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HISTORY OF MATHEMATICS: SOME THOUGHTS ABOUT THE GENERAL SITUATION

Oscar Sheynin

International Statistical Institute,
International Academy of History of Science
e-mail: oscar.sheynin@googlemail.com
ORCID: 0000-0002-5039-4314

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

1. Introduction

I consider the situation in the history of probability and statistics which is almost the same, as I presume, in the history of mathematics and perhaps in the history of science in general. The main circumstances are: the estimation of the work of scientists is based on wrong premise; the imposed standardization of scientific work is useless and very harmful; and neither the scientific community nor governments properly understand the great importance of information. All this seriously and negatively influence scientific work, and *a disgrace on science is readily seen*.

Below, in § 2, I consider these circumstances and apply appropriate examples of mistakes made by some authors.

1.1. Why are authors guilty?

1.1. Carelessness. This 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. A long 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 a lot of 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.

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 it 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 the 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 …*

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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. I submitted a tiny letter to the *Journal* of that Society in which I criticised the author of a paper published there. After a considerable delay my letter was rejected with a flimsiest justification.

Nowadays, the scientific community does not value reviewing. Apparently, this most important work is not recognized as a 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 the 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 the 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 consult 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 fate). Long and really scientific reviews were published there by eminent authors. A good example to emulate!

1.5. The sledgehammer law. I have 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.* The 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 another) 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 publishers' clerks usually holding positions classified as editors, (…) *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 editors will spoil it anyway*.

No one requires any standards in general literature, suffice it to compare the writings of Tolstoy and Chekhov.

And no one will ever know how many worthy materials have not been published because their authors were unable to overcome the sledgehammer law!

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.

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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.

1.6. Conclusion. The history of probability and statistics (and likely the 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.

Sheynin O., 2017, *Black Book of History of Probability and Statistics*, Berlin. S, G, 80.

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.

2. Examples

I provide critical comments on the work of some authors listed in an alphabetical order.

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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.

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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 Uranus. 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*.

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Bessel F.W., 1818, *Fundamenta astronomiae*, Königsberg.

Bessel F.W., 1823, *Persönliche Gleichung bei Durchgangsbeobachtungen*, In Bessel (1876, Bd. 3, pp. 300-304).

Bessel F.W., 1838, *Untersuchung über die Wahrscheinlichkeit der Beobachtungsfehler*, ibidem, Bd. 2, pp. 372-391.

Bessel F.W., 1843, *Sir William Herschel*, ibidem, Bd. 3, pp. 468-478.

Bessel F.W., 1845, *Übervölkerung,* ibidem, Bd. 3, pp. 387-407.

Bessel F.W., 1876, *Abhandlungen*, Bde 1-3. Leipzig.

Sheynin O. 2000, *Bessel: some remarks on his work*, Hist. Scientiarum, vol. 10, pp. 77-83.

Vladislav Bortkevich, Ladislaus von Bortkiewicz

Bortkiewicz was not mathematically educated. He ((Bortkevich and 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 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: *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 and 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. 926.

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.

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

Bortkiewicz L. von, 1917, *Die Iterationen*. Berlin.

Bortkiewicz L. von, 1930, *Die Disparitätsmasse des Einkommenstatistik*, *Bull. Intern. Stat. Inst.,* t. 25, no. 3, pp. 189-298.

Bortkiewicz L. von, 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.

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

Sheynin O., 2012, L. *von Bortkiewicz: a scientific biography. Dzieje matematyki polskiej*, Wroclaw, pp. 249-266, Editor, W. Wieslaw.

Whitaker L., 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.

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

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(p. 214) held that various lotteries are *equally harmless* if the expected winnings are the same and equal the [same] stakes, and the 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 the 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, no. 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.

Sheynin O., 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

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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 the 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, and 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.

Anderson O., 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.

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

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

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

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

Chuprov A.A., 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.

Chuprov A.A., 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.

Chuprov A.A., 1960, *Voprosy Statistiki* (*Issues in Statistics*), Moscow.

Chuprov A.A., 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.

Markov A.A., 1911 (in Russian), *On the basic principles of the calculus of probability etc.*, In Ondar (1977/1981, pp. 149-153).

Ondar Kh. O. (ed.), 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.

Romanovsky V.P., 1938, *Matematicheskaia Statistika*, Moscow–Leningrad.

Slutsky E.E., 1925 (in Russian), *On the law of large numbers*, Vestnik Statistiki, no. 7-9, pp. 1-55.

Slutsky E.E., 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.

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

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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.

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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.
Thierry M., 1995, *Probabilité et philosophie des mathématiques chez Cournot*, Rev. hist. math., t. 1, n. 1, pp. 111-138.

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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∞ = cot∞ = $\pm\sqrt{-1}$. 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 S., 1882, *Memoir of Augustus De Morgan*, London.

I. Ekeland

His book (2006) contains many absurdities indeed. 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 qualifying remarks, that *the normal law appears wherever we collect measurements*.

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

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*

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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.
Fisher R.A., 1939, *Student*, Annals Eug., vol. 9, pp. 1-9.
Fisher R.A., 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.

A.T. Fomenko

After studying Ptolemy's star catalogue, Efremov and 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 and Fomenko (2004) somehow decided that Jesus was the star 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.
Efremov Yu. N., Pavlovskaia E.D. (1989, in Russian), Same title, Istoriko-Astronomicheskie Issledovania, vol. 21, pp. 175-192.

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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.

B.V. Gnedenko

Gnedenko was co-author of a popular booklet Gnedenko and 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 forgotten

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.

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.

A. Hald

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*.

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.

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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, no. 7, pp. 36-46. S, G, 7.

Khinchin A.Ya., 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.

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 general 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 general theory of statistics.

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 random variable and its expectation.

In translation (Gnedenko, Sheynin 1978/2001, p. 255) that phrase 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, no. 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, ed. A. N. Kolmogorov, A.P. Youshkevich, 1992 and 2001, pp. 212-288.

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, no. 14, pp. 99-112.

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,

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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, ed. 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.
Laplace P.S., 1812, *Théorie analytique des probabilités,* Oeuvr. Compl., t. 7. Paris, 1886.
Laplace P.S., 1814, in French), *Philosophical Essay on Probabilities*, New York, 1995. Translated by A. Dale.
Laplace P.S., 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.
Sheynin O., 2009, *Theory of Probability. Historical Essay*, Berlin. S, G, 10.

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*, [in:] *Hauptschriften zur Versicherungs- und Finanzmathematik*, ed. E. Knobloch. Berlin, 2000, pp. 428-445, with a German translation.
Leibniz G.W., (1682, 1866), *Quaestiones, Ibidem*, pp. 520-523, with a German translation.

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.

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 (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 as 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.

Why contradictory? Indeed, *the Gauss- Markov theorem* on one side, and nothing except *clear statement* on the other side.

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).

Markov A.A., 1900, *Ischislenie Veroiatnostei* (Calculus of Probability). Later editions: 1908, 1913, posthumous edition Moscow, 1924. German edition 1913.

Markov A.A., 1951, *Izbrannye Trudy* (Sel. Works). No place.

Ondar Kh.O (ed.), 1977 (in Russian), *Correspondence between Markov and Chuprov etc.*, New York, 1981.

Sheynin O., 1988, *Review of Porter (1986)*, Centaurus, 1988, vol. 31, pp. 171-172.

Sheynin O., 2006, M*arkov'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.

J. Neyman

Neyman (1934, p. 595) mistakenly attributed to Markov the second Gaussian justification of least squares of 1823. David and 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.
Neyman J., 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 the authorities since he belonged to an ethnic 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) 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.
Ondar Kh.O., 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 the Editor of the Russian edition.
Sheynin O., 1990 (in Russian), *Aleksandr A. Chuprov. Life, Work, Correspondence*. V&R Unipress, 2011.

ŚLĄSKI PRZEGLĄD STATYSTYCZNY

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S.D. Poisson

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*., 2me sér., 46e année, t. 44, pp. 463-468.
Chetverikov N.S. (ed.), 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.
Poisson S.-D., 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.
Sheynin O., 2002, *Sampling without replacement*, Intern. Z. f. Geschichte u. Ethik d. Natur-wissenschaften, Techn. u. Med., Bd. 10, pp. 181-187.
Sheynin O., 2012, *Poisson and statistics*, Math. Scientist, vol. 37, pp. 149-150.
Sheynin O., 2013, *Poisson et la statistique*, [in:] *Poisson. Les mathématiques au service de la science*, Y. Kosmann-Schwarzbach, Palaiseau, pp. 357-366.

T.M. Porter

His book (1986) abounds with mistakes. 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 the strangest statements, for example: *Even mathematicians cannot prove the fourth dimension*. The treatise of Thomson and Tait of 1867 (reprinted in 2002) is impudently called *standard Victorian*.

Quite recently, Porter was elected a full member of the International Academy of the History of Science … which goes to show that the procedure for election of new members is not good enough.

Grattan-Guinness I. (ed.), 1994, *Companion Enc. of the History and Philosophy of the Math. Sciences*, vol. 1 – 2, London–New York.

Porter T.M., 1986, *The Rise of Statistical Thinking, 1820-1900,* Princeton. My review: Centaurus, 1988, vol. 31, pp. 171-172.

Porter T.M., 2003, *Statistics and Physical Theories*, [in:] M.J. Nye (ed.) *Modern Phys. and Math. Sciences,* Cambridge, pp. 488-504.

Porter T.M., 2004a, Karl Pearson's Utopia of scientific education etc., [in:] R. Seising et al. (ed.) *Form, Number, Order etc. Festschrift for Ivo Schneider* etc., Stuttgart, pp. 339-352.

Porter T.M., 2004b, *Karl Pearson* etc., Princeton−Oxford. My review: Hist. Scientiarum, 2006, vol. 16, pp. 206-209.

S. M. Stigler

See: Sheynin (2014)

Sheynin O.

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