Keynes among the Statisticians

John Aldrich

In 1909 after returning to Cambridge as a don, Maynard Keynes made a list of the books and articles he would write (see O'Donnell 1992, 769). It is the prospectus of a statistician/monetary economist with a taste for abstract inquiry. Keynes joined the Royal Statistical Society and contributed to its journal on eleven occasions between 1909 and 1912. However, statistics gave way to monetary economics and Keynes's only substantial publications were an article in 1911 on the theory of averages and a book in 1921, *A Treatise on Probability.* The latter treated probability as a department of logic but it contained material on the foundations of statistical inference—nearly one-quarter of the whole—as well as the substance of the 1911 article. This essay describes these productions: how they came to be written, what they contained, and how they were received by statisticians in England and America.

Correspondence may be addressed to John Aldrich, Economics Division, School of Social Sciences, University of Southampton, Southampton SO17 1BJ, U.K.; e-mail: john.aldrich@soton.ac.uk. This is an extended and much revised version of a paper presented to a seminar at NUI Maynooth. I am grateful to Denis Conniffe for the invitation as well as for discussions over the years. Keynes's publications were reissued as *The Collected Writings of John Maynard Keynes* published in London by Macmillan for the Royal Economic Society between 1973 and 1989. The editors were Donald Moggridge and Elizabeth Johnson. Many of the volumes have valuable editorial contributions. In the text that follows, all page references are to the *Collected Writings*. The papers at King's College Cambridge were microfilmed by Chadwyck-Healey in 1993 and issued as *The Keynes Papers: The John Maynard Keynes Papers, King's College, Cambridge.*

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Some of the publications Keynes planned came out of his fellowship dissertation, *The Principles of Probability*. This was submitted in 1907, revised and resubmitted in 1908, and, after much laying aside and rewriting, published in 1921. All three *Probabilities* had some coverage of the analysis of data, and the coverage increased with each rewriting. Through a combination of inclination and force of circumstances Keynes's work touched several epochs of statistical thought. In 1907 he was generalizing Carl Friedrich Gauss on the distribution of errors, in 1908 he was disputing Karl Pearson's inferences about associations, and in 1921, as the age of Ronald Fisher was beginning, he was calling for a return to the methods of Wilhelm Lexis. Keynes seemed to be rejecting the "English direction" in statistics in favor of the "Continental."

The *Treatise* was noticed by all the intellectual communities with an interest in probability; Anna Carabelli (1988, 252) and Rod O'Donnell (1989, 25) between them list over twenty reviews. Its home ground was philosophy and it was welcomed by Keynes's friends in Cambridge philosophy, Bertrand Russell (1922) and C. D. Broad (1922), although the young Frank Ramsey (1922) was skeptical. In philosophy the *Treatise* established itself as the classic statement of a particular interpretation of probability and was a source of further developments; see Gillies 2000 for a modern view.

In statistics things were not so happy. The reviews were generally unfriendly and with them interest in the book largely ceased: Keynes did not reply to his critics and only returned to statistical inference when he reviewed the first volume of Jan Tinbergen's work on the statistical testing of business cycles in 1939. Mary Morgan's (1990, 256) observation that the book was "rarely cited in econometrics" applies equally to statistics. The statistician reviewers were F. Y. Edgeworth and A. L. Bowley in England and W. L. Crum in the United States. Representing the biometricians—who were making the running in statistical method—were Raymond Pearl and Ronald Fisher; Karl Pearson, the biometrician-in-chief, did not review the book. From physics and mathematics came Harold Jeffreys and E. B. Wilson. The Continental reviewers included Emile Borel and Ludwig von Mises. The only review by an economist qua economist was by A. C. Pigou (1921) in the *Economic Journal*; he admired the book and even more its author for making so accomplished a contribution to "another field" (512).

The account below divides in two, separated by the publication of the *Treatise*: section 1 sketches the English statistical scene in 1909; sections

2–4 trace the development of Keynes's ideas on averages and on association/correlation; section 5 describes the overall stance of the book, for there was reaction to that as well. The second part of the paper, sections 6–10, considers the reactions. There is not much literature on these topics: Denis Conniffe (1992) and Stephen Stigler (2002) have commented on Keynes's work in statistics, and one episode, Keynes's controversy with Pearson in 1910–11, has become well known but the disciplinary background and the development of Keynes's thinking are not familiar. The histories of statistics by Theodore Porter (1986) and Stephen Stigler (1986) discuss much of the literature Keynes drew on but they stop in 1900; Donald MacKenzie (1981) treats the period 1865–1930, but from a very special angle, while works on the history of econometrics, such as Morgan 1990, do not find much going on in England at this time. In recent decades interest in the *Treatise* has been strong with some Keynes scholars promoting it as his *other* book and the sure path to the *General Theory*—see Mizuhara and Runde 2003 for a guide to this literature. However, apart from Carabelli 1988 and O'Donnell 1989, there is little here on Keynes the statistician or philosopher of statistics.

1. The Prospective Statistician

Keynes's biographers, Robert Skidelsky (1983, chap. 6) and D. E. Moggridge (1992, chap. 5), associate his interest in "probability as a branch of logic" (the preface to the *Treatise*) with an interest in the "application of probability to conduct" (chap. 26 of the *Treatise*), or "ethics and mathematical expectation" (chap. 21 of the 1908 *Principles*). Recent interest in the *Treatise* has concentrated on the link between probability and conduct and on the interpretation of probability. The link played no part in Keynes's negotiations with the statisticians and the interpretation only a very small part, even though Keynes rejected the frequency theory to which they subscribed. Unlike Frank Ramsey (1926) or Rudolf Carnap (1950), Keynes held that there was one true probability, which belonged to the logic of partial belief. However, as Keynes complained, there was no tight connection between the statistical methods of, say, Karl Pearson and his views on probability (Aldrich 2007b investigates the connection). Yet the same applies to Keynes's own work. The criticism of the frequency interpretation is most prominent in the 1908 dissertation; Keynes wrote more than in 1907 and in 1921 he wrote so much more about everything that this discussion was swamped.

Two projects on the list of January 1909 directly reflect the *Principles*, a book with the same title, and an article, "The Logical Basis of the Theory of Correlation," but there were two further statistical projects, a *Methods of Statistics*, and an article, "Mathematical Notes on the Median." Keynes was not only consolidating in statistics, he was beginning in monetary economics and he planned works on index numbers, the subject where the two met. His first extended new piece was "The Method of Index Numbers" (over one hundred pages in vol. 11 of the *Collected Writings*), the essay he wrote in April 1909 for the Adam Smith Prize. His first important publication was a monetary analysis of recent economic events in India (1909b).

The Royal Statistical Society organized meetings, published a journal, and made a home for those interested in numerical facts about society. Economists were prominent and Marshall once expressed the hope that the society and the British Economic Association (the future Royal Economic Society) would "ultimately amalgamate" (Whitaker 1996, 2:81). Even in 1892 statistics was more than economics in figures, but the disciplines diverged as the statistician came to be defined as one possessing special techniques applicable to any kind of material—including the nonsocial. The main force behind the reconception of the subject, the new methods, and the new applications, was Ronald Fisher (1890–1962). His *Statistical Methods for Research Workers* (1925a) begins by declaring, "The science of statistics . . . may be regarded as mathematics applied to observational data" (1) and ends by outlining "the principles underlying modern methods of arranging field experiments" (224). In 1934, its centenary year, the society established an Industrial and Agricultural Research Section and launched a new journal. The center of gravity of the society shifted although even today it holds a brief for economic statistics. (For the history of the society, see Hill 1984 and Plackett 1984; for Fisher, see Aldrich 2003–5.)

In 1909 these changes were far away and Keynes joined a company of three mathematical contributors, F. Y. Edgeworth (1845–1926), A. L. Bowley (1869–1957), and G. U. Yule (1871–1951). Keynes (1883–1946) showed his potential when he reviewed a book about West Ham, an area with serious social problems: "The importance of the volume . . . lies not so much in the attention it calls to questions with which most persons were already acquainted in a general way, as in the statistical methods and tables by means of which precise facts relating to these questions are scientifically collected and displayed" (1908b, 175). A *Methods of Statistics* was not an improbable project for one with such an eye for technique. West Ham even contained a lesson on the median: “The immense value of the median and its superiority to the arithmetic average in such investigations as these receives strong empirical support” (176). Keynes’s enthusiasm for the median had an element of *épater le bourgeois*, although Edgeworth had been an enthusiast since the 1880s—see Aldrich 1992, 675—and Keynes’s reasoning about averages—see section 2 below—was anything but revolutionary.

Edgeworth was the senior mathematical, the first person to discuss probability before the Royal Statistical Society—in the 1880s, half a century after the founding of the society and a century after probability had entered physical science. He was the statistician whose interests overlapped most with Keynes’s and there is something in Philip Mirowski’s (1994, 48) take on him as Keynes’s intellectual father; he even had a serious interest in philosophy, although his philosophical mind had been formed by John Stuart Mill and Henry Sidgwick. Edgeworth was a master of mathematical statistics and the authority on the theory of index numbers, the only branch of economic statistics where higher mathematics and probability had a recognized role. To William Stanley Jevons’s idea that the measurement of exchange value could be treated by the methods of the theory of errors Edgeworth had applied the full resources of mathematical statistics. (See Aldrich 1992 for the development of the stochastic approach, including Keynes’s doubts about it; for Edgeworth more generally, see Stigler 1978, 1986, and 2002, Mirowski 1994, and, of course, Keynes’s obituary [1926b].)

Bowley was the leading economic statistician and, looking ahead, the only reviewer of the *Treatise* to reply to its criticisms. Like Keynes, he was a Cambridge wrangler who studied with Marshall after finishing his mathematics degree. Bowley’s primary interest, from his earliest work, *England’s Foreign Trade in the Nineteenth Century* (1893), was in getting the numbers and writing about them. However, he could deal with the technical problems that arose, as his 1897 paper on the accuracy of means demonstrates; in statistical theory he was a disciple of Edgeworth. He had published a textbook, *Elements of Statistics*, in 1901. While it is mainly about preparing data and calculating descriptive statistics, a brief part 2 considers “applications of the theory of probability to statistics.” The interpretation of chance (266) is based on Venn’s frequency theory, but the probability calculations (269) rest on counting equally likely cases. There is also an outline of the theory of correlation, as developed by Pearson (1896). Alfred Marshall was unenthusiastic:

> If I were younger I would study the abstract mathematical doctrine of correlated curves. . . . I think it may occasionally be helpful in determining a controversy as to whether two movements have a causal connection. But at present, we are not ripe for that, I think. (Whitaker 1996, 2:307)

Keynes's friend C. P. Sanger, another wrangler, thought it "the best book on the Elements of Statistics written in English, French, German, or Italian" (1901, 193), a compliment reflecting as well on his reading as on Bowley's writing. (See Darnell 1981 for Bowley and Keynes 1930a for a short tribute to Sanger, a lawyer and occasional statistician/economist.)

Bowley's work in statistical theory was subordinated to his work in economics while Edgeworth's was only remotely connected to applications; the statistical work of Pearson—and later Fisher—had roots in biological problems. Yule was a modern statistician before there was modern statistics, his professional identity based on the techniques he developed, not the subjects he applied them to—economics, genetics, medicine, agriculture, etc. In 1909 he was best known for his work on correlation where his authority was second only to Pearson's. Yule joined the Royal Statistical Society in 1895 when he was in Pearson's department and he acted as a channel between the two; see his invitation to Pearson's lectures (Yule 1897a). Yule had most to do with the economists in the years between 1895 and 1914. In the early days he published two notes in the *Economic Journal* showing economists how to use correlation; in the second (1896) he introduced the concept of partial ("nett") correlation. Yule and his friend R. H. Hooker (1867–1944) were interested in the relation between demographic and economic variables. This was an important topic in Marshall's *Principles* although Marshall froze the discussion in the fourth edition (1898, 268) and never incorporated the correlation results of Bowley 1901, Hooker 1901, or Yule 1906. Hooker produced some purely economic examples of time series correlation analysis before moving on to agricultural meteorology (see Klein 1997). From 1912 Yule was a lecturer in statistics in the Cambridge University School of Agriculture, a position Keynes helped him to. The society remained the center of Yule's professional life but he did not publish his work on genetics and agricultural experiments in the journal or change the direction of the society. No disciples came out of the School of Agriculture, and Yule's influence was through his papers and his *Introduction to the Theory of Statistics* (1911), a more theoretical book than the early editions of Bowley's *Elements* and one that aimed beyond the traditional statistician. (See Edwards 2001 for

Yule's life and further references; Stigler 1986, chap. 10, and Aldrich 1995, 2005a, for Yule's work on correlation and regression; and Morgan 1997 for an account of the economic-demographic work.)

The person teaching the world—including any economists who cared to listen—the new techniques was Karl Pearson (1857–1936), face-to-face in the case of H. L. Moore and through his writing in the case of Warren Persons, Irving Fisher, and Eugen Slutsky. Pearson, another Cambridge wrangler and, like Keynes, a Kingsman, had founded biometry in the early 1890s. The "mathematicals" in the Royal Statistical Society worked alone and usually published their contributions in the "Miscellanea" section of the *Journal*, but Pearson had people working for him and his own journal, *Biometrika*. He never joined the Statistical Society and his relations with its members were often strained; those who could not get on with him found a home in the society. When Keynes joined, Yule and Pearson were in dispute over how to analyze associations between attributes (see MacKenzie 1981, chap. 7, and Stigler 1999). (For Pearson generally, see Aldrich 2001–5; for Moore and Persons, Morgan 1990; and for Slutsky, Aldrich 2005a.)

Keynes was already a critic of Pearson when he joined the society—see section 3 below. He was soon reviewing a study of the effects of parental alcoholism written by Ethel Elderton with the "assistance of Karl Pearson." Keynes's argument is discussed below and here I only want to warn against overinterpreting it. Keynes criticizing Pearson was not Keynes criticizing statistics; Yule asked him to write the review and approved the result—Yule had already published a criticism of a study from Pearson's Laboratory for National Eugenics on the influence of environment on the intelligence of school children. Nor was Keynes criticizing eugenics, as Skidelsky (1983, 236) seems to imply, for Keynes and Pearson were both eugenists. When a branch of the Eugenics Education Society—another society Pearson did not join—was formed at Cambridge University in 1911, Keynes became the treasurer. Marshall told Keynes when he sent his subscription, "I am hugely delighted [the society] has been formed" (Whitaker 1996, 3:284). Keynes's activities in the society have only recently been noticed by Keynes scholars—see Toye 2000—perhaps because there is nothing about them in the King's College archives.

There was no missing eugenics in the case of Ronald Fisher, who became the great figure in postwar statistics, for it shaped both his career and personal life. Fisher was still at school when Keynes made his list but by 1911 he was an undergraduate and a member of the council of the Cambridge University Eugenics Society (see Box 1978, 26). Fisher did not belong to Keynes's Cambridge of W. E. Johnson, G. E. Moore, and Bertrand Russell, for his outside interests were genetics and biometry. One constant inside Cambridge mathematics from before Pearson's time to after Jeffreys's was the course given by an astronomer on the combination of observations. Keynes may have drawn on this teaching when he wrote about means in his dissertation; Fisher certainly did in his "On an Absolute Criterion for Fitting Frequency Curves" (1912). Keynes's discussion of means was published as "The Principal Averages and the Laws of Error Which Lead to Them" (1911b); this was not on the 1909 list, although the projected "Mathematical Notes on the Median" must have covered some of the same ground.

At this time Keynes had many commitments outside statistics: he was still setting up as an economics teacher, working on Indian finance—lectures in the spring of 1911, a book in 1913, and a seat on the Royal Commission in 1913–14—and from 1912 he was editing the other economics journal, the *Economic Journal*. And then came the war. Broad (1922, 72) recalled going over the proofs of the *Principles/Treatise* with Keynes and Russell in the summer of 1914 when "from these innocent pleasures Mr. Keynes was suddenly hauled away on a friendly sidecar to advise the authorities in London on the moratorium and the foreign exchanges." In 1915 Keynes began the first of his three terms on the council of the society, and in 1919 he published a second economics book. His permanent place among the statisticians would be determined, not by what he wrote in statistics, but what, as an economist, he demanded from statistics and for statistics—the role familiar from Patinkin 1976 and Stone 1978. It is time to consider the ideas of the early Keynes—the inference specialist.

2. The "Principal Averages"

The paper published in the *Journal* in 1911 was based on material in the 1907 dissertation and was Keynes's earliest work on statistical inference. "The doctrine of means and the allied theory of Least Squares . . . are among the most important practical applications of the pure theory of probability" he wrote in 1907 (286). "Principal averages" like Fisher's "absolute criterion" descends from the first of Gauss's arguments for least squares, a Bayesian argument based on normality and a uniform prior for the coefficients. Gauss's second argument, the modern Gauss-Markov theorem, was not widely taught in the early twentieth century. To get the first argument going, Gauss obtained the normal distribution by asking, for what distribution is the arithmetic mean the maximum posterior value of the location parameter, assuming a uniform prior? He then used that distribution in the indirect measurement situation which was his real interest. Keynes stopped at the first stage and posed the same question for all the "principal averages"—the arithmetic mean, geometric mean, harmonic mean and median. For the purposes of this paper Keynes's answers and the technique he used—routine for one with his training—are not as interesting as his way of framing the inference problem and what he considered a reasonable answer.

Keynes (1911b, 159) states the measurement problem as follows: "We are given a series of measurements . . . of the true value of a given quantity; and we wish to determine what function of these measurements will yield us the *most probable* value of the quantity"; the solution is a model of reasoning in the mode of inverse probability, or Bayesian inference as it is called today. For Keynes this was how reasoning from data to (quantitative) probabilities about conclusions was conducted. He expresses the argument of article 176 of Gauss [1809] 1963 in his own notation. The first formula is "the ordinary rule of inverse probability" or Bayes's rule,

$$A_s / X_p H = \frac{X_p / A_s H \cdot A_s / H}{\sum_{r=1}^{r=n} X_p / A_r H \cdot A_r / H}.$$

Here A_s is the proposition that the true value is a_s, and X_p is the proposition that the measured value is x_p. On Keynes's conception of probability as a logical relation between propositions, only conditional probabilities have any meaning, and the term A_s/H represents the prior probability that the true value is a_s given H, where H stands for "any other relevant evidence which we may have." Keynes's analysis is based on a uniform prior: "Let us assume that $A_1 / H = A_2 / H = \text{K} = A_n / H\text{K}$, that is to say, that we have no reason *a priori* (i.e., before any measurements have been made), for thinking any one of the possible values of the quantity more likely than any other" (162). In Keynes's version of inverse probability—and Gauss's—the best conclusion is that for which the posterior probability $A_s / X_p H$ is greatest; with the uniform prior this is the conclusion for which $X_p / A_s H$ is greatest. Keynes, like Gauss, specified a finite number (n) of alternatives, although both assumed a continuum of values when they maximized; the impropriety of such a prior was not registered by Keynes; it became an important issue in Jeffreys's writings of the 1930s.

The reasoning was old-fashioned but it was clear and conceptually clean, unlike most of the contemporary English textbooks and journal literature. Modern statisticians are sensitive to different shades of frequentist and Bayesian reasoning but this sensitivity has only been acute since the inference wars of the 1930s between Fisher, Jeffreys, and Jerzy Neyman. The book that reflects what Fisher was taught in 1911, David Brunt's *Combination of Observations* (1917), moves innocently between Bayesian and frequentist arguments: Gauss's first argument is used—without mentioning the prior—for estimating the coefficients while there is a medley of principles for estimating precision. Fisher (1912) attacked those principles and made the prior-less Bayes strand in the teaching a general principle—the "absolute criterion" (1912) or "maximum likelihood" (1922); see Aldrich 1997 for details. The research literature was as confusing. Identifying the principles in Pearson's work on correlation is not easy: Pearson 1896 seems to be discussing the posterior distribution of the correlation coefficient (obtained from a prior-less Bayesian argument), while Pearson and Filon 1898 seems to be discussing the sampling distribution of the correlation coefficient. Pearson's lack of logic was something Keynes complained of (section 3 below) as did Fisher (1922, 329 n) from the viewpoint of a different theory of inference. The inferential basis of Yule's "Theory of Correlation" (1897b) is also unclear; the argument is that the use of correlation and regression need not be confined to the multivariate normal distribution yet Yule's inference machinery applies only to that case. Edgeworth was quite clear about the differences between Bayesian and frequentist arguments and used both. (For Pearson and Yule, see Aldrich 1997 and 2005a and, for Edgeworth, Pratt 1976.)

Conniffe (1992, 485) thought Fisher must have seen Keynes's paper and that he was probably influenced by it. The *Statistical Journal* was not required reading for an aspiring biometrician but if Fisher followed the Keynes-Pearson controversy he would have found Keynes's final word and "principal averages" in the same issue. Yet I doubt that he ever studied Keynes's paper—he does not refer to it and the two papers are utterly different in approach. David Brunt (1917, 27) does not mention Keynes when he treats the Keynesian question, For what law of error is the median the most probable value of the unknown? Evidently the Cambridge astronomers did not notice Keynes's work; indeed nobody wrote about it until it was reissued in the *Treatise* when it was generally praised. Fisher 1912 was too minor a piece to be noticed either by the astronomers (from whose world it had come) or by Keynes.

Keynes presented "principal averages" on five occasions without ever claiming much for it. It appears in both versions of the *Principles*, though never as an organic part of the work and never linked to the discussion of the principle of indifference on which most authors based the uniform prior. It appears in the index numbers essay (1909a) as appendix B to chapter 8, "The Measurement of General Exchange Value by Probabilities." Yet a demonstration of how the choice of index should reflect the law of distribution of price changes is no priority if, as Keynes thought, obtaining those laws was impractical. The article in the *Journal* is a contribution to the theory of errors which incidentally shows off the A_sH notation. In chapter 17 of the *Treatise* the "analytical power of the method" developed in earlier chapters of the book is being shown off. Keynes (1921, 206) advised the reader that the material in the chapter "is without philosophical interest and should probably be omitted by most readers." Statisticians were not "most readers" and this contribution to Kuhnian normal statistics found a modest place in the literature; Maurice Kendall and Alan Stuart (1967, 677) give it the improbably Fisherian designation, "characterizations of distributions by forms of [maximum likelihood] estimators." One can imagine a chapter 17 Keynesian taking its analysis as a model and recasting the arguments of Pearson, Brunt, etc. as rigorous inverse probability. This was one of the tasks of Jeffreys's *Theory of Probability* (1939)—whether Jeffreys could be considered a follower of Keynes is debated in section 6 below.

Keynes's work on the measurement problem stands apart from his other work on statistical inference as both mathematically constructive and unquestioning—it did not press the question, under what conditions can we go from data to a statement of probability? This was his question for proportions and for correlations. While Keynes was most concerned about correlation, he saw it as a variation on the simpler case of the proportion and he treated the two together. The next two sections follow Keynes's thinking on the "logical basis of the theory of correlation" from 1907 to 1921. Curiously, apart from an allusion to the marriage rate and the size of the harvest—see section 1 above—in the *Treatise* (360), Keynes's first discussion in print of the use of correlation in economics was the review of Tinbergen in 1939. For the logician/statistician, correlation in economics had no special significance, but the monetary economist could not have missed the irruption of correlation into this central part of numerical economics. Yule 1909 surveys some of the activity—see also Morgan 1990—and more came in the form of Irving Fisher's *Purchasing Power of*

Money. When Keynes (1911c) reviewed this for the *Economic Journal*, he approached the statistical part as an index number specialist and condemned the figures used in the correlation analysis without ever getting to the analysis. Keynes (1911a) appeared again as the index number authority when he discussed one of Hooker's (noncorrelation) papers.

3. "By what logical process . . . "

This section and the next follow the development of Keynes's views on correlation, the only form of modern statistical inference he took any interest in. The *Principles*—both editions—criticized correlation analysis from the standpoint of inverse theory and, although the attack was continued in the *Treatise*, a new "constructive theory" was added there. That Continental turn—it used the ideas of the German statistician Lexis—is discussed in the next section.

In the 1907 *Principles* correlation is discussed in the chapter on averages and means. Keynes (1907, 320) states his objection to the work of Pearson and other writers on the subject: "I do not understand . . . by what logical process they derive their conclusions in probability from their complicated calculations in statistics" (cf. 1908a, 243). He stressed that he "did not mean to assert that the organon, which they are seeking to develop, is necessarily incapable of a sound foundation or to deny that even now it possesses a practical utility." Keynes wanted an argument which, like that for the average, produced a posterior distribution from a prior and a data distribution. He did not provide the argument himself or state precisely what was required and unfortunately, when he tried to elaborate the critique, he became as unclear as Pearson.

When Keynes discussed averages it was with estimation in mind. In 1908 he relocated correlation to a new section on testing. Correlations are treated with proportions in the chapter on Bernoulli's theorem and correlation. Bernoulli's theorem (the weak law of large numbers for Bernoulli trials) is a direct argument in which the behavior of a proportion is derived from a proposition about probability; the inverse argument goes from the proportion to the probability and a similar inverse argument is, or should be, involved in correlation inference.

Keynes (1908a, 236) states the relevant form of the inverse argument as follows: "If . . . a divergence is realised which was à priori very improbable, it is argued inversely that there must have been a regular cause at work, that all the trials, in fact, had some important and unknown circumstance in common." Keynes processes the argument through Bayes's theorem with l representing the "proposition that the events and the circumstances are connected by laws of the required kind . . . and l the contradictory of this." His point is that a small value for the probability of the data given l and a large value for the probability of the data given l is not sufficient to produce a large posterior probability for l. The prior probabilities of l and l must be considered; often they are not.

Keynes (1908a, 251) gives a "simple instance of the error resulting from a neglect of the comparative values of the à priori assumption and its denial." In a certain community 51,600 out of 100,000 children proved to be male, a significant divergence from the accepted probability that a child will be male of 1050/2050 and sufficient ground for modifying the probability of a future male birth in this community. Against this reasoning Keynes argues

> if the proportion 1050/2050 is based upon a vast number of observations derived from a great variety of periods and places, many of them showing conditions of life not apparently dissimilar to those of the community in question, and if the proportion has been found to apply in spite of occasional divergences, not merely on the average of all periods and places but on the average in each; then I should require a much more startling divergence before I would modify my opinion. (252)

This example illustrates a general point about testing, that the significance level should reflect the prior probability of the hypotheses, but it also shows the kind of evidence on which that prior should rest. In 1907 Keynes had a formal concern with making Bayesian sense of statistical procedures but in 1908 there was an additional concern: the usual inferences might be wrong. The problem of rationalizing significance tests was not directly posed in the *Treatise* and the problem disappeared until Jeffreys (1939) set about reconstructing Fisher's procedures.

Keynes thought that Pearson's analysis of correlation/association also overlooked the prior. His discussion of generalities does not achieve much but his skepticism is clear from his commentary on Pearson 1904. From a 2×2 table of 2000 smallpox cases Pearson had calculated that "the deviation from independent probability . . . is such that the . . . table could only arise 718 times in 10^{40} cases if the two events [recovery and vaccination] were absolutely independent." Keynes (1908a, 252) objects:

> These figures give a high statistical-correlation, and no-one would deny them to be an argument *pro tanto* in favour of the efficiency of vaccination. But, if there were no other evidence whatever, how low a

> probable-correlation this statistical-correlation would justify. Suppose there were no scientific grounds of any kind in support of vaccination and these were the only figures available, how many alternative explanations, in the absence of elaborate knowledge of the individual cases might we not imagine. It would be the height of credulity to affirm with high probability on the sole basis of this statistical table the efficacy of vaccination. If we are to draw substantial conclusions from statistical correlation unsupported by other evidence our statistics must be on a scale commensurable with those on which the fundamental inductions of science have been based.

Besides the issue of how to represent the contribution of "other evidence" in transforming a high statistical correlation into a high probable-correlation Keynes introduces the possibility of "alternative explanations" to the direct causal one. This was not a topic he pursued though he discussed causation in all three *Probabilities*. The pathologies of correlation and causation had received intermittent attention from Yule and Pearson from the late 1890s. Yule reopened the question with a note (1910) on Pearson's treatment of spurious correlation arising from the use of ratios. Keynes wrote to Yule on the subject (see Keynes Papers, TP/1/1) but it is not possible to determine from Yule's replies what exactly Keynes was saying; none of it left any mark on the *Treatise*. Years later Keynes (1939, 310) wrote, "My mind goes back to the days when Mr. Yule sprang a mine under the contraptions of optimistic statisticians by the discovery of spurious correlations," but neither context nor reference is specific enough to identify the particular discovery Keynes had in mind. Keynes seems never to have discussed partial correlation/association which had an essential role in Yule's analysis of illusory correlations/associations. (The varieties of spurious, illusory, and nonsense correlations in the work of Yule [and Pearson] are set out in Aldrich 1995.)

Keynes's last word on correlation in 1908—it also represented his final position—was this:

> Statistical-correlation affords a valuable method of summarising a certain kind of evidence. But we must not incautiously accept conclusions which depend on nothing but the observation of the statistical-correlation, when they are offered in solution of practical problems of politics or science. (252)

Similar warnings can be found in the writing of some of the correlationists, including Yule (1910, 647) and Fisher (1925a, 133), although they employed a distinction between statistical inference and scientific inference foreign to Keynes's undifferentiated notion of inductive inference (cf. "the fundamental inductions of science"). Their arguments fit into the population-structure framework formalized in Koopmans and Reiersøl 1950, in which the pivotal concept is identification; see Aldrich 1994 for an account.

In his index numbers essay, written a few months after the 1908 *Principles* was submitted, Keynes (1909a, 51) reflected on the state of the theory of statistics. He emphasized the fragmentary nature of the contributions and admitted how "it is not even easy to say what precisely the theory of statistics should comprise." In his view, however,

> it should fall into three main divisions. In the first we should discuss questions dealing with the collection, arrangement, and description of statistical data. In the second we should deal with the theory and practice of the measurement of the quantitative characters of groups of statistics. And in the third we should call in the aid of principles of probability to discover what kind of inferences we are permitted to derive from statistical data, regarding the causes and correlations of phenomena.

Keynes complained that the first and third "have not always been clearly distinguished" and went on complaining in the *Treatise* (1921, 359) and the Tinbergen review (1939, 315). (Index numbers fell in the second division.)

One of the "practical problems of politics" was temperance reform, and the Elderton-Pearson study of the effects of parental alcoholism—see section 1 above—had some relevance to this. Keynes wrote three pieces for the *Journal* and two letters to the *Times* on that research. The controversy is familiar from the biographies and from Bateman 1990, among several others, but the most thorough analysis is Stigler's (1999), which encompasses the larger debates between Pearson and the Cambridge economists (and so includes Marshall and Pigou) and between Pearson and the medical doctors. The narrow Keynes-Pearson dispute was mostly about details in the data, but the detail was essential because Keynes (1910b, 205) emphasized that "gaps, beyond repair, in [Pearson's] original materials" cannot be mended by "elaboration of method."

Keynes noticed Elderton's report before the possibility of a review for the *Journal* came up. In a letter to the *Times* he laid down some general criteria:

> If we seek . . . to draw general conclusions from partial evidence in such a case as this, we cannot put much faith in our conclusions until we

> are satisfied (1) that the experiment is on a considerable scale, (2) that its field is truly representative of the population at large, (3) that the classification—in this case into alcoholic and non-alcoholic—has been skilfully and uniformly carried out. (1910c, 186–87)

The report was wanting in all three departments. Keynes described his objections as paralleling those of Yule (1910) to an earlier memoir from Pearson's laboratory. They also follow on from the critique of association in the 1908 *Principles*. In the *Journal* Keynes (1910b, 192) connects the three considerations with the realistic prior question; in 1908 the issue was the credibility of an association when there were "no scientific grounds of any kind" for it; now it was the credibility of a non-association when there are scientific grounds for an association.

Keynes brings his economist's expertise—prejudice to Pearson—to bear on point 2 in the quotation just above, the degree to which the sample is representative. "The effect on offspring . . . of truly alcoholic parentage has been swamped . . . by untrustworthy classification." He does not comment on the logic of the correlation analysis; as in his review of the *Purchasing Power of Money*, Keynes stops when he is convinced that the data is worthless.

In 1910–11 Keynes reviewed several foreign works on probability for the *Journal*; he was looking out for that "systematic exposition of the whole subject" he thought would "soon" be possible (1909a, 51–51). In particular he wanted a clear-headed outsider to clarify the English theory of correlation/association: see Keynes 1910a, 183, on Borel, and Keynes 1911d, 567, on Emanuel Czuber. In the 1911 review of Czuber there are some comparisons:

> Professor Czuber's methods are in direct line of descent from those of the classical writers on probability and error, and they possess the style and lucidity which such a history naturally gives them. But the reader must feel that these methods have reached their limit of accomplishment, and that nothing very novel can result from attempts to perfect them further. Recent English contributions, on the other hand, fragmentary and often obscure or inaccurate though they now are, seem to have within them the seeds of further development, and to carry the methods of mathematical statistics into new fields. (567)

Keynes mentioned Yule's new book and thus implied that the *Introduction* was not what was needed—perhaps no English work before Jeffreys's *Probability* of 1939 would have been! From Czuber, Keynes learned of

Panufny Chebyshev and Keynes became his first British advocate—see his review of Markov (1912a) and the *Treatise* (1921, 386–91). Chebyshev was not, however, put up against the English but against Pierre-Simon Laplace, whom Keynes disparaged even more than Pearson.

When Keynes next spoke on correlation—in chapter 33 of the *Treatise*—the works on trial were Yule's *Introduction* and the new enlarged edition of Bowley's *Elements*. Again Keynes (1921, 461) complained about the passage from description to inference: "The transition from defining the 'correlation coefficient' as an algebraical expression to its employment for purposes of inference is very far from clear even in the work of the best and most systematic writers on the subject, such as Mr. Yule and Professor Bowley." Here the lack of clarity was more in Keynes's mind, or in his reading, than in the texts, for both adopted a large-sample Bayesian approach. It is true that they did not give the details for correlation but there was more than the "vague appeal" to inverse probability reported by Keynes (1921, 465).

Part 5 of the *Treatise* reviews other probability failures. Chapter 30 on the methods of Laplace is largely concerned with criticism of the "rule of succession"—for its history see Zabell 1989. Keynes had first treated this in the 1907 *Principles* but in the *Treatise* (1921, 413–17) he savages Pearson's "On the Influence of Past Experience on Future Expectation" (1907). Pearson's variant is to find the predictive distribution of r successes in m further trials given p successes in n trials assuming Bernoulli trials (an assumption Pearson does not specify) and a uniform prior for the probability of a success. The article illustrates perfectly Pearson's lack of concern with assumptions and eagerness to get on with the mathematics, in this case to improve upon an approximation used by Laplace. Keynes objects: "The argument does not require . . . that we have any knowledge of the manner in which the samples are chosen, of the positive and negative analogies between the individuals, or indeed anything at all beyond what is given in the above statement." He is equally unimpressed with Pearson's conclusion that there is a 50 percent chance that between 7.85 and 13.71 of a group of 100 will have a certain disease given that 10 out of 100 had the disease in a previous sample; Keynes called it a "*reductio ad absurdum* of the arguments upon which they rest."

The first two paragraphs of chapter 31, "The Inversion of Bernoulli's Theorem," contain a blazing attack on the methods discussed in the previous chapter but they are followed by "nevertheless it is natural to suppose that the fundamental ideas from which these methods have sprung are not wholly *égarés*" (419). This grudging introduction leads into a drab account of the conditions under which in large samples the posterior distribution will concentrate around the true value. Keynes mentions no authorities, either to praise or to criticize. This was surprising given that most of the statisticians were chapter 31 Keynesians, large-sample Bayesians—see, for example, Bowley 1920, 414; Yule 1911, 273; Edgeworth 1921, 82–83 n; and Wrinch and Jeffreys 1919, 726—and they recognized the limits Keynes put on their activities.

Keynes (1921, 428) was giving up the quest for a "sound foundation" for the correlation organon and delivered his verdict against probabilistic statistical inference:

> Generally speaking, therefore, I think that the business of statistical technique ought to be regarded as strictly limited to preparing the numerical aspects of our material in an intelligible form, so as to be ready for the application of the usual inductive methods. Statistical technique tells us how to "count the cases" when we are presented with complex material. It must not proceed also, except in the exceptional case where our evidence furnishes us also from the outset with data of a particular kind, to turn its results into probabilities; not at any rate, if we mean by probability a measure of rational belief.

This was not the same verdict as Marshall's—see section 1 above—for Keynes had a use for complicated methods of counting the cases like correlation. Provided safeguards were observed, the method could be used now. Of course the statisticians disagreed about the exceptionalness of the "exceptional" case; nor were they as exercised about the safeguards—see section 5 below.

In chapter 33, Keynes wrote about "abandoning" the method of inverse probability in favor of the "less precise but better founded processes of induction" (466). We now consider the newcomer, the "constructive theory" based on Keynes's theory of induction.

4. Induction, Analogy, and the Continental Turn

Skidelsky (1983, 259) and O'Donnell (1989, 18) describe how after the dispute with Pearson, Keynes decided to expand the treatment of statistical inference in his book; in July and August 1911 he was at work on the new chapters. What appeared ten years later in the *Treatise* is nicely summarized by Broad (1922, 85):

> [Past statisticians] never have clearly distinguished between the problem of stating the correlations which occur in the observed data, and the problem of inferring from these the correlations of unobserved instances. There is nothing inductive about the former; but as it involves considerable difficulties, the statistician as been liable to suppose that, when he has solved all these, all is over except the shouting. Thus the inductive theory of statistical inference practically does not exist, save for beginnings in the works of Lexis and Bortkiewicz. These beginnings Mr. Keynes describes and tries to extend.

(Broad thought the diagnosis "exactly hits the nail.") Keynes had emphasized the description-inference gap in 1909 but now in 1921 he was advising statisticians to close it by calling in Lexis and Ladislaus Bortkiewicz, rather than the "principles of probability."

Keynes's inductive theory of statistical inference was a synthesis of his own theory of analogy in induction and the dispersion theory of Lexis and Bortkiewicz. In the 1907 *Principles* Keynes conceived of induction as a process by which a generalization gains credibility through the multiplication of instances. In the 1908 version he added a chapter on analogy and in the *Treatise* the discussion of analogy spreads over several chapters of part 3. Keynes developed terminology and some formalism for the analysis of analogy but, while he was able to use Bayes's rule to show how each additional instance increases the probability of the generalization it instantiates ("pure induction"), he was not able to express in the probability calculus his ideas about how varying the circumstances—what he called increasing the "negative analogy"—of instances increase the probability of the generalization. Keynes was not able to formalize the process of universal induction and he recognized that the process of establishing statistical generalizations would be more difficult. In other directions his analysis of induction was well developed, for instance his critique of the principle of the uniformity of nature and his advocacy of the principle of "limited independent variety." (For a fuller account of Keynes's views on induction, see Carabelli 1988, chaps. 4–5.)

Chapter 32 of the *Treatise* presents the method of Lexis, which Keynes (1921, 428) describes as a "valuable aid" to inductive correlation:

> This method consists in breaking up a statistical series, according to appropriate principles, into a number of sub-series, with a view to analysing and measuring not merely the frequency of a given character over the aggregate series, but the stability of this frequency among the sub-series, that is to say the series as a whole is divided up on some

> principle of classification into a set of sub-series, and the fluctuation of the statistical frequency under examination between the various sub-series is then examined. It is, in fact a technical method of increasing the analogy between the instances, in the sense given to this process in Part III.

The basic concern of the dispersion theory is whether the probability of an event in repeated trials remains constant. Christopher Heyde and Eugene Seneta (1977, 49–61) give a brief review of the development of the subject. In their notation the statistical series of N trials is divided into m sub-series with n trials in each sub-series so that $N = mn$. It is assumed that in the ith subseries the probability of a success is p_i ($i = 1,..,m$). Denoting by P_i the actual proportion of successes in the ith block and by P ($=\sum P_i / m$) the proportion of successes in all N trials, we have a measure of fluctuation or divergence in

$$D = \frac{\sum\left(P_i - P\right)^2 / m}{P\left(1 - P\right) / n}.$$

The work of Lexis is described by Stigler (1986, chap. 6) and Porter (1986, chap. 8), but their larger story is of the emergence and confirmation of "the English direction"—represented by Francis Galton, Edgeworth, and Pearson and their work on correlation. Oscar Sheynin's 2005 book, which also finishes around 1900, is written from the perspective of the "continental direction" and emphasizes the statisticians Keynes admired—not only Lexis but Chebyshev and Chuprov as well.

Lexis (1837–1914) and Bortkiewicz (1868–1931) were new to Broad but not to the statisticians. In the 1880s Edgeworth had paid considerable attention to Lexis and to the basic Lexian question, is the data from a single population? Edgeworth, however, lost interest and quarrelled with Bortkiewicz in the 1890s; see Stigler 1978. There is a brief exposition of the Lexis theory in Bowley 1901, 298–300, and when Bowley reviewed the *Abhandlungen* in 1903 he took the view that Lexis's contribution had long been absorbed. Pearson and Yule knew Lexis's work but paid less attention to it. Gunnar Myrdal's (1939, 8) comment on the *Treatise on Money*, that it "suffers somewhat from the attractive Anglo-Saxon kind of unnecessary originality, which has its roots in certain systematic gaps in the knowledge of the German language," does not apply to the earlier *Treatise*, which was the most German of English works on probability and statistics. Keynes could not be an uncritical admirer and he criticized Bortkiewicz for letting the mathematics run away with him (1921, 440).

The result of Keynes's (1921, 446) struggle to produce a "constructive theory" is disappointing, justifying Stigler's (2002, 162) comment that "the last chapter is only half-hearted, the work of someone eager to get the book out and move on." Keynes sought a procedure that would do for correlation what the Lexis theory did for proportions. The discussion of how the constructive theory is to be applied to correlation is contained in one (long) sentence:

> We must proceed as in the case of frequency coefficients; that is to say we must have before us, in order to found a satisfactory argument, many sets of observations, of which the correlation coefficients display a significant stability in the non-essential class characteristics (i.e. those class characteristics which our generalisation proposes to neglect) of the different sets of observations. (1921, 467)

Keynes had Continental-ized correlation or at least made a gesture in that direction but, like earlier English contributions, the outcome was "fragmentary" and "obscure" and it did not match Keynes's aspirations of a decade before. When he revived the suggestion in his review of Tinbergen (Keynes 1939, 316), it was in the more plausible form of calculating regression coefficients for each decade taken separately to consider whether these differed from the coefficients calculated for the whole time series.

With hindsight, signs of Keynes's new outlook can be found in his earlier work. In 1908—see section 3 above—he emphasized the importance of a "great variety of periods and places" in establishing the value of the sex ratio. Lexis is not mentioned although the sex ratio was his primary example. It is unlikely that Keynes was unaware of his work, for both his book and Bowley's easily accessible review appear in the bibliography of the 1907 *Principles*. The Elderton/Pearson controversy may have motivated Keynes to write more on statistical inference but it does not seem to have generated any new ideas. Reading Czuber may have been more stimulating. Keynes (1911d, 565) criticizes some of its calculations on the sex ratio and insists on the fact that "statistical inductions do not differ fundamentally from any other kind of induction"; the calculations reappear in the *Treatise* (1921, 384). Czuber describes the work of Lexis, although Keynes does not mention this. By 1912 Keynes had turned, for when he reviewed a study of the sex ratios of twins by Kazimierz Horowicz, he considered it worth notice because "these important methods [of Lexis] have been made use of so seldom by practical statisticians" (1912b). When Lexis died, Keynes (1914, 318) wrote a notice for the *Economic Journal* emphasizing his contribution to statistics where he was responsible for "new theoretical contributions of the highest importance." When Edgeworth died, Keynes (1926b, 260) recalled how "his more general articles . . . were of great value in keeping English students in touch with the work of the German school founded by Lexis." The passing of the great Continentals was better marked in the *Economic Journal*—see also Keynes 1926a and Schumpeter 1932—than in the *Statistical Journal*.

The last three sections have traced the development of Keynes's thought on statistical inference from the first *Principles* to the *Treatise*. After some comments on the shape of the work as a whole, we consider the statisticians' reactions.

5. *A Treatise on Probability* (1921)

Extra mass and supercharged rhetoric made the book of 1921 much more powerful than the dissertation of 1908. There were thirty-three chapters—instead of twenty-one—organized into five parts: "Fundamental Ideas," "Fundamental Theorems," "Induction and Analogy," "Some Philosophical Applications of Probability," and "The Foundations of Statistical Inference." (For overviews, see Braithwaite 1973, Carabelli 1988, and O'Donnell 1989, and, for a statistician's view, Conniffe 1992.) The material on averages found a home in part 2, while that on proportions and correlation went into part 5. For all except a Cambridge few, this was a new book—the statisticians had only seen "principal averages" and a few reviews.

The *Treatise* ranged widely: it was a positive contribution to probability as a branch of logic, an attack on false conceptions of probability, an entertaining exposé of the follies of probabilists, and a critique of modern statistics. *Economica* used two reviewers, a philosopher and a statistician—see section 7—while Edgeworth wrote one review for philosophers and another for statisticians—see section 6. The *Treatise* had so much to say on so many loosely connected topics that the reviewers ignored most of it. The only reviewer to respond to the critique of the frequency theory was the anti-frequentist Jeffreys, who complained that it missed the most effective version of the theory! Harrod's ([1951] 1972, 160) rebuke, "Certain persons of actuarial training showed irritation, not realizing that they themselves had not the faintest idea what the philosophical problems were that Keynes was trying to solve," does not recognize how overextended

Keynes was; the persons were irritated by Keynes's performance in their subject—see section 9.

The *Treatise* begins by laying down massive foundations in the form of Fundamental Ideas and Fundamental Theorems. However, the constructive theory of chapter 33 rests only on chapter 32 for the methods of Lexis, chapter 27 on the nature of statistical inference and the introductory remarks from chapter 18 on induction and analogy—the rest of part 3 and the whole of parts 1 and 2 are superfluous. There was also a disproportion in tone, between the sympathy Keynes expected—"the reader will perhaps excuse me if I have sometimes pressed on . . . with decidedly more confidence than I have always felt" (467)—and the ferocity of his criticism.

In the rewriting, the *Principles* was turned into a very aggressive book, an *Economic Consequences of the Peace* without the self-censorship; "this is so foolish a theorem that to entertain it is discreditable" (1921, 417) is a typically stylish insult, directed at Pearson, while "error and delusion" and "charlatanry" (419) were broadcast to all. "Never perhaps since ancient biblical times has such a redoubtable army of philistines been so deftly slain," commented Wilson (1923, 319) on one onslaught. The imagery is apt for a book set so much in the past: the particular philistines were Condorcet, Laplace, Poisson, Cournot, and Boole; the only example of modern English writing Keynes examined thoroughly was Pearson 1907—see section 3 above. Wilson (1923, 322) did not object to the weight of the past because in a work of reflection, "it would be unreasonable to expect any discussion of categories to reach nearer the date of issue than about 50 years." Keynes had some new references but, given his belief that the moderns were repeating the old mistakes and that he was pressing for a return to the methods of the 1870s, it is no wonder that Stigler (2002, 161) found "no sensitivity at all to the striking statistical developments of the period 1880 to 1920." R. A. Fisher's opinion of advice based on fifty-year-old intelligence is noted in section 9 below. The past presented no difficulty to the senior reviewer—he had been doing statistics for forty years, had experience of the old methods, and knew the remoter past from his extensive reading.

6. "Confirmation"—Edgeworth

Keynes (1926b, 261) regarded Edgeworth as a philosopher fallen from grace with a "bad conscience about the logical, as distinct from the pragmatic, grounds of current statistical theory." Keynes recalled often pressing Edgeworth to "give an opinion as to how far the modern theory of statistics and correlation can stand if the frequency theory falls as a logical doctrine" (260):

> He would always reply to the effect that the collapse of the frequency theory would affect the universality of application of statistical theory, but that large masses of statistical data did, nevertheless in his opinion, satisfy the conditions required for the validity of statistical theory, whatever these might be. I expect that this is true. It is a reasonable attitude for one who is mainly interested in statistics to take up. But it implied in Edgeworth an unwillingness to revise or take up again the more speculative studies of his youth. (260)

Of course, Keynes himself was no longer a youth.

In 1907 Keynes told W. H. Macaulay—fellow of King's and contemporary of Pearson—"Edgeworth's work does not seem to me to come to very much; he is very ingenious but often a little perverse and very old fashioned" (Moggridge 1992, 161). Edgeworth went so much his own way that even his contributions to the Pearsonian mainstream were easily overlooked. The very abstract and technical work, which Keynes regarded as Edgeworth's way of salving his conscience, is admired today—see, for example, the papers on the "generalised law of error" (1905) for Edgeworth expansions and on "the probable errors of frequency-constants" (1908–9) for what Fisher (1922) called the efficiency of maximum likelihood estimation. At the time it had little impact.

Keynes (1921, 473) considered the *Treatise* had only one predecessor among "systematic works in the English language on the logical foundations of probability"—John Venn's *Logic of Chance* published fifty-five years earlier. Edgeworth's first substantial essay on probability (1884) was a meditation on Venn's second edition, and his meditation on the *Treatise* in *Mind* (1922a) was a meditation on that earlier meditation. For the *Statistical Journal* Edgeworth (1922b, 108) confined himself to that "portion of the author's [Keynes's, this time] philosophy which bears directly on statistical science." Yet this was just as idiosyncratic, a review only Edgeworth could have written and one whose purpose could only have been Keynes's instruction. In Edgeworth's mind, Laplace, Gauss, and his own younger self were as present as Keynes, and the newcomer did not add much beyond an incomplete knowledge of the past.

Edgeworth (1922b, 108) begins by considering Keynes's treatment of the principle of indifference which had been used to justify a uniform prior. Edgeworth doubts whether Keynes's modification of the principle could cover situations like that put by von Kries—and discussed by Keynes—whereby a uniform distribution applied to the specific density is inconsistent with a uniform distribution applied to its reciprocal, the specific volume. (Ramsey [1922] had similar doubts.) Edgeworth refers to three of his own papers, including his "On the Probable Errors" (1908–9), before concluding that

> we do not gather that our author differs fundamentally from the views which have been expressed in this *Journal*. Rather we find in his more philosophically worded and carefully limited statements confirmation of what has been advanced in this *Journal* . . . respecting *à priori* or "unverified" probabilities. (109)

Edgeworth's impersonal form of reference is curious, as though his work were a collective achievement of the Royal Statistical Society, or something to which the society would subscribe.

Edgeworth was always well up in the current literature, but Laplace was his chief inspiration. He had to react to Keynes's (1921, 391) bouquet to Chebyshev and his followers Markov and Chuprov: "The Laplacian mathematics . . . is really obsolete, and ought to be replaced by the very beautiful work which we owe to these three Russians." After examination, Edgeworth concluded that the praise was excessive. Edgeworth praises Keynes's extension of Gauss's investigation of the arithmetic mean but the longest section of the review (111–13) is a criticism of Keynes's account of Laplace's theory of errors: Keynes, like "some respectable authorities" (presumably Todhunter [1865]), had overlooked Laplace's later frequentist theory of least squares which was based on the central limit theorem. Keynes did not know quite enough history. Edgeworth scarcely notices the material in part 5 but from his concluding paragraph he does not appear to find anything remarkable in it. He quoted Keynes's statement, "This is the way in which in fact we do think and argue," and let it speak for itself.

Edgeworth liked talking about the *Treatise* because its subject was close to his heart—see the one hundred papers in the three volumes edited by Charles McCann (1996). He discussed the book through 1923 and into 1924 with Edwin Wilson, the reviewer for the American Mathematical Society; Mirowski (1994, 433–39) presents extracts from their correspondence. Wilson (1879–1964), a professor in the Harvard Department of Vital Statistics, was a mathematician at large who expounded Willard Gibbs's vector analysis in 1901 and theorized about utility in the

1940s; see Hunsaker and MacLane 1973. Wilson professed awkwardness about discussing a philosophical work but he loved it. He saw more good in the book than some other American authors, notably Raymond Pearl and Arne Fisher—see section 9 below—although he never said precisely what the good was.

7. "A vast field of investigation to which he hardly alludes"—Bowley

Bowley reviewed the *Treatise* for *Economica* in tandem with the philosopher Abraham Wolf (1876–1948), who took parts 1–4. Wolf appeared occasionally as the statisticians' philosopher and was a discussant of Fisher 1935—see section 10 below. Of the reviewers, Bowley had most cause to be irritated with the *Treatise*, for the *Elements* was punished in chapter 33. Keynes (1921, 464–65) comments on Bowley's interpretation of correlation as follows: "By this time the student's mind, unless anchored by more than ordinary scepticism, will have been well launched into a vague, fallacious sea." The interpretation of correlation as a measure of the extent to which variables are acted upon by common causes was in the first edition—it came from Pearson 1896—but it was more prominent in the new edition of 1920. The new *Elements* might have been written for the Keynes of 1907, but now it just met disapproval.

Bowley (1922, 97) described the thrust of part 5 of the *Treatise* and registered his dissent:

> A reader of the book, not versed in modern statistical investigations, would undoubtedly obtain the opinion that the conclusions which statisticians reach with the help of mathematics commonly go far beyond the premises, and that their legitimate sphere is very limited. . . . but there is a vast field of investigation to which [the author] hardly alludes, which is nearly free from the errors he discusses.

In the portion cut, Bowley admits that some statistical work is open to criticism and even agrees that "the rigid criticism which Mr. Keynes develops is very useful in exposing faulty deductions and in suggesting rules by which the validity of arguments can be tested" (97). From the sequel, though, it seems that Bowley saw no use for these rules in his work.

Bowley accepts without demurring Keynes's criticism of hasty inference from group to group (98). While the chemist may be able to rely on the homogeneity of his material, "the statistician, when dealing with a heterogeneous and progressive society, cannot make a similar assumption." However, Bowley insists that

> a very great part of statistical analysis is concerned not with extending a measurement made on one group to other separate groups, but in inferring from a sample selected at random or by rule from a single group what is the composition of the group. (98)

This was the case with his own work.

Bowley did not reply in kind to Keynes's ridicule but instead he met Keynes's central point about statistical inference using an example from part 2 of the *Elements*—this now had three chapters on correlation:

> From the examination of the circumstances of 600 households in Reading it was computed that the (partial) correlation coefficient between rent and the number of (equivalent) adults in the household was –.136, when variation of income was eliminated. . . . Can we infer from this without other data (1) that there is a connection between size of family and rent; (2) that nearly the same measurement would be obtained if we had examined the 12,000 working-class households in Reading instead of only 1 in 20; (3) does the actual number –.136 in any sense measure the amount of connection between rent and size of family; and (4) can we generalise to other towns of South England, of England, or of Great Britain? We understand Mr. Keynes would answer each of these questions in the negative. (99)

The data on which these calculations are based was first described by Bowley and Alexander Burnett-Hurst (1915). The sample households were chosen by taking every twentieth from the list of households (178). This book contained no correlations, only tables; the new part 2 was Bowley's first real venture into correlation.

Bowley (1922, 99) deals with the four questions as follows:

> With the answer to (4) we may agree, and add that it is known that relevant conditions are not the same. For (3) we can only in this case make the general statement that a great number of coefficients, varying between +1 and –1 have been computed for a great variety of phenomena, in some of which the whole system of causation is known, and that a general impression has been obtained of the correspondence between the values of r found and the scale of correlation on which zero stands for complete independence and unity for complete connection. As to (1) we should say that if, in fact, there were no relationship but that rent

> and numbers were independent, the chance against so great a value as .136 being found in a random selection was approximately 1400 to 1, while there was no a priori impossibility of a negative correlation. For (2) we should express the correlation coefficient as −.136 ± .040, where .040 is the standard deviation of the measurement; the chance is about 2 to 1 against the observed value appearing if the value for the whole town was not between −.10 and −.18, and very great against its appearance if the value were beyond, say, −.4. We can make such a statement more definite if we use inverse probability, and can then estimate the probability of the value for the town. In this case the precision of the measurement is low; as the number in the sample increases, the precision rises.

These answers would have been assented to by the other statisticians although Yule would have found much more to say about (3), the interpretation of correlation, because he had written extensively on illusory correlations; see Aldrich 1995.

Bowley knew that Keynes's point about non-homogeneity did not apply to his work but he conceded that it might apply to the work of statisticians dealing with a "progressive society," that is, time series analysts. Bowley reports without comment that "the method used by Lexis is approved." Of the statistical writers only Keynes's old friend Sanger (1921, 652) approved Keynes's enthusiasm for Lexis. Yule was just beginning his work in time series analysis—see his "On the Time-Correlation Problem" (1921)—and it is a pity that this friend from 1909 did not review the *Treatise* or apparently leave any reaction to it. A time series analyst, W. L. Crum, reviewed the book for the *Journal of the American Statistical Association*; the American association was similar in its purpose and composition to the Royal Statistical Society. Crum's review is an enthusiastic and undiscriminating summary that concludes that this is "the most stimulating book on the fundamentals of statistical theory that [the reviewer] has read in many months" (1923, 681). It pays no special attention to Keynes's constructive theory. Crum was associated with the Harvard economic barometer project—see Morgan 1990, 56–63—and the project's senior statistician, W. M. Persons, also admired Keynes's book. His presidential address to the American Statistical Association contains a "vehement rejection of probability" (Morgan's [1990, 235–36] phrase). Persons (1924, 6) states that "the view that probability provides a method of statistical induction or aids in the specific problem of forecasting economic conditions, I believe, is wholly untenable." He pointed to Keynes for support and praised the "great skill" with which Keynes had argued "that statistical probabilities give us no aid in arriving at a statistical inference" (7). Persons took the heterogeneity objection to its limit and argued that we have too much information to "view 1924 as any year taken at random." For Persons, probability was a matter of independent draws and a stochastic process was beyond his imagining: "Granting as one must that consecutive items of a statistical time series are, in fact, related makes inapplicable the mathematical theory of probability" (7). The American friends of the *Treatise* read its lesson as, keep away from probability! They did not use Keynesian methods to found their inductions, nor, of course, did those others in the association (such as Working and Hotelling [1929] and Schultz [1930]) who ignored Keynes's book and used probability in time series analysis.

The reactions of Bowley and Edgeworth and Crum and Persons show how Keynes was read by some of the more mathematical statisticians. Outsiders read the book, too, and we examine the response of two—Harold Jeffreys and Ronald Fisher.

8. "A searching analysis"—Jeffreys

Probability in natural science—physics and biology—barely figures in the *Treatise*. Wilson (1923, 321) thought a discussion of Gibbs's statistical mechanics would have been useful but did not complain because of his fifty-year rule. The book was reviewed for the British science magazine *Nature* by the physicist/applied mathematician Harold Jeffreys (1891–1989). Jeffreys was not put out by the lack of attention to physics—indeed he liked the book almost as much as the philosophers. This is not so strange for, at one remove, he was one of them: his collaborator Dorothy Wrinch had attended the lectures of W. E. Johnson. Jeffreys was only ever a philosopher at a distance; he spent his life in Cambridge but he did not know Keynes—David Howie (2002, 98) tells how they met once, sharing a railway compartment. Howie is excellent for Jeffreys's life and Jeffreys's Cambridge; see also Aldrich 2003–6.

Jeffreys was asked to review the *Treatise* because of the work with Wrinch. Later he became irritated at being described as a follower of Keynes, and he pointed out that the first Wrinch and Jeffreys paper appeared in 1919 before the *Treatise* was published when all that he had read was Broad 1918 (Jeffreys 1948, v). The review begins by describing the *Treatise* as "a searching analysis of the fundamental principles of the theory of probability and of the particular judgements involved in its application to concrete problems" (Jeffreys 1922, 132) and ends by urging that it "be read by every student of science who aims at a real understanding of his subject" (133). The praise disguised the fact that Jeffreys missed most of what Keynes was saying to the student of science. Jeffreys does not mention part 3, "Induction and Analogy," and nothing in his current or future system corresponded to it. Jeffreys wanted to use "the theory of probability" (his term for Bayesian inference) to evaluate the theory of relativity or different theories of the origin of the earth, and Keynes's analysis of induction was of no assistance.

The body of the review details the differences between Keynes's probability system and the Wrinch-Jeffreys system; the principal ones are that the latter admits only numerical probabilities and it gives more scope to the application of the principle of sufficient reason. Wrinch and Jeffreys assign numbers to combinations of propositions and data; their "property 2" states:

> If in one combination [of proposition and data] the proposition is more probable relative to the data than in another, the number corresponding to the first is greater than that corresponding to the second. (1919, 720)

This axiom "yields as an obvious corollary the famous 'principle of sufficient reason'; according to this, equal probabilities are assigned to propositions relative to data when the data give no reason for expecting any one rather than any other" (720). In the *Treatise*, Keynes (1921, 45) presented this argument but indicated that it fails because probabilities are not usually comparable.

In his review Jeffreys naturally responded to chapter 4 on the principle of indifference, which describes "the paradoxical and even contradictory conclusions" to which the principle leads. Jeffreys (1922, 132) comments that Keynes "criticises severely many previous applications of this principle (so severely that an unprepared reader is likely to be betrayed into expecting him to reject the principle altogether)." Jeffreys believed that Keynes's severity was generally unjustified and that some of the paradoxes were generated by neglecting differences in information. Jeffreys came back to this point in 1933 in the course of a controversy with Fisher; see section 7 above. Jeffreys's interest was almost entirely confined to parts 1 and 2, although he quarrels with the analysis of the law of succession in chapter 30, which conflicts with his and Wrinch's analysis of "sampling inference."

The Wrinch-Jeffreys collaboration came to an end in 1923, but Jeffreys returned to probability in his 1931 book, *Scientific Inference*. The final chapter, titled "Other Theories of Scientific Knowledge," has a section on Keynes but it adds little to the review. Jeffreys gave his definitive opinion of the *Treatise* in his *Theory of Probability* of 1939:

> This book is full of interesting historical data and contains many important critical remarks. It is not very successful on the constructive side, since an unwillingness to generalise the axioms has prevented Keynes from obtaining many important results. (25)

More than any other reviewer, perhaps, Jeffreys misconstrued Keynes's objectives. It is unlikely that Keynes ever read anything by Jeffreys—Wrinch and Jeffreys 1919 is not referred to in the *Treatise*—but had he, Jeffreys might well have seemed the modern Laplace. On the other hand, Jeffreys's use of probability was very controlled and limited to the kind of problem Keynes treated in "Principal Averages."

In 1921 Jeffreys was already practised in combining astronomical observations but it was a decade before he went back to basics and considered measurement problems using his theory of probability. *Scientific Inference* contains his first result, an extension of Gauss's first argument for least squares—see section 2 above—to accommodate unknown precision. Numerous papers followed, and in 1939 he published the *Theory of Probability*, a comprehensive objective Bayesian version of "modern statistics"; see Aldrich 2005a. Keynes might have found it useful in 1909; in 1939 it was respectfully rejected by the statisticians and little notice was paid to it before the Bayesian revival of the 1960s.

R. A. Fisher was the reason why the *Theory of Probability* was such a marginal work in 1939. Just as the *Treatise* was appearing Fisher was beginning to produce the works that would have a decisive influence on the course of statistics for the next two decades. He would replace Karl Pearson as the central figure and would have much more influence on the economists' use of statistical techniques than the economists Bowley and Edgeworth.

9. "Elaborate show of critical exactitude"—Fisher

If Jeffreys was the friendliest of the non-philosopher reviewers, the biometricians Pearl and Fisher were the most hostile. Raymond Pearl (1879–1940) reviewed the *Treatise* for *Science*, the American equivalent of *Nature*; "the most distinguished student of Pearson in this country" was how Wilson described him to Edgeworth (Mirowski 1994, 437). Pearson did not write a review of the *Treatise* or apparently anything on it; there is, however, a reference to its contention that "it is almost discreditable to base any reliance on so foolish a theorem" in E. S. Pearson's (Karl's son) paper on Bayes's theorem (1925, 388).

Pearl (1923) reviewed the *Treatise* with the *Mathematical Theory of Probabilities* by Arne Fisher, the Danish-American actuary. Pearl liked the latter, a work written from the point of view of the scientist, "who sees in the theory of probability one of the most potent tools the human mind has ever devised for penetrating deeper into the relations and laws of phenomenal nature" (51). Pearl did not like books written from the point of view of one who regards the theory as "essentially only a branch of metaphysics and finds its usefulness in the fact that it furnishes an entertaining and involved subject to speculate and talk about" (51). Keynes's book was worse than superfluous:

> The thing which makes it not only an unreliable guide, but in the reviewer's judgement a positively pernicious one for at least a large group of students who wish to make practical use of the theory of probability in scientific research, is its abandonment of the experiential basis of probability, and the substitution in its place of the thesis that the basis of probability is simply a logical relation, independent in respect of its ultimate philosophical validity of any experience whatever. (51)

Pearl directs the reader to the pages Arne Fisher added to his book (1922, 277–79) in which he "flays Keynes and tacks his integument up for public display and ridicule" (51). The books by Keynes and Fisher might have been complementary, for Fisher was a spokesman for the Continental direction and he gave Keynes's heroes, Lexis and Chebyshev, their due. However, Keynes's combination of attitude and amateurism was too much for Fisher.

Ronald Fisher, like Keynes, was a critic of the statistical inference establishment but his rejection of the Bayesian argument set him against Keynes as well as against Edgeworth, Bowley, Pearson, Yule, and Jeffreys. Fisher first noticed the *Treatise* in his "Mathematical Foundations of Theoretical Statistics" (1922) in which many of his mature ideas first appear; for elaboration see Aldrich 1997 and Stigler 2005. Fisher's footnote reference to the *Treatise* comes after the polemic against Bayes in a calm passage explaining the difference between likelihood and probability:

> Likelihood . . . is not only fundamentally distinct from mathematical probability, but also from the logical probability by which Mr. Keynes has recently attempted to develop a method of treatment of uncertain inference, applicable to those cases where we lack the statistical information for the application of mathematical probability. Although, in an important class of cases, the likelihood may be held to measure the degree of our rational belief in a conclusion, in the same sense as Mr. Keynes' probability, yet since the latter quantity is constrained, somewhat arbitrarily, to obey the addition theorem of mathematical probability, the likelihood is a quantity which falls definitely outside its scope. (1922, 327)

This description of what Keynes "attempted to develop" is so wild that I suspect Fisher read the beginning of the book and guessed the rest.

The "Mathematical Foundations" makes no further references to Keynes, yet, in laying the foundations for statistical methods, it goes into his territory. Fisher took a brisk line on the issue of the appropriateness of the population, or the correctness of the specification. He begins by maintaining, "It should be noted that there is no falsehood in interpreting any set of independent measurements as a random sample from an infinite population" (313), and ends with a remark on the choice of mathematical form of the specification:

> For empirical as the specification of the hypothetical population may be, this empiricism is cleared of its dangers if we can apply a rigorous and objective test of the adequacy with which the proposed population represents the whole of the available facts. (314)

From Keynes's viewpoint, this is to wish away the problem of induction, and the test of significance was not an adequate control. For his part Fisher soon found a way of rationalizing the Lexian approach within his scheme.

Turning to the review, where Arne Fisher had been amused and patronizing, Ronald Fisher was angry. He was repelled by the author's manner, what Pearl (1923, 52) called his "flippancy, super-smartness and debonair conceit." However, Fisher (1923, 47) maintained that

> the question of taste would be of secondary importance, if this elaborate show of critical exactitude were supported by the announcement of valid and applicable criteria, or even by a clear and thorough acquaintance with the subject.

Fisher matched Keynes for righteous indignation, using the voice he usually kept for criticisms of his own work. In the final paragraph he explained why:

> It would be unnecessary to occupy so much space with a criticism of a work which will be chiefly of interest to logicians, were it not that statistics is a practical means of research, attempting in all directions the problems which accumulated data present. Statistical science offers to the applied mathematician a region of thought which may be described almost as unexplored: it is a science, too, in which the English student enjoys exceptional advantages: and if the views of the last section of Mr. Keynes' book were accepted as authoritative by mathematical students in this country, they would be turned away, some in disgust, and most in ignorance, from one of the most promising branches of applied mathematics. (50)

Fisher was outgrowing biometry which, in the person of Pearson, had rejected him and he saw the book as striking at his ambitions for his new subject, "statistical science"; see Aldrich 2006. The "exceptional advantages" enjoyed by the English student was a positive gloss on the situation Keynes lamented—see section 3 above. Another perspective on Englishness was Major Greenwood's likening of Pearson to Newton and his foreseeing the decline of the English school unless it adopted the methods and notation of the Continentals (Farewell, Johnson, and Armitage 2006, 2164). As things turned out, these hopes for union were not realized until well after the Second World War; see Aldrich 2003 for further aspects of the divide.

Fisher (1923, 46) described Keynes's conception of probability and, like Pearl, thought it useless:

> To the statistician probability appears simply as the ratio which a part bears to the whole of a (usually infinite) population of possibilities. Mr. Keynes adopts a psychological definition. It measures the degree of rational belief to which a proposition is entitled in the light of given evidence. Often, as Venn has pointed out, have writers on Probability formally adopted some such psychological definition. But when anything has to be proved about this probability, the definition based upon statistical probability has always to be used.

Fisher was prepared to dictate on probability but not to discuss it, as later non-exchanges with William Burnside and Jeffreys would show;

for these see Aldrich 2005b and 2006. Apart for the odd sentence and a whole paragraph in (1925b, 700), Fisher did not discuss probability until his 1956 book, *Statistical Methods and Scientific Inference*.

In statistical science Fisher lets Keynes speak only to trip him up; he does not answer or even state Keynes's criticisms of statistics; Keynes was too evidently unqualified to make any. Nothing in the book is praised even though there were points with which Fisher agreed. Where there was something they both criticized, Fisher criticized Keynes's criticism. On the Laplace rule of succession—the main topic of chapter 30—Fisher (1923, 47) writes, "In the present writer's opinion the assumption of such equal distribution is usually illegitimate, but it involves no such inconsistency as Mr. Keynes imagines."

Some of Fisher's criticisms are unfair; for instance, to illustrate the "unnecessary detraction" in which the book "abounds," he quotes Keynes on Quételet—"There is scarcely any permanent, accurate contribution to knowledge, which can be associated with his name"—but without the sequel: "But suggestions, projects, far-reaching ideas he could both conceive and express, and he has a very fair claim, I think, to be regarded as the parent of modern statistical method." Paradoxes were part of what Richard Stone (1978, 62) called the "splendours and . . . fun" of the style.

For Fisher, Keynes's greatest disqualification was his "apparent lack of acquaintance with the modern developments of Statistical Science." This was a fair point, though Fisher's examples were not quite fair. However, fairness was irrelevant when Fisher was producing so many "modern developments." Many entered circulation through the *Statistical Methods* Fisher began writing in 1923 (see section 1 above). This was a book on significance testing, a practice whose logic Keynes found defective (section 3 above), but one which the *Treatise* did not engage. The *Treatise* appeared before the explosion of Fisherian tests but that development was not anticipated at all; the book missed the previous contributions that Fisher (1925a, 16–17) identified: Pearson 1900 on χ^2 is in the bibliography but is not discussed; Student 1908 on the probable error of the mean is not even in the bibliography.

Fisher's review could not have been read much—he was not yet an authority and the *Eugenics Review* had no authority in this field—and Fisher did not mention Keynes in his other writings. Yet there were aftereffects. Fisher, Thornton, and Mackenzie (1922) used χ^2 as an index of dispersion for sets of parallel plates of soil bacteria where the distribution was the Poisson. The work is reported among the applications of χ^2 in Fisher's paper on distributions based on the normal distribution, but there he notes the possibility of a similar procedure for the binomial and multinomial distributions, adding that

> the case of the binomial is interesting to economists, in that it leads at once to a test of the significance of the Divergence-Coefficient of Lexis. In fact, the method of Lexis was completed and made capable of exact application, from the time of the first publication of the tables of χ^2. (Fisher 1928, 807)

Fisher must have enjoyed trumping Keynes, for he made the same point in the *Statistical Methods* (1925a, 79). However, this line from the new statistical mainstream did not make the method of Lexis any more prominent than it had been in Pearson's day. It was visible in the 1920s, particularly in the United States—in Arne Fisher 1922, chaps. 10–12, and Coolidge 1925, chap. 4—but seen only occasionally later, for instance, in Irwin 1932 and David 1949. When modern econometricians follow Keynes's instruction and test for the stability of relationships, as in Hendry 1995, 529, the means they use descend from Fisher.

The reviewing experience produced other ripples in the *Methods*. The passage on statistics as a branch of applied mathematics (quoted in section 1 above) echoes the concluding paragraph of the review; on the next page Fisher (1925a, 2) writes:

> 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 "pain" may not have been wholly Keynes's doing, for by 1925 Fisher had troubled relations with the economist officers of the Royal Statistical Society; see Box 1978, 86–87, and section 10 below.

The *Treatise* may have turned away "in disgust" some students of economics but some economists—foreign ones at least—grasped Fisher's "truth." In 1929 Harold Hotelling went to Fisher to learn, as Henry Ludwell Moore had gone to Pearson twenty years before, and Mordecai Ezekiel consulted Fisher over *Methods of Correlation Analysis* (1930), which from Pearsonian origins became the first book after Fisher's own to present *t*- and *z*-tests; for more on Fisher's relations with Ezekiel and Hotelling, see Aldrich 2000. In the fifth edition of *Statistical Methods* of 1934, "the painful misapprehension" was softened to "the unfortunate misapprehension." In a 1936 letter to the mathematician A. C. Aitken he was still complaining that in universities statistics "is often absurdly confused with economics" (Bennett 1990, 2). By the late 1930s the econometricians' regression was Fisher's regression—see Aldrich 2005a for the Fisher transformation of regression—and Koopmans (1937) was even thinking like Fisher and applying maximum likelihood to errors in variables. (For a general account of how the ideas of Fisher and other statisticians entered econometrics, see Aldrich 2007a.)

Keynes the economist figures in some correspondence between Fisher and Leonard Darwin in 1931 (Bennett 1983, 141). Fisher was preparing an article on family allowances, a topic of some interest to eugenicists, and Darwin suggested some economic reading, including an article by Keynes. Fisher replied, "[I] heartily condemn his one incursion into theoretical statistics. But he does write well, and is wonderfully clever in characterizing different points of view" (Bennett 1983, 142).

Fisher and Jeffreys collided when the latter started doing statistics in his objective Bayesian way—see section 7 above; see also Howie 2002 and Aldrich 2005a. After a frustrating exchange, Jeffreys (1933, 532) decided he "should explain why a theory of probability is necessary, and what is the scope of such a theory." He had covered the ground in 1919 and 1931 but this time he put more emphasis on the principle of non-sufficient reason (528), as though this were the most contested point. Jeffreys quoted a strong prima facie argument against the principle from page 44 of Keynes's *Treatise*:

> If . . . we have no information whatever as to the area or population of the countries of the world, a man is as likely to be an inhabitant of Great Britain as of France. And on the same principle he is as likely to be an inhabitant of Ireland as of France. And on the same principle he is as likely to be an inhabitant of the British Isles [Great Britain plus Ireland] as of France. And yet these conclusions are plainly inconsistent. For our first two propositions together yield the conclusion that he is twice as likely to be an inhabitant of the British Isles as of France.

Jeffreys argues that the contradiction is only apparent because the conclusions are based on different information. However, criticizing Keynes on this point was no way to establish common ground with Fisher, who agreed with Keynes and went further in comprehensively rejecting the principle: "As a succession of writers has shown . . . this supposed principle leads to inconsistencies which seem to be ineradicable" (1934, 4). Fisher would have included himself in that succession on the strength of his "Mathematical Foundations" (1922) and perhaps even his "Criterion for Fitting Frequency Curves" (1912). Teddy Seidenfeld (1995, 44) suggests that the Fisher-Jeffreys disagreement over the Keynes argument reflects two "more substantial" themes in Keynes's work that divide Jeffreys and Fisher: (1) that the logical probability between pairs of propositions is not always defined; and (2) that in part 5 of the *Treatise* Keynes tries to ground statistical inference on empirical premises only.

10. "Flimsy throughout"—Keynes on Fisher

On one occasion Keynes reviewed Fisher—it was for the Royal Statistical Society. Although Fisher was the main force behind the reconceptualizing of statistics—see section 1 above—he was not really in the society during the most critical period. He had become interested in the society after Pearson rejected one of his papers for *Biometrika*; he published two articles in the *Journal* in 1922 but he left the society when it rejected another; see Box 1978, 87, and Bennett 1983, 76–77. In 1929 Darwin engineered Fisher's reentry—see Bennett 1983, 103–4—but Fisher took no part in the society's activities until 1934 when he was elected to the society's council. He was asked to present a paper, an invitation he interpreted as a conciliatory gesture.

Fisher's paper reviewed the direction of his thinking since he had last communicated with the society:

> I have called my paper "The Logic of Inductive Inference." It might just as well have been called "On making sense of figures." For everyone who does habitually attempt the difficult task of making sense of figures is, in fact, essaying a logical process of the kind we call inductive, in that he is attempting to draw inferences from the particular to the general; or, as we more usually say in statistics, from the sample to the population. (1935, 39)

Fisher chose the word *logical* because he wanted to consider the principles of statistical inference rather than describe the mathematical theory. By "inductive inference" he meant simply inference from sample to population.

Keynes was a member of the society's council and an authority on the paper's ostensible subject. After reading the paper, he wrote to the assistant secretary, Catharine Thorburn:

> I am sorry to say that I have not been able to make very much of Dr Fisher's paper. It seems to me that the logical concepts from which he sets out are far from clear and, indeed that the logical treatment (as distinct from the mathematical) is somewhat flimsy throughout.
>
> Perhaps, however, one is rather misled by the title of the article, for assuredly it is a very minor contribution, if any, which this paper makes to the Logic of Inductive Inference. Its real interest seems to me rather different. It is in effect an account of the sort of way in which a statistician of Dr Fisher's experience uses his intuition when he believes himself to be working within a field in which the Gaussian law probably operates more or less though not with accuracy.
>
> At any rate, apart from some of the introductory remarks, it is really within this more restricted field that the paper moves and not in the more general field of inductive inference as indicated by the title.
>
> I should hesitate to suggest that a paper of Dr Fisher's should not be accepted, and I do not do so. But I ought, perhaps, since I have been asked to report, to set down as I have done above such reserves as I feel about it. The Council should not infer from the above that the paper should not, in my opinion be accepted. But I do think that it is not quite what it purports to be. (1934)

For a philosopher something has to explain the success of the unreflective practitioner, but *intuition* and *experience* were not terms Fisher liked being applied to his own work—even when meant as compliments.

Fisher presented the paper but Keynes did not attend the meeting. It proved a disastrous exercise in peace-making; Fisher's (1935, 76) reply to the discussion begins, "The acerbity . . . with which the customary vote of thanks has been moved [by Bowley] and seconded . . . does not, I confess, surprise me." Fisher next presented a paper to the society in 1953 as its president.

Keynes returned to the logic of inductive inference when he reviewed Tinbergen's econometric study of investment. He made the link with his earliest work: "Thirty years ago I used to be occupied in examining the slippery problem of passing from statistical description to inductive generalisation in the case of simple correlation; and today in the era of multiple correlation, I do not find that in this respect practice is much improved" (1939, 317). The contrast is not quite accurate for, while Keynes wrote only about simple correlation, multiple correlation was already well established. Like everyone, Tinbergen was using Fisher's regression methods but, from Keynes's point of view that would have been a detail and Fisher's name did not come up in the review or in the correspondence around it. Paradoxically, Fisher was more involved in econometrics than Keynes but less interested. Keynes's debate with Tinbergen has been discussed many times; see, among others, Patinkin 1976, Stone 1978, Bateman 1990, and Hendry and Morgan 1995, as well as a useful afterword by O'Donnell (1997). As noted in section 4, Keynes suggested that Tinbergen perform regressions for each decade to consider whether they differed from the regression for the entire series.

Keynes disappears from Fisher's writing and (published) correspondence to reappear in his 1956 book *Statistical Methods and Scientific Inference*. Of Fisher's books, this most resembles the *Treatise* except that it is the work of an angry old man demanding acceptance for principles he had been applying for more than thirty years and not a young man outlining a theory to be constructed. Like the *Treatise*, it presents a string of previous attempts before presenting its own but the historical scholarship is lighter and probably owed something to the older work, as in the treatment of Laplace.

Of course Fisher did not agree with Keynes but he was polite in dissent: "Even so shrewd a writer as Keynes has exposed himself to this criticism" (1956, 44), and he chose a quotation from the *Treatise* as an epigraph for his own book. Between 1921 and 1956 Fisher's attitude to Keynes and the Keynes-Jeffreys line on probability changed—which is why it is so variously reported by Conniffe (1992), Seidenfeld (1995), and Stigler (2002). The infant statistical science had not been smothered and the present danger was from the Neyman-Wald version of frequentist statistics. (The shift of econometricians to the Neyman-Wald camp is familiar from Morgan 1990, chap. 8.) Again the appearance of a third party made the disagreement between the original parties seem smaller—in the 1930s it was Jeffreys who had made Keynes and Fisher seem not so antipodal. Another possible extraneous influence was Fisher's friendship with Roy Harrod, Keynes's friend and biographer. Fisher and Harrod were drawn together in the 1940s by eugenics and their concern with the falling British birthrate. In the preface to the second edition of his *Economic Essays* published after Fisher's death Harrod writes how the thinking of "the great R. A. Fisher" had influenced him.

11. Keynes and the Statisticians

In *The Principles of Probability* Keynes considered a number of problems in statistical theory. His approach was Bayesian and, though he brought his treatment of averages to a successful conclusion, correlation/association defeated him. He knew that the existing work did not meet his standards but he never had the time, technique, or inclination to produce a satisfying account. When he joined the Royal Statistical Society in 1909 he was drawn to three areas of statistics: index numbers, statistical methods, and the philosophy of statistics. Index numbers were part of the technique of traditional monetary theory and Keynes stayed with them as long as he stayed with that theory; the culmination, both high point and finish, was the "Value of Money," part 2 the *Treatise on Money* of 1930. In methods and philosophy the *Principles* gave him a flying start and this work promised to complement the new technique-driven statistics of Yule. Keynes did not continue with statistical methods but he had a more lasting relationship with the philosophy of modern statistics, working on the foundations of statistical inference in 1911 and again in 1920–21 when he was giving the *Treatise* a final polish. In the *Principles* Keynes had looked for a Bayesian grounding for correlation but in the *Treatise* he was more concerned with an inductive grounding for correlation.

Once the *Treatise* was out, author and work largely went separate ways—"I shall write no more philosophy having at the age of thirty-seven reached a time of life when the brain relents and easier subjects recommend themselves," he wrote in a draft preface (Moggridge 1992, 364). Because of Ramsey, his interest in probability was maintained but there was nobody in Keynes's circle with the same interest in statistical theory, not to say a "Circus" to develop its ideas. Ramsey was actually inclined to allow the statisticians their own probability—see Zabell 1991. After the publication of the *Treatise* Keynes contributed only one article to the *Journal*, a centenary tribute (1936) to Jevons. Yet, while he stopped working in statistics, he did not lose interest in the subject or feel he had become out of date; in reviews for the *Economic Journal* he indicated the kind of statistical work he thought worthwhile, as in his 1928 review of Mills and 1929 review of Warren and Pearson—in the latter Keynes went back to his youth and enthusiastically added median calculations of his own. Later he reviewed Funkhouser on the graphical representation of statistical data (1938) and wrote an essay on "Professor Tinbergen's method" (1939).

The *Treatise* became a standard work on the "logical foundations of probability," but it was not a standard work in statistics or mathematics. Statisticians and mathematicians were aware of it but considered it not for them, as Coolidge (1925, 3) wrote early on: "It is perfectly evident that a line of reasoning which starts from the premiss that a certain subject is non-mathematical, is not a good introduction to a mathematical treatment of that subject." Maurice Kendall, a Cambridge mathematician who turned into a frequentist statistician with an interest in the foundations of probability, was just as unwelcoming in the *Advanced Theory of Statistics* (1943, 165):

> Several writers have explored the more general problem, foreshadowed as early as Leibniz, of developing a logic of probabilities, and the reader who is interested may refer to the work of Keynes, F. P. Ramsey and Johnson. From the statistical viewpoint the interest of the subject centres on the numerical theory of probability, which alone will concern us in this book.

In the 1930s and 1940s Cambridge mathematics undergraduates, the successors of Keynes and Fisher, were drawn to logic and the foundations of mathematics and some of this interest extended to Keynes's book. Bartlett (1933) referred to it when he tried to arbitrate between Jeffreys and Fisher. I. J. Good "read it religiously even in queues for shows," remarks Howie (2002, 97). Good was unique in making a career of combining statistics and philosophy but Keynes's book was well known in the underworld of probability.

The logic of probabilities was visible to statisticians and "principal averages" became a very minor part of their subject. The part of the book that disappeared was the critique of statistics and the presentation of the "constructive theory." The "others" to whom Keynes (1921, 457) left the task of taking forward the analysis of statistical induction never came forward. Students of Keynes are sometimes puzzled by his failure to establish the problem of statistical induction and his method of treating it; for instance, Bradley Bateman (1990, 378) posed a string of questions that "need to be answered by historians of econometrics":

> Why is the Keynes-Tinbergen debate the only part of Keynes's contribution with which we are widely familiar? Why were Keynes's criticisms in that debate largely ignored? Why did Keynes's original approach to examining inductive arguments not become more widely known?

We have seen how Keynes's original approach was met by the statistical reviewers with a mixture of indifference and hostility and why they did so; the approach was part of a wholesale attack on statistical method and, although Keynes served an apprenticeship as a statistician, his starting point was so far removed from theirs that he never treated what they considered problems. Only Bowley addressed Keynes's criticisms and he considered them irrelevant in the kind of work he did. They had a relevance for there is a problem of statistical induction that even the new Fisherian methods did not address: deciding that a manure was effective in a particular trial in a particular field at Rothamsted leaves open the question of its effectiveness on other fields at other times.

The *Treatise* has grown into another role, as historical reference work, especially on early probability, a companion to Todhunter's *History*, a book designed for quite a different purpose. E. S. Pearson (1925) mentioned the *Treatise* in connection with Bayes's theorem, and the only other reference in *Biometrika* before 1940 was when John Wishart (1927, 4) noted it for its bibliography on the same subject. When modern historians, like S. L. Zabell (1989) and Andrew Dale (1999), write on the topics covered in the *Treatise*, they take its scholarship seriously; even Stigler (2002, 161) recognizes Keynes's "wonderfully wide reading in the early history of probability."

This has been a long account of a career that wasn't, a Continental turn nobody followed and a critique that did not even become a footnote in the methodology of statistics. The career that was was in economics. Of course this had a statistical dimension; on Keynes's death, Ralph Hawtrey (1946, 169) recorded for the *Journal*, "No economist was ever more alive to the need for a statistical foundation for economic policy." Volume 2 of Keynes's *Treatise on Money* contains a "quantitative study of the facts"; this is a theoretically informed narrative of British monetary statistics rather than a study of regularities such as had been attempted by Irving Fisher. However, Keynes (1930b, 177–84) tried hard to explain one of Fisher's correlations: the "Gibson paradox" is "one of the most completely established empirical facts within the whole field of quantitative economics." Volume 2 of the *Treatise on Money* was Keynes's most extended exercise in doing things with figures. Hawtrey's theme has been developed by Don Patinkin (1976) and Stone (1978). Stone characterized Keynes as an applied economist who "liked to get a feel of the order of magnitude of the problems with which he was dealing" (64) and showed where this led him as a political arithmetician and as a campaigner for better economic statistics. Patinkin and Stone disagreed about the magnitude—possibly even the sign—of Keynes's contribution but not about his industry.

The treatment of Keynes in the standard reference works on statistics reflects his ambiguous position in the subject—and perhaps the ambiguous nature of the subject; in 1946 the Economic Society objected to the Royal Statistical Society's plan to organize examinations on the grounds that "the term 'statistician' is not a definite expression" (Plackett 1984, 141). There is no Keynes entry in the *Encyclopedia of Statistical Science*, and the one in the *International Encyclopedia of the Social Sciences* by the subjective Bayesian theorist D. V. Lindley (1968) concentrates on the probability system, casting it as Jeffreys without the numbers, and, like Jeffreys, seeing little gain in the generalization. Both encyclopedias are written from a modern post-1934 view of statistics. The *Statisticians of the Centuries* volume interprets "statistician" less narrowly and the Keynes entry, O'Donnell 2001, makes Keynes's role in the society and its recognition of his contribution more understandable.

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