Book Reviews (mainly from the Zentralblatt MATH)

My reviews are very critical since they reflect the horrible situation, at least in my small region of the history of mathematics, mostly occasioned by irresponsible reviewing. Reviewing is not considered, as it should be, an essential component of scientific work. In turn, scientific work is at least partly a rat race, whose essence is *Publish or perish*! No possibility is left for honest reviewing.

During my scientific life, I have been independent, did not participate in that race, was able to pay utmost attention to reviewing. In all, I have reviewed much more than a hundred contributions (mostly articles). However, the new editors of the section on the history of mathematics of the *Zentralblatt* became worried: I stated just what I thought rather than provided smarmy reviews, sweet nothings, and thus spoiled the game.

I quit, and the (former) Editor of that journal thanked me for my work of a few decades; its text is on my website sheynin.de (copied by Google). On my computer, I have the texts of almost all of my reviews, and will send the appropriate document to those interested. There also is the text of my Russian paper explaining in detail the situation with reviewing and the pressure of the iron heel of science studies (of scientology, would have been a better expression had it not been usurped by quite another gang of scientists), of happy-go-lucky ignoramuses.

Abbreviation: NKzR = *Novye Knigi za Rubezom* (New Books Abroad)

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!).

References

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Hist. Scientiarum (Tokyo), vol. 16, 2006, pp. 212 – 214

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

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 13th 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 analyzes 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

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'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 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

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

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

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

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, p. 296.

Physis, vol. 29, 1992, pp. 633 – 638

B. V. Gnedenko, A. Ya. Khinchin, An Elementary Introduction to the Theory of Probability. Many Russian editions beginning from 1946, an American translation of 1961 and my own translation of 2015

The following text is extracted from the Introduction to my translation (available on my website, Document 65).

I. The book has been greatly successful, witness the Forewords to some of its previous editions. To my surprise, it is hardly satisfactory,

1. The book is written very carelessly. Just one example: artillery firing is mentioned more than once, and each time the scatter of the shells is only considered along the line of firing. Only once the authors obliquely remark that the shells *fall around*. Carelessness was apparently the reason for mentioning quite unnecessary details as well. Thus, four main causes of stoppages of looms are listed.

2. Several opportunities to insert important remarks are missed although *the student is a torch to be fired rather than a container to be filled*. The shortcomings of the Bayesian approach are not indicated, the Bernoulli theorem is discussed unsatisfactorily, chaotic motion is not mentioned, direct and inverse theorems are not discussed in a general way and neither is sampling. Nothing is said about the required number of significant digits in approximate calculations and the authors themselves mistakenly indicated doubtful and unnecessary digits.

3. The notions of probability and expectation are justified by common sense without indicating the accepted formal method; moreover, statistical probability is described as theoretical. Possibly confusing additional words (*always*, *purely* random etc.) are inserted into statements and definitions.

4. Historical comments are unsatisfactory. Chebyshev is properly mentioned in connection with the law of large numbers, but Poisson is left out.

5. Some examples concerning the measurement of distances and artillery firing belong to fairyland and remind how Mark Twain edited an agrarian paper (pole-cats should be domesticated etc), and the discussion of the errors of measurements is unsatisfactory.

6. Population statistics is represented by two examples concerning the sex ratio at birth (carelessly stating the probability of a male birth). The authors should not, however, be blamed for neglecting this field of statistics: millions perished in the GULAG, and the war claimed still more lives. For many years population statistics remained a touchy subject. The results of the census of 1937 were allegedly sabotaged and the Central Statistical Directorate decimated. Kolmogorov avoided mentioning population statistics in a report of 1954.

The complete absence of examples based on games of chance seems doubtful.

II. Its American translation of 1961 is dated since Gnedenko had inserted new additions and even a whole new part (Part 3). Then, the translator, Leo E. Baron, followed the Russian original without any comments and too often left the (naturally, Russian) structure of phrases unaltered. He, or the *Editorial collaborator* Sidney F. Mack, appended a Bibliography but it has no connection with the text itself.

III. The authors are generally known, but I am adding some comments. In 1978 I published a joint paper with Gnedenko and certainly know that he had successfully studied the work of Chebyshev, Markov and Liapunov, but that he left aside the history of probability as developed by foreign scholars. This fact is clearly visible here as well as in his essay published in 2001(or before that) which should have appeared 30 years earlier.

Among other methodical and pedagogical contributions Khinchin left a concise treatise on mathematical analysis (1948), a possibly rather too shortened textbook for university students (1953) and a posthumously published essay on the Mises theory (1961). Gnedenko edited it and explained that the celebrated journal, *Uspekhi Matematicheskikh Nauk*, had rejected its manuscript. Unfortunately, the cause of rejection remains unknown.

Little known is Kolmogorov's acknowledgement inserted in his great book heralding the best known axiomatization of probability:

I wish to express my warm thanks to Mr. Khinchine who has read carefully the whole manuscript and proposed several (*mehrere*!) *improvements*.

On the other hand, Khinchin's invasion of statistical physics (in (1943) was unsuccessful. Here is Novikov whose paper of 2002 deserves to be translated in full:

Khinchin attempted to begin studying the justification of statistical physics, but physicists met his contribution on [that subject] *with deep contempt. Leontovich* [an eminent and widely known physicist] *said … that Khinchin does not understand anything*.

But the most disturbing fact is the appearance of Khinchin's glorification of the Soviet regime published at the peak of the Great Terror. In October 1937 a "Colloque des probabilités" took place at the Genève University. Among the participants were Cramér, Feller, Hostinsky and other most distinguished scholars who signed *Compliments* to Born on the occasion of his birthday (Staatsbibl. Berl. Preussische Kulturbesitz. Manuskriptabt. Nachl. Born 129). No wonder that there were no Soviet participants! Information about the Great Terror should have been prevented. So much for Khinchin's kowtowing …

In 1986, a second edition of the Russian translation of part 4 of Jakob Bernoulli's *Ars Conjectandi* had appeared complete with three commentaries, one of which was mine. A subeditor told me to suppress my reference to Khinchin. He had not elaborated and I, regrettably, did not ask for any explanations. The Editor was the late Yu. V. Prokhorov, a well-known student of Kolmogorov.

Gnedenko B. V. (2001), *Ocherk Istorii Teorii Veroiatnostei* (Essay on the History of the Theory of Probability). Moscow, 2009.

Gnedenko B. V., Sheynin O. (1978, in Russian), Theory of probability. A chapter in *Mathematics of the 19th Century*, vol. 1. Editors, A. N. Kolmogorov, A. P. Yushkevich. Basel, 1992, 2001, pp. 211 – 288.

Khinchin A. Ya. (1937, in Russian), The theory of probability in pre-revolutionary Russia and in the Soviet Union. *Front Nauki i Tekniki*, No. 7, pp. 36 – 46. Translation: **S, G**, in No. 7.

--- (1943, in Russian), *Mathematical Foundations of Statistical Mechanics*. New York, 1949.
--- (1948, in Russian), *Eight Lectures on Mathematical Analysis*. Heath & Co., 1965.
--- (1953), *Kratkii Kurs Matematicheskogo Analiza* (Brief Course on Mathematical Analysis). Moscow.
--- (1961, in Russian), The Mises frequency theory and modern ideas of the theory of probability. *Science in Context*, vol. 17, 2004, pp. 391 – 422.
Kolmogorov A. N. (1933, in German), *Foundations of the Theory of Probability*. New York, 1956.
--- (1955, in Russian), [Description of his report at a statistical conference of 1954]. Anonymous (1955, pp. 156 – 158). Translation in **S, G,** No. 6.
Novikov S. P. (2002, in Russian), The second half of the 20th century and its result: the crisis of the physical and mathematical community in Russia and the West. *Istoriko-Matematich. Issledovania*, vol. 7(42), pp. 326 – 356.
Sheynin O. (1998), Statistics in the Soviet epoch. *Jahrbücher Nat.-Ökon. und Statistik*, Bd. 217, pp. 529 – 549.

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 life 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, 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. Scientiarum. 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

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

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

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

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 (13pp.) 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

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

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

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 defectively. 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

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

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

A. L. Schlözer: Theorie der Statistik nebst Ideen über das Studium der Politik überhaupt. Göttingen, 1804.

The author was a prominent scholar, a member of three academies, best known in Russia (where he had worked and lived for many years) and in the German world. His son, Christian Schlözer, now almost forgotten, was a distinguished statistician. I have partly devoted to him a (Russian) paper whose English translation is to appear soon (in 2016) in *Silesian Stat. Rev*.

I return to Schlözer the Father. Having been the successor of Achenwall, he provides a picture of the hardly known early history of the Staatswissenschaft (University Statistics) and supplies quotations from many sources of the 18th century. He discusses the theory of statistics, and its relations of this (then emerging) science with related sciences, especially history and geography. Much attention is devoted to the education of *politicians* (civil servants!), and the reader will be surprised to find out how seriously had travelling abroad been considered.

I am only concerned with statistics. Schlözer did not provide any clear-cut definitions. Statistics itself, its theory, the problem of studying causes and effects is treated in several sections without however any final statements. Thus, he even killed his pithy expression, *History is statistics flowing, and statistics is history standing still* (p. 86), by stating, on p. 93, that statistics is a part of history. Another glaring inconsistency is on p. 98: a young man is compelled to stay home but loses his travelling expenses!

Schlözer minutely describes the notion of *remarkable features* of countries (indicators of descriptive statistics), but, unlike Süssmilch, does not say anything about epidemics or measures preventing them. For some reason, at least until the 20th century, statisticians avoided this subject. Population statistics is barely mentioned, mean values not at all, and bibliographic information is as bad as possible. I cannot say that Schlözer published a really scientific contribution.

Unpublished

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 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

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

Oscar Sheynin

Antistigler

Unpublished

Stigler is the author of two books (1986; 1999) in which he dared to profane the memory of Gauss.

I had vainly criticized the first one (1993; 1999a; 1999b), but not a single person publicly supported me, whereas several statisticians, only justifying themselves by arguments *ad hominem*, urgently asked me to drop that subject. The appearance of Stigler's second book showed that they were completely wrong but the same general attitude is persisting. One of those statisticians, apparently believing that a living dog was more valuable than a dead lion, was the President of the International Statistical Institute (2008). But to go into detail.

1) A few years ago Stigler was elected President of that same Institute (and had served in that capacity). He is now member of the Institute's committee on history to which I was also elected (chosen?) without my previous knowledge or consent. I refused to work together with him (and with Descrosières, – of all members of the Institute, see below!).

2) A periodical (*Intern. Z. f. Geschichte u. Ethik* (!) *der Naturwissenschaften, Technik u. Medizin*, NTM) refused to consider my proposed subject, – the refutation of Stigler. The Editor politely suggested I should apply to a statistical periodical.

3) The Gauss-Gesellschaft-Göttingen is silent and had not even answered my letter urging them to support me.

4) Healy (1995, p. 284) indirectly called Stigler the best historian of statistics of the 20th century, and Hald – yes, Hald (1998, p. xvi) even called Stigler's book (1986) *epochal*. Epochal, in spite of slandering Gauss, of humiliating Euler (below), and of its being an essay rather than THE HISTORY (!) of statistics, as Stigler had the cheek to name it.

So much is absent in THE HISTORY, – cf. my book Sheynin (2005/2009), – in spite of which it became the statisticians' Bible, that I shall extrapolate this phenomenon by reducing it with Lewis Carroll's help *ad absurdum*:

Other maps are such shapes, with their islands and capes:
But we've got our brave Captain to thank
(So the crew would protest) "That he's bought us the best –
A perfect and absolute blank!"

Stigler is regarded as a demigod. *Historia Mathematica* had published a review of his book (1999). Instead of providing its balanced account, the reviewer (an able statistician; H. M. vol. 33, No. 2, 2006) went out of his way to praise, *to worship* both the book and Stigler himself.

5) *Centaurus* rejected the manuscript of my paper (1999a) initially submitted to them since the anonymous reviewer, contrary to facts and common sense, did his damnedest to exonerate Stigler.

In addition to my papers mentioned above, I can now add two more publications (2005; 2006, see their Indices), but I ought to add several points here.

1. Stigler (1986, p. 145): *Gauss solicited reluctant testimony from friends that he had told them of the method* [of least squares, MLSq] *before* [the appearance of the Legendre memoir in] *1805*.

And in 1999, p. 322, repeating his earlier (of 1981) statement of the same ilk: *Olbers did support Gauss's claim … but only after seven years* *of repeated prodding by Gauss*. Grasping at straws, Stigler adds an irrelevant reference to Plackett (1972).

So what happened with Olbers? On 4.10.1809 Gauss had asked him whether he remembered that he had heard about the MLSq from him (from Gauss) in 1803 and again in 1804. Olbers apparently did not answer (or answered through a third party). On 24.1.1812 Gauss asked even more: Was Olbers prepared to confirm publicly that fact? And Olbers answered on 10.3.1812: *gern und willig* (with pleasure), and at the first opportunity. However, during 1812 – 1815 Olbers had only published a few notes on the observation of comets (*Catalogue of Scientific Literature*, Roy. Soc. London), and he therefore only fulfilled Gauss' request in 1816. Much later Gauss, who became sick and tired of the whole dispute, in a letter of 3.12.1831 to Schumacher, mentioned that his friend had acted in good faith, but that he was nevertheless displeased by Olbers' testimony made public.

2. Again in 1999, Stigler had deliberately omitted to mention Bessel's statement on the same subject. I discovered it while being prompted by Stigler's attitude and quoted Bessel in a paper (1993) which Stigler mentioned in 1999. Bessel's testimony, all by itself, refutes Stigler's accusation described above.

3. Stigler (1999, pp. 322 – 323) mentions von Zach, his periodical (*Monatl. Corr.*) and some material published there in 1806 – 1807 which allegedly (indirectly) proved that von Zach had not considered Gauss as the inventor of the MLSq. Stigler leaves out a review published in the same periodical in 1809 whose anonymous author (von Zach?) described the actual history of the discovery of the MLSq, see p. 191. Incidentally, I (1999a, p. 258) found von Zach's later statement in which he repeated Gauss' explanation to the effect that he, Gauss, discovered the MLSq in 1795.

4. Stigler (1986, p. 57): "It is clear […] that Legendre immediately realized the method's potential". And, on p. 146: "There is no indication that [Gauss] saw its great general potential before he learned of Legendre's work". Stigler thus denies Gauss' well-known statement that he had been applying the MLSq since 1794 or 1795, denies simply because he is inclined to dethrone Gauss and replace him by Legendre.

5. Stigler (1986, p. 143): Only Laplace saved Gauss' first justification (in 1809) of the MLSq from joining "an accumulated pile of essentially ad hoc constructions". And how about Legendre? Stigler (1986, p. 13): *For stark clarity of exposition the presentation* [by Legendre in 1805] *is unsurpassed; it must be counted as one of the clearest and most elegant introductions of a new statistical method in the history of statistics*. His work (Stigler, p. 57) revealed his "depth of understanding of his method". All this in spite of two mistakes made by Legendre and lack of any demonstration of the method. Legendre alleged that the MLSq agreed with the minimax principle, and he mentioned errors instead of residual free terms of the initial equations. And can we believe that Stigler did not know that the Gauss' proof of 1809, which allegedly almost joined "the accumulating pile" of rubbish, had been repeated in *hundreds* of books on the treatment of observations? Was it only due to Laplace?

6. Stigler (p. 146): *Although Gauss may well have been telling the truth about his prior use of the method, he was unsuccessful in whatever attempts he made to communicate it before 1805*. The first part of the phrase was appropriate in respect to a suspected rapist, but not to Gauss. As to his "attempts", Gauss had communicated his discovery to several friends and colleagues but did not proclaim it through a public crier or by a publication in a newspaper.

Other pertinent points.

7. Stigler (1986, p. 27) denounced Euler as a mathematician who did not understand statistics. After I (1993) had refuted that pernicious statement, Stigler (1999, p. 318) declared that, in another case, Euler *was acting in the grand tradition of mathematical statistics*. He did not, however, renounce his previous opinion. More: in that second case, Euler had rejected the method of maximum likelihood, because, as he put it, the result should not change whether an outlying observation be rejected or not (read: the treatment should be such that …). Euler suggested to keep to the known and reliable method, to the mean; he had not mentioned the median although it (but not the term itself) had actually been earlier introduced by Boscovich.

8. Descrosières (1998, transl. from French) believes that Poisson had introduced the strong law of large numbers and that Gauss had derived the normal distribution as a limit of the binomial law, see my review in *Isis*, vol. 92, 2001, pp. 184 – 185. And Stigler (1999, p. 52)? He called Descrosières *a scholar of the first rank*!

9. There also, Stigler named another such high ranking scholar, Porter, and he (p. 3) also called Porter's book of 1986 *excellent*. I reviewed it (*Centaurus*, vol. 31, 1988, pp. 171 – 172) and declared an opposite opinion. In 2004 Porter published Pearson's biography, see my review in *Hist. Scientiarum*, vol. 16, 2006, pp. 206 – 209. I found there such pearls of wisdom as (p. 37) *Even mathematics has aspects that cannot be proven, such as the fourth dimension*. In my opinion, that book is barely useful.

10. In 1983, issuing from a biased stochastic supposition, Stigler declared that another author rather than Bayes had actually written the Bayes memoir. In 1999, while reprinting his 1983 paper, in spite of his sensational finding being stillborn and forgotten, Stigler got rid of its criticisms in a tiny footnote (p. 391).

11. Stigler (1986) is loath to mention his predecessors. On pp. 89 – 90 he described the De Moivre – Simpson debate forgetting to refer to me (1973a, p. 279). And on pp. 217 – 218 he discussed the once topical but then completely forgotten conclusion concerning statistics of population without citing his only possible source of information, Chuprov's letter to Markov of 10.3.1916 (Ondar 1977/1981, No. 72, pp. 84 – 85).

Long before that Stigler (1977) dwelt on Legendre's accusation of Gauss concerning number theory without naming me (1973b, p. 124, note 83).

So why does Stigler remain so popular? Because the statistical community is crassly ignorant of the history of its own discipline; because it pays absolutely no attention to the slandering of Gauss' memory (even if realizing that fact, as the reviewer for *Hist. Math.* did, see above, – I personally informed him about it in 1991, but he had known it himself); because it possesses a narrow scientific Weltanschauuung; and because the tribe of reviewers does not feel any social responsibility for their output. And of course there is a special reason: Stigler published his book (1986) when there was hardly anything pertinent except for papers in periodicals. The same happened to a lesser extent with Maistrov's book of 1974 which is still remembered!

To end my pamphlet, I quote, first, the most eminent scholar and historian of science, the late Clifford Truesdell (1984, p. 292), whom I will never forget and whose alarm bell apparently fell on deaf ears, and, second, Einstein's letter of 1933 to Gumbel, a German and later an American statistician (Einstein Archives, Hebrew Univ. of Jerusalem, 38615, in translation):

1) *No longer is learning the objective of scholarship.* […] *By definition, now, there is no learning, because truth is dismissed as an old-fashioned superstition.*

2) *Integrity is just as important as scientific merits.*

I adduce a list of my other (Russian) reviews from the NKzR, ser. A and B

1. Hristov, V. K., *Matematicheskaia Geodesia* (Mathematical Geodesy). Sofia, 1956. A1958, No. 3, pp. 21 – 22.
2. Hristov, V. K., *Osnovy Teorii Veroiatnostei, Oshibok i Uravnivania* (Elements of the Theory of Probability, Theory of Errors and Adjustment). Sofia, 1957. B1960, No. 2, pp. 132 – 134.
3. Grossmann, W. *Grundzüge der Ausgleichungsrechnung.* Berlin, 1961. B1962, No. 11, pp. 8 – 10.
4. Jordan, W., Eggert, O., Kneissl, M., *Handbuch d. Vermessungskunde*, Bd. 1. *Ausgleichungsrechnung nach d. Methode d. kleinsten Quadrate*. Stuttgart, 1961. B1963, No. 5, pp. 105 – 108.
5. Bomford, G., *Geodesy.* Oxford, 1962. B1963, No. 12, pp. 92 – 93. Coauthor, A. V. Kondrashkov.
6. *Bibliographie géodésique internationale*, t. 9. Paris, 1963. B1965, No. 8, pp. 109 – 110.
7. Barry, B. A., *Engineering Measurements*. New York, 1964. B1966, No. 6, pp. 21 – 22.
8. Hristov, V. K., *Rasshirenie Uravnivania po Sposobu Naimenshikh Kvadratov* (Generalized Adjustment by the Method of Least Squares). Sofia, 1966. B1967, No. 3, pp. 108 – 109.
9. Hultzsch, E., *Ausgleichungsrechnung mit Anwendungen in d. Physik*. Leipzig, 1966. B1967, No. 5, p. 10.
10. Richardus, P., *Project Surveying.* Amsterdam, 1966. B1967, No. 10, pp. 109 – 110.
11. Hazay, I., *Adjustment Calculations in Surveying*. Budapest, 1970. A1972, No. 4, pp. 49 – 50.

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--- (1993), On the history of the principle of least squares. Ibidem, vol. 46, pp. 39 – 54.
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