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Mathematics in the London/Royal Statistical Society 1834-1934

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Abstract

This paper considers the place of mathematical methods based on probability in the work of the London (later Royal) Statistical Society in the first century of its existence, 1834-1934. Regular mathematical contributions began appearing in 1883 and in the next half-century three movements can be distinguished, associated with major figures in the history of mathematical statistics–F. Y. Edgeworth, Karl Pearson and R. A. Fisher. The first two movements were based on the conviction that the use of mathematical methods could transform the way the Society did its traditional work in economic/social statistics while the third movement was associated with an enlargement in the scope of statistics. The Society recognised that enlarged scope in 1933 when it formed an Industrial and Agricultural Research Section: to promote these particular applications was to encourage mathematical methods.

Key names: Arthur Bowley, F. Y. Edgeworth, R. A. Fisher, Egon Pearson, Karl Pearson, Ernest Snow, John Wishart, G. Udny Yule.

Keywords: History of Statistics, Royal Statistical Society, mathematical methods.

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1 Introduction

The Statistical Society of London was founded in 1834 and became the Royal Statistical Society in 1887. Today mathematical methods based on probability theory are central to the Society's activities but in its first century they were not. Regular mathematical contributions only started to appear in 1883 and fifty years later they still only formed a minor part of the Society's published output. In 1933-4, however, there was a development that fixed the shape of things to come.

In the half-century after 1883 three movements can be identified, linked directly or indirectly to major figures in the development of mathematical statistics, F. Y. Edgeworth, Karl Pearson and R. A. Fisher. Edgeworth was in the Society for over forty years but Pearson never joined and Fisher was in and out–their influence was through their followers. The earlier movements rested on the conviction that mathematical methods could assist in the Society's traditional work but the third involved a change of direction. Economic and vital statistics were the Society's staples but in the 1920s a new kind of statistician appeared with new interests and in 1933-4 the Society responded by establishing an Industrial and Agricultural Research Section with its own organ, the *Supplement to the Journal of the Royal Statistical Society*. The new statisticians thought more naturally in terms of developing and applying a body of mathematical principles than had the old economic/social statisticians and to encourage them was, in fact, to encourage mathematics. In the Society's post-war reorganisation, the Industrial and Agricultural Research Section became the Research Section and the *Supplement* became *Series B* (*Statistical Methodology*). Section and journal are both still active.

The Statistical Society likes recalling its own story: it marks important anniversaries and, on a more continuous basis, it records the deaths of its Fellows. The changing position of mathematics was recognised at the centenary by Bonar and Macrosty (1934) and at the sesquicentenary by Hill (1984) and, in preparing what is almost a collective biography of the Society's 'mathematicians', the obituaries have been most useful; they are listed by Elliott and Farebrother (1994) and Farebrother and Neal (2006). For my biographees I supply dates and a reference to the obituary on the pattern of **Francis Ysidro Edgeworth** (1845-1926; Price (1926)). I have also tried to integrate these local materials with contributions from the general literature of the history of mathematical statistics.

I begin with the mathematically almost void first half-century and then take the more mathematical half-century movement by movement. The movements actually overlapped: thus Fisher's first recorded appearance was at a meeting in 1921 at which one of Pearson's old students (Yule) spoke and Edgeworth proposed the vote of thanks. I conclude with some comparisons–between the Society then and later and between its experience and that of some of its foreign counterparts.

2 Statistics 1834-83

The object of the Statistical Society was to promote statistics by accumulating knowledge of a special kind and getting that knowledge used. My interest is in the first activity and on the scientific meetings and publications it entailed. At meetings–until the 1920s always in London–a paper was presented and discussed. The proceedings appeared in the *Journal* (which began publication in 1837) alongside "miscellaneous" items ranging from news items to full articles. All members of the Society–called "Fellows"–received the *Journal*, some attended meetings and some spoke and contributed papers; inevitably I will be following the activities of these most visible few. Non-fellows could contribute to the *Journal* but they did so only a very occasional basis; the story of mathematics in the Society is that of mathematicians in the Society. The Society had a large governing Council but most of the business, including the production of the *Journal*, was in the hands of a few Honorary Secretaries. Some Fellows were academics but it was not a society of academics and the leading statisticians were as likely to be journalists, bankers or civil servants. There were no courses in statistics before the 1880s and the typical English statistician of the nineteenth century was a self-taught enthusiast. For more on the Society in our period see Bonar and Macrosty (1934) and Rosenbaum (1984) and for international comparisons Westergaard (1932) and Desrosières (1998).

The Society's original project was "the collection and comparison of Facts which illustrate the condition of mankind, and tend to develop the principles by which the progress of society is determined." (Anon., 1838, p. 1). This had an arithmetical dimension because "The Statist commonly prefers to employ figures and tabular exhibitions, because facts, particularly when they exist in large numbers, are most briefly and clearly stated in such forms." (Anon, 1838, p. 1.) The objectives of the founders and how they were realised have been discussed by Bonar and Macrosty (1934), Cullen (1975), Hilts (1977) and Rosenbaum (1984) among others; Bonar and Macrosty (pp. 56-63, 118-13) analyse the contents of the *Journal*.

Probability was not part of the original statistical project although some of the founding generation of the Society and its older sibling, the Statistics Section of the British Association for the Advancement of Science, were familiar with the subject–vide Quetelet, Drinkwater and Lubbock, Whewell and Babbage. However the statisticians found no use for probability methods and these were seldom even mentioned in the *Journal*. Hill (1984, p. 134) finds a discussion by the physician **William Augustus Guy** (1810-1885; Anon. (1885)) who (1850, p. 43) judged that the "formulae of the mathematicians have a very limited application to the results of observation." The probability specialists–mathematicians and astronomers–and the statisticians seemed not to talk.

In speaking of *mathematics*, I am referring to mathematical statistics and applications of mathematical statistics. By modern standards the mathematics was light and an applications paper did not necessarily have any mathematical formulae, e.g. Jevons's (1869) application of the probable error to prices has none and Hooker's (1907) application of correlation to crops and the weather has only a few; the point is that such applications rested on a knowledge of mathematical theory. There was another kind of mathematics in the Society's purview, the mathematics of life contingencies, a subject often coupled with probability, as in Lubbock and Drinkwater's *On Probability* (1830) and de Morgan's *Essay on Probabilities and on their Application to Life Contingencies* (1837). From 1848 this form of mathematics had a home in the Institute of Actuaries but the social science of demography was not consolidated in Britain until after the Second World War; in our half-century the *Journal* published more papers on life tables than on mathematical statistics; Schweber (2006) considers the treatment of vital statistics in the first decades of the Statistical Society and Grebenik (1997) sketches the British demographic scene in the early twentieth century. Actuarial techniques were essential for the vital statisticians but the economists had no use for them and there was never a movement to make the Society one for the social application of actuarial methods.

Vital statistics was a more advanced subject than economic statistics, or so it seemed to the banker and economic statistician **William Newmarch** (1820-1882; Anon. (1882a)). In his presidential address Newmarch (1869, p. 373) extolled the virtues of averages based on many instances, affirming that

> What has been done in Vital Statistics, will, in progress of time, be achieved in other branches of inquiry. But there is a preliminary stage to go through, and that is the improvement of methods and notation.

The second sentence referred to his hope that more would be done on the "Mathematics and logic of Statistics." This was no. 18 in a list of 18 fields "which in this country require most urgent attention" (pp. 365-6) and here Newmarch had no progress to report. Yet another kind of mathematics, probability as pure mathematics, only came into view in the Society in the 1930s and no Fellows were involved until after the Second World War. Pre-war attitudes to this kind of mathematics are reported in Aldrich (2009b).

One Fellow on one occasion used probability to solve a problem in the pages of the *Journal*–**William Stanley Jevons** (1835-1882; Anon (1882b)). Jevons was an authority in political economy and logic and he had been taught mathematics by Augustus de Morgan (1806-1871), the leading British probability author of the time. In "The depreciation of gold," Jevons (1869, p. 448) proposed using probability to draw conclusions from price statistics:

> It has been abundantly shown by M. Quetelet and others, that many subjects of this nature are so hopelessly intricate, that we can only attack them by the use of averages, and by trusting to probabilities.

So, using the standard methods of the theory of errors, he could conclude, "*it is as likely as not the true alteration of gold lies within* $2\frac{1}{2}$ *per cent. of 16 per cent.*" The arithmetic of prices was already a Society topic–largely through Jevons's earlier efforts–but now he was proposing that the science of means be applied. Although this short piece was Jevons's only work in this direction, it inspired a stream of contributions from Edgeworth, what became known as the "stochastic" approach to index numbers; see Aldrich (1992). Jevons joined the Society in 1866 but was only fully involved for the few years that he was in London and in good health. Nevertheless Bonar and Macrosty (1934, p. 115) write, "No other economist so distinguished was so closely connected with the Society." Jevons's statistical work is surveyed by Stigler (1982) and Aldrich (1987).

Francis Galton (1822-1911; Yule (1911b)) is usually given a central place in accounts of the development of statistical theory and methods in nineteenth century Britain; see e.g. MacKenzie (1981), Stigler (1986) and Porter (1986). Galton was certainly in the Society–a Fellow from 1860, a Council member 1869-79 and Vice-President in 1875–but he rarely took part in meetings or published in the *Journal* and his work was rarely noticed. Galton's main effort in–what *we* think of as–statistics was bound up with the study of heredity and a study belonging to anthropology or biology was no business of the Society. Galton began his investigations into heredity in the 1860s but it was only in the '90s that the techniques he devised made any impression on the statisticians; see Sections 3 and 4 below. The paradox of the statistician who was not at home with the statisticians runs through Yule's (1911b) obituary in the *Journal*; 50 years later an even grander obituary recorded another life largely outside the Society–Ronald Fisher's. In MacKenzie's *Statistics in Britain 1865-1930* the Society appears only as a weak counterpoint to the main Galton-Pearson-Fisher theme.

In 1877 Galton proposed that the British Association's Section F (Economic Science and Statistics) be discontinued on the ground that the contributions were not scientific enough; his "Considerations" with replies by **William Farr** (1807-1883; Anon (1883)) and the Society's Secretaries were reproduced in the *Journal*–see Anon (1877). Galton did not mention the Statistical Society but much of his criticism applied equally to its work. Galton and Jevons were not typical Fellows: they were scientists at large and this is reflected in what they read and what influenced them. The economist Fellows read the *Economist* and the *Statist* (founded by **Robert Giffen** (1837-1910, Bateman (1910)) a prominent figure in the Society) and the vital statisticians read the *Lancet* and the *British Medical Journal* while Galton and Jevons were equally at home–as readers and contributors–in the science weekly *Nature.*

3 Edgeworth and the economist-mathematicians

Francis Ysidro Edgeworth (1845-1926; Price (1926)) was the Society's first, most constant and most prolific writer on mathematical statistics, contributing around 50 papers over a period of more than 40 years. I will be focussing on how Edgeworth presented his ideas to the Society and how it responded; the ideas themselves are examined by Stigler (1978, 1986, ch. 9), Porter (1986, pp. 253-69) and Aldrich (1992). Edgeworth has a key role in Stigler's (1986, Part III) account of the "English breakthrough" of the late nineteenth century but the important interactions between Edgeworth, Galton and Karl Pearson took place outside the Society and the most important publications did not appear in the *Journal.*

Jevons had died the year before Edgeworth joined the Society and Edgeworth was in some respects his heir. Both were interested in mathematical economics and Edgeworth carried on some of Jevons's statistical projects. For Bowley (1934, p. 113) they were pioneers of econometrics, though with different strengths: "For actual measurement [Edgeworth] would give place to Jevons, for the theory of measurement to no economist." Having become interested in statistics, Edgeworth quickly mastered the mathematical literature and became equally familiar with the latest work of Lexis and Galton and with the older works of Laplace, Gauss and Quetelet. While Jevons had only a student's knowledge, Edgeworth could work at the level as the masters. In the Society Edgeworth quickly made himself useful by serving on the Council in 1885-6 and contributing to the Jubilee celebrations in '85. Edgeworth's (1883) first piece in the *Journal* was a *Miscellany* note on the Jevonian topic of the value of gold and in the course of the decade he established himself as *the* authority on the theory of index numbers; see Aldrich (1992). He achieved this position through his work as secretary to the British Association committee on the "value of the monetary standard." Expertise on the subject was concentrated in the Society and the BA committee consisted entirely of Fellows. None had Edgeworth's taste for theory but they could see that he was theorising about something worthwhile and that he did it very well.

Compared with older members of the Society, Edgeworth had an enlarged conception of "statistics." Early in his statistical career he (1885a, p. 363; 1885b, pp. 181-2) considered three definitions:

> the arithmetical portion of social science,..., the science of Means in general (including means of physical observations)..., the science of those Means which relate to social phenomena.

Developing “the arithmetical portion of social science” was Edgeworth’s gloss on the Society’s chief object from the 1830s through to the 1930s. His own interest, however, was in the science of means, or mathematical statistics (a later term), and he worked on both the pure theory and on applications to social phenomena. In the 1880s he was alone in the Society in emphasising the mathematical method and the importance of applying it to social phenomena. By the 1930s more Fellows agreed about the centrality of mathematical statistics although very few worked on social applications.

Edgeworth’s Jubilee piece, “Methods of statistics” (1885b), set out to show how the Society’s work would be advanced by the use of significance tests and the examples were from Jevons’s study of commercial fluctuations, Guy’s studies of mortality and Neison’s study of the unhealthiness of drinking. In Edgeworth’s (1885a, p. 363) scheme for the science of means significance tests came under (1):

> The science of Means may be summed up in two problems; (1) To find how far the difference between any proposed Means... is accidental, or indicative of a law; (2) to find what is the best Mean.

The Jubilee examples showed the application of (1) to social phenomena, while the search for the best index number was a case of (2) applied to price statistics.

Of all the papers Edgeworth published under the Society’s auspices, the Jubilee paper claimed most for the science of means. The effort produced no response and–with no great exaggeration–that would be the story of Edgeworth’s forty years in the Society. One kind of effort Edgeworth did *not* make: he did not use the important positions he held in British economics–in 1891 he became professor of political economy at Oxford and founding editor of the *Economic Journal*. Edgeworth did not have the temperament to create a school–as Price’s (1926) account of his friend makes clear–but he could also be very diffident in his claims for social applications of the science of means: thus reviewing John Neville Keynes on method in economics, he (1891, p. 422) wrote, “[The] points in social statistics, where the mathematical method is applicable, are comparatively few. For it is generally better to attain certainty by augmenting observations, rather than by a nice use of the theory of errors to extract the utmost degree of probable evidence which may be afforded by a limited number of observations.”

The *Journal* published Edgeworth’s work on the applied science of means but his main work was in the general science and he sent those papers went to the *Philosophical Magazine*, a leading physical science and applied mathematics journal. In the ‘80s the *Philosophical Magazine* published Edgeworth on the theory of errors and in the ‘90s on the theory of correlation. Edgeworth’s contribution to correlation, described by Stigler (1986, pp. 315ff), was to its theory and his applications papers in the *Journal* were curious rather than compelling. Later in the decade Edgeworth altered his publishing strategy and made the *Journal* the main outlet for his theoretical work and thus for all his statistical work: the first of the new papers was "On the representation of statistics by mathematical formulae" (1898). The change was possible because Edgeworth now had company. In the early '90s the *Journal* published articles by Venn (1891) and Pearson (1893)–related to Edgeworth's work but less focussed on Society concerns–and it had seemed possible that the invisible college of mathematical statisticians would fix itself in the Society. However Venn's interest was waning and Pearson's eyes were elsewhere. Others came later in the decade and settled. The most important was Yule, whose entry inaugurated the Pearsonian movement described in the next Section, but there were others, economists like Edgeworth, able to contribute to the science of means or, at least, provide an audience for it.

The economists were pupils of **Alfred Marshall** (1848-1924, Bonar (1925)) professor of political economy at Cambridge from 1885. Marshall had joined the Society in 1880 and contributed a piece on the graphical method to the Jubilee volume but in later years his involvement was limited to attending the occasional meeting. There had been professors of political economy in England before Marshall but he gave economics–his preferred term–its modern form as a university subject. Another modernising move was the founding of the *Economic Journal* in 1891; for this see Hey and Winch (1990). The *Economic Journal* came to serve a different constituency–academic economists–from the *Statistical Journal*, and a division of labour developed with the old journal taking numerical economics; one sign of amiable coexistence was that Edgeworth, the most prolific contributor to the old journal, edited the new. There was a mathematical element in Marshall's theory of economics and some of his best pupils had studied mathematics before taking up economics. Marshall's pupils, **Alfred William Flux** (1867-1942; Leake (1942)), **Arthur Lyon Bowley (**1869-1957; Allen and George (1957)**), Charles Percy Sanger** (1872-1930; Hawtrey (1930)) and **John Maynard Keynes** (1880-1946; Hawtrey (1946)), were more active than in the Society than he ever was. Flux was the best undergraduate mathematician–Second Wrangler in 1887–but he became an economic statistician with little interest in mathematical statistics. Keynes and Sanger were more interested–one of Keynes's subjects was the logic of statistics–but they had other careers and did not publish much in the *Journal*. They stayed in touch with the Society, however, and later served on the Council; their statistical activities are described in Aldrich (2008a).

Flux and Bowley became great figures in the Society–important contributors to the *Journal* and eventual Presidents. Flux's greatest moment in the Society's mathematical turn was probably his precipitating Fisher's departure in 1922–see below–but Bowley was more positively engaged. Bowley was becoming established as an economic statistician when he joined the Society in 1894. The following year he was appointed to a part-time lectureship in Statistics at the new London School of Economics and he sought Edgeworth's advice on the subject, becoming Edgeworth's follower in index numbers and mathematical statistics. On one occasion they collaborated–Edgeworth and Bowley (1902)–with Edgeworth supplying the theory and Bowley the application to wage statistics. Although Bowley was an enthusiast for mathematics, economic statistics remained his chief occupation and only a few of his many papers in the *Journal* were mathematical. His main service to Edgeworth's theory was through his textbook, *Elements of Statistics* (first edition, 1901). This has a long Part I on preparing data and calculating descriptive statistics and a short Part II on "applications of the theory of probability to statistics" where Bowley tried to reinforce the message of Edgeworth's Jubilee piece and persuade statisticians to use significance tests. Bowley's old teacher–Marshall–reacted to "this great and glorious book" by advising its author "to leave mathematics for a little on one side"–see Whitaker (1996, vol. 2, p. 300)–and Bowley's main work in theory only came much later in the expanded fourth edition of the *Elements* (1920) and in his memorandum on sample survey theory for the International Statistical Institute (1926); for these and for Bowley generally see Aldrich (2008b).

After around twenty years of contributing *Miscellany* pieces and discussing the work of others Edgeworth presented a paper at an ordinary meeting. His "Generalised law of error" (1906) was the first theoretical paper to be presented and the discussants were Bowley, Flux, Yule and the meteorologist W. N. Shaw. In 1912 Edgeworth was President of the Society and the subject of his address was "On the use of the theory of probabilities in statistics relating to society." In the Society the Oxford professor was highly esteemed and quite isolated for only Bowley followed what he was doing; Bonar and Macrosty (1934, pp. 23-9) recall his singular position and how the Society honoured it by sponsoring Bowley's (1928) attempt to explain what Edgeworth had been saying for all those years.

In 1896 the Society's President the banker and economic statistician **John Biddulph. Martin** (1841-1897; Anon (1897)) surveyed statistical education across the world. In Britain he (1896, p. 612) found only the Newmarch lectures (financed by the Society) and Bowley's lectures at the LSE. Edgeworth and Bowley were among the Newmarch lecturers but these were mainly more traditional statisticians. Expansion at the LSE was slow–Bowley's position became full-time in 1917 and he only acquired statistical colleagues in the 1920s: **Edmund Cecil Rhodes** (1892-1964; Grebenik (1965)) arrived in 1924 and **Roy George Douglas Allen** (1906-1983; Grebenik (1984)) in 1928. Rhodes was trained by Pearson and his first contributions to the *Journal* drew on Edgeworth's work but he later moved from theory; Allen's first interest was in mathematical economics–an interest in common with Bowley and Edgeworth–and he did not publish in the *Journal* until 1940. Bowley's students did not become mathematical statisticians although some became prominent in the Society, including **Josiah Stamp** (1880-1941; Bowley (1941)) the economic statistician, **Ronald George** (1902-67; Benjamin and Douglas (1970)), the government statistician, and **Lewis Connor** (1886-1965; Morell (1965)) who worked with Bowley on a theoretical paper (Bowley and Connor (1923)) and then went into industry. Connor re-appears below in Section 6.in connection with the formation of the Industrial and Agricultural Research Section.

The first attempt to establish mathematics in the Society was Edgeworth's work with some, mostly moral, support from Bowley and a few fellow travellers. The Society found a place for him and he went on presenting mathematical statistics until his death in 1926; Bowley continued to preach, if not to practise so much, into the 30s. Edgeworth did not produce an influential body of work or bring students with him but he established precedents by opening the Society to his conceptions of statistics as "the science of Means in general" and "the science of those Means which relate to social phenomena." The second movement began in 1895 when **George Udny Yule** (1871-1951; Kendall (1952)) joined the Society. For several decades the Society's mathematical statisticians came from Karl Pearson's department and Yule was the first and most impressive of them.

4 Yule and the students of Karl Pearson

In 1936 the President of the Society Major Greenwood (1936, p. 674) departed from the subject of his address to speak on the recent death of Karl Pearson.

> I saw him for the first time in 1902, and since then there can hardly have been a day in which some thought of Karl Pearson has not passed through my mind, and there have been long periods when what he did, advised or suggested was a dominant motive.

The influence of **Karl Pearson** (1857-1936; Greenwood and Yule (1936)) on other Fellows was seldom so total but it was often considerable. The foundations for this influence were laid in 1892 when Pearson, professor of applied mathematics at University College London, began working with Raphael Weldon, the professor of zoology, on what they would call biometry. There are biographies of Pearson by E. S. Pearson (1936/8) and Porter (2004) and bibliography on the man and his place in modern statistics see Aldrich (2001/10).

Tippett (1972, p. 560) commented on Pearson's relationship with the Society,

> He was not a Fellow and did not associate with the Society, although it is known that he urged several of the men whom he helped to launch on their statistical careers (e.g. Udny Yule, David Heron and E. C. Snow) to enter the Society in the hope of "reforming" it.

Whether these three and their successors–Major Greenwood, Leon Isserlis, E. C. Rhodes, H. E. Soper, Ethel Newbold, Oscar Irwin, John Wishart, E. S. Pearson, E. C. Fieller and F. N. David–reformed the Society in the way Pearson desired, they produced nearly all of the mathematical statistics, pure and applied, that was not contributed by Edgeworth.

As well as a research programme and a course of instruction, there was journal, *Biometrika*, which served as a text from which outsiders like the medical statistician **John Brownlee** (1868-1927; Greenwood (1927)) and **Ronald Aylmer Fisher** (1890-1962; Irwin, Barnard, Mather, Yates, & Healy (1963)) could teach themselves; its history is sketched by Cox (2001). *Biometrika* was founded in 1901 by Pearson, Weldon and Galton for "the statistical study of biological problems" and the new biometry overlapped with the old statistics when the 'biological' was human and the "statistical" theoretical: the *Journal* and *Biometrika* both took articles on vital and medical statistics and where one published Edgeworth's version of statistical theory, the other published Pearson's. Otherwise the journals, like the institutions behind them, were very different. Not all the papers in *Biometrika* were written or re-written by Pearson or even came from University College, yet the journal–certainly after Weldon's death–expressed one vision and one will. The *Journal* and the Society had no single line and would gain from the permanent exclusion of Yule and Fisher from the *Biometrika* community.

Edgeworth was Pearson's guide to the statistical literature–see Stigler (1986, p. 328)–and among the subjects they discussed was the use of skew frequency curves; this probably explains why Pearson's essay on the subject–the first of his "Contributions to the mathematical theory of evolution" (1893)–appeared in the *Journal* as well as in the Royal Society's *Proceedings*. The *Journal* reproduced two further "contributions" by Pearson as well as a few original papers but they were minor pieces and Pearson's influence on the Society was through his students.

Research training at University College began small: Pearson's first class on statistical theory–in 1894-5–had two students, Yule and Alice Lee. Lee did *not* join the Society and her successors included Ethel Elderton, Julia Bell and G. M. Morant. A year after joining Yule (1896) was showing the Society how to apply the methods of biometry–the Pearson curves–to a problem of interest to the statisticians–pauperism. There was an impressive line-up of discussants. Galton spoke first and he used the occasion to speak on modern mathematical methods, beginning (1896, p. 350) by informing statisticians of their "large debt of gratitude to University College for the variety of statistical investigations carried on there, both mathematical and experimental." Also contributing were W. F. Sheppard (1863–1936), a mathematician Galton had recruited to help with his own investigations, and **Charles Stewart Loch** (1849-1923; E. P. C. (1923)) Secretary of the Charity Organisation Society an authority on pauperism; for a recent discussion of Loch see Denis and Docherty (2007). **Sir Rawson W. Rawson** (1812-1899; Anon (1899)) had been an Honorary Secretary in 1836-43 and his remarks (1896, p. 335) recalled the original conception of the statistician's task:

> the Society and statistical science generally were greatly indebted to Mr. Yule for showing how valuable and important, under careful management, mathematical processes were in testing and correcting the masses of statistical information which it is the province of the Society to collect and apply.

There was some follow-up to Yule's initiative–Galton contributed a note applying the method of percentiles to Yule's data and Pearson allowed two more of his "contributions" to be reproduced from the Royal Society *Proceedings* but these efforts petered out and there was no spontaneous movement from inside the Society.

Correlation was one of the statistical investigations being pursued at University College. Pearson sent his correlation papers to the Royal Society journals but Yule dispersed his, publishing in the *Economic Journal* and the *Statistical Journal* as well. Yule's correlation papers in the *Statistical Journal* were perhaps the first really important papers on statistical methodology to appear there; "On the theory of correlation" (1897) was one of the *Journal's* early theory publication and "Investigation into the causes of changes in pauperism in England" (1899) provided Stigler with the high note on which to end his *History* (1986, pp. 345ff). Again Yule was marrying a biometric technique to a statistical application but this time the strategy succeeded and he had a convert, **Reginald Hawthorn Hooker** (1867-1944; Yule (1944)), who was working on demographic and economic problems. Correlation was more appreciated elsewhere: by 1909 Yule had enough on "applications of the method of correlation to social and economic statistics" to justify a survey but all the items in the *Journal* were by him, by Hooker or both together. There was foreign work on economic correlation and this would be noticed in the *Journal's* book review pages but it would not be imitated in the main part; see Aldrich (2010) for discussion and references on the development of economic correlation.

Yule's methods did not sweep the Society but its interests were his interests and he stayed. The Society became more important to him after 1906 when he separated from Pearson, having criticised the master's treatment of association in the Royal Society's *Proceedings*. Yule (1936, p. 101) later reflected on such separations: "Those who left him and began to think for themselves were apt, as happened painfully in more instances than one, to find that after a divergence of opinion the maintenance of friendly relations became difficult, after express criticism impossible." There were differences too on Mendelism and Yule did not publish again in *Biometrika* until after Pearson's death; the affair is described by MacKenzie (1981, pp. 161ff). From 1902 until 1909 Yule was the Newmarch lecturer and his lectures formed the basis of his successful *Introduction to the Theory of Statistics*. This was a more theoretical work than the early editions of Bowley's *Elements* and, with medical and genetical examples, not exclusively focussed on economic and vital statistics. In 1912 Yule was appointed lecturer in statistics in the Cambridge University School of Agriculture. The creation of the lectureship reflected changes in British agricultural science–these are reviewed in Russell (1966, ch. XIII-IX)–but the Society was not involved and Yule never brought his work on genetics and agricultural experiments home. For Yule, the Society *was* home or at least his club: in 1907 he began a twelve year stint as an Honorary Secretary, in 1924-6 he was President and he went on contributing to the *Journal* until ill health forced him to retire in 1930; see his reminiscences in Yule (1934). Yule seems very much the modern statistician: Edgeworth and Bowley, Pearson and Fisher had careers in economics or genetics but all Yule had was a way of thinking and a set of techniques to be deployed on any problem that interested him.

Three more Pearsonians joined the Society before 1914, **David Heron** (1881-1969; E. S. Pearson (1970)) in 1906, **Major Greenwood** (1880-1949; Hill and Butler (1949)) in 1909 and **Ernest Snow** (1886-1959; White (1960)) in 1910, but, unlike Yule, they came without fanfare. There had been changes that led Pearson to put more emphasis on the human/social/statistical side of his work: in 1906 he took over Galton's Eugenics Record Office, which became the Francis Galton Laboratory for National Eugenics, and Raphael Weldon, his zoologist partner, died; Magnello (1999) reviews the range of Pearson's activities. Heron, Greenwood and Snow had distinctly human interests and the Society was a natural destination. Yet it was not a particularly comfortable one as some Fellows, notably Yule and Keynes, handled social science productions from University College very roughly. Keynes's (1910) hostile review of a publication from Pearson's laboratory is well known–see Stigler (1999) and Aldrich (2007)–but Yule was a more persistent critic: Snow's piece in *Biometrika*, "Biometric workers and statistical reviewers" (1912), was a reply to criticism from Yule. In 1912, when Yule addressed the Society on the contested topic of "methods of measuring association between two attributes", Edgeworth, Sanger and Hooker spoke for him while the newcomers, Greenwood and Snow, spoke for Pearson. Pearson and Heron (1913, p. 159) opined that "if Mr Yule's views are accepted, irreparable damage will be done to the growth of modern statistical theory." Nevertheless, the Pearsonians appear to have settled into the Society and, as MacKenzie (1981, pp. 176-7) reports, Greenwood even went over to Yule's side.

Heron, Greenwood and Snow were all additions on the mathematical side of the Society but Yule was the only pre-war Pearsonian who continued in mathematical statistics after the war, even of the applied variety: Heron and Snow made careers outside the university while Greenwood concentrated on epidemiology. (Snow will re-appear in connection with the formation of the Industrial and Agricultural Research Section.) Pearsonians continued to join what was a companionable society and one that offered another journal in which to publish. **Leon Isserlis** (1881-1966; Irwin (1966)) was one of the most mathematical of Pearson's students and most like Edgeworth in his taste for pure theory. His debut paper in the *Journal* in 1918 was auspicious: it presented a finite population central limit theorem, filling a gap Bowley had identified in sample survey theory and stimulating a comment from Edgeworth (1918). Isserlis published less after he joined the Chamber of Shipping but he remained active in the Society. **Herbert Edward Soper** (1865-1931; Greenwood (1931)) and **Ethel Newbold** (1892-1933; Greenwood (1933)) went into medical statistics, following the path Greenwood had pioneered before the war.

The inflow from University College does not seem to have changed the nature of the Society. Bonar and Macrosty's (1934, pp. 204-223) analysis of the papers presented at meetings in 1909-33 shows some increase in mathematical content but the *Journal* remained a journal of economic and vital statistics with a sprinkling of statistical theory. Among economics journals it was distinguished from the *Economic Journal* and *Economica* (the LSE house journal established in 1921) by its emphasis on economic numbers and by having more contributions from outside the academy. Although little appeared to have changed in the Society, there was movement below the surface.

5 R. A. Fisher in and out

In 1919 R. A. Fisher went to Rothamsted Experimental Station and something started, just as something had started in 1892 at University College and again there was no obvious connection to the Statistical Society. In the 1920s Fisher was the most vital force in mathematical statistics–both theory and applications–and Rothamsted was soon challenging the supremacy of University College: one sign was that Gosset, who had gone to Pearson, sent his assistants at Guinness to Rothamsted and the Cotton Industry Research Association sent **L. H. C. Tippett** (1902-1985; Ford (1986)) to Rothamsted as well as to University College. There is a large literature on Fisher–see Aldrich (2003/10)–but the most useful single reference is Box's (1978) biography.

Fisher was employed to support agricultural science but he continued his work in biometry, genetics and statistical theory. He joined the Statistical Society although it did not cover his new interests and his old were aligned with those of *Biometrika*. However, in 1920 *Biometrika* rejected another of his papers and Fisher sent no more. Some of the Society's work interested him–he spoke at the meeting when Yule (1921) discussed the time-correlation problem–but its chief attraction seems to have been the *Journal* and its potential as an outlet for his work on statistical theory; his applied papers went to the *Journal of Agricultural Science* or the *Annals of Applied Biology.* In 1922 the *Journal's Miscellany* section included two papers by Fisher and they fitted in, being in the theoretical tradition of Edgeworth and the Pearson-correcting tradition of Yule. The paper on contingency tables (1922a) actually set a record for mathematical papers by producing *two* responses in the *Journal*–from Yule and from Brownlee. The equally fundamental regression paper (1922b) did not resonate though it could be read as a follow-up to Slutsky's (1913) paper in the *Journal*, a paper Pearson declined and then criticised. The regression history is discussed by Aldrich (2005).

To Fisher, statistics was a branch of applied mathematics. "The science of statistics ... may be regarded as mathematics applied to observational data" Fisher wrote on page 1 of *Statistical Methods for Research Workers* (1925), making it clear, on page 2, that he was not impressed by statistics as social science:

> Statistical methods are essential to social studies, and it is principally by the aid of such methods that these studies may be raised to the rank of sciences. This particular dependence of social studies upon statistical methods has led to the painful misapprehension that statistics is to be regarded as a branch of economics, whereas in truth economists have much to learn from their scientific contemporaries, not only in general scientific method, but in particular in statistical practice.

The importance of statistical methods to social studies was one of the Society's articles of faith but Fisher had difficulty in believing that social studies could make an original contribution to statistics: see his (1934) remarks on surveys when discussing Neyman (1934). Fisher's special irritation with economics probably stemmed from his experience with the economist officers of the Society following the initial honeymoon.

When the *Journal* rejected Fisher's third submission he was not pleased. His friend Leonard Darwin (1850-1943), who had been on the Council in 1919-21, identified Flux, the Honorary Secretary, as the "man chiefly concerned"; see Bennett (1983, pp. 76-7). Darwin advised against precipitate action but Fisher eventually resigned from the Society; see Box (1978, p. 87). The *Journal* did take another of his papers, a response (1924) to Brownlee's (1924) experiments testing Fisher's goodness of fit results. Separation was not so bad for Fisher for he found that the *Proceedings of the Cambridge Philosophical Society* would take his theoretical papers. Later Wishart and Bartlett also published there.

Fisher's first publications on the design of experiments appeared in 1925-6 and thus he was out of the Society in the formative period for his work in this area.

In 1929 Darwin finessed Fisher's re-entry into the Society by making him the gift of a life-time subscription–see Bennett (pp. 103-4)–but Fisher was only fully back in 1933 when he was elected to the Council and began attending meetings again. There is no sign that he was involved in the formation of the new Section; in that period he was corresponding with both Wishart and E. S. Pearson–who were involved–and the topic did not come up. In 1934 Fisher addressed a general meeting for the first time on the subject of his work in statistical theory and he (1935, p. 76) did not feel he had been made welcome:

> The acerbity, to use no stronger term, with which the customary vote of thanks has been moved and seconded [...] does not, I confess surprise me. From the fact that thirteen years have elapsed between the publication [...] of my first rough outline of the developments, which are the subjects of today's discussion, and the occurrence of that discussion itself, it is a fair inference that some at least of the Society's authorities on matters theoretical viewed these developments with disfavour, and admitted them with reluctance.

This time Fisher did not withdraw and he often spoke at meetings of the Society and of the Section, though after 1936 he seemed to participate less and he was next fully involved in the Society in 1953 when he became its President.

Once before there had been a hugely productive individual on the fringes of the Society. Pearson did not join but Yule and others brought in Pearson's methods and applied them to the traditional Society subjects. Fisher had no Yule, or, if he had, it was **Harold Hotelling** (1895-1973; Arrow and Lehmann (2005)) who took Fisher to the American Statistical Association. The Society met the new challenge in a new way: it had not adopted biometry but it adopted agricultural statistics.

6 Industrial and Agricultural

1933, the year in which the Statistical Society established the Industrial and Agricultural Research Section, was important for international mathematical statistics: Stigler (1996) refers to the publication of Kolmogorov's *Grundbegriffe* in the Soviet Union and the founding of *Sankhya* in India but he emphasises events in the United States leading to the founding of the Institute of Mathematical Statistics separate from the American Statistical Association. For the Society these developments were out of sight but there were local developments. Karl Pearson retired from his chair and his University College properties were divided between Fisher and Karl's son, **Egon Sharpe Pearson** (1895-1980; Bartlett & Tippett (1981)).

Less noticed was the publication of Neyman and Pearson (1933), the most important product of Egon Pearson's collaboration with **Jerzy Neyman** (1894-1981; Hammersley (1982)) which had begun in 1927. Neyman moved to England in 1934 and was in Pearson's department until 1938 when he moved to Berkeley.

The first issue of the *Supplement to the Journal of the Royal Statistical Society* has a note–Anon. (1934)–explaining the objectives of the Industrial and Agricultural Research Section and relating its history; there is further information in Bonar and Macrosty (1934, pp. 201-4). Events had moved very quickly: the possibility of a section was aired in December 1932, the first meeting was held in November 1933 and the first issue of the *Supplement* appeared a few months later. In 1928 the Society had shown that it could react to new needs when it established the Study Group to hold less formal meetings but the Section and *Supplement* were innovations of a new order.

Rothamsted was the intellectual force behind the agricultural side of the new enterprise; Fisher's "Contributions of Rothamsted to the development of the science of statistics" (1933) shows how much was achieved while he was there. Though Fisher was in and out of the Society, his colleagues were more constant. **John Wishart** (1898-1956; Bartlett (1956)) was Fisher's first assistant. Wishart came from Pearson in 1927 and he left in 1931 to replace Yule at Cambridge. There he taught mathematics students as well as agricultural students so that Cambridge mathematicians no longer had to teach themselves statistics or learn on the job as the Pearsons, Fisher, Bowley, Hooker, Irwin, Isserlis, Rhodes, Soper, Fieller, Yates and Maurice Kendall all did. Fisher's next recruit was **Joseph Oscar Irwin** (1898-1982; Armitage (1982)) who arrived in 1928–also from University College–and stayed until 1931 when he moved to the Medical Research Council at the London School of Hygiene and Tropical Medicine. Fisher's final Rothamsted assistant, **Frank Yates** (1902-1994; Dyke (1995)), arrived in 1931 and replaced Fisher as Head of Statistics in 1933 when he became Galton Professor at University College. Wishart and Yates worked on the design of experiments and, like Fisher, published in the *Journal of Agricultural Science*. Irwin joined the Society in 1926, Wishart in 1929 and Yates in 1933; Yates arrived too late to influence the formation of the Section but he had a paper in the first volume of the *Supplement*.

Irwin and Wishart contributed to the Society in different ways, Irwin mainly by writing and Wishart by organising. In the *Journal* Irwin represented mathematical statistics as practicioner, advocate and teacher: from his (1929) review of the second edition of Fisher's *Statistical Methods* through his "Mathematical theorems involved in the analysis of variance" (1931) to his surveys of "Recent advances in mathematical statistics" (1932-8) he tried to take the Society towards mathematical statistics. Wishart did not publish in the *Journal* but he "has taken a prominent part in the Study Group" (Anon. 1931, p. 603) and from 1931 he was on the Society's Council.

Industrial statistics lacked the achievement and intellectual fire-power of agricultural statistics but it provided the immediate impetus for the new development. Its main champions were E. C. Snow, a pre-war student of Karl Pearson and now in industry, and Egon Pearson. White (1960, p. 355) writes of Snow that he "was the Honorary Secretary deputed to guide the new infant in its formative years and he did a great deal to help the more youthful enthusiasts on the steering committee of the Section." However, it seems that Snow took initiatives and largely deputed himself. For Egon Pearson industrial statistics was an applied field removed from his father and from Fisher, the dazzling influence on anyone starting in mathematical statistics. Egon, who had joined his father's department in 1921, began working on theoretical problems arising from industrial situations in 1929, publishing the results in *Biometrika*. He joined the Statistical Society in 1929-30 and first served on the Council in 1934-5 after the new Section had been established. In 1936 he described the British industrial and agricultural scene for an American audience and in 1973 he recalled events for a younger audience; his own intellectual development is recounted in his book *Student* (1990).

There was some irony in the formation of an industrial and agricultural journal away from *Biometrika* when it was Egon Pearson's base and when it had always published the work of **William Sealy Gosset** (1876-1937; Hunter and Wishart (1938)). Gosset, more than anybody, embodied the industry and agriculture ideal but his work was not followed up in *Biometrika* and the name "Student" was made famous by the absent Fisher. Gosset only joined the Society when the Section was formed; he spoke at the inaugural meeting and became a keen supporter. Although Egon Pearson had been "assisting" in the editing of *Biometrika* since 1924, he only became editor when his father died at the end of 1936. Welch (1937) was the first paper in *Biometrika* to treat experiments in the way conceived at Rothamsted; the *Biometrika* contribution to design is reviewed by Atkinson and Bailey (2001).

The formation of the Section was precipitated by the visit to London of **Walter A. Shewhart** (1891-1967; Tippett (1967)) of the Bell Telephone Laboratories, the leader in industrial statistics and the first American to have an impact on Statistics in Britain. Like Quetelet at the birth of the Society, Shewhart is the one exotic in an otherwise domestic story. The Statistical Society was a good international citizen and played its part in the International Statistical Institute but there seems to have been little inflow of ideas either about the organisation of the discipline or about statistical theory. Lexis was an influence on both Edgeworth and Keynes and Isserlis was eager to pass on the work of Russian authors, especially Chuprov (**Alexander Alexandrovitch Tschuprow** (1874-1926; Isserlis (1926)) who was made an Honorary Fellow of the Society. However neither Karl Pearson nor Fisher expected to learn anything from abroad and in the early twentieth century English statistics of all varieties looked inwards.

In May 1932 Shewhart gave a course of lectures at University College–at Egon Pearson's invitation–and he spoke at a meeting of "representatives of science and industry" (Anon. (1932, p. 585)). The British Standards Institution set up a Committee on Statistics which included Pearson, Snow (representing the Society) and L. R. Connor (an industry representative but also a member of the Council). The momentum was carried into December when, on Snow's invitation, Pearson spoke to the Society on the "uses of statistical method in the control and standardization of the quality of manufactured products" (1933). In 1932 the Society was better prepared than it had been in 1885 when Edgeworth spoke, or even in 1896 when Yule spoke. Pearson's paper was welcomed by everyone, ranging from Wishart and Irwin to people from industry. Snow (1933, p. 69) expressed the hope that "the Royal Statistical Society might, now that it was approaching its one hundredth birthday, consider some extension of its scope by which it could provide the platform for discussion on the practical every-day application of statistical methods applied to sampling."

The matter was discussed at the next meeting of the Council on January 11th 1933 with the Minutes recording simply that:

> Dr Snow read a draft memorandum of a resolution for the formation within the Society of a section for Technical Statistics (defined as the application of the theory of statistics to problems met with in the routine matters of industry, commerce and agriculture).
>
> They agreed in principle, and ordered that the details be left to the Honorary Secretaries.

Snow brought a plan to the February meeting and the Minutes record:

> Authorised the formation of the Group, on the lines proposed by Dr. Snow, and appointed a committee to draw up a scheme of organisation, and define the functions of the Group, the Committee to consist of: Dr. Snow, Dr. Wishart, Mr. Connor, Dr. Pearson, Dr Wishart, Mr. McKay.

The committee nicely balanced interests, generations and backgrounds: Snow of the United Tanners' Federation had been taught by Karl Pearson, Connor of Imperial Chemical Industries had been taught by Bowley and A. T. McKay of the Boot Trade Research Association had worked with Egon Pearson.

At the committee's first meeting on March 1st Snow tabled a draft report which after discussion and amendment went to the Council which approved it at its April meeting. The first paragraph of the report set out the object of the new section:

1. The Committee recommend that the scope of the Group should be "The Application and development of Statistical Methods in the fields of Industry and Agriculture" and that the title of the group should be "The Industrial and Agricultural Research Section of the Royal Statistical Society". The Section will touch only to a small extent problems which are already handled by the Society. Only one of the papers which have been read to the Society in recent years would, under the proposed scheme fall to be dealt with by the Section, though there have been a few papers of a mathematical character published in Miscellanea which would, under this proposal, fall within its purview. The Committee lay special emphasis upon the "application" of methods which have already been developed. It will be necessary to hold a balance between new mathematical developments and the application of existing (and new) mathematical knowledge. Although the Section will be the section of the Society which will probably be particularly concerned with mathematical developments, it is the application of these developments in the fields of industry and agriculture which is the primary function of the Section.

There is nothing to indicate that the Section was conceived as a Trojan horse to get mathematicians into the Society. Indeed the Section genuinely wanted to reach "the practical man who had little or no knowledge of statistical technique and terminology" and, looking back, E. S. Pearson (1936, p. 365) acknowledged this was not easy and required special measures. The new Section had less trouble about what it would exclude–"It would contain nothing falling under the heading of economic, financial or demographic statistics."

The Society's next step was to publicise the Section by circulating a letter informing all interested parties of its plans; this is reprinted in the Note, Anon. (1934, p. 1). The inaugural meeting was held in November 1933. The kind of work the new Section encouraged is specified in the Note, Anon. (1934, p. 3):

For the present, in order that a paper should be acceptable at least one of the following conditions must be satisfied:

1. That it shows new applications of established theory to practical problems.

2. That it is technological in substance and statistical methods form an integral part, i.e. that the chief conclusions are evolved by the use of statistical data.

3. That it describes new methods of computation or new instruments likely to be of use in handling statistical data, e.g. a "Set-up" for a calculating machine or a new Nomogram.

4. That it contributes something new to statistical theory or methods and therefore adds to existing knowledge.

5. That it offers for the benefit of practical workers a new or clarified interpretation of advanced theory already published.

A few contributions appeared that met condition 4 but this side of the statistician's activity was better served by *Applied Statistics* which was formed in a later reorganisation–in 1952–with Tippett as editor.

The Section brought new people into the Society and the *Supplement* introduced new contributors: the first volume had articles by Wishart, Yates and **Maurice Stevenson Bartlett** (1910-2002; Gani (2002)), making 1934 a record year for publishing new mathematical authors. Bartlett was noteworthy as the first of the Cambridge statisticians with training in statistics; he had a spell working at University College before joining ICI's Agricultural Research Station at Jealott's Hill. The first volume of the *Supplement* comprised 250 odd pages in two issues compared to the main journal's 760 pages in 4 issues. There was a big increase in the space given to mathematics for the main journal continued to publish mathematical articles. Articles with no agricultural or industrial angle, such as Neyman (1934) on sample surveys, Fisher (1935) on inductive inference and Bartlett (1935) on the time correlation problem, appeared in the main journal; Neyman and Fisher presented their papers at meetings while Bartlett kept up the tradition of publishing in the *Miscellany*.

In 1934 statistics in England had a new order and for the first time there was a place in the Statistical Society for anybody calling himself a statistician. To its original project of a science of numbers registering the "progress of society" the Society had added the science of using numbers to raise productivity in industry and agriculture. The order was reflected in the journals: *Biometrika* specialised in theory, though it went on calling itself a journal for the "statistical study of biological problems" until 1947; the *Supplement* was the modern applications journal; the main journal covered the traditional topics. The modern applications were applications of new methods to new topics; the traditional topics continued to be treated in the traditional ways.

7 Comparisons

When **Henry W. Macrosty** (1865-1941; Greenwood (1941)) called his period, 1909-33, "the age of mathematical methods" he was responding to what he had seen and to the spirit of the new age. Yet Tippett's (1972, p. 545) comment that "the new age was slow to affect the working of the Society" is justified by Bonar and Macrosty's (1934, pp. 204-223) account of the papers published in the

Journal. Indeed a similar conclusion would be drawn from an analysis running on into the immediate post-War period. The slowness of change is not surprising given the longevity of careers and that Fellows were mathematical on entry or not at all: we found only two converts, Bowley and Hooker, and both had done mathematics at Cambridge. The Second World War brought mathematicians into statistics and into the Society on an unprecedented scale and Tippett (1972, p. 550) records a significant change: "In 1945 the Research Section (without any taint of application) was formed." Rosenbaum (1984) and Plackett (1984) follow developments up to the sesquicentenary.

When Edgeworth characterised varieties of statistics in 1885 the mighty biometric project was still in its Galtonian infancy and the development of a science of means devoted to agricultural and industrial applications could not be foreseen but the two versions of the science of means that he envisaged grew and continue in the modern Society. Although Edgeworth's later contributions to the *Journal* dealt overwhelmingly with the "general science," his first contributions were on the "science of those Means which relate to social phenomena" and around 1900 the Society had the leading workers in this science–Edgeworth, Yule and Bowley. Yet the applied science did not stimulate the development of the pure science and nor was the applied science further developed in the Society–or even in England. Econometrics, the economic version of the applied science, was developed in Continental Europe and America–see the accounts by Morgan (1990), Louçã (2007) and Aldrich (2010)–to be re-introduced into the Society at the end of the Second World War when **John Richard Nicholas Stone** (1913-1991; Weale (1992)) presented an "Analysis of market demand" (1945). Statisticians like Stone had a place in the new order: he was on the committee of the Research Section, now released from its industrial and agricultural mission, and the first article in the first issue of *Series B*–Orcutt (1948)–came from his group at the Cambridge Department of Applied Economics. The "working of the Society" was beginning to change and the change suited the econometricians but what of the traditional economic statisticians? In 1944 a proposal came forward to formalise the training of statisticians by creating a diploma in statistics. A "sister society"–the Royal Economic Society–expressed its opposition: the editor of the *Economic Journal*, Austin Robinson (1944, p. 266) insisted there were fields in which the application of mathematical statistics "is impossible, or even positively misleading, and in which the capacity to make logical inferences, based on an economic training,is incomparably more important." The proposal failed–see Plackett (1984, p. 141).

In the half-century 1883-1933 the world around the Society changed. The mathematics out there was originally in astronomy and then in biometry and agricultural statistics. The organisations and journals also changed and at different times the *Philosophical Magazine*, the *Proceedings of the Cambridge Philosoph-*

ical Society and the *Journal of Agricultural Science* were to be reckoned with. However, for most of the period the outstanding other for mathematics was Karl Pearson's operation at University College. This was not a body with a written constitution but an informal grouping yet one that was much more exclusive than the formally organised Statistical Society. In the age of KP the *Journal* and *Biometrika* were very different. Pearson was the leading contributor to *Biometrika* and many of the other pieces were written by his students, in more or less formal collaboration with him. In our period the *Journal* had no concurrently publishing Fellows one of whom had taught the other, or joint papers by teacher and student. Wishart and Bartlett were the first teacher/student pair (in 1934) and the first teacher/student(s) joint publication was by Matuszewski, Neyman and Supinska in 1935; such collaboration is an important part of the modern PhD system but that system was new in Britain and slow to affect statistics–see Aldrich (2006/9). Because Pearson had students to develop his ideas *Biometrika* had a continuity lacking in the *Journal.* The *Journal* published no general discussion of statistical inference between Edgeworth's "On the probable errors of frequency-constants" (1908) and Fisher's "Logic of inductive inference" (1935) and the topic only became one for continuous discussion around 1950. While the Society's publications came to look more like *Biometrika*, that journal in its "second golden age" under Egon Pearson–see Cox (2001, p. 8)–was characterised by an openness traditionally associated with the Society.

Considering the role of accidents and personalities in the story, the Society's mathematical turn seems anything but inexorable. Mathematics might have remained a minor stream, or have swollen only much later, if an English society with the scope of the International Biometric Society (established in 1947) had been formed in the 1920s. In April 1922 Gosset commented to Fisher on the prospects for such a group (McMullen (1970)):

> Of course if the 'Biometers' are to be any use they should include the leading practicioners, but I rather fancy that Pearson's idea is that it is a sort of University College Club. Besides which, as you say, he is perhaps a little intolerant of criticism, most of us tend to that I fancy as we grow older.

The impasse was that only Pearson could lead such a society and he was unacceptable. University College had another opportunity with industrial statistics but that went instead to the Society.

The sense of contingency is reinforced if we look at the experience of some societies born in the same age and of the same enthusiasms as the London Statistical Society: the American Statistical Association (founded in 1839), the Dublin Statistical Society (1847), the Dutch Vereeniging voor de Statistik (1857) and the

Société de Statistique de Paris (1860). The history of the American Association is sketched by Mason, McKenzie and Ruberg (1990) but Stigler (1996) focusses on the events of 1933 when the Association let its mathematicians go off and form their own Institute of Mathematical Statistics. Stamhuis (2007) describes how the Dutch society "abandoned statistics" to specialise in economics after a government statistics organisation was created in 1892; a new and mathematically oriented Dutch Statistical Society was formed in 1945. The Irish society, under the name of the Statistical and Social Inquiry Society of Ireland, stayed loyal to its original mission but since 1981 there has been an Irish Statistical Association, supported by academic statisticians, and since 1986 an Irish Economic Association; see Daly (1997) and Conniffe (1998). In France two new societies, L'Association pour la Statistique et ses Utilisations and La Société de Statistique de France, were formed in the 20th century but then all three combined in 1997 becoming La Société française de Statistique; see Droesbeke (2005).

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