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in the History of Statistics and Probability

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Contents

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Introduction by the compiler

Notation

Notation **S, G,** *n* refers to downloadable file *n* placed on my website www.sheynin.de which is being diligently copied by Google (Google, Oscar Sheynin, Home. I apply this notation in case of sources either rare or translated by me into English.

General comments on some items

I

Oscar Sheynin

Reviews

published in a few sources over many years

I list those published in several sources including the Russian reviewing journal *Matematika* and the *Novye Knigi za Rubezhom* (NKzR). The NKzR had been a highly reputable periodical publishing book reviews only. Its (former?) existence shows that reviewing can after all be a serious scientific pursuit.

Amunàtegui, Golodefredo Iommi: À propos d'une lettre de Pascal à Fermat. Rev. Quest. Sci. 175, 429 – 433 (2004)
The author considers Pascal's letter of 24.8.1654 to Fermat concerning the problem of points (of determining the division of stakes in an interrupted game). The game ends after one gambler wins the agreed number of sets. The maximal possible number of sets still left can also be considered. However, as Pascal noted, in case of three gamblers two of them can *win*; example: with score 4:3:3 this can happen in the remaining three possible sets. The author perceives here a general philosophical principle which can somehow help to discern the *choses angéliques* and the *choses plates et communes* in the Scripture.

Zentralblatt MATH 1067.01004

Armatte, Michel: Lucien March (1859 – 1933). Une statistique mathématique sans probabilité? J. Électron. Hist. Probab. Stat. 1, No. 1, Article 1, 19 pp. (2005)
March graduated from the École Polytechnique, for many years headed the Statistique Générale de France, was President of the Société de Statistique de Paris (1907) and initiated the establishment of the Société Française d'Eugenique.

He applied statistics to economics (partly following Pareto), studied economic barometers and was the main French partisan of Pearsonian ideas and methods (and translated Pearson's *Grammar of Science* into French). March objected to stochastic interpretation of the movement of prices, but, in philosophy of science, upheld the primacy of contingency. And in statistics, like many other statisticians of the time, he came out against probability theory (but did not deny mathematical methods in general); in this connection, Armatte mentioned "l'impression d'éclectisme".

The author wrongly stated that Poisson had applied Quetelet's concept of the *homme moyen* and did not say that the main objections to probability during that time was the absence of *equally possible cases* in statistics (rather than lack of normality). That Jakob Bernoulli had long ago made this opinion worthless was somehow forgotten.

Zentralblatt MATH 1062.01014

Atiqullah, M.: Statistics education in Pakistan. Pakistan J. Stat. 11, 219 – 225 (1995)

The development of statistics in Pakistan is traced back to the impact of Fisher and Mahalanobis (1943). In all, Pakistan now has about 40 Ph. D.'s in statistics *or allied subjects* with some 12 universities offering Master degrees in statistics. Further promotion, as the author remarks, hardly depends however on scientific achievement and the general public underestimates the role of statistics. The author also formulates recommendations about the necessary changes in the system of statistical education.

Zentralblatt MATH 864.01006

Barbut, Marc: Machiavel et la praxéologie mathématique. In: Martin, Thierry, ed., Mathematics and Political Actions. Historical and Philosophical Studies on Social Mathematics. Paris: INED, 43 – 56 (2000)

This paper first published in *Mathématiques, informatique et sciences humaines* 37, 19 – 30 (1999) reproduces some passages from the author's note of 1970. It describes Machiavelli (1469 – 1527) as a forerunner of the decision theory, mostly on the strength of his opinions about the conduct of war, and quotes many passages from the works of his hero.

The author attributes to Machiavelli the three main aspects of decision making (but not their methodical discussion): knowledge of facts; their evaluation; and rules of conduct. He stresses Machiavelli's sound reasoning, quotes as pertinent Laplace's definition of the theory of probability, – *le bon sens mis en calcul* (which could have described the early 19th century mathematics in its entirety), – and several times uses such expressions as *conséquences probables* although without ascribing them to Machiavelli. The author also credits Machiavelli with the *règle du moindre mal* and cites him as saying that, in spite of *fortune*, man can *govern* about a half of his *oeuvres*.

Tolstoy ridiculed the excessive attention to decision making, – the preparation of a monster disposition of the Austrian and Russian armies for the Battle of Austerlitz (which they lost), see his *War and Peace* (misnomer! Correct translation of title: *War and Society*), pt. 1, section 58.

Zentralblatt MATH 1097.01017

Barbut, Marc: Une application de l'algèbre linéaire. Le calcul des probabilités. Math. Sci. Hum. 150, 81 – 98 (2000)

Regarding an almost identical version of this paper, see M. Serfati, Editor, *La recherché de la vérité.* Paris, 1999, pp. 97 – 116. Without repeating its abstract I note that the author axiomatically introduced the notion of expectation and claimed that he thus relegated the Kolmogorov axioms of the theory of probability to theorems. Huygens proved that expectation was a "just" criterion for solving stochastic problems. Jakob Bernoulli upheld that viewpoint but later scholars have been introducing expectation without formal substantiation. However, many authors attempted to justify the similar notion of arithmetic mean by deterministic axioms and Gauss regarded the first such effort (J. F. Encke, 1831) "nicht ohne Interesse". This information is not provided by Barbut. Then, he did not mention the

Kolmogorov axiom of continuity that deals with an infinitely large number of events and his claim is therefore dubious.

Zentralblatt MATH, 990.01004

Basharin, Gely P.; Langville, Amy N.; Naumov, Valeriy A.: The life and work of A. A. Markov. Linear Algebra Appl. 386, 3 – 26 (2004)

This is a careless essay on Markov's life and on his work in probability theory. Repeating mistakes made by previous contributors, the authors believe that Tolstoy (who died in 1910) was excommunicated from the Russian Orthodox Church in 1912 (actually in 1901) and they attribute to Markov rather than to Pushkin the verse (not limerick) "Count (not Duke!) Dundook". They also state that Markov "implicitly accused" Chebyshev of plagiarism; actually, of failing to cite his predecessors. Some inaccuracies are also present and the references are given without page numbers which makes it difficult to check the provided formulation of Markov's findings. Missing references include important papers by Markov Jr and Linnik et al. Describing Markov's correspondence with Chuprov, the authors were unaware that in 1996 I published a book on Chuprov containing newly found letters between these scholars.

Zentralblatt MATH, 1049.01014

Bellhouse, David: Decoding Cardano's Liber de Ludo Aleae. Hist. Math. 32, 180 – 202 (2005)

The author describes Cardano's educational background in the context of the state of mathematical learning of his time and examines his *Liber de Ludo Aleae* (written in mid-16th century, first published 1663, English translation 1953). He argues that that book was based on the anonymous poem *De Vetula* (ca. 1250) and that Cardano's aim was to establish conditions under which games of chance might be approved (as opposed to their flat rejection by Aristotle) rather than to compile a mathematical tract. Consequently, as the author remarks, Cardano's mathematics is faulty but notes that Aristotle's concept of justice led him to state that the ratio of the wagers of two gamblers ought to be equal to that of their chances of winning (e. g., that their expected winnings be equal).

Zentralblatt MATH 1072.01008

Bernoulli, Jakob: Wahrscheinlichkeitsrechnung (Ars Conjectandi). Mit dem Anhänge Brief an einen Freund über das Ballspiel. Translated by R. Haussner. Ostwalds Klassiker 107/108. Frankfurt/Main: Deutsch (1999). (Reprint of the translation of 1899.)

Bernoulli's Latin book, *Ars Conjectandi*, and his French piece, *Lettre … sur les parties du jeu de paume*, were published posthumously in 1713. They both, together with related material including the probability-theoretic part of his *Meditationes* [Diary], are now available in their original language in Bernoulli's *Werke*, Bd. 3 (Basel 1975). Pt. 2 of the *Ars* was translated into English (1795), and pt. 1, into French (1801); pt. 4 exists in Russian (1913 and 1986), and an English (1966) and a French (1987) version, and the entire *Ars* was translated into German (1899), – together with the *Lettre*, but did not appear in any other living language.

The *Ars* contains a reprint of Huygens's treatise on probability (1657) with essential comment (pt. 1); a study of combinatorial analysis where the author introduced and applied the *Bernoulli numbers* (pt. 2); solutions of problems concerning games of chance (pt. 3); and, in pt. 4, an attempt to create a calculus of stochastic propositions and the proof of the law of large numbers (LLN) with an unfulfilled promise of applying the law to *civil, moral and economic issues*. For a large number of observations, the LLN established parity between theoretical and statistical probabilities (i. e., between deduction and induction) and thus furnished a foundation for statistical inquiries. Being unable to use the still unknown Stirling formula, Bernoulli had not provided a practically effective law, and Karl Pearson (1924) harshly and unjustly commented on this point. Niklaus Bernoulli adduced a preface to the *Ars* (omitted from the translation). Before that, in 1709, he borrowed from the text (and even from the *Meditationes*, never meant for publication). In his *Lettre*, Bernoulli calculated the players' expectations of winning in different situations of the game.

The translator commented on the texts and adduced helpful information about the history of probability and Jakob's contributions.

Zentralblatt MATH 957.01032

Bernoulli, Jacob: *The Art of Conjecturing* together with *Letter to a Friend on Sets in Court Tennis*. Translated with an introduction and notes by Edith Dudley Sylla. Baltimore, 2006

Jakob (as spelled in his native tongue rather than in Latin) Bernoulli died in 1705 and his unfinished *Ars Conjectandi* was published in 1713 together with his French piece, *Lettre à un amy sur les parties du jeu de paume*. Strangely enough, these titles do not appear on the reverse of the title-page of the book under review. Both, as also the stochastic part of his *Meditationes* (Diary, not published previously), are now available in their original languages in Bernoulli's *Werke* (1975) which also contains related materials. The entire *Ars* and the *Lettre* were rather freely translated into German (1899) with interesting comments and the most important part of the *Ars* (pt. 4) was translated into Russian (1913, second edition, 1986) and French (1987) and I myself rendered it into English and commented on it (2005). The second Russian edition contains three commentaries (my general overview; Yu. V. Prokhorov's "The law of large numbers and the estimation of probabilities of large deviations" and Jakob Bernoulli's biography by A. P. Youshkevich).

Pt. 1 of the *Ars* is a reprint of the Huygens tract of 1657 (likely reflecting the fact that Bernoulli had not completed his work) with essential comment. Note that this tract is thus also available in English. Pt. 2 is a study of combinatorial analysis and it is there that Bernoulli introduced and applied the *Bernoulli numbers*. Pt. 3 is the application of this analysis to games of chance (which were also the object of pt. 1, where, however, combinatorics was not needed). This part is not sufficiently known; the early history of these games is usually associated with other authors, from Pascal and Fermat to De Moivre.

Pt. 4, whose title promised to describe applications of the "preceding doctrine", contains nothing of the sort (and any applications should have been discussed in a separate part). As it is, pt. 4 is an attempt to create a calculus of stochastic propositions and the proof of the (weak) law of large numbers (LLN; Poisson's term) and it also contains Bernoulli's reasoning on certainty, probability, contingency, a somewhat informal definition of probability (not applied in the sequel), and a definition of the "art of conjecturing or stochastics" (p. 318 of the present translation). This is "the art of measuring the probabilities of things as exactly as possible" for choosing what "has been found to be better, more satisfactory, safer, or more carefully considered".

When combining his stochastic propositions, Bernoulli tacitly (since he did not introduce probabilities here) applied the addition and the multiplication theorems. These probabilities were non-additive; thus, in one of his examples a certain proposition and its opposite had 2/3 and 3/4 of certainty respectively. Such probabilities began to be studied beginning with Koopman (1940). Bernoulli possibly thought of applying his calculus of propositions in this unfinished part.

For a large number of observations, the LLN established parity between theoretical and statistical probabilities (between deduction and induction; the latter probability occurred to be a consistent estimator of the former) and thus provided a foundation for statistical inquiries. Indeed, Bernoulli attempted to ascertain whether or not the statistical probability had its "asymptote", – whether there existed such a degree of certainty, which observations, no matter how numerous, would never be able to reach. In such case "it will be all over with our effort" (pp. 328 – 329).

Being, however, unable to use the yet unknown Stirling formula, and overlooking the possibility of somewhat weakening his assumptions and strengthening his intermediate inequalities, Bernoulli had not provided a practically effective law, and Karl Pearson (1925) harshly and unjustly commented on this point.

In the last lines of his *Ars* Bernoulli actually and without any justification discussed the inverse problem: if observations were to continue "the whole of eternity", then "in even the most accidental and fortuitous we would be bound to acknowledge a certain quasi-necessity and, so to speak, fatality" (p. 339). In other words, he stated that the theoretical probability determines its statistical counterpart. De Moivre (1756, p. 251) made a similar declaration and only Bayes clearly perceived the difference between the two problems and derived with proper precision the theoretical probability given its statistical value for the finite and, actually, infinite cases. I hold therefore that, together with the De Moivre limit theorem, his memoir of 1764 completed the creation of the first version of the theory of probability.

The *Lettre* is a study of probabilities in a complicated game depending both on chance and skill. I doubt that it is of general interest.

The translation provides a general picture of the *Ars*, but its mathematics is often wrong, doubtful or incomprehensible. Difficult points are not explained (pp. 329, 168 – 169 and 308). In the two last cases Bernoulli's wrong term *logarithmic* (instead of *exponential*) curve persists, and on p. 208 appears a mysterious binomial root. On p. 324 Bernoulli's *product of cases* should have been replaced by *product of the number of cases*; even a classical scholar (who Sylla undoubtedly is) should have noticed this mistake. And on p. 198 Bernoulli's statement that the number of stars is "commonly set at 1022" is left without comment; actually, we see about six times more with a naked eye.

References are numerous but reprints of most important sources (Montmort, De Moivre, Bayes) are not mentioned. In a nasty tradition, the dates of publication of some memoirs (Arbuthnot, Bayes) are not provided and two names (Couturat, Kendall) are misspelt. The listing of the first edition of the Russian translation of the *Ars* is a fabrication, pure and simple, and thus undermines Sylla's integrity, and a wrong statement about its being rendered from a French translation (then not yet existing) is tentatively repeated. The second edition of the Russian translation is not listed.

Sylla's Introduction, notes and comments take up ca. 160 pages. She describes the history of the Bernoulli family, Bernoulli's life and his studies of logic and his religious views and relations with contemporaries. However, probabilism, the medieval doctrine according to which the opinion of each theologian was probable and which can be linked with non-additive probabilities, is not mentioned. Also missing is a discussion of a most influential book Arnauld & Nicole (1662). In a sense, it was a non-mathematical background for Bernoulli. Hardly anything is said about the rapidity of the convergence in the LLN or about its importance or further history and many facts are simply wrong (De Moivre's attitude to the Huygens method of solving stochastic problems; his relations with Newton; his criticism of Niklaus Bernoulli). Daniel Bernoulli's theorem on fluid dynamics is attributed to Niklaus (p. ix) and Jakob Bernoulli's proof of the LLN "is mathematical, not scientific" (p. 43) and neither is his art of conjecture "scientific" (p. 109). We also ought to know that the *Ars*, together with previous work, "was part of the pre-paradigm stage" whereas De Moivre "established the paradigm of … mathematical probability" (p. 58), whatever all this means. And, apart from some of the topics listed in the beginning of my last paragraph (history of the Bernoulli family etc.), Sylla's Introduction and comments are best ignored. She corroborated the old saying: *Ne sutor ultra crepidem*! (Cobbler, stick to your last!).

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--- (2005), *On the Law of Large Numbers*. Berlin. This is my translation of pt. 4 of the *Ars*.

De Moivre, A. (1756), *Doctrine of Chances*, 3rd edition. New York, 1967.
Koopman, B. O. (1940), The bases of probability. *Bull. Amer. Math. Soc.*, vol. 46, pp. 763 – 774.
Pearson, K. (1925), James Bernoulli's theorem. *Biometrika*, vol. 17, pp. 201 – 210.
Hist. Scientiarum (Tokyo), vol. 16, 2006, pp. 212 – 214

Bernstein, S. N.: Chebyshev's influence on the development of mathematics. Transl. by O. Sheynin. Math. Scientist 26, 63 – 73 (2001)

The original Russian version written by a leading Soviet mathematician and member of the Paris Academy of Sciences appeared in 1947. The author describes Chebyshev's biography, indicates the main features of his scientific method (unification of theory and practice; no inclination to general studies without concrete aim; clever use of elementary methods and tricks) and discusses his work. Main attention is given to such fields as distribution of prime numbers; the theory of orthogonal polynomials; creation of the constructive theory of functions; theory of probability. A short comment on the work of two students of Chebyshev, Markov and Liapunov, concludes the author's account.

Zentralblatt MATH 991.01018

Bolobás, Béla: Paul Erdös and probability theory. Random Struct. Algorithms 13, 521 – 533 (1998)

Erdös was born in Hungary and worked in England and the USA; after 1954 he became a *wandering scholar* officially residing in Israel. He wrote about 1,500 papers (many still unpublished) and his main achievements pertained to number theory; combinatorics; interpolation theory; set theory; theory of probability. Together with his co-authors (who numbered, in all, about 500) he created probabilistic number theory, the theory of random graphs and extremal graph theory. In probability theory he sharpened the law of iterated logarithm (discovered by Khinchin rather than by Kolmogorov, as the author mistakenly remarked), and, together with M. Kac, he proved several versions of the central limit theorem and made important findings concerning the arc sine law.

Zentralblatt MATH 960.01009

Brady, Michael Emmett: J. M. Keynes' position on the general applicability of mathematical, logical and statistical methods in economics and social science. Synthese 76, 1 – 24 (1988)

The author holds that concerning the use of mathematics in economics Keynes objected to the particular misuse of certain methods rather than to the general use of quantitative methods. Among his arguments is a quotation from Keynes who declared that *mathematical reasoning now appears as an aid in its symbolic rather than its numerical character*. He also notes that Keynes' general approach is indirectly supported by the failure to improve political forecasts, or to help to explain past political events, by straightforward applications of game theory. The author claims that Keynes anticipated some modern conclusions according to which statistical analysis cannot be applied in economics just as in natural sciences.

Zentralblatt MATH 647.90020

Bru, Bernard: Doeblin's life and work from his correspondence. In: Cohn, Harry, ed. Doeblin and Modern Probability. Proc. Doeblin Conf. 1991 Univ. Tübingen Heinrich Fabri Inst., Blaubeuren, Germany. Contemp. Math. 149, 1 – 64 (1993)

The paper is based on archival sources and contains a biography of Wolfgang Doeblin (1915 – 1940) with a description of his work, both published or not, and contacts with the leading specialists in probability of his time (Fréchet, Lévy, Kolmogorov, Doob); and with extensive notes and bibliography including a list of Doeblin's papers reprinted from Loève (1963). Also appended is Doeblin's previously unpublished correspondence (letters to and from Fréchet, 1936 – 1940; to and from Lévy, 1938; and to Doob, 1938 – 1939). Among these letters is Doeblin's undated manuscript *Sur la solution de M. Hostinský de l'équation de Chapman*, and, among the notes, a passage from Kolmogorov's letter to Fréchet (1937) with a phrase *Doeblin doit publier sur les chaînes de Markoff indépendamment, comme il les inventées*. Being a Jew and a soldier in the French army in World War II, Doeblin shot himself rather than surrender.

Zentralblatt MATH 786.01014

Bru, Bernard: Poisson, the probability calculus and public education. J. Élecron. Hist. Probab. Stat. 1, No. 2, Article 1, 25 pp. (2005)

This is a translation with some comments (by Glenn Shafer assisted by Laurent Mazliak and José Sam Lazaro) of the author's essay *Poisson, le calcul des probabilités et l'instruction publique* from *Siméon Denis Poisson et la science de son temps.* Editors, M. Métivier et al. Palaiseau, 1981, pp. 51 – 94.

Bru provided an important account of Poisson, the probabilist and educator (1781 – 1840). It is set against the background of the French turbulent society of the time and written without due regard for non-French readers. The description (p. 11) of one of Fourier's lecture notes is faulty; his statement (p. 12) to the effect that, given enlightened specialists, statistical data are barely needed is attributed to Poisson, but only in a recent private communication; Poisson's influence on Chebyshev is not mentioned; and, finally, the Bibliography is substandard and the references lack page numbers.

Poisson began in 1811 – 1812 by non-remarkably abstracting Laplace's memoirs and his "Théorie analytique" and he misunderstood Laplace's loose presentation of the estimate of the population of France (not recorded by Bru). Later, Poisson had been following Laplace by filling in several missing points, explaining unclear circumstances and furthering his results. Thus, since Laplace had originated an academic method of least squares issuing from a large number of observations and drawing on his non-rigorously proven central limit theorem, Poisson continued in the same vein. To his own detriment, he never mentioned Gauss, let alone applied any of his results. This, however, Bru has not discussed.

Again, like Laplace (but unlike Lagrange), Poisson had subordinated methods of research to concrete applications. Together with a slipshod introduction of his most important law of large numbers, this led to his work being undervalued. As Bru commented, in 1881 no-one thought of celebrating his centennial.

Poisson continued Laplace's stochastic investigation of the sex ratio at birth and of the statistics of the criminal justice system introducing, as I note, the prior probability of the defendant's guilt (not to be applied to any given individual). He paid utmost attention to checking the significance of empirical discrepancies between the results of two long series of observations and thus became the Godfather of the Continental direction of statistics.

From 1820 to his death Poisson, the notorious unbeliever, had been member of the Conseil Royal de l'Instruction Publique and its treasurer since 1822. He proved himself indispensable and had been able to manoeuvre politically. The Conseil governed supreme over appointments, creation of positions, curriculums and sanctions, and, as treasurer, Poisson had to examine the accounts of all the royal colleges.

Bru reasonably explains the decline of French mathematics in the mid-century by its excessive centralization rather than by Poisson's personal or scientific traits.

Zentralblatt MATH, to appear

Bru, Bernard: The Bernoulli code (in French). J. Électron. Hist. Probab. Stat. 2, No. 1, Article 1, 27 pp. (2006)

This is the text of the author's report made in 2005 which he (p. 21) regards as a commentary on Cournot's first contribution to probability theory (1828, reprinted in 2005 in the same electronic journal and included in the forthcoming t. 11 of his *Oeuvres Complètes*). The text is anonymously supplemented by additional notes.

Bru (p. 2) attempts to *préciser* the Jakob Bernoulli's law of large numbers from the standpoint of *philosophie naturelle, du moins* as seen by Cournot. He delves into antiquity (Plato, Cicero) and the Renaissance (discussing , for example, a Latin book by Sébastien Castellion, 16th century, published in a French translation in Leiden in 1981 as *De l'art de douter et de croire, d'ignorer et de savoir*). Among later authors Bru dwells on Arbuthnot (without providing the date of the publication of his note) and Niklaus Bernoulli, but ignores Laplace's relevant explanation of the appearance of remarkable coincidences. In general, his text belongs to philosophy, certainly not to mathematics.

Bru fails to mention Niklaus' borrowing from Jakob Bernoulli's still unpublished book and even from his diary (Kohli, K., Kommentar zur Dissertation von Niklaus Bernoulli. In J. B., *Werke*, 3, 541 – 556. Basel (1975), see p. 541). He (p. 21) calls Stigler's *History of Statistics* (1986) a *beau livre* and, just like everyone else, passes over in silence Stigler's slanderous statements about Gauss (Sheynin, O., Gauss and the method of least squares, *Jahrbücher f. Nationalökonomie u. Statistik* 219, 458 – 467 (1999)). He also positively mentions Shafer's shallow paper (Zbl 0858.01014) and (p. 21) gives a wrong date for the reprint of one of Cournot's books which he himself edited. Finally, Bru does not explain Bernoulli's difficult phrase to the effect that his theorem illustrated the Platonist belief in the return of everything to its original position.

Zentralblatt MATH, to appear

Bru, Bernard; Bru, Marie-France; Bienaymé, Olivier: La statistique critiquée par le calcul des probabilités. Deux manuscrits inédits d'I. J. Bienaymé. Rev. Hist. Math. 3, 137 – 239 (1997)

The authors publish two manuscripts kept by Bienaymé's direct descendant and complement them with a foreword, extensive notes describing the French statistical scene of the mid-19th century, and bibliography. The text of the first manuscript is apparently a report on Bienaymé's communication which remained unpublished *par hasard*.

1) An *Extrait d'une communication à la Société Philomatique* [de Paris] of 1842 with its first five pages missing. It is devoted to philosophical problems in probability and to criticizing the Poisson law of large numbers.

2) A *Communication à l'Académie des sciences morales et politiques* of 1855. Here, Bienaymé again criticizes the law of large numbers and notes that the errors *d'observation ou d'expériences* do not always compensate each other even in large numbers.

Zentralblatt MATH 902.01008

Bernard Bru; Bru, Marie-France; Kai Lai Chung: Borel et la martingale de Saint-Pétersbourg. Rev. Hist. Math. 5, 181 – 247 (1999)

In addition to its main subject, this essay describes the related work and the biographies of Le Dantec (1869 – 1917) and Ville (1910 – 1989) and provides general information about Borel. It is based in part on archival sources.

Borel believed that the dissemination of mathematical knowledge was socially important even though his technique lagged behind his advanced ideas. In 1909, he non-rigorously studied the problem of the return to a draw in a long game of heads and tails which later gave rise to the arc sine law and led him to the strong law of large numbers. In 1911 Borel noted the connection of this problem with the Petersburg paradox to which he turned his attention in 1939 by applying the notion of martingale and proved that, by regulating the stakes at each round and choosing the moment for stopping, a gambler can make a fair play advantageous for himself.

The authors also touch on Le Dantec's non-recognition of the probability of a single event and his views on evolution theory, on Mises' frequentist theory, and on Borel's anticipation of the theory of games. When referring to books, they fail to mention the appropriate pages.

Zentralblatt MATH, 979.01018

Bru, Bernard; Jongmans, François; Seneta, Eugene: I. J. Bienaymé. Family information and proof of the criticality theorem. Intern. Stat. Rev. 60, 177 – 183 (1992)

Drawing on archival sources, the authors describe Bienaymé's biography. It occurs that it was due to lack of time and bad health that he was often unable to provide demonstration of his findings. From among Bienaymé's numerous eminent descendants at least two living persons are professors. The authors also dwell on the proof of Bienaymé's criticality theorem of the simple branching process which one of them (Bru) found in Cournot's *De l'origine et des limites de la correspondence entre l'algèbre et la géometrie* (1847; reprint 1989). They state that Bru is to publish a separate article on this proof. However, his contribution, *A la recherché de la démonstration perdue de Bienaymé*, has already appeared [*Math. Inf. Sci. Hum*. 114, 5 – 17 (1991)].

Zentralblatt MATH 759.01003

Bru, Bernard; Martin, Thierry: Le baron de Férussac, la couleur de la statistique et la topologie des sciences. J. Électron. Hist. Probab. Stat. 1, No. 2, Article 3, 43 pp. (2005)
This is an extract from a contribution on Cournot's participation in the *Bull. général et universel des annonces et des nouvelles scientifiques* (1823 – 1831) edited by André (Etienne Juste, or Just, Pascal Joseph François) d'Audebard, Baron de Férussac, 1786 – 1836, and usually called *Bull. de Férussac.* The contribution will be included in t. 11 of Cournot's *Œuvres Complètes*. Here, the authors' names only appear at the end of their detailed notes partly based on archival sources. They state that Cournot, an author of the *Bulletin*, had likely acquired from it his *culture scientifique*.

An officer (he rose to become lieutenant colonel) and a natural scientist, whose study of shells was positively reported by Cuvier in 1805 and 1812, Férussac only belonged to the academic fringe. This was caused by his general vision of science and personal traits. His main interest was the systematization and internationalization of science and its geographical distribution and the authors called his *Bulletin* the French World Wide Web of the time. It was published by those responsible in 8 sections, but Férussac, helped by one or two assistants, supervised all of them. The first embraced mathematics, astronomy, physics, and chemistry; in all, 16 of its volumes were published, and regarding their content the authors refer to R. Taton, *Arch. Intern. Hist. Sci.* 26, 100 – 125 (1947).

The sixth section was devoted to geography, *économie publique* and voyages, and, implicitly, statistics which was thus separated from probability. However, as the authors remark, Férussac would not have objected to philosophical probabilities (Cournot). During its first five years, the *Bulletin* published 80 thousand papers, partly by distinguished authors (I myself mention Poisson, 1830). The office of the *Bulletin* became the scientific centre of Paris and in general Férussac's activities *a accéléré le progress des sciences mathématiques au XIX^{e} siècle* (p. 15).

Being unsatisfied with university statistics and largely following Fourier, Férussac formulated the aims of social statistics and he also advocated the use of numerical tables and pictorial representation of data.

Zentralblatt MATH, to appear

Cantor, Georg: Historische Notizen über die Wahrscheinlichkeitsrechnung (read 1873; reprint 1932**). J. Électron. Hist. Probab. Stat. 2, No. 1b, Article 8, 11 pp. (2006)**
This is a reprint of Cantor's popular scientific report of 1873 from his *Gesammelte Abhandlungen mathematischen und philosophischen Inhalts, mit erläutern den Anmerkungen sowie mit Ergänzungen aus*

dem Briefwechsel Cantor – Dedekind, Hrsg E. Zermelo, A. Fraenkel. Berlin, Springer (1832), 357 – 367. This time, the reprint is accompanied by its French translation (Décaillot [see below]), and the bibliographic description given above is available only there.

Cantor dwells on the main heroes of probability calculus from Pascal and Fermat and Huygens to De Moivre, Laplace and Gauss without going into mathematical explanation. One point is obscure: on p. 362 Cantor properly states that Jakob Bernoulli proved his law of large numbers with "einige Beschränkungen", but (aber) that his proof can "vollkommen strenge gemacht werden". I am unable to understand the "aber" and I also note that Bernoulli's proof is generally considered unimpeachable.

Cantor also sets high store by Spinoza's letter of 1666 in which the philosopher applied expectation, but he is not sure whether Spinoza was acquainted with the Pascal – Fermat correspondence. However, J. Dutka [Spinoza and the theory of probability. *Scripta Math*. 19, 24 – 33 (1953)] stated that Spinoza was friendly with Huygens. Cantor does not mention Todhunter's (1865) classic on the history of probability which possibly means that that source had not been known in Germany.

Cantor had not contributed to probability calculus, which does not contradict his choice of the subject for his report. And it seems that he had not lost some interest in probability: he privately called Kronecker, who had been denying the emerging set theory, "Herr De Méré", see Fraenkel, A. G. Cantor. *Jahresber. Deutschen Mathematiker-Vereinigung* 39, 189 – 266 (1930), p. 199. Fraenkel also contributed an essay on Cantor included in the *Ges. Abh*., and there he (p. 459) repeated this remark.

The *Ges. Abh*. does not provide an exact date of the original publication of the report; it only mentions the *Sitzungsberichte der Naturforsch. Ges. Halle* 1873. At the time, these *Berichte* had been published together with the *Abhandlungen* of the said *Gesellschaft*. Bd. 12 of the *Abh*. only contains the *Berichte* of 1871; the (defective?) copy of Bd. 13 (1877) which I saw had no *Berichte* at all whereas Fraenkel (1930, see above) had stated that Cantor's report was published in 1877. I can only conclude: published in 1877 or even later.

Zentralblatt MATH, to appear

Celmins, Aivars: The method of Gauss in 1799. Stat. Sci. 13, 123 – 135 (1998)

In 1799, Gauss, proceeding from a meridian arc measurement separated into four parts, derived the parameters of the earth's ellipsoid of revolution without explaining his approach. The author unsuccessfully attempted to reconstruct the calculations and concluded that Gauss could have applied the method of least squares, but only if he made arithmetical errors. He also repeated Stigler's wrong statement claiming that, prior to Legendre's publication of 1805, Gauss hardly informed anyone of his invention of the method. The reviewer has refuted Stigler (who also dared to defame Euler), see *Hist. Scientiarum* 8, 249 – 264 (1999), where all the cases in which

Gauss could have applied the method of least squares before 1805 are also discussed.

Zentralblatt MATH 964.01023

Chatterjee, S. K.: H. K. Nandi's contributions to statistics – an appreciation. Bull. Calcutta Stat. Assoc. 40, 1 – 22 (1991)
In actual fact, this is a section of an obituary. It deals exclusively with Haru Kinkar Nandi's scientific work and contains a list of 47 of his publications. Nandi was active in diverse fields of mathematical statistics and generous in helping his students and colleagues by sharing his ideas with them.

Zentralblatt MATH 743.01026

Chebysheva, K. V.: Some information on ancestors and descendants of the Chebyshev family. Istor.-Matematich. Issled 32/33, 431 – 451 (1990). In Russian
According to the Chebyshev family tradition, its ancestor was one of the sons of the Tatar military leader Khan Chebysh. The family is mentioned in chronicles from the beginning of the 17th century. In the second half of that century three of its members received Tsar's charters for feats of arms and loyal service and the author appends their texts in her own translation into modern Russian. She also adduces information on the male posterity of Petr Lvovich Chebyshev, a brother of the great mathematician Pafnuty Lvovich, and states that the latter pronounced his name with a stress on the last syllable. She does not say anything about her own relation to the family.

Zentralblatt MATH 728.01016

A. Cournot, Exposition de la théorie des chances et des probabilités. Translated by N. S. Chetverikov. Editor of transl. A. L. Weinstein. Moscow, 1970. In Russian
Cournot (1801 – 1877) was an eminent French scholar. In this book, he discussed the theory of probability and its applications to statistics (population statistics in particular), theory of errors, natural sciences, jurisprudence. Laplace's writings made an extremely difficult reading, and a much more popular exposition was badly needed. Cournot's book answered this goal. However, he was also original. Only he (§18) offered a generalized definition of probability covering the continuous case as well. He (Préface and §§238 and 240) had argued that statistical probability was indeed important, and Mises [1, Einleitung] regarded him as one of his predecessors. Cournot's reasoning on posterior (Chapt. 8) and *philosophical* probabilities (Chapt. 17 and Résumé), unyielding to numerical estimation and based on the confidence in the simplicity of the laws of nature, deserves attention.

Cournot abandoned the *Laplacean determinism* and the subjective definition of probability and defined chance (Chapt. 4) as an intersection of independent chains of causative events. He investigated the statistical significance of discrepancies between empirical magnitudes by means of the De Moivre – Laplace limit theorem and was one of the first after Laplace who attempted to link directly statistics to probability.

Then, Cournot (Chapt. 6) explained the notion of density. Yes, Laplace widely applied density curves, but [apart from the treatment of observations] he restricted his studies to concrete problems; Gauss formally introduced these, but statisticians did not know his writings sufficiently. And it was Cournot who offered the first exposition of this topic suitable for a broader circle of readers. Following Gauss, he also directly discussed that parameter of the density which determines the variance of observational errors. Finally, half a century before Pearson and his school, Cournot (§171) mentioned a problem pertaining to zoology (longevity of individuals in the animal kingdom [elsewhere [2, §3.3.7] he discussed the evolution of species]).

In general, the book under review reflects the development of stochastic ideas from Laplace to Poincaré. Cournot acquainted his readers with contemporary work and especially interesting are his references to Bienaymé [under whose influence he passed over in silence Poisson's law of large numbers]. In Russia, his ideas were taken up by A. Yu. Davidov (1823 – 1885), Professor at Moscow University.

Some criticism is due with regard to historical information provided. Cournot (§47) states that *Les grands genies* of the 17th c. *n'avaient non plus en vue que la règle des parties*. Huygens, however, foresaw the origin of a *spéculation fort intéressante et profonde* [and studied stochastic problems in mortality]. In §88 he formulates a rule *attribuée à Bayes*, but due to Laplace. In the sequel, he unreservedly mentions the Bayes *rule* and *theorem*, and apparently it was he who introduced these wrong terms. And, when describing the history of the method of least squares, Cournot (§135) does not cite Gauss' most important memoir of 1823.

The translation is not free from inaccuracies and misprints. … Nevertheless, it is done conscientiously and supplemented by notes (written by the translator and the Editor) tracing the connections between Cournot and the later Continental direction of statistics (Lexis, Bortkiewicz, Chuprov, Markov). The introductory article is really interesting.

The classical literature of probability theory is difficult to come by. A number of Russian translations made at the beginning of this century (Jakob Bernoulli, Laplace, Poincaré) are only available at the largest libraries; the translations of Mises and Smoluchowski, as well as many writings of Kolmogorov, Bernstein and Khinchin also became rare. And, without the translations accomplished by a few enthusiasts (mainly by Chetverikov), important contributions of Lexis, Bortkiewicz and Chuprov would have remained hardly known. I wish Chetverikov to continue his noble activities in this direction.

1. Mises, R. von (1931), *Wahrscheinlichkeitsrechnung* etc. Leipzig – Wien.
2. Sheynin, O. B. (1980), On the history of the statistical method in biology. *Arch. Hist. Ex. Sci*., vol. 22, pp. 323 – 371.

Ekonomika i Matematich. Metody, vol. 7, 1971, pp. 635 – 636

Coumet, E.; Barbut, M.; Bru, B.: Le séminaire *Histoire du calcul des probabilités et de la statistique* (1982 – 1991). Math. Inf. Sci. Hum. 113, 57 – 75 (1991)

The Seminar under the direction of the three persons mentioned above was organized in 1982 by the Centres of Alexandre Koyré d'Histoire des Sciences et de Techniques, and of the Analyse et de Mathématique Sociales (Paris). The conferences are held twice monthly and speakers include both French and foreign researchers. Following appendices are adduced: 1. Chronological list of meetings (speakers and titles of reports). 2. List of scholars discussed. 3. Alphabetical list of speakers with titles of their reports and references to their subsequent publications.

Zentralblatt MATH 753.01015

Crombie, A. C.: Some general effects of mathematics on western natural philosophy. Istoriko-Matematich. Issled. 21, 22 – 50 (1976)

[This is my translation from English. Regrettably, the original bibliographical data is dubious and anyway incomplete: *perhaps*, the source is a chapter from the author's *Galileo and Mersenne* …, 1976.]

The author describes the standpoint of a number of scholars of the 12^{th} – 16^{th} centuries (Gundissalvi, Leonardo da Vinci, Ficino, Valla) regarding science and art, and, in particular, on mathematics and its place in the system of sciences.

The studied issues are the separation of architects from practical workers (ca. 12^{th} century); the origin of a layer of masters cum engineers in Italian cities (14^{th} c.); practical application of the laws of linear perspective by painters and sculptors (same time); recognition of the necessity of science in general (same time) and mathematics in particular for architecture (16^{th} c., but hoes back to Vitruvius).

The author also adduces long passages (in English) from works of many scholars, notably from Archytas of Tarentum's lost book *On Mathematics* from its Latin translation by Valla. He argues that the rise of mathematics and experimental sciences in the West after the rediscovery of the Greek science was especially fostered by the habit of reasonable argumentation and calculations, and that the main achievement of the philosophical discussions of the 16^{th} c. was the specification of the intellectual Weltanschauung, of moral duties and expectations in the culture of each period.

Matematika 12A8

Dale, A. I.: Bayes or Laplace? An examination of the origin and early applications of Bayes' theorem. Arch. Hist. Ex. Sci. 27, 23 – 47 (1982)

The author describes in detail the Bayes posthumous memoir of 1764, Laplace's memoir of 1774 as well as the solution of the problem about the probability of the next sunrise by Price. In the first case the main attention is paid to Proposition 10: If an unknown event has happened p times and failed q times in $(p + q)$ trials, the probability of the event x satisfies inequalities

$$P(\alpha \le x \le \beta) = \int_{\alpha}^{\beta} x^p(1-x)^q dx \div \int_0^1 x^p(1-x)^q dx.$$

Turning to Laplace, the author considers the application of the principle of inverse probability and the solution of several problems, including the following two (the second of which, as he notes, was also solved by Bayes). In both cases, the original number of tickets is infinite. 1. The ratio of white tickets to black ones contained in an urn is unknown; p white and q black tickets are extracted and it is required to calculate the probability that m white and n black tickets will appear after $(m + n)$ further extractions. 2. In $(p + q)$ extractions p white and q black tickets have appeared so that the ratio of the tickets contained in the urn is greater than $\{[p/(p + q)] – w\}$ and less than $\{[p/(p + q)] + w\}$. It is required to estimate w if p and q are large.

The author does not note that the Bayes memoir is available, for example, in *Biometrika* (1958) or that Laplace (§ 28 of his *Théorie analytique*) applied the pattern of the problem concerning the next sunrise to population statistics. And it would have been natural to add that Bayes considered the case of large p and q in the second part of his memoir (1765) and that Timerding, the Editor of its German translation, proved the relevant limit theorem.

Dale, Andrew I.: A History of Inverse Probability. From Thomas Bayes to Karl Pearson. 2nd edition. New York, 1999

The author expanded the first edition of this book (1991) by some 175 pages. Understandably, his main heroes are Bayes, Condorcet, Laplace and Poisson; he also paid much attention to Michell, Cournot, De Morgan, Boole, Edgeworth and Karl Pearson and quoted a host of commentators sometimes forgetting to state his own opinion.

The author is fond of rare words; his *prolocution* and *feracious* are lacking in the *Concise Oxford Dict*. (1973). He does not translate French or German quotations and even a passage from Jakob Bernoulli's *Meditationes* is only offered in Latin. And the exact sources of his numerous epigraphs remain a mystery. At best, he indicates the titles of the pertinent books, as *Pickwick Club*, from which I quote now: "I wouldn't be too hard upon him at first. I'd drop him in the water-butt and put the lid on …" (Sam Weller in Chapter 28).

The book is loosely written mainly because the connections between inverse probability, induction and statistics in general are not even hinted at. A history of the last-mentioned subject written by this well-read author would have been more useful.

The Bibliography now contains about 650 items, 36 of them published in 1991 or after. The collected works of Bernstein, Edgeworth and Huygens are not made use of; new editions of the books of Condorcet, Lacroix, Cournot and others are not mentioned and a few bibliographical mistakes are repeated.

Zentralblatt MATH, 922.01006

Andrew I. Dale: Most Honourable Remembrance. The Life and Work of Thomas Bayes. New York, 2003

This is indeed a description of the life and work of Bayes complete with commentated reprints of his published works and, partly, manuscripts (on the doctrine of fluxions; on "semi-convergent" series; the memoir of 1764 – 1765 on the doctrine of chances; an "Item on Electricity"; the portion of his notebook devoted to mathematics, electricity, celestial mechanics). Once again Bayes is shown as a mathematician of the highest calibre. Adjoining material includes a discussion of the contemporaneous visitations of the plague.

There is so much more pertaining to general history, ethics and theology that the book should have at the very least been separated into two or three parts. Thus, Bayes' theological tract is also reprinted, and with long commentaries. For that matter, Dale confuses his readers with excessive and often unnecessary details (on p. 259 he even discusses whether modesty is a virtue and refers to three sources [one of these is Aristotle]) but often fails to present concise information. Bayes' biography is too lengthy and meandering; a bibliography of his works as also the history of the Bayes theorem in the 20th century are lacking; Latin passages are sometimes left without translation, but Newton's *Principia*, whose English text is readily available, is extensively quoted both in Latin and in translation (by whom?) on pp. 224ff, and far-fetched epigraphs, mostly without exact references, are often adduced. It also remains unclear to what extent does this book go further than the author's previous publications on Bayes taken together.

Zentralblatt MATH, 1030.01031

Dasgupta, Someth: The evolution of the D^2–statistic of Mahalanobis. Sankhya A55, 442 – 459 (1993)

The author dwells on Mahalanobis' statistical analysis (1922 – 1949) of anthropometric differences between populations belonging to different races and on the history of the pertinent general statistical tool, the D^2 statistic (the Mahalanobis *distance*). He remarks that Karl Pearson, in 1930, did not agree with Mahalanobis and mentions the related papers of Fisher (1930), Hotelling (1931) and Bose & Roy (1938).

Zentralblatt MATH 810.01002

Daston, Lorraine: How probabilities came to be objective and subjective. Hist. Math. 21, 330 – 344 (1994)

The author contends that the difference between subjective and objective probabilities began to be studied in earnest in the 1840s (Cournot, Poisson, Ellis) and that the scholars involved held divergent opinions about the exact meaning of these terms. Concerning her additional discussion of the dialectics of chance and determinism I remark that De Moivre did not simply deny chance (the pertinent quotation is incomplete), nor did Laplace's (or, by implication, De Moivre's) *ironclad determinism* impede them from developing the theory of probability, i. e., from discovering the laws of chance.

Zentralblatt MATH 805.01009

David, Herbert A.: Statistics in U. S. universities in 1933 and the establishment of the statistical laboratory at Iowa State. Stat. Sci. 13, 66 – 74 (1998)

This is a sketch of the early history of mathematical statistics in the USA. A Department of Biometry and Vital Statistics was founded in 1918 at Johns Hopkins; in 1930, *Annals of Mathematical Statistics* began to appear; and in 1935, the Institute of Mathematical Statistics was established.

The Iowa State [University] Statistical laboratory exists since 1933. Its first leading figures were Snedecor and Henry Wallace (the future Vice-President of the USA). Fisher visited Iowa in 1931 and 1936 and played an important part in its development. Initially, the Laboratory was mostly engaged in agricultural statistics and economics.

The author also describes the work of several contemporary American statisticians, notably Hotelling.

Zentralblatt MATH 964.01026

Décaillot, Anne-Marie: Présentation du texte [Cantor, see above] suivi de sa traduction en français. J. Électron. Hist. Probab. Stat. 2, No. 1b, Article 9, 15 pp. (2006)
This is indeed a French translation of Cantor's report of 1873 with a short description of his life and work (which surprisingly omits to mention the dates of Cantor's birth and death, 1845 – 1918). The author notes that the timing of Cantor's report was unusual in two respects. First, German scholars had not then been really interested in probability (although the treatment of observations was a splendid exception). Second, he favourably discussed French science (Pascal, Fermat, Laplace) in the aftermath of the Franco-Prussian war.

That "en effet" there had been no German "text" on probability is the author's mistake. For example, I mention Hagen (1837), Fries (1842), and Öttinger (1852) as well as Bessel's attempt (1838) to prove the central limit theorem.

Zentralblatt MATH, to appear

Derriennic, Yves: Pascal et les problèmes du chevalier de Méré. De l'origine du calcul des probabilités aux mathématiques financières d'aujourd'hui. Gaz. Math., Soc. Math. Fr. 97, 45 – 71 (2003)
The author describes the problem of points as studied by Pascal, both in correspondence with Fermat and in his *Traité du triangle arithmétique*, and connects this subject with the recent notion of (stopped) martingale [F. Black, M. Scholes, *J. Political Econ*, 81, 637 – 654 (1973)].

Zentralblatt MATH 1034.01023

Desrosières, Alain: The Politics of Large Numbers. A History of Statistical Reasoning. Translated by Camille Naish. Cambridge (Mass.) – London, 1998
In this book Desrosières describes the history of the relations between the work of government and statistics in France, England, Germany and the United States (he omits Russia with its *zemstvo* statistics). In examining the history of statistics he has paid special attention to sampling, group building ("classifying and encoding", p. 236), and the birth of econometrics. His style is ponderous (long sentences are not rare), and his translator has preferred unusual words (a "construct", "to format", "militate", "ineluctable"); retained Jakob Bernoulli's French name, Jacques; and (p. 91) wrongly translated the title of Cournot's classic work of 1843.

Desrosières attributes a mortality table to Christiaan Huygens (p. 18), sometimes calling him Huyghens; and he believes that the strong law of large numbers was formulated by Poisson (p. 89), that Gauss derived the normal law as the limit of the binomial distribution (p. 75), and that De Moivre's discovery of this distribution occurred in 1738 (p. 286). He describes Simpson's distribution incorrectly (p. 64) and imagines that the law of large numbers is not connected with variances (p. 214). He never mentions Continental work on statistics or the opposition to Karl Pearson's empiricism. Further, his description of Quetelet's average man (l'homme moyen) and of the work of Lexis is highly superficial. The mathematical level of the book is therefore low: Desrosières is simply ignorant of statistics and its history.

For a number of events Desrosières gives different dates on different pages (discrepancies appear in references to the statistical congresses: pp. 80 and 154; the first yearly report on criminality in France: pp. 89, 152, 247; and the publication of the Bayes memoir: pp. 7 and 57, where the dates are wrong in both cases). His presentation of the philosophical underpinning of statistics is misguided. The views of Leibniz, of the authors of the *Logique de Port-Royal* (1662), and of Bernoulli are not discussed; instead, holism and nominalism are liberally offered. Mass random phenomena and "necessity versus randomness" are forgotten. The topics of public hygiene and epidemiology are appropriately included, but such figures as Snow, who discovered how cholera epidemics spread; Pettenkofer, who studied statistics on cholera; and Jenner, the discoverer of vaccination, are not.

So what is really left? Two chapters on statistics and the state, each devoted to two of the four countries studied, and three more chapters on the issues mentioned above, in which the author discusses the changing attitudes of society and government toward such phenomena as poverty, unemployment, and immigration; appropriate local and centralized statistical activities; the choice of statistical indicators; and the coming together of economists, mathematicians and statisticians (which became possible only after statisticians had accepted the essential role of probability theory, a circumstance Desrosières does not examine). The exposition is not however efficient or well organized: discussions of poverty, for example, appear in four chapters. [No attempt is made to trace the boundaries of contemporary statistics so that the title of the book is not justified.]

The book contains around 230 references, practically all of them to French and English sources, dating up to 1993 inclusively. Desrosières makes no mention of such German authors as Knapp and von Mayr or even of the French scientists Fourier, Dufau and Guerry. The book is largely a failure.

Isis, vol. 92, 2001, pp. 184 – 185

Dictionary of Scientific Biography. Editor, C. C. Gillispie. Vol. 1, Abailard – Berg. New York, 1970

This volume is written by 231 authors, 11 of them from the Soviet Union, among whom are eminent scholars, well-known historians of science (Clagett, Costabel, Crombie, Dorfman, Freudenthal, Ore, Struick, Taton, Vogel, Whiteside, Youshkevich). In addition to Gillispie the Editorial Board consists of nine prominent scientists and there are 38 consultants from more than 14 countries.

The volume includes about 400 biographies of scholars of all times and nations (except those living) whose work belonged mainly to mathematics, astronomy, physics, chemistry, biology and earth sciences. As stated in the Preface with regard to ancient Babylonia and Egypt, a Supplement will include essays on their several schools.

There are too few scientists of the 20th c. since it is sometimes difficult to describe their work. The situation in this respect will apparently become more serious with each new decade and excepting a narrow circle of specialists the newest history of some branches of knowledge can slip out of reach of readers.

The list of those included is not without lacunas. Among geographers Amundsen is missing; specialists in engineering occur seldom. Thus, the metallurgist N. T. Beliaev is included, but P. P. Anosov is not. True, although not many Russian names begin with an A, we found N. I. Andrusov, D. N. Anuchin, V. K. Arkadiev, and then A. N. Bach, A. A. Balandin, N. N. Beketov, F. F. Bellingsgausen, V. M. Bekhterev, A. A. Belopolsky, L. S. Berg and others.

The length of the biographies (including the appended bibliographies) greatly differ from half of a (large) page to 4 – 8 pages (Abel, Bach, D'Alembert, Ampère) and to 14 – 18 pages (Apollonius, Archimedes) whereas Aristotle is honoured by four articles with a total length of 32 pages.

The *Dictionary* thus describes the life and work of the most eminent scholars, and, for that matter, in much more detail than, for example, the *Biografichesky Slovar* (Biogr. Dict. of Workers on Nat. Sci. and Technology), vols 1 – 2. Moscow, 1958 – 1959, where, however, the number of those included is greater. As a whole, the *Dictionary* is done conscientiously and skilfully although for such a large number of authors the scientific level of the biographies could not have been the same. A general remark concerns the adduced bibliographies: Russian sources are not at all sufficiently included there.

Aristotle is described as the most influential ancient exponent of the methodology and division of sciences who also contributed to physics, physical astronomy, meteorology, psychology and biology. The articles devoted to him are: Method, physics and cosmology (G. E. L. Owen); Natural history and zoology (D. M. Balme); Anatomy and physiology (L. G. Wilson); and Tradition and influence (L. Minio-Paluello). Taken together, they provide biographical information, a short bibliography of his writings and a critical discussion of his methodology of science. His ideas concerning separate branches of natural sciences and the relations between his mathematics and natural sciences are described; the correlation of the concepts of Plato and Aristotle is discussed and Aristotle's concrete achievements are appraised. Apparently in line with the general orientation of the *Dictionary* his philosophical views are only considered in a general context of natural sciences and, for that matter, insufficiently. Minio-Paluello considered the history of the translations of Aristotle's works and attempted to ascertain his influence on subsequent science but he did not study deeply enough the influence of his philosophy. Owen compared Aristotle with other classics of antiquity. He concluded that Aristotle's influence was occasioned not by concrete findings in natural sciences (as was the case with Eudoxus and Archimedes) but by ability to argue. Perhaps: by Aristotle's ability to explicate convincingly all which was known in his time.

Thomas Aquinas (W. A. Wallace) was not a scientist but a philosopher and theologian whose synthesis of Christian revelation with Aristotelian science has influenced all areas of knowledge including modern science. Thomas turned the attention of theologians to a study of the pagan Aristotle, generalized a number of branches of science (the medieval counterparts of physics, astronomy, chemistry and the life sciences) and influenced Oresme and Gilbert.

Once again, apparently because of the orientation of the *Dictionary*, we do not find here any analysis of Thomas' philosophy or ethics, or any description of his part in the history of the Christian religion. That the *Dictionary* is mostly restricted to mathematics and natural sciences is proper, but, when dealing with such figures as Aristotle or Thomas (or Newton, or Leibniz), it was necessary to describe their philosophical views.

The late eminent expert on Abel and an author of a book devoted to him [4], Ore, wrote about his hero. He provided a vivid biography, but Abel's scientific work and his great contribution to mathematics of the 19th c. are described cursorily. Whiteside, the most prominent student of Newton, compiled an item about Barrow. The problem *Barrow – Newton* naturally arrests the attention of the reader. The author critically appraises the mathematical and optical writings of Barrow and questions his influence on Newton. To some extent, contemporary Russian authors [2] share this opinion, but unconditional statements[1] to the effect that Barrow was Newton's teacher are still being pronounced.

The piece on Becquerel (A. Romer) who is known first and foremost in connection with the discovery of radioactivity seems uninteresting since there are hardly any blank spaces either in Becquerel's biography or work and the author's task (successfully fulfilled) was not that difficult. Still, he should have named Becquerel's predecessors [3, p. 32]. However, even such articles, written in a uniform manner and compiled in a single source are undoubtedly useful. Consider also that many authors provide lesser known facts and formulate original conclusions (e. g., Whiteside, see above), and it becomes clear that the *Dictionary* is an indispensable reference book and that historians of science failing to consult it will run the risk of producing inferior work.

The *Dictionary* is brought out scholarly. In particular, additional versions of spelling of the names is furnished in necessary cases and the bibliographies are distinctly separated into original sources and secondary literature. Regrettably, portraits are completely lacking.

1. Anonymous, Barrow. *Great Sov. Enc.*, 3rd edition, vol. 3, 1970. This edition of the *Encyclopedia* is available in an English translation (New York – London, 1973 – 1983).

2. *Istoria Matematiki* … (Hist. Math. from the Most Ancient Times to the Beginning of the 19th Century), vol. 2. Editor, A. P. Youshkevich. See Chapters 7 (Youshkevich aided by M. V. Chirikov) and 8 (Youshkevich).

3. Kapustinskaia, K. A., *Becquerel*. Moscow, 1965. In Russian.

4. Ore, O. *Abel, Mathematician Extraordinary*. Univ. Minnesota, Minneapolis, 1957.

NKzR, A1972, No. 5, pp. 5 – 8. Coauthor: A. B. Paplauskas

Dictionary …, vol. 2, Berger – Buys Ballot. New York, 1970. This volume was written by roughly the same number of authors and under the same Editorial Board as vol. 1. Included are eminent non-living mathematicians and natural scientists of all times and all nations; specialists in engineering again occur (the metallurgist Brinell is honoured, but Bessemer is not). Among those omitted are the zoologist Berlese, the physiologist A. N. Bernstein, the physician and physiologist Botkin, the mathematicians Bugaev and Buniakovsky. S. N. Bernstein, who died in 1968, will be included in a supplementary volume; there also we shall hopefully see a piece on Born (died in 1970).

Somewhat unusual is the inclusion of Bourbaki (R. P. Boas, Jr), but the reader will hardly complain: the article is interesting and rich in content. True, the author should have mentioned Bourbaki's predecessor, Hilbert (and possibly even Leibniz).

Boscovich (Z. Markovic), although he was a foreign member of the Petersburg academy of Sciences, is not known here sufficiently. The author calls him the last polymath and argues that his work methodologically influenced physics and philosophy of the 19th c. Boscovich apparently deserves more credit: physicists seem to feel his influence even now. As to his versatility, the author should have additionally mentioned Lomonosov. And he is wrongly claiming that Boscovich developed an exact (?) theory of errors. It was Laplace and mostly Gauss who created this theory.

F. A. Yates maintains that Giordano Bruno intuitively arrived at most important principles of philosophy, cosmology and biology. He stresses Bruno's influence on later generations of scientists and philosophers and notes that it was felt when modern science had been appearing in the 17th c. In an article on Tycho C. D. Hellman describes his astronomical instruments and observational methods. It can also be argued that (at least in Europe) Tycho introduced the method of regular observations into experimental sciences.

J. E. Hofmann states that Jakob Bernoulli solved some important problems and essentially contributed to algebra, mathematical analysis, theory of probability and mechanics. H. Straub compiled an interesting article on Daniel Bernoulli whose works concerned applied mathematics, technology, mechanics and physics and greatly influenced the origin of hydrodynamics and the kinetic theory of gases. He studied vibrations of elastic strings and introduced moral expectation into economics. The author also maintains that Daniel, during his lectures, communicated the Coulomb law to his listeners. It can be added that Daniel perceived a very universal law of nature in the expansion of the vibrations of a string into a set of independent harmonic oscillations and that his merit in attempting to introduce mathematics into economics and in defining the so-called risk functions is unquestionable.

In compiling his piece on Bohr, L. Rosenfeld made use of his personal recollections and archival sources. He called Bohr a greatest physicist and a progressive scientist of our time. S. G. Brush describes in detail Boltzmann's work on the kinetic theory of gases and the statistical justification of thermodynamics. He stresses that Boltzmann defended the molecular theory. Unfortunately, he barely mentions the other directions of Boltzmann's work (in physics and mathematics).

Like vol. 1, this volume contains important and rich information about outstanding scientists and will be very valuable for historians of science.

NKzR, A1972, No. 10, pp. 6 – 7

Dictionary …, vol. 3, Cabanis – Dechen. New York, 1971

The volume contains about 360 articles. It is compiled by an international group of authors including scientists from the Soviet Union and Eastern Europe. Among them are Costabel, Dieudonné, Freudenthal, Grigorian, Hofmann, Price, Scriba, Struick, Taton, Whiteside and Youshkevich. …

As in the previous volumes, the *Dictionary* includes prominent non-living mathematicians and natural scientists of all times and all nations; for example, the ancient Greek scholar Conon of Samos, the Indian astronomer Dasabala, the medieval Arab natural scientist Al-Damiri, representatives of the Chinese algebraic school of the 13^{th} c., Ch'in Chiu-shao and Chu Shih-Chieh, and European scientists beginning with the Renaissance. Among the last-mentioned are Russian scholars: the mathematicians and mechanicians Davidov, Chaplygin, Chebotarev, Chebyshev; the geologist Chernyshev; the chemists Chernyaev, Chichibabin and Chugaev.

The *Dictionary* also covers other scientific disciplines. Included are the metallurgists Carpenter and Chernov; engineers Castigliano (known also for his theorem in the theory of elasticity) and Congreve, an author of many patents (one of these for perpetual motion!) and the inventor of military rockets; the educationist and teacher Comenius. It was hardly proper to include Chaucer, who was a little known astronomer, whereas a much more famous astronomer Chauvenet is left out. For some reason geographers remain unlucky: Amundsen and Barents were not included in vol. 1, this time we do not find Columbus.

We shall dwell now on some biographies. Copernicus (E. Rosen), whom his contemporaries knew as a statesman and physician and the creator of the revolutionary heliocentric system of the world, is shown in the making, as though in a debate with Ptolemy. Many passages from his writings are adduced, but nothing is said about his scholasticism or his work in spherical trigonometry. Even the ban imposed by the Catholic Church on his main writing is passed over in silence. As a result, the biography is incomplete.

In describing Cardano, M. Gliozzi pays much attention to his merits in algebra (solution of equations of the third degree, introduction of imaginaries). He even thinks that Cardano originated the theory of algebraic equations. Cardano knew the so-called classical definition of probability and a rudimentary form of the law of large numbers. He was also a philosopher, mechanician, and geologist and his contemporaries recognized him as a physician so that he could well be called a person of encyclopaedic knowledge. Cardano's life was extremely unusual; for some time he was persecuted as a heretic, but then the Pope granted him an annuity. It seems that we do not know his (and not only his) biography well enough.

Freudenthal wrote a really good article on Cauchy. He described Cauchy's fundamental achievements in various branches of mathematics, mechanics and celestial mechanics but considers that his greatest contribution was the creation of the theory of elasticity. [He also asserted that Cauchy had rigorously proved the central limit theorem, a statement hardly accepted by other authors.] The author made critical remarks about the publication of Cauchy's *Oeuvres Complètes* which, after many years, is still dragging on.

Chebyshev (A. P. Youshkevich) is shown as a versatile scholar having great merits in a number of branches of mathematics and mechanics. No lesser was his achievement in educating a group of eminent scientists and in creating the Petersburg mathematical school. The author provided a comprehensive characteristic of Chebyshev's contribution to the national and international science, but perhaps his achievements in mechanics deserved a somewhat more detailed discussion. [He also said nothing about Chebyshev's non-acceptance of new directions in mathematics then appearing in Western Europe.]

Cantor (H. Meschkowski) was born in Petersburg. He created the set theory and attained other outstanding achievements in mathematics among which was the origination of one of the first theories of real numbers. He was also meritorious for his work on uniting mathematicians on an international scale and its direct result was the first International Congress of Mathematicians (1897). Describing in detail the essence of the paradoxes of the set theory and pointing out that Cantor's ideas had a philosophical aspect, the author says nothing about the recent achievements in studying formal axiomatic systems of the theory which possess greatest mathematical and philosophical importance.

A student of Zhukovsky, Chaplygin (A. T. Grigorian) left a deep trace in classical mechanics. He originated gas dynamics and high-velocity aeromechanics. Appraising his work, the author indicates that it was partly ahead of his time. Chaplygin also devised a method of approximately integrating differential equations. This fact is noted, but not commented upon, and the reader will be hard put to it to appraise the importance of Chaplygin's mathematical findings.

A cofounder of thermodynamics, Sadi Carnot (J. F. Challey), the son of the well-known mathematician and mechanician Lazare Carnot, is remembered owing to his sole writing of great theoretical and practical importance where he considered the problem of transforming heat into motion. The author analyses this work and sketches the development of Carnot's ideas to William Thomson and Clausius inclusively. Perhaps it would have been opportune to discuss briefly the prehistory of the Carnot problem. Indeed, even ancient scholars knew that heat was a source of energy.

Darwin (G. de Beer), who was unable to complete his studies as a student-physician and took a poor degree as a theologian, joined the survey ship *Beagle* as an unpaid naturalist. During the five years on board the ship he distinguished himself as an eminent geologist, zoologist and botanist and arrived at the main ideas concerning his evolution theory of the origin of species. After collecting a great body of facts about the variability of species Darwin understood that an evolution theory can explain this variability and that the motive force of the evolution of each species was the need to secure food under conditions of a changing environment.

Darwin was naturally unable to explain all the difficulties of evolution; he apparently posed more questions than he solved. Still, what he managed to do was so important that he [along with

Boltzmann] might be considered the most eminent natural scientist of the 19th c. The author does not offer such an appraisal (concluding remarks are absent in most biographies), nor does he mention that Darwin originated the statistical understanding of the laws of natural sciences, and, in particular, served as an impetus for the birth of mathematical statistics.

The collected biographies are a most valuable material for historians of science, natural scientists and educationists. They also provide sources for studying the problems of heredity of genius (the dynasties of Bernoullis, Carnots, Curies, Darwins et al), of selecting a profession (Darwin), for estimating the influence of the social environment and social and political conditions on science (Copernicus) etc.

NKzR, A1973, No. 1, pp. 7 – 10. Coauthor: A. I. Volodarsky

Dictionary …, vols 1 – 5. New York, 1970 – 1972

Over many years and decades, quite a few similar reference books, for example Sarton (1927 – 1947), covering scholars up to the mid-14th c., the national dictionary (Zvorykin 1958 – 1959), and, of course, since 1893, the regularly supplemented Poggendorff, have been appearing . However, with regard to the wealth of information none of them is comparable to the *Dictionary*. At present, five of its volumes out of the intended 13 have appeared … [I omit those parts of this review which largely repeat what was said about the three first volumes.]

Each volume consists of 370 – 400 items, biographies of outstanding scholars … mostly mathematicians and natural scientists, to a considerably lesser part technicians. … It seems that technicians were non-methodically selected. Thus, the metallurgists Beliaev, Brinell, Carpenter and Chernov are included, but not Anosov or Bessemer. Then, we find the engineers Edison, Castigliano and Congreve, but Diesel, Farman, Fulton, Gutenberg as also Friese-Greene, the English inventor of the cinematograph, are absent. The geographers [and travellers] Amundsen, Barentz, Byrd, Dezhnev, Dumont d'Urville, Columbus, Fra Mauro, Frobisher are ignored. And, apparently beginning with vol. 2, the *Dictionary* became somewhat stingy. Many scientists were omitted, among them the mathematicians and mechanicians Bugaev, Buniakovsky, Galerkin; the physiologist Botkin; the zoologist Berlese; the palaeontologist D'Orbigny; the chemist Flavitsky; the hygienist Erismann; the surgeon Esmarch; the geologist Gubkin; the botanist Engler; and Fedorov, the founder of structural crystallography. …

In spite of the mentioned shortcomings and omissions, the *Dictionary* has already become an irreplaceable source of information. Little known facts are cited in many articles and the work of many scholars is appraised anew. For example, Daniel Bernoulli's work in biomechanics, never mentioned by Russian historians of science, is described. His biography is now supplemented by the first easily available and apparently comprehensive bibliography of his works which include his contributions on biomechanics; one of these is lacking in the well-known bibliography compiled by V. V. Bobynin. …

The *Dictionary* will be interesting not only for historians of science, but for professorial staff, postgraduates and students. We hope that its publication, complete with the promised general index of names, will be sufficiently soon accomplished.

Sarton, G. *Introduction to the History of Science*, vols 1 – 3. Baltimore, 1927 – 1947.

Zvorykin, A. A., Editor, *Biografichesky Slovar Deiatelei Estestvoznania i Tekhniki* (Biographical Dictionary of Workers in Natural Sciences and Technology), vols 1 – 2. Moscow, 1958 – 1959.

Voprosy Istorii estestvozn. i Tekhniki, No. 3, 1973, pp. 74 – 75.

Coauthors: A. I. Volodarsky, A. B. Paplauskas

Doob, Joseph L.: Probability vs measure. In: Ewing, John H., ed., et al, Paul Halmos. Celebrating 50 Years of Mathematics. New York, 189 – 193 (1991)

The author remarks that some probabilists believe that the absorption of probability by measure theory was useless. He himself thinks that the *psychological integration* of the former by the latter is incomplete and that a certain aspect of probability does not need *subtle measure-theoretic concepts*. He also maintains that the previous stress on independence in probability is replaced now by an emphasis on conditional expectation and that the study of the *historical non-mathematical context* of probability led to success both in measure theory and probability proper.

Zentralblatt MATH 791.60001

Doob, Joseph L.: The development of rigor in mathematical probability (1900 – 1950). Am. Math. Monthly 103, 586 – 595 (1996)

This paper, an *informal outline*, containing many passages from classical sources without any exact references, is reprinted from [the author's paper in *Development of Mathematics 1900 – 1950*, ed., J.-P. Pier, Basel, 157 – 170 (1994)]. The author reviews the introduction of measure theory into probability; notes the pertinent methodological and psychological difficulties connected with the disappearance of *romantic connotations* of probability; discusses the impact of the new probability theory on analysis and the present relations between these two disciplines.

Zentralblatt MATH 865.01011

Dutka, Jacques: The incomplete Beta function – a historical profile. Arch. Hist. Ex. Sci. 24, 11 – 29 (1981)

This essay on the use of the incomplete Beta function and, also, on the methods of its calculation, covers the period from Newton to these very days. The works of a number of scholars (Bayes, 1763; Laplace, 1778 and 1785; Gauss, 1812; Markov, 1899; K. Pearson, 1934) are discussed. Along with E. S. Pearson the author notes that K. Pearson was only acquainted with achievements obtained within probability theory.

On p. 16 the author asserts that in Chapt. 3 of the *Théorie analytique des probabilités* Laplace proved the *earliest version of what later came to be known as the central limit theorem* and on p. 18, ftn 17, he states that Montmort published [the second edition of] his book on games of chance in 1714.

Zentralblatt MATH 465.01002

Dutka, Jacques: On the problem of random flights. Arch. Hist. Ex. Sci. 32, 351 – 375 (1985)
This is an essay on random walks with a continuous change of direction (random flights, as Rayleigh called them in 1919). The author also treats the prehistory of his subject including random walks in general, although not the gambler's ruin. Accordingly, he discusses the work of Crofton (1865), Rayleigh (1880), Ross (1905), Kluyver (1905), Smoluchowski (1906), Watson (1922) et al up to the mid-20th century.

The author pays special attention to the application of characteristic functions, and, from the early 19th century, of discontinuity factors as well as to the stochastic study of the summation of sinusoidal oscillations having fixed amplitudes and frequencies but with random phases which goes back to Rayleigh (1880). He also finds a clear formulation of two-dimensional random walks in 1905 (Ross).

Buniakovsky (1846) considered a simple case of a generalized random walk of a castle in a game of chess [two-dimensional walk!].

Matematika 12A11

Dutka, Jacques: On Gauss' priority in the discovery of the method of least squares. Arch. Hist. Ex. Sci. 49, 355 – 370 (1996)
Issuing from the same meridian arc measurements as Gauss did in 1799, the author computes the flattening of the earth's spheroid by the method of least squares (MLSq) and, comparing his result with that of Gauss, concludes that Gauss had indeed used the same method. He thus opposes (rather than "supplements") Stigler's opinion of 1981. The author makes a similar inference concerning Gauss' (1799) reduction of Ulugh Beg's table of the equation of time and notes that von Zach (1809) agreed that Gauss had used the MLSq "since 1795 and [had] shared [it] at that time with some of his … friends". Von Zach, however, did not state that Gauss had acquainted him with the method. The article is especially important since Stigler's (1986) treatment of the work of Gauss (and Euler) is misguided. I refuted him and, in particular, noted that Bessel was one of Gauss' confidants (*Arch. Hist. Ex. Sci*. 46, 1993, pp. 39 – 54). The author has strengthened my arguments. However, he is not sure that Gauss had a number of confidants (and he does not mention Bessel); he does not prove his attribution of the repeating theodolite to Borda rather than to Mayer, and two mistakes corrupt his bibliography. [Since then, I discovered several more confidants, e. g., Wolfgang Bolyai and of course Olbers about whom Stigler should have known.]

Zentralblatt MATH, 854.01015

F. Y. Edgeworth, Writings in Probability, Statistics and Economics. McCann, Charles Robert Jun., Editor.Vol. 1: The Theory of Probability and the Law of Error. Vol. 2: The Theory of Statistics. Vol. 3: Applications of Probability and Statistical Theory. Cheltenham, 1996
These volumes of Francis Ysidro Edgeworth (1845 – 1926) contain reprints of 76 papers and 13 reviews, and an Introduction by the Editor. Among the figures reproduced 7 reflect nothing but black rectangles. An alien footnote is printed on p. 283 of vol. 1, but a proper one (vol. 3, p. 291) is missing. There is no portrait or

bibliography of the author's contributions (or of works devoted to him) and the existing unpublished bibliography (M. G. Kendall, 1968) is not mentioned. The papers included are largely those listed by M. G. Kendall & Alison G. Doig (1968) but their relation to the set published by P. Mirowski in 1994 remains unknown. (The latter source, but not its exact title is mentioned by the Editor.)

The heads chosen are doubtful; it is difficult to distinguish between "Law of Error" and the theory of errors in vol. 2; demography hardly belongs to social science; psychology is a discipline of natural sciences; and the paper on correlated averages should not have appeared under "Applications".

Edgeworth was a witty and original scholar (an economist and a statistician). He was well acquainted with the work of the Continental statisticians, but he objected to replacing the "Laplacean mathematics" by the findings of the Russian school (vol. 1, p. 156). He studied asymmetrical density curves, strove to make use of the mechanism of least squares in the Pearsonian statistics and applied the statistical method in most various fields. He (vol. 1, p. 62) did not recognize Gauss' second formulation of least squares; did not believe that the Poisson law of large numbers generalized the Bernoulli theorem (vol. 1, p. 403); and, unlike Kepler, did not realize that the eccentricities of the planetary orbits were occasioned by random causes (vol. 3, p. 371). More important, he failed to exert adequate influence because of his aloofness, involved style and insufficient trust in quantification. Chuprov (1909) [and Kendall (1968)] believed, however, that he had paved the way, in England, for an understanding of statistics as a general tool.

Zentralblatt MATH, 860.01035

Edwards, A. W. F.: Pascal and the problem of points. Intern. Stat. Rev. 50, 259 – 266 (1982)

The author discusses the solution of the problem of points in the correspondence of Fermat and Pascal (1654). He emphasizes the difference between the methods used by the two savants and maintains that exactly Pascal introduced the concept of expectation of winning a game of chance and devised the *method of expectations*. The author also stresses the significance of Pascal's *Traité du triangle arithmétique* (1665) for the subsequent development of the theory of probability.

The fact that both Fermat and Pascal used expectation as a criterion for solving the problem of points seems more important. As to the methods of solution, there is a case for attaching lesser significance to the difference between them, see p. 239 of my contribution in *Arch. Hist. Ex. Sci*. 17, 1977, 201 – 259.

Zentralblatt MATH 501.01005

Edwards, A. W. F.: R. A. Fisher on Karl Pearson. Notes Rec. Roy. Soc. Lond. 48, 97 – 106 (1994)

In 1945 Fisher contributed a paper on Pearson for the *Dict. Nat. Biogr*. Next year he commented on its edited draft stating that Pearson's *technical* contributions to the statistical method *now cuts rather little ice*, that the chi-squared test was the *most important* of these and that the work of Edgeworth and Student suffered because of Pearson's personal traits. In a later letter of the same year Fisher wrote that Pearson should not be represented as a *towering genius*. Finally, because of disagreements with the Editor, Fisher quit his work (the entry on Pearson was written by Greenwood) but he used much of it in his article in *Contributions to Mathematical Statistics* (New York 1950). The author, who drew on archival sources kept at Adelaide, adduced the first draft of Fisher's paper where Fisher stressed Galton's influence on Pearson and maintained that the last-mentioned did not recognize the importance of the Mendel theory and that his *bitter criticisms has retarded real progress* in statistics.

Zentralblatt MATH 792.01034

Edwards, A. W. F.: Pascal's Arithmetic Triangle.The Story of a Mathematical Idea. Revised reprint of the 1987 original. Baltimore, 2002

The first edition of this book carried [both editions carry] reprints of two of the author's papers (Pascal and the problem of points, 1982; Pascal's problem: the gambler's ruin, 1983). I enlarge on the review of the first edition.

Pascal's *Traité du triangle arithmétique* was published posthumously, but already in 1654 Fermat possessed its beginning. It consists of four tracts the last of which was partly written in Latin. Except for the solution of the problem of points, the material of the *Traité* had been known previously, but Pascal was the first to prove rigorously some important propositions.

The author describes the early history of the arithmetic triangle and the subsequent discoveries in mathematical analysis, probability and combinatorics (Wallis, Newton, Leibniz, Jakob Bernoulli) partly made by means of the arithmetic triangle although mostly without knowledge of the *Traité*. Accordingly, a better title for Edward's contribution would have been "History of the Arithmetic Triangle".

The second edition of his book contains an Epilogue (new literature) and a further discussion of the relevant chapters of Jakob Bernoulli's *Ars Conjectandi*. That Niklaus Bernoulli prepared the *Ars* for publication (p. 121) is wrong and two pertinent sources are not mentioned (A. P. Youshkevich, *History of Mathematics in the Middle Ages*, 1961, in Russian, and R. Rashed, Kombinatorik und Metaphysik, in *Festschrift zum siebzigsten Geburtstag von M. Schraum.* Berlin, 2000, 37 – 54).

Zentralblatt MATH, 1032.01013

Ekeland, Ivar: The Broken Dice and Other Mathematical Tales of Chance. Translated by Carol Volk. Chicago, 1993

The original French title (1991) of this book is *Au hasard*. Several of its parts are non-mathematical. There, the author dwells on historical events (many of them pertaining to Scandinavia) whose outcomes were decided by chance, on divination by lot, and on psychology of taking risks. He (p. 145) remarks that "the industrial civilization moves forward without measuring the risks incurred …"

The remainder is mainly given over to the imitation of chance (with a discussion of a MS written in 1240 – 1250 by Brother Edwin, a Norwegian monk), strange attractors and exponential instability. During the latest few decades the understanding of the role of chance in nature has essentially changed and the author should have put more emphasis on this point. Regrettably, he did not mention either Mises or the fundamental problem of defining a finite random sequence.

Two statements, viz., that Kolmogorov was the "founder" of the theory of "probabilities" (p. 47) and that the normal law appears "whenever we collect measurements" (p. 158) are not accompanied by qualification remarks.

Zentralblatt MATH, 785.60002

Ekeland, Ivar: The best of all possible worlds. Mathematics and Destiny. Chicago and London: University of Chicago Press, 2006, 207pp.

A somewhat differing version of this review appeared in Russian (*Voprosy Istorii Estestvoznania i Tekhniki*, No. 2, 2009, pp. 211 – 213)

The main story begins with Leibniz who stated that everything is possible if not contradictory and that God had created the world by choosing the most perfect alternative. In 1740, Maupertuis explained the choice (true, only of the course of some natural physical processes) by the principle of least action (of least product of distance travelled by the velocity of motion and mass which remains constant or the least value of the appropriate integral) and applied it to justify (mistakenly) the Snell law of refraction. Euler applied the same principle for studying important problems in mechanics and physics (partly even preceding Maupertuis), introduced it into mathematics and thus, along with Lagrange, initiated the calculus of variations.

The author then describes the work of Hamilton and C. G. Jacobi (Ostrogradsky is not mentioned) who showed that the Maupertuis principle was doubtful (what is possible motion? And how to calculate the appropriate action of forces?), transferred it to the phase space (position + velocity), and finally replaced it by the principle of stationary action (the quantity of action should be insensitive to small changes in the appropriate path).

Ekeland does not here recall the earlier mentioned Fermat principle according to which light travelled along the fastest possible route. Religious and philosophical views prevailing in the 18th century were forgotten; instead, according to Poincaré and Mach, a theory had only to be fruitful but necessarily true. Regrettably, the author had not explained all this clearly enough although he obviously intended his book for a broader circle of readers. Thus, in 1752 Chevalier d'Arcy discovered that in a certain case light did not pick the shortest path, but Ekeland did not connect this mentioned fact with the new principle.

Turning his attention to randomness and rejecting its usual interpretation as intersection of two (or a few) chains of determinate events, the author suggests that reality "lies somewhere between" order and dependence of everything on everything (p. 86). He thus refuses to study randomness, and he never mentions its regularity in case of mass random events.

Instead, he considers the example of the motion of a ball on a non-elliptical billiard table. Owing to unavoidable small uncertainty of its initial conditions, the path of the ball becomes a cloud which fills a certain region. This chaos, which the author (p. 125) unfortunately compares with a game of chance, actually defies quantitative definition, and, unlike Brownian motion, cannot be stochastically studied.

Ekeland attributes the foundation of the chaos theory to Poincaré who started from the principle of stationary action distorted by perturbations, and he concludes (p. 128) that randomness (contrary to Einstein's opinion) exists at the subatomic level with the most likely paths of elementary particles corresponding to stationary action (Feynman, p. 120) and chaos governing at our scale with the principle "caught somewhere in the middle". But where can that middle exist?

The following chapters are devoted to the theory of evolution and the existing situation in the world. He somehow understands evolution as a tendency towards an equilibrium between species (not as a stochastic process, as I suggested in 1980) and does not mention Mendel. Moreover, there is a suggestion that biological evolution is chaotic, and the author should have commented on it. It is perhaps permissible to add that Lamarck (*Histoire naturelles des animaux sans vertèbres*, t. 1. Paris, 1815, p. 169) stated that the equilibrium between "universal attraction" and "L'action repulsive des fluids subtiles" was the cause of all observed facts and especially those concerning living creatures.

It would have been opportune to mention the mistaken theory of spontaneous generation of the simplest organisms which had been yet received by Lamarck, i. e., the most serious significance attributed to randomness in biology even long before Darwin.

As to our situation, "God had receded, leaving humankind alone in a world not of its choosing" (p. 180). This quote also shows Ekeland's style, as does the very first phrase of the book: "The optimist believes that this is the best of all possible worlds, and the pessimist fears that this might be the case".

The book is interesting and instructive. A special example concerns the actually not so well-known trial of Galileo: he was accused of believing that a mathematical hypothesis reflected reality, "something that mathematicians would never do". Copernicus, or rather his publisher had indeed denied this connection, but had there been other such instances? Another statement (p. 25) is however doubtful: Descartes unified geometry and algebra "thereby creating modern mathematics".

The contents of the book are not presented clearly enough and bibliographic information is simply poor. Even the "second uncertainty principle in classical mechanics" that states, that in some sense the uncertainty in the initial data of motion cannot be lessened, is without any further details attributed to Gromov, 1980. The author could have surely done much better. He is Director of the Pacific Institute of Mathematical Studies, and he put out several books including *Mathematics and the Unexpected* (1988) and *The broken Dice* (1993), both issued by the same publisher.

Almagest, vol. 2, No. 2, 2011, pp. 146 – 147

Fancher, Raymond E.: Galton on examinations. An unpublished step in the invention of correlation. Isis 80, No. 303, 446 – 455 (1989)
Upon discussing the early work of Galton on correlation (1874 – 1888), the author describes his unpublished study from the Galton papers at Univ. College London *dating from 1883*. Galton attempted to discover the connection between examination marks and success in life, and, in particular, he made steps toward rank correlation. He failed, but his analysis contained methodological innovations which contributed to his later breakthrough in correlation theory. The author notes Galton's high opinion on the benefit of academic examinations and indicates that in 1901 he wanted to, but obviously did not, resume his concrete study.

Rank correlation dates back to L. Seidel (1865 – 1866) if not to Laplace. Again, Seidel quantitatively, although in a round-about way, estimated the significance of correlative relation between two and three variables [the reviewer, *Arch. Hist. Ex. Sci.* 26, 277 – 279 (1982)].

Zentralblatt MATH 691.01010

Feldman, Jacqueline; d Lagneau, Gérar; Matalon, Benjamin, Editors. Moyenne, milieu, centre. Histoires et usages. Paris, 1991.
The volume consists of 18 articles written by the Editors themselves and by 13 other authors, mainly after discussions from 1989 onward. It is separated into three parts (means in statistics, 6 papers; means in physical sciences and sciences on man, 9 papers; and geographical centres, 3 papers, possibly useful for tourism). Two of the papers appeared earlier and are reprinted, with or without change. There are no indices. The papers deal with the history of their subjects (statistics; sociology; theory of errors; psychology; and to some extent philosophy, biology, economics, anthropology, and public hygiene). The chronological boundaries of the papers differ essentially, the extreme points being ca. 1660 and the middle of this century. Accordingly, the main heroes are Quetelet, Comte, A. and L.-A. Bertillons, Broca and Galton.

A few words about some articles. M. Barbut dwells on the history of the central limit theorem and discusses stable distributions. He pays special attention to *Pareto – Lévy* laws. In another paper, he discusses various means from a deterministic point of view and concludes that, for numerical variables, only the ordinary means make sense. M. Armatte describes the history of the theory of errors in connection with meridian arc measurements. B. Monjardet dwells on Fréchet's modification of Quetelet's *homme moyen* and describes the history of the problem of determining the point, the sum of whose distances from three given points is minimal (Fermat). He comments on the use of several metrics and examines many interesting applications.

There are serious shortcomings. Astronomy and meteorology are not discussed and nothing is said about the ancient teaching on means. Snow, in 1855, just by comparing two means, showed how to combat cholera, but he is not even mentioned. On p. 70 Simpson is wrongly called De Moivre's student and on p. 85 Süssmilch rather than Graunt and Petty is considered the creator of political arithmetic. On the alleged incompetence of Euler in statistics (p. 69) see my opinion in *Centaurus* 31, 1988, pp. 173 – 174 [and *Arch. Hist. Ex. Sci*. 46, 1993, pp. 49 – 50].

Zentralblatt MATH, 747.01002

Field, J. V.: Tycho Brahe, Johannes Kepler and the concept of error. In: Festschrift for Volker Bialas. 47. Münchener Universitätsschriften, 143 – 155 (2005)
The author notes that Tycho made long series of observations partly under the astrological influence of Paracelsus, and allegedly regardless of earlier practice and states that Kepler estimated their error as 4′ or less (which compelled him to reject the Ptolemaic system of the world). She concludes that the notion of observational error was introduced into astronomy "somewhere between" Tycho, his instrument-makers and Kepler.

Her reasoning on the earlier history is wrong and her conclusion is therefore false. Ptolemy, Al-Biruni and Levi ben Gerson discussed errors of observation and knew how to minimize the influence of some of them. And even Ptolemy testified that he and Hipparchus before him had made regular observations, so that in this sense Tycho's practice was not new. New was their much higher precision which necessitated their adjustment. See my paper in *Arch. Hist. Ex. Sci.*, 46, 1993, 153 – 192.

Zentralblatt MATH, 1086.01022

Field, J. B. F.; Speed, F. E.; Speed, T. P.; Williams, J. M.: Biometrics in the CSIR: 1930 – 1940. Austr. J. Stat. B30, 54 – 76 (1988)
This is an essay on the scientific work and teaching activities of three women statisticians, Frances Elizabeth Allan (1905 – 1952); Mildred Macfarlan Barnard (b. 1908), and Helen Alma Newton Turner (b. 1908), mostly during 1930 – 1940, when all of them were connected with the Australian Council for Scientific and Industrial Research. The essay describes the education and the biometrical work of these statisticians. All three of them studied and/or worked for some time under leading British scientists. The authors partly draw on unpublished sources. According to one of these, Fisher, in 1934, stated that Barnard *won't learn anything* with E. S. Pearson, whereas Pearson told her the same with regard to Fisher. However, she attended lectures of both these scholars.

Zentralblatt MATH 704.01013

Fierz, Markus; Fierz, Martin: Zur Genauigkeit von Newton's Messung seiner Interferenzringe. Helv. Phys. Acta 67, 923 – 929 (1994)
The authors discuss Newton's study of the interference of light. Providing some calculations and comparing the result obtained with their own figure based on a modern estimate of the spectral receptivity of the eye, they conclude that Newton measured the diameter of an interference fringe with a precision of 0.002 or 0.01 *mm*. They admit, however, that their own figure is somewhat in error; they do not consider properly the number of significant figures in their calculations; and they assume that in 1670 the inch was practically the same as it is now *in Anbetracht der konservativen Haltung der*

Engländer. The authors defend their conclusion by stating that Tycho's observations were precise to 24" (which is doubtful) and that their relative precision (1/40,000) was equal to that of Newton's measurements. However, the last two figures do not tally and the term *relative precision* is hardly applicable to angle measurements.

Zentralblatt MATH 854.01011

De Finetti, Bruno: Cambridge probability theorists. Riv. Mat. Sci. Econ. e Soc. 8, 79 – 91 (1985)

This is an essay on Keynes, *Treatise on Probability* (1921) and Jeffreys, *Scientific Inference* (1931) [both sources reprinted, the second one before 1985]. The author discusses the relations between probability theory and logic; subjective probability (to which he himself, unlike his heroes, adheres); induction; and the principles of the calculus of probability.

He believes that the books which he discusses must not be ignored, that their sources are insufficiently known and that the *Cambridge philosophy* continues in the tradition of Locke, Berkeley and Hume.

Matematika 10A14

Fischer, Hans: Dirichlet's contributions to mathematical probability theory. Hist. Math. 21, 39 – 63 (1994)

The author mainly describes Dirichlet's unpublished courses on probability theory (1838 – 1846) which the latter began delivering in 1829. Dirichlet did not study either the "moral" applications of probability or its philosophical aspects and, while discussing the method of least squares, neglected its substantiations made by Gauss. He based his course on the integral calculus and, in proving the central limit theorem, presented it more rigorously than Chebyshev did in 1879/1880. However, the author does not remark that 1) Chebyshev himself noted that his derivation was not rigorous or that 2) Later (1887) he offered a much better substantiation.

Zentralblatt MATH, 795.01007

Fischer, Hans: J. F. Fries und die Grenzen der Wahrscheinlichkeitsrechnung. Festschrift for Ivo Schneider. Stuttgart, 2004, pp. 277 – 299

The author stresses the distinction between objective and subjective probabilities in the 18th and 19th centuries and notes that Poisson and Cournot attempted to distinguish between them. He then criticizes Poisson's stochastic study of the administration of justice and states that such applications of probability became objectionable because of ethical issues (actually, because Laplace and Poisson only studied ideal models, and because the public thought that statistical considerations applied to a given individual).

The author's main hero is Fries (1773 – 1843) with his contribution of 1842. He notes that Kries, who owed much to Kant, stressed the importance of subjective and philosophical (qualitative) probabilities and denied the universal applicability of stochastics, in particular because it was useless for making a single decision. With respect to the last-mentioned statement, I note that many scientists beginning with Newton kept to an opposite viewpoint. Then, Fries attempted to explain philosophically the stability of statistical means, criticized the application of probability to jurisprudence and called the principle of least squares arbitrary.

Finally the author explains the decline of probability theory after Laplace, but fails to mention its real causes (random variables were not studied as such; statisticians were denying the law of large numbers and were only dealing with the Bernoulli pattern; the creation of a truly mathematical theory remained impossible).

Zentralblatt MATH, 1072.01007

Forcina, Antonio; Giorgi, Giovanni Maria: Early Gini's contributions to inequality measurement and statistical inference. J. Élecron. Hist. Probab. Stat. 1, No. 1, Art. 3, 2005, 15 pp.

The authors mainly discuss Corrado Gini's (1884 – 1965) contributions to the measurement of economic inequality, the theory of statistical series and the notion of exchangeability. In the first field, Gini proved that Pareto's conclusion that the distribution of wealth persisted over space and time was wrong and proposed his own measures of inequality one of which (the concentration index) is still of interest.

The second subject is dealt with superficially; even Gini's debates with Bortkiewicz (in which the latter was apparently in the right) are not mentioned. The appearance of the idea of exchangeability in Gini's paper of 1911 was already reported by the first author in a discussion of a relevant article (*J. Roy. Stat. Soc*. A156, 1993). The notion itself is still to be properly attributed to Chuprov and his student J. Mordukh (Seneta, *Hist. Math*. 14, 1987).

Zentralblatt MATH, 1076.01026

Franklin, James: The Science of Conjecture. Evidence and Probability before Pascal. Baltimore, 2001

The author studies the history of the methods of dealing with uncertainty (p. ix) from antiquity to Huygens and Leibniz (rather than to Pascal) and pays special attention to the relevant qualitative stochastic reasoning. His book contains useful, sometimes hardly known information concerning law, philosophy, medicine, religion, and he argues that the Middle Ages were fruitful and important for the further development of science (and of probability theory in particular). The author also discusses astronomy (Copernicus, Galileo, Kepler), aleatory contracts, dice games and lotteries, again with the least possible use of numbers, and he describes an early solution of the problem of points (ca. 1400).

Many shortcomings are conspicuous. Ptolemy's reasonable treatment of direct observations is lamely dealt with; the idea underpinning the law of large numbers (Cardano, Kepler) is neglected and the fundamental problem of separating law from chance mentioned only in passing. Then, passed over are the links between the medieval doctrine of probabilism and non-additive probabilities (Jakob Bernoulli); between the qualitative approach to decision making and the very nature of ancient science, or the recently introduced assessment of expert estimations. The non-numerical "methods" of dealing with uncertainty are left non-systematized; moreover, they hardly exist, they should have been called principles, and connected strongly, not in the author's feeblest way, with Newton's rules of reasoning in philosophy.

Many sources and a host of commentators are quoted but the references are not alphabetically arranged, nor are the pertinent authors included in the index and in many cases the dates of the original publications are not provided. Documentation is often offered only in general, and some specific statements might be mistakenly attributed to Franklin himself.

Zentralblatt MATH, 996.01001

Freudenthal, Hans: Huygens' foundations of probability. Hist. Math. 7, 113 – 117 (1980)

The author discusses the terminology in the translations of Huygens' treatise of 1657 originally written in Dutch. He offers his own English translations of the piece in which Huygens introduced chance and its value (i. e., effectively, expectation) and he calls Huygens' considerations *quite sophisticated*. He also formulates one of Huygens' definitions in a modern way: *The expectation of a pay-off table is the money I need to propose a game with the given pay-off table as a fair one*.

Matematika 1A7

Garibaldi, U.; Penco, M. A.: Intensional vs extensional probabilities from their origins to Laplace. Hist. Math. 18, 16 – 35 (1991)

This is a study of an anonymous paper *Calculation of the credibility of human testimony* (1699) and its comparison with the relevant considerations of J. Bernoulli (with his *pure* and *mixed* arguments) and Laplace. The authors conclude that the 17th century notion of degree of certitude measures the correctness of the *internal state of the witness*. They do not explain the meaning of the adjective used in the title of their article and, while commenting on the contribution of J. Craig (1699), they fail to mention Stigler's recent interpretation of his mathematics.

Zentralblatt MATH 716.01013

Ghosh, J. K.: Mahalanobis and the art and science of statistics. The early days. Indian J. Hist. Sci. 29, 89 – 98 (1994)

Prasanta Chandra Mahalanobis (1893 – 1972), was a Fellow of the Royal Society, and a pioneer of the statistical science in his native country, India. His areas of work included multivariate analysis, sample surveys and philosophical problems of probability and statistics, as well as application of statistics to anthropometry, meteorology and flood control. His strong points were intuition and ability to use simple statistical tools.

Zentralblatt MATH 795.01023

Ghosh, J. K.; Maiti, P.; Rao, T. J.; Sinha, B. K.: Evolution of statistics in India. Intern. Stat. Rev. 67, 13 – 34 (1999)

The authors describe the development of statistics in India from the fourth century BC, when (apparently, in some regions) detailed data were collected on agriculture, economics, population; to the British period, when, in 1816, a comprehensive report covering ca. 15 *mln* people in the spirit of Staatswissenschaft was compiled, and when, in 1881, decennial censuses also including information on religion, geography and sociology have begun; and to the present days. The role of the Indian Statistical Institute and of Mahalanobis is emphasized, the main area of theoretical and applied statistical work as well as education in statistics and training of foreign students during the latest decades are described. The main impression (not unexpected) is that, as far as statistics is concerned, India is an advanced nation. The list of references is impressive but not at all comprehensive, even a paper of one of the authors on Mahalanobis is not mentioned there.

Zentralblatt MATH 927.01015

Gigerenzer, Gerd; Swijtink, Zeno; Porter, Theodore; Daston, Lorraine; Beatty, John; Krüger, Lorenz:The Empire of Chance. Cambridge; 1990.

This book is envisioned for a broad audience (p. xvi). Its main subjects are the history of classical probabilities up to the death of Poisson; of statistical probabilities, 1820 – 1900 (statistics, correlation, determinism); of scientific inference (analysis of variance, experimental design, significance testing, the controversy between Fisher and Neyman & E. S. Pearson); of the application of the statistical method to biology, physics, psychology, to the study of baseball, extrasensory perception, public opinion and to mental testing. The book ends by dwelling on determinism, probability and statistical inference. References take up some 33 pages with name and subject indices completing the account. The authors "used a lottery to order [their] names on the title page" (p. xvi).

A historically written *Empire of Chance* would include general historical accounts of 1) The mathematical theory of probability; 2) Statistics; 3) Mathematical statistics; 4) Applications of the statistical method. The exposition should hinge upon the history of the notion of randomness. In a general sense, the authors did organize their exposition according to this pattern, although perhaps they did not do it systematically enough.

The Theory of Probability. This theory studies the laws of chance, a fact that the authors did not mention directly. The assertion (p. 6) concerning the St. Petersburg paradox that mathematicians "anxiously amended definitions and postulates to restore harmony" with the outside world is strange because neither definitions, nor postulates need to be changed at all, and because they were not really changed. What could be, and perhaps was, changed, is the interpretation of a theory. And in this connection probability has the same relation to nature (or to such human activities as gambling) as mathematics in general.

There are many details where the authors are not as accurate as they might be. It is suggested that "the mathematics of the earliest formulation of probability theory was elementary" (p. 2) – but Bernoulli's law of large numbers is hardly "elementary". The treatment of the normal distribution is not always sound. For example, on p. xiv its history is stated as beginning in astronomy; on p. 62 the reader is told that the "error curve … of course [!] had been worked out in the context of gambling problems and error theory, but was first conceived as applicable to real variation by Quetelet". Finally, on p.

53 the formula of the standard normal distribution is said to be "invented by De Moivre and applied by Laplace to statistical matters". Actually, however, De Moivre, in 1733, was the first both to derive the normal distribution (in the general case!) and to apply it to studying the ratio of male/female births. The distinction between mean and probable durations of life is wrongly compared with the difference between usual and moral expectations (p. 22) and the probable error is improperly introduced on p. 82.

It is not indicated that the introduction of the notion of random variable, even in a heuristic sense, was only due to Poisson, that its systematic use did not begin before Chebyshev, and that, accordingly, early probabilists did not study densities (or characteristic functions) in their own right so that the theory of probability belonged to applied mathematics. This later statement indirectly follows from what is said in the book, but the authors were unable to explain this fact satisfactorily.

The central limit theorem is mentioned only once, and then only indirectly (p. 168). Laplace demonstrated it non-rigorously and used it in his theory of probability. He poetically described the action of this theorem in his *Essai philosophique sur les probabilités*.

By restricting themselves chronologically, the authors do not mention that Markov chains (to name only one mathematical object introduced after Chebyshev) greatly widened the possibility of statistical studies of nature.

Statistics and Mathematical Statistics. Again owing to chronological restrictions the history of political arithmetic is not studied. And some more space might have been found for *Staatswissenschaft*. Although it was not connected with chance, its history helps to picture the development of statistics proper. As far as it was concerned with figures, it had to do with counting objects rather than with estimating their number. In this respect it was akin to the 'numerical method' in medicine developed by French physicians (notably by Louis) by ca. 1825. The authors briefly discuss this method without indicating its connection with counting; moreover, the method is indirectly attributed to statistics proper, and not to be found in the subject index (pp. 46 – 47 and 129 – 130).

Quite appropriately, the authors' main statistical hero of the 19th century is Quetelet, but the description of his work is quite limited. First, they do not indicate that his failure to apply the Poisson law of large numbers greatly weakened his attempt to introduce the *homme moyen*. Second, the authors did not point out that Rehnisch[1] noticed serious mistakes in Quetelet's figures pertaining to crime. Third, they repeat the not altogether true, although generally accepted conclusion that Quetelet believed in the regularity of crime (pp. 43 – 44)[2]. In actual fact, Quetelet thought that society as a whole was responsible for criminality, that crime figures were determined in advance by social conditions. He did not say, but it followed, that these figures should, after all, change with time.

So much for population statistics. The account is continued by a non-mathematical description of the work of Galton on correlation and by studying the statistical critique of determinism. Both topics are connected with physics and biology and any apparently strict boundaries between the contents of several chapters are therefore eased, the more so since determinism and statistical inference are once more treated in the last of them.

In another chapter devoted to scientific inference the authors continue their account, this time centring it on the application of statistics in agriculture and astronomy (with remarks on the method of least squares being included) and bringing it up well into this century. The exposition is interesting, but the authors did not indicate that the Biometric school was established in order to link Darwinism and statistics[3] and they are rather brief on the work of the *Continental direction of statistics*. Only the work of Lexis, who originated this direction, is described. Poisson, who systematically estimated the significance of discrepancies between statistical figures, might be called the Godfather of the Continental direction, but his approach is not mentioned.

Applications of the Statistical method. In biology, the authors naturally study chance and its role in the evolution of species and the random drift of gene frequencies. Darwin and Mendel are prominently discussed and some space is given over to Lamarck and von Baer. In physics, the authors dwell on the limitations of its classical branches which were to lead to the introduction of randomness into that science, for example in radioactive decay and quantum mechanics. They also give some space to the method of least squares and mathematical treatment of observations, although the exposition is hardly suitable for the general reader. Regrettably chaos theory receives only a mention so what may be the most burning contemporary issue concerning randomness in physics and mechanics is left out. However, it would have indeed been difficult to compile a popular account of this theory (or, for that matter, of the whole subject).

A special chapter is devoted to psychology. The authors expound the situation from 1940 and almost to our days. At first, psychologists used statistics as a simple tool; then the ideas of Fisher and Neyman & E. S. Pearson became generally known (in a curious mixed form); finally the mind itself is now compared with an intuitive statistician [4]. Psychology thus became the third science under discussion after biology and physics, where probability is extremely important. The account is interesting especially since it covers present-day activities.

Other fields of statistical applications considered in the book (for example baseball) again belong to the areas quite recently occupied by statistics. There are also discussions of medical therapeutics, of jurisprudence, and of the attempts to rationalize the phenomenon of gambling.

Randomness. The authors naturally devote much attention to determinism and randomness; in the last chapter they even distinguish five types of the former, from metaphysical down to effective determinism, but they do not use their classification in the previous account. I take issue with them on several points.

Laplace was indeed a determinist (pp. 2, 11 and 277), but he also found room for chance[5]. Thus, he qualitatively explained the existence of trifling irregularities in the system of the world by the action of countless [small] differences between temperatures and between densities of the diverse parts of the planets, although it is true that he did not mention randomness[6]. Again, following several of his predecessors, Laplace held reasonable notions on the stability of statistical series, i. e., on the regularity of the total result of many random acts or events[7].

Finally, as an astronomer Laplace systematically estimated the significance of observations (without which he would have been unable to make many of his classical discoveries). I especially notice that Laplace's determinism did not influence Boltzmann who simply did not read (or at least did not even once refer to) him.

The authors believe that "oppressive scientific determinism seemed to follow" from several philosophers and scientists including Darwin (pp. 242 – 243). However, their remark is far from sufficient. Indeed, I myself have indicated that Darwin's theory of evolution might be qualitatively described by a random process[8]. Poincaré repeatedly strove to explain the notion of randomness[9] and a description of his attempts is sadly really missing.

References. The authors often refer to books without indicating the appropriate pages. There are also epigraphs which are impossible to check. References to some classics (Jakob Bernoulli, Gauss) are only given to the original editions of their works in Latin and Gauss' "Theoria combinationis" is not even mentioned. Collected works of Daniel Bernoulli and Fisher (and in one case of Laplace) are not referred to. And the list of references is not subdivided in any way so that its obvious value is partly lost.

Some Further Points with Which I Take Issue. That Talleyrand, in 1789, criticized the French national lottery as a tax upon unreasonable gamblers (p. 20) I do not deny, but Condorcet preceded him (with Laplace following suit in 1819) and Petty preceded them both[10]. The unnamed compiler of Halley's data on mortality (p. 20) was Caspar Neumann and Leibniz did *not* prompt him to begin this work[11]. Arbuthnot's and De Moivre's reasoning on the sex ratio at birth (p. 275) is described incorrectly. Darwin, in his *Origin of Species*, allegedly did not mention that even fit individuals could be killed (p. 66). However, on p. 86 of the 1859 edition he remarked that the accidental destruction of individuals might be "ever so heavy". The testimony of a statistician (of Alphonse Bertillon) was used in the notorious Dreyfus case and his arguments were indeed later discredited (p. 259). By implication, however, the reader is led to infer that the discredit was brought about upon statistical reasoning as such rather than upon Bertillon's specific arguments[12].

The book contains passages which are difficult to understand (pp. 21, 40, 167 and 229). On p. 40, for example, an unspecified Bernoulli is credited for something not really specified. On p. 50 I find *Manchestertum*, a word not included in ordinary dictionaries, and on p. 240 two names, obviously only familiar to American baseball fans, are mentioned. Style editing is badly needed on pp. 1, 80 and 171 and a few lines concerning one of Fisher's books (p. 92) are almost verbatim repeated on p. 118.

Jurisprudence is treated all too briefly. Among the new fields of application of the statistical method philanthropy is missing and meteorology and astronomy are not treated; accordingly, Lamarck does not receive due credit and such scholars as Buys Ballot, William Herschel, Humboldt, Kapteyn, or F. G. W. Struve are not even mentioned.

Overall, six pioneers have attempted the impossible: they really needed much more space and, consequently, time. Even as it is described, the empire of chance is enormously wide and the authors' decision to be collectively responsible for the entire book (p. 1) was unfortunate.

Notes

1. Sheynin, O. B. (1986), Quetelet as a statistician. *Arch. Hist. Ex. Sci.* (AHES), vol. 4, pp. 281 – 325, see §4.1.

2. I personally am also guilty in this respect.

3. There is some wavering in stating who founded this school (pp. 142 and 144).

4. In another chapter, jurors are compared with intuitive statisticians.

5. Quite correctly, the authors (p. 11) assert that the determinists "had carved out a place for chance in the natural and moral sciences", but they only mention De Moivre and they add that these determinists believed that variability would prove illusory "when fully investigated". However, it is too much to suppose that De Moivre (say) thought that the registered numbers of male and female births should be, in principle, exactly in the divine ratio (18:17). Not variability as such, but unlikely combinations of chance are [unlikely variability is] apt to disappear with a larger number of observations.

6. Laplace, P. S. (1894), *Exposition du système du monde. Oeuvr. Compl.*, t. 6, reprint of the edition of 1835. See p. 504.

Regrettably the authors did not cite Poincaré: "Dans chaque domaine, les lois précises ne décidaient pas de tout, elles traçaient seulement les limites entre lesquelles il était permis au hasard de se mouvoir". See his *Calcul des probabilités*. Paris, 1912, p. 1. The entire Introduction to which p. 1 belongs is a reprint of his article of 1907.

7. Cf. also my remark on Talleyrand below.

8. Sheynin, O. B. (1980), On the history of the statistical method in biology. AHES, vol. 22, pp. 323 – 371, see §5.1.

9. Sheynin, O. B. (1991), On Poincaré's work in probability. AHES, vol. 42, pp. 137 – 172, see §9. Cf. also Note 6.

10. Condorcet, M. J. A. N. Caritat de (1788), Des impôts volontaires et des impôts sur le luxe. *Oeuvr. Compl.*, t. 14. Brunswick – Paris, 1804, pp. 162 – 190, see p. 162.

Petty, W. (1662), A treatise on taxes and contributions. In his *Econ. Writings*, vol. 1. Cambridge, 1899, pp. 1 – 97, see p. 64.

11. Sheynin, O. B. (1977), Early history of the theory of probability. AHES, vol. 17, pp. 201 – 259, see §2.4.6.

12. The authors could have referred to Poincaré *lui-même*, who, in connection with the Dreyfus case, severely criticized Bertillon and came out against applying the theory of probability "aux sciences morales". History proved that, in the general sense, the great savant was wrong, as well as some earlier French scientists were. See

Sheynin, O. B. (1973), Finite random sums. AHES, vol. 9, pp. 275 – 305, see p. 296.

Physis, vol. 29, 1992, pp. 633 – 638

Godfroy-Génin, Anne-Sophie: Pascal. The geometry of chance. Math. Sci. Hum. 150, 7 – 39 (2000)

In this non-mathematical exposition the author stresses the legal nature of the problem of points solved by Pascal and Fermat; studies the difference between the Latin and the French versions of Pascal's *Traité du triangle arithmétique* (1665); notes an embryo of expectation contained there (*droit d'attendre*); and remarks that Pascal had not treated statistical probabilities (or chances). She adduced 65 references (three of them to Pascal) but mentioned only 11 of them.

Zentralblatt MATH 988.01002

Gnedenko, B. V.; Peres, M.-T.: On the history of the concept of random event. Voprosy Istorii Estesvozn. i Tekhniki No. 1, 71 – 75 (1984)
The authors trace the origin of the classical definition of probability and adduce a passage from Ostrogradsky's unpublished manuscript on the beginnings of the theory of probability. They indicate that it was Jakob Bernoulli, who introduced (somewhat informally) the classical definition and argue that it had been the investigations of Graunt and Petty which evoked both this fact and Bernoulli's application of statistical probabilities. [Bernoulli never cited Petty.]

Matematika 8A15

Good, I. J.: Some statistical applications of Poisson's work. Stat. Sci. 1, 157 – 180 (1986)
The author takes up some subjects treated by Poisson and competently traces their history up to the present days. Among these subjects are the two different kinds of probability (logical and objective); the law of large numbers; the summation formula which neither Poisson himself (1827) nor Cauchy (1817) ever put to statistical use; the Poisson distribution; judicial decisions.

The text includes a discussion by five authors and the author's rejoinder. One of these authors (Herbert Solomon) argues that Poissons's study of the work of the jury is an excellent example of using models in the behavioural sciences. The author does not mention that Cournot also distinguished between the two kinds of probability and he does not refer either to S. S. Demidov, Des parentheses de Poisson aux algèbres de Lie, in M. Métivier et al, ed., *S. D. Poisson et la science de son temps*, 1981 (Zbl 476.01001) or to the reviewers paper [*Arch. Hist. Ex. Sci*. 18, 245 – 300 (1978; Zbl 0383.010119)]

Zentralblatt MATH 611.60001

Grigorian, A. A.: The history and the philosophical and methodological foundations of R. von Mises' probability theory. Istor.-Matematich. Issled., ser. 2, 3(38), 198 – 220 (1999). In Russian
This is a superficial essay. The author heavily draws on Khinchin's relevant review of 1961 [Engl. transl.: *Science in context* 17, 391 – 422 (2004)] and indicates that Kolmogorov, in 1963, essentially softened his attitude towards the theory.

The essay contains numerous mistakes and ambiguities. Mises had indeed described his axiomatic natural scientific frequentist theory in his lectures of 1914, but he did not publish anything relevant until 1919, so that S. N. Bernstein (1917) [reprinted in his *Coll. Works*, vol. 4, 1964; Engl. transl. in *Probability and Statistics. Russian Papers of the Soviet Period*. Berlin, 2005, pp. 49 – 111] was the first to put out an axiomatic probability theory. Then, it is far-fetched to call *axiomatic* a theory not belonging to mathematics or physics. The dates of several publications (e. g., of Khinchin's review) are wrong;

Poisson is alleged to have applied his law of large numbers to dependent events, etc.

Zentralblatt MATH 969.01016

Gurzadyan, Vahe G.: Kolmogorov and Aleksandrov in Sevan monastery, Armenia, 1929. Math. Intell. 26, 40 – 43 (2004)
In 1929, Kolmogorov and his life-long friend P. S. Aleksandrov lived for about 20 days in a closed-down monastery on an island of Sevan in Armenia. While there, they completed some portions of their future (German) publications with Aleksandrov helping Kolmogorov with the language. They also climbed a summit of a mountain situated more than 2 km above Sevan *which did not present any complications* (Kolmogorov).

Zentralblatt MATH 1055.01017

Hacking, Ian. The emergence of probability. A philosophical study of early ideas about probability, induction and statistical inference, 2nd ed. Cambridge, Cambridge University Press, 2006

Review of fist edition (1975) see Zentralblatt MATH 0311.01004. This edition is its reprint with additional 23 unnumbered pages of “Introduction 2006” mentioning the usual set of related new books (a few of them unworthy and one undeservedly praised to the skies and notorious for slandering the memory of Gauss).

The book is written by a well-read author endowed with a good style. As stated in the earlier review, it describes the rapid growth of the (future) theory of probability since mid-17th century, the development of the dual concept of probability (statistical and subjective) beginning from signs and opinion and of the method of induction.

There is no generally accepted definition of philosophy, but in any case it reinterprets (at least discusses) concepts and principles, which the author had not even attempted. Then, *emergence* is not history, but he had to describe the history of his subject, although abandoning Aristotle (p. 17) and forgetting Levi ben Gerson (to whom the appearance of the method of induction is due) and Oresme (who discussed probability without defining it).

The missing philosophical and historical issues of considerable philosophical interest, some of which even belong to probability and/or statistics proper, include: hypotheses (and their discussion by Laplace); moral aspects of stochastic applications (only Pascal’s wager is described, but not the Petersburg paradox and the moral expectation, or the somewhat dangerous inoculation of smallpox, including religious objections to it); correlation; the Bayesian approach in statistics; true value of a measured constant; transition from mean values to frequencies; axiomatization versus frequentist theory; randomness; relevant problems posed by natural sciences.

Then, the history of probability is not separated into stages and its place in mathematics (pure or applied) is not discussed. De Moivre’s attempt to apply Newton’s philosophy for separating necessity from randomness (the initial aim of the theory of probability) is omitted, but klife annuities (although not the related moral problems) are for some reason treated (non-mathematically) in detail.

Jakob (called Jacques!) Bernoulli's law of large numbers is not adequately described and he is wrongly named as the last author to consider non-additive probabilities (p. 144) whereas the medieval doctrine of probabilism is not mentioned in this connection. Süssmilch is wrongly dismissed (p. 113). A mathematically mistaken proof of a conclusion made by Graunt is offered (p. 108), and the dates of publication of the memoirs of Arbuthnot, Daniel Bernoulli and Bayes are wrong (pp. 169, 125, 129). The book was a failure and its reprint is scandalous − unpublished sentence.
Zentralblatt MATH 1140.01007

Hald, A.: Nicholas Bernoulli theorem. Intern. Stat. Rev. 52, 93 – 99 (1984)
In 1713, N. Bernoulli communicated his theorem to Montmort. The latter had time to insert it in his *Essay d'analyse sur les jeux de hazard* (1713) before Jakob Bernoulli's *Ars Conjectandi* was published. The author notes that Nicholas essentially improved some intermediate estimates made by Jakob and concludes that Nicholas' achievement forms the "missing link" between the results due to Jakob and De Moivre.

In his Preface to the Russian translation of pt. 4 of the *Ars* (1913), Markov refused to recognize Nicholas' theorem because the latter had introduced an arbitrary assumption in estimating the ratio of some terms of a binomial. In turn, the author does not pay special attention to this assumption. While considering the precision of the Nicholas theorem he only adduces a numerical example. Finally, his account of the work of De Moivre on the subject is incomplete. One of my Russian articles which the author did not mention is partly devoted to the same theorem, see *Istoria i Metodologia Estesvennykh Nauk*, vol. 9, 1970, pp. 199 – 211.

Zentralblatt MATH, 563.60002

Hald, A.: On De Moivre's solutions of the problem of duration of play, 1708 – 1718. Arch. Hist. Ex. Sci. 38, 109 – 134 (1998)
In 1708 Montmort formulated a stochastic problem on the duration of play between two gamblers to be continued until one of them is ruined. The first to study this problem was Montmort himself and Niklaus Bernoulli. In 1712 and 1718, De Moivre published his own pertinent findings. The author briefly discusses the work of the first two scholars and describes De Moivre's contributions in detail and offers a reconstruction of the lacking demonstrations of De Moivre's formulas.

The most interesting of the author's conclusions concerns the probability that the game between two gamblers having an equal number of counters will not end in a given number of rounds. He believes that De Moivre issued from a certain recurrent relation and determined the functions sought as a linear combination of a finite number of finite geometric progressions.

In 1990 the author published his book *History of Prob. and Stat. and Their Applications before 1750* where the relevant sections carry an additional reference to Fieller (1931) who had studied some of De Moivre's pertinent findings.

Zentralblatt MATH 760.01003

Hald, A.: Pizzetti's contributions to the statistical analysis of normally distributed observations, 1891. Biometrika 87, 213 – 217 (2000)

The author describes how Pizzetti, in 1891, issuing from n independent and normally N(0; σ^2) distributed errors ε_i,

1) derived the chi-squared distribution with n degrees of freedom (already obtained by several authors).

2) Considering the residuals $e_i = \varepsilon_i - \bar{\varepsilon}$, calculated the corresponding distribution, the $\sigma^2\chi^2$ law with $(n - 1)$ degrees of freedom, already known to Helmert.

3) Generalized his account to a linear normal model obtaining the same distribution with the appropriate number of degrees of freedom.

4) Determined the confidence limits for σ^2 for the previous case. This result remained unknown until 1933.

5) Developed the one factor analysis of variance for the within and between series of observation (by then also known to several authors).

Zentralblatt MATH 949.01012

Hald, Anders: A history of parametric statistical inference from Bernoulli to Fisher, 1713 – 1935. New York (2007)

Hald directs his readers "for more proofs, references and information on related topics" to his previous books, *History of Probability and Statistics and Their Applications before 1750*. New York (1990) and *History of Mathematical Statistics from 1750 to 1930*. New York (1998); Zbl 0979.01012 and tells us that he borrowed about 50 pages from the second one. It is difficult to say what is essentially new, but at least it is only now possible to see at once what was contained in a certain memoir of Laplace (say). As always, Hald's exposition is on a high level and I doubt that it will be an "easy" reading for those who attended an "elementary course in probability and statistics". He concentrates on three "revolutions" in parametric statistical inference: Laplace, early memoirs; Laplace and Gauss, 1809 – 1828; and Fisher, 1912 – 1956 (note the closing date 1935 on the title!).

I take issue on many points. Jakob Bernoulli's classic did not become a "great inspiration" for statisticians (p. 14) until the turn of the 19th century. The cosine error distribution (p. 2) was one of the "most important"? Introduced by Lagrange, it was hardly ever applied. The statement (p. 4) that in 1799 the "problem of the arithmetic mean" was still unsolved, ought to be softened by mentioning the appropriate studies by Simpson and Lagrange. The integral of the exponential function of the negative square between infinite limits was first calculated by Euler rather than Laplace (pp. 38, 58). Legendre's memoir was neither clear nor concise (p. 53); he all but stated that the method of least squares (MLSq) provided the least interval of the possible errors, and he mentioned errors instead of residuals. In 1818 Bessel had indeed stated that observational errors were almost normal (pp. 58, 98), but in 1838 he dropped his reservation and provided a patently wrong explanation for the deviation from normality. Actually, he developed a happy-go-lucky trait, see my note Bessel: some remarks on his work. Hist. Scient. 10, 77 – 83 (2000).That Gauss, in 1809, had applied inverse probability (pp. 57, 58), is true, but Whittaker & Robinson, 1924, noted that this was already implied by the postulate of the mean. Two differing causes why Gauss abandoned his first justification of the MLSq (pp. 56 and 101) are both wrong. Much is reasonably said about Laplace's application of the central limit theorem, but its non-rigorous proof is left over in silence.

The Bibliography does not mention the collected works of Edgeworth, 1996, or the reprints of Poisson, 1837, Todhunter, 1865 or of K. Pearson's *Grammar of Science* after 1911. Missing are Montmort, 1713 (although referred to!), Gauss' collected German contributions on the MLSq, and Cramér, 1946, as well as the *Dict. Scient. Biogr.*, the *Enc. of Stat. Sciences*, and Prokhorov, Yu. V., ed., *Veroiatnost i Matematicheskaia Statistika. Enziklopedia* (Probability and Math. Stat. An Enc.). Moscow (1999). The unworthy books Porter, 1986, and Maistrov, 1974 are included, but my *Theory of Probability. Hist. Essay.* Berlin (2005), also at www.sheynin.de, which is incomparably better than Maistrov, is not.

Zentralblatt MATH, 1107.01006

Hall, Peter; Selinger, Ben: Statistical significance. Balancing evidence against doubt. Austr. J. Stat. 28, 354 – 370 (1986)
The authors enquire into the different approaches to statistical significance by professionals and laymen. Drawing on the views of K. Pearson, W. S. Gosset (*Student*) and R. A. Fisher, they explain the wide-spread acceptance of the 5% level of significance and emphasize that in many cases scientists and lawyers have to study evidence showing considerably more doubt.

They apparently object to any prior choice of a level of significance and recommend the use of more understandable odds ratio instead of, or along with this indicator. They do not mention that Jakob Bernoulli (1713) suggested that a certain probability be officially introduced in law courts, or that in 1840 Gavarret [the reviewer, *Arch. Hist. Ex. Sci*. 26, 241 – 286, p. 255 (1982)] proposed a certain level of significance for use in therapeutics.

Zentralblatt MATH 621.62002

Hashagen, Ulf: Wahrscheinlichkeitsberechnung für Ingenieure: Eine Fallstudie zur Institutionanalisierung und Unterrichtspraxis an Technischen Hochschulen. In: Seising, Rudolf, ed, et al, Form, Number, Order. Studies on the History of Science and Technology. Festschrift for Ivo Schneider. Stuttgart: Franz Steiner, 301 – 338 (2004)
The author describes the teaching of probability theory and the method of least squares in the Munich Technische Hochschule from its creation (1868) to 1929, notably by Seidel and Bauschinger. He provides documented information about the changing demands on these disciplines (considered alternatively as required or optional; necessary for general education or from the standpoint of practice) against the background of the general attitude in Germany towards pure versus applied mathematics and concerning the role of probability in mathematical education.

Zentralblatt MATH, 1072.01014

Havlová, Veronika; Mazliak, Laurent; Sisma, Pavel; Le début des relations mathématiques franco-tchécoslovaques vu á travers

la correspondance Hostinský – Frechet. J. Élecron. Hist. Probab. Stat. 1, No. 1, 2005, Article 4, 18 pp.
The ties between Bohuslav Hostinský (only date of birth, 1884, given) and Maurice Fréchet (1878 – 1973) are seen against the background of the cultural history of Europe and the beginning of their correspondence about 1919 is explained by the sympathy of the latter, then at Strasbourg, for an "autre terre libérée de l'impérialisme allemande". This political remark sems too strong, especially with respect to Hostinský's homeland, Czechoslovakia.

The correspondence itself, kept partly at Université Masaryk, Brno, and, apparently, at the Académie des Sciences, Paris, is not described sufficiently although the authors intend to continue their work. Except for general subjects (exchange of mathematical information), they only mention that Hostinský's work on geometric probability turned Fréchet to probability. They also describe Hostinský's biography. He graduated from the Philosophical faculty of the Czech University, Prague, in 1906; visited France in 1908/1909; began his research in several branches of mathematics in 1912; about 1919 became professor of physics in Brno; was influenced by Czuber and the lesser known E. Schoenbaum.

Zentralblatt MATH, 1062.01015

Heidelberger, Michael: Origins of the logical theory of probability: von Kries, Wittgenstein, Waismann. Int. Stud. Philos. Sci. 15, 177 – 188 (2001)
The author describes von Kries' *Principien de Wahtscheinlichkeits-Rechnung* (1886), Wittgenstein's *Tractatus* (1921) and Waismann's relevant work (1930 and later).

Kries distinguished between nomology and ontology and attempted to replace the obscure equipossibility inherent in the classical definition of probability by his *Spielraum* or range theory stating that probability is the appropriate Spielraum of possibilities. He foreshadowed Poincaré's explanation of uniform randomness by *arbitrary functions* and, without mentioning randomness, justified the stochastic kinetic theory by the principle of small causes leading to large effects. Boltzmann (1886) [who is known for his uncertain attitude towards randomness] attributed to him a *logical justification* of stochastic calculations. Forgetting Jakob Bernoulli and many other scholars up to Venn, Heidelberger implicitly calls Kries the originator of the logical theory of probability and fails to mention that von Mises denied Kries.

Turning to Wittgenstein and Waismann, he notes that they *amputated* the physical component of the Kries theory and alleges that they thus missed an opportunity for constructing an empirical alternative to the frequentist theory of probability. Finally, he remarks that Waismann generalized the concept of Spielraum to propositions and rejected Mises.

Zentralblatt MATH 1027.01007

Herr, David G.: On the history of the use of geometry in the general linear model. Amer. Statist. 34, No. 1, 43 – 47 (1980)
Let y be an n-dimensional vector of observations, b – a k-dimensional

($k \leq n$) vector of parameters, both situated in Euclidean space R^n, and X a given matrix n by k. It is required to determine b in accordance with a linear model $y = Xb$ + error.

The author, who does not claim to be comprehensive, compares the algebraic and geometric approaches to this problem, and to some adjoining ones and briefly considers the works belonging to the latter from one of Fisher's papers of 1915 onward. He argues that the importance of the geometric method for mathematical statistics is certainly undervalued owing to existing traditions and disregard of analytic geometry as well as due to widespread imagined or real lack of geometric vision and inability to conceive abstractly.

Matematika 1980, 11A6

Higgs, Edward: The general register office and the tabulation of data, 1837 – 1939. In: Campbell-Kelly, Martin, ed., et al, The History of Mathematical Tables. From Sumer to Spreadsheets. Oxford: Oxford Univ. Press, 209 – 232 (2003)

The Office was established in 1837 for supervising the statistics of the movement of population of England and Wales with Farr being it superintendent of statistics until 1879. The author describes the difficulties in the work of the Office and especially the unavoidable simplification of data. Even in 1911, as he notes, it had to assume that the life of the population was simple, and deaths, uncomplicated. Complexity has been *reintroduced* in the 1930s together with the application of the elements of correlation theory.

In 1858, the Office began using, partly successfully, the printing unit of the Babbage difference engine, and in 1870 it acquired an arithmometer; after 1890, Hollerith tabulators came into use.

Zentralblatt MATH 1063.01012

Hochkirchen, Th.: Wahrscheinlichkeitsrechnung im Spannungsfeld von Maß- und Häufigkeitstheorie – Leben und Werk des "Deutschen" Mathematikers Erhard Tornier 1894 – 1982. N. T. M. (N. S.) 6, 22 – 41 (1998)

The author describes the life and work of Tornier showing his mathematics as a "vermittelndes Element" between the axiomatic and the frequentist theories of probability. His direct work lasted for ten years only (1929 – 1939) after which he was retired because of unbecoming private behaviour (apparently caused by bad psychological health), but later (when exactly?) Tornier corresponded with Hilda Geiringer, the assistant and wife of von Mises, and influenced the posthumous edition (1964) of Mises' treatise prepared by her.

From 1932 Tornier was a card-carrying Nazi. He was instrumental in ousting Feller from Kiel University (1933) and, in 1936, contrasted applicable theories with "judisch-liberalistischer Vernebelung" achieved by "logisch geschlossener" constructions. Khinchin (1961, posthumous publication) believed that Tornier, by partly abandoning the irregularity of Mises' Kollektiv, saved the frequentist theory but still left it inexpedient as compared with the axiomatic theory.

Zentralblatt MATH, 1064.01535

Hoeffding, Wassily: The Collected Works. Editor N. I. Fisher & P. K. Sen. New York, 1994

Hoeffding (1914 – 1991) was born in Petersburg and educated in Berlin, but lived since 1945 in the USA. The book contains reprints of 51 of his contributions and their ad hoc reviews (K. Oosterhoff and W. van Zwet, W. Hoeffding's work in the sixties; G. Simons, The impact of W. Hoeffding's work on sequential analysis; and P. K. Sen, the impact of W. Hoeffding's research on nonparametrics). No list of Hoeffding's publications is provided, but, except for a mimeo report (1963) mentioned on p. 53, neither his own references, nor those of his reviewers include any missed article. The reprints include three German papers (1940 – 1942) translated here into English, five entries from the *Enc. Stat. Sciences*, six book reviews, and Hoeffding's autobiography (1982).

Zentralblatt MATH, 807.01034

Holgate, P.: Waring and Sylvester on random algebraic equations. Biometrika 73, 228 – 231 (1986)

This is a description of E. Waring's (1782) and J. J. Sylvester's (1864 and 1865) probability-theoretic studies of the number of real roots of algebraic equations. Waring stated many findings without demonstration and some of them remain doubtful; some of the others were obviously based on assumptions which do not hold. Sylvester studied *superlinear equations* $\sum \varepsilon_i u_i^m = 0$, $u_i = a_i + b_i$, $b_i \geq 0$, $\varepsilon_i = -1$ or 1 and he regarded each equation as chosen at random from a set of equations. His work led him to consider *runs in a ring*. While considering a problem concerned with the mutual arrangement of four *random* points, he gave thought to the idea of *genuine* randomness. Among related material is Michell's problem on the scatter of stars with discussions and the calculation of the probability that a random fraction might be reduced (Dirichlet; Chebyshev; its prehistory dates back to Oresme).

In 1836, Buniakovsky calculated the probability that a quadratic equation with integral coefficients chosen at random from numbers $\pm 1, \pm 2, \ldots, \pm m$ has real roots.

Zentralblatt MATH 598.01004

Howie, David: Interpreting Probability. Controversies and Developments in the Early Twentieth Century. Cambridge (2002)

The author's main subject is the fate of the Bayesian approach in the first half of the 20th century. He describes the relevant work and opinion of Fisher and Jeffreys making available unpublished material concerning the latter any pays attention to the application of probability to physics and biology and to general scientific problems (simplicity of the laws of nature). No clear definitions of the main notions (inverse probability, principle of insufficient reason) are offered which means that his readers do not need them, but then the author provides a definition of an effective estimator, and a wrong one at that (p. 66). He forgets that Liapunov proved the central limit theorem (p. 216) and does not know (p. 219) that dialectical materialism recognizes the connection between necessity and randomness. His use of rare words (to decouple, p. 216; to laud, p. 225) is regrettable.

The previous history of probability theory as discussed in a preliminary chapter is a complete failure. Several from among the 15 mistakes noticed by me concern our classics (Graunt, p. 15; de Moivre, p. 20; Poisson, p. 20, who *tinkered* with calculations, p. 29; and Newton, who allegedly thought that the system of the world was stable rather than needing regular Divine reformation, pp. 27 and 200). Some quotations are given without any references being adduced (p. 32, and on p. 54 Mendel is called a Czech monk. Mendel was always considered as of Czech – German origin, but he was German and in 1945 the descendants of his relatives were driven out of the then Czechoslovakia (W. Mann, grandson of Mendel's nephew, private communication).

Zentralblatt MATH 1031.01012

Ibragimov, I. A.: On S. N. Bernstein's work in probability. Transl., ser. 2, Amer. Math. Soc. 205, 83 – 104 (2002). Transl. from Trudy St-Petersb. Mat. Obshch. 8, 96 – 120 (2000)

Two aspects of Bernstein's work, viz., an axiomatic justification of probability theory (1917) and a study of limit theorems for sums of random variables, are discussed. Such directions as mathematical statistics and application of probability to heredity are left out.

Following Glivenko (1939), the author states that both Bernstein's and Kolmogorov's approaches to the first problem *adopted the structure of normed Boolean algebras as the basis of probability theory*. In the second field, Bernstein achieved fundamental results in generalizing and furthering the discoveries of Markov and Liapunov, and subsequent authors, both in Russia and elsewhere, continued his investigations. In particular, he introduced a new class of random processes that at least sometimes is called after him.

The author stresses Bernstein's unusual attitude towards some mathematical constructions. Thus, he was dissatisfied with the notion of convergence almost everywhere. Some references lack page numbers.

Zentralblatt MATH 1037.01009

Ineichen, Robert: Zufall und Wahrscheinlichkeit – einst ganz getrennt, jetzt eng verbunden Elem. Math. 54, 1 – 14 (1999)

The author discusses the early history of games of chance (including the problem of points) and notes that the concept of probability was introduced later that the notion of expectation. He defends the thesis formulated as the title of his paper and believes that Jakob Bernoulli was the first major figure to bring together randomness and probability. However, Aristoteles thought that a random event had a logical or subjective probability less than 1/2; the *Laws of Manu* (ancient India) and the Talmud actually understand random events as such that possess low probabilities; etc, see my article in *Annals Sci.* 55, 185 – 198 (1998). Nevertheless, I agree that probability was definitely quantified only by Bernoulli.

Zentralblatt MATH 940.60008

Ineichen, Robert: Chancen im Zahlenlotto – die frühesten Berechnungen. Mitt. Dtsch. Math.-Ver. No. 2, 12 – 13 (2000)

This is a description of Juan Caramuel y Lobkowitz' discussion (1670) of the classical Genoese lottery. He correctly calculated the probability (without formally defining this concept) that a gambler will guess several numbers drawn in any, or in a given succession out of a hundred. Caramuel failed to solve more difficult related problems and on this point the author refers to his earlier articles. At least in one of these, “Juan Caramuels Behandlung der Würfelspiele und des Zahlenlottos”, NTM 7, 21 – 30 (1999), he discussed all the stochastic findings of Caramuel including those which he describes now.

Math. Rev. 2001f:01027

Ineichen, Robert: “Es ist wie bei den Spielen” – Nicole Oresme und sein Beitrag in der Vorgeschichte der Stochastik. NTM (N. S.) 9, 137 – 151 (2001)
The author discusses Oresme’s *De proportionibus proportionum* and *Ad pauca respicientes* (Latin – Engl. edition by E. Grant, Madison – London, 1966). He expounds Oresme’s notion of commensurability and use of *rations* (relations rather than quantities) which led him to the introduction of positive fractional exponents and he attributes to Oresme an actual understanding of probability, both epistemic and aleatory, and an elementary scale of the probable.

It is difficult to say what exactly is new in this paper. In any case even the Talmud stipulated the ratios of forbidden/allowed food in mixtures, i. e., the corresponding numerical probabilities, whereas scales of logical or subjective probabilities go back to Aristotle.

Zentralblatt MATH 1010.01010

Ineichen, Robert: Die ersten kombinatorischen Untersuchungen zum Zahlenlotto. Die Beiträge von Juan Caramuel y Lobkowitz und Frenicle de Bessy. In: Seising, Rudolf, ed., et al, Form, Number, Order. … [see bibl. inform. in review of Hashagen]**, 257 – 267**
This is a description of the work of Caramuel (1606 – 1682) published in 1670 and of a posthumous contribution of de Bessy (1605 – 1675). Here is Caramuel’s main problem. Given, natural numbers 1, 2, …, p from which sets of five different natural numbers are chosen. How many such sets are needed for two given different natural numbers, both less than p, to occur in one of them?

De Bessy compared the theoretically possible gain of a gambler participating in a lottery with the ratio of the favourable cases to the unfavourable ones for the *banquier*.

The author partly repeated his earlier paper, Juan Caramuels Behandlung der Würfelspiele und des Zahlenlottos. NTM, 7, 21 – 30 (1999).

Zentralblatt MATH 1072.01009

Jongmans, François; Seneta, Eugene: The Bienaymé family history from archival materials and background to the turning-point test. Bull. Soc. R. Sci. Liège 62, 121 – 145 (1993)
Continuing their earlier work made together with B. Bru [1992, see above] and drawing on additional archival sources, the authors describe the lives of Bienaymé, of his ancestors, posterity, and other relatives. They discovered Bienaymé’s previously unknown note (1861) on the numerical solution of equations by Stevin and maintain that Bienaymé played a prominent part in connecting Sylvester with other French mathematicians. The authors also discuss Bienaymé’s turning-point test for randomness. In addition to its description in C. C. Heyde and E. Seneta, *Bienaymé, Stat. theory anticipated* (1977; Zbl

371.01010), the authors describe the relevant work of Liagre and Bertrand and attempt at a reconstruction of Bienaymé's proof (which he did not publish).

Zentralblatt MATH 792.01023

Jongmans, François; Seneta, Eugene: A probabilistic "new principle" of the 19th century. Arch. Hist. Ex. Sci. 47, 93 – 102 (1994)

The *new principle* is E. Catalan's theorem (1877) stating that unknown modifications of the causes of a random event do not change its probability. The authors discuss Catalan's relevant papers of 1841 and 1877 as well as his later work (1886) where he specified his theorem; reveal their connection with one of Poisson's urn problems (1837) and with the work of other French mathematicians; and show that, when generalized, the Catalan problem leads to a martingale. The authors also describe a pertinent unpublished letter (1878) from Bienaymé to Catalan which contains a phrase *Beyond mathematical reasoning, everything in the world is only probabilities, or even just conjectures*.

Zentralblatt MATH 802.01003

Kallianpur, G.: Random reflections. In Ghosh, J, K., ed., et al, Glimpses of India's Statistical heritage. New Delhi, pp. 47 – 66 (1993)

This is a scientific autobiography complemented by a list of Kallianpur's works but the date of his birth is not given. The author graduated from the Univ. of North Carolina (one of teachers was Hotelling), worked at Berkeley and Princeton and returned to India in 1953. He worked at the Indian Statistical Institute (ISI) and was its Director in 1976 – 1978, then, in 1979, joining his Alma Mater. The author also provides information about several scholars. Mahalanobis established a liberal atmosphere at the ISI, but his *autocratic ruling* led to *controlled chaos*; and, being a physicist, he was *impatient with the* [high] *level of rigor and abstraction* in mathematics. Einstein (ca. 1948) was genuinely interested to know about the new developments in probability theory; and Wiener, to whom the author is in *profound scientific debt*, claimed that he was a descendant of Maimonides.

Zentralblatt MATH 829.01020

Kalman, R. E.: Probability and science. Nieuw Arch. Wiskd., IV. ser, 11, 51 – 66 (1993)

This is a non-mathematical lecture. The author states that the applications of probability to problems of the real world made during the last few decades were often too abstract and that there is no interaction between the notions of probability and chaos as considered in scientific literature. He defines randomness as lack of complete uniqueness in the appropriate data and notes accordingly that $\sqrt{2}$ is a *random number*. The author mistakenly dates one of Daniel Bernoulli's memoirs.

Zentralblatt MATH 785.01033

Kassler, Jamie C.: The emergence of probability reconsidered. Arch. Intern. Hist. Sci. 36, No. 116, 17 – 44 (1986)

The authoress describes the origin of stochastic ideas in astronomy. In this connection she pays attention to the rule of composing music and stresses the importance of the combinatorial aspects of the Cartesian mechanical philosophy. While putting forward arguments in favour of both commensurability and incommensurability of the motions of celestial bodies, Oresme (14th century) substantiated the former by *testimony* of *wise men* and based the latter on higher probability. From the beginning of the 14th century music disregarded restrictions imposed by the Pythagorean theory of propositions. In the authoress' opinion, this was a shift from order to defective order, a notion which she considers to be akin to randomness. Music theorists studied the art of combinations (Mersenne, 1623) while random composition of melodies dates back to the 1670s.

Zentralblatt MATH 658.01005

Katasonov, V. N.: Genesis of the theory of probability in the context of ideological searches of the 17th century. Voprosy Istorii Estestvozn. Tekn. No. 3, 43 – 58 (1992). In Russian

The author intends to prove that science in general and the theory of probability in particular *only arranges some cultural space ..., as given by more fundamental acts of man's spiritual self-determination*. He touches several aspects of the early probability theory and makes a few mistaken or dubious statements. His contribution is hardly useful.

Zentralblatt MATH 783.01003

Kendall, M. G.; Doig, A. G.: Bibliography of Statistical Literature Pre-1940 with Supplements to the Volumes for 1940 – 1949 and 1950 – 1958. Edinburgh, 1968

This is vol. 3 of the entire *Bibliography* covering the period until 1958; the first two volumes appeared in 1962 and 1965. No further volumes are planned since in 1959 the International Statistical Institute began publishing an abstracting journal now called *Statistical Theory and Methods Abstracts*. According to the authors' aims and methodology as described in vol. 1, the *Bibliography* includes almost all the articles from 12 main periodicals and a number of papers from 42 other journals. In addition, the authors made use of the bibliographies appended to many papers and of the abstracting journals (although not of the Soviet *Matematika*). They believe to have covered 95% of the existing articles on statistics and its applications.

Each volume of the *Bibliography* is actually an author index (no subject indices are provided). The literature published in Russian and several other languages is described in English, French or German. In all, this vol. 3 lists about 10 thousand monographs and articles separated into two time intervals, – before 1900 and from 1900 to 1939 (2,360 and 7,630 items respectively) as well as 148 sources for 1940 – 1949 and about 1,170 for 1950 – 1958. All the books entered here had appeared before 1900. Neither the second part, nor the first two volumes include any books, which is in line with the practice of the abovementioned quarterly. This is an essential setback but the *Bibliography* is nevertheless very valuable.

Vol. 3 is also useful for historians of mathematics since it lists classical works (of Laplace, Gauss et al) including writings of such authors for whom probability was a minor subject (Euler), forgotten writings of eminent mathematicians, commentaries and essays, translations of various works into any of the three main languages.

There are some shortcomings. The selected literature, even of the 20th century, was not checked *in visu*; likely because of the general direction of the *Bibliography* there are hardly any references to collected works; of the 14 writings of Euler included in t. 7 of his *Opera omnia*, ser. 1 (1923) and pertaining to probability and statistics the authors listed only seven, and one of these called *Wahrscheinlichkeitsrechnung* either does not exist or is wrongly named; the descriptions contain mistakes and inaccuracies (Süssmilch's *Göttliche Ordnung* first appeared in 1741, then in 1761 – 1762, but not in 1788; the second part of Daniel Bernoulli's "Mensura sortis" (1771) is omitted); and cross-references are lacking. Finally, the spelling Ladislaus von Bortkiewicz as given in the second part does not coincide with that in the first part, Vladislav Bortkevich. Having emigrated from Russia to Germany in 1901 and being a nobleman, he changed his name accordingly, but that fact is not explained.

In 1962, the authors estimated that about a thousand articles on their subject were being published yearly. This means that already now it would be expedient to issue a bibliography for 1959 – 1970. Neither abstracting journals, nor their cumulative author indices are substitutes for bibliographies (to be compiled in the first place by scanning such sources). I also believe that a single bibliography for 1900 – 1970 with books being certainly included is also needed.

NKzR, A1969, No. 10, pp. 21 – 24

Kolmogorov, A. N.: On the notions of quantity and number. Istor.- Matematich. Issled. 32/33, 474 – 484 (1990). In Russian
This is a discourse on the notion of number and quantity (magnitude) and on the commensurability of magnitudes. The author intended to continue his work, but obviously did not.

Zentralblatt MATH 728.01012

Abramov, A. M.; Tikhomirov, V. M.: A commentary to the work of A. N. Kolmogorov [just above]. **Ibidem, 484 – 487.** In Russian
The authors explain that Kolmogorov's discourse likely written during his student years in 1923 was discovered (by whom?) among his posthumous papers. They themselves supplied its title and they note that Kolmogorov returned to the notions of quantity (magnitude) and number in his other contributions. In his *Vvedenie v Analis* (Intro. to Analysis), Moscow, 1966, Kolmogorov showed that the theory of real numbers can be constructed by issuing from the notion of magnitude.

Zentralblatt MATH 728.01020

Kreith, Kurt: Euclid turns to probability. Intern. J. Math. Educ. Sci. Technol. 20, 345 – 351 (1989)
This is an attempt to show how Euclid could have constructed the elements of probability theory without, however, any indication of limit regularities. Assuming that the theory would have been based on the axiomatic method, the author points out that the difficulty would have consisted in defining independent events. He believes that Euclid could have introduced non-independence rather than dependence with

the product of the probabilities of events A and B being either higher, or lower than the probability of AB. This approach, the author maintains, would have been similar to Euclid's wording of the Parallel Postulate which discussed non-parallelism rather than parallelism.

Zentralblatt MATH 691.01001

Krengel, Ulrich: Von der Bestimmung von Planetenbahnen zur modernen Statistik. Math. Semesterber. 53, 1 – 16 (2006)
This is an essay on Gauss' decisive role in the discovery and development of the method of least squares with a short description of further pertinent events from Laplace to modern findings. The author believes that Gauss was the first who *die eingangs gegebene Begriffsbestimmung des mathematischen Statistikers voll erfüllte*. He does not refer to my much more detailed papers of 1999, *Hist. Scientiarum* 8, 249 – 264 or *Jahrbücher f. Nationalökonomie u. Stat*. 219, 458 – 467, and some of his statements should be commented upon. Thus, it is doubtful that Gauss knew De Moivre's derivation of the normal law and Laplace had not at all proved (several versions of) the central limit theorem rigorously. Finally, the author refers to Stigler but passes over in silence his dreadful and slandering accusations such as *Gauss solicited reluctant testimony from friends that he had told them of the method* (of least squares) *before 1805*; see his *History of Statistics*, 1986 (not 1981 as cited by the author), p. 145.

Zentralblatt MATH, 1101.01008

Kunert, Joachim; Montag, Astrid; Pöhlmann, Sigrid: The quincunx. History and mathematics. Stat. Papers 42, 143 – 169 (2001)
A quincunx is an arrangement of five objects, four of them at the vertices of a square or rectangle, and the fifth at its *centre*. About 1873 Fr. Galton invented a simple device which he called quincunx. It showed that shot, falling through an array of pins, collected in a figure resembling a normal curve.

The authors describe Galton's work at the time; argue that the quincunx was his natural-scientific approach to the central limit theorem (CLT); dwell on the generalizations of that devise (Galton himself; Pearson in 1895); and provide an appropriate mathematical background. They did not remark that the conditions for the CLT established at the time were less restrictive than Galton thought (p. 149) and their expression (p. 159) *The percentage of balls … converges to infinity* was unfortunate. That Galton invented identification by fingerprints (p. 144) is wrong: he had predecessors (*New Enc. Brit.*, 15th ed., vol. 4, article *Fingerprints*).

Zentralblatt MATH 986.01015

Kupper, Josef: Versicherungsmathematik und schweizerische Hochschule. Mitt., Schweiz. Aktuarver. No. 1, 33 – 53 (1998)
This is a review of the history of actuarial science and its teaching in Switzerland. Beginning with Jakob and Niklaus Bernoulli (the latter studied the application of probability to jurisporudence and compiled the first Swiss mortality table) the author describes the work of several of his compatriots, especially G. A. Zeuner (1828 – 1907), and touches on Euler's pertinent findings. He maintains that the actuarial science really began to develop in Switzerland about 50 years ago

because of higher demands on its mathematical foundation and of the advances in various kinds of insurance other than insurance of life.

Zentralblatt MATH 905.01012

Lancaster, H. O.: Bibliographies of Statistical Bibliographies. Edinburgh, 1968

The book was written on contract with the International Statistical Institute. It reflects the literature published before 1965 – 1966 in the main pertinent periodicals, abstracting journals included, some general mathematical periodicals and other types of publications as well as such fundamental sources as the *British Museum Catalogue*. The contents of the book are wider than its title since bibliographies of bibliographies only make up its insignificant part.

Chapt. 1 (Personal bibliographies) lists the books and articles devoted to some 330 eminent scholars, mainly those mentioned in fundamental writings and bibliographies and honoured by invited collected papers. Thus, six sources have to do with Gauss, eleven, with Laplace, and three, with Kolmogorov. Also here are the collected works of such scholars who strongly but indirectly influenced statistics (Darwin) and who mainly worked beyond this science (Euler). Finally, also included are authors of writings on combinatorial analysis.

Chapt. 2 (Subject bibliographies) lists about a thousand sources – bibliographies and writings of a more general nature published mostly during the latest 10 – 15 years. Apart from literature pertaining to various applications of probability and statistics, there are items belonging to other mathematical disciplines, such as Fourier analysis and theory of graphs. This breadth of contents is naturally seen in a long (13 pp.) subject index to both these chapters. Here are some of its main headings: Accident proneness; Analysis, mathematical; Astronomy; Canonical variables. The author explains that Chapt. 2 covers such subjects that are often taught "in a department of statistics" or closely associated with these. An index of authors to the same chapters is also provided.

The book will undoubtedly be useful for statisticians and (its Chapt. 1) historians of mathematics. Chapt. 2 is of a mixed character and its volume is not so large as to impede its reading. The index of national bibliographies is apparently comprehensive enough but international bibliographies are not listed alongside, although, for example, two volumes of the celebrated Kendall & Doig bibliographies are included in Chapt. 2. Soviet literature is sufficiently represented but there are no references to the Soviet abstracting journal *Matematika*.

NKzR, A1968, No. 9, pp. 23 – 25

Lancaster, H. O.: Statistical Society of New South Wales. Austr. J. Stat. B30, 99 – 109 (1988)

Nine early Australian statisticians are mentioned and the work of two of them (E. J. G. Pitman and C. H. Wickens) are briefly described. The establishment of the Statistical Society of New South Wales (after 1962, the NSW branch of the Statistical Society of Australia) is discussed. The work of its special groups; symposia held; general meetings; and the publication of its Bulletin (now, the *Australian Journal of Statistics*) are examined. Readers will find only indirect indications on the dates of the creation of this Society (1948) and of the Australian Society (1962).

Zentralblatt MATH 704.01024

Laplace, Pierre-Simon: Philosophical Essay on Probabilities. Transl. from the 5th French edition of 1825 by Andrew I. Dale. Berlin, 1995

In addition to the translation itself (showing the changes between the first and the last editions of the *Essai philosophique sur les probabilités*), the book provides extensive notes (with proper borrowings from those of the German translation of 1932 and the French reissue of 1986), a bibliography (ca. 250 items) and a Glossary (which includes tiny biographies of scholars). The English text seems good enough although some words are hardly well-chosen (whither, p. 1; ad hoc-eries, p. 121). The Notes pertain to general history, mathematics and astronomy. They are helpful, but modern developments are not always described (e. g., those concerning the Petersburg paradox or the Daniel Bernoulli – Laplace – Ehrenfests' urn model). The Bibliography is defective in that a) It is often restricted to initial editions; thus, neither later editions, nor the translations of Jakob Bernoulli's *Ars Conjectandi* are included). b) It contains explicit or tacit mistakes (the date of Arbuthnot's note is given as 1710; and it is not stated that William Herschel's *Scient. Papers* were issued in two volumes). The Glossary is again helpful although it has its own shortcomings. Tycho was indeed "the greatest pre-telescopic observer", but why not add that without him there would have been no Keplerian laws? And the term triangulation is explained wrongly. [Many other glaring mistakes and omissions there.]

For many decades, perhaps from 1850 to 1930, Laplace's work in probability (and his *Essai* as well) was forgotten. Instead, the general public regrettably turned over to Quetelet and even natural scientists abandoned Laplace. Boltzmann, who referred to Kant, Darwin and many other scholars, did not mention him at all. The present translation helps to see probability in its historical perspective and is therefore valuable.

Zentralblatt MATH, 810.01015

Lausch, Hans: Moses Mendelssohn. "Wir müssen uns auf Wahrscheinlichkeiten stützen". Acta Hist. Leopold. No. 27, 201 – 213 (1997)

The author discusses Mendelssohn's (1728 – 1786) papers of 1756 (revised in 1761) and 1785. In the first of these, Mendelssohn stated without proof that, if two events coincided n times in succession, the probability of the coincidences being determinate was $n/(n + 1)$. The author notes that the (Price – Buffon – Laplace) calculations of the probability of the next sunrise yield almost the same result, but does not notice that Mendelssohn, who hardly thought about subtle points concerning prior distributions, could have regarded his problem as identical with the one treated in his second source.

There, again without justification, Mendelssohn maintained that in n tosses of a coin the probability of heads occurring at least once was

$n/(n + 1)$. The author connects this statement with D'Alembert's notorious conclusion (1754) that in two tosses of a coin the probability of the same event was 2/3. (Indeed, this particular case may easily be generalized to n trials.)

The author also quotes a passage from Mendelssohn's first paper. It is similar to Laplace's later pronouncement that the theory of probability owes its origin to the feebleness of the human mind.

Math. Rev., 1998k:01008

Leha, G.: Wahrscheinlichkeitstheorie und das Postulat der beliebigen Wiederholbarkeit. Jahrb. Überblicke Mathematik 1983, 81 – 94 (1983)

The author points out that Gibbs was the first to base stochastic reasoning on the possibility of infinitely many repetitions of events. Indicating that an approach of this kind is not sufficient, he also stresses the importance of a statistical approach, i. e., of estimating parameters of laws of distribution according to one or another criterion. In this connection he pays particular attention to Gauss' derivation and use of the normal distribution in the theory of errors (1809).

In 1823 Gauss renounced the use of this derivation. The author only indirectly acknowledges this fact and his argumentation is thus incomplete.

Zentralblatt MATH 512.60001

Leti, Giuseppe: The birth of statistics and the origins of the new natural science. Metron 58, No. 3 – 4, 185 – 211 (2000)

The author sketches the history of statistics up to the 19th century. He believes that the same causes occasioned both its birth and the origin of modern natural sciences; notes Sébastien Le Prestre Vauban's priority (1686) in suggesting a national census; and describes the prehistory of the Staatswissenschaft, or university statistics (Italy, 16th and early 17th centuries). The merging of the two main branches of statistics is indirectly dated as ca. 1800 (actually, it occurred many decades later) and Leibniz' work in political arithmetic is ignored.

Vauban's role in the general development of statistics is greatly exaggerated but at the same time his sampling study of the agricultural production in France is not mentioned.

Zentralblatt MATH

Levy, Philip: Charles Spearman's contributions to test theory. Br. J. Math. Stat. Psychol. 48, 221 – 235 (1995)

The author examines Spearman's English writings of 1904 – 1913 on the correction of correlation for errors of measurement; his German paper (*Z. Psychol.* 44, 1906, together with F. Krüger, listed by Doig & Kendall, *Bibl. Stat. Lit*., 1968) is not mentioned. He also discusses the criticisms levelled against Spearman by Karl Pearson and William Brown and describes the positive modern appraisal of Spearman's work which was also important for the history of factor analysis.

Zentralblatt MATH 921.01036

Lewin, Christopher; de Valois, Margaret: History of actuarial tables. In: Campbell-Kelly, Martin, ed., et al [see review of Higgs]**, 79 – 103**

This short essay describes the appearance of tables of compound interest (Trenchant, 1558; Stevin, 1585) and mortality tables (Graunt, 1662; Halley, 1694; et al) and explains several methods of compiling the latter. Events in the US and Russia are however left out. The authors note that in 1829 Finlaison formulated important questions concerning the possible existence of a law of mortality and that its several formulas (now discarded) were proposed in the 19th century. They pay some attention to sickness tables and multiple decrement tables (for population decrease owing to several causes). Their general source was *History of Actuarial Science*, 10 vols, ed. Steven Haberman et al. London, 1995.

Zentralblatt MATH 1063.01013

Loveland, Jeff: Buffon, the certainty of sunrise, and the probabilistic reduction ad absurdum. Arch. Hist. Ex. Sci. 55, 465-477 (2001)

The author discusses the problem of the probability of the next sunrise as treated by R. Price (1764) and especially G.-L. Buffon in his *Essai d'arithmétique morale* (1777). He considers 1. The origin of the problem (thought experiments; the feelings of an ignorant person observing a succession of identical events; philosophical conclusions about such events and about sunrises in particular). Several scholars are mentioned, e. g., Locke, Leibniz, Pascal, Hume, E. B. de Condillac.

2. The previous work of Buffon. The author believes that Buffon's simple astronomical calculations of 1749 could have provided the model for computing the probability of the sunrise. 3. The possiblility that Buffon compiled his *Essai* much earlier than 1777, and likely before 1764. I note that the notion of geometric probability also discussed in the *Essai* was described in an anonymous note in the *Histoire* of the Paris Academy, année 1733 (1735). 4. The difference between the formulas provided by Price, Buffon and Laplace.

The date of Arbuthnot's note of 1712 is mistakenly stated as 1710.

Zentralblatt MATH 978.01022

Lysenko, V. I.: The method of least squares in Russia in the 19th century. Istor.-Matematich. Issled. 2 ser., 5 (40), 333 – 361 (2000). In Russian

The author outlines the pertinent classical work and the Russian writings of the 19th century. He makes many mistakes, barely refers to present day foreign research and often provides lengthy quotations instead of offering his own comments. The essay can be useful because of its bibliography that lists Russian contributions of the first half of that century as well as lesser known later sources.

Zentralblatt MATH 970.01009

Mackenzie, Donald A.: Arthur Black, a forgotten pioneer of mathematical statistics. Biometrika 64, 613 – 616 (1977)

Independently from the founders of the Biometric school, Black (1851 – 1893) aimed at constructing a quantitative evolution theory. He had no time for publishing anything, but his extant MSS contain a study of the polynomial distribution and an independent derivation of the Poisson distribution. The MS of Black's main work, *Algebra of*

Animal Evolution, is lost but the problem of estimating a certain multiple integral was published in 1898.

Matematika 6A21

Mclean, Ian: Thomas Harriot on combinations. Rev. Hist. Math. 11, 57 – 88 (2005)
Thomas Harriot (1560 (?) – 1621) was a mathematician and natural philosopher. The author studies Harriot's manuscripts pertaining to the application of combinations to language (anagrams), atomism and mathematics in the context of the late Renaissance opposed mentalities (occult and scientific). He concludes that Harriot had investigated combinations in the abstract (mathematical) spirit.

The author had not attempted to describe comprehensively Harriot's mathematical achievements. He did no cite Harriot's posthumous *Artis analyticae praxis*. London, 1631, see A. P. Youshkevich, Arithmetic and algebra, in *Matematika s Drevneishikh Vremen do Nachala 19-go Stoletia*, vol. 2. Moscow, 1970, 22 – 53, or the several pertinent contributions of J. A. Lohne mentioned by A. W. Edwards, *Pascal's Arithmetic Triangle*. Oxford, 2002, who listed them and whom the author refers to with regard to the elements of the number theory.

Zentralblatt MATH 1083.01009

Malaguerra, Carlo: Stefano Franscini. From statistics to simple truths. In: Proc. 51st Session, Intern. Stat. Inst., Istanbul, 1997, vol. 1. Voorburg, 71 – 74 (1997)
The author describes the life and the work of Franscini (1796 – 1857), a Swiss educationist and, mostly, statistician. He published several books, organized and carried out the first national census (1850) and contributed to the development of a common national awareness. Working alone and unacknowledged in his native country, he advocated knowledge through measurement and inspired the creation of the federal university.

Zentralblatt MATH 914.01019

Markov, A. A.: Extension of the law of large numbers to quantities depending on each other. J. Électron. Hist. Probab. Stat. 2, No. 1b, Article 10, 12 pp. (2006)
This is a reprint, possibly warranted by Markov's anniversary (he was born in 1856), from the original Russian text of 1906 rather from Markov's *Izbrannye Trudy* (Selected Works). No place, 1951. The text is understandably written in accordance with the old system of spelling which is not conducive to its study, and is not accompanied by commentaries written in 1951.

Markov indicated sufficient conditions for the law of large numbers to be applicable to the sums of dependent random variables; in particular, to those connected into a simple homogeneous Markov chain. It was in this memoir that the author first introduced his "chains". The term "Markov chain" is apparently due to S. N. Bernstein, Sur l'extension du théorème limite du calcul des probabilités. *Math. Annalen* 97, 1 – 59 (1926), beginning of section 16.

Markov also offered an example of dependent and bounded variables not obeying the law of large numbers but he ended his memoir by stating an important general corollary: Independence of variables is not necessary for the law to remain valid.

An English translation of the memoir is available in my collected translations *Probability and Statistics. Russian papers*. Berlin, 2004, also at www.sheynin.de.

Martin, Thierry: La valeur objective du calcul des probabilités selon Cournot. Math. Inf. Sci. Hum., No. 127, 5 – 17 (1994)
The author considers Cournot's work on probability. He is mainly concerned with the principle of negligible probabilities and discusses it from the philosophical point of view. He does not indicate that the concept of moral certainty (i. e., of the moral impossibility of the complementary event) was introduced by Descartes in 1644, in the *Logique des Port-Royal* in 1662 and upheld by Jakob Bernoulli [or that in 1777 Buffon suggested 1/10,000 as a negligible probability].

Zentralblatt MATH, 821.01015

Martin, Thierry: Probabilités et philosophie des mathématiques chez Cournot. Rev. Hist. Math. 1, 111 – 138 (1995)
The author stresses that Cournot, like Poisson before him, distinguished between subjective and objective probabilities and thus elevated the theory of probability to the realm of pure mathematics (without achieving its *transformation profonde*). Actually, however, the theory remained in the domain of applied mathematics since, until the beginning of the 20th century, densities or characteristic functions did not become objects of study per se. The author also discusses Cournot's attitude towards mathematics and its interrelation with reality as well as towards the theory of knowledge as related to mathematics. At the very least, Cournot was in this respect closer to modern ideas than Engels who defined mathematics as a science of quantifying nature and whose thoughts fettered Soviet mathematicians. A related paper is L. Daston, How probabilities came to be objective and subjective, *Hist. Math.* 21, 330 – 344 (1994).

Zentralblatt MATH 822.01002

Meusnier, Norbert: La passé de l'esperance. L'émergence d'une mathématique du probable au XVIIème siècle. Math. Inf. Sci. Hum. 131, 5 – 28 (1995)
This article belongs in the first place to philosophy. The author is verbose, quotes too many known passages and hardly makes any original findings.

Zentralblatt MATH 854.01010

Nikulin, M. S.: On L. N. Bolshev's result in the theory of testing statistical hypotheses. Zap. Nauchn. Seminar Leningr. Otd. Mat. Inst. Steklova 153, 129 – 137 (1986). In Russian
In 1976, Bolshev, in his lectures at Moscow State Univ., generalized the Neyman – Pearson theorem on hypothesis testing. The author published Bolshev's result since the latter died (in 1978) without doing it himself.

Suppose that a random variable has density $p_i(x)$, the corresponding hypothesis being H_i, $i = 1, 2$. Using the ratio $p_2(x)/p_1(x)$ Bolshev derived an optimal decision function $D(x)$ which leads to H_i with probabilities $D_i(x)$ and, with another probability, to refusal of distinguishing between H_1 and H_2. The test $D(x)$ is such that, given the boundaries for the probabilities of wrongly favouring H_2 instead of H_1 and vice versa, the unconditional probability of arriving at a wrong decision is minimal.

Zentralblatt MATH 623.62018

Ondar, Kh. O.: A short description of the unpublished correspondence between Markov and Chuprov (1910 – 1917). Proc. XIII Intern. Congr. Hist. Sci. 1971, section 5. Moscow, 163 – 165 (1974). In Russian

Markov and Chuprov discussed a number of important issues (the Lexis test for stability of statistical series; the Bortkiewicz law of small numbers; the Pearson chi-squared test; random variables weakly depending one on another) and thus influenced each other.

Matematika 1975, 1A61

Ondar, Kh. O.: On the first applications of probability theory to medicine. Istoria i Metodologia Estestven. Nauk 14, 159 – 166 (1973)

The author describes the work of Russian physicians P. D. Enko (1873) and K. V. Tovstitsky (1906) who solved some of their problems by applying statistical and stochastic methods. Their studies included the comparison of empirical and theoretical (calculated in accordance with the binomial law) frequencies, estimation of parameters of empirical functions by least squares, application of Laplacean formulas. Similar investigations in Western Europe are not considered.

Matematika

Ondar, Kh. O.: On the influence of Markov and Chuprov on each other in scientific methodology. Ibidem 16, 154 – 158 (1974)

The author discusses the (then yet) unpublished correspondence between Markov and Chuprov, which, as he states, contains more than a hundred letters.

[In 1977, Ondar published 105 of these letters (translated in 1981). I have found many mistakes in his presentation as well as 13 more letters and published this material in 1990 (translated in 1996: *Chuprov: Life, Work, Correspondence*. Göttingen, 1996 and 2011).]

Matematika 6A42

Parmentier, Marc: Concepts juridiques et probabilistes chez Leibniz. Rev. Hist. Sci. 46, 439 – 485 (1993)

The author connects Leibniz' philosophy of inductive reasoning and studies in jurisprudence with the notions of degrees of proof and expectation noting however that the former was not altogether quantitative. Thus, testimonies should be estimated, not enumerated. Estimation was essential for Leibniz: his celebrated statement that 1 – 1 + 1 – 1 + … = 1/2, which is the mean between 0 and 1, was a metaphysical estimation. Leibniz' reasoning on some moral problems did not contradict the later notion of moral expectation.

The author does not discuss the history of non-additive probabilities or Leibniz' refusal to accept Jakob Bernoulli's law of large numbers.

Zentralblatt MATH 804.01004

E. S. Pearson: 'Student'. A Statistical Biography of William Sealy Gosset. Editors, R. L. Plackettt, G. A. Barnard. Oxford, 1990

Gosset (1876 – 1937), alias Student, "the Faraday of statistics", as Fisher is reported to have called him, was active in many areas of statistics and he additionally influenced Karl Pearson, Fisher, and Egon Pearson by his correspondence and contacts with them. It is difficult to imagine biometry developing into (a branch of) mathematical statistics without Gosset's participation.

The book describes his life, work and correspondence with the three main chapters properly given over to his relations with the abovementioned scholars respectively. The book is generously interspersed with passages from Gosset's correspondence and a helpful general commentary is provided. However, the "Student distribution" is not written out and Gosset's part in establishing the independence of the sample parameters of the normal distribution is not described. And contemporary Russian statisticians are virtually non-existent. Then, the Editors should have indicated what exactly is new as compared with Egon Pearson's articles of 1939 and 1968. Gosset's (or rather Student's) *Collected Papers* (1942 and 1958) are listed in the Bibliography, but his individual articles are not, and this is a serious deficiency. References to several contributions by Laplace and Gauss are given without mentioning their collected works.

Math. Rev., 1994k:62001

E. S. Pearson, M. G. Kendall, Editors: Studies in the History of Statistics and Probability. London, 1970

This is a collection of reprints of 29 papers published 1906 – 1968, mostly in *Biometrika*. These may be separated under three headings: the prehistory; the 17th and 18th centuries; and the Biometric school. As the Editors say in their Preface, English statisticians became interested in the history of their science after Karl Pearson, in the 1920's, had given a series of pertinent lectures, and they hope that these lectures will be available [published in 1978].

Among others, the first group of papers includes F. N. David, Dicing and gaming; M. G. Kendall, The beginnings of a probability calculus, and Where shall the history of statistics begin; and A. M. Hasover, Random mechanisms in Talmudic literature. David believes that religious ceremony and superstition had impeded the origin of the theory of probability; any attempt at forecasting the throw of dice for purposes of divination would have been interpreted as impiety. Kendall, in his first paper mentioned, is of the same opinion. He also remarked that in the 16th c. the Catholic Church had banned insurance of life. However, in the 18th c., scientists, who had always striven to cognize the laws of nature, began to apply stochastic reasoning. Hasover indicates that the casting of lots was made use of in Judaism and for the division of Israel. In his second paper Kendall maintained that political arithmetic including insurance of life actually originated in 1660 (i. e., with John Graunt [who had not however studied insurance]). Without denying the fundamental importance of Graunt's work I add that a sample estimation of harvest is known to have been made in 1648 [1] [and that in England sampling for assaying the new coinage goes back to the 13th c.].

In the second group I single out the papers of M. Greenwood, Medical statistics from Graunt to Farr (a detailed description of the work of Graunt, Petty, Halley, of a number of English statisticians up to Farr inclusively, and of Struick, Deparcieux and Süssmilch); R. L. Plackett, The principle of the arithmetic mean (the treatment of astronomical observations by Ptolemy, Tycho Brahe, the memoirs of Simpson and Lagrange); A. R. Thatcher, On the early solutions of the problem of the duration of play (De Moivre, Niklaus Bernoulli, Montmort); E. Royston, On the history of the graphical representation of data (statistical diagrams of A. F. W. Crome and W. Playfair); Kendall, Th. Young on coincidences (a derivation of the Poisson law with unit parameter in 1819); Todhunter's *History* (a short biography of Todhunter in connection with its centenary); and Edgeworth; H. L. Seal, Historical development of the Gauss linear model; Sheynin, On the early history of the law of large numbers; and Karl Pearson, Notes on the history of correlation.

The articles of Plackett, Thatcher, Royston and Kendall's second paper are very short. Plackett does not reveal Simpson's part in the error theory and does not at all mention Lambert. Thatcher has not sufficiently described De Moivre's achievements and Royston's narrative is too restrictive: she does not consider the so-called tabular direction in Staatswissenschaft, nor does she say that graphs of statistical data included those of empirical distribution functions (Huygens, 1669). In the history of probability Todhunter is known not less than Laplace is in probability proper. Kendall argues that Todhunter's book is important for contemporary readers and lists the other works of his hero.

Edgeworth (1845 – 1926) was one of the first to apply mathematics in economics and he also published many writings on the theory of probability, statistics and error theory. He was Pearson's predecessor in that he paved the way for the spread of the ideas of the Biometric school. [His collected works appeared in three volumes in 1996.]

Seal provided a broad essay on the findings of Gauss, Cauchy, Bienaymé, Chebyshev, Karl Pearson, Fisher and other scholars. He formulated interesting conclusions including a passage about the reasons for the insufficient use of the theory of errors by the founders of mathematical statistics. Regrettably, he did not study the 18th c. when linear methods first came to be widely used for treating observations.

Pearson devoted his paper to correlation in the classical error theory and in Galton's work. He made an interesting statement about the different understanding of independence in the theory and in mathematical statistics. This is only one of the aspects describing the gap that gradually took shape between these two disciplines. I indicate Pearson's disappointing mistake (p. 185): Gauss based the theory of errors on the normal law in 1809 rather than in 1823 – 1826.

I especially mention that the book includes the reprint of the first part of Bayes' Essay towards solving a problem in the doctrine of chances (1764) with a biographical note by G. A. Barnard and a translation of Daniel Bernoulli's memoir (1778) with Euler's commentary of the same year and an introductory note by Kendall.

A great many books were written about Bayes' philosophical concepts, but his memoir is hardly known. For some reason pt. 2 of the memoir (1765) is attributed here to Price (p. 133) who had indeed communicated both parts (after Bayes' death) and supplemented them by lengthy commentaries. In pt. 1 Bayes for the first time applied the B distribution. In his pt. 2 he considered the case of a large number of trials and he could have arrived at a limit theorem (but apparently did not want to). Also there he introduced curves later called after Pearson (Types I and II).

In studying the treatment of observations, Bernoulli formulated the principle of maximum likelihood (due to Lambert). Assuming that the distribution of errors was an arc of a parabola, he arrived at a statistic for which the posterior weights of the observations increased to the tails of the arc. This would have appeared unusual, but Euler mistakenly concluded that the weights possessed a contrary property. Rejecting maximum likelihood but retaining Bernoulli's distribution law, he estimated the location parameter sought by means of a statistic which, practically speaking, led to the arithmetic mean and [indirectly] to the principle of least squares.

The third group of papers includes a number of important writings on the history of the Biometric school (detailed biographies and description of the work of leading scientists, continuity of ideas).

The book lacks indices. There are no references to later literature or to the other pertinent papers in *Biometrika*. Nevertheless, it is undoubtedly valuable not only for historians of mathematics, but also, as it seems, for statisticians.

Akty Khoziastva Boiarina V. I. Morozova (Documents of the Boyar Morozov Economy), pt. 1. Moscow – Leningrad, 1940, p. 100.

NKzR, A1971, No. 9, pp. 21 – 24

Pechenkin, A. A.: Mandelstam – R. von Mises correspondence. Istor.-Matematich. Issled., Ser 2, 4 (39), 269 – 276 (1999)
The author describes the extant correspondence (27 letters from the physicist Mandelstam, 1879 – 1944, and 12 from his wife, written in 1918 – 1937, to Mises, none from Mises) kept at Harvard Univ., and the cordial relations between the two scholars who first met in 1909 at Strasbourg.

Mises influenced Mandelstam both as a Machian and as the originator of the frequentist theory of probability. The author believes that the correspondence was discontinued because of the Great Terror (1936 – 1938) and notes that in 1927 – 1928 Mandelstam supervised the work of Boris Hessen (executed in 1937) on the Mises concept of probability.

Zentralblatt MATH 970.01017

Pelzer, Hans: Detection of errors in the functional adjustment model. Deutsche Geodät. Kommission Bayer. Akad. Wiss., A98, 61 – 70 (1983)
The author discusses least square adjustment of geodetic data containing systematic errors. In case the distrurbance parameters may be considered as random variables with zero expectations and known covariance matrix, they can be included in the adjustment of indirect observations. If information on the parameters is lacking, there is a possibility of applying a significance test for their presence; a simple indicator of these disturbances is provided by the ratio of the empirical

variance of unit weight to its theoretical value. The latter quantity (σ_0^2), however, is rarely known. Finally, when conditional observations are adjusted, the author recommends studying discrepancies (w_i) and, in particular, testing their approximate normality: $w_i \sim N(0; \sigma_0^2)$.

Zentralblatt MATH 534.62070

von Plato, Jan: Creating Modern probability. Its Mathematics, Physics and Philosophy in Historical Perspective. Cambridge (1995)

The subject of this book is probability from 1900 onward with emphysis being laid on statistical physics, quantum theory, Mises' frequentist theory, the measure-theoretic approach and subjective probability and exchangeability. A supplement on Oresme's understanding of the relative frequencies of rational and irrational numbers is appended. The author looked up many sources in Russian and Swedish and some archival materials.

The history of random processes is not studied comprehensively, chaos theory is left out and explanatory notes for non-physicists are missing. The main deficiencies, however, stem from the author's superficial knowledge of the history of classical probability and tacit refusal to search for continuity between old and new. Then, there are many repetitions of statements, many linguistic errors and the sentences are often short and jerky.

Examples of mistakes and omissions: Buffon needle problem *of 1777* (p. 5) is several decades older; Boole and Lomnicki are not mentioned in discussing the history of axiomatizing probability (p. 32); the notion of true value is not obsolete (p. 73); metrologists still use it having independently defined it (as Fourier did) as the mean of an infinitely large number of observations; the Ehrenfests' urn model (p. 92) was first considered by D. Bernoulli, then by Laplace; Markov (pp. 132 – 133) had begun work on his *chains* in 1906 rather than in 1908, and the term *Markov chains* appeared in 1926 rather than in the 1930s; the probability of the next sunrise (p. 165) was first discussed by Price; an erroreous description of the Poisson theorem by Mises is repeated without comment (p. 182); normal numbers (p. 193) were intuitively anticipated by Lambert; the history of exchangeability (p. 246) should begin with Chuprov (Seneta 1987).

The author avoids referring to the reviewers papers on Newton (p. 5) and Poincaré (p. 170) and excessively praises another author (Schneider, see Zbl 681.01001).

Zentralblatt MATH 829.01012

Porter, Theodore M.: The Rise of Statistical Thinking 1820 – 1900. Princeton (The University Presses of Columbia & Princeton), 1986

The book consists of four parts: The social calculus (political arithmetic – the rise of statistics in the 1820s – Quetelet and Buckle – English scholars of the mid-century – Cournot – Fries); The supreme law of unreason (the normal law – the study of variations – the penetration of the statistical method into physics (Maxwell and Boltzmann) and biology (Galton)); The science of uncertainty (criticisms of Quetelet – the free will – the time's arrow – Peirce's philosophy); and Polymathy and discipline (various points of view about statistics – its connection with the theory of errors – the study of statistical series (Dormoy and Lexis) – Edgeworth – the Biometric school (Galton and Pearson)).

The author pays special attention to the social and political background against which statistics had developed and to the ideological views of his heroes. This is the most [the only] valuable feature of his book. Together with C. C. Gillispie, I. Hacking and D. Mackenzie he follows the 'social' line originated by K. Pearson. However, I take issue about many points.

The arrangement of the material is such that many subjects are discussed discontinuously; there is no general list of references, and, in a nasty tradition, the exact sources of the epigraphs are not given. Some assertions are repeated in part (on Fourier, pp. 28 and 97, on Galton, pp. 8, 139, 271); other remarks are even contradictory (on Quetelet, pp. 42 and 46, on the founders of mathematical statistics, pp. 3, 68, 312, 314) so that the author does not present a precise view on some important subjects, witness also his discussion of amassing observations, pp. 152, 155, 162, and the lack of his own definition of statistics.

The exposition could have been more coherent. De Moivre's ideas on statistical regularity (p. 50) are not linked with his understanding of randomness; the recognition of such regularity by Dickens is regarded with surprise (p. 57) although later Tolstoy and Dostoevsky expressed similar thoughts; Fourier is unreservedly called a physicist (p. 28); Pearson's idea of causation being the limiting form of correlation (p. 298) is only mentioned in passing. The influence of Poisson, Bienaymé, Chuprov and Markov is not studied (cf. below).

Several branches of science (astronomy, medicine, meteorology) are treated insufficiently; thus, the study of statistical regularities in the solar and stellar systems and that of correlative relations in medicine in 1865 – 1866 are not taken up, and the disciplines which emerged in the 19th century and were (and are) directly connected with statistics, such as climatology, geography of plants, epidemiology, public hygiene and stellar statistics are not even mentioned.

The exploratory data analysis is not mentioned either, although it is now considered as an integral part of statistical studies. The introduction of isotherms (Humboldt) and the discovery of anticyclones (Galton) were the fruits of this analysis.

The work of Quetelet is explained faultily. That he carefully studied the writings of the French scholars (p. 43) is a mistake. The author does not improve Quetelet's notion of the *homme moyen* as it is usually done by referring to the Poisson form of the law of large numbers; and neither Quetelet's religious views or his urge to unify population statistics are mentioned.

Mathematics and its history is rendered much too inaccurately. When discussing the difference between the theory of errors and mathematical statistics, the author says nothing about estimating the parameters of distributions; the studies of the coefficient of dispersion by Chuprov and Markov are dismissed as being purely mathematical (p. 254) whereas exactly these studies allowed a rigorous use of this coefficient and thus constituted a contribution to early mathematical statistics. Historical remarks on the theory of errors (pp. 236, 245, 266, 295) are either wrong or leave a false impression; some of Laplace's thoughts are described incorrectly (pp. 73, 94); De Morgan's remarks on the benefits of insurance (p. 76) are not traced to Laplace; the first appearance of the normal distribution and De Moivre's results and ideas are described wrongly (pp. 93, 94) and the coining of the term itself is not attributed to Peirce (p. 13); Maxwell's statistical research is incorrectly even if tentatively connected with his study of Saturn's rings (p. 124); the distribution of the free paths of molecules is wrongly identified with the Poisson law (p. 117) etc, etc. Six dates are wrong (pp. 12, 95, 247) and in some instances the mathematical expressions are careless (pp. 96, 117, 271). Graphical methods of statistics are not discussed.

There are no references to Humboldt; Chuprov (cf. above) and Kendall are forgotten. From my series of papers in the *Archive for History of Exact Sciences* on the history of statistical method only two out of the four published before 1985 are mentioned – politely, but not really used. I am compelled to say that the book might mislead the uninitiated and that its importance is limited [the book is at best useless].

Centaurus, vol. 31, 1988, pp. 171 – 172

Porter, Theodore M.: Statistics and physical theories. In Nye, Mary Jo, ed. The Modern Physical and Mathematical Sciences. Cambridge, 488 - 504 (2003)

This is an unworthy essay dealing with the work of Maxwell and Boltzmann and emphasizing that these scholars noted the similarity of molecular regularities with those discovered in moral statistics. However, they never attributed free will to molecules, and, more to the point, Boltzmann also remarked on the similarity between physics and the movement of population. And lacking here is the statement that the connecting link was the regularity inherent in mass random events.

There are many more superficial utterings which I am now complementing. Thus, the assumptions introduced by Maxwell when deriving his distribution were weakened by Kac and Linnik (independently). Clausius was content to introduce the mean velocity of molecules, but, at that time, the transition from mean values and states to distributions was just beginning in many branches of natural sciences then being penetrated by statistics. Two different physical definitions of probability were indeed introduced, but the ensuing ergodic hypothesis is not mentioned. Admiring Maxwell, Boltzmann was nevertheless dissatisfied with the shortness of his contributions.

Boltzmann invoked probability to confine uncertainty; yes, but stochastic considerations are indeed aimed at discovering the laws of chance, so this statement tells us nothing new. Quetelet was a bureaucratic reformer? Perhaps conservatively inclined, but he was convinced that statistics could foster social development and believed in a near better future for mankind.

Zentralblatt MATH, to appear

Porter, Theodore M.: Karl Pearson's Utopia of scientific education. From graphical statics to mathematical statistics. In: Seising, Rudolf, ed., et al, Form, Number, Order [see further bibl. inform. in Hashagen]**, 339 – 352 (2004)**
The author states that at the beginning of his career Pearson strove to transform technical education into a union of teaching and research and that he chose geometry in general and geometric statics in particular as a suitable tool for his goal. Then Pearson offered statistics as a wide field for applying graphical methods and began his studies of biological problems by geometrical means. Porter told much the same story in his book (see next Item). On p. 339 the author indirectly called Pearson rather than Fisher the founder of modern mathematical statistics which is quite wrong.

Zentralblatt MATH 1072.01016

Porter, Theodore M.: Karl Pearson. The Scientific Life in a Statistical Age. Princeton and Oxford: Princeton University Press, 2004. Pp. viii + 342.
Born 150 years ago, Pearson (1857 – 1936) was an English applied mathematician, biologist and philosopher, but, above all, the cofounder of biometry, the main branch of the later mathematical statistics.

In 1875 Pearson entered King's College in Cambridge and took his degree with mathematical honours in 1879. In 1877, he entered a period of religious doubts and began to study philosophy, especially Spinoza and German philosophers. Until 1884 he had also been undertaking literary, historical and political efforts and came to regard science as description of phenomena. Porter (p. 64) believes that Pearson reached this Machian conclusion all by himself.

In 1880 Pearson began calling himself a socialist, soon exchanged a few letters with Marx, thought of translating *Das Kapital* (the author declined) and was studying the social and economic role of religion, especially in medieval Germany. These pursuits led Pearson to consider, in 1880 – 1884, the possibility of lecturing in German literature and history at Cambridge and in any case in 1882 he supported himself by lectures on German medieval and Reformation history and the role of science and religion in society. Religion he defined as the relation of the finite to the infinite (Porter, p. 111). Porter (p. 93) remarks that Pearson "was a born historian" and that his pertinent writings were "deeply researched and startlingly original". He (p. 118) also tells us that "at this time Pearson was immensely busy with the most exciting mathematical work of his life" but provides neither its date (perhaps 1883) nor title and I did not find anything suitable.

In 1884 Pearson became Professor of applied mathematics at University College London. Next year he established a *Men and Women's Club* which existed until 1889 and discussed all issues concerning women and the relations between the sexes.

During these years up to roughly 1893 Pearson actively worked on mathematical physics and stated extremely interesting ideas ("negative matter" exists in the universe; "all atoms … appear to have begun pulsating at the same moment"; gravity results from the curvature of space) but he did not mention the Riemannian space. Porter cites some of these statements but does not connect them with modern concepts. Thus, on the contrary, curvature of space is now thought to result from forces acting there.

As to his professorial duties, Pearson widely used graphical methods in statics and "as a corollary" (Porter, p. 216) began to investigate the same methods in statistics which he came to consider as a general scientific tool and thus certainly useful and conforming to his ideas about broad learning. "In the early 1890s statistics was especially appealing to him as a bastion of support for the creed of science" (Porter, p. 288).

Pearson continued in the same vein after having been appointed, in 1891, Professor of geometry at Gresham College in London. Soon, however, "evolutionary discussions" (Porter, p. 237) with the zoologist Weldon and Galton's contributions turned Pearson's attention to biology and to eugenics in particular, hence to its study by statistical means. In eugenics, Pearson advocated scientific planning, reasonably thought that "nature was more powerful than nurture" and endorsed state intervention in human reproductive decisions (Porter, pp. 280 and 278). Following now is my own discussion of Pearson's work in statistics.

At the very end of the 19th century the much older Galton, Pearson and Weldon established the Biometric school that aimed at justifying natural selection by statistical studies. Weldon, however, died in 1906 and Pearson became the head of the new school and chief (and for many years the sole) Editor of their celebrated periodical, *Biometrika*. In 1901, an editorial in its first issue stated that "the problem of evolution is a problem in statistics"; although Darwin's theory of descent lacked mathematical conceptions, his every idea "seems at once to fit itself to mathematical definition and to demand statistical analysis". Much later Pearson (1923, p. 23) stated that "We looked upon Charles Darwin as our deliverer, the man who had given a new meaning to our life and to the world we inhabited".

Pearson advanced the theory of correlation, issued a large number of statistical tables, studied a number of distributions (partly recommended by himself) and the estimation of their parameters, but his most important single contribution was the introduction of the chi-squared test for goodness of fit.

In spite of his studies of history, Pearson had not thought about Continental statisticians who had been working on population statistics. Quetelet, the most influential statistician of the 19th century (whom Pearson praised for his efforts) was a true-blue believer and never ever mentioned Darwin. However, important developments were taking place on the Continent since 1877 and for a number of years Chuprov had been attempting to bring together the Biometric school and the Continental direction of statistics. Slutsky, in a letter of 1912, stated that Pearson's shortcomings were temporary and that a rigorous basis for his writings will be created in due time (Sheynin 1996, pp. 45 – 46).

A serious case in point was that biometricians substituted frequency for probability and failed to distinguish, in their writings, between sample and theoretical parameters (in part, possibly because of Pearson's Machian views) so that European statisticians regarded Pearson with contempt. "The notions of the logical structure of the theory of probability, which underlies all the methods of mathematical statistics, remained [in England in 1912] at the level of eighteenth century results" (Kolmogorov 1948, p. 68).

An example of Pearson's misguided opinion about a historical event is his statement (1925) to the effect that Bernoulli's law of large numbers is too week and may be compared with Ptolemy's wrong system of the world. Strangely enough, this paper appeared while he had been delivering lectures on the history of statistics "against the changing background of intellectual, scientific and religious thought" (1978). There, on p. 1, he owned that it had been "wrongful … to work for so many years at statistics and neglect its history".

It is generally agreed that at the very least Pearson paved the way for Fisher to construct modern mathematical statistics and that he was a difficult man to get on with. Thus, "Between 1892 and 1911 he created his own kingdom of mathematical statistics and biometry in which he reigned supremely, defending its ever expanding frontiers against attacks (Hald 1998, p. 651). Here is one more statement: "He was singularly unreceptive to and often antagonistic to contemporary advances made by others in [his] field. [Because of this] the work of Edgeworth and of Student, to name only two, would have borne fruit earlier"; Fisher, letter of 1946, quoted by Edwards (1994, p. 100). In any case, Pearson, in a letter of ca. 1914, wrote to Oskar Anderson that "Student ist nicht ein Fachmann" – Student, who by that time published five papers in *Biometrika*! Fisher (1937, p. 306) also left a most serious charge: Pearson's "plea of comparability [between the methods of moments and maximum likelihood] is … only an excuse for falsifying the comparison …"

There exist testimonials of another kind as well. "I came in touch with [Pearson] only for a few months, but I have always looked upon him as my master, and myself, as one of his humble disciples"; Mahalanobis, in a letter of 1936, quoted by Ghosh (1994, p. 96). And here is Newcomb (who never was Pearson's student) in a letter to him dated 1903 (Sheynin 2002, p. 160): "You are the one living author whose production I nearly always read when I have time and can get at them, and with whom I hold imaginary interviews while I am reading".

Pearson (1887, pp. 347 – 348) opposed revolutions and (1978, p. 243) unfavourably mentioned Lenin: Petrograd (as it was called during 1914 – 1924) "has now for some inscrutable reason been given the name [Leningrad] of the man who practically ruined it".

Now, since Lenin (1909, pp. 190 and 274) called Pearson an enemy of materialism and a Machian, Soviet statisticians had been considering him almost as an *enemy of the people*. Here is a prime example (Maria Smit 1934, pp. 227 – 228) containing a most vulgar utterance: Pearson's curves are based "on a fetishism of numbers, their classification is only mathematical. Although he does not want to subdue the real world as ferociously as Gaus [yes, this is her spelling] attempted it, his system nevertheless only rests on a mathematical foundation and the real world cannot be studied on this basis at all".

For Porter (p. 309), Pearson is almost a tragic figure: the founder of what "symbolizes … the utter impersonality of science", but the "other", the forgotten Pearson stands for "generality and wisdom" (p. 314). I doubt that such a contradistinction is justified and in any case tragic, in a sense, were scholars and philosophers from Plato to Tolstoy and Darwin to Einstein. Darwin (1871, p. 188) believed in the forthcoming international brotherhood of mankind, Einstein denied randomness in the microcosm.

Porter's Bibliography is not updated, even the two 1991 editions of Pearson's *Grammar of Science* (Bristol and Tokyo) are missing; it fails to mention many important items but includes worthless books (Desrosières). References cited in footnotes (Einstein, Fisher) are absent there and some authors (Hald) are not included in the Index. The dates of the original publication of translated books are not provided.

Porter, who compiled his book after "eight years of research" and calls himself a historian (pp. 310 and 305), heaps details upon meandering details through which the reader has to squeeze himself but he fails to provide important facts. Indeed, I have to add that Pearson was elected to the Royal Society (1896) and invited by Newcomb, the President of the then forthcoming extremely prestigious International Congress of Arts and Sciences (St. Louis, 1904), to report on the methodology of science. Pearson declined for personal reasons (Sheynin 2002, pp. 143 and 163, note 8). Then, Pearson held that unmarried women may exercise sexual freedom and at least in England the change from condoning associations with prostitutes to regarding it as degrading was largely due to "men like Pearson" (Haldane 1957, p. 305).

I continue. Epigraphs are not properly documented and there are wrong or meaningless statements. Thomson & Tait's most influential treatise is called "standard Victorian" (p. 199); there exist "lines and other curves" (p. 259); "even mathematics" cannot prove the fourth dimension (p. 37); the theory of errors is poorly treated on pp. 257 and 259. And of course invited specialists should have dealt with mathematical physics and statistics. The book under review is of limited value mostly justified by passages from numerous archival sources.

Darwin, C. (1871), *The Descent of Man*. London, 1901.

Edwards, A. W. F. (1994), R. A. Fisher on Karl Pearson. *Notes Records Roy. Soc. Lond.*, vol. 48, pp. 97 – 106.

Fisher, R. A. (1937), Professor Karl Pearson and the method of moments. *Annals of Eugenics*, vol. 7, pp. 303 – 318.

Ghosh, J. K. (1994), Mahalanobis and the art and science of statistics: the early days. *Indian J. Hist. Sci.*, vol. 29, pp. 89 – 98.

Hald, A. (1998), *History of Mathematical Statistics from 1750 to 1930*. New York.

Haldane, J. B. S. (1957), Karl Pearson, 1857 – 1957. *Biometrika*, vol. 44, pp. 303 – 313.

Kolmogorov, A. N. (1948, in Russian), Slutsky. *Math. Scientist*, vol. 27, 2002, pp. 67 – 74.

Lenin, V. I. (1909, in Russian), *Materialism i Empiriokritizism. Polnoe Sobranie Sochineniy* (Complete Works), 5th edition, vol. 18. Moscow, 1961.

Pearson, K. (1923), *Darwin*. London.
--- (1925), James Bernoulli's theorem. *Biometrika*, vol. 17, pp. 201 – 210.
--- (1978), *The History of Statistics in the 17th and 18th Centuries against the Changing Background of Intellectual, Scientific and Religious Thought.* Lectures 1912 – 1933. Editor E. S. Pearson. London.
Sheynin, O. (1996), *Chuprov. Life, Work, Correspondence.* Göttingen.
--- (2002), Newcomb as a statistician. *Hist. Scientiarum*, vol. 12, pp. 142 – 167.
Smit, Maria (1934, in Russian), Against the idealistic and mechanistic theories in the theory of Soviet statistics. *Planovoe Khoziastvo*, No. 7, pp. 217 – 231.
Hist. Scientiarum, vol. 16, 2006, 206 – 209

Pressat, Roland: Christian Huygens et la table de mortalité de Graunt. Math. Sci. Hum. 153, 29 – 36 (2001)
The author describes Huygens' study of mortality (use of Graunt's table of mortality; correspondence with his brother Lodewijk and the introduction of probable and mean durations of life; appearance of conditional probability and conditional expectation).

No references to Huygens are provided and the year when his correspondence with Lodewijk had taken place (1669) is not mentioned. I have put on record the introduction of the conditional notions by Huygens (*Arch. Hist. Ex. Sci*. 17, 1977, pp. 241 and 249).

Zentralblatt MATH 988.01003

Pritchard, Chris: The contributions of four Scots to the early development of statistics. Math. Gaz. 76, No. 475, 61 – 68 (1992)
The Scots are John Sinclair, William Playfair, John Arbuthnot and James Stirling. The description is too short while the account of Stirling contains errors and does not (and could not have) presented him as a statistician.

Zentralblatt MATH 751.01008

Pritchard, Chris: Bagatelle as the inspiration for Galton's quincunx. BSHM Bull. 21, 102 – 110 (2006)
The essence of this paper is included in the author's doctoral thesis of 2005. It considers how Francis Galton came to devise the quincunx that simulated the effect of a large number of Bernoulli trials to yield an empirical normal curve. It suggests that the likely inspiration for the design was the popular bagatelle, a version of billiards with holes instead of pockets. The author traces the history of the bagatelle, notes its appearance in the *Pickwick Papers*, and the use of the word *quincunx* in the 17th century England to describe an arrangement of four trees forming a square and a fifth one in its centre, which reminded Galton of the design of a Roman coin.

Pritchard (p. 104) also called S. M. Stigler's *History of Statistics*. Cambridge, Mass., 1986, incomparable. Exactly, since Stigler is the sole author who dared to ridicule Gauss, see for example my review of Krengel or *Antistigler* on my website www.sheynin.de

Zentralblatt MATH 1101.01006

Rabinovitch, Nahum L.: Early antecedents of error theory. Arch. Hist. Ex. Sci. 13, 348 – 358 (1974)
Legal problems and rituals of Judaism demanded measurements of distances and areas. The author believes that the estimation of the errors of such measurements and discussion of their possible sources, already present in the Talmud, represented facts known to ancient surveyors. The measure of volume mentioned in the Talmud, a hen's

egg, was defined as the mean between the *largest* and the *smallest* of them.

The author maintains that the Rabbibic literature contains direct and oblique formulations of the stochastic properties of typical random errors of measurement and in this connection he discusses the considerations of Levi Ben Gerson, a 14th century Rabbi and astronomer regarding the experimental method in science.

Matematika 6A43

Ramsey, F. P.: Philosophical Papers. Editor, D. H. Mellor. Cambridge, 1990.

Ramsey (1903 – 1930) wrote about 30 papers on philosophy of science, mathematical logic and mathematical economics. The editor of this book (who is also the author of its valuable introduction) selected for publication the philosophical and logical works of Ramsey all of which however had already appeared in at least one of his two previous collections of articles. Ramsey's contributions are extremely valuable even now; moreover, in many instances his contemporaries did not grasp their importance. On the other hand, Ramsey had no time to prepare some of his last notes for publication. Philosophy of probability is a special topic of his works.

Zentralblatt MATH, 713.01019

Rao, C. Radhakrishna: Statistics as a last resort. In Ghosh, J. K., ed., et al, Glimpses of India's Statistical Heritage, 153 – 213. New Delhi (1993)

This is a scientific autobiography written not later than in 1991 and complemented by a list of Rao's works (11 books and 100 articles). His *Sel. Papers* in 5 vols with a complete bibliography is being prepared by the Indian Statistical Institute where he worked from 1941 to 1978 (formally, until 1984) having been its Professor (1949 – 1972) and Director (1972 – 1976). After 1978 Rao works in the USA but visits India *every year*.

Upon graduating from Andhra Univ., Rao (b. 1920) was unable to find a job and statistics occurred to be his *last resort*. He describes his main results achieved over several decades and meetings with other leading statisticians (Neyman, Linnik, and especially Fisher). He notes that *numerous obstacles* and *sensitive issues … in the context of the complex socio-economic-political-linguistic milieu in India* had delayed his work and quotes Fisher as saying that he sets great store by *numerical work* rather than by *imposing formulae*.

Zentralblatt MATH 829.01023

Rao, C. Radhakrishna: Statistics must have a purpose, the Mahalanobis dictum. Sankhya, A55, 331 – 349 (1993)

This is a slightly expanded version of a paper published in *Bull. Intern. Stat. Inst.* No. 1, 21 – 36 (1993). The author describes the life of Mahalanobis and his work in multivariate methods in taxonomy, sample surveys and econometry and quotes his hero and other scholars (Haldane, Hotelling, Deming, and especially Fisher). He states that *Apart from his work in India, Mahalanobis* [was] *one of the pioneers, who, along with Pearson, Fisher, Neyman and Wald, laid the foundations of statistics as a separate discipline*. Mahalanobis himself declared that the only justification of statistics *lies in the help it can* *give in solving a problem … statistical theory is not a branch of mathematics. … Mathematical statistics as a separate discipline cannot simply exist*. The two passages seem to contradict each other.

Zentralblatt MATH 811.01003

Rao, C. Radhakrishna: R. A. Fisher, The founder of modern statistics. In C. R. Rao, ed., et al, Statistics for the 21st Century. New York, 2000, pp. 311 – 350

This essay first appeared in *Stat. Sci.*, 7, 1992, pp. 34 – 48; now, it additionally carries an Addendum on Fisher's work on multivariable methods.

Fisher's one-time student, the author has been "largely influenced" by his teacher's ideas. He indicates Fisher's shortcomings (cryptic style; omission of intermediate calculations; lack of some rigorous proofs) and states that some of his findings turned out less generally valid than Fisher had claimed. At the same time Rao notes the variety and depth of Fisher's writings and regards him as the originator of modern statistics.

Briefly discussing Fisher's results, he concludes that the establishment of the design of experiments was the most outstanding contribution of his hero to statistics, and he approvingly quotes Fisher to the effect that no monolithic structure of statistics is possible.

Zentralblatt MATH, 1030.01032

Rashed, Roshdi: Kombinatorik und Metaphysik. In: Thiele, Rüdiger, ed. Festschrift zum siebzigsten Geburtstag von Matthias Schramm. Berlin, 37 – 54 (2000)

The author traces the birth of combinatorial analysis. Ibn Sina formulated the philosophical principle of emanation from the One to the entire world. Then, At-Tusi (1201 – 1274) examined this principle mathematically; when calculating sums of binomial coefficients he applied the appropriate summation identity. He was also the follower of Al-Halil Ibn Aimad (718 – 786) who used the combinatorial approach for solving linguistic problems, and Al-Karagi (died 1030) who discovered the arithmetic triangle. Finally, Al-Halabi (died 1549) devoted a book to combinatorial analysis which contained several summation identities involving binomial coefficients.

The early history of combinatorial analysis should also include the relevant achievements made in India and in China as well as the works of Levi Ben Gerson and Al-Kashi. And, not later than in the 8th century a Jewish author described by elementary combinatorial means how the 22 letters of the Hebrew alphabet had created the world (Rabinovitch, N. L., *Probability and Statistical inference in Ancient and Medieval Jewish Literature.* Toronto, 1973, p. 143).

Zentralblatt MATH 972.01010

Rice, Adrian: 'Everybody makes errors'. The intersection of De Morgan's logic and probability, 1837 – 1847. Hist. Philos. Log. 24, 289 – 305 (2003)

In spite of its title, the paper describes De Morgan's entire work on the application of probability to logic as well as his efforts to simplify Laplace's *oeuvre* and his merits in furthering the actuarial science. The author concludes that his hero had attempted to evaluate the likelihood of logical deductions (thus actually following Leibniz' thoughts!) but that he later moved away to philosophy so that this direction of his work did not essentially influence subsequent events.

I note that in 1864 (*Trans. Cambr. Phil. Soc.* 10, p. 421) De Morgan declared that if the probability of a certain event was 2.5, it will happen twice with an even chance of happening a third time. Confidence in his work in probability is thus undermined.

Zentralblatt MATH 1049.01013

Rohrbasser, Jean-Marc; Véron, Jacques; Préface, Marc Barbut: Leibniz et les raisonnements sur la vie humaine. Paris, 2001

This is a discussion of Leibniz' manuscripts on mathematical demography and its application to the evaluation of life annuities, all of them written in 1680 – 1683 (except for one dated 1675) and only published in the 19th century or later; in some cases the dates of the first publication are not provided. One of the manuscripts, the "Essay de quelques raisonnemens nouveaux sur la vie humaine et sur le nombre des hommes", is reprinted.

The authors (p. 75) stressed that Leibniz had preferred deduction to statistical data but did not mention his relevant correspondence with Jakob Bernoulli, neither had they compared Leibniz' thoughts about randomness (pp. 73 – 74) with the "Laplacean determinism". They (p. 85) connected Leibniz' reasoning on the value of life annuities with his theory of monads (which was far-fetched), paid scant attention to political arithmetic in general although this was the subject of Leibniz' reprinted "Essay" and their commentary lacked modern notions of mathematical statistics.

Zentralblatt MATH, 1054.01006

Salles, Maurice: The launching of 'social choice and welfare' and the creation of the 'society for social choice and welfare'. Soc. Choice Welfare 25, 557 – 564 (2005)

The author discusses the appearance of the economic discipline previously (in the 1970s) called *Social choice and welfare* and he refers to P. K. Pattanaik & M. Salles' book thus entitled (Amsterdam 1983). He does not describe the essence of either the discipline or the book but mentions in passing that Condorcet and Borda studied voting procedures from a mathematical standpoint. The author is Secretary of the Society for Social Choice and Welfare (established 1992) whose prehistory consisted in launching a periodical of the same name.

Zentralblatt MATH, 1103.01016

Sarkar, Sahotra: J. B. S. Haldane and R. A. Fisher's draft life of Karl Pearson. Notes Rec. Roy. Soc. Lond. 49, 119 – 124 (1995)

Edwards described Fisher's contribution on Pearson for the *Dict. Nat. Biogr*. (withdrawn by the author before publication) and his correspondence with Legg, the Editor of the *Dictionary*. Here, evidence is presented suggesting that it was Haldane who advised Legg to reject Fisher's (yet unwritten) entry. The relations between Fisher and Haldane are also discussed.

Zentralblatt MATH 813.01015

Schilar, H.: Optimization and political economy. In Shatalin, S. S., ed. Economic-Mathematical Models and Methods. Coll. of

Scient. Works. To the Memory of L. V. Kantorovich. Voronezh, 33 – 39 (1989). In Russian
The author believes that the determination of optimal values in economics can be fully utilized only in a society with a planned economy and that, as Kantorovich stated, linear programming had influenced the political economy of socialism. The author lists several problems connected with optimization, viz., the study of 1) The relation between particular economic problems as well as between them and the general management of the economy; and 2) The relation of the obtained theoretical estimates of the price of commodities to their actual prices (fixed by the government).
Zentralblatt MATH 802.01014

Schneider, Ivo (Editor): Die Entwicklung der Wahrscheinlichkeitstheorie von den Anfängen bis 1933: Einführungen und Texte. Darmstadt: Wissenschaftliche Buchgesellschaft, 1988.
This is a source-book containing (fragments of) classical works and introductions to its 11 chapters (games of chance up to the 17th c.; the notion of the probable; probability before Laplace; the law of large numbers (LLN) and the central limit theorem (CLT); applications to mortality; to the theory of errors; to physics; mathematical methods; axiomatization; Markov chains and processes; celebrated problems). The sources are mostly in German (they include existing and ad hoc translations), but English contributions not previously done into German are left intact. No claim is made about comparing new translations from Latin with those into English or French.

Bibliographic information is incomplete: it is difficult to identify the original texts of some fragments (on pp. 74 – 75 these are taken from §§ 39, 40 and 43 of Cournot, 1843, but only § 39 is mentioned); in many instances only the first, hardly available edition of a source is referred to (p. 41); sometimes (pp. 9, 44, 186) the language of the source is not stated; and even the main commentators of classical works are not named. True (p. VI), the Editor intends to do so, and to supply much more meaningful commentaries of his own in a companion volume [that never appeared].

Mathematical statistics is included only in part and such scholars as Pearson and Fisher are absent. Population statistics except for mortality is excluded and there are many more omissions: Huygens' letter on the emergence of probability; De Moivre's dedication of his *Doctrine* to Newton; the [indirect] anticipation of the method of least squares (Simpson, Euler); the Ehrenfests' model and its precursor (the urn problem due to Daniel Bernoulli and Laplace); the notion of randomness; Cauchy's work on the CLT; Michell's problem; Price, Buffon and Laplace on the probability of the next sunrise etc. And instead of the luxurious fragments from Pacioli, Cardano and Tartaglia a few passages from Liapunov in the *C. r.* of the Paris Academy should have been included.

The introduction contains mistakes. Too much stress is laid on Laplace's denial of randomness (p. 49); applications of probability to the law are wrongly claimed to result in the former's stagnation (p. 50, partly refuted on p. 487). De Moivre is credited with having proved the De Moivre – Laplace theorem only in a particular instance (p. 118). In 1969 Schneider knew better than that! And a common mistake concerning the date of publication of Arbuthnot's memoir is repeated on p. 507. Also, the reader with not find either the formula of the Bernoulli LLN or the uniform distribution in connection with mortality, or any recognition of the discovery that some fundamental laws of nature are stochastic.

Zentralblatt MATH 860.01035

Schneider, Ivo: De Moivre's central limit theorem and its possible connections with Bayes' essay. In: Splinter, Susan, ed., et al, Physica et Historia. Festschrift for Andreas Kleinert. Acta Historica Leopoldina 45, 155 – 161 (2005)

The essence of this paper is a study of Bayes' possible relations and personal acquaintance with De Moivre. I take issue on many points. Jakob Bernoulli's theorem is described faultily: no mention is made of his estimate of the rapidity of the convergence to the limit; and De Moivre's formula of his limit theorem is presented wrongly. The dates of the publication of the Bayes memoir and its supplement are not given (and *Phil. Trans.* for 1764 is only correct for the latter). There was no need to prove that Price was familiar with De Moivre's work since he indicated its shortcomings in his covering letter to the Bayes memoir. There was no *competition* between Stirling and De Moivre, see the latter's note of 1733. And it is strange that Newton is all but absent in the description of De Moivre's life and work.

The author had not touched on the quantitative difference between the results of De Moivre and Bayes. I had described it in *Biometrika* 58, 234 – 236 (1971) (which Schneider did not cite). Accordingly, I believe that Bayes rather than Laplace or Poisson completed the pre-Chebyshev stage of probability theory.

Zentralblatt MATH 1098.01006

Schucany, William R.: Donald B. Owen's contributions to the statistics of quality. In Ghosh, Subir, ed., et al, Statistics of Quality. Dedicated to the memory of Donald B. Owen. New York, 1 – 9 (1996)

This is a biography of Owen (1922 – 1991). He taught statistics, (co)edited statistical periodicals and published eight books and ca. 80 articles (whose list is appended). Owen is mostly remembered for his handbooks of statistical tables and distributions and for work on statistical quality control.

Zentralblatt MATH 931.01025

Schwartz, Laurent: Quelques réflexions et souvenirs sur Paul Lévy. Les processus stochastiques, Coll. Paul Lévy. Palaiseau, 1988,pp. 13 – 28

This is a scientific biography of Paul Lévy (1886 – 1971) written by his son-in-law. Lévy was Professor at the École Polytechnique (1920 – 1958). For this reason he had almost no disciples and the French university world did not appreciate him all the more since scholars such as Hadamard and "Bourbaki" were not really interested in the theory of probability. Lévy was not elected to the Académie des Sciences until 1964 and his works were only recognized in France after having been acknowledged by American mathematicians.

Describing Lévy's fundamental achievements (though not providing a bibliography of his writings) and calling him "un virtuose d'acrobatie mathématique", the author concludes that the modern theory of probability was created in the first place by Kolmogorov and Lévy in spite of the latter's refusal to make use of such notions as Borel field of events.

Zentralblatt MATH, 658.60003

Seal, Hilary L.: Multiple decrements of competing risks. Biometrika 64, 429 – 439 (1977)

Daniel Bernoulli's memoir of 1766 and D'Alembert's commentary on the expected increase in the mean duration of life due to inoculation of smallpox were the first writings to pose and solve the problem of calculating competing risks. After indicating this fact, Seal briefly describes the relevant contributions of the mathematical theory of insurance against disability (19^{th} and 20^{th} centuries) and argues that they were important for mathematical statistics in general.

[Pascal' celebrated *Infini-rien* wager might have also been mentioned.]

Matematika 6A22

Seneta, E.: On the history of the strong law of large numbers and Boole's inequality. Hist. Math. 19, 24 – 39 (1992)

The author describes the contribution of Borel and Cantelli to the discovery of the strong law of large numbers. He adduces translations of the texts of two non-mathematical letters written by Slutsky in 1928 and comments on the later history of the Boole inequality for the probability of the simultaneous occurrence of a series of events (on its use by Cantelli, its generalization by Fréchet, 1935, and on its likely influence on Bonferroni, 1936).

It appears that Slutsky, who was the first to notice Borel's finding, had to defend the latter at the Congress of Mathematicians in Bologna (1928) against Cantelli.

In 1923 – 1924 Chuprov maintained that it was impossible to connect frequency with probability. In 1925 Slutsky echoed this opinion. However, also in 1925, he declared, referring to Cantelli, that the stochastic limit of a function was equal to the function of the stochastic limit. And it was Chuprov who attracted Slutsky's attention to Cantelli. [See my later paper *Hist. Math.* 20, 1993, 247 – 254.] Seneta acknowledged my help in obtaining important materials, but he was afraid of harming me by stating expressly that he had received the copies of Slutsky's letters from me, to whom Chuprov's disciple, Chetverikov, had sent me in 1970.

Zentralblatt MATH 744.01008

Seneta, E.: Carl Liebermeister's hypergeometric tails. Hist. Math. 21, 453 – 462 (1994)

In 1877, in a medical context, Liebermeister studied the possibility of distinguishing between equality and inequality of success probabilities in two (small) series of binomial trials. Starting from a Laplacian formula based on the existence of a uniform prior distribution and assuming that the two probabilities coincided, he considered the size of the tail probability (of the hypergeometric distribution). The author reconstructs Liebermeister's insufficient intermediate calculations and indicates that his test can still be applied and that his main formula has hardly ever reappeared.

Zentralblatt MATH 813.01006

Seneta, E.: Markov and the birth of chain dependence. Intern. Stat. Rev. 64, 255 – 263 (1996)

This paper is reprinted from *Bull. Intern. Stat. Inst*. 56, No. 3, 1261 – 1276 (1995). The author examines Markov's first memoir on *Markov chains* (1906) setting high store by his intuition and connects it with the work of Bernstein. He also emphasizes that Nekrasov's (only partly correct) remark about the conditions for the weak law of large numbers became the starting point for Markov's study of dependent variables.

The author provides a wrong date (1912 rather than 1901) for Tolstoy's excommunication from the Russian Orthodox Church [Tolstoy died in 1910!] and does not refer to the reviewer's book *Chuprov: Life, Work, Correspondence*. Moscow, 1990, in Russian [1996 and 2011: English translation].

Zentralblatt MATH 918.60008

Seneta, E.: I. J. Bienaymé: criticality, inequality, internationalization. Proc. 51st Session, Intern. Stat. Inst., Istanbul, 1997. Voorburg, vol. 1, 67 – 70 (1997)

The author reminds his readers of Cournot's part in studying the criticality theorem (the extinction of surnames), of the ties between Ostrogradsky, Buniakovsky and Chebyshev with the French mathematical world and on Bienaymé's role in the discovery of the Bienaymé – Chebyshev inequality. For some reason he is surprised that Markov defended Bienaymé's priority in this last-mentioned issue.

Zentralblatt MATH 914.01015

Seneta, E.: Early influences on probability and statistics in the Russian Empire. Arch. Hist. Ex. Sci. 53, 201 – 213 (1998)

The author discusses early Russian works on probability (but does not mention Davidov) and examines the background of Chebyshev's pertinent contributions and his ties with France (mostly through Bienaymé). Only from among Western sources, his references do not include the piece on Chebyshev from the *Dict. Scient. Biogr.* (1971) or the English translation of *Mathematics in the 19th Century* [vol. 1]. Basel, 1992.

Seneta states that lectures in probability began *in some* (Russian) *universities* before 1837. I know only one such case: Bartels, in Dorpat (Tartu), in 1836.

Zentralblatt MATH 917.01019

Seneta, E.: M. V. Ostrogradsky as probabilist. Ukrain. Mat. Zh., 53, 2001, pp. 1038 – 1047; reprinted in Ukrainian Math. J., 53, 2001, pp. 1237 – 1247

Ostrogradsky (1801 – 1862) and Buniakovsky were the two Russian pre-Chebyshev probabilists. In his essay, the author draws on Gnedenko's pertinent article (1951) but studies in much more detail two of Ostrogradsky's papers, – on judgements pronounced by a panel of jurors and on sampling without replacement from an urn whose composition is unknown. Understandably, Seneta pays less attention to ideological issues and describes his hero's achievements in a broader context of contemporary European science.

He does not dwell on Ostrogradsky's attempts to introduce a statistical method of quality control; he agrees with Ostrogradsky's mistaken statement that Laplace had not considered unequal prior probabilities in a Bayesian setting; and he wrongly interprets my remark on Ostrogradsky's criticism of Buniakovsky.

Math. Rev., 2003b:01054

Shafer, Glenn: The significance of Jacob Bernoulli's Ars Conjectandi for the philosophy of probability today. J. Econom., 75, 15 – 32 (1996)

This is a non-comprehensive discussion of the *Ars Conjectandi* and even its date of publication is stated wrongly. The connection between Jakob's deliberations and the theory of probabilism (allowing a person to follow any probable opinion of any father of the Catholic Church, and leading to non-additive probabilities recently introduced into mathematics) is not mentioned. His law of large numbers is downgraded as being obsolete with respect to Niklaus Bernoulli's finding of 1713, and Jakob's insistence that, for the *Bernoulli trials*, induction was not inferior to deduction ("woran vielleicht niemand bisher auch nur gedacht hat") is passed over. The "De Moivre – Laplace" limit theorem (1733 rather than 1738) is not seen as a development of Jakob's result. That Niklaus had plagiarized Jakob and became acquainted with his law before 1713 is apparent now since Jakob's *Werke*, Bd. 3, are published (1975). The author did not refer to the Russian source, Bernoulli, J., *O zakone bolshikh chisel* (On the law of large numbers), 1986, containing Prokhorov's, Youshkevich's and the reviewers comments; or to Youshkevich's paper (*Theory Prob. and Appl.*, 31, 1987), to the reviewers description of N. Bernoulli's finding (Pearson & Kendall, *Studies Hist. Stat. Prob*., 1970) but he approvingly mentions T. Porter for whom everything goes (*Centaurus* 31, 1988, 171 – 172).

Zentralblatt MATH, 858.01014

Sheynin, O. B.: Markov's publications in the newspaper 'Den' in 1914 – 1915. Istoriko-Matematich. Issled. 34, 194 – 206 (1993). In Russian

Markov published many newspaper letters on social problems. Three of such letters are reprinted here. 1. On the introduction of probability into school curricula (expressing doubts about the programme compiled by Florov and Nekrasov). 2. On the enrolment of graduates of the theological seminaries in university faculties of mathematics and physics (stating that these graduates should not be preferred to other entrants). 3. On his polemic with Nekrasov on the notion of limit (protesting against Nekrasov's methods of disputation).

The commentary includes information about Nekrasov's style (a jumble of mathematics, religion etc) and views. In a letter of 1916 to Florensky Nekrasov wrote that *The German-Jewish* (misprinted: German-European) *culture and literature drives us to a crossroads*.

Zentralblatt MATH 805.01016

Sheynin, O. B.: Sampling and processing of results of observations by D. I. Mendeleev. Istoriko-Matematich. Issled. 35, 56 – 64 (1994). In Russian
In spite of my request for suppressing this paper, the Editor of the IMI had mistakenly put it out. [An essentially new version is in *Hist. Math.* 23, 54 – 67 (1996).] Its reviewer restricted his attention to mathematics proper and did not mention that I had thrown light on the treatment of observations as practised by natural scientists of the second half of the 19th century.

Zentralblatt MATH 905.01011

A. N. Shiryaev: Andrei Nikolaevich Kolmogorov: in memoriam (with list of publications). Teor. Veroyatn. Primen., 34, 5 – 118 (1989)
Kolmogorov is shown as a scholar and an organizer of science, as a teacher (68 of his distinguished students, including the author himself, among them 14 members and corresponding members of the Soviet Academy of Sciences or of its union republics are named), and as an editor (though without attempting to list the numerous books edited by him). He is considered to be on a par with the classics of natural sciences of the previous centuries.

The role of such scientists as Urysohn, Luzin, Khinchin, and P. S. Aleksandrov in Kolmogorov's life is explained, but the relations between science and society are left aside and it is not mentioned that textbooks for mathematical schools written and/or edited by him caused negative popular response and sharp professional criticism (Pontriagin).

The author deals with Kolmogorov's work on descriptive set theory, trigonometric series, topology, classical analysis, mathematical logic etc and, in much more detail, on the theory of probability (including its applications to physics) and information theory. The appended bibliography lists 477 of Kolmogorov's writings (not specified are those included in his selected works (three volumes, 1985 – 1987)), with an additional list of 28 of his newspaper articles, lists of his popular contributions, articles from encyclopaedias, works on mathematical linguistics, etc., all of them compiled from the main list; a list of 96 of his reports at the Moscow Mathematical Society; and a list of Russian contributions devoted to Kolmogorov.

The main list is incomplete; my extremely careless examination revealed two omissions, one of them being Kolmogorov's epilogue, written together with A. P. Youshkevich, to the Russian edition of G. Cantor's works, 1985. Not mentioned are translations of Kolmogorov's writings into foreign languages. The last list fails to mention Youshkevich's article (*Voprosy Istorii Estestvoznania i Tekhniki*, No. 3, 1983, pp. 67 – 74).

Zentralblatt MATH, 664.01013

A. N. Shiryaev: Everything about Kolmogorov was unusual. CW1 Q, 4, 189 – 193 (1991)
This is the text of an address delivered at the Second International Congress of the Bernoulli Society (Uppsala 1990) and, actually, a supplement to the author's earlier detailed biography of the same person. Kolmogorov (1903 – 1987) first exhibited his mathematical

gift at the age of five or six. At school, he made up a fake perpetuum mobile just to tease his physics teacher. At fourteen, he began studying higher mathematics by reading an encyclopaedia and reconstructing the necessary proofs. While a student, Kolmogorov had to teach mathematics and physics at an ordinary school; all his life he was proud of his social work there. As a graduate student under Luzin he wrote 14 original papers in lieu of holding the same number of examinations. Kolmogorov avoided the "technical" stage of the development of scientific topics and was unable to concentrate fully on any one problem for more than two weeks. Instead, he was a pioneer in many fields and developed generalized theories. In 1953 Gelfand stated that "The fact that mathematics is still felt to be a single science is due to a large part to Kolmogorov". Simplicity of ideas; abstract investigations coupled with a feeling for applied problems; and excitement and hard work were the main features and aspects of his method. Kolmogorov had many students and inspired many other scholars. One of his students (unnamed) confessed that he felt "panic respect" towards his teacher. Having been a man of many interests, Kolmogorov made pioneering discoveries in several areas outside mathematics (e. g., meteorology, hydrodynamics).

Zentralblatt MATH, 746.01011

Sol de Mora-Charles, Maria: Quelques jeux de hazard selon Leibniz. Hist. Math. 19, 125 – 157 (1992)

The author publishes Leibniz' MSS *Du jeu du quinquenove* (1678), *Le jeu du solitaire* (ca. 1678), and *Jeu des productions* (1698, an invented game). He points out several mistakes made by Leibniz (e. g., enumeration of combinations rather than permutations) and emphasizes Leibniz' approach to games of chance which enable to *perfectionner l'art d'inventer*.

He does not mention that Leibniz 1) Made a similar statement in his *Neue Abh. über den menschlichen Verstand*, or 2) Effectively used the *classical* definition of probability and offered a definition of the ratio of probabilities. I do not understand the author's diagrams and do not know what is meant by stating that Leibniz' MSS *se trouvent ... sous la cote* of Brouillon LH XXXV.

Zentralblatt MATH 754.01004

Soloviev, A. D.: A. P. Nekrasov and the central limit theorem of probability theory. Istor.-Matematich. Issled., 2nd ser., 2 (37), 9 – 22 + 327 (1997)

Pavel Alekseevich Nekrasov (1853 – 1924) contributed to algebra, analysis, probability theory and to mechanics. The author studies his work on the central limit theorem and concludes that he, by essentially applying the complex variable theory, had proved it for lattice variables (which, however, he understood in a wrongly excessive sense) in the new case of large deviations. The author remarks that some of Nekrasov's conditions were too strict and his other restrictions could not be checked. The author agrees with earlier commentators in that Nekrasov's pompous style, his lumping together of mathematics, pseudophilosophy and religion as well as his glaring mistakes (e. g., his misunderstanding of the notion of infinitesimal) caused Markov and Liapunov to dismiss his work.

The author also participates in describing Nekrasov's role in originating the saddle point method (S. S. Petrova & Soloviev, *Istor. Matematich. Issled*. 35, 1994) and now he properly mentions Seneta (*Math. Sci*. 9, 1984). [Nekrasov's debates with Markov and Liapunov are translated (P. A. Nekrasov, *The Theory of probability*. Berlin, 2004, also on my website www.sheynin.de).]

Zentralblatt MATH, 970.01010

Sprott, D. A.: Gauss' contributions to statistics. Hist. Math. 5, 183 – 203 (1978)

The author describes Gauss' contributions to the treatment of observations (mainly his *Theoria motus*, 1809, and *Theoria combinationis*, 1823 – 1828) and stresses their connection with later statistical ideas and methods. He (correctly) maintains that it is wrong to call the second Gauss justification of least squares after Gauss and Markov.

Matematika 2A15

Stamhuis, Ida H.; Klep, Paul M. M.: The stubbornness of various ways of knowledge was not typically Dutch; the statistical mind in a pre-statistical era. Centaurus 46, 2004, pp. 287 – 317

This is an essay on the subject of the book *The statistical mind in a pre-statistical era. The Netherlands 1750 – 1850*. Amsterdam, 2002, edited by them. They included (apparently all the) 12 contributions collected in that source in their valuable bibliography; they also (separately one from another) were the authors of five of these pieces.

The authors' main thesis is that, in the Netherlands, Staatswissenschaft and political arithmetic (= statistics proper) developed independently of each other since they belonged to "humanities" (ordinarily understood as literature, history and philosophy) and science respectively. However, Staatswissenschaft collected information about the political structure, meteorological and geographical features etc. of a given state and I do not therefore agree with their explanation. The divide between the two disciplines was rather occasioned by differing attitudes towards numerical description of states, see my paper in *Jahrb. Nationalökon. Stat*., 231, 2003, 91 – 112. The authors also attempted to link measurement to statistics, but they failed to mention the triangulation of their country (considered by Gauss in 1828).

The factual substance of the essay includes little known information about Rehuel Lobatto (1797 – 1866) and Simon Vissering (1818 – 1888), the leading representatives of the mathematical and qualitative directions in statistics respectively; on the collection and publication of unofficial and official statistical data; and on the influence of other nations (and of Quetelet) on statistics in Netherland.

The essay is corrupted by mistakes and incomprehensible statements (Halley's mortality table was published in the 18th century; "the smaller the normal curve, the higher the precision"; "moral statistics or the theory of probability", a statement attributed to "the French" and left without comment; and, astonishingly, "new … concepts, such as average and probability, were developed" [in the Netherlands between 1750 and 1850]).

Zentralblatt MATH, 1062.01010

Stigler, Stephen M.: Napoleonic statistics. The work of laplace. Biometrika 62, 503 – 517 (1975)
This is a review of Laplace's findings in the field now called mathematical statistics. In more detail the author dwells on one of his works of 1787 and on a few of his publications from 1820 onward on the influence of the Moon on the atmospheric pressure, where, without indicating that the data were not independent, Laplace at least partly allowed for this circumstance.

In 1787, in an astronomical context, Laplace solved a system of 24 linear equations in 4 unknowns by forming 4 appropriately composed linear combinations of the initial equations without applying any direct stochastic ideas or methods.

Among Laplace's main results Stigler singles out the [non-rigorous] proof and application of the central limit theorem, introduction of loss functions and an essential extension of the Bayes approach.

Matematika 1976, 2A16

Stigler, Stephen M.: Mathematical statistics in the early States. Ann. Statist. 6, 239 – 265 (1978)
The author describes the publications of Adrain, Benjamin and Charles Sanders Peirce, Newcomb, and Erastus Lyman De Forest (1834 – 1888) some of which (although not the first-mentioned) soon became known in Europe. He notes [after Hogan (1977)] that Adrain's memoir appeared in 1809 rather than in 1808.

That the theory of probability and statistics had mostly been developing in Europe rather than in USA is explained by the same general situation in astronomy and mapping as well as with an insufficient level of higher education.

Matematika 1978, 11A12

Stigler, Stephen M.: R. Smith, a Victorian interested in robustness. Biometrika 67, 217 – 221 (1980)
Stigler reprinted and commented on Smith's note *True average of observations*? (1888). Smith advocated the application of posterior weighing rather than the simple arithmetic mean and the author notes that this recommendation was tantamount to introducing a robust estimator and that Daniel Bernoulli whose unpublished Latin memoir was described by Johann III Bernoulli in 1789 acted in the same way.

Stigler considers it strange that Daniel dropped his proposal in his published memoir of 1778. [The first to apply posterior weighting in a published memoir was J. Short (1763). However, such weights only provide a correction for asymmetry of the empirical values of the observations.]

Matematika 8A6

Stigler, Stephen M.: Who discovered Bayes's theorem? Amer. Stat. 37, 290 – 296 (1983)
In 1764 – 1765, the Royal Society published an *Essay towards solving a problem in the doctrine of chances*, parts 1, 2. The MS of this *Essay* was communicated by R. Price who found it in the papers of the late T. Bayes. Contemporary specialists in probability very often refer to, and study the more influential pt. 1 of this pathbreaking work. Neither Laplace, nor, apparently, any other scholar of the past is dealt with in such a manner as Bayes.

The author made known a passage from D. Hartley's *Observations on Man* (London, 1749) which begins thus: *An ingenious friend has communicated to me a solution of the inverse* (as compared with De Moivre's theorem) *problem of determining the probability of an event given the number of times it happened and failed*.

Hartley's account, which only occupies 12 lines, contains a reference to the case of a large number of trials but does not include any formulas. Drawing on literary and some archival sources, the author discovered that Hartley had substantially completed his book in 1739; that he was a good friend of a blind mathematician N. Saunderson who died in 1739, aged 56; and that De Moivre highly esteemed Saunderson both as a man and as a scholar. Since there are no known connections between Bayes and De Moivre or Hartley, the author contends that it is more likely that the *Essay* was written by Saunderson. The author did not say whether Price had known Bayes' handwriting and he implies, without direct substantiation, that Saunderson was familiar with De Moivre's limit theorem.

My final remark concerns pt. 2 of the *Essay* where the case of a large finite number n of trials is discussed. It seems that the author of the *Essay* did not want to consider $n \to \infty$ and this conjecture agrees with Bayes' objection to the use of divergent series voiced in a posthumous note (*Phil. Trans. Roy. Soc.* 53, 1764). There, without naming anyone, Bayes adduced several examples including one from De Moivre's *Method of Approximation* (1733).

Zentralblatt MATH 537.62004

Stigler, Stephen M.: Laplace's 1774 memoir on inverse probability. Stat. Sci. 1, 359 – 378 (1986)

The author discusses the first six sections of Laplace's *Mémoire sur la probabilité des causes par les événements* and adduces their English translation (Laplace devoted section 7, the last one, to differential equations.)

Several authors, beginning with Todhunter, have commented on this seminal work of the great master. Stigler's achievement consists in presenting its full description in modern statistical language. He did not say, however, to what extent did Laplace use the findings of this *Mémoire* in his later work.

Zentralblatt MATH 618.62002

Stigler, Stephen M.: John Craig and the probability of history. From the death of Christ to the birth of Laplace. J. Amer. Stat. Assoc. 81, 879 – 887 (1986)

This is a description of J. Craig's *Theologiae Christianae Principia Mathematica* (1699). Craig attempted to ascertain the date of the second coming of Christ, which, judging by the hints contained in the Holy Writ, will coincide with the disappearance of (Christian) faith. Accordingly, Craig examined the decrease in the reliability of historical events with time, but his definitions were extremely vague and commentators regarded his investigation as cranky.

Noting that in 1699 the classical definition of probability was not yet generally known, the author rewrote Craig's formulas assuming that his 'probability' may be understood as $\log[P(\mathrm{E}|\mathrm{H})/P(\mathrm{E}|\text{not H})]$, i.e., as the logarithm of the likelihood ratio in favour of the event H given the evidence E. After pointing out the deficiencies of Craig's model, the author concluded that the *Theol. Christ*. was a remarkable early example of applying stochastic considerations to social science. Possibly laughing in his sleeve, he also fit Craig's model to the discordant data on the birth dates of Laplace.

Zentralblatt MATH 618.62003

Stigler, Stephen M.: The History of Statistics. The Measurement of Uncertainty before 1900. Cambridge (Mass.) etc. The Belknap Press of Harvard University Press, 1986.

The book consists of three parts: The development of mathematical statistics in astronomy and geodesy before 1827, i. e., before Laplace's death (the theory of errors – least squares – the theory of probability – Laplace and Gauss); The struggle to extend a calculus of probabilities to the social sciences (Quetelet – Lexis – psychophysics); and A breakthrough in studies of heredity (Galton – Edgeworth – Pearson and Yule). There are two luxury appendices (syllabuses for Edgeworth's lectures). Ornaments include portraits of a large number of scholars, reproductions of original drawings and of pages from classical works.

The author understands mathematical statistics as a logic and methodology for measuring uncertainty and for examining its consequences (p. 1). This is a restricted definition[1]. Its victims are: the exploratory data analysis (Halley's introduction of isogonic lines and Humboldt's bringing isotherms into use) and also such disciplines as climatology, geography of plants, stellar statistics and even epidemiology and public hygiene, two subjects which are closer to the social sciences than psychophysics. At the same time, Stigler's definition subordinates the theory of probability to statistics.

Even under his own chosen terms of reference the account is narrow. The determinate part of the theory of errors (the predecessor of the design of experiments) is left out, and almost no attention is given to Lambert, Gauss' precursor in the theory of errors and the first to measure the uncertainty of observations, and to Daniel Bernoulli, who (in addition to his statistical study of smallpox) offered the first bifurcation of errors into constant and random ones; furthermore, Darwin's influence on Pearson is not brought out sufficiently. Again, Poisson's study of the significance of empirical discrepancies and even Galton's work in psychophysics are forgotten; the history of the notion of variance (the main measure of uncertainty!) is unstudied, and the Bienaymé – Chebyshev inequality, wrongly attributed to Chebyshev alone, is mentioned only in passing.

The mathematical description of the works of Mayer, Jakob Bernoulli, Laplace and many other scientists including Fechner is sound indeed, and in some instances no other worthy discussions exist. Still, the author does not describe the relation between the results of De Moivre and Bayes and ignores many other achievements contained in previous literature. Thus, my findings of Euler's heuristic [and indirect] introduction of the principle of least squares and of Gauss' knowledge of an important theorem in linear programming are neglected; Stigler's own discovery that even Simpson [indirectly] expressed the same principle is also left out. That all the appropriate contributions are included in the Bibliography is by no means sufficient. Even the annotations of the *particularly useful* works do not help in this respect. And the Bibliography itself, although impressive, is incomplete. It does not include Chuprov and it leaves out several of my relevant papers from the *Archive for History of Exact Sciences*. I also note that many quotations from Laplace are referred to the appropriate pages of the original editions rather than to his *Oeuvres Complètes*.

The author offers patently wrong or inadmissible assertions such as 1) Jakob Bernoulli did not want to publish his work since his main theorem was not effective enough (p. 77). 2) Laplace's reaction was the only reason why Gauss' introduction of least squares did not pass "relatively unnoticed"(p. 143). 3) "Gauss may well gave been telling the truth" about being the first to use least squares, but he was unsuccessful "in whatever attempts he made to communicate his discovery before 1805" (p. 146).

There are doubtful statements as well, for example 1) Distrusting the combination of equations, Euler used the minimax principle (p. 28). But Kepler and Laplace used this principle to ascertain whether a theory stood an observational test. In addition, Stigler's argument contradicts my general findings[2, 3]. 2) Cotes' rule of treating observations "had little or no influence on Cotes's immediate posterity" (p. 16). In my paper (Note 3), on p. 111, I quoted Laplace as saying that *tous les calculateurs* have followed Cotes' rule. 3) Bayes did not want to publish his work since he was unable to evaluate the incomplete beta function well enough (p. 130). However, Laplace was also unable to evaluate this function, but he did publish his work.

[The statistical community unreservedly praised this book which only goes to show how ignorant it is of, and/or indifferent to the history of statistics. For reasons best known to himself Hald lui-même called the book *epochal*.]

1. Cf. A. N. Kolmogorov & Yu. V. Prokhorov, Mathematical statistics. *Bolshaia Sov. Enz*., 1974, vol. 15, pp. 1428 – 1438, see p. 1428. There is an English translation of the entire *Enziklopedia* (*Great Sov. Enc.*).

2. O. B. Sheynin, Lambert's work on probability. *Arch. Hist. Ex. Sci.*, 1971, vol. 7, pp. 244 – 256, see p. 254.

3. ---, Mathematical treatment of astronomical observations. Ibidem, 1973, vol. 11, pp. 97 – 126, see p. 122. Not mentioned in Stigler's Bibliography.

Centaurus, vol. 31, 1988, pp. 173 – 174

Stigler, Stephen M.: The Bernoullis of Basel, J. Econom. 75, 7 – 13 (1996)

The author offers several remarks on the Bernoulli family, stresses the importance of Daniel Bernoulli's original work on utility theory and comments on his treatment of observations. He falsely accuses three authors (including the reviewer) of *confusing* the chronology of Daniel's two contributions on the last subject and argues that the method of maximum likelihood (rejected by Gauss in 1823) is conceptually preferable to posterior weighting of observations. His reference to the reviewer's paper on Daniel B. is undecipherable. He mentions Euler's note on Daniel's treatment of observations and has nothing positive to say about it; [in 1997, he highly praised Euler's note!]; from 1986 onward, he avoids commenting on the reviewer's discovery of Euler's intuitive anticipation of least squares.

Zentralblatt MATH 858.01013

Stute, W.: History of controversies between R. A. Fisher and J. Neyman or a picture of manners in time of the rise of the English school of statistics. Ann. Soc. Math. Pol., ser. 2. Wiad. Mat. 29, 205 – 221 (1992). In Polish

This is a translation of the original German text published in 1989 (*Math. Semesterber.* 36, 61 – 84).

Zentralblatt MATH 786.01008

Tassi, Philippe: De l'exhaustif au partiel. Un peu d'histoire sur le développement des sondages. J. Soc. Statist. Paris 129, 116 – 132 (1988)

This is a historical essay on the development of sample surveys describing events up to our time. In France, estimations of population drawing on sample investigations began in the second half of the 18th century, and in England, at the turn of that century. In the 19th century, regular general censuses had been carried out instead. However, from the 1920s onward, sample public opinion polls were also practised.

In 1895 the report made by Kiaer at the session of the Intern. Stat. Inst. on the application of sample surveys was severely criticized, but, nevertheless, pertinent theoretical research began to appear at the beginning of the 20th century. The author briefly describes the findings, in this field, of the Chuprovs, father and son, and the later work of Kovalevsky (1924) and Neyman (1934) and the present situation of the theory of sampling is explained. Finally, the origin of the French word *sondage* (statistical questioning) is studied.

Matematika 2A16

Tikhomirov, V. M.: Alexei Ivanovich Markushevich. Reminiscences. Istoriko-Matematich. Issled. 3 (38), 137 – 142 (1999)

The author describes the life and work of Markushevich (1908 – 1979) in the theory of functions of a complex variable as well as his efforts in popularizing mathematics and its history and his educational activities (Vice-President of the Academy of Pedagogical Sciences of the Russian Federation), – and, according to other sources, of the same All Union Academy; Deputy Minister of Education of the Soviet Union).

Zentralblatt MATH 970.01016

Toyoda, Toshiyuki: Essay on Quetelet and Maxwell. From La physique sociale to statistical physics. Rev. Quest. Sci. 168, 279 – 302 (1997)

About a half of this essay is given over to quotations from Quetelet and Maxwell. [The other half is garbage.]

Zentralblatt MATH 929.01015

Véron, Jacques; Rohrbasser, Jean-Marc: Lodewijk et Christiaan Huygens. La distinction entre vie moyenne et vie probable. Math. Sci. Hum. 149, 7 – 21 (2000)

The authors describe the correspondence between the Huygens brothers (1669). Issuing from Graunt's conclusions, the brothers introduced two measures of longevity, discussed their essence and the possible use of each of them.

The article provides a detailed account of its subject and marginal information but hardly contains anything really new; the reviewer treated the same issue in *Arch. Hist. Ex. Sci*. 17, 1 – 61 (1977).

Vitányi, Paul: Randomness. CW1Q. 8, 67 – 82 (1995)
This is an essay on randomness of finite and infinite number sequences compiled from *An Introduction to Kolmogorov Complexity and Its Applications* by M. Li and the author. Berlin, 1993. The author discusses randomness as unpredictability and as incompressibility of data and describes the pertinent work of von Mises, Kolmogorov and Martin-Löf. Numerous quotations are given without providing the exact sources and one of them even without naming its author.

Zentralblatt MATH 833.01019

Weintraub, E. Roy, Editor: Towards a History of Game Theory. Annual Supplement to vol. 24 of "History of Political Economy". Durham, NC, 1992.
Apart from the Editor's Introduction, the volume consists of 11 articles written by 12 authors. It describes the history of game theory beginning with Morgenstern and von Neumann (1944) and even from Borel, as well as the connections of the theory with operational research and its entry into political science. Archival materials written by Morgenstern, von Neumann and four other authors are used in several articles.

Zentralblatt MATH, 822.01001

Wightman, A. S.: On the prescience of J. Willard Gibbs. Proc. Symp. Occas. J. W. Gibbs 150th Anniv., New Haven/CT 1989, 23 – 38 (1990)
The author comments on Gibbs' *Elementary Principles in Statistical mechanics* … (1902) stating that it contains several conceptual contributions which are now recognized as permanent features of classical mechanics and connecting some of Gibbs' ideas with those of quantum mechanics. He also describes the contemporary reaction to the *Elem. Principles* indicating that Zermelo and the Ehrenfests, unlike Hadamard, Lorentz, and Einstein, were rather critical. Finally the author suspects that Hilbert did not read the Gibbs book.

Zentralblatt MATH 733.01014

Williams, E. J.: A survey of experimental design in Australia. Austr. J. Stat. B30, 110 – 130 (1988)
This is a survey of work done in Australia, in 1930 – 1987, on experimental design. The author concludes that *Australian researchers played a significant role … at the forefront of new areas of endeavour …* The appended bibliography is 7.5 pages long.

Zentralblatt MATH 704.01023

Ycart, B.: Le process des étoiles entre De Moivre et Laplace. Cubo Mat. Educ. 3, 1 – 11 (2001)
This is a queer paper. Its title is strange and its essence is superficial and dubious. That Laplace's demonstration of the De Moivre – Laplace theorem was more precise than that of his predecessor is patently wrong as are several more pronouncements. Thus, contrary to the author's opinion, there existed no link between their proposition

and the determination of the figure of the Earth in the 18th century. The only interesting bit is the unsubstantiated statement that Bouguer opposed the arithmetic mean as an estimator of a series of direct measurements.

Zentralblatt MATH, 1070.01004

Zabell, S. L.: Alan Turing and the central limit theorem. Am. Math. Monthly 102, 483 – 494 (1995)

The author dwells on Turing's lone work in probability, a manuscript *On the Gaussian error function* (1934, Smith's prize, 1935) kept at King's College and devoted to the central limit theorem.

Turing rediscovered a version of Lindeberg's theorem and partly anticipated later results due to Feller and Lévy. He chose distribution functions (rather than densities) as his tool, studied their properties as well as those of their convolutions, and proved a particular case of the later Cramér theorem on the normality of the summands given that their sum is normally distributed.

During World War II Turing had applied statistical methods for breaking German codes, and his former assistant, I. J. Good, described these in 1993.

Zentralblatt MATH 833.01016

Zabell, S. L.: Symmetry and Its Discontents. Essays on the History of Inductive Probability. Preface by Brian Skyrms. New York: Cambridge Univ. Press (2005)

This is a valuable collection of the author's 11 contributions (1982 – 1997) which are sufficiently documented and contain many quotations (also from archival sources). The main subject is philosophy of probability and, accordingly, such notions as induction, principles of sufficient and insufficient reason, inverse probability, fiducial inference (*Fisher's great failure*, p. 161), exchangeability are treated. Also described is the life and work of many scholars; thus, De Moivre's proof of his limit theorem is thoroughly investigated. A general index is provided, which is not always the case for collections of such kind. However, it is perhaps not comprehensive; fiducial inference (or probability) is lacking there.

The author is included in the list of advisory editors of the Cambridge Studies …, and only there his first name is given in full: Sandy (a shortened form of Alexander). The absence of his contributions after 1997 is not explained. Zabell often refers to a sloppy and misleading book, T. Porter, The Rise of … [see my review in this collection].

Zentralblatt MATH 1100.01001

II

Oscar Sheynin; § 1 by Miodrag Lovric

Statistics, History of

Intern. Enc. of Statistical Sci. Göttingen, 2011, pp. 1493 – 1504. Editor, M. Lovric

1. Statistics: origin of that term. Many authors described this subject, notably Pearson (1978). It is generally understood that our term originated from the Latin *Status* (situation, condition) of population and economics; in late Latin, the same term meant State. Also, the Italian *statista, statistica*; in 1589, the Italian historian Girolamo Ghilini referred to *civile, politica, statistica e militare scienza* and in 1587 Giovanni Botero described the political structure of several states in his *Della ragione di stato* (English translation 1956) latinised as *De Disciplina status*. And Humboldt (1815, Introduction), wrote "political arithmetic (see my § 2) or, in latino-barbare (late Latin), statistics".

None of the above belonged to statistics or statisticians in our sense and the same is true for later sources: *Hamlet*, see below, Naudé (1639), Politanus (1672), and for Hermann Conring's lectures (from 1660, published 1673). In the 18th century (§ 2), he became the godfather of the *Staatswissenschaft* (Achenwall 1748; 1763).

In English, *statist* and *statistical* appeared in 1584 and 1600, statist also in Shakespeare's *Hamlet*, Act V, Scene 2, 1601, and *statistics* in 1770 (W. Hooper) and 1787 (E. A. W. Zimmermann), all that again in the old sense, and in 1790, in the new sense (J. Sinclair). The main sources here are the *Oxford English Dictionary* and Yule (1905).

Zimmermann was professor at Brunswick Collegium Carolinum, and Gauss, who graduated from that institution, gratefully recalled the "noble" Zimmermann's "fatherly friendship".

2. Staatswissenschaft and political arithmetic. The Staatswissenschaft or University statistics, was born in Germany in the mid-17th century and a century later Achenwall established its Göttingen school which described various aspects of a given state, mostly without use of numbers. His successor Schlözer (1804, p. 86) coined a pithy saying: *History is statistics flowing, and statistics is history standing still*. His followers adopted it as the definition of statistics (which did not involve studies of causes and effects).

Also during that time political arithmetic had appeared (Graunt, Petty). It widely used numbers and elementary stochastic considerations and discussed causes and relations thus heralding the birth of statistics. Graunt (1662/1899) stated that it was necessary to know "how many people there be" of each sex, age, religion, trade etc (p. 396), provided appropriate estimates (sometimes quite wrongly), especially concerning medical statistics. He was able to use sketchy and unreliable statistical data for estimating the population of London and England as well as the influence of various diseases on mortality and attempted to discover regularities in the movement of population. Contradicting the prevailing opinion, he established that both sexes were approximately equally numerous and derived a rough estimate of the sex ratio at birth (p. 389). Graunt also reasonably noted that mortality from syphilis was underestimated because of moral considerations (p. 356). Graunt doubted, however, that statistical investigations were needed for anyone except the King and his main ministers (p. 397).

He also compiled the first ever mortality table (p. 387); although rather faulty but of great methodological importance, it was applied by Jakob Bernoulli and Huygens.

One of the main subjects of political arithmetic was indeed population statistics, and it certainly confirmed that "In a multitude of people is the glory of a king, but without people a prince is ruined" (Proverbs 14:28). And here is another link between the Old Testament and that new discipline: Moses sent spies to the land of Canaan to find out "whether the people [there] are strong or weak, whether they are few or many, […] whether the land is rich or poor […]" (Numbers 13: 17 – 20).

Tabular statistics which appeared in the mid-18th century could have served as a link between the two new disciplines, but its representatives were being scorned as "slaves of tables" (Knies 1850, p. 23). However, in the 1680s Leibniz recommended to compile "statistical tables" with or without numbers and wrote several papers belonging to both those disciplines. They were first published in the 19th century, then reprinted (Leibniz 1986).

Numerical description of phenomena without studying causes and effects also came into being. The London Statistical Society established in 1834 declared that all conclusions "shall admit of mathematical demonstrations" (which was too difficult to achieve), and stipulated that statistics did not discuss causes and effects (which was impossible to enforce), see Anonymous (1839). Louis (1825) described the *numerical method* which was actually applied previously. Its partisans (including D'Alembert) advocated compilation of numerical data on diseases, scarcely applied probability and believed that theory was hardly needed.

A similar attitude had appeared in other natural sciences; the astronomer Proctor (1872) plotted 324 thousand stars on his charts wrongly stating that no underlying theory was necessary. Compilation of statistical yearbooks, star catalogues etc can be mentioned as positive examples of applying the same method, but they certainly demand preliminary discussion of data. Empiricism, underlying the numerical method, was also evident in the Biometric school (§ 8).

The Staatswissenschaft continued to exist, although in a narrower sense; climate, for example, fell away. At least in Germany it is still taught at universities, certainly includes numerical data and studies causes and effects. It thus is partly the application of the statistical method to various disciplines and a given state. Chuprov's opinion (1909/1959, p. 50; 1922, p. 339) that the Staatswissenschaft will revive, although with an emphasis on numbers, and determine the essence of statistics was partly wrong: that science did not at all die, neither does it determine statistics.

3. Statistics and the statistical method. The theory of errors. Kolmogorov & Prokhorov (1988/1990) defined mathematical statistics as a branch of mathematics devoted to systematizing, processing and utilizing statistical data, i. e., the number of objects in some totality. Understandably, they excluded the collection of data and their exploratory analysis. The latter is an important stage of theoretical statistics which properly came into being in the mid-20th century. Debates about mathematical versus theoretical statistics can be resolved by stating that both data analysis and collection of data only belong to the latter and determine the difference between it and the former.

The first definition of the theory of statistics (which seems to be almost the same as theoretical statistics) worth citing is due to Butte (1808, p. XI): it is a science of understanding and estimating statistical data, their collection and systematisation. It is unclear whether Butte implied applications of statistics as well. Innumerable definitions of statistics (without any adjectives) had been offered beginning with Schlözer (§ 2), but the above suffices, and I only adduce the definition of its aims due to Gatterer (1775, p. 15) which seems partly to describe both political arithmetic and the new Staatswissenschaft (§ 2): To understand the state of a nation by studying its previous states.

The statistical method is reasoning based on mathematical treatment of numerical data and the term is mostly applied to data of natural sciences. The method underwent two previous stages. During the first one, statements based on unrecorded general notions were made, witness an aphorism (Hippocrates 1952): Fat men are apt (!) to die earlier than others. Such statements express qualitative correlation quite conforming to the qualitative nature of ancient science.

The second stage was distinguished by the availability of statistical data (Graunt). The present, third stage began by the mid-19th century when the first stochastic criteria for checking statistical inferences had appeared (Poisson, see Sheynin 1978, § 5.2). True, those stages are not really separated one from another: even ancient astronomers had collected numerical observations.

Most important discoveries were made even without such criteria. Mortality from cholera experienced by those whose drinking water was purified was eight times lower than usual (Snow 1855, pp. 74 – 86) which explained the spread of cholera. Likewise, smallpox vaccination (Jenner, end of the 18th century) proved absolutely successful.

The theory of errors belongs to the statistical method. Its peculiar feature is the use of the “true value” of the constants sought. Fourier (1826/1890, pp. 533 – 534) defined it as the limit of the arithmetic mean of observations which is heuristically similar to the frequentist definition of probability and which means that residual systematic errors are included in that value.

From its birth in the second half of the 18th century (Simpson, Lambert who also coined that term (1765, § 321)) to the 1920s it constituted the main field of application for the probability theory, and mathematical statistics borrowed its principles of maximal likelihood (Lambert (1760, § 303)) and least variance (Gauss (1823, § 17)).

Gauss' first justification of the method of least squares (1809) for adjusting "indirect observations" (of magnitudes serving as free terms in a system of redundant linear algebraic equations with unknowns sought and coefficients provided by the appropriate theory) was based on the (independently introduced) principle of maximum likelihood and on the assumption that the arithmetic mean of the "direct observations" was the best estimator of observations. He abandoned that approach and offered a second substantiation (1823), extremely difficult to examine, which rested on the choice of least variance. Kolmogorov (1946) noted in passing that it was possible to assume as the starting point minimal sample variance (whose formula Gauss had derived), – with the method of least squares following at once!

Gauss (1823, § 2) stated that he only considered random errors. Quite a few authors had been favouring this second substantiation; best known is Markov (1899/1951, p. 247) who (p. 246) nevertheless declared that the method of least squares was not optimal in any sense. On the contrary, in case of normally distributed errors it provides jointly efficient estimators (Petrov 1954).

One of the previous main methods for treating indirect observations was due to Boscovich (Cubranic 1961; 1962; Sheynin 1971) who participated in the measurement of a meridian arc. In a sense, it led to the median. Already Kepler (Sheynin 2009, § 1.2.4) indirectly considered the arithmetic mean "the letter of the law". When adjusting indirect observations, he likely applied elements of the minimax method (choosing a "solution" of a redundant system of equations that corresponded to the least maximal absolute residual free term) and of statistical simulation: he corrupted observations by small arbitrary "corrections" so that they conform to each other. Ancient astronomers regarded observations as their private property, did not report rejected results and chose any reasonable estimate. Errors of observation were large, and it is now known that with "bad" distributions the arithmetic mean is not better (possibly worse) than a separate observation.

Al-Biruni, the Arab scholar (10^{th} 11^{th} cc.) who surpassed Ptolemy, did not yet keep to the arithmetic mean but chose various estimators as he saw fit (Sheynin 1992).

There also exists a determinate theory of errors which examines the entire process of measurement without applying stochastic reasoning and which is related to the exploratory data analysis and experimental design. Ancient astronomers selected optimal conditions for observation, when errors least influenced the end result (Aaboe et al 1964). Bessel (1839) found out where should the two supports of a measuring bar be situated to ensure the least possible change of its length due to its weight. At least in the 17^{th} c. natural scientists including Newton gave much thought to suchlike considerations. Daniel Bernoulli (1780) expressly distinguished random and systematic errors. Gauss and Bessel originated a new stage in experimental science by assuming that each instrument was faulty unless and until examined and adjusted.

Another example: the choice of the initial data. Some natural scientists of old mistakenly thought that heterogeneous material could be safely used. Thus, the English surgeon Simpson (1847 –

1848/1871, p. 102) vainly studied mortality from amputations performed in many hospitals during 45 years. On the other hand, conclusions were sometimes formulated without any empirical support. William Herschel (1817/1912, p. 579) indicated that the size of a star randomly chosen from many thousands of them will hardly differ much from their mean size. He did not know that stars enormously differed in size so that their mean size did not really exist and in any case nothing follows from ignorance: *Ex nihilo nihil*!

4. Jakob Bernoulli, De Moivre, Bayes. Chance and Design. The theory of probability emerged in the mid-17th century (Pascal, Fermat) with an effective introduction of expectation of a random event. At first, it studied games of chance, then (Halley 1694) tables of mortality and insurance, and (Huygens 1699) problems in mortality. Halley's research, although classical, contained a dubious statement. Breslau, the city whose population he studied, had a yearly rate of mortality equal to 1/30, the same as in London, and yet he considered it as a statistical standard. If such a concept is at all appropriate, there should be standards of several levels.

Equally possible cases necessary for calculating chances (not yet probabilities) were lacking in those applications, and Jakob Bernoulli (1713, posthumously) proved that posterior statistical chances of the occurrence of an event stochastically tended to the unknown prior chances. In addition, his law of large numbers (the term was due to Poisson) determined the rapidity of that process,

Markov (1900/1924, pp. 44 – 52) improved Bernoulli's crude intermediate calculations and strengthened his estimate. Pearson (1925) achieved even better results, but only by applying the Stirling formula unknown to Bernoulli (as did Markov providing a parallel alternative improvement on pp. 102 – 115). Pearson also unreasonably compared Bernoulli's estimate with the wrong Ptolemaic system of the world. He obviously did not appreciate theorems of existence (of the limiting property of statistical chances).

Statisticians never took notice of that rapidity, neither did they cite Bernoulli's law if not sure that the prior probability really existed and they barely recognized the benefits of the theory of probability (and hardly mentioned the more powerful forms of that law due to Poisson and Chebyshev). They did not know or forgot that mathematics as a science did not depend on the existence of its objects of study. The actual problem was to investigate whether the assumptions of the *Bernoulli trials* (their mutual independence and constancy of the probability of the studied event) were obeyed, and it was Lexis (§ 8) who formulated it. The previous statement of Cournot (1843, § 86), whose book was not duly appreciated, that prior probability can be replaced by statistics in accord with *the Bernoulli's principle* was unnoticed.

The classical definition of probability, due to De Moivre (1738, Intro.) rather than to Laplace, with its equally possible cases is still with us. The axiomatic approach does not help statisticians and, moreover, practitioners have to issue from data, hence from the Mises frequentist theory developed in the 1930s which is not, however, recognized as a rigorous mathematical discovery.

Arbuthnot (1712) applied quite simple probability to prove that only Divine Providence explained why during 82 years more boys were invariably born in London than girls since the chances of a random occurrence of that fact were quite negligible. Cf. however the D'Alembert – Laplace problem: a long word is composed of printer's letters; was the composition random? Unlike D'Alembert, Laplace (1814/1995, p. 9) decided that, although all the arrangements of the letters were equally unlikely, the word had a definite meaning, and therefore composed with an aim. His was a practical solution of a general and yet unsolved problem: to distinguish between a random and a determinate finite sequence of unities and zeros.

Arbuthnot could have noticed that Design was expressed by the binomial law, but it was still unknown. Even its introduction by Jakob Bernoulli and later scientists failed to become generally accepted: philosophers of the 18th century almost always only understood randomness in the "uniform" sense.

While extending Arbuthnot's study of the sex ratio at birth, De Moivre (1733) essentially strengthened the law of large numbers by proving the first version of the central limit theorem thus introducing the normal distribution, as it became called in the end of the 19th century. Laplace offered a somewhat better result, and Markov (1914/1951, p. 511) called their proposition the *De Moivre – Laplace theorem*.

De Moivre devoted the first edition of his *Doctrine of Chances* (1718) to Newton, and there, in the Dedication, reprinted in 1756 (p. 329), we find his understanding of the aims of the new theory: separation of chance from Divine design, not yet the study of various and still unknown distributions etc.

Such separations were being made in everyday life even in ancient India in cases of testimonies (Bühler 1886/1967, p. 267). A misfortune encountered by a witness during a week after testifying was attributed to Divine punishment for perjury and to chance otherwise.

Newton himself (manuscript 1664 – 1666/1967, pp. 58 – 61) considered geometric probability and statistical estimation of the probability of various throws of an irregular die.

Bayes (1764), a memoir with a supplement published next year, influenced statistics not less than Laplace. The so-called Bayes theorem actually introduced by Laplace (1814/1995, p. 10) was lacking there, but here is in essence his pertinent problem: a_i urns (i = 1, 2) contain white and black balls in the ratio of α_i/β_i. A ball is extracted from a randomly chosen urn, determine the probability of its being white. The difficulty here is of a logical nature: may we assign a probability to an isolated event? This, however, is done, for example, when considering a throw of a coin. True, prior probabilities such as $\alpha_i/(\alpha_i + \beta_i)$ are rarely known, but we may keep to Laplace's principle (1803, p. xi): adopt a hypothesis and repeatedly correct it by new observations, – if available!

Owing to these difficulties English and American statisticians for about 30 years had been abandoning the Bayes approach but then (Cornfield 1967) the Bayes theorem *had returned from the cemetery*.

The main part of the Bayes memoir was his stochastic estimation of the unknown prior probability of the studied event as the number of *Bernoulli trials* increased. This is the inverse problem as compared with the investigations of Bernoulli and De Moivre, and H. E. Timerding, the Editor of the German translation of Bayes (1908), presented his result as a limit theorem. Bayes himself had not done it for reasons concerned with rigour: unlike other mathematicians of his time (including De Moivre), he avoided the use of divergent series. Bayes' great discovery also needed by statisticians was never mentioned by them. Great, because it did not at all follow from previous findings and concluded the creation of the initial version of the theory of probability.

Both Bernoulli and De Moivre estimated the statistical probability given its theoretical counterpart and declared that they had at the same time solved the inverse problem (which Bayes expressly considered). Actually, the matter concerned the study of two different random variables with differing variances (a notion introduced by Gauss (1823)), and only Bayes understood that the De Moivre formula did not ensure a good enough solution of the inverse problem.

5. Statistics in the 18th century. Later statisticians took up De Moivre's aim (§ 4) who actually extended Newton's idea of discovering the Divinely provided laws of nature. They, and especially Süssmilch, made the next logical step by attempting to discover the laws of the movement of population, hence to discern the pertinent Divine design. Euler essentially participated in compiling the most important chapter of the second edition, 1761 – 1762, of Süssmilch (1741), and Malthus (1798) picked up one of its conclusions, viz., that population increases in a geometric progression.

Süssmilch also initiated moral statistics by studying the number of marriages, of children born out of wedlock etc. Its proper appearance was connected with A. M. Guerry and A. Quetelet (1830s and later).

Euler published a few elegant and methodically important memoirs on population statistics and introduced such concepts as increase in population and period of its doubling, see Euler (1923). Also methodically interesting were Lambert's studies of the same subject. When examining the number of children in families he (1772, § 108) arbitrarily increased by a half their total number as given in his data likely allowing for stillbirths and mortality.

Most noteworthy were Daniel Bernoulli's investigations of several statistical subjects. His first memoir was devoted to inoculation (1766), to not a quite safe communication of a mild form of the deadly smallpox from one person to another (Jenner introduced vaccination of smallpox at the turn of that century) and proved that it lengthened mean life by two years plus and was thus highly beneficial (in the first place, for the nation). Then, he investigated the duration of marriages (1768), which was necessary for insurance depending on two lives. Finally, he (1770 – 1771) turned to the sex ratio at birth. He evidently wished to discover the *true value* of the ratio of male/female births (which does not really exist) but reasonably hesitated to make a final choice. However, he also derived the normal distribution although without mentioning De Moivre whose statistical work only became known on the Continent by the end of the 19th century.

Laplace (1812, Chapter 6) estimated the population of France by sampling (§ 7) and studied the sex ratio at birth. In this latter case he introduced *functions of very large numbers* (of births a and b) $x^a(1-x)^b$ and managed to integrate them. As usual, he had not given thought to thoroughly presenting his memoirs. While calculating the probability that male births will remain prevalent for the next hundred years, he did not add *under the same conditions of life*; and the final estimate of France's population was stated carelessly: Poisson, who published a review of that classic, mistakenly quoted another figure. Laplace's *Essai philosophique* (1814) turned general attention to probability and statistics.

6. The theory of probability and statistics. Quetelet. Both Cournot (1843) and Poisson (1837) thought that mathematics should be the base of statistics. Poisson with co-authors (1835) were the first to state publicly that statistics was "the functioning mechanism of the calculus of probability" and had to do with mass observations. The most influential scholars of the time shared the first statement and likely the second as well. Fourier, in a letter to Quetelet (1869, t. 1, p. 103) written around 1820, declared that statistics must be based on *mathematical theories*, and Cauchy (1845/1896, p. 242) maintained that statistics provided means for judging doctrines and institutions and should be applied "avec tout la rigueur".

However, Poisson and Gavarret, his former student who became a physician and the author of the first book on medical statistics (1840), only thought about large numbers (e. g., when comparing two empirical frequencies) and a German physician Liebermeister (ca. 1877) complained that the alternative, i. e., the mathematical statistical approach was needed.

The relations between statistics and mathematics remained undecided. The German statistician Knapp (1872, pp. 116 – 117) declared that placing coloured balls in Laplacean urns was not enough for shaking scientific statistics out of them. Much later mathematicians had apparently been attempting to achieve something of the sort since Chuprov (1922, p. 143) remarked that "Mathematicians playing statistics can only be overcome by mathematically armed statisticians". In the 19th, and the beginning of the 20th century statisticians had still been lacking such armament.

Quetelet, who dominated statistics for several decades around the mid-19th century, popularized the theory of probability. He tirelessly treated statistical data, attempted to standardize population statistics on an international scale, initiated anthropometry, declared that statistics ought to help foresee how various innovations will influence society, collected and systematized meteorological data. Being a religious person, he (1846, p. 259) denied any evolution of organisms which to some extent explains why Continental statisticians were far behind their English colleagues in studying biological problems. And Quetelet was careless in his writings so that Knapp (1872, p. 124) stated that his spirit was rich in ideas but unmethodical and therefore un-philosophical. Thus, Quetelet (1836, t. 1, p. 10) stated without due justification that the crime rate was constant although he reasonably but not quite expressly added: under invariable social conditions.

Quetelet paid attention to preliminary treatment of data and thus initiated elements of the exploratory data analysis (§ 3); for example, he (1846, p. 278) maintained that a too detailed subdivision of the material was a *charlatanisme scientifique*. He (1848, p. 38) introduced the concept of Average man both in the impossible physical sense (for example, mean stature and mean weight cannot coexist) and in the moral sphere, attributed to him mean inclinations to crime (1836, t. 2, p. 171) and marriage (1848, p. 77) and declared that that fictitious being was a specimen of mankind (1832, p. 1).

Only in passing did he mention the Poisson law of large numbers, so that even his moral mean was hardly substantiated. Worse: he had not emphasized that the inclinations should not be attributed to individuals, and after his death German statisticians, without understanding the essence of the matter, ridiculed his innovations (and the theory of probability in general!) which brought about the downfall of *Queteletism*.

Fréchet (1949) replaced the Average man by *homme typique*, by an individual closest to the average. In any case, an average man (although not quite in Quetelet's sense) is meant when discussing per capita economic indications.

7. New times. Great progress and the Soviet cul-de-sac. In the main states of Europe and America statistical institutions and/or national statistical societies, which studied and developed population statistics, came into being during the first five decades of the 19th century. International statistical congresses aiming at unifying official statistical data had been held from 1851 onward, and in 1885 the still active International Statistical Institute was established instead.

A century earlier Condorcet initiated and later Laplace and Poisson developed the application of probability for studying the administration of justice. The French mathematician and mechanician Poinsot (1836) declared that calculus should not be applied to subjects permeated by imperfect knowledge, ignorance and passions and severe criticism was levelled at applications to jurisprudence for tacitly assuming independence of judges or jurors: "In law courts people behave like the *moutons de Panurge*" (Poincaré 1912, p. 20). Better known is Mill's declaration (1843/1886, p. 353): such applications disgrace mathematics. Laplace (1812, Supplement of 1816/1886, p. 523) only once and in passing mentioned that assumption.

However, stochastic reasoning can provide a "guideline" for determining the number of witnesses and jurors (Gauss, before 1841/1929, pp. 201 – 204) and the worth of majority verdicts. Poisson (1837, p. 4) introduced the mean prior (statistically justified) probability of the defendant's guilt, not to be assigned to any individual and akin to Quetelet's inclination to crime. Statistical data was also certainly needed here. Quetelet (1836, t. 2, p. 313) studied the rate of conviction as a function of the defendant's personality, noted that in Belgium the rate of conviction was considerably higher than in France (1833, p. 18) and correctly explained this by the absence, in the former, of the institution of jurors (1846, p. 334).

Statistical theory was also invariably involved in jurisprudence in connection with errors of the first and second kind. Thus (Sheynin 2009, p. 17), the Talmud stipulated that a state of emergency (leading to losses) had to be declared in a town if a certain number of its inhabitants died during three consecutive days. Another example pertaining to ancient India is in § 4.

A number of new disciplines belonging to natural science and essentially depending on statistics had appeared in the 19th century. *Stellar statistics* was initiated earlier by William Herschel (1784, p. 162) who attempted to catalogue all the visible stars and thus to discover the form of our (finite, as he thought at the time) universe. In one section of the Milky Way he replaced counting by sample estimation (p. 158). He (1783) also estimated the parameters of the Sun's motion by attributing to it the common component of the proper motion of a number of stars. Galileo (1613) applied the same principle for estimating the period of rotation of the Sun about its axis: he equated it with the (largely) common period of rotation of sunspots.

Most various statistical studies of the solar system (Cournot 1843) and the starry heaven (F. G. W. Struve, O. Struve, Newcomb) followed in the mid-19th century and later (Kapteyn). Newcomb (Sheynin 2002) processed more than 62 thousand observations of the Sun and the planets and revised astronomical constants. His methods of treating observations were sometimes quite unusual. Hill & Elkin (1884, p. 191) concluded that the "great Cosmical questions" concerned not particular stars, but rather their average parallaxes and the general relations between star parameters.

Daniel Bernoulli was meritorious as the pioneer of *epidemiology* (§ 5). It came into being in the 19th century mostly while studying cholera epidemics. The other new disciplines were *public hygiene* (the forerunner of ecology), *geography of plants, zoogeography, biometry and climatology*.

Thus, in 1701 Halley published a chart of North Atlantic showing (contour) lines of equal magnetic declination, and Humboldt (1817) followed suit by inventing lines of equal mean yearly temperatures (isotherms) replacing thousands of observations and thus separating climatology from meteorology. These were splendid examples of exploratory data analysis (§ 3). Also in meteorology, a shift occurred from studying mean values (Humboldt) to examining deviations from them, hence to temporal and spatial distributions of meteorological elements.

Statistics ensured the importance of public hygiene. Having this circumstance in mind, Farr (ca. 1857, published 1885, p. 148) declared that "Any deaths in a people exceeding 17 in 1,000 annually are unnatural deaths". Data pertaining to populations in hospitals (*hospitalism*, mortality due to bad hygienic conditions), barracks and prisons were collected and studied, causes of excessive mortality indicated and measures for preventing it made obvious.

At least medicine had not submitted to statistics without opposition since many respected physicians did not understand its essence or role. A staunch supporter of “rational” statistics was Pirogov, a cofounder of modern surgery and founder of military surgery. He stressed the difficulty of collecting data under war conditions and reasonably interpreted them.

Around the mid-19th century, statistics essentially fostered the introduction of anaesthesia since that new procedure sometimes led to serious complications. Another important subject statistically studied was the notorious hospitalism, see above.

Biometry indirectly owed its origin to Darwin, witness the Editorial in the first issue of *Biometrika* in 1902: “The problem of evolution is a problem of statistics. […] Every idea of Darwin […] seems at once to fit itself to mathematical definition and to demand statistical analysis”.

Extremely important was the recognition of the statistical laws of nature (theory of evolution, in spite of Darwin himself), kinetic theory of gases (Maxwell), stellar astronomy (Kapteyn). And the discovery of the laws of heredity (Mendel 1866) would have been impossible without statistics. Methodologically these laws were based on the understanding that randomness in individual cases becomes regularity in mass (Kant, Laplace, and actually all the stochastic laws).

Laplace (1814; English translation 1995, p. 2) declared that randomness was only occasioned by our failure to comprehend all the natural forces and by the imperfection of analysis, and he was time and time again thought only to recognize determinism. However, the causes he mentioned were sufficiently serious; he expressly formulated *statistical determinism* (for example, stability of the relative number of dead letters, an example of transition from randomness to regularity); and his work in astronomy and theory of errors was based on the understanding of the action of random errors. It is also opportune to note here that randomness occurs in connection with unstable movement (Poincaré) and that a new phenomenon, chaotic behaviour (an especially unpleasant version of instability of motion), was discovered several decades ago. Finally, Laplace was not original: Maupertuis (1756, p. 300) and Boscovich (1758, § 385) preceded him.

In the 19th century, but mostly perhaps in the 20th, the statistical method penetrated many other sciences and disciplines beyond natural sciences so that it is now difficult to say whether any branch of knowledge can manage without it.

There are other points worth mentioning. *Correlation theory* continued to be denied even in 1916 (Markov), actually because it was not yet sufficiently developed. Its appearance (Galton, Pearson) was not achieved at once. In 1865 – 1866 the German astronomer and mathematician Seidel quantitatively estimated the dependence of the number of cases of typhoid fever on the level of subsoil water and precipitation but made no attempt to generalize his study. And in the 1870s several scientists connected some terrestrial phenomena with solar activity but without providing any such estimates.

According to Gauss (1823, § 18), for series of observations to be independent, it was necessary for them not to contain common measurements, and geodesists without referring to him have been intuitively keeping to his viewpoint. For two series of about m

observations each, n of them common to both, the measure of their interdependence was thought to be n/m. In 1912 Kapteyn made the same proposal without mentioning anyone.

Estimation of precision was considered superfluous (Bortkiewicz 1894 – 1896, Bd. 10, pp. 353 – 354): it is a *luxury* as opposed to the statistical feeling. *Sampling* met with protracted opposition although even in 1812 the German statistician Lueder (p. 9) complained about the appearance of "legions" of numbers. In a crude form, it existed long ago, witness the title of Stigler (1977). In the 17th century in large Russian estates it was applied for estimating the quantity of the harvested grain, and, early in the next century Marshal Vauban, the *French Petty*, made similar estimations for France as a whole.

No wonder that Laplace, in 1786, had estimated the population of France by sampling, and, much more important, calculated the ensuing error. True, Pearson (1928) discovered a logical inconsistency in his model. As a worthy method, sampling penetrated statistics at the turn of the 19th century (the Norwegian statistician Kiaer) and in 1906 Kapteyn initiated the study of the starry heaven by stratified sampling, but opposition continued (Bortkiewicz 1901).

The *study of public opinion* and *statistical control of quality of industrial production*, also based on sampling, had to wait until the 1920s (true, in 1848 Ostrogradsky proposed to check samples of goods supplied in batches), and *econometrics* was born even later, in the 1930s.

A curious side issue of statistics, *sociography*, emerged in the beginning of the 20th century. It studies ethnic, religious etc. sub-groups of society, does not anymore belong solely to statistics and seems not yet to be really scientific. And in sociology it became gradually understood that serious changes in the life of a society or a large commercial enterprise should be based on preliminary statistical studies.

Soviet statistics became a dangerous pseudoscience alienated from the world (Sheynin 1998). Its main goal was to preserve appearances by protecting Marxist dogmas from the pernicious influence of contemporary science and it frustrated any quantitative studies of economics and banished mathematics from statistics. In 1909 Lenin called Pearson a Machian and an enemy of materialism which was more than enough for Soviet statisticians to deny the work of the Biometric school lock, stock and barrel.

Culmination of the success in that direction occurred in 1954, during a high-ranking conference in Moscow. Its participants even declared that statistics did not study mass random phenomena which, moreover, did not possess any special features. Kolmogorov, who was present at least for his own report, criticized Western statisticians for adopting unwarranted hypotheses …

Soviet statisticians invariably demanded that quantitative investigations be inseparably linked with the qualitative content of social life (read: subordinated to Marxism), but they never repeated such restrictions when discussing the statistical method as applied to natural sciences.

8. The two streams of statistical thought. Lexis (1879) proposed a distribution-free test for the equality of probabilities of the studied event in a series of observations, the ratio *Q* of the standard deviation of the frequency of the occurrence of the studied event, as calculated by the Gauss formula, to that peculiar to the binomial distribution. That ratio would have exceeded unity had the probability changed; been equal to unity otherwise, all this taking place if the trials were independent; and been less than unity for interdependent trials. Lexis (1879, § 1) also qualitatively isolated several types of statistical series and attempted to define stationarity and trend.

Bortkiewicz initiated the study of the expectation of *Q* and in 1898 introduced his celebrated law of small numbers which actually only essentially popularized the barely remembered Poisson distribution. In general, his works remain insufficiently known because of his pedestrian manner, excessive attention to detail and bad composition which he refused to improve. Winkler (1931, p. 1030) quoted his letter (date not given) stating that he expected to have five readers (!) of his contribution.

Markov and mostly Chuprov (1918 – 1919) refuted the applicability of *Q* but anyway Lexis put into motion the Continental direction of statistics by attempting to base statistical investigations on a stochastic basis. Lexis was not, however, consistent: even in 1913 he held that the law of large numbers ought to be justified by empirical data. Poisson can be considered the godfather of the new direction.

On the other hand, the Biometric school with its leader Pearson was notorious for disregarding stochastic theory and thus for remaining empirical. Yet he developed the principles of correlation theory and contingency, introduced *Pearsonian* curves for describing asymmetrical distributions, devised the most important chi-squared test and published many useful statistical tables. To a large extent his work ensured the birth of mathematical statistics.

Pearson successfully advocated the application of the new statistics in various branches of science and studied his own discipline in the context of general history (1978, posthumous). There (p. 1) we find: "I do feel how wrongful it was to work for so many years at statistics and neglect its history". He acquired many partisans and enemies (including Fisher). Here is Newcomb in a letter to Pearson of 1903 (Sheynin 2009, § 10.9.4) and Hald (1998, p. 651): "You are the one living author whose production I nearly always read when I have time […] and with whom I hold imaginary interviews […]"; "Between 1892 and 1911 [he] created his own kingdom of mathematical statistics and biometry in which he reigned supremely, defending its ever expanding frontiers against attacks".

Nevertheless, the work of his school was scorned by Continental scientists, especially Markov, the apostle of rigour. Chuprov, however, tirelessly, although without much success, strove to unite the two streams of statistical thought. Slutsky also perceived the importance of the Biometric school. He (1912) expounded its results and, although only in a letter to Markov of 1912, when he was not yet sufficiently known, remarked that Pearson's shortcomings will be overcome just as it happened with the non-rigorous mathematics of the 17^{th} and 18^{th} centuries.

Chuprov also achieved important results, discovering for example finite exchangeability (Seneta 1987). He mainly considered problems of the most general nature, hence inevitably derived unwieldy and too complicated formulas, and his contributions were barely studied. In addition, his system of notations was horrible. In one case he (1923, p. 472) applied two-storey superscripts and, again, two-storey subscripts in the same formula!

Markov, the great mathematician, was to some extent a victim of his own rigidity. Even allowing for the horrible conditions in Russia from 1917 to his death in 1922, it seems strange that he failed, or did not wish to notice the new tide of opinion in statistics (and even in probability theory).

9. Mathematical statistics. In what sense is mathematical statistics different from biometry? New subjects have been examined such as sequential analysis, the treatment of previously studied problems (sampling, time series, hypothesis testing) essentially developed, links with probability theory greatly strengthened (Pearson's empirical approach is not tolerated anymore). New concepts have also appeared and this seems to be a most important innovation. Fisher (1922) introduced statistical estimators with such properties as consistency, efficiency etc some of which go back to Gauss who had used and advocated the principle of unbiased minimum variance.

It is known that the development of mathematics has been invariably connected with its moving ever away from Nature (for example, to imaginaries) and that the more abstract it was becoming, the more it benefited natural sciences. The transition from true values to estimating parameters was therefore a step in the right direction. Nevertheless, the former, being necessary for the theory of errors, are still being used in statistics, and even for objects not existing in Nature, see Wilks (1962, § 10.1), also preceded by Gauss (1816, §§ 3 and 4) in the theory of errors.

Rao (*Math. Rev*. 2005k:62007) noted that modern statistics has problems with choosing models, measuring uncertainty, testing hypotheses and treating massive sets of data, and, in addition, that statisticians are not acquiring sufficient knowledge in any branch of natural science.

References

Abbreviation: JNÖS = *Jahrbücher f. Nationalökonomie u. Statistik*

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III

Fr. E. Croxton, D. J. Cowden

Applied General Statistics

New York, 1939. Eighteenth printing, 1950

Only Preface and the general contents,
without copying the contents of the chapters

Preface

This book is intended for … readers who are interested in the understanding of statistical methods and their applications in various fields. … The present text … does present a greatly amplified treatment of analytical methods. … Probably [the scope of the book] is so great that it cannot be covered in most introductory courses …

Contents

IV

G. K. Mikhailov, O. B. Sheynin

Demographic parts of the extant correspondence between Daniel Bernoulli and N. Fuss

Euler, *Opera omnia* ser. IVa, t. 3, 2016
Basel, pp. 948 – 950

2.40
N. Fuss an D. Bernoulli
Petersburg, 17. (6.) Januar 1777

Vous recevrés bientôt Monsieur un petit ouvrage allemand intitulé: *Allgemeine Leihebank* [7], qui vient d'être imprimé et qui est une suite des *Éclaircissemens sur les établissemens publics* etc. [8], que j'ai calculés sous Mr <Leonhard> Euler. Je ne scais pas, si Vous le trouvérés conforme à l'idée que Vous Vous en étés formé sur ce que Mr Euler Vous en avoit dit [9]. Nous n'avons point d'observations sur la multiplication et la mortalité en Russie, ensorte que, la diversité du climat influant tant sur ces deux articles, on a été obligé de laisser beaucoup d'arbitraire dans la détermination des prix, des rentes, etc. Néanmoins il pourra être de quelque utilité même s'il ne faisoit que celle de rendre le public d'ici plus circonspect et plus méfiant envers cette infinité d'établissemens particuliers de l'institution la plus absurde et la plus préjudiciable qui nous innondent dépuis quelque temps.

[7] Cf. N.Fuss (1776b).
[8] Cf. N.Fuss (1776a).
[9] Wir wissen nichts über diese Unterhaltung L.Eulers mit D.Bernoulli.

2.41
D.Bernoulli an N.Fuss
Basel, 07. Juni (27. Mai) 1777

Encore dernièrement M. votre père <J.H.Fuss> m'a remis de votre part un exemplaire de votre excellent ouvrage sur les tontines [1]. Si les remerciemens que j'ai l'honneur de vous faire de ce beau présent littéraire, sont un peu tardifs, je n'en ai pas moins senti tout le prix. Pourrois-je mésestimer un ouvrage dont le titre porte les deux noms qui m'ont toujours été les plus respectables et les plus chers, celui de M. Léonard Euler et de M. Nicolas Fuss. N'accusez donc à cet égard que mes infirmités inséparablement attachées à la vieillesse; *ipsa senectus morbus est*! A cet état naturel d'accablement et de souffrance s'est joint une fièvre catarrhale assez forte pour enlever un home bien plus robuste que moi, et qui cependant n'a pas voulu achever son ouvrage. Mais ce n'est que depuis peu de tems que j'ai pu commencer à lire votre traité; j'ai relu même le mémoire de M. Euler inséré dans les *Mémoires de l'académie de Berlin* pour l'année 1760 [2]. On ne sauroit mieux traiter cette matière que vous l'avez fait l'un et l'autre: mais c'est avec beaucoup de raison que vous vous plaignez du peu d'accord mutuel dans les tables nécrologiques les mieux faites, desquelles cependant dépend uniquement tout le résultat que vous voulez déterminer dans vos différentes questions, et qui malheureusement en est trop affecté pour négliger le désaccord. Il y a des tables qui accusent l'âge de 5 ans au delà duquel la moitié de la génération est déjà éteinte. Suivant la table de M. Smart [3], établie sur les registres mortuaires de Londres, les trois premières années de la vie suffisent pour enlever la moitié de toute une génération. La table de M. Halley [4], suivant les registres mortuaires de Breslau, si on suppose la génération de 1200 (puisqu'on y part de la valeur (1) = 1000) nous apprend que c'est à l'âge de 20 ans que le nombre des vivans est réduit à la moitié, et ce terme me paroît le plus vraisemblable. M. Kerseboom {Kersseboom} trouve 31 ans [5], et vous adoptez le même âge: Un tel terme est sans doute excessif pour le général, et il me semble qu'en suivant la liste mortuaire de M. Kerseboom, on augmente trop l'avantage de la banque. Cependant cette excessive inégalité est considérablement diminuée si on suppose qu'on ne parte que depuis l'âge de 15 ans accomplis. Mais il y a une autre réflexion à faire qui me paroît très essentielle pour l'établissement des rentes destinées simplement aux femmes devenues veuves; c'est que, selon le célèbre M. Wargentin [6], l'ordre de mortalité est fort différent entre les hommes et les femmes, à l'avantage des femmes. C'est une des raisons qui fait que, dans les villes, il y ait ordinairement 3 ou 4 fois plus de veuves que de veufs, quoiqu'il y ait encore plusieurs autres raisons à alléguer là-dessus. Si la chose est bien vraie, comme je n'en doute pas, elle demanderoit une grande réforme dans vos calculs sur les Witwen-Cassen [7], quoique tout-à-fait justes pour l'hypothèse communément reçue. Vous aurez sans doute à Pétersbourg l'ouvrage de M. Déparcieux de l'an 1746 [8]; mais peut-être n'avez vous pas l'addition à cet ouvrage imprimée l'an 1760 [9] qui finit par une table déduite des dénombremens faits en Suède, pour l'un et l'autre sexe, chacun à part.

[1] Cf. N.Fuss (1776a).
[2] Cf. L.Eulers Abhandlung über die Sterblichkeit und Vermehrung der Menschheit (E.334).
[3]. Cf. Smart (1726).
[4]. Cf. Halley (1693).
[5]. Cf. Kersseboom (1742, 1970).
[6]. Cf. Wargentin (1766).
[7]. Cf. N.Fuss (1776b).
[8]. Cf. Déparcieux (1746).
[9]. Cf. Déparcieux (1760).

2.42
N. Fuss an D. Bernoulli
Petersburg, 25. (14.) Juli 1777

Je viens aux réflexions sur les Tables nécrologiques, et principalement à celle sur la table de Mr Keerseboom {Kersseboom} [1], que j'ai employée dans mes calculs. Elle est sans doute fort différente de celles de Halley [2], Deparcieux [3], Susmilch {Süßmilch} [4], Lambert [5], etc. et beaucoup plus de Simpson [6], Smart [7], Hogdeson {Hodgson} [8]; mais les dernières, dressées sur Londres, c'est à dire sur la ville du monde la moins ressemblante à tout autre, ne scauroient être comparées à aucune des premières, et ne pourroient servir que pour Londres même, à cause de la grande difformité qui se trouve justement depuis l'âge de 20 ans jusqu'à 60 et comprend par conséquent les âges les plus intéressans dans ces sortes de calculs. Les tables de Déparcieux et de Dupré de S^{t} Maur [9] pour Paris diffèrent moins des autres, par rapport à ces âges, mais elles sont fautifes ou bien inconcluantes pour l'enfance. Celle de Mr Wargentin [10] l'est suivant Süssmilch pour tout autre endroit que Stockholm, parceque le nombre des morts entre 20 et 40 ans y est même plus grand que dans les listes de Londres. La table de Mr Halley est de même trop spécielle et restrein[te] à la seule ville de Breslaw et autres endroits semblables et n'a pour garant de son accuratesse que 5 ans d'observation, terme trop court, pour qu'on en puisse construire des proportions générales. Celle de Mr Süssmilch me paroit être la plus conforme aux loix de la nature et à l'ordre véritable de mortalité; elle est fondée non seulement sur une infinité d'observations de tout pays et de plusiares [!] années, mais elle contient en même temps les proportions moyennes des grandes et petites villes et de la campagne, et doit par conséquent représenter au juste l'ordre général de mortalité, et les limites, où il faut se restreindre dans le calcul des rentes, tontines, etc. Mr Lambert l'accuse à la vérité (*Beyträge zur Mathematik* Tom. III) [11] de s'écarter trop de la moyenne raison entre les grand[es] villes et la campagne; mais ce repproche ne touche que les âges au dessus de 60 ans, âge où le reste de force vita[le] doit être partout à peu près le même, où l'homme éloigné de la fougue des passions et de toute profession dangereuse a autant d'espérance pour le reste de ses jours au milieu du désordre d'une ville commercante maritime et débauchée que dans la plus paisible retraite sous la chaumière du paysan. Il est vray que le nombre des vivans à la campagne est un peu moindre dans la table de Süssmilch, dont Mr Lambert s'est servi en faisant cette observation, que dans celle des moyennes raisons; mais Mr Süssmilch dit lui même qu'il y a eu beaucoup d'années épidémiques parmi celles, dont il a fait usage pour la construction de sa table pour la campagne, et outre cela Mr Lambert a mis le terme (0) = 10000, d'où la différence qu'il trouve entre la moyenne raison de Süssmilch et les nombres des morts aux environs de Berlin réduite au terme (0) = 1000 devient presque imperceptible. J'aurois donc fait usage de la table de Mr Süssmilch sans la réflexion de Mr Euler (*Mém. de l'acad. de Berlin* 1760 pag. 151) [12] qu'il ne faut pas régler le calcul des rentes etc. sur les registres de la vraye mortalité, mais qu'il vaut mieux se servir de celle des registres des rentes viagères, qui commencent par des enfans au dessous d'un an échappés déjà aux dangers des premiers mois, par ce qu'on ne s'enga[ge] guères que pour des personnes d'une complexion forte. On ne scauroit nier, et toutes les observations le confirme[nt,] que la table de Mr Kerseboom donne les nombres des vivans trop grands, surtout pour les premiers âges; car dès l'âge de 6 ans elles approchent beaucoup de celles de Süssmilch et depuis 36 jusqu'à 75 elles n'en différen[t] presque rien du tout. J'aurois donc pu employer celle de Süssmilch, et aller même avec la dernière exactitude jusqu'au terme de la génération, moyennant la table de Mr Struick {Struyck}, insérée dans l'ouvrage de Mr Süssmilch [13] Tom. II pag. 318: où il assigne, combien il meurt d'un certain nombre d'enfans dans le 1r, 2d, 3e etc. mois jusqu'à la fin de la première année; mais cette scrupulosité auroit été inutile, par la raison, que dans toutes ces sortes d'engagemens on a toujours égard à la vigueur et à l'état de santé de celui, pour qui l'on s'intéresse, et de l'un et de l'autre côté on prend des précautions telles par rapport au choix des participans, que le nombre des morts, doit être effectivement moindre, que les tables mortuaires les assignent. Au surplus en ne partant, comme Vous remarqués que de l'âge de 15 ans accomplis, l'avantage de la banque trop augmenté suivant le calcul fondé sur la table de Mr Keerseboom sera bien réduit et les loix de la parfaite égalité moins offensés que le premier coup d'oeil pourroit le persuader.

Quant à la seconde remarque, que l'ordre de mortalité soit différent entre les deux sexes à l'avantage des femmes, elle seroit sans doute très importante et désavantageuse à l'accuratesse des calculs, si la différence étoit aussi énorme, que Mr Wargentin le prétend. Il est décidé, par une infinité de listes mortuaires, qu'il meurt beaucoup plus de garcons que de filles et cela dans la raison 26 : 25; la raison en est évidente; car on voit dans les tables de multiplication qu'il nait aussi plus de garcons que de filles dans la raison 26 : 25 ou 21 : 20 ensorte que vers l'âge de la puberté l'égalité entre les deux sexes soit à peu près rétablie. C'est donc le terme a quo dans le calcul pour la Wittwenkasse [14], d'où l'on part en supposant une parfaite égalité entre les deux sexes; mais encore depuis ce terme il n'est pas moins vray qu'il meurt plus de males que de l'autre sexe, mais tout au plus dans la raison susdite 26 : 25 ou 79 : 75, toujours trop peu considérable pour produire cette grande différence entre les veuves et les veufs, qui doit monter, comme Vous dites, jusqu'à la raison de 1 : 3 et 1 : 4; qui me paroit trop forte, du moins pour le général; car après avoir consulté là dessus les tables de Lambert, Süssmilch, Struick, Deparcieux, etc. je les trouve tous asses d'accord sur la raison des veufs aux veuves = 11 : 16 ou 11 : 17. Cette différence doit avoir sa source principale dans les difficultés, que trouvent les veuves des premières noces à se remarier; car il y a toujours un plus grand nombre de veufs, qui entrent en second mariage; la raison en est facile à concevoir. Or puisque cette dernière circonstance n'a la moindre influence sur la détermination des prix assignés dans les tables de l'établissement pour les veuves, et que d'ailleurs on peut prendre des précautions semblables (quoique moins rigoureuses) à celles que l'établissement de Berlin observe dans le choix des maris participans, les inconvéniens, qu'on auroit à craindre de la différence de l'ordre de mortalité seront bien diminués ou même réduits à rien.

[1] Cf. Kersseboom (1742).

[2]. Cf. Halley (1693).
[3] Cf. Déparcieux (1746).
[4] Cf. Süßmilch (1761–1762). Die erste Ausgabe dieses Werkes war 1741–1742 veröffentlicht.
[5] Cf. Lambert (1772).
[6] Cf. Simpson (1742, 1743, 1752).
[7] Cf. Smart (1726).
[8] Cf. Hodgson (1747)
[9] Cf. Dupré de Saint-Maur (1749).
[10] Cf. Wargentin (1769).
[11] Cf. Lambert (1772).
[12] Cf. L.Eulers Abhandlung über die Sterblichkeit und Vermehrung der Menschheit (E.334).
[13] Cf. Struyck (1762, 1912).
[14] Cf. N.Fuss (1776b).

Zur Einleitung zum Briefwechsel von D.Bernoulli mit N.Fuss

Umständlich diskutieren die Briefpartner demographische und damit verbundenen Versicherungsprobleme.

Die nachfolgende Übersicht dieses Teiles des Briefwechsels ist hier nach der Skizze von Professor Oscar Sheynin angeführt.

Der erste, der die Bevölkerungsstatistik untersuchte, war Graunt (1662). Im 18. Jahrhundert befassten sich mit demographischen und Versicherungsproblemen de Moivre, D.Bernoulli, L.Euler, später Laplace, sowie mehrere andere Gelehrte. D.Bernoulli und N.Fuss besprechen in ihrem Briefwechsel aktuelle Probleme der Demographie und Versicherungswesens und zwar die Mortalität verschiedener Altersgruppen der Bevölkerung, Unterschied in der Sterblichkeit der Stadt- und Landbevölkerung, der Männer und Frauen, damalige Totenlisten und Tontine. Diesen Problemen widmeten L.Euler und D.Bernoulli einige Untersuchungen in den 1760er Jahren (cf. Sheynin 1972, 2007 und Sofonea 1957). Am Anfang der Diskussion erwähnt N.Fuss zwei seine Abhandlungen (1776 a, b), die erste von denen war von ihm zusammen mit Euler vorbereitet (sie ist sogar in Eulers *Opera omnia* als E.473 eingeschlossen) und einem neuen System der Tontine gewidmet. Tontine [1] bildet eine Gruppe Rentnern, die die jährliche Prozenten von ihren Einlagen nur unter den noch lebenden Mitgliedern verteilt. Die Langleber bekamen dabei beträchtliche Summen und nach dem Ableben des letzten Mitglied der Tontine verschwand sie. Wahrscheinlich Euler und nicht Fuss schlug vor eine *ewige* Tontine mit veränderlichen Einlagen und Möglichkeit des Eintretens neuer Mitglieder (1776), aber dieser Vorschlag war nicht realisiert.

Euler war auch Mitverfasser von Süßmilch, dessen Werk (1761–1762) N. Fuss oft zitiert. Ein bedeutender Teil eines Kapitels von diesem Werk (1761) ist in Eulers *Opera omnia* wiedergegeben, obwohl diese Publikation in Eneströms Verzeichnis nicht angezeigt ist. Stützend auf willkürlichen Gründen, Euler zeigte trotzdem dabei,

[1] Benennung *Tontine* stammt vom Namen des italienischen Bankier Lorenzo de Tonti, der dieses Versicherungssystem in Frankreich in der Mitte des 17. Jahrhunderts vorschlug. Cf. McKeever (2010).

dass die Bevölkerung wächst in geometrischen Progression (was man auch jetzt zugibt); dieser Satz war bald darauf von Malthus (1798) übernommen. Süßmilch beging einige Fehler in seinen theoretischen demographischen Rechnungen, doch begründete er die *moralische* Statistik der Verbrechen, Suizide, unehelichen Geburten, und seine Todtafel waren noch anfangs des 19. Jahrhunderts benutzt. Er deklarierte (1758), dass die Armut und Unwissenschaft fördert Epidemien.

Euler (1767) was his main publication on the subject partly following from his work with Süssmilch

Speziell soll man die Sterbetafeln von Halley (1693) erwähnen, auf deren Grund de Moivre (1725) einen kontinuierlichen gleichmässigen Gesetz der Sterblichkeit für die Älter nach 12 Jahren einführte. Doch unterschätzte Halley den Einfluss der systematischen Fehler und nahm Breslau als statistischen Standart an, obwohl die Sterblichkeit in Breslau merkwürdigerweise egal dem London sich herausstellte.

D.Bernoulli und N.Fuss benutzen und analysieren in ihrem Briefwechsel vor allem die Werke[2] von Halley, Kersseboom (cf. Heuschling 1857), Deparcieux (cf. Ptoukha 1938), Süßmilch (cf. Birg 1986), Wargentin (cf. Nordenmark 1929), Lambert (cf. Sheynin 1971), sowie auch Hodgson, Struyck, Smart, Dupré de Saint-Maur und Simpson. Eine inhaltsreiche Übersicht aller diesen Werke findet man in der fundamentalen Monographie von Pearson (1978). Pearson gibt manchmal unerwartet gerade und nicht traditionelle Bewertungen der Beiträge einiger Gelehrten. So qualifiziert er Simpson, wegen dessen Verhältnis zu de Moivre, als «a most disreputable character» und «unblushing liar» (p. 145, 184) und schätzt Struyck als mehr bedeutenden Vorgänger der modernen Wissenschaft als Süßmilch (p. 347). Simpson's attitude towards De Moivre was described earlier, but the "liar" etc. were new epithets.

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[1] Benennung *Tontine* stammt vom Namen des italienischen Bankier Lorenzo de Tonti, der dieses Versicherungssystem in Frankreich in der Mitte des 17. Jahrhunderts vorschlug. Cf. McKeever (2010).
[2] Bei den Namen einiger erwähnten Gelehrten sind hier Hinweise zu ihnen gewidmeter Sekundärliteratur beigefügt. Die Hinweise selbst zu ihren Abhandlungen werden in Fußnoten zu den Brieftexten gegeben.

Bernoulli, D.
1766 Essai d'une nouvelle analyse de la mortalité causée par la petite vérole, et des avantages de l'inoculation pour la prévenir. – Mém. Paris (1760), p. 1–45 (DBW 2, p. 235–267).
1768 De duratione media matrimoniorum, pro quacunque coniugum aetate, aliisque quaestionibus affinibus. – Novi comm. Petrop. 12 (1766–1767), p. 13–15, 99–126 (DBW 2, p. 288–303).

[2] Bei den Namen einiger erwähnten Gelehrten sind hier Hinweise zu ihnen gewidmeter Sekundärliteratur beigefügt. Die Hinweise selbst zu ihren Abhandlungen werden in Fussnoten zu den Brieftexten gegeben.

1770 Mensura sortis ad fortuitam successionem rerum naturaliter contingentium applicata. – Novi comm. Petrop. 14 (1769: I), p. 8–9, 26–45 (DBW 2, p. 325–338).
1771 Continuatio argumenti de mensura sortis ad fortuitam successionem rerum naturaliter contingentium successionem applicata. – Novi comm. Petrop. 15 (1770), p. 5–8, 3–28 (DBW 2, p. 339–360).
Birg, S. (ed.)
1986 Ursprünge der Demographie in Deutschland: Leben und Werk J.P.Süßmilchs. Frankfurt/Main.
1919 Pehr Wargentin als Statistiker. Helsinki.
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1746 Essai sur les probabilités de la durée de la vie humaine. Paris, 1746 (Repr. 2003).
1760 Addition à l'Essai sur les probabilités de la durée de la vie humaine. Paris, 1760 (Repr. 2003).
Dupré de Saint-Maur, N.F.
1749 Table de mortalité des paroisses de Paris. In: Buffon, G.L. Histoire naturelle générale et particulière, t. 2, Histoire naturelle de l'homme, De la vieillesse et de la mort. Paris, 1749, p. 590–601 [auch mehrere spätere Ausgaben].
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1761 Von der Geschwindigkeit der Vermehrung und der Zeit der Verdoppelung. *In*: Süssmilch J.P. Die göttliche Ordnung in den Veränderungen des menschlichen Geschlechts … 1. Th., 2. Ausg. Berlin (Opera omnia I-7, p. 507–534).
1767 Recherches générales sur la mortalité et la multiplication du genre humain. – Mém. Berlin 16 (1760), 1767, p. 144–164 (Opera omnia I-7, p. 79–100).
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1776a Éclaircissemens sur les établissemens publics, en faveur tant des veuves que des morts, avec la description d'une nouvelle espèce de tontine aussi favorable au public qu'utile à l'état, calculés sous la direction de Léonard Euler. SPb. Deutsch übers. Altenburg, 1782.
1776b Entwurf einer allgemeinen Leihe-Bank, wo nicht nur Kapitalien zu gewissen Zinsen sowohl ausgelehnt als angenommen, sondern auch zugleich andere verschiedne Anstalten als Leibrenten, Sterbe- und Witwen-Kassen damit verbunden werden können. SPb., 1776.
van Haaften, M.
1925 Bibliographie van Nicolaas Struyck. – Verzekerings-Archief 6, p. 65–86.
Halley, E.
1694 An estimate of the degrees of the mortality of mankind, drawn from curious tables of the births and funerals at the city of Breslaw with an attempt to ascertain the price of annuities upon lives. – Phil. Trans. Roy. Soc. 17, No. 196 (Jan. 1693), p. 596-610, 654-656. Reprint: Baltimore, 1942.
Heuschling, X.

1857 Notice sur la vie et les ouvrages de Kersseboom, statisticien hollandais du XVIII^{me} siècle. – Bulletin de la Commission centrale de statistique (Bruxelles) 7, p. 397–413.
Hodgson, J.
1747 The valuation of annuities upon lives deduced from the London bills of mortality. London, 1747.
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V

Oscar Sheynin

Science Is left Somewhere Behind

Reviews. Scientific information is indispensable. However, according to Mikhailov (1975), only 6% of the necessary published materials become known to a specialist unaided by abstracting journals, but about 80% otherwise, i. e., when reading reviews. His estimates were certainly approximate, reflected sciences in general, and were largely dependent on the Soviet *Referativny Zurnal* (Abstracting Journal). In 1975, as he added, it contained 25 general volumes (the highly respected *Matematika* was one of them) composed of 140 parts and 44 partial booklets. The amount of information is steadily increasing but it is difficult to say whether the gap between those two figures is widening or narrowing.

Not many mathematicians beyond Russia read Russian, still less have access to that journal. The two other mathematical abstracting journals are *Mathematical Reviews* and *Zentralblatt MATH* (not on paper anymore), but they are now too expensive and therefore difficult to come by. Regrettably, they do not additionally provide (at a more reasonable cost) bibliographic information about the same materials but without the reviews, and at least the latter does not offer subscriptions to its separate sections.

Now I turn to reviewing in general, to the extremely important work that ought to be, but is not recognized as such by the scientific community.

Even reliable publishers are sometimes putting out unsatisfactory books, see my reviews Sheynin (2006), and one of the reasons apparently is that the internal reviewing of manuscripts was either irresponsible or based on insufficient information. The latter circumstance can in turn be occasioned by poor published reviews of related literature. Later, the published book is reviewed in periodicals and/or journals of abstracts. These reviews are again possibly unsatisfactory for the same reasons and also, as I suspect, because reviewing is not appreciated.

There are at least two underlying causes of this situation. As mentioned above, the scientific community does not set high store on reviewing. This cause is aggravated by bad reviewing: why should it be valued? A vicious circle! Then, publishers are sending free copies of their new books to appropriate journals for reviewing, so that the editors are tempted to publish positive reviews.

There also exist special points. First, according to the existing practice, collected articles and books covering a wide range are nevertheless reviewed by a single reviewer. Second, authors who had not yet seen a contribution they ought to mention, customarily praise it instead of honestly admitting their failure; in this connection I refer to Slutsky (1912, p. 130n). Third, some periodicals, for example, the *Journal of the Royal Statistical Society*, refuse to admit critical papers or comments on their published papers. Infallible statistics! Fourth, in scarcely populated branches of mathematics (e. g., in its history) everyone knows almost everyone else, and, as a group, they tend to scratch each other's back by delivering sweet nothings rather than state their actual opinion. Fifth, some reviewers apparently see a man with a small pimple as a great pimple and the man somewhere behind. In other words, they pay no attention to the findings, they are happy to find a pimple and to magnify it many times over.

I single out my sixth and last point concerning the important essays on, or reviews of the work of our great forerunners are usually written by their compatriots who often prettify their heroes. This is true with regard to Condorcet and Cournot, and, yes, to Laplace. Contrary to Newton, the last-mentioned (1796/1884, p. 504) somehow managed to attribute the ellipticity of the planetary orbits to random causes. The same attitude prevails in connection with essays on Chebyshev and his eminent students published by Russian authors. Then, previous results are usually provided in our modern way. So far, so good, but readers will sometimes be unable to find the exact place where these results were initially contained, or to understand how our contemporary author transformed them into their modern form, see for example Sprott (1978).

This, now, is an episode which shows that the trouble with reviewing is not new at all. The late Professor Truesdell, a great scientist, collected his reviews, notably on the works of Euler and the Bernoullis, published over three decades. Here is his account (1984, p. 397) of bygone times:

We have seen the Royal Society twice in thirty years [in the mid-19th century] *with maximally pompous humbuggery and humbugging pomposity stifle the truth in favour of the wrong, twice bury a great man in contempt while exalting tame, bustling boobies whose whole lives add nothing to the science passed on into our day*. […] *The Society, or at least its officers, regarded the Society itself committed to support any paper it published.*

Recall my point three! The *great* men were Herapath and Waterston. Truesdell adds that similar happenings had been occurring in many European academies. Elsewhere, he (p. 292) states that

By definition, now, there is no learning because truth is dismissed as an old-fashioned superstition.

This is an exaggeration, but anyway the sacred duty of a reviewer all the more consists in describing literature truthfully and knowingly, showing a spade as a spade.

Splendid examples of reviewing are known; Truesdell provided many. Then, in 1915, the Russian Imperial Academy of Sciences awarded its gold medal to Chuprov for reviewing a book on its behalf (Sheynin 2011, p. 50). Von Bortkiewicz, nicknamed the Pope of statistics, was a most competent reviewer (Woytinsky 1961, pp. 452 – 453). Then, I remember the Soviet periodical *Novye Knigi za Rubezom* (New Books Abroad) or at least its series devoted to mathematics and physics. It consisted of detailed reviews written by eminent scientists and I doubt that there elsewhere was, or is anything comparable.

I have no magic wand to wave, but I think that periodicals should regularly publish reviews and inform readers about valuable reviews published elsewhere, should thus show its respect to reviewing. The scientific community ought to be awakened. Indeed, in a general setting (although only dealing with meteorology), Shaw et al (1926/1942, p. v) stated that

For the community as a whole, there is nothing as extravagantly expensive as ignorance.

2. Publishing Papers. Many periodicals **conceal their e-mail addresses.** In one case a friend found for me the address of the editors of a series of journals published by a certain publisher, *my* periodical included. I have thus penetrated its first line of defence.

I wrote to one of those editors, and just in case sent her my manuscript and she advised me to click on an address stated in her *platform*; to mention an address *a few lines below* was apparently not scientific enough. I vainly attempted to connect to that address independently, returned to the *platform*, found the periodical needed (the second line of defence penetrated), but was unable to authorize.

I do not describe the following troubles. There were other lines of defence and again that editor helped me. Only I do not understand why she had not sent my manuscript to the periodical at once.

A cannibal from an African jungle will find it easier to settle in Europe.

Another point: **the language**. I sent a MS to *Isis*, and certainly mentioned that it was published in Russian. Oh, no! They never publish such manuscripts. Suppose that they have a thousand regular readers. Only 1 or 2 of them will encounter somewhere a reference to my Russian paper, manage to get hold of it and read it. Rigid regulations are more important than common sense.

A third manuscript, a third periodical: They refused to publish it for three reasons. Yes, the Russian language was one of them. Then, the MS was **too short** (about twice shorter than their favourite standard). For the same reason Einstein is known to have added a few meaningless lines to his dissertation, but I am not Einstein, and in any case I was unable to add about 15 pages. And the last reason: my text was **not suited for their readers.** This is serious. There are too many periodicals having a small number of readers, so manuscripts have to concentrate on a small issue. Many years ago that same *Isis* rejected one of my MSS; the Editor informed my in black and white that its scope was too wide …

The format. About 20 years ago, I submitted my translation of a classical paper by Liapunov to a periodical. It demanded a text in their format and refused to reject/admit it in principle as it stands. I did not agree since in case of refusal my rewriting would have been meaningless. Rigid and thus stupid regulations led to their losing an important item.

Another example. A Russian periodical published an essay on the life and work of a scientist of the 19th century complete with a list of his publications, more than 200 of them. And his name was printed more than 200 times …

All the cases above testify that science is to a certain extent a victim of unnecessary regulations, and I recall that Bismarck was quoted as saying that Germany was ruled by bureaucrats.

Spelling of proper names does not hinder publication but constitutes a special example of stupid regulations. There was a Russian mathematician Bernstein, a foreign member of the Paris Academy of Sciences. He published many notes in their *Comptes rendus*, always spelling his name as stated above. Now, someone, at some time, because of a mysterious reason, decided that such names should be written otherwise, so Bernstein became Bernshtein. Ugly and at the very least contradicting his choice of a pen name. I am unable to understand why apparently each editor complies with such decisions. I myself have transliterated my Russian name somewhat wrongly, but *Sheynin* became my pen name, and I do not want to be called *Sheinin*.

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VI

A. A. Chuprov: Obituaries

J. M. Keynes, Professor A. A. Tshuprow.

Econ. J., vol. 36, 1926, pp. 517 – 518.
Reprinted: *Coll. Works*, vol. 10. Cambridge, 1972, pp. 121 – 122.

We much regret to announce the death of Professor A. A. Tschuprov on April 19 last in Geneva in his fifty-third year. He commenced his studies in the University of Moscow, and in 1902, at the opening of the great Polytechnical Institute of Petrograd [Petersburg], he became for some years Lecturer in Economíc (?) Statistics at that institution. But both early and late in life he was much connected with German Universities and many of his most important papers are published in German. He studied economics and statistics in Berlin and Strassbourg and he prepared his first important work, entitled *Die Feldgemeinschaft*, as a pupil of Knapp[1]. Since the Russian Revolution, after a stay in Scandinavia, Tschuprow had lived mainly in Dresden. It was his nature always to avoid the ties of a professorial chair and to keep his mind entirely free for original work. And the professorship at Prague which he was driven by financial circumstances to accept near the end of his life proved uncongenial[2].

Regardless of poverty and the material difficulties of the post-was period, whether in Russia[3] or in Germany, he always placed a very high price on complete intellectual independence. The result was that some of his most important papers on theoretical statistics belong to his years at Dresden. Earlier papers in *Biometrika* were followed up by a series in the *Nordisk Statistiks Tidskrift*. His latest work, *Grundbegriffe und Grundprobleme der Korrelationstheorie* was published by Teubner last year.

Passing from economics, mathematics (?) and practical statistics to theoretical statistics, Tschuprow became one of the most important writers on the boundary line between statistical theory and the theory of probability. He provided a link in some respects between the work of English statisticians and that of the German and Russian schools. His early death is a severe loss to the subject.

Notes

1. Chuprov's first important work is dated 1897, see Tschetverikov.

2. Prague: see Sheynin (2011, p. 44). Chuprov never got paid since he only was a honorary member of a local institution. Below, I do not repeat suchlike mistakes made by other authors if noted already.

3. In this context, Russia should not have been mentioned at all.

L. I. Isserlis, Alexander Alexandrovich Tschuprov, Formerly Professor of Statistics, Petrograd [Petersburg]

J. Roy. Stat. Soc., vol. 89, 1926, pp. 619 – 622

Professor A. A. Tschuprow, who died at Geneva on April 19 last at the early age of 52, was a brilliant worker in the field of mathematical statistics. His death, like those of Liapounoff and Markoff, was accelerated by the hardships in which the Russian Revolution involved many of Russia's most prominent scientists, He was born in 1874. His father, A. I. Tschuprow, was an economist of note, Professor of Political Economy at Moscow and editor of a liberal newspaper, *Russkiye Vedomosti*[1]. His grandfather was a village priest. As was the case with many Russian middle-class families fifty years ago, Tschuprow's early education was conducted at home by members of his family and visiting teachers. As a child, he showed great aptitude for arithmetic, but his progress at Greek and Latin was still more striking. His school days at the gymnasium, which he entered at the age of fourteen, were not happy. The low moral tone of his schoolfellows and the reactionary tendencies of his teachers were a shock to the sensitiveness brought up in the family circle of a Liberal publicist.

When in 1892 he entered the mathematical faculty at the University of Moscow, his mind was made up that he was to learn mathematics as an instrument for the study of social phenomena. Jevon's *Principles of Science* accompanied him, to be constantly read and re-read. Tschuprow's thesis for his first degree, the logical foundations of the theory of probability[2], was significant of the direction his future work was to take.

After graduating at Moscow in 1896 he studied in Germany, first in Berlin and Göttingen, where he met great encouragement from Bortkevich, who introduced him to Lexis. The latter's influence on the young Russian statistician was considerable. Tschuprow's researches on the stability of statistical series and the associated problem of sampling owed their initiative to him. The years from 1897 to 1901 were spent at Strasburg University where his teachers were Bortkewitsch, Sartorius[3] and Knapp. Knapp was apparently a strict teacher with severe views on the need of exact reasoning and exhaustive analysis as well as clear exposition. To his training Tschuprow owed much and his first important investigation, *Die Feldgemeinschaft. Eine morphologische Untersuchung* was carried out under Knapp's supervision.

Tschuprow returned to Russia, graduated in Law[4], and in autumn of 1902 commenced his teaching career as docent at the Polytechnic Institute at St. Petersburg. That Institute was a creation of M. Witte. Those were its early days, but some of the best Russian minds were already on its staff. Tschuprow was one of the most active in organizing the Department of Economics. It was his duty to lecture on general statistical theory to first-year men. He found this a difficult and exhausting task. In later years he never got over his feeling of stage fright when lecturing to large classes of elementary students, although he had less difficulty with senior classes.

Much of his time was devoted to the organization of a statistical bureau and the collection of an exceptionally fine library of great scientific value. But most of all he was occupied in conversations with his students and in guiding their statistical research work. When the pressure of the work of organization at the Polytechnic relaxed, Tschuprow was able to complete his *Outlines of Statistical Theory*, a dissertation for which he was awarded the doctor's degree at Moscow. It is impossible to over-emphasize the influence of this work on the development of statistical method in Russia: the clearness of the exposition brought the subject within the compass of every intelligent statistician, even if lacking any special training in mathematics and logic, while its breath of outlook and its incisive analysis gave the reader a feeling of confidence in his mastery of the subject. Russian statistical investigations are characterized by the importance attached to mathematical methods, more so perhaps than elsewhere, and Tschuprow's *Outlines* have gone far to provide the necessary foundations[5].

Tschuprow's scientific work gained much from his constant discussions, orally, during vacations, and by correspondence at other times, with his father, the economist. He early formed regular habits, to which he adhered strictly throughout his life. These were necessitated by a feeble constitution which would have proved unequal to heavy and continuous mental efforts without them. He was a lover of poetry and art, particularly the works of the Italian School. Tschuprow was passionately fond of music. As a young man, he earnt to play the piano, but gave it up when convinced that it would interfere with his scientific work.

1909 marked the commencement of a new period in his work[6]. His interest in economics and the associated statistical problems gave place to an intensive study of statistical methodology and of the statistical problems that were arising from the applications of statistics to biology and physics. He began to read the work of the English biometricians. The first researches belonging to this period were embodied in his paper on the Law of Large Numbers read before the St. Petersburg Academy of Sciences in 1912 on the occasion of the bicentenary of the law, and in the report on the Sex-Ratio problem made to the International Statistical Institute in 1913. It is of interest that this last paper contains almost no mathematics although it is based on a hypothesis which was itself an elaborate piece of mathematical analysis[7]. This was followed in 1916 by his first study of the method of Mathematical Expectation, a paper entitled *Mathematical Expectation of the Coefficient of Dispersion* also read before the Academy of Sciences.

In May, 1917, he, as usual, went abroad for the long vacation. He never returned to Russia, preferring to wait abroad for the issue of the revolution. His home was broken up, but his letters and manuscripts were saved, as well as his library, which was transferred to the Statistical Bureau.

Tschuprow's first years of exile were spent in Scandinavia. When the opportunity came of entering Germany he went first to Berlin. But in Berlin he found too many friends and lacked the peace and quietness he needed for his work. So he moved to Dresden, where he lived the life of a hermit seeing only occasionally the few who, passing through Germany, visited his modest retreat. On the whole, and in spite of financial embarrassment, he succeeded in creating an atmosphere suitable for his work, and devoted himself to what had become his life-work, the building up of a sound logical foundations for theoretical statistics, applied to the theory of dispersion as developed by Lexis and to the theory of correlation as developed by

Pearson and the English School. Much of the work of this period is still unpublished.

Gradually his material position worsened. His small savings melted away, and with many misgivings he accepted a chair in Prague. Shortly before this, in the words of the Russian correspondent to whom the present writer is indebted for most of the material in this note[8],

Fortune once more smiled on Tschuprow. His election as an Honorary Fellow of the Royal Statistical Society was one of the really joyful occasions in his life.

The move to Prague proved to be a serious mistake. He was unable to arrange for himself that quiet, regular mode of life which h ad become essential for productive research work. The town was noisy, he was unable to sleep. He did not get on well with the people he met. Relations with many were strained, and his little stock of nervous energy was rapidly dissipated. His heart had been affected by rheumatism in childhood and the soil was thus prepared for the endocarditis, which first showed itself at the Rome meeting of the International Statistical Institute in 1925. In November last Tschuprow made his last move – this time to stay with old friends in Geneva[9]. His condition grew gradually worse and he died on April 19 last.

Notes

1. In that newspaper, from 1902 to 1916, Chuprov published 64 articles. His main subjects were system of landownership in Russia; parliamentary life in Russia; statistics and demography; and economic situation in Germany and Austro-Hungary during WWI.

2. Chuprov's thesis was entitled *Mathematical Foundation of the Theory of Statistics.*

3. I did not find any relevant mention of Sartorius von Walterhausen.

4. Chuprov graduated at the Law faculty of Moscow University.

5. This is a mistake. My own opinion about that book is extremely negative, see the piece about Chuprov in Sheynin (2018).

6. A new period in Chuprov's scientific life began in 1910 because of his correspondence with Markov. He started to feel himself as a mathematician.

7. Some account of this paper is given incidentally by E. M. N. in her review of a work on Sex-Ratio in the March issue of the *Journal*. L. I.

8. Isserlis did not name that correspondent. I refer to this fact in my general commentary.

9. Chuprov came to his intimate friend, C. N. Gulkevich.

N. S. Tschetwerikoff, Al. A. Tschouproff, 1874 – 1926

Metron, No. 3 – 4, 1926, pp. 315 – 320

Le 19 Avril 1926 mourut à Genève le professeur Al. A. Tschouproff. C'était un investigateur des plus renommés de la statistique théorétique, qui a grandement contribué au developpement de cette science. Doué d'un esprit apte à une critique et une synthèse des plus profondes, il avait projeté le plan admirablement elaboré et complet d'un édifice théorique où les différentes écoles, anglaise, allemande et russe, se trouvent unies entre ells par les liens organiques.

Tschouproff naquit le 18 Février 1874 dans la famille d'un des plus célèbres professeurs et politiciens: A. I. Tschouproff. Son père, un homme d’une haute culture et un savant des plus éminents, a été le créateur de la statistique provinciale (des zemstvos). Il avait sur ses enfants une influence très grande, et les liens qui l’unissaient aavec son fils se sont encore resserrés pendant les années des études d’ Al. A. T. à l’Université quand au prestige personnel d’ A. I. T. vinrent se joindre les intérêts scientifiques communs. Et c’est justement à l’influence paternelle que doivent être attribués l’amour de la vie réelle et le penchant délicat et pénétrant vers le fait concret, qui ont été légués par l’école d’Al. A. T.

Tschouproff reçut son instruction primaire dans sa maison paternelle où il fit ses études avec ses soeurs et plusieurs camarades. Son maître pour les langues mortes (pour lesquelles T. était tout particulièrement doué) était N. V. Spéransky qui a exercé sur son élève et ami une influence inéffaçable, que ne peut être comparée qu’à influence du père et de soeur aînée. Spéransky a transmis à T. le fin et la précision des idées et de leur exposition qui ont à tel point marqué tous les travaux scientifiques.

T. entra au gymnase assez tard, à l’âge de 14 ans, et y étudia pendant quatre ans. L’école réactionnaire de l’époque avait un choix fort defectueux de professeurs et un système routinier d’études et ne pouvait rien offrir à T. qui était developpé bien au dessus de son âge. Au gymnase T. commença à s’occuper de logique, étudia les oeuvres de J. S. Mill et de Jevons et fit des reflexions approfondies sur la possibilité d’adapter la mathématique à l’investigation des phenomènes sociaux. Il avait déjà alors envisagé la signification de la théorie des probabilités comme base des méthodes de la statistique, et il entra à la faculté des mathématiques de l’Université de Moscou ayant devant lui un plan d’occupations complètement déterminé. La thèse choisie par T. pour obtenir le grade de bachelier *Les bases mathématiques de la statistique* et presentée au professeur P. A. Nekrassoff qui était alors chargé du cours de la théorie des probabilités[1], était un travail volumineux, faisant preuve d’une vaste érudition du jeune savant. Cependant nous n’avons rien à dire sur une influence quelconque de Nekrassoff: le professeur s’intéressait aux calculs faisant partie du travail, tandis que T. faisait ressortir à la première place la logique ainsi que les bases précises de l’adaptation de la théorie des probabilités à la méthodologie statistique.

Ayant terminé ses études à l’Université en 1896, T. partit de suite en Allemagne, d’abord à Berlin et pour le semestre d’été de l’anée suivante il se rendit à Strasbourg. A l’étranger T. se voua aux études d’économie politique sans pourtant discontinuer de s’occuper de logique et de statistique. A Berlin il fit connaissance de L. von Bortkiewicz avec lequel il a depuis lors toujours entretenu les meilleures rélations amicales. De Berlin T. alla à Göttingen visiter Bortkiewicz qui soumit à une analyse détaillée les idées exprimées par T. dans son travail présenté à l’Université qui l’avait particulièrement interessé. C’est là aussi qu’eut lien l’entrevue de T. avec W. Lexis qui a exercé une influence très marquée sur les travaux scientifiques du statisticien russe.

En 1897 fut rédigé et publié le premier travail scientifique de T.: *La statistique morale*, un article inseré dans le *Dictionnaire*

Encyclopédique de Brokhaus et Efron. Dans cet article T. a posé distinctement le problème de l'élaboration de méthodes mathématiques suffisamment souples et approfondis pour permettre d'apprécier correctement les phénomènes de la vie sociale.

A Strassbourg T. menait une vie solitaire vouée à l'étude de la science (il s'occupait à sa thèse intitulée *Die Feldgemeinschaft* en vue d'obtenir le grade de docteur) et interrompue par de courtes excursions dans les montagnes du Schwarzwald avec plusieurs amis, ou dans l'Italie septentrionale pour y voir ses parents et avant tous son père bienaimé qui ne cessait d'être son soutien dans sa vie et dans son travail. En Italie T. aimait surtout à frequenter les petites villes écartées pour y faire des recherches des monuments de l'art italien dont il était un connoisseur des plus achevés.

A Strassbourg T. travaillait au séminaire de Bortkiewicz, mais son attention principale était dirigée vers sa these *Die Feldgemeinschaft* ainsi que vers les séminaires de Knapp. Et c'est en Knapp que nous devons reconnaître le véritable maître de T.: il éprouvait une vive affection pour son élève russe malgré la timidité de celui-ci et son caractère replié sur lui même. La thèse doctorale subit une révision laborieuse par T. conjointement avec Knapp et c'est peut-être à ce dernier plus qu'à tous les autres que T. était redevable du succés de son travail postérieur *Précis de théorie de la statistique*. Le style et la composition de ce volume étaient admirablement réussis et le rendaient intellegible à tout statisticien malgré toute la complexité du contenu et les difficultés qui étaient inhérentes au problème dont il s'agissait.

Après avoir subi ses examens pour obtenir le grade de docteur à Strassbourg (en 1901) et publié sa dissertation, T. alla subir ses examens au grade de licencié (*magistre*) en Russie à la Faculté de droit de l'Université de Moscou, et eu automne de 1902 il s'installa à Petersbourg, ou il fut invité comme professeur adjoint de statistique à l'Institut Polytechnique qui venait d'être inaugure. L'Institut Polytechnique de Petersbourg était dans ce temps là la première école supérieure de type nouveau qui devait posséder, outre les sections techniques, aussi une section économique basée sur un plan dúne vaste étendue. L'organisation de cette section fut confiée à un groupe de professeurs partisans des idées du libéralisme démocratique et jouissant d'une haute renommée scientifique.

Cependant T. put, grâce à son enérgie juvénile et son esprit clair et éveille, jouer une rôle très important dans l'organisation de la section économique. Il ne serait pas facile de ce rendre compte de toute la quantité de travail parfois très dur, qu'Al. A. T. a dû consacrer à l'organisation du système des études qui devait contenir, excepté les lectures, aussi des occupations pratiques d'un caractère très serieux avec les étudiants. Il fallait aussi créer un Cabinet de Statistique, pourvu d'une bibliothèque spéciale ayant une valeur toute exceptionelle; il fallait en même temps réussir à participer aux diverses commissions et aux sessions de la Faculté en prenant part à toutes les besognes qu'exigeaient l'élection, l'invitation et la nomination des nouveaux professeurs.

Les relations personnelles avec les étudiants, auxquels T. consacrait souvent ses soirées, lui procurèrent les joies qui recompensent le travail de professeur. Il était témoin des progrès rapides de son travail de pédagogue réfléchi et assidu et se sentit entrainé par cette oeuvre qui était nouvelle. Quelque temps plus tard, quand il fut possible de confier une partie du travail aux professeurs qui furent invités, T. put consacrer son énergie à la réalisation d'un plan d'études élargi: il ouvrit un cours spécial pour les étudiants des derniers semestres et organisa des occupations aux séminaires, où son *école* a pu se développer et où les étudiants qui étaient suffisamment avancés purent s'habituer aux travaux d'investigation scientifique qu'ils exécutaient sous la direction de leur précepteur, toujours plein de sollicitude et d'attention envers eux.

En hiver de 1909 T. présenta à l'Université de Moscou sa thèse pour la licence: *Précis de théorie de la statistique*. Ce travail fut si hautement apprécié par l'Université qu'elle décerna à l'auteur le plus haut grade scientifique existant en Russie, celui de docteur.

Le *Précis* ent un succés retentissant et dut être reimprimé en seconde édition dans un délai de moins d'une année. Cet ouvrage contient les chapitres se rapportant à la théorie de la connaissance (le rôle de la statistique dans le système des sciences de Rickert les problèmes de logique; la notion du hasard, la critique des méthodes de l'induction et son rapport aux méthodes de la statistique) et de mathématiique (les principes de la théorie des probabilités, la théorie de la dispersion de Lexis et de Bortkiewicz). Le livre ne se bornait pas à donner une revue des nombreux travaux traitant les problèmes des domaines de la logique et de la statistique, mais maintes questions y furent soumises à une analyse indépendante et très approfondie. Le grand intérêt qu'ont éprouvé les statisticiens russes des Zemstvos (provinces) et des Universités par rapport aux questions théoriques de la statistique doit en une grande partie son origine au *Précis* de T.

Pendant ce temps T. étudiait laborieusement les investigations faites par les statisticiens anglais, Edgeworth et Pearson. D'un autre côté l'examen des travaux des mathématiciens russes, Tchébycheff et Markoff, qui suivaient la tradition léguée par le statisticien français Bienaymé, amena T. à la tendance de donner une base strictement logique et mathématique à la conception de l'école de Pearson. Simultanément T. continuait à étudier la question de la stabilité des séries statistiques en se basant sur les travaux des auteurs français, allemands et italiens.

En 1913 T. prononça à la session solennelle de l'Académie des Sciences tenue à l'occasion du deuxième centenaire de la loi des grands nombres un discours, où la statistique était envisagée comme base de conception scientifique dans le cycle des études sociales et dans le domaine des sciences naturelles.

En 1916 T. publia son premier travail mathématique traitant la question de l'espérance mathématique du coefficient de la dispersion , où la méthode de l'espérance mathématique fut adoptée d'une manière qu'on pourrait à juste titre nommer splendide. A cette période de la vie de T. se rapporte la correspondance animée de T. avec l'académicien An. A. Markoff qui ne tarda pas à se rendre compte de la manière approfondie et strictement logique de T. de traiter les problémes de la statistique et de la théorie des probabilités. T. s'effforçait de sa part à intéresser Markoff aux investigations de Pearson, pour lesquelles ce savant éprouvait un scepticisme manifeste.

A cette époque T. avait déjà élaboré les problèmes fondamentaux de la statistique théorique dans leurs traits essentiels (du domaine de la théorie de la dispersion et de la méthode des moments), mais leur publication fut differée à plus tard.

En mai 1917 T. se rendit selon son habitude à l'étranger afin de profiter des vacances d'été pour des travaux assidus dans les bibliothèques de l'étranger. Cependant il ne lui fut point destiné de revenir en Russie vu que les circonstances y devinrent trop défavorables au travail pédagogique et scientifique.

A ce temps T. était déjà membre correspondant de la Société Royale Economique de Londre[2], membre correspondant de l'Académie Impériale des Sciences en Russie et membre de l'Institut International de Statistique, aux Congrès duquel il prit une part active (en 1913 il fit un rapport, qui se distinguait par sa haute valeur scientifique, *Sur la question de la baisse de l'excédant des garçons*).

Après avoir passé trois ans à Stockholm et à Oslo T. se rendit en Allemagne pour y mener une vie paisible et solitaire à Dresde en se vouant exclusivement à l'activité scientifique. Il dut cependant de temps en temps interrompre cette existence recuellie tantôt pour entreprendre quelque autre travail qui lui procurait les moyens de vivre, tantôt à cause de l'arrivée de quelques uns de ses élèves ou de ses collègues, tantôt pendant ses departs de Dresde pour faire une conférence, ou un cours épisodique de lectures. Pendant son séjour à l'étranger T. s'occupa à résoudre le problème important qui était surgi devant lui encore à Petersbourg: il effectua une synthèse des écoles allemande et anglaise dans des articles publiés l'*Actuarietidskrift*, la *Biometrika* et le *Metron*. Le grand chois des méthodes des statisticiens anglais exigeait une base plus solide sous le point de vue autant de la logique que des mathématiques. Ayant entrepris l'élaboration de la théorie de la dispersion, T. se persuada de la nécessité d'analyser la méthode des moments et de la méthode de la corrélation sous le point de vue de la réalisation de la condition de l'invariabilité de la loi de distribution de la variable accidentelle et de la condition de l'indépendance des éprouves. Ce fut aussi une indication de la possibilité d'adapter les méthodes de la statistique aux différentes conditions de travail de l'invesigateur-praticien.

T. ne se borna pas à étudier les problèmes de la statistique théorique. Il prenait aussi soin à ce que les résultats de ces travaux fussent propagés parmi les statistiens. En 1923 il fit une conférence à Leipzig à la Société des mathématiciens d'assurance et son voyage en 1924 au Danemark et en Norvège provoqua un accueil enthousiaste du lecteur et de ses lectures de la part des staticiens scandinaves. Ce voyage fut, peut-être, un de plus heureux moments dans la vie de T. qui fut témoin des liens reserrés existant entre son travail de savant et le progrès général des idées scientifiques et de la théorie de la statistique.

Les conclusions tirées de ces conférences et les lectures furent exposées sous une forme compréhensible à la masse des lecteurs dans une série d'articles imprimés dans le *Nordisk Statistisk Tidskrift* et le *Vestnik Statistiki* (Messager de Statistique), ainsi que dans un ouvrage plus détaillé sur la théorie de la corrélation (*Principes et ptoblèmes essentiels de la théorie de la corrélation*, éditions allemande et russe).

En étudiant la méthode de l'espérance mathématique T. s'occupait laborieusement du problème des grands nombres. Son article paru dans le *Metro*n (t. 1, No. 4), sert, pour ainsi dire, de préface à l'étude publiée dans le *Journal of the Royal Statistical Society*, vol. 88 (On the asymptotic frequency distribution of the arithmetic means etc.). Ce dernier article était une digne réponse à l'élection de T. au grade de membre honoraire de la Royal Statistical Society.

Au commencement de l'anée 1925 T. accepta l'invitation de s'installer à Prague en partie à cause de la nécessité de se procurer un gain plus sûr par son travail, en partie à la suite de son désir de reprendre ses occupations de professeur. Cependant les conditions de son éxista à Prague ne lui furent point favorables et ruinèrent la santé de T. qui avait toujours été fragile.

Avant encore de se rendre à Rome á la Session de l'Institut International de Statistique, T. subit l'accès d'une maladie cardiaire et fut obligé après la clôture de la Session de se loger dans une des cliniques de Rome. Les explorations des médecins n'ont longtemps pas pu déterminer la nature de la maladie. Dans le but de créer l'entourage le plus convenable au malade, qui exigeait avant tout du repos, les médecins consentirent à son depart pour Genève, ou il trouva chez son ami intime C. N. Goulkévitch des soins tendres et empressés. Cependant, malgré tous les efforts, le progrès de la maladie n'a pu être arrêté et après neuf mois Al. A. Tschouproff succomba au mal. L'histoire ne nous donnera que peu d'exemples d'une vie intègre, aussi régulièrement utilisée et entièrement consacrée au culte de la science.

Notes

1. Nekrassoff only read a cours in elementary theory of probability (Wikipedia).

2. The Royal Economic Society had no corresponding members. Chuprov was its correspondent in Russia but his only contribution to the RES was his own paper (1912) accepted by Keynes.

P. Georgievski, Tchouproff Alexandre, 1874 – 1926

Bull. Stat. de la Rép. Tchécoslovaque, No. 4 – 6, 1927
Bull. Intern. Stat. Inst., No. 1, t. 23, 1928, pp. 345 – 349

Monsieur A. A. Tchouproff est né le 5/17 février 1874 à Moscou. En 1892 il entra la section de mathématiques de la Faculté des sciences physico-mathématiques de Moscou. Après avoir achevé ses études, en 1896, il partit à l'étranger pour perfectionner son éducation en suivant les cours des Facultés de Berlin et de Strasbourg. En 1901 il obtint à Strasbourg le grade de Docteur en sciences économiques et politiques, au printemps de 1902 il retourna en Russie pour passer sa licence à la Faculté de droit de Moscou. Depuis l'autumne de 1902 il enseigna la statistique à la section économique de l'Institut Polytechnique de Petersbourg.

En 1908, après une dissertation sur les *Principes de la théorie statistique* il fut promu à Moscou docteur en économie politique et en statistique, et fut nommé professeur de statistique à l'école [Institut] polytechnique de Petersbourg.

En 1917 il quitta la Russie, il passa plusieurs années en Suède et en Allemagne. En 1925 il s'établit à Prague où il fut nommé professeur à la Faculté russe de droit. En automne de la même année il se rendit à Rome où il prit part à la XVIe session de l'Institut international de statistique. C'est là qu'il fut terrassé par la maladie dont il mourut à Genève le 19 avril 1926 après plusieurs mois de souffrances.

La valeur scientifique des travaux du T. est universellement reconnue et appréciée. Jusqu'à maintenant, comme on le sait, les statisticiens éminents ont des divergences d'opinions sur nombre de problèmes concernant la statistique, en commençant par celuiu de savoir si la statistique est une science ou seulement une méthode. Par différence avec les anciens auteurs, Jahnson, Mayr, Lexis et autres, la nouvelle école des statisticiens et, parmi ceux-ci le prof. T., considèrent la statistique comme une simple méthode, mais les caractéristiques et l'importance de cette méthode méritent, d'après T., une attention spéciale[1].

The future historian of human thought, when examining our contemporary epoch of the end of the nineteenth and beginning of the twentieth century, will observe how its characteristic feature, the striving for scientific knowledge, takes on a statistical form. Those fields grow from year to year where human thought, turning away from single occurrences, concentrates on their combined result, on total or mean values. We can say without exaggeration that the development of contemporary science is in the direction of interest in collective phenomena and that soon there will not be a branch of knowledge where, with more or less success, the statistical form of knowledge has not spread its influence.

A l'époque où les auteurs éminents, par exemple Lexis, soutiennent l'opinion que les conclusions statistiques ne sont qu'une autre forme de l'induction ordinaire, le prof. T. renouvelle l'opinion oubliée de Rümelin[2], qui avant un demi-siècle formula que

L'observation des phénomènes collectifs ne peut pas être considérée simplement comme un moyen auxiliaire de l'induction, mais qu'elle doit être considérée comme lui étant egale, équivalente et parallèle.

Le prof. T. développe largement et motive ce point de vue sur l'importance et la place de la méthode statistique. Dans son oeuvre principale *Principes de théorie statistique*, il essaya de faire la synthèse des bases générales logiques de la théorie statistique. Les travaux de l'école Pearson des statisticiens-mathématiciens en Angleterre, les résultats de l'école Lexis des statistiens-sociologues en Allemagne, et, enfin, la nouvelle école statistique découlant de Windelband et Rickert, ces trois écoles scientifiques sont unies, dans leur contenu logique, dans les *Principes* de T. qui constitue une introduction logique excellente à la théorie de la statistique.

Parmi les points contestés de la statistique un y range également le problème du rôle et de importance qu'il faut attribuer à l'emploi de hautes mathématiques dans l'analyse statistique.

L'éducation mathématique spéciale que le prof. T. a reçue, lui a permis de se servir abondamment den formes mathématiques dans ses investigations, mais il n'est jamais allé jusqu'aux extrêmes comme certains autres savants, qui déclarèrent que la statistique est une science essentiellement mathématique.

Après la publication de ces *Principes*, il s'est livré à l'élaboration des bases mathématiques de la théorie statistique et dans ces 15 dernières années il consacra aux questions portant sur ce sujet, toute une série de monographies publiées dans différents périodiques scientifiques, qui ont été très appréciées par tous les spécialistes. Ces travaux sur le bases mathématiques de la théorie de statistique ont le même caractère synthétique. Dans cet ordre d'idées il s'est imprégné aussi bien des mouvements scientifiques représentés par l'école anglaise de Pearson et de celle de Lexis en Allemagne, que des idées de Tchebyschev et de Markov[3].

Comme il est connu, il n'a pas d'unanimité das la conception de la notion de la probabilité, les savants se divisant en partisans du *principe de la raison manquante*, et en partisans du *principe de la raison cogente*. Le Prof. T. ainsi que la plupart des savants contemporains adaptent la seconde manière de voir, c'est-à-dire, le principe de la probabilité objective par différence des partisans du principe de la probabilité subjective[4].

Le Prof. T. dans ses recherches sur les bases statistiques les envisage au point de vue *stochastique*. El explique lui-même ce qui signifie ce point de vue de la façon suivante:

Quel que soient les chiffres statistiques obtenus par l'observation, il faut les envisager comme étant le reflet des grandeurs à priori, qui sont à leur base, mais plus ou moins déformées par le hazard.

L'une de ces grandeurs à priori qui leur sert de base est la probabilité même dont le reflet est la fréquence empirique. Au fond d'une moyenne empirique le statisticien cherche à découvrir l'expression à priori de *l'éspoir mathématique* d'une grandeur variable donnée[5]. Le statisticien qui adapte le point de vue stochastique s'efforce de formuler plus clairement le problème des rapports entre les grandeurs à priori et les grandeurs empiriques et de rationaliser les méthodes d'investigation des premières à l'aide des secondes. D'après les differents *espoirs mathématiques* des grandeurs variables simples on peut établir *l'espoir mathématique* des différentes grandeurs statistiques et caractéristiques plus ou moins complexes vers lesquelles tendent ces grandeurs dans le cas d'un grand nombre d'observations.

Le Prof. T. s'efforça d'établir tout l'abord le système des grandeurs basées sur les *espoirs mathématiques* pour les cas où il s'intéressait à une grandeur variable, puis pour ceux où il s'intéressait à la dépendance statistique *de deux ou plusieurs* grandeurs variables, c'est-à-dire à leur corrélation.

Dans le domaine des recherches statistiques concernant une grandeur variable, le Prof. T. s'est voué surtout au problème de la stabilité des nombres statistiques sous le rapport mathématique.

Les notions de la stabilité normale, subnormale et surnormale, introduites dans ce problème complexe par Lexis, ainsi que le critérium employé pour la distinction empirique de ces catégories de stabilité des séries statistiques, formulé par Lexis sous forme des soidisants coefficients de dispersion, ont été soumis par le Prof. T. à une analyse mathématique plus approfondie. Il a démontré leurs inconvénients et proposé quelques perfectionnements méthodiques essentiels devant servir à la détermination de la stabilité des données des séries. Les résultats auxquels il est arrivé ont été exposés dans son travail *Zur Théorie der Stabilität statistischer Reihen*, publié en 1918 dans le périodique *Skandinavisk Aktuarietidskrift*.

Continuant ses recherches il est arrivé à la conclusion que ni le critérium de Lexis ni d'autres sembables ne s'adaptent à la détermination de la nature normale de la stabilité sur la base d'une simple observation du mouvement des séries empiriques sans d'autres points de repère. Cette conclusion qui est d'une grande importance a été publiée par T. dans son article *Ist die normale Stabilität empirisch nachweisbar*. A ses travaux sur le problème de la stabilité viennent s'ajouter ses conclusions auxquelles il est arrivé dans le domaine des bases mathématiques de la loi des grands nombres.

Dans un article anglais publié dans le périodique statistique international *Metron* en 1923, il développe le système des formules pour la caractéristique statistique de la distribution d'une grandeur variable accidentelle où il part des suppositions les plus générales de la théorie des probabilités:

Nous considérons comme grandeur variable accidentelle de l'odre K la grandeur qui peut avoir un nombre K de valeurs différentes avec certaines probabilités. La somme de valeurs possibles de la grandeur variable accidentelle et de leurs probabilités correspondantes sera désignée comme la loi de la distribution des valeurs de la grandeur variable accidentelle.

Le Prof. T. détermine les cas concrets où la loi des grands nombres ne suffit pas, tant dans ce sens où la conclusion faite sur la grandeur à priori d'après la grandeur empirique ne devient pas plus exacte si l'on augmente le nombre des observations, que dans celui où la grandeur empirique ne tend pas vers une valeur, c'est-à-dire vers une valeur à priori, même dans le cas extrême, c'est-à-dire dans le cas d'un nombre infini d'observations, mais vers plusieurs valeurs dont chacune a sa propre probabilité.

De cette manière le problème de l'élucidation des bases probables sur lesquelles repose le matériel statistique empirique prend uner importance spéciale.

Les autres travaux du Prof. T., traitant la théorie des rapports entre deux ou plusieurs grandeurs variables, ont été publiés dans son oeuvre *Grundbegriffe und Grundprobleme der Korrelationstheorie* 1925 et dans d'autres articles publiés dans le *Russkij Ekonomitcheskij Sbornik*, tt. 1 et 4.

L'auteur y donne d'après ses travaux antérieurs un exposé systématique des bases de la théorie actuelle de corrélation entre deux grandeurs variables. En plus de la question de l'orientation préliminaire sur le schema de la probabilité à appliquer à tel ou tel matériel donné, l'auteur explique sa notion de la *corrélation stable normale* entre deux grandeurs variables ainsi que son critérium de la stabilité normale de la corrélation qui est analogue au criterium connu de Lexis – Bortkiewicz établi pour une grandeur variable.

En plus de nombreuses recherches dans le domaine de la théorie statistique, grâce auxquelles le Prof. T. fut élu, en 1924, membre honoraire de la Société Royale de Statistique de Londres, il nous faut encore signaler des travaux sur la *Répartition des nouveau-nés d'après le sexe, sur l'influence de la guerre sur le mouvement de la population, sur l'économique mondiale après la guerre* etc.

En la personne du Prof. Tchouproff la science a perdu un travailleur éminent qui lui a sacrifié ses forces jusqu'au dernier moment.

Notes

1. Instead of the author's French translation I inserted the existing English translation (Chuprov 1914, Russian/1981, p. 164).

2. The author had not provided the necessary reference. I found the passage (in its original German) in Chuprov (1909/1959, p. 98).

3. Pour la caractéristique des ouvrages sur la statistique mathématique nous avons profité des articles du Docent privé J. Kohn publiés dans le *Rousski Economitcheskij Sbornik* 1926, livr. 5 et 6. P. G.

4. The author should have discussed the so-called classical definition of probability rather than commenting on the objective and subjective probability. For that matter, subjective probability is used when relying on expert opinions.

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I included here four obituaries and one more was published by E. E. Slutsky (1926) but its English translation is available (Slutsky 2010). And so, three Russian authors published their work abroad. In the Soviet Union, only one short obituary of Chuprov had appeared, and that in a Leningrad newspaper. Chuprov left Russia for a short while before the Bolshevik coup d'état but did not return and was therefore highly politically suspected. Moreover, the statistical authorities accused every Western statistician beginning with Süssmilch of applying stability of statistical ratios for proving the eternity of capitalism. The mind was slavishly subordinated to the governing ideology that determined every aspect of Soviet life (and essentially curtailed scientific work), see Sheynin (2011, pp. 159 – 160). No wonder Isserlis did not name his Russian correspondent.

The obituary by Keynes is noteworthy because of his high scientific standing. He it was who accepted Chuprov's manuscript for publication (1912) in the *Economic Journal* and was glad to find out that Chuprov had been *safely established in Sweden* (Sheynin 2011, p. 157). What is frustrating about the other authors is their ignorance of some main scientific issues. Thus, only Georgievski knew that Chuprov had done away with the Lexian pattern of stability (Sheynin 2017, pp. 249 – 250). Even more puzzling is the slavish praise of Chuprov's unworthy book (1909), see Note 5 to Isserlis. Incidentally, Isserlis translated and published Chuprov's posthumous Russian manuscript (1931). Finally Chuprov attempted to study most widely statistical inference but Romanovsky (Sheynin 2011, p. 156) reasonably argued that his formulas, although interesting theoretically, were too unwieldy and complicated and barely useful.

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VII

Oscar Sheynin

Gauss and the method of least squares before 1805
An additional comment

There exist many instances in which Gauss could have well applied his innovation, at least for preliminary, trial calculations or short cuts, before the 1805 publication of Legendre (Sheynin 2017, § 9A.1.4). For him, the method of least squares (MLSq) was not a cut and dry procedure. And weighing of observations as well as possible mistakes in his calculations could have hampered justification. In my book, I also mentioned friends and colleagues whom Gauss had notified about the MLSq, again before 1805.

Now, I mention two new circumstances. One of them, see the previous page in my contribution, was known to me previously, but I failed to mention it in its proper place. In his letter of 1841 (published in 1842) to W. E. Weber Gauss expressed his general opinion about applications of the theory of probability. If only based on numbers, as Gauss decided, such applications can be greatly mistaken, so it followed that all relevant circumstances ought to be considered. This requirement, however, was hardly complied with by any critic of Gauss.

My second new argument seems to be barely known. Tutubalin (1973, section 2.4, p. 27) described the result of an experiment. Sixteen mathematicians and engineers smoothed over an empirical broken line by naked eye. The results were *quite comparable in precision* with the MLSq.

Now think seriously before deciding that Gauss had not known that …, had not arrived at …, did not see that … And he likely thought that this graphical least-squares procedure was good enough. But how about estimating the precision of such procedures? Gauss was able to achieve this goal fairly well by repeating several times the naked-eye method. Anyway, estimation of precision was never mentioned by any critic of Gauss.

And now I quote Kronecker (1901, p. 42):

After proving his theorems, Gauss always got rid of all traces of his train of thoughts which led him to his results.

May (1972) voiced a similar opinion.

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