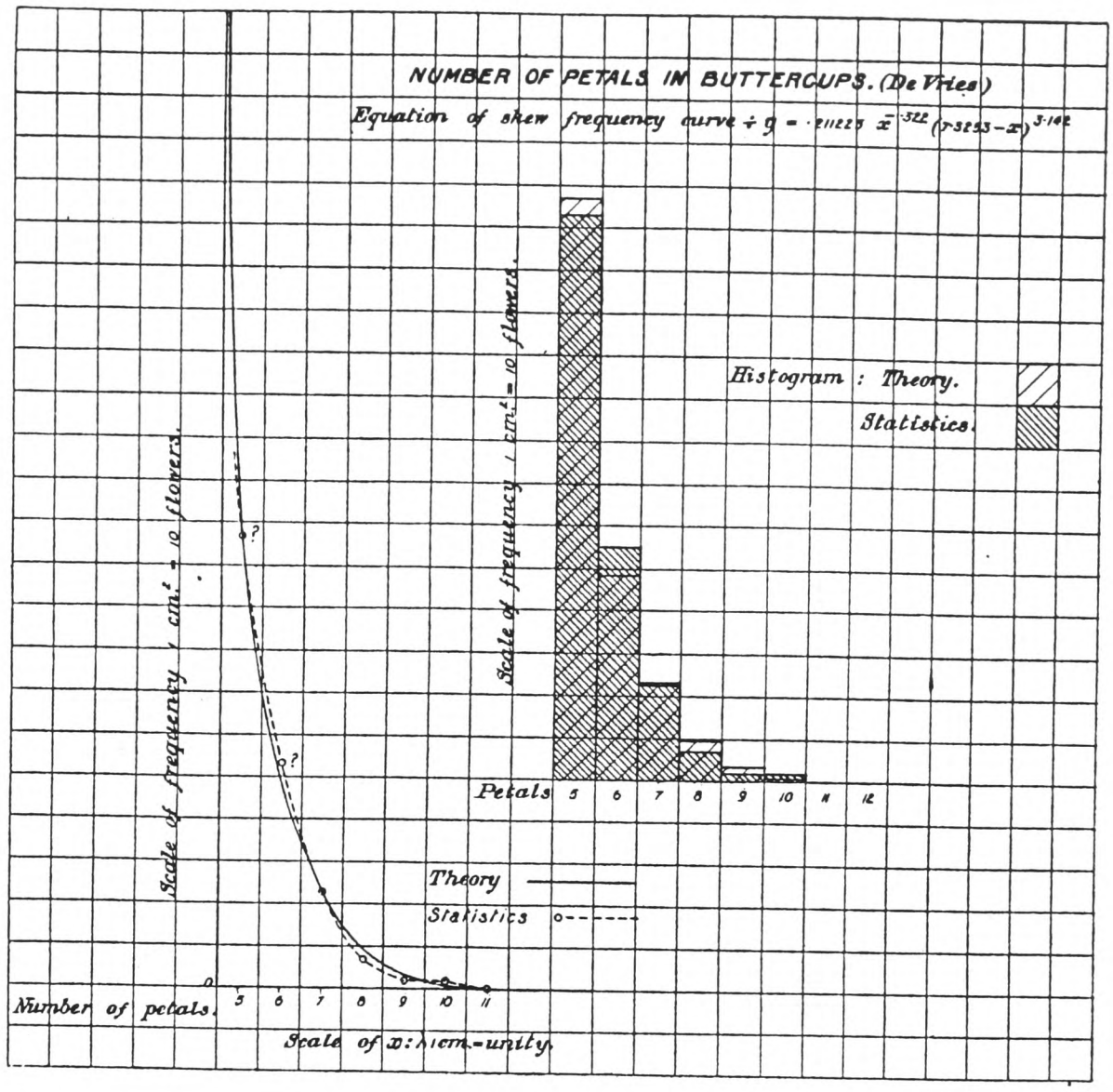
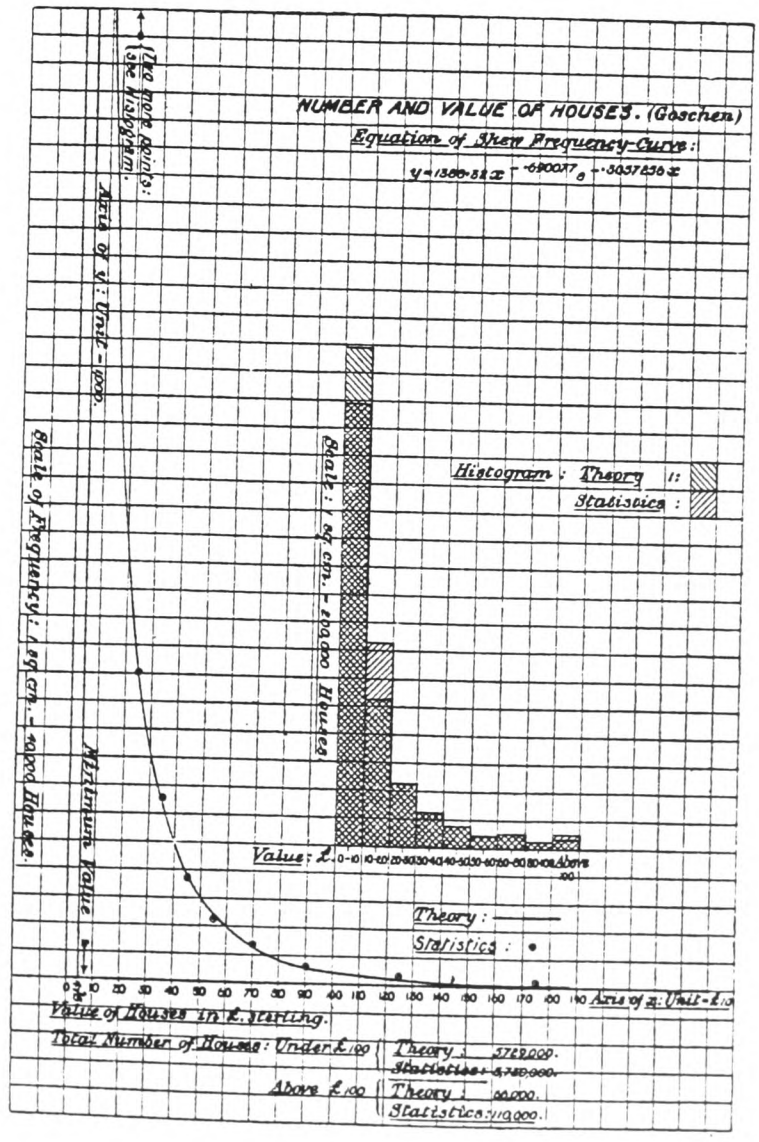


Figure 6.5

PEARSON'S SKEW CURVE OF HOUSE EVALUATIONS (1895)



especially that of infant mortality and old age mortality which will be discussed in greater detail in the section on 'The Chances of Death'.

THE RELATIVE FREQUENCY APPROACH TO PROBABILITY

Whilst the thrust of Pearson's statistical innovations centred around curve-fitting for asymmetrical and symmetrical empirical distributions in his attempts to find a measure of the goodness-of-fit, this work came to fruition when he incorporated probability theory into the corpus of his statistical theory. This work on probability theory, which he undertook whilst he was looking for a measure of the goodness-of-fit test, provided the material for sixteen of his Gresham Lectures which he delivered four times a term from November 1892 to November 1893.

The type of data that Pearson used in these lectures came from such games of chance involving cards, coins, dice and Monte Carlo roulette. He gave this work greatest public exposure with his lecture at Leeds on the 'Chances of Death' in January 1895. It will be argued that this relationship between chance and death led to a new conception of probability in statistical theory in Pearson's development of the relative frequency approach to probability. Subsequently, these lectures would play a role in his work on natural selection when determining differential mortality rates.

One of Pearson's earliest readings of probability came from Edgeworth's Newmarch Lectures 1892.[46] In these early lectures, Pearson referred to work of Edgeworth in 'Roulettes' and 'Laws of Chance I' (two of his Gresham lectures on probability delivered in January 1894).[47] During this time he also read the works of Augustus DeMorgan, John Stuart Mill and John Venn.[48]

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46 Edgeworth gave his first set of Newmarch lectures on 11 May 1892. (See Stigler, *History of Statistics*, p.316). The Newmarch lectures were established in 1883 when a fund was raised in memory of William Newmarch which was then transferred to University College London. The stipendiary 'was to deliver in each year at least six lectures on political economy as illustrated by statistics'. H. Hale-Bellot, *University College London 1826-1926* (University of London Press, 1929), p.386.

47 Pearson, 'Roulettes', Gresham Lecture, January 1894, KP:UCL, pp.19-20: 'Laws of Chance I', Gresham Lecture, January 1894, KP:UCL, p.44.

48 *Ibid.*

Figure 6.6
PEARSON'S SKEW CURVE OF PAUPERISM (1895)

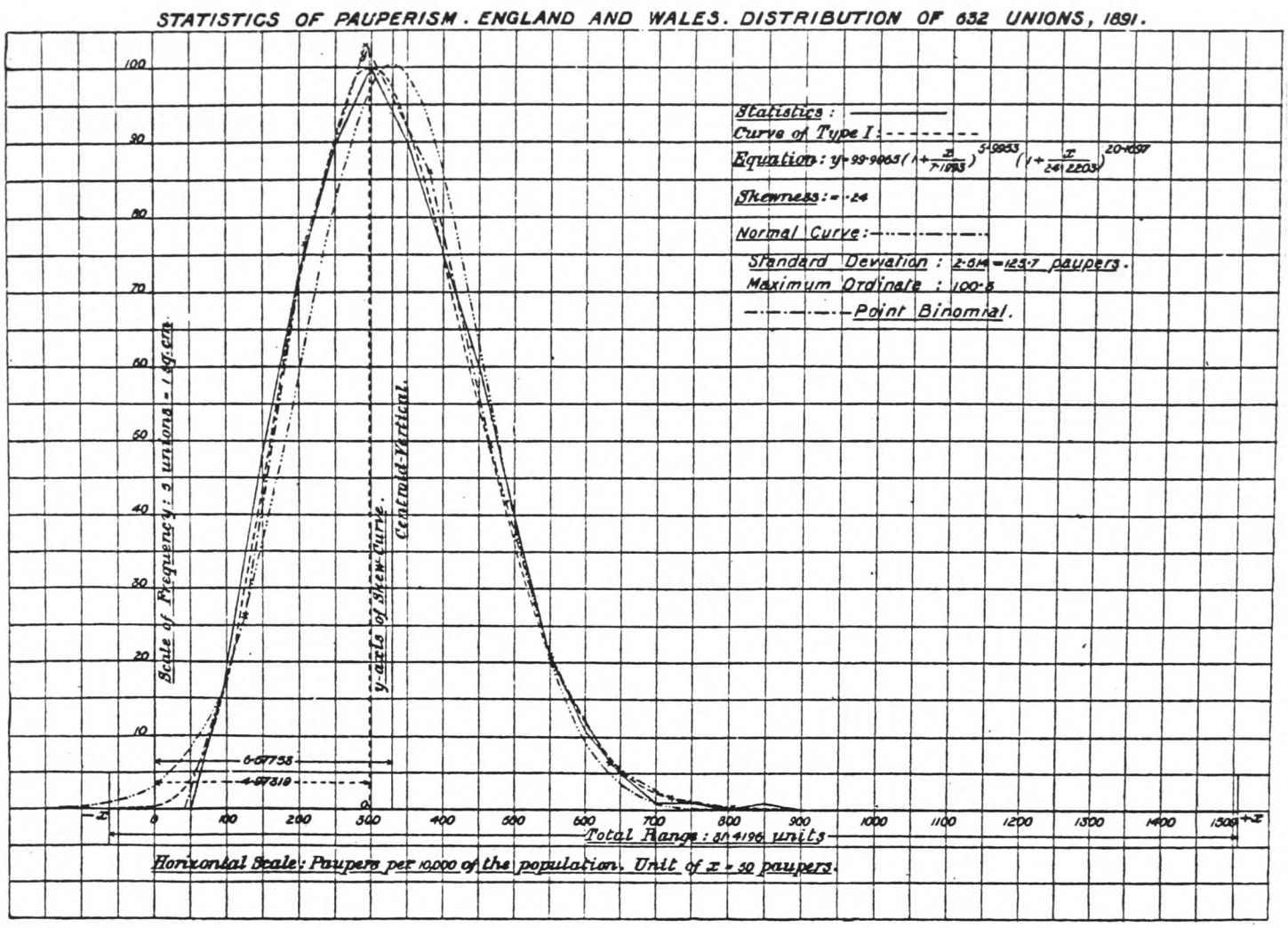
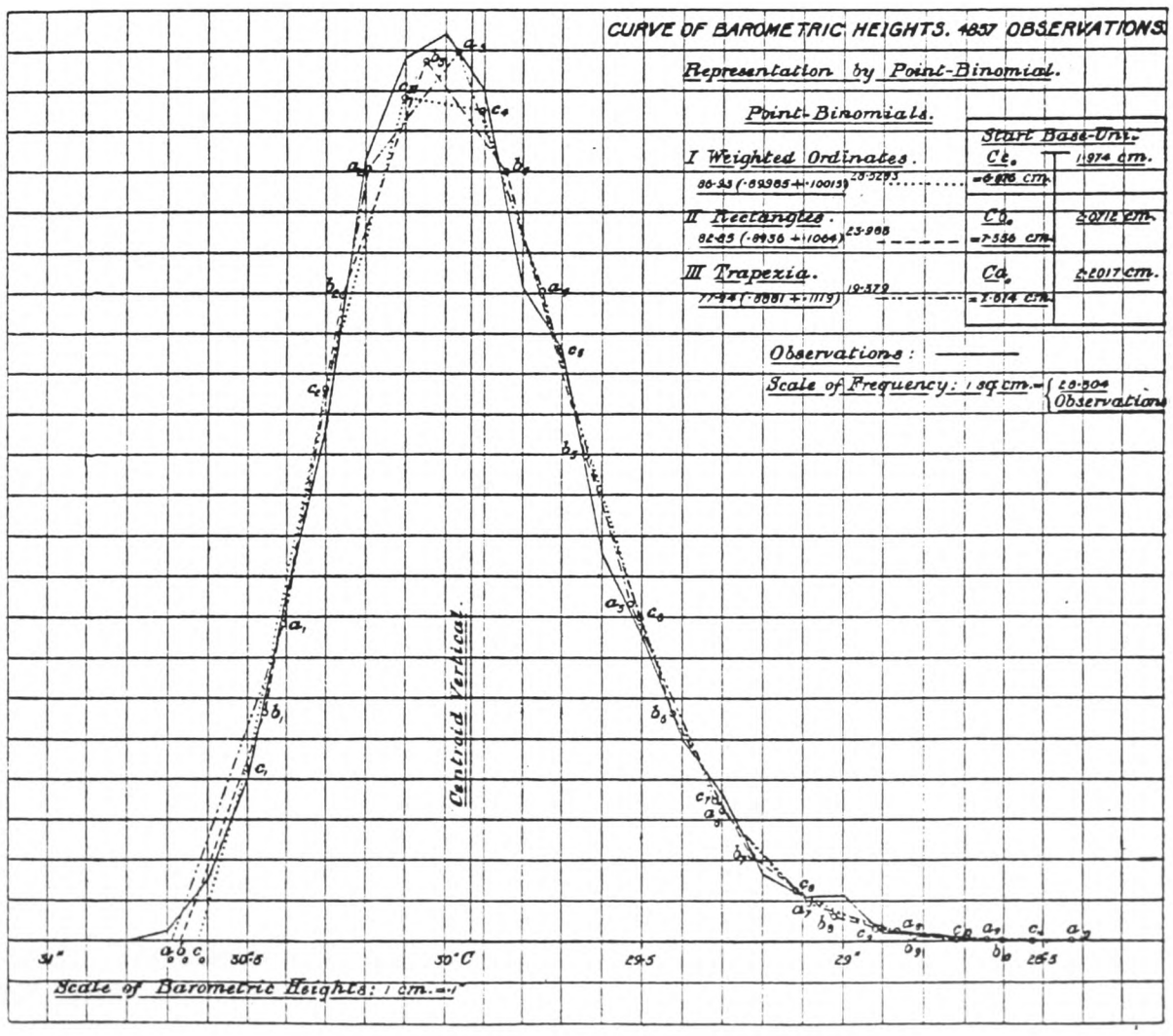


Figure 6.7
PEARSON'S SKEW CURVE OF BAROMETRIC HEIGHTS (1895)



A year after he delivered his Hilary term lectures in January 1893 on 'The Logic of Chance', in which he used a fair amount of data from his returns from Monte Carlo Roulette, he published his first theoretical paper on probability in 'Science and Monte Carlo' in 1894.[49] This lecture on 'Science and Monte Carlo' formed one of his series of popular lectures on chance and was 'especially designed to introduce the audience to theorems of probability'.[50] Though written ostensibly about gambling, his aim was to indicate the considerable importance of developing 'probabilistic statistics' for purposes of evolutionary biology.

The Monte Carlo roulettes gave Pearson a way to demonstrate that the scientific theory of chance when observed in natural phenomena did not fit into the well ordered view of chance as was conceived by natural philosophers. Whilst the 'moralist' had

> boldly asserted that the service of the goddess of Chance leads to a complete demoralisation of her worshippers from the high priest downwards, the mathematician [had] based their arguments on the fact that the individual with a limited fortune plays against the public with an almost unlimited one, or against a banker with a small but persistent advantage. The mathematicians, however, start from the hypothesis that gambling is chance—chance defined in their own perfectly clear and definite sense.[51]

The idea of chance to the natural philosopher was not based on the idea of determining the empirical outcome of any individual event (e.g., the spin of a teetotum), but it was determined instead by

> his knowledge of the percentages of successes and failures which are sure to occur in a considerable number of trials... The scientific conception of chance is that of a measure based on experience; a knowledge of the average results of many events is used to replace ignorance of the result of any individual event.[52]

Pearson's conception of chance would help to bring about a new approach to using probability in statistical theory. Although the philosopher George Boole had arrived at a 'frequentist interpretation of probability' in 1850, it was Pearson who

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49 Pearson, 'Science and Monte Carlo', *Fortnightly Review*, 50 (February 1894), 183-93. This was reprinted in vol. 1 in the *Chances of Death and Other Studies in Evolution* under the title 'The Scientific Aspects of Monte Carlo Roulette'.

50 Pearson, *Chances of Death and other Studies in Evolution* (Edward Arnold: London, 1897), p.vii.

51 Pearson, 'The Scientific Aspects of Monte Carlo Roulette', *Chances of Death*, p.42.

52 *Ibid.*, p.43.

would be instrumental in advancing the relative frequency approach of probability. Boole argued that the rules employed

> in life-assurance, and in the other statistical applications of the theory of probabilities, are altogether independent of the *mental* phenomena of expectations. They are based on the assumption that the future will bear a resemblance to the past; that under the same circumstances the same event will tend to recur with numerical frequency; not upon any attempt to submit to calculation the strength of human hopes and fears.[53]

The mathematical formalisation of the relative frequency approach to probability came about when Pearson argued for the necessity of determining empirical observations without relying solely on formal mathematical models and by establishing a more rigorous and systematic form of significance testing.[54] Before examining his work on probability in any further detail, it may be helpful to consider briefly the different probabilistic approaches which can be classified as subjective, formal mathematical and empirical.

The subjective approach, which can also be termed the *Degree of Rational Belief*, is used subjectively or intuitively to refer to an attitude of doubt with respect to some future event. For example, a person may say, 'it will probably rain tomorrow' or 'the probability is high that a particular horse will win at Royal Ascot'. In these situations probability may be assessed through a betting scheme which is based on what the person thinks the probability of a certain outcome will be. The idea is to locate probability where it is in the *mind of the observer* rather than in the outside world. This approach is neither hypothetical nor operative. (One of the difficulties that arises with this approach is that people with equal knowledge and skills can come to different and sometimes conflicting answers.) In his lecture on the 'Laws of Chance II'

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53 George Boole, *An Investigation of the Laws of Thought on Which are Founded the Mathematical Theories of Logic and Probability* (1854), p. 2 (cited in Porter, *The Rise of Statistical Thinking*, p.80).

54 The idea of significance tests has been traced back to John Arbuthnott, 'An Argument for Divine Providence, taken from the Constant Regularity observ'd in the Births of both Sexes', *Philosophical Transactions of the Royal Society*, 27 (1710), 186-90. (See Stigler, *History of Statistics*, pp. 225-6. Stigler also found that in 1852 and 1878 Charles Sanders Pierce published a significance test designed to tell an investigator whether an outlier should be rejected and that from 1874-7, De Forest discussed tests that were analgous to 'large sample significance tests based on the magnitude of the residuals', in Stigler, 'Mathematical Statistics', p.246 and 256.

in January 1893, Pearson defined *subjective chance* as 'the chance based upon the knowledge and past experience of any individual' and *objective chance* was 'the accurate statistical measurement of chance based upon as definite a class of similar events as can be obtained'.[55]

Pearson also made distinctions between *artificial* and *natural* events to indicate the 'difference [between] chance associated with events under human control and chance independent of any human agency'.[56] The first included games of chance (i.e., coins, cards, roulettes, teetotums and lotteries) and the second, were 'accidents' produced by human nature. Of natural events this included:

> anthropological measurements... there is [also] chance in matters of life, death, birth and marriage (i.e. vital statistics) which [led] to problems of insurance and annuities... [he next considered] natural events in relation to other forms of life than human, in particular the measurements of the organs of living form, zoological measurements. This as Professor Weldon has insisted is the keynote to the scientific measurement of the forces of evolution.[57]

His example included 'games of chance, the distribution of human heights, birth rates and Professor Weldon's measurement on shrimp'.[58]

The *Mathematical Approach* or the 'logical definition of probability', is based on hypothetical situations and in particular on 'games of chance'. The earliest uses of games of chance have been traced to the beginning of the first millennium.[59] A formal use of mathematics in games of chance did not, however, emerge until the seventeenth century from the work of Jacob Bernoulli (1654-1705). The binomial distribution had been derived some time around 1713 which was published in Bernoulli's posthumous treatise, *Ars Conjectandi*.[60] A binomial can be expressed as

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55 Pearson, 'The Laws of Chance II', Gresham Lecture, January 1893, KP:UCL, Janaury 1893, p.2.
56 Pearson, 'The Laws of Chance I', Gresham Lecture, Janaury 1893 KP:UCL, p.4.
57 *Ibid.*.
58 *Ibid.*, p.6.
59 F.N. David, *Games, Gods and Gambling: A History of Probability and Statistical Ideas* (Griffin: London, 1970), p.6. Her examples include dice made from crystal, ivory, sandstone, ironstone and wood.
60 See Stigler, *History of Statistics*, Chapters 1-4, who has written extensively on the development and growth of probability during the eighteenth and nineteenth centuries. Lorenz Krüger, Lorraine J. Dalton, and Michael Heidelberger, *The Probabilistic Revolution*, volume 1, *Ideas in History* and Lorenz Krüger, Gerd Gigerenzer, and Mary S. Morgan, *The Probabilistic Revolution*, volume 2, *Ideas in the Sciences* (MIT Press: Cambridge, Mass., 1990).

$(p + q)^n$ being the probability of two possible outcomes (e.g. p = heads and q = tails in a coin): the binomial expansion for two different combinations of outcomes = $p^2 + 2pq + q^2$. By the end of the eighteenth century, DeMoivre, LaPlace and Gauss derived the normal curve which is itself related to the binomial distribution (i.e., as *n* approaches infinity, the binomial approaches the normal distribution as its limit).[61] Hence, the framework of a formal mathematical definition of probability had been established.

To some extent, this approach involves a degree of intuitive plausibility in relation to some specific events found in cards, dice and coins. As this approach does not deal with real world objects, it is seen primarily in theoretical terms. This approach thus depends on having determined an *a priori* knowledge of the frequencies of a specific number of particular outcomes (or what may be termed 'the favoured event') and of the total number of possible outcomes (or of all events, including the favoured one) and all of its alternatives in the situation. Probability then becomes the statistical ratio between the number of *n* particular outcomes and the number of *n* possible outcomes.[62] Hence mathematical probability

$$= \frac{\text{number of particular outcomes}}{\text{total number of particular outcomes}}.$$

In this way one simply hypothesizes certain conditions and then works out the resultant probability. For example, one may hypothesize that a coin is unbiased (i.e., a coin comes up equally heads and tails) and attempt to determine the probability of getting various combinations of heads and tails in a number of flips in a coin. Pearson explained that the limitations to this approach arose as

> the laws of chance [were based on] a purely intellectual axiom, then [the] theoretical results [would] be solely justified by their agreement with facts. This appear[ed] to

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61 In the early middle part of the nineteenth century Poisson devised the exponential distribution. Pearson, *History of Statistics*, pp. 646-55; R.A. Fisher, 'On a distribution yielding the error functions of several well known statistics', *Proceedings of the International Congress of Mathematics*, 12 (1924), 805; Stigler, *History of Statistics*, p.183.

62 J.P. Guilford and Benjamin Fruchter, *Fundamental Statistics in Psychology and Education.* (McGraw Hill: New York, 1978), p.100. John David Yule (ed.), 'Probability', *Metheun Concise Encyclopedia of Science and Technology* (Methuen: Sydney, 1979), p.458.

> have been the method adopted by the founders of the theory of probability. They did not appeal to experience, but to what appeared to them intellectually obvious postulates.[63]

With Pearson, we come to the *Relative Frequency Approach*, which permits one to find out about the real world. Whereas the mathematical model approach *hypothesizes* what the outcome should be, the relative frequency approach finds the outcome by performing actual *operations*. Pearson was interested in determining an approximation of the relative frequency of a series of real events rather than hypothesizing what the probability of these events might be. This interest formed the basis of his paper on 'The Scientific Aspect of Monte Carlo Roulettes'.

The difference between these two approaches may be exemplified by determining the probabilities of the outcome in coin-tossing (the ratio of heads to tails in 100 tosses). If the mathematical approach is used, there would be a degree of uncertainty in determining the number of times necessary to flip a coin or roll a set of die for that to be an adequate test. For example, if a coin is flipped 100 times, this could produce a ratio of 60 heads and 40 tails but, it would be very highly unlikely that the same combination would arise in the next 100 flips of a coin: the probabilities would be changing continually. One way to determine the probabilities, albeit tedious and time-consuming, would be to flip a coin for some 20,000 times (as Pearson and Weldon had done on two separate occasions). Rather than repeating the 20,000 flips each time for each new problem, another approach was to reconceptualise probability as the 'Limit of ratio' or the relative frequency ratio (i.e., as the number of trials approaches infinity, the total number of heads will approach 0.5 as its limit: this limit is called the probability). Probability is then defined as the limit approached by the relative frequency as the number of trials increases. Probability then becomes the relative frequency in population.[64]

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63 Pearson, 'The Laws of Chance I', Gresham Lecture, Janaury 1893, KP:UCL, p.1.

64 The binomial distribution could thus be used to describe one aspect of the results of a series of events (i.e., the probability of a given number of successes). The expansion of the binomial [e.g., $p^2 + 2pq + q^2$] provides a model for ascertaining the various outcomes that can occur. To take the example of tossing three coins, this would produce a total number of eight combinations. The binomial expansion for this could be expressed as:

[Footnote continued on next page]

From this perspective Pearson then argued (in November of 1892) that

> one of the fundamental problems in chance is to determine the frequency with which an event which deviates by a given amount from the most probable will occur. The knowledge of this relation between deviation and frequency enables us to test very accurately how far any series of event is due to chance. A series of events will not be due to chance, if the most probable occurs too often, or if on the other hand the improbable does not occur sufficiently frequently.[65]

Two months later he further explained that the statistical determination of probability depends on finding first the 'percentage of time in which it occurs' (i.e., the frequency) and then by determining the frequency of any 'divergence' (i.e., the standard deviation) from the mean. Having determined this, he then asked his students 'does not the whole theory of evolution depend upon the answer to—what is the frequency with which a variation of [a] given size of a given organ occurs?'[66]

In his lecture on 'Roulettes' in January 1893, Pearson gave a demonstration of the limitations of the mathematical approach and the necessity of using the relative frequency approach by means of the returns from Monte Carlo roulettes. He was led to the idea of using the Monte Carlo roulettes when he was preparing one of his lectures on the law of chance and realised that since he was not able to lead his

> audience through the mazy and oversure paths of mathematical theory, [he] adopted the experimental method of an appeal to statistics, and the deduction by easy arithmetic of the characteristic laws of my subject. The chief difficulty of this method [however] was to make or discover the necessary material.[67]

In preparing for his Gresham Lecture, he wanted to determine a set of probabilities by using the tosses of a coin. He recorded each individual outcome (heads or tails) from 25,000 tosses of a shilling which was undertaken during the long summer vacation and 'consumed much of [his] time'. (As it was often done outside, he 'had little doubt

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[Footnote continued from previous page]

$$(p+q)^3=p^3 + 3p^2q^1 + 3p^1q^2 + q^3 = 1 \quad \text{or}$$

$$(1/2 + 1/2)^3 = (1/2)^3 + 3(1/2)^3 + 3(1/2)^3 + (1/2)^3 = 1/8 + 3/8 + 3/8 + 1/8 = 1$$

Hence, the probability of obtaining 3,2,1 and 0 heads is $1/8$, $3/8$, $3/8$ and $1/8$ respectively.

65 Pearson, 'Normal curves of Frequency', Gresham lecture on The Geometry of Chance, KP:UCL, pp.4-5.

66 Pearson, 'Laws of Chance II', Gresham Lecture, January 1893, KP:UCL, p.6.

67 Pearson, 'The Scientific Aspects of Monte Carlo Roulette', p.44.

that this gave [him] a bad reputation in the neighbourhood where [he] was staying'.)[68] He found the probabilities of his results to be in close agreement with his theoretical expectations.

For purposes of illustration, Pearson thought it would be valuable to have a machine which could generate 'several million tosses of twenty coins at a time'; such a machine was, however, unavailable in the first instance and impractical in the second. Looking for more extensive and readily accessible material, a friend suggested to him that he use the Monte Carlo roulette tables. It then occurred to Pearson that 'science [had] every right to invade a temple of chance as Monte Carlo called itself and to apply her tests of what is and what is not a worship of the goddess'.[69] He thought first of spending several months in Monte Carlo recording the spin of roulettes, but this was not only 'personally an impossibility', it also did not

> seem likely that the Royal Society or the British Association would make a grant to pay the expenses of an agent engaged in such a novel form of scientific investigation.[70]

He discovered 'luckily' that he could gain sufficient material from the returns of the Monte Carlo roulettes when he learnt that the records of the Monte Carlo tables were published in a special journal entitled *Le Monaco* and issued 'weekly in Paris at the price of one franc'.[71]

Pearson recorded his chances of success and failures from the roulette and compared this to De Whalley's (see Table 6.1) in addition to other games of chance from Westergaard, Quetelet and Buffon.

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68 *Ibid.*.
69 Pearson, 'Roulettes', Gresham Lecture, January 1893, KP:UCL, p.44.
70 Pearson, 'The Scientific Aspects of Monte Carlo Roulette', 44.
71 *Ibid.*, 45.

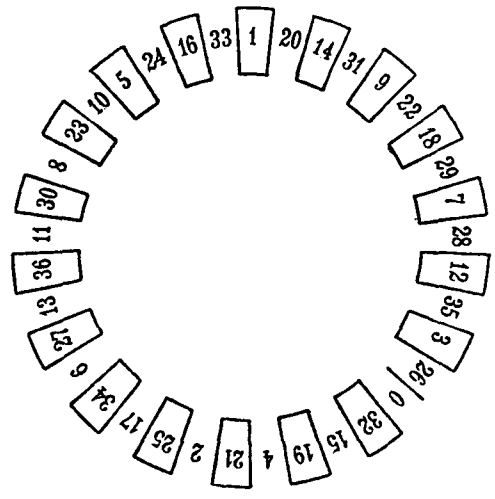
Table 6.1

Method	Percentage		Trials	Experimenter or Calculator
	Success	Failure		
Roulette.....	50·15	49·85	16,141	Pearson
„	50·27	49·73	16,019	De Whalley
Bags of balls	50·11	49·89	10,000	Westergaard
„	50·4	49·6	4,096	Quetelet
Tossing	51	49	4,040	Buffon
„	50·05	49·95	4,092	De Morgan's pupil
„	50·04	49·96	8,178	Griffitli
„	50·16	49·84	12,000	Pearson
„	50·05	49·95	24,000	Pearson
Lottery	50·034	49·966	7,275	Westgaard

Source: Pearson, 'The Scientific Aspects of Monte Carlo Roulette' (1894)

In a random spinning of the roulette,

> the distribution on chance in the game depends upon the mechanical perfection of the teetotum; it must be equally likely to fall on all of its thirty-seven sides [see picture below], i.e., the *frequency* of all of the numbers must in the long run be very nearly the same... Admitting the mechanical accuracy of the instrument, and remembering the keen and watchful eyes of the numerous players, it is difficult to conceive a machine better calculated to illustrate the laws of chance than a Monte Carlo roulette.[72]



In the 16,563 throws from four weeks of play, Pearson then calculated the standard deviation as a measure of the frequency with which the deviation of various trials occurred. Having then spent some time looking at the published records of tables in *Le Monaco*, he discovered that the standard deviation of 20.87 from this theoretical data was considerably different from the observed standard deviation of 15.85 obtained from his 16,563 throws (see Figure 6.8 for a copy of one of Pearson's *Le*

72 *Ibid.*, 47.

Sixième Année. — N° 248. Le Numéro Blanc avec Permanences : Un Franc Du 5 au 11 Août 1892.

LE MONACO

HEBDOMADAIRE

Le *tiret* indique le changement de croupier

Reproduction interdite

STATISTIQUE HEBDOMADAIRE DE PERMANENCES de Roulette

Tous droits réservés

Le *tiret* indique le changement de croupier

Séance du 25 juillet 1892 | Séance du 26 juillet 1892 | Séance du 27 juillet 1892 | Séance du 28 juillet 1892 | Séance du 29 juillet 1892 | Séance du 30 juillet 1892 | Séance du 31 juillet 1892

NR NR NR NR NR NR NR NR NR NR

N = Noir
R = Rouge

N. 290
R. 295

N. 305
R. 297

N. 295
R. 284

N. 283
R. 293

N. 288
R. 291

N. 282
R. 290

N. 281
R. 291

N. 2024
R. 2041

Monaco). He then wanted to know what was 'the reasonable amount that the standard deviation of [his] experiment could differ from its theoretical value'. Mathematically, he resolved this by 'finding the standard deviation of the standard deviations' which he expressed as SD = $\sigma \div \sqrt{2n}$ (i.e., the standard error of the standard deviation). He found that this equalled 2.43 and that 'the odds against this divergence were 21 to 1'. He repeated the experiment by increasing the number of groups which produced a standard deviation of 7.20: the theoretical standard deviation was 10.34 and the standard deviation between experiment and theory was only 0.60. The odds

> against a divergence as great at this [were] roughly 2,000,000 to 1... which was so improbable in its characteristics that it would occur, on the average, *once* in 167,000 years of continuous roulette-playing.[73]

Though taken aback with these results, Pearson did not yet assume that the laws of chance did not apply to Monte Carlo. He did, however, consider himself to have been

> very unfortunate to have hit upon a month of roulette which was so improbable in its characteristics that it would only occur, on the average, *once* in 167,000 years of continuous roulette-playing. [These] were clearly not the most suitable returns for illustrating the laws of chance![74]

Table 6.2

Runs.	1	2	3	4	5	6	7	8	9	10	11	12	Over 12
ROULLETTE–													
Experiment	2462	945	333	220	135	81	43	30	12	7	5	1	0
Theory	2137	1068	534	267	134	67	33	17	8	4	2	1	0
Standard deviation.	33	28	22	16	12	8	6	4	3	2	1.5	1	–
Actual deviation	325	123	201	47	1	14	10	13	4	3	3	0	0
TOSSING—													
Experiment	2163	1056	479	240	120	68	40	15	4	5	0	1	0
Theory	2095	1048	524	262	131	65	33	16	8	4	2	1	0
Standard deviation	32	28	21	16	11	8	6	4	3	2	1.5	1	–
Actual deviation	68	8	45	22	11	3	7	1	4	1	2	0	0

After his lecture on 'Roulettes', he then sought some independent confirmation by using data from one of his students, Howard Griffith. He compared Griffith's calculations of 2,462 trials of runs at the roulette wheel and 2,163 coin tosses with his results (see Table 6.2). Having made these comparisons, he then realised that the

73 *Ibid.*, 52-3.
74 *Ibid.*, 53.

application of probability theory to predict the average runs in Monte Carlo Roulette were doubtful; subsequently, this led him to conclude that 'roulette as played at Monte Carlo is not a scientific game of chance'.[75] The results published in *Le Monaco*, then put to Pearson's probabilistic analyses, would thus

> give the deathblow to the mathematicians theory of the random spinning of a mechanically accurate roulette. The Monte Carlo roulette defies theory by improbabilities repeating themselves in two or three months play, against which the combined odds are many hundred thousands to one.[76]

By using the relative frequency approach, Pearson showed that Monte Carlo roulette defied theory by coming up with improbabilities that repeated themselves 'in two or three months' play, against which the combined odds are many hundred thousands to one'.[77] Given these improbabilities, Pearson offered an alternative explanation that

> the random spinning of a roulette manufactured and daily readjusted with extraordinary care is not obedient to the laws of chance, but is chaotic in its manifestations.[78]

Since the Casino did not seem to be of any scientific value and as it could not be used as a huge laboratory for the preparation of probability statistics, Pearson thought that it had 'no scientific *raison d'être*'. Moreover, men of science could not have

> their most refined theories disregarded in this shameless manner! The French Government must be urged by the hierarchy of science; it would be, of course, a graceful act to hand over the remaining sources of the Casino to the *Académie des Sciences* for the endowment of orthodox probability; in particular, of the new branch of that study, the application of the theory of chance to the biological problems of evolution, which is likely to occupy so much of men's thoughts in the near future.[79]

After more of Pearson's students had provided him with ample data, he recommended that

> attention should be turned to measurement of men, animal or insects with a special view to those problems of evolution and correlation which Professor Weldon has made us familiar... Such measurements it must, however, be remembered must not be for a few hundred, but for the thousand or more specimens.[80]

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75 *Ibid.*, 55-6.
76 *Ibid.*, 61.
77 *Ibid.*.
78 *Ibid.*.
79 *Ibid.*, p.62
80 Pearson, 'Normal Curves of Frequency', Gresham lecture on The Geometry of Chance, November 1892, KP:UCL, p. 3.

Without this knowledge of probability, Pearson argued that 'it [was] impossible... to appreciate the arguments for and against the Darwinian theory of evolution'.[81]

From this work, he had established the statistical framework of the relative frequency approach of probability which would underpin subsequent statistical developments in the nineteenth century and nearly all of his statistical work (including the establishment of significance test) in the twentieth century. When J.F. Stephenson wrote *Some Recent Researches in the Theory of Statistics and Actuarial Science* in 1930, he remarked that it was

> the lasting merit of Pearson... to have pointed out convincingly that the *natural source* of practically useful types of frequency functions is the elementary calculus of probabilities.[82]

This work would find greatest application for determining the probability of differential mortality and fertility rates. Though Pearson used the relative frequency approach throughout the 1890's, by the beginning of the twentieth century he was making reference to the Bayesian approach to probability.[83] Stigler noted that the aspect of

> Bayes's analysis that was of primary importance to Pearson was that it served as a nearly perfect example of the type of mechanism he was already committed to: underlying the discrete distribution that was observed was an unobservable continuous mechanism.[84]

At the beginning of 1893, Pearson decided to resign from his post at Gresham College. He wrote to William Herrick Macaulay, on 19 February 1893 to let him know that 'I have sent in my resignation of Gresham as I have had no holiday since I undertook it, and the doctor said at Christmas that I must reduce my work'.[85] He delivered his final Gresham lecture on 11 May 1894. In his last two Gresham lectures

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81 Pearson, *Chances of Death*, pp.vii-viii.

82 George Udny Yule, book review of J.P. Stephenson, *Some Recent Researches in the Theory of Statistics and Actuarial Science* (Cambridge University Press, 1930), p.36 in *Biometrika* 22 (1931), 425.

83 Karl Pearson, 'On the Influence of Past Experience on Future Expectation', *The London, Edinburgh and Dublin Philosophical Magazine and Journal of Science*, Series 6, 13 (1907), 365-78.

84 Steven M. Stigler, 'Thomas Bayes's Bayesian Inference', *Journal of the Royal Statistical Society*, Series A (General), 145 (1982), 255.

85 Pearson, *Letter to William Herrick Macaulay*, 19 February 1893, KP:UCL.

delivered in May 1894, he dealt with 'Problems of Evolution' and 'A Course of Lectures on Chance Mortality Curves'. Having introduced the idea of chance mortality curves, Pearson would make much use of this material in his lecture on the 'Chances of Death'.[86]

CHANCES OF DEATH: Pearson seems to have been first thinking about the relationship between probability and death in 1875 after he had come across some woodcuts and block-books depicting artistic aspects of the mediaeval ideas of the Dances of Death when he was in Germany standing

> on the well-known wooden bridge at Luzern, with its pictures of the Dance of Death, it struck [him] that something might be done to resuscitate the mediaeval conception of the relation between Death and Chance, and to express it in a more scientific form.[87]

He was able to work out this idea some nineteen years later when he reached a formal statistical generalisation of the Theory of Chance in his Gresham Lecture 'On Chance Mortality Curves' which was published immediately afterwards in the *Fortnightly Review*. (In the following year he delivered this paper to the Leeds Philosophical Society.) His aim was to place before the reader the modern conception of the Dance of Death which 'formed a favourite theme for [mediaeval] man rarely free for a few years from the ravages of war or the horrors of the plague'.[88] He wanted to demonstrate how he could use notions of the mediaeval dances of death to determine how to find regularity in a chance distribution when the idea of death was viewed as

> the lawless one, the one who strikes at random, [which] arose early in mediaeval tradition, and is represented in the well-known Dances of Death, from the primitive to the finished design of Holbein [which is illustrated in Plate 6.1]. Parallel with this notion of the random character of Death's aim, has run in the mind of the folk a vague idea of Chance as that which obeys no rules and defies all measures and prediction. The two conceptions cross one another in the mediaeval representation of Death seizing the gambler's dice-box and casting the dice with him for his life.[89]

Notions of chance, Pearson explained, could be traced to early mediaeval times where the origin of the word chance was

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86 Pearson , 'Chance Mortality Curves', Gresham Lecture (11 May 1894),.
87 Pearson, 'Chances of Death' in *Chances of Death*, pp.8 and p.11.
88 *Ibid.*, p.33.
89 *Ibid.*, p.1.







HOLBEIN.

> very possibly related to the *falling* of the dice from the box—and certainly the first attempts at a theory of Chance arose from the general interest in gambling with dice. It is indeed a fifteenth century commentary on the lines of Dante —
>
> When from the games of dice men separate,
> He, who hath lost, remains in sadness fix'd,
> Revolving in his mind, what luckless throws
> He cast—
>
> that we find the first attempt to rise above the folk-conception of Chance as the chaotic, to the modern notion of Chance as obedience to law. Let us bear these points in mind, the association of Death and Chance, the notion of both as chaotic in their action and their embodiment in a great artistic ideal—the Dance of Death.[90]

Fragments of the earliest Dances of Death appeared to be those on the walls of the cloister of a former nunnery at Klingenthal, near Klein Basal. Pearson thought that the language and costume of this design suggested the first quarter of the fourteenth century (depicted in Plate 6.2). During the fifteenth century Dances of Death could be found in Lübeck, Bern and Strasburg much like the reproduction from a 1450 design depicted in Plate 6.3 which Pearson found in a library in Heidleberg. The artistic value of the Dances of Death had been

> usually supposed to culminate in the Holbein cut published first at Lyons in 1538 (see [his] figures reproducing the Empress and the Old Man [Plate 6.1]), but throughout the sixteenth and even the seventeenth centuries... vigorous productions [were found] dealing with the same central idea and illustrating how widespread and deeply rooted this traditional representation of the random action of death remained for more than four centuries.[91]

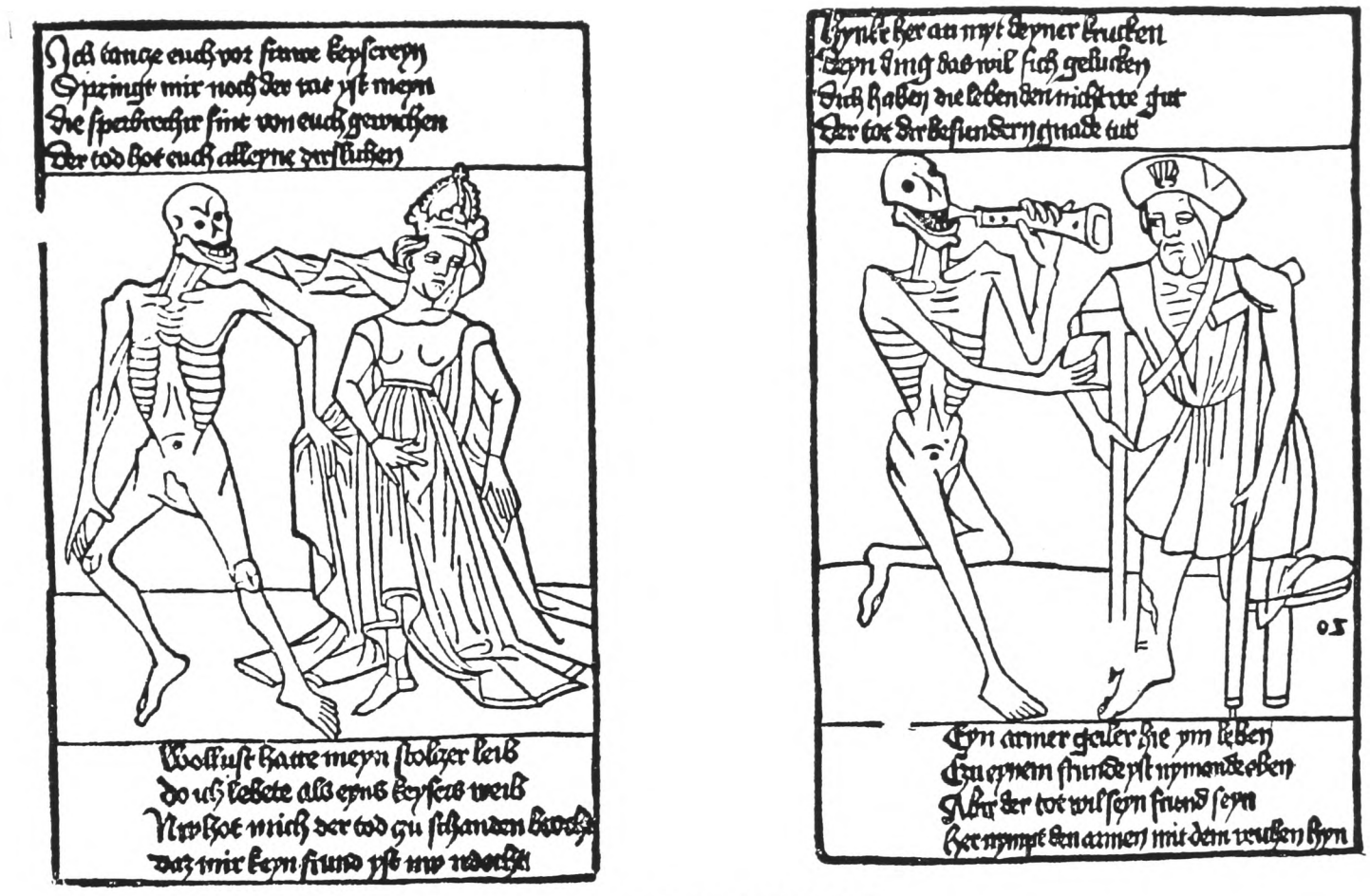
The old Dances of Death were to have supplied what failed so much in modern life of 'an artistic representation appealing to all classes at least one experience common to the whole of humanity'.[92] To express this scientifically would involve 'one or two general principles of the theory of Chance' in addition to the use of some 'dry' statistics from the Census and the Registrar-General returns. Whilst Pearson's conception of Chance was very different from how it was considered during mediaeval times, he went on to say that where

> we cannot predict, where we do not find order and regularity,... we should now assert (as in the case of the Monte Carlo roulette returns) that something else than Chance is at work. What we are to understand by a chance distribution is one in accordance

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90 *Ibid.*, p.2.
91 *Ibid.*, p.8.
92 *Ibid.*

Plate 2
DANCE OF DEATH FROM THE FOURTEENTH CENTURY



HEIDELBERG BLOCK-BOOK.

Plate 3
DANCE OF DEATH FROM THE FIFTEENTH CENTURY



GROSS-BASEL.

> with law, and one the nature of which can for all practical purposes be closely predicted.[93]

For Pearson the modern 'conception of chance [was] one of law and order in large numbers: it [was] not that idea of chaotic incidence which vexed the mediaeval mind'.[94]

WELDON'S DICE: The mediaeval conception of chance could be further illustrated with some dice-throwing and card-drawing experiments. For his audience, he used data from twelve dice which were thrown together '26,306 times of which these returns were due to the kindness of Professor W.F.R. Weldon, F.R.S. [and] the number of dice having five or six pips on their upper faces was recorded'. Pearson then found the 'mode' which he defined as an occurrence which happens more frequently than any other. The expected theoretical value of the most frequent occurrence from tossing 12 dice, when recording those dice with only five or six points, was four. As Weldon was interested in only two sides of any die, which represented one-third of the six possible faces of a die, and as he threw 12 dice each time, then $^1/_3$ of 12 = 4. In a letter Weldon sent to Pearson, in February 1894, he enclosed a distribution of the frequencies of these dice from 7,000 tosses (see Table 6.3). As can be seen, Weldon's distribution does indeed indicate that the mode is four.

Pearson then plotted a frequency distribution of all 26,306 tosses from Weldon's dice (see the bottom left-hand corner in Figure 6.9). This was expressed in binomial terms as $[26{,}306 \times {}^2/_3 + {}^1/_3]^{12}$. Weldon's results indicated that there was a good fit between experiment and theory (i.e. the two distributions are in such close agreement that they are almost indistinguishable). It then could be concluded that

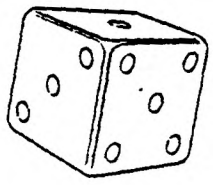
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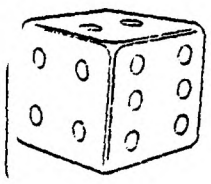
93 *Ibid.*, p.11. Following the publication of his two-volume book on *The Chances of Death and other Studies in Evolution* in 1897, Pearson sent a copy to Galton who replied that he had been
> 'astonished... at its wide range and serious reasonings and at its substantial unity among apparent diversity. You must indeed have had difficulty in assigning a title to it. What an awful time to live in the 14th century must have been to most persons, with its plagues and endemic manias of flagellations, tarentellas and the like, and savage wars. No wonder that dances of death were popular. I look forward greatly to reading the two volumes properly.

(Francis Galton, 'Letter to Karl Pearson', 20 May 1897, in Karl Pearson, *Life of Galton*, vol. IIIb. (Cambridge University Press: Cambridge, 1930), p.502).

94 Pearson, 'Chances of Death' in *Chances of Death*, p.15.

Table 6.3
FREQUENCY DISTRIBUTION BY WELDON OF 'TWELVE DICE TOSSED 7000 TIMES AND NUMBER OF DICE IN EACH THROW OF 12 WITH 5 OR 6 POINTS RECORDED'
(Enclosed in a letter of Karl Pearson, February 1894)





Twelve dice Tossed 7000 times, and number of dice in Each Throw of 12 with 5 or 6 points recorded.

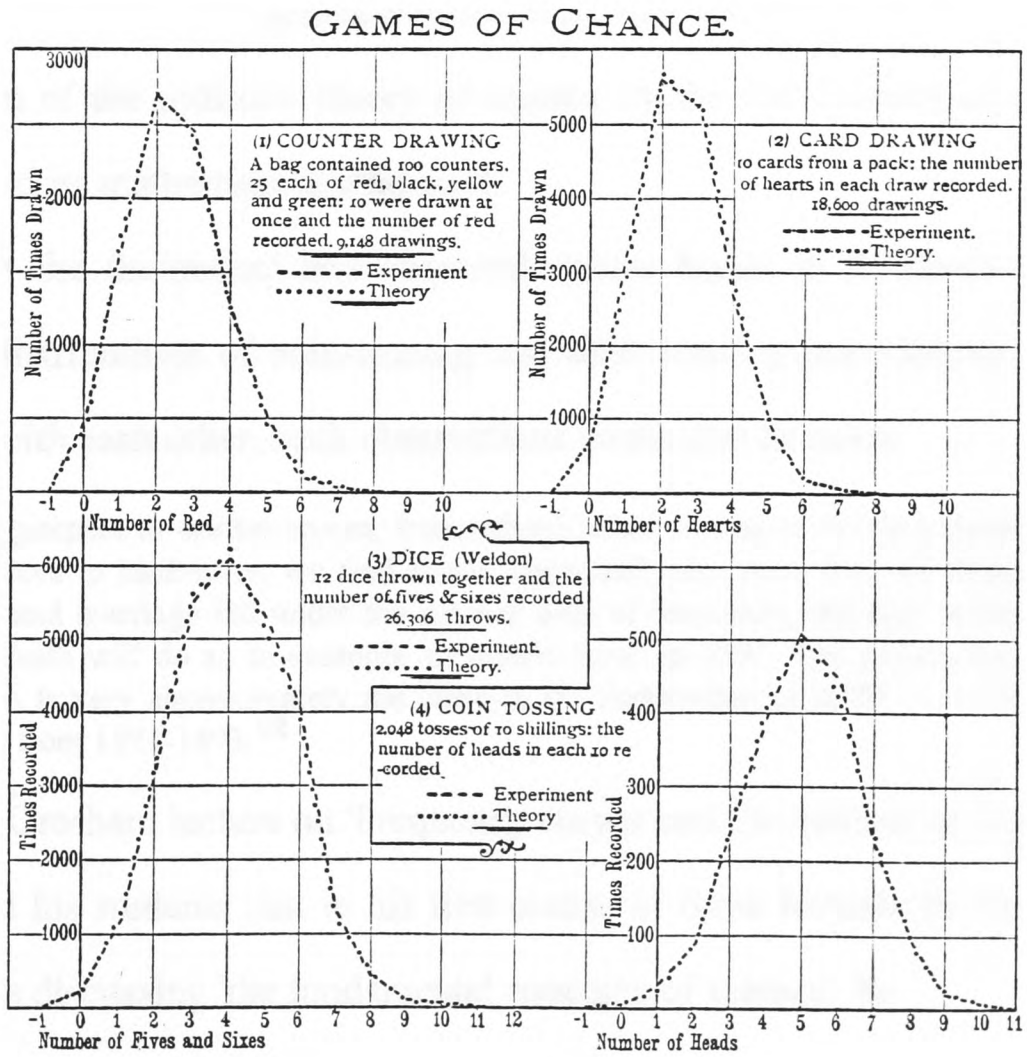
No. of dice	Frequency	Theory
0	45	54
1	327	324
2	880	890
3	1475	1484
4	1571	1669
5	1404	1335
6	787	779
7	367	334
8	112	104
9	29	23
10	2	3 } 1
11	1	–
12	0	–
	7000	7000.

N. B. — These 7000 do not include those previously tabulated, and are made with different dice. — The same dice were used throughout this set of Throws.

> all the combinations mathematically possible in tossing 12 dice together do actually occur in their due proportions when we throw 12 dice several thousand times. This is not a result which we have any right to assume beforehand; that the mathematically *possible* actually does occur in experiment as demonstrated and can only be demonstrated by actual experience.[95]

The results from the drawing of cards did not, however, have the same statistical properties of dice-tossing as the first draw of a card will influence the chances of all that succeed it (as will remaining draws); hence, the cards are not independent. This aspect of probability was considered to be of vital importance for the consideration of mortality curves, as 'the frequency of death at later ages must depend on the incidence of death at earlier ages'.[96]

Figure 6.9
Frequency Distribution of Weldon's Dice by Pearson (1894)



Pearson thought that his reader might have wondered, 'what do frequency curves deduced from coin tossing and dice experiments have to do with mortality?' He explained

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95 Pearson, 'The Scientific Aspects of Monte Carlo Roulette', 61.
96 Pearson, 'Chances of Death' in *Chances of Death*, p.12.

> If the law of frequency we are dealing with hold very generally in cases where we have no knowledge of how the individual instance will turn out, but only statistics of what happens in the mass, may we not reasonably assume that they are essentially the laws of all large a numbers and that even the frequency of death, its distribution with age will obey the same laws?[97]

To examine mortality curves Pearson suggested looking first at frequencies more directly related to the product of nature and less influenced by the hand of man. To do this he then referred to the work of Galton who had shown by numberless measurements in man and of Weldon who had looked at thousands of measurements on crab and shrimp and found that the apparently random sizes of living things have a frequency closely corresponding to Pearson's theoretical curves. After the series of experiments on cards and lotteries, which brought a wider knowledge of variation in the organs of animals and plants, Pearson was then convinced that 'there was need of an extension of the ordinary theory of chance by the introduction of the concept of skewness and its mathematical treatment'.

Whilst the theoretical and observed values found in Pearson's and Weldon's frequency distributions of coin-tossing and card-drawing experiments were in close agreement with each other, such observations could also be made

> from paupers to cricket scores, from school-board classes to ox-eyed daisies, from crustacea to birth-rates, we find almost universally the same laws of frequency. If birth and marriage fall under the general laws of frequency, we may surely expect that death will do so to example an enteric fever in 1891. The distribution of 745 deaths is very approximately the same as the distribution of 8689 cases for the 20 years from 1871-1893.[98]

In his Gresham lecture on 'Frequency curves and Deviations' in November 1893, he reminded his students that in his first course of these lectures in November 1891, when he was discussing 'the fundamental concepts of science', he

> endeavoured to show [his students] that whereas the great results of physical science were established beyond question, the foundations of physics, the ideas of force and matter, the laws of motion were in a state of hopeless and illogical confusion. It is possible that the theory of chance applied to the existing biological views of evolution will change entirely our idea of the problems of selection and heredity. I did not say that it must do, but that it *may* do so. At the very heart it will place the doctrine of evolution on a sound exact basis and deprive it of its present chaotic want

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97 *Ibid.*, p.19.
98 *Ibid.*, p.27.

> of definition. [This was] the method in which [his] frequency curves [were] to be applied to various biological problems in the New Year.[99]

Hence, the topic of his last Gresham lecture in May 1894 was 'On Chance Mortality Curves'.

MORTALITY FREQUENCY CURVES

The statistical principle that Pearson wanted to impress upon his audience with his illustrations was that of the close-fitting of theory and observations indicated by the *regularity* in his mortality distributions. Thus, it could then be seen that mortality curves were *not* chaotic and that the conception of skewness and of the standard deviation were essential in understanding the nature of the variation around the mode. Nonetheless, Pearson thought that his 'ancestors were correct in supposing the frequency of death to be a chance distribution, but we now know that such a distribution follows regular laws'.[100]

Pearson then went on to devise his series of mortality curves by using mortality data from the *Annual Reports of the Registrar General*. He fist tried to use the normal distribution for mortality as was suggested by Wilhelm Lexis, but he found that this became problematic when he was trying to fit old age death into the normal curve. He discovered that if the normal distribution were used for old age mortality, then the theoretical limit of life, according to Lexis, would have been 969 years—'only extremely improbable, not impossible'.[101] Unlike the one normal that Lexis used, Pearson's five chance distributions gave 'an absolute end of life' which was not greater than 106 years.[102] Whilst Theodore Porter has argued that Pearson's derivation of his five mortality curves 'bore a strong resemblance to Lexis's division of the same curve', he also seemed to have thought that Lexis attained a better solution than Pearson as he thought that 'Lexis had been content to establish the normality of death in old age' (where the theoretical limit would have been 969 years).[103]

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99 Pearson, 'Notes on frequency curves & deviations', Gresham Lecture KP:UCL, May 1894,
100 Pearson, 'Chances of Death' in *Chances of Death*, p.25.
101 *Ibid.*, p.27.
102 Pearson, 'Chance Mortality Curves', Gresham Lecture, 11 May 1894, KP:UCL, p.13.
103 Porter, *The Rise of Statistical Thinking*, p.309.

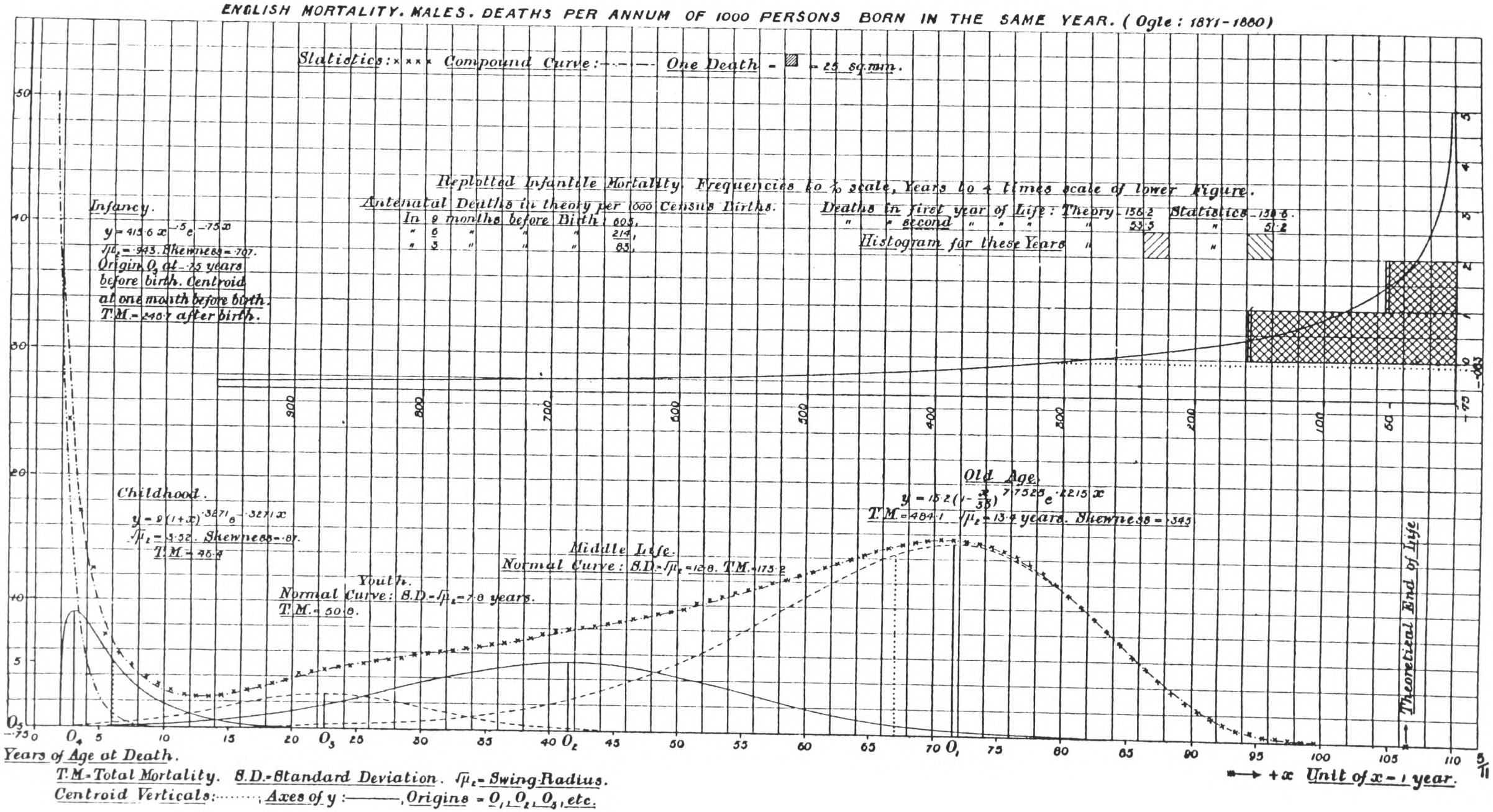
Table 6.4
PEARSON'S ESTIMATED MEAN AGE FOR HIS FIVE STAGES OF LIFE

	Mortality	Mean Age	
1.	Old Age	67	
2.	Middle life	41	
3.	Youth	22	
4.	Childhood	6	
5.	Infancy	3	months

The statistical procedures that Pearson undertook to determine his five mortality curves, derived from 1000 English males born in 1871, were twofold: the first stage required the conception of a generalised frequency curve and this involved a determination of the three mathematical properties underlying the frequency distribution which included central tendency (i.e. the mean, median and mode), variability (i.e. the standard deviation) and skewness. Having established this, he then tried to find a measure of the goodness of fit by fitting the empirical curve to the theoretical curve of mortality: he then produced five chance distributions defined by the total mortality of that period. (See Table 6.4.)

It was only with his introduction of the concept of skewness and its mathematical treatment did he overcome his difficulty with old age mortality. Using this procedure, he found that he could represent mortality curves with a very close accuracy by a skew frequency curve; he thus argued that 'the mortality curve [was] not one simple curve, but [was] made up of several components'. Since death could act upon an organism at different stages in the life cycle, then one mortality curve based on the normal distribution, was seen as inadequate to measure the variation in the distributions of death that occurred at infancy, childhood, youth, middle age and old age. In each situation, he produced a 'chance distribution' defined by the total mortality of that period; by using his method of moments, he could calculate the standard deviation and skewness of each distribution plus find a measure of the goodness of fit between observed and theoretical values (see Figure 6.10).

Figure 6.10
PEARSON'S FIVE MORTALITY CURVES (1894)



These problems, nonetheless, had to be

> solved [by using] other forms of life than man. One of the chief difficulties of the theory of evolution is the want of an exact measure of selection, and, in particular, of selection at different ages. The analysis of the mortality curve enables us at once to reject much that passes for science in the discussion of social evolution. It enables us to localise the time and manner of selection, and until this is done for many forms of life, we shall hardly be able to separate the effects of growth from those of selection, when we come to consider numerical measures of the process described by Darwin.[104]

The principal reason for using data from man to resolve this aspect of selection was because

> with few exceptions, man is the only form of life whose age at death is at any rate approximately known. One of these exceptions [however] is the thoroughbred racehorse, and it has been suggested to me that a study of the *Studbook* would enable me to complete for the horse what the Registrar-General's statistics allow us to do for man.[105]

Before Pearson delivered his last Gresham lecture on 'Chance Mortality Curves' on 11 May 1894, Weldon suggested to Pearson that 'it might be possible to investigate the life curve for thorough-bred-horses, the whole record of the lines of which are recorded in Ruff's Guide in the studbook'.[106] Weldon was to begin an extensive study of thoroughbred race-horses at the beginning of the twentieth century.

Pearson wanted to replace the mediaeval notion of the Dance of Death with an impression he had been considering since the first time he saw Holbein's Dances of Death in Germany some twenty years earlier. The new portrayal was to have been of greater contemporary interest and one that would also correspond to his chance distributions. He put this idea to two different artists to create a picture that would envisage a contemporary view as

> artistically, we no longer think of Death as striking chaotically; we regard his aim as perfectly regular in the mass, if unpredictable in the individual instance. It is no longer the Dance of Death which pictures for us Death carrying off indiscriminately the old and young, the rich and poor, the toiler and the idler, the babe and its grandsire. We see something quite different, the cohort of a thousand tiny mites starting across the Bridge of Life (see Plate 4 [Figure 6.11]), and growing in stature as they advance, till at the far end of the bridge we see only the gray-beard and the 'lean and slippered pantaloon'. As they pass along the causeway and the throng is

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104 Pearson, 'Chances of Death' in *Chances of Death*, pp.40-1.

105 *Ibid.*, p.41.

106 Pearson, 'A Course of Lectures on Chance Mortality Curves', Final Gresham Lecture (11 May 1894), p. 32.

more and more thinned; five Deaths are posted at different stages of the route alongside the bridge, and with different skewness of aim.. till none remain to read the end of a causeway—the limit to life.[107]

Figure 6.11
Pearson's Bridge of Life



The Bridge of Life.

Pearson pursued the statistical determination of differential death rates intermittently for another twenty years. In the spring of 1916 he published an article 'On Criteria for the existence of Differential Death Rates' with James Fowler Tocher: they would determine then that the chi-square goodness of fit test was the most appropriate method for testing the statistical significance of differential mortality rates.[108] In addition to his work on differential mortality rates, Pearson was also interested in finding a measure of the goodness of fit for fertility curves. A year after he delivered his lecture on 'The Chances of Death' to the Leeds Philosophical and Literary Society in December 1895, he began to pursue this idea more fully.

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107 Pearson, 'Chances of Death' in *Chances of Death*, p.40.

108 Karl Pearson and James Fowler Tocher, 'On Criteria for the Existence of Differential Death Rates', *Biometrika* (1916), 159-84.

FERTILITY CURVES

In February 1896, Weldon began to assist Pearson with a project on determining fertility rates. Pearson used Anglo-Saxon data from *Burke's Landed Gentry* whilst Weldon used *Debrett's* list of baronet and peers and they both compiled data from 2,279 families taken from the *Whitney Family of Connecticut of 1875*.[109]

Pearson presented his initial ideas on differential mortality curve or 'Reproductive Selection' to the Royal Society in the early part of 1896. By then, one of Pearson's students, F. Howard Collins had given him some data from *Debrett's*. Before he read this paper, he sent a manuscript to Weldon who wrote to Pearson on 2 February 1896 telling him that

> you gave me a sort of challenge to produce criticisms of your essay, so I have set down all these points which I think could be used if one held a brief against you. Let me say, before making cautious remarks, that I quite appreciate the general importance of 'reproductive selection' and that the worst I can do is to attempt a criticism of the statistics used as a particular example.[110]

Weldon noted that as Howard Collins selected fertile marriages only from *Debrett's*, he had not used all of the available data. Weldon therefore re-examined the marriage patterns of peers and baronets who were alive in 1895 from *Debrett's* list: he included those who were 'either unmarried or married once or twice without having children living at the beginning of that year'.[111] The total number of peers and baronets was about 1,500 and of these, 218 peers and 366 baronets did not have children. He then suggested that Pearson include this data in his paper. Weldon wanted to talk to him soon as he had

> much to say about the effect of reproductive selection on the struggle between classes: and about the necessity, if the Darwinian theory is to hold, that fertility should be inherited; but I have laboured all day in your vineyard, and I know I shall not get a penny for my pains; so you shall have some more later.[112]

By the end of the year, this work was expanded in his chapter on 'Reproductive Selection' in Volume 1 of the *Chances of Death and other Studies in Evolution*. In this

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109 Weldon, 'Letter to Karl Pearson', 2 February 1896, KP:UCL. Weldon's data came from the Whitney Family of Connecticut.(New York, 1878). See also Pearson, 'Reproductive Selection' in *Chances of Death* (1896).

110 Weldon, 'Letter to Karl Pearson', 2 February 1896, KP:UCL, p.1.

111 *Ibid.*, pp.6-17.

112 *Ibid.*, p.5.

article, Pearson 'calculated the moments of the whole system' and plotted a polygon (with observed and theoretical values) which he found to be skewed.[113] He got a 'very close approximation to the distribution of fertility of human marriage in the Anglo-Saxon race' (see Figure 6.12). He used another fertility curve from Copenhagen from Rubin and Westergaard and the distribution was also found to be skewed (see Figure 6.13). He then found that the 'constants of the Danish curve showed that [this was] a markedly more fertile group than the Anglo-Saxon' when the mean, modal and median families for both groups were compared.[114]

In conclusion, Pearson suggested that reproductive selection was 'with some probability the most potent factor of evolution in the case of civilised man'.[115] Nevertheless, the

> relative degrees of fertility in the various classes of a community, and the correlation of various social or anti-social characteristics with fertility seem to deserve the attention of statesmen more than statisticians... Natural selection is sensibly at work, but its influence *within any class* seems to be of an entirely different order to that of reproductive selection. In civilised man the survival of the fittest appears to be replaced by the survival of the most fertile, and the identification of the most fertile with the socially fittest had not yet been asserted by a statist. To bring this identification about may indeed form the hardest problem which the present evolution of civilised man is setting the statement of the future.[116]

MATHEMATICAL PROPERTIES OF THE CHI-SQUARE DISTRIBUTION

After seven years of extensive curve-fitting (with a particular emphasis on chance mortality curves), in the pursuit of obtaining an empirical measure of the goodness of fit, Karl Pearson reached the apex of his statistical innovations when he derived the chi-square distribution and devised the chi-square (χ^2, P) goodness of fit test in 1900. This was, without doubt, his most fundamental contribution to a modern theory of statistics.

The derivation of the chi-square distribution has its roots in the *method of least squares* which had been derived from the theory of errors by astronomers in the

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113 Pearson, 'Reproductive Selection' in Volume 1 of the *Chances of Death*, p.73.
114 *Ibid.*, p.89.
115 *Ibid.*, p.102.
116 *Ibid.*.

Figure 6.12
ANGLO-SAXON FERTILITY CURVE (1896)

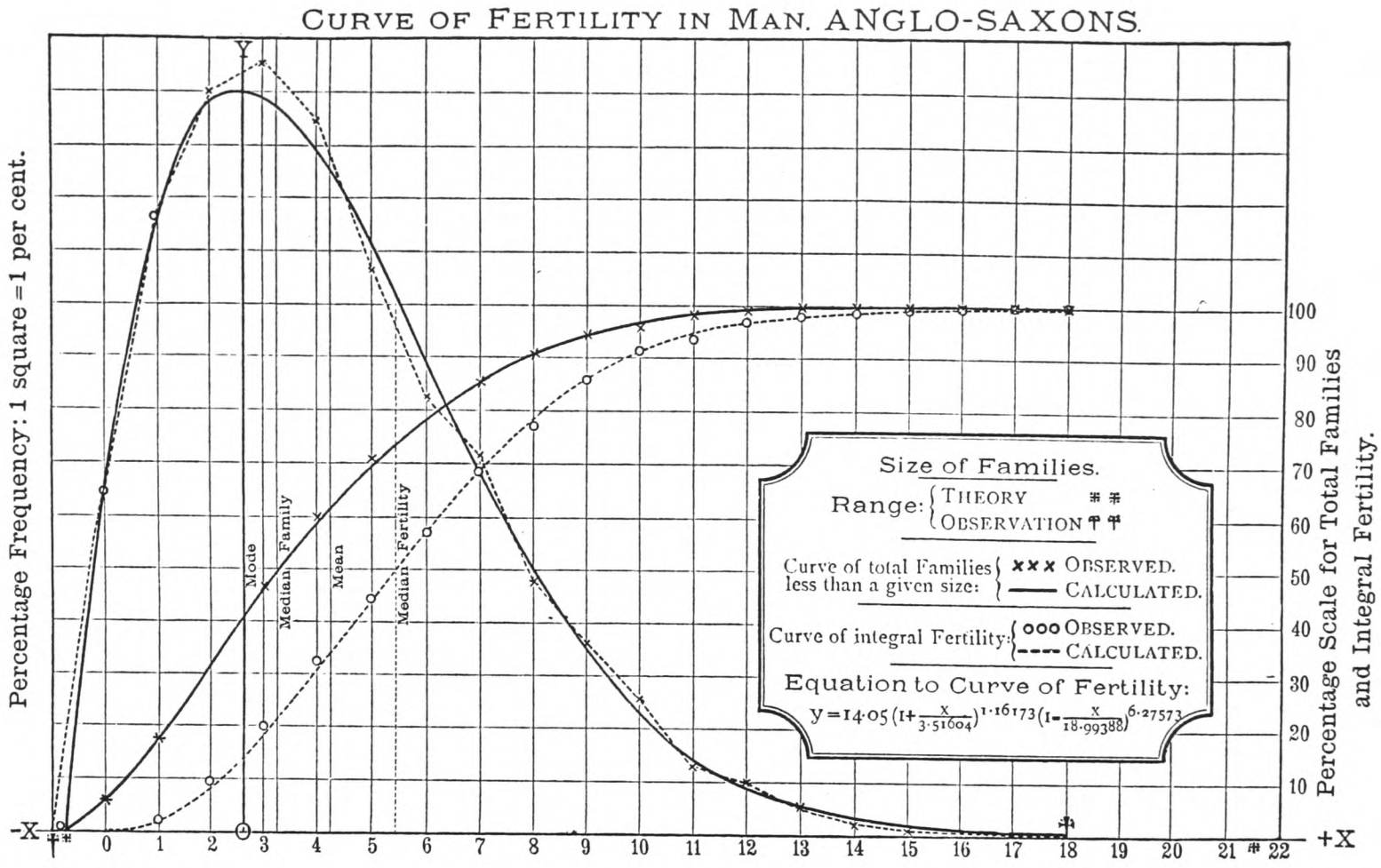
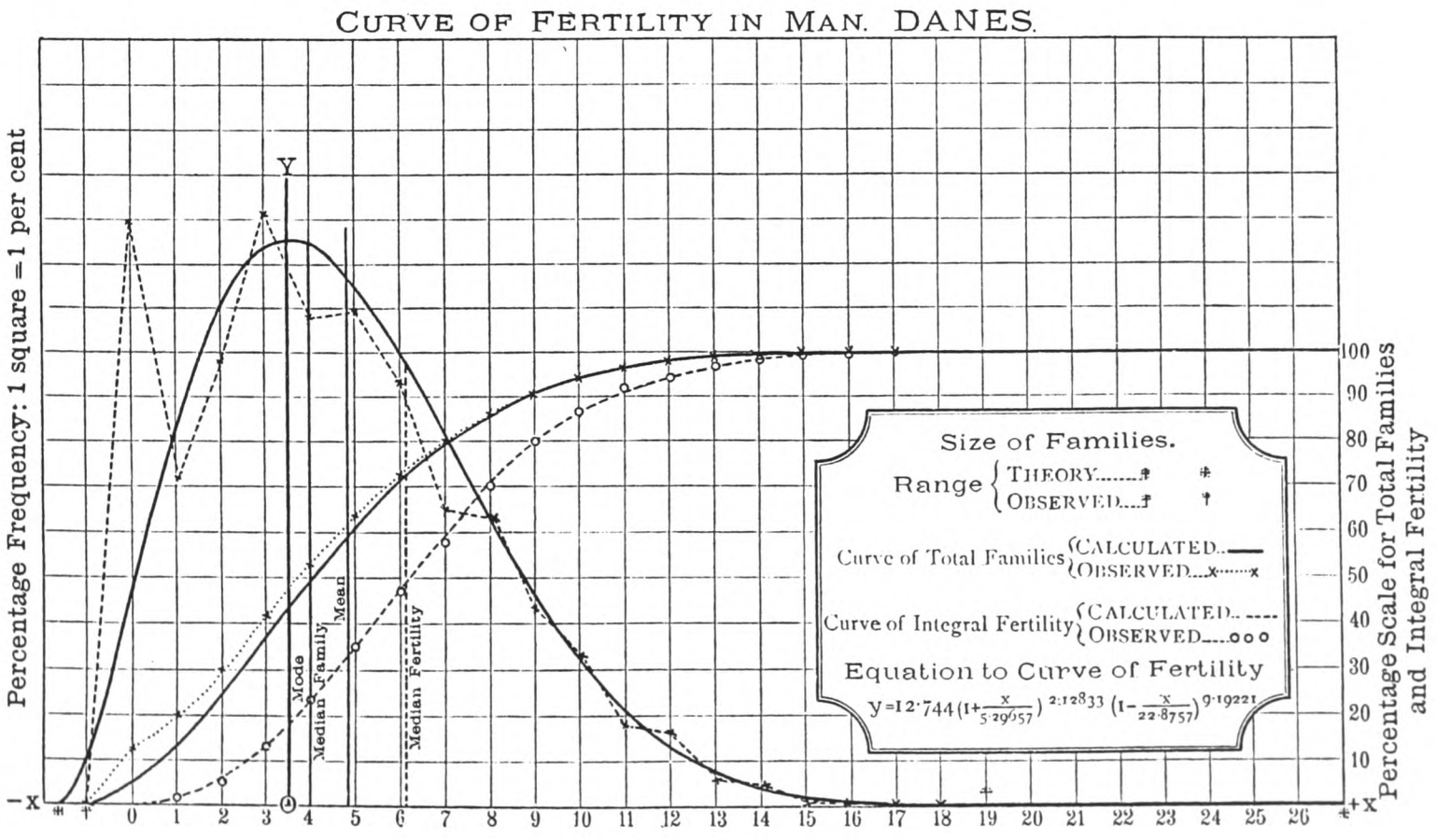


Figure 6.13
DANISH FERTILITY CURVE (1896)



middle of the nineteenth century. The astronomers arrived at the method of least squares when they realised they needed to choose from alternative estimators (i.e., other measures of dispersion) and they came up with the first deviation of the distribution of a sum of squares $\Sigma(\overline{X} - X)^2$ or Σx^2: this provided the mathematical basis for the chi-square distribution.[117] The chi-square distribution is produced by generating a random collection of a series of these square deviational values (i.e., Σx^2).

Harold Lancaster argued that Pearson's chi-square distribution can be regarded as the 'culmination of the least squares theory applied to discrete distributions'.[118] Gamma distributions were used by Pierre Simone LaPlace for his work in error-theory in the early part of the nineteenth century. Though Laplace had not obtained the distribution of the sum of squares nor the usual gamma distributions for the continuous chi-square distribution, he found instead 'a distribution of the precision underlying a Bayesian hypothesis [gave] the observations'.[119] Moreover he

> had provided all the necessary mathematical techniques for Bienaymé to obtain the distribution of chi-square as an asymptotic result without the assumption of normality.[120]

When Bienaymé extended the work of LaPlace in his 'Bayseian study of the lunar form of multivariate variables [in 1838]', Lancaster thought that 'he had very nearly anticipated Pearson's work on the normal approximation to the multinomial'.[121] Fourteen years after Bienaymé extended LaPlace's work, he then used the gamma distribution of the sum of square in the least squares theory.[122] When Pearson derived the sampling distribution of the χ^2 in large samples, he found that it was a specialised form of his Type III distribution which he derived in 1896 (see Figure 6.14.1 which may be compared to the χ^2 distribution in Figure 6.14.2). As

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117 Egon Pearson, 'Some Reflexions on the Continuity in the Development of Mathematical-Statistics. 1885-1920', *Biometrika*, 54 (1967), 341.

118 Lancaster, 'Forerunners of the Pearson χ^2', 117.

119 *Ibid.*

120 *Ibid.*, 121.

121 *Ibid.*, 117.

122 *Ibid.*.

can be seen, the distribution is positively skewed (unless n increases and then the χ^2 distribution will approach the normal distribution as its limit).

Figure 6.14.1
Pearson's Type III Distribution (1896)

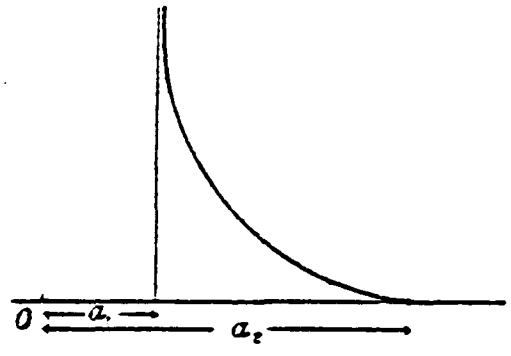
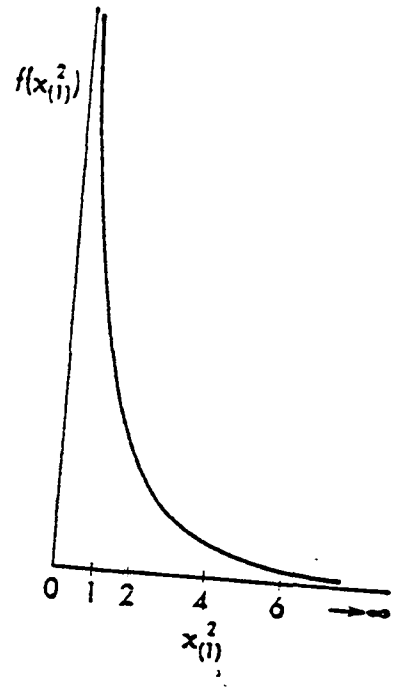


Figure 6.14.2
The Chi-Square Distribution



THE 1900 CHI-SQUARE GOODNESS-OF-FIT PAPER

The object of this paper was to find 'a criterion of the probability on any theory of an observed system of errors, and apply it to the determination of goodness of fit in the case of frequency curves'.[123] This was to be used to determine how well an observed or empirical distribution could be fitted to a theoretical frequency distribution. Comparing procedures used for the normal distribution, Pearson remarked that

> In the current text-books of the theory of errors it is customary to give various series of actual errors of observation, to compare them with theory by means of a table of distributions based on the normal curve, or graphically by means of a plotted frequency diagram, and on the basis of these comparisons to assert that an experimental foundation has been established for the normal law of errors... The works referred to [in this paper] generally give analytical proofs that the normal law of errors is the law of nature—there is often difficulty... in seeing what assumptions have been really made... having deduced the normal curve of errors, they give as a rule some meagre data of how it fits the observation.[124]

123 Pearson, 'System of Deviations', 157.
124 *Ibid.*, 171.

Table 6.5.1
Pearson's Probability Table for the Chi-Square Goodness of Fit Test (1900)

TABLE OF VALUES OF P FOR VALUES OF χ^2 and n' ; χ^2 from 1 to 70, n' from 3 to 20 *.

χ^2 \ n'	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	20.
1	·606,531	·801,253	·909,796	·962,566	·985,612	·994,829	·998,249	·999,438	·999,828	·999,950	·999,986	·999,997	·999,999	1·	1·	1·	1·	1·
2	·367,879	·572,407	·735,759	·849,146	·919,699	·959,839	·981,012	·991,466	·996,340	·998,494	·999,406	·999,772	·999,917	·999 969	·999,990	·999,996	·999,999	·999,999
3	·223,130	·391,633	·557,825	·699,994	·808,847	·885,010	·934,357	·964,303	·981,424	·990,734	·995,544	·997,942	·999,074	·999,605	·999,830	·999,938	·999,972	·999,997
4	·135,335	·261,470	·406,006	·549,422	·676,676	·779,783	·857,123	·911,418	·947,347	·969,923	·983,436	·991,197	·995,466	997,743	·998,903	·999,489	·999,763	·999,899
5	·082,085	·171,799	·287,298	·415,882	·543,813	·659,965	·757,576	·834,310	·891 178	·931,168	·957,979	·975,195	·985,813	·992,128	·995,754	·997,772	·998,860	·999,433
6	·049,787	·111,611	·199,148	·306,220	·423,190	·539,750	·647 232	·739,919	·815,263	·873,366	·916,082	·946,154	·966,491	·979,749	·988,095	·993,187	·996,197	·997,930
7	·030,197	·071,888	·135,888	·220,631	·320,847	·428,870	·536,632	·637,110	·725,544	·799,074	·857,613	·902,142	·934,711	·957,640	973,260	·983,539	·990,125	·994.203
8	·018,316	·046,012	·091,578	·156,236	·238,103	332,594	·433,470	·534,146	·628,837	·713,304	·785,131	·843,601	·889,327	·923,783	948,867	·966,547	·978.637	·986,671
9	·011,109	·029,291	·061,099	·109,064	·173,578	·252.656	·342,296	·437,274	·532,104	·621,892	·702,931	·772,944	·831,051	·877,518	913,414	·940,262	·959,743	·973,479
10	·006,738	·018,567	040,428	·075,236	·124,652	·188,574	·265,026	·350,486	·440,493	·530,388	·615,960	·693,935	·762,183	·819,740	866,628	·903,611	·931,906	·952,946
15	·000,553	·001,817	·004,701	·010,363	020,256	·036,000	·059,145	·090,810	·132,061	·182,371	·241,436	·307,227	·378,154	·450,691	·524,638	·594,156	·661,967	·721,272
20	·000,045	·000,170	·000,499	·001,250	·002,769	·005,570	010,336	·017,913	·029,253	·045,341	·067,086	·095,212	·130,141	·171,934	·220,220	·274,231	·332,819	·394,580
25	·000,004	000,016	·000,050	·000,139	000,341	·000,759	·001,554	·002,971	·005,345	·009,117	·014,822	·023,084	·034,566	·049.943	·069,824	·094,710	·124,915	·160,642
30	·000,000	·000,001	·000,005	·000,015	·000,039	·000,095	·000,211	·000,439	·000,857	·001,585	·002.792	·004,710	·007,632	·011,921	018.002	·026,345	·037,446	·051,798
40	·000,000	·000,000	·000,000	·000,000	·000,000	·000,001	·000,003	·000,008	·000,017	·000,036	·000,072	·000,138	·000,255	·000,453	·000,778	·001,294	·002,087	·003,272
50	·000,000	·000,000	000,000	·000,000	000,000	·000,000	·000,000	·000,000	·000,000	·000,000,5	000,001	·000,003	·000,006	·000,012	·000,023	·000,042	·000,075	·000,131
60	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	000,000	·000,000	·000,000	·000,000	·000,001	·000,002	·000,004
70	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	·000,000	000,000	C00,000	·000,000	·000,000

* I have to thank Miss Alice Lee, D.Sc., for help in the calculation of part of this table. The certainty, *i. e.* the 1· in columns 16 to 20, denotes, of course, something greater than ·999,9995, *i. e.* unity to six figures.

Table 6.5.2
Elderton's χ^2 Table of 1902

χ^2	$n'=3$	$n'=4$	$n'=5$	$n'=6$	$n'=7$	$n'=8$	$n'=9$	$n'=10$	$n'=11$
1	·606531	·801253	·909796	·962566	·985612	·994829	·998249	·999438	·999828
2	·367879	·572407	·735759	·849146	·919699	·959840	·981012	·991468	·996340
3	·223130	·391625	·557825	·699986	·808847	·885002	·934357	·964295	·981424
4	·135335	·261464	·406006	·549416	·676676	·779778	·857123	·911413	·947347
5	·082085	·171797	·287298	·415880	·543813	·659963	·757576	·834308	·891178
6	·049787	·111610	·199148	·306219	·423190	·539750	·647232	·739919	·815263
7	·030197	·071897	·135888	·220640	·320847	·428880	·536632	·637119	·725444
8	·018316	·046012	·091578	·156226	·238103	·332594	·433470	·534146	·628837
9	·011109	·029291	·061099	·109064	·173578	·252656	·342296	·437274	·532104
10	·006738	·018566	·040428	·075235	·124652	·188573	·265026	·350485	·440493
11	·004087	·011726	·026564	·051280	·088376	·138619	·201699	·275709	·357518
12	·002479	·007383	·017351	·034787	·061969	·100558	·151204	·213308	·285057
13	·001503	·004637	·011276	·023379	·043036	·072109	·111850	·162607	·223672
14	·000912	·002905	·007295	·015609	·029636	·051181	·081765	·122325	·172992
15	·000553	·001817	·004701	·010363	·020256	·036000	·059145	·090937	·132061
16	·000335	·001134	·003019	·006844	·013754	·025116	·042380	·066881	·099632
17	·000203	·000707	·001933	·004500	·009283	·017396	·030109	·048716	·074364
18	·000123	·000440	·001234	·002947	·006232	·011970	·021226	·035174	·054964
19	·000075	·000273	·000786	·001922	·004164	·008187	·014860	·025193	·040263
20	·000045	·000170	·000499	·001250	·002769	·005570	·010336	·017913	·029253
21	·000028	·000105	·000317	·000810	·001835	·003770	·007147	·012650	·021093
22	·000017	·000065	·000200	·000524	·001211	·002541	·004916	·008880	·015105
23	·000010	·000040	·000127	·000338	·000796	·001705	·003364	·006197	·010747
24	·000006	·000025	·000080	·000217	·000522	·001139	·002292	·004301	·007600
25	·000004	·000016	·000050	·000139	·000341	·000759	·001554	·002971	·005345
26	·000002	·000010	·000032	·000090	·000223	·000504	·001050	·002043	·003740
27	·000001	·000006	·000020	·000057	·000145	·000333	·000707	·001399	·002604
28	·000001	·000004	·000012	·000037	·000094	·000220	·000474	·000954	·001805
29	·000001	·000002	·000008	·000023	·000061	·000145	·000317	·000648	·001246
30	·000000	·000001	·000005	·000015	·000039	·000095	·000211	·000439	·000857
40	·000000	·000000	·000000	·000000	·000001	·000001	·000003	·000008	·000017
50	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000
60	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000
70	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000	·000000

Jan Dangerfield claimed recently that Pearson approached the mathematical construction of the chi-square distribution by

> using a data-vector specifying the co-ordinates of a point in multi-dimensional space and using a generalised distance between the point representing the observed frequencies and the point representing the expected frequencies to measure the discrepancy. In this way, he construct[ed] the equation of an n-dimensional 'ellipsoid', over the surface of which the frequency of the system of errors is constant (and equal to χ^2). He then construct[ed] a ratio of two n-fold integrals to measure the probability of a frequency as great as, or greater than, χ^2. This [was] then simplified to eventually produce a pair of series for the cases n odd and n even.[125]

Thus χ^2 equals a constant that is the equation of a generalised 'ellipsoid' and is the surface of a frequency of the system of errors or deviations x_1, x_2, ... x_n. The value given to χ, which covered the whole of the generalised space, ranged from 0 to ∞. As soon as the observed deviational value, the standard deviation and the correlation of errors were known, the value of χ could be found. Pearson then determined the probability of the ellipsoid and found a value of P for a series of values of χ^2 for different cases in Table 6.5.1 which may be compared to Elderton's χ^2 table of 1902 in Table 6.5.2. The value of P could be determined once χ^2 had been calculated and when the sample size or 'number of independent observations' (or what is termed today *degrees of freedom*) n' = n-1 was found.

He considered first cases in which the theoretical probability is known *a priori*, where $\chi^2 = S\dfrac{(m'-m_s-\mu)^2}{m_{s_1}}$.

m' = observed frequencies in a distribution and
m = theoretical frequencies in a distribution known *a priori*.

However,

> in a great many cases this is not the fact; the theoretical distribution has to be judged from the sample itself. The question we wish to determine is whether the sample may be reasonably considered to represent a random system of deviations from the theoretical frequency distribution of the general population, but this distribution has to be inferred from the sample itself.[126]

Then for distributions not known *a priori* 'm' becomes $m = m_s + \mu$,: Thus

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125 Jan Dangerfield, 'Karl Pearson and the goodness of fit test', Paper read at the Conference for the History of Statistics Day in association with the British Society for the History of Mathematics. Open University, Mathematics Faculty, Milton Keynes. (12 June 1992).

126 Pearson, 'System of Deviations' (1900), 164.

$$\chi^2 = S\frac{(m'-m_s-\mu)^2}{m_s+\mu}$$

which could be simplified as $\chi^2 = S\frac{e^2}{m}$ where the quantity

$$\chi = \sqrt{S\frac{e^2}{m}}$$

was 'a measure of the goodness of fit'.

From this Pearson concluded that 'χ only differs from χ_s by terms of the order of the squares of the probable errors of the constants of the sample distribution'.[127] The goodness of fit was not based on the general order of the magnitude of the probability P and not on slight differences in its value. With the P, χ^2 measure, it was then possible to determine whether an empirical frequency curve could describe effectively the sample drawn from the given population (i.e., the theoretical curve).

If it was a bad fit, then this curve could be used to describe other samples from the same population and when the value for the χ^2 test increases, the fit becomes worse.

WELDON'S DICE—frequencies known or supposed known *a priori*

When Pearson examined distributions that could be known *a priori*, he used the data from Weldon's dice (that he first used in his Gresham lecture in November 1893). On 4 February 1894, Weldon wrote to Francis Galton that he had

> collected 26,306 tosses of groups of 12 dice, for use at the Royal Institution. In each group the event recorded is the number of dice with 5 or 6 points, so that the chance of success with each die is $^1/_3$.[128]

Thus, the most probable result in the table was obtained from the binomial expansion $(^1/_3 + ^2/_3)^{12}$. A couple of days before Weldon wrote the letter, Pearson had asked him for 'some records of the [same] kind in a hurry, in order to illustrate a lecture'.[129] Weldon then gave Pearson a set of 7,000 tosses which had been made for Weldon by a

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127 *Ibid.*, 165.

128 Weldon, 'Letter to Francis Ysidro Edgeworth' (4 February 1894), in Egon Pearson, 'Early History of Biometry and Statistics. 1890- 94', *Biometrika* (1968), 331.

129 *Ibid.*

clerk in the office of University College whose accuracy was 'excellent'. (The frequency table of these results was shown first in Table 6.3 on p.247.)

Pearson examined this set of data to determine the probability of such an occurrence. He then 'rejected' the results because he thought that 'the deviation from the theoretically most probable result [was] so great as to make the record intrinsically incredible'.[130] Weldon, however, did not think the result was incredible and he wrote to Galton for his judgement in the matter.

Weldon also sent the same results to Edgeworth who responded Weldon a few days after receiving the letter. Edgeworth wrote simply that, 'the tests which I have applied to the cases with *four* and *five* dice do not yield a result which excites much suspicion. I shall be curious to know your final decision'.[131] Two days later, Edgeworth wrote to Pearson suggesting that his

> method would be all right so long as you are given *two* results of the kind operated on. But it is not open to you I think to apply twelve (independent) tests to a composite event such that is considered... You should consider the growing likelihood, as you increase the number of your trials, that such extraordinary results will be presented. You surely would not make the assertion if there had been *fifty thousand* data.[132]

At this time Pearson was interested in finding the probability for each pair (or each datum) rather than for the entire set. Edgeworth wanted to convince Pearson that the 'system as a whole' should be considered.[133] (Some six years later, Pearson would devise a summary value for the probabilities of observed and expected frequencies when he devised his χ^2, P goodness of fit test.)

For his 1900 paper, Pearson then used 'the observed frequency of dice with 5 or 6 points when a cast of twelve die was made 26,306 times' which had been given to him by Weldon. These results showed a bias from the theoretical results as the 5 and 6 points occurred more frequently than they should have. Pearson wanted to know if the results were 'due to random selection' or did these results indicate 'a real bias towards

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130 *Ibid.*.

131 Edgeworth, 'Letter to W.F.R. Weldon' (7 February 1894), in Egon Pearson, 'Early History of Biometry and Statistics. 1890-94'. *Biometrika* (1968), 333.

132 *Ibid.*

133 *Ibid.*

those faces with the maximum number of points appearing uppermost?'[134] Pearson then set up a table and computed the frequencies for the observed and theoretical distributions shown in Tables 6.6.1 and 6.6.2.

Table 6.6.1

No. of Dice in Cast with 5 or 6 Points	Observed Frequency, m'	Theoretical Frequency, m	Deviation, e
0	185	203	- 18
1	1149	1217	- 68
2	3265	3345	- 80
3	5475	5576	- 101
4	6114	6273	- 159
5	5194	5018	+ 176
6	3067	2927	+ 140
7	1331	1254	+ 77
8	403	392	+ 11
9	105	87	+ 18
10	14	1	+ 1
11	4	1	+ 3
12	0	0	0
	26306	26306	

Table 6.6.2

Group.	e^2.	e^2/m	Group.	e^2.	e^2/m
0	324	1.549606	7	5929	4.72807
1	4624	3.79951	8	121	0.30903
2	6400	1.91330	9	324	3.72414
3	10201	1.82945	10	1	0.07346
4	25281	4.03013	11	9	9.00000
5	30976	6.17298	12	0	.00000
			Total	...	43.87241

He found that $\chi^2 = 43.87241$. To find the probability from 13 groups he calculated:

$$P = e^{-\frac{1}{2}x^2}\left(1+\frac{\chi^2}{2}+\frac{\chi^4}{2\cdot4}+\frac{\chi^6}{2\cdot4\cdot6}+\frac{\chi^8}{2\cdot4\cdot6\cdot8}+\frac{\chi^{10}}{2\cdot4\cdot6\cdot8\cdot10}\right)$$

hence, P = .000016.

> or the odds are 62,499 to 1 against such a system of deviations on a random selection. With such odds it would be reasonable to conclude that dice exhibit bias towards the higher points.[135]

Following Pearson's results, Weldon thought there was another way to find the theoretical distribution. He suggested to Pearson that rather than using binomial expansion 26,306 $(1/3 + 2/3)^{12}$—which he first suggested to Galton in 1894—they

134 Pearson, 'System of Deviations' (1900), 167.
135 *Ibid.*, 168.

should take, instead, the 26,306 tosses of the dice and multiply it by $(.3377 + .6623)^{12}$. The difference between .3377 and $^1/_3$ represented the bias of the dice. Pearson then recalculated the expected values and came up with the results shown in Table 6.7 below. Hence $\chi^2 = 17.775,7555$ and P now = .1227 with

> the odds now only being 8 to 1 against a system of deviations improbable as or more improbable than this one. It may be said accordingly that the dice experiments of Professor Weldon are consistent with the chance of five or six points being thrown by a single die being .3377, but they are excessively improbable, if the chance of all of the faces is alike and equal to $^1/_6$th.[136]

Table 6.7

Group.	m'	m	e	e^2/m
0.....	185	187	- 2	.021,3904
1.....	1149	1146	+ 3	.007,8534
2.....	3265	3215	+ 50	.777,6050
3.....	5475	5465	+ 10	.018,2983
4.....	6114	6269	- 155	3.991,8645
5.....	5194	5115	+ 79	1.220,1342
6.....	3067	3043	+ 24	.189,2869
7.....	1331	1330	+ 1	.000,7519
8.....	403	424	- 21	1.040,0948
9.....	105	96	+ 9	.841,8094
10.....	14	15	- 1	.666,6667
11.....	4	1	+ 3	9
12.....	0	0	0	0

Sixty-eight years after Pearson published Weldon's results in his χ^2 paper, Egon Pearson decided to recalculate his father's chi-square test.[137] Egon used George Udny Yule's lecture notes of 1894 which contained the data from Weldon's original sample of 7,000 tosses of 12 dice. Egon's total of observed frequencies summed up to 7,006 rather than Weldon's 7,000: this is due to the difference between their frequencies for '2' (i.e., those times when only 2 out of the 12 dice showed 5 or 6 points). Weldon's observed frequency for 2 = 880 whereas Yule's and Egon's 2 = 886.

Egon thought that the 'column 2 of the table ... [was] presumably the result of the clerk's dice throwing 7,006 (not 7,000) tosses of 12 dice'.[138] As Pearson used Weldon's sample of 7,000, the error would not seem to fall on the side of the clerk; instead, the error may have been Yule's who could have inadvertently mistaken the 0

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136 *Ibid.*, 323-38.
137 Egon Pearson, 'History of Biometry and Statistics', 323-1.
138 Pearson, 'System of Deviations' (1900), 445-57.

Table 6.8

No. of 5's or 6's	Observed frequency	Expected frequency	O–E	$\frac{(O-E)^2}{E}$
0	45	54.0	-9.0	1.50
1	327	323.7	3.3	0.03
2	886	890.2	-4.2	0.02
3	1475	1483.7	-8.7	0.05
4	1571	1669.2	-98.2	5.78
5	1404	1335.3	68.7	3.53
6	787	788.9	-1.9	0.00
7	367	333.8	33.2	3.30
8	112	104.3	7.7	0.57
9	29	23.2	5.8	1.45
10	2}	3.5}		
11	1}	0.4}	-0.9	0.21
12	0}	0.0}		
Totals	7006	7010.2	-4.2	16.44

$\chi^2 = 16.44$

Table 6.9
4274 Sets at Roulette

Runs	1	2	3	4	5	6	7	8	9	10	11	12	Over 12
Actual.....	2462	945	333	220	135	81	43	30	12	7	5	1	0
Theory.....	2137	1068	534	267	134	67	33	17	8	4	2	1	0

for a 6. Egon then recalculated the χ^2 using 7,006 as the total number of observed frequencies. He then set up this data in a contemporary fashion (see Table 6.8) and as can be seen, Egon's χ^2 value, with modification, is indeed very similar to the one derived from Weldon's suggestion.

The returns from the Monte Carlo roulettes were given a final consideration in this paper. In one of Pearson's last analyses of the probability of the throws of the roulette ball, for a fortnight's play in the summer of 1892, he had shown that the odds were 'at least 1000 million to one against such a fortnight of runs as occurred in July 1892 being a random result of a true roulette'. He now was able to find the value of this data (as can be seen in Table 6.9).[139] From this data, Pearson found that $\chi^2 = 172.43$ and the

> improbability of a series as bad or worse than this is about $14.5/10^{30}$! From this it will be more than ever evident how little chance had to do with the results of the Monte Carlo roulette in July 1892.[140]

When Pearson considered theoretical distributions that could not be known *a priori* he examined the data from DeVries's buttercups (that he first examined in his memoir on skew variation in 1895). In that paper he fitted the statistics for the frequency of petals in 222 buttercups with a skew curve. From the data in Table 6.10, he found that $\chi^2 = 4.885{,}528$ where $P = .5586$. Hence, in 56 cases out of 100 on the basis of random selection, the fit could be considered 'remarkably good'.

Table 6.10
THE POSSIBLE RANGE IS FROM 5 TO 11 PETALS, AND THE FREQUENCIES ARE:

No. of Petals.....	5	6	7	8	9	10	11
Observation.....	133	55	23	7	2	2	0
Theory.....	136.9	48.5	22.6	9.6	3.4	0.8	0.2

139 *Ibid.*, 169.
140 *Ibid.*.

Having established this goodness of fit test, which could be used for distributions of varying shapes, Pearson concluded that the

> normal curve possess[ed] no special fitness for describing errors or deviations such as arise either in observing practice or in nature. We want a more general theoretical frequency, and the fitness of any such to describe a given series can be investigated by aid of the criterion discussed in this paper. For the general appreciation of the probability of the occurrence of a system of deviations defined by χ^2 (or any greater value), a Table [was] calculated.[141]

In July 1911, Pearson extended his goodness of fit test to determine if 'two independent distributions of frequency are really samples of the same population'.[142] Then in 1916, he gave a new derivation of the chi-square distribution as

> the limiting distribution of the class frequencies of the multinomial distribution as the sample size N + ∞... and extended χ^2 technique to testing whether the frequencies arrayed in two (2 x c) contingency tables can be considered random samples from the same bivariate population.[143]

Pearson maintained that the extension of the χ^2 goodness of fit test could be used to determine,

> in the case of districts, special diseases or class groups... whether mortality experiences corrected for age distributions are or are not significantly different and its extension to occupation groups provide the proper means of ascertaining whether the populations at risk in various insurance offices or friendly societies are or are not materially different in character.[144]

Some twenty years after Pearson introduced his χ^2 goodness of fit test, he was preparing a lecture on the work of Joseph Louis LaGrange (1736-1813). In the first mathematical memoir of LaGrange, he recognised that errors exist in all sorts of measurement and he was looking for a way to examine and evaluate the uncertainty of such errors.[145] Pearson then realised that LaGrange then reached the normal multinomial surface and devised a procedure that involved 'precisely the χ^2, P that

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141 *Ibid.*, 174.

142 Karl Pearson, 'On the Probability that Two Independent Distributions of Frequency are really samples of the same Population', *Biometrika*, 8 (July 1911), 250-4.

143 Karl Pearson, 'On the general theory of multiple contingency with special reference to partial contingency', *Nature*, 11 (May 1916), p. 158. See Eisenhart, 'Karl Pearson', p.460.

144 Pearson, 'On the General Theory of Multiple Contingency', 158.

145 Karl Pearson, 'Joseph Louis LaGrange', in Egon Pearson (ed.), *The History of Statistics in the Seventeenth and Eighteenth Centuries against the Changing Background of Intellectual, Scientific and Religious Thought*, Lectures by Karl Pearson given at University College London during the academic sessions 1921-1933. (Charles Griffin: London, 1978), p.587.

[he] obtained by a most troublesome algebraic process in 1900' as LaGrange had deduced the 'probability that the true values differ from the observed values but the inverse relation did not hold'.[146] In 1927, Pearson would use the chi-square goodness of fit test with the Poisson distribution.[147]

SIGNIFICANCE TESTS FOR DIFFERENTIAL DEATH RATES

In his paper with James Fowler Tocher, 'On Criteria for Differential Death Rates' in 1916, Pearson wanted to 'determine whether the general death-rate or the death-rates for special diseases in two towns or two classes of the general population [were] significantly different' as this was considered to be a problem of 'very great importance'. By then he had recognised that crude death rates were of little service for special diseases as the age distribution in two districts or two classes may differ widely and the general deathrate or the special deathrate (i.e., the deathrate for a special disease) is usually a marked function of age. The death-rate was usually considered to be

> corrected by reduction to a standard population. If p be the chance of the person dying in the year, q the chance of a person surviving, then the standard deviation [for the binomial distribution] of the number of deaths is $\sqrt{npq}$ ... If there be a second town of death-rate m' and population n', and chances p' and q,...m'–m [might be compared to npq] with $\sqrt{\frac{pq}{n}} + \sqrt{\frac{p'q'}{n}}$ [which] could be used to obtain a measure of significant differentiation.[148]

Pearson and Tocher, however, thought that this method was 'for several reasons unreliable and fallacious'. For many diseases or for many ages in the death-rate, p and p' would have been very small and the standard binomial distribution would have been inappropriate: as the probability of occurrence was very small and the sample size (n) was very large, Pearson recognised that 'the binomial approach[ed] Poisson's exponential limit instead'. The Poisson distribution was thus more appropriate to use than the binomial as the standard deviation of the difference would need to be treated

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146 *Ibid.*, pp.600-1.

147 Karl Pearson, 'Note on the Relation of the (P,χ^2) Goodness of Fit Test to Distributions of Standard Deviations in Samples from a Normal Population', *Biometrika*, 19 (1927), 215.

148 Pearson and Tocher, 'On a Criteria for Differential Death Rates', 159.

differently since the probabilities would not have been given 'by a table of the probability integral'.

They considered this method to be inadequate as it tested 'whether a certain *single* character of the distribution does or does not present a significant difference in two populations compared'. If the 'distribution as a whole' were to be tested, they recommended using a modification of Pearson's 1911 paper on the chi-square for partial contingency to determine 'the probability that two systems of frequency are random samples of the same population'.[149]

To further determine if there were any significant differences in cancer death rates between Edinburgh and Dundee as well as in general death rates between Liverpool and Birmingham, Pearson wanted to test the goodness of fit for these empirical distributions. The 'theoretical' distributions he used included data from the *General Population of Scotland 1891-1901, the General Population of England and Wales (1891-1901) and The Registrar-General's 77th Annual Report* for 1913.[150] Using the chi-square goodness of fit test for partial contingency, he found a significant difference in cancer mortality between Edinburgh and Dundee and differences were also found in cancer death rates between England and Wales.[151]

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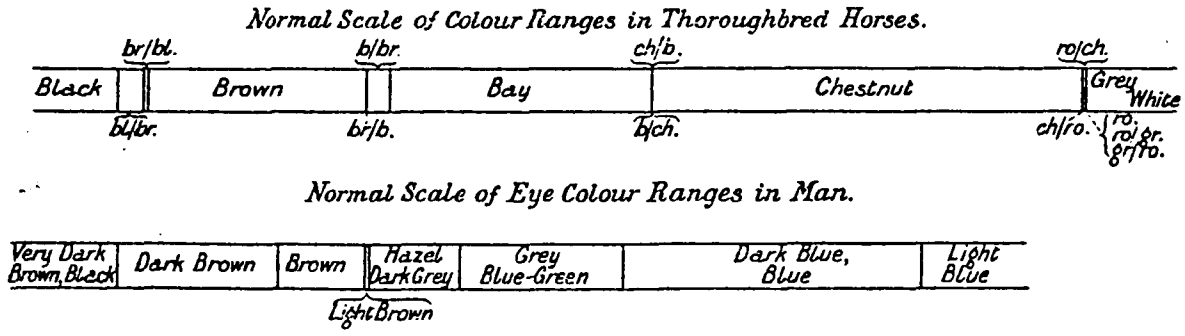
THE ROLE OF CONTINGENCY TABLES IN PEARSONIAN STATISTICS

Having established methods for continuous variables during the 1890's, by the beginning of the twentieth century, Karl Pearson had devised statistical coefficients for contingency tables to analyse non-continuous or discrete variables. He had already been confronted with variables that could not be treated as continuous when he began

149 *Ibid.*, 167. Pearson discussed his chi-square for partial contingency in his paper 'Two Independent Distributions of Frequency', 250-4.
150 Pearson and Tocher, 'On a Criteria for Differential Death Rates', 167-84.
151 *Ibid.*, p.172.

to look at inheritance of eye colour in man and of colour of coat skin in horses and dogs in 1899. In these situations, the only form of classification available for the variables is one that involved 'counting', rather than that of 'measuring': eye-colour cannot be measured in the same way that stature, weight or time can be measured. The values assigned to such variables as eye-colour are achieved by using either a 'nominal' scale of measurement which requires simply 'naming' the values of the variables (i.e., black, brown, blue) or, in the case of Pearson's classification of colours, an 'ordinal' scale may also be used here the intensity of shades were ordered from the darkest (i.e., 'black') and to the lightest (i.e., 'light blue'). See Figure 6.15 which depicts Pearson's classification scheme of colour-ranges in horses and in eyes for man. (Pearson used Galton's eye-colour scheme and for coat-colour of thorough-bred horses, he used the 17 categories classified in Wetherby's studbook.) This problem of classification led to the development of a series of techniques designed to handle discrete variables.

Figure 6.15



By 1901, the hereditarian work of Weldon and Pearson was no longer restricted to the statistical analysis of continuous variables. With the rediscovery of Mendel's work in 1900, they began to apply Mendel's particulate theory of inheritance to the chi-square goodness of fit test when testing empirical distributions against a Mendelian theoretical distributions. Eight months after Weldon learnt of the rediscovery of Mendel's work, he re-examined Mendel's data on peas which was published subsequently in the first issue of *Biometrika*.[152] By 1904, they were

152 Weldon, 'Mendel's Laws of Inheritance in Peas', *Biometrika*, 1 (1902), 228-54.

analysing Mendel's discrete variables using the chi-square statistic (or Pearson's mean-square contingency coefficient) to test for significant differences. About the same time that Pearson began to work on these developments, one of his former students, George Udny Yule, was putting forth his own views for the analysis of discrete variables for contingency tables.

In November 1899 Pearson read a paper to the Royal Society on the inheritance of coat-colour of dogs and horses and of eye-colour in man: he would use this data again some three months later in his 1900 paper on the 'Mathematical Contributions to the Theory of Evolution. On the Correlation of Characters not Quantitatively Measurable'.[153] He was assisted by two of his co-workers in the Biometrics Laboratory, Alice Lee (whose name appeared with Pearson's on the second part of the paper) and Leslie Bramley Moore. He also received some assistance from the French mathematician, Louis Napoleon George Filon, who was the Professor of Applied Mathematics from 1912-1937.[154] Pearson presented this paper in two sections; the first section dealt with issues that were purely theoretical and the second was concerned with the application of his new statistical methods.

In this paper he introduced the tetrachoric correlation coefficient (r_t), the mean square contingency coefficient (ϕ^2) and his phi coefficient (ϕ) known later as 'Pearson's phi-coefficient' for discrete variables. The tetrachoric correlation requires that both X and Y represent continuous, normally distributed and linearly related variables whereas the phi-coefficient was designed for so-called point distributions, which implies that the two classes have two point values or merely represent some qualitative attribute. Both methods were designed for 2 x 2 (or fourfold) tables; hence only dichotomous variables can be used. As the tetrachoric correlation assumes an underlying continuous distribution, the dichotomy is artificial (i.e., when stature is measured in inches or centimetres it is continuous, but a dichotomy can be created by

153 Pearson, 'Mathematical Contributions to the Theory of Evolution. VII. On the Correlation of Characters not Quantitatively Measurable', *Philosophical Transactions*, A, 195 (1900), p.2.

154 Negley Harte and John North, *The World of University College London 1828-1978* (Grosvenor Press: Portsmouth, 1978), p.148.

classifying subjects into 'short' or 'tall' groups). Whilst Pearson used his phi coefficient to find the correlation between the standard deviations in two populations, it was used later (especially by psychometricians) for situations where there exists a true dichotomy (i.e., 'true' or 'false' test items).

Pearson began the paper with a general theorem in normal correlation where the frequency surface

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

N = total number of observations,

σ_1, σ_2 = standard deviations of organs x and y,

r = correlation of x and y,

This was then divided into 'four parts by two planes at right angles to the axes x and y at distances h' and k' from the origin'.[155] The total volumes or frequencies in these parts were represented by *a*, *b*, *c*, *d*, in the manner indicated in Pearson's plan:

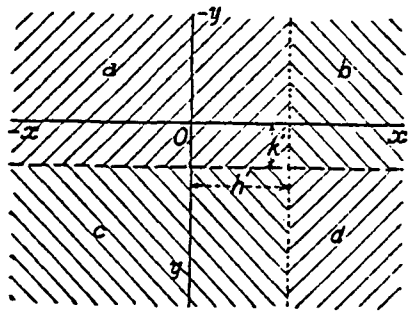


Table of Frequencies

a	b	a+b
c	d	c+d
a+c	b+d	N

Then clearly

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

$$= \frac{N}{2\pi\sqrt{(1-r^2)}}\int_{h}^{\infty}\int_{k}^{\infty} e^{-\frac{1}{2}\frac{1}{1-r^2}(x^2+y^2-2rxy)}dx\,dy \quad \ldots\ldots\ldots \text{(i.)},$$

if $h = h'/\sigma_1$ and $k = k'/\sigma_2$.

Further,
$$b + d = \frac{N}{\sqrt{2\pi}\,\sigma_1}\int_{h'}^{\infty} e^{-\frac{1}{2}\frac{x^2}{\sigma_1^2}}dx,$$
$$= \frac{N}{\sqrt{2\pi}}\int_{h}^{\infty} e^{-\frac{1}{2}x^2}dx \quad \ldots\ldots\ldots$$

and
$$c + d = \frac{N}{\sqrt{2\pi}}\int_{k}^{\infty} e^{-\frac{1}{2}y^2}dy \quad \ldots\ldots\ldots$$

$$\frac{(a+c)-(b+d)}{N} = \sqrt{\frac{2}{\pi}}\int_0^h e^{-\frac{1}{2}x^2}dx \quad \ldots\ldots\ldots$$

$$\frac{(a+b)-(c+d)}{N} = \sqrt{\frac{2}{\pi}}\int_0^k e^{-\frac{1}{2}y^2}dy \quad \ldots\ldots\ldots$$

155 Pearson, *op.cit.*, p.142.

When *a*, *b*, *c*, and *d* were known, *h* and *k* could be found by the ordinary table of the probability integral provided by William Fleetwood Sheppard.[156] Pearson then reached 'a generalised form of Sheppard's result for *any* class index whatever' and expressed the tetrachoric correlation as[157]

$$r = \sin 2\pi \frac{(ad-bc)}{N^2} = \cos \pi \frac{b}{a+b}$$

A correlation could be found only if groupings had been made into the four divisions a, b, c, and d. Pearson derived the value of the tetrachoric correlation r from the value of the correlation parameter ρ of the bivariate normal distribution with frequencies from each of the four quadrants of the x, y plane by lines parallel to the co-ordinate axes. This division, in fact, did agree exactly with four cell frequencies of the fourfold table in terms of arbitrary and precise dichotomous division of two trait scales.[158]

Pearson then went on to find the probable error of the tetrachoric correlation where

$$\sigma a = \sqrt{\frac{a(N-a)}{N}}$$

If $b + d = n_1$, $c + d = n_2$, then

$$\sigma n_1 = \sqrt{\frac{n_1(N-n_1)}{N}} \quad and \quad \sigma n_2 = \sqrt{\frac{n_2(N-n_2)}{N}} =$$

the standard deviation of both variables.

Thus, $\sigma n_1 = NH\sigma_n$ and similarly $\sigma n_2 = NK\sigma_x$; this gave the probable error of the means from any double groupings of observations such that $\sigma n_1 \sigma n_2 r n_I n_2 = N^2 HK\sigma_n \sigma_k r_{hk}$ and $r n_1 n_2 = r_{hk}$. Therefore

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

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156 William Fleetwood Sheppard, 'On the Application to the Theory of Error to Cases of Normal Distribution and Normal Correlation', *Philosophical Transactions*, A, 192 (1898), p.167, Table VI.

157. In more contemporary terms, the tetrachoric correlation coefficient may be expressed as:

$$r_{tet} = \frac{\cos 180^{O}}{1+\sqrt{\frac{BC}{AD}}}$$

158 See Eisenhart, 'Karl Pearson', p.453.

which Pearson would refer to four years later as 'Pearson's phi coefficient' as he had begun to use the symbol ϕ (from the Greek letter phi). This was an important result as it expressed the 'correlation between errors in the position of the two characters' (i.e., of two means when each is measured in terms of the standard deviation of its own distribution).[159]

One month after Pearson read his paper on the inheritance of eye-colour in man and coat-colour in horses, George Udny Yule read a paper to the Royal Society on 7 December 1899 and gave one of the first measures of association when he introduced the 'Q' statistic (named for Quetelet).[160] At this meeting, Yule introduced the word 'association' for measuring relationships that were not made up of continuous variables for which a correlation coefficient was used normally. Yule's measure of association was intended to be used for such 'practical cases... where either no variation is thinkable at all, or else is not measured or possibly measurable'.[161] He thus wanted to devise 'some sort of coefficient of association to take the place of Pearson's coefficient of correlation for continuous variables'.[162] In this paper he used material from Darwin's cross and self-fertilised peas. He also wanted to determine if there was an association between 'presence' or 'absence' of vaccination with those who survived or died as a result of small pox.[163] (In the paper that Pearson read in November, he was interested in trying to find the correlation between strength to resist small-pox and the degree of the effectiveness of the vaccination.)

Yule wanted to create a measure that was free of any assumptions of an underlying normal continuous distribution where

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159 Pearson, 'MCTE. VII. On the Correlation of Characters not Quantitatively Measurable' (1900), 79-150. See also David Heron, 'The Danger of Certain Formulae Suggested as Substitutes for the Correlation Coefficient', *Nature*, 8 (1911), 109-22.

160 George Udny Yule, 'On the Association of Attributes in Statistics with examples from the Material of Childhood Society &c.', *Proceedings of the Royal Society* (1899). In his 1912 paper 'On the methods of measuring association between two attributes', *Journal of the Royal Statistical Society*, 45, p. 586, Yule remarked that he choose the letter Q for Quetelet.

161 Yule, 'Association of Attributes in Statistics', p.7.

162 *Ibid.*, p.39.

163 *Ibid.*, pp.45-46 and p.39.

$$Q = \frac{(AB)(\alpha\beta) - (A)(\alpha\beta)}{(AB)(\alpha\beta) + (A)(\alpha\beta)}$$

in which A and B were used to denote the number of objects or individuals with the qualities A, B, while the corresponding Greek letters were used to denote the number of objects or individuals who were 'not A' or 'not B' and AB denoted number of objects or individuals who were both A and B. The more usual notation of Yule's coefficient of Association was expressed as

$$Q = \frac{ad - bc}{ad + bc}$$

where a, b, c, and d represent the frequencies in the four quadrants of a fourfold table. Yule found that Q was always slightly in excess of the Pearson's tetrachoric correlation coefficient.

Having established the mathematical properties of the tetrachoric correlation and the phi coefficient, Pearson then examined Yule's coefficients of association Q, the coefficient of colligation W and the Boas-Yulean phi-coefficient. Pearson thought that 'the primary reason to use Q was because it was freed from the assumption of an underlying normal distribution'. Yule did not determine the mean of the arrays for Q, instead gross groupings under certain divisions were given instead. Thus the measure of association was not taken as something peculiar to the normal distribution but something significant for all distributions whatever.[164]

THE COEFFICIENT 'C' AND THE χ^2 TEST OF INDEPENDENCE FOR CONTINGENCY TABLES

Four years later, Pearson introduced these three new measures for contingency tables: C the contingency coefficient, χ^2 the square contingency coefficient, and ψ the mean contingency coefficient. Whilst the tetrachoric correlation and the phi coefficient could be used with fourfold tables only, with the introduction of the first two techniques (χ^2 and C) the analysis of contingency tables was extended to manifold tables. The object of this paper 'was to deal with the novel conception of what

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164 Pearson, 'MCTE. VII. On the Correlation of Characters not Quantitatively Measurable' (1900), 17-18.

[Pearson] termed *contingency* and to see its relation to [the] older notions of association and normal correlation'.[165] The practical importance of this paper appeared to be great, for it freed the biometricians from dealing with scale orders and from the need for supposing normal frequency. Since the contingency is independent of the order of grouping, Pearson concluded that

> when we are dealing with alternative and exclusive sub-attributes, we need not insist on the importance of any particular order or scale for the arrangement of the sub-groups.[166]

Thus, for Pearson the great value of the idea of contingency for biometric, economic and social statistics was being able to use nominal or ordinal scales of measurement. He then termed 'any measure of the total deviation of the classification from independent probability a measure of its contingency'.[167] Thus, when taking

> any measure of contingency any function whatever of such quantities as $N_{nv}-Y_{nv}$ [theory-observation] its magnitude will be absolutely independent of the order of classification, i.e., its values will be unchanged if we re-arrange the A's and the B's in any manner whatever. This [was] the fundamental gain of this new conception of contingency.[168]

The conception of contingency led at once to the generalisation of the notion of the association of two attributes developed by Yule. Individuals could now be classed into more than two alternate groups or into many groups with exclusive attributes. The contingency coefficient and the mean square contingency could then be used to determine the extent to which two such systems were contingent or non-contingent. This was accomplished by using a generalised theory of association along with the mathematical theory of independent probability. From this the notion of a pure contingency table was reached and the order of the sub-groups was of no importance whatsoever.

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165 Karl Pearson, 'Mathematical Contributions of the Theory of Evolution. XIII. On the Theory of Contingency, and its Relation to Association and Normal Correlation', *Drapers' Company Research Memoirs. Biometric Series*, I (1904), 3. [Hereinafter referred to as Pearson, 'MCTE, XIII: On the Theory of Contingency'.]

166 *Ibid.*, 34-5.

167 *Ibid.*, 34.

168 *Ibid.*, 6. Pearson's statistical thinking at this time is still influenced by his earlier work on mechanics. This can be detected in his discussion on contingency, which is independent of the order of classification, could be measured '...precisely as we can measure position or acceleration in a great variety of ways, so it is possible to measure contingency.'

He wanted to find a way that could bring contingency into line with the customary notions of correlation and association and would not require laborious calculations. He then showed that in his chi-square goodness of fit paper of 1900 if m'_1, m'_2, ... m'_n of any system of observed frequencies and if m_1, m_2,...m_n of any system of theoretical frequencies are known *a priori* then

$$\chi^2 = Sum \frac{(m-m')^2}{m} \qquad (6.1)$$

After calculating the χ^2 value, the probability (P) could be found from the χ^2 table which was 'the probability than in any trial a system m'_1, m'_2, ... m'_n of observed frequencies will occur, which deviates more than m_1, m_2,...m_n than the actually observed system does'.[169] Tables had been worked out by Palin Elderton giving the value P for a considerable range of χ^2 which were first published in *Biometrika* in 1902.[170]

If a measure of contingency was needed, the next step was to measure the deviation from the observed results from independent probability. Following this, the system of χ^2 for measuring frequency distributions could be used to correspond to the actually observed system of n_{nv} where

$$\chi^2 = S\frac{(n_{uv}-v_{uv})^2}{v_{uv}} \qquad \text{for cell frequencies} \qquad (6.2)$$

This was then the proper quantity to calculate and P would, therefore, be 'a measure of the extent to which the observed system was not compatible with a basis of independent probability'.[171] Pearson had thus introduced the χ^2 statistic (or the mean square contingency coefficient) to examine the difference between the observed and theoretical frequencies for cells in any size contingency table. An association between attributes could be made 'if P is small' (i.e., $P < .05$). Pearson then proposed that 1–P would be a 'proper measure of contingency' which he also referred to as the *contingency grade*. He then thought it would be convenient to have a name for a

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169 *Ibid.*.

170 William Palin Elderton, 'Tables for Testing the Goodness of Fit of Theory to Observation', *Biometrika*, 1 (1901), 159.

171 Pearson, MCTE. XIII: On the Theory of Contingency' (1904), 6.

function closely related to χ^2, and showed that ϕ^2 (which he introduced in 1900) was related to χ^2 as

$$\phi^2 = \chi^2/N$$

and ϕ^2 was the *mean square contingency*.

In this paper he used the symbol Σ (rather than S for summation) which was used specifically for 'a summation of all the *positive* contingencies' such that

$$\psi = \Sigma(n_{nv} - V_{nv}/N$$

and ψ was termed the *mean contingency*. Hence, any function of ψ or ϕ^2 would serve as a measure of contingency. He then showed that when choosing ϕ^2 and ψ in the case of normal correlation, then

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

which Pearson termed the *first coefficient of contingency*.

Thus, the coefficient of contingency became the coefficient of correlation when the groups are small. With a view of lessening the number of coefficients, Pearson adopted the following convention:

> Any expression or function of either the mean square contingency (ϕ^2) or the mean contingency (ψ) (or indeed of any other measure of the contingency), which, when the grouping are sufficiently small is theoretically equal to the coefficient of correlation—on the hypothesis of normal frequency—shall be termed a coefficient of contingency. All such coefficients of contingency must, on the same hypothesis, become equal on a sufficiently small grouping, and they will scarcely differ widely from each other when the frequency is not absolutely normal and the grouping is merely moderately small.[172]

In the case where the groups are numerous, a great deal of labour was involved in finding either the coefficient of contingency or the coefficient of correlation by the method of the mean square contingency as this 'depends upon the squaring of the contingencies and dividing by the frequency to be expected on the basis of independent probabilities'. This labour could be avoided by using the mean contingency ψ instead of the mean square contingency χ^2. He cautioned, however,

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172 *Ibid.*, 9.

that 'there was no simple relation between the correlation and the mean contingency such as exists between the mean square contingency and correlation'.[173]

If n_{nv} be the actual frequency of a group in the population, and N which combines the characters A_u and B_v, $V_{uv}k$ is the frequency of this groups on the hypothesis of independent probability, then n_{uv}—v_{uv} is simply a sub-contingency to summarise:

$S\frac{(n_{uv}-v_{uv})^2}{v_{uv}} = \chi^2$ was termed the 'square contingency'

$S\frac{(n_{uv}-v_{uv})^2}{Nv_{uv}} = \phi^2$ was the 'mean square contingency'

$\sum\frac{(n_{uv}-v_{uv})^2}{N} = \psi$, where Σ was the sum for positive (or negative) sub-contingencies only, is the 'mean-contingency'.[174]

Pearson stressed that 'any one of these expressions is a measure of the deviation of the system from independent probability, and therefore of the amount of association or correlation between the characters or attributes involved'. Hence, any function of these expressions was also considered to be a proper measure. His functions included:

(a) The contingency grade. This was 1 – P, where P was to be found from χ^2 by using the tables for the 'goodness of fit'.

(b) The mean square contingency coefficient = C_1 where

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

(c) The mean contingency coefficient = C_2 where C_2 could be found by using Table 6.11.

In the case of sufficiently small grouping and normal correlation he gave

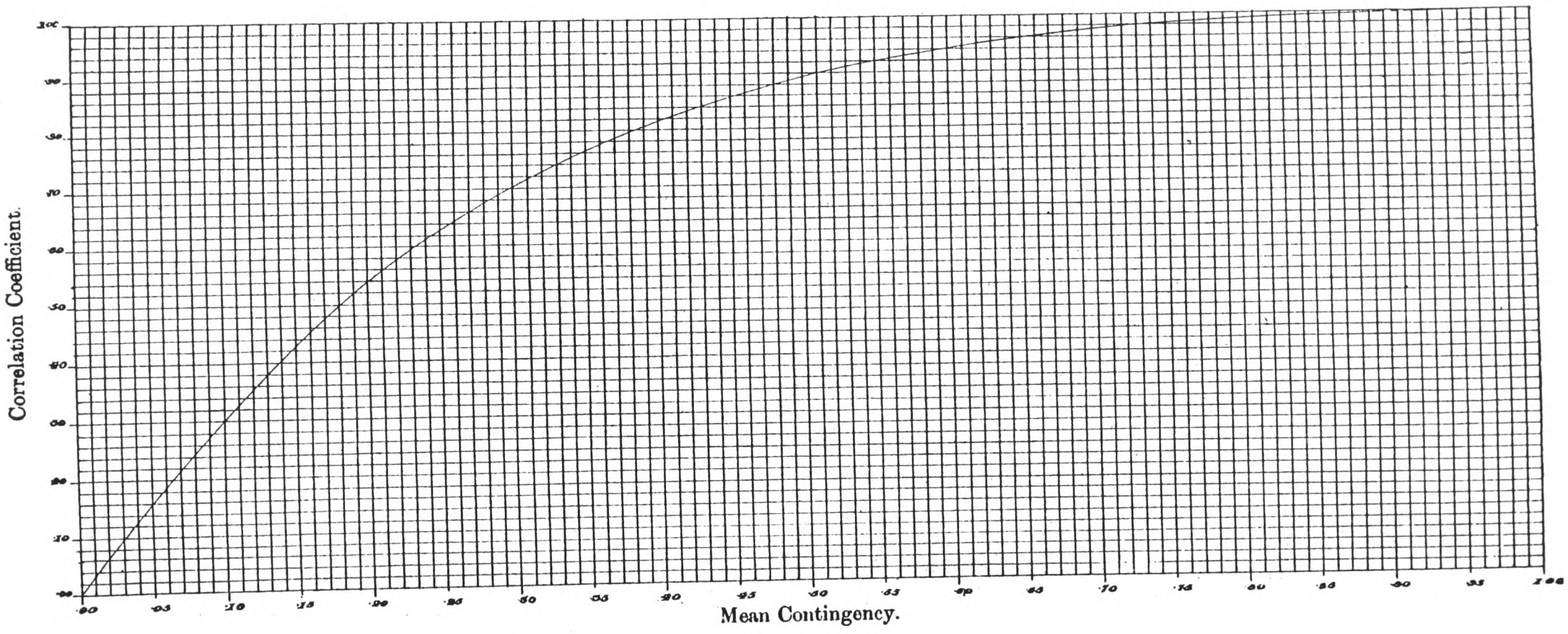
$$C_1 = C_2 = \text{coefficient of correlation}$$

The results he obtained from the above formulas when using contingency tables were exactly similar to those which occur when dealing with outlying individuals by the test for 'goodness of fit'. Pearson considered that the general conception of

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173 *Ibid.*.
174 *Ibid.*, 455.

Table 6.11
DIAGRAM FOR THE CONTINGENCY COEFFICIENT C AND THE TETRACHORIC CORRELATION COEFFICIENT (1904)



contingency developed in this memoir was of *theoretical* importance. He then went on to determine the probable error of the contingency coefficients.

Some nine years later Pearson determined the corrections (or degrees of freedom) for the chi-square contingency table. He considered (a) those due to the number of cells and (b) the correction for class-index. If x = number of rows and λ = number of columns, then on the average of many samples, the correction for number of cells is $(x-1)(\lambda-1)/N$ to be subtracted from ϕ_2. This was his chief but not the only correction for number of cells.[175] Another correction was for the use of class-indices which Pearson developed in 1913.[176]

In actual practice it was not desirable to take too many or too fine sub-groupings when using contingency tables. Under these conditions, the correlation coefficient as determined by the product moment or fourfold division methods (i.e., the tetrachoric correlation) was approximated more closely in the case of the contingency coefficient found from mean square contingency (i.e., χ^2). Pearson also thought that '16 to 25 sub-groups would have given fairly good results in the case of the mean square contingency'.[177]

He concluded that contingency may be applied 'of course at first tentatively and with caution' to the whole class of problems in which no attempt at a scale or order of sub-groups was possible, in short, where an alphabetical order was considered to be as good as any other: this was also seen as being applicable to 'a vast range of problems of exclusive and alternate inheritance'.[178] For the intermediate case, when one variable was continuous and the other discrete he introduced a coefficient in 1909 (termed later the biserial correlation).[179]

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175 Karl Pearson and David Heron, 'Theories of Association', *Nature* (1913), 215-6. See also Karl Pearson, 'On the Measurement of the Influence of "Broad Categories" on Correlation'. *Biometrika*, 9 (1913), 116-39.

176 *Ibid.*

177 Pearson, 'On the Theory of Contingency'.

178 *Ibid.*, 37.

179 Karl Pearson, 'On a new method of determining correlation between a measured charactaer A, and a character B, of which only the percentage of cases wherein B exceeds (or falls short of), a given intensity is recorded for each grade of A.' *Biometrika*, 7 (1910), 96-145.

Whilst the biometricians used statistical methods for continuous variables in the nineteenth century, by the time Pearson had introduced the square contingency coefficient (which he also referred to as the chi-square test of independence) in 1904, they could employ a full range of statistical methods for variables that could be classified as discontinuous or discrete. This included, in particular, Mendel's factors (or alleles) such as 'blue' and 'brown' eye colours which Pearson regarded as the best example of discrete data for the chi-square contingency coefficient.

MENDEL AND THE CHI-SQUARE

The determination of 'the probable error of the individual frequency in measuring the goodness of fit' when examining empirical and theoretical Mendelian distributions was first tested by W.F.R. Weldon in 1901. This was followed up later by Wilhelm Johannsen and soon afterwards by the Mendelian workers in the Biometrics Laboratory.[180] Weldon, who published five papers on Mendel between 1901 and 1904, used the chi-square goodness of fit test on Mendel's set of seven discrete characters of the common sweet pea (*Pisum sativum*) in his first paper on Mendel (published in the first volume of *Biometrika*).[181] From 1904 to 1917, Pearson published 11 papers relating to the application of Mendelian data for his statistical methods: in 1908 he made an attempt to synthesise Mendel with his biometric procedures 'from suggestions and notes of the late W.F.R. Weldon'.[182]

The belief that the biometricians analysed continuous variables only in their work on inheritance, can be traced to at least 1912 when Charles Percy Sanger

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180 *Ibid.*, 331.

181 Weldon, 'Mendel's Laws'. Pearson also referred to his two articles on, 'Mathematical Contribution to the Theory of Evolution. XII. On a Generalised Theory of Alternative Inheritance with special reference to Mendel's Laws', *Philosophical Transactions*, 203, A (1904), 53-86 and 'The Theory of the Ancestral Contribution in Heredity', *Royal Society Proceedings*, 81 (1909), B, p.225. His work was followed by a whole series of papers from some of the biometricians in the Biometric Laboratory (See for example Jacobs, *Proceedings of the Royal Society,* 84, p.23; Snow, *Proceedings of the Royal Society*, 83 B, 37 and *Nature*, 8, 420.)

182 Karl Pearson, 'On a Mathematical Theory of Determinantal Inheritance from Suggestions and Notes of the late W.F.R. Weldon', *Nature*, 6 (1908), 80-93.

remarked that 'the biometricians never analysed discrete units'.[183] This led Pearson to remark that the use of discrete variables could be found 'in Galton's paper of the treatment of eye-colour in mane and of coat-colour in Basset hounds [where] Galton himself treated them as discrete quantities and applied 'his laws' to them'.[184] It will be shown in the next chapter that the framework underpinning the historiographical debate on the Biometrician-Mendelian debate is grounded in the belief that Pearson measured continuous variables only in his studies of inheritance.

The cases where discrete variables existed were considered by Pearson to be

> exceedingly few in number; they exist[ed] as a rule only in the class-names not in the things classified. Personally [Pearson and Heron had] rarely found them except as already stated in *theoretical* Mendelian investigations.[185]

When looking at 'the usual case, however, of Mendelian practice', Pearson and Heron maintained that what was needed was 'not the value of correlation, but an investigation of whether observation is a reasonable fit to theory, i.e., the ordinary 'goodness of fit test'.[186] They further maintained that 'Pearson has never used a normal correlation coefficient on a true-fourfold table [i.e., with underlying *continuous* distribution] which he believed to be Mendelian in character'.[187] Though such individuals as 'Dr Brownlee had used continuous methods on Mendel.[188] The Biometric School criticised

> not the application to Mendelian theory of these methods in which they only followed what had already been done, but Dr. Brownlee's applying tetrachoric r to such theoretical tables and then suggesting that he had got at root of the difference between theoretical and actual heredity correlation tables![189]

If there were 'only alternative categories, e.g., the total (RR)'s compared with the total of (DR)'s + (DD)'s Pearson's [chi-square] test [was] appropriate'. (Pearson's RR's were the two recessive alleles, his DR's corresponds to one recessive and one dominant allele and the DD's are the dominant alleles.) If the 'assumption made is that

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183 Charles Percy Sanger, 'Discussions of Mr. Yules' Paper', *JRSS*, 75 (1912), 646.
184 Pearson and Heron, 'On Theories of Association', 210.
185 *Ibid.*, 239.
186 *Ibid.*, 213.
187 *Ibid.*, 210.
188 Brownlee, *Proceedings of the Royal Society, Edinburgh*, 30, 473.
189 Pearson and Heron, 'On Theories of Association', 259.

a Mendelian character is a discrete unit' then Pearson maintained that the biometricians were 'absolutely the first to apply [statistical] methods [for discrete variables] treating the Mendelian theoretical characters as units'.[190] Pearson's, Weldon's and the biometricians use of the chi-square test for Mendelian data will be examined more fully in the next chapter.

THE DEBATES ON CONTINGENCY TABLES

Karl Pearson and David Heron became entangled in a series of acrimonious disputes with George Udny Yule on various measures of association beginning in 1911 and culminating in 1913 when Pearson and Heron wrote a 156 page article on 'Theories of Association'. The article was written to 'reply to certain criticisms, not to say charges, Mr. Yule [had] made against the work of one or both of [them]'.[191]

Following the publication of George Udny Yule's book *An Introduction to the Theory of Statistics* in December 1910, David Heron replied to some of Yule's criticisms of Pearson's methods of association in an article in *Biometrika* some seven months later on 'The Danger of Certain Formulae Suggested as Substitutes for the Correlation Coefficient'.[192] Heron compared Yule's coefficient of association with the normal product-moment correlation coefficient and showed that Yule's consistently produced much higher values of correlation (see Table 6.12). In this example Yule wanted to determine the association between advancing date of 'blindness' and 'mental derangement': Heron used Yule's material for males only when calculating Yule's coefficient of association and Pearson's tetrachoric correlation.

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190 *Ibid.*, 210.
191 *Ibid.* 160.
192 Heron, 'The Danger of Certain Formulae', 109-22.

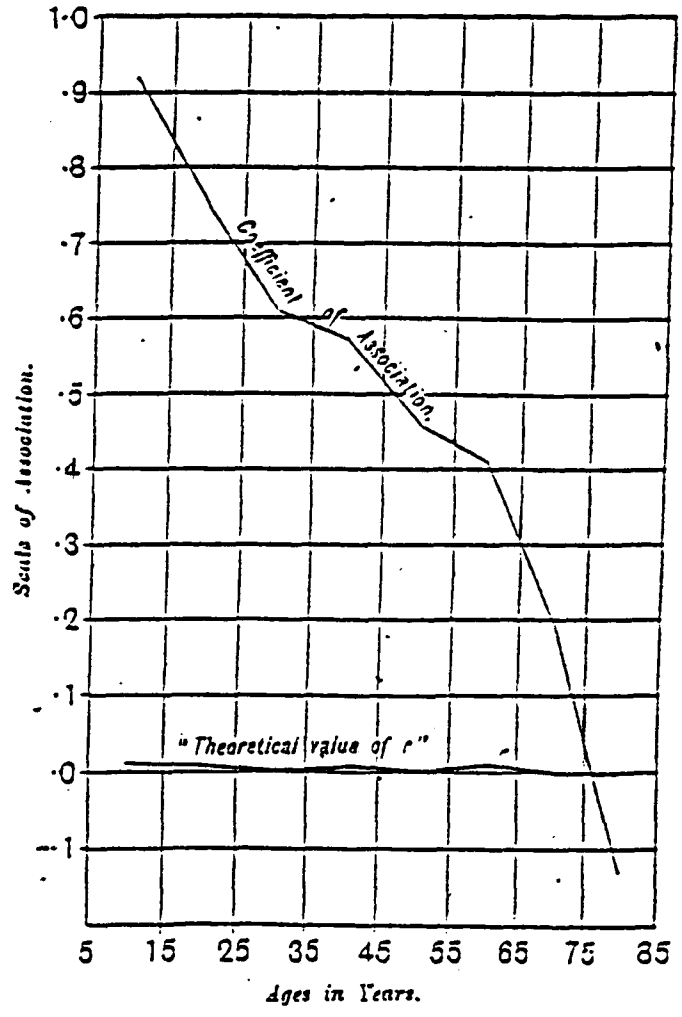
Table 6.12
THE RELATIONSHIP BETWEEN BLINDNESS AND MENTAL DERANGEMENT FOR DIFFERENT AGE GROUPS

Age Group	Coefficient of Association	"Theoretical Value of *r*"
5–	.921± .013	.011
15–	.753± .034	.006
25–	.607± .049	.005
35–	.572 ± .041	.006
45–	.459 ± .048	.005
55–	.412 ± .049	.006
65–	.198 ± .070	.003
75–	– .126 ± .111	- .003

Heron then plotted a series of values using both methods which revealed a considerable discrepancy between the two methods (see Figure 6.16). In this book, Yule presented another method of association as

$$Q' = \frac{N\alpha}{\sqrt{(A)(\alpha)(B)(\beta)}}$$

Figure 6.16
David Heron's Diagram of the Discrepancy Between Pearson's and Yul\e's Measure of Association (1911)



Heron then showed that when this method was expressed in Yule's 'more usual notation', this was nearly identical to the formula that Pearson had introduced for his

phi coefficient some ten years earlier. At that time, however, Pearson did not use the formula for as a measure of association, as Yule had proposed, but rather it was a measure of the correlation between errors.

In 1912, Yule introduced the coefficient of colligation 'w' in his paper 'On the Methods of Measuring Association between Two Attributes'.[193] Pearson and Heron considered that this paper was 'singularly acrimonious' in his account of Pearson's methods of association.[194] Moreover, they also thought that

> if Mr. Yule's views are accepted, irreparable damage will be done to the growth of modern statistical theory. Mr. Yule has invented a series of statistical methods which are in no case based on a reasoned theory, but which possess fascination of very easy and ready application, and therefore are at one seized upon as applicable to all sorts of problems by those without adequate training in statistical theory or without mathematical knowledge requisite to weigh cautiously their logical basis.[195]

They were concerned with the discussion at the Royal Statistical Society with regard to a paper read by Charles Percy Sanger on the history of association.[196] Sanger claimed that 'all statisticians before Mr. Yule had this passion for the normal curve'.[197] Heron and Pearson thought that this statement was 'not only unfair to [the Italian statistician, Luigi] Perozzo, but to one of us [i.e., Karl Pearson] to whom Mr. Yule was attacking'. Responding to Sanger's claim, they went on to say that Pearson had first discussed a means of coping with distributions of varying shapes in his memoir on 'Skew Variation in Homogeneous Material' which he presented to the Royal Society on 19 December 1894. Yule's memoir on association, to which Sanger had referred, had been presented to the Royal Society some five years later on 20 October 1899. Moreover, Yule had not only attended the statistical lectures of Pearson, but he had been an assistant to him

> during a period when nearly the whole work of the statistical laboratory on non-Gaussian distributions. A collection was then made of non-Gaussian material with a view to dealing with the correlation of continuous and non-Gaussian variates.[198]

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193 George Udny Yule, 'On the Methods of Measuring Association between Two Attributes', *Journal of the Royal Satistical Society* (1900), 7-69.
194 Pearson and Heron, 'On Theories of Association', 159.
195 *Ibid.*, 159-60
196 Sanger, 'Discussion of Mr. Yules' Paper', 642.
197 Pearson and Heron, 'Theories of Association', 160.
198 *Ibid.*, 161.

At the time when Pearson and Heron were writing the paper, the Biometric Laboratory

> still possess[ed] the contour lines of the correlation drawn by Mr. Yule under Pearson's direction indicate extreme cases of what [Pearson] termed skew variation or wide deviation from the normal surface.[199]

They then explained that

> the promise made in 1894 to deal with skew correlation *surfaces* only remained unfulfilled because the differential equations to the surfaces obtained in that year have so far defied integration. Undoubtedly for continuous variates a generalised correlation surface should be the starting-point for attacking the problems of association. That it has remained unsolved shows only the extreme difficulty of the problem; it does not indicate that all statisticians before Mr. Yule had 'the passion for the normal curve'.[200]

Yule had not discussed 'whether his attributes [were] really continuous or [were] discrete, or whether the use of the term 'discrete' signified a true continuous variates'.[201] Class-indices such as 'death' or 'recovery' and 'employment' or 'unemployment' of mother were viewed as measures of continuous variates—which were not necessarily Gaussian. They argued that they were

> really seeking how far the continuous variate physical exertion of women affects infant welfare, and this is not a discrete variate any more than survival or death of infant is a discrete variate when you view them merely as class-indices of physical fitness to survive in the child. In other words, for the great bulk of attributes, to which Mr. Yule without analysis of their nature applies association, we should assert continuous variates.[202]

They thought that Yule confused 'discreteness' in the class-index ('a mere verbalism') with discreteness in the attribute classified under it; this reduced his investigations from 'the plane of practical statistics in the field where he had originally placed them [to] that of theoretical logic'.[203]

Pearson had tested Yule's coefficient Q against other similar coefficients and 'finding it less stable than any of them, it was dropped and ha[d] never been and never [would] be used in any work done under his supervision'.[204] Yule claimed that the

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199 *Ibid.*.
200 *Ibid.*.
201 *Ibid.*.
202 *Ibid.*, 161.
203 *Ibid.*, 171.
204 *Ibid.*, 162.

coefficient of association could be applied without any general theory of frequency whereas the theory of frequency distributions was the cornerstone of Pearsonian statistical theory. Moreover, 'since the full development of the contingency method, fourfold tables [such as the tetrahchoric correlation] have not been used by the Biometric School, except as controls, where contingency tables could be formed on the given data'.[205]

With Yule's coefficient of association passing into French statistical literature as *l'indice de corrélation* a term originally introduced by Galton for the coefficient of correlation, they protested against Yule's coefficients of association and claimed that if 'not made now and strongly, there will be a great set-back to both modern statistical theory and modern statistical practice'.[206] They further maintained that there was 'no universal method of dealing with an n x n table, except the method of mean square contingency (i.e., ϕ^2) leading to a probability measure of the independence of the two characters'.[207] They wanted to show that Pearson's phi and Yule's Q contradicted each other; thus, both could not be valid.

> We should most likely use ϕ [phi], although certainly not in the sense of a correlation of four points. We should question how far death and recovery are independent of treatment or non-treatment; that is to say, we should ask what is the probability that recovery was independent of treatment. If n be the total number of observations, then if $n\phi = \chi^2$ be calculated, the chance of independence is at once given by finding the P corresponding χ^2 from the tables for 'goodness of fit'.[208]

DONALD MACKENZIE AND 'THE POLITICS OF THE CONTINGENCY TABLE'

The historiography of Pearson statistical work on contingency tables and on the chi-square in particular has been limited in scope with the exception of Donald Mackenzie, who has written a fairly extensive account of Pearson's and Yule's work on contingency tables and measures of association. Particular attention will be given

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205 *Ibid.*, 182.
206 *Ibid.*, 299.
207 *Ibid.*, 300.
208 *Ibid.*, 176-7.

to Mackenzie's analysis as his argument embodies the claim that the impetus of Pearsonian statistics arose from Pearson's interest in eugenics.

Mackenzie's interpretation of Pearson's statistical work, which is underpinned by his social-constructivist outlook, has neither captured the 'internalistic' side of Pearson's work on goodness of fit testing nor Pearson's chi-square goodness of fit test. The chi-square test of independence (or the 'square contingency coefficient') is never addressed explicitly; when discussing Pearson's 1900 chi-square goodness of fit test for frequency distributions, Mackenzie refers to 'cell frequencies' instead (i.e., the frequencies contained in contingency tables which were devised in Pearson's paper of 1904). Moreover, little consideration has been given to Pearson's derivation of the exact chi-square distribution.

The primary focus of Mackenzie's analysis relates instead to Pearson's work on the tetrachoric correlation (r_2) and the contingency coefficient (C) in addition to Yule's measures of association (Q) and his measure of colligation (w). Thus less attention was given to the following contingency coefficients: the mean square contingency (ϕ^2), the mean contingency (ψ), Pearson's phi coefficient (ϕ) and, as mentioned above, the square contingent coefficient χ^2 (or the chi-square statistic for contingency tables).

Though Mackenzie argued, quite rightly, that from 1900 onwards Pearson's statistical work shifted to the analysis of nominal or discrete variables and that the invention of the tetrachoric coefficient by no means concluded Pearson's theoretical work on the measure of association, he also argued that 'this area was a major focus of his work in mathematical statistics from 1900-1922'.[209] Yet Pearson published some 51 papers on other aspects of 'mathematical-statistics' quite separately from his nine mathematical-theoretical papers on contingency tables (of which three were on the tetrachoric) during this time period. Of these 51 papers, 16 dealt with other measures of correlation (e.g., the biserial correlation coefficient, the correlation ratio η^2 as well as additional work on the product-moment correlation, partial and multiple correlation

209 Mackenzie, *Statistics in Britain 1865-1930*, p.157.

from 1900 to 1922); 13 covered skew curves and skew variation (from 1901 to 1931); 10 papers examined frequency constants and frequency curves (from 1903 to 1936); in addition to the chi-square goodness of fit paper of 1900, he published seven more papers on the chi-square plus four other papers on testing the goodness of fit (from 1911-1934). Thus Pearson's work in statistical theory, in the twentieth century, would not seem to have been confined primarily to the tetrachoric correlation. Nonetheless, Mackenzie argues that the tetrachoric correlation coefficient was important to Pearson because

> 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 analysing it... such as eye colour, and a range of mental characteristics such as 'ability' and 'conscientious' in 1898... to prove the dominance of nature over nurture.[210]

He further argues that

> Pearson thus had, by use of the tetrachoric coefficient, been able to forge a connection between physical and mental human characteristics along which inductive inferences could pass... [which he thought] were largely determined by heredity... The polemical possibilities that this opened up for eugenists were obviously important.[211]

The tetrachoric correlation and related developments were seen as being part of 'the distinctive approach of the biometric school [which] were widely applied to empirical data, primarily in the eugenic field'.[212] Moreover, the 'tetrachoric was [regarded as] a social institution as it was needed for the school's eugenic work' and was used in 'the typical manner of 'biometric eugenics' to argue for the importance of the hereditary factor in tuberculosis'.[213] Mackenzie thus gave little consideration to Pearson's hereditarian studies at the turn of the century when his attention was directed towards the application of Mendelian distributions to the chi-square goodness of fit test. Moreover, Pearson considered it 'inappropriate to use the tetrachoric

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210 *Ibid.*, p.171.
211 *Ibid.*, p.172.
212 *Ibid.*, p.176.
213 *Ibid.*, p.177. Mackenzie supports this by citing Major Greenwood's papers 'The Problems of Marital Infection in Pulmonary Tuberculosis', *Proceedings of the Royal Society of Medicine*, 2 (1909), 259.

correlation for Mendel or for any situation in which the variables were not continuous and assumed an underlying normal distribution'.[214]

Pearson thought it would have been 'convenient to have a name for a function closely related to χ^2' which he termed the mean square contingency or ϕ^2 and was equal to χ^2/n.[215] Mackenzie, however, interpreted this to mean that 'the value of χ^2 [could] not serve as a measure [of association]'; instead, it was thought that the χ^2 could be used only in conjunction with the mean square contingency coefficient ϕ^2.[216] Dividing χ^2 by N, however, merely shows the mathematical relationships between ϕ^2 and χ^2 for 2 x 2 tables only; this mathematical relationship cannot be expressed with manifold tables and would not seem to suggest that 'χ^2 cannot serve as a measure'.

When comparing the contingency coefficient with the tetrachoric correlation, Mackenzie thought that

> C_1 [the contingency coefficient] did not displace r_t [the tetrachoric correlation] in Pearson's affection. Person 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 did not hold, and thus C_1 was a bad estimate of the correlation of underlying variables.[217]

In his paper of 1904, Pearson, however, recommended that the 'mean square contingency' (i.e., the χ^2 statistic) could be used for up to 'probably 16 to 25 sub-groups [which would] give fairly good results in the mean-square contingency'.[218]

In his closing argument, Mackenzie suggests that modern statistical theory has its foundations in the work of Yule rather than Pearson as

> contemporary statistical opinion tends to deny that any one coefficient has unique validity. To take one influential opinion Goodman and Kruskal argued that measures of association 'should be carefully constructed in a manner appropriate to the problem in hand' in such a way as to have operational interpretations. This type of theoretical pluralism seems to be sufficiently widespread to allow us to conclude that the general approach of modern statistics is closer to Yule than that of Pearson.[219]

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214 Pearson and Heron, 'Theories of Association', 210.
215 Pearson, 'MCTE. XIII. On the Theory of Contingency', 6.
216 Mackenzie, *Statistics in Britain 1865-1930*, p.158.
217 *Ibid.*, p.159.
218 Pearson and Heron, 'Theories of Association', 475.
219 Mackenzie, *Statistics in Britain 1865-1930*, p.180.

He justifies this with the belief that 'Yule's Q remains a popular coefficient, especially among sociologists (see, for example, Davis 1971)'.[220] Yet the statistical work of Davis dealt with the methodological procedures known as 'Q sorting technique' (rather than Yule's Q coefficient) which Davis used in conjunction with such multivariate statistical procedures as factor analysis. William Stephenson introduced *Q methodology* in 1953 as a general name to 'characterise a set of philosophical, psychological, statistical and psychometric ideas oriented to research on the individual'. *Q technique* is a set of procedures used to implement Q methodology.[221] The technique involves a method of rank-ordering cards (called *Q sorts*) and then calculating the Spearman rank-order correlation coefficient between persons. This may be further analysed by using factor analysis (as Davis had used) or by using a multivariate analysis of variance.

Modern statistical theory could perhaps be better viewed as the synthesis of probability theory (incorporating Pearson's relative frequency approach), error theory and the mathematical theory of determinantal matrix algebra which, for Pearson, were underpinned by his goodness of fit testing. It would thus seem that the creation of a modern theory of statistics owes more to the work of Pearson than the work of Yule. This is not to dismiss the contributions that Yule made to statistical theory (particularly his work on multiple and partial correlation plus his use of the method of least squares to interpret statistical regression), but there seems to be little evidence to support Mackenzie's claim that 'the general approach of modern statistics is closer to Yule than that of Pearson'.

CONCLUSIONS

It has been shown that the underlying feature distinctive of Pearson's statistical work was the determination of an empirical measure of the goodness of fit for theoretical

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220 *Ibid.*

221 Fred N. Kerlinger, *Foundations of Behavioral Research*, 2nd ed. (New York: Holt, Rinehart and Winston, Inc., 1973), p.582. For an early account of Q methodology see William Stephenson, *The Study of Behavior* (Chicago University Press: Chicago, 1953).

and empirical distributions. This work began in November 1892 when he introduced his method-of-moments in one of his Gresham lectures and used the sixth moment as an empirical measure of the goodness of fit; this found further expression in 1894 when he introduced the forerunner of the chi-square goodness of fit test, and culminated in 1900 when he devised the (P, χ^2) chi-square goodness of fit test. With the introduction of the chi-square distribution and the P, χ^2 goodness of fit test. Pearson's theoretical work was nearly finished: by then he had developed the mathematical infrastructure of a modern theory of statistics by synthesising probability theory, the theory of frequency distributions, and the mathematical theory of matrix algebra into the corpus of his statistical theory.

Whilst the impetus to his statistical work originated with problems in evolutionary biology (especially those connected with speciation and natural selection), application was made later to problems of vital statistics, economics, barometrics and of inheritance. The final phase of the Pearsonian corpus of statistical theory came to fruition in the early part of the twentieth century when he developed statistical methods for the analysis of manifold contingency tables with the χ^2 statistic (or the 'square contingency coefficient') in addition to further work in significance testing. George Udny Yule sparked an interest in contingency tables with his measures of association and quite separately introduced the method of least squares of regression.[222]

Pearson's work on curve fitting and his chi-square goodness of fit test was to underpin much of the mathematical statistical work in the Drapers' Biometric Laboratory in the twentieth century. Under his supervision, a number of his students would produce a series of innovative theoretical papers on this topic. His system would be used for problems of physical anthropology when curve fitting skull measurements derived from their work on English skulls in addition to their work on

222 See Stigler's discussion on Yule and least squares in *History of Statistics*, pp.349-53, 359.

Egyptian and French skulls.[223] The continuation of the statistical work that was devised in the Biometric School, in addition to the establishment of the Drapers' Biometric Laboratory in 1904 and the Galton Eugenics Laboratory, in 1907, will be examined in the next chapter. It will be shown that the subsequent development of two different methodologies, arose from varying and separate problems in both laboratories.

A quantitative resolution of Mendelian inheritance *vis-à-vis* Galton's law of ancestral heredity would become central to the joint biometrical projects Pearson undertook with Weldon from 1901 until Weldon's death in 1906. Pearson's personal loss of his colleague and closest friend was to become a major contributory factor in the shift of the locus of research in the Drapers' Biometric Laboratory. This loss would also become an emotional burden for Galton who was to relinquish control of the Eugenics Record Office seven months after Weldon's death.

The mathematical-statistical theory and methods of the Drapers' Biometric Laboratory, that Pearson developed in the Biometric School in the 1890s, emerged firstly and primarily from the impetus of Weldon and later from that of Galton. After 1906, the biometricians would undertake research on craniometry and physical anthropology. This work would lead subsequently to the development of craniometric instruments and other quantitative methods in the twentieth century. Pearson's attempts to deal with problems in the Eugenics Laboratory in 1907 was to engender the development of a new methodology underpinned by his use of a complex-interconnecting network of family pedigrees and by the adoption of actuarial death rates. This latter development, which has not yet been addressed in the historiography of Pearsonian statistics, will also be examined in greater detail in the next chapter.

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223 When William Palin Elderton reviewed William Flinders Petrie's book on *Migrations*, he commented on the necessity of using 'a systematic method of curve fitting' rather than assuming that 'skewness is due to heterogeneity and that the skew material can be split up into two curves as had been done by Flinders Petrie. William Palin Elderton, 'Notices and Bibliography' (Book review of *Migrations* by William Flinders Petrie). *Biometrika*, 5 (1906), 480.

Chapter VII

THE DRAPERS' BIOMETRIC LABORATORY AND THE GALTON EUGENICS LABORATORY

Pearson was to establish and run two laboratories during the first three decades of the twentieth century. The Drapers' Biometric Laboratory was the older of the two and grew out of the Biometric School which had begun about 1892. The number of students in Pearson's Biometric School grew from about four students in 1893 to nearly 20 at the end of the nineteenth century. In 1903 Pearson established the Drapers' Biometric Laboratory after he had received a grant from the Worshipful Company of Drapers' (from whom he would receive funding until his retirement in 1933). By then, his work and his School had achieved international recognition with students coming from America, France, Germany, Italy, India, Japan, Russia and Sweden to learn Pearson's biometrical methods. From 1903 to 1918 the total number of memoirs and papers published by those working in the Drapers' Biometric Laboratory was 297 and 62 of his students had published articles in *Biometrika*.[1]

The bulk of the original Drapers' grant enabled Pearson to do the following: (1) supply the statistical section with such instruments as calculating machines and integrators, (2) build and equip the Drapers' Company Transit House which was the foundation of the teaching of practical astronomy at University College London and (3) pay for the printing and publication of original work. The general aim of the expenditure was to 'maintain a statistical school for post-graduate work'.[2] Under the direction of Karl Pearson, the Drapers' Biometric Laboratory was to provide 'a

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1 Karl Pearson, 'History of the Biometric and Galton Laboratories', Appendix 1, Report to the University of London, 1903-1909, KP:UCL/246; Karl Pearson, 'Report on the Galton and Biometric Laboratories especially with regard to their Expenditure', March 1925, KP:UCL/223, p.1.

2 Pearson, 'Report on the Galton and Biometric Laboratories', March 1925, p.1.

complete training in modern statistical methods and [was] especially arranged so as to assist research workers engaged on biometric problems'.[3]

The Eugenics Record Office was established in the autumn of 1904 when Francis Galton gave the University of London £1,000 for 'the furtherance during three years of the scientific study of eugenics'.[4] The Office was set up in two rooms at 50 Gower Street and the staff consisted of Edgar Schuster as Research Fellow and Ethel Elderton as his assistant. (Schuster, who had investigated the variation of snails in Naples, had been recommended by W.F.R. Weldon.[5]) Galton supervised this office himself. Pearson's role was minimal: he was not on the Advisory Committee for the Eugenics Record Office nor had Galton 'ever consulted [him] ... as to the research in his office'.[6] According to Pearson, his only relation to the Office was that 'its members used to come in occasionally to the Biometric Laboratory for friendly consultation and advice'.[7]

Two years later, in 1906, Schuster retired as he wanted to undertake more purely biological work. Galton, who was surprised at Schuster's resignation, wrote to Pearson on 24 October as he wished that the Eugenics Record Office could 'somehow be worked into your biometric laboratory, but I am far too ignorant of the conditions to make a proposal.... If any feasible plan occurs to you, pray tell me'.[8]

An agreement for Pearson's takeover was to be made by the end of the year. Thus, the Galton Eugenics Laboratory (as renamed by Pearson) was established on 1 February 1907 under the direction of Pearson, in two small rooms at 50 and 85 Gower Street. Galton was to provide £500 a year during his lifetime and would leave the residue of his estate of £30,000 to UCL in 1911.[9] The Laboratory was to carry on

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3 Karl Peason, Frontispiece of the first volume of *Annals of Eugenics* (1926).
4 Pearson, *Life of Galton*, IIIA, p.258. Galton had requested that it should be titled the Eugenics Record Office.
5 W.F.R. Weldon, Letter to Karl Pearson, 25 June 1900, KP:UCL.
6 Pearson, *Life of Galton,* IIIA, p.258.
7 Karl Pearson, Report on the Galton and Biometric Laboratories especially with regard to their Expenditure, March 1925, KP:UCL/233, p.1.
8 Pearson, *Life of Galton,* IIIA, p.297
9 Pearson, 'Report to the Drapers' Court.' (March 1918), p.3.

Galton's aim of determining 'those causes under social control that may improve or impair the racial qualities either physically or mentally'.[10] From the beginning, Pearson recognised that the statistical methods he devised and the instruments he used in the Drapers' Biometric Laboratory were not adequate to answer some of Galton's questions on matters relating to eugenics.

It will be shown that the two laboratories not only had separate goals, but that Pearson devised different quantitative procedures and statistical methods and also utilised different types of instruments for the various problems that arose in the laboratories. In spite of the ongoing historiographical interests in Pearson and eugenics, there seems to have been no attempt made to examine the methods and techniques that underpinned the Galton Eugenics Laboratory. It will be argued that the principal methodology in Pearson's Eugenics Laboratory involved the use of family pedigrees and actuarial death rates, whereas the methodology of the Drapers' Biometric Laboratory was underpinned by curve-fitting and goodness of fit testing. (The biometricians, however, used selective death rates when looking for disturbances in the distribution from normality to detect empirical evidence of natural selection as a consequence of differential mortality rates.) Whilst the specific Pearsonian statistical methods and the underlying mathematical theory that were devised in the Biometric School (from 1892 to 1903) have been examined in the preceding four Chapters, in this chapter, consideration will be given to those statistical methods and the instruments used in the Drapers' Biometric Laboratory from 1903 to 1936.

The historiography of Pearson's work in the Drapers' Biometric and the Galton Eugenics Laboratories has been underpinned by the belief that the methods used by Pearson and his co-workers for biometrics and eugenics were inextricably bound to each other to the extent that no distinctions can be made between the methods, procedures and tools of the two laboratories. Thus, Lyndsay Farrall, for example, claimed that

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10 *Ibid.*

> both Pearson and Galton looked to the methods of the biometric school as the suitable methods for the study of human evolution to which Galton had given the name eugenics... The methods used were the statistical methods that [Pearson] had developed in response to Weldon's questions.[11]

Moreover, the developments associated with the laboratories in the twentieth century, seems to have engendered the belief that Pearson's work in the nineteenth century played a minor role in the development of Pearsonian statistics. When Gigerenzer, Swijtink, Porter, Datson and Beaty discussed Pearson's work they thought that

> the work of Karl Pearson and the biometric school could conveniently be dated to 1900, when Pearson published his chi-square test of 'goodness of fit'.[12]

HISTORIOGRAPHICAL INTERPRETATIONS OF PEARSON'S LABORATORIES

Writing on Galton and eugenics in 1952, C.P. Blacker thought that Pearson considered 'biometry which includes anthropometry and psychometry [to have formed] the scientific basis of eugenics'.[13] Whilst Pearson does say that 'the science of eugenics is in fact only highly developed and *applied* anthropology [which would] demand of the student intensive preliminary training in mathematical statistics, anthropology and general genetics', he also considered that 'the study of eugenics centre[d] around the actuarial treatment of human societies in all of its phases, healthy and morbid'.[14] Pearson does not use the term biometry to describe his procedures nor does he seem to imply that biometry formed the 'scientific basis of eugenics'. As Blacker uses the term 'biometry' differently from Pearson, his statement could be misleading; it would seem that consideration must also be given to Pearson's principal methods of eugenics which drew upon a set of quantitative procedures that came from actuarial offices and from family pedigrees (which seems to have been borrowed from genealogical charts).

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11 Lyndsay Farrall, 'The Origins and Growth of the English Eugenics Movement. 1865-1926' (PhD Thesis, Indiana University, 1970), p.101.

12 Gerd Gigerenzer, Zeno Swijtink, Theodore Porter, Lorraine Datson, John Beatty and Lorenz Krüger, *The Empire of Chance* (Cambridge University Press, 1989), p.79.

13 C.P. Blacker, *Eugenics: Galton and After,* (London: Duckworth, 1952), p. 105.

14 Karl Pearson, 'Foreward', *Annals of Eugenics*, (1928), p.1; see Pearson's Robert Boyle Lecture delivered before the Oxford University Junior Science Club on 17 May 1907: 'The Scope and Importance to the State of Science of National Eugenics' (Dulau: London, 1909), p.15.

When Lyndsay Farrall examined the two laboratories, two decades later, he claimed that

> the real bond between the two laboratories was the techniques of research used rather than the subject matter of research. In both laboratories the basic technique of research was the statistical analysis of large masses of observations on collected data.[15]

Though it may be said that the basic *approach* (rather than 'technique') involved the analysis of large masses of observation, this view also overlooks the complexity of the variety of different methods that were developed in the two laboratories. He then argued that as the biometric laboratory was already carrying out the work that Pearson felt was important research in eugenics it was also quite natural that two laboratories of which he was director, the Biometric and the Galton Eugenics Laboratories, should come to be seen by him as part of one institution.[16] Yet, Pearson would explain, in 1918, that the Galton

> Eugenics Laboratory...did not replace the Drapers' Biometric Laboratory...[as] their work was confined to a relatively narrow field, having nothing to do with statistical theory or its general application to biology.[17]

Donald Mackenzie, Bernard Norton, and Theodore Porter subsequently reached the same conclusion as did Farrall. Bernard Norton argued in 1978 that 'certainly in Pearson's time statistics was always associated with eugenics'.[18] However, this view has neither taken into consideration (1) the full range of statistical methods used in Pearson's time such as vital statistics, economic statistics, mechanical statistics, geometric statistics or government statistics, as well as the bulk of Pearson's statistical methods used in the Biometric Laboratory, that had no association with eugenics nor (2) those methods used in the Eugenics Laboratory which were not 'statistical'. Donald Mackenzie maintained that 'to separate a "neutral" biometry from "ideological" eugenics would be ahistorical and would fail to capture the integral notion of

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15 Farrall, 'English Eugenics Movement, 1865-1926', p.116.
16 *Ibid.*, p. 111.
17 Pearson, 'Report to the Drapers' Court , March 1918, p. 3.
18 Bernard Norton, 'Karl Pearson and Statistics: The Social Origin of Scientific Innovations', *Social Studies of Science*, 8 (1978), 5.

Pearson's thought'.[19] After Karl Pearson's son, Egon, read Mackenzie's article, he wrote to Bernard Norton, on 23 April 1978, that he had

> spent (and I fear rather wasted) two days reading Donald Mackenzie's offprint of his paper in the sociological journal. I won't make many comments as I know that he is a friend of yours, but I have written him a longish letter which I hope he won't find too discouraging.[20]

Mackenzie thought that 'the needs of eugenics figured large in [Pearson's] work in statistical theory'.[21] When he examined the methods used in both of Pearson's laboratories, he claimed that in Pearson's

> last report to the Worshipful Company of Drapers, Pearson warned of the need to keep statistical theory 'in touch with practical needs'... and there is no doubt that in his mind eugenics was the source of the most central of these practical needs. In reality, there seems to have been little clear demarcation between the Biometric and Eugenics Laboratories, which shared personnel, methods and problems. The Laboratories are best seen as a unified research institute pursuing, at least in the period up to 1914, a multi-faceted but still integrated research programme.[22]

Daniel Kevles, who shared Mackenzie's view, thought that

> roughly speaking, the statistical techniques for dealing with the data were developed in the Biometric Laboratory, and the anlysis was carried out in its Galton counterpart, but the symbiosis was so close as to make the distinction meaningless.[23]

Theodore Porter, who used much of Mackenzie's work, maintained that 'Pearson's eugenic conviction provided the principal explanation for the enthusiasm with which he took up the study of statistics'.[24] A number of other historians with interests in Pearsonian statistics have subsequently repeated these views. Rosaleen Love, for example, claimed that Pearson 'founded the Biometric Laboratory in order to collect statistics relating to eugenics' and Judith Walkowitz thought that Galton's

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19 Donald Mackenzie, 'Statistical Interests and Social Interests: A Case Study', *Social Studies of Science*, 8 (1978), 64.

20 Egon Pearson, Letter to Bernard Norton, 23 May 1978 (Paperrs of Bernard Norton, University of Leicester), p.1.

21 Mackenzie, *Statistics in Britain*, p.105.

22 *Ibid.*

23 Daniel Kevles, *In the Name of Eugenics* (Alfred A. Knopf: New York, 1985), p.39.

24 Theodore Porter, *The Rise of Statistical Thinking*, (Princeton University Press: Princeton, 1986), p.305.

Natural Inheritance 'converted [Pearson] to eugenics, the statistical study of human manipulation'.[25] Gigerenzer *et al.*, thought that it was worth

> considering ... the extent to which the biometricians and Mendelians were involved in eugenic research and eugenics propagandizing. Indeed the biometric and Mendelian school were the academic/intellectual *foci* of eugenical discussions during the last part of the nineteenth century and the first part of this century.[26]

Richard Soloway argued that the link between Pearsonian statistics and eugenics occurred

> even before [Pearson] turned his considerable mathematical talents to the cause of Galtonian eugenics in the early 1890's. Pearson called for careful evaluation of gender capacity and ability and tried to formulate accurate statistical procedures for measuring the hereditary contribution of each sex.[27]

Hence, the techniques and the methodologies in the two laboratories have tended to be glossed over; the general historiographical trend seems to suggest that since Pearson used a measure of correlation in both laboratories, no distinction can be made between methodologies in the two laboratories. Though Pearson devised a number of statistical methods to measure correlation (e.g. the product-moment, point-biserial and tetrachoric correlation coefficients; the phi coefficient; multiple and partial correlation; the chi-square test of independence and the contingency coefficient (C), in addition to the correlation ratio), specific measures of correlation are rarely identified. (Mackenzie has, however identified the tetrachoric correlation coefficient and the contingency coefficient.)

It will thus be argued that a critical examination of the underlying Pearsonian methodologies along with the conceptual ideas that underpinned the infrastructure of the Drapers' Biometric Laboratory and the Galton Eugenics Laboratory needs to be undertaken to determine the full extent to which Pearson's earlier work in the Biometric Laboratory (beginning with the Biometric School in 1892) may have influenced later developments in the Eugenics Laboratory in 1907. Such an

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25 Rosaleen Love, 'Feminism and Eugenics: Alice Lee and Ethel Elderton', *Annals of Science*, 36, (1979), 147; Judith R. Walkowitz, 'Science, Feminism and Romance: The Men's and Women's Club', *History Workshop*, 21 (1986), 148.

26 Gigerenzer, *et al.*, *Empire of Chance*, p.148.

27 Richard Soloway, *Demography and Degeneration* (University of North Carolina Press: Chapel Hill, 1990), pp.117-18.

undertaking may also help to determine if it can be said that Pearson ran two laboratories that supposedly utilised the same methodology, techniques and co-workers who, in turn, produced the same kind of articles (as has been argued in the historiography of Pearsonian statistics for more than half a century). It will then be shown that the one common feature shared between the two laboratories was their use of such elementary descriptive statistical methods as Pearson's standard deviation, the coefficient of variation and approximately four (of his 14) correlational methods.

In addition to the developments in the laboratories, there are a number of developments and events in the early twentieth century which will be addressed in this chapter and include the following: (1) the joint biometrical projects undertaken by Weldon and Pearson from 1898 to 1906; (2) Weldon's death in 1906 and its impact on Pearson's work; (3) the creation of the Francis Galton Chair of Eugenics in 1911 which led to the establishment of Pearson's Department of Applied Statistics; (4) the completion of the Bartlett Building, in the summer of 1914, to house the Drapers' Biometric Laboratory, the Galton Eugenics Laboratory, the Anthropometric Laboratory, an Anthropology Museum, a library and lecture rooms; (5) the founding of seven different journals by Pearson from 1900 to 1928 (apart from *Biometrika* which he co-founded) which would become the organs of the two laboratories; (6) the statistical work undertaken by Pearson and those who worked in the Biometric Laboratory during the First War (during which time the new building was used as a military hospital) and finally, (7) the official opening of the Bartlett Building in 1922 (renamed the Pearson Building in 1980).

THE BIOMETRIC TEAS: Once Weldon moved to Oxford in 1899, Pearson and Weldon were to make arrangements to be together every year throughout the summer months. (They would also spend part of the Easter and Christmas vacations together as well.) During the summer, they spent most weekends usually in Oxfordshire or in London near Pearson's home in Hampstead Heath, and occasionally they would go to some other place together for two weeks. In the summer of 1899, Pearson started a 'biometric camp' at Peppard on the Chilterns to investigate biometric problems and

Weldon took a week-end cottage to be near by. Different members of Pearson's staff from the Biometric Laboratory and another contingent from Oxford joined in the activities; Francis Galton was there when they were in the London area. From this summer work, arose the Friday 'biometric teas' where plans would be made to discuss the work for Saturday and Sunday which were 'given to calculating and reducing the weekly work'.[28]

Pearson and Weldon began their biometric work on Shirley poppies at Hampden Farm House on the Chilterns where they had 'a considerable strip of garden covered with Shirley poppies' to further develop Pearson's theory of homotyposis (i.e., 'the quantitative degree of resemblance to be found on the average between the like parts of organisms'.[29] Pearson's homotyposis was to have indicated the extent to which certain patterns could be found in specific characteristics of plants (such as petals or leaves) or of insects (such as 'the black spot on the boarder of the orange tip butterfly'): though each petal or each leaf was not exactly identical, Pearson's homotypic correlations attempted to measure the degree of 'likeness or resemblance'.[30] The study of homotyposis in Shirley poppies had begun in March of 1900 and would continue to the end of the summer.[31] This was a study on a larger scale than any studies attempted previously and they received assistance from up to eight biometricians daily: Weldon gave Pearson the 'criticism, suggestions, and encouragement, in which he never failed'.[32]

The collection and reduction of this data took nearly a year to complete. The memoir was presented to the Royal Society in October of 1900. The meeting was to be held on November 15. The day before the meeting Weldon wrote to Pearson,

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28 Pearson, 'W.F.R. Weldon', 310.

29 [Karl Pearson], 'Cooperative investigations on Plants. I. On the Inheritance of the Shirley Poppy, *Biometrika*, 2 (November 1902), 56-100. Four years later they published their second paper on the 'Cooperative investigations on Plants. II. On the Inheritance of the Shirley Poppy, *Biometrika*, 4 (November 1906), 394-426. See also Pearson, 'W.F.R. Weldon', 310.

30 Karl Pearson, Manuscript on the 'Fundamental ideas of Heredity' (1920), KP:UCL 78/2.

31 Weldon wrote about 20 letters to Pearson on the poppies from 20 March to 5 September 1900 and Pearson wrote five letters. KP:UCL.

32 Karl Pearson, 'W.F.R. Weldon', 301.

> I wish I could do a little investigation myself. Your flow of papers makes me feel so ashamed: the homotyposis paper seems very splendid. I wish I could come up tomorrow.[33]

After Weldon learnt that Bateson attacked Pearson's paper at the meeting, he wrote to Pearson:

> I wish I had come up yesterday. It is an infernal shame that the Royal should be so mismanaged...your paper cannot be rejected....Do you think it would be too hopelessly expensive to start a journal of your own kind? Surely you would be glad to treat your homotypes more liberally in the matters of describing experimental data than is possible in *Phil Trans*[34] ?

Pearson then sent a letter to Weldon informing him that

> I wrote a line to Michael Foster last night on what seemd to me the very premature nature of Bateson's atttack. The council has not yet formally decided to accept the Homotyposis paper; it has not even gone to press. But the Asst. Secretary wrote to me that the Committee had recommended its publication, and asked if I would like to make any revision of it before the Council's formal sanction against it. Because I did not wish to modify substantially, I replied *No*!... I, of course, feel very grateful for your letter because I think it is a most dangerous precendent.[35]

By the end of November Pearson had suggested to Weldon that 'the science in future should be called Biometry and its official organ would be *Biometrika*.[36] Weldon and Pearson were co-editors and Galton was asked to act 'in consultation'. Nonetheless, the results from their work on homotyposis led to the publication of two papers in *Philosophical Transactions of the Royal Society* in Pearson's series on the 'Mathematical Contributions to the Theory of Evolution'.[37] About a month before the meeting, on 16 October 1900, Weldon wrote to Pearson that

> among pleasanter things, I have heard of and read a paper by one, Mendel, on the results of crossing peas, which I think you would like to read. It is in the *Abhandlungen des naturforschenden Vereins in Brünn* for 1865. I have the R.S. copy here, but I will send it to you if you want it.[38]

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33 W.F.R. Weldon, Letter to Karl Pearson, 14 November 1900, KP:UCL.
34 W.F.R. Weldon, Letter to Karl Pearson, 16 November 1900, KP:UCL.
35 Karl Pearson, Letter to W.F.R. Weldon (written sometime after 16 November 1900), KP:UCL.
36 Pearson, 'W.F.R. Weldon', 302.
37 Karl Pearson with Alice Lee, Ernest Warren, Agnes Fry, Cicely D. Fawcett and others. 'Mathematical Contributions to the Theory of Evolution. IX. On the Principle of Homotyposis and its Relation to Heredity, to the Variability of the Individual, and to that of the Race. Part I. Homotyposis in the Vegatable Kingdom. *Philosophical Transactions of the Royal Society*. series A, 197 (1901), 285-379. Two years later he published, Karl Pearson, 'Mathematical Contributions to the Theory of Evolution. On Homotyposis in Homologous but Differentiated Organs. *Proceedings of the Royal Society*, 71, (1903), 288-313.
38 W.F.R. Weldon, Letter to Karl Pearson, 16 October 1900, KP:UCL.

Much of Weldon's time over the next six years was to be spent working on problems of Mendelian inheritance (from Bateson's perspective) and its relationship to Galton's Law of Ancestral Heredity.

Several months later, Weldon began to analyse Mendel's seven set of characters of *pisum sativum* which Mendel had first described in 1865.[39] As he thought it was 'clearly important to test these results by a careful study of the numerical results by the application of such tests as may be possible', Weldon performed a couple of statistiscal tests on Mendel's results.[40] He found firstly that the probable errors for inheritance with dominant characters in the second hybrid generation and found that his 'results [were] in accord so remarkably with Mendel's discovery'; and secondly, he applied Pearson's chi-square goodness of fit test to a distribution of the 'frequency of the various possible combinations in hybrids of the second generation from races which differed in three characters'. Weldon examined these three characters: shape of pea, colour of pea and colour of seed-coat (i.e., roundness/angularity, greenness/yellowness and greyness/yellowness respectively). He found that the 'chance that a system will exhibit deviations as great or greater than these from the results indicated by Mendel's hypothesis is about 0.95.'[41] Weldon was to be the first person to test a Mendelian distribution by using the chi-square goodness of fit test.

In 1936, R.A. Fisher would use the chi-square goodness of fit test for a series of Mendelian distributions. Fisher's analysis was based on Mendelian gametic ratios as well as bifactorial and trifactorial experiments. He found that

> a χ^2 of only 2.8110 [with eight degrees of freedom]—almost as low as [Weldon's] 95 per cent.point... was strongly significant and so low a value could scarcely occur by chance one in 2000 trials.[42]

Some 50 years after Fisher published his paper, A.W.F. Edwards would use the chi-square goodness of fit test on Mendelian segregations of the seven characters of *pisum*

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39 W.F.R. Weldon, 'Mendel's Law of Alternative Inheritance in Peas', *Biometrika*, 1 (1901), 228-54 (1902). Weldon used Galton's term 'alternative' for non-blending or particulate inheritance.

40 *Ibid.*, 233.

41 *Ibid.*, 235.

42 R.A. Fisher, 'Has Mendel's work been Rediscovered?' *Annals of Science*, 1 (15 April 1936), 130.

sativum. His results indicated (as did Weldon's and Fisher's) that 'the segregation are in general closer to Mendel's expectation than chance would dictate'.[43]

In an earlier paper on Mendelian inheritance, Weldon examined the ambiguity of Mendel's categories. The 'ambiguity' that Weldon found most problematic was that no consideration was given to 'continuous' variables. Weldon argued that it was possible for such variables as colour (such as green and yellow) or stature, which had been categorised as 'discontinuous', to be treated as continuous variables. As Bateson had not addressed the role of continuous variation for problems of inheritance, Weldon found Bateson's view of inheritance to be especially problematic.[44] (R.A. Fisher would later find the resolution to Mendelian inheritance and continuous variation in his 1918 paper on 'The correlation between relatives on the supposition of Mendelian Inheritance.'[45]

Weldon's next Mendelian project involved his experimental work on the inheritance of coat-colour in mice in the early part of 1901. He was convinced that the cross between the European albino mouse and the Japanese waltzing mouse did not admit of simple Mendelian unit factor. He was hoping to publish a stud-book on his mice which included the completion of 600 pedigrees at the time of his death. In his notebook, Weldon kept a series of records on the cross-breeding of the litters of 'Waltzers' and 'Albinos' for eye-colour and coat-colour.[46] By 17 December 1901, he also crossed hybrid x hybrid which he found 'always produced albinos'.[47] From this work, he published his paper on 'Albinism in Sicily and Mendel's Law'.[48]

During the summer of 1902, Weldon and Pearson were in Bainbridge in Wensleydale and they also 'cycled to the churchyards of the Yorkshire dales' to collect

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43 A.W.F. Edwards, 'Are Mendel's Results Really too Close?' *Biological Review*, 61 (1986), 295-312.

44 W.F.R. Weldon, 'On the Ambiguity of Mendel's Categories', *Biometrika*, 2 (1902), 47.

45 R.A. Fisher, 'The correlation between relatives on the supposition of Mendelian Inheritance.' *Transactions of the Royal Society of Edinburgh*, 52, (1918), 399-433.

46 W.F.R. Weldon, 'A Notebook with a synopsis of Darbishire's records of mice, with notes relating to breeding experiments', (1901-1902), KP:UCL/260/8. See also Pearson, 'W.F.R. Weldon', 308.

47 *Ibid.*, p.2.

48 W.F.R. Weldon, 'Albinism in Sicily and Mendel's Law'. *Biometrika*, 3, (1904), p. 107.

material for their joint paper 'On Assortative Matings in Man'.[49] They returned to Wensleydale in the summer of 1903 where they 'collected daisy petals on Stake Fell Brush, Wendsleydale' from 13 to 19 July 1903.[50] Pearson's son, Egon, who was then seven years old, sent a letter to his father that summer and wrote: 'This comes hoping to find you well as it leaves me. I hope you and Mr. Weldon are having a nice time.'[51] During this time, a joint study was made at Weldon's suggestion on the relationship between Mendelian inheritance and Galton's Law of Ancestal Heredity. It was shown that 'there was no essential antagonism between the two methods'. A paper was published, but Weldon declined co-authorship for this paper as 'he had taken no part in certain portions of the more complicated algebraic analysis"'[52]

From 1902 to 1903, Weldon was studying the influence of environmental seasonality on the variation and correlation of the floral part of *Lesser celandine*.[53] During July and August of 1904, Pearson took a cottage in Oxfordshire in Witney at Clement Field, Cogges. Before going up, Pearson asked Major Greenwood, on 14 July, to join Weldon and the other biometricians for their summer work.[54] From Witney, Weldon cycled into Oxford bi-weekly to look after his mice. Weldon worked continuously and late into the evenings; moreover, he rarely took summer holidays (though Pearson took regular fortnightly holidays to Germany or Italy). By the end of the summer, a number of Weldon's friends were beginning to think that Weldon's 'life was being lived at too fast a pace'.[55]

The first part of the following summer was spent in London. Weldon wrote to Galton in June to let him know that he was planning 'to spend the summer in Pearson's

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49 *Ibid.*, p.309. See also Karl Pearson and W.F.R. Weldon, 'On Assortative Matings in Man', *Biometrika*, 2, (1903), pp. 481-498.

50 Karl Pearson and W.F.R. Weldon, 'Notebook of Daisy Petals', 13-19 July 1903, KP:UCL/117.

51 Egon Pearson, Letter to Karl Pearson, 26 July 1903, KP:UCL.

52 Karl Pearson, 'Mathematical Contributions to the Theory of Evolution. XII. On the Generalised Theory of Alternative Inheritance, with special reference to Mendel's Laws', *Philosophical Transactions of the Royal Society*, Series A, 203 (1904), 505-9. See also Pearson, 'W.F.R. Weldon', 310.

53 *Ibid.*, p. 309.

54 Karl Pearson, Letter to Major Greenwood, 14 July 1904, KP:UCL.

55 Pearson, 'W.F.R. Weldon', 311.

neighbourhood' and he hoped to see him there.[56] The remaining portion was spent in Oxford and Pearson took a cottage at East Isley, about 17 miles from Oxford. They spent this time working jointly on their determinantal theory of inheritance (which Pearson published after Weldon's death).[57]

After the summer, Weldon began to pursue his work on the inheritance of coat-colour in horses. By November he was working nine hours a day and wrote to Pearson

> I can do nothing else until I have found out what it means...The question between Mendel and Galton's theory of Reversion ought to be answered out of these. Thank God, I have not finished that book. There must be a chapter on Race Horses![58]

He continued to work on this throughout the winter of 1905 and into the early spring of 1906. In January of 1906 he sent a 'Note on the Offspring of Thoroughbred Chestnut Mares' to the Royal Society.[59]

During the Easter vacation of 1906, the Pearsons were in Oxfordshire where they took a cottage at Longcote a mile away from the Weldons, who were staying at the little Inn at Woolstone, at the foot of the White Horse Hill. On Sunday 8 April, Weldon cycled into Oxford and Pearson joined him for part of the journey. They stopped by the roadside and discussed some of Weldon's problems which Pearson was to solve by Tuesday. When Pearson saw Weldon on Tuesday afternoon, he found him in bed suffering from 'what appeared to be an attack of influenza'.[60] Weldon decided he was well enough to go to a picture-gallery on the Wednesday and on Thursday, he went to the dentist. By the evening, however, his health had worsened and he was taken to a Nursing Home. After Pearson returned to his home from visiting Weldon that evening, he wrote to Mrs. Weldon that 'I do hope that you have found the husband less seriously ill than we feared. I hope we shall hear it is so in the

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56 W.F.R. Weldon, Letter to Francis Galton, 6 June 1905, FG:UCL.

57 Pearson, 'W.F.R. Weldon', 313. See also Karl Pearson, 'On a Mathematical Theory of Determinantal Inheritance fromb Suggestions and Notes of the Late W.F.R. Weldon'. *Biometrika*, 6 (1908), 80-93.

58 W.F.R. Weldon, Letter to Karl Pearson, November 1905, KP:UCL.

59 W.F.R. Weldon, 'Note on the Offspring of Thoroughbred Chestnut Mares', *Royal Society Proceedings*, 77B (1906), 394-8.

60 Pearson, 'W.F.R. Weldon', 318

morning'.[61] On Good Friday, 13 April, Mrs. Weldon sent a telegram to Pearson to let him know that her 'husband died tonight' at 7 o'clock of double pneumonia.[62]

The news of Weldon's death, at 46 years of age, was a shock to the scientific community and no one more than Pearson felt the loss so poignantly: he had not only lost 'the closest friend he ever had', but he found himself, at once, alone in the scientific community with no one to guide him with such biological problems as Mendelian inheritance and no one to share his enthusiasm for his statistical work.[63] His grief was almost unconsolable throughout the remaining eight months of that year: 'without Galton's continuous sympathy, aid and counsel, it would have been impossible in that year to continue [his] work'. Weldon's death was an irreparable and fundamental loss to Pearson, in part because of his intellectual interdependence with Weldon and also because of the emotional sustenance he received from Weldon. The depth of Pearson's sorrow was expressed in a series of letters to Galton until 1911 and to Mrs. Weldon until 1932.

Galton, who was by then 84 years and whose sister, Bessie, had died at the beginning of the year (who was the 'last tie that brought the family together') thought that 'Weldon's death [was] a terrible and disasterous blow, so utterly unexpected'.[64] He wrote to Pearson on 16 April that

> few if any men will feel it more deeply than you who were so intimately associated with him, not many more than I do. We have lost a loved friend, and Biometry has lost one of its protagonists. I feel intensely miserable and shall feel the void for probably the rest of my life. I should greatly like to pay the last tribute of friendship to his remains by attending the funeral, but I dare not risk it.[65]

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61 Karl Pearson, Letter to Mrs. Weldon, 12 April 1906, KP:UCL.

62 Florence Weldon, Telegram to Karl Pearson, 13 April 1906, KP:UCL. Pearson wrote the time and cause of death on the telegram.

63 Karl Pearson, Letter to Mrs. Weldon, 29 April 1906, KP:UCL.

64 Francis Galton, Letter to Karl Pearson regarding his sister's death, 1 February 1906, FG:UCL. Francis Galton, Letter to Karl Pearson following Weldon's death, 16 April 1906, in Pearson, *Life of Galton*, IIIA, p.280.

65 Galton's incipient mild phlebitis made him think that he should not risk a visit to Merton Chapel. Francis Galton, Letter to Karl Pearson, 16 April 1906, in Pearson, *Life of Galton*, IIIA, p.280.

To Pearson's surprise, he saw Galton at the funeral in Merton College Chapel who had been escorted by his devoted Swiss servant, Gifi. Galton thought that 'the funeral was inexpressibly sad'.[66]

The loss was also felt deeply by Weldon's one time colleague, and later arch-rival William Bateson, who wrote to his wife Beatrice,

> To Weldon I owe the chief awaking of my life. It was through him that I first learnt that there was work in the world which I could do. Failure and uselessness had been my accepted destiny before. Such a debt is perhaps the greatest that one man can feel towards another; nor have I been backward in owning it. But this is the personal, private obligation of my soul.[67]

A number of Weldon's students at Oxford would feel the void of their former teacher who 'was aiming at nothing less than a revolution in biological study—at rendering it quantitative rather than qualitative'.[68]

In the immediate aftermath of Weldon's death, Pearson wrote at least one letter a week to Mrs. Weldon: from 14 April to 28 December, he wrote 43 letters. Pearson, who 'used always to write to [his] mother on the anniversary of [his] Brother's death', felt that he needed to have an 'anniversary of the kind' for Weldon.[69] He was to send a letter to Mrs. Weldon on the anniversary of Weldon's death for the next 23 years, although he would also write regularly to her until the end of his life. Pearson wrote 269 letters to Mrs. Weldon (of which 260 were written after Weldon's death) and Mrs. Weldon wrote 111 to Pearson (nearly all were written after her husband's death).

Two days after Pearson learnt than Weldon died, he wrote to Mrs. Weldon that

> I cannot begin to understand that our talk a week ago today sitting by the roadside smoking, with our bicycles alongside—the notes then made for work that I was to do for him—can never occur. I feel it is all too impossible.No one knew so much his clearness and transcending powers intellectually.[70]

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66 Francis Galton, Letter to Karl Pearson, in Pearson, *Life of Galton*, pp.280-1.

67 William Bateson, Letter to Beatrice Bateson, in Beatrice Bateson, *William Bateson, F.R.S. Naturalist. His essays and addresses together with a short account of his life.* (Cambridge, 1928), p. 103, cited in P.C. Froggatt and N.C. Nevin, 'Galton's 'Law of Ancestral Heredity': Its influence on the early development of Human Genetics'. *History of Science*, 10 (1971), 20-1.

68 H.H. Turner, Letter to the Editor, *The Oxford Magazine* (31 October 1906).

69 Karl Pearson, Letter to Mrs. Weldon, 12 April 1907, KP:UCL.

70 Pearson, Letter to Mrs. Weldon, 15 April 1907, KP:UCL.

Pearson did not want Weldon's mice collection to be left to his successor and he asked Mrs. Weldon if she would be willing to help him sort out the material.

By the time Pearson wrote his third letter to Mrs. Weldon, on 24 April, he addressed her as 'My dear Friend' and by 15 May, Mrs. Weldon also began to address Pearson as 'My Dear Friend': this was to be a form of address that they would continue to use for the rest of their lives. Two days after he wrote this letter and feeling more depressed, he wrote:

> You have hardly realised and I don't think *he* did how mentally refreshing it was for me being near him for a few weeks and how it sent me back fit for work with new vigour and new ideas. He always gave me courage to go on and now I don't feel as if I could ever want to go on again'.[71]

Pearson was to spend the next 26 years trying to put Weldon's unpublished papers in order and by publishing much of the material in *Biometrika* from 1907 to 1932. Whilst Pearson worked quietly, sometimes with the assistance of Mrs. Weldon, on the inheritance of European albino mice and Japanese dancing mice by using data from Weldon's mice stud book in addition to his work on the inheritance of coat colour from his horse stud book, the locus of the work in the Biometric Laboratory shifted to craniometry and physical anthropology (owing, to some extent, to the work of Julia Bell, Alice Lee and Geoffrey Morant). The directorship he was to assume in the Eugenics Laboratory, less than a year after Weldon's death, was linked by Pearson to Galton's reaction to 'the shock of Weldon's death [which] was more intense and lasting in the case of a much older man'.[72] It seemed to Pearson that from that date, Galton 'was less able to take independent action and to find reliance on other people more needful'.[73]

By the end of April, Pearson had begun to work on Weldon's obituary for *Biometrika*. Mrs. Weldon began to send some of her husband's papers and letters to Pearson. Six days later, on 5 May, he wrote that

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71 Pearson, Letter to Mrs. Weldon, 26 April 1907, KP:UCL.
72 Pearson , *Life of Galton*,p. 292
73 *Ibid.*

> I do not like to let a long interval pass without sending you a line although it must be a short note [which was eight pages long].... your wealth of papers will keep me going for many years, you know it will really be used.[74]

Pearson estimated that the number of letters Weldon wrote to him must have 'numbered 400 to 500 letters' (the total number of papers extant in the collection is 491). Referring to Weldon occasionally by 'R' (as his wife referred to him as Raphael), he wrote that they should be published as 'much of R's real greatness lies in these letters. But reading them makes me feel how much I missed in the past and has made me very miserable about the future'.[75] Working on Weldon's papers was to bring him closer to his own work as well. He wrote to Galton on 28 May that

> I will write again [on the possibility of a Weldon Memorial Medal] in a few days. My heart is very full just now. We have had Mrs. Weldon with us for two days going through papers and letters. I am beginning to see the lines of my papers a little better.[76]

At the end of June, Galton sent Pearson his letters from Weldon for the obituary in *Biometrika.*[77] Shortly after Pearson received these letters, he wrote to Galton, 'You will, I am sure, understand that I am placing Weldon at the centre of my stage'.[78] On 3 December, Galton received his copy of *Biometrika.* After he read Pearson's biography of Weldon, he wrote to Pearson

> I have read with the fullest appreciation your excellent and affectionate memoir of Weldon. It seems to me a model memoir so well-proportioned... one sees in every page the great care you have taken. If he could have [only] written more of his book on Heredity! He was such a man so familiar of biological facts.[79]

With the passing of each year, Pearson felt older and continued to feel the poignancy of his loss. The letters he wrote to Mrs. Weldon (on the anniversary of Weldon's death) over the next four years, seem to suggest very strongly Weldon's influence on Pearson was powerful . In 1907 he wrote that

> It seems so much more than a year. I feel to have grown rapidly old in that year. W.F.R.W. always helped me to feel young. I don't see very far in many things and

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74 Pearson, Letter to Mrs. Weldon, 5 May 1906, KP:UCL.
75 *Ibid.*
76 Karl Pearson, Letter to Francis Galton, 28 May 1906, FG:UCL.
77 Francis Galton, Letter to Karl Pearson, 29 June 1906, FG:UCL.
78 Karl Pearson, Letter to Francis Galton, 8 August 1906, FG:UCL.
79 Francis Galton, Letter to Karl Pearson, 4 December 1906, FG:UCL.

> my only gospel is to do the work immediately to hand as well as I can, and leave other things to work out their own courses.[80]

Whilst his 'thoughts [were] a good deal with [Mrs. Weldon]' in April of 1908, the spring of 1909 'had been very uphill for [him]'.[81] He wrote to Mrs. Weldon on 6 June 1909, that 'I suppose that I am getting rather old for work and small maths grow much harder. I have missed W.F.R.W. more than ever this year.'[82] And yet one year later he would write to her:

> Each anniversary seems to me rather to emphasise than lessen the feeling of personal loss. It is, and I supppose will remain, impossible to find any one in the least to fill R's place. I have no contemporary from whom to seek aid and advice.[83]

Pearson and Mrs. Weldon finished the first paper on inheritance in mice by in 1907.[84] Before the paper was published, Pearson wrote to Mrs. Weldon, on 27 September 1906, that

> there is nothing opposed in Biometry and Mendelism. Your husband and I worked *that* out at Peppards and you will see it referred in the *Biometrika* memoir. The Mendelian formula leads up to the 'ancestral law'. What we fought against was the slovenliness in applying Mendel's categories and asserting that such formulae apply in cases when they did not.[85]

Pearson published Weldon's 'A Determinantal Theory of Inheritance' in 1908. When Weldon wrote this paper, he was seeking two reconciliations: the first was Mendel's particulate theory of inheritance for characteristics (that showed discontinuous variability) with Pearson's use of correlation for characteristics with continuous variability, and the second was Galton's law of ancestral heredity with Mendel's recessiveness. After Pearson published this paper, his views on inheritance hardly ever changed.

Neither Weldon nor Pearson were prepared to accept Bateson's view of Mendelian inheritance which not only rejected biometry, but also rejected the inheritance of those characteristics which would be classified as 'continuous'. Pearson

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80 Pearson, Letter to Mrs. Weldon, 12 April 1907, KP:UCL.
81 Pearson, Letters to Mrs. Weldon, 16 April 1908; 6 June 1909, KP:UCL.
82 Pearson, Letter to Mrs. Weldon, 6 June 1909, KP:UCL.
83 Pearson, Letter to Mrs. Weldon, 13 April 1910, KP:UCL.
84 [Florence Joy Weldon and Karl Pearson] 'On Heredity in Mice from the Records of the Late W.F.R. Weldon on the Inheritance of the Sex-Ratio and of the Size of Litter', *Biometrika*, 5 (1907), 436-45.
85 Pearson, Letter to Mrs. Weldon, 27 September 1906, KP:UCL.

alone was not prepared to modify Weldon's views to incorporate the work of Fisher, perhaps, partly because of their mutual antagonism and partly because he had no formal grounding in biology. It seems that Pearson's views on inheritance hardly ever changed after the posthumous publication of Weldon's theory of inheritance. It will be suggested that Pearson's reluctance to modify his views may well have had an emotional basis to it. That is, if Pearson were to incorporate a newer view, he would have had to relinquish Weldon's views and this may have very likely led to a near abandonment of the spirit of Weldon which Pearson tried to kept alive, in part, by publishing his unfinished work on Mendelian inheritance until about 1932 (four years before his own death). It was not until 1929 when Pearson seems to have reached a quietude in his bereavement .

THE STATISTICAL METHODS AND TOOLS OF THE DRAPERS' BIOMETRIC LABORATORY

When the Biometric Laboratory was established in 1903 following the Drapers' Grant in 1903, Pearson had already developed and formulated the fundamental corpus of his statistical theory. In the early part of the twentieth century, the primary interest of the biometricians and Weldon, in particular, included problems of natural selection, homotyposis in the plant and animal kingdom and in determining the relationship between Galton's law of ancestral heredity to Mendelian inheritance.

After Weldon's death in 1906, Pearson and his co-workers began to explore problems of craniometry and physical anthropology which had been first suggested about 1895. This work was to lead to the development of new procedures for analysing skulls and bones The one statistical method Pearson devised for this purpose was the coefficient of racial likeness (C.R.L.) in 1921.[86] Some of the statistical methods that Pearson would devise in the twentieth century included the

86 Pearson first used the C.R.L. in his paper with Adelaide G. Davin, 'On the Sesamoids of the Knee-Joint, Part I. Man. Part II. Evolution of the Sesamoids', *Biometrika*, 13 (1921), Part I, 133-75', Part II, 350-400. The C.R.L. was defined in his paper 'On the Coefficient of Racial Likeness', *Biometrika*, 18 (1926), 105-17.

biserial correlation, triserial correlation, correlation ratio, eta (η) used to measure a non-linear correlation and the polychoric correlation which he devised with his son Egon in 1922.[87]

The methodology incorporated in the Drapers' Biometric Laboratory was twofold: the first was mathematical and included the use of Pearsonian statistical methods (which sometimes required the use of determinantal matrix algebra) and analytical solid geometry; the second, involved the use of such instruments as integrators, analysers and curve plotters plus the cranial coordinatograph, silhouettes, and cameras.[88] The underlying statistical procedure used in the Drapers' Biometric Laboratory, particularly by Pearson, involved curve-fitting and goodness of fit testing. The chi-square (χ^2, P) goodness of fit test was used when the theoretical distribution was (1) known *a priori* such as already established sets of data from the General Registrar Office or Hosptial records on mortality; (2) not known *a priori*, therefore the theoretical distribution would need to be calculated by determining the probabilities of the empirical distribution and (3) when fitting the Poisson distribution and (much later in Pearson's time) the Normal distribution.[89] This test would remain paramount in Pearson's statistical work throughout the rest of his life.

This statistical test was to stimulate a number of Pearson's students who would bring about further theoretical developments in curve-fitting.[90] Three years before his death, three of his students wrote articles which involved finding a better fit for a

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87 Karl Pearson with Egon Pearson, 'On the Polychoric Coefficients of Correlation', *Biometrika*, 14 (1922), 127-56.

88 Pearson discusses the integrators, analysers and curve plotters in Karl Pearson, 'History of the Biometric and Galton Laboratory', Appendix 1, Report to the University of London, KP:UCL/247 (1903-1909), p.3. The other instruments appear in various articles in *Biometrika* from 1907 to 1936.

89 Geoffrey Morant and Otto Samson used their distributions from measurements of stature and fitted this to the normal curve by using the χ^2 goodness of fit test. See Morant and Samson, 'An Examination of Investigations by Dr. Maurice Fishberg and Prof. Frank Boas Dealing with Measurements of Jews in New York', *Biometrika*, 28 (1936), 9.

90 See, for example, William Palin Elderton, 'Adjustments to the Moments of J-shaped Curves. *Biometrika*, 25 (1933), 179-180; Idem, 'Tables for Testing the Goodness of Fit of Theory to Observation', *Biometrika*, 1 (1901), 155-63; R.C. Geary, 'A special expression of the Moments of certain Symmetrical Functions', *Biometrika*, 25 (1933), 184-6; V. Romanovsky, 'Note on the Method-of-Moments', *Biometrika*, 28 (1936), 188-90; M.R.E. Shanawany, 'An illustration of the accuracy of the chi-square approximation.', *Biometrika*, 28, (1936), 179-81.

hypergeometric distribution, non-normal symmetric distributions and re-examining the idea that the starting point of a distribution for curve-fitting is fixed at zero.[91]

Pearsonian correlational methods and regression were perhaps the second most frequently used set of techniques in the Biometric Laboratory (apart, of course, from Pearson's standard deviation and the coefficient of variation). They were much more extensively employed than in the Eugenics Laboratory. By 1918, the following 15 different correlational methods had been used in a number of articles in *Biometrika* (the first three were devised in the nineteenth century and the remaining 12 in the twentieth century:

1. Pearson's product-moment correlation coefficient, (r)
2. Multiple correlation (R)
3. Partial correlation
4. Tetrachoric correlation (r_t)
5. Equaprobable tetrachoric correlation
6. Mean contingency coefficient (φ),
7. Mean square contingency coefficent or Pearson's phi-coefficient (ϕ),
8. Square contingency coefficient or the chi-square test of independence (χ^2)
9. Marginal centroids
10. Biserial correlation
11. Triserial correlation
12. The correlation ratio
13. Eta (η)
14. Three row η
15. Variate difference methods including the correlation of grades and ranks

CRANIOMETRY AND PHYSICAL ANTHROPOLOGY

Sometime in 1895, Pearson had sent out some letters of enquiry in his search for 'about 100 skulls of a homogeneous race'.[92] Flinders Petrie responded to Pearson's

91 O.L. Davies, 'On asymptotic formulae for the hypergeometric series'. *Biometrika*, 26 (1934), 59-107; G.H. Hansmann, 'On certain non-normal symmetrical frequency distributions', *Biometrika*, 26, (1934), 129-95; E.J. Martin, 'On corrections for the moment coefficients of frequency distributions when the start of the frequency is one of the characteristics to be measured', *Biometrika*, 26 (1934), 12-58;

92 Pearson, 'MCTE. III. Heredity, Panmixia and Regression' (1896).

request and let him examine his newly discovered Egytian race of 103 skulls of the adult male crania from 'the general population who occupied the district near Naganda in Upper Egypt some 5000 years ago'.[93] Pearson considered this to have been the 'finest anthropological collection—skeletons as well as crania—known to [him]'.[94] Pearson's younger brother, Arthur Beilby Pearson-Gee (1862-1896), had not only provided the funds for the collection, but he also packed and brought back the collection to England.[95] (Arthur acquired his double-barrelled surname by Royal Licence in 1885, in addition to a portion of the estate from the late wife of his father's friend Thomas Gee who left him no children.[96])

In the late nineteenth century, the biometricians ideas of inheritance centred around determining the extent to which stature could be found to be correlated in offspring and parents (or with collateral relatives). The emphasis would change in the twentieth century when the biometricians began to measure skulls by using the cephalic index B/L (Breadth divided by Length). They decided that cephalic index measurements, when used to test any theory of heredity, possessed these two merits: firstly, it was said that the 'cephalic index remained sensibly constant after two years of age'; thus the strength of inheritance could be ascertained by measurements on young children whose parents were still alive. Secondly, they thought this could have been 'a marked racial character [which] might be considered to be strongly inherited'.[97] Rather than focusing on stature as the primary means to measure inheritance, Cicely Fawcett, Alice Lee and Karl Pearson were suggesting that the measurement of the cephalic index might shed light on problems of inheritance. The

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93 Karl Pearson, 'Variation in Man and Woman', *Chances of Death*, p.281.

94 *Ibid.*, p.279

95 Pearson, 'MCTE. III. Heredity, Panmixia and Regression' (1896); *idem*, *Drapers' Biometric Series* (1919). Like his father and older brother before him, Arthur read law at the Inns of Court (Inner Temple) and was called to the bar in 1877.

96 UCL Archive of Thomas Gee and Arthur Beilby Pearson-Gee. Karl Pearson's granddaughter, Sarah Pearson, remarked that her grandfather was offered Gee's surname and estate initially (when he was 28 years old), but he refused to take on an additional surname. (Personal correspondence, 2 June 1993).

97 Karl Pearson and Cicely D. Fawcett, 'Mathematical Contributions to the Theory of Evolution: On the Inheritance of the Cephalic Index', *Proceedings of the Royal Society*, 62 (1898), 413.

view that the skull 'remained sensibly constant' (whereas stature is more variable) not only seems to have engendered the develoment of craniometry in the biometric laboratory, but it probably was pivotal in the later development of Pearsonian physical anthropology.[98]

There were a range of instruments developed and utilised in the Biometric Laboratory to obtain craniometric measurements which included the following:

> an osteometer of Broca type, an osteometer of Hepburn's model lent by Professor Thane, calipers of various types, a modified Klaatsch cranial contour tracer used for projecting various points of the bone onto a horizontal plane, a torsiometer of [their] own design, a whole plate of photographic camera with Goerz astigmatic lens used for obtaining an image of the head of the bone on a ground-glass drawing plate in the focal plane, a powerful electric lantern for illuminating the bone, and a solid stand with a universal joint for supporting the bone in any position, such as in general use in any physical or chemical laboratory.[99]

One of Weldon's colleagues, Herbert Thompson (who had taken measurements of the frontal breadth of the carapace of the Plymouth shore crab) had made a series of measurements on 301 male and female skulls. In an attempt to find a measure of the 'constancy of race' (as had been suggested by Weldon in 1890 when he used Galton's method of correlation for his work on the common shrimp), Pearson had taken Thompson's measurements and compared them to Professor J. Ranke's measurements in the *Beinhaüser* of the Bavarian churchyards of 900 modern Bavarian peasant skulls and also with Paul Broca's skull measurements of Parisians.[100] Pearson found the correlation between length and breadth of skull, the coefficient of variation for length and breadth of skull and a measure of the cephalic index for each group. Instead of using standard deviations to determine the amount of variation in the different groups, he found that when he used his *coefficient of variation* that

> the breadth of skull [was] in all cases a sensibly more variable quantity than length and ... possibly that "the more civilised races are more varible. Both of these results

98 Karl Pearson with Alice Lee, 'Data for the Problem of Evolution in Man. VI. A first study of the Correlation of the Human Skull', Invoice No. *Philosophical Transactions of the Royal Society*, series A, 196 (1901), 225-64. A second study was completed in the following year in Karl Pearson and Cicely Fawcett assisted by Alice Lee, 'A second study of the Variation and Correlation of the Human Skull, with special reference to the Naqada Crania', *Biometrika*, 1 (1902), 408-67.

99 Karl Pearson and Julia Bell, 'A study of the long bones of the English Skelton: Part 1. The Femur', *Drapers' Company Research Memoirs*, Biometric Series X, (1919), p.3.

100 Karl Pearson, 'Variation in Man and Woman', *Chances of Death*, p. 278.

> have, I believe, very important bearing on the mathematico-statistical theory of evolution.[101]

The arithmetic involved was of such a very lengthy and laborious character that Pearson 'would have hardly as got as far as it has already done, had it not been for the hearty assistance of [his] very zealous helpers, Miss Alice Lee and Mr. G. U. Yule'.[102] Because the values from the correlation coefficients from the Bavarian (r = .2849) and Egyptian (r = .2705) skulls were nearly identical, Pearson thought he 'might have easily concluded that the coefficient of correlation was constant for local races of man, and had remained so for nearly 5,000 years'; the French numbers, however, upset this view completely (as they produced a smaller r of .1265). Thus, Pearson was 'compelled to conclude that it is very unlikely that "Galton's functions" [were] constant for all local races'.[103] This research was set aside for ten years and they did not begin to work on it again until 1907 (a year after Weldon's death). Work continued on this subject until the outbreak of the First World War. A year after the war had ended, and 22 years after Pearson and the biometricians first began this project, the results were published in a two part 505 page article with the assistance of Julia Bell.[104]

Pearson and the Biometric Laboratory continued to use these craniometric tools up until Pearson's retirement in 1933. In Pearson's last paper on craniometry, which was given at a lecture before the Oxford University Anthropological Society on 25 May 1933 (just before he retired), he expressed concern for the 'unsatisfactory nature of the "standard" planes of the skull'.[105] He wanted to illustrate what could be achieved by the use of a cranial coordinatograph and the application of analytical solid

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101 *Ibid.*

102 *Ibid.*, p.271.

103 *Ibid.*

104 Karl Pearson and Julia Bell, 'A study of the long bones of the English Skelton: Part 1. The Femur', Chapters I to VI *Drapers' Company Research Memoirs*, Biometric Series X (1919), pp.1-224. Section II. The Femur of Man with special reference to other primate femora. Chapters VII to X. Appendices, Bibliography and Indices, *Biometric Series XI*, 226-505.

105 Karl Pearson, 'The cranial coordinatograph, the standard planes of the skull, and the value of Cartesian Geometry to the Craniologist, with some illustrations of the uses of the new Method', *Biometrika*, 25 (December 1933), 251.

geometry to craniometry.[106] He showed that by using a cranial coordinatograph, it was possible to obtain a measure of the coordinates of any point on the skull referred to by three rectangular planes of reference. He then obtained an equation by using analytical solid geometry for any line on the skull. He considered this to be a most promising field for the craniometricians who would apply Cartesian geometry to the skull.[107]

SILHOUETTES: The biometricians also used silhouettes for their craniometric work.The first paper published in *Biometrika* on type silhouettes came from the work of Karl Pearson, Ida McLean and Geoffrey Morant in 1928.[108] A year later, Miriam L. Tildesley, who did some of the most extensive work on silhouettes in the Biometric Laboratory, began to collect some measurements from the Albanians.[109] As the Albanians were divided into two groups, Gegë in the North and Toskë in the South, Tildesley wanted to know, 'as a physical anthropologist', if the 'difference of name cover[ed] also a difference of type'.[110] Her silhouettes were constructed mainly from 67 photographs from each group.

The first step was to trace from the photographs with extreme care the ouline of the head, introducing the

> marked positions of the sub-orbital point and tragion. These drawings were then enlarged by two stages to exactly four times their linear dimension by aid of a Coradi precision pantograph.[111]

The process involved first enlarging the profile of photographs by the aid of a Coradi pantograph which was done in two stages to reduce error of judgement. By the aid of a somewhat elaborate co-ordinate systems, a large number of measurements were taken on the profile outline. This process was repeated on every individual photograph

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106 *Ibid.*, 253.

107 *Ibid.*

108 Karl Pearson with Ida McLean and Geoffrey Morant, 'On the importance of the type of silhouettes for racial characterisation of Anthropology', *Biometrika*, 20B, (1928), 389-400.

109 Miriam L. Tildesley, 'The Albanians of the North and South. (1) Introductory Accounts of Measurements and Photographs taken in 1929', *Biometrika*, 25 (May 1933), 21-51.

110 *Ibid.*, 21.

111 *Ibid.*, '(2) Discussions of Miss Tildesley's Measurements by the staff of the Biometric Laboratory', 47.

which were then pooled to obtain a composite; the average of each of the co-ordinates was found and then plotted to give the average type or contour. Figures 7.1 and 7.2 represent Miriam Tildesley's contour types, derived from the co-ordinate system, of the Northern Albanian and Southern Albaninan groups respectively.

She thought that the advantage of using silhouettes from photographs arose from the

> rapidity with which a hundered *standardised* photographs could be obtained in the field as compared with 30 or 40 measurements on 100 men [was] a great advantage, and it [left] the laborious task of measuarement to be undertaken in the laboratory with proper instruments under standardised conditions.[112]

The biometricians who examined Tildesley's work concluded that there were at least two differentiated groups in Albania, those from the extreme North and the extreme South. It was thought that the comparative value of silhouettes of racial types would only be settled once many others were constructed. Both groups had from the

> European standpoint small [heads], and in the case of the Southern group extremely small heads. [Though] it is possible for two differentiated groups to have faces closely alike... we must accept the fact that a strong facial resemblance by no means connotes racial identity. Thus physiognomic characters do not necessarily provide the best method of discriminating races.[113]

Whilst the corpus of mathematical-statistical methods that Pearson devised in the nineteenth century for problems of natural selection, would continue to be used in the Drapers' Biometric Laboratory in the twentieth century, it has been shown that a new set of mathematical and statistical procedures, along with the development of non-mathematical instruments, arose in Pearson's Biometric Laboratory for problems of craniometry and physical anthropology which would not find application in the Eugenics Laboratory.

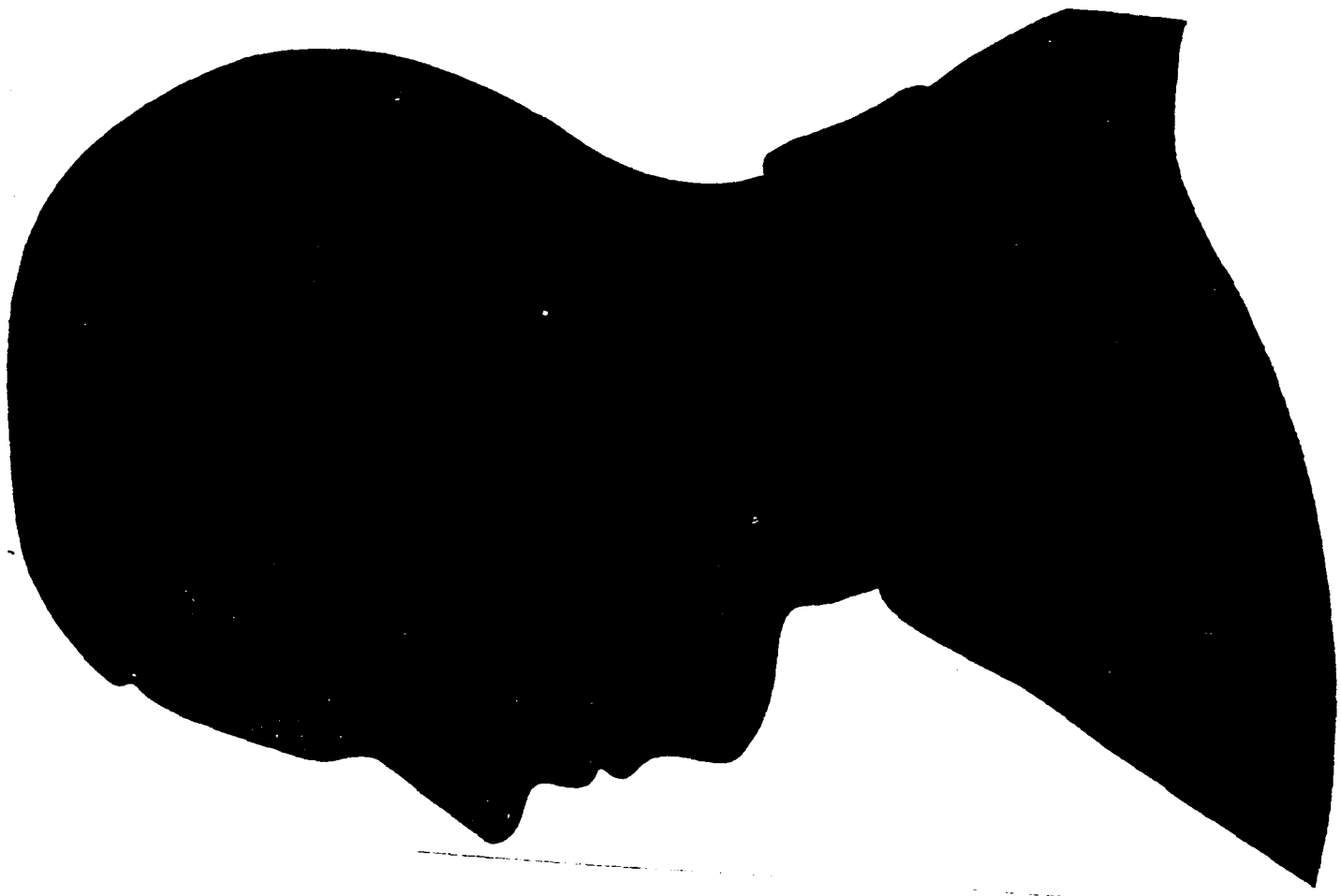
THE METHODOLOGY OF THE EUGENICS LABORATORY

After Weldon's death in 1906, Galton spent the autumn and winter months at the Hoe in Plymouth (where Weldon had spent his summers from 1887 to 1898). In the late

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112 *Ibid.*, 42.
113 *Ibid.*, 51.

Figure 7.1
TYPE OF SILHOUETTE OF NORTHERN ALBANIAN GROUP (1933)



Type Silhouette of the Northern Albanian Group.

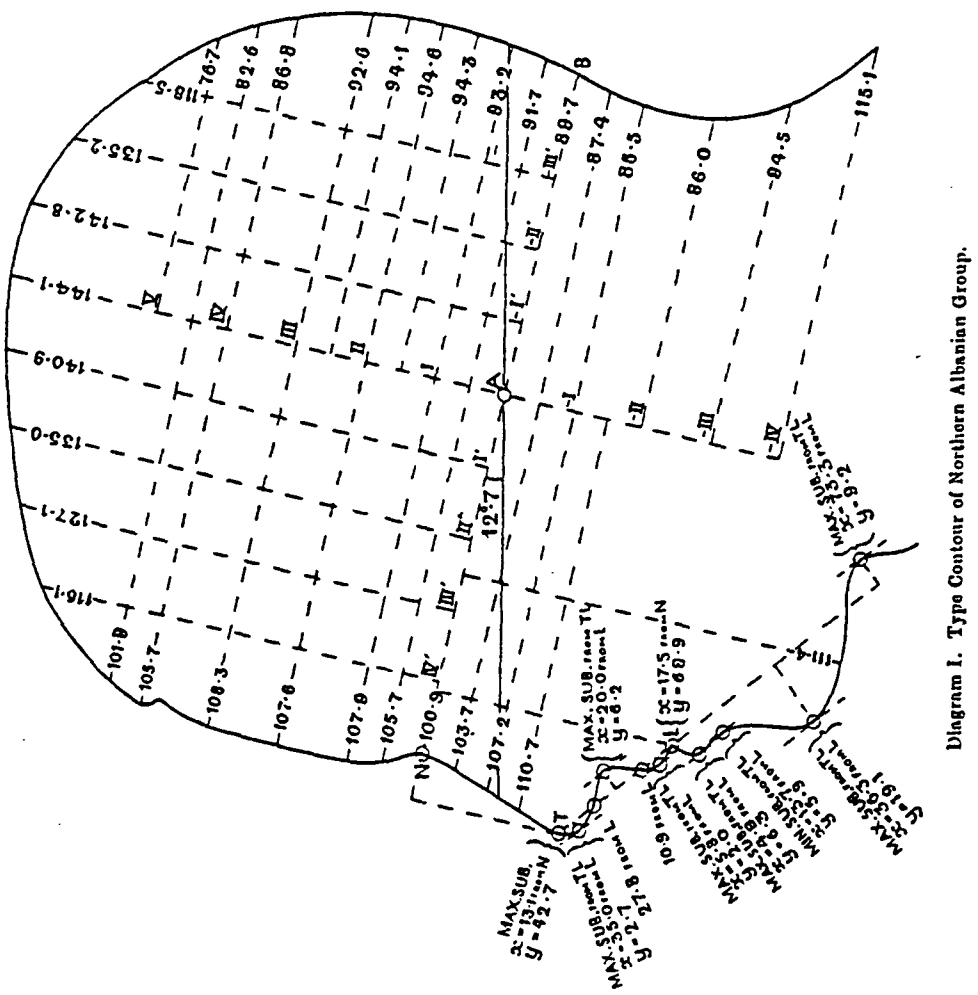
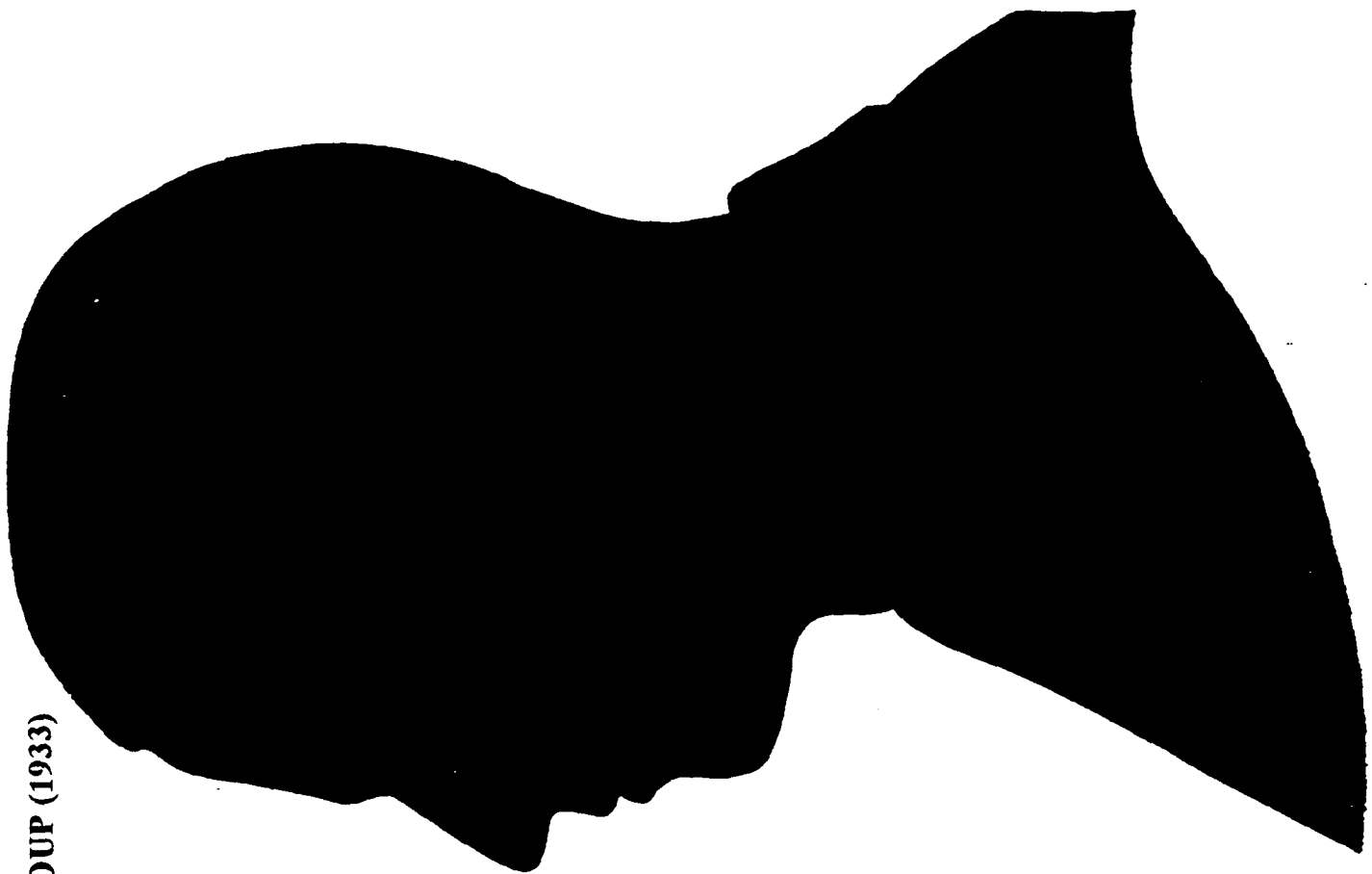


Diagram 1. Type Contour of Northern Albanian Group.

Figure 7.2
TYPE OF SILHOUETTE OF SOUTHERN ALBANIAN GROUP (1933)



Type Silhouette of the Southern Albanian Group.

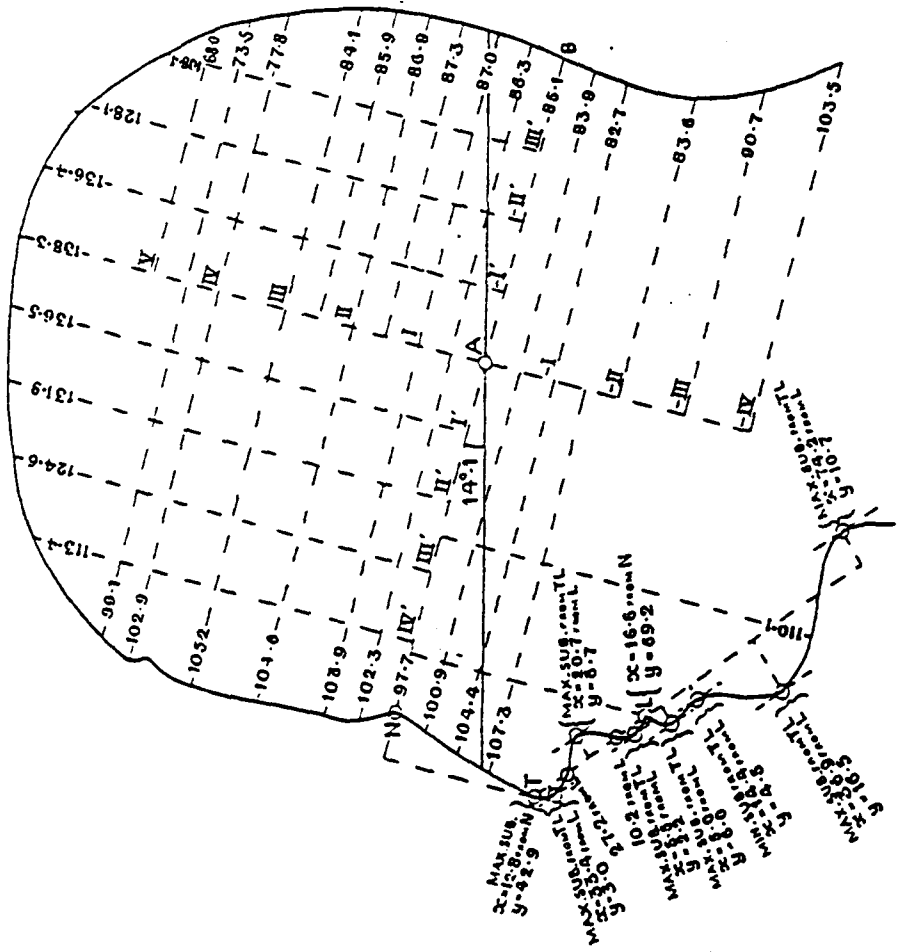


Diagram II. Type Contour of Southern Albanian Group.

autumn of 1906, seven months after Weldon's death, Galton wrote to Pearson: 'could you be persuaded to take control of the Eugenics office as a branch of the Biometric Laboratory?'[114] Galton had already approached Pearson some three weeks earlier following Schuster's resignation. Pearson sent a letter the next day and explained to Galton that

> I have great hesitation in taking any initiative at all in the Eugenic Records Office work, because I did not want you to think that I was carrying all things into the biometric vortex![115]

He then emphasised that there were marked differences in the way that research was undertaken in the two laboratories. In the Drapers' Biometric Laboratory there were about a dozen workers always engaged in research with one inquiry going on for five or six years and through two or three generations of students; the work, nonetheless, got done and was eventually published. One of Pearson's investigations of school children took five years to collect and two additional years to reduce the data. Pearson, therefore, thought that this system would be too slow for Galton's interests: it would would have been almost impossible to undertake Pearson's biometric style of work without continuity nor could one person alone (in the Eugenics Record Office) attempt such an inquiry.[116] Moreover, Pearson was also of the impression that Galton held the view that there was room for more than biometric treatment of the sociological problems that interested Galton.

Pearson, who had longed to discuss the plan with Weldon, wrote to Mrs. Weldon in December to let her know that Galton had asked him

> to take entire charge of the Eugenics Office and I am planning its rearrangement. I do wish I could talk it out with R. now. It seems only yesterday that he left the room with these studbooks and only a day or two earlier that those UC lectures were in progress![117]

Pearson finally suggested to Galton that he wanted

> to make a Eugenics Laboratory a centre for information and inquiry... it ought to associate some half dozen men ... I think we should do no better than [to appoint

114 Francis Galton, Letter to Karl Pearson, 15 November 1906, FG:UCL .
115 Karl Pearson, Letter to Francis Galton, 25 October 1906, Pearson, *Life of Galton*, IIIA, p. 298
116 *Ibid.*
117 Karl Pearson, Letter to Mrs. Weldon, 22 December 1906, KP:UCL.

> [David] Heron and Miss [Ethel] Elderton should be made a Francis Galton *Scholar*...I should suggest a series of Eugenics Laboratory Publications [and I can provide] aid in the Biometric Laboratory.[118]

After Pearson had agreed to take over on 1 February 1907, Galton wrote to him three days later that the 'news about the Eugenics Laboratory is delightful. It is such a great relief to me to know that it rests in such strong hands as your own'.[119]

THE WORK IN THE GALTON EUGENICS LABORATORY: Pearson instituted a regular course of lectures on the 'science of eugenics' on 23 February 1908. Four years later he introduced the *Eugenics Laboratory Lecture Series* (to publish these lectures from 1912 to 1921). From the beginning, the work in the Laboratory involved 'a great deal of heavy tabling and calculating' for constructing the family pedigrees and calculating actuarial death rates. Basic descriptive statistical methods (such as the mean, standard deviation and the coefficient of variation) were also calculated from time to time. Pearson outlined the objects of the Galton Eugenics Laboratory, in a letter to the *Times*, to have been

> (i) research in all that tends mentally or physically to the improvement of the races,
> (ii) the spread of the knowledge thus gained by publication and public lectures, and
> (iii) the accumulation of material learning on problems of racial fitness.[120]

In his first report to University College on the work in the Galton Eugenics Laboratory, Pearson described the routine work undertaken in the eugenics laboratory; this involved the continual collection of family histories which was to be published in a *Thesaurus* of family pedigrees of pathological, physical and mental characters.[121] This thesaurus, which would become known later as the *Treasury of Family Inheritance*, was published in three volumes (with approximately seven parts to a volume) beginning in March 1909 and ending in 1930. Though the idea of the *Treasury* was not Galton's, it 'met with his full sympathy'.[122] One of the first tasks

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118 Pearson, Letter to Francis Galton, 22 December 1906, in Pearson, *Life of Galton*, IIIA, p.304

119 Francis Galton, Letter to Karl Pearson, 4 February 1907, FG:UCL.

120 Karl Pearson, 'The position of the Galton Laboratory at the end of the War.', Letter to the Editor of the *Times*, June 1919, KP:UCL.

121 Karl Pearson, Report on the work of the Francis Galton Laboratory for National Eugenics, February 1908 to June 1909, KP:UCL/235.

122 Pearson, *Life of Galton*, IIIA, p.361.

that was undertaken in the Eugenics Laboratory was a very extensive collection of pedigrees which also required draughtsmen for the engraving. Less than six weeks after the Laboratory had been established, he wrote to Major Greenwood that the

> idea of a *Thesaurus* [he] suggested is not only needed for medical men but for all theorists of heredity so that we should get together a classical work. We have now nearly 400 albino pedigrees. Some of them quite short, others long trees with 100 to 200 individuals and on an average of 10 pedigrees go to a plate.[123]

By then, David Heron, who had been reappointed Research Fellow for the year had given a lecture on Heredity (followed up by a discussion on the work of the Eugenics Laboratory) before the newly formed Eugenics Education Society of which Galton was the Honorary President.[124] Ethel Elderton decided to remain on the Laboratory Staff even though she was offered an important secretarial post in one of the Schools of the University. Amy Barrington, who held a teaching appointment at Bedford College for two days a week, finished her work on the 'Inheritance of Vision and the Influence of Environment on Eyesight'.[125] Miss K. Ryley had prepared nearly thirty pages of pedigrees in more than a hundred families.

Family pedigrees were used in most of the articles in the *Annals of Eugenics* and in a number of their public lectures whilst actuarial death rates were used fairly regularly as well. There was no one particular Pearsonian statistical method that was favoured over another (as has been argued by Donald Mackenzie).[126] The statistical methods that were chosen were appropriate to the 'scale of measurement'. The following four statistical methods were used when examining either continuous variables (such as stature, weight, time) or discrete (such as father's occupation) or a combination of the two: (1) Pearson's product-moment correlation coefficient was used for two continuous variables, (2) the point-biserial correlation coefficient was used when one variable was discrete and the other was continuous, (3) the tetrachoric

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123 Karl Pearson, Letter to Major Greenwood, 14 April 1908, KP:UCL.

124 Karl Pearson, Report on the work of the Francis Galton Laboratory for National Eugenics, February 1908 to June 1909, KP:UCL/235, p.57.

125 *Ibid.*, p.56.

126 Mackenzie identified the tetrachoric correlation coefficient as the dominant methodology—as was discussed in Chapter VI.

correlation coefficient was used when both variables were dichotomous and assumed an underlying normal distribution, (4) the chi-square test of independence was used for discrete variables that could be classified in three or more categories. The chi-square goodness of fit test was used for the first time in 1933.[127] In addition to these methods, such quantitative methods as 'sorting-cards' were also used.[128]

FAMILY PEDIGREES Pearson and his co-workers in the Eugenics Laboratory constructed family pedigrees across four or five generations consisting of as many as 400 ascendents and collaterals.[129] To form an 'effective' pedigree, it was necessary to make 'inquires in all parts of the country, possibly abroad, and to journey about seeing innumerable people and often in strange environments'.[130]

The groupings consisted of the father and mother ('with reference to their family group numbers'), their offspring, the wife or husband of each child with their family group members, and the family group number which gave the offspring of each marriage in the first family group. They also included the birthdays of the parents in every family group, to distinguish couples with the same names.[131] In Pearson's Presidential Address on 'Nature and Nurture: The Problems of the Future', delivered on 28 April 1910 to the Social and Political Education League, he devised pedigrees for cataracts and general degeneracy (see Plate 7.1), musical ability in the Bach Family (Plate 7.2) plus dramatic ability in the Kemble Family (Plate 7.3).[132] Pauline Mazumdar has described the pedigree as

> a network of relationships, demonstrating inheritance of defect in terms of the biological connections within a social class. The hereditary transmission of the defects which are the characteristics of this class it made obvious at a glance... The pedigrees [were] not arranged like diagrams of Mendelian inheritance, with two

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127 Percy Stocks and Mary Noel Karn used the chi-square goodness of fit test in their paper on 'A cooperative study of cancer and control patients', *Annals of Eugenics*, 5 (1933).

128 Margaret Moul used Sorting-Cards (in 'Problems of Alien Immigration', *Annals of Eugenics*, 1, (1928), 5-127: a full description of this procedure is given on 91.

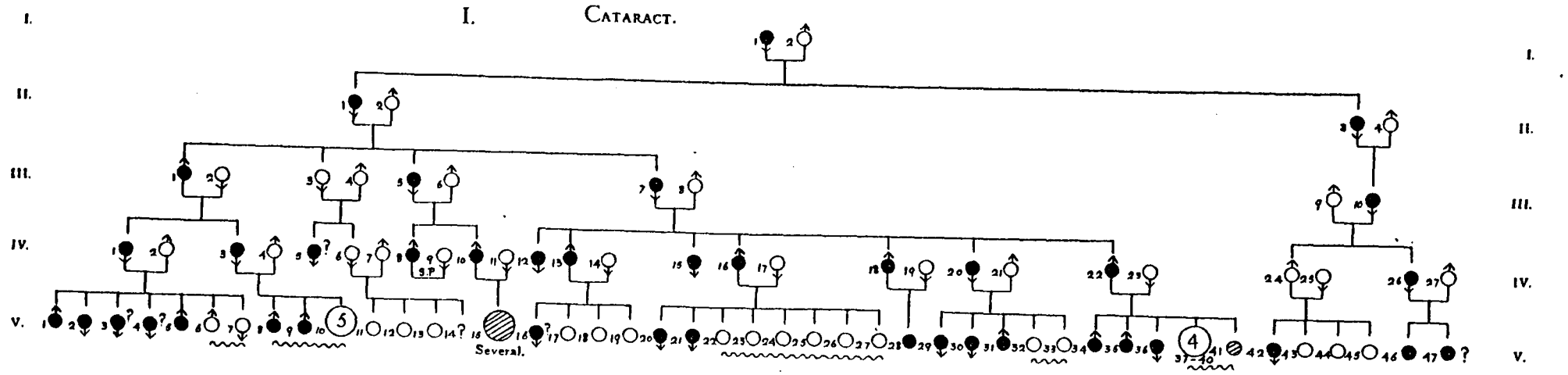
129 Karl Pearson, 'On the inheritance of Mental Disease', *Annals of Eugenics*, 4 (1932), 362-380.

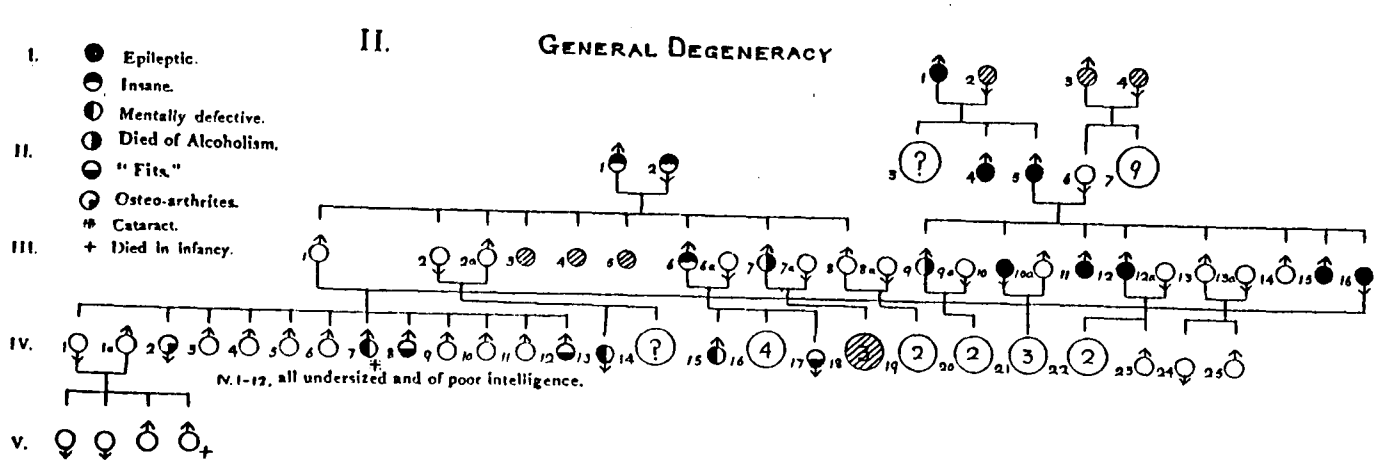
130 *Ibid.*, 363.

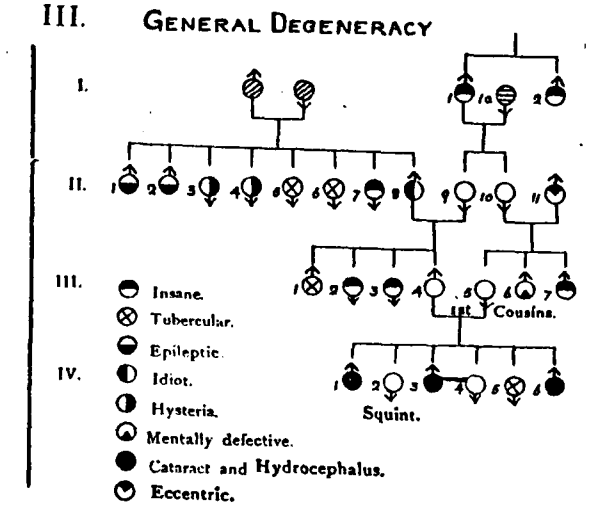
131 Pearson, *Life of Galton*, IIIA, pp.103-4

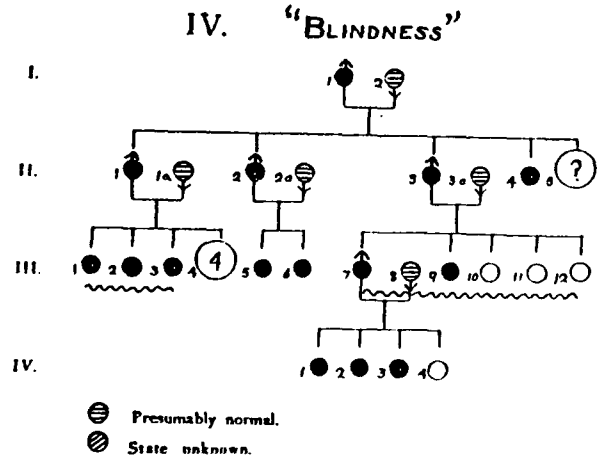
132 Karl Pearson, 'Nature and Nurture: The problems of the Future', *Eugenics Laboratory Lecture Series*, (28 April 1910). The reproduction of the plates used here are amongst Pearson's papers on 'Work on the Inheritance of Diseases and Insanity', 199/1, KP:UCL.

Plate 7.1
PEDIGREE OF CATARACTS AND GENERAL DEGENERACY (1910)









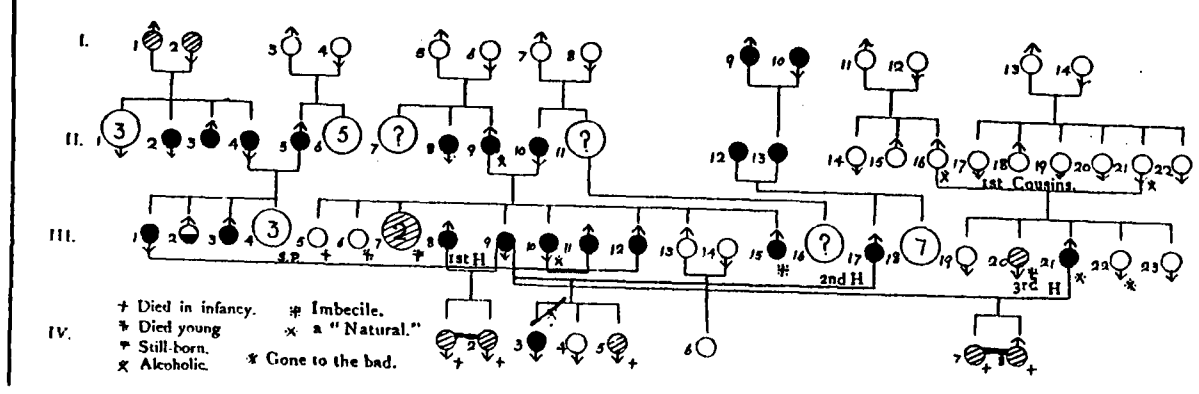


Plate 7.2

PEDIGREE OF MUSICAL ABILITY IN BACH FAMILY AND PEDIGREE OF INTELLECTUAL ABILITY IN SCIENCE (1910)

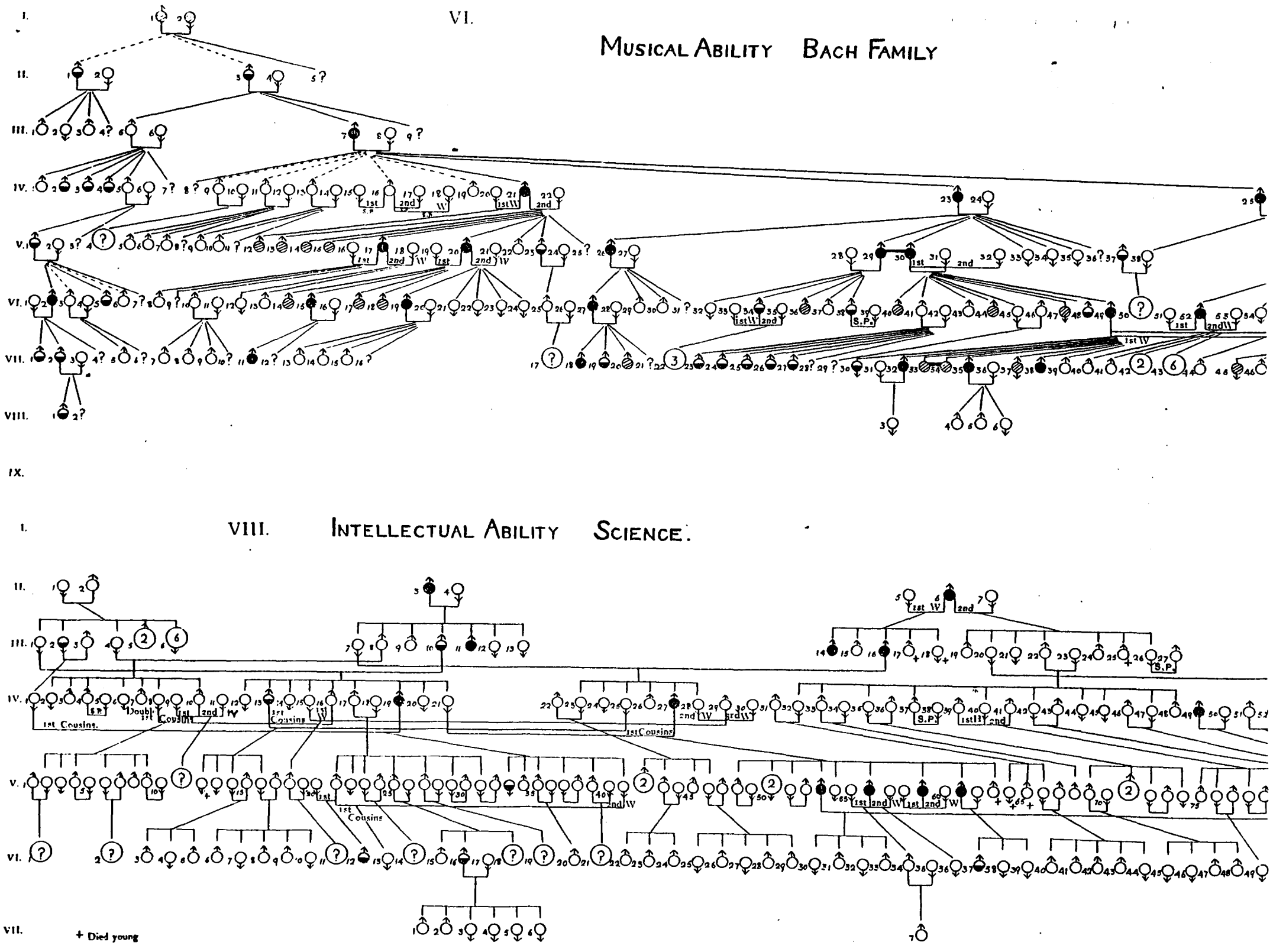
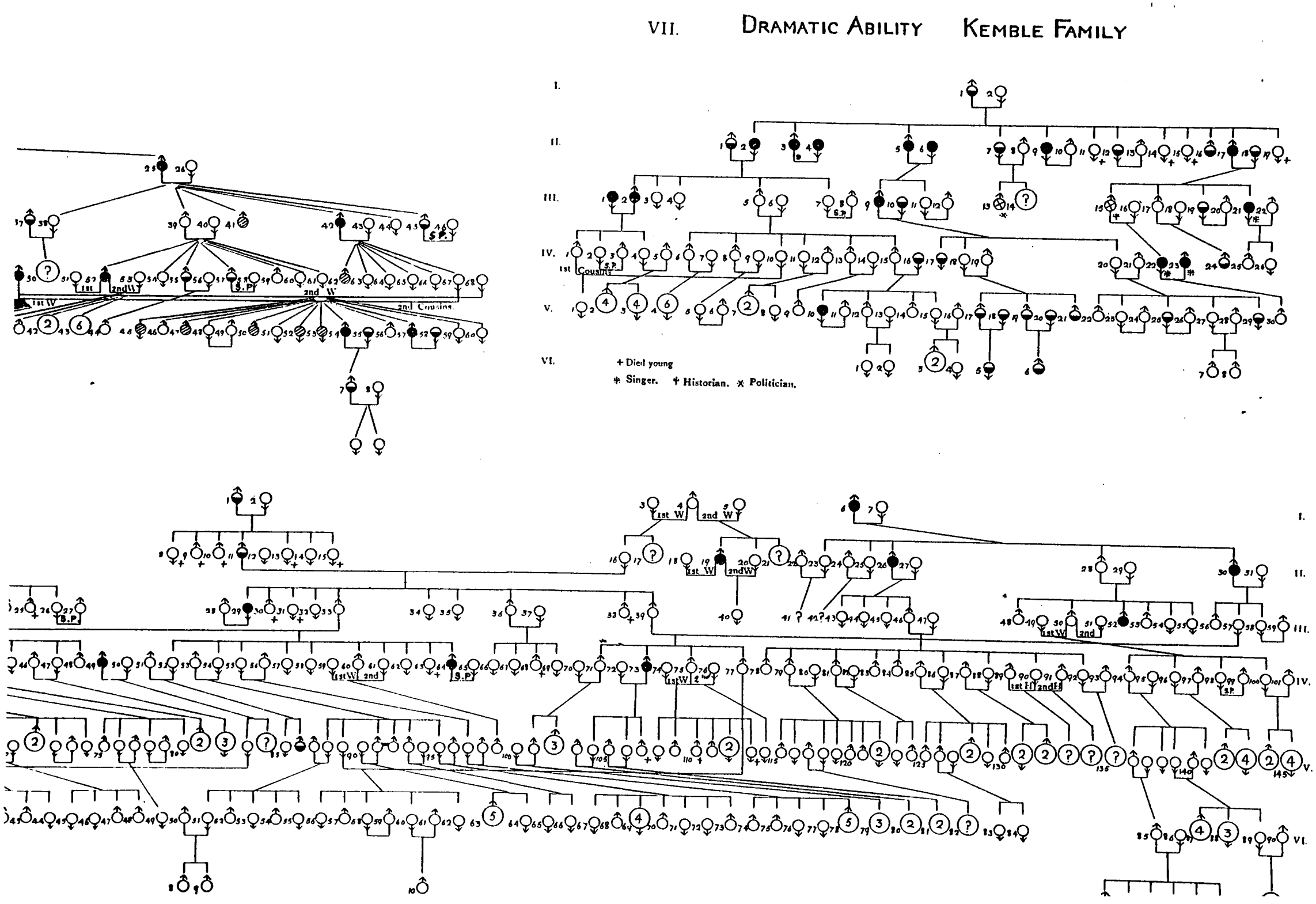


Plate 7.3
PEDIGREE OF DRAMATIC ABILITY IN KEMBLE FAMILY (1910)



> parents and two filial generations. They [were] more like huge kinship networks, spreading often across several fold-out pages.[133]

Pearson was interested in looking at the inheritance of 'mental disease' which he regarded as

> only one of the ills of humanity with which we have to deal in the course of our eugenetic researches; it [was] from the national standpoint one of utmost gravity, and at the same time, unfortunately, one upon which it is hardest to obtain accurate information.[134]

When looking for patterns of mental disease or of feeble mindedness, Pearson found a number of anomalies in the families for whom he had devised pedigrees across five generations (see Plate 7.4 which is a typical size pedigree used in the *Annals of Eugenics*). Thus it seemed that there was not only one form of insanity but 'there was a fringe of other types of failures'.

Of the attempts made to fit insanity and feeble-mindedness into the Mendelian theory, Pearson thought that they 'failed hopelessly for they overlooked essentially the fact that insanity and feeble-mindedness are far from being simple-unit characters'.[135] Moreover, he did not agree with those 'geneticists who believe[d] that feeble-mindedness was a simple Mendelian unit character recessive of normal mentality!'[136] There seemed to be no clear-cut boundary between mental defect and normality: the states of mental efficiency and inefficiency was not a single unit gene, but was instead thought to be 'continuous'.[137]

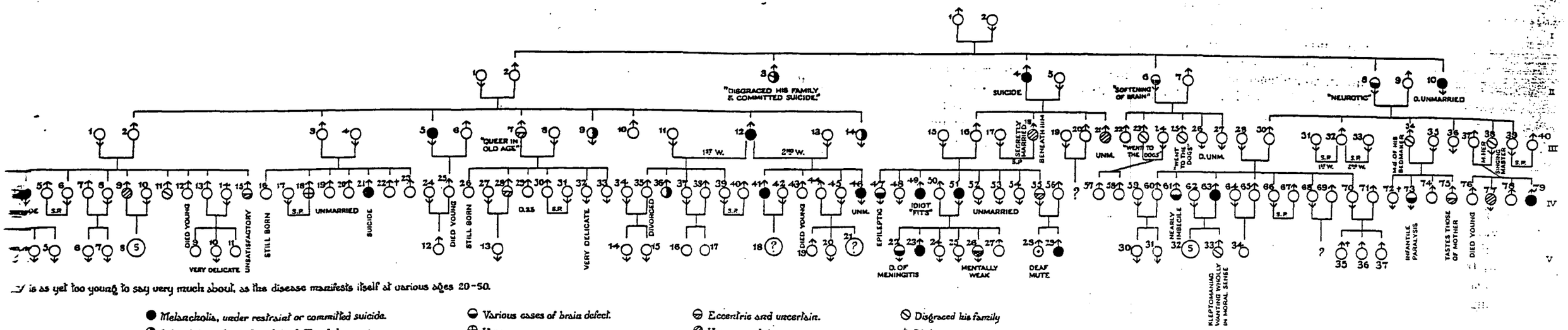
The immense biological complexity to problems of the nature and nurture of degeneracy required

> not verbal disquisition, but an intensive study of heredity in man of differential death rates, differential fertility, selective death rates, to say nothing of immigration and emigration, and of the correlation of all of these social and antisocial qualities of the several reproductive groups in the community.[138]

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133 Pauline M.H. Mazumdar, *Eugenics, Human Genetics and Human Failings* (Routledge: London, 1992), p.87.
134 Pearson, 'On the inheritance of Mental Disease', 362.
135 *Ibid.*, 366.
136 *Ibid.*, 374
137 *Ibid.*, 379.
138 Pearson, 'Nature and Nurture: the Problems of the Future', p.3

Melancholia (Suicide), Eccentricity and Mental Deficiency.

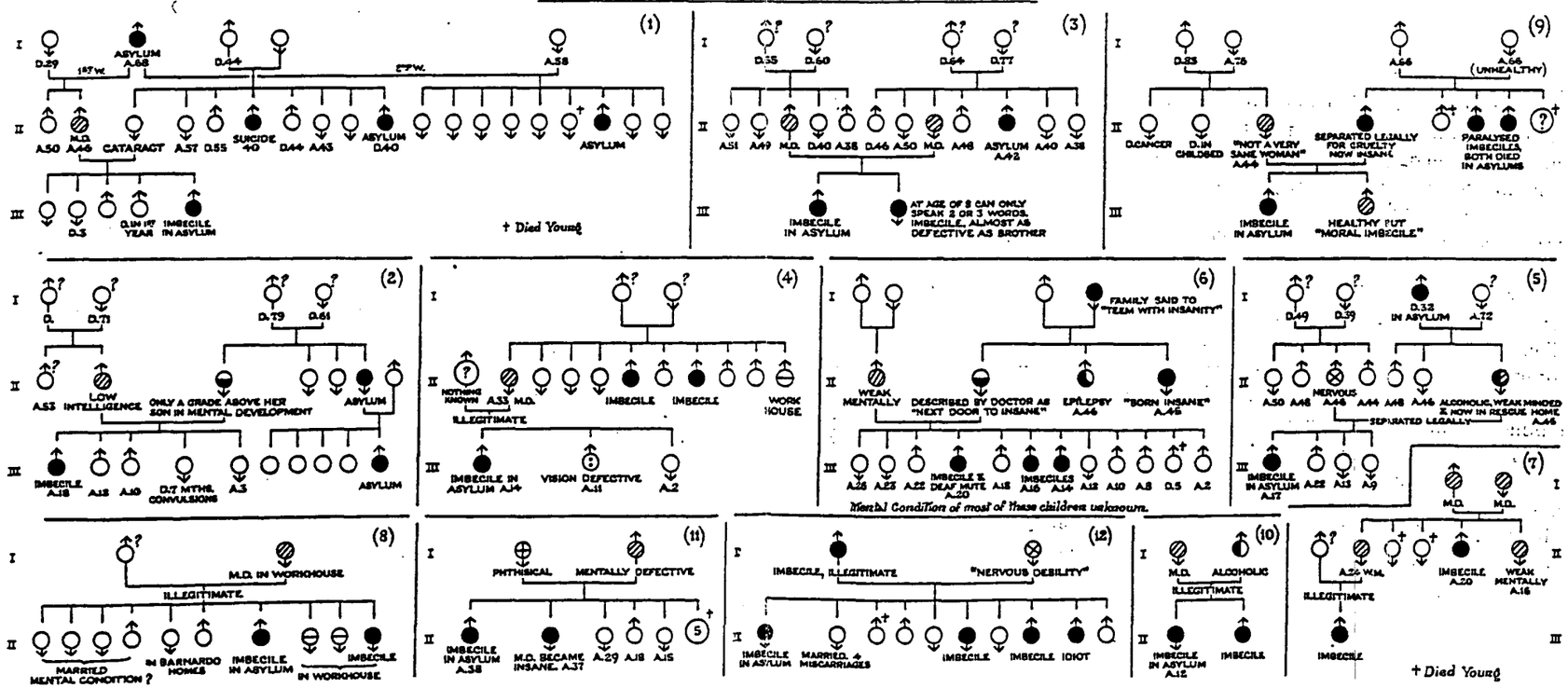


? is as yet too young to say very much about, as the disease manifests itself at various ages 20–50.

● *Melancholia, under restraint or committed suicide.*
◑ *Subject to prolonged and bad fits of depression.*
◒ *Various cases of brain defect.*
⊕ *Very nervous.*
⊖ *Eccentric and uncertain.*
⊘ *Very eccentric.*
⊘ *Disgraced his family*
† *Died young.*

Diagram I.

Incomplete Pedigrees of "Imbeciles"
showing relationship to insanity and feeblemindedness



M.D. = Mental Defective.

Diagram VI.

† Died Young

[To face p. 365

After having established the various family pedigrees, the next step involved measuring relationships using either the product-moment correlation coefficient, the biserial correlation and the tetrachoric coefficient for such biological measure as length and weight or of vaccination and recovery in the case of small pox.

In his 'Nature and Nurture' paper, Pearson deduced that the effects of environment were small. He began by measuring relationship between siblings for a series of physical characteristics such as stature, eye-colour, cephalic index; the correlation coefficients for these variables ranged from .43 to .65. Pearson then addressed problems of nurture and attempted to find correlations between 'economic conditions of home and keenness of vision of offspring': 'employment of mother and weight of son/daughter', or 'wages of father and weight of child'. (For these variables, he used partial correlations, biserial correlations and the product moment correlation coefficients.) He deduced that the effects of environment were negligible as the results produced very low correlations ranging from .00 to .11). Pearson's interpretation of his data seems to be one instance where he placed too much empahsis on the results of his statistical methods. There are a number of reasons that his data could have produced low correlations: the results may have been artefacts of the construction of his variables or of the range of the scales used (especially for so-called 'nature' variables), there may have been 'outliers' in the data set or the sample may not have been as representative as Pearson would have wanted. Though he would

> not dogmatically assert that the environment matters not at all ... [it was] quite safe to say that the influence of environment is not one-fifth that of heredity.... There is no realy comparison between nature and nurture; it is essentially the man who makes the environment, and not the environment which makes the man.[139]

The Eugenics Society, founded a year after Pearson established the Eugenics Laboratory, adopted the family pedigrees (which had been devised in the Eugenics Laboratory) as their methodology. C.P. Blacker considered that 'the main orientation of the Laboratory [was] towards the scientist; that of the Society towards the

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139 *Ibid.*, p.27.

public'.[140] Pauline Mazumbar, who was interested in the development of the Society's scientific methodology and its relationship to the ideological needs of those who used it, has argued that the

> earliest method adopted was the pedigree. It was the Eugenics Society's preferred tool for both investigation and propaganda, in Britain as it was generally throughout Europe and America in the years before the First World War... The British group did not use it in the way it was used in America, to claim that a trait was inherited as a Mendelian unit character. Instead they saw a pedigree as a straightforward demonstration that like engendered like, with no specific theory of inheritance implied.[141]

Though she argues convincingly that the pedigree was the methodology of the Eugenics Society and has addressed Pearson's work on pedigrees in the *Treasury*, when considering the methods used in the Eugenics Laboratory, however, she argued that

> Pearson used his department mainly for the collection and statistical analysis of data on inheritance. The statistical tool which was most typical of the biometric style was the calculation of the correlation coefficient.[142]

Thus she gave greater consideration to Pearson's correlation coefficient rather than his very extensive use of family pedigrees.

Two years after the Eugenics Laboratory was established and after the collection of more than 500 pedigrees, Pearson issued the first volume of the *Treasury of Human Inheritance* in March of 1909. This was to have 'provided students of eugenics and of sociology, medical men and others with an organ where their investigations could be published'.[143] This work demanded the development 'of special methods of forming, drawing and engraving pedigrees, and of briefly describing the character of individuals'.[144] Pearson had hoped to issue the first part in 1909, but as the laboratory

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140 C.P. Blacker, *Galton, Eugenics and After* (Duckworth: London, 1952), p.14.
141 Mazumdar, *Eugenics, Human Genetics and Human Failings*, p.4.
142 *Ibid.*, p.60.
143 Karl Pearson, 'The Treasury of Human Inheritance issued by the Francis Galton Laboratory for National Eugenics', *Drapers' Company Research Memoirs. Studies in National Deterioration*, (1910), Frontispiece.
144 Karl Pearson, *'Report on the work of the Francis Galton Laboratory for National Eugenics'*, February 1908 to June 1909, KP:UCL/235, p.57.

> had to break new ground, form new channels of information, [and] devise new methods of representation, these difficulties must largely excuse the delay in publications.[145]

The first volume of the *Treasury*, which was issued in six parts, contained the following types of pedigrees:

> Parts I and II: diabetes, tuberculosis, polydactylism, deaf-mutism and insanity and legal ability;
>
> Part III: Hermaphroditism, Angioneurotic Oedema, Insanity and Commercial Ability; Part IV: Hare-lip, cleft plate, Congenital cataract;
>
> Parts V and VI: contained family pedigrees of Haemophilia only.

ACTUARIAL DEATH RATES: On 16 June 1910, Palin Elderton informed Pearson that he had sent out a series of letters to various actuaries in the London area.[146] He wrote to the actuaries as he understood that they had 'libraries at their chief offices containing books and papers dealing with the subject connected with Life Insurance statistics and Medical matters'; he thus enclosed a list of publications

> issued by the Eugenics Laboratory. I may call your special attention to the *Treasury of Human Inheritance* which deals amongst other things with actual pedigrees of families suffering from various illnesses and I hope your company will become a subcriber to this publication.[147]

By the end of that year Palin Elderton with Sidney Perry published the results of their work on actuarial death rates.[148] They were among the first in the Eugenics Laboratory to use actuarial death rates. In the forward of their article, Pearson stressed that this work indicated the 'extreme importance of collecting data as to the treatment of the tuberculosis in a form capable of actuarial reduction'.[149]

Elderton and Perry compared the mortality among tuberculosis patients in the following two conditions: (1) after treatment, with that of the general population, and (2) from those who underwent sanatorium treatment compared to the mortality of

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145 *Ibid.*, KP:UCL/235, pp.56-7.

146 William Palin Elderton, Letter to Karl Pearson, 16 June 1910, KP:UCL/682/8.

147 Enclosed with letter of 16 June 1910 to Pearson, KP:UCL/692/8.

148 William Palin Elderton and Sidney J.Perry 'A third study of statistics of Pulmonary Tuberculosis'. The Mortality of the Tuberculosis and Sanatorium Treatment', *Drapers' Company Research Memoirs. Studies in National Deterioration*, (1910), 3.

149 Karl Pearson, 'Prefatory Note to 'A Third Study of the Statistics of Pulmonary Tuberculosis. The Mortality of the Tuberculosis and Sanatorium Treatment', *Drapers' Company Research Memoirs. Studies in National Deterioration* (1910), 2.

patients before that treatment was used.[150] They explained that mortality in these situations could be compared accurately even from only 'a few years [of] observations' as it was possible to determine how many people were 'exposed at various ages to risks of death—or 'had the chance of dying—in each year under consideration'.[151] From the numbers 'exposed to risk', they then worked out the number of deaths that would have occurred if the mortality had been the same as that of the general population. The method they used was derived from the actuarial method of death-rates used by insurance offices who

> determine whether the sum paid in claims by an insurance office in a year or series of years exceeds or falls short of the amount that the office would have expected to pay if the mortality had been the same as that assumed in the valuation of the liabilities of the office.[152]

They found that the mortality of tuberculous patients who underwent treatment was much greater than that of the general population: when the disease was at an incipient state, the mortality was about 'four times as heavy' whereas the mortality of the apparently cured cases was 'twice as heavy as that of the general population'.[153]

By 1912, Elderton began to work as an actuarialist for the Star Assurance Society (though he continued to see Pearson quite regularly).[154]

At the end of the 1920s, Mary Noel Karn undertook the second most extensive use of actuarial death rates in her work in the Eugenics Laboratory which was published in 1931. The method she used to determine the influence of the death rates from some particular disease on duration of life was derived partly from the work of Daniel Bernoulli (1700-1782) in 1760.[155] Bernoulli's method of obtaining a measure of the influence of special diseases on the duration of life involved a comparison of two life-tables; he compared Halley's life table with a hypothetical life-table showing

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150 Elderton and Perry, 'A Third Study of the Statistics of Pulmonary Tuberculosis', p.3.
151 *Ibid.*
152 *Ibid.*, 3.
153 *Ibid.*, 20.
154 Palin Elderton, Letter to Karl Pearson, 1912, KP:UCL/682/8.
155 Mary Noel Karn, 'An inquiry into various death-rates and the comparative influence of certain diseases on the duration of life', *Annals of Eugenics*, 4 (1931), 280.

the number of survivors there would have been at each age.[156] Bernoulli was especially interested in the mathematical theory of obtaining life-table populations freed from the mortality of a particular disease.[157]

Karn wanted to measure the effect of the death-rates from cancer and other diseases on the expectation of life as was given in the English Life-Tables. Her aim was to find out if their had been an increase in the mean duration of life once these diseases were eliminated as causes of death.[158] The material was taken from the Registrar-General's records of deaths 'year by year for certain age groups, and from the ten-yearly census populations'.[159] The method of obtaining the various death-rates was the same for all the diseases considered: the average number of deaths per 100,000 for given ages for 5 years grouped around the census years 1891, 1901, 1911 and 1921. These death rates were then applied to the life-table population appropriate to that period.

When she wanted to determine the proportion of cancer deaths to all death in different types of districts she used the ratio

$$\frac{\text{cancer deaths}}{\text{all deaths}}$$ at given age groups for London and Rural Areas. (see Figure 7.3.1). The Ratio

$$\frac{\text{cancer deaths}}{\text{all deaths}} \times 100$$ was used to determine the death rates for given periods in various age groups (see Figure 7.3.2).[160]

[For other types of death rates see Figure 7.4 for death rates of Cancer and Pulmonary Tuberculosis per 100,000, Figure 7.5 for death rates of Cancer Fatality were eliminated and Figure 7.6 for 'Old Age Death-Rates'.]

Two years later, Karn followed up this paper with a short ten page paper in *Biometrika*. In the previous paper she discussed D'Alembert's formula, which gave 'instantaneous values' for calculating life tables for a population from which cancer and tuberculosis were supposed to be eliminated as causes of death in order to

156 *Ibid.*
157 *Ibid.*, 292-3.
158 *Ibid.*, 303. The first observation made of the increase in cancer death rates was made by William Farr, Assistant to the Registrar General in 1872 in the *Thirty-fifth Annual Report of the Registrar General* (1874), p.224.
159 *Ibid.*
160 *Ibid.*, p. 315.

Figure 7.3.1 and 7.3.2
CANCER DEATHS: ALL DEATHS FROM 1869-1923 OF MALES (7.3.1) and FEMALES (7.3.2) IN ENGLAND AND WALES

DIAGRAM VII . 3.1

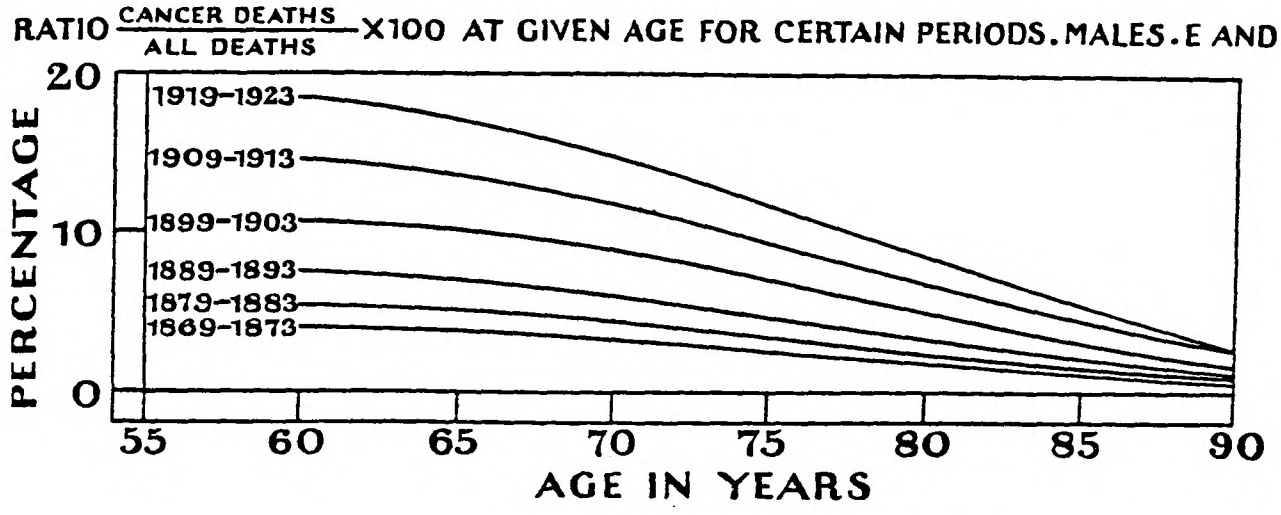


DIAGRAM VII . 3.2

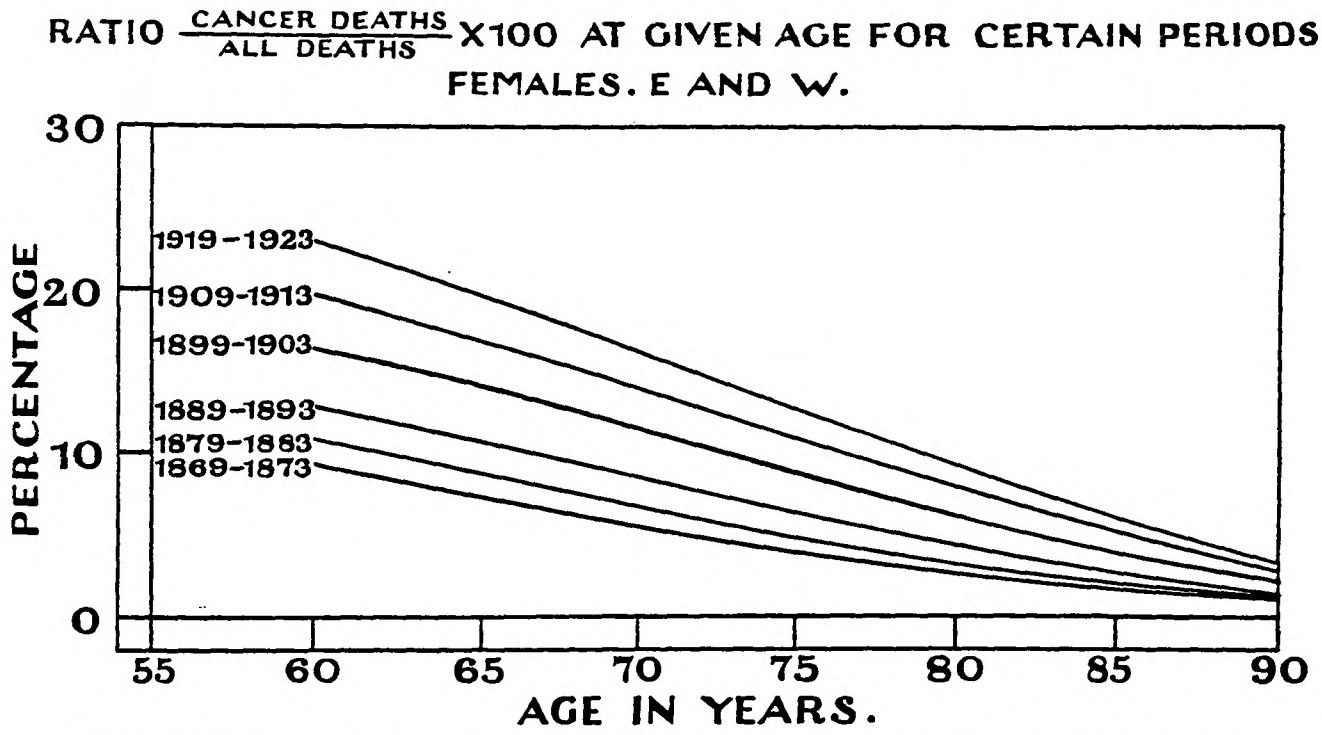


Table XVI A. *Deaths from Cancer as a Percentage of the General Death-rate for Periods between* 1869 *and* 1923. *England and Wales. Females.*

Age	1869–1873	1879–1883	1889–1893	1899–1903	1909–1913	1919–1923
55–	9·06	10·74	12·63	16·20	19·47	22·75
65–	5·28	6·52	8·19	11·19	13·67	15·96
75–	2·37	2·99	4·07	5·90	7·61	8·87
85 and over	1·10	1·30	1·75	2·71	3·46	4·16
Mean at 88·2						

EUGENICS IV, III & IV 41

Figure 7.4

CANCER AND PULMONARY TUBERCULOSIS DEATHS PER ANNUM PER 100,000 POPULATION AT EACH AGE OVER GIVEN DECENNIAL PERIODS, ENGLAND AND WALES: MALE

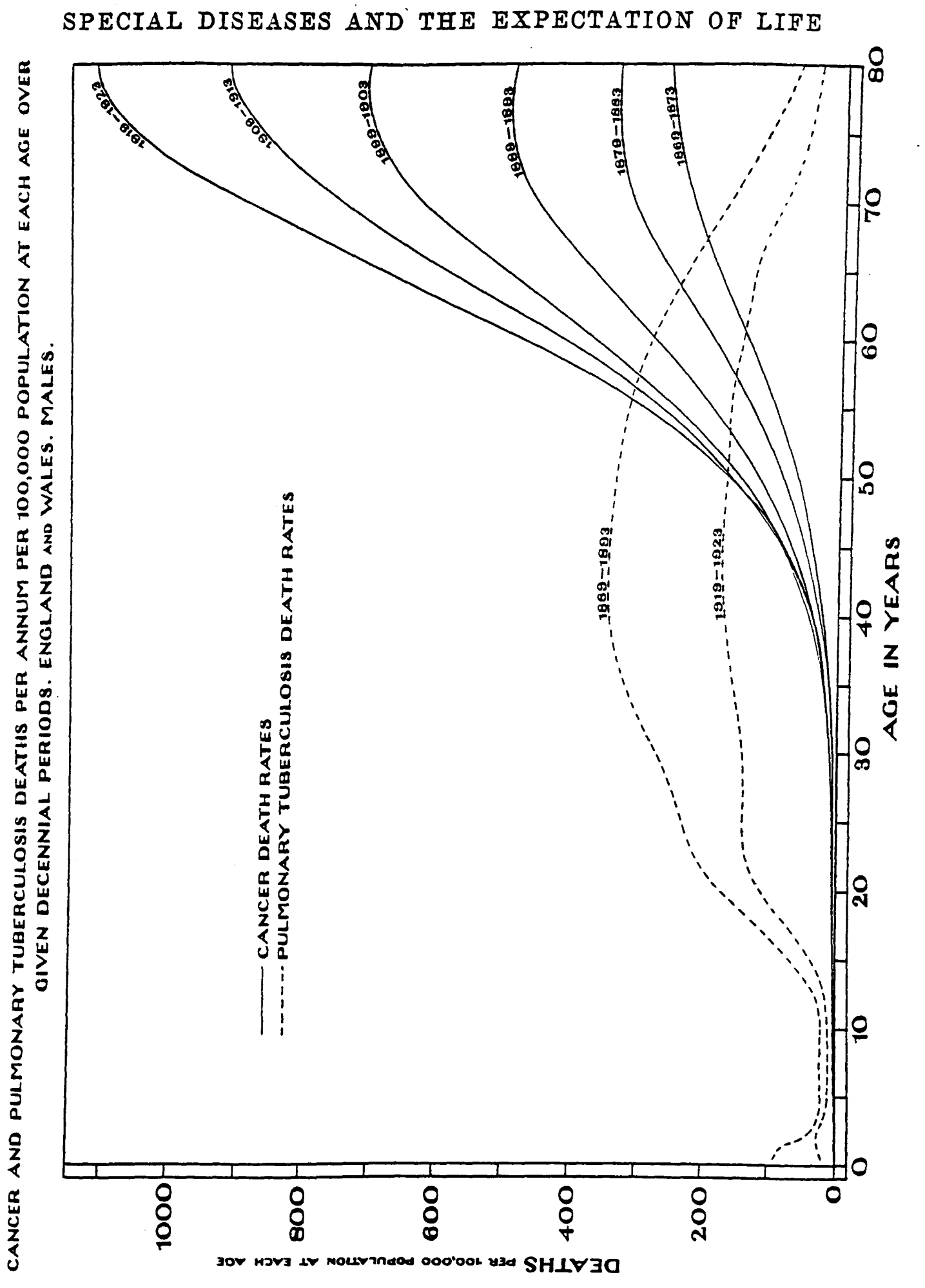


Figure 7.5
CURVE SHEWING THE CHANGE IN MORTALITY IF CANCER WERE ELIMINATED

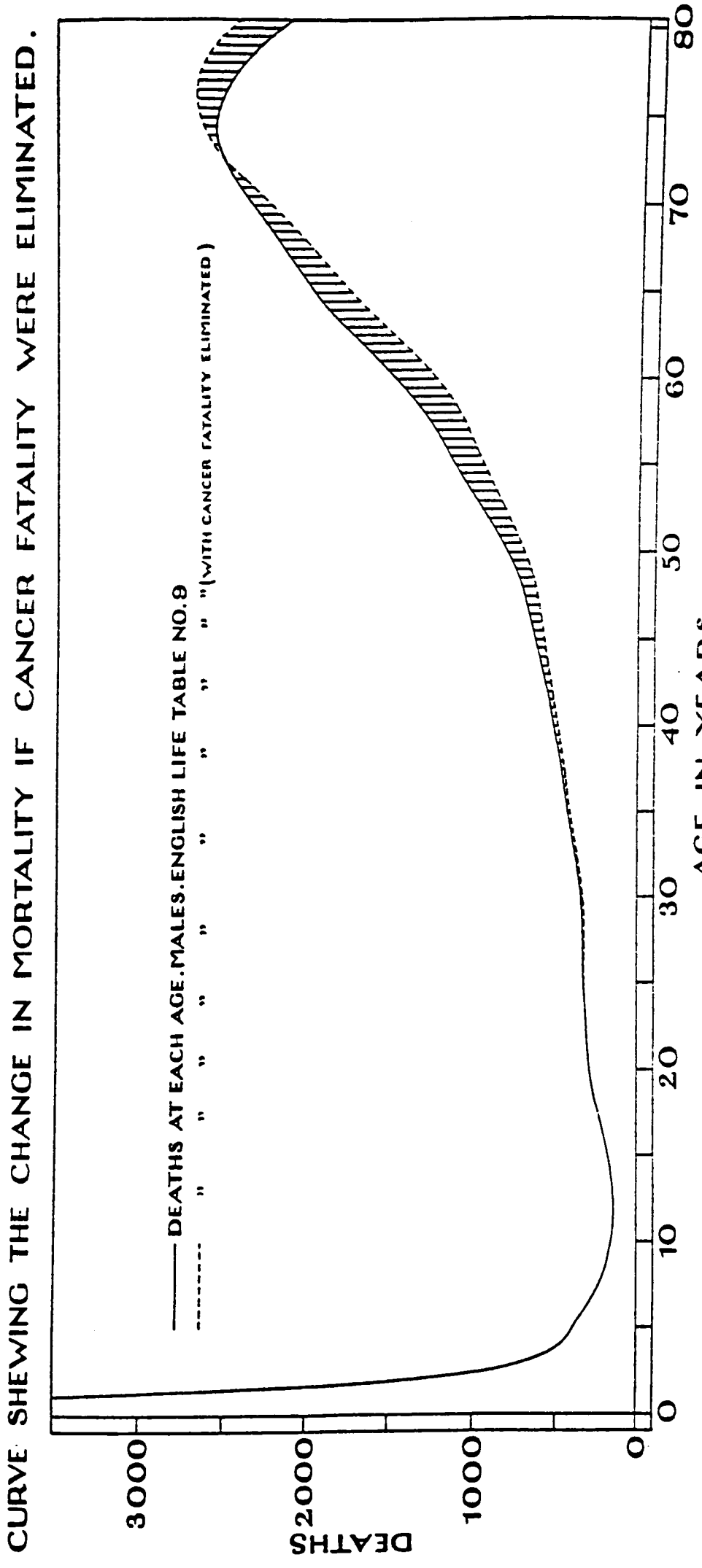


Figure 7.6
OLD AGE DEATH RATES

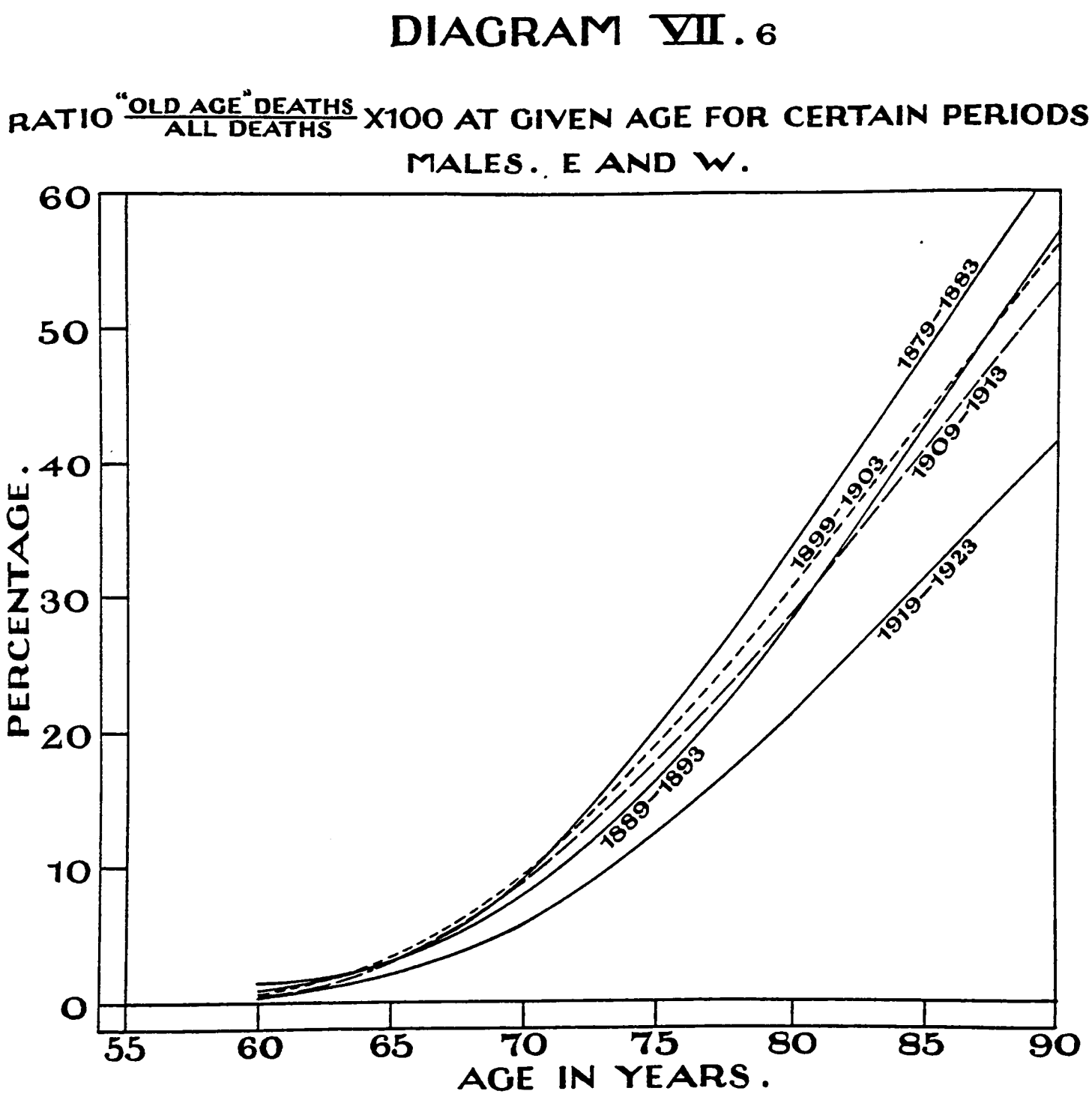


Table XVI. *Deaths from Cancer as a Percentage of the General Death-rate for Periods between* 1869 *and* 1923. *England and Wales. Males.*

Age	1869–1873	1879–1883	1889–1893	1899–1903	1909–1913	1919–1923
55–	4·10	5·48	7·40	10·66	14·35	18·47
65–	3·27	4·52	6·14	8·95	11·85	14·73
75–	1·77	2·24	3·22	4·94	6·63	8·22
85 and over	·82	1·04	1·32	2·08	3·07	3·40
Mean at 88·6						

estimate the effect of these disease in shortening the duration of life.[161] In this paper she discussed the work of William Farr and Louis I. Dublin. Farr used the quinquennial age-groups where the number of deaths in age-groups X to X + 5 was divided by the population of that age group which gave the probability *p* of 'living one year in the middle of the period'.[162] (Thus Farr's method was only applicable after 5 years whereas Dublin had worked out formula values at yearly intervals.) She concluded that for

> rapidity of calculation, combined with accuracy, the formula giving yearly values has some advantages over that giving instantaneous values. In either case the results are arbitrary to some extent, as the original figures are usually obtainable only in quinquennial age-groups.[163]

These procedures and techniques used in the Eugenics Laboratory were to remain the principal methodology used during Pearson's directorship in the laboratory.

THE ORGANS OF THE LABORATORIES: In a period of 28 years Pearson founded and edited the following seven journals for the dissemination of the work that went on in the laboratories: (1) *Biometrika* (of which he was a co-founder with Weldon and Galton), (2) *Drapers' Company Research Memoirs: Biometric Series*, (3) *Drapers' Company Research Memoirs: Technical Series*, (4) *Eugenics Laboratory Lecture Series*, (5) *Drapers' Company Research Memoirs: Studies in National Deterioration*, (6) *Treasury of Human Inheritance*, and (7) the *Annals of Eugenics*. A systematic examination of these journals has indicated that there were distinct methodological differences between the journals in the Biometric Laboratory (1 to 3) from those in the Eugenics Laboratory (4 to 7).

Biometrika was the oldest and the most international of all of his journals; it was the only journal that would not undergo a change of titles and still retains its name today. Prior to 1900, Pearson had published his most original and seminal statistical work in *Philosophical Transactions of the Royal Society*. *Biometrika* was to be in the

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161 *The Annual Report of the Registrar General*, 252 (1933) p.91.
162 *Ibid.*
163 Mary Noel Karn, 'A further study of methods of constructing life tables when certain causes of death are eliminated', *Biometrika*, 25, (1933), 100.

Pearson family for more than 80 years: Karl Pearson was co-editor with W.F.R. Weldon from 1901 to 1906, principal editor from 1906 to 1933 and co-editor with his son Egon until 1936. Following his father's death in 1936, Egon was principal editor until 1982. In 1903, he established the *Drapers' Company Research Memoirs: Biometric Series* and in 1904 the *Drapers' Company Research Memoirs: Technical Series* after he had received a grant from the Worshipful Company of Drapers'.

The early issues of *Biometrika* dealt with the quantification of natural history, demonstrating empirical evidence of natural selection in a variety of plants and animals plus extending Pearson's work on homotyposis. From 1901 to 1906 Weldon edited the papers on natural selection and he also provided assistance to his students at Oxford who were writing articles on natural selection.[164] Weldon's research work on *Clausilia Laminata* (Montagu) published in 1902, has been considered to be some of his most convincing work in demonstrating empirical evidence of natural selection.[165] He found that the species of *Clausilia Laminata* (Montagu) showed possibly

> more clearly than other species, the way in which small and apparently unimportant differences of structure [in the whorls in the snail] with the difference between the survival and the total extinctions of a race of a particular locality.[166]

By 1907, articles on craniometry and physical anthropology began to appear regularly. Interspersed throughout all of the articles were papers on further theoretical development of mathematical statistics. Unlike the articles in the Eugenics Laboratory, nearly all biometric articles demanded a fairly high and specialised level of mathematical statistics.

The length of articles in *Biometrika* varied considerably and ranged generally from 5 to 80 pages: the longest article was 142 pages. Approximately half the publications in *Biometrika* came from those who in worked in the Biometric

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164 See, for example, A.P. Cesnola, 'Preliminary note on the Protective Value of Colour in *Mantis Religiosa*, *Biometrika*, 3 (1904), 28-60. *Idem*, 'A first study of Natural Selection in 'helix Arbustorum' (Helicogene).', *Biometrika*, 5, (1907), 387-99.

165 See Bowler, *The Defence of Darwinism*, p.39 and Maynard-Smith, *The Theory of Evolution*, p.156.

166 W.F.R. Weldon, 'A first study of *Clausilia Laminata (Montagu'), Biometrika*, 1 (1902) , 122.

Laboratory (as staff or visitor) and the other half came from biometricians from around the world. As principal author, Pearson wrote 219 articles for *Biometrika* and 10 for the *Drapers' Biometric Series*. The 229 articles he wrote for these two journals represented 35% of his total number of the (648) publications and 56% of the (406) statistical publications. (The corpus of all of his statistical papers, as principal author, represent 62% of his total number of publications.) He usually wrote about two or three articles in every issue of *Biometrika*; his last article was written three months before his death and published posthumously in *Biometrika* two months after his death.

Once the Galton Eugenics Laboratory had been set up in 1908, Pearson established these journals:

1. *Eugenics Laboratory Lecture Memoirs* were reprints of lectures given in the Eugenics Laboratory from 1909 to 1912.

2. *Studies in National Deterioration* involved studies of alcholism and tuberculosis (1909-1912);

3. *Treasury of Human Inheritance* which was the thesarus of family pedigrees (1909 to 1922);

4. *The Annals of Eugenics* (now the *Annals of Human Genetics*) published mainly re-prints of public lectures (founded in 1928 and edited by Pearson until 1933).

On 16 April 1925, Pearson wrote to the American biometrician, Raymond Pearl (1879-1940), to let him know that he and his co-workers in the Eugenics Laboratory had

> settled to issue a new journal to be entitled *The Annals of Eugenics*, a journal for the scientific study of racial problems... The journal will, of course, be run on biometric lines and it will have 'no development of theory' papers and be confined to man or experimental work on animals bearing on man.[167]

The Annals of Eugenics would be different from *Biometrika* as the *Annals* would 'concentrate on man' whereas *Biometrika* was 'the measurement of life' and thus, was concerned with *all* living organisms.[168] Pearson wanted to know if Pearl would contribute a paper to the first issue; Pearl responded 'yes as soon as he possibly could,

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167 Karl Pearson, Letter to Raymond Pearl, 16 April 1925, Raymond Pearl papers, The Library of the American Philosophical Society. [Hereinafter referred to as RP:APS.]

168 *Ibid.*

but it would not be in time for the first volume'.[169] The publications from the Eugenics Laboratory were primarily 'in-house' journals. *The Annals of Eugenics* had a small group of co-workers who contributed regularly to the journal. Those who contributed most regularly from 1928 to 1933 included: Ethel Elderton, Mary Noel Karn, Margaret Moul, Percy Stocks, and Geoffrey Morant. Occasionally, papers came from other parts of London such as the work from H.J. Martin, an epidemiologist at the London School of Hygiene and Tropical Medicine.[170]

As the *Annals* were quite often re-prints of public lectures, they were aimed for a far more general audience than serviced by *Biometrika*. The articles were, on average, 210 pages long and usually divided into three parts (to be published in three different issues). Ethel Elderton (who wrote several articles in the first volume), Mary Noel Karn, Margaret Moul and Percy Stocks each produced *at least* one 300 page article which involved the use of pedigrees. Morant, however, was more closely aligned to the Biometric Laboratory as he was interested in craniometry: his main contribution was a 180 page article on 'Studies on Paleolithic Man'. He felt that this article would have been appropriate for the *Annals of Eugenics* as

> the most direct approach to the wider problems of the ancestry of man of racial heredity and development is to be made, at present, by studying the ever increasing number of pre-historic remains which have been preserved.[171]

Though there was very little overlap between *Biometrika* and *Annals of Eugenics*, a biometrican such as Geoffrey Morant wrote papers on craniometry for the eugenicists and Mary Noel Karn wrote a short paper for the biometricians on actuarial death rates. This seems to have been the extent of the overlap between the two laboratories during Pearson's editorialship. When the eugenicists used biometrical techniques in their articles, the biometricians usually assisted with the calculations (except for Ethel Elderton who seemed to have done her own calculations—possibly with some

169 Raymond Pearl, Letter to Karl Pearson, 30 April 1925, RP:APS.
170 H.J. Martin, 'A biometric study of the weights of infants during the first days of life', *Annals of Eugenics*, 4 (1931), 327.
171 Geoffrey M. Morant, 'Studies in Paleolthic Man', *Annals of Eugenics*, 1, (1928), 257.

assistance from her brother Palin). It may be seen that the exchange of the articles served to inform the respective laboratories of their work.

Of the number of articles that Pearson wrote for these journals, he published 11 in the *Annals of Eugenics*, 11 in the *Eugenics Laboratory Lecture Series*, five in *Studies in National Deterioration* and two for the *Treasury of Human Inheritance*: these 29 papers represented .04% of his total publications. In addition to these articles, Pearson published an additional 20 articles on matters relating to eugenics in such journals as the *Journal of the Royal Sanitary Institute*, the *British Medical Journal* and *Questions of the Day and Fray*. Thus, the total number of Pearson's articles on eugenics include 49 papers which represents .09% of his total number of publications. Nonetheless, this .09% has not only remained the most controversial of Pearson's writings, but it has assumed greater prominence in the historiography of Pearsonian statistics than his other scientitfic contributions.

THE GALTON CHAIR OF EUGENICS

From the shared loss 'of an irreplaceable friend and colleague... which touched them both with nearly equal sorrow', Pearson and Galton were to develop a warm and intimate friendship. Some 18 months after Weldon's death, on 16 October 1908, Galton wrote to Pearson, 'I value friendship more and more the older I grow and am delighted to feel that I possess yours.'[172] With Galton's advancing age, he became concerned with the future of the Eugenics Laboratory. On 20 October 1908, Galton made his will and 'bequeathed all the residue of [his] estate ... unto the University of London for the establishment and endowment of a Professorship at the said University to be known as "The Galton Professorship of Eugenics"'.[173]

Several months later, Galton was discussing with Pearson potential men who might be appropriate for the Galton Chair of Eugenics. Pearson let Galton know that

172 Francis Galton, Letter to Karl Pearson, 16 October 1908, FG:UCL.
173 Francis Galton, 'Codicil to the Will of Francis Galton', in Pearson, *Life of Galton*, IIIA, p.437.

he was 'wholly unwilling to give up the superintendence of the Biometric Laboratory [he] had founded and confine [his] work to Eugenics Research'.[174] Galton, however, thought that Pearson would have been 'the most suitable man for the first Galton Professor'.[175] He then decided to add a codicil to his will, on May 1909, stating that he 'desired that the first Professor of the post shall be offered to Professor Karl Pearson and on such conditions as will give him the liberty to continue his Biometric Laboratory'.[176]

In 1909, Galton's health began to deterioriate and towards the end of 1910, he had become physically frail though still mentally active. On 17 January 1911, at the age of 89, he died of heart failure. It was only after Galton's death that Pearson learned of the codicil. In the summer of 1911, Pearson relinquished the Goldsmid Chair of Applied Mathematics after 27 years of tenure to take up the Galton Chair. The two laboratories, which would continue to receive separate funding, then became incorporated into the Department of Applied Statistics. The essential aim in combining both laboratories was to enable Pearson to give up his undergraduate teaching of applied mathematics and to devote himself 'solely to what had for many years been the main element of [his] research: the advancement of the modern theory of statistics'.[177]

At the time of Galton's death, Pearson was trying to put together another posthumous paper on Weldon's mice. A year after his death, he wrote to Mrs. Weldon that 'I want badly a staunch friend like R.W. The mice would have been finished, but for Galton's death and I have felt it a great duty to write the life of him.'[178] He had learnt recently that one of Weldon's former students, Gilbert Charles Bourne, had been asked to write up the entry for Weldon in the *Dictionary of National Biography*. He then informed her that 'I was not asked to write the Weldon life for the DNB. I say

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174 Pearson, *Life of Galton*, IIIA, p. 436
175 *Ibid.*, p. 436.
176 op. cit. [codicil]
177 Karl Pearson, Report to the Drapers' Court on 'Past history', (March 1918), 3-4.
178 Karl Pearson, Letter to Mrs. Weldon, 16 January 1912, KP:UCL.

this in case you think I refused. I was asked to do the Galton Life, but it had to be done by December and I did refuse unless they gave me a clear six months'.[179]

When Galton left £30,000 to UCL, he realised that his funds were inadequate for 'the complete carrying out of his scheme'.[180] In his will he specified that

> his laboratory might, if possible, be associated with the Drapers' Company Biometric Laboratory of the University of London, for he realised that if Eugenics was to become a real branch of knowledge its facts must be submitted to measurement and number for without such a basis it could not assume the status and dignity of a science.[181]

Galton had hoped that the university would provide 'adequate housing to the laboratory that was to bear his name'.[182] Adequate funding had been raised between 1908 and 1914 and contracts for the fittings had been made.[183] Some eight years later, Pearson would write to Mrs. Weldon recalling the events that led to completion of the building:

> Like his cousin Charles Darwin, Francis Galton had devoted the whole of his life in unpaid scientific service. In view of this he made it a condition that the University should not spend his endowment on a building, but provide for the necessary accommodations of the Drapers' Company Biometric Laboratory and the Galton Eugenics Laboratory and then came the slow disheartening process of trying to collect funds for the Building and equipping a suitable *locus* for our laboratories. In the middle of what seemd a wearisome and endless process, the Provost told me that the College had arranged to give a site and Sir Herbert Bartlett had offered to provide us with a building for our laboratories. It was like the first streak of light in the morning after a night of post-influenza nightmares.[184]

Thus, owing to the generosity of Sir Herbert Bartlett a 'fine building for the housing of the Drapers' Biometric Laboratory and the Galton Eugenics Laboratory was completed in 1914'.[185] The building contained a public lecture theatre, a museum, a library, machine rooms, instrument rooms, experiment rooms and private rooms for research workers.[186]

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179 *Ibid.*
180 Karl Pearson, 'The position of the Galton Laboratory at the end of the War', Letter to the *Times* (June 1919), p.1.
181 *Ibid.*
182 *Ibid.*, p.2.
183 *Ibid.*, pp. 2-3.
184 Karl Pearson, Letter to Mrs. Weldon, 4 December 1922, KP:UCL.
185 Pearson, 'The Galton Laboratory at the end of the War', p.2.
186 *Ibid.*, pp.2-3.

WORK UNDERTAKEN IN THE BIOMETRIC LABORATORY DURING THE FIRST WAR

In the early summer of 1914, the new laboratory building was complete and preparations were underway for the occupation and the fitting up of the public musuem and the anthropometric laboratory.[187] It was hoped that the building would be occupied by October 1915.[188] These developments and further biometric work were shattered by the onset of the First War. The new Laboratory building was taken over by the Government as a military hospital to be used as an annexe to Queen Alexandra Hospital.[189] All contracts were subsequently rescinded.[190]

These changes made it difficult for Pearson to undertake pure research with his highly trained staff. Their position at the beginning of the war was 'an extremely difficult one'. Pearson felt it was

> essential for the future to retain if possible a highly trained staff, but the funds at [his] disposal neither enabled [them] to compete with the high salaries offered to competent statisticians, nor, if they had been, would it have appeared justifiable to keep members of the staff who were urgently needed for national work of importance.[191]

Pearson thought that 'the only reasonable solution of the difficulty [was] the voluntary employment of the Laboratory as a whole on war work'.[192] He discussed the matter with his staff in the first week of August 1914, and his staff agreed to dispense with the best part of their holidays and to devote their time to war work.[193] Very early in the war, several members of his staff took on special war duties 'for which their training in computing largely fitted them'.[194] Many of Pearson's older students and

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187 Karl Pearson, 'Report to the Court of the Worshipful Company of Drapers on the Present Position and Past History of the Laboratories to which their Annual Grant has been Made'. KP:UCL/233. (March 1918) p.4.

188 Pearson, 'The Galton Laboratory at the end of the War', pp.2-3.

189 Karl Pearson, 'University of London. Opening of the new building—given by—Sir Herbert Bartlett, Bt., for the Department of Applied Statistics Drapers' Company and the Galton Laboratories at University College', 4 June 1920, KP:UCL/247.

190 Pearson, 'The Galton Laboratory at the end of the War', pp.2-3.

191 Pearson, 'Report to the Drapers' Court', (1918), p.4.

192 *Ibid.*

193 *Ibid.*

194 Herbert Edward Soper left to do work on electrical apparatus for war purposes; P.F. Everitt left to train women in the polishing of prisms and lenses for periscopes; Beatrice M. Cave went as a computer to the Admirality for naval air-plane work; David Heron left as a statistical adviser to a large insurance company, of which he had since become secretary.

past assistants of the Department were employed in one form or another of statistical work, often of a very confidential character, either for the War Office or for the various new Ministries.[195] Biometric studies could only be carried on

> in brief intervals of rest taken from the computation of machine gun-sights for use against low flying airplanes, from the construction of gun charts and high angle range tables for protection against aircraft, or from other problems of war-like urgency.[196]

STATISTICAL CHARTS AND GUNNERY COMPUTATIONS: In August the workers in the Biometric Laboratory were requested by the government to prepare fortnightly statistical charts on unemployment for the Board of Trade, Labour Department. From their charts they could show 'the state of unemployment for insured labour and uninsured labour in all English, Irish and Scottish towns of over 20,000 inhabitants, and in all county districts'.[197] By July of 1915, they had prepared 600 charts which were used by the Central Relief Committee for the control of local conditions. Their work had shown that 'the possibility of great labour difficulties which had been so marked in December 1914, had vanished, and accordingly these charts ceased'.[198] Their second set of charts was prepared for the Board of Trade, Census of Production Department. These charts indicated the tonnage used for each type of import at each season of the year and hence used to assist the special officials who had the task of controlling the amount and character of imports and the shipping to be used for them.[199]

After nearly two years of producing these statistical charts and computations which left them little time for biometric work, Pearson began to wonder if he and his staff would ever be able to take up their biometric work again. On 23 May 1916, Pearson wrote to Mrs. Weldon that

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195 Pearson, 'Report to the Drapers' Court' (1918), p.4
196 Karl Pearson, 'Prefatory Note', *Drapers' Company Research Memoirs: Biometric Series*, 10, (1919), p.2.
197 Pearson, 'Report to the Drapers' Court' (1918), p.4
198 *Ibid.*
199 *Ibid.*, pp. 4-5.

> the war seems to have come as a cloud of poison and gas choking all one's usual activities and I don't feel that I shall ever have the energy to build up a new staff, fit up a big laboratory, and take up old treads again, should the war ever end.[200]

That summer their work turned to the more military side of war work. In July, their energies were directed to several months of work on elaborate calculations of bomb trajectories 'both through air and air and water'.[201] During a good deal of 1916, Pearson and his co-workers were occupied with 'theory and computations concerning torsional strain in the blades of air-plane propellers for a department of the Royal Aircraft at Farnborough'. A report was also prepared on the elastic constants of wood for the head of a section of the Admiralty Air Department.[202]

On New Year's Day 1917, Pearson and his team of workers assisted Captain A.V. Hill, of the Anti-Aircraft Experimental Section of H.M.S. Excellent, with gunnery computations for anti-aircraft guns. They had in charge the preparation of

> the whole of the charts and high-angled range tables for the anti-aircraft guns in both Army and Navy... The work was so urgent and of such value that the Ministry of Munitions had placed eight to ten computer draughtsmen at [Pearson's] disposal, and with the exception of one week at Christmas the Laboratory was never closed from January 1st, 1917 to January 1st, 1918.[203]

In the 'great bulk of cases', Pearson had to develop new methods from the very rough processes originally suggested to them especially for producing 'high-angled range tables for anti-aircraft guns [which] were unknown before the war had developed the aeroplane as a new instrument of warfare'.[204] One of their last tasks occured just before Christmas 1917 when an urgent demand came from the Front to General Bingiham for 'a remedy against the low-flying German air-planes, which were making things "unhealthy" for [the] men in trenches'.[205] It was a source of great satistisfaction for Pearson that in recognition of the work done

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200 Karl Pearson, Letter to Mrs. Weldon, 23 May 1916, KP:UCL.
201 Karl Pearson, 'History of the Biometric and Galton Laboratories', Appendix 1, Report to the University of London, KP:UCL/247 (1903-1909), p. 3.
202 Karl Pearson with Andrew W. Young and Ethel M. Elderton, 'On the Torsion resulting from Flexure in Prisims with Cross-Sections of Uni-Axial Symmetry'. *Drapers' Company Research Memoirs. Technical Series*, VII (1918), p.75.
203 Pearson, 'Report to the Drapers' Court' (1918), p.5.
204 *Ibid.*
205 *Ibid.*

> they were at once asked by the Ministry to undertake the urgent work of calculating sights for the Hotchkiss, Lewis and Vickers machine guns to meet the cases of air-planes flying with various speeds in various directions and at various low altitudes. The task was a novel and difficult one, but the theory was worked out in the Laboratory, and within four or five weeks of the order the tables were sent to France, arriving there just before Christmas.[206]

Pearson had also 'received requests for trained statisticians and computer from nearly all the chief Government offices and [after] America [had] joined the war even from America'.[207]

Due to war-time activities, nearly all of Pearson's biometric publications had been delayed; during the years 1915 to 1917, Pearson issued one volume of *Biometrika*. Though he tended to publish, on average, 12 articles a year, his output began to decrease steadily during the war: he published 15 articles in 1914, ten in 1915, seven in 1916, five in 1917 and just one in 1918. In 1914 Pearson finished the first part of his *Tables for Statisticians and Biometricians* of which 400 copies had been sold during the war.[208]

THE OPENING OF THE BARTLETT BUILDING

By June 1919, Pearson was in possesion of his building and plans were underway for the opening in October 1920. New specifications were made which had shown that the estimates had increased by 300%: Pearson's staff '[stood] before the alternatives of occupying a single floor or postponing occupation entirely'.[209] The laboratories had thus been

> crippled in its equipment ... [and] in its funds ... the staff that had been absorbed into national work during the past five years could not be re-established in the old scale. The war had swept out of existence institutions as well as men.[210]

The official opening of the Bartlett Building was 4 June 1920. In preparation of the opening, Pearson wrote to Mrs. Weldon on 27 May that

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206 *Ibid.*
207 *Ibid.*
208 Karl Pearson, *Tables for Statisticians and Biometricians*, (Cambridge University Press: 1914). *Idem*, 'Report to the Drapers' Court' (1918),p.5.
209 Pearson, 'The Galton Laboratory at the end of the War'.
210 *Ibid.*, p.3.

> after Francis Galton himself, the man I could have wished to be here for 4 June would be your husband. He would have understood where none others will. Failing him I should like his widow above everybody. The list is a list not ruled by my wishes, but the rule of the authorities that 'all close relatives and all subscribers to the equipment fund of 1911 are to be invited'. Do come, of course, my wife will be there.[211]

There was to be a delay of 18 months before the building would be occupied. On 4 December 1922 the work had been completed and the building was occupied: eight years after Pearson had first thought the building was ready to be occupied. When the final piece had been put into place, Pearson wrote to Mrs. Weldon remembering that in 1914 the building had become

> a reality then the war and the building seemed to disappear into a dim futurity. We now have in the building a bust (by Hope Pinker) of our former colleague Walter Raphael Weldon to whom much of the inspiration towards biometric work came to us.[212]

Pearson kept the bust of Weldon in his room until his retirement in 1933 when it was transferred to the University Museum in Oxford.

There were three floors in the building and the first and second floors were the 'working floors'. On the ground floor, was the 'public floor' as it contained a large museum which illustrated heredity, statistical processes and social problems. There was also a convenient lecture theatre with adjacent diagram and committee rooms, a room for the exhibition of Galton relics and apparatus, an anthropometric laboratory and a publication room.

The First floor had a library, a class room and staff common room; there were four private dining rooms which included the director's room with rooms for the secretary and the librarian. The Second floor had a photographic studio with dark rooms, a large room for biometric workers in craniometry, a workshop, a room for experimental work and two spacious rooms, the one for the archives (which stored observations, pedigrees and schedules). There was another room for the instruments integrators, analysers and curve plotters for the use of draughtsmen; additionally there were three further private rooms for the staff. A site had been reserved for extensions

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211 Karl Pearson, Letter to Mrs. Weldon, 27 May 1920, KP:UCL.
212 Karl Pearson, Letter to Mrs. Weldon, 4 December 1922, KP:UCL.

which was to include animal breeding experiments. In the basement were cloak and service rooms, plus large stores for the craniological and osteometric collections of the laboratory.[213] The Animal House and the Anthropometric Laboratory opened in 1924 and the latter was 'chiefly rendered possible by a gift from the Assistant Professor, Miss Ethel M. Elderton'.[214]

Pearson continued to attract students from many parts of the world througout the rest of his tenure. In 1915 he instituted a degree course in statistics at UCL.[215] By 1922 he had established two types of classes for British post-graduates: one consisted of teachers in education who sought higher degree as a means of advancing their position and the second, involved persons sent by Government departments or commericial firms to obtain an adequate statistical training. (Guinness sent William Sealy Gosset ['Student'] to UCL for two years and the Department of Scientific and Industrial Research also sent some of their co-workers.) Pearson found 'the work here with 12 to 14 post-graduate students mentally much more fatiguing than the teaching in the old days of 80 to 100 students of undergraduate standing'.[216]

In 1922 the University Reader in Medical Statistics, Dr. Major Greenwood, was transferred from the Lister Institute to the Biometric Laboratory to give 'courses on vital statistics and the history of epidemiology'.[217] By 1925 Most of his students were post-graduates; there were also 'a number of junior students working for the B.Sc. or M.Sc. in statistics'. Some of his students wished 'to obtain professional posts in which statistics will be of service (including Civil Service candidates)[218] Ethel Elderton became Assistant Professor in the Galton Eugenics Laboratory in 1925; Percy Stocks was the Medical Officer whilst Mary Noel Karn and J. Oscar Irwin became assistants.

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213 Karl Pearson, 'History of the Biometric and Galton Laboratory', Appendix 1, Report to the University of London, KP:UCL/247 (1903-1909), p.3

214 Karl Pearson, 'Report to the Court of the Worshipful Company of Drapers for the years 1922, 1923, and 1924', 29 February 1924, KP:UCL/233.

215 Egon Pearson, 'Karl Pearson', *Biometrika*, (1936), 206.

216 Karl Pearson, Report to the Court for 1922 to 1924, 29 February 1924.

217 *Ibid.*

218 Karl Pearson 'Report on the Galton and Biometric Laboratories with regard to Expenditure' 1925.

Some six years after Pearson was made the Galton Professor, some of the members of the College Committee suggested that the Galton endowment provided adequately for the Biometric Laboratory. Pearson maintained that this was 'very far from the case and [was] due to a misunderstanding of the relation of the two laboratories'.[219] In a report on the expenditures of the two laboratories, he explained that

> it is well known to the University Authorities that Galton established first a fellowship in the science of Eugenics and a Eugenics Record Office. With this I had practically nothing whatever to do. Galton supervised this office himself, and my only relation to it was that its members used to come in occasionally to the Biometric Laboratory for friendly consultation and advice.[220]

He further explained that when Galton offered the University of London £500 annually for the support of research in Eugenics, he thought that it was only possible to obtain his research workers from those trained in the Biometric Laboratory or in its statistical methods. Though Pearson appointed some biometricians to assist Galton, 'the staff were in no way under [his] control or direction'. The association of the two Laboratories arose, therefore, from the fact that 'the Drapers' Biometric Laboratory appeared to be the only school at that time producing the type of worker required for carrying on the eugenics research desired by Francis Galton.[221]

With the increasing costs of maintaining the laboratories and of publication, Pearson wrote to Sir Edwin Deller, Principal of the University of London, in the spring of 1924 seeking advice on funding. Deller suggested to Pearson that he write to the Rockefeller Foundation outlining a budget proposal for his laboratories. Pearson then wrote to Edwin Embree, the Secretary to the Rockefeller Foundation, that he

> cannot and does not pay the Staff adequately. We are always impecunious in our publishing funds. We have no funds for carrying on the museum, nor proper maintenance funds for library or animal house or field investigations of any kind.[222]

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219 Pearson, 'Report to the Drapers' Court', March 1918, p. 3

220 Karl Pearson, 'Report on the Galton and Biometric Laboratories especially with regard to their Expenditure', March 1925, KP:UCL/233, p.17.

221 *Ibid.*, p.1

222 Karl Pearson, Letter to Edwin Embree, (24 April 1924). Galton Laboratory Archives, Genetics 401A: 16/317, Rockefeller Archive Centre, North Tarrytown, New York.

Though there is no record of any response from Embree, a number of Pearson's former American students (Henry Ruger, Gaius E. Harmon and Raymond Pearl) wrote to the Foundation on his behalf.[223] Harmon wrote that

> American statistical science owes a great debt to Professor Pearson, for many of our statisticians have studied with him and thus obtained inspiration and knowledge of his methods in a most direct manner. [Moreover,] more of the post graduate workers at the Biometric laboratory come from America than any other country save England.[224]

Embree replied to Harmon that one of the laboratories to which the Foundation would 'would certainly give careful attention is that of Professor Pearson'. Two of their considerations were 'Pearson's age' (who was then 69 years old) ... and what [would] become of the institution after his death'.[225] The Rockefeller Foundation did not, however, begin to provide grants for University College London until 1934 (a year after Pearson's retirement).

In making his seventh and last appeal to the Worshipful Court of Drapers' for continuation of the grant 'that it has made to the Laboratory for the space of nearly thirty years', he wrote that

> my object during the past forty years has been to build up a Laboratory unique of its kind, a place where a novel calculus should be applied to problems concerning living forms. This purpose involved the development of a new form of mathematical analysis.... I am writing these pages fully aware that this may form my last Report on the work done here to the Court of the Drapers' Company, and I do so with the full sense of all that Company has done for thirty years to enable me to carry out the aim of my scientific life, the realisation of my dream of forty years ago.[226]

Pearson continued to receive funding from the Drapers' Company until his retirement in 1933.

By the time he had written his last Report to the Worshipful Company of Drapers', Pearson had finished writing the *Life* of Galton. A project that he thought

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223 Raymond Pearl, Letter to Karl Pearson, January 1925, RP:APS. Henry Ruger, Letter to Edwin Embree (20 August, 1926) and Gaius E. Harmon, Letter to Edwin Embree (16 September 1926), Galton Laboratory Archives (1924-1930), Genetics 401A:16/218, Rockefeller Archive Centre.

224 Gaius E. Harmon, Letter to Edwin Embree (16 September 1926), Galton Laboratory Archives: (1924-1930), Genetics 401A: 16/218, Rockefeller Archive Centre.

225 Edwin Embree, Letter to Gaius E. Harmon (20 September 1926), Galton Laboratory Archives: (1924-1930), Genetics 401A: 16/218, Rockefeller Archive Centre.

226 Karl Pearson, 'Report to the Court of the Worshipful Company of Drapers', KP:UCL/233, (1930), p.1

would be completed in 'six months' (when he wrote to Mrs. Weldon in January of 1912) took instead nearly 20 years to finish: the three volume biography was published in four parts from 1912 to 1930. Having just completed the writing of the biography, he wrote to Mrs. Weldon, in the summer of 1929, that his

> occupation this vacation on the last chapters of Galton's *Life* had brought me this week to 1906, and in the mass of letters between Galton and myself in the first six months of that year the one subject is W.F.R.W. They bring it all back so vividly, Longcott, the days at Oxford, the Weldon medal and the éloge, and those days at Darby! The one bright spot in all of the darkness of that time was that the common loss, the common feeling of the great tragedy for both of us -although we were witnessing it from different wings of the stage—brought Francis Galton and myself into firmer touch, into the close friendship, which remained to the end .... it was solely his advice and his help which carried me through, and helped me to echo his words 'who am I to judge'... It was the common stress on both our faiths which seemed to knit us closer together and this was the one solid good—all the rest was drear right and sorry ill—that came out of 1906. It is going through all those letters, that has brought facts long forgotten back to me, and made me hasten a letter which I had meant to write this vacation to you.[227]

Though the biography was indeed written as 'a token of his deep affection and admiration for Galton', it will be suggested that his involvement in Galton's *Life* may have helped to diminish the sense of loss following Weldon's death. When Pearson first began the biography in 1912 (six years after Weldon's death), he still wanted 'badly a staunch friend like R. W.'. The biography may have thus given Pearson a chance to focus his energies on Galton instead. In this respect, this 18 year project may be seen as a form of therapy for Pearson which provided the emotional catharsis for his sorrow. The quietude that Pearson seems to experience in 1929 may have been engenderd by the completion of the Galton biography in 1929. It will also be suggested that a biography of this proportion size may have helped to establish the link to Galton rather than Weldon in the historiography of Pearsonian statistics (particularly as the biography has been used extensively). In this respect it might be seen that Pearson unwittingly 'buried' Weldon's contributions under Galton's papers and correspondence. If this has contributed to the emphasis on Galton at the expense of Weldon, perhaps this may, in part, explain why Weldon's work and his influence on Pearson has not yet received the recognition he deserves.

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227 Karl Pearson, Letter to Mrs. Weldon, 13 August 1929, KP:UCL.

It has been shown that the Drapers' Biometric Laboratory and the Galton Eugenics Laboratory were underpinned by two separate methodologies which incorporated different quantitative procedures and utilised different tools for varying problems. There is no doubt that Pearson was interested in trying to make eugenics more scientific by trying to measure and analyse problems that arose in the Eugenics Laboratory, but he seems to have pursued these matters by creating a separate methodology in the Eugenics Laboratory. Moreover, it has been argued that Weldon's death not only affected Pearson's views on inheritance, but that this engendered a shift in the locus of problems pursued by researchers in the Biometric Laboratory to that of craniometry and physical anthropology. This loss also created an emotional burden for Francis Galton who relinquished control of the Eugenics Record Office six months after Weldon's death; Pearson then undertook the directorship of the Eugenics Laboratory. It has also been shown that publications emanating from his research in the Drapers' Biometric Laboratory massively outnumbered those arising from his work in the Galton Eugenics Laboratory. Thus, it would not seem that the mathematical statistical theory that Pearson devised in the Biometric Laboratory arose from his interests in eugenics.

Chapter VIII

THE FINAL YEARS

After working at University College London for nearly half a century and having established the first Department of Applied Statistics in the country, Karl Pearson retired in 1933 at the age of 76. As Emeritus Professor he was given a room in the Zoology Deparment which he used as the 'Office of Biometrika'.[1] He relinquished the editorship of the *Annals of Eugenics* in 1933, but continued to edit *Biometrika* up until his death in 1936.[2]

From 1933 to 1936, he published 34 articles and notes of which 23 were statistical (published mainly in *Biometrika*) and four were published in the *Annals of Eugenics*. Some of the papers he published included the following: his paper on the use of the cranial coordinatograph (discussed in Chapter VII); two more statistical tables (one for the 'Incomplete Beta Functions' and the other for the 'Complete and Incomplete Elliptic Integrals'); a short note on his original purpose and use of the chi-square goodness of fit test (discussed in Chapter VI) and a recollection of the 'Old Tripos Days at Cambridge for the *Mathematical Gazette*.[3]

He began his career at UCL in 1884 when, at the age of 27, he was appointed to the Goldsmid Chair of Applied Mathematics and Mechanics succeeding Olaus Henrici (a chair held previously by William Kingdom Clifford). Some eight years later, in November of 1892, he gave the first set of his statistical lectures at Gresham College where he occupied the Chair of Geometry from 1891 to 1894. Here, he introduced for the first time his standard deviation and the method of moments as a mathematical-

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1 This was the letterhead that appeared on Pearson's stationery from 1933 to 1936.

2 See Karl Pearson, 'Vale', *Annals of Eugenics*, 5 (1933), 416 on relinquishing the editorialship of the journal.

3 Karl Pearson, 'Incomplete Beta Functions', Issued by the *Biometrika* Office (1934); idem, 'Tables of the Complete and Incomplete Elliptic Integrals', issued by the *Biometrika* Office (1934); Karl Pearson, 'Old Tripos Days at Cambridge, as seen from another Viewpoint', *Mathematical Gazette*, 20 (1936), 27-36.

statistical system for curve fitting of symmetrical and asymmetrical distributions; this system was also used to determine an empirical criterion for a goodness of fit test. The method of moments was initially devised to help his closest friend and colleague, W.F.R. Weldon, with the analysis of his data on the carapace of female crabs at Naples. From Weldon's so-called 'double-humped curve' (i.e., a bimodal distribution), Pearson and Weldon thought this suggested that two separate species had arisen; hence, Pearson hoped to have been able to offer 'an exact measure of species [for sympatric speciation] or dimorphism'.[4] This work was followed subsequently by his first set of statistics lectures at UCL beginning, in October 1894, with the 'Theory and Practice of Statistics' for one hour a week for those 'desiring to study Animal Variation, to deal with Errors of Physical Observations or to become Actuaries'.[5]

The double-humped distribution that Weldon had found in the frontal breadth of the forehead of the female shore crab in the Bay of Naples in 1893, had not only indicated dimorphism to both Weldon and Pearson, but this also seemed to have implied that they had found a quantitative measure of speciation. This work, which led to Weldon's first project for the Evolution Committee of the Royal Society in 1894, led him to believe that he would find dimorphism in herring as well. After Weldon discovered that his measurements from his herring data did not indicate that all of the characters of the herring were dimorphic, neither he nor Pearson pursued any further work on speciation. Before Weldon had met Pearson, he had already begun to look for an empirical criterion for the identification of species in 1889 (which was termed, in Chapter III, the 'biometrical species criterion'). This project continued for a short time only until Pearson realised that 'correlation was just as much an outcome of natural selection'.[6] Weldon's most fruitful venture with Pearson came about when he demonstrated empirical evidence of natural selection in the carapace of *Carcinus*

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4 Karl Pearson, 'Conclusion of the Normal Curve' Gresham Lecture, KP:UCL, November 1892, p.7.
5 Anon, 'The Retirement of Karl Pearson', *U.C.L. Magazine* (Summer 1933), 166.
6 Pearson, 'MCTE. XI. On the influence of natural selection on the variability and correlation of inheritance', *Philosophical Transactions of the Royal Society*, A 200 (1902), 29.

mœnas at Plymouth Sound from 1894 to 1898, and then with *Clausilia laminata* (Montagu) in Oxford and in Hampstead Heath in 1902.

Weldon's work led to Pearson's synthetic statistical work on the development of a theory of frequency distributions in tandem with the use of the relative frequency approach to probability. In turn, this provided the framework for his most innovative work which involved finding an empirical measure of a goodness of fit test for asymmetrical and symmetrical distributions; this became his main pursuit throughout the 1890s. Pearson achieved his aim in 1900 when he devised the chi-square goodness of fit test (his single most important contribution to a modern theory of mathematical statistics).

His published papers (mainly in the *Philosophical Transactions of the Royal Society*) and his lectures at University College provided the impetus for the founding of the Biometric School in the early 1890s. He established the Drapers' Biometric Laboratory in 1903 (followed by a grant from the Worshipful Drapers' Company from whom he continued to receive support until his retirement).

Galton's law of ancestral heredity led to Pearson's development of multiple correlation and multiple regression; this new statistical procedure twinned with Weldon's interests in natural selection led to Pearson's introduction of determinantal matrix algebra in 1896 in an attempt to provide a synthetic mathematical model for evolutionary biology. This use of a higher form of algebra was perhaps the most important mathematical contribution to statistical theory at the end of the nineteenth century and was pivotal in the professionalisation of the emerging discipline of mathematical statistics.

From 1898 to 1906, Pearson and Weldon undertook a series of biometric investigations involving Pearson's homotyposis, Galton's law of ancestral heredity and Mendelian inheritance. Galton's law of ancestral heredity, which was underpinned by continuous variation, was by no means the exclusive nor dominant form of inheritance for Weldon and Pearson. They first considered discontinuous variation for problems of inheritance in 1898 and following Weldon's first paper on Mendel in 1902, they

considered Mendelism as an additional form of inheritance for discontinuous variation (as had been advocated by William Bateson). Hence, Pearson and Weldon thought that both continuous variation were to be considered for theories of inheritance which was also to have incorporated Darwinian natural selection. The historiography of the Biometrician-Mendelian debate has placed considerable attention on the role of continuous variation for the biometricians. It has not only been argued that Pearson and Weldon were interested in continuous variation only, but it has also been argued that Pearsonian statistics was built around the notion of continuous variation.[7] These historiographical views have, however, failed to address these two issues: (1) the statistical methods that Pearson devised for discontinuous variation, from 1899 to 1904, for such inherited characteristics as eye-colour and hair-colour (including Pearson's chi-square test of independence which he considered to have been the most appropriate method for Mendel's 'factors', and this remains the case today) and (2) Weldon's view, in particular, that natural selection was paramount to any theory of inheritance (which R.A. Fisher demonstrated in 1918 when he incorporated Mendelism and Darwinism for the new synthesis of evolutionary biology).

Whilst Pearson established the fundamental corpus of his statistical theory before the turn of the century, he seemed to have lost his most creative and productive impetus for the development of mathematical statistics after Weldon's death in 1906. Weldon's death was not only a heavy blow to science but it was to Pearson 'the greatest personal loss of his life'.[8]

The Galton Eugenics Laboratory was set up in 1907 following Galton's resignation from the Eugenics Record Office. Pearson's work in this laboratory led to the developement of family pedigrees and the use of actuarial death rates. J.B.S. Haldane was to regard Pearson's pedigrees as his most 'valuable contribution to

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7 See Reginald C. Punnett, 'Early days of Genetics', *Heredity*, 4 (April 1950), 1-10; William Provine, *The Origins of Theoretical Population Genetics* (Chicago University Press, 1971), see especially pp.55-64.

8 Egon Pearson, 'Karl Pearson: An Appreciation of Some Apsects of his Life and Work. Part 1', *Biometrika* (1936), 241.

biology'.[9] After the Galton Chair of Eugenics was established in 1911, Pearson was able 'to give up the undergraduate teaching of applied mathematics and devote [himself] solely to what had for many years been the main element of [his] researches, the advancement of the modern theory of statistics'.[10]

A new building for the Department of Applied Statistics had been built and was near completion by the summer of 1914. With the onset of the First War, the building was taken over in August and used as a military hospital. Nearly all of the work in the Drapers' Biometric Laboratory was delayed for more than five years. By 1918 Pearson had already thought, albeit portentously, that there was

> undoubtedly work enough for two professors—one to carry on the pure statistical work and biometry and the other the Galton Eugenics Laboratory. That indeed should be the goal aimed at, but it is an ideal of the distant future.[11]

Biometric activies did not resume completely until 1920 with the opening of the Bartlett Building on 4 June.

When Pearson was attempting to finish some of Weldon's work for publication in 1931, he wrote to Mrs. Weldon (after having looked back at all of his work on inheritance), 'even today I reproach myself for not having done more than I did or not more now to complete what could be completed. Perhaps it is now idle, for the Mendelians have long left Bateson behind them.[12] By the time Pearson wrote this letter, Bateson had been dead for less than a year. Pearson selected a paragraph from the recent book of R.A. Fisher, on *The Genetical Theory of Natural Selection*, to illustrate Bateson's position among Mendelians after his death:

> Unfortunately he (Bateson) was unprepared to recognise the mathematical or statistical aspects of biology, and from this and other causes he was not only incapable of framing an evolutionary theory himself, but entirely failed to see how Mendelism supplied the missing parts of the structure first erected by Darwin. His interpretation of Mendelian facts was from the first too exclusively coloured by his earlier belief in the discontinuous origin of specific forms. Though his influence on evolutionary theory was thus *chiefly retrogressive*, the mighty body of Mendelian researches throughout the world has evidently outgrown *the fallacies with which it*

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9 J.B.S. Haldane, 'Karl Pearson', *U.C.L. Magazine* (June 1936), 252.
10 Karl Pearson 'Report to the Drapers' Court', p.4
11 *Ibid.*
12 Pearson, Letter to Mrs. Weldon, KP:UCL, 15 April 1931, KP:UCL.

> *was fostered.* As a pioneer of genetics, he has done more than enough to expiate the *rash polemics of his early writings.* (Pearson's italics.)[13]

Six months before his retirement, he wrote to Mrs. Weldon again on the role of Bateson in her husband's work:

> I don't think we can attribute anything to Bateson's influence [on Weldon]. His reputation will remain as one, who boomed Mendelism in England not for what he contributed to it. Mendel himself would not recognise what is now called the 'science of genetics' from the biological side and most biologists are too little mathematical to grasp its methods! That is one of the evils of the day. There are only three men in this country who deal with it, Fisher, Haldane and Hogben, and probably only the mathematical knowledge of the first is really adequate.[14]

He then explained that a Committee had been drawing up new plans for the Laboratory to have a large biological element, but 'their idea of genetics is *not* statistical'. They wanted a geneticist whilst the 'statistics was to be separated off as a mere College Department. 'They know nothing of the years it has taken to build up the Institute to its present complex whole.'[15] A couple of days later, Pearson wrote to Raymond Pearl that he did not know

> what the future of the Biometric and Galton Laboratories [was] likely to be; my *advice* has not been asked, but the scheme appears to be to break it up into fragments, separate the statistics from the eugenics, and generally break up what has taken me a lifetime to build up! Our folk here do not understand the value of a *research* institute, and appear only to consider what can be used for undergraduate teaching. It was a crime to associate the Galton endowment with University College... I have not the least idea who will be appointed my successor as I am not on the Committee in charge of the reconstruction.[16]

After Major Greenwood learnt that Pearson sent in his letter of resignation during the first week of September 1932 'to take effect at the end of the session 1932-33', he wrote to Edwin Deller, the Principal of UCL, suggesting that the post be offered to Raymond Pearl.[17] Greenwood thought that Pearl would have been a good choice as there was more continuity between Pearl's and Pearson's work than between Pearson's and Fisher's.[18] Greenwood then wrote to Pearl that it was his 'personal

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13 *Ibid.*; R.A. Fisher, *Genetical Theory of Natural Selection* (Oxford University Press, 1930), pp.ix-x.

14 Pearson, Letter to Mrs. Weldon, 29 December 1932, KP: UCL.

15 *Ibid.*

16 Karl Pearson, Letter to Raymond Pearl, Pearl Papers, 31 December 1932, the American Philosophical Society Library. [Hereinafter referred to as RP:APS.].

17 Major Greenwood, Letter to Raymond Pearl, 7 September 1932, RP:APS.

18 *Ibid.*

desire' to have Pearl at UCL rather than 'R.A. Fisher or J.B.S. Haldane' both of whom he 'dislike[d]'.[19] Pearl replied that he was 'not prepared to say no... and [he could] not flatly say yes without knowing more precisely what [he was] agreeing to'.[20] In the the late spring of 1933, Pearson learnt that Fisher was to be his successor.[21] By the beginning of 1934, Pearson's son, Egon, 'had been made Reader in Statistics'.[22]

A couple of weeks before Pearson's official retirement from the Galton Chair, the secretary to Ramsay MacDonald wrote to Pearson:

> At the Prime Minister's request, it is his intention in respect of the forthcoming list of Birthday Honours to submit your name to the King with a recommendation that he may be graciously pleased to approve that the honour of knighthood be conferred upon you.[23]

Pearson refused the knighthood.[24] At the end of the year, Major Greenwood wrote to Pearson that the Royal Statistical Society wanted to honour him with the Guy Medal in their centenary year in 1934.[25] Pearson wrote to Greenwood, that it was 'certainly not want of gratitude or appreciation of the action of the Society's Council which leads my hesitation instead of a very hearty and thankful acceptance'.[26] He then went on to explain his reason for declining the honour:

> When I first started my statistical career about 1889, the field occupied by the society was very distasteful to my mind and the one man who had worked in what I considered statistics proper, had little influence in the Society, and incidentally oft regretted its lack of appreciation of new methods. Accordingly I stood aside, and it is only in comparatively recent years that a younger generation has changed the character of the Society. That change I have not taken part in and the society owes its changed outlook to its own younger members. To them the society owes its debt, not to me.
>
> Medals are a great encouragement to young men and lead them to feel their work is of value, I remember how keenly I felt this when in the nineties I received the Darwin Medal and the Huxley Medal. When one is old, one wants no encouragement and one goes on with one's work to the extent of one's power, because it has become

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19 *Ibid.*
20 Raymond Pearl, Letter to Major Greenwood, 29 October 1932, RP:APS.
21 Pearson, Letter to Mrs. Weldon, 26 May 1933, KP:UCL.
22 Major Greenwood, Letter to Raymond Pearl, 12 February 1934, RP:APS.
23 Secretary to Ramsay MacDonald, Letter to Karl Pearson (17 May 1933) Pearson papers 820/10.
24 The words 'knighthood refused' were written on the envelope of the letter informing Pearson of his knighthood. Karl Pearson, Letter to the Secretary to Ramsay MacDonald, 18 May 1933, KP:UCL/820/10.
25 The choice of the medal was mentioned in a letter Major Greenwood wrote to Raymond Pearl on 17 April 1935, RP:APS
26 Pearson, Letter to Major Greenwood, 8 December 1933, KP:UCL.

> habitual. I shall appreciate the action of the R[oyal] S[tatistical] S[ociety] in going outside their own limits and considering me, just as much as if I had received a medal... hesitation has not arisen from any past events, but largely because from the belief that [for] medals to be effective [they] must be given to young men.[27]

When Weldon decided to submit Pearson's name for the Darwin Medal some 35 years earlier, he wrote to Galton that he 'want[ed] to put Pearson's name forward for the Darwin Medal as T.H. Huxley said "the medal was founded for the encouragment of younger men" and no one seems more qualified than Pearson'.[28] After Galton wrote to Pearson to 'congratulate [him] heartily on the forthcoming award of the Darwin Medal' on 16 November 1898, Pearson replied that it

> was indeed a high and unexpected honour... Any mathematician could have done what I have done, a dozen of the better men far better -- especially if they had the suggestions from Weldon almost daily at lunch for four or five years. Hence the medal is not a recognition of *my* mathematics, but of the fact that the quantitative treatment of biological problems which you have initiated is to be in future one of the great instruments of research in matters of evolution.[29]

For Pearson, the significance of the Darwin Medal seems to not only have stemmed from his conviction that medals should be awarded to 'young men', but it almost seems that Weldon's nomination of Pearson also made the honour especially meaningful. Indeed, Pearson's refusal of the knighthood and the Guy Medal from the Royal Statistical Society in 1933, was not the first time he declined an honour. One of the first times seems to have been after the Weldon Memorial Medal, for work in Biometry, had been established shortly after Weldon's death. After he received the news from Oxford, in February 1912, that he was chosen to be the first recipient of the medal, he did not think that it was right to accept the Weldon Medal. He wrote to Mrs. Weldon (before replying to the Oxford Electors) that:

> I do fully appreciate your kindness and the desire of the Electors, but you know I knew your husband very closely & can still feel what he would think and say. The Darwin medal came to me when I was relatively young & it encouraged me as a young man & made me feel that medals and prizes might be helpful to young men, directing their energies and telling them they were appreciated. The R.S. usually gives its medals to old men, whose reputations are already made, it gives them momentary pleasure and saves the R.S. much trouble in selection, but from the standpoint of science the medals are idle.-- Now what I have written down is what

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27 *Ibid.*

28 Weldon, Letter to Francis Galton, 27 May 1898, FG:UCL.

29 Francis Galton, Letter to Karl Pearson, 16 November 1898, FG:UCL and Karl Pearson, Letter to Francis Galton, 16 November 1898, FG:UCL.

> W.F.R.W. would have said & how he acted when he prepared my name for the Darwin medal. The Weldon medal must go to encourage young men if it is to be fruitful to science.[30]

Some 23 years after Pearson was first nominated for the Weldon Medal, he wrote to Mrs. Weldon to let her know that his son 'Egon got the Weldon Memorial Prize Medal in 1935'.[31]

One of the few medals Pearson accepted in 'old age' was the Rudolf Virchow Medal 1932 as the 'only anthropologist [who was] not a German to have received this honour'.[32] After Pearson received this medal he wrote to Raymond Pearl that

> after surmounting the surprise I felt at the Germans giving me the honour, I felt great admiration at their courage in giving it to an ex-enemy. This especially so in the present state of excessive national feeling in their country.[33]

A retirement dinner was given to Pearson on 23 April 1934. The Vice-Chancellor, L.N.G. Filon (who was one of Pearson's oldest students and successor to the Goldsmid Chair of Applied Mathematics) presented Pearson with a 'Brunsviga calculating machine, a portrait plaque, a cheque and a book containing all the signatures of all subscribers.[34] Nearly 100 people attended including Alice Lee, James Tocher, David Heron, G. Udny Yule, William Palin Elderton and his sister Ethel.[35]

In the summer of 1935, he had received the wedding anouncement of Raymond Pearl's daughter Ruth, Pearson, who could not attend and was then 78 years old, wrote:

> I am fairly at the end of my tether now, but I have had a happy life and my chief regret is learning the laboratory I built up is in a sad condition. Professor Fisher as you know in charge of the old Galton Laboratory and my son, created recently Professor of Statistics, is in charge of the remainder. But a house divided against itself cannot stand! The two are of totally different natures. RAF's latest step is to get the palæntological collections, artefacts and casts of palæolithic man, turned out, apparently on the ground that they have no relation to human evolution! Still less to Eugenics. He has removed many of the mottos on the wall and some of the pictures, as well as breaking up the instrument room. But his chief aim seems to cast scorn on

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30 Pearson, Letter to Mrs. Weldon, KP:UCL, 2 February 1912.
31 Pearson, Letter to Mrs. Weldon, KP:UCL, 17 May 1935.
32 Anon, 'The Retirement of Karl Pearson', *U.C.L. Magazine* (Summer 1933), 167.
33 Karl Pearson, Letter to Raymond Pearl, 31 December 1932, RP:APS.
34 [Karl Pearson] *Speeches Delivered at a Dinner held in University College, London in honour of Professor Karl Pearson*, (Cambridge University Press, 1934).
35 Major Greenwood , Letter to Raymond Pearl, 25 April 1934, RP:APS.

> his predecessor and all who use any of his methods. It is a somewhat hopeless outlook for students, who wished to attend the lectures of either professor, still more so of both. A man may knock to pieces in a year what another took forty years to build up. *All this please in strict confidence.* K.P.[36]

By the end of the year Pearson had become quite frail; he wrote to Ethel Elderton that he had

> not been outdoors for three weeks, and fear that I shall not be able to go again regularly to College. My wife means a good deal to me, but I want her to get out and about more, otherwise life will be too wearisome. Don't think I am too depressed. I have had a very good life and I fully recognise that it cannot last forever. I don't want to end up with a year or even more of weakness and incapacity.
>
> Always affectionately yours, Karl Pearson.[37]

Nine days later, he wrote to his daughter Helga, that he had been recommended to go into 'UC Hospital private patient room for a thorough haul' to be kept under observation.[38] He stayed at home as he did not think he could 'get on without [his wife] Greta, her help or her reading to [him]'.[39] Helga regarded this time as the 'commencement of [her father's] last illness'.[40] By this time, Pearson's wife began to write most of his letters for him which Pearson usually dictated.[41]

In the spring of 1936 when Pearson was seventy-nine-years old, he was finishing what was to be his last statistical paper on a comparison of his Method of Moments to R.A. Fisher's Method of Maximum Likelihood.[42] He was to end his statistical career in the way that he had begun when he introduced the Method of Moments in his Gresham lecture in November of 1892. The paper was written primarily in response to Fisher's recent work on the *Method of Maximum Likelihood* as a means of curve fitting (and would be published two months after his death).

The foundations of Fisher's method had not only been built upon Pearson's statistical work, but it may be seen that his paper represented a translation of the statistical language that Pearson used in the late nineteenth and early twentieth

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36 Pearson, Letter to Raymond Pearl, 30 August 1935, RP:APS.
37 Pearson, Letter to Ethel Mary Elderton, 9 November 1935, KP:UCL.
38 Pearson, Letter to Helga Hacker (née Pearson), 18 November 1935, 916/1. KP:UCL.
39 *Ibid.*
40 Helga Hacker (née Pearson), Note attached to her father's letters to her. (No date), KP:UCL.
41 Pearson's wife specified on her husband's letters when they had been dictated by Pearson himself.
42 Karl Pearson, 'Method of Moments and the Method of Maximum Likelihood', *Biometrika*, 28 (published posthumously in June 1936), 34-59.

centuries into, what was to become, the vernacular of contemporary mathematical-statistical theory. Whilst much of Pearson's statistical methods and language would remain a part of contemporary statistical theory (and would be regarded later as the first stage in the development of a modern theory of statistics), Fisher's work was to become the second stage in the development and advancement of a modern theory of statistics when he determined the following: the exact sampling distribution for a statistic (e.g. the correlation coefficient in 1915); statistical methods for small samples derived, in part, from the work of William Sealy Gosset (i.e. 'Student') and the introduction of experimental design into statistical theory.[43] Fisher's paper was to become seminal in the development of *inferential statistics*. In this paper, he introduced criteria for consistency, efficiency and sufficiency.[44] Of Pearson's statistical system, Fisher wrote that the

> development by Pearson of a very extensive system of skew curves, the elaboration of a method of calculating their parameters and the preparation of the necessary tables, a body of work which has enormously extended the power of modern statistical practice, and which has been, by pertinacity and inspiration alike practically the work of a single man.[45]

Fisher then remarked that Pearson's 'introduction of an objective criterion of goodness of fit', was of even greater importance than his method of moments. Though, Fisher also thought 'that very little progress has been made in the study of the distribution of statistics derived from samples [since Pearson] gave the exact form of the distribution of χ^{2}'.[46] Whilst Gossett had introduced the t-distribution in 1908, Fisher wanted to extend this work.[47]

He went on to say that perhaps the 'most extensive use of the criterion of consistency has been developed by Pearson' and his method of moments was

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43 R.A. Fisher, 'Frequency distribution of the value of the correlation coefficient in samples from an indefinitely large population', *Biometrika*, 10 (1915), 507-21; Idem, *The Design of Experiments* (Oliver and Boyd: London, 1935).

44 Idem, 'On the Mathematical Foundations of Theoretical Statistics', *Philosophical Transactions of the Royal Society*, 221 (1922), *passim*.

45 *Ibid.*, 314.

46 *Ibid.*

47 William Sealy Gosset, ['Student'], 'The Probable Error of a Mean', *Biometrika*, 6 (1908), 1.

> without question of great utility, [whereby] different forms of frequency curves are fitted by calculating as many moments of the sample as there are parameters to be calculated..., but to discover whether this approximation is a good or a bad one, and to improve it, if necessary, a more adequate criterion is required.[48]

It was only after Fisher had criticised Pearson's method of moments as being 'inefficient' in 1934 that Pearson wrote the rejoinder.[49]

After Pearson read Fisher's paper and his criticisms, he was astonished to find out that Fisher's method depended on

> first working out the constants of the frequency curve by the Method of Moments, and then superposing on it, by what Fisher terms the 'Method of Maximum Likelihood'... The additional computing work after the determination by moments is as long as, I think longer than, the first fitting by moments. The practical value of the method therefore turns on the gain in accuracy obtained by the increased labour.[50]

The main point of Pearson's paper was to suggest to Fisher that he 'should state, now that the Method of Likelihood has taken hold of so many of the younger generation of mathematical-statisticians, wherein he conceives it to give "better" results than the method of minimum $\chi^{2'}$.[51] As Pearson thought that the latter had 'a firmer basis and [gave] the practical statistician what he requires, the "best" fitting curve according to $\chi^{2'}$. Pearson thus wanted to know 'what [did] the Maximum Likelihood give [Fisher]?.. He must define what he means by "best" before he can prove that the principle of Likelihood provides it'.[52]

One of the last letters he wrote by himself was to his daughter Helga on 23 April 1936. He died four days later in his eightieth year, at his summer cottage in the Old Schoolhouse in Coldharbour, Dorking, Surrey, 'dropping dead when going in the night to the water closet next [to the] bathroom' .. [and] the local G.P. put coronary thrombosis as cause of death'.[53] A memorial service was held at the Golders Green Crematorium on 30 April.[54] Pearson's wife, his son Egon and William Palin Elderton

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48 R.A. Fisher, 'On the Mathematical Foundations of Theoretical Statistics', *Philosophical Transactions of the Royal Society*, 222, A (1922), 321.
49 R.A.Fisher, Book review of Feldman's *Biomathematics*, *Annals of Eugenics*, 6 (1934), 252.
50 Pearson, 'Method of Moments and .. Likelihood', p.34-5.
51 *Ibid.*, p.47.
52 *Ibid.* For further details on developments associated with Fisher's maximum likelihood, see A.W.F. Edwards, 'History of Likelihood', *International Statistical Review*, 42 (1974), 9-15.
53 Helga Hacker, Note attatched to her father's letters to her. (No date), KP:UCL.
54 Anon, 'In Memoriam. Prof. Karl Pearson', *The Daily Telegraph*, (1 May 1936).

were named the executors of his estate. The total value of his personal goods in his house was £534/5s/6d; his personal assets were valued at £10, 484/15s/11d.[55] He left a further £36,701 to University College London.[56]

On 2 May, Major Greenwood wrote to Raymond Pearl: 'What a week. On Monday night I was rung up by *The Times* and told that K.P was dead and could I possibly do something *sofort.* I did my best.'[57] On Thursday, Greenwood was at the funeral at the Golders Green Crematorium. He mentioned to Pearl that of the older people whom he might have known were the Herons, the Eldertons, old James Bonar, Mrs. Florence Joy Weldon, James Fowler Tocher, William Bulloch [representing the Royal Society of Medicine], Raymond Wilson Chambers (English Professor at U.C.L. oldish). George Udny Yule and Leon Isserlis were not there.[58] Others who attended, not mentioned by Greenwood, included Bradford Hill, Joseph Oscar Irwin, Mary Noel Karn and Geoffrey Morant.

Pearl had learnt of Pearson's death in the *New York Times* which 'upset [him] a good deal for a variety of reasons'.[59] In the last ten years of Pearson's life, Pearl felt that he was 'really getting closer to the old man' as Pearson wrote Pearl 'long and intimate personal letters'.[60] He wanted to know what would become of Pearson's skull collection (which numbered 4,000). Greenwood replied to Pearl that

> R.A. Fisher, of course, thinks all K.P.'s craniometry pure bunk. While the old man was so bloody rude to the old fashioned anatomist that they are not showing much enthusiasm for Carlovgnian [Pearsonian] craniometry. I will do what I can, but it may not be easy to persuade the Medical Reseach Council that measuring skulls is something they should support.[61]

Sometime at the beginning of summer 1936, Greenwood and Pearl began to discuss who should do a biography of Pearson. Suggestions had been made that

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55 Anon, 'In the matter of the Estate of Professor Karl Pearson', *Hamstead and Highgate Express* (27 April 1936).

56 Anon, 'Scientist's Gift to the London University', *News Chronicle* (7 July 1936); Anon, 'Wills and Bequests', *Daily Telegraph* (7 July 1936).

57 Major Greenwood, Letter to Raymond Pearl, 2 May 1936, RP:APS.

58 *Ibid.* See also [Major Greenwood], 'Professor Karl Pearson. Eugenics and Statistics', *The Times*, (28 April 1936).

59 Raymond Pearl, Letter to Major Greenwood, 18 May 1936, RP:APS.

60 *Ibid.*

61 Major Greenwood, Letter to Raymond Pearl, 26 June 1936, RP:APS.

something could be done on 'the lines of Huxley by Leonary Huxley'.[62] Greenwood, who thought that Pearl would 'do K.P. admirably [and] nobody would do it better refused to do an official biography' as he did not want to be an official biographer of anybody.[63] He felt partly discouraged as it seemed to him that to do such a biography 'properly would take *years* of work'.[64] Both of them had agreed that Pearson's son, Egon, could write up the biography. Egon was to assemble a biography of his father, based largely on the two-part (138 page) obituary notice in *Biometrika* in 1936 and 1938; however, an official biography of Karl Pearson has not yet been written.[65]

Six months after Pearson's death, his old friend, Florence Joy Weldon, died. Though this was a friendship that emerged from the sorrow of Weldon's death, it lasted until the end of their lives. She left the residue of her estate for the founding of the Weldon Chair of Biometry in the University of London: J.B.S. Haldane became the first to occupy the chair.[66]

THE ASSESSMENT OF KARL PEARSON IN 1936

The emphasis given to Pearson's work in his obituaries is rather revealing. The popular press, in general, placed a greater emphasis on eugenics and Galton than on his statistical work whereas obituary notices in scientific, actuarial, mathematical, medical and engineering professional journals tended to place greater empahsis on his achievements as a statistician. In the *The Mancester Guardian*, for example, it was written that Pearson's 'early interest in biology was whetted by meeting Francis Galton' and his application of statistics to Galton's law of ancestral inheritance 'became accordingly, the lifework of Karl Pearson'.[67] *The Guardian* thought that

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62 *Ibid.*
63 *Ibid.*
64 *Ibid.*
65 Egon Pearson, *Karl Pearson*, Part I: *Biometrika*, 28 (December 1936) 193-247; Part II, 29 (February 1938), 161-248. Reprinted by Cambridge University Press, 1938.
66 Egon Pearson, 'Karl Pearson. Part II', 232.
67 Anon, 'Death of Dr. Karl Pearson: A Famous Eugenist', *The Manchester Guardian* (29 April 1936).

'under the influence of Francis Galton, [Pearson] almost invented and enormously developed his peculiar science of biometry'.[68]

The medical profession considered Pearson's influence to have been wholly beneficial. Thus, the *Lancet* wrote:

> Karl Pearson made the name of statistics respectable in medical circles. What [William] Farr did for official statistics, Pearson did on a larger scale.. no English man of science of the last fifty years has more profoundly influenced the thought of his time.[69]

The *British Medical Journal* thought that 'more, and more accuarate, use of statistical methods has been made by medical investigators, and the great majority of the methods were invented or improved by Karl Pearson'.[70] William Palin Elderton, who wrote the obituary notice for *Journal of the Institute of Actuaries*, thought that

> so far as modern actuarial work is concerned, it owes much to Karl Pearson... there is no member of our profession today who is not aware of Pearson's system of frequency curves, his method of fitting curves by the use of moments, his treatment of correlation and contingency and his χ^2 test.[71]

Whilst the *Journal of the Royal Statistical Society* acknowledged Pearson's contributions to modern statistical theory, they also suggested that the impetus to Pearson's work was derived exclusively from that of Galton.[72] Though he had never become a member of their society, it was to Pearson they owed

> not only the initial impulse to the whole development of statistical theory during the past forty years—and Pearson owes *his* impulse to Galton—but large and important contributions to the theory of frequency curves, the great bulk of the theory of correlation and many methods derived therefrom.. [including] important contributions also to the theory of sampling.[73]

Thus it may be seen that differing outlooks engendered different perceptions of Pearson's contributions to science since, at least, 1936.

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68 Anon, 'Professor Karl Pearson', *The Guardian* (1 May 1936). See also Anon, 'Professor Pearson's Great Work', *Sunday Times*, (4 May 1936) who wrote that 'his predominant interest [was]... establishing a practical science of eugenics [as his] ultimate goal' and the *News Chronicle*, which considered Professor Karl Pearson, the expert on eugenics and many other scientific subjects'. Anon, 'Scientist's Death', *News Chronicle*, 30 April 1936.

69 Anon, 'The death of a Grammarian', *Lancet* (2 May 1936), 43.

70 Anon, 'Karl Pearson', *British Medical Journal* (2 May 1936), 56.

71 William Palin Elderton, 'Memoirs: Professor Karl Pearson', *Journal of the Institute of Actuaries*, 68 (1936), 1.

72 Anon, 'Obituary. Professor Karl Pearson, F.R.S. 1857-1936.' *Journal of the Royal Statistical Society* (1936) 424-5 followed by G. Udny Yule's obit from *Nature*, 425-8.

73 *Ibid.*, 424-5.

It has been shown that when Pearson has been viewed as a 'disciple' of Galton, the historiographical tendency has been to emphasise Pearson's contributions to a statistical theory of *simple correlation* to the neglect of the greater part of the corpus of Pearsonian statistics; moreover, as Pearsonian correlation has been subsequently linked to his work in the Eugenics Laboratory, this has in turn led to the neglect of the methodology he devised in the Eugenics Laboratory. Whilst Galton's work and influence on Pearson deserves consideration in the development of Pearsonian statistics, to redress the balance in the historiography of Pearsonian statistics, consideration must also be given to all the other actors on Pearson's stage and especially to Weldon whom he placed at its 'centre'.

A complete assessment of the totality of Pearson's contributions to statistical theory should also include the earliest of his statistical work (not only the work from 1892-1895, but for all of the work in the 1890s as this is the time when he erected the foundations for a modern theory of statistics) rather than drawing primarily from twentieth century historiographical views of his work. As one of the last of Pearson's students, Helen Walker, remarked on the centenary of his birth:

> the contribution of a scientist should not be evaluated chiefly in terms of how much of his theory and method are still considered to be the best theory and method known at some later date but should be evaluated in terms of how far the field has advanced by his life and work.[74]

It may be seen that Pearson's statistical achievments not only provided continuity from the mathematical and statistical work that preceded him (including John Venn, Francis Galton, Adolphe Quetelet, Francis Ysidro Edgeworth) or occurred at the same time (such as George Udny Yule and W.F.R. Weldon), but his work also engendered the creation of a modern theory of statistics in the twentieth century which, in turn, provided the foundation for such statisticians as R.A. Fisher who went on to make further advancements for a modern theory of mathematical-statistics.

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74 Helen M. Walker, 'The Contributions of Karl Pearson', *American Statistical Association Journal*, 53 (1958), 12.

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