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BLACK BOOK

of

HISTORY OF PROBABILITY AND STATISTICS

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Something is rotten in the State of Denmark …
Shakespeare, *Hamlet*. Act 1, Sc. 4

Introduction

I have included *guilty* authors (certainly not all of them) but did not mention their positive results. Now, how had their deficiencies occurred? And why, more generally, is the situation really bad? In more detail, I (2016) described it elsewhere.

1. Why do authors become guilty?

1.1. Carelessness. It is sometimes explained by the inevitable haste, by the scientific rat race. Publish or perish! Sweet nothings fall under the same category. Much worse, carelessness is sometimes occasioned by ignorance aggravated by impudence.

1.2. Insufficient or faulty knowledge of existing sources. Much time ago, Mikhailov (1975), the director of the academic Institute for Scientific Information, somehow estimated that abstracting journals (that Institute published several dozens of them on most various disciplines and sciences) ensure 80% of the necessary knowledge of such sources whereas otherwise 94% of them remain unknown.

These figures were certainly approximate, and they concerned sciences as a whole. The situation had drastically changed. First, abstracting journals became too expensive and are now difficult to get. I believe that at the very least funds ought to be found for publishing readily available lists of new publications, each in its own field. Indeed, meteorologists (Shaw et al 1926/1942, p. v) decided that

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

Their statement is universally true.

Second, enter the Internet. It supplies very much information, but it is a dangerous machine. It conveys the feeling of being *with it* although earlier sources become forgotten or are difficult to come by.

Two special points. Publishers often reprint previous editions of collections without asking the authors to update their papers (which is sometimes quite possible). Then, many authors positively refer to sources which they never saw. The mentioned rat race does not exonerate them.

1.3. The language barrier. The main barrier is between the Russian language and the main languages of Western Europe. It existed in the 19th century, but then it was only one-sided: Russian scientists knew about Western Europe. Later, however, the situation drastically worsened: It did not befit Russia, the birthplace of elephants (a Soviet joke, but perhaps expressing the truth), to kowtow to all foreign. In 1951, I myself had to obtain a special permit to read foreign geodetic literature.

Since ca. 1985 the elephants are forgotten, but in Russia foreign literature is insufficiently known whereas many foreigners, just like previously, do not deem necessary to understand Russian. Some Russian journals are being translated into English, but, as I happened to hear from prominent Western scientists, at least in some of them the translations are too formal whereas the original Russian is often too concise (a national sin.)

Book catalogues of the main German (and, as I suspect, not only German) main libraries are only compiled in the Roman alphabet, and it is difficult to find there a Russian name containing a hissing letter. This restriction testifies that Russian literature is not sufficiently used. There is one more pertinent circumstance which I describe below, in § 2.

1.4. Appalling reviewing. Here is an example from olden days (Truesdell 1984, p. 397):

The Royal Society twice in thirty years [in 1816 and 1845] *stifle*[d] *the truth in favour of the wrong, twice bur*[ied] a great man [Herapath and Waterston] *in contempt while exalting tame, bustling boobies …*

Truesdell added: the officials defended any paper published by the Society. The same is true nowadays with respect to the Royal Statistical Society, as I know from my own experience.

Nowadays, the scientific community does not value reviewing. Apparently, this most important work is not recognized as scientific activity. Anyway, I am listing the possible reasons of bad reviewing.

1) Many reviewers just do not understand their duties.

2) They are afraid to lose face by refusing to review alien material or collections of essentially differing papers, − by refusing to object to wrong decisions of those responsible.

3) Publishers send free copies to editors of journals for reviewing. The editors obviously want to preserve that mutually beneficial practice and, at the expense of readers, are loath to publish negative reviews.

4) Many journals have a small number of readers, and their editors are therefore afraid of publishing unusual papers

5) In a scientific field with a comparatively small number of researchers (for example, in history of mathematics) all of them know each other and do not want to reveal unpleasant circumstances.

6) Reviews or essays of/on earlier classical works, especially written by compatriots, are very often downrightly prettified.

7) Reviews written for publishers are meant to advise them about the advisability of issuing one or another book. However, some of the circumstances mentioned above apply to them as well with the addition of the influence of commercial interests.

In short, the situation with reviewing is horrible. How many unworthy books and papers are therefore published? And how many of the worthy contributions rejected? And in both cases the mistakes are sometimes intentional.

There exist fine examples of proper reviewing. In 1915, the Imperial Academy of Sciences awarded a gold medal to Chuprov for reviewing on its behalf (Sheynin 1990/2011, p. 50). During the last years of his life Chuprov had published many decent and comprehensive reviews which I listed in that source.

In Germany, Bortkiewicz was called the *Pope of statistics*. *The publishers have stopped asking* [him] *to review their books* [because of his deep and impartial reviews] (Woytinsky 1961, pp. 451 – 452).

And many weak works had probably never appeared since their authors were afraid of his response.

In the Soviet Union, a special abstracting journal, *Novye Knigi za Rubezhom* (New Books Abroad), had been issued (but I do not know its further destiny). Long and really scientific reviews were published there by eminent authors. A good example for emulation!

2. The sledgehammer law

I bear in mind the unnecessary, highly harmful and burdensome strict standardization of manuscripts. Here, again, is Truesdell whose memory I cherish. He had time to edit 49 volumes of the highly prestigious *Archive for History of Exact Sciences.* Authors of papers published in one and the same issue of that journal submitted their manuscripts in their own (reasonable) format, and just imagine: nothing bad happened! Nevertheless, the new editors (the two co-editors) promptly returned the *Archive* to its proper place …

Fitting manuscripts to a requested format (probably different from one journal to anther) embitters authors and diverts them from their main duty. Manuscripts differ in many respects (length, subject, aim of work, style), but authors are still required to toe the line. Is Truesdell's statement (1984, p. 206) too exaggerated? Here it is:

The army of publisher's clerks usually holding positions classified as <u>editors</u>, who by profession lay waste to the texts that pass through their hands [and] *many authors no longer trouble to write a decent text since they know that <u>editors</u> will spoil it anyway*.

And the spelling of names? S. N. Bernstein was a foreign member of the Paris Academy of Sciences, published many notes in their *Comptes rendus* and always signed them just so. Nowadays, however, editors unanimously require the ugly spelling *Bernshtein* and thus find themselves on the wrong side of the law: *Bernstein* should at least be considered as the author's penname.

Manuscripts translated from Russian are rejected, period! Suppose that a journal has a thousand readers which is a more than generous premise. How many of them will establish a Russian article, get hold of it and more or less understand it? One or two, so the ban is stupid and antiscientific.

Everything now is ruled by the sledgehammer law. But there should be no standardisation, no straitjackets. And who is wielding the sledgehammer? I have only one answer: the damned scientologists (no connection with the religious meaning of that term) who wish to estimate numerically scientific products, but, all the same, miserably fail. Such an aim is probably unattainable.

And here in addition is the rage: change every previously established expression! The theory of errors, for example, is now usually called *error analysis*, just to appear modern. *The address is on my platform*, a correspondent once informed me. He should have said: … *is a few lines below*. Truesdell had diagnosed this novelty: rat catchers are now called rodent operators!

3. Conclusion

History of probability and statistics (and likely history of mathematics in general) is not considered a scientific discipline. Such sloppy work as seen below is hardly imaginable in physics or mathematics, but is perhaps encountered in history itself.

Cross-references in my main text are sometimes only indicated by italics. Thus, *Johns* means see Johns among the selected authors. Then, **S, G,** i denotes a downloadable document i on my website www.sheynin.de My abbreviation shows that the source in question is translated there into English or that that source is rare but available on my site. Google is honouring me by diligently copying my website, see Google, Oscar Sheynin, Home. Hence the letter **G** of my abbreviation.

Mikhailov A. I. (1975, in Russian), Abstracting journal. *Great Sov. Enc.*, third edition, vol. 22, pp. 53 – 54.
Shaw N., Austin E. (1926), *Manual of Meteorology*, vol. 1. Cambridge, 1942.
Sheynin O. (1990, in Russian), *Alexandr A. Chuprov. Life, Work, Correspondence.* V&R Unipress, 1991.
--- (2016, in Russian), The sin of the scientific community and the triumph of the bureaucracy. *Finansy i Biznes*, No. 1, pp. 176 – 184.
Truesdell C. (1984), *An Idiot's Fugitive Essays on Science*. New York. This is a reprint of many essays and reviews on/of classical works published over many years. Idiot, as Truesdell explains, is derived from Greek and properly denotes a non-specialist , but I do not understand why did he thus call himself.
Woytinsky W. S. (1961), *Stormy Passage*. New York.

Main Text

The authors are arranged alphabetically. Many more are included in the texts which describe the work of those chosen authors.

R. Adrain

Adrain (1809) offered two derivations of the normal distribution of random errors of observation. He assumed wrong properties for those errors and the derivations themselves were not rigorous at all. Nevertheless, some authors (for example, John Herschel, and, in a physical context, Maxwell who tacitly proposed that the components of the velocities of molecules were independent), repeated them without referring to Adrain. Adrain's article actually appeared in 1809 (Hogan 1977).

Adrain R. (1808), Research concerning the probabilities of the errors which happen in making observations. In Stigler (1980, vol. 1).

Dutka J. (1990), R. Adrain and the method of least squares. *Arch. Hist. Ex. Sci.*, vol. 41, pp. 171 – 184.

Hogan E. R. (1977), R. Adrain: American mathematician. *Hist. Math.*, vol. 4, pp. 157 – 172.

Sheynin O. (1965, in Russian), On the work of Adrain in the theory of errors. *Istoriko-Matematicheskie Issledovania*, vol. 16, pp. 325 – 336.

Stigler S. M. (1980), *Amer. Contributions to Math. Statistics in the 19th Century*, vol. 1. New York.

R. Al-Biruni

He (1967) repeatedly reported about his regular observations. However, one of their aims was highly doubtful: he thought of predicting landslides by measuring the coordinates of some points on the surface of the earth. If, nevertheless, the error of his observed latitude was 1′ (it was certainly larger) it would have prevented him from any predictions, to say nothing about the measurement of longitudes.

Al-Biruni R. (1967), *Determination of the Coordinates of Cities*. Beirut.

J. Arbuthnot

Arbuthnot (1712) studied the sex ratio at birth. He drew on data covering the baptisms in London for 82 years and assumed an equal probability of the birth of both sexes. Baptisms of boys were invariably more numerous, the probability of such an occurrence ($1/2^{82}$) was too low, and Arbuthnot rejected his assumption in favour of the action of Divine Providence. Baptisms differed from births; Graunt, in 1662, at the end of Chapter 3 of his book, stated that in 1650 – 1660 less than half of English Christians had thought that baptisms were necessary. Then, Christians perhaps somehow differed from other groups of population, and London was perhaps an exception. Nevertheless, the predominance of male births is still being observed and thought to be occasioned by some constant regularity, and the study of the sex ratio at birth prompted most important discoveries including De Moivre's derivation of the simplest version the central limit theorem. Freudenthal (1961, c. xi) called Arbuthnot's note the first contribution in mathematical statistics.

In Arbuthnot's time, the binomial distribution was yet unknown. But even at the end of the next century eminent scholars somehow still equated randomness with *uniform randomness*. Thus, discussing Darwinism, Baer (1873, p. 6) and Danilevsky (1885, pt. 1, p. 194) rejected his evolution theory by issuing from that restriction. They independently mentioned the philosopher depicted in *Gulliver's Travels* (but borrowed by Swift from Raymond Lully, 13th – 14th centuries). That *inventor*, hoping to get to know all the truths, was putting on record each sensible chain of words that appeared from among their uniformly random arrangements. The first to recall that philosopher was, however, John Herschel (1861/1866, p. 63n).

Arbuthnot J. (1712), An argument for Divine Providence taken from the constant regularity observed in the birth of both sexes. M. G. Kendall & R. L. Plackett, Editors (1977), *Studies in the History of Statistics and Probability*, vol. 2. London, pp. 30 – 34.

Baer K. (1873), *Zum Streit über den Darwinismus*. Dorpat.

Danilevsky N. Ya. (1885, in Russian), *Darwinism*, vol. 1, pts 1 – 2. Petersburg.

Freudenthal H. (1961), 250 years of mathematical statistics. In *Quantitative Methods in Pharmacology*. Amsterdam, pp. xi – xx. Editor H. De Jonge.

Herschel J. (1861, lecture), Sun. In author's *Familiar Lectures on Scientific Subjects*. London – New York, 1866, pp. 47 – 90.

Shoesmith E. (1987), The Continental controversy over Arbuthnot's argument etc. *Hist. Math.*, vol. 14, pp. 133 – 146.

Basharin G. P., Langville A. N., Naumov V. A.

Their essay (2004) is superficial and they wrongly stated that Tolstoy, who died in 1910, had been excommunicated from the Russian Orthodox Church in 1912 (actually, in 1901). A branchy cranberry tree, as the Russian saying goes!

Basharin G. P. et al (2004), The life and work of A. A. Markov. *Linear Algebra Appl.*, 386, pp. 3 – 26.

Daniel Bernoulli

In 1769, when considering the treatment of observations x_i, D. B. assumed the density of the distribution of their (random) errors as a *semiellipse* or semicircle of some radius r, but, for simplifying calculations, he finally chose an arc of a parabola. He certainly did not know that the variance of the result will then be changed. As the parameter of location he assumed the weighted arithmetic mean of the observations with posterior weights

$$p_i = r^2 - (\bar{x} - x_i)^2 \qquad (1)$$

where $\bar{x}$ was the ordinary mean. Successive approximations were possible.

In 1778 D. B. turned to the principle of maximal likelihood (introduced in 1760 by Lambert) for observations with errors having a semicircle as the density of the distribution. The equation of likelihood became unimaginably complicated and it is difficult to understand why D. B. did not represent it as

$$\frac{x - x_1}{r^2 - (x - x_1)^2} + \frac{x - x_2}{r^2 - (x - x_2)^2} + \ldots = 0\,. \qquad (2)$$

By applying successive approximations he could have derived the arithmetic mean with posterior weights reciprocal to (1). They increased towards the tails of the density curve which contradicted the contemporary (but not modern) view as well as his own preliminary qualitative statement. Such an estimator was nothing but the usual mean corrected for the asymmetry of the empirical density, and D. B. himself noticed this fact. See also *Short*.

In 1780, D. B. considered errors of pendulum observations although only in the simplest case. He derived the normal distribution but did not even hint at a possible dependence between the periods of successive swings of the pendulum. There also, he formally introduced, for the first time ever, the notions of random and systematic errors, although only in the simplest case.

Bernoulli D. (1769, in Latin),The most probable choice between several discrepant observations etc. *Festschrift for Lucien Le Cam*. New York, 1997, pp. 358 – 367.

--- (1778, in Latin), Same title. In English, together with Euler's commentary of 1778: *Biometrika*, vol. 48, 1961, pp. 3 – 13; E. S. Pearson & M. G. Kendall, Editors (1970), *Studies in the History of Statistics and Probability*. London, pp. 155 – 172.

--- (1780), Specimen philosophicum de compensationibus horologicus etc. In author's *Werke,* Bd. 2. Basel, 1982, pp. 376 – 390.

Jacob Bernoulli

When considering *Bernoulli trials*, J. B. determined the approach of the statistical probability of an event to its theoretical probability and proved their asymptotic equality. He also mentioned the inverse case, even if the theoretical probability did not exist, but, like De Moivre (1718/1756, p. 251) after him, did not notice that that case was more difficult (required more trials for achieving the same precision of approximation). Indeed, in both cases trials were given, but the theoretical probability was only known in the direct case. It was Bayes who noted this circumstance.

For many decades statisticians continued to believe that the theory of probability (and the Bernoulli theorem) was only applicable in the case of Bernoulli trials and only if the theoretical probability of the studied event existed, and no one thought about the precision of the result obtained.

Haussner's German translation of the *Ars Conjectandi* is modernized, and the English translation by Edith Dudley Sylla ought to be publicly burned (Sheynin 2006).

Bernoulli J. (1713), *Ars Conjectandi*. *Werke*, Bd. 3. Basel, 1975. Editor, B. L. van der Waerden, pp. 107 – 259.

--- (2005), *On the Law of Large Numbers*. Berlin, this being my translation of pt. 4 of Bernoulli (1713). **S, G,** 8.

De Moivre A. (1718), *Doctrine of Chances*. London, 1738, 1756. New York, 1967.

Sheynin O. (2006), Review of Sylla's translation of the *Ars Conjectandi*. *Hist. Scientiarum*, vol. 16, pp. 212 – 214.

N. Bernoulli

His dissertation (1709) contained interesting theoretical results and was at least partly useful for the administration of justice. However (Kohli 1975, p. 541), not only did N. B. pick up some hints included in the manuscript of the *Ars Conjectandi*, he borrowed passages from Jacob Bernoulli's Diary (*Meditationes*) never meant for publication.

Bernoulli J. (1975), *Werke*, Bd. 3. Basel. Editor, B. L. van der Waerden.

Bernoulli N. (1709), De usu artis conjectandi in jure. In J. Bernoulli (1975, pp. 289 – 326).

Kohli K. (1975), Kommentar zur Dissertation von N. Bernoulli. Ibidem, pp. 541 – 556.

J. Bertrand

The style of his book (1888) is wonderful, but it is written carelessly, certainly in great haste, and contains wrong statements and cumbersome calculations. Bertrand was obviously muddled by wishing to criticize everything possible and impossible. He had not mentioned Chebyshev and even Laplace and Poisson were all but absent.

Statistical probability and calculations (p. 276). A coin was tossed a million times and heads appeared in $m = 500{,}391$ cases. Unbelievably, *not a single digit* of the statistical probability $p_1 = 0.500\,391$ *can be trusted*! Bertrand then compared two hypotheses about that probability: it is either p_1 or $p_2 = 1 - p_1$. Instead of calculating

$$p_1^m p_2^n \div p_2^m p_1^n,\ n = 499{,}609,$$

he applied the De Moivre limit theorem and declared that $p_1 = 3.4p_2$. So what? And, anyway, why such a doubtful p_2?

Repeated event (p. 160). Bertrand *condemned* the premise of equal prior probabilities (as suggested by Bayes) only because the second appearance of a studied event became too high. But its first occurrence tells us almost nothing, and, anyway, Bertrand did not propose anything instead.

Moral applications of probability. Bertrand did not refer to Laplace or Poisson and was unable to say anything interesting.

The length of a randomly drawn chord of a given circle (p. 4); both he and his commentators considered uniform randomness. It is required to determine the probability that such a chord is shorter than the side of an equilateral triangle inscribed in the circle. Bertrand considered three natural versions of his problem and arrived at three different answers. Commentators discovered other natural cases of that problem, but De Montessus (1903), although he made an unforgivable arithmetical mistake, proved that there were uncountably many solutions and that the mean value of the probability sought was 1/2. A number of later commentators, although without referring to De Montessus, agreed with that value. According to the theory of information, that value of probability means complete ignorance, and the discussion of this problem which went on for many decades thus came to nothing.

Bertrand J. (1888), *Calcul des probabilités*. New York, 1970, 1972.
De Montessus R. (1903), Un paradoxe du calcul des probabilités. *Nouv. Annales Math.*, sér. 4, t. 3, pp. 21 – 31.
Sheynin O. (1994), Bertrand's work on probability. *Arch. Hist. Ex. Sci.*, vol. 48, pp. 155 – 199.

F. W. Bessel

This eminent scholar was at the same time an inveterate happy-go-lucky scribbler; two souls lived in his breast (Goethe's *Faust*, pt. 1, sc. 2). I (2000) found 33 elementary errors in his calculations and thus undermined the trust in the reliability of his more complicated computations. Bessel (1823) discovered the personal equation by observing the passage of stars simultaneously with another astronomer, but he wrongly treated one of the observations.

In 1818 and 1838 Bessel studied three series of a few hundred observations each made by Bradley. At first, he noted that large errors had occurred *somewhat oftener* than required by normality but wrongly stated that that discrepancy will not happen in longer series. And he had not noted that small errors were obviously rarer than required. Moreover, he missed the opportunity to be the first to state that normality was only approximately obeyed.

In 1838 Bessel even stated that normality was accurately obeyed, but he thus obviously and misleadingly defended the version of the central limit theorem which he proved (certainly non-rigorously, but this is not here essential) in the same contribution.

Another lie: in a popular essay (1843) Bessel stated that William Herschel had seen the disc of the yet unknown planet Uran. Actually, Herschel only saw an unknown moving body and thought that it was a comet. Mistakes and unjustified statements occur in Bessel's other popular writings. His paper (1845) is outrageous: without even a hint of having statistical information he made fantastic statements about the population of the U. S.

And here are excerpts from Gauss' correspondence.

1. Gauss (Gauss – Olbers, 2 Aug. 1817). Bessel had overestimated the precision of some of his measurements.

2. Gauss (Gauss – Schumacher, between 14 July and 8 Sept. 1826) stated the same about Bessel's investigation of the precision of the graduation of a limb.

3. Gauss (Gauss – Schumacher, 27 Dec. 1846) negatively described some of Bessel's posthumous manuscripts. In one case he was *shocked* by Bessel's *carelessness*.

Bessel F. W. (1818), *Fundamenta astronomiae*. Königsberg.
--- (1823), Persönliche Gleichung bei Durchgangsbeobachtungen. In Bessel (1876, Bd. 3, pp. 300 – 304).
--- (1838), Untersuchung über die Wahrscheinlichkeit der Beobachtungsfehler. Ibidem, Bd. 2, pp. 372 – 391.
--- (1843), Sir William Herschel. Ibidem, Bd. 3, pp. 468 – 478.
--- (1845), Übervölkerung. Ibidem, Bd. 3, pp. 387 – 407.
--- (1876), *Abhandlungen*, Bde 1 – 3. Leipzig.
Sheynin O. (2000), Bessel: some remarks on his work. *Hist. Scientiarum*, vol. 10, pp. 77 – 83.

K.-R. Biermann

Biermann (1991) states that for a century Gauss had been portrayed, mostly by Sartorius von Waltershausen, as a hero's marble statue, and that Gauss himself conscientiously or otherwise, assisted those portraitists. However, in ordinary life he was vulnerable, so those portraitists had been hushing up the real story. And, restricting now my comments to science, I say: Yes, Gauss did assist his biographers, but that was happening inevitably.

Early in life he published two classical contributions, the *Disquisitiones arithmeticae* in 1801, and the *Teoria motus* in 1809, so was it morally possible for him to lower his standards? Then Biermann notes that Gauss had experienced pleasure in playing with numbers which he had collected even if they had no connection with science. He is known, however, to apply the collected numbers to discover regularity in disorder (for example, to study the distribution of primes among natural numbers). So here we have an additional argument in favour of his inevitable assistance in sculpting that statue. His manuscripts ought to be in good order!

Biermann K.-R. (1991), Wandlungen unseres Gaussbildes. *Mitt. Gauss-Ges. Göttingen*, No. 28, pp. 3 – 13.

O. E. L. Bismarck von Schönhausen

Bismarck was *barely sympathetic to statistics and actually thought that it is not needed* (Saenger 1935, p. 452). An unjustified and strange statement. See *Lueder*.

Saenger K. (1935), Das Preußische statistische Landesamt, 1805 – 1934. *Allg. stat. Archiv*, Bd. 24, pp. 445 – 460.

L. Boltzmann

Boltzmann (1886/1905, p. 28) stated that the 19th century will be a century of *mechanical Weltanschauung*, *of Darwin*, and that (1904a/1905, p. 368) the theory of evolution was understandable in mechanical terms. He (1904b, p. 136) also thought that electricity and heat will perhaps be mechanically described. Bessel did not recognize objective randomness, and Rubanovsky (1934, p. 6) noted that his mechanical Weltanschauung *had gained a Pyrrhic victory* over randomness but *completely retreated in the ideological sense*.

Boltzmann L. (1886), Die zweite Hauptsatz der mechanischen Wärmetheorie. In author's book (1905, pp. 25 – 50).

--- (1904a), Entgegnung auf einen von … Ostwald … gehaltenen Vortrag. Ibidem, pp. 364 – 378.

--- (1904b), *Vorlesungen über die Prinzipe der Mechanik*, Tl. 2. Leipzig.

--- (1905), *Populäre Schriften*. Leipzig, 1925. [Braunschweig – Wiesbaden, 1979.]

Rubanovsky L. M. (1934), *Metody Fizicheskoi Statistiki* (Methods of Physical Statistics). Leningrad – Moscow.

Vladislav Bortkevich, Ladislaus von Bortkiewicz

Bortkiewicz was not mathematically educated. He (Bortkevich & Chuprov 2005, Letters 14 of 1896/1897 and 15 and 17 of 1897) did not know that an integral can be differentiated with respect to its limit. And he (1917, p. III) objected to the use of generating functions.

For several decades his law of small numbers, LLN (1898) remained the talk of the town although it only repeated the results of Poisson (Whitaker 1914; Sheynin 2008, specifying Kolmogorov's statement of 1945). Just as many other authors, Bortkiewicz (1917, pp. 56 – 57) thought that the LLN ought to be understood as a qualitative statement about the stability of statistical indicators when the number of observations is large. He (1894 – 1896, Bd. 10, pp. 353 – 354) stated that the study of precision was an accessory aim, a luxury and that the statistical flair was much more important.

The works of Bortkiewicz make difficult reading. He knew it well, but refused to budge. Winkler (1931, p. 1030) cited his letter, regrettably without providing its date or the name of the appropriate memoir: *I am glad to find in your person one of the five of my expected readers*.

A special case concerns his accusation of plagiarism by Gini: in his *great treatise* (1930), as Andersson (1931, p. 17) called it, on the distribution of incomes, he had not referred to Gini (1912). Andersson had described in detail the whole episode and completely exonerated Bortkiewicz who died soon afterwards and his answer (1931) to Gini appeared posthumously. But still, this is not the whole story. Chuprov received a reprint of Gini's paper, (too) briefly described it to Bortkiewicz (Bortkevich & Chuprov 2005, Letter 122 of 1913) and added: *I can send you Gini, if you will not find it in the library.*

In the next letter Bortkiewicz repeated that Gini's work [or rather the source where it appeared] was not available *in the local Royal Library* (in the present *Staatsbibliothek zu Berlin*), so that he can *rightfully ignore those papers*. A strange attitude! In spite of their heated discussion of the LLN twenty years ago, he should have mentioned Gini as his possible predecessor.

For his biography see Sheynin (2009, § 15.1.2; 2012).

Andersson T. (1931), Ladislaus von Bortkiewicz. *Nordic Stat. J.*, vol. 3, pp. 9 – 26.

Bortkevich V. I., Chuprov A. A. (2005), *Perepiska* (Correspondence) *(1895 – 1926)*. Berlin. **S, G**, 9.

Bortkiewicz L. von (1894 – 1896), Kritische Betrachtungen zur theoretischen Statistik. *Jahrbücher f. Nationalökonomie u. Statistik*, Bde 8, 10, 11, pp. 611 – 680, 321 – 360, 701 – 705 respectively.

--- (1898), *Das Gesetz der kleinen Zahlen*. Leipzig.

--- (1917), *Die Iterationen.* Berlin.

--- (1930), Die Disparitätsmasse des Einkommenstatistik. *Bull. Intern. Stat. Inst.*, t. 25, No. 3, pp. 189 – 298.

--- (1931), Erwiderung. Ibidem, pp. 311 – 316.

Gini C. (1912), Variabilità e mutabilità. *Studio Economico-Giuridici. Univ. Cagliari*, t. 3.

Sheynin O. (2008), Bortkiewicz' alleged discovery: the law of small numbers. *Hist. Scientiarum*, vol. 18, pp. 36 – 48.

--- (2009), *Theory of Probability. Historical Essay*. Berlin. **S, G,** 10.

--- (2012), L. von Bortkiewicz: a scientific biography. *Dzieje matematyki Polskiej*. Wrozlaw, pp. 249 – 266. Editor, W. Wieslaw.
Whitaker Lucy (1914), On the Poisson law of small numbers. *Biometrika*, vol. 10, pp. 36 – 71.
Winkler W. (1931), Ladislaus von Bortkiewicz als Statistiker. *Schmollers Jahrbuch f. Gesetzgebung, Verwaltung u. Volkswirtschaft im Deutschen Reich*, 55. Jg, pp. 1025 – 1033.

B. Bru

The bibliography appended to his paper (1981) is greatly substandard. There, on p. 69, is a sequence of numbers exceeding unity, but they are called probabilities.

Bru B. (1981), Poisson, le calcul des probabilités et l'instruction publique. Métivier M. et al, Editors, *S.-D. Poisson et la science de son temps*. Palaiseau, pp. 51 – 94.

G. L. L. Buffon

Buffon (1777, § 8) proposed 1/10,000 as a universally disregarded probability, the probability of a 56-years-old man to die during 24 hours. He also quoted a letter of 1762 from Daniel Bernoulli who suggested the value 1/100,000 and Buffon agreed if only healthy people were meant. Pearson (1978, p. 193) argued, however, that, when issuing from his reasoning, Buffon should have chosen 1/1000. Nevertheless, it is difficult to admit any such probability.

Pearson (p. 190) described and rejected Buffon's strange conclusion which he made in the second volume of his *Histoire naturelle*: *In the beginning of the world all things were less condensed than now*, and *men did not reach puberty till 130,* but then everything hardened and the duration of life shortened. Pearson also noted that Buffon's mortality table showed probable rather than mean life and that he did not distinguish baptisms from births.

Buffon made mistakes when studying the classical problem about the probability of the next sunrise. Thus, most curiously, he (Loveland 2001, pp. 466 and 470) somehow allowed probability to exceed unity.

Buffon G. L. L. (1777), *Essai d'arithmétique morale*. *Œuvr. Philosophiques*. Paris, 1954, pp. 456 – 488.
Loveland J. (2001), Buffon, he certainty of sunrise and the probabilistic reductio ad absurdum. *Arch. Hist. Ex. Sci.*, vol. 55, pp. 465 – 477.
Pearson K. (1978), *History of Statistics in the 17th and 18th Centuries.* London.

V. Ya. Buniakovsky

I am commenting on his main work (1846). He (Sheynin 1991, p. 208) calculated the probability of the trustworthiness of some witnesses who refuted other witnesses but he contradicted his own premises. While repeating Poisson (1837, § 119), he (p. 210) wrongly maintained that, if the probabilities of just decisions of jurors are the same, the probability of a correct verdict depended on the difference between the votes rather than on the total number of jurors. However, Poisson himself (§ 120) noted that the probability of that difference depended on the number of the jurors.

Bortkevich sharply criticised Buniakovsky's mortality tables and tables on the distribution of the Orthodox population of Russia by ages. Later authors disagreed, whereas Buniakovsky's initial data were inaccurate and incomplete.

Buniakovsky (1846, p. I) first stated that

The analysis of probabilities considers and quantitatively estimates even such phenomena […] *which, due to our ignorance, are not subject to any suppositions.*

But in actual fact he never considered such phenomena and on p. 364 and elsewhere (1866, p. 24) he went back on his opinion.

In 1988, I (Preprint No. 17, Inst. Hist. Nat. Sci. and Technology) reprinted the text of Buniakovsky's newspaper article of 1848 on the dread of cholera and (1991, pp. 216 – 217) repeated my criticisms.

Buniakovsky V. Ya. (1846), *Osnovania Matematicheskoi Teorii Veroiatnostei* (The Principles of the Mathematical Theory of Probability). Petersburg.

--- (1866, in Russian), Essay on the laws of mortality in Russia and on the age distribution of the Orthodox population. *Zapiski Imp. Akad. Nauk*, vol. 8, Suppl. 6. Separate paging.

Sheynin O. (1991), On the work of Buniakovsky in the theory of probability. *Arch. Hist. Ex. Sci.*, vol. 43, pp. 199 – 223.

Poisson S.-D. (1837, 2003), *Recherches sur la probabilité des jugements* etc. Paris. English translation: Berlin, 2013. **S, G,** 53.

N. R. Campbell

Campbell (1928) denied the method of least squares, but he did not even know about the existence of Gauss' memoir of 1823. In a few years he (Eddington 1933, p. 283) called the theory of errors

The last surviving stronghold of those who would reject plain facts and common sense in favour of remote unverifiable guesses.

He was still ignorant.

Campbell N. R. (1928), *An Account of the Principles of Measurement and Calculations*. London.

Eddington A. S. (1933), Notes on the method of least squares. *Proc. Phys. Soc.*, vol. 45, pp. 271 – 287. With discussion.

A. L. Cauchy

At first, Cauchy (1821/1897, p. V) stated that mathematical sciences ought not to go *beyond their boundaries*, but than he (1845/1896, p 242) formulated an opposite opinion.

Cauchy A. L. (1821), *Cours d'analyse de l'Ecole Royale Polytechnique*. *Œuvr. Compl.*, sér. 2, t. 3. Paris, 1897. [Cambridge, 2009.]

--- (1845), Sur les secours que les sciences du calcul peuvent fournir aux sciences physiques ou même aux sciences morales. *Œuvr. Compl.*, sér. 1, t. 9. Paris, 1896, pp. 240 – 252.

Yu. V. Chaikovsky

His paper (2001) abounds with mistakes and doubtful statements. Thus, Chaikovsky invented a Cardano – Bernoulli law of large numbers without even providing a reference to Cardano. If Cardano did indicate some embryo of that law, he still would not have qualified as its co-author.

Chaikovsky also stated that Bernoulli had not known about statistical probability and that therefore (!) he found it in Cardano. Again, nonsense in both cases.

Chaikovsky Yu. V. (2001, in Russian), What is probability? Evolution of this concept from antiquity to Poisson. *Istoriko-Matematicheskie Issledovania*, vol. 6 (41), pp. 34 – 57.

A. S. Chebotarev

Chebotarev (1881 – 1969), a Honoured Science and Technology Worker of the Russian Federation (a Honoured Dinosaur), was the most eminent representative of the old Soviet school of the theory of errors, a non-party Bolshevik with more zeal than sense. Here he is.

1951, p. 7. *An irreconcilable enemy of scientific socialism* [Pearson] was unable *to develop honestly the theoretic or technical problems of a concrete science*.

1951, pp. 8 – 9; 1953, p. 24. Romanovsky dared to write *Probability* […] *is described by* … Chebotarev: Marx, however, insisted that the world ought to be changed rather than described.

1958, p. 579. *For fourteen centuries Ptolemy's system held mankind in spiritual bondage*.

Pearson was not an enemy of socialism, but Lenin had criticised Pearson's general philosophical views and Chebotarev simply wished to outpope the Pope. He was not the only one, see *Smit* and Boiarsky & Zirlin (1947, p. 74): Pearson is the author of *some ideas of a racist nature which for five decades forestalled the Göbbels department*.

Just horrible!

Boiarsky A. Ya., Zirlin L. (1947), Bourgeois statistics as a means for apologizing capitalism. *Planovoe Khoziastvo*, No. 6, pp. 62 – 75.

Chebotarev A. S. (1951, in Russian), On the mathematical treatment of observations. *Trudy Moskovskogo Instituta Inzhenerov Geodezii, Aerofotos'emki i Kartografii*, № 9, pp. 3 – 16.

--- (1953, in Russian), Same title. Ibidem, № 15, pp. 21 – 27.

--- (1958), *Sposob Naimen'shikh Kvadratov* etc. (Method of Least Squares). Moscow.

P. L. Chebyshev

Novikov (2002, p. 330):

In spite of his splendid analytical talent, Chebyshev was a pathological conservative. V. F. Kagan [an eminent geometrician], while being a privat-Dozent, heard *his scornful statement about trendy disciplines such as the Riemann geometry and complex analysis*.

This feature certainly influenced *Markov* and *Liapunov*. And here is Solzhenitsyn (2013, vol. 2, p. 192):

While loving your people, it is necessary to be able to mention our mistakes, and, when necessary, without mercy.

Liapunov wrote down Chebyshev's lectures (1879 – 1880/1936). In spite of the statement of A. N. Krylov, their Editor, Prudnikov (1964, p. 183) maintained that was much more likely Liapunov's text is *fragmentary*. Anyway, we cannot unreservedly say that Chebyshev (p. 214) held that various lotteries are *equally harmless* if the expected winnings are the same and equal the [same] stakes. And overheads and the profit of the organizers should be taken into account.

Chebyshev (pp. 224 – 252) poorly described the mathematical treatment of observations since he obviously did not read Gauss and had not grasped the significance of his final justification of least squares (Sheynin 2009, § 13.2-7).

Chebyshev (pp. 152 – 154) investigated the cancellation of a random fraction, but Bernstein (1928/1964, p. 219) refuted his result (Sheynin 2009, § 13.2-8). On that problem and on the stochastic number theory see Postnikov (1974).

The published text of the *Lectures* contains more than a hundred mathematical mistakes. Ermolaeva (1987) discovered their more detailed text but had not explained what was new there as compared with the Liapunov text. Moreover, that new text remains unpublished which strongly testifies against her.

Chebyshev had not been interested in philosophical problems of probability and dissuaded his students from studying them. This at least was the likely conclusion of Prudnikov (1964, p. 91).

Bernstein S. N. (1928, in Russian), The present state of the theory of probability and its applications. *Sobranie Sochineniy*, vol. 4. Moscow, 1964, pp. 217 – 232. **S, G.** 7.

Chebyshev P. L. (lectures 1879/1880), *Teoria Veroiatnostei* (Theory of Probability). Moscow – Leningrad, 1936. **S, G,** 3.

Ermolaeva N. S. (1987, in Russian), On Chebyshev's unpublished course on the theory of probability. *Voprosy Istorii Estestvoznania i Techniki*, № 4, pp. 89 – 110.

Novikov S. P. (2002, in Russian), The second half of the 20th century and its result … *Istoriko-Matematicheskie Issledovania*, vol. 7 (42), pp. 326 – 356.

Postnikov A. G. (1974), *Veroiatostnaia Teoria Chisel* (Stochastic Number Theory). Moscow.

Prudnikov V. E. (1964, in Russian), *P. L. Chebyshev* etc. Leningrad, 1976.

Sheynin O. (1994), Chebyshev's lectures on the theory of probability. *Arch. Hist. Ex. Sci.*, vol. 46, pp. 321 – 340.

--- (2009), *Theory of Probability. Historical Essay*. Berlin. **S, G,** 10. Russian version: 2013. **S, G,** 11.

Solzhenitsyn A. (2013), *Dvesti let Vmeste* (Together for Two Hundred Years), pt. 2. Moscow.

A. A. Chuprov

His *Essays* (1909 and 1910) were reprinted in 1959 in spite of the author's much earlier refusal (Chetverikov 1968a, p. 51). A dozen or more enthusiastic reviews had appeared including the opinion of Slutsky (1926) whereas Anderson (1957, p. 237, Note 2/1963, Bd. 2, p. 440) indicated that the *Essays greatly influenced Russian statistical theory*. However, no one ever proved this statement.

My opinion (1990/2011, pp. 9 – 10, 11 – 124, 142) is quite different. Markov (1911/1981, p. 151) indicated, fairly enough, that the *Essays lacked that clarity and definiteness that the calculus of probability requires*. A bit earlier, in a letter to Steklov of 5 December 1910, Markov (1991, p. 194) noted that Chuprov made many mistakes (but did not elaborate).

Anderson (1926/1963, Bd. 1, p. 33) approvingly mentioned that two thirds of the *Essays* had already been contained in his candidate composition; we, however, believe that Chuprov should have changed much over 12 or 13 years. And in that composition Chuprov revealed his superficial knowledge and exorbitant self-importance (Sheynin 1990/2011, Chapter 9).

The composition of the *Essays* is unfortunate. The description, verbose in itself, is from time to time interrupted by excessively long quotations from foreign sources (without translation) and in 1959 nothing was changed. In addition, each chapter should have been partitioned into sections. And here are our definite remarks about the *Essays* (1909/1959).

1. Chuprov (pp. 21 – 26) briefly described the history of the penetration of the statistical method into natural sciences and he treated the same subject in two papers (1914; 1922b). I myself had busied myself with that subject for several years and may quite definitely say that Chuprov's efforts were here absolutely insufficient. And his indirect agreement (p. 26) with the opinion that in the history of the theory of probability Pearson occupies the next place after Poisson is wrong: where are Chebyshev, Markov and Liapunov? And why the theory of probability rather than mathematical statistics?

2. A prominent place in the *Essays* is devoted to the plurality of causes and actions. True, the differential and integral *forms of the law of causality,* which were essential in Chuprov's candidate composition (Sheynin 1990/2011, p. 110), are lacking in the *Essays* as well as in his papers (1905; 1906). But, anyway, what kind of law was it if only described qualitatively? That law remained in the *Essays* although only in the Contents. And correlation is not mentioned there at all.

3. Also essential in the *Essays* was the separation of sciences according to Windelband and Rickert into ideographic (historical, the description of reality) and nomographic (natural-scientific, the description of regularities). Note that in English both these terms are applied in other senses.

At the end of his life, Chuprov (1922a) returned to idiographic descriptions, and we therefore stress that, first, in the history of philosophy Windelband and Rickert are lesser figures whereas they are never mentioned in the history of probability and statistics. Second, we may safely abandon ideographic sciences and replace them by the numerical method (Louis 1825). Louis calculated the frequencies of the symptoms of various diseases to assist diagnosing.

Third, already Christian von Schlözer, the son of his eminent father, correctly remarked that only narrow-minded people believe that history is restricted by description of facts and does not need general principles (Sheynin 2014/2016, p. 18).

Now, Chuprov (p. 50), and clearer in a review (1922a), expressed an interesting idea abut the inevitable rebirth of the university statistics, although *in a modern haircut*. And he (pp. 50 – 51) also stressed the impossibility of restricting statistics to idiographic descriptions. This, however, became clear about 70 years previously, see *Fourier*.

At least in Germany university statistics was never forgotten. Nowadays, unlike the olden times, it happily applies numerical data and quantitative considerations (which was possibly what Chuprov had in mind).

4. Chuprov discussed induction as one of his main subjects but did not mention Bayes, did not numerically consider the strengthening of induction with the number of observations confirming a certain event.

5. Chuprov paid too little attention to randomness which was actually recognized by the most eminent scholars, Kepler and Newton.

6. Chuprov clearly indicated that the Lexian theory was insufficiently justified, but even in the concluding theses (p. 302) he unconditionally accepted the so-called law of small numbers (Bortkiewicz 1898) which was directly connected with that theory.

7. On p. 166 Chuprov absolutely wrongly stated that Cournot (1843) had proved the law of large numbers *in a canonical form*. Cournot did not prove it in any form.

8. The title of the *Essays* is strange since he (p. 20) acknowledged that

A clear and rigorous theoretical justification of the statistical science is still urgently necessary.

Later, Chuprov repeatedly returned to the Lexian theory and finally abandoned it in 1921. In Letter 151 of 20 January of that year he (Bortkevich & Chuprov 2005) expressed his desire to *do away absolutely with it* (Bortkiewicz categorically disagreed.) And in a letter of 30 January to Gulkevich he (2009) indicated that the [Lexian] *theory of stability is essentially based on a mathematical misunderstanding*.

Chetverikov (Chuprov 1960, Introductory remarks) maintained that Chuprov's philosophical reasoning was timely. Nevertheless, statistics could have simply disregarded, and actually did disregard, the outdated views prevalent, say, in England. Indeed, suppose that the *Essays* were almost at once translated into English; would the Biometric school get rid of its one-sided direction under the influence of the *Essays*? Certainly not, it would have advanced on its own (as it actually happened). And the two papers written by Chuprov in German (1905; 1906) changed nothing in German statistics.

As to logic, Chuprov even in 1923 wrote to Chetverikov (Sheynin 1990/2011, p. 122) that, just as in 1909, he saw

No possibility of throwing a formal logical bridge across the crack separating frequency from probability.

He never mentioned the strong law of large numbers about which he certainly knew (Slutsky 1925, p. 2) and did not therefore recognize that mathematics was here much more important than logic.

Chuprov did not agree to publish a third edition of his *Essays*, see above, and Chetverikov (1968b, p. 5) thought that he was unsatisfied with the theory of stability of statistical series as described above. But was he satisfied with all the rest? Indeed, in Letter 162 of 1921 he (Bortkevich & Chuprov 2005) remarked that *during the latest years*, he had *turned aside* from philosophy to mathematics. Quite possibly, from logic as well, and that process had certainly been occasioned by his correspondence with Markov of 1910 – 1917.

Chuprov studied problems in a nonparametric setting, and his contributions necessarily contain many complicated formulas which no one or almost no one ever attempted to check. Considering his formulas of the theory of correlation, Romanovsky (1938, p. 416) remarked: *being of considerable theoretical interest*, they are *almost useless* due to the involved complicated calculations. And (p. 417): the estimation of the empirical coefficient of correlation for samples from arbitrary populations was possible almost exclusively by Chuprov's formulas which were however *extremely unwieldy*, […] *incomplete and hardly studied*. See also Romanovsky (1926, p. 1088).

Many years previously, it was Chuprov (Sheynin 1990/2011, pp. 72 and 73), who noticed serious mistakes in Romanovsky's early work of 1923 and 1924 …

Chuprov's notation was often really bad, although their improvement was sometimes easily done, for example, by introducing Greek letters. But who will ever look twice on his five-storeys monster (1923, p. 472), a formula with two super- and two subscripts?

Anderson O. (1926, in Bulgarian), Zum Gedächtnis an … A. A. Tschuprow … In author's book (1963), *Ausgewählte Schriften*, Bde 1 – 2, Bd. 1. Tübingen, pp. 12 – 27.

--- (1957), Induktive Logik und statistische Methode. *Allg. stat. Archiv*, Bd. 41, pp. 235 – 241. Ibidem, Bd. 2, pp. 938 – 944.

Bortkevich V. I., Chuprov A. A. (2005), *Perepiska* (Correspondence) *1895 – 1926*. Berlin. **S, G,** 9.

Bortkiewicz L. von (1898), *Das Gesetz der kleinen Zahlen.* Leipzig.

Chetverikov N. S. (1968a, in Russian), Notes on the work of W. Lexis. In author's book (1968b, pp. 39 – 54).

--- (1968b), *O Teorii Dispersii* (On the Theory of Dispersion). Moscow.

Chuprov A. A. (1905), Die Aufgabe der Theorie der Statistik. *Schmollers Jahrb. f. Gesetzgebung, Verwaltung u. Volkswirtschaft im Dtsch. Reich*, Bd. 29, No. 2, pp. 421 – 480.

--- (1906), Statistik als Wissenschaft. *Arch. f. soz. Wiss. u. soz. Politik*, Bd. 5 (23), No. 3, pp. 647 – 711.

--- (1909), *Ocherki po Teorii Statistiki* (Essays on the Theory of Statistics). Moscow, 1959. Third edition.

--- (1914, in Russian), The law of large numbers in contemporary science. In Ondar (1977/1981, pp. 164 – 181).

--- (1922a, review), E. Zizek (1921), *Grundriß der Statistik*. München – Leipzig. *Nordisk Statistisk Tidskrift*, Bd. 1, 1922, pp. 329 – 340.

--- (1922b), Das Gesetz der großen Zahlen und der stochastisch-statistische Standpunkt in der modernen Wissenschaft. *Nordisk Statistisk Tidskrift*, Bd. 1, No. 1, pp. 39 – 67.
--- (1923), On the mathematical expectation of the moments of frequency distributions in the case of correlated observations. *Metron*, t. 2, No. 3, pp. 461 – 493; No. 4, pp. 646 – 683.
--- (1960), *Voprosy Statistiki* (Issues in Statistics). Moscow.
--- (2009), *Pisma* (Letters to) *K. N. Gulkevich, 1919 – 1921*. Berlin. Publication by G. Kratz, O. Sheynin, K. Wittich. **S, G,** 28
Louis P. C. A. (1825), *Recherches anatomico-pathologiques sur la phtisie.* Paris.
Markov A. A. (1910, in Russian), Letter to V. A. Steklov. *Nauchnoe Nasledstvo*, vol. 17. Leningrad, 1991.
--- (1911, in Russian), On the basic principles of the calculus of probability etc. In Ondar (1977/1981, pp. 149 – 153).
Ondar Kh. O., Editor (1977, in Russian), *The Correspondence between A. A. Markov and A. A. Chprov* etc. New York, 1981.
Romanovsky V. P. (1926), On the distribution of the arithmetic mean in series of independent trials. *Izvestia Akad. Nauk SSSR*, ser. 6, vol. 20, No. 12, pp. 1087 – 1106.
--- (1938), *Matematicheskaia Statistika*. Moscow – Leningrad.
Slutsky E. E. (1925, in Russian), On the law of large numbers. *Vestnik Statistiki*, № 7 – 9, pp. 1 – 55.
--- (1926), A. A. Tschuprov. *Z. angew. Math. Mech.*, Bd. 6, pp. 337 – 338.
Sheynin O. (1990, in Russian), *Alexandr A. Chuprov: Life, Work, Correspondence.* V&R Unipress, 2011.
--- (2009), *Theory of Probability. Historical Essay*. Berlin. **S, G,** 10. Later Russian version: (2013): **S, G,** 11.
--- (2014, in Russian), On the history of university statistics. *Silesian Stat. Rev.,* No. 14 (18), 2016, pp. 7 – 25.

R. Clausius

Clausius (1889 – 1891, p. 71) offered a proof of the equality $E(\xi/E\xi) = 1$ for the velocities ξ of molecules. This shows that the notion of expectation was then not yet sufficiently studied. A similar statement was due to Newcomb and Holden (1874, pp. 270 – 271). For a systematic error s and (independent) random errors r_1 and r_2 they proved, although only for the normal distribution, that

$$E[(s + r_1)(s + r_2)] = s^2.$$

Clausius R. (1889 – 1891), *Die kinetische Theorie der Gase.* Braunschweig.
Newcomb S., Holden E. S. (1874), On the possible periodic changes of the Sun's apparent diameter. *Amer. J. Sci.*, ser. 3, vol. 8 (108), pp. 268 – 277.

A. Comte

Comte (1830 – 1842/1877, t. 2, p 255; Ibidem, t. 3, № 40, p. 329; 1854, p. 120) denied the theory of probability:

The philosophical notion which underlies the calculus of probability […] *is fundamentally wrong.*

The application of this calculus for improving social sciences is illusory whereas the concept of probability *is only suitable if at all for games of chance*. Common sense better indicates useful applications.

The imaginary application of what is called statistics to medicine […] *leads to an essential and direct degeneration of that science which is then reduced to simple enumerations*.

The efforts of geometers to elevate the calculus of probability above its natural applications are useless.

The first excerpt hints at a scornful attitude towards statistics which reveals regularities in social life whereas Comte attempted to establish tendencies of its development (Sheynin 1986, § 3.1).
A. V. Vasiliev (Bazanov 2002, p. 131) positively estimated Comte's general views on mathematics.

Bazanov V. A. (2002, in Russian), Professor A. V. Vasiliev. *Istoriko-Matematicheskie Issledovania*, vol. 7 (42), pp. 120 – 148. Bazanov compiled a list of 192 Vasiliev's contributions, and his name was printed 192 times. More zeal than sense!

Comte A. (1830 – 1842), *Cours de philosophie positive*. Paris, 1877, tt. 2 – 3.4.

--- (1854), *Système de philosophie positive*, t. 4. Appendice général. Paris.

Sheynin O. (1986), Quetelet as a statistician. *Arch. Hist. Ex. Sci*., vol. 36, pp. 281 – 325.

M. J. A. N. Condorcet

After considering Condorcet's stochastic reasoning, Todhunter 1865, p. 352) concluded:

In many cases it is almost impossible to discover what Condorcet means to say.

In a letter of 1772 to Turgot Condorcet (Henry 1883/1970, pp. 97 – 98) remarked that he is *amusing* himself by calculating probabilities and that he is keeping to D'Alembert's convictions. A telling statement!

Condorcet compiled antiscientific eulogies of Daniel Bernoulli and Euler (Sheynin 2009). Here is an episode described by him. Two students of Euler calculated 17 terms of some complicated series, but their results differed by a unity in the 50th decimal place (apparently, in the 5th place) and the blind Euler checked their calculation. (And who checked him?) A new labour of Heracles! Strangely enough, Pearson (1978, p. 251) described this episode but did not comment.

Condorcet (Date unknown, p. 65) maintained that Huygens rather than Pascal (Fermat was not mentioned) was the forefather of probability since his treatise was published first. Nevertheless, correspondence of that period is considered on a par with publications, and Condorcet's statement is of no consequence.

Huygens died in 1695, so the date of Condorcet's eulogy was ca. 1697.

Condorcet M. J. A. N. (*), Eloge d'Huygens. *Oeuvr*., t. 2. Paris, 1847, pp. 54 – 72.

Henry M. Ch. (1883), *Correspondance inédite de Condorcet et de Turgot*. Genève, 1970.

Pearson K. (1978), *History of Statistics in the 17th and 18th Centuries*. London.

Sheynin O. (2009), *Portraits. Euler, D. Bernoulli, Lambert*. Berlin. **S, G,** 39.

Todhunter I. (1865), *History of the Math. Theory of Probability*. New York, 1949, 1965.

Fr. Corbaux

Corbaux (1833, pp. 170 – 172) had somehow compiled separate mortality tables for men and women. Furthermore, he separated each table into five parts according to the property status of the various strata of population. How did he establish these strata is only evident for annuitants (one of those five parts), and I suspect that he fabricated everything. Quetelet & Smits (1832, p. 33) noted that separate mortality tables for men and women only *recently* began to appear, but the idea of such separation had certainly been conceived much earlier.

Courbaux Fr. (1833), *On the Natural and Mathematical Laws concerning Population, Vitality and Mortality*. London.

Quetelet A., Smits Ed. (1832), *Recherches sur la reproduction et la mortalité de l'homme*. Bruxelles.

R. Cotes

Cotes (1722), see Gowing (1983, p. 107), was the first to consider the adjustment of direct measurements. Without any justification, he recommended the weighted arithmetic mean, which he compared with the centre of gravity of the system of the points of measurement, as the *most probable* estimator of the constant sought. He had not explained the rule of weighing or the meaning of *most probable*, but his authority apparently supported the existing common feeling. Laplace (1814/1995, p. 121) stated that *all calculators* followed him.

Cotes R. (1722), Aestimatio errorum in mixta mathesis etc. *Opera misc*. London, 1768, pp. 10 – 58.

Gowing R. (1983), Roger Cotes – Natural Philosopher. London.

Laplace P. S. (1814, in French), *Philosophical Essay on Probabilities*. New York, 1995.

A. A. Cournot

Cournot (1843) intended his book for a broader circle of readers. However, not being endowed with good style and evidently attempting to avoid formulas, he had not achieved his goal. And in Chapter 13 he had to introduce terms of spherical astronomy and formulas of spherical trigonometry.

Cournot had not mentioned the law of large numbers (denied by his friend Bienaymé) although considered it in his paper of 1838. He obviously did not read Gauss and was never engaged in precise measurements, and his Chapter 11 devoted to measurements and observations is almost useless.

Then, according to the context of his book, Cournot should have mentioned the origin of stellar astronomy (William Herschel), the study of smallpox epidemics (Daniel Bernoulli) and the introduction of isotherms (Humboldt), but all that was missing. The description of tontines (§ 51) is at least doubtful, and the Bayes approach and the Petersburg game are superficially dealt with (§§ 88 and 61). Philosophical probabilities which Cournot introduced had appeared a bit earlier (Fries 1842, p. 67), see Krüger (1987, p. 67).

Thierry (1994; 1995) exaggerated Cournot's merit. Yes, Cournot introduced disregarded probabilities, but they had actually been present in the Descartes moral certainty (1644/1978, pt. 4, No. 205, 483, p. 323), see also *Buffon*. Then, Thierry ignorantly stated that, by insisting (just as Poisson did) on the difference between subjective and objective probabilities, Cournot had moved the theory of probability from applied to pure science.

Cournot A. A. (1843), *Exposition de la théorie des chances et des probabilités*. Paris, 1984. B. Bru, the editor of the second edition, compiled thorough bibliographic comments. English translation: **S, G,** 54.

Descartes R. (1644, in Latin), *Principes de la philosophie*. *Oeuvr.*, t. 9, No. 2. Paris, 1978.

Fries J. F. (1842), *Versuch einer Kritik der Prinzipien der Wahrscheinlichkeitsrechnung*. Braunschweig. *Sämtl. Schriften*, Bd. 14, pp. 1 – 236. Aalen, 1974.

Krüger L. (1987), The slow rise of probabilism etc. In L. Krüger et al, Editors, *Probabilistic Revolution*, vol. 1. Cambridge (Mass.) – London, pp. 59 – 89.

Thierry M. (1994), La valeur objective du calcul des probabilités selon Cournot. *Math. inf. sci. hum.*, No. 127, pp. 5 – 17.

--- (1995), Probabilité et philosophie des mathématiques chez Cournot. *Rev. hist. math.*, t. 1, No. 1, pp. 111 – 138.

E. Czuber

Czuber was a most eminent representative of statistics of his time, but he hardly furthered statistics (except his study of mortality) or probability, and he did not recognize sampling (see *Mendeleev*).

Czuber (1884) gave thought to geometric probability, but his solution of one of the problems there (p. 11) was thoughtless. Two random points, M and N, are situated on interval AB. It was required to determine the probability of MN > NA. He had not noticed that MN can be replaced by NM so that the answer was obvious.

Czuber E. (1884), *Geometrische Wahrscheinlichkeiten und Mittelwerte*. Leipzig.

A. A. Dale

His book (1991/1999), just as all of his contributions, abounds in epigraphs lacking indication of sources and often useless. Dale is also fond of only referring to the first (and often rare) editions of books, and he quotes French, German and even Latin texts without translation. All this means ostentation. The same wrong attitude of only referring to first editions is seen in Loveland (2001).

His translation of Laplace (1814/1995) is accompanied by notes, bibliography and glossary. Some of his notes (for example, on the Petersburg game and the Daniel Bernoulli – Laplace – Ehrenfests model) do not consider modern results and the glossary includes ignorant explanations of the terms *triangulation* and *repeating theodolite*.

Dale's book (2003) includes much material pertaining to general history, ethics and theology, and some commentaries are unnecessary. The biography of Bayes is too detailed and diffused and a bibliography of his works is lacking. Many Latin quotes are left without translation, but some places from Newton's *Principia* are both translated (apparently, by Dale himself) and provided in the original Latin, obviously for his own pleasure. And it is unclear what is new in this book as compared with Dale's previous papers.

Dale A. A. (1991), *History of Inverse Probability from Thomas Bayes to Karl Pearson*. New York, 1999.

--- (2003), *The Most Honourable Remembrance. The Life and Work of Thomas Bayes*. New York.

Laplace P. S. (1814, in French), *Philosophical Essay on Probabilities*. New York. Translated by A. A. Dale from the edition of 1825.

Loveland J. (2001), Certainty of sunrise and the probabilistic reductio ad absurdum. *Arch. Hist. Ex. Sci.*, vol. 55, pp. 465 – 477.

J. D’Alembert

D’Alembert (1768a, pp. 254 – 255) distinguished physical and mathematical probabilities; thus, he alleged that after one of the two contrary events had occurred several times in succession, the appearance of the other one becomes physically more probable. Known best of all, however, is his absurd statement (1754) that the occurrence of heads in two tosses of a coin has probability 1/3 rather than 1/4. At the same time he denied the difference between the mean and the probable duration of life and even considered its existence as an (additional) argument against the theory of probability itself (1768b). In general, D’Alembert (1768c, pp. 309 − 310) did not consider the theory of probability *a precise and true calculus either in its principles or results*.

In a private letter of 1763 Euler (Juskevic et al 1959, p. 221) noted that D’Alembert *most shamelessly defends all his mistakes*. And here are his statements about medicine (1759/1821, p. 163):

Systematic medicine seems […] *to be a real scourge of mankind. Sufficiently numerous observations, more detailed and better agreeing one with another is* […] *what the reasoning in medicine should be reduced to*.

And (p. 167) a physician is *a blind man* and with his stick he hits either the illness, or the ill man. *We ought to consult the physician who least trusts medicine.* In 1759 these statements were lacking, and D’Alembert died in 1783.

Daniel Bernoulli (1768) was indignant at the *extremely vulgar reasoning of the great D’Alembert* about the theory of probability. *Too often* he speaks about me *unjustly*, criticizes my memoir [of 1766 dedicated to the prevention of smallpox] *in most different ways, all of them equally ridiculous*.

D’Alembert had criticized that memoir before it was published which was highly improper, but not ridiculously (Dietz & Heesterbeek 2002, pp. 12 − 13).

The following has no connection with D’Alembert’s unworthy opinion about probability theory, but it concerns that theory in general.

M. A. Tikhomandritsky (1898, p. iv) testified that in 1887 he had shown Chebyshev his *course* (which I am unable to identify) and the latter

Stated that […] *it is necessary to transform the entire theory of probability*.

He did not elaborate and nothing more is known about that statement. Then, in our time, Tutubalin (1971, p. 59) expressed his opinion about the modern theory of probability:

In probability theory, only very small (as compared, for example, with physics) groups of authors refer to each other. This means that the interest has narrowed which was largely caused by its unwieldy mathematical machinery and which is a typical sign of degeneration.

For good measure Tutubalin (p.60) added:

Limit theorems are usually rather decently formulated, but as a rule their proofs are helplessly long, difficult and entangled. Their sole *raison d'être consists in obtaining comparatively simple stochastic distributions possibly describing some real phenomena.*

Bernoulli Daniel (1768, in Latin), Letter to Euler. *Priroda*, № 5, 1982, pp. 103 − 104. In Russian.
D'Alembert J. Le Rond (1754), Croix ou pile. *Enc. ou dict. raisonné des sciences, des arts et des métiers*, t. 4, pp. 512 – 513. Stuttgart, 1966.
--- (1759), *Essai sur les éléments de philosophie*. *Œuvr. Compl.*, t. 1, pt. 1. Paris, 1821, pp. 116 – 348.
--- (1768a), Doutes et questions sur le calcul des probabilités. *Mélanges de littérature, d'histoire et de philosophie*, t. 5. Amsterdam, pp. 239 – 264.
--- (1768b), Sur la durée de la vie. *Opusc. math*., t. 4. Paris, pp. 92 – 98.
--- (1768c), Sur le calcul des probabilités. Ibidem, pp. 283 – 310.
Dietz K., Heesterbeek J. A. P. (2002), D. Bernoulli's epidemiological model revisited. *Math. Biosciences*, vol. 180, pp. 1 – 21.
Juskevic (Youshkevich) A. P. et al, Editors (1959), *Die Berliner und die Petersburger Akademie der Wissenschaften in Briefwechsels Eulers*, Bd. 1. Berlin.
Tutubalin V. N. (1977, in Russian), *Granitsy Primenimosti. Veroiatnostno-statisticheskie Metody i Ich Vozmozhnosti* (The Boundaries of Applicability. Stochastic Methods and Their Possibilities). Moscow. **S, G,** 45.

Lorraine Daston

Apart from her main subject, Daston (1994) discussed the dialectics of randomness and determinism and wrongly stated that De Moivre denied chance and that Laplace was a *staunch determinist*.

On the contrary, De Moivre considered the separation of chance from Divine Design (from regularity) as the main goal of his *Doctrine of Chances*, and effectively said so in the Dedication of his book to Newton. Laplace maintained that determinism would have only been inherent for an imaginary omniscient intellect. In 1756, Maupertuis, and Boscovich, in 1758, tentatively kept to the future non-existing Laplacean determinism but both disclaimed any possibility of total determinism (Sheynin 2009, § 7.3).

Daston was one of the authors of a book named *Empire of Chance*! See *Gigerenzer*.

Daston Lorraine (1994), How probabilities came to be objective and subjective. *Hist. Math.*, vol. 21, pp. 330 – 344.
Sheynin O. (2009), *Theory of Probability. Historical Essay*. Berlin. **S, G,** 10.

P. Dedekind

Dedekind (1860/1930, p. 97) noted that the Gauss formula of the most probable (of the mean) sample variance was not applicable when the numbers of the unknowns and observations coincided. This was obvious; more important, however, was that the title of Dirichlet's note mentioned the method of least squares whose application was not necessary for the validity of the Gauss formula.

Dedekind P. (1860), Über die Bestimmung der Präzision einer Beobachtungsmethode nach der Methode der kleinsten Quadrate. *Ges. math. Werke*, Bd. 1. Braunschweig, 1930, pp. 95 – 100.

A. De Morgan

De Morgan (1864) uttered incomprehensible statements about the appearance of negative probabilities and probabilities exceeding unity. In a letter of 1842 (Sophia De Morgan 1882, p. 147) he mentioned that $\tan\infty = \cot\infty = \pm\sqrt{-1}$. How on earth did he allow himself such nonsense?

De Morgan A. (1864), On the theory of errors of observation. *Trans. Cambr. Phil. Soc.*, vol. 10, pp. 409 – 427.
De Morgan Sophia (1882), *Memoir of Augustus De Morgan*. London.

W. Derham

Derham (1713) maintained that there existed a negative correlation between the duration of the life of animals and their fecundity. Pearson (1978, pp. 290 – 294) refuted that statement by mentioning the deer, the cow and the dog. Derham also stated that the duration of the life of man had been shortening with the increasing density of population. He adduced tables of the population in Europe and its main cities but did not indicate his sources. Concerning the influence of the Biblical commandment to fill the Earth on statistical investigations see *Struyck*.

Derham W. (1713), *Physico-Theology*. London, 1768.
Pearson K. (1978), *History of Statistics in the 17th and 18th Centuries*. London.

A. Desrosières

His book (1998) is riddled by mistakes. Thus, Poisson introduced the strong law of large numbers and Gauss derived the normal distribution as the limit of the binomial. The author had not attempted to delimit the field of statistical applications and his *large numbers* meant that small samples do not belong to statistics. See my review: *Isis*, vol. 92, 2001, pp. 184 – 185.

Desrosières A. (1998), *The Politics of Large Numbers*. Cambridge (Mass.) – London.

J. De Witt

When determining the cost of life annuities, DeWitt (1671) identified four age groups and supposed that the probability of death increases in a definite manner from a group to the next one, but remains constant inside each of them. However, Eneström (1896/1897, p. 66) noted that the chosen probabilities of death contradicted De Witt's calculations.

De Witt J. (1671), Value of life annuities in proportion to redeemable annuities. In Hendriks F., Contributions to the history of insurance. *Assurance Mag*., vol. 2, 1852, pp. 232 – 249. French translation (1937): *Verzerkerings-Archief*, t. 18, pp. 41 – 85.

Eneström G. (1896), Sur la méthode de J. de Witt etc. *Archief voor de verzerkerings-wetenschap*, t. 3, 1897, pp. 62 – 68.

C. Dickens

In his book *Hard Times*, 1854, Dickens justly argued against those who see nothing except numbers and mean values, but he also thought that statistics eclipses moral issues.

Bailey M. (2007), Hard times and statistics. *Brit. Soc. Hist. Math.*, vol. 22, No. 2, pp. 92 – 103.

A. S. Eddington

Eddington (1933, pp. 275 – 276 and 271 – 272) claimed to have justified the method of least squares

Without postulating a Gaussian error law, provided […] *that the method is not concerned with most probable values*. […] *The proof from the principle of the arithmetic mean is altogether fallacious*.

Eddington was thus ignorant of the second substantiation of this method by Gauss. And he had not justified his second, and strange, statement.

Eddington A. S. (1933), Notes on the method of least squares. *Proc. Phys. Soc.*, vol. 45, pp. 271 – 287. With discussion.

F. Y. Edgeworth

His portrait is lacking in his collected works (1996) and neither does it include a complete bibliography of his works. There are many figures but seven of them are simply black rectangles and the distribution of the items among the three volumes is often unfortunate.

Edgeworth (1908; vol. 1, p. 62) did not agree with Gauss' second justification of least squares, did not think that the Poisson law of large numbers generalizes the Bernoulli theorem (1906; vol. 1, p. 403) and greatly belittled the merits of Chebyshev, and, by implication, Markov and Liapunov (1922; vol. 1, p. 156).

He was too quaint and original and his works were not therefore sufficiently perceived, but he paved the way for a prompt recognition of the Biometric school.

Edgeworth F. Y. (1996), *Writings in Probability, Statistics and Economics*, vols 1 – 3. Cheltenham, UK. Editor McCann C. R., Jr.

A. Einstein

Einstein apparently never came to believe in a stochastic picture of the microcosm (Feynman 1963, vol. 1, pt. 1, Chapter 6, p. 15):

Our most precise description of nature must be in terms of probabilities. There are some people who do not like this way of describing nature. They feel somehow that if they could only tell what is really going on with a particle, they could know its speed and position simultaneously. In the early days of the development of quantum mechanics, Einstein was quite worried about this problem. He used to shake his head and say, "But surely God does not throw dice in determining how electrons should go!" He worried about that problem for a long time and he probably never really reconciled himself to the fact that this is the best description of nature that one can give.

See also his letter to Born of 7 September 1944 (Born 1969, pp. 204 and 235).

Born M. (1969), *Briefwechsel 1916 – 1955*. München.
Feynman R. P. et al (1963), *Lectures on Physics*. München – Wien.

I. Ekeland

His book (2006) contains many absurdities. He compares a chaotic path with a game of chance; he somehow understands the evolution of species as a tendency toward some kind of equilibrium between them and does not mention Mendel. In 1752, Chevalier d'Arcy discovered that in a certain case the light did not pick the shortest path, and, according to the context, Ekeland somehow connects this fact with the principle of least action. He refuses to study randomness and does not mention the regularity of mass random events and he compares chaos with a game of chance. Finally, bibliographic information is poor.

In a previous book (1993, p. 158) he states, without any qualification remarks, that *the normal law appears wherever we collect measurements*.

Ekeland I. (1993), *The Broken Dice and Other Math. Tales of Chance*. Chicago.
--- (2006), *The Best of All Possible Worlds*. Chicago – London.
Sheynin O. (2011), Review of Ekeland (2006). *Almagest*, vol. 2, pp. 146 – 147.

L. Euler

Probability theory forms an insignificant part of Euler's work and is almost absent in his popular *Lettres à une princesse d'Allemagne* of 1768 – 1772. Perhaps he was not specifically interested in that theory.

When discussing Daniel Bernoulli's memoir of 1778, the practically blind Euler (1778) misunderstood how D. B. weighed the observations and suggested to return to the arithmetic mean as the parameter of location. However, coupled with his additional desirable condition, he should have recommended the median, see *Daniel Bernoulli*.

Euler did not further the adjustment of indirect observations either. In one case he (1755), the grand master of calculations, had not introduced approximate values of his unknowns and had to deal with numbers with many significant figures. Elsewhere, Euler (1770, p. 207) tacitly assigned equal weights to observations of obviously unequal precision. See also *Ivory*.

Euler prepared the important mathematical part of one of the chapters of Süssmilch's *Göttliche Ordnung*, see *Süssmilch* (Sheynin 2007, pp. 300 – 301). Nevertheless he also introduced fanciful results. Thus, by introducing arbitrary and oversimplified assumptions Euler compiled a table of the population of the Earth for 900 years beginning with Adam and Eve. One of his assumptions: the period required for doubling the population increased in time. In that chapter, he provided three tables all of which contained insignificant errors.

Euler L. (1755), Eléments de la trigonométrie spheroidique tires de la méthode des plus grands et plus petits. *Opera Omnia*, ser. 1, t. 27. Zürich, 1954, pp. 309 – 339.

--- (1770), Expositio methodorum, cum pro determinanda parallaxi solis …Ibidem, ser. 2, t. 30. Zürich, 1964, pp. 153 – 231.

--- (1778, in Latin), Observations on the foregoing dissertation of Bernoulli. *Biometrika*, vol. 48, 1961, pp. 3 – 13; E. S. Pearson & M. G. Kendall, Editors (1970), *Studies in the History of Statistics and Probability*. London, pp. 155 – 172. This is a translation of both D. B. and Euler.

Sheynin O. (2007), Euler's work in probability and statistics. In *Euler Reconsidered. Tercentenary Essays*. Heber City, UT. Editor R. Baker, pp. 281 – 316.

Süssmilch J. P. (1741), *Die Göttliche Ordnung*. Berlin, 1765. Several later editions.

G. T. Fechner

Fechner (1855/1864) missed the opportunity to comment on the emerging kinetic theory of gases. He (1874, pp. 7 and 9; 1897, p. 15) repeatedly treated physics on a par with practical astronomy: he stated that both had to do with symmetric distributions and true values of magnitudes sought. His mathematical approach was primitive, and almost all of his results had to be repeated on a much higher level. Ebbinghaus (1908, p. 11) called Fechner *a philosopher full of phantasies*. His style was not good enough: his sentences were up to sixteen lines long.

Fechner (1897) attempted to discover the non-existing general asymmetric distribution of observational errors in natural science. When solving a system of redundant observations with two unknowns, he separated it into all possible groups of two equations each and averaged the partial solutions. Elsewhere, Fechner (1887, p. 217) he stated that that method asymptotically approached the method of least squares; actually, as it was already known, leads to the same result if those pairs of equations were properly weighed.

Ebbinghaus H. (1908), *Abriss der Psychologie*. Leipzig.

Fechner G. T. (1855), *Über die physikalische und philosophisches Atomlehre*. Leipzig, 1864.

--- (1874), Über das Ausgangswert der kleinsten Abweichungssumme … *Abh. Kgl. Sächs. Ges. Wiss.,* Bd. 18 (Bd. 11 of the Math.-Phys. Kl.), No. 1, pp. 3 – 76.

--- (1887), Über die Methode der richtigen und falschen Fälle. *Abh. Kgl Sächsische Ges. Wiss.*, Bd. 13 (22), pp. 109 – 312.

--- (1897), *Kollektivmasslehre*. Leipzig. Editor and actual co-author G. F. Lipps.

Sheynin O. (2004), Fechner as a statistician. *Brit. J. Math. Stat. Psychology*, vol. 57, pp. 53 – 72.

Jacqueline Feldman, G. Lagneau, B. Matalon

According to the aim of their collection (1991), one or a few of the items should have discussed astronomy and meteorology, but these sciences are not mentioned. Neither is Snow, who, in 1855, discovered the way of the spreading of cholera by comparing two means, see the title of this collection. On p. 70 Simpson is wrongly called De Moivre's student, and (p. 85) Süssmilch rather than Graunt and Petty is considered the creator of political arithmetic. The alleged incompetence of Euler in statistics, a statement likely borrowed from *Stigler*, (p. 69) is wrong.

Jacqueline Feldman, G. Lagneau, B. Matalon, Editors (1991), *Moyenne, milieu, centre. Histoire et usages*. Paris.

J. V. Field

Field (2005) wrongly concluded that the notion of observational error only appeared in astronomy *between* Tycho and Kepler. All the previous astronomers were stupid! Again, she was obviously not well versed in the treatment of observations. This fault is usual for modern astronomers. For example, in 1992, W. Donahue, the translator of

Kepler’s Latin *New Astronomy*, did not comment on Kepler’s treatment of his observations.

Field J. V. (2005), Tycho Brahe, Johannes Kepler and the concept of error. *Festschrift for Volker Bialas*. München, pp. 143 – 155.

R. A. Fisher

The investigations made by Fisher, the founder of the modern British mathematical statistics, were not irreproachable from the standpoint of logic. The ensuing vagueness in his concepts was so considerable, that their just criticism led many scientists (in the Soviet Union, Bernstein) to deny entirely the very direction of his research (Kolmogorov 1947, p. 64).

Fisher was barely acquainted with the theory of errors. He (1925/1990, p. 260) stated that the method of least squares was *a special application of the method of maximal likelihood in the case of normal distribution*. He (1939, p. 3; 1951, p. 39) wrongly maintained that the Gauss formula of the sample variance was due to Bessel. And he much too strongly criticised Pearson (Sheynin 2010, p. 6).

Fisher R. A. (1925), *Statistical Methods for Research Workers*. In author's *Statistical Methods* (1973), *Experimental Design and Scientific Inference*. Oxford, 1990.

--- (1939), "Student". *Annals Eug.*, vol. 9, pp. 1 – 9.

--- (1951), Statistics. In *Scientific Thought in the 20th Century*. Editor A. E. Heath. London, pp. 31 – 55.

Kolmogorov A. N. (1947, in Russian), The role of Russian science in the development of the theory of probability. *Uchenye Zapiski Mosk. Gos. Univ.*, No. 91, pp. 53 – 64. **S, G,** 7.

Sheynin O. (2010), Karl Pearson. A centenary and a half after his birth. *Math. Scientist*, vol. 35, pp. 1 – 9.

J. Flamsteed

His relations with Newton and Halley had been complicated ((Thoren 1972; Sheynin 1973, pp. 109 – 110) since he invariably wished to improve his observations and was loath to publish them. He did not consider the arithmetic mean as a standard estimator and did not enter many of his observations in any of his manuscript catalogues (Baily 1835, p. 376). Bradley (1748, p. 24) sometimes did not select that mean, but rather preferred *that observation which best agreed with it*.

Baily F. (1835), *Account of the Rev[d] J. Flamsteed*. London.

Bradley J. (1748), Letter … concerning an apparent motion observed in some of the fixed stars. *Phil. Trans. Roy. Soc.*, vol. 45, pp. 1 – 43.

Thoren V. E. (1972), Flamsteed. *Dict. Scient. Biogr.*, vol. 5, pp. 22 – 26.

Sheynin O. (1973), Mathematical treatment of astronomical observations. *Arch. Hist. Ex. Sci.*, vol. 11, pp. 97 – 126.

A. T. Fomenko

After studying Ptolemy's star catalogue, Efremov & Pavlovskaia (1987; 1989) stated that the events (not only scientific) which are attributed to antiquity, actually appeared in 900 – 1650. See also Fomenko et al (1989).

They should have compiled *beforehand* a list of important ancient events and studied each from the standpoint of chronology.

Later, Nosovsky & Fomenko (2004) somehow decided that Jesus was the tsar of the Slavs. It is opportune to quote Gauss (*Werke*, Bd. 12, pp. 401 – 404). About 1841 he stated that applications of the theory of probability can be greatly mistaken if the essence of the studied phenomenon is not taken into account.

An eminent mathematician, A. N. Shiryaev, favourably commented on Fomenko's book of 1992, but admitted to Novikov (1997, § 3) that he only saw its abstract. It seems unimaginable, but (Novikov) for many years the Soviet Academy of Sciences supported and actively furthered the scientific career of that crazy Fomenko and his followers. And I found out that Shiryaev also recommended the paper of *Chaikovsky*, again apparently only after seeing its abstract. This is how a mathematician (a specialist in probability!) scorns the history of his science.

Efremov Yu. N., Pavlovskaia E. D. (1987, in Russian), The dating of the *Almagest* by the proper motion of the stars. *Doklady Akademii Nauk SSSR*, vol. 294, № 2, pp. 310 – 313.

--- (1989, in Russian), Same title. *Istoriko-Astronomicheskie Issledovania*, vol. 21, pp. 175 – 192.

Fomenko A. T., Kalashnikov V. V., Nosovsky G. V. (1989), When was Ptolemy's star catalogue … compiled in reality? *Acta Applicandae Mathematicae*, vol. 17, pp. 203 – 229.

Nosovsky G. V., Fomenko A. T. (2004), *Tsar Slavian* (The tsar of the Slavs). Petersburg.

Novikov S. P. (1997, in Russian), Mathematics and history, *Priroda*, No. 2, pp. 70 – 74. **S, G,** 78.

J. B. J. Fourier

In accord with contemporary views, Fourier (1821 – 1829, 1821, pp. iv – v) stated that,

Generally speaking, the spirit of considerations and premises hinders the real progress of statistics, which is in the first place a science of observation.

Indeed, Delambre (1819, p. LXVII) uttered a similar pronouncement and thought that political arithmetic ought to be distinguished from statistics. And the London Statistical Society (Anonymous 1839, p. 1) declared that *statistics does not discuss causes, or reasons upon probable effects*. However (Woolhouse 1873, p. 39), many papers in the journal of that society disregarded these *absurd restrictions*. French statisticians possibly even earlier began to oppose the previous tradition.

Anonymous (1839), Introduction. *J. Stat. Soc. London*, vol. 1, pp. 1 – 5.

Delambre J. B. J. (1819), Analyse des travaux de l'Académie … pendant l'année 1817, partie math. *Mém. Acad. Roy. Sci. Inst. de France*, t. 2 pour 1817, pp. 1 – LXXII of the *Histoire.*

Fourier J. B. J., Editor (1821 – 1829), *Recherches statistiques sur la ville de Paris et de département de la Seine*, tt. 1 – 4. Paris.

Woolhouse W. S. B. (1873), On the philosophy of statistics. *J. Inst. Actuaries*, vol. 17, pp. 37 – 56.

J. Franklin

In his book (2001), Franklin superficially discussed the treatment of observations. The fundamental idea of separating chance from regularity (De Moivre) is only mentioned. There is no connection between the medieval doctrine of probabilism and Jacob Bernoulli's non-additive probabilities or between the qualitative approach to decision making and the very essence of ancient science. The Bibliography is compiled unfortunately and many authors discussed in the text are not included in the Index. References are often unclear and some statements can be mistakenly attributed to the author.

Franklin J. (2001), *The Science of Conjecture. Evidence and Probability before Pascal*. Baltimore.

H. Freudenthal

Freudenthal (1971, p. 142) maintained that Cauchy had proved the central limit theorem rigorously even according to modern standards, but he did not justify his statement, and on p. 135 he called Cauchy *the most superficial* scholar among the great mathematicians.

Freudenthal & Steiner (1966, pp. 181 – 182) mistakenly attributed to Gavarret the change, in medicine, from unconditional certainty of conclusions to a reasonable degree of probability, i. e., attributed the creation of elements of medical statistics, see *Poisson*.

Freudenthal H. (1971), Cauchy. *Dict. Scient. Biogr*., vol. 3, pp. 131 – 148.
Freudenthal H., Steiner H.-G. (1966), Aus der Geschichte der Wahrscheinlichkeitstheorie und der math. Statistik. In *Grundzüge der Mathematik*, Bd. 4. Göttingen. Editor H. Behnke et al, pp. 149 – 195.

C. F. Gauss

Humboldt called Gauss a *scientific despot* (Biermann 1991, p. 9, without an exact reference) and Bessel (Biermann 1966, p. 14) considered him an *insensitive egoist*. Indeed, in 1833 Gauss published an essential contribution on terrestrial magnetism, *typically acknowledged the help of Weber but did not include him as a joint author* (May 1972, p. 305, right column), and his sons by his second marriage stated (Ibidem, p. 308, right column) that he *had discouraged them from going into science* [since] *he did not want any second-rate work associated with his name*. May (p. 307, right column) also indicated *personal* ambition (along with *intellectual isolation*) and *deep conservatism*. Gauss (May, p. 309, left column) was *hostile or indifferent to radical ideas in mathematics*, which, however, was somewhat far-fetched since Gauss is known to have studied the *anti-Euclidian* geometry

Biermann (1966, p. 18) described Gauss' reluctance to refer to other authors and quoted Gauss: he, Gauss, refers to other authors only after convincing himself of their merit, but he has neither time nor inclination for literary studies.

However, Gauss had a few times mistakenly referred to others which could have strengthened his resolve. Thus, in 1770, Boscovich had offered a certain method of treating observations and Gauss (1809, § 186) mentioned him and mistakenly stated that Laplace had modified that method. There also, in § 177, Gauss attributed to Laplace rather than to Euler the computation of the integral of the exponential function of a negative square. Later, as Börsch and Simon, the Editors of Gauss (1887), noted on p. 207, that he revealed his mistake but did not correct it since Euler had not presented that integral in its final form and, moreover, a correction was undesirable since the material was in print.

The Note of 1810. It appeared in an encyclopaedia on the history of literature which, however, included items on natural science and mathematics. Biermann (1983), who reprinted that note about German mathematics and astronomy in the 18th century, reasonably remarked that Gauss had to overcome his dislike of writing popular accounts to satisfy a request by a colleague.

Gauss insufficiently described the merits of Lambert and Daniel Bernoulli and called Süssmilch a mathematician. I do not know whether Jacob and/or Johann Bernoulli considered themselves German or Swiss, but Lambert called himself a Swiss, and Euler (whom Gauss highly praised) was partly a Russian scholar. Herschel, whom Gauss also called a German scientist, was after all an English scholar. Moreover, why then Gauss had not mentioned German scholars working in Russia (e. g., Goldbach)? Gauss also mistakenly described some discoveries made by eminent scholars.

The Memoir of 1823. Some places there are still incomprehensible. Here is Stewart (1995, p. 222) about its §§ 12 and 13:

It requires great generosity on the part of the reader to conclude that he [Gauss] *actually proved anything*.

For many decades textbooks had therefore only been describing the first justification of least squares of 1809. *Eddington*, for example, knew nothing about the second one. And here is Eisenhart (1964, p. 24):

[The existence of the second formulation of the method of least squares] *seems to be virtually unknown to all* [of its] *American users* […] *except students of advanced mathematical statistics.*

A special point here is that the principle of least squares can be derived without any intermediate considerations (as in §§ 12 and 13), see Sheynin (2012).

The Memoir of 1828. Gauss was determining for the second time the latitudinal difference between the observatories in Göttingen and Altona but he did not say anything about its first determination. In several tables of the results of observations 16 stars remained unnamed without any explanation. In two cases (pp. 172 and 189) Gauss calculated the probable error of some results only tacitly assuming the appropriate normal distributions. On p. 161 Gauss called the arithmetic mean the most probable estimator (which it indeed is, but only for normal distributions) although in 1823 he turned instead to most reliable estimators. Finally, Gauss (p. 177) not quite properly equated residual free terms of an initial system of equations with errors. The same, however, can be said about Legendre and Laplace.

Gauss indicated that Legendre was the first to publish the principle of least squares, but claimed it for himself, since he had applied it from 1794 or 1795. Legendre had protested whereas Gauss, about 25 years younger, did not answer his letter. As a result, for a long time French mathematicians including Poisson but not Laplace did not mention the appropriate works of Gauss. All that could have been different if only Gauss had answered Legendre, or, even better, if Legendre, instead of writing to Gauss, would have remarked at a later occasion, that everyone will agree with him rather than with Gauss. And here is the final stroke (letter of Gauss to Schumacher of 17 Oct. 1824):

With irritation and distress I have read that the pension of the old Legendre, an ornament to his nation and age, was cut off.

Biermann K. R. (1966), Über die Beziehungen zwischen C. F. Gauss und F. W. Bessel. *Mitt. Gauss-Ges. Götingen*, No. 3, pp. 7 – 20.

--- (1983), C. F. Gauss als Mathematik- und Astronomiehistoriker. *Hist. Math*., vol. 10, pp. 422 – 434.

--- (1991), Wandlungen unseres Gaussbildes. *Mitt. Gauss-Ges. Göttingen*, No. 28, pp. 3 – 13.

Eisenhart C. (1964), The meaning of "least" in least squares. *J. Wash. Acad. Sci.*, vol. 54, pp. 24 – 33.

Gauss C. F. (1809, Latin), *Theorie der Bewegung* etc., Book 2, Section 3. In Gauss (1887, pp. 92 – 117).

--- (1823, Latin), English translation: Stewart (1995).

--- (1828), Bestimmung des Breitenunterschiede zwischen den Sternwarten von Göttingen und Altona etc. In Gauss (1887, pp. 152 – 189) and in Gauss, *Werke*, Bd. 9, 1903, pp. 5 – 64.

--- (1887), *Abhandlungen zur Methode der kleinsten Quadrate*. Hrsg, A. Börsch, P. Simon. Vaduz, 1998.

May K. O. (1972), Gauss. *Dict. Scient. Biogr.*, vol. 5, vol. 298 – 315.

Sheynin O. (2012), New exposition of Gauss final justification of least squares. *Math. Scientist*, vol. 37, pp. 147 – 148. *Silesian Stat. Rev*., No. 12 (18), 2014, pp. 39 – 47.
Stewart G. W. (1995), *Theory of the Combination of Observations Least Subject to Error. C. F. Gauss.* Translation & Afterword. Philadelphia.

T. Gerardy

Gerardy (1977) unfortunately described his subject. Some places are difficult to understand, but it is much more important that he directed his main attention to the calculation of intersections but did not intelligibly explain his hint at Gauss' application of least squares before 1805. It is highly desirable for someone to study the archival source which Gerardy had used. Indeed, this is the only instance in which a *direct* confirmation of the hinted fact could thus be discovered.

Gerardy T. (1977), Die Anfänge von Gauss' geodätische Tätigkeit. *Z. f. Vermessunswesen*, Bd. 102, pp. 1 – 20.

G. Gigerenzer

The book (1990) was written by six authors including Gigerenzer. It is devoted to the history of the theory of probability, statistics and their applications during 1820 – 1900. Many most eminent scholars (Wilhelm Herschel, Humboldt) are not mentioned and the theory of errors, the main field of applications of the theory of probability in those times, is not studied, Gauss' main memoir on that subject is not included in the bibliography. There are many mistakes and bibliographic information is bad. The book is actually a draft on its subject. See my review (1992).

Gigerenzer G. et al (1990), *The Empire of Chance*. Cambridge.
Sheynin O. (1992), *Physis*, vol. 29, No. 2, pp. 633 – 638.

B. V. Gnedenko

Gnedenko was co-author of a popular booklet Gnedenko & Khinchin (1946) which ran into many editions and was translated into several languages. Khinchin died in 1959 whereas Gnedenko outlived him by about 36 years and had time to insert many changes The English translation of that booklet became dated (and lacked any commentaries) and I translated it anew.

The booklet is written extremely carelessly and the possibility of providing, in passing, useful and even necessary information was not used. Thus, nothing is said about elementary approximate calculations and in § 9 (such) a calculation was done with an excessive number of digits. Statistical and theoretical statistics are supposed to coincide (§ 1), the essence of the Bayesian approach is not explained etc.

Being a graduate of the Odessa artillery school and a certified geodetic engineer, I declare that the numerous examples of artillery firing are fantastic and that the examples of linear measurements in the field, only a bit better. When reading the former, I recalled how Mark Twain edited an agricultural newspaper: *Domesticate the polecat* etc. And in general, many years ago all those examples became helplessly obsolete and should have been omitted. In spite of its commercial success, the booklet deserved to be burned.

At the end of his life Gnedenko published an essay on the history of probability. He knew nothing about developments in that field and his essay is useless and even misleading.

Gnedenko B. V., Khinchin A. Ya. (1946), *Elementarnoe Vvedenie v Teoriyu Veroiatnostei* (Elementary Introduction into the Theory of Probability). Latest Russian edition: Moscow, 2013. My English translation: Berlin, 2015. **S, G,** 65.

B. B. Golitsin

Galitsin is the spelling of the author himself which he adopted in 1902 (and perhaps always when writing in German). In 1902, he hastily and thoughtlessly described his experiments with the solidity of glass tubes. He possibly did not quite understand the significance of a proper mathematical treatment of measurements and Markov (ca. 1903) criticized him in detail but made methodical mistakes and his Bibliography was really bad (Sheynin 1990).

Galitzin B. (1902), Über die Festigkeit des Glasses. *Izvestia Imp. Akademii Nauk*, ser. 5, vol. 16, № 1, pp. 1 – 20.
Markov A. A. (ca. 1903, published 1990, in Russian), On the solidity of glass tubes. *Istoriko-Matematicheskie Issledovania*, vol. 32 – 33, pp. 456 – 467.
Sheynin O. (1990, in Russian), The opinion of A. A. Markov about a paper by Galitsin. Ibidem, pp. 451 – 455.

I. J. Good

Good (1978) is a very superficial essay since it does not really analyse its subject. Good refers to several allied publications including Huff (1954), whereas Good published a review of Stigler's book of 1986 but did not note the author's astonishing accusations of Gauss. See also *Lueder*.

Good I. J. (1978), Fallacies, statistical. In W. Kruskal, Judith M. Tanur, Editors, *Intern. Enc. of Statistics*. New York – London, pp. 337 – 349.
Huff D. (1954), *How To Lie with Statistics*. Penguin Books, 1973.

P. Gorrochurn

Bernstein, Bohlmann, Chuprov, Markov, Kolmogorov and Slutsky are absent or almost so in his book (2016). The Gauss – Markov theorem is not buried. Mathematical statistics is not defined and comments on the earlier history of his subject (e. g., on De Moivre or Bayes) cannot be trusted. And Liapunov did not live in Gorkiy!

Gorrochurn P. (2016), *History of Modern Mathematical Statistics from Laplace to More Recent Times*. Hoboken, NJ.

I. Grattan-Guinness

A few decades ago, I noticed an extremely negative review of a paper published by Grattan-Guinness in the same source, the *Archive for History of Exact Sciences.* The review was written by one of the editors, Freudenthal, but the paper had to be communicated by another editor. About 1988 I met the late Truesdell, the chief editor of that journal, and asked him how all of it happened. It turned out that Truesdell had asked G.-G. to recall his manuscript but received an impudent answer: he, Truesdell, is demanding unnecessary (!) rigor and is therefore lagging behind lífe.

G.-G. published two unworthy reviews in the *Math. Rev*. on Kolmogorov & Youshkevich (1978) and on Truesdell (1984) and, disregarding my advice, published three items written by Porter (see *Porter*) in his *Companion Enc*. of 1994.

Kolmogorov A. N., Youshkevich A. P., Editors (1978), *Matematika XIX Veka* (Century), vol. 1. Moscow. Later translation: Basel, 1992, 2001.
Truesdell C. (1984), *An Idiot's Fugitive Essays on Science*. New York.

59. J. Graunt

Graunt's life table was practically useless owing to excessive errors, but its methodological importance was inestimable. Willcox (Graunt 1939, p. x) compared him with Columbus. In general, however, his book (1662) was corrupted by several mistakes.

Items 48 and 74 of the Index were practically identical. In Chapter 12, section 12, he stated that the provinces were healthier than London, but in section 13 modified that statement: Newcastle was an exception. Expressions like *the number of* [those who died from] *Worms and Teeth* […] are now unacceptable, but were they good enough in his time?

Hull noted defects and inconsistencies in Graunt's calculations in Chapter 12 which was much worse. Then, in Chapter 3, section 25, Graunt stated that the Moon experiences *starting or jerking backwards*.

Hull (Petty 1899, vol. 1, p. lii) decided that Petty qualifies as Graunt's co-author and for many decades, if not for much longer, Petty was thought to be the sole author of the *Observations*. No one noted Petty's remark (1674, Address to Lord Brounker): *I have also like the author of those Observations* [like Graunt] *Dedicated this Discourse to* […] *the Duke of Newcastle*.

Graunt J. (1662), *Natural and Political Observations Made upon the Bills of Mortality*. London. Some further editions: in Petty W. (1899), *Economic Writings*, vols 1 – 2. Editor C. H. Hull. See vol. 2, pp. 317 – 435. London. Also, Baltimore, 1939. Editor W. F. Willcox.

Petty W. (1654), *Discourse Read before the Royal Society*. London, 1674.

W. J. 'sGravesande

'sGravesande (1688 – 1742) allegedly calculated a certain magnitude up to 47 decimal digits. Pearson (1978, p. 302) mentioned this episode as was stated by Nieuwentit (1654 – 1718) but reasonably did not believe him.

Pearson K. (1978), *History of Statistics in the 17th and 18th Centuries*. London.

A. A. Grigorian

His superficial essay (1999) contains a lot of mistakes and inaccuracies. The non-mathematical (and non-physical) Mises theory is wrongly called axiomatic.

Grigorian A. A. (1999, in Russian), The Mises theory of probability: history and philosophical and methodological principles. *Istoriko-Matematicheskie Issledovania*, vol. 3 (38) pp. 198 – 220.

E. J. Gumbel

Gumbel was known as an eminent statistician and a staunch enemy of Nazism but absolutely unknown was his kowtowing to the Stalinist regime (Sheynin 2003, pp. 8 – 16). Being guided by Otto Schmidt, that Bolshevik scholar, he was nevertheless quite able to see through the Soviet propaganda. Indeed, he lived in the Soviet Union for some time, and he was a statistician! Here is just one of his stupid statements of 1927 (Ibidem, p. 37; Gumbel (1927/1991, p. 159):

Peasants are freed from the knout and workers may look with a proud hope on the first attempt at realizing socialism.

Serfdom was abolished in Russia in 1861 and, in 1927, such hopes of the workers became thin.

I (2003, pp. 33 – 36) have attempted to explain the attitude of many Western intellectuals who had continued to paint rosy pictures about the conditions of life in the Soviet Union without knowing, or even wishing to know anything.

Gumbel E. J. (1927), Vom Russland der Gegenwart. In his book *Auf der Suche nach Wahrheit. Ausgew. Schriften*. Berlin, 1991, pp. 83 – 164.

Sheynin O. (2003), *Gumbel, Einstein and Russia*. Moscow. English – Russian edition. **S, G,** 12.

I. Hacking

Erudition and a pleasant style do not exonerate Hacking's book (1975/2006). A philosophical discussion of concepts, principles or definitions is lacking, there is no general outline of the history of probability theory, of its transition from pure to applied science (Laplace). Induction and hypotheses are treated superficially. There are mistakes and the only mathematical reasoning (p. 108) is wrong. The author insists that *Emergence* differs from history, but this does not justify the disregard of Aristotle. Even the first edition of his book was unsatisfactory.

Hacking I. (1975), *Emergence of Probability. Phil. Study of Early Ideas about Probability, Induction and Stat. Inference.* Cambridge, 2006.

Sheynin O. (2008, in Russian), Review of Hacking (2006). *Voprosy Istorii Estestvoznania i Techniki*, No. 2, pp. 175 – 178.

A. Hald

There are many mistakes in his book (2007) and the bibliography does not include essential sources although mentions some (almost) useless works. In 1990 Hald passed over in silence Nic. Bernoulli's plagiarism and had not mentioned the mistake in De Witt's calculations. Contrary to his opinion, statisticians had for many decades been ignoring the Bernoulli law. In 1998 he stated that Laplace rather than Euler was the first to calculate the integral of the exponential function of a negative square.

That book (1998) does not treat the Continental direction of statistics or the contributions of Bernstein and its title is therefore misleading. Then, Hald presented classical results in modern language, but had not explained the transition from their original appearance. Some authors (Linnik 1958; Sprott 1978) acted similarly.

Hald arranged the material in such a way that it is difficult to find out what was contained, for example, in a certain memoir of Laplace. And, finally, Hald mentioned Stigler's book of 1984 in an extremely strange manner, see *Stigler*.

Hald A. (1990), *History of Probability and Statistics and Their Applications before 1750*. New York.

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

--- (2007), *History of Parametric Statistical Inference from Bernoulli to Fisher, 1713 – 1935*. New York.

Linnik Yu.V. (1958, in Russian), *Method of Least Squares and Principles of the Theory of Observations*. Oxford, 1998.

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

E. Halley

Halley (1693) compiled a life table for Breslau, a city with a closed population. The calculated yearly rate of mortality there was 1/30, the same as in London. Halley should have at least been surprised by his finding, but, instead, he proposed to consider that city a statistical standard. If such a notion is permissible, several standards should be chosen.

Halley stated that the irregularities in his data

Would rectify themselves were the number of years [of observation] *much more considerable*.

Such irregularities, however, mostly indicate the action of systematic influences.

Böckh R. (1893), Halley als Statistiker. *Bull. Intern. Stat. Inst*., t. 7, No. 1, pp. 1 – 24.

Halley E. (1693), *An Estimate of the Degree of Mortality of Mankind*. Baltimore, 1942.

D. M. Haushofer

Statisticians had been able to apply the Bernoulli law, but for many decades they all but ignored it. Knapp (1872, pp. 116 – 117) somehow decided that that law was almost useless since statisticians always (when, for example, conducting a census of population) observe only once. Haushofer (1872, pp. 107 – 108) maintained that statistics is based on induction, and therefore has no *intrinsic ties* with mathematics (including the theory of probability) which is based on deduction. Accordingly, he contradicted Bernoulli who proved that, in case of Bernoulli trials, empirical probabilities approach theoretical probabilities.

Maciejewski (1911, p. 96) introduced a *statistical law of large numbers*, which only qualitatively stated that the fluctuations of statistical indicators dampen with the increase of the number of observations. Romanovsky (1912, p. 22; 1924, p. 15; 1961, p. 27) held similar views, and, in the last mentioned source, called the law of large numbers a physical law. But at least no one repeated Maciejewski's strange utterance that the Bernoulli theorem *hindered the development of statistics*.

In general, statisticians continued to believe that the theory of probability and the law of large numbers are only applicable in case of Bernoulli trials and only if there existed equally probable cases. The situation only changed probably in the first quarter of the 20th century.

Even in the 20th century von Mayr, an eminent representative of the old statistical school, privately told Bortkiewicz (Bortkevich & Chuprov 2005, Letter 109 of 1911) that mathematical formulas are useless and that he does not endure mathematics.

Here, indeed, is Wittstein (1867): he compared the situation in statistics with the childhood of astronomy and stressed that it (and especially population statistics) requires a Tycho and a Kepler to reveal regularities by means of reliable observations. Statisticians, as he continued, do not understand the essence of probability theory and never estimate the precision of their results. Note that Wittstein was apparently the first to introduce the term *mathematical statistics*.

Bortkevich V. I., Chuprov A. A. (2005), *Perepiska* (Correspondence) *1895 – 1926*. Berlin. **S, G,** 9.

Haushofer D. M. (1872), *Lehr- und Handbuch der Statistik.* Wien.

Knapp G. F. (1872), Quetelet als Theoretiker. *Jahrbücher f. Nationalökonomie u. Statistik*, Bd. 18, pp. 89 – 124.

Maciejewski C. (1911), *Nouveaux fondements de la théorie de la statistique*. Paris.

Romanovsky V. I. (1912), *Zakon Bol'shikh Chisel i Teorema Bernoulli* (The Law of Large Numbers and the Bernoulli Theorem). Warsaw. Also in the *Protokoly Zasedaniy Obshchestva Estestvoispytatelei Varshavskogo Univ*. for 1911, No. 4, pp. 39 – 63.

--- (1924, in Russian), Theory of probability and statistics etc. *Vestnik Statistiki*, No. 4 – 6, pp. 1 – 38.

--- (1961), *Matematicheskaia Statistika*, book 1. Tashkent.

Wittstein Th. (1867), *Mathematische Statistik*. Hannover.

W. Herschel

Herschel (1817/1912, p. 579) stated that

It may be presumed that any star promiscuously chosen […] *out of such a number* [out of more than 14 thousand] *is not likely to differ much from a certain mean size of them all*.

Herschel certainly did not know that with regard to size the stars are incredibly different. A mean size is a worthless magnitude, and, in general, statements made in the absence of data are hardly useful: *ex nihilo nihil*.

A similar statement was due to Simpson (1848/1871, p. 102):

The data I have adduced [..] *have been objected to on the ground that they are collected from too may hospitals and too many sources. But* […] *I believe all our highest statistical authorities will hold that this very circumstance renders them more, instead of less trustworthy.*

Simpson was utterly wrong and the more so since his data covered a period of 45 years.

Bernard (1865/1926, t. 2, pp. 117 – 118) provided an unusual example of a meaningless mean: the mean male European urine, a sample from a toilet in a station of a railway which carries passengers from various countries.

Bernard Cl. (1865), *Introduction à l'étude de la médecine expérimentale*, tt. 1 – 2. No place, 1826.

Hershel W. (1817), Astronomical observations and experiments etc. In author's book (1912, pp. 575 – 591).

--- (1912), *Scientific Papers*, vol. 2. London. [Bristol 2003.]

Simpson J. Y. (1848), Anaesthesia. *Works*, vol. 2. Edinburgh 1871, pp. 1 – 288.

D. Hilbert

His famous report (1901) included Problem 6:

To treat in the same manner, by means of axioms, those physical sciences in which mathematical physics plays an important part; in the first rank are the theory of probabilities and mechanics. As to the axioms of the theory of probabilities, it seems to me desirable that their logical investigation should be accompanied by a rigorous and satisfactory development of the method of mean values in mathematical physics and in particular in the kinetic theory of gases.

Mathematical physics was then understood as somehow related to probability. Consider, indeed, the title of Poincaré (1896/1912):

Cours de la Faculté des sciences de Paris
Cours de physique mathématique
Calcul des probabilités

It was Condorcet, who, in 1805 originated, although not properly, the theory of means as an introduction to statistics. It became more general than the theory of errors since it included the study of the means of variable magnitudes. Quetelet attributed that theory to statistics but it also partly belonged to the theory of errors, and Hilbert was probably one of the last scientists who mentioned it. See Sheynin (2007).

Here is his wrong statement from an unpublished lecture of 1905 (Corry 1997, p. 161):

If many values derived from observations are available for a certain magnitude, its most probable value will be the arithmetic mean of all those observed values.

This is only true for the normal distribution. Moreover, Hilbert apparently did not know that Gauss had abandoned most probable estimators in favour of those most reliable. See also *Ekeland*.

Corry L. (1997), Hilbert and the axiomatization of physics. *Arch. Hist. Ex. Sci.*, vol. 51, pp. 83 – 198.

Hilbert D. (1901, in German), Mathematical Problems. *Bull. Amer. Math. Soc.*, vol. 8, 1902, pp. 437 – 479. Reprinted in *Mathematical Developments Arising from Hilbert Problems.* Amer. Math. Soc., 1976. Editor, F. Brouder.

Poincaré H. (1896), *Calcul des probabilités*. Paris, 1912.

Sheynin O. (2007), The true value of a measured constant and the theory of errors. *Hist. Scientiarum*, vol. 17, pp. 38 – 48.

D. Howie

His book (2002) lacks clear definitions of the discussed concepts and is not free from many mistakes. The most astonishing mistake is that Newton thought that the system of the world was stable. Mendel is called a Czech, but he was German. The descendants of his relatives were driven out of the then Czechoslovakia.

Howie D. (2002), *Interpreting Probability* etc. Cambridge.

C. Huygens

In his correspondence with his brother Huygens (1669/1895, vol. 6, p. 538) decided that the number of deaths among a group of men decreases in time. However, under his assumed law of mortality (continuous uniform distribution), order statistics will separate the given interval of time into approximately equal parts. That law was the first continuous law in the theory of probability, but Nic. Bernoulli was the first, in 1709, to *publish* an application of the same law (again when treating mortality). For those times, however, correspondence is considered on a par with publication.

Elsewhere Huygens (1698, p. 115) assumed that the diameter of Jupiter was 20 times larger than the diameter of the Earth and concluded that the size of the (imagined) inhabitants of that planet should be larger than ours. Actually, owing to the much more powerful force of gravity, that size should have been much smaller.

Huygens C. (1669/1895, *Oeuvr. Compl.*, t. 6), Correspondence.

--- (1698), *Cosmotheoros*. *Oeuvr. Compl.*, t. 21, 1944, pp. 653 – 842. *The Celestial Worlds Discovered* (Latin and English)*, 1698* and London, 1968.

--- (1888 – 1950), *Oeuvr. Compl*., tt. 1 – 22. La Haye.

I-Hsing

When discussing the arc measurement of the eighth century in China, Needham (1962, pp. 723 – 726) concluded:

In all probability I-Hsing [one of the astronomers] *thought it very undesirable to admit … a mass of raw data showing considerable scatter, and not being able to assess it statistically, he used it only to satisfy himself that his calculated values came about were they should – indeed, he probably believed that they were much more reliable than most of the observations*.

See Beer A. et al (1961) for a detailed account of that measurement.

Beer A. et al (1961), An 8th century meridian line etc. *Vistas in Astronomy*, vol. 4, pp. 3 – 28.

Needham J. (1962), *Science and Civilization in China*, vol. 4, pt. 1. Cambridge.

J. Ivory

Ivory (1826b, pp. 244 – 245) treated pendulum observations made for deducing the Earth's flattening and corroborating the theory of an elliptical form of the Earth. He solved a redundant system of linear equations with unity coefficients of one of the unknowns. Denote the residual free terms of the equations by w_i. Then, as he stated, the condition $\sum w_i = 0$ was preferable to the principle of least squares. However, in his case least squares led to that same condition. Unavoidable local anomalies of gravity required the rejection of some observations, but Ivory (p. 242) obviously rejected too many of them.

Elsewhere, Ivory (1826a, p. 9) had only 5 – 7 observations and only one of them was situated in the equatorial zone. He combined that observation in turn with each of the other ones and calculated the sought flattening from each of the thus obtained pairs of observation with large latitudinal differences. This was indeed proper, but he had actually assigned a greatly exaggerated weight to the equatorial observation and it is difficult to say whether he acted in the most favourable way which *Euler* had applied much earlier.

He had not used variance as the measure of precision. Then, bearing in mind the corroboration of the theory (see above), he should have applied the method of minimax or at least its elements; in 1789 Laplace derived algorithms for such an application, see Sheynin (2009, § 6.3.2).

Ivory J. (1826a), On the ellipticity of the Earth. *Lond., Edinb. and Dublin Phil. Mag.*, vol. 68, pp. 3 – 10 and 92 – 101.

--- (1826b), On the methods … for deducing … the length of seconds pendulum. Ibidem, pp. 241 – 245.

Sheynin O. (2009), *Theory of Probability. Historical Essay.* Berlin. **S, G,** 10.

V. N. Katasonov

His paper (1992) is pompous, empty and contains some mistakes and doubtful statements.

Katasonov V. N. (1992, in Russian), The origin of the theory of probability in the context of the attempts at a Weltanschauung in the seventeenth century. *Voprosy Istorii Estestvoznania i Techniki*, № 3, pp. 43 – 58.

A. A. Kaufman

Kaufman (1922, p. 152):

Such methods as the construction of the curves of distribution, adjustment of series, interpolation, not only are not conducive to the ascertaining of the real nature of the studied phenomena, but, on the contrary, they can provide ideas about them which corrupt reality. […] *The so-called method of correlation adds nothing in essence to the results of an elementary analysis* […].

In a posthumous edition of his book (Moscow, 1978, p. 214) the theory of correlation is called *a most important and surprising section of modern statistics*. The book was essentially rewritten (but the author's name remained!) and that statement was due to Romanovsky.

I ought to add Bernstein's opinion (1928/1964, p. 231):

Excluding biological applications, most of its [of correlation theory] *practical usage is based on misunderstanding*.

Bernstein S. N. (1928, in Russian), The present state of the theory of probability and its applications. *Sobranie Sochineniy*, vol. 4. Moscow, 1964, pp. 217 – 232. **S, G.** 7.

Kaufman A. A. (1922), *Teoria i Metody Statistiki* (Theory and Methods of Statistics). Moscow. Fourth edition. German edition: *Theorie und Methoden der Statistik*. Tübingen, 1913.

Slutsky E. E. (1916, in Russian), Statistics and mathematics. *Statistichesky Vestnik*, bks 3 – 4, pp. 104 – 120. A review of an edition of the Kaufman book. **S, G,** 36.

M. G. Kendall

His lecture (1972/1977) is incomplete; much more is contained in *Lécuyer & Oberschall*, and You Po Seng (1951), not cited either, provided a lengthy account of the history of sampling. Kendall reasonably argued that sociological studies ought to be carried out by specialists in various branches of knowledge, but did not mention econometrists. Then, he remarked that the statistical theory had successively progressed in biology and meteorology. However, first, I hold that until Fisher the theory of statistics did not exist. And, second, physics certainly has no relations with sociology (Kendall's subject), but medicine should have been mentioned as well both here and elsewhere.

Strange as it is, in spite of the title of his lecture, Kendall only in passing mentioned political arithmetic.

Kendall M. G. (1972, lecture), Measurement in the study of society. In Sir Maurice Kendall, R. L. Plackett, Editors (1977), *Studies in the History of Statistics and Probability*, vol. 2. London, pp. 35 – 49.

You Poh Seng (1951), Historical survey of the development of sampling etc. *J. Roy. Stat. Soc.*, vol. A114, pp. 214 – 231. Reprinted in Sir Maurice Kendall & R. L. Plackett, Editors (1977), *Studies in the History of Statistics and Probability*, vol. 2. London – High Wycombe, pp. 440 – 457.

M. G. Kendall, A. G. Doig

Their *Bibliography* (1968) hardly contains any references to collected works. In Euler's t. 7 of ser. 1 of his *Opera Omnia* published in 1923 there are fourteen items pertaining to probability and statistics, but only seven of them are included. Furthermore, one is called *Wahrscheinlichkeitsrechnung*, but it either does not exist, or misnamed. The second part of Daniel Bernoulli's *Mensura sortis* is missing and Maxwell is absent.

Kendall M. G., Doig A. G., Compilers (1968), *Bibliography of Statistical Literature Pre-1940 with Supplements to the Volumes for 1940 – 1949 and 1950 – 1958*. Edinburgh – London.

J. Kepler

According to ancient belief, the end of the world will come when all planets return to their original position (at the moment of creation). Kepler (1596/1621, Note 5 to Chap. 23) thought that the end of the world was at least unlikely since two (randomly chosen) numbers will probably be incommensurable. He said nothing about the applicability of that notion to physical bodies. See *Chebyshev* who considered that difficult problem.

A similar reasoning was due to Oresme, 1323 − 1382 (1966, pp. 247 and 422) and even earlier to Levi ben Gerson, 1288 − 1344 (1999, p. 166) although he had not considered the end of the world.

No one mentioned knew that a dynamic system will however nearly return to its previous state.

I am unable to say for how long astronomers continued to believe in Kepler's explanation (1596) of the system of the world by regular polyhedrons.

Kepler (1618 – 1621, 1620/1952, book 4, pt. 3, § 1, p. 932) explained the eccentricities of the planetary orbits by mysterious *natural and animal faculties*.

According to the Bible, the Sun rotates around the Earth but Kepler (1609/1992, Author's Introduction, pp. 59 – 62) maintained that that wrong statement was necessitated by offering an understandable account of the situation. He decided that Joshua stopped the motion of the Earth's rotation around the Sun. And, if that were possible, it was also perhaps possible that no one felt it, but Kepler did not say anything about it.

Great Books (1952), *Great Books of the Western World*, vols. 1 – 54. Chicago.

Kepler J. (1596, 1621, in Latin), *Weltgeheimnis*. Augsburg, 1923. [München – Berlin, 1936. *The Secret of the Universe*. New York, 1981.]

--- (1609, in Latin), *New Astronomy*. Cambridge, 1992.

--- (1618 – 1621, 1620, in Latin), *Epitome of Copernican Astronomy*, books 4 and 5. In *Great Books* (1952, vol. 16).

Levi ben Gerson (1999), *The Wars of the Lord*, vol. 3. New York.

Oresme N. (1505/1966, in Latin and English), *De proportionibus proportionum and Ad pauca respicientis.* Madison.

A. Ya. Khinchin

Khinchin's invasion of statistical physics (1943) was unfortunate. Novikov (2002, p. 334) testified that

Physicists had met his attempts with great contempt. Leontovich told my father [both were academicians] *that Khinchin was absolutely ignorant*.

Khinchin (1937) praised the Soviet regime and the freedom of scientific work in the Soviet Union at the peak of the Great Terror. In October of that same year, a colloquium on probability theory was held at Geneva University. Among its participants were Cramer, Feller, Hostinsky and other eminent scholars whose names are known since they signed an address to Max Born on the occasion of his birthday. The address is kept at the Staatsbibliothek zu Berlin, Preußische Kulturbesitz, Manuskriptabt., Nachlass Born, 129. There were no Soviet participants! Indeed, it was inadmissible to allow the dissemination of information about the terror.

Khinchin certainly described the situation in tsarist Russia as terrible, but here is a telling episode (Archive of the Russian Acad. Sci., Markov's Fond 173, Inventory 1, 11, No. 17). Liapunov was nominated for membership in the Academy, and, when answering Markov's question (letter of 24 March 1901), informed him that 10 most eminent foreign scientists (whom he named) had referred to him.

See also *Gnedenko*.

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

--- (1943, in Russian), *Mathematical Foundations of Statistical Mechanics*. New York, 1949.

Novikov S. P. (2002, in Russian), The second half of the 20th century and its result etc. *Istoriko-Matematicheskie Issledovania*, vol. 7 (42), pp. 326 – 356.

G. King

King (1648 – 1712) unwarrantably extrapolated statistical data, even for 3000 years, and unjustifiably reasoned about the fecundity of families and the family tendency to produce babies of one or another sex, see Pearson (1978, pp. 109 – 110).

Chalmers G. (1802), *An estimate of the Comparative Strength of Great Britain*. Second edition. A Supplement contains the text of a manuscript by King.

Pearson K. (1978), *History of Statistics in the 17th and 18th Centuries*. London.

P. M. M. Klep, Ida H. Stamhuis

These authors (2004) made a few mistakes. Halley's life tables allegedly appeared in the 18th century; the *smaller* is the normal curve, the higher is the precision of the appropriate observations; moral statistics is tantamount to the theory of probability (!); and the notions of *mean* and *probability* had been developed in the Netherland between 1750 and 1850.

Klep P. M. M., Stamhuis Ida H. (2004), The stubbornness of various ways of knowledge was not typically Dutch etc. *Centaurus*, vol. 46, pp. 287 – 317.

G. F. Knapp

Bortkiewicz (1904, p. 822) noted that Knapp had opposed the application of the theory of probability to statistics and (1910, p. 358) called him *a most convinced enemy* of such applications (see *Haushofer*). In turn, Chuprov (Letter of 2 December 1896 to Bortkiewicz; Bortkevich & Chuprov 2005) noted Knapp's *negative attitude to probability*.

Bortkevich V. I., Chuprov A. A. (2005), *Perepiska* (Correspondence) 1895 – 1926. Berlin. **S, G,** 9.
Bortkiewicz L. von (1904), Anwendung der Wahrscheinlichkeitsrechnung auf Statistik. *Enc. math. Sci.*, Bd. 1, Tl. 2. Leipzig, pp. 822 – 851.
--- (1910, in Russian), The issues of scientific statistics. *Zhurnal Ministerstva Narodnogo Prosveshcheniya*, No. 2, pp. 346 – 372 of second paging.

A. N. Kolmogorov

Kolmogorov (Anonymous 1954, p. 47):

We have for a long time been cultivating a wrong belief in the existence, in addition to mathematical statistics and statistics as a social and economic science, of something like yet another non-mathematical although universal <u>*general*</u> *theory of statistics which essentially comes to mathematical statistics and some technical methods of collecting and treating statistical data. Accordingly, mathematical statistics was declared a part of this* <u>*general theory of statistics*</u>.

Yes, theoretical statistics is indeed wider than mathematical statistics, but the *technical methods* are general scientific methods.

Pontriagin (1980) sharply criticized the mathematical school curriculum compiled by Kolmogorov. He reasonably argued that students of ordinary schools will be unable to cope with it [and will be hating mathematics].

A strange statement is due to Anscombe (1967, p. 3n):

The notion of mathematical statistics is a grotesque phenomenon.

Kolmogorov (1947, p. 56) maintained that

Chebyshev was the first to appreciate clearly and use the full power of the concepts of <u>*random variable*</u> *and its expectation*.

In translation (Gnedenko & Sheynin 1978/2001, p. 255) that phrase was somehow became wrongly attributed to us. Now, Chebyshev had not introduced even a heuristic definition of random variable or any special notation for it and was therefore unable to study densities or generating functions as mathematical objects. Furthermore, the entire development of probability theory may be described by an ever more complete use of the concepts mentioned.

Anonymous (1954, in Russian), Account of the All-Union Conference on problems of statistics. *Vestnik Statistiki*, № 5, pp. 39 – 95.

Anscombe F. J. (1967), Topics in the investigation of linear relations […]. *J. Roy. Stat. Soc.*, vol. B29, pp. 1 – 52.

Gnedenko B. V., Sheynin O. (1978, in Russian), Theory of probability. A chapter in *Mathematics of the 19th Century*, vol. 1. Basel, 1992 and 2001, pp. 212 – 288. Editors, A. N. Kolmogorov & A. P. Youshkevich.

Kolmogorov A. N. (1947, in Russian), The role of Russian science in the development of the theory of probability. *Uchenye Zapiski Mosk. Gos. Univ.*, No. 91, pp. 53 – 64. **S, G,** 7.

Pontriagin L. S. (1980, in Russian), On mathematics and the quality of teaching it. *Kommunist,* № 14, pp. 99 – 112.

M. Kornfeld

Kornfeld, whose note (1955) was communicated by an eminent physicist, M. A. Leontovich, maintained that it is sufficient to estimate the precision of the constant *a* as determined by *n* observations by the formula

$$P(x_1 \le a \le x_n) = 1 - (1/2)^{n-1} \quad (1)$$

where x_1 and x_n were the extreme observations. Statisticians have similar formulas for calculating non-parametric confidence intervals for the population median, but those formulas make use of all the available observations.

The first to propose formula (1) was Bervi (1899) whom Kornfeld did not cite.

Bervi N. V. (1899, in Russian), The determination of the most probable value of the measured object apart from the Gauss postulate. *Imp. Moskovsk. Obshchestvo Liubitelei Estestvoznania, Antropologii i Etnografii*, section of phys. sciences, vol. 10, No. 1, pp. 41 – 45.

Kornfeld M. (1955, in Russian), On the theory of errors. *Doklady Akad. Nauk SSSR*, vol. 103, No. 2, pp. 213 – 214.

S. Kotz

Kotz (2006) contains a large number of dated items reprinted from the first edition of this *Encyclopedia* and other sources. Information concerning the history of statistics is mostly fragmentary, sometimes wrong and in many cases lacking altogether. The mythical Gauss – Markov theorem is considered in vol. 4 (p. 2647), but there is no biography of Euler. Historians of science can only rely on this source at their own peril. The item on Süssmilch (Pfanzagl & Sheynin 1997) has somehow appeared anonymously.

Kotz S., Editor-in-Chief (2006), *Encyclopedia of Statistical Sciences*, vols 1 – 16. Hobokan, New Jersey.

Pfanzagl J. & Sheynin O. (1997), Süssmilch. Reprinted anonymously (!) In Kotz (2006, vol. 13, pp. 8489 – 8491).

J. B. Lamarck

Lamarck (1802) introduced the term *statistical meteorology*, and decided (1800 – 1811, № 3, p. 194) that meteorology ought to have its own theory, principles and aphorisms. This, however, was impossible. Cournot (1843, § 105) stated almost the same about statistics.

Lamarck, as it was thought in those times, believed that the Moon influences the atmosphere and (1800 – 1811, № 6, p. 13), and even isolated 23,520 cases of the mutual position of the Sun, Earth and Moon.

Correctly noting that the previous state of the atmosphere influences its later state, he (Ibidem, № 5, pp. 5 − 8), only qualitatively and hardly convincingly justified this opinion (1818, p. 465). His yearbooks (1800 – 1811) contained dubious statements and unjustified weather forecasts, and Napoleon declared that they *disgraced his grey hair* (Sheynin 1984, § 6.5).

Lambert also studied the influence of the Moon on the atmosphere, and Daniel Bernoulli approved the subject of his investigation (Radelet de Grave et al (1979, p. 62).

Being a versatile scholar (a biologist in the first place), Lamarck (1820, p. 226) declared that a suicide is ill (by definition) but did not suggest to study that *illness*. However, Casper (1825, pp. 3 – 95) soon recommended that measure. Zhuravsky (1846, p. 26) remarked that in Russia during 1835 – 1840 there occurred more suicides than murders.

Casper J. L. (1825), *Beiträge zur medizinischen Statistik*, Bd. 1. Berlin.

Cournot O. (1843), *Exposition de la théorie des chances et des probabilités*. Paris, 1984. Editor, B. Bru. English translation: **S, G,** 54.

Lamarck J. B. (1802), Météorologie-statistique. *Annales stat*., t. 3, pp. 58 – 71; t. 4, pp. 129 – 134.

--- (1800 – 1811), *Annuaire météorologique*, NNo. 1 – 11. Paris, pour l'an 8 − pour 1810. An extremely rare edition.

--- (1818), Météorologie. *Nouv. Dict. Hist. Natur.*, t. 20, pp. 451 – 477.

--- (1820), *Système analytique* etc. Paris.

Radelet de Grave P., Scheuber V. (1979), *Correspondance entre Daniel Bernoulli et J.-H. Lambert*. Paris.

Sheynin O. (1984), On the history of the statistical method in meteorology. *Arch. Hist. Ex. Sci.*, vol. 31, pp. 53 – 95.

--- (2009), *Theory of Probability. Historical Essay*. Berlin. **S, G,** 10.

Zhuravsky D. P. (1846), *Ob Istochnikakh i Upotreblenii Statisticheskikh Svedeniy* (On the Sources and Use of Statistical Information). Kiev.

D. Landau, P. F. Lazarsfeld

They (1978) published a superficial paper containing mistakes. Thus, they equated moral statistics with sociology (p. 822, right column); the Quetelet law of error to which they referred (p. 828, right column) was only a misunderstanding: apart from the binomial and normal distributions, Quetelet introduced asymmetric curves of mean tendencies to marriage and crime, and he knew that such curves also occur in meteorology. And it is a mistake to call his *Sur l'homme* a greatest book of the 19th century (end of their paper).

Landau D., Lazarsfeld P. F. (1978), Quetelet. In W. Kruskal, J. M. Tanur, Editors, *Intern. Enc. of Statistics*, vols 1 – 2. New York − London, pp. 824 – 834.

Sheynin O. (1986), Quetelet as a statistician. *Arch. Hist. Ex. Sci.*, vol. 36, pp. 281 – 325.

P. S. Laplace

Laplace described his reasoning too concisely and sometimes carelessly, and many authors complained that it is extremely difficult to understand his works.

Laplace is extremely careless in his reasoning and in carrying out formal transformations (Gnedenko & Sheynin 1978/2001, p. 224).

Thwarting the efforts of his predecessors (Jacob Bernoulli, De Moivre, Bayes), Laplace (1812) transferred the theory of probability to applied mathematics. Indeed, many of his proofs were non-rigorous, and, what should not have been required of his forerunners, he had not introduced either densities or characteristic functions as mathematical objects. Here is Markov's remark in his report of 1921 partly extant in the Archive of the Russian Academy of Sciences (Sheynin 2006, p. 152):

The theory of probability was usually regarded as an applied science in which mathematical rigor was not necessary.

It was Lévy (1925) who made the first essential step to return probability to the realm of pure science. He (Cramér 1976, p. 516) provided

The first systematic exposition of the theory of random variables, their probability distributions and their characteristic functions.

Laplace (1812) made a mistake when studying the problem of the *Buffon needle*, and, when calculating the population of France by sampling, he had chosen an unsuitable model and presented his final result in a hardly understandable manner (1812/1886, pp. 399 and 401) so that Poisson (1812) misunderstood it. Laplace (1814/1995, p. 40) later corrected his negligence.

Laplace (1814/1995, p. 81) most strangely described the compilation of mortality tables, and the same is true about both his statement (1819) on the study of refraction and about the compilation of astronomical tables without even mentioning the inherent systematic errors (1812, § 21). Laplace (1814/1995, p. 40) explained an unusual sex ratio in Paris by *rustic or provincial parents sending relatively fewer boys than girls* […] *to the Foundling Hospital* in that city. He had not, however, corroborated this conclusion by statistical data from, say, London.

Laplace's theory of errors, which he had not abandoned in spite of the work of Gauss, was insufficiently justified and barely useful. Finally, contrary to Newton, Laplace (1796/1884, p. 504) stated that the eccentricities of the planetary orbits were due to *countless variations in the temperatures and densities of the diverse parts* of the planets. In 1813, appeared the last, during his lifetime, edition of that book, but Laplace had not corrected his mistake. Fourier (1829, p. 379) had not noticed, or did not want to mention, Laplace's failure.

Laplace possibly borrowed that wrong idea from Kant (1755/1910, 1. Hauptstück, p. 269; 8. Hauptstück, p. 337) or even Kepler.

Cramér H. (1976), Half a century with probability theory. *Annals Prob.*, vol. 4, pp. 509 – 516.

Fourier J. B. J. (1831, in French), Historical Eloge of the Marquis De Laplace. *Lond., Edinb. and Dublin Phil. Mag.*, ser. 2, vol. 6, 1829, pp. 370 – 381.

Gnedenko B. V., Sheynin O. (1978, in Russian), Theory of probability. A chapter in *Mathematics of the 19th Century*, vol. 1. Basel, 1992, 2001, pp. 211 – 288. Editors, A. N. Kolmogorov, A. P. Youshkevich.
Kant I. (1755), *Allgemeine Naturgeschichte und Theorie des Himmels* etc. *Ges. Schriften*, Abt. 1, Bd. 1. Berlin, 1910, pp. 215 – 358.
Laplace P. S. (1796), *Exposition du système de monde*. *Oeuvr. Compl*., t. 6. Paris, 1884. Reprint of the edition of 1835.
--- (1812), *Théorie analytique des probabilités. Oeuvr. Compl.*, t. 7. Paris, 1886.
--- (1814, in French), *Philosophical Essay on Probabilities*. New York, 1995. Translated by A. Dale.
--- (1819), Sur l'application du calcul des probabilités aux observations etc. *Oeuvr. Compl.*, t. 14. Paris, 1912, pp. 301 – 304.
Lévy P. (1925), *Calcul des probabilités*. Paris.
Poisson S.-D. (1812). *Nouv. Bull. des Sciences Soc. Philomatique de Paris*, t. 3, pp. 160 – 163.
Sheynin O. (2006, in Russian), On the relations between Chebyshev and Markov. *Istoriko-Matematicheskie Issledovania*, vol. 11 (46), pp. 148 – 157.
--- (2009), *Theory of Probability. Historical Essay*. Berlin. **S, G,** 10.

L. Le Cam

Le Cam (1986, p. 81) ungenerously stated that

Bertrand and Poincaré wrote treatises on the calculus of probability, a subject neither of the two appeared to know.

In those times no one *knew* probability theory, and only later Markov came to the fore.

Le Cam L. (1986), The central limit theorem around 1935. *Stat. Sci.*, vol. 1, pp. 7 – 96.

B. Lécuyer, A. R. Oberschall

In their superficial essay, the authors (1978) forgot about Bismarck's social laws. They had not provided any information about Italy or India, and certainly ignored the zemstvo statistics. They, just like some other authors, confused the French and the Paris academies of sciences and they also expressed dubious or unclear statements. Thus, they mentioned Quetelet's discussion of budgets without specifying: budget of crime.

Lécuyer B., Oberschall A. R. (1978), Social research, early history of. In W. Kruskal, Judith M. Tanur, Editors, *Intern. Enc. of Statistics*, vol. 2. New York, pp. 1013 – 1031.

A. M. Legendre

Legendre (1805) introduced the principle of least squares though justified it only qualitatively and made two mistakes. First, he equated the observational errors with the residual free terms of the initial equations. Second, according to the context, it followed that least squares ensure the least possible interval between the extreme absolute errors (again: extreme … residuals). Actually, this is what the minimax method ensures.

In a letter of 1809 to Gauss, who (although mentioning Legendre) called least squares *his own* principle, Legendre declared that priority was only attained by publication. Gauss, regrettably, did not answer, and in 1820 Legendre publicly accused him of appropriating his discovery. This episode infuriated French scientists, see *Poisson*. Legendre could have not written his letter, but on a later occasion stated that Gauss contradicted conventional practice.

Legendre A. M. (1805), *Nouvelles méthodes pour la détermination des orbites de comètes*. Paris.
--- (1820), *Nouvelles méthodes* …, Suppl. 2. Paris.

G. W. Leibniz

His manuscript (1680 – 1683, published 1866) was extremely unfortunate. He mistakenly decided that the probability of achieving 7 points after a toss of two dice was thrice (actually, six times) higher than the probability of 12 points. He had not separated mean and probable durations of life and introduced arbitrary assumptions. The strangest of all of them, see the end of that work, was this: nine or ten times more babies can be born than it really happens.

It is senseless to discuss his carelessly compiled manuscript of 1682, also published in 1866, since he possibly regarded it as a draft.

Leibniz G. W. (1680 − 1683, 1866), Essai de quelques raisonnements nouveau sur la vie humaine. *Hauptschriften zur Versicherungs- und Finanzmathematik*. Editor, E. Knobloch. Berlin, 2000, pp. 428 – 445, with a German translation.
--- (1682, 1866), Quaestiones. Ibidem, pp. 520 – 523, with a German translation.

P. Lévy

Lévy (1925, p. vii) maintained that, without the application to the theory of errors, his contribution (his main work on stable laws of distribution) would have been useless. But for that theory his book was absolutely useless! He insisted that a real estimation of the precision of observations was only possible if the appropriate law of distribution was stable. However, laws of distribution are never known; the presence of the Cauchy (actually, the Poisson) stable law means, nevertheless, that the observations are deficient, and in this practically useless case Lévy was in the right.

Lévy (p. 79) mistakenly thought that the method of least squares is only applicable in case of the (stable) normal law. I (1995) have described his work.

Lévy P. (1925), *Calcul des probabilités*. Paris.

Sheynin O. (1995), Density curves in the theory of errors. *Arch. Hist. Ex. Sci.*, vol. 49, pp. 163 – 196.

W. Lexis

Lexis is deservedly called the originator of the Continental direction of statistics, but he had not got rid of dated understandings. He (1877, p. 17) stated that equally possible cases of the appearance of the studied event can be assumed if (!) its statistical probability tended to its theoretical probability, but he (p. 14; 1886, p. 437) also maintained that the theory of probability was subjective since it was based on the existence of such cases. Even later he (1903, pp. 241 – 242) noted that the existence of such cases was necessary *for the pattern of the theory of probability*.

Bortkiewicz L. von (1915), Wilhelm Lexis. *Bull. Intern. Stat. Inst.*, vol. 20, No. 1, pp. 328 – 332.

Lexis W. (1877), *Zur Theorie der Massenerscheinigungen in der menschlichen Gesellschaft.* Freiburg i. B.

--- (1886), Über die Wahrscheinlichkeitsrechnung und deren Anwendung auf der Statistik. *Jahrbücher f. Nationalökonomie u. Statistik*, Bd. 13 (47), pp. 433 – 450.

--- (1903), *Abh. zur Theorie der Bevölkerungs- und Moralstatistik.* Jena.

A. M. Liapunov

Liapunov (1895/1946, pp. 19 – 20) called the Riemann ideas abstract, pseudo-geometric and sometimes fruitless, having nothing in common with *deep geometric investigations* of Lobachevsky. He forgot that in 1871 Klein presented a unified picture of the non-Euclidean geometry whose particular cases were the works of both Riemann and Lobachevsky. And here is Bernstein (1945/1964, p. 427) who was satisfied with the likely, but should have known better: Liapunov

Understood and was able to appreciate the achievements of the West European mathematicians, made in the second half of the [19th] *century, better than the other representatives of the* [Chebyshev] *Petersburg school*.

Bernstein S. N. (1945, in Russian), On Chebyshev's work on the theory of probability. *Sobranie Sochineniy* (Coll. Works), vol. 4. Moscow, 1964, pp. 409 – 433. **S, G,** 6.

Liapunov A. M. (1895, in Russian), P. L. Chebyshev. In P. L. Chebyshev. *Izbrannye Matematicheskie Trudy* (Sel. Math. Works). Moscow – Leningrad, 1946, pp. 9 – 21. **S, G,** 36.

P. C. A. Louis

Louis (1825) calculated the frequencies of the symptoms of various diseases to assist diagnosing. He (pp. xvii – xviii) even thought that, given sufficient observations, physicians ought to make the same conclusions. Tolstoy (1884 – 1886/2003, p. 27) apparently criticized that numerical method:

The only problem was to compare the probabilities of a floating kidney, a chronic catarrh and appendicitis. The problem was not about Ivan Ilyich's life.

He was late: the numerical method remained in vogue only for about 25 years. Gavarret (1840, p. 10) severely criticized it even before that:

It is impossible to see a scientific method and [or] *general philosophy in the numerical method*.

However, that method is an aid to investigations. Astronomical yearbooks, for example, are just collections of data. Another example out of many is Babbage (1857), see also *Proctor*.

Babbage C. (1857), On tables of constants of nature and art. *Annual Rept Smithsonian Instn* for 1856, pp. 289 – 302.

Gavarret J. (1840), *Principes généraux de statistique médicale*. Paris. German translation: Erlangen, 1844.

Louis P. C. A. (1825), *Recherches anatomico-pathologiques sur la phtisie*. Paris.

Tolstoy L. N. (1884 – 1886, in Russian), The death of Ivan Ilyich. In *The Death of Ivan Ilyich and Master and Man*. New York, 2003, pp. 3 – 59.

A. F. Lueder

Lueder (1817, p. V) aimed at *destroying statistics and politics which is tightly connected with it*. He (p. IX) also declared that *statistics was similar to astrology*. At that time, statistics was not yet duly separated from some other disciplines (see *Schlözer*), but it was never linked with astrology.

Arbitrary interpretation of statistical results is indeed possible, witness the saying wrongly (Sheynin 2003) attributed to Disraeli: *Lies, damned lies and statistics* … No one except Lueder thought of destroying statistics, but for a very long time it remained unworthy, it *Became the object of mockery* (Obodovsky 1839, p. 102); *About fifteen years ago statistics had* [still] *been all but an object of jibing* (Anuchin 1872, the very beginning of book). Quite a few other authors up to 1883 can be additionally cited (Sheynin (2003).

When reading sources of the very end of the 18th century, I began thinking that the author of that saying was Leonard Henry Courtney, and looked him up in the Internet. And so he was, but I did not see the provided source: *The Nat. Rev*., No. 26, pp. 21 – 26 (p. 25). London, 1895.

Anuchin E. (1872), *Znachenie Statistiki kak Nauki* … (The Significance of Statistics As a Science). Petersburg.

Lueder A. F. (1817), *Kritische Geschichte der Statistik.* Göttingen.

Obodovsky A. (1839), *Teoria Statistiki*. Petersburg.

Sheynin O. (2003), Lies, damned lies and statistics. *Intern. Z. f. Geschichte u. Ethik d. Naturwiss., Techn. u. Med.*, Bd. 11, pp. 191 – 193.

V. P. Lysenko

In one of his French papers, Buniakovsky called his invented calculating device an *équerre*, and Lysenko (1994) translated it as *eker*. However, that Russian word denotes a device for producing angles of 45 and 90° in the field and the title of Lysenko's paper is meaningless. His paper (2000) contains many mistakes and, anyway, its scientific level is low.

Lysenko V. P. (1994, in Russian), A summing eker of Buniakovsky. *Istoriko-Matematicheskie Issledovania*, vol. 35, pp. 17 – 22.

--- (2000, in Russia), The method of least squares in Russia in the 19th century. Ibidem, vol. 5 (40), pp. 333 – 361.

L. E. Maistrov

He was a petty Marxian philosopher turned (a weak) mathematician with a poor knowledge of German, his only foreign language he claimed to know. His book (1967) was translated into English for want of anything better. It contains lengthy expositions of classical results almost in the original phraseology and very often without any interesting comments.

Maistrov L. E. (1967, in Russian), *Probability Theory. A Historical Sketch*. New York – London, 1974.

A. A. Markov

Markov was too peculiar and his aspiration for rigor often turned against him. In 1910, he (Ondar 1977/1981, p. 52) declared that he *will not go a step out of that region where my competence is beyond any doubt*. This possibly explains why he did not even hint at applying his *chains* to natural science and why, being Chebyshev's student, he underestimated the [theoretical] significance of the axiomatic direction of probability or the theory of the functions of complex variable (A. A. Youshkevich 1974, p. 125).

Markov refused to apply such terms as *random magnitude* (the Russian expression), *normal distribution* or *correlation coefficient*. He did not number his formulas but rewrote them (even many times), did not recognize demonstrative pronouns and the structure of his *Treatise* (1900) became ever more complicated from one edition to another. And in spite of his glorification by Bernstein (1945/1964, p. 425) and Linnik et al (1951, statement about number theory, p. 615), I categorically refuse to consider Markov an exemplary author in the methodical sense. He himself (Ondar 1977/1981, p. 21) *often heard that my presentation* [his presentation of the method of least squares] *is not sufficiently clear*. Then, Linnik et al (1951, p. 637) maintained that Markov *in essence introduced new important notions identical with the now current concepts of unbiased and effective statistics*. Actually, they should have mentioned Gauss instead.

Markov (following quite a few other authors) defended Gauss′ second justification of the method of least squares, but stated that he (1899/1951, p. 246) does *not ascribe the ability of providing the most probable or most plausible results to that method and only consider*[s] *it as a general procedure which furnishes approximate values of the unknowns along with a hypothetical estimate of the results obtained*.

He thus destroyed his own defence of the method. At the end of his life Markov's health seriously deteriorated and the general situation in Russia became horrible which most essentially additionally affected his work. However, he hardly recognized Pearson, never mentioned Yule or Student and the references in the posthumous edition of his *Treatise* (1924) were the same as in the previous edition of 1913. Finally, Markov somehow decided that he transferred probability to the realm of pure science. See Sheynin (2006).

Many authors had remarked that Markov was very rude and sometimes unjust. Here is the clearest statement to this effect (Chirikov & Sheynin 1994, letter of 24 Oct. 1915 from K. A. Andreev to P. A. Nekrasov):

Markov remains an old inveterate sinner with respect to provoking controversies. I understood it long ago and decided that the only possibility to escape the bait of that provoker consists in passing over in silence any of his attacks.

Bernstein S. N. (1945, in Russian), Chebyshev's work in the theory of probability. *Sobranie Sochinenii* (Coll. Works), vol. 4. Moscow, 1964, pp. 409 – 433. **S. G,** 5.

Chirikov M. V., Sheynin O. (1994, in Russian), The correspondence between P. A. Nekrasov and K. A. Andreev. *Istoriko-Matematicheskie Issledovania*, vol. 35, pp. 124 – 147.

Linnik Yu. V. et al (1951, in Russian), Sketch of the work of Markov in number theory and theory of probability. In Markov (1951, pp. 614 – 640). Partly translated: **S, G,** 5.
Markov A. A. (1899, in Russian), The law of large numbers and the method of least squares. In Markov (1951, pp. 230 – 251).
--- (1900), *Ishislenie Veroiatnostei* (Calculus of Probability). Later editions: 1908, 1913, posthumous edition Moscow, 1924. German edition 1913.
--- (1951), *Izbrannye Trudy* (Sel. Works). No place.
Ondar Kh. O., Editor (1977, in Russian), *Correspondence between Markov and Chuprov* etc. New York, 1981.
Sheynin O. (2006), Markov's work on the treatment of observations. *Hist. Scientiarum*, vol. 16, pp. 80 – 95.
Youshkevich A. A. (1974), Markov. *Dict. Scient. Biogr.*, vol. 9, pp. 124 – 130.

D. I. Mendeleev

Mendeleev(1895/1950, p. 159) recommended to choose the arithmetic mean rather than the median even if the *relative worth of the measurements is absolutely unknown* and never referred to the second justification of least squares.

Here is another statement which needs to be corrected, Mendeleev (1875/1950, p. 209):

The probable conclusion […] *perfectly agrees here with the arithmetic mean, and this indicates that the* [observational] *errors follow a definite law assumed by the Gauss theory of probability* [of errors]*, i. e., that the observations do not contain large random deviations, but are subject to unavoidable observational errors.*

Probable conclusion is an imprecise and therefore unfortunate substitute for median. In 1809, Gauss arrived at a *definite* law by assuming that its mode (not median!) coincided with the arithmetic mean. That definite (the normal) law nevertheless allows large errors to happen (with low probabilities). And the introduction of both random and observational errors is disturbing.

Mendeleev (1876/1946, p. 267; 1885/1952, p. 527) reasonably argued against the unnecessary amassing of observations, although here he repeated many previous authors (Lueder 1812, p. 9; Obodovsky 1839, p. 102; Biot 1855; Airy ca. 1867 as quoted by De Morgan 1915, p. 85). Airy doubted that the addition of *millions of unnecessary observations to the millions already made* will lead to the establishment of a meteorological theory.

A qualification is necessary. Descartes (1637/1982, p. 63) had remarked that *expériences* become ever more necessary with the advance of knowledge, and Bradley (1748, p. 2) stated the same.

And so, sampling had been introduced in statistics with great difficulties (You Poh Seng 1951). Quetelet (1846, p. 293) argued against it, Bortkiewicz (1904, p. 825) mentioned tentative calculations (Konjektural-Berechnung), and Czuber (1921) in spite of the title of his book had not considered sampling.

Biot J. B. (1855), Sur les observatoires météorologiques etc. *C. r. Acad. Sci. Paris*, t. 41, pp. 1177 – 1190.
Bortkiewicz L. von (1904), Anwendung der Wahrscheinlichkeitsrechnung auf Statistik. *Enc. math. Wiss.*, Bd. 1, pp. 821 – 851.
Bradley J. (1748), Letter … concerning an apparent motion observed in some of the fixed stars. *Phil. Trans. Roy. Soc.*, vol. 45, pp. 1 – 43.

Czuber E. (1921), *Die statistische Forschungsmethode*. Wien.
De Morgan A. (1915), *Budget of Paradoxes*, vol. 1. Chicago – London.
Descartes R. (1637), *Le discourse de la méthode et les essais*. *Oeuvres*, t. 6. Paris.
Lueder A. F. (1812), *Kritik der Statistik und Politik*. Göttingen.
Mendeleev D. I. (1875, in Russian), Progress of work on the restoration of the prototypes of measures of length and weight. *Sochinenia* (Works), vol. 22, 1950, pp. 175 – 213.
--- (1876, in Russian), On the temperatures of the atmospheric layers. Ibidem, vol. 7, 1946, pp. 241 – 269.
--- (1885, in Russian), Note on the scientific work of A. I. Voeikov. Ibidem, vol. 25, 1952, pp. 526 – 531.
--- (1895, in Russian), On the weight of a definite volume of water. Ibidem, vol. 22, 1950, pp. 105 – 171.
--- (1934 – 1952), *Sochinenia* (Works), vols 1 – 25. Moscow – Leningrad.
Obodovsky A. G. (1839), *Teoria Statistiki.* Petersburg.
Quetelet A. (1846), *Lettres sur la théorie des probabilités*. Bruxelles.
You Poh Seng (1951), Historical survey of the development of sampling theories and practice. *J. Roy. Stat. Soc.*, vol. A114, pp. 214 – 231. M. G. Kendall & R. L. Plackett, Editors (1977), *Studies in the History of Statistics and Probability*, vol. 2. London, pp. 440 – 458.

E. Mendoza

Mendoza (1991, p. 283) attempted to single out astronomy and geodesy from other natural sciences. He indicated that there, the observer makes

Many repeated measurements of the same simple quantity which has *no obvious relevance to a small number of complex measurements of atomic weight or specific heat*.

This is incomprehensible. In astronomy and geodesy, methods of observation are developed, instruments have to be adjusted and several corrections to the *simple quantities* applied. In spite of all this, systematic errors corrupt observations and are no less dangerous than impurity of samples is in chemistry. For geodesy, the difference exists, although elsewhere: a chain of triangulation is measured only once, whereas physical, chemical (and astronomical) constants can be measured in many places and over a long period of time. Did Mendoza *at least see* a theodolite of even the 18th century?

Mendoza E. (1991), Physics, chemistry and the theory of errors. *Arch. Intern. Hist. Sci.*, t. 41, pp. 282 – 306.

J. S. Mill

Here is his celebrated but partly unreasoned statement (1843/1886, p. 353):

A very slight improvement in the data by better observations, or by taking into fuller consideration the special circumstances of the case, is of more use than the most elaborate application of the calculus of probabilities founded on the data in their previous state of inferiority. The neglect of this obvious reflection has given rise to misapplications of the calculus of probability which have made it the real opprobrium of mathematics. It is sufficient to refer to the applications made of it to the credibility of witnesses, and to the correctness of the verdicts of juries.

The first part of this declaration is indeed important and conforms to the opinion of Gauss (ca. 1841; *Werke*, Bd. 12, pp. 401 – 404): applications of the theory of probability can be greatly mistaken if the essence of the studied object is disregarded.

Its second part, however, testifies that Mill was not familiar with the well-known investigation of Poisson.

Mill J. S. (1843), *System of Logic*. London, 1886. Many more editions, and included in Mill's *Coll. Works*, vol. 8. Toronto, 1974.

P. A. Nekrasov

His works on the theory of probability and statistics are known to become, after ca. 1900, unimaginably verbose, corrupted by mathematical mistakes and unclear statements and connected with moral, political and religious considerations. (Nothing similar happened with his works, say, in mechanics.)

The culmination of his work in probability should have been the proof of the central limit theorem for large deviations, but he was unable to publish anything intelligible here mainly since he approached his subject in a purely analytic rather than stochastic way, see Soloviev (1997), Bortkiewicz (1903) and Sheynin (2003).

Here is the opinion of one of his colleagues, K. A. Andreev, in a letter to Liapunov of 1901, see Gordevsky (1955, pp. 40 – 41):

Nekrasov reasons perhaps deeply but not clearly and he expresses his thoughts still more obscurely. I am only surprised that he is so self-confident. In his situation, with the administrative burden weighing heavily upon him, it is even impossible, as I imagine, to have enough time for calmly considering deep scientific problems, so that it would have been better not to study them at all.

In 1896, Nekrasov accepted the *candidate composition* of Chuprov, a final-year student of Moscow University but only left marginal notes in its first part (Sheynin 1990/2011, pp. 109 – 110). He hardly read the other parts of the composition and, if he had not, he deprived Chuprov of really necessary advice. Indeed, by that time he published lithographic editions of his course in probability theory in 1888 and 1894. Curiously enough, he was hardly conversant with statistics: he did not understand the term *variance* (Chuprov, Letter No. 5 in Bortkevich & Chuprov 2005).

After 1917, Nekrasov became *a queer shadow of the past*, *seemed decrepit physically and mentally* (Liusternik 1967, p. 222). His course on the theory of probability (year not provided) was *absolutely useless* (Beskin 1993, pp. 168 – 169).

Beskin N. M. (1993, in Russian), Recollection about the physical and mathematical faculty of Moscow University at the beginning of the 1920s. *Istoriko-Matematich. Issledovania*, vol. 34, pp. 163 – 184.

Bortkevich V. I. (1903, in Russian), The theory of probability and the struggle against sedition. *Osvobozhdenie*, bk 1. Stuttgart. Partly translated: **S, G,** 4. The article was obviously only printed in some copies of that journal since two or three other copies which I saw had not contained it. Bortkevich only signed his article there by letter B and revealed his name in 1910 (*Zhurnal Ministerstva Narodnogo Prosveschenia*, p. 353). In Russia, *Osvobozhdenie* was distributed illegally.

Bortkevich V. I., Chuprov A. A. (2005, in Russian), *Perepiska* (Correspondence) *(1895 – 1926)*. Berlin. **S, G,** 9.

Gordevsky D. Z. (1955, in Russian), *K. A. Andreev*. Kharkov.

Liusternik L. A. (1967, in Russian), The youth of the Moscow mathematical school. *Uspekhi Matematich. Nauk*, vol. 22, No. 2, pp. 199 – 239. Second part of the paper. That journal is being translated as *Russ. Math. Surveys*.

Sheynin O. (1990, in Russian), *Alexandr A. Chuprov. Life, Work, Correspondence*. V&R Unipress, 2011.

--- (2003), Nekrasov's work on probability: the background. *Arch. Hist. Ex. Sci.*, vol. 57, pp. 337 – 353.

Soloviev A. D. (1997, in Russian), Nekrasov and the central limit theorem etc. *Archives Intern. d'Hist. des Sciences*, t. 58, NNo. 160 – 161, pp. 353 – 364. My translation.

S. Newcomb

Newcomb (1886) discovered that in a long series of astronomical observations their precision can change. Unlike his predecessors, he considered in detail the ensuing consequences but he had to choose subjectively the appropriate measures of precision and his calculations proved too complicated.

Lehmann-Filhés (1887) somewhat changed Newcomb's premise by supposing that the measure of precision is a continuous random variable obeying its own normal law rather than being a discrete variable. Then Ogorodnikov (1928, 1929a), without referring to Lehmann-Filhés, removed his restriction (the normal law) and (1929b) generalized the problem still more.

I believe that the proposals of both these authors were only interesting in the methodological sense. And Hulme & Symms (1939, p. 644) discovered that some simplifications also suggested by Newcomb connected his proposal with the principle of maximal likelihood. See also *Claisius*.

Hulme H. R., Symms L. S. T. (1939), The law of error and the combination of observations. *Monthly Notices Roy. Astron. Soc.*, vol. 99, pp. 642 – 649.
Lehmann-Filhés R. (1887), Über abnorme Fehlerverteilung etc. *Astron. Nachr.*, Bd. 117, pp. 121 – 132.
Newcomb S. (1886), A generalized theory of the combination of observations. *Amer. J. Math.*, vol. 8, pp. 343 – 366.
Ogorodnikov K. F. (1928), A method for combining observations etc. *Astron. Zhurnal,* vol. 5, № 1, pp. 1 – 21.
--- (1929a), On the occurrence of discordant observations etc. *Monthly Notices Roy. Astron. Soc.*, vol. 88, pp. 523 – 532.
--- (1929b), On a general method of treating observations. *Astron. Zhurnal,* vol. 6, pp. 226 – 244.
Sheynin O. (1995), Density curves in the theory of errors. *Arch. Hist. Ex. Sci.*, vol. 49, pp. 163 – 196.
--- (2002), Simon Newcomb as a statistician. *Hist. Scientiarum*, vol. 12, pp. 142 – 167.

R. R. Newton

The author (1977, p. 379; 1980, p. 388) called Ptolemy *the most successful fraud in the history of science*. I (1993) have collected the statements made by many commentators about Ptolemy. They included positive opinions of Laplace (of 1796) and Newcomb (of 1878) whereas Kepler (1609/1992, p. 642) reservedly formulated a similar statement:

We have hardly anything from Ptolemy that we could not with good reason call into question prior to its being of use to us at the requisite degree of accuracy.

Koyré (1956/1968, p. 150) remarked that

The scientific literature of the seventeenth century – and not only of the seventeenth century – is full of those fictitious experiments.

To return to Ptolemy: perhaps he borrowed from Hipparchus, but in those times everyone knew what was done by whom, and the practice of borrowing was not scorned.

Gingerich O. (1980), Was Ptolemy a fraud? *Q. J. Roy. Astron. Soc.*, vol. 21, pp. 253 – 266.

Kepler J. (1609, in Latin), *New Astronomy*. Cambridge, 1992.

Koyré A. (1956), Pascal savant. In author's book *Metaphysics and Measurement*. London, 1968, pp. 131 – 156.

Newton R. R. (1977), *The Crime of Claudius Ptolemy*. Baltimore – London.

--- (1980), Commentary on Gingerich (1980). *Q. J. Roy. Astron. Soc.*, vol. 21, pp. 388 – 399.

Sheynin O. (1993), Treatment of observations in early astronomy. *Arch. Hist. Ex. Sci.*, vol. 46, pp. 153 – 192.

J. Neyman

Neyman (1934, p. 595) mistakenly attributed to Markov the second Gaussian justification of least squares of 1823. David & Neyman (1938) repeated that mistake, but then Neyman (1938/1952, p. 228) admitted it. Still, that mistake is alive (see *Kotz*). H. David (after 2001) noted, in an unpublished manuscript, that it was Lehmann (1951) who invented that unfortunate name. Neyman's wrong initiative seems strange since he (1934, p. 593) contradicted himself:

The importance of the work of Markov concerning the best linear estimates consists, I think, chiefly in a clear statement of the problem.

David F. N., Neyman J. (1938), Extension of the Markoff theorem on least squares. *Stat. Res. Mem.*, vol. 2, pp. 105 – 117.

Lehmann E. L. (1951), A general concept of unbiasedness. *Annals Math. Stat.*, vol. 22, pp. 587 – 592.

Neyman J. (1934), On two different aspects of the representative method. *J. Roy. Stat. Soc.*, vol. 97, pp. 558 – 625. In author's book (1967), *Selection of Early Statistical Papers*. Berkeley, pp. 98 – 141.

--- (1938), *Lectures and Conferences on Math. Statistics and Probability*. Washington, 1952.

Kh. O. Ondar

I knew him well. He hardly read any foreign language and his mathematics was poor, but he was a *nazmen* (supported by authorities since he belonged to a national minority) and a highly trusted citizen. Indeed, he lived in a student hostel of Moscow University in the same room with a few foreign students. He defended his candidate dissertation being supervised (apparently, mightily assisted) by Gnedenko. At least one of his papers (1970) and some of the comments in Ondar (1977) were way above his head.

In that latter work, I (Sheynin 1990/2011, pp. 103 – 108) have discovered about 90 mathematical mistakes and most of them had been transferred to its translation of 1981. Ondar had thus treated his archival source as a bull in a china shop, and the damage done by him will remain for a very long time.

Ondar Kh. O. (1970, in Russian), V. A. Steklov's paper on the theory of probability. *Istoria i Metodologia Estestvennych Nauk*, vol. 9, pp. 262 – 264.

--- (1977, in Russian), *The Correspondence between A. A. Markov and A. A. Chuprov on the Theory of Probability and Math. Statistics*. New York, 1981. Ondar was Editor of Russian edition.

Sheynin O. (1990, in Russian), *Aleksandr A. Chuprov. Life, Work, Correspondence*. V&R Unipress, 2011.

A. Orlov

Orlov (1990, pp. 67 – 69):

For many decades determinism had been preached to students (<u>*science is the enemy of the accidental*</u>). *We cannot reconcile ourselves anymore to the situation in statistics brought about by the legacy of Stalinism. The split in statistics, and the lack of necessary knowledge typical of many* <u>*specialists*</u> *are leading to an ever increasing lag behind the advanced nations with respect to the mass application of modern statistical methods.*

Only during the perestroika [Gorbachev's doomed attempt to reform the Communist regime] *the veil of secrecy began to open slightly. And methods of falsifying statistical data, which made it possible to create a semblance of well-being, were at once revealed. We reject the decisions of the All-Union conference of 1954 as impeding the perestroika. The mistaken attribution of statistics to the social sciences considerably delayed the development of national economy. A barrier had been erected between modern theoretical* (*mathematical*) *statistics and the agencies* [of government statistics] *whose activities were almost reduced to registration. Vast trustworthy statistics was not needed*, [it was] *even dangerous for the Soviet system*.

Gnedenko (1950, p. 8), perhaps responding to the wish of other mathematicians (Kolmogorov?), explained the situation:

Some hotheads, without gaining an understanding of what Academician (!) *Lysenko really said and being ignorant of the theory of probability, decided to declare a war against it*.

Anonymous (1954, in Russian), Account of the All-Union Conference on problems of statistics. *Vestnik Statistiki*, № 5, pp. 39 – 95.

Gnedenko B. V. (1950, in Russian), The theory of probability and cognition of the real world. *Uspekhi Matematich. Nauk*, vol. 5, pp. 3 – 23.

Orlov A. (1990, in Russian), On the perestroika of statistical science and its application. *Vestnik Statistiki*, № 1, pp. 65 – 71.

K. Pearson

He (1978) had not paid due attention to Halley's life table and did not notice his lines of equal magnetic declination (for the North Atlantic), i. e., his brilliant anticipation of the preliminary data investigation.

When discussing the law of Bernoulli, Pearson (1925) only criticized the practical uselessness of his estimate of the rapidity of the convergence of statistical probability to its theoretical counterpart (mostly occasioned by the ignorance of the not yet known formula of Stirling). He also inadmissibly compared the Bernoulli law with the wrong system of the world due to Ptolemy. Then, Pearson paid no attention either to the Bernoulli existence theorem or his philosophical reasoning. On the very first page of his book (1978) Pearson stated that

A most fundamental principle of statistics has been attributed to Bernoulli instead of its real discoverer De Moivre.

Fisher (1937, p. 306) discovered that the late Pearson had exonerated a falsified comparison of some statistical methods, and Pearson's son Egon kept silent.

When discussing the Daniel Bernoulli memoir of 1778 about the adjustment of observations and Euler's commentary of the same year, Pearson (1978, p. 269) once more only paid attention to the practical side of those contributions and most unjustifiably decided that they both

Seem to reach false conclusions by starting from arbitrary premises, but Euler more completely so than Bernoulli.

In the same utilitarian way, and even wrongly, Pearson commented on Euler's work on demography and insurance.

In an undated letter Chuprov (Sheynin 1990/2011, pp. 75 – 76) stated:

Because of Pearson's insufficiently rigorous, to their taste, approaches to mathematical problems, Continental mathematicians look down on him to such an extent, that they do not even bother to study his works.

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

Pearson K. (1925), James Bernoulli theorem. *Biometrika*, vol. 17, pp. 201 – 210.

--- (1978), *History of Statistics in the 17th and 18th Centuries*. London.

Sheynin O. (1990, in Russian), *Alexandr A. Chuprov: Life, Work, Correspondence*. V&R Unipress, 2011.

--- (2010), Karl Pearson a century and a half after his birth. *Math. Scientist*, vol. 35, pp. 1 – 9.

M. Pettenkofer

Pettenkoffer (1886 – 1887) published a monstrous collection of statistical materials pertaining to cholera, but was unable to process them. See *Mendeleev*.

Pettenkofer M. (1886 – 1887), Zum gegenwärtigen Stand der Cholerafrage. *Arch. f. Hyg*., Bd. 4, pp. 249 – 354, 397 – 546; Bd. 5, pp. 353 – 445; Bd. 6, pp. 1 – 84, 129 – 233, 303 – 358, 373 – 441; Bd. 7, pp. 1 – 81.

W. Petty

Petty was an eminent and extremely versatile scholar, but at the same time he was careless, light-hearted and biased (Greenwood (1942/1970, p. 73). Some of his statements are wildly wrong (p. 63) and sometimes (p. 64) belong to *the region of pure fantasy*.

Greenwood M. (1942), Petty's scientific work. *Biometrika*, vol. 32.
E. S. Pearson, M. G. Kendall, Editors (1970), *Studies in the History of Statistics and Probability*. London, pp. 61 – 73.

Jan von Plato

The author (1995) studied the history of the theory of probability, statistical physics and quantum theory from 1900. He superficially discussed stochastic processes and did not mention chaos. Slutsky was rendered lip service and the history of probability contained many mistakes and was not connected with events which happened after 1900. See also *Gorrochurn*.

Plato Jan von (1995), *Creating Modern Probability*. Cambridge.

H. Poincaré

In his main work (1896) Poincaré did not mention Russian mathematicians and, in addition (Bortkevich, letter of 1897 № 19 to Chuprov, see Bortkevich & Chuprov 2005),

The excessively respective attitude towards […] *Bertrand is surprising. No traces of a special acquaintance with the literature on probability are seen. The course is written in such a way as though Laplace and Poisson, especially the latter, never lived*.

Much else is also *surprising*. In a complicated way Poincaré (§§ 103 – 106) calculated the complete (i. e, the unknown) number of asteroids. Under his assumptions, that number could have been reckoned at once. He explained the uniform distribution of the asteroids along the ecliptic (§ 42) without referring to the ergodic property of homogeneous Markov chains with a finite number of possible states, and not really understandably. By applying this property rather than by means of hypercomplex numbers, Poincaré (§ 225) could have derived the uniform distribution of cards in a pack after their long shuffling.

Poincaré gave much consideration to the theory of errors, and he (1921/1983, p. 343) later indicated that that theory had *naturellement* been his main aim in probability. At the same time, however, he did not directly mention Gauss and made mistakes and (§ 127) did not recognize his second justification of least squares.

In ca. 1899 Poincaré, in a letter of ca. 1899, see *Procès* (1900, t. 3, p. 325) even generalized Mill to declare that probability ought not to study *moral sciences* and declared that the appropriate findings made by Condorcet and Laplace were senseless (he did not mention Poisson).

He (1896/1902/1923, p. 217) somehow decided that all the sciences were just an *unconscious application* of the calculus of probability, that the theory of errors and the kinetic theory of gases were based on the law of large numbers (wrong with respect to the former), but that the calculus of probability will evidently ruin them (*les entrainerait évidemment dans sa ruine*). No wonder that in his treatise on thermodynamics of 1892 he had not mentioned the statistical nature of that science.

In a popular booklet of 1907 Poincaré attempted to explain the notion of randomness and reprinted that material in the second edition (1912) of his main treatise, but I will only note that he had not mentioned the regularity of mass random events.

All in all, we see here an excellent example of a classic who barged in an alien field.

Bortkevich V. I., Chuprov A. A. (2005, in Russian), *Perepiska* (Correspondence) *1895 – 1926*. Berlin. **S, G,** 9.

Poincaré H. (1896, 1912, 1923, 1987), *Calcul des probabilités*. Paris.

--- (1902), *La science et l'hypothèse*. Paris, 1923, 1968.

--- (1905), *La valeur de la science*. Paris, 1970.

--- (1921), Résumé analytique [of his own works]. *Math. Heritage of H. Poincaré*. Providence, RI, 1983. Editor F. E. Browder, pp. 257 – 357.

Procès (1900), *Procès Dreyfus*, tt. 1 – 3. Paris.

Sheynin O. (1991), Poincaré's work in probability. *Arch. Hist. Ex. Sci.*, vol. 42, pp. 137 – 172.

L. Poinsot

When discussing a report made by Poisson, Poinsot (Poisson 1836, p. 380) resolutely argued against the application of the calculus of probability to *moral things* and called it a *dangerous illusion* and *false*. His statement contradicted Laplace's opinion (to whom he nevertheless referred). A decade later Zhuravsky (1846, p. 6) declared that *application of probability to social issues is premature*.

Here are similar pronouncements of two other authors.

Double (1837, pp. 362 – 363): For him, *each patient was a new and separate problem*. A correct but incomplete conclusion.

D'Amador (1837): *The application* [to medicine] *of probability* means the *application of chance* and medicine becomes *a lottery* (p. 14). The introduction of the calculus of probability into medicine is antiscientific (p. 31).

D'Amador R. (1837), *Sur le calcul des probabilités appliqué à la médecine*. Paris.

Double F. J. (1837, in French), Inapplicability of statistics to the practice of medicine. *Lond. Medical Gaz*., vol. 20, No. 2, 1837, pp. 361 – 364. Translated from the *Gaz. Médicale*.

Poisson S.-D. (1836), Note sur la loi des grands nombres. *C. r. Acad. Sci. Paris*, t. 2, pp. 377 – 382. With discussion.

Zhuravsky D. P. (1846), *Ob Istochnikakh i Upotreblenii Statisticheskikh Svedeniy* (On the Sources and Use of Statistical Information). Kiev.

In many cases he considered subjective probabilities. One of his examples (1837, § 11) led to probability 1/2, that is (§ 4), to *complete perplexity*. His conclusion agrees with the theory of information. Catalan (1884) later formulated a principle (in 1877 he called it a theorem): If the causes of the probability of an event changed in an unknown way, it remains as it was previously. Poisson (1825 – 1826) actually guided himself by that principle (which only applied to subjective probability) when studying a socially important card game.

Bortkiewicz (1894 – 1896, p. 661) formulated a wrong conclusion: *The difference between objective and subjective probability is unjustified since each probability presumes some knowledge, and some ignorance and is therefore necessarily subjective*.

Chetverikov (1968) translated Bortkiewicz' essay, and, on p 74, inserted Chuprov's marginal remark which he left on his copy of Bortkiewicz: *The difference, and not a small one, does exist*.

Poisson (1837) broadly interpreted his law of large numbers as a principle. He based the application of statistics (he had not used this term!) on large numbers. In a footnote to the Contents of his book (!) he declared that medicine ought to be based on large numbers, and his follower, Gavarret (1840), repeated this statement. Large numbers were indeed necessary in some branches of medicine (for example, in epidemiology), but Liebermeister (ca. 1876) resolutely opposed their use in therapeutics.

Poisson's book (1837) is corrupted by many misprints. The discussion of the Petersburg game (§ 25) and the Bayes principle (Introduction) is superficial. When considering the probability of possible verdicts, Poisson included too complicated and therefore useless cases of testimonies provided by witnesses.

The discussion of angle measurements in geodesy was meaningless since Poisson remained far from such work and, just as other French scientists except Laplace, did not recognize the appropriate results of Gauss. Their greatly exaggerated sympathy for Legendre turned against themselves.

Methodically following Laplace, Poisson often remained satisfied with non-rigorous proofs (e. g., did not examine the boundaries of the admitted errors), and his theory of probability still belonged to applied science.

Bortkiewcz L. von (1894 – 1896), Kritische Betrachtungen zur theoretischen Statistik , 3. Folge, Bd. 8, pp. 641 – 680; Bd. 9, pp. 321 – 360; Bd. 11, pp. 701 – 705.

Catalan E. C. (1884), Application d'un nouveau principe de probabilités. *Bull. Acad. Roy. des Sciences, des Lettres et des Beau-Arts de Belg*., 2^{me} sér., 46^{e} année, t. 44, pp. 463 – 468.

Chetverikov N. S., Editor (1968), *O Teorii Dispersii* (On the Theory of Dispersion). Moscow.

Gavarret J. (1840), *Principes généraux de statistique médicale*. Paris.

Liebermeister C. (ca. 1876), Über Wahrscheinlichkeitsrechnung in Anwendung auf therapeutische Statistik. *Sammlung klinischer Vorträge* No. 110 (Innere Med. No. 39). Leipzig, pp. 935 – 961.

Poisson S.-D. (1825 – 1826), Sur l'avantage du banquier au jeu de trente-et-quarante. *Annales math. pures et appl*., t. 16, pp. 173 – 208.

--- (1837, 2003), *Recherches sur la probabilité des jugements* etc. Paris. English text: Berlin, 2013. **S, G,** 52.

Sheynin O. (1978), Poisson's work in probability. *Arch. Hist. Ex. Sci.*, vol. 18, pp. 245 – 300.
--- (2002), Sampling without replacement. *Intern. Z. f. Geschichte u. Ethik d. Naturwissenschaften, Techn. u. Med*., Bd. 10, pp. 181 – 187.
--- (2012), Poisson and statistics. *Math. Scientist,* vol. 37, pp. 149 – 150.
--- (2013), Poisson et la statistique. In *Poisson. Les mathématiques au service de la science*. Palaiseau. Editor Yvette Kosmann-Schwarzbach, pp. 357 – 366.

T. M. Porter

His book (1986) abounds with mistakes and nothing positive can be said about it. Three short items in Grattan-Guinness' *Companion Enc.* (1994, vol. 2, Chapter 10) are extremely superficial and contain mistakes, inaccuracies and strange statements. Nothing sensible is (or could have been) contained in his paper (2003). The article (2004a) is mainly repeated in the book of the same year (2004b) where on p. 339 Porter indirectly called Pearson rather than Fisher the founder of modern mathematical statistics. That book is a superficial investigation, it contains unnecessary details but fails to report that Pearson was elected to the Royal Society or that Newcomb had insistently invited him to report at a forthcoming prestigious international congress. And there are other omissions, many mistakes and strangest statements, for example: *Even mathematicians cannot prove the fourth dimension*. The treatise of Thomson & Tait of 1867 (reprinted in 2002) is impudently called *standard Victorian*.

Quite recently, Porter was elected full member of the International Academy of the History of Science …

Grattan-Guinness I., Editor (1994), *Companion Enc. of the History and Philosophy of the Math. Sciences*, vols 1 – 2. London – New York.
Porter T. M. (1986), *The Rise of Statistical Thinking, 1820 – 1900.* Princeton. My review: *Centaurus*, vol. 31, 1988, pp. 171 – 172.
--- (2003), Statistics and physical theories. In Mary Jo Nye, Editor, *Modern Phys. and Math. Sciences.* Cambridge, pp. 488 – 504.
--- (2004a), Karl Pearson's Utopia of scientific education etc. In R. Seising et al, Editors, *Form, Number, Order etc. Festschrift for Ivo Schneider* etc. Stuttgart, pp. 339 – 352.
--- (2004b), *Karl Pearson* etc. Princeton − Oxford. My review: *Hist. Scientiarum*, vol. 16, 2006, pp. 206 – 209.

R. A. Proctor

Proctor (1873) plotted 324 thousand stars on his charts. He attempted to sidestep any theories on the structure of the stellar system, but the development of astronomy proved him wrong. Star charts had then existed; for example, Bessel (Repsold 1920) diligently compiled them, but no one apparently thought of leaving theories aside. See *Louis*.

Proctor R. A. (1873), Statement of views respecting the sidereal universe. *Monthly Notices Roy. Astron. Soc.*, vol. 33, pp. 539 – 552.

Repsold Joh. A. (1920), Friedrich Wilhelm Bessel. *Astron. Nachr.*, Bd. 210, NNo. 5027 – 5028, columns 160 – 214.

A. Quetelet

Quetelet was rich in ideas, but inconsistent and pathologically light-hearted. He thought that his *Homme moyen*, who was even physiologically impossible (mean stature incompatible with mean weight), became a specimen of the nation and even of mankind. His statement about the (relative) constancy of the number of criminal deeds was unjustified (Rehnisch 1876) and his imagined *law of random causes* proved absolutely incomprehensible. Nevertheless, he (1848, p. 267 and 45) additionally introduced a law of random variations, no doubt for good measure.

After the mid-1830s Quetelet's creative power apparently left him (Galton, letter of 1891, in Pearson 1914 – 1930, 1924, p. 420): Quetelet *made no progress from 1830 to this day*. Marx, in a letter of 1869 (Marx 1952, pp. 81 – 82), expressed the same opinion.

Marx K. (1952), *Briefe an L. Kugelmann*. Berlin.

Pearson K. (1914 – 1930), *Life, Letters and Labours of Fr. Galton*, vols 1, 2, 3A, 3B. Cambridge. Published, respectively, in 1914, 1924, 1930 and 1930.

Quetelet A. (1848), *Du système sociale.* Paris.

Rehnisch E. (1876), Zur Orientierung über die Untersuchungen und Ergebnissen der Moralstatistik. *Z. Philos. u. phil. Kritik*, Bd. 69, pp. 43 – 115.

Sheynin O. (1986), Quetelet as a statistician. *Arch. Hist. Ex. Sci.*, vol. 36, pp. 281 – 325.

N. L. Rabinovitch

Rabinovitch (1973) discussed marginal issues (inference by analogy, combinatorics) and maked too much about the claimed appearance of the law of large numbers and sampling. On p. 180 he explained that $x = y$ meant that x equals y, but obviously expected his readers to understand the so-called Bayes formula.

On p. 163 he attributed the introduction of *expectation* to Leibniz rather than to Huygens, identified randomness with uniform randomnness (p. 33), provided a wrong date for the publication of the Bayes memoir (1793) in his Bibliography and did not comment on Maimonides' statement that *the lives of women are gererally shorter than those of men* (p. 164).

Elsewhere, Rabinovitch (1970, p. 205/1977, p. 23) had wrongly stated that *the equal distributon of boys and girls* (he considered the sex ratio at birth) […] *seems to reflect natural law*.

Rabinovitch N. L. (1970), Combinations and probability in rabbinic literature. *Biometrika*, vol. 57, pp. 203 – 205. Reprinted in *Studies in History of Statistics and Probability*, vol. 2. Editors, Sir Maurice Kendall & R. L. Plackett. London, 1977, pp. 21 – 23.

--- (1973), *Probability and Statistical Inference in Ancient and Medieval Jewish Literature.* Toronto.

J.-M. Rohrbasser, J. Véron

The authors (2001, p. 85) connect Leibniz' reasoning on the cost of life annuities with his theory of monads, which seems far-fetched, and pay scant attention to political arithmetic which was the subject of his *Essai* (1680 – 1683/1866). There was much more in this *Essai*, see *Leibniz*. Their commentaries lack modern stochastic notions.

Leibniz(1680 − 1683, 1866), Essai de quelques raisonnements nouveau sur la vie humaine. *Hauptschriften zur Versicherungs- und Finanzmathematik*. Editor, E. Knobloch. Berlin, 2000, pp. 428 – 445, with a German translation.

Rohrbasser J.-M, Véron J. (2001), Leibniz et les raisonnements sur la vie humaine. Classiques de l'Economie et de la population. Etudes et enquêtes historique. Paris.

G. Rümelin

Rümelin (1867, p. 25) recognized that mortality tables are telling the sad truth: how many chances people have to continue living and to die during a certain period of time. At the same time he vigorously opposed, in himself, any inclination to crime in spite of the existence of some such mean inclination (Quetelet's innovation).

He simply did not understand that mean values are not applicable to individuals. Chuprov (1909/1959, p. 159, note 4) quoted another author to the effect that no one knows his future passions [and circumstances].

Chuprov A. A. (1909), *Ocherki po Teorii Statistiki* (Essays on the Theory of Statistics). Moscow, 1959.

Rümelin G. (1867), Über den Begriff eines sozialen Gesetzes. *Reden und Aufsätze*. Freiburg i/B – Tübingen, 1875, pp. 1 – 31.

A. L. Schlözer

His book (1804) is unworthy. The construction of many phrases is defective and their meaning is therefore difficult to understand. Population statistics is hardly mentioned, political arithmetic, Jacob Bernoulli and Arbuthnot are forgotten, mean values of measured magnitudes do not exist, and bibliographic description is often lacking or inadequate. Many unnecessary details are offered (§ 14: travellers wasted great amounts of money in poor ancient Rome).

Following Achenwall, Schlözer describes states by their remarkable features and explains how to interpret that term. Medical features (for example, repeated visitations of cholera or smallpox or the attitude of the authorities and population to inoculation of smallpox) do not, however, enter. It was perhaps Schlözer who originated the wrong tradition of completely separating population statistics from medical problems. A few decades later sanitary conditions of life began to be studied statistically in England in connection with the Industrial Revolution and its dire consequences, but that separation somehow persisted.

This is strange; even Leibniz (1680a) paid special attention to those problems. Elsewhere he (1680b) advocated the compilation of state tables with numerical data and their comparison over time and between states.

And here we recall Schlözer's pithy saying (p. 86): *History is statistics flowing and statistics is history standing still*. Commentators did not notice that Schlözer was deadly wrong: First, the real significance of statistics was the study of such comparisons; it does not at all stand still. Second, what, then, is history? I ought to add: For Schlözer, his saying was only an illustration but many later statisticians regarded it as definitions of the two sciences. A special point is here that Schlözer himself (p. 37) (independently) repeated Leibniz' idea!

Schlözer gave much thought to the separation of statistics from history, geography and politics, but failed to formulate clear and definite conclusions. Incidentally, the title of his book placed statistics under politics. Finally, in spite of its title, he did not discuss any theory of statistics. This, however, was typical for many authors and even for *Chuprov*. It seems that the tacit general feeling was that *theory* meant an orderly presentation of the remarkable features of a given state concerning its territory and the population with its activities.

Obodovsky (1839, p. 2) seems to be the only author who defined *theory*: *It ought to discern, estimate, collect and order statistical sources*. This, indeed, is the aim of preliminary data investigation, an important part of the theory of statistics.

In spite of my negative opinion about Schlözer's book, I quote A. I. Chuprov (1910, p. 27), father of his better known son: Schlözer is a *man of great intellect and vast knowledge.*

Chuprov A. I. (1910), *Kurs Statistiki* (Course in Statistics). Moscow. Lectures at Moscow University delivered a few decades earlier.

Leibniz G. W. (1680a), Von Bestellung eines Registratur-Amtes. Berlin, 1986, pp. 376 – 381. First published in 1866.

--- (1680b), Entwurf gewissen Staatstafeln. Ibidem, pp. 340 – 349. First published in 1866.
--- (1986), *Sämtliche Schriften und Briefe*, 4. Reihe, Bd. 3. Berlin.
Obodovsky A. (1839, in Russian), *Teoria Statistiki*. Petersburg.
Schlözer A. L. (1804), *Theorie des Statistik nebst Ideen über das Studium der Politik überhaupt*. Göttingen.

I. Schneider

Schneider (2005) unnecessarily proved that Price was familiar with De Moivre's *Doctrine of Chances*: Price referred to it in his commentary on the Bayes memoir. The Bibliography in his collected fragments from classical and generally known works (Schneider 1988) is not good enough. In addition, Liapunov (his notes in the *C. r.* of the Paris Academy of Sciences), De Moivre's Dedication of his *Doctrine* to Newton, the Ehrenfests model, and Pearson and Fisher are missing. There are some mistakes and De Moivre is only credited with a particular case of his limit theorem. Some papers of Schneider, for example, the article of 1980, are almost useless.

Schneider I. (1980), Huygens' contributions to the development of the calculus of probability. *Janus*, vol. 67, pp. 269 – 279.
--- (1988), *Die Entwicklung der Wahrscheinlichkeitstheorie* … Darmstadt.
--- (2005), De Moivre's central limit theorem and it possible connection with Bayes' essay. In S. Splinter et al, Editors, *Physica et historia. Festschrift for Andreas Kleinert*. *Acta Hist. Leopoldina*, Bd. 45, pp. 155 – 161.

H. L. Seal

In 1823 Gauss proved that in case of a continuous unimodal and symmetric density curve a random variable ξ with variance m^2 obeys the inequality

$$P(|\xi| \le 2m) = 0.89.$$

Seal (1967/1970, pp. 209 – 210) therefore stunningly concluded that exactly that finding led Gauss to abandon his first justification of least squares. What Gauss himself stated about his decision is of no consequence!

Seal (1978) considered De Moivre's *Misc. Analytica* (1730) barely interesting although it contained an essential movement towards his limit theorem (and was later, in 2009, translated into French). Actually following Helen Walker (see her essay on De Moivre in his *Doctrine*, 1756, p. 355), she stated that De Moivre had not thought about the applications of the theory of probability, but this is patently wrong. He considered many card games, published a table of the logarithms of factorials (1730), introduced the continuous uniform law of mortality and had been the most eminent scholar of his time in the field of mathematical insurance. Finally, he proved his limit theorem to study the sex ratio at birth.

De Moivre A. (1730), *Miscellanea analytica de seriebus et quadraturis*. London.

Seal H. L. (1967), The historical development of the Gauss linear model. *Biometrika*, vol. 54, pp. 1 – 24. E. S. Pearson, M. G. Kendall, Editors (1970), *Studies in the History of Statistics and Probability*. London, pp. 207 – 230.

--- (1978), Moivre. In W. Kruskal, Judith M. Tanur, Editors, *Intern. Enc. of Statistics*. New York – London, pp. 601 – 604.

E. Seneta

Seneta published a number of papers about the theory of probability and statistics in Russia. In one of them (2003) he absolutely wrongly described the social activities of the obscurantist Nekrasov. My paper on Poisson (1978) in which I have been disappointed for a long time now, nevertheless described Poisson's discovery of two notions (of random variable and distribution function). Seneta (*Zentralblatt MATH* 383.01011) justly criticized it but failed to mention those discoveries.

Seneta E. (2003), Statistical regularity and free will: Quetelet and Nekrasov. *Intern. Stat. Rev.*, vol. 71, pp. 319 – 334.

Sheynin O. (1978), Poisson's work in probability. *Arch. Hist. Ex. Sci.*, vol. 18, pp. 21 – 72.

G. Shafer

He published an unworthy paper (1996) on Jacob Bernoulli. He called J. B.'s law of large numbers dated, did not study his philosophical reasoning and even wrongly stated the date of the publication of the A*rs Conjectandi*. The connection of J. B:'s non-additive probabilities with the medieval doctrine of probabilism is not indicated, Nic. Bernoulli's plagiarism is forgotten, no Russian sources are mentioned, but the ignorant Porter is positively named. Shafer even included that paper in the Bibliography of Shafer & Vovk (2001).

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

Shafer G., Vovk V. (2001), *Probability and Finance. It's Only a Game*. New York.

J. Short

When treating observations, Short (1763) applied a generalized mean with subjectively assigned weights depending on the distance of the observations from the *middle*. However, his proposal only meant that the usual mean was corrected for the asymmetry of the empirical density.

Short J. (1763), Second paper concerning the parallax of the Sun. *Phil. Trans. Roy. Soc.*, vol. 53, pp. 300 – 342.

T. Simpson

In 1756, Simpson was the first to treat stochastically observational errors. He considered the uniform and the triangular discrete distributions and proved that in both cases the mean was preferable to a single observation. Then, however, he somehow decided that his conclusion was valid for any distribution. He also stated that the precision of the mean increases unboundedly with the number of observations. Bayes criticized this statement and possibly for this reason Simpson added in 1757 (see below) that he excludes systematic errors from his consideration. (The notion of systematic error was explicitly isolated by *Daniel Bernoulli*) This restriction is not, however, sufficient (Hald 1998, pp. 34 – 39).

In 1757 Simpson studied the continuous triangular distribution but the density curve of the errors of the mean, which he showed, did not possess the distinctive form of the normal distribution. He missed the opportunity to prove the appropriate version of the central limit theorem.

Long before those years De Moivre and Simpson had argued about priority (Sheynin 1973, p. 279), and in this connection Pearson (1978, pp. 145 and 184) called Simpson *a most disreputable character* and *an unblushing liar*.

Hald A. (1998), *History of Math. Statistics*. New York.
Pearson K. (1978), *History of Statistics in the 17th and 18th Centuries*. London.
Sheynin O. (1973), Finite random sums. *Arch. Hist. Ex. Sci.*, vol. 9, pp. 275 – 305.
Shoesmith E. (1985), T. Simpson and the arithmetic mean. *Hist. Math.*, vol. 12, pp. 352 – 355.
Simpson T. (1756), On the advantage of taking the mean of a number of observations. *Phil. Trans. Roy. Soc.*, vol. 49, pp. 82 – 93. An extended version of same in author's book *Misc. Tracts on Some Curious … Subjects* … London, 1757, pp. 64 – 75.

E. E. Slutsky

Here is his opinion (1912/2009, § 31, Note 1) about Nekrasov (1912): *An extremely interesting work*. And here is his letter to Markov of 13 November 1912 where he (Sheynin 1990/2011, p. 64) said something very much different:

When Nekrasov's book had appeared, I began to think that my work [1912] *was superfluous; however, after acquainting myself more closely with his exposition, I became convinced that he did not even study sufficiently the relevant literature.*

An attitude regrettably seen even now!

Nekrasov P. A. (1912), *Teoria Veroiatnostei* (Theory of Probability). Moscow. Second edition.
Sheynin O. (1990, in Russian), *Alexandr A. Chuprov. Life, Work, Correspondence*. V&R Unipress, 2011.
Slutsky E. E. (1912, in Russian), *Theory of Correlation and Elements of the Doctrine of the Curves of Distribution*. Berlin, 2009. **S, G,** 23.

M. N. Smit

Maria Smit was a wildly ignorant diehard Bolshevik. Here are her statements.

The crowds of arrested saboteurs are full of statisticians (1931, p. 4, literal translation).

The theory of probability is unable to describe mass social phenomena since it issues from the notion of equal probability which does not exist in a planned economy (1934, pp. 218 – 222).

Pearson is a Machian and his 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 [her spelling] *had attempted it, his system only rests on a mathematical foundation and the real world cannot be studied on this basis at all.* Smit (1934, pp. 227 – 228).

*A great positive novelty: i*n two recently appeared books *the theory of probability is not anymore considered a necessary foundation of statistics* (Anonymous 1954, p. 46).

In 1939, this troglodyte, who likely participated in extending that *crowd of saboteurs*, became corresponding member of the Soviet Academy of Sciences. Much earlier she had been co-editor of the first edition of the *Great Soviet Encyclopaedia* which began appearing in 1926 and had time to corrupt the statistical part of its first volumes. And that same year she took up a leading position in the editorial office of the *Vestnik Statistiki*, the only Soviet statistical journal. It is difficult to imagine the dirty tricks she had done. An extensive article of N. S. Chetverikov commemorating the deceased Chuprov was accepted for publication, but the issue of that journal for the second half of 1926 had never appeared. It is likely that she sacrificed that issue to prevent any notice about the suspected *nevozvrashchenez* (a person who left Russia or the USSR but does not return back). See the attitude of the Soviet officialdom towards him in Sheynin (1990/2011, pp. 159 – 160). I have appended the text of Chetverikov's manuscript to my book (1990/2010). And let us recall Schlözer (1804, p. 51): *Statistics and despotism are incompatible*.

Smit denied mathematics in the spirit of the resolution of the Soviet statistical conference of 1954 (see *Orlov*), a resolution adopted no doubt under her influence. Joint work of mathematicians and statisticians apparently went against her grain. Cf. Buniakovsky (1866, p. 154):

Anyone who does not examine the meaning of the numbers with which he performs some calculations is not a mathematician.

Anonymous (1954, in Russian), Account of the All-Union Conference on problems of statistics. *Vestnik Statistiki*, № 5, pp. 39 – 95.

Buniakovsky V. Ya. (1866, in Russian), Essay on the laws of mortality in Russia and on the age distribution of the orthodox population. *Zapiski Imp. Akad. Nauk*, t. 8, Suppl. 6. Separate paging.

Schlözer A. L. (1804), *Theorie der Statistik*. Göttingen.

Smit M. (1930, 1931), *Teoria i Praktika Sovetskoi Statistiki* (Theory and Practice of Soviet Statistics). Moscow.

--- (1934, in Russian), Against idealism and mechanistic theories in the theory of Soviet statistics. *Planovoe Khoziastvo*, № 7, pp. 217 – 231.

Sheynin O. (1990, in Russian), *Alexandr A. Chuprov. Life, Work, Correspondence*. Russian edition: Berlin, 2010. English edition: V&R Unipress, 2011.

S. M. Stigler

In his book (1986), Stigler dared to vomit abuse on the memory of Gauss, but I was the only author who protested against this moral crime. A honoured statistician, the late William Kruskal, who had been the mentor of that not yet fully fledged scum, proudly informed me in a letter about the forthcoming appearance of that book. Stigler called it ***The** History* …, but it did not amount to any history of full value, and such classics as Kepler, Lambert, Daniel Bernoulli or Helmert are not even mentioned there.

Enthusiastic reviews had appeared with not a single word about either the insult to Gauss' memory or the essential weakness of the book. Five or six statisticians, to whom I had sent my judgement about the book and Stigler in general (see below), were unable to provide any objection and *actually* answered: He is *our s. o. b.* I managed to publish my thoughts (1999; 2000) but before that the *Intern. Z. f. Geschichte u. Ethik* (!) *d. Naturwiss., Techn. u. Med*. refused to publish my manuscript and *Math. Intelligencer* kept silent. No answer came from the Gauss Ges. Göttingen which is only understandable in a small way by the lack of modern interest in practical astronomy. And *Centaurus* rejected my manuscript since a reviewer defended Stigler like his own son. Almost surely, it was Hald, who (see p. xvi of his book of 1998) called Stigler's book *epochal*, apparently in virtue of its impertinence.

All this proves that the history of statistics became an unnecessary luxury and that the scientific community does not value truth anymore (see *Grattan-Guinness*). And here is Truesdell (1984, p. 292):

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

I will trace the contents of my note, Antistigler (2014).

1) Stigler decided that Gauss had not communicated to anyone his discovery of least squares. In 1999, he deliberately omitted to mention Bessel's statement to the contrary, which I noted in 1993.

2) Stigler alleged that Gauss repeatedly prodded his friends into admitting that he had indeed communicated them his discovery before 1805. Actually, Olbers agreed to admit it *gern und willig* but was only able to fulfil his promise about five years later since during that period he had not published anything suitable.

3) Stigler denied Gauss' statement that he had applied least squares since 1794 or 1795 since he was hell-bent to dethrone Gauss and replace him by Legendre. Now, just hold your breath! Only Laplace had allegedly saved Gauss' first justification of least squares from oblivion. Hundreds of textbooks which described that attempt simply do not exist.

4) Stigler (1986, p. 143): *Although Gauss may well have been telling the truth* … Quite appropriate with respect to a suspected rapist …

5) Other heroes. Stigler stated that Euler did not understand statistics but he himself did not understand Euler. He called the ignorant Descrosières *a scholar of the first rank* whereas the book of 1986 of another ignoramus, Porter, was *excellent*. By issuing from a patently rotten premise, he denied Bayes the authorship of his celebrated memoir of 1764.

Integrity is just as important as scientific merits (Einstein, letter of 1933 to Gumbel. Einstein Archives, Hebrew Univ. of Jerusalem, 38615).

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

Sheynin O. (1993), On the history of the principle of least squares. *Arch. Hist. Ex. Sci.*, vol. 46, pp. 39 – 54.

--- (1999), Discovery of the principle of least squares. *Hist. Scientiarum*, vol. 8, pp. 249 – 264.

--- (2000, in Russian), History of the theory of errors. *Istoriko-Matematicheskie Issledovania*, vol. 5 (40), pp. 310 − 332.

--- (2014), Antistigler. *Silesian Stat. Rev.*, No. 12 (18), pp. 48 – 52. **S, G,** 31.

Stigler S. M. (1986), *The* **(!)** *History of Statistics*. Cambridge (Mass.).

--- (1999), *Statistics on the Table*. Cambridge (Mass.). Reprint of previously published papers.

Truesdell C. (1984), *An Idiot's Fugitive Essays on Science*. New York.

Strabo (− 64 or − 63, 23 or 24)

Such a distribution of animals, plants and climates as exists, is not the result of design – just as the difference of race or of language is not either, but rather of accident and chance (Strabo 1969, 2.3.7).

A strange statement.

Strabo (1969), *Geography*, vol. 1. London.

N. Struyck (1687 – 1769)

Pearson (1978, p. 332) named in English Struick's book of 1740: *Introduction to General Geography, besides Certain Astronomical and Other Memoirs*. On p. 337 he comments as follows:

Struyck holds that it must be the Creator's will that [the population of the Earth] *should remain stationary* […] *because it cannot be the Creator's wish that the world should become depopulated, or, because overpopulated so that the people should starve*. […] *Apparently his view is that* [the Creator] *would have no objection to plague or war*!

Indeed, statisticians had been unable to bring into agreement statistical data and the Biblical command to multiply and fill the world, see for example, *Derham, Euler* and *King*.

Pearson K. (1978), *History of Statistics in the 17th and 18th Centuries*. London.

J. P. Süssmilch

Süssmilch collected a great amount of statistical materials but treated it very freely. While averaging data from towns and rural districts, he tacitly assumed that these categories had the same number of inhabitants, and when studying mortality, he did not even attempt to consider the difference between the age structures of various places. Pearson (1978, p. 320) decided that his dealing with epidemic years *sounds much like figure-juggling*. A. I. Chuprov (1910, p. 36), the father of A. A. Chuprov, wrongly decided that Süssmilch's investigations were *exemplary* […] *even for our time*.

See *Euler* about his joint work with Süssmilch.

Pearson (p. 316) noted that, according to Süssmilch, the populating will finally *of itself come to a standstill* without *violent and exceptional means*.

Chuprov A. I. (1910), *Kurs Statistiki* (Course in Statistics). Moscow. Lectures at Moscow University delivered a few decades earlier.

Pearson K. (1978), *History of Statistics in the 17th and 18th Centuries*. London.

Pfanzagl J., Sheynin O. (1997), Süssmilch. In *Enc. of Statistical Sciences*, vol. 13, pp. 8489 – 8491. Hoboken, NJ, 2006. Wrongly published anonymously.

Süssmilch J. P. (1741), *Die Göttliche Ordnung*. Berlin, 1765. Several later editions.

I. Todhunter

It is well known that Todhunter (1865) did not see the wood for the trees. He mostly turned his attention on mathematical transformations rather than on the stochastic nature of the work of various authors.

Kendall M. G. (1963), I. Todhunter's *History of the Math. Theory of Probability*. *Biometrika*, vol. 50, pp. 204 – 205; E. S. Pearson & M. G. Kendall, Editors (1970), *Studies in History of Statistics and Probability*, vol. 1, pp. 253 – 254.

Todhunter I. (1865), *History of the Mathematical Theory of Probability*. New York, 1949, 1965.

V. Ya. Tsinger

On the very first page of his dissertation (1862) he compared the results of Laplace and Gauss:

Gauss and Laplace are representatives of two absolutely different opinions on the meaning of the method of least squares. In Laplace's work we find a rigorous [?] *and impartial study of this problem. His analysis shows that the results of the method ... only enjoy a more or less substantial probability when the number of observations is large whereas Gauss attempted to attach absolute meaning to this method, using extraneous considerations. If we turn our attention to the fact that all the essence of the Theory of chances is contained in the law of large numbers, and that all the properties of random phenomena only take real importance when the number of trials is large, it would not be difficult to perceive the correctness of the Laplacean inference. However, when the number of observations is limited, we cannot at all reckon upon the mutual cancellation of errors* […] *and* […] *any combination of observations can* […] *lead as much to the increase of errors as to their decrease.*

The author was ignorant of the second Gaussian justification of the method of least squares; of Gauss' qualification remark about the arbitrariness of his method; and of Gauss' correct decision to restrict his attention to the case of a small number of observations. Finally, both the history of the sciences of observation and of mathematical statistics proved that Tsinger's last lines contradicted reality and theory, respectively. It was bad that no one apparently argued with him. See also *Laplace*.

Tsinger V. Ya. (1862), *Sposob Naimen'shikh Kvadratov* (The Method of Least squares). Moscow.

B. P. Urlanis

Without referring to anyone, he (1963, p. 152) indicated that Graunt had discussed the number of carps [in a pond] and their distribution by size and thus applied statistical methods in pisciculture. This is quite wrong. Birch (1756 – 1757, vol. 1, p. 294) only reported about Graunt's most elementary measurements which had nothing to do either with statistical methods or pisciculture.

Urlanis B. P. (1963, in Russian), The tercentenary of population statistics. *Uchenye Zapiski po Statistike*, No. 7, pp. 150 – 160. **S, G,** 58.

Birch Th. (1756 – 1757), *History of the Royal Society*, vols 1 – 4. London [New York, 1968.]

W. G. Wesley

He (1978) only mentioned Tycho's random, instrumental and human errors and thus showed that he was not familiar with the theory of errors.

Wesley W. G. (1978), The accuracy of Tycho Brahe's instruments. *J. Hist. Astron.*, vol. 9, pp. 42 – 53.

R. S. Westfall

In spite of its being widely cited, Westfall's paper (1973) should not be taken seriously. He himself (1993) never recalled it and only in passing mentioned that factor. And here is the opinion of Truesdell (private communication of 1992):

Westfall knows nothing serious abut Newton's work. He has no understanding of mathematics. His paper is nonsense. Nevertheless, Newton did fudge, make errors, use wrong data etc.

The late Professor Truesdell never minced his words. It was Rosenberger (1895, pp. 183 – 184) who first mentioned Newton's fudging with respect to the law of universal gravitation.

Selecting observations at will and/or *doctoring* them is, or at least was widespread. W. H. Donahue, the translator of Kepler (1609), maintained in his Note 7 on p. 3 of the translation that

The entire table at the end of Chapter 53, for example (!) *is based on computed longitudes presented as observations*.

See also Babbage (1874).

Einstein is reported as saying that, had Eddington not confirmed his theory, he would have been *sorry for the dear Lord – my theory is correct*. A probable inference: if someone argues with Einstein by issuing from his observations, Einstein would have replied: … *sorry for my respected opponent, but my theory* …

Gingerich (1980, p. 264) borrowed this story from another author who had based himself on an archival source.

Much more illustrations concerning classics of science are possible, and all of them point to the same conclusion: *Quod licet Jovi non licet bovi*! See also *R. R. Newton*.

Babbage C. (1874), Of observations. *Annual Rept Smithsonian Instn* for 1873, pp. 187 – 197.

Gingerich O. (1980), Was Ptolemy a fraud? *Q. J. Roy. Astron. Soc.*, vol. 21, pp. 253 – 266.

Kepler J. (1609, in Latin), *New Astronomy*. Cambridge, 1992.

Rosenberger F. (1895), *Isaac Newton und seine physikalischen Principien*. Leipzig.

Westfall R. S. (1973), Newton and the fudge factor. *Science*, vol. 179, No. 4075, pp. 751 – 758.

--- (1993), *The Life of Isaac Newton*. Cambridge.

S. A. Yanovskaya

She was an eminent specialist in mathematical logic, and, at least in early life, a devoted communist. No wonder that she (1931), apparently being then ignorant of statistics, praised an unworthy (Anderson 1959, p. 297) book (Boyarsky et al 1930):

For the first time they had discovered how to insert dialectical materialism into mathematical statistics the method of the theory of probability.

The same year she also stated (the journal *Planovoe Khoziastvo*) that the theory of probability is inadequate for justifying mathematical statistics.

Anderson O. (1959), Mathematik für marxistisch-leninistische Volkswirte. *Jahrbücher f. Nationalökonomie u. Statistik*, 3. Folge, Bd. 171, pp. 293 – 299.

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