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Introduction by the compiler

Notation

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

General comments on some items

[iii] Posse was generally respected (I especially note: by Markov and Steklov who had been close to Markov). True, he apparently had not taken into account the new direction in mathematical analysis, the complex analysis, which had been then developing in Europe. This, however, was a common feature of Russian mathematics of that period and is explained by Chebyshev's conservatism (Sheynin 2017, § 13.3). A negative aspect of Sergeev's paper is his failure to separate bibliographic information from notes.

[iv] I have translated this article since it is interesting for mathematicians in general and especially for those who are studying the history of Soviet science. The horrible persecution of scientists had been certainly going on under Stalin's yellow (as some authors claim) eyes, in probability and statistics in particular, see Sheynin (2017, Note 8 to Chapter 15 with an additional reference).

Tikhomirov arranged his references in their order in the text, whereas I consider this method only possible for a few of them. I had to spend much time to sort them out properly. And a special point: it is astonishing that Luzin, not yet being a master, was able to change mathematical life in Moscow.

Many authors had later described the same subject. I name four sources from the same periodical (*Istoriko-Matematich. Issledovania*): V. A. Volkov, vol. 10 (45), 2005; V. M. Tikhomirov, *Ibidem* A. K. Tiulina, vol. 11 (46), 2006; I. M. Nikonov et al, vol. 13 (48), 2009

[vi] Two preliminary points. **1.** The formulas were badly printed and I can only hope that now they are correctly reprinted. **2.** The author was ignorant of the probability theory. Indeed, he all but forgot Markov's pioneer studies of dependent magnitudes and did not himself mention the chains.

Markov is prettified out of all proportions. His sharp and groundless statements are not mentioned, although even Zhukovsky, a most eminent scholar, had admonished Markov for this reason. And here is a quote from Andreev's letter of 1915 (Chirikov & Sheynin (1994, p. 132): Markov

Remains [...] an old and hardened sinner in provoking debate. I had understood this long ago, and I believe that the only way to save myself from

the trouble of swallowing the provocateur's bait is a refusal to respond to any of his attacks...

The author praises Markov's *Calculus of Probabilities* although the method of least squares is not properly treated there (and Bezikovitch helplessly discussed it), see Sheynin (2006). There also I severely criticize Markov for his utterly unmethodical way of writing which at least partly was occasioned by his disregard of readers. To say it bluntly: Markov did almost nothing with regard to the method of least squares, and the Gauss – Markov theorem only exists as the theorem of Gauss alone.

Chebyshev, *In spite of his splendid analytical talent, was a pathological conservative* (Novikov 2002, p. 330) and his students, Lyapunov (Sheynin 2017, p. 226) and Markov (A. A. Youshkevich 1974), had regrettably underestimated the revolutionary new developments of mathematical analysis in Western Europe. Concerning the method of moments see Sheynin (2017, pp. 101 and 254) and in much more detail previously (Sheynin 2011, Chapter 5).

Emeliakh (1954) diligently described the archival facts concerning Markov's demand to be excommunicated from the Church (which was denied: *too much honour*; he was considered *fallen away*). The author's explanation is generally believed, but I think that the reason was different: Markov became still more opposed to the Church because of its shameful attitude to the notorious Beylis case (the Russian, and much more favourably concluded version of the Dreyfus case).

[viii] This paper provides little known information about Nekrasov; see also Sheynin (2003) and Soloviev (1997). However, numerous mistakes, see Notes 2, 4, 6, 7 and 10 (and title of Nekrasov (1904) was written mistakenly), mean that the author had not sufficiently cared about his work or his readers.

[ix] Hansen (1795 – 1874) was an eminent astronomer. The Royal Statistical Society awarded him two gold medals, and he received the Copley medal from the Royal Society. His geodetic work is not described in any general source, but Kendall & Doig (1968) listed ten of his geodetic contributions (1830 – 1874). Here, he apparently and astonishingly did not mention Gauss (1823) and his deliberations about most probable values and the arithmetic mean are barely needed. Interesting is the case of two bases supporting a triangulation net (Note 4) and the mention (and study?) of dependent unknowns (theme No 3 in his list of themes).

[xi] Schreiber remains virtually unknown, even a bibliography of his contributions is not available. However, he should be certainly named along with Helmert whereas some of his ideas had been akin to Krasovsky's opinions. Contrary to Schreiber, the latter had been responsible for geodesy of a great country

[xiii] Matvievskaya was co-author of a book on Romanovsky which was published in 1997 and which I translated in 2018. The title-page

of the Russian edition mentioned S. S. Demidov as *responsible editor*, but Bogoliubov, the other co-author, was extremely careless to such an extent that I informed Demidov of my intention to omit his name. As the wise man that he is, he kept silent, and my translation does not mention him. Actually he only was a VIP at the wedding, a Hochzeitgeneral. For that matter, Matvievsкая herself is a VIP.

The book includes excerpts from that paper of Romanovsky, and I am now copying them.

[xiv] Romanovsky is seen here as an ardent lover of Turkistan. It is instructive to compare his paper with Newcomb's (1876) account of the scientific history of the USA. Romanovsky's opinion about Germany's supremacy at the beginning of WWI should be scaled down. Von Ratenau, the future Foreign Minister of Germany at the time of the Weimar Republic, played a key role in setting up the War Raw Materials Department (and stated that Germany lacked industrial readiness), see Wikipedia. There exists an opinion that without Ratenau Germany would have capitulated in 1915. The Tashkent University was opened during the Civil War in a very complicated way (Bogoliubov & Matvievsкая 1997, Chapter 4). Finally, the description of the American efforts of organizing science was too detailed.

[xv] In addition to my comments in Note 9 I indicate that Chetverikov was Chuprov's closest student, see Sheynin (2011). In 1923, he informed Chuprov about Romanovsky. See there pp. 70 – 74 about the last-mentioned and pp. 7 – 78 about Chetverikov. As stated in the Bibliography about Bernstein (1924), that contribution was reprinted in a book published somewhere in 1964.

Oscar Sheynin

Early history of the arithmetic mean

1. In ancient Greece approximate calculations, which had been attributed to the lower, applied science since Plato's time, may have been the basic refuge of the idea of randomness since randomness was thought to be only inherent to the terrestrial, lower processes (Sambursky 1956). Thus, the circumference (obviously the most perfect closed curve) was the path of the most perfect objects in the world, of the celestial bodies.

The Pythagorean school studied average values including the arithmetic mean (Makovelsky 1914, p. 63). That mean occurred in many diverse formulas for approximately calculating areas of closed figures, volumes of bodies and square roots of imperfect squares (Hero Aleksandrinus 1903). In ancient India, when calculating the volumes of excavations, the length, width and depth of the excavations should have been measured in several places with the subsequent calculation of the three arithmetic means (see Colebrooke 1817 [and Sheynin 1973, p. 104]):

The greater the number of these places, the nearer will the mean measure be to the truth, and the more exact will be the consequent computation.

It was apparently thought that the influence of the accepted inexact mathematical model will thus be decreased. Vayman (1961, p. 204) indicated, that in ancient Babylon areas of quadrangles were *fairly often* assumed to be equal to the product of the half-sums of their opposite sides.

Repeated measurements of a somewhat varying magnitude had thus been carried out in antiquity. Note that in geodetic work an angle is considered to change in time, for example due to the changing horizontal refraction, and is therefore (but not for this reason alone) measured repeatedly.

2. The arithmetic mean also appeared in connection with equivalent transformations of figures (Vayman 1961, p. 99) required, as I add, by surveying. Lur'ie (1934) stated that the application of the arithmetic mean was *an extremely widespread method of finding the true value of things* whereas its appearance in approximate calculations should be explained by its penetration from economics. He had not, however, justified his opinion.

For his part, Leibniz (1704, Bk 4, chapter 16) stated that

The basis for all these theoretical constructions [in the calculus of probability] is the so-called prostapheresis, i. e., we take the arithmetic mean from several equally acceptable hypotheses. Our peasants, following natural mathematics, have been using this method for a long time.

The cost of a land lot was assumed to be the arithmetic mean of values ascertained by three groups of valuers. On prostapheresis (in astronomy) see § 4 below.

Leibniz continued:

This axiom, aequalibus aequalia is also assumed in calculations with equivalent hypotheses, and even the juridical standards and practice had noted half-degrees of reliability.

According to Vayman (cf. his opinion as described above) the main factor for the origin of the arithmetic mean as a theoretical concept was the sphere of economics.

3. Gamblers thought that the arithmetic mean possessed a certain positive property which was evident since they had been distressed if the sum of the points thrown with dice or astragali was less than the arithmetic mean of possible outcomes. For astragali the matter was not so simple: the four possible outcomes had differing probabilities of occurrence. Ore (1953, p. 170) indicated that Cardano, when reasoning about those outcomes, used the law of large numbers in the most rudimentary form.

Galileo (1718) remarked that gamblers had decided that in a toss of three dice 10 and 11 points show up more often than 9 and 12. If only the probabilities of these results are compared with each other the (small) difference between them can be empirically ascertained thus proving that the gamblers were right.

4. Prostapheresis indicates the difference between the central (centre of the deferent) and the true (on the epicycle) position of a planet (Lalande 1789) or more generally (Idelson 1947, p. 154) any periodic inequality added to the values of a uniformly increasing angular value. In the 16th century this term was associated with calculations based on formulas of the type

$$2\sin A \sin B = \cos(A - B) - \cos(A + B).$$

In calculations of $\sin A \sin B$ and $\cos A \cos B$ the half-sums will thus appear on the right sides.

However, the previous definition of that term leads us to the geocentric system of the world which clearly explained the known facts: the average motion (the deferent) was distinguished from the true motion which was investigated in relation to that average. Much later Copernicus (1543 Bk 2, Chapter 3) wrote that

In any [...] irregular movement it is necessary to consider a certain mean, with whose aid we can determine the extent of the irregularity.

For determining the position of the centre of the epicycle it was required to compute the arithmetic mean of two extreme positions of the planet in question on the epicycle. In the absence of such calculations, the theoretical possibility of this method became nevertheless known which would have led to the strengthening of the idea of an average. And we can also find an application of the arithmetic mean of measurements by Ptolemy (Manitius 1912, Bd. 1, p. 44) although it was not at all a rule.

5. From the 17th – 18th centuries onward that mean became a universal estimator in arc measurements (Snellio (1617, pp. 175 – 176; Maupertuis 1737/1808, pp. 240 – 247). In those times, some

scientists devoted several lines to the principle of the arithmetic mean. Copernicus (1543, Bk 5, Chapter 7) asserted that

If between the extreme limits of the measurements there is no appreciable difference, it is safer to use the averages.

Kepler (1609/1992, p. 200) decided that the arithmetic mean was *the letter of the law* [Sheynin 2017, § 1.2.4]. Picard (1693/1729, pp. 330, 335, 343) called the arithmetic mean the *true* value. Condamine (1751, p. 223) thought that

Having adopted the average, we hardly risk any error, even if certain observations contain considerable defects.

Cotes (1722), [see Gowing 1983, p. 107] stated that the most probable place of the object was the weighted arithmetic mean.

6. The Leibniz axiom (§ 2) is at the heart of the notion of expectation, see Jakob Bernoulli (1713, pt. 1, Commentary on Huygens' first Proposition).

Then came the median, the general reliability, as I would say, and the probable error, the probable expectation, although not in the mathematical sense.

In 1966, this note was deposited at the Institute for Scientific Information in Moscow. Now, in translating it, I added a few remarks and a few references in square brackets and I left out some references. Much more could have been added.

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II

Oscar Sheynin

Probability and Statistics in the 18th Century

This, now revised text is intended for a broader circle of readers. It appeared in Italian, although with suppressed references, as

Lo sviluppo della teoria della probabilità e della statistica in Storia della Scienza, t. 6. Roma, Ist. Enc. Ital., 2002, pp. 529 – 541

Contrary to the official agreement, the original English version was not published. My revision certainly left intact the main body of this paper. Many more details can be found in my recent book (2017).

1. Introduction

The theory of probability can be traced back to 1654 when Pascal and Fermat, in solving the problem of points (of sharing the stakes in an uncompleted series of games of chance), indirectly introduced the notion of expected gain (of the expectation of a random variable). In 1657, Huygens published the first treatise on probability. There, he applied the new notion (although not its present term) for studying games of chance. His materials of 1669, which remained unknown during his lifetime, included solutions of stochastic problems in mortality. Later, in 1690, following Descartes, he stated that natural sciences only provided morally certain (highly probable) deductions.

Moral certainty and the application of statistical probability were discussed in philosophical literature (Arnauld & Nicole 1662) which influenced Jakob Bernoulli, the future cofounder of probability theory (§ 2). Petty and Graunt, in the mid-17th century, created political arithmetic whose most interesting problems concerned statistics of population and its regularities.

Having extremely imperfect data, the latter was nevertheless able to compile the first mortality table and to study medical statistics. In 1693 Halley calculated the second and much better table and laid the foundation of stochastic calculations in actuarial science. Newton applied stochastic reasoning to correct the chronology of ancient kingdoms, and, in a manuscript written between 1664 and 1666, invented a simple mind experiment to show that the then yet unknown geometric probability was capable of treating irrational proportions of chances.

2. The First Limit Theorem

Jakob Bernoulli blazed a new trail in probability. His *Ars Conjectandi* posthumously published in 1713 contained a reprint of Huygens' treatise with essential comment; a study of combinatorial analysis; solutions of problems concerning games of chance; and an unfinished part where he provided (but had not applied) a definition of

theoretical probability, attempted to create a calculus of stochastic propositions, and proved his immortal theorem.

Here it is. Bernoulli considered a series of Bernoulli trials, of $v = (r + s)^n$ independent trials in each of which the studied event A occurred with probability $p = r/(r + s)$. If the number of such occurrences is μ , then, as he proved,

$$P\left(\left|\frac{\mu}{v} - p\right| \leq \frac{1}{r + s}\right) \geq \frac{c}{1 + c},$$

where c was arbitrary and $v \geq 8226 + 5758 \log_{10}c$. It followed that

$$\lim P\left(\left|\frac{\mu}{v} - p\right| < \varepsilon\right) = 1, v \rightarrow \infty. \quad (1)$$

Bernoulli thus offered the (weak) law of large numbers and established the parity between the theoretical probability p and its statistical counterpart μ/v .

Given a large number of observations, the second provided moral certainty and was therefore not worse than the first. To paraphrase him: He strove to discover whether the limit (1) existed and whether it was indeed unity rather than a lesser positive number. The latter would have meant that induction (from the v trials) was inferior to deduction! The application of stochastic reasoning well beyond the narrow province of games of chance, sufficiently serviced by the theoretical probability, was now justified, at least for the Bernoulli trials.

3. Montmort

His treatise on games of chance (1708) unquestionably influenced De Moivre. Unlike Huygens' first attempt (§ 1), his contribution was a lengthy book rich in solutions of many old and new problems. One of the former, which Galileo solved in a particular case by simple combinatorial formulas, was to determine the chances of throwing k points with n dice, each of them having f faces (alternatively: having differing number of faces). In this connection Montmort offered a statement that can now be described by the formula of inclusion and exclusion: For events A_1, A_2, \dots, A_n ,

$$P(\sum A_i) = \sum P(A_i) - \sum P(A_i A_j) + \sum P(A_i A_j A_k) - \dots$$

where $i, j, k, \dots = 1, 2, \dots, n, i < j, i < j < k, \dots$ This formula is a stochastic corollary of the appropriate general proposition about sets A_1, A_2, \dots, A_n overlapping each other in whichever way. For $f = \text{Const} = 6$ (say), the problem stated above is tantamount to determining the

probability that the sum of n mutually independent random variables taking equally probable values 1, 2, ..., 5, 6 equals k .

In 1713 Montmort also inserted his extremely important correspondence with Niklaus Bernoulli. One of the topics discussed by them in 1711 – 1713 was a strategic game (her), – a game depending both on chance and the decisions made. A theory of such games was only developed in the 20th century. For other subjects of their letters see §§ 6 and 10.2.

4. De Moivre

His main contribution was the *Doctrine of Chances*, where, beginning with its second edition, he incorporated his derivation of the De Moivre – Laplace limited theorem privately printed in 1733 but accomplished by him a dozen years or more earlier. And his memoir of 1711, which appeared before Jakob Bernoulli's posthumously published *Ars Conjectandi*, can be considered as its preliminary version. It was there that he introduced the classical definition of probability, usually attributed to Laplace.

The *Doctrine* was written for non-mathematical readers. It provided solution of many problems in games of chance but did not concentrate on scientific topics, and the proofs of many propositions were lacking. Nevertheless, this book contained extremely important findings, see below and § 10.1, and both Lagrange and Laplace thought of translating it into French, see Lagrange's letter to Laplace of 30.12.1776 in t. 14 of his *Oeuvres*.

I describe now the theorem mentioned above. Desiring to determine the law underlying the ratio of the births of the two sexes (§ 6), De Moivre proved that for n Bernoulli trials with probability of success p , the number of successes μ obeyed the limiting law

$$\lim P(a \leq \frac{\mu - np}{\sqrt{npq}} \leq b) = \frac{1}{\sqrt{2\pi}} \int_a^b \exp(-z^2/2) dz, n \rightarrow \infty \quad (2)$$

with $q = 1 - p$. Note that $np = E\mu$ and $npq = \text{var}\mu$, the expectation and variance of μ (the second notion is essentially due to Gauss). The convergence implied in (2) is uniform with respect to a and b , but, again, this is a concept introduced in the 19th century. When deriving his formula, De Moivre widely used expansions of functions into power series (sometimes into divergent series calculating the sums of several of their first terms).

Thus appeared the normal distribution. De Moivre proved (2) for the case of $p = q$ (in his notation, $a = b$) and correctly stated that his formula can easily be generalized to $p \neq q$; furthermore, the title of his study included the words *binomial* $(a + b)^n$ *expanded* ... He had not however remarked that the error of applying his formula for finite

values of n increased with the decrease of p (or q) from $1/2$, or, in general, had not studied the rapidity of the convergence in (2).

In following the post-Newtonian tradition, De Moivre did not use the symbol of integration; his English language was not generally known on the Continent; Laplace (1814) most approvingly mentioned his formula but had not provided an exact reference or even stated clearly enough his result; and Todhunter (1865), the best pertinent source of the 19th century, superficially described his finding. No wonder that for about 150 years hardly any Continental author noticed De Moivre's theorem. In 1812, Laplace proved the same proposition (hence its name introduced by Markov) by means of the McLaurin – Euler summation formula and provided a correction term which allowed for the finiteness of the number of trials.

Scientific demands led to the studying of new types of random variables whose laws of distribution did not coincide with Jakob Bernoulli's or De Moivre's binomial law. Nevertheless, the convergence of the sums of these variables to the normal law persisted under very general conditions and this fact is the essence of the central limit theorem of which (2) is the simplest form.

De Moivre defined independent events:

$$P(B) = P(B/A), P(A) = P(A/B),$$

whereas for (say, three) dependent events

$$P(ABC) = P(A)P(B/A)P(C/AB).$$

The main aim of his work was the separation of randomness from Divine Design (from necessity) although randomness was still understood only as its uniform case; its generalization took much time. In his Dedication of the first edition of the *Doctrine* to Newton (reprinted on p. 329 of its third edition) he wrote: we will thus learn

From your Philosophy, how to collect [...] the Evidence of exquisite Wisdom and Design which appear in the phenomena of Nature throughout the Universe.

5. Bayes

His fundamental posthumous memoir of 1764 was communicated and commented on by Price. Bayes' converse problem, as Price called it, was to determine the unknown theoretical probability of an event given the statistical probability of its occurrence in Bernoulli trials. Here, in essence, is his reasoning. A ball falls $\alpha + \beta = n$ times on a segment AB of unit length so that its positions on AB are equally probable and c is somewhere on AB with all its positions also equally probable; α times the ball falls to the left of c (α successes) and β times, to the right (β failures; statistical probability of success, α/n). It is required to specify point c . For any $[a; b]$ belonging to AB

$$P(c \in [a; b]) = \int_a^b C_n^\alpha x^\alpha (1-x)^\beta dx \div \int_0^1 C_n^\alpha x^\alpha (1-x)^\beta dx. \quad (3)$$

This is the posterior distribution of c given its prior uniform distribution with the latter representing our prior ignorance. The letter x in (3) also stands for the unknown Ac which takes a new value with each additional trial. At present we know that

$$P = I_b(\alpha + 1; \beta + 1) - I_a(\alpha + 1; \beta + 1)$$

where I is the symbol of the incomplete Beta function. The denominator of (3), as Bayes easily found out, was (the complete Beta function times the factor C_n^α) the probability

$$P(\text{The number of successes} = \alpha \text{ irrespective of } Ac) = 1/(n + 1)$$

for any acceptable value of α . Even up to the 1930's the estimation of the numerator for large values of α and β had been extremely difficult and some commentators believe that Bayes did not publish his memoir himself because he was dissatisfied with his efforts in this direction (he did not provide the proper answer to that problem). In addition, as Bayes stated in another posthumous publication, the application of (the first terms of) divergent series should not be allowed. He obviously thought about De Moivre; Timerding, as I note, had not applied them.

Anyway, it seems that he had not rested content with limiting relations since they were not directly applicable to the case of finite values of n (at least Price said so with regard to the work of De Moivre). However, Timerding, in his translation of the Bayes memoir into German (1908), proved that the latter's calculations could have led to

$$\lim P \left(a \leq \frac{x - \alpha/n}{\sqrt{\alpha\beta/n^{3/2}}} \leq b \right) = \frac{1}{\sqrt{2\pi}} \int_a^b \exp(-z^2/2) dz, n \rightarrow \infty$$

where, as I myself note, $\alpha/n = Ex$ and $\alpha\beta/n^3 = \text{var}x$.

It is remarkable that Bayes, who (just like De Moivre) certainly had not known anything about variances, was apparently able to perceive that an elementary and formal transformation of the left side of (2) leading to

$$P \left(a \leq \frac{u/n - p}{\sqrt{pq/n}} \leq b \right)$$

did not provide the proper answer to his problem. Both Jakob Bernoulli, and De Moivre mistakenly thought that they had solved the inverse problem as well just by solving the direct problem.

Only Bayes correctly perceived the proper relation between the statistical and theoretical probabilities and thus completed the first version of the theory of probability. Mises, who postulated that the theoretical probability of an event is the limit of the statistical probability of its occurrence, could have referred to Bayes; moreover, in various applications of probability this Mises conception is inevitably made use of, but the references could be and even should be made to Bayes as well!

On another level, Bayes' main result was that, given a random variable with a superficially known distribution, it is possible to specify it by means of observation. Thus, all possible positions of c on AB were thought to be equally possible, but the n trials led to distribution (3).

Price provided an example which presumed complete previous ignorance: Sunrise had been observed a million times in succession; how probable becomes the next sunrise? According to formula (3) with $a = 1/2$, $b = 1$, $\alpha = 10^6$ and $\beta = 0$, he found that the odds of success were as the millionth power of 2 to one. Hume (1739/1969, p. 124) was the first to mention this problem (and resolutely, although indirectly decided that certain knowledge was needed whose existence Price and later authors had been rejecting). Among those authors I name Buffon (1777), Laplace (1814/1995, p. 11), see below, Jorland (1987), Loveland (2001) and most certainly Zabell (1989).

Chebyshev (1879 – 1880/1936, p. 158), who shunned philosophy, stated the same problem on an everyday level: determine the probability of a student's successful answer to the next question after his previous successes.

Just as it was with De Moivre (§ 4), Continental mathematicians were hindered from studying the Bayes memoir by his English language and his failure to interpret his subtle reasoning. For about thirty years the *Bayesian approach* had been denied, by Fisher (1921, pp. 311 and 326) in the first place, apparently because of the introduction of barely known prior distributions, but then (Cornfield 1967, p. 41) noted that Bayes had returned from the cemetery. See also Gillies (1987), who discusses the recent debates (and reasonably describes Price's own contribution).

Let incompatible events A_1, A_2, \dots, A_n , have probabilities $P(A_i)$ before an event B happens; suppose also that B occurs with one, and only one of the A_i 's, after which these events acquire new probabilities. Then

$$P(A_i/B) = P(B/A_i)P(A_i) \div \sum_{j=1}^n [P(B/A_j) P(A_j)].$$

This is the so-called Bayes formula, see Cournot (1843, § 88), nevertheless lacking in the Bayes memoir. However, in the discrete case it also describes the transition from prior probabilities to posterior. The Bayes theorem was thus first named by Lubbock et al (1730/1744, p. 48). It was Laplace (1774b) who had expressed it (in words only) and proved it later (1781, p. 414). Laplace (1786) also extended the Bayes method by treating non-uniform prior distributions. And, without mentioning Bayes, he solved several problems leading to formulas of the type of (3). Best known is his calculation of the probability of the next sunrise already observed α times in succession.

He (1814/1995, p. 11) stated, but did not prove, that this probability was $(\alpha + 1)/(\alpha + 2)$ but the explanation is in one of his earlier memoirs (1781). In 1774 he began to consider relevant urn problems, and in 1781 he went on to study the sex ratio at birth (also see § 6).

An urn contains an infinite number of white and black balls. Drawings without replacement produced p white balls and q black ones; determine the probability that a white ball will be extracted next. Denote the unknown ratio of the number of white balls to all of them by x , then the obtained sample has probability $x^p(1-x)^q$, and, since all values of x should be regarded as equally probable, the probability sought will be

$$P = \int_0^1 x^p(1-x)^q dx \div \int_0^1 x^p(1-x)^q dx = \frac{p+1}{p+q+2}.$$

Hence (if $p = \alpha$ and $q = 0$) the conclusion above. Note that the result obtained coincides with the expectation of a random variable with density

$$\varphi(x) = Cx^p(1-x)^q, \quad C = 1 \div \int_0^1 x^p(1-x)^q dx.$$

Determine now the probability of drawing m white balls and n black ones in the next $(m+n)$ extractions if these numbers are small as compared with p and q . This time making use of approximate calculations, Laplace got

$$P = \frac{p^m q^n}{(p+q)^{m+n}}$$

and noticed that this was in agreement (as it should have been) with assuming that $x \approx p/(p+q)$.

Finally, also in 1774, Laplace proved that for an arbitrary $\alpha > 0$

$$\lim P\left(\frac{p}{p+q} - \alpha \leq x \leq \frac{p}{p+q} + \alpha\right) = 1, p, q \rightarrow \infty.$$

In 1781 he applied this result to state that, when issuing from extensive statistical data, the sex ratio at birth could be calculated as precisely as desired [provided that it remained constant!]. See § 11 for still another related problem studied by Laplace.

The difference between the statistical and the theoretical values of such magnitudes as $p/(p+q)$ could have also been estimated by means of the De Moivre – Laplace theorem; indeed, for $p, q \rightarrow \infty$ the probabilities of extracting balls of the two colours remain constant even when they are not returned back into the urn.

For about thirty years the *Bayesian approach* had been denied, by Fisher (1921, pp. 311 and 326) in the first place, apparently because of the introduction of barely known prior distributions, but then (Cornfield 1967, p. 41) noted that Bayes had returned from the cemetery. See also Gillies (1987), who discusses the recent debates (and reasonably describes Price’s own contribution).

6. Geometric Probability

This term appeared in the 18th century. Newton (1967, pp. 58 – 61) was the first to apply it in his manuscript written between 1664 and 1666 (Sheynin 2017, § 2.2.3). In 1735, Daniel Bernoulli tacitly used it when reasoning about the Solar system. The inclinations of the orbits of the five (excepting the Earth) then known planets with respect to the Earth (considered as random variables with a continuous uniform distribution) were small, and the probability of a “random” origin of that circumstance, as he concluded, was negligible. It was possible to study, instead of the inclinations, the arrangement of the poles of the orbits (Todhunter 1865, p. 223).

Again, geometric probability was tacitly applied when considering continuous distributions beginning at least with De Moivre, but it was the Michell problem (1767) which applied it and became classical: determine the probability that two stars from all of them, uniformly distributed over the celestial sphere, were situated not farther than 1° from each other. See Sheynin (2017, § 6.1.6) where I also describe the following discussion. Thus, Bertrand (1888, pp. 170 – 171) remarked that without studying other features of the sidereal system it was impossible to decide whether stars were arranged randomly.

Buffon expressly studied geometric probability; the first report on his work (Anonymous 1735), obviously written by himself, had appeared long before his contribution. Here is his main problem: A needle of length $2r$ falls “randomly” on a set of parallel lines. Determine the probability P that it intersects one of them. It is easily seen that

$$P = 4r/\pi a$$

where $a > 2r$ is the distance between adjacent lines. Buffon himself had, however, only determined the ratio r/a for $P = 1/2$. His main aim was (Buffon 1777/1954, p. 471) to “put geometry in possession of its rights in the science of the accidental [du hasard]”. Many commentators described and generalized the problem above. The first of them was Laplace who noted that the formula above enabled to determine [with a low precision] the number π .

A formal definition of geometric probability, or, rather, a general definition suited for both the discrete and continuous case, was due to Cournot (1843, § 18). He replaced the ratio of chances by the ratio of their extents (*étendues*). Now, we would say, of their measures.

Bertrand (1888, p. 4) formulated his classical problem of the length of a random chord of a given circle.

7. Applications of the Theory of Probability

7.1. Population Statistics. The fathers of political arithmetic (§ 1) had good grounds to doubt, as they really did, whether quantitative studies of population were necessary for anyone excepting the highest officials. Indeed, social programmes began appearing in the 1880’s (in Germany); before that, governments had only been interested in counting taxpayers and men able to carry arms.

A new study belonging to population statistics, the calculation of the sex ratio at birth, owed its origin and development to the general problem of isolating randomness from Divine design. Kepler and Newton achieved this aim with respect to inanimate nature, and scientists were quick to begin searching for the laws governing the movement of population.

In 1712 Arbuthnot put on record that during 82 years (1629 – 1710) more boys had been yearly christened in London than girls. Had the probability of a male birth been $1/2$, he continued, the probability of the observed fact would have been 2^{-82} , i.e., infinitesimal. He concluded that the predominance of male births was a Divine law which *repaired* the comparatively higher mortality of men.

Nevertheless, his reasoning was feeble. Baptisms were not identical with births; Christians were perhaps somehow different from others, and London could have differed from the rest of the world; finally, the comparative mortality of the two sexes was unknown. Graunt (1662, end of Chapter 5) indirectly testified that between 1650 and 1660 less than a half of the population of England had been convinced in the need of christening. Then, any sequence of two symbols 82 terms long would have the same insignificant probability and even now the separation of random and non-random number sequences (and finite sequences especially) is extremely difficult. True, the Arbuthnot’s sequence was certainly non-random.

A special point is that Arbuthnot only understood randomness in the sense of equal chances of a male and female birth whereas the supposed Divine law could have well been expressed by a general binomial distribution with $p > 1/2$. And even now the divide between random and non-random sequences remains more than subtle, but at

least Arbuthnot's series m, m, m, \dots could not have been attributed to chance.

He had missed the opportunity to overcome Laplace who solved a similar D'Alembert – Laplace problem. The word *Constantinople* is composed of separate letters; what is the probability of its random composition? D'Alembert (1768b, pp. 254 – 255) suggested that problem but his solution was unconvincing. Laplace (1776/1891, p. 152; 1814/1995, p. 9) just remarked that the word was meaningful and therefore hardly composed randomly.

Arbuthnot could have treated his data by a binomial distribution with the business of Divine Design of choosing its parameter, and De Moivre (1733/1756, p. 253) said so, but such steps were still in the making.

Niklaus Bernoulli had developed Arbuthnot's arguments. Here is the latter's result which he formulated in a letter to Montmort of 1713. Denote the ratio of registered male births (let them be births) to those of females by m/f , the total yearly number of births by n , the corresponding number of boys by μ , set

$$n/(m + f) = r, m/(m + f) = p, f/(m + f) = q, p + q = 1$$

and let $s = 0(\sqrt{n})$. Then Bernoulli's derivation (Montmort 1708/1713, pp. 388 – 394) can be presented as follows:

$$P(|\mu - rm| \leq s) \approx (t - 1)/t, t \approx [1 + s(m + f)/mfr]^{s/2} \approx \exp[s^2(m + f)/2mfn],$$

$$P(|\mu - rm| \leq s) \approx 1 - \exp(-s^2/2pqn), P[|\mu - np|/\sqrt{npq} \leq s] \approx 1 - \exp(-s^2/2).$$

The last formula means that Bernoulli indirectly, since he had not written it down, introduced the normal law as the limit of the binomial distribution much earlier than De Moivre (directly) did. However, his finding does not lead to an integral limit theorem since s should remain small as compared with n (see above), and neither is it a local theorem since it lacks the factor $\sqrt{2/\pi}$.

Hald (1998, p. 17) had not mentioned that deficiency. Basing themselves on his description, three (!) modern mathematicians (Youshkevich 1986) decided that Niklaus had come close to the local theorem.

In the mid-18th century Achenwall created the Göttingen school of *Staatswissenschaft* (statecraft, university statistics) which strove to describe the climate, geographical position, political structure and economics of given states and to estimate their population by means of data on births and deaths. In this context, the gulf between political arithmetic and statecraft was not therefore as wide as it is usually supposed to have been, and Leibniz' manuscripts written in the 1680's indeed testify that he was both a political arithmetician and an early advocate of tabular description (with or without the use of numbers) of a given state. By the 19th century statecraft broke down because of the heterogeneity of its subject, whereas statistics, as we now know it, properly issued from political arithmetic. Nevertheless, it still exists,

at least in Germany, in a new form: it fully recognizes quantitative data and does not shirk from studying causes and effects.

Tabular statistics which originated with Anchersen (1741) could have served as an intermediate link between words and numbers but this had not happened (Sheynin 2017, § 6.2.1).

The father of population statistics was Süssmilch. He collected vast data on the movement of population and attempted to prove Divine providence as manifested in every field of vital statistics. He treated his materials faultily; thus, he combined towns and villages without taking weighted means, and he had not tried to allow for the difference in the age structures of the populations involved. Nevertheless, he turned Quetelet's attention to population statistics (to moral statistics, i. e. statistics of illegitimate births, crime and suicide, in particular) and his life tables remained in use well into the 19th century.

In his § 14 he allegorically pictured his main result. His occupation (a military priest) explained its name and essence: *An army regiment on the march*. Mankind marches through time in regular columns (age groups) and each experiences appropriate change owing to deaths and births. For more details see Sheynin (2017, § 6.2.2). There, in particular, I comment on the great difficulties experienced by early statisticians (although not by Süssmilch) who attempted to reconcile the Biblical command (*Be fruitful ...*) with the rapid growth of the population.

Euler actively participated in preparing the second edition (1765) of Süssmilch's main work, the *Göttliche Ordnung*, and one of its chapters was partly reprinted in his *Opera omnia*. Later on Malthus, without any references, adopted their indirect conclusion that population increased in a geometric progression, which is a still more or less received proposition. Euler left several contributions on population statistics, now collected in his *Opera omnia*. With no censuses (as we understand them now) at his disposal, he was unable to recognize the importance of some demographic factors, but he introduced such concepts as increase in population, and the period of its doubling. He worked out the mathematical theory of mortality and formulated rules for establishing life insurance in all its forms, cf. § 7.2 where I mention several previous scholars whom Euler had not cited.

During 1766 – 1771 Daniel Bernoulli contributed three memoirs to population statistics. In the first of these he (1766) examined the benefits of inoculation, – of communicating a mild form of smallpox from one person to another one, – which had been the only preventive measure against that deadly disease. The Jennerian vaccination became known at the turn of the 18th century, whereas inoculation had been practised in Europe from the 1720's. This procedure was not safe: a very small fraction of those inoculated were dying, and, in addition, all of them spread the disease among the population.

Bernoulli's memoir was the first serious attempt to study it, but even he failed to allow properly for the second danger. He formulated (necessarily crude) statistical hypotheses on smallpox epidemics and calculated the increase in the mean duration of life caused by inoculation. Concluding that this treatment prolonged life by more

than two years, he came out in its favour for the state as a whole. He thus applied the stochastic method for solving essentially new problems and heralded the advent of epidemiology.

Even before Bernoulli's memoir had appeared, D'Alembert (1761b) improperly voiced reasonable objections. Not everyone, he argued, will agree to expose himself to a low risk of immediate death in exchange for a prospect of living two remote years longer. And there also existed the moral problem of inoculating children. In essence, he supported inoculation, but regarded its analysis impossible.

In his second memoir Bernoulli (1768b) studied the duration of marriages, a problem directly connected with the insurance of *joint lives*. He based his reasoning on an appropriate problem of extracting strips of two different colours from an urn which he solved in the same year (1768a). He thus took into account the different mortalities of the sexes.

Bernoulli devoted his third memoir (1770 – 1771) to studying the sex ratio at birth. Supposing that male and female births were equally probable, he calculated the probability that out of $2N$ newborn babies m were boys:

$$P = [1 \cdot 3 \cdot 5 \dots (2N - 1)] \div [2 \cdot 4 \cdot 6 \dots 2N] = q(N).$$

He calculated this fraction not by the Wallis formula or the local De Moivre – Laplace theorem (which he possibly had not known), but by means of differential equations. After deriving $q(N - 1)$ and $q(N + 1)$, he obtained

$$dq/dN = -q/(2N + 2), dq/dN = -q/(2N - 1)$$

and, “in the mean”, $dq/dN = -q/(2N + 1/2)$. Assuming that the particular solution of this equation passed through point $N = 12$ and $q(12)$ as defined above, he obtained

$$q = 1.12826 / \sqrt{4N + 1}.$$

Application of differential equations was Bernoulli's usual method in probability.

Bernoulli also determined the probability of the birth of approximately m boys:

$$P(m = N \pm \mu) = q \exp(-\mu^2/N) \text{ with } \mu = 0(\sqrt{N}). \quad (4)$$

He then generalized his account to differing probabilities of the births of both sexes, and, issuing from some statistical data, compared two possible values of the sex ratio but reasonably had not made a definite choice.

A special feature of this memoir is that Bernoulli determined such a value of μ that the total probability (4) from $\mu = 0$ to this value ($\mu = 47$) was $1/2$. He calculated this total by summing rather than by integration and thus failed to obtain directly the De Moivre – Laplace theorem (2).

In 1772 Lambert followed Daniel Bernoulli in studying population statistics. He offered a purely speculative law of mortality, examined the number of children in families and somewhat extended Bernoulli's memoir on smallpox by considering children's mortality from this disease. Before treating the second-mentioned subject, Lambert increased the number of children by 1/2 thus apparently allowing for stillbirths and infant mortality. This rate of increase was arbitrary, but at least he attempted to get rid of a gross systematic mistake. Along with Bernoulli and Euler he created the methodology of mathematical demography.

7. Civil Life; Moral and Economic Issues

Jakob Bernoulli thought of applying probability to civil life and moral and economic affairs, but he did not have time to accomplish much in this direction. One aspect of civil life, i. e., games of chance, had indeed promoted the origin of the theory of probability (§ 1) and offered meaningful problems whose solutions became applicable in natural sciences and led to the creation of new mathematical tools used also in probability (§ 10.1). I shall now discuss other pertinent points.

In 1709, Niklaus Bernoulli published a dissertation on applying the *art of conjecturing* to jurisprudence, and, it ought to be added, he plagiarized Jakob Bernoulli (Kohli 1975b, p. 541) by borrowing from his as yet unpublished classical book of 1713 and even from his *Meditationes* (Diary) never meant for publication. Niklaus repeatedly mentioned his late uncle, which does not exonerate him.

Niklaus recommended the use of mean longevity and mean gain (or loss) in calculations concerning annuities, marine insurance, lotteries and in deciding whether an absent person ought to be declared dead. Both he and Jakob were prepared to weigh the appropriate probabilities against each other. Mentality really changed since the time when Kepler correctly, but in a restricted way, had simply refused to say whether the absent man was alive or dead.

When determining the life expectancy of the last survivor of a group of men (a problem important for life insurance) Niklaus (Todhunter 1865, p. 352) effectively introduced an order statistics and the continuous uniform distribution which was the first continuous law to appear in probability. Important theoretical work inspired by life insurance was going on from 1724 (De Moivre) onward (Thomas Simpson). Actually, insurance societies date back to the beginning of the 18th century, but before the second half of the 19th century more or less honest business, based on statistics of mortality, hardly superseded downright cheating. And, although governments sold annuities even in the 17th century, their price had then been largely independent from statistical data.

Stochastic studies of judicial decisions, of the voting procedures adopted by assemblies and at general elections, had begun in the late 18th century, but many later scientists denied any possibility of numerically examining these subjects. Thus, probability, misapplied to jurisprudence, had become "the real opprobrium of mathematics" (Mill 1843/1886, p. 353); or, in law courts people act like the "moutons de Panurge" (Poincaré 1896/1912, p. 20).

So, is it possible to determine the optimal number of jurors, or the optimal majority of their votes (when a wrong decision becomes hardly possible)? To determine the probability of an extraordinary fact observed by witnesses? Condorcet studied these and similar problems although hardly successfully. First, it was difficult to follow his exposition. Thus, Todhunter (1865, p. 352) concluded:

It is in many cases almost impossible to discover what Condorcet means to say.

Second, Condorcet had not made clear that his attempt was only tentative, that he only meant to show what could be expected in the ideal case of independent decisions being made. But at least he emphasized that *les hommes* should be educated and unprejudiced.

Laplace followed suit declaring that the representation of the nation should be the *élite* of men of exact and educated minds. Later he (1816, p. 523) remarked, although only once and in passing, that his studies were based on the assumption that the jurors acted independently one from another.

One of Condorcet's simple formulas (which can be traced to Jakob Bernoulli's study of stochastic arguments in his *Ars Conjectandi* and which Laplace also applied in 1812) pertained to extraordinary events (above). If the probabilities of the event in itself, and of the trustworthiness of the report are p_1 and p_2 , then the event acquires probability

$$P = \frac{p_1 p_2}{[p_1 p_2 + (1 - p_1)(1 - p_2)]}$$

This formula is however hardly applicable. Indeed, for $p_1 = 1/10,000$ and $p_2 = 0.99$, $P \approx 0.01$ so that the event will not be acknowledged by a law court, and a second trustworthy witness had to be found.

Moral applications of probability at least emphasized the importance of criminal statistics and assisted in evaluating possible changes in the established order of legal proceedings. As Gauss correctly remarked in 1841, the appropriate studies were unable to help in individual cases, but could have offered a clue to the lawgiver for determining the number of witnesses and jurors.

Applications of probability to economics began in 1738 with Daniel Bernoulli. In attempting to solve the Petersburg paradox (§ 10.2), he assumed that the advantage (y) of a gambler was connected with his gain (x) by a differential equation (likely the first such equation in probability theory)

$$y = f(x) = c \ln(x/a)$$

where a was the initial fortune of the gambler. Bernoulli then suggested that the *moral expectation* of gain be chosen instead of its usual expectation,

$$\sum p_i f(x_i) / \sum p_i \text{ instead of } \sum p_i x_i / \sum p_i;$$

the p_i 's were the probabilities of the respective possible gains and $f(x)$ was the logarithmic function (a very successful choice!).

The distinction made between gain and advantage enabled Bernoulli to replace the infinite expectation (10) appearing in a paradoxical situation by a new expression which was finite and thus to get rid of the paradox, see § 10.2. Neither did he fail to notice that, according to his innovation a fair game of chance became detrimental to both gamblers.

Bernoulli next applied moral expectation to study the shipping of freight and stated that (in accordance with common sense) it was beneficial to carry the goods on several ships. He did not prove this statement (which was done by Laplace).

Moral expectation became fashionable and Laplace (1812/1886, p. 189) therefore qualified the classical expectation by the adjective *mathematical*. Nowadays, it is still used in the French and Russian literature. In 1888 Bertrand declared that the theory of moral expectation had become classical but remained useless. However, already then economists began developing the theory of marginal utility by issuing from Bernoulli's fruitful idea.

The term *moral expectation* is due to Gabriel Cramer who had expressed thoughts similar to those of Daniel Bernoulli. The latter (1738) published a passage from his pertinent letter of 1732 to Niklaus Bernoulli.

8. The Theory of Errors

8.1. The Main problem. Suppose that m unknown magnitudes x, y, z, \dots are connected by a redundant system of n physically independent equations ($m < n$)

$$a_i x + b_i y + c_i z + \dots + s_i = 0 \quad (5)$$

whose coefficients are given by the appropriate theory and the free terms are measured. The approximate values of x, y, z, \dots were usually known, hence the linearity of (5). The equations are linearly independent (a later notion), so that the system is inconsistent (which was perfectly well understood). Nevertheless, a solution had to be chosen, and it was done in such a way that the residual free terms (call them v_i) were satisfying the properties of *usual* random errors.

The case of direct measurements ($m = 1$) should be isolated. Given, observations s_1, s_2, \dots, s_n of an unknown constant x (here, $a_i = 1$); determine its true value. The choice of the arithmetic mean seems obvious and there is evidence that such was the general rule at least since the early 17th century. True, ancient astronomers treated their observations in an arbitrary manner and in this sense even astronomy then had not yet been a quantitative science. However, since errors of observations were large, the absence of established rules can be justified. Thus, for *bad* distributions of the errors the arithmetic mean is not stochastically better (or even worse) than a single observation.

In 1722, Cotes' posthumous contribution appeared, see Gowing (1983). There, he stated that the arithmetic mean ought to be chosen, but he had not justified his advice, nor did he formulate it clearly enough. Then, in 1826, Fourier had defined the *veritable object of*

study as the limit of the arithmetic mean as the number of observations increased indefinitely, and many later authors including Mises, independently one from another and never mentioning Fourier introduced the same definition for the *true value*.

The classical problem that led to systems (5) was the determination of the figure of the Earth. Since Newton had theoretically discovered that our planet was an ellipsoid of rotation with its equatorial radius (a) larger than its polar radius (b), numerous attempts were made to prove (or disprove) this theory. In principle, two meridian arc measurements were sufficient for an experimental check (for deriving a and b), but many more had to be made because of the unavoidable errors of geodetic and astronomical observations (and local deviations from the general figure of the Earth).

At present, the adopted values are roughly $a = 6,378.1 \text{ km}$ and $b = 6,356.8 \text{ km}$. That $2\pi \cdot 6,356.8 = 39,941$ which is close to 40,000 is no coincidence: in 1791, the meter was defined as $1/10^7$ of a quarter of the Paris meridian. This *natural* standard of length lasted until 1872 when the meter of the Archives (called for the place it was kept in), a platinum bar, was adopted instead. From 1960, the meter is being defined in terms of the length of a light wave. The introduction of the metric system as well as purely astronomical problems had necessitated new observations so that systems (5) had to be solved time and time again, whereas physics and chemistry began presenting their own demands by the mid-19th century.

8.2. Its Solution. Since the early 19th century the usual condition for solving (5) was that of least squares

$$v_1^2 + v_2^2 + \dots + v_n^2 = \min.$$

Until then, several other methods were employed. Thus, for $m = 2$ the system was broken up into all possible subsystems of two equations each, and the mean value of each unknown over all the subsystems was then calculated. As discovered in the 19th century, the least-squares solution of (5) was actually some weighted mean of these partial solutions.

The second important method of treating systems (5) devised by Boscovich consisted in applying conditions

$$v_1 + v_2 + \dots + v_n = 0, |v_1| + |v_2| + \dots + |v_n| = \min \quad (6a, 6b)$$

(Maire & Boscovich 1770, p. 501). Now, (6a) can be disposed of by summing up all the equations in (5) and eliminating one unknown. And (6b) led exactly to m zero residuals v_i (Gauss 1809), which follows from an important theorem in the then not yet known linear programming. In other words, after allowing for restriction (6a), only $(m - 1)$ equations out of n need to be solved, but the problem of properly choosing these still remained. Boscovich himself applied his method for adjusting meridian arc measurements and he chose the proper equations by a geometric trick. Then, Laplace repeatedly applied the Boscovich method for the same purpose, for example, in vol. 2 of his *Mécanique céleste* (1799).

A special condition for solving systems (5) was $|v_{\max}| = \min$, the minimax principle. Kepler might have well made his celebrated statement about being unable to fit the Tychoonian observations to the Ptolemaic theory after attempting to apply this principle (even in a general setting rather than to linear algebraic equations). In 1749, Euler achieved some success in employing its rudiments. The discussed principle is not supported by stochastic considerations, but it has its place in decision theory and Laplace (1789, p. 506) clearly stated that it was suited for checking hypotheses (cf. Kepler's possible attitude above) although not for adjusting observations. Indeed, if even this principle does not achieve a concordance between theory and observation, then either the observations are bad or the theory wrong.

8.3. Simpson. I return now to the adjustment of direct observations. In 1756 Simpson proved that at least sometimes the arithmetic mean was more advantageous than a single observation. He considered the uniform, and the triangular distributions for the discrete case. After calculating the error of the mean he recommended the use of this estimator of the true value of the constant sought. Simpson thus extended stochastic considerations to a new domain and effectively introduced random observational errors, i. e. errors taking a set of values with corresponding probabilities. His mathematical tool was the generating function introduced by De Moivre in 1730 for calculating the chances of throwing a certain number of points with a given number of dice. De Moivre first published the solution of that problem without proof in 1711, somewhat earlier than Montmort (§ 3) who had employed another method.

For that matter, no doubt following De Moivre, Simpson himself had earlier (1740) described the same calculations, and he now noted the similarity of both problems. Consider for example his triangular distribution with errors

$$-v, \dots, -2, -1, 0, 1, 2, \dots, v \quad (7)$$

having probabilities proportional to

$$1, \dots, (v-2), (v-1), v, (v-1), (v-2), \dots, 1.$$

Simpson's (still unnamed) generating function was here

$$f(r) = r^{-v} + 2r^{-v+1} + \dots + (v+1)r^0 + \dots + 2r^{v-1} + r^v$$

and the chance that the sum of t errors equalled m was the coefficient of r^m in $f^t(r)$.

In 1757 Simpson went on to the continuous triangular distribution by introducing a change of scale: the intervals between integers (7) now tended to zero so that it became possible to assume that the segment $[-v; v]$ consisted of an infinitely large number of such intervals, and the distribution, as though given on a continuous set. But he arbitrarily and wrongly decided that his conclusions were valid for any given distribution of errors.

In 1776 Lagrange extended Simpson's memoir to other (purely academic) distributions. He introduced integral transformations, managed to apply generating functions to continuous distributions and achieved other general findings.

8.4. Lambert. Let $\varphi(x; \hat{x})$ with unknown parameter \hat{x} be the density law of independent observational errors x_1, x_2, \dots, x_n . Then the value of

$$\varphi(x_1; \hat{x}) \cdot \varphi(x_2; \hat{x}) \dots \varphi(x_n; \hat{x}) \quad (8)$$

corresponds to the probability of obtaining such observations. Hence the maximal value of (8) will provide the *best* value of \hat{x} . Now suppose, as it was always done in classical error theory, that the density is $\varphi(x - \hat{x})$, a curve with a single peak (mode) at point $x = \hat{x}$. The determination of the true value of the constant sought can then be replaced by calculation of the most probable value of \hat{x} . The derivation of the unknown parameter(s) of density laws became an important problem of statistics, and the principle of maximum likelihood (of maximizing the product (8)) provides its possible solution.

And so, it was Lambert who first formulated this principle for unimodal densities in 1760. Actually, he studied the most important aspects of treating observations and returned to this subject in 1765, this time attempting to determine the density of pointing a geodetic instrument by starting from the principle of insufficient reason (this term was introduced later) and to estimate numerically the precision of observations.

At the end of the 19th century the just mentioned principle was applied to substantiate the existence of *equally possible cases* appearing in the formulation of the notion of probability and soon afterwards Poincaré managed to soften essentially this delicate issue. In actual fact, the very notion of expectation, if not understood as an abstract concept (which it really is), can hardly be justified in any other way excepting *insufficient reason*.

Lambert (1765, § 321) also defined the *Theorie der Fehler* and included into its province both the stochastic and the deterministic studies of errors. Bessel had picked up this term, *Theory of errors*, and, although neither Laplace, nor Gauss ever applied it, it came in vogue in the mid-19th century.

A classical example of the deterministic branch of the error theory is Cotes' solution (1722) of 28 problems connecting the differentials of the various elements of plane and spherical triangles with each other. He thus enabled to calculate the effect of observational errors on indirectly determined sides of the triangles.

8.5. Daniel Bernoulli. In 1778, Daniel Bernoulli denied the arithmetic mean and, without mentioning Lambert, advocated the principle of maximum likelihood. Taking a curve of the second degree as the density law of the observational errors, and examining the case of only three observations, he obtained an algebraic equation of the fifth degree in \hat{x} , the estimator of the constant sought.

In a companion commentary, Euler reasonably denounced the principle of maximum likelihood since in the presence of an outlying observation the product (8) becomes small, and, in addition, contrary to common sense, the decision of whether to leave or reject it becomes important. He therefore advocated the arithmetic mean although the median would have best answered his remark. That term was only introduced by Cournot (1843).

Then, nevertheless following Bernoulli but misinterpreting him, Euler derived a cubic equation in \hat{x} and noted that it corresponded to the maximal value of the sum of the squares of the weights of the observations. If the small terms of this sum are rejected, his condition becomes

$$(\hat{x} - x_1)^2 + (\hat{x} - x_2)^2 + \dots + (\hat{x} - x_n)^2 = \min \quad (9)$$

which leads to the arithmetic mean, still alive and kicking!

Heuristically, (9) resembles the condition of least squares (and, indeed, in case of $m = 1$ least squares lead to this mean). Furthermore, Gauss (1809) derived it from the principle of maximum weight which might, again heuristically, be compared with Euler's condition (9).

Finally, in 1780 Bernoulli considered pendulum observations. Drawing on his previous memoir, he applied formula (4), i. e., the normal law, for calculating the error of time-keeping accumulated during 24 hours. He then isolated random (*momentanearum*) errors, whose influence was proportional to the square root of the appropriate time interval, from systematic (*chronicarum*), almost constant mistakes. These two categories are still with us, but his definitions were much too narrow.

8.6. Laplace. Laplace's main achievements in error theory belong to the 19th century. Before that, he published two memoirs (1774b; 1781) bearing on this subject and interesting from the modern point of view but hardly useful from the practical side. Thus, he introduced, without due justification, two academic density curves. Already then, in 1781, Laplace offered his main condition for adjusting direct observations: the sum of *errors to be feared of* multiplied by their probabilities (i. e., the absolute expectation of error) should be minimal. In the 19th century, he applied the same principle for justifying the method of least squares, which was only possible for the case of normal distribution (existing on the strength of his non-rigorous proof of the central limit theorem when the number of observations was large).

Also in 1781, Laplace proposed, as a density curve,

$$\varphi(\alpha x) = 0, x = \infty; \varphi(\alpha x) = q \neq 0, x \neq \infty, \alpha \rightarrow 0.$$

His deliberations might be described by the Dirac delta-function. However, one of his conclusions was based on considering an integral of

$$\varphi[\alpha(x - x_1)] \cdot \varphi[\alpha(x - x_2)] \dots \cdot \varphi[\alpha(x - x_n)]$$

(where the x_i 's were the observations made) which has no meaning in the language of generalized functions.

From its very beginning, the theory of errors belonged to probability theory (Simpson), but its principles of adjusting observations (of maximal likelihood; of least absolute expectation; of least squares) had been subsequently taken over by statistics.

9. Laplace's Determinism

According to Laplace's celebrated utterance (1814/1995, p. 2), for an omniscient intelligence "nothing would be uncertain, and the future, like the past, would be open to its eyes". He did not say that initial conditions could not be known precisely and of course he did not know anything about instability of motion (Poincaré) or about modern ideas on the part of randomness (or chaos) in mechanics.

Already in the beginning of his career he (1776, p. 145) denied randomness ("Le hasard n'a ... aucune réalité en lui-même") but remarked that "le plus grand nombre des phénomènes" could only be studied stochastically and attributed the emergence of the "science des hasards ou des probabilités" to the feebleness of the mind. The real cause for the origin of probability was rather the existence of stochastic laws determining the behaviour of sums (or other functions) of random variables; or, the dialectical interrelation between the randomness of a single event and the necessity provided by mass random phenomena.

A case in point is the *statistical determinism*. Thus, in 1819 Laplace noticed that the receipts from the Lottery of France had been stable. Elsewhere, he (Lectures 1795/1812, p. 162) remarked that the same was true with regard to the yearly number of dead letters. The generally known statement about the figures of *moral statistics* (of marriages, suicides, crimes) is due to Quetelet. It is hardly known that he actually meant stability under constant social conditions, and said so.

Two additional points are worth stating. First, nobody ever claimed that Laplace's philosophy had hindered his studies in astronomy or population statistics (based on stochastic examination of observations, see § 11). Moreover, he (1796/1884, p. 504), when discussing the eccentricities of planetary orbits and other small deviations from "une parfaite régularité", effectively recognized randomness. He was unforgivably mistaken: Newton had proved that the eccentricities were determined by the velocity of the planets.

Second, belief in determinism and actual recognition of randomness did not begin with Laplace. Kepler denounced chance as an abuse of God, but he had to explain the eccentricities by random causes. Laplace (and Kant) likely borrowed this idea from him, or from Newton (1718/1782, Query 31, p. 262) who actually recognized randomness as Kepler did: the "wonderful uniformity in the planetary orbits" was accompanied by "inconsiderable irregularities ... which may have risen from the mutual actions of comets and planets upon one another". Finally, Laplace might have found his statement about the omniscient intelligence in earlier literature (Mauvertuis 1756, p. 300; Boscovich 1758, §§ 384 – 385).

10. Some Remarkable Problems

10.1. The Gambler's Ruin. A series of games of chance is played by A and B until one of them is ruined. How long can the series be? What is the probability that A (or B) will be ruined not more than in n games? These are some questions here. In its simplest form the problem of ruin is due to Huygens.

Suppose that A has a counters, the probability of his winning a game is p , and the respective magnitudes for B are b and q ($p + q = 1$). Call P_a the probability of A 's loosing all his counters before winning all those belonging to B , let P_{an} be the probability of his ruin in not more than n games and denote the respective magnitudes for B by P_b and P_{bn} . The entire game can be imagined as a movement of a point C along a segment of length $(a + b)$, up to b units to the left and up to a units to the right. After each game C jumps to the left with probability p or to the right with probability q , and the play ends when C arrives at either end of the segment. Between these barriers C will walk randomly. And a random walk (which can also be imagined in a three-dimensional space) is a crude model of diffusion and Brownian motion.

Jakob Bernoulli several times treated this problem either incompletely (like Huygens did) or leaving the proof of his formula to his readers. It was De Moivre, who already in 1711 proved the same formula by an ingenious reasoning. He established that

$$\frac{P_A}{P_B} = \frac{a^q(a^p - b^p)}{b^p(a^q - b^q)}, a \neq b.$$

He also offered rules for calculating either the probability ($P_{an} + P_{bn}$) that the play will end within n games or the probabilities P_{an} and P_{bn} separately, and, in addition, he considered the case of $a = \infty$. De Moivre extended his research: in 1718 he provided answers to other problems although without justifying the results obtained. The demonstrations are now reconstructed (Hald 1990, § 20.5).

De Moivre's later findings were especially important because of the new method which he devised and applied here, the method of recurring sequences. Laplace discussed the problem of the gambler's ruin in several memoirs. He (1776) solved it by means of partial difference equations even for the case of three gamblers. Lagrange devoted the last section of his memoir of 1777 on these equations to their application in probability. There, he solved several problems which, in particular, were concerned with the gambler's ruin. On the duration of play see for example Kohli (1975a).

10.2. The Petersburg Paradox. In a letter to Montmort of 1713 Niklaus Bernoulli described his invented game (Montmort 1708/1713, p. 402). A gives B an *écu* if he throws a six at the first attempt with a common die; he also promises 2, 4, 8, ... *écus* if the six first appears at the second, the third, the fourth, ... throw. Required is the expectation of B 's gain (call it $E\xi$). The conditions, but not the essence of the problem soon changed with a coin replacing the die. In this new setting

$$E\xi = 1 \cdot 1/2 + 2 \cdot 1/4 + 4 \cdot 1/8 + \dots = \infty, \quad (10)$$

whereas no reasonable man would have given much in exchange for a promised $E\xi$. This remarkable paradox has been discussed to this very day; here are the pertinent points.

a) It introduced a random variable with an infinite expectation.

b) It inspired scholars to emphasize that a low probability of gain (lower than some positive α) should be disregarded, i. e., that only a few terms of the infinite series be taken into account). But how large ought to be the maximal value of α ? And a similar question for probabilities of loss higher than $1 - \alpha$? There is no general answer, everything depends on circumstances lying beyond the province of mathematics. The value $\alpha = 1/10,000$ recommended by Buffon (1777), – the probability that a healthy person aged 56 years dies within the next 24 hours, – had intuitive appeal, but it was too low and never really adopted as a universal estimate. Cf. the concept of moral certainty introduced by Descartes and Huygens (§ 1) and taken up by Jakob Bernoulli.

c) It prompted Daniel Bernoulli to introduce the moral expectation (§ 6) which enabled him to solve the paradox by getting rid of the infinity in (10). His contribution was published in a periodical of the Petersburg Academy of Sciences, hence the name of the paradox.

d) It led to an early and possibly the first large-scale statistical experiment: Buffon, in the same contribution of 1777, described his series of 2,048 Petersburg games. The average payoff per game occurred to be only 4.9 and the maximal number of tosses in a game was nine, and then only in six cases.

e) Condorcet, and later Lacroix discovered a more proper approach to the paradox: the possibly infinite game, as they maintained, presented one single experiment so that only a mean characteristic of many such games can provide a reasonable clue. Freudenthal (1951) studied a series of Petersburg games with the gamblers taking turns by lot in each of them.

f) A digression. Buffon's experiment illustrated runs (sequences) of random events with one and the same probability of *success*. Montmort testified that gamblers were apt to make wrong conclusions depending on the appearance (or otherwise) of a run in a series of independent games of chance. At present, runs are made use of to distinguish between chance and regularity. Suppose that a certain dimension of each machine part in a batch is a bit larger than that of a standard part; how probable is it that something went wrong?

De Moivre solved important problems connected with probabilities of number sequences in sampling. In 1767 Euler met with similar problems when studying lotteries and solved them by the combinatorial method. In 1793 John Dalton applied elementary considerations when studying the influence of auroras on the weather and in the 19th century Quetelet and Köppen described the tendency of the weather to persist by elements of the theory of runs.

10.3. The probability of sunrise. I touched on the determination of the probability of the next sunrise as discussed by Price (§ 5).

10.4. The Ehrenfests' Model. Each of two urns contains an equal number n of balls, white and black, respectively. Determine the (expected) number of white balls in the first urn after r cyclic interchanges of one ball. Daniel Bernoulli solved this problem by the combinatorial method and, in addition, by applying differential equations. He also generalized his problem to three urns with balls of three colours and noted the existence of a limiting case, of an equal (mean) number of balls of each colour in each urn. At present, this can be proved by referring to a theorem concerning homogeneous Markov chains.

In 1777 Lagrange solved a similar problem for any finite number of urns and balls of two colours. He employed partial difference equations as did Laplace in 1811 when solving a similar problem. Laplace (1814/1995, p. 42) also poetically interpreted the solution of such problems:

These results may be extended to all naturally occurring combinations in which the constant forces animating their elements establish regular patterns of actions suitable to disclose, in the very midst of chaos, systems governed by admirable laws.

Nevertheless, it is difficult to discover his *constant forces*, and a later author (Bertrand 1888, p. xx) put it better: "Le hazard, à tout jeu, corrige ses caprices". True, he only connected his remark with the action of the law of large numbers; in his case, the less was the relative number of white balls (say) in an urn, the less probable became their future extractions.

The future history of such urn problems as described above includes the celebrated Ehrenfests' model (§ 10.4) which is usually considered as the beginning of the history of stochastic processes.

11. Mathematical Statistics

Roughly speaking, the difference between probability and statistics consists in that the former is deductive whereas the latter (excepting its own theoretical part) is inductive and has to do with making conclusions from quantitative data. Mathematical statistics emerged in the 20th century and the term itself had hardly appeared before C. G. A. Knies introduced it in 1850.

However, problems connected with inductive inference are very old: even ancient scholars and lawgivers, drawing on numerical data, strove to distinguish between causality and randomness, e. g., between deaths from an emerging epidemics and the "normal" mortality (the Talmud, see its treatise Taamit). Beginning with Petty and Graunt (§ 1), crude statistical probabilities were being applied for estimating populations, and Arbuthnot's problem concerning the births of boys and girls (§ 6) was also inductive. The main goal of De Moivre's *Doctrine of Chances*, as he himself declared, was the choice between Design and randomness.

By studying the statistical determination of the probability of a random event, Bayes (§ 5) opened up a chapter of mathematical statistics. For Laplace, probability became the decisive tool for discovering the laws of nature (he never mentioned Divine Design). Thus, after establishing that the existence of a certain astronomical magnitude, as indicated by observations, was highly probable, he

(1812/1886, p. 361) felt himself obliged to investigate its cause and indeed proved its reality. Several chapters of his classic *Théorie analytique ...* can now be called statistical. Since he based it on his earlier memoirs, it is natural that there we find him (1774b, p. 56) mentioning *un nouveau genre de problème les hasards* and even *une nouvelle branche de la théorie des probabilités* (1781, p. 383). The expression *nouvelle branche* was due to Lagrange, see his letter to Laplace of 13.1.1775 in t. 14 of his *Oeuvres*, who thus described the latter's estimation of a certain probability.

A remark made by Laplace (1812) can be connected with the present-day statistical simulation. He enlarged on Buffon whose study was first announced in an anonymous abstract in 1735 and published in 1777, see his needle problem in § 6. A curious and wrong statement made by the astronomer William Herschel (1817/1912, p. 579) shows that statistics was sometimes thought to be more powerful than it was (or is). He argued that the size of any star, "promiscuously chosen" out of the 14,000 stars of the first seven magnitudes was "not likely to differ much from a certain mean size of them all". Unlike observational errors (say), stars (of differing physical nature!) could not have belonged to one and the same statistical population. Only in the former case we may estimate (by applying the later Bienaymé – Chebyshev inequality and issuing from data!) the deviations of the possible values of a random variable from their mean.

Sampling theory is a chapter of statistics, but the practice of sampling in England goes back at least to the 13th century when it began to be applied for assaying the new coinage (Stigler 1977). For many years, W. Herschel engaged in counting the stars in heaven. In his report of 1784 he noted that in one section of the Milky Way their multitude prevented those counts, so that he only counted the stars in six "promiscuously chosen" fields, i. e., applied the principle of sampling. He also counted the stars in a "most vacant" field, obviously for checking the lower bound of his calculated estimate of the total number of stars in the section.

In the absence of censuses, Laplace (1786) employed sampling for calculating the population of France (M). He knew the population of a small (sample) part of the country (m), the yearly number of births both there and over entire France (n and N), and, assuming that the ratio of births to population was constant, he concluded that $M = Nm/n$. Laplace then applied his earlier formulas (end of § 5) for estimating the possible error of this figure. In 1928 Karl Pearson reasonably remarked that Laplace's urn model (§ 5) of which he made use here was not adequate and that his relevant approximate calculations were imperfect. Still, Laplace was the first to study the error of sampling whereas his method of calculation (of the incomplete B function) was not improved for more than a century, cf. § 5 on the appropriate efforts made by Bayes.

12. The Opposition

The theory of probability did not develop unopposed. Leibniz, in his correspondence with Jakob Bernoulli (Kohli 1975c), denied that statistical probability should be regarded as an equal of its theoretical counterpart. The former, he argued, depended on an infinity of

circumstances and could not be determined by a finite number of observations. Jakob, however, remarked that the opposite might be true for the ratio of two infinities (apparently: for the rate of success in Bernoulli trials). Later on Leibniz changed his opinion. In any case, in a letter of 1714 he even claimed, without any justification, that the late Bernoulli “a cultivé” probability “sur mes exhortations”.

De Moivre (1718/1756, p. 254) stated that

There are Writers, of a Class indeed very different from that of James Bernoulli, who insinuate as if the Doctrine of Probabilities could have no place in any serious Enquiry ... [that its study was] trivial and easy [and] rather disqualifies a man from reasoning on every other subject.

Simpson (1756, p. 82) defined the aim of his memoir on the arithmetic mean (§ 8.3) as refuting

Some persons, of considerable note, who ... even publicly maintained that one single observation taken with due care, was as much to be relied on as the mean of a great number of them ...

Indeed, natural scientists might have persisted in Robert Boyle’s belief (1772/1999, p. 376) that “experiments ought to be estimated by their value, not their number”. However, the two approaches should be complementary rather than contradictory.

The main culprit was however D’Alembert (who nevertheless did not check the advance of probability). In 1754 he claimed that the probability of throwing two heads consecutively was $1/3$ rather than $1/4$. He also believed that after several heads in succession tails will become more likely and he aggravated this nonsense by an appeal to determine probabilities statistically (which would have proved him wrong). Then (1768a), he was unable to understand why the mean and the probable duration of life did not coincide.

Euler (Juskevici et al 1959, p. 221), in a letter of 27 May/7 June 1763, mentioned D’Alembert’s “unbearable arrogance” and argued that he had tried “most shamelessly to defend all his mistakes” [possibly not only in probability]. Witness also D’Alembert’s invasion (1759/1821, p. 167) of an alien field of knowledge: “The physician most worthy of being consulted is the one who least believes in medicine”.

True, D’Alembert also put forward some reasonable ideas. He remarked, after Buffon, that low probabilities of gain ought to be discarded and noted that the benefits of inoculation (§ 6) should be reassessed. In general, some of his criticisms were ahead of the time since they implied that the theory of probability ought to be built up more rigorously.

13. On the Threshold of the Next Century

The new century began with the appearance, in 1812, of Laplace’s *Théorie* (which I had to mention above). There, he brought together all his pertinent memoirs (including those of 1809 – 1811), but failed to merge them into a coherent whole. True, he applied the De Moivre – Laplace limit theorem wherever possible, but he did not introduce, even on a heuristic level, the notion of a random variable, did not therefore study densities or characteristic functions per se; his theory

of probability, which reasonably belonged to applied mathematics, did not admit of development.

But what was achieved up to 1801? The first limit theorems were proved; generating functions and difference equations were introduced and applied; and integrals were approximated by new and complicated methods. The study of games of chance originated important topics with future applications in natural sciences and economics. Probability became widely applied to population statistics and treatment of observations (and jurisprudence), but natural sciences did not yet yield to this new discipline. Problems really belonging to mathematical statistics were being solved again and again and the time became ripe for Gauss to develop the method of least squares.

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Abbreviation: AHES = *Arch. Hist. Ex. Sci.*

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The aftermath

For the community as a whole, there is nothing as extravagantly expensive as ignorance (Shaw et al 1926/1942, p. v).

This statement by meteorologists is generally true, and reviewing of manuscripts essentially contributes to the spread of knowledge. To apply an old word in a new meaning: It is a duty of *scientologists* to define guidelines for reviewing rather than to attempt to solve an unsurmountable problem of quantifying science.

For about five years I had been a staff subeditor of the Soviet journal of abstracts *Astronomia i Geodesia*, and I know what a reviewer should, and should not do. Reviewers often do not understand their duty, too often are motivated by sympathy or antipathy or by fear of losing face. This is especially true when a field is being cultivated by a small number of researchers as it occurs in the history of mathematics. The situation is greatly worsened since the scientific community does not appreciate reviewing, an anonymous and unpaid activity. Anyway, I concentrate on the relations between editors and authors.

I describe my attempt to publish a paper (downloadable file 21a on my website www.sheynin.de) originally published in 2002 in Italian. About six months ago I submitted its manuscript, in a slightly revised form, to the *Archives Internationales d'Histoire des Sciences*. In August I received the texts of the reviews with the key statement in the first of them: *Il ne s'agit d'un travail de recherché* and I append their texts. I am now describing my correspondence with the Editor.

In July, she warned me that the texts of the reviews to be sent to me are to remain confidential and I am forbidden to show or discuss them with others. I disregarded this strange requirement. On 30 July she wrote:

As the editor I am expert enough to decide if a paper could or not be published. The double blind reports ... are of the greatest help to me ... You will find the reports ... These are confidential, only you and

the members of the editorial board could have access to them.

Michela

She wrote out the address. It is very long and very complicated and is printed in small letters and I am unable to reproduce it. In a few days I finally pressed that address once more to find the second report. That was my mistake: I was so disgusted with the first report that did not read it properly and did not realise that there were both reviews in one document. Especially disgusted since Prof. Cappelletti (vice-president, Diretto scientifico dell'Istituto della Enc. Italiana) had sent me a thankyou letter which I did not regrettably keep. This is what I read at that address:

Le fichier que vous avez demandé n'est plus disponible seu téléchargement. Ce service ne conserve pas les fichiers après expiration.

So she refused to hear me out, and I, an effective member of that International Academy, left it. My final conclusion: the right of an author to defend himself against review(s) is of paramount importance. Deprive him of that right, and the entire institution of reviewing manuscripts becomes hardly useful and often damaging.

Report No. 1

Ce texte ne constitue pas un article. C'est, au mieux, une note issue d'un travail de master, au pire un catalogue de formules mathématiques que l'on trouve dans n'importe quel ouvrage de cette discipline consacré au calcul des probabilités ou aux statistiques. Il ne s'agit en aucun cas d'un travail de recherche. Les formules sont présentées le plus souvent par mathématiciens et selon l'ordre plus ou moins chronologique, sans aucune tentative systématique de regroupement ou de classement thématique, voire, encore moins de réflexion ou d'analyse du processus de développement et de la constitution de ces objets très précis et subtils que sont les concepts (mais il n'est aucunement question dans ce texte de "concepts") mis en œuvre par le calcul des probabilités et l'histoire des techniques statistiques.

Sans entrer dans le détail (à quoi bon ?), une seule remarque montrera la confusion temporelle entretenue par ce texte qui n'obéit même pas aux règles élémentaires permettant d'éviter la vision progressive et finaliste de l'histoire présentée ; on trouve en effet ce propos p. 3 : « Continental author noticed De Moivre's theorem. In 1812, Laplace proved the same proposition (hence its name introduced by Markov) by means of the McLaurin – Euler summation formula and provided a correction term which allowed for the finiteness of the number of trials. » Quel est le but de cette référence sybilline à la présentation par Markov de la formule de Laplace ? Pourquoi ne pas l'explicitier en quelques phrases ? Et ce n'est peut-être pas un hasard si Andrei Markov présente cette formule de la manière dont il la présente. Mais cette référence n'est dans le texte que troublante et aucunement pertinente pour le propos qu'il prétend tenir.

Chaque "partie" est d'ailleurs constituée d'un méli-mélo de références et de renvois à d'autres auteurs sans aucune méthode (voir par exemple l'incroyable 6e partie « population statistics », commençant p. 6 et présentant dans un étourdissant tourbillon un catalogue d'auteurs de toutes disciplines ayant contribué à la complexe histoire de la statistique démographique au XVIIIe siècle, avec d'ailleurs l'oubli de contributions tout aussi imporrantes que celles qui sont mentionnées, à savoir, entre autres les travaux de Nicolas Struyck, Antoine Deparcieux (deux auteurs traitant à la fois de calculs de probabilités et de statistique démographique), Willem van

Kersseboom et Pehr Wargentín, pour ne nommer que ces "célèbrités", ici laissées de côté.

Notons enfin l'absence d'une quelconque réflexion sur les usages différenciés des calculs présentés dans le texte ; quitte à prétendre faire de l'histoire de la technique mathématique dans le domaine des probabilités et des statistiques, autant la faire avec un minimum de subtilité et de sens historique. Ce n'est guère le cas ici où tout se passe dans le monde enchanté d'une bibliothèque qui serait d'ailleurs quelque peu en désordre.

Ce texte est donc à mon sens inutile et ne constitue même pas un compendium des ouvrages de référence que sont ceux de Isaac Todhunter, Harald Westergaard, Stephen Stigler ou Anders Hald, d'ailleurs tous mentionnés en bibliographie (est-ce pour respecter une règle académique nécessitant de mentionner les grands Anciens ?) et dont la lecture pourrait aisément remplacer celle de ce texte dépourvu de toute pertinence.

Je m'étonne donc de constater que ce texte ait pu être déjà publié ; on pourrait penser que les défauts qui lui sont inhérents seraient dus à une mauvaise traduction de l'italien en anglais. Mais cette hypothèse me paraît tout à fait improbable : c'est bien fondamentalement de la faiblesse du contenu dont il s'agit ici et non simplement d'une question de forme. En conclusion, je considère ce texte comme ne pouvant pas du tout être publié.

Report No. 2

La contribution se propose d'aborder une question déjà bien connue: la genèse de la probabilité et de la statistique au 18^e siècle. L'auteur ne problématise jamais la question et son texte se présente comme une histoire essentiellement chronologique. Des thématiques organisent le propos (« Population statistics », « Civil life », etc.) elles-mêmes organisées parfois en sous-ensembles (Section 7: la vie civile, longévité, décisions de justice, économie, etc.), le tout invariablement traité sous un angle chronologique. Les apports des différents savants évoqués sont résumés et non pas analysés de telle sorte qu'on lit une succession d'abrégés qui n'apporte rien sur le plan analytique et philosophique.

On peut être extrêmement surpris par des raccourcis et une méconnaissance de certaines questions qui sont abordées. À titre d'exemple, l'œuvre de Condorcet sur la probabilité est expédiée en quelques lignes et accompagnée d'un jugement de Todhunter (1820-1884) (Section 7). L'auteur de l'article ne remet pas en cause ce jugement vieux de 150 ans et semble donc ignorer les nombreuses recherches collectives et individuelles de ces trente dernières années qui l'ont invalidé et mis en perspective. Le court passage sur Süßmilch me paraît très indigent.

La bibliographie ne mentionne pas que des sources ; elle fait aussi appel à la littérature critique moderne. On s'étonne alors de ne voir mentionné aucun travaux récents sur Condorcet comme sur beaucoup d'autres auteurs. D'Alembert aurait certainement mérité plus qu'une seule référence. À propos du sex ratio, l'auteur aurait pu citer plusieurs ouvrages, notamment celui É. Brian et M. Jaisson, *Le Sexisme de la première heure. Hasard et sociologie* (2007) où il aurait au moins trouvé des analyses précieuses sur le contexte historique et idéologique.

La section intitulée « introduction » ne permet pas de deviner dans quelle direction l'auteur veut conduire son propos. La conclusion (section 13) témoigne d'une conception très ancienne de l'histoire des idées.

L'ensemble de ces choix réduit l'article à une notice, ou plus précisément à une série de notices pour manuel qui pourraient être instructives par leurs aspects factuels si elles ne comportaient des erreurs manifestes.

PS : Dans la bibliographie de D'Alembert, mieux vaudrait signaler l'édition de 1767 des *Mélanges de littérature*, celle de 1768 étant une contrefaçon hollandaise. Ce n'est qu'un point de détail que ne pouvait pas connaître l'auteur.

My comments

Some of the reviewers' statements are very strange, and some are unsubstantiated. I am even accused of omitting many sources published after the appearance of my paper (more properly, after I had submitted it, likely in 2000). This especially concerns Condorcet to whom I in any case had allegedly paid too little attention. And I resolutely oppose the general tone of the reports.

Condorcet compiled an unscientific obituary of Daniel Bernoulli (Sheynin 2009) and in a letter of 1772 (Sheynin 2017, § 6.1.5) he stated that he was *amusing himself* by calculating probabilities and that he was keeping to the opinions of D'Alembert. Everyone familiar with the history of probability will be surprised.

I am now proposing a draft of a reasonable review:

Only the author had described his subject in a short paper. His description of the merits of Bayes is quite new. However, the paper should be updated, a few mistakes ... corrected and additional information ... provided.

I asked S. S. Demidov, the president of the Academy, to intervene, but he kept silent. He certainly did not read attentively those reviews (whose texts I had sent him). We had been friendly rubbing shoulders for a few decades, and I have rendered him a few substantial services, so he should have communicated with me. I think that his attitude was unworthy.

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III

A. A. Sergeev

Scientific Biography of K. A. Posse

Istoriko-Matematicheskie Issledovania, vol. 35, 1994, pp. 64 – 95

Konstantin Aleksandrovich Posse was a representative of the Petersburg mathematical school and a direct student of Chebyshev. For ten years he had been working together with his mentor in the Petersburg University. He also collaborated with such outstanding scientists as E. I. Zolotarev, A. N. Korkin, A. A. Markov and Yu. V. Sokhotsky¹. Posse's contribution to science is less weighty than the achievements of some of them which probably is the reason why his scientific life is little known even to historians of mathematics. However, the biography of this scientist and teacher deserves attention. Lyapunov, Markov, V. A. Steklov and other prominent mathematicians invariably respected him. His vast correspondence preserved by the Archive of RAN allows a reconstruction of his relations with contemporaries.

My paper consists of three parts: his biography mostly based on archival documents; his pedagogic activity; and a survey of his scientific work [not translated²]. I append the review written by Korkin and Sokhotsky of the doctor dissertation of Posse [not translated]. All the dates [concerning Russia] until 1918 are in the old style.

1. Biography

[1.1] Posse was born on 29 September 1847³ in the country estate of Obrechie, Borovich district, Novgorod province. His grandfather on the father's side, Fedor, a medical practitioner⁴, was a Swede whose forefather had arrived in Russia at the time of Peter the Great. He left his small fortune to his daughter since he thought that his sons, Ivan (the eldest) and Aleksandr (the youngest) ought to regulate their lives by themselves. Both took to a military career. The eldest graduated from an artillery school, the youngest, the father of K. A., graduated from the Institute of the Corps of engineers of means of transportation. In those times it was a military educational institution and the graduates were given the rank of lieutenant.

Aleksandr participated in the construction of the railway connecting Petersburg and Moscow. After retirement he became engaged in commerce, was successful and bought a house in Petersburg. Then, however, his business became shaky and he moved to his (?) estate in the Novgorod province and lived there to the end of his life.

He and his wife, Elizaveta Yakovlevna, née Kozlianinova, had six children. A son died in childhood. His eldest daughter Adelaida married I. I. Borgman, later a physics professor and the first elected rector of Petersburg University. Another daughter, Maria, taught mathematics in girls' gymnasiums [...]. *She knew her business and carried out her business perfectly well* (V. A. Posse 1933, p. 28).

Their sister Ekaterina married V. B. Struve, a teacher of mathematics and grandson of the great astronomer and the eldest brother of the not less famous political figure⁵. The youngest of the children, Vladimir, a lawyer and a physician by education, participated in the revolutionary movement (in the 1900's he kept to *legal Marxism*) and in the first third of the 20th century became a renowned publicist⁶.

[1.2] Until the age of twelve Konstantin Posse had been brought up at home, then he entered the fourth class of the Second Petersburg gymnasium, the oldest in the city established in 1805. It attracted the best educationalists of the city including teachers of the University; according to the bylaws of the day the gymnasium was managed by the University. In 1837 – 1856 the head of the gymnasium was A. F. Postels who successfully organized the teaching there. Later, he became member of the Council of the Ministry of public education.

Posse graduated in 1864 as one of the eleven students out of the 22 who learned in the seventh class, but two of them who had failed the final examinations only received certificates proving that they had completed the course of studies. Nine graduates were granted the right to enter a university (Central State Historical Archive of Leningrad, CSHAL, 174 – 1 – 264, p. 31).

The level of education was high. Indeed, during 1856 – 1866 the graduates of the gymnasium included the future professors of Petersburg University A. N. Diatlov, A. A. Inostrantsev, and Borgman [see above]; a professor of the Military Medical Academy N. G. Egorov and a vice-president of the Academy of Sciences L. N. Maikov. The famous traveller Mikloaho-Maclay studied in that gymnasium at the same time as Posse. In 1864, Posse was awarded a gold medal, the only one thus distinguished in the group of eleven (Ibidem, p. 34 rev.).

[1.3] In the autumn of that same year Posse began to attend lectures at the physical & mathematical faculty of Petersburg University although, not yet being seventeen, only as a lecture-goer. Next autumn, however, he became a regular student of the second year. It seems that professors O. I. Somov and Korokin had soon understood his abilities (CSHAL 14 – 5 – 3065, p. 5). At the time, there were not more than a thousand students (*Dela i Dni* 1920, book 1, p. 165). Outstanding scientists read mathematical courses. Somov read differential calculus and its application to geometry, and, also,

mechanics for undergraduate students. Chebyshev read integral calculus and number theory, and probability theory for students of the highest year. Korkin taught spherical trigonometry, analytic geometry and higher algebra; A. N. Savich, astronomy, D. K. Bobylev, the future famous mechanic (applied mathematician), a corresponding member of the Academy of Sciences, professor at Petersburg University and Lyapunov's teacher, studied at the same time as Posse.

In 1868 Posse submitted a composition *Euler integrals of the first and the second kind* and graduated as a candidate of mathematical sciences⁷. On 12 November 1870 he successfully passed the master examinations; his examiners were Korkin, Sokhotsky and Zolotarev (CSHAL 14 – 3 – 14798a, pp. 52 – 52 rev.).

Also at that time he married his cousin Emilia, the daughter of his uncle Ivan Fedorovich. Posse touchingly and thoughtfully looked after his family (The Russian Nat. Library, MS dept, fond 760, No. 397; Leningrad section, Archive of RAN 162 – 2 – 354 and 355) but he had to live through the death of two sons, of a daughter and an adopted son (CSHAL 14 – 3 – 9924; V. A. Posse 1933, p. 20)⁸.

[I.4] On 5 October 1871 the Scientific Council of the Institute of Engineers of Means of Transportation had elected Posse as teacher of mathematics and he began to read analytic geometry and differential and integral calculus. His educational activities lasted until extreme old age. He had been working in that Institute until 1881 when two junior classes were abolished (?) and again in 1890 – 1896 (CSHAL 14 – 3 – 9924, p. 114).

According to the recollections of his contemporaries he passionately loved teaching and attained an essential level in this field. The renowned physicist B. P. Weinberg (Leningradsky 1963, p. 141) noted in his memoirs:

The students had been enjoying his elegant, well-composed, serene and melodious speech. Even some lawyers attended his lectures. They often had not understood the content of his lectures but became imbued with their musicality and convincingness.

On 13 May 1873 Posse defended his master dissertation *On functions similar to the Legendre functions* (Petersburg, 1873). His opponents were Chebyshev (1951, pp. 297 – 298) and Sokhotsky.

In the autumn of that same year, after a test lecture, he, now a privat (unestablished)-docent, began reading analytic geometry to students of the mathematical department of Petersburg University. His connection with this university had lasted almost to the end of his life.

Posse was very versatile. When being young, he had been keen on theatre, and not only as a spectator. In his large apartment he organized performances and played the main masculine roles with feminine roles performed by his sister Adelaida. The repertoire was

diverse, from comedies by Ostrovsky⁹ to Shakespeare tragedies and it also included improvisations (V. A. Posse 1933, pp. 17 – 18).

For many years his apartment had been attracting city dwellers. Renowned scientists, men of letters, musicians often gathered there. Kovalevskaya also came. V. A. Posse (Ibidem) noted that

My [his] brother regarded Kovalevskaya with great respect, but remarked that a man with similar publications would not have become famous.

[1.5] Posse's attitude to women's education in Russia should be discussed in more detail. In 1878, the Higher women's (Bestuzhev¹⁰) courses were established but most of the eminent mathematicians negatively regarded women's occupation with their science and at first Posse had been no exception (S.– Peterburgskie 1965, p. 206):

I ought to confess that in those times I rather reservedly regarded the girl's fancy of busying themselves with mathematics. I thought that that was not serious, just an idle pastime for unoccupied young ladies.

Nevertheless, he was the first university mathematician to begin teaching in those courses and continued working there from 1878 to 1886 and again from 1900. During academic year 1906/1907 Posse established there the first scientific mathematical seminar (CSHAL 14 – 3 – 9924, p. 170). Among its participants were Vera Iosifovna Shiff and Nadezhda Nikolaevna Gernet, the first women to teach mathematics in Russian higher education institutions. Gernet later became professor of the Petrograd – Leningrad University and Polytechnic Institute. During the last year of Posse's work at the Bestuzhev courses among his listeners was the future academician P. Ya. Kochina.

[1.6] On 1 September 1880 Posse became staff docent of the University. He read the following courses: from 1873, analytic geometry for students of the first year; in 1879/1880, in addition, introduction to analysis. And next year, again in addition, differential calculus for second-year students.

In 1882, Chebyshev left the University and the duties were redistributed. Introduction to analysis was entrusted to Markov and analytic geometry to N. S. Budaev. Posse continued to read lectures for students of the second year: in the autumn, differential calculus; and, in the spring, its application to geometry, and integration of functions. This arrangement persisted until 1899 when he left the University.

During those years Posse wrote a doctor dissertation *On functions θ of two variables and on the Jacobi problem*. On 28 November 1882, at its defence, the opponents were Korkin and Sokhotsky. Next year, on 17 January, Posse was confirmed as extraordinary professor, chair of pure mathematics. In the autumn of 1884 academicians V. Ya. Buniakovsky and V. G. Imshenetsky nominated Posse for

corresponding membership of the Academy of Sciences, but elected were an Italian, F. Brioshi, K. A. Andreev and A. V. Letnikov (Moscow) and V. P. Ermakov (Kiev).

On the other hand, on 8 April 1886 Posse became ordinary professor. Apart from the University, the Institute of means of transportation and the Bestuzhev courses, he had been then teaching at the Technological Institute.

[1.7] As stated above, Posse's occupations and interests had not been exhausted by science and teaching. He was an expert and connoisseur of classical music and an outstanding pianist. From the 1890s, he had been member-performer of the Petersburg Society of Chamber Music, 50 – 60 strong at the time. It mostly consisted of professional composers and musicians, suffice it to name Tchaikovsky, Rimsky-Korsakov, Glasunov, A. G. Rubinstein, A. K. Lyadov, Napravnik, Auer and Taneev.

The Society gave yearly about 50 closed and a few open concerts. Posse performed each year. In 1898, together with professional musicians Pugni, Homilius and others, he participated in five concerts and performed pieces of Bach, Brahms and Schubert (Otchet 1899 – 1913; Russ. Nat. Library, MS dept. 187 – 1010). Grave (Dobrovolsky 1968, p. 11) recollected:

Posse [...] finely felt and understood music. Almost blind, he gave serious pieces and compositions of modern composers which required good technique.

Then, Posse actively and in various ways participated in social life. In 1893 – 1905 he was committee member of the Literary Fund. It was established in 1859 with an active participation of A. K. Tolstoy, Turgenev, N. G. Chernyshevsky, N. A. Nekrasov and Dostoevsky. It supported young and aged men of letters and scientists, their widows and orphans, assisted creative trips abroad. Efforts of Posse and his friend N. S. Tagantsev, a famous lawyer and state figure, assisted in the prompt release of Gorky who was arrested in May 1898 (Russian Nat. Library, MS dept., fond 760, No. 397, p. 2).

There were many suchlike episodes in Posse's life. Neither did he remain aloof from the improvement of the legal system: for many years he had been elected juror. But his broad social activity had not overshadowed the students' interests from him. Posse invariably and attentively regarded their wants and defended their rights. In 1898 he pleaded for the release of an arrested student, then attempted to secure him permission to continue his education (Ibidem, pp. 1, 1 rev., 73 rev.).

[1.8] In March 1899 students' unrest had erupted in the University, and the rector attempted to normalize the situation by administrative measures. A few students were expelled and the studies discontinued. This, however, only backfired. Posse, together with professor

Borgman and V. T. Shevyakov declared that they disagreed with such methods and after some time the Council of the University secured the reinstatement of the expelled students without a humiliating written repentance as was proposed by the rector (CSHA 1129 – 1 – 63).

The school had invariably been in Posse's field of vision, and not only with regard to the teaching of mathematics. In the beginning of 1898 a famous educationist V. A. Gerd, director of a recently opened school, was arrested on political grounds which threatened the very existence of that princess M. K. Tenisheva education establishment. Later it became famous owing to many of its outstanding alumni. Posse pleaded for the release of Gerd so that an important pedagogic novelty will not perish from its beginning (Russian Nat. Library, MS dept., fond 760, No. 397, pp. 5 and 5 rev).

[1.9] Meanwhile, Posse's staff work at the University ended. On 25 October 1898, for service 25 years long he deserved his pension but remained for another year. His eyesight had essentially worsened and during the last years of his life he was practically speaking blind.

In 1899 Posse passed to the Electrotechnical Institute although remained an honorary member of the University. In 1905 he quit because of poor health and the Institute reluctantly let him go but persuaded him to continue as member of the Institute's Council, and he remained as such for ten years more. He it was who organized the teaching of mathematics there on a high level and to this very day that Institute is thus distinguished from many other technical institutes of the city.

Posse was unable to abandon completely his beloved occupation and continued to read additional lectures at the Electrotechnical Institute. He transferred his remuneration to the A. S. Popov¹¹ prizes fund (CSHAL 990 – 2 – 2744, p. 67). He also continued to lead the seminar in various problems of analysis not included in the curriculum at the Bestuzhev courses. There, he took into account the latest research. On 21 September 1913 he (Archive RAN, Leningrad branch 162 – 2 – 354, p. 85 rev.) wrote to V. A. Steklov:

Yesterday, I began the seminar and allowed myself to suggest the derivation of your formula [not written out] and the expansion of $[1 \div (e^x + e^{-x})]$ into fractions (with $\tanh x$ certainly appearing here). I naturally mentioned that you had informed me about all that.

Posse also participated in social activities connected with science. At that time, he was member of the Moscow and Petersburg mathematical societies and an honourable member of the Mathematical Society of Kharkov which also played an important role in the development of national mathematics apparently (?) since Lyapunov, Steklov and later Bernstein worked there.

There are many examples testifying that Posse invariably took care of the beginners in science. Many years later V. F. Kagan (Posse

1929, p. vii), who had come to Petersburg to pass his master examinations, thankfully recalled Posse. Indeed, Posse always pleaded for someone and among those whom he had assisted were [six people and *many others* are mentioned] (Archive RAN, Leningrad branch 869 – 4 – 630, pp. 1 – 4, 22, 33, 57, 58, 60 – 63, 133, 149, 150).

He (CSHAL 14 – 3 – 14885, pp. 225, 230; No. 14914, pp. 16, 16 rev.) was among the first ones who recommended mathematicians of the second generation of the Petersburg school (G. F. Voronoy, Grave, N. M. Günther) for scientific work and the first to appreciate duly the talent of V. A. Markov (Ibidem, No. 14895, p. 29):

V. A. Markov, a student of the seventh term, had submitted a composition On functions least deviating from zero on a given interval. I examined it and concluded that it deserves special attention and its author ought to be especially encouraged.

Later, Posse (Ibidem, p. 247) recommended the retention of Markov [after graduation] at the University with a stipend¹².

[1.10] Posse painfully took in the beginning of WWI. In spite of old age, bad health and work in a ministerial committee on reforming the secondary school [see end of § 2.3] he agreed to head the society *People's help* which required much efforts and energy. The Society assisted disabled soldiers and their families. He also collected and sent books and money to Russian students, prisoners of war, and participated in organizing help for them from the University (Archive, RAN, Leningrad branch 162 – 2 – 355, pp. 14, 14 rev., 26).

In those times his authority as the eldest national mathematician had been strengthening and in December 1916 he was elected honourable member of the Academy of Sciences. In autumn of 1917 Posse left Petrograd and went to Khvalynsk, Saratov province, where he hoped to teach mathematics in the Military Topographic School evacuated from the capital. He went alone hoping to move his family later.

Economic dislocation had already occurred and the trip lasted eleven days. He arrived exactly on 25 October 1917¹³, his aim was to live peacefully until his last day, remote from the stormy political life of the capital. However, fate decided otherwise. Until January 1918 he read analytic geometry and differential and integral calculus, but then the school closed. Until the navigation began, Posse had been living in Khvalynsk since he did not venture to go by rail. Judging by his extant correspondence with Steklov and N. S. Tagantsev, the time spent far from his friends and nearest and dearest, without anything to occupy himself, caused him much emotional suffering. Finally, in the spring of 1918 he became able to return to Petrograd.

[1.11] With no means for living, Posse, already quite sick, was compelled to return to teaching. He became professor at the Higher women courses, and in 1919, when they were merged with the University, began working there. Work, however, became ever more

difficult. He (Leningrad branch, Archive of RAN, 162 – 2 – 355, pp. 36, 36 rev.) wrote to Steklov on 23 December 1919:

I am unable to reach the University, to say nothing about your place. When walking in a freezing weather unbearable burning pain in the breast, makes me housebound. [...] I am therefore compelled to ask my listeners (five of them) to come to me, but am unable to get a blackboard. [...] promised to bring me one, but had not found a listener who would agree to fetch it even for a payment. But what will happen next half-year when two girls will come as well?

They wish to hear my lectures on the application of the integral calculus to geometry. As to my spirits, I am driven to despair. My eyesight has weakened to such an extent that I can only read a little by means of a powerful magnifying glass and even so, with difficulty. My memory is also essentially weakened and my capacity for work is rapidly disappearing.

In 1921, because of bad health, Posse had to quit work in the University. From then onward, his life had been grievous. He spent his last years in a house for aged scientists. Much efforts and energy was required for overcoming incessant troubles about his pension: both he and Sokhotsky received it irregularly. For January 1925 they only received 16½ roubles each out of which 40% had to be paid for life in that house (Ibidem, pp. 43 – 65)¹⁴. A touching undated postcard came from Steklov (Ibidem, p. 66): he tactfully offered Posse a few pounds of millet.

Material distress was intensified by being needless and neglected in spite of his unappeasable craving for useful work.

On 4 April 1924 he (Ibidem, pp. 45, 45 rev.) wrote to Steklov:

I was informed that apparently the library of RAN has a new [Russian] edition (Berlin, State Publishing House) of my course in differential and integral calculus. I have vainly applied to the publisher with a request to send me at least one copy of this book to judge how it was printed.

According to my bitter experience with the translation of Genocchi [1922] with 400 misprints on 320 pages (whole lines of my manuscript have been left out). I fear that this book is also spoiled. I cannot go to the library (I am still very weak after a serious illness). So I venture to ask you to order the delivery of my book to me (to the House of [aged] Scientists) with an obligation to send it back to you. If needed, I will append a list of the misprints (I will entrust its compilation to someone with a good eyesight). It is apparently the first time that an author did not get a single copy of a new edition of his book.

Posse died on 24 August 1928 in Leningrad, in the House of [aged] Scientists, in Leningrad, a month short of age 81. His wife, Emilia Ivanovna, outlived him for three months.

2. Pedagogic Activity

[2.1] Extant are lithographed courses of Posse's lectures in spherical trigonometry, analytic geometry and differential and integral calculus which he read in 1878 – 1910. Each new course was revised so that his pedagogic activity represented an incessant process of improvements.

It might be stated that, by his lectures and, to a greater extent, his textbooks on the differential and integral calculus, Posse was the first to acquaint with the elements of mathematical analysis most mathematicians and engineers educated in Russia from the 1870s to 1930s. Later some of his methodical approaches and problems have been naturally transferred to other renowned manuals, e. g., written by Fichtenholz¹⁵.

Posse published the first Russian textbook on mathematical analysis. Classical lectures (Cauchy 1831) were naturally somewhat dated. Autenheimer (1895) suited naturalists but not mathematicians or engineers. Bussinesk (1899), as he stated in the Introduction, was intended for those *who wished to study by themselves*. For student-mathematicians studying in universities there was a course in differential and integral calculus written by Serret (1833 – 1834). Posse himself recommended his students Serret (1900) and Hermite (1879).

I think that that Serret (1833 – 1834) and Posse's textbook finely supplemented each other. Serret considered the elements of differential geometry and differential equations in more detail, but Posse more completely described the differential calculus and the theory of integrals.

The first edition of Posse's *Course of integral calculus* appeared in 1891 and its second edition in 1895. In 1903 Posse widened the book which now included the differential calculus. It was reprinted in 1907, 1912, 1923 and 1929. Following the development of science, Posse extended and supplemented both the theoretical part and the numerous examples and exercises of each edition which appeared while he had been capable of work. Thus, on 16 July 1910 he (Leningrad branch, Archive RAN, 162 – 2 – 354, pp. 45, 45 rev.) wrote Steklov:

I found a few rather interesting examples in Goursat, and their solution seriously interested me. All had been going smoothly until an example of calculating surfaces. I was held up and cannot understand what Goursat wants.

After Posse's death his courses on differential and integral calculus had been published separately: in 1934 and 1935 (the differential calculus), in 1934, twice, and 1938 (the integral calculus) under the editorship of I. I. Privalov. They were intended for higher technical education and mathematical faculties of pedagogic institutes. Later Privalov still better adapted this textbook [these textbooks] to modern

requirements and the book[s] began to be published under two authors¹⁶.

Posse (1914, p. 124) was very modest and only considered his course as an educational aid:

Any course can be scientific if its content is expounded scientifically. [...] I never thought that my course was suited for the universities.

For university students he recommended classical works and aids written by French authors (Serret 1900; Hermite 1879). Meantime, however, in the 1913/1914 academic year D. F. Selivanov, who read differential calculus and integration of functions at the mathematical faculty of Petersburg University, recommended to students the textbook of Posse, and Steklov who read differential equations, began his list of recommended literature with Posse's textbook (Obozrenie 1913).

We still ought to mention three books which Posse translated: Tisserand & Andoyer (1908); Cesaro (1913 – 1914) and Genocchi (1922).

When working in technical institutions of higher education Posse attempted to adapt the mathematical arsenal to practice. Indicative is his work in the Electrotechnical Institute (Posse 1905). Applying the Fourier theorem on the expansion of a periodic function into a series, he solves the differential equation

$$\frac{\partial^2 V}{\partial x^2} - \lambda q \frac{\partial^2 V}{\partial t^2} - \left(r q + \frac{\lambda}{r_1} \right) \frac{\partial V}{\partial t} - \frac{r}{r_1} V = 0$$

which describes the phenomena of the steady-state current in a circuit with capacity and self-induction.

[2.2] Posse was keenly interested in the problem of teaching mathematics in Russian higher education institutions. Before beginning work at the Electrotechnical Institute he spent the summer abroad. Indicative was his letter of that time, of 8 June 1899, to the rector of that Institute (CSHAL 990 – 2 – 2744, pp. 3, 3 rev.):

When passing Berlin, I saw Professor E. Lampe who reads mathematics and conducts exercises in the Technische Hochschule. On his invitation I was present on 22 May 1899 at those exercises and became convinced in that his method quite coincides with what I intend to introduce in your Institute. I personally was persuaded in its practical advantage.

The results about which Professor Lampe informed me confirm its usefulness. In personal contact I will inform you about this in detail. The success certainly essentially depends on the ability and diligence of the teacher and on the industry of the students. These conditions

are fulfilled in the Berlin Technische Hochschule. Future will tell how it will work in your Institute.

Posse's book (1910) is devoted to more general aspects of the same topic. He considered the mathematical curricula and programmes of Russian universities, the system of examinations of each kind (from entrance to master examinations). There also Posse analyses in detail the methods of teaching (of lectures, seminar studies, educational aids, lithographic courses of lectures, competitive compositions). He also expressed his views on the role and the place of universities and pedagogic institutes in the education of teachers of secondary schools and institutes. Just as detailed he described the organization of teaching mathematics in higher technical and military schools.

Posse was troubled by the need to elevate the level of education, especially in technical institutes. Among his proposed urgent measures were (Posse, *Ibidem*, p. 92)

The decrease to the most possible extent of the overload in the curricula by a more profound separation of courses according to specialities. The revision of the curricula of the main courses to rid them of abstract sections and establish a tighter connection with technical applications.

He (p. 95) concluded:

To be industrial, a state needs a larger number of engineers who, in turn, will be able to bring up intellectual proletariat.

[2.3] In 1913 owing to the state of his health Posse was unable to go to the International pedagogic conference in the Hague. However, on 1 – 4 April 1914, new style, he participated as a delegate from Russia, in the work of the International Commission on Teaching Mathematics in Paris. The conference discussed the result of introducing, in the higher classes of the secondary school, of the elements of differential and integral calculus and the place of mathematics in higher education institutions.

At the previous conference in Heidelberg each national subcommission had received questionnaires and Posse answered them on behalf of Russia. Sixteen states were represented with 167 participants. Reports were delivered by such outstanding scientists as Hadamard, Appell, Darboux, D'Ocagne, Lebesgue. F. Klein initiated and organized the conference,

In his report, Posse (1914) stated his opinion about the inadmissibility of depreciating the role of mathematics in higher technical education and on the desirability of separating engineers into practical workers and theoreticians which was in solidarity with a statement voiced [by whom?] during the conference.

Posse was one of the few scientists and professors who were actively interested in the situation in secondary schools. From 27 December 1911 to 3 January 1912 the First All-Russian Conference of

Teachers of Mathematics took place. Some famous mathematicians and professors of the higher education institutions (S. N. Bernstein, S. E. Savich, A. V. Vasiliev) participated along with school teachers.

Posse was the vice-chairman of the organizing committee and he also delivered a report (Posse 1913, pp. 452 – 458). He proposed to establish additional specialized classes for future mathematicians and engineers. He expressed concern about the low level of knowledge obtained in secondary schools which the teaching in the universities had to take into account (Ibidem, pp. 454 – 456).

In 1915 Posse submitted a report on the resolution of the Second (!) All-Russian Conference of Teachers of Mathematics to the Ministry [of public education]. He was then chairman of the subcommission on mathematics which studied [a proposed] fundamental reform of the secondary school. The main result of his work there was, as it seems, testimonials about more than twenty mathematical textbooks for secondary and higher schools (Central State Historical Archive 733 – 196 – 743 – 746, 836, 931, 1144 – 1146). Some of these testimonials were published in 1914 – 1915 in the *Zhurnal Ministerstva Narodnogo Prosveshcheniya* (J. Ministry Public Education).

Each testimonial was a thorough analysis of the reviewed work. Taken together, they show his main requirements for educational literature the most important of which was its scientific character. Thus, he (1914, p. 121) wrote about Rashevsky (1914):

The author states that Napier had compiled the first logarithmic table with logarithms to the base of the irrational number 2.7182818284 ... First, the Napier table did not mention any base; second, after telling school students without any explanation that Napier had chosen a strange number for the base they will possibly decide that he was not in his right mind. If the author had no occasion to acquaint himself with the history of the invention of logarithms, it would have been better to say nothing about natural logarithms.

Posse (1915) took Markov's side in his protracted dispute with P. A. Nekrasov on the expediency of introducing the theory of probability into gymnasiums. Nekrasov intended to use that theory as a means for upbringing the students as loyal subjects¹⁷.

Notes

1. On Russian scientists of that period see Youshkevich (1968). Posse corresponded with Steklov, and I note that from 1919 to his death in 1926 Steklov was vice-president of the Russian Academy of Sciences (RAN). O. S.

2. The scientific work of Posse is beyond the field of my studies. O. S.

3. In various sources the date of Posse's birth is stated differently. I followed his certificate of birth (Central State Historical Archive of Leningrad, CSHAL, fond 14, inv. 3, No. 9924, p. 1). A. S.

Below, I abbreviate such references; for example, the above would have been written CSHAL 14 – 3 – 9924. The page numbers will certainly remain as they were. O. S.

4. This is not altogether clear. O. S.

5. Petr Berggardovich Struve (1870 – 1944), political figure, economist, philosopher, historian, publicist. O. S.

6. His book was mentioned above and is repeatedly cited below. The author had somehow forgotten to say, that K. A. Posse was also the son of Aleksandr and his wife. O. S.

7. According to the universities' bylaws of 1863 those graduates who passed the [final] examinations were called *actual students*. For obtaining a candidate diploma the graduates [the actual students] had to present a dissertation. Posse's dissertation (above) had not regrettably survived. A. S.

8. This tragedy should have been explained. Later, the author mentions Posse's family, so who were its members? O. S.

9. Aleksandr Nikolaevich Ostrovsky (1823 – 1886), a dramatist. O. S.

10. These courses were named after their first director, Bestuzhev. O. S.

11. Popov was the first who invented the radio, but, unlike Marconi, he did not take out a patent. The reason seems to be that the Russian fleet secretly began using the new invention. O. S.

12. Posse pleaded for V. A. Markov with the Council of the University and his work was published in 1892 at the expense of the University. Markov is known to have died early, in 1897. A. S.

13. That was the day of the Great October Socialist Revolution, as it was (and is?) officially called; actually, of a coup d'état. O. S.

14. By themselves these figures are meaningless. O. S.

15. Fichtenholz published a repeatedly reprinted course on differential and integral calculus in three volumes (translated into a few languages including German but not English) and a shorter version of same in two volumes and under a different name. O. S.

16. The Course in differential calculus was published in 1937, 1938, and 1939. And the course in integral calculus, in 1939. A. S.

The combined course was published in 1903, 1923 (also in Russian, in Berlin) and 1929. O. S.

17. See Sheynin (2003). O. S.

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IV

V. M. Tikhomirov

The birth of the Moscow mathematical school and France

Istoriko-Matematicheskie Issledovania, vol. 9 (44), 2005, pp. 238 – 252

[1] The phenomenon of the Moscow mathematical school, or more precisely, of the mathematical school of Moscow University, of Egorov and Luzin, is startling. This school appeared in 1914 – 1916. In the previous decades only one narrow direction in differential geometry (bending of surfaces) had been developed in Moscow. It began to be cultivated by Karl Mikhailovich Peterson (born in Riga, a teacher in one of the Moscow gymnasiums).

The most prominent Moscow mathematician of that time was certainly Egorov, and he was obviously interested in differential geometry. At the beginning of the 1910s *only one* seminar had been working in Moscow University, his seminar. In the 1920s that word was granted civic rights by the University. Dozens of seminars had been working there in the 1930s and in the 1950s they numbered more than a hundred.

But suddenly, in the mid-1920s, the interest of the Moscow scientific community shifted and it began following the path indicated by French mathematicians, E. Borel, R.-L. Baire and A. Lebesgue. During only seven years there appeared an entire galaxy of outstanding researchers: P. S. Aleksandrov, N. K. Bari, A. N. Kolmogorov, M. A. Lavrentiev, L. A. Liusternik, D. E. Menshov, P. S. Novikov, I. G. Petrovsky, M. Ya. Souslin, P. S. Urison, A. Ya. Khinchin, L. G. Snirelman. All of them except Petrovsky who was Egorov's disciple, were the students of Luzin. And each apart from the early died Souslin had chosen his own path. At the mid-1930s (after the German mathematical school was wrecked by the Hitler regime) the Moscow mathematical school along with the French school became the leaders of the mathematical world¹.

How can we explain this unprecedented phenomenon? There existed both global and local causes. We should admit that the two revolutions of 1917 opened the door to education for a wide section of the population and inspired great many people to science². But it is necessary to mention the other cause of the sudden appearance in the world science of a new outstanding mathematical school. This cause is connected with the creative activity of one person, Nikolai Nikolaevich Luzin, whose scientific and general biography was inseparably linked with France.

[2] But we ought to say a bit more about his teacher, Dmitry Fedorovich Egorov. He was born in Moscow in 1869, graduated from

Moscow University in 1891 and taught there from 1893, became professor in 1903 and, from 1923 to 1930 had been president of the Moscow mathematical society. In 1930 he was arrested, then exiled to Kazan and died there in 1931³.

Egorov was a staunch believer and kept to exceptionally high moral principles. There is no evidence of his ever acting against his conscience. Unbending, he firmly defended those principles. During that period lecture-rooms had been lacking and in some educational institutions lectures were held in church premises. Egorov (who had been working not only in Moscow University) refused to lecture there.

He wholly understood the tragic consequences of a totalitarian ideology and attempted to resist it as much as possible. At the end of the 1920s he had been cruelly criticised since he did not want to obey the new regime. During a public meeting he was reproached for being a reactionary and an oppressor of freedom. Egorov impassively objected: *You yourselves are the oppressors of the freedom of thought.* Once he was summoned to an assemblage especially arranged by revolutionary young men for re-educating old professors. All those, being re-educated, without exception vowed fidelity to the new authorities. And then it was his turn, he was asked about his political views. He answered:

I am not sure that this audience will understand me, but I consider it unworthy to conceal my thoughts. I am a partisan of constitutional monarchy.

Such behaviour was absolutely unprecedented and his tragic future became unavoidable.

Egorov represented the type of the traditional professor of old. He was very restrained, punctual in everything, serious and reserved. His lectures were always thoroughly thought out, and he strictly expounded them.

[3] Luzin was a man with a quite different turn of mind. He was born in Tomsk in 1883, had graduated from a private school and entered the gymnasium of the Tomsk province. There, mathematics was one of his least beloved subjects and his parents had to engage as tutor. Later, Luzin (Bari & Golubev 1959, pp. 468 – 469) wrote:

Luckily, he was a highly talented student of the just opened Tomsk Polytechnic Institute⁴. He created [...] a strongest impression by showing [...] mathematics not as a system of materials mechanically learned by heart, but as a system of reasoning directed by vital imagination.

Luzin decided to become a mathematician and entered Moscow University. Later he (Ibidem, p. 470) said:

Splendid lectures in pure mathematics had been charming me.

Mathematics presented itself to him as a science replete with tempting secrets.

In 1911 Egorov proved one of the most fundamental theorems of the theory of functions which developed the Lebesgue doctrine. It is generally known and I do not formulate it. Egorov had acquainted Luzin with the beginnings of the theory of functions of a real variable and the latter, issuing from that theorem, derived the fundamental C-property of measurable functions.

Upon graduation Luzin was left at the University *for preparing himself to the professorial status* and Egorov arranged his scientific trip to France and Germany. In 1905 – 1906 and then in 1912 – 1914 Luzin had been in Paris and in 1910 – 1912, in Göttingen. In Paris, he attended lectures of outstanding mathematicians, H. Poincaré, J. S. Hadamard, Ch. E. Picard, G. Darboux, and of many others. He had fruitfully met both scientifically and personally, with Borel and especially Lebesgue whom he reverentially admired all his life.

After returning to Moscow, Luzin abruptly changed the style of the Moscow mathematical life which led to the birth of an outstanding mathematical school. How did it all begin? In the 1960s I happened to be present at a meeting of the professors and instructors of the chair of the theory of functions and functional analysis with students. The chair, Menshov, a most eminent specialist in the theory of trigonometric series, was asked to describe this beginning. Here is how he began his story [no reference]:

When I was entering Moscow University, Luzin had been abroad. However, he arranged with Egorov that they will together organise a seminar for students. And in 1914 Egorov did organize it. It was devoted to numerical series. Next year Luzin returned to Moscow and began to head the seminar himself. In 1915 we were occupied with functional series, and in 1916, with orthogonal series. And then came the year 1917, a very memorable year in our lives. A most important event had happened then which influenced all our further lives: we began to study trigonometric series.

In Menshov's memory those series outweighed both Russian revolutions.

Luzin *invented* absolutely new methods of work with young men. First, he proposed to those, who had just crossed the University's threshold, problems of the highest possible level which were given up by eminent scholars renowned over the world. This is how Aleksandrov (1978, pp. 373 – 374) described his first meeting with his teacher.

I was a second-year student. My impression of it was startling, as it is possible to say straightforwardly, and I have memorized it for my whole life. After his lecture I asked him how to study mathematics further. And first of all I was surprised by his attention and, I cannot find another word, respect to his interlocutor, however strange it

rings. Indeed, I am describing the talk of an already celebrated although young scientist with an eighteen-year-old student.

After hearing me out, Luzin, by skilfully formulated questions very soon found out the nature of my mathematical inclination and at once in an easily understood way sketched the main directions which he was able to suggest to me for further study. Very carefully he induced me to choose one of those. He achieved all this very delicately without any pressure, and now I can say, correctly.

Luzin suggested the problem of the continuum for Borel sets which interested Lebesgue himself and which such eminent mathematicians as W. H. Young and F. Hausdorff vainly attempted to solve. Similarly Luzin acted on his other students and really inclined them to scientific exploits.

Second, Luzin introduced individual lessons for his students. Kolmogorov once managed to solve a problem which Luzin formulated and which was discussed in V. V. Stepanov's seminar. Uspensky (1985, p. 7) who interviewed Kolmogorov, quotes him:

When Luzin was told about it, he turned to me (on the University staircase) and asked me to come regularly to him, like other students, for studying to his place once weekly on a fixed weekday.

Finally, Luzin assisted the unification of young mathematicians, who were infatuated by science, into a single close collective which his students called it *Lusitania*⁵. However, I have got somewhat ahead of my story.

[4] The years 1915 – 1916 occupy a special place in the history of the Moscow mathematical school. In 1915 Luzin compiled his dissertation *Integral and trigonometrical series* and brilliantly defended it on 27 April 1916, becoming at once doctor rather than master. Four of his students of the first generation, Aleksandrov, Menshov, Mikhail Yakovlevich Souslin and Khinchin obtained remarkable results in the descriptive and metric theory of sets and functions. Khinchin provided a natural extension of the asymptotic derivation, Menshov constructed a non-trivial trigonometric series which converged to zero almost everywhere. This result became a sensation the world over. Aleksandrov solved the problem of the continuum for Borel sets. But the main events which tragically broke out after twenty years had occurred in the descriptive theory of sets; I mean the theory of A-sets.

Luzin suggested to Souslin to read and think out Lebesgue's work (1905). After a while Souslin discovered a gap in one of his reasoning. Lebesgue wrongly proved that the projection of a Borel set remains a Borel set. Luzin, however, had been sure that intuition was unable to let Lebesgue down and asked his student to provide a proper proof. Instead, Souslin constructed an example of a Borel set whose projection was not a Borel set. He applied the construction which was

suggested by Aleksandrov for proving his theorem on the power of those sets and called it *A-operation* and the new type of sets, *A-sets*. For a long time Luzin's students interpreted the discovery of *A-sets* as the summit of the entire world mathematics. At the end of 1916 Souslin submitted his work. Luzin thoroughly checked it and revealed new approaches to the proof of the Souslin's result. Souslin (1917) and Luzin (1917) briefly discussed them.

[5] Then, however, a difficult and hungry period set in and the University actually stopped functioning. Wishing to lighten his students' distressing burden of ordeals, Luzin moved together with some of them (including Souslin) to Ivanovo-Voznesensk where conditions of life were better. However, in 1919 Souslin, who did not get along with the heads of the [local] Polytechnic school, left the city and tried to settle in the Saratov University. Hindrances occurred once more, and he decided to return for some time to his native village in the Saratov province. There, he fell ill with typhoid and died. That was the first tragic loss in the formed anew Moscow Luzin school.

In 1920 life in Moscow had been gradually normalizing and Luzin returned there. The next three or four years can be called the time of the flourishing of *Lusitania*. Liusternik (1965, p. 22) described in verse the atmosphere which reigned in *Lusitania* in the beginning of the 1920s:

The deity was already surrounded by a constellation of demigods: Privalov, Menshov, the strung-up Aleksandrov, the nice Urison, the philosophically-minded Khinchin and several others.

The deity was of course Luzin. During those years the *Lusitania march* which imitated Mayakovsky was composed. Here is its beginning:

Our God is Lebesgue, our idol is the integral. In rain, tempest and snow we celebrate our carnival.

Liusternik (Ibidem, p. 27) recalls that the likely author was S. A. Bernstein, later a professor of applied mechanics. There exist lots of recollections and statements about that period, enthusiastic, eulogistic, merry, glorifying *Lusitania* and Luzin. Here is one of them. In 1942, during the Great Patriotic War⁶, while reflecting about the future, Kolmogorov (2003 [reference incomplete]) wrote:

In its historical aspect mathematics consists not only of theorems⁷ but also of the joint beating of hearts which had been occurring in Lusitania.

The joint beating of hearts ... He wrote it after 1936, when many students of Luzin had hurled monstrous accusations to his face. But it is not yet the time to speak about this tragedy. Let us first discuss France and the role of the French mathematical school (and other mathematical schools) in the formation of the Moscow school during the 1920s.

[6] I have mentioned some of Luzin's students of the first generation. Almost all of them had a chance (until the Iron Curtain dropped) to go to Europe and the USA. We should not belittle the significance of those trips for the scientific biographies of all those outstanding scientists. Kolmogorov lived for a long time in Göttingen and Paris. Bari, Lavrentiev, and Menshov had been working in Paris. Menshov left touching recollections about his report at the seminar of Hadamard. Khinchin and Snirelman lived in Göttingen, Liusternik participated in the mathematical congress in Bologna.

Aleksandrov lived abroad, in Germany, the USA, in France, longer than anyone else. In France, in Bretagne, he saw with his own eyes how his friend, Pavel Samuilovich Urison, a most promising scientist, lost his life: he went swimming in a stormy weather. For the Lusitania that was the second, after Souslin, tragic loss.

Trips to Paris, contacts with the most eminent scientists of those times, had left an incredible sign in the life and work of all those who had been happy to visit France during those years. From about 1932 trips abroad became impossible and scientific contacts were interrupted. Only Aleksandrov's fantastic energy allowed him to organize in Moscow, in September 1935, the first international topological conference unusual in its representation.

[7] Then began the year 1936, began the period of the escalated Stalinist terror [the Big Terror]. After many years I had an occasion to here personally from Pavel Sergeevich Aleksandrov only enthusiastic opinions about Luzin and Lusitania. I was astonished by a fragment of his recollections which he, 83 years old, had been preparing for publication (1979, p. 34). He gave me his manuscript to read and discuss [with him]. Here are a few lines from it:

When I got to know Luzin in his earliest creative years, I became accustomed with a really enthusiastic scientist and teacher. He only lived by and for science; I came to know a man who had been living in the sphere of supreme human spiritual values into which no pernicious spirit can enter. After leaving that sphere (which he did later) a man inevitably falls under the reign of those powers about whom Goethe⁸ wrote (Ibidem, p. 242):

*You bring us into life, // You make the poor fellow guilty
Then you put him to torture
Because here on Earth every guilt is revenged.*

In the last years of his life Luzin had drained to the dregs the *cup of revenge* mentioned by Goethe. What is the meaning of that? I only understood it in our days, when there appeared some publications devoted to those tragic pages of the history of Soviet mathematics, the pages called *The Luzin case* (Youshkevich & Dugac 1988; Demidov & Levshin 1999; Dugac 2000). That monstrous campaign against Luzin began with an article in *Pravda* (Demidov & Levshin 1999, pp.

254 – 255) for 2 July entitled *The answer to academician Luzin* and written by G. I. Shuliapin, a school director. Its meaning is reduced to rebukes for uncritically estimating the situation in the Soviet school.

There also, on the next day, appeared a cruel and shameful article *On the enemies in Soviet disguise* (Ibidem, pp. 255 – 257) which heralded Luzin's massed persecution. Thus, on 10 July under the same title (Ibidem, pp. 276 – 277) there appeared a fragment from the *Resolution unanimously adopted at a meeting of professors and instructors of the physical & mathematical faculty, and scientific workers and postgraduates of research institutions of mathematics, mechanics and astronomy of Moscow University*. It questioned *Luzin's further effective membership in the Academy*.

The conclusion of this *national condemnation* was hardly doubtful: Luzin had to repeat the fate of Egorov. Looking at the list of the staff workers of that faculty for the mid-1930s we are easily convinced in that only a handful of their professors, instructors, scientific workers and postgraduates were heirs of other mathematical schools (of the Moscow Peterson – Egorov school of differential geometry, the Kiev D. A. Grave algebraic school, the Odessa V. F. Kagan school of geometry), almost all were either direct students or scientific grandsons of Luzin. And still, they all *unanimously* voted for expelling Luzin from the Academy. So what terrible deeds had that *enemy in Soviet disguise* committed?

One of the main accusations consisted in that he loved France and kowtowed to Lebesgue. And also, that he was connected with reactionary professors and, first of all, with Egorov. I allow myself only one quote:

At a sitting of an academic commission (Ibidem, p. 132) which considered *the Luzin case*, he was accused of

Continuing in essence in all his activities the work of the French mathematical school and orienting himself, first of all, on the opinion of foreign, and in particular Paris scientists. In science, this is absolutely unusual and borders on servility.

Here is Luzin's answer:

As far as Borel is concerned – no. But I ought to say that my former connections with Lebesgue had been very warm. It is necessary to say that he is a quite special man of the people. He is extremely tactful and I have switched to him the tenderness which I had been feeling but was unable to display with respect to Egorov.

Surprising words! At that moment, standing under the knife of a guillotine, Luzin did not betray either his French friend, Lebesgue (who had been then accused of belonging to the bourgeoisie and *servicing the imperialistic, aggressive French policy*), or his teacher, a representative of *reactionary professors, a state criminal* who ended his life so terribly in prison.

[8] During the Soviet-style trial of Luzin the behaviour of the representatives of the elder generation (A. N. Krylov, S. N. Bernstein) was very deserving, but the same, regrettably cannot be said about younger scientists including Luzin's direct students. One of his main abusers was [that same] Aleksandrov. Luzin was accused for all to see of that, which only his students could have known. In particular, in that he wrongly estimated their contribution to science, Souslin's contribution in the first place.

Bernstein, who was the first to take floor [at the sitting of the academic commission] threw out all the accusations against his colleague. About Souslin he said that it was unworthy to delve into the relations between teacher and student⁹ just as to discuss publicly the personal life of people. These remarkable words had not stopped Luzin's students and the so-called trial continued.

Dugac (2000, p. 120), the French historian of science, uttered remarkable words:

*Today, we are surprised by what totalitarianism was able to do to sensible and honest people. We may hope never to encounter this insanity once more*¹⁰.

The unanimity with which not only the professors and instructors, scientific workers and postgraduates of the research institutes of Moscow University, but *the entire Soviet nation* stigmatized academician Luzin is one of the infinitely many testimonies of what totalitarianism can do to human souls. But still not only totalitarianism was guilty of that shameful event which Luzin's students performed on him.

Apart from Bernstein and Krylov other academicians, Vernadsky, Kapitsa, Chaplygin and some other representatives of Russian intellectuals had also defended Luzin, but not his students! So what had separated the teacher and his students, certainly *sensible and honest people*?

Consider now a man on the threshold of death¹¹, an 83-years old Luzin's student crowned with glory, who succeeded in procuring all which he could have only expected. What was the reason for him to publish shamelessly impossible, as it seems, words about Luzin who, in the last years of his life, *drained to the dregs the cup of revenge*, as Goethe formulated it? Revenge, but *for what* [crime]?

I have no answer. Perhaps some time a genius like Dostoevsky will be able to reveal the secret *black holes* in human souls even in those who have the calling to be bearers of splendid spiritual culture¹². I have explained my own point of view on this subject in my paper (1993).

I, just like many hundreds of mathematicians, am a scientific grandson of Luzin. Aleksandrov had been one of my teachers and the closest friend of my teacher Kolmogorov¹³. This obliges me to be

always respectful to his name, but, alas, a feeling of bitterness caused by his participation in an unjust case is now admixed to my recollection of Pavel Sergeevich Aleksandrov to whom we are so much obliged and to whom we are so thankful.

[9] The mentioned publication of Dugac shows how readily the French colleagues, certainly including Lebesgue, answered the appeal [see below] to support Luzin and overcome the horrible and imminent threat. Here is a fragment from Danjoy's letter to Sierpinski of 5 August 1936 (Dugac 2000, p. 125).

Dear colleague and friend, yesterday I received your letter about the villainous act committed against Luzin. I warned Lebesgue, Montel and Borel if only you haven't done it yourself. I will send them a copy of your translation of the article from Pravda and offer them a draft of an official statement to the Soviet embassy [in Paris] and see how they will interpret my proposal. If they agree (they will possibly be afraid that an interference from beyond will lead to worse repressions for Luzin), so how do they imagine to carry it out?

Borel and Langevin presented the somewhat revised letter to the Soviet embassy.

Today, it is impossible not to be delighted by those few who stood up for the outraged honour of their colleague and I name them once more. They were our great scientists who ran a great risk as well as Polish and French scientists:

Sergei Natanovich Bernstein, Vladimir Ivanovich Vernadsky, Petr Leonidovich Kapitsa, Aleksei Nikolaevich Krylov, Sergei Alekseevich Chaplygin, Emil Borel, Arnaud Danjoy, Paul Langevin, Henri Lebesgue, Waclaw Sierpinski.

The subject *Moscow mathematics and France* can be much more developed. It is possible to discuss the post-war connections, to mention Kolmogorov who became a foreign member of the Paris Academy of Sciences, visited France many times, loved very much that country and its culture; mention Bernstein, Sergei Lvovich Sobolev, Mikhail Alekseevich Lavrentiev, all of whom had been also elected members of that Academy, and very many others, but I have used up my time.

[10] Addition

I have presented my report at the Russian – French symposium in Moscow University in February 2002. Now, I would like to add some material.

I have many times heard from Sergei Petrovich Novikov (the last time during a conference devoted to Leonid Vitalievich Kantorovich in Petersburg in January 2004) that his father, Petr Sergeevich, used to say that much written about Luzin in that notorious article in *Pravda* and in other publications of the 1930s was true. In particular, he appropriated¹⁴ the discoveries made by his students. Now, I am

explaining my own viewpoint based on a long talk with Aleksandrov in the summer of 1979, on some of Luzin's publications and his letter (Demidov & Tokareva 1999).

As I see it, Luzin had propagandised an ideology among his students which they later applied against himself. He inspired them a craving for creative work and established for those young and still immature people an atmosphere of rivalry as though considering *the overcoming of obstacles* more important than servicing science. And the students began to overcome one obstacle after another¹⁵.

For a long time Luzin had been highly estimating the achievements of his students and communicating their works to prestigious foreign journals. Then, however, as I imagine, he began to feel something like jealousy, perhaps partly because the students themselves got used to indicate insufficiently his role in their own accomplishments.

It seems that Souslin's discovery somewhat shocked Luzin. Trusting Lebesgue's intuition, Luzin was unable to imagine that he was mistaken in principle. At first Souslin's proof of the existence of a new type of sets was confusing and for a long time Luzin had been securing clarity. A detailed proof revised by Luzin apparently became cumbersome (Souslin's memoir which contained that proof is lost, and we have to judge by indirect information). Luzin, as we may suppose, having applied serious efforts, proved Souslin's result differently, and published his proof alongside Souslin's paper (Souslin 1917; Luzin 1917).

[11] And then a phenomenon very typical for the scientific milieu began to occur. A reasonable [German] word *Nostrifikation* (which I first heard from V. I. Arnold) from the Latin *Nostra*. It meant *pulling the rug from under you*, an auto-suggestion, when your own role in some discovery is being increased [appropriation]¹⁶. A scientist writes about a discovery of his colleague (about which he himself thought and afterwards commented on): at first *He discovered but I described it differently*; then *We discovered*; and finally the author of the discovery is somehow forgotten.

Something similar occurred to Luzin, but his students were much guilty. They, as I imagine, began to hint to him too clearly that he himself had not overcome anything special. Luzin himself said that Souslin had stated something offensive of that kind to his face. And Aleksandrov clearly told me in 1979: *So what so special, properly speaking, did Luzin do?* The same idea had been repeatedly stated during the sittings of the academic commission (see above). And Luzin became guilty of *Nostrifikation*. To say the truth, this is painful to describe, but we still ought to discuss some facts.

In a paper published right after Souslin's paper [in 1917] Luzin wrote (1958a, p. 270): *I intend to indicate some inferences from Souslin's results*. In 1918 (Luzin & Serpinski, *Ibidem*, p. 273) he

stated: *Souslin introduced an important class of sets which he called A-sets. But already in 1925 he (Ibidem, pp. 301 – 302) wrote:*

The theory of analytic sets [thus he renamed the A-sets. V. T.] originated from Lebesgue's memoir [...]. In 1916 Souslin and I, while intending to carry out his programme, adopted the terminology which Lebesgue himself had used [...]. Souslin applied the index method which goes back to Lebesgue whereas I only used purely geometric considerations.

Then Luzin's monograph (1930) appeared; a Russian translation in 1953 – 1958 followed. Souslin's A-sets are not mentioned at all, there only exist *analytic sets* and Lebesgue is declared their original discoverer.

This statement had cheered up Lebesgue and he (1985, p. 10) expressed his feelings with an unusual elegance in the Introduction to Luzin's book:

Each reader will be probably surprised when, upon reading Luzin, he finds out that I, incidentally, discovered the method of sieve¹⁷ and was the first to construct an analytic set. However, no one will be more surprised than I myself was. Luzin becomes absolutely happy only when he is able to attribute his own discovery to someone else. This is a strange fancy. It seems forgivable since there is no danger that he will create a school in that region.

In 1927, at the First All-Russian Mathematical Congress, Luzin (1933) read out a report. It had a section *Descriptive theory of functions* where the names of Poincaré, Hilbert, Hadamard, Zermelo, Borel, Baire, Lebesgue, Brouwer, H. Weyl, Banach, Tarski, Serpinski, Fichtenholz and his student Zaretsky, and two other names which I do not recognize, Barzum (?) and Herrera (?) are mentioned, but not the names of Aleksandrov, Souslin, Kolmogorov who managed to overturn the descriptive theory of sets and functions. Neither did Luzin mention himself. What surprising deformations can occur in the human soul!

At the same time Aleksandrov also paid tribute to Nostrifikation and thus corrupted history, but for his own benefit. All this is certainly very bitter.

[12] Time, however, had arranged everything in its proper place. Luzin's dissertation had appeared (1951). Then, in translation, followed his monograph (1953) and there P. S. Novikov and Liudmila Vsevolodovna Keldysh (Ibidem, p. 6) properly rewarded their teacher:

His exceptional ability to select a fruitful direction, to formulate correctly a problem and to find the necessary definition unified around him a large group of talented young mathematicians who had been working on the problems which he formulated. Not only did

Luzin work intensively, he directed a large collective of young scientists to the solution of the most urgent and difficult problems of the descriptive theory of sets. A large series of these problems had been solved in a comparatively short time.

Then they expounded the real history (Ibidem, p. 7) with a short remark:

Luzin calls the A-sets analytic sets, but this name had not become established, and they are known as A-sets or Souslin sets.

Here we can see an apology for their colleagues who had persecuted their teacher; neither of the two authors had participated in that campaign. However, as far as I know, none of the perpetrators had publicly repented. I think that that is strange. And I do not want to conceal that in that history I very much feel sorry about Luzin himself. I think that the bitter *cup of revenge* was absolutely disproportionate to his *guilt*.

Notes

1. In the USA, a mathematical school was possibly established later.
2. Such scientists as those mentioned above did not need any inspiration from beyond.
3. Cf. [v].
4. That student regrettably remains anonymous.
5. *Lusitania* was the name of a British ocean liner sunk by a German U-boat during WWI with a loss of about 1200 lives. Those students hardly knew that.
6. The name invented by Stalin for the German – Soviet war of 1941 – 1945.
7. Apart from theorems mathematics needs axioms and definitions.
8. The author provided both the original text of those lines and its Russian translation from which I rendered it into English.
9. See however Note 14.
10. That hope proved futile. Suffice it to note that millions of Russians are still venerating the name of their butcher.
11. Aleksandrov was born in 1896 and died in 1982.
12. The author forgot totalitarianism.
13. It is more or less known that those *closest friends* were lovers.
14. I applied a usual word instead of the barely understandable expression in *Pravda*. B. E. Gelfgat, an astronomer and a mountaineer, who perished somewhere in the mountains, told me a story about Luzin. It had been certainly transmitted through a chain, but I believe it. Here it is.
Luzin's student had told him about his finding and rather soon saw it published under Luzin's name. *That's a lesson for you. Never tell anyone about your unpublished discoveries.*
15. I would say that prompt study is indeed essential.
16. *Nostrifikation* was derived from two Latin words: *noster* (our) and *facere* (to do something).
17. I only know about the sieve of Eratosthenes.

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The language of the items is obvious with an exception: the paper of Luzin & Serpinski was possibly published originally in French.

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Six unknown autographs of D. F. Egorov

Istoriko-Matematicheskie Issledovania, vol. 35, 1994, p. 324

The name of the Soviet (?) mathematician Dmitry Fyodorovich Egorov (1869 – 1931), honorary member of the Academy of Sciences of the USSR, co-founder of the Moscow mathematical school, is entered in the golden fund of national mathematicians. He had many students among whom were N. N. Luzin, I. I. Privalov, S. P. Finikov, V. V. Golubev, V. V. Stepanov, A. M. Razmadze, I. G. Petrovsky and L. N. Sretensky. As the head of the seminar which he established in 1910 and the president of the Moscow Mathematical Society in 1922 – 1931, Egorov became able to unite Moscow mathematicians under himself and assisted them in undertaking scientific studies.

The Central Municipal Archive of Moscow is keeping his reports about the scientific work and biographies of K. A. Andreev, A. K. Vlasov and B. K. Mlodzeevsky which he had written in 1923, apparently in connection with the need to fix pensions to the relatives of those late scientists. These reports are very interesting not only as a testimony of Egorov's recollections about his late colleagues but also as a valuable material about them. Thus, we note that the date of birth of Vlasov was not known. I have checked the mentioned facts against literary and archival sources and, when needed, commented on them.

Translator's commentary

In 1930 Egorov was arrested as a *religious sectarian*, then transferred to a prison in Kazan where he died. The main cause of his persecution was different (Tikhomirov 2005, p. 339): he publicly accused the Bolsheviks: *You are the oppressors of the freedom of thought* and, again publicly, declared that he was a partisan of *constitutional monarchy*. So much for his being a *Soviet mathematician!*

I did not translate the materials about Vlasov and Mlodzeevsky since they had not essentially contributed to the theory of probability or statistics. However, I note that **Vlasov** published a treatise on probability (Moscow, 1909) and edited the Russian translation (Moscow, 1908) of Laplace's *Essai philosophique*. See about him N. A. Glagolev in *Matematicheskij Zbornik*, vol. 32, No. 2, 1925, pp. 273 – 275.

Mlodzeevsky was Chuprov's teacher (Sheynin 1990/2011, p. 161, Note 2.1). In 1923, upon hearing about his death, Chuprov wrote to his disciple, Chetverikov:

I am [...] in a sense a disciple of Bol. Korn., [...] much obliged to him [...]. He taught math. in our eighth [in the graduation] class.

And so, Mlodzeevsky died in 1923, a fact not mentioned by Egorov. Concerning **Andreev** I am additionally saying, first, that he corresponded with Nekrasov see Chirikov & Sheynin (1994), an eminent mathematician whose work in probability was almost useless because of a serious methodical mistake and his attempt to combine it with religion and loyalty to autocracy (Sheynin 2003). Second, under his influence Lyapunov essentially extended his manuscript which was devoted to criticising Nekrasov (Sheynin 1989). On Andreev see also Gordevsky (1955) whom I cited. Incidentally, Andreev critically expressed himself about the university reform.

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Curriculum Vitae of Professor Konstantin Alekseevich Andreev

Istoriko-Matematicheskie Issledovania, vol. 35, 1994, pp. 325 – 327

Andreev was born on 14 March 1848 in Moscow into a merchant family. His father and grandfather had been selling furs. However, during K. A.'s early childhood his father's business fell into decay and the family had to live through poverty. Also in childhood K. A. had wounded an eye and was never able to use it which postponed his schooling. He entered the Third Moscow Gymnasium in 1860, learned successfully and from age fourteen supported himself by tutoring.

In 1867 K. A. became a student of the mathematical department of the physical & mathematical faculty of Moscow University and attended lectures of professors A. Yu. Davidov, N. V. Bugaev and V. Ya. Tsinger¹. When being a fourth-year student he compiled a composition on the subject proposed by the faculty², *On Tables of Mortality* which was awarded a gold medal, and, when K. A. graduated, it was published in the *University Zapiski*.

K. A. was left at the university for two years with a stipend to prepare himself for professorship after which he passed an examination for the degree of master in pure mathematics. At about the same time a professorial position in Kharkov became vacant and the young master³ recommended by Bugaev became able to put himself as a candidate for privat-docent there⁴. In December 1873, in Kharkov, he defended his just published contribution on tables of mortality and became privat-docent. From January 1874 K. A. began to read lectures on analytical geometry. In 1875 he defended a master dissertation *On geometrical generation of flat curves* and, after its approval by the Council of the University, was elected staff docent.

At the end of 1876 K. A. was sent abroad for 1½ years and worked in Berlin, Heidelberg and mostly Paris. There he attended lectures read by Bonne, Bertrand, Hermite and Jordan⁵ and prepared his doctor dissertation. In the autumn of 1878 K. A. returned to Russia and published that dissertation *On geometric conformities as applied to the construction of curves* and defended it in 1879 in Moscow. Soon Kharkov University elected him extraordinary professor.

That same year the Kharkov Mathematical Society was established and K. A. had been most actively participating in its life. He reported at its sittings, edited its transactions, became its secretary, assistant chairman and chairman. From 1872 K. A. was also member of the Moscow Mathematical Society. There, still a young man left at the University, he made his first report.

The Russian Academy of Sciences⁶ elected him corresponding member. During that year K. A. was abroad, in La Rochelle (France), at the Congress of French Mathematicians and Naturalists. He read a report there and it was published in the proceedings of that Congress. Later it appeared in Russian in the *Soobshchenia* of the Kharkov Mathematical Society, entitled *On Poncelet's polygons*. Then he was also elected member of the Society of Science in Bordeaux.

In 1886 K. A. was elected ordinary professor. From 1885, the year when the Kharkov Technological Institute was established, K. A. had been its professor until his move to Moscow. That happened in October 1898 when he became professor of Moscow University. There he remained almost all the rest of 23 years of his life. At the same time K. A. became director of the Aleksandrovsky Commercial School and worked there until 1907 and achieved much for the Russian secondary school. He also closely participated in the life of the then opened Moscow Pedagogic Society, for some time was even its chairman and also taught in the Moscow Higher Technical School.

In Moscow University, K. A. had to work during the period of its highly intensive life: some fundamental changes were made and K. A. became the first elected dean of the physical & mathematical faculty and had been fulfilling his duties from 1905 to 1911. Under his guidance and with his own closed participation a new system of teaching was introduced.

In 1911 K. A. left his position. At the same time a serious illness (a tumour in his throat) compelled him for some time to quit reading of lectures. He had to go abroad and undergo an operation in Freiburg (Baden-Württemberg, Germany) which essentially relieved him.

After that, beginning in 1913, K. A. read lectures until 1917, when an illness compelled him to leave Moscow and move to Crimea. There he spent the last years of his life, overshadowed by physical and moral suffering and material privation. He was evicted from his dacha, deprived of his library which was taken away and separated from a part of his family. He died in Aleksandriada near Sevastopol on 29 (16) October 1921.

Notes

1. Avgust Yulievich **Davidov** (1823 – 1886), mathematician. Graduated from Moscow University in 1845, worked there. Professor from 1853. Nikolai Vasilievich **Bugaev** (1857 – 1903), mathematician. Graduated from Moscow University in 1859, worked there. Professor from 1866. Corresponding member of the Petersburg Academy of Sciences from 1879. Vasily Yakovlevich **Tsinger** (1836 – 1907), mathematician, professor of Moscow University from 1862. Andreev (1909) published a booklet devoted to the memory of his teacher. V. V.

Bugaev worked mostly in number theory, analysis and arithmology, now understood as a doctrine of discrete functions and even as a Weltanschauung. During his last years Bugaev was regarded as a talented eccentric (Youshkevich 1968, p.

485). See also Bugaev (1897). His students included Nekrasov and Egorov (see [v]). See also Nekrasov (1905) and Buckingham (1999). O. S.

2. Apparently suggested by Davidov. In 1857, he published his report mainly concerned with tables of mortality. O. S.

3. Andreev only became master after defending a dissertation, see below. O. S.

4. A privat-docent received fees from his students rather than from the appropriate institution. O. S.

5. Pierre Ossian **Bonne** (1819 – 1892). Graduated from the Ecole Polytechnique in 1838, worked in the Paris Faculty of Sciences, professor from 1878. Charles **Hermite** (1822 – 1901), member of the Paris Academy of Sciences. In 1869 – 1897 professor of the Paris Faculty of Sciences. Marie Ennemond Camille **Jordan** (1838 – 1922), member of the Paris Academy of Sciences and its president in 1916. From 1873 worked in the Ecole Polytechnique, its professor from 1876. From 1875 worked also in the Collège de France. V. V.

6. Official name: Imperial Academy of Sciences (in Petersburg). O. S.

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D. F. Egorov

Report about the scientific and pedagogic work of Professor K. A. Andreev

Ibidem, pp. 328 – 329

Professor of Moscow University Konstantin Alekseevich Andreev who died in 1921 is one of the most eminent Russian geometers. He published many outstanding contributions in projective geometry which was created by Poncelet, Chasles, Steiner and Staudt¹. In Russia, this discipline was implanted by Andreev's teacher, V. Yu. Tsinger. It was then first developed by Andreev's independent and brilliant work and especially in his doctor dissertation *On geometric conformities as applied to the construction of curves*. Published in 1879, it can still really delight any geometer.

K. A. also published many works in other branches of mathematics. They do not perhaps possess the same fundamental significance as

those in geometry, but in any case all of them are distinguished by original thinking, witty proofs and brilliant exposition. As an example I may mention at least a short note on the proof of the formula of integral calculus which extended the formulas of Chebyshev and Imshenetsky. All the results there are obtained, as it is possible to say, by a single stroke of the pen, by a fortunate and witty application of a simple relation from the theory of determinants. The Russian Academy of Sciences estimated his scientific work at its true worth and elected him corresponding member.

As a professor, K. A. worked in Kharkov, then in Moscow. He was a teacher of many generations of mathematicians and the universities in both these cities are much obliged to him. Only a serious illness compelled him to quit his teaching activity and therefore his work for the good of Russian education. Indeed, he had also been teaching in higher technical schools and in addition worked for the secondary school. He published a widely disseminated textbook on analytic geometry² and actively participated in the life of many scientific and educational societies.

11.7.1923

Notes

1. Jean-Victor **Poncelet** (1788 – 1867), mathematician and mechanic (applied mathematician). Founder of projective geometry, member of the Paris Academy of Sciences from 1834 and its president in 1842. Michel **Chasles** (1793 – 1880), mathematician and historian of mathematics, member of the Paris Academy of Sciences from 1851. Founded kinematic geometry. Jakob **Steiner** (1796 – 1863), member of the Berlin Academy of Sciences from 1834. Main works in projective geometry. Karl Georg Christian **von Staudt** (1798 – 1867). Main directions of work: projective geometry and synthetic geometry of which he was co-creator. V. V.

2. *Osnovnoi Kurs Analiticheskoi Geometrii* (Main Course in Analytic Geometry), parts 1 – 2. Kharkov, 1887 – 1888. Fourth edition, 1905.

Also, educational aids: *Vysshaia Algebra* (Higher Algebra), 1899. Fifth edition, 1909. *Zbornik Uprazhneniy po Analiticheskoi Geometrii* (Coll. Exercises in Analytic Geometry), 1892. Third edition, 1915. V. V.

VI

N. I. Akhiezer

A. A. Markov, a Russian mathematician

Priroda, No. 8, 1947, pp. 76 – 81

Twenty five years, on 20 July 1947, have passed after the death of the renowned Russian mathematician¹, academician Andrei Andreevich Markov. Just like E. I. Zolotarev (1847 – 1878) and A. M. Lyapunov (1857 – 1918) Markov was Chebyshev's student. His merits are great and diverse. He left about 70 contributions, and many of them contained discoveries of paramount importance. He compiled excellent treatises (1889 – 1891 and later editions; 1900 and later editions)² which had been used by several generations of students and still are reference books. For many years a professor of Petersburg University, Markov educated many mathematicians. And being the head of the Petersburg mathematical school for more than 30 years, he was the teacher of many outstanding scientists, including, first of all, G. F. Voronoy (1868 – 1908).

Markov was born on 14 June 1856, attended the Fifth Petersburg gymnasium and graduated in 1874. Already then his brilliant mathematical talent was revealed. Nevertheless, he had not belonged to the best students: for him, each discipline except mathematics was difficult and Latin especially oppressed him. For a while, he even thought of leaving the gymnasium and entering a technical educational institution.

After graduation Markov entered Petersburg University. Unlike other Russian universities, the staff of its physical-mathematical faculty was not inferior to that of the best universities in Western Europe. Suffice it to name professors Chebyshev, A. N. Korkin and E. I. Zolotarev³. The last two mentioned especially influenced the students. Apart from usual lectures, they devoted special lessons for outstanding students, mostly at home. A. A. started participating at once and soon distinguished himself by solving difficult problems which had been formulated there.

Markov graduated in 1878 with a gold medal for his composition *On the integration of differential equations by means of continuous fractions*⁴. After two years he became privat-docent of the University and in 1886 was elected extraordinary professor. That same year, on Chebyshev's recommendation, Markov was elected adjunct of the Academy of Sciences. Then, in 1893, he became ordinary professor and after another three years, full academician at the chair of mathematics.

In 1905, after 25 years of teaching, Markov became distinguished professor and retired. He was only 49 years old but, as he himself explained, he did not want to stand in the way of younger men. However, he continued to teach: as an academician, he was thus entitled, and he had been teaching almost until the end of his life. Markov taught probability theory and the theory of continued fractions. He desired to publish that latter course [the former was published in 1900 and later], and, even bedridden, during his last weeks, he corrected its manuscript. Regrettably, however, it was lost in the documents of some publisher.

As a human being, A. A. was distinguished as an exceptional man of principle incapable of compromises and he organically did not tolerate diplomatic behaviour of anyone which had especially often occurred in the academic milieu. In his address read at the general meeting of the Academy of Sciences on 3 November 1922 Steklov (1924) characterized that feature of Markov:

He could stand any sharp statement about himself if only it really touched on the essence of the business at hand, not deflected him, not distracted him from the main subject towards personal feelings or a compromise settlement which usually did not satisfy anyone.

Markov always began his objections and statements with a sharp definiteness to which he was accustomed in his scientific studies. This often annoyed touchy people who were not used to such objective and logical forms of talks. His opponent often put the essence of the debate aside and began to object diplomatically to its form as shaped by Markov, and that invariably unbalanced him. Such debates led to conflicts and mutual misunderstanding. Markov's proposals, essentially proper, had been often rejected only because of their discomforting form.

Everyone knows his encounters with academician V. G. Imshenetsky, then with his defenders, Professors K. A. Andreev and P. A. Nekrasov, his special debate with the last-mentioned and with the Moscow Mathematical Society about Nekrasov's frames in the theory of probability, and with academicians B. B. Golizsin and F. A. Bredikhin et al.

These and similar cases gave occasion for the dissemination, especially beyond Petersburg, of sharp negative opinions about Markov's disposition and actions which I personally had to hear all the time in Moscow and Kharkov even before my acquaintance with him.

His strict principles had been revealed in many actions which were sometimes ascribed to eccentricity or to a desire to show off. Many of them were of a political nature and could have led to some reprisals, so that even Markov's friends sometimes supported the opinion about eccentricity. Known, for example, is his renunciation of orders and ranks; his sharp statement about Gorky's expulsion from honorary membership of the Academy on Royal command; his demand to be

excommunicated from the Russian Orthodox Church just like Tolstoy was⁵.

Markov's student, N. M. Günther (1923), the late professor of Leningrad University, described curious episodes which characterized A. A.'s sharpness. And many times I heard similar stories from another Markov's student, from my late teacher D. A. Grave:

For many years Markov guided a student mathematical study group and attentively followed the reporters. Upon noticing a mistake or lack of rigour, or even an insufficient stress on some circumstance which he thought was important, he immediately interrupted the reporter, sometimes for a long time and sometimes completely shutting him down.

Günther recalled: sometimes a neighbour, another professor or docent, had quietly restrained Markov, who, as though having absolutely forgotten everything except his own idea, exclaimed: *But what does he say? Or, How can I listen if I do not understand?*

In spite of this sharpness the members of his group had been willingly reading out reports and communications.

As a scientist, Markov was also distinguished by an exceptional loyalty to his principles. These were characteristic of the Petersburg mathematical school which he had headed, as I mentioned, for many years. They, these principles, told on everything: on the formulation of problems; on requirements on their solution; and on the axiomatics⁶. Only concrete problems were chosen whereas various abstract constructions were thought to be barely important. Neither were studies of axiomatics acknowledged. In his *Calculus of Probability* [1900 and later], when considering the notion of probability, Markov stated that notions were learned not by their formal definitions but by our attitude to them which is gradually ascertained⁷.

Markov attached importance not to philosophizing, but to the trade, not to the construction of abstract theories, but to the capacity of complete solutions of concrete problems. Just like his great teacher, A. A. had been undoubtedly inclined to calculation and knew how to calculate and a specimen of his calculation is the table (1898). At the same time, the main feature of the entire Markov's scientific work was its impeccable rigour.

[I am leaving out a passage about Markov's work in the number theory.]

A large group of Markov's contributions was devoted to the limit values of integrals. In 1854 Chebyshev published a note *Sur le valeurs limites des integrales* in which he indicated the importance of the following problem:

A real non-negative function $f(x)$ is only determined by the values of the integrals

$$\sigma_0 = \int_0^l f(x)dx, \sigma_1 = \int_0^l f(x)x dx, \dots, \sigma_n = \int_0^l f(x)x^n dx,$$

and it is required to determine the precise boundaries for the integral

$$\int_0^t f(x)dx$$

for any t taken in the interval $(0, l)$.

Chebyshev provided those boundaries without proof for t being a root of some algebraic equation, i. e., for some special upper limits. He also mechanically interpreted his problem for $n = 2$. [...]

In two notes⁸ Markov proved Chebyshev's inequalities for any n and for $n = 2$. His first note was devoted to the second mentioned case, and there Markov indicated:

It remains unknown how Chebyshev derived his result and, as far as I know, no one has yet indicated the approach to the solution of this problem. However, this problem is peculiar and therefore deserves attention, and I hope that my present reasoning which leads to its solution will not be uninteresting.

And, in the other note:

After a few fruitless attempts I became at last able to find a very simple proof of the mentioned inequalities.

He concluded by expressing his most vivid thanks to K. A. Posse who

Turned my attention to the problem here considered and showed its solution for some particular cases.

Markov also published his results abroad. It is remarkable that almost at the same time the celebrated Dutch mathematician Stieltjes had provided and proved these inequalities⁹. After that Markov and Stieltjes followed differing paths which intersected only seldom and occasionally. The former turned to the general problem with t taking an arbitrary position, then busied himself with various generalized problems, see below. Stieltjes however considered the cases of $t = \infty$, $n = \infty$, i. e., the distribution of the mass [perhaps according to Chebyshev's mechanical interpretation] when that distribution is sought on a half-line and the moments of all orders are known. [...]

Markov's studies were characterized by a specific algebraic imprint which to a large extent reminded the works of Chebyshev. The proof and generalization of the Chebyshev inequalities became one of the deepest chapters of Markov's doctor dissertation (1884). There also Markov was the first to derive the expression of the additional term of the Gaussian quadrature formula as well as the expression of formulas of such kind.

I will indicate now the generalizations of the problem considered above as studied by Markov. One of them consisted in an additional restriction $f(x) < L$ where L was a given positive number, and, as before, $f(x) > 0$ was the density sought. In another generalization Markov introduced generalized moments

$$\int_a^b f(x) \lambda_k(x) dx, \quad k = 0, 1, \dots, n$$

instead of the usual moments

$$\int_0^l f(x) x^k dx.$$

Apart from the work which had to do with the latter generalization, everywhere else the main Markov's instrument was the arsenal of continued fractions. Just as Chebyshev did, Markov considered not only the density $f(x)$, but the integral

$$\int_a^b \frac{f(x)}{z-x} dx,$$

its expansion into the series

$$\frac{\alpha_0}{z} + \frac{\alpha_1}{z^3} + \frac{\alpha_2}{z^5} + \dots$$

and the corresponding continued fraction

$$\frac{k_0}{z + l_0 + \frac{k_1}{z + l_1 + \frac{k_2}{z + l_3 + \dots}}}$$

The main point here is that the denominators $\varphi_0(x)$, $\varphi_1(x)$, $\varphi_2(x)$, ... of the convergents satisfy the condition of orthogonality

$$\int_a^b \varphi_k(x) \varphi_n(x) f(x) dx = 0, \quad k \neq n.$$

For different functions $f(x)$ we obtain different systems of polynomials $\varphi_k(x)$ which are called orthogonal polynomials with regard to weight $f(x)$. For example, if $f(x) = 1$, $a = -1$, $b = 1$, we get the

so-called Legendre polynomials; if $f(x) = e^{-x}$, $a = -\infty$, $b = \infty$, we have the Hermite – Chebyshev polynomials.

Markov proved some important theorems about the roots of these polynomials. I indicate the theorem about the roots of the Legendre polynomials which was somewhat later proved by Stieltjes in another way: the positive roots of these polynomials raised to the power of n are located separately one from another in each of the intervals

$$\left[\cos \frac{m\pi}{n+1}, \cos \frac{(2m-1)\pi}{2n}\right]; \left[\cos \frac{(m-1)\pi}{n+1}, \cos \frac{(2m-3)\pi}{2n}\right]; \dots;$$

$$\left[\cos \frac{\pi}{n+1}, \cos \frac{\pi}{2n}\right],$$

where m is the integral part of number $n/2$.

Then, I note a theorem about the Hermite – Chebyshev polynomials, which is very important for probability theory (Markov 1898): all the roots of such a polynomial raised to the power of n are located in the interval

$$\left[-\frac{n}{\sqrt{\ln n}}, \frac{n}{\sqrt{\ln n}}\right].$$

Finally, I indicate Markov's remarkable paper (1894) which was reprinted a few years ago in English. It contains very important theorems about the change either of the roots of orthogonal polynomials $\varphi_k(x)$ or, in a second theorem, of the moments $\sigma_0, \sigma_1, \sigma_2, \dots$. Somewhat earlier Chebyshev discovered particular cases of those theorems.

The next group of papers consists of the works of Markov about functions least deviating from zero. I describe only one such contribution (1890), but first here is the history of his problem. The celebrated chemist [and metrologist] Mendeleev studied the specific weights of aqueous solutions and encountered the following problem. Given, the polynomial

$$p_0z^2 + p_1z + p_2.$$

In interval (a, b) it does not intersect in absolute value a given number Z . It is required to find the upper boundary of the derivative $|f'(x)|$ in a given point x which belongs to (a, b) or everywhere in that interval.

Markov generalized this problem. He replaced the given polynomial by raising it to an arbitrary power n . Then it occurred¹⁰ that

$$\max |f^n(x)| \text{ on } (a, b) \text{ was } \frac{2n^2 L}{b-a}.$$

If, however, the upper boundary is required for $f^n|x|$, three cases ought to be considered depending on the location of x on (a, b) . In each case this problem is reduced to the solution of some algebraic equation. Under Markov's influence his brother Vladimir Andreevich¹¹ considered a more difficult problem: the determination of the

$$\max |f^{(k)}(z)|, a \leq z < b \text{ for a given } k (1 < k < n).$$

Markov's bent for calculation which I mentioned above was inseparably linked with his entire mathematical Weltanschauung. He considered the solution of a problem to be completely accomplished if such an algorithm was found which allowed the determination of the sought magnitude either precisely or to any degree of precision. And if a check of some circumstance is needed, the necessary number of operations should be indicated if possible¹².

By issuing from these propositions Markov admitted only such approximate formulas whose error can be estimated. He was the first to estimate the errors of mechanical quadratures mentioned above. In addition, he himself introduced a whole class of new quadrature formulas whose errors can be estimated.

Issuing this time from the claims of the calculators A. A. provided a remarkable transformation of series (called after him) which increased the rapidity of their convergence. It can be found in his course (1889 – 1891 and later editions).

Especially important are his works on probability theory, about 25 memoirs and notes, and a fundamental course (1900 and later editions). Markov's investigations were devoted to limit theorems, the law of large numbers and the method of least squares. In his last works he considered some problems belonging to mathematical statistics. He completely realized Chebyshev's idea about proving the limit theorems of probability theory by the method of moments.

Markov published his main results belonging here in 1900. Soon, however, there appeared two contributions by Lyapunov who achieved the same results by another method and excelled them. Indeed, the method of moments presupposes the existence of such expectations which are not needed in the Liapunov method at all. Steklov (1924) described that situation:

In his peculiar frank way, Markov often stated in Liapunov's presence that he played a really dirty trick on me. [...]

However, after seven years the trick much pleased him. Not ceasing to think about this problem, Markov found a means for generalizing the method of moments. Not only did he thus obtain the Liapunov result for magnitudes independent one from another, which all previous authors had assumed, but he generalized the main

propositions of the theory of probability on many cases of magnitudes connected one with another in a definite way. [...]

The last years of Markov's scientific life had been largely devoted to the study of this new chapter of the calculus of probability, to the probability of events connected, as he expressed it, in a chain.

The method of moments, wrote Markov's student, Professor Bezikovitch (1924),

Was one of the most brilliant and most peculiar reflections of A. A.'s scientific activities. Here, the property of his talent was clearly expressed: to provide complete solutions of problems without hesitating because of however great difficulties. And, in addition, to study, exhaustively and thoroughly, the area of investigation. These very principles characterize Markov's work on the law of large numbers: he shows the possibility of essentially widening its applicability and in addition indicates the area in which that law does not reign.

The investigation of the method of least squares was also a completely concluded work¹³.

Andrei Andreevich did not quit working almost to his last days. He submitted his last paper (1924) to the Academy. Markov gave it to Steklov as the latter indicated in his recollections (1924):

When passing me this work for a report at the Academy, he asked me to tell [those responsible] that under normal conditions he would have never published not quite a prepared study, but now, as he added, he feels that death is approaching and is afraid that he will be unable to complete the work. He therefore decided to publish it as his last contribution.

Markov died at about 10 o'clock in the evening of 20 July 1922 and was buried in Leningrad, in the Mitrofanievsky cemetery.

Notes

1. Both here and in the title Markov is called a Russian mathematician. Certainly true, but why mention it? Perhaps because of the current fierce campaign against all foreign and possibly inserted by the editor.

2. Excellent treatises: see my general comments about the latter.

3. Something should have been stated about Chebyshev.

4. This composition was hardly published. Drawing on an archival source, Grodzensky (1987, p. 54) quoted Markov: another student, E. V. Borisov, was awarded a silver medal for a composition on the same subject, but he, Markov, was convinced that Borisov's work was better.

5. See my general comments.

6. Cf. Note 7.

7. Definition rather than axiomatics.

8. Both had appeared, as the author stated, in 1884, in the journal of the Kharkov Math. Society, but I was only able to establish one of them, see Bibliography.

9. See Sheynin (2017, p. 218).

10. Symbol L had appeared above and its meaning had not apparently changed.

11. V. A. Markov (1871 – 1897), see A. P. Youshkevich (1968, p. 412).

12. I translated this phrase according to its meaning. In Russian, it was left uncompleted.

13. I have left out the author's subsequent and utterly ignorant description, see my general comments.

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VII

Oscar Sheynin

The Correspondence between A. A. Markov and A. A. Chuprov on the Theory of Probability and Mathematical Statistics

This is the title of the book edited by Kh. O. Ondar (Spinger, New York a.o., 1981) translated from the Russian edition of 1977 by Charles and Margaret Stein with an Introduction by Jerzy Neyman.

The letters of Chuprov from that correspondence are kept in two archives, one of them in Petersburg, the other, in Moscow, and those of Markov, in the Moscow archive. In his Preface, Ondar acknowledged that the academician (of the Ukrainian academy of sciences) B. V. Gnedenko and Professor (of Moscow University) K. A. Rybnikov were *very helpful in composing the commentary*.

I had not checked the translation, and only noticed a few mistakes and an unsuitable modernization: Markov, who became to a certain extent a victim of his own rigidity (Sheynin 2006), refused to apply the comparatively new term, *random magnitude* (as it is regrettably still in use in Russia). Instead, he preferred to say *indefinite magnitude* which was really bad, but the translators replaced it by *random variable*.

I (1990/2011, pp. 102 – 108) corrected more than 90 mathematical mistakes made by Ondar and discovered, inserted and commented upon thirteen additional letters from the Moscow archive (pp. 86 – 108). Readers ought to take into account my corrections, but in any case Ondar's work was a serious scientific crime.

For about ten years beginning in the mid-1960s I rubbed shoulders with Ondar at the seminar on history of mathematics of the mathematical and mechanical faculty of Moscow University and later occasionally met him until 1990. He came to Moscow as a postgraduate of some institution in Tuva and had been living in the university hostel in the same apartment with young men from East Germany. With foreigners! That was only possible for those enjoying complete ideological trust.

Ondar made a few reports on Russian sources at the seminar and revealed his poor knowledge of mathematics, and he hardly knew any foreign languages. Nevertheless, he successfully defended his candidate dissertation! Indeed, he was a *nazmen* (member of an ethnic minority and in addition promoted by the Party). So Rybnikov (closely associated with high party organs) and Gnedenko (the ideologist among the specialists in probability) mightily helped him. I am sure that the real mathematics contained in the commentaries on the Correspondence was simply written by the latter.

In the Soviet Union, at that time (certainly before 1977) xerox facilities had been hardly available and Ondar who came to Leningrad to study in the pertinent archive undoubtedly asked someone to copy the letters on a typewriter, went back and returned to take the prepared copies. He had examined the copies only superficially if at all. Indeed,

why worry when being nursed and directed? The letters which he missed in Moscow also testify to his happy-go-lucky attitude.

Some phrases are missing in the published correspondence and there are many misprints or mistakes but most of the 90 mistakes are totally wrong phrases partly occasioned by ignorance. Even more unpleasant is (lo and behold!) the mysterious appearance of dates on many undated Markov letters.

In 1990 Ondar told me that he had asked Gnedenko whether he may prepare a doctoral dissertation, but Gnedenko frankly answered that his knowledge of mathematics was insufficient. And quite recently a colleague informed me that some years ago Ondar had died. No loss for science.

I know that *the Ondar case* was only one of many similar instances in which quite unworthy people (not only *nazmen*) successfully defended candidate and even doctoral dissertations and seriously clattered up Soviet science. A well-known variety performer told a true story about a professor who had to write a dissertation for a fool just to get rid of him.

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

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VIII

M. Ya. Vygodsky

Mathematics and its workers in the Moscow University in the second half of the nineteenth century

Istoriko-Matematicheskie Issledovaniya, vol. 1, 1948, pp. 141 – 183

I am only translating pp. 175 – 182

Pavel Alekseevich Nekrasov (1853 – 1924) was the son of a priest. He attended an ecclesiastic seminary and in 1878 graduated from Moscow University. Bugaev¹ left him at the University [for preparing himself to professorial duties]. In 1885 he became [privat-]docent and in 1886, professor of the University². His dissertation (1882) had been noticed in Russia and was translated into German (1887).

In 1883 – 1893 Nekrasov studied various issues in analysis and theoretical mechanics and published many papers (twenty of them in *Matematichesky Zbornik*) on the current scientific level. In the end of 1893 he was appointed rector of Moscow University, exactly at the time when reactionary forces began to attack the universities. The new rector had to be a police agent.

The tsarist government³ was not mistaken: Nekrasov proved to be the necessary man. When the term of his office had been ending, he asked to be retired but the Minister of people's education decided to pass his request to the tsar Aleksandr III. The latter indicated Nekrasov's merits and commanded him to remain in office. His command is being kept in Nekrasov's file at the Archive of Moscow University.

Nekrasov remained rector for four years more, then was appointed civil functionary responsible for the Moscow educational region and finally member of the scientific council of the Ministry of people's education. Soon he abandoned the mathematical issues which had formerly interested him and, from 1898, started to publish contributions on probability theory⁴. Already then he adopted an official manner of writing: he lay down his results without bothering to justify them properly⁵.

I will submit a detailed derivation of all the abovementioned results if circumstances permit me to put my calculations in an order convenient for their appearance in print.

Markov immediately indicated his mistakes⁶, but Nekrasov had not admitted them and their polemic lasted for more than fifteen years. Its sharpness depended not only from, and not to the same extent on the essence of Nekrasov's mathematical errors but rather on his turn from scientist to apologist for autocracy and orthodoxy. Readers who would

like to acquaint themselves with Nekrasov's false scientific methods can look through his book (1913)⁷ sponsored by the Ministry of public education, then headed by a reactionary minister Kasso. Nekrasov did not shy away from profoundly thanking that scientific gendarme⁸.

Kasso would have hardly found a better use of the spent money: in Nekrasov's book mathematical formulas were interspersed with *chemical formulas of a normative state* such as (p. 119) *the constitutional formula ABC: it presumes the concentration of reasonable forces A, B, and C at the head of the political body. The monarch with officials (force C), the patriarch (the synod) (force A) and the state Duma with science and the press (force B). For a believing mind these animated central symbols are the sovereign sacred pledges [...] of the aspiration for bringing the God's kingdom nearer to the terrestrial fatherland.*

In 1902 Markov asked the synod to excommunicate him from the Russian Orthodox Church⁹, and he also attacked Nekrasov with all the then possible might. In 1915 their strong disagreement came to a head when the latter, as member of the scientific council of the Ministry of Public Education, established a commission for studying the possibility of introducing the elements of probability theory into the curriculums of the secondary schools. By pseudo-scientifically applying that theory it was thought to inspire school students with Nekrasov's gibberish about that triangle.

And so, Markov initiated the establishment of a commission at the Academy. Apart from himself, its members were A. M. Lyapunov, V. A. Steklov, D. K. Bobylev and A. N. Krylov¹⁰. Here is a quote from its report:

Mathematicians are acquainted with Nekrasov's views for along time now, but until having been only discussed in special mathematical periodicals, they could have been considered harmless. The situation changes when they are disseminated by an official organ which the school teachers cannot help regarding as an authoritative guide to scientific pedagogic issues. Therefore, the Academy of Sciences, as the most important scientific estate of the Russian Empire (Chapter, § 1), which might enter into everything concerning education (§ 8), and is obliged to care about the dissemination of education in general and to direct it to the general weal (§ 12b), – the Academy ought to express its judgement about the main mistakes and the wrong (hence, harmful) ideas spread by Nekrasov so as to put them into common school use.

That was how the mathematicians of the Academy of Sciences had qualified Nekrasov's activities, but Nekrasov literally terrorized the Moscow mathematical community. He was rector of Moscow University, then the official responsible for the Moscow educational region, and, after Bugaev's death, president of the Moscow

Mathematical Society. During Bugaev's lifetime, Nekrasov, as far as I know, had never attempted to connect his reactionary views with the name of the Society, but after becoming its president that situation changed: On 16 April 1904 he (1904) devoted a speech to Bugaev's memory. He underpinned his Black Hundred propaganda by Bugaev and all the founders of the Society, and his *likeminded personalities* included Fermat, Descartes, Pascal, Newton, Leibniz and other scientists along with Pobedonostsev, Khomyakov and the Reverend Antony Khrapovitsky¹¹.

It seems that Nekrasov himself was somewhat embarrassed when reporting to a scientific society about his nonsense. In any case, he thought it necessary to preface his speech by an explanation, and there we find, in particular:

I consider it my duty to mention the peculiarity of my statement and style. The generally accepted language is not quite suitable for expressing the mathematical contents of the principles of the world's structure. The translation of these contents into the current language is almost insurmountable. This compels mathematicians either to retreat into themselves and thus to abandon forever the expression of the most important vital metrical notions, or to apply most complicated turns of scientific, philosophical, political, social and church language and to repetitions and difficult terms which are incomprehensible to readers who are accustomed to the smart style of fiction writers and empirical dialecticians.

Indeed, after opening his paper at random we encounter (p. 165), for example, such *most complicated* specimen of language as this one:

The moraltriangulation¹² which is provided by the fact of a family (father + mother + son or father + mother + daughter) or by the commandment honour thy father and thy mother, naturally and artificially (?), i. e., moraltechnically, develops into the freelyconnection of society.

As stated above, Nekrasov aimed at wrongly portraying Bugaev as a militant reactionary. True, Bugaev's senile muddled statements helped Nekrasov. However, the latter was not yet satisfied and unmasked himself (1904, p. 239):

The completeness of the contemplation of the world belongs to the entire union and does not allow us to separate Bugaev from Tsinger or Bredikhin, Davidov and Chebyshev or all of them from the rest [scientists].

It is possible to think that these statements were the ravings of a madman, but they are extremely purposeful. When necessary, he was able to express his thoughts in *the generally accepted language*. So it happened in his polemics with Markov (see above) to which we are now returning

Buniakovsky (1846, p. 326) considered it necessary to warn his readers about the application of his formulas of the probability of testimonies to religious faith:

These formulas were derived under the premise that certain physical laws were present, but in the spiritual world there exist facts which are not subordinated to physical laws, so that all the ill-intentioned sophistications of the pseudo-philosophers fall down.

Markov (1913, p. 225) [1924, pp. 213 – 214] bravely opposed Buniakovsky:

Irrespective of mathematical formulas it is clear that we should regard the stories about the probability of events which allegedly happened in bygone times with extreme doubt. And we cannot at all agree with Acad. Buniakovsky in that we ought to separate a certain class of stories the doubt about which he considers blameworthy. However, to avoid still more severe judges and imputations of shaking the foundations, I am not dwelling on this theme.

After Nekrasov was rebuffed he decided to accuse Markov of shaking the fundamentals. In plain words, Nekrasov reported him (1916, p. 12):

Not the ideas incriminated to me but those of Markov are really inadmissible for the education of the teachers of secondary schools. For justifying this statement I am turning the readers' attention to Markov's treatise Calculus of Probability.

He adduced the quote (see above) and declared:

By destroying Buniakovsky's abovementioned fundamentals Markov facilitates the spread of the fundamentals of historical materialism. [...] A better guide for a systematic propaganda of extreme groundless materialism than his book [...] is not needed. [...] And now I can only appeal to the world of scientists and pedagogues and ask them to discuss who, Markov or I, converts pure science into a vehicle for harmfully influencing the civil and religious cult which educates the rising generation.

We see that Nekrasov's statements cannot be only considered as the display of a mental disorder. All the more sorrowful is that the Moscow Mathematical Society tolerated a person who disgraced them as their president. Justice demands, however, to indicate that that Society terrorized by Nekrasov only endured his indecent behaviour but did not share it.

The records of the Society's sitting on 23 March 1905 (*Matematicheskij Zbornik*, vol. 25) stated:

The secretary reported Nekrasov's letter¹³ in which he informed us about his wish to publish in the Matematicheskij Zbornik his paper Organic fundamentals of a state. A moral arithmetical sketch about electors and the elected in their mutual relations and their relations with the supreme authority. He also asks to enter his statement into

the records and, also, owing to the peculiarity of his subject, to register the opinion of the Society.

Referring to the small number of those present, the Society postponed its decision, but never returned to that matter. On 20 September Nekrasov renounced his presidency. The Society resolved to express its *gratitude* to him for his long-term activities. Having observed the etiquette the members of the Society probably took a long breath.

After Nekrasov the most eminent scientist, N. E. Zhukovsky became president¹⁴.

Notes

1. See [v, Note 1]
2. Extraordinary professor in 1886, full professor in 1890.
3. This is a Soviet cliché: everything *tsarist* was allegedly bad in one or another sense.
4. Not 1898 but 1896, see Note 7.
5. The same may be said about Chebyshev.
6. The author's reference was wrong. See the archival correspondence of Markov and Nekrasov during 1898 in **S, G, 4**.
7. Not 1913 but 1912.
Its first edition of 1896 was a currently usual university course of probability theory. Author
Currently usual is doubtful. At the physical & mathematical faculty of Moscow University the theory of probability was not taught in 1902 – 1904, 1912/1913, 1916/1917, see *Obozrenie Prepodavania na Fiziko-Matematicheskom Fakultete* [of Moscow University] *za ... God* (Survey of the Teaching etc. for the Year ...). A yearly published without title-page or indication of place.
8. A meaningless remark since Nekrasov and Kasso were likeminded.
9. See my comments to [vi].
10. The author forgot the astronomer N. Ya. Tsinger. I have translated the entire Report, see **S, G, 4**.
11. Konstantin Petrovich Pobedonostsev (1827 – 1907), jurist, statesman, advisor to three tsars, oberprocurator of the Most Holy Synod. Chief spokesman for reaction. Wikipedia.
A. Blok: *In those remote and God-forsaken years/Sleep and shadows reigned in hearts/And Pobedonostsev spread/Over Russia his owl's wings*.
Aleksii Stepanovich Khomyakov (1804 – 1860), theologian, philosopher, poet, co-founder of the Slavophil movement. Wikipedia
Metropolitan Antony (Aleksy Pavlovich Khrapovitsky, 1863 – 1936). Theologian, held high positions (rector of Kazan Theologian Academy). Was active in the notorious Union of Russian people. Emigrated in 1921. Ibidem
12. Nekrasov certainly borrowed *triangulation* in that gibberish from geodesy. For some years he doubled at the Moscow Land Surveying Institute.
13. Nekrasov had then moved to Petersburg. From February 1905 he had not participated in the Society's sittings. Author
14. The end of the author's paper is beyond the borders of my translation.

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MZb = *Matematich. Zbornik*. All Nekrasov's papers there are in Russian

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IX

P. A. Hansen

On the method of least squares in general and on its application to geodesy

Von der Methode der kleinsten Quadrate im allgemeine
und im ihrer Beziehung auf der Geodäsie.

Abh. Math.-Phys. Kl. Kgl. Sächsisches Ges. Wiss.,

Bd. 8, 1868 für 1865, pp. 571 – 806

I am translating the beginning of this contribution which was continued in Bd. 9, 1871, pp. 1 – 184.

The essence of this discourse is the application of the method of least squares (MLSq) to geodesy, or the adjustment of the angles of a net of triangles according to what I think is the most suitable way. Gauss (1828) was the first to present a special case of that application, and Bessel almost at the same time published his solution of the same problem¹. Later he (1838) provided his solution of the general problem whereas I almost at the same time published as though only the framework (1839) of an essentially differing and still more general solution.

It is this last mentioned solution which I have completely revised here. It differs from Bessel's solution in many ways; in particular, I have entirely avoided the indefinite solution of systems of linear equations² required by him. And I tend to believe that my method largely decreases calculations. I have completely developed the instructions for calculating the weights of any function of the unknowns which were not provided by Bessel³. I have also shown how we should act when more than one base is measured⁴ or when a yet unadjusted net of triangles is connected with another already adjusted net⁵.

When adjusting a large net of many triangles it is important to have at hand the necessary general formulas in such a way and in such order that the complete overview [of the work] will be never lost, otherwise the calculator increases his work. For this reason, while deriving everything and devising an example, I have already taken care to provide the explanation as completely as possible and finally to recapitulate all the instructions and formulas which otherwise could have well become superfluous.

The application of the MLSq to geodesy is indeed the main reason for compiling this discourse, but I do not intend to deprive it from its generality. I rather try to develop its entire scope and then to follow the path about which I had thought for many years. Usually, the MLSq in general is derived from the universal principles of the calculus of probability, but it always happened that to some extent it became

necessary to assume that, when determining an unknown from a number of equally good observations, their arithmetic mean was the most probable value of the unknown. And I assumed this statement as an axiom placed at the summit of the derivation and applied it for devising a method for determining the value of many unknowns from a larger number of observations of unequal weight. I had thus come to the MLSq, which was possible to see in advance, and the theorem proved here can be strictly formulated as follows:

With the same justification with which we assume in the simplest case that the arithmetic mean of the observations is the most probable value of a single unknown, we ought to consider in the general case that those values of the unknowns are the most probable for which the weighted sum of their squared residual errors is minimal.

I think that for the MLSq this theorem is situated on the border of those rigorously proved. During the proof of this theorem it became possible to provide an easy and suitable explanation of the notion of weight of an observation or of its result but it remains impossible to establish the relation between weights and relative precision of two or more observations⁶. Here, it became necessary to apply two known theorems from the elements of probability theory and connect them with the axiom stated above.

One of the following results is known: weight is proportional to the square of precision⁷. It is only necessary to return this study to the case of one unknown since then the inferences can be extended to any number of unknowns. For this reason they ought to be included in the text preliminary to the complete proof of the formulated theorem.

My contribution consists of the following themes:

1. Derivation of the most probable value of one unknown by issuing from observations, §§ 1 – 17.
2. Extension of No. 1 to the case of a larger number of independent unknowns, §§ 18 – 27.
3. Extension of the stated problem to dependent unknowns, §§ 28 – 63.
4. Its extension to geodesy if only one base was measured
 - a) The first method, §§ 64 – 107.
 - b) The second method, §§ 108 – 118.
5. Extension of those methods to the case of a larger number of measured bases or of a connection of a net of triangles to a neighbouring set, §§ 119 – 132.
6. Recapitulation of the instructions and formulas pertaining to the adjustment of a net of triangles, § 133 – 148.
7. Calculation of the mean [square error?] of the results obtained above, §§ 149 – 152.
8. Supplement to the *Geodätische Untersuchungen*⁷, §§ 153 – 156.

Notes

1. The author only referred to the *Astron. Nachr.*, Bd. 8, No. 121 but Bessel had not published anything there, see the Bibliography of his contributions in Bd. 3 of his *Abhandlungen*, 1876, pp. 490 – 504. The reference (not quite correct) was undoubtedly to Rosenberger (1827), to Bessel's student who mentioned his teacher in his Acknowledgement. Bessel himself (Bessel & Baeyer 1838, beginning of Chapter 3) mentioned Rosenberger: he applied Bessel's method.

2. Gauss (1828, § 18) mentioned *imperfecta* or *manca* (incomplete) solutions. Hansen had not mentioned that contribution at all!

3. Gauss (1823, § 29) derived the weight of linear functions of the unknowns.

4. The allowance for the base (and azimuth) conditions became self-evident, but Hansen was perhaps the first to mention (and study?) it. Laplace and Legendre preferred to calculate each half of a triangulation chain by issuing from *its own* base (Sheynin 2017, p. 97, Note 19).

5. Gauss (1823, § 35) studied the inclusion of one observation.

6. This is difficult to understand.

7. Gauss (1823, §6) *defined* weight.

8. These studies (in the same Bd. 8 of 1868, pp. 1 – 224) considered spheroidal geodesy.

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Ludwig Seidel

**On the calculation of the most probable values of such unknowns
between which there exist conditional equations**

Über die Berechnung der wahrscheinlichsten Werte solcher Unbekannten,
zwischen welchen Bedingungs-Gleichungen bestehen.

Astron. Nachr., Bd. 84, No. 2005 – 2006, 1874, columns 193 – 210

1. When establishing by redundant observations the most probable values of a certain number of unknowns which can be corrupted by errors, we usually distinguish two main cases. They oppose each other just as in dynamics the premise of a completely free system of masses and an interconnected system. To the first case belong such problems in which all values of the unknowns agreeable with the observations are admissible. On the contrary, in the second instance the unknowns are not completely independent from each other since between them there exist inevitable relations (conditional equations) which ought to be satisfied for the system of values to be possible.

For example, in geodesy the sum of the three angles of a triangle should have a stipulated value, or the redundant equations on which depends the existence of an intersected point [should be satisfied]. However, we may say that problems of the second case only occur when more unknowns are introduced in the calculation than were necessary for a complete mathematical expression. Therefore, such problems can be reduced to the first case by another choice of the unknowns whose number is diminished by the number of conditional equations.

[I do not continue. First, the adjustment of observations in both cases became known long ago, and at least beginning with Helmert (1872). Second, Seidel proceeded clumsily. He introduced infinite terms, then had to reject them. Additional points ought to be mentioned. **1.** There also exists a third case, adjustment of conditional observations without any observational equations. Strangely enough, neither Gauss, nor Bessel mentioned it. Perhaps Encke was the first to treat it, and Helmert (1872, p. 197) definitely considered it. **2.** Gauss (1826) stated that the second case differs from the first one only in its form, not in essence. **3.** Gauss (1828) considered this second case and the beginning of Seidel's paper closely repeats him. **3.** Bessel jealously and not quite properly claimed priority, see also Biermann (1966). **4.** Seidel discusses *most probable* values whereas Gauss is known to have replaced them by *plausible* values.]

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XI

Morsbach

Lieutenant General Dr. Oskar Schreiber

Generalleutnant Dr. Oskar Schreiber.

Z. f. Vermessungswesen, No. 24, 1905, pp. 529 – 537

[1] With the death of this excellent man which occurred in Hanover on 14 July 1905 after long suffering, geodetic science and practice had lost one of its most outstanding and successful representatives.

He was born on 17 February 1829 in Stolzenau on Weser in the Hanover district and from 1848 belonged to the Hanover army. For many years the deceased had been participating in the topographic survey, mostly in the moorland, in the middle reaches of Ems. Shortly before the [Austro-Prussian] war of 1866 the then captain Schreiber published the *Theorie der Projektionsmethode des Hannoverschen Landesvermessung* (Theory of the Method of the Projection of the Hanover Survey) which caused a sensation among geodesists and scientific cartographers [specialists in math. cartography].

Gauss and Bessel should be certainly thanked, in the first place for the fact that in the first half of the 19th century the leadership in higher geodesy had passed from France to Germany¹. For the Hanover survey Gauss had devised a system of coordinates, provided the necessary basic formulas but left their scientific justification until another time. He was unable to fulfil this promise and the geodesists in Hanover had been applying those formulas automatically. No one had derived them until Schreiber filled that gap.

[2] In the spring of 1867 captain Schreiber served in the unit of the Prussian army aggregated with a Hanover regiment, but his scientific achievement already on 27 December led to his move to the existing Bureau of triangulation of the province for establishing the field of its work. There, his special abilities were able to unfold fully.

Already on 1 April 1868 he became the leader of the survey, then for many years, until 1874, he had been mostly engaged in the laying out of the primary triangulation in various regions. In January 1875 a new organization of the survey of the province was introduced, and, although [only] a major since 1873, he took over the leadership of the trigonometric department from the commendable general von Morozowicz. And already in the autumn of 1874 the chair of higher geodesy at the Military Academy was transferred to him.

We may describe as a specially lucky circumstance that he, a lieutenant-colonel from 1879 and colonel from 1883, had been able to remain in that position for more than thirteen years which allowed him to develop into a geodesist of the first rank and to show, in many

directions, new paths for geodesy. This advance required an extremely sharp understanding of the basis and preconditions and a sound mathematical gift. He often complained that he did not possess a really extensive memory, but instead he had a virtually inexhaustible endurance and a never failing diligence which drove him to work in winter and summer at the earliest morning hours.

His outstanding talent of order and sketchiness (?) should not be underestimated and the same ought to be said about the contents of all his works and instructions which showed most meticulous care and had been arranged advantageously for a large staff. This circumstance can hardly be overestimated.

It is impossible to expound exhaustively the scientific and practical advances for which geodesy is thankful to General Schreiber so that only the most important can be pointed out.

[3] The form of a chain of triangles and nets of the primary triangulation were completely altered. First of all, the form of the triangles became more advantageous after more thorough reconnaissance by means of a higher scaffolding, by preliminary rough measurements and essential advances in the construction of survey signals. By a more rational construction and employment of especially suitable inconspicuous people developed into specialists Schreiber soon became able to devise previously unknown firmer platforms for observers higher above the ground.

The previously applied numerous diagonal connections [of braced quadrilaterals] whose mostly difficult measurement had not been at all warranted by the achieved increase in precision, and they were left out. In future, the chains should as often as possible consist of triangles of good form, measured as precisely as possible and arranged in a straight line.

This was characteristic of, and a guiding view at each kind of measurements, as General Schreiber stressed. He never turned to an unsystematic amassing of checks of the value of measurements, he rather most carefully reckoned and practically attempted to establish those elements which in the first place ensured the precision of the results. According to a preliminary considered plan, time, forces and moneys should always be spent for achieving the highest quality of the results.

The transition from the large triangles of the primary triangles to the net of the second order proved difficult¹ so intermediate stations of the former had been incorporated in such nets. Their determination without essential additional expenses of time and means offers a serious lightening [heightening of precision] of the triangulation of the second order.

[4] The extremely important aim of base nets has been the derivation of the length of the sides of the large triangles by issuing

from the measured bases. All the mostly numerous redundant directions were usually measured without carefully studying whether the work and the means will be spent more advantageously by observing more often those directions which most precisely determined the long sides. General Schreiber completely solved this problem in *Die Anordnung der Winkelbeobachtungen im Göttinger Basisnetz* (The Arrangement of the Angle Measurements in ...), *Zeitschrift f. Vermessungswesen* (ZfV), No. 6, 1882. From then onwards the three base nets measured by the trigonometric department had been [preliminarily] investigated according to those main propositions and observed with the best allowance for the advantageous distribution of weights. They are distinguished by a surprising simplicity.

Schreiber had guided the measurement of three [named] bases [they only partly coincide with those mentioned above] and in 1871, after participating in the measurement of another base with the Bessel base apparatus and thoroughly revealing its strong and weak sides, repeatedly studied in detail all of its parts and their work in combination.

A thorough comparison of the measuring base rods prepared by the Berlin Commission on Standards [of one rod with another?] and provided for him, an improved device for aligning the rods, their better stability by means of wrought-iron supports with micrometre horizontal and vertical regulation by Korbelschrauben, essential perfection of arraignment [of the rods] by plumb line and many other improvements sped up measurements and heightened the precision of their results.

[5] Primary triangulation was completely altered by introducing observations of angles rather than of directions. In two basic contributions, *Über die Anordnung von Horizontalwinkelbeobachtungen auf der Station* (On the observation of horizontal angles at a station) and *Richtungsbeobachtungen und Winkelbeobachtungen* (Observation of directions and angles), Schreiber (ZfV, No. 4, 1878 and No 3, 1879) discussed and justified the new method and developed its benefits. These, apart from many other advantages, include a more precise determination of the angles between directions; the possibility of a preliminarily compilation of a definite and easily changeable plan of observation; a more perfect elimination of constant errors and those of graduation; exactly equal weights of angles measured at a station and almost equal weights when measured in a net; their essentially easier station and net adjustment.

Nowadays there is no more doubt that the introduction of angle measurements had been the most important novelty which, during a generation, ensured such a high measure of perfection in primary

triangulation completed by the trigonometric department. Other countries had been ever more accepting that method. Even France applied the *principes posés par M. le Général Schreiber* when undertaking a large new meridian arc measurement at Quito [capital of Ecuador] as stated in the report of *Kommandant* [officer in charge of a military training establishment] *Bourgeois* at the Fourteenth General Conference of the International Geodetic Association (Copenhagen, 1903).

[6] The calculation of the triangulation of the third order was completely transformed. Previously only the measurements of the first two orders had been adjusted by the method of conditional observations [according to the method of least squares, MLSq], but the [coordinates of the] separate stations of the triangulation of the lower orders were computed by issuing from the means of coarsely calculated sides. Consistent values had not been obtained for about 9/10 of those stations.

For adjusting all the triangulation down to the lowest order by the MLSq and for coping with the corresponding heavy burden of work rectangular coordinates on a plane were chosen for the second and third orders since they best answered the occurring problem. This necessitated the transition of the measurements from the spheroid³ onto a plane. Among many kinds of possible transitions the conformal double projection was most advantageous. At first the measurements were transferred to a sphere according to the law developed by Gauss, then in a conformal projection resembling the Mercator ditto transferred to a plane. For the measurements of the third order the adjustment became essentially simpler since the second stage was completely sufficient and the spheroid proved unnecessary. From 1876 all the stations determined by the trigonometrical department (20 stations in the mean for 100 km²) had been adjusted by the MLSq. It had also been applied when new parts were included into the existing net thus providing a consistent net spread over the whole province.

Rechnungsvorschriften (Instructions covering calculation) which had been underlying that large work included everything necessary for the transition, adjustment and registration of measurements in an objective and official arrangement. These instructions of 1877 were imposed and reproduced for use in the department.

General Schreiber provided these instructions in three booklets for the triangulation of the three higher orders respectively. They included the necessary formulas and tables for calculating geographical coordinates from the bearing angles and sides of the triangles measured on the spheroid.

Schreiber provided the scientific justification of the methods of calculation in a significant work of 1897 *Die konforme Doppelprojektion der trigonometrischen Abteilung der Kgl*

Preussischen Landaufnahme. Formeln und Tafeln (The conformal double projection of the trigonometric department of the Royal Prussian Survey. Formulas and tables)⁴. That was a fruit of his retirement: having been overwhelmed by work, he was unable to finish that contribution earlier.

[7] It is impossible to mention here the advances of the methods of calculation; of the requirements of field work; perfection of instruments and invention of technical aids; but all the more of the fundamental study of standards and errors of graduation for all of which geodesy is thankful to General Schreiber. However, I do not want to refrain from but will rather quite especially stress that one of his worries was a lasting preservation of triangulation nets and benchmarks of levelling, i. e., of the expensive work which required very much efforts.

This circumstance prompted him to introduce many new marks and enact exceedingly tough and painstaking instructions. He was hardly able to achieve enough with regard to especially important places or such objects whose future dislocation [or destruction] was feared (e. g., bell towers). In such cases [he ensured] most precise definition of the points, on which depended the results of measurements, so that their identification will be always possible. Before 1875 the stations of the triangulation of the third and fourth order had been only marked by granite pillars but in addition he ordered underground slabs.

[8] All the technical instructions which he enacted were the results of most careful theoretical consideration and extensive practical attempts. For weeks on end he took over levelling and triangulation of the third order, scrutinized every particular of that work and did not rest until something unclear or doubtful had remained. It is therefore possible to say that his instructions were almost always reliable.

General Schreiber was able to provide incomparable contributions for the International Geodetic Association. He therefore enjoyed a high reputation there although had not regularly attended either its General Conferences or conferences of its Permanent Commission.

He did not like to appear in large meetings, but, on the other hand, he was especially satisfied and glad to promote a common aim and remain in incessant consent with the excellent director of the Geodetic Institute, Professor Dr. Helmert. Schreiber was always ready to support his work with all his might and attached a very high value on the opinion of that outstanding scientist. Their contacts bore nice fruit.

He was not less obliging to, and ready to help the ever increasing number of leading officers and scientists who had been sent from abroad to Berlin for getting acquainted with the achieved methods and the ensued advances, and for personally participating in the [field] work. A correspondence developed and often required too much of his time.

Whenever possible, Schreiber promoted geodesy as much as possible and gladly offered a hand when scientific geodesists, and especially docents of geodesy from technical colleges, desired to participate in the practical work of his department. At times he attended the general meetings of the Dtsch Geometerverein (German Geometrical Society) and manifested an active interest in its development in general and especially in its periodical, the ZfV, to which he submitted valuable contributions.

In some of his work Schreiber considered land use, and his common sense told him that his efforts will only be fully needed if he presented the results of measurement in a handy, clear form devoid of any doubt. Bearing this in mind, and armed with ever new considerations and tireless attempts, he published his *Abrisse, Koordinaten und Höhen sämtlicher von der Trigonometrischen Abteilung der Landesausnahme bestimmten Punkte* (Sketches, Coordinates and Heights of All the Stations Determined by the Trigonometric Department of the Survey of the Land). It was supposed to appear in 24 volumes of which 16 had been published. To this day, anything comparable to his masterpiece has hardly appeared elsewhere.

It is not surprising that after the briefly mentioned reform of the technical work of the trigonometric department had been implemented, the work of Schreiber's distinguished predecessors was as though left behind. Now, more than seventeen years after he had handed the leadership of the department to his successor [to the author, see below], the entire essence of his instructions is still in full force; according to human estimation, it will thus remain for a long time.

[9] On 1 May 1888 Colonel Schreiber became the chief of that department, on 2 August of the same year he acquired the rank of major general, and on 18 November 1890, the rank of lieutenant general. He retained his position of member of the board of guardians of the physical-technical institute of Germany (Reichsanstalt).

That large extent of work brought him, to his joy, in close official relationship with topographic surveys with which he had been well acquainted long ago. This offered him the possibility to promote geodesy in a wider sense and posed many fine problems for his efficiency and enthusiasm. Nevertheless, the main field of his achievements, as he himself quite knew, was located in the work of thirteen years as the leader of the trigonometric department, and he only separated from it [for a while] with a heavy heart.

After his letter of resignation of 8 April 1893 and transfer to the reserve, he moved to Hanover. Shortly before that the philosophical faculty of the Berlin University conferred on him the degree of Honorary Doctor which greatly gladdened him. He had been tirelessly using up the free time granted him by leaving active service for

scientifically promoting geodesy until the increasing suffering stole the pen from his hand.

[10] His rich life's work had served for a lasting honouring of German science as well as of the army to which he belonged for 45 years. He spent only a relatively small part of that period in actual service in the troops. During the [Franco-Prussian] war he was a company commander in the 16th infantry regiment and for some time the commander of its first battalion. He was wounded on 7 October 1870, but on 28 November once more in the ranks at Beaune la Rolande [département Lorret, Centre-Val de Loire]. His comrades recognized that their regiment had no fearless officers remaining cold-blooded in critical situations [except him?] when, with an Iron Cross, he returned in April 1871 to his significant geodetic activity.

[11] For twenty years I had been his colleague and subordinate, almost daily closely connected with him, then his successor as the chief of his department. And I cannot conclude this sketch without seeing once more a personality in that important geodesist.

General Schreiber had a reserved and peculiar disposition. His views and opinions had been certainly resulting from his own experience and thoughts whereas alien influences barely manifested themselves. Averse to any appearances, in any circumstances he liked truth and was opposed to any coverings. But he never deceived his noble unselfish convictions, even when experiencing painful disappointment and ingratitude. We can be surprised by his firmness. And he was always prepared to defend with all his might the wellbeing of his subordinates, either officers or insignificant people, which they will never forget.

Notes

1. It seems that no one comparable to Schreiber had then appeared in France. Both Gauss and Bessel had achieved very much in addition to what is stated here. Finally, at the time, Germany often meant the entire world of the German language.

2. Difficult to ensure precision.

3. More properly, from the chosen reference ellipsoid, perhaps the Bessel ellipsoid.

4. See Zur konformen Doppelprojektion, ZfV 1899, pp. 491 – 502, 593 – 613; 1900, pp. 257 – 281, 289 – 310. Morsbach

XII

F. R. Helmert

Lieutenant General Dr. Oscar Schreiber

Vierteljahrsschrift Astron. Ges., Bd. 40, 1905, pp. 303 – 310

[1] He died on 14 July 1905 in Hanover at the age of 77. He is well known to all [German] geodesists since he created the trigonometric control for the surveying of Prussia. Schreiber had not worked in astronomy but was a member of the *Astronomische Gesellschaft*. The closeness of geodesy to astronomy is all the more justified when we think about the merits of this man, the merits which in their totality could have been only deserved by a brilliant spirit.

Oscar Scheiber was born on 17 February 1829 in Stolpenau on the Weser, Hanover district. He began his career as an officer of the survey of his region for which Gauss had provided the trigonometric control. He attempted to derive the Gauss formulas for calculation and generalize them and succeeded brilliantly. All of Schreiber's later work proves him as the best authority on the Gauss geodetic methods.

For elucidating them he thoroughly investigated den an (written in the German text) the Prussian triangulation the extant part of the geodetic unpublished work of the great mathematician. Owing to his penetrating mind, he had the good fortune to enrich them further. Thereby he became able to protect that large trigonometric survey from the danger of exhaustion by extensive calculations. Until then, that was the usual situation: the growing expansion of the work threatened to hinder any progress. And along with this remarkable simplification a significant increase of precision was attained. Therefore, under Schreiber the Royal Prussian Survey soon found itself at the head of all similar institutions. It became a specimen for organizing trigonometric work in other states [not only of Germany]. Schreiber worked through and reformed to the tiniest detail the theory and practice of trigonometric work.

Schreiber's scientific publications belong to those which each geodesist ought to know thoroughly and place them at the top of his science. And they also provided a stimulus for meaningful investigations in astronomy. On 25 March 1903 this effectiveness also prompted the philosophical faculty of the Berlin University to elect the Lieutenant General Schreiber Honorary Doctor (honoris causa) of Philosophy.

Each German mathematician should be wholly satisfied by the fact that Schreiber developed and ripened the decisive ideas of Gauss for

theoretically and practically treating geodetic problems and deservedly influenced the surveys.

A splendid picture of the life of the late scientist was painted by Morsbach, his colleague and friend of long standing, a Lieutenant General transferred to the reserve [xi]. In general, we ought to refer to this obituary from which I have borrowed some figures. Here, however, we can only appreciate in somewhat more detail Schreiber's scientific achievements according to that publication.

[2] A wide place in his life was occupied by the mathematical development of the conformal mapping of the spheroidal surface of the Earth on the plane. Gauss had devised such a mapping and applied it in the arc measurement and survey of Hanover for simplifying the calculations. This was especially accomplished for the inclusion of points of a lower order on a plane in the net of a higher order by calculation in plane rectangular coordinates. Schreiber's pertinent paper *Theory of the method of projecting the Hanover survey* appeared in 1866 with a Foreword by the eminent mathematician Wittstein¹.

When in 1868 Schreiber became responsible for the Prussian triangulation he had to consider necessarily how to apply the Gauss projection to a larger region. According to vol. 3 of [his?] *Hauptdreiecke* [Main triangles] which appeared in 1876, he first thought to apply a spherical conformal projection for including the triangulation accomplished in 1873 – 1874 by adjustment in polar coordinates, in the existing rigid system. However, the promise stated in the Foreword that the derivation of the necessary formulas will soon be published, was not fulfilled. Indeed, Schreiber had meanwhile thought out a more beneficial method for that inclusion and applied it at once, in 1876. That was the conformal double projection.

In 1897 Schreiber published a detailed and exhaustive description of his formulas in *The Conformal Double Projection of the Trigonometrical department of the Royal Prussian Survey*. The first short account [of same] is contained in vol. 1 of Jordan and Steppes, *das deutsche Vermessungswesen (Höhere Geodäsie und Topographie des deutschen Reichs von W. Jordan, 1882)*². Schreiber himself published a detailed derivation of the formulas (*ZfV*, 1899 and 1900).

In the Gauss projection of Hanover the mean meridian was mapped without distortion, but this fact had not taken place in the double projection. That projection first conformably mapped the spheroid on a sphere, once more exactly by the Gauss method so that a certain arc of a parallel was not distorted. Then, according to Gauss, the surface of the sphere was conformably mapped on a plane. The mean meridian on the sphere was true to *Darstellung*, but it was not a true representation of the original on the spheroid and therefore not on the plane. The scale factor changes in all neighbourhoods of the Himmel

but in case of a direct projection it only changes in the directions to the east and west.

Given the form of Prussia, for practical application this makes no difference but the arsenal of the formulas is much more beneficial in case of the double projection. Indeed, for short distances it is also very simple to include points of the third order on the boundaries of the land.

The rectangular plane coordinates only serve for adjustment and Schreiber found them impractical for transforming them directly into geographic coordinates which were also needed. These latter were therefore mapped back on the spheroid along with the sides and azimuths. The applied formulas were a shortened version of the formulas for the sides³ of the triangles of the first order which Schreiber published in 1878 in a quite clear and practical form. For the main triangles all the calculations were done directly on the spheroid.

[3] Hand in hand with Schreiber's efforts to simplify the inclusion of points into a net by a conformal projection were his thoughts about the simplification and improvement of the observations of directions at trigonometric stations. In Germany, the eminent authorities on arc measurements, Gauss and Bessel, applied two different methods [for observations in general]. Gauss observed angles by the method of repetition and included angles which did not belong to the observed net. He observed *until each angle received its due* (Schreiber, *ZfV* 1879, p. 141)⁴. This practice seems to become known after repeated adjustments made because of the accumulation of observations. The results of the adjustment were thought to be similar to a set of directions observed with a large weight which greatly simplified the adjustment.

On the contrary, Bessel observed directions with a turn of the limb from time to time⁵. On the face of it, this method seems preferable since all the angles can be measured in a single set. However, as a rule, the totality of the directions is not measurable at once, and mostly an involved station adjustment is needed. The precision was therefore diminished since the errors of graduation were eliminated insufficiently⁶. Nevertheless, the [thus estimated] weights of the directions were applied in the adjustment of the net which led to further complication and doubtfulness.

In 1871 – 1874, when observing the net of triangles of the first order, Schreiber abandoned observations by directions and observed angles, at first only to test the economical aspect. He became convinced and convinced the other members of the trigonometric department that the purely formal loss of weight in observation of angles practically leads to an insignificant loss of the time of observation and that the observation of angles was preferable because of its advantages. From 1875 observation of angles became generally

applied and the admiration of the personnel cannot be denied. The followed change and the directions about the use of the new method became known to a wider circle [of geodesists] owing to [Schreiber's paper] *On the observation of horizontal angles at a station* (*ZfV*, 1878). The leading idea was that all possible angles between all directions of the main net of triangles were measured at each station until the weight of each direction at the adjustment was about 24. Each angle was measured at each position of the limb only by the turn of the telescope in one and in the other direction or (with two microscopes) on different and symmetrical with respect to the half-circle subdivided positions. For different angles these positions were different.

In a second paper *Observation of directions and angles* in 1879 Schreiber thoroughly discussed the benefits of his method⁷ as compared with observations of directions. He indicated the increased precision which followed from the application of the same position of the limb. The Schreiber method of angle measurements by its adopted separation of observations over the limb not only provides a much better elimination of the errors of graduation but to a much larger extent ensured the condition for a successful application of the method of least squares (MLSq)⁸. The essential significance of the best possible graduation of the horizontal limb inclined Schreiber to a thorough study of the quality of the available graduations and he ordered Wanschaff in Berlin to construct a special device for studying limbs. In a witty paper *Investigation of the graduation of limbs with two or four microscopes* (*Z. f. Instrumentenkunde*, Bd. 6, 1886) he provided indications for an easy study of the limbs. H. Bruns (*Astron. Nachr.*, No. 3098 – 3099, 1892) developed a supplement especially concerning astronomers.

[4] As the leader of surveying from 1868 and the chief of the trigonometric department of the established Survey of the Land [of Prussia] from 1875 Schreiber had an incentive to attach special attention to the MLSq. After [someone's] accidental oral statement he apparently had not studied that science in more detail, but now he investigated it as a geodesist intrinsically familiar with the requirements of rational practice. With regard to the justification of the method he wholly adopted Gauss' new viewpoint. When calculating [according to the MLSq?] and applying the obtained results he acted extremely skilfully in every detail. The reduction of observational equations with partly negative weights provides an interesting example of an essential simplification of the calculations⁹.

Really brilliant was the solution of his own problem about the most favourable distribution of the observation of angles in a base net¹⁰. He followed Gauss (1828). A thorough study of the works of Gauss occurred at the right time. And we ought to remark regrettably that

even today many of those who had been applying the MLSq for a very long time have no idea about that wonderful article¹¹.

Schreiber's solution is theoretically extremely simple but in practice it requires some attempts, a circumstance which he explained by examples, see his paper *The arrangement of the angle measurements in the Göttingen base net* (*ZfV*, 1882). When he observed those angles in 1880, that solution was not yet available, but he very nearly obtained it by attempts. In Meppen [Lower Saxony] in 1884 and in Bonn in 1892 he applied it with small deviations introduced for practical reasons.

H. Bruns, in 1886, in an elevated theoretical paper *A problem in adjustment* which was prompted by Schreiber's work and an astronomical problem derived the most advantageous distribution of weights for the case in which many functions of observations were taken into account at the same time.

[5] It is self-evident that Schreiber devoted his interest not only to the extension of bases by base nets but to the measurement of the bases itself. The Survey of the Land owns the Bessel apparatus for measuring bases and applied only it. Schreiber was not inclined to abandon it but had very much improved its handling and the speed of measurement increased and the internal convergence of measurements improved. A contributory factor was that the equation of the length of the rod took into account not only a linear temperature term but a quadratic term and a dynamical term which allowed for the velocity of the change of temperature¹². The random mean [mean square?] error was somewhat less than $1/4 \cdot 10^6$.

A new thorough determination of the length of the four rods revealed a change greater than 0.01 lines [1 line = $1/10 - 1/40$ of an inch] the cause of which remained unknown. (It can be mainly due to faulty materials applied in the zinc rods – Helmert?) General Schreiber understood all the significance of this cause of error. However, when the precision is calculated by issuing from random mean errors, there still remains sufficient reliability since the total mean error can be estimated as $1/600,000$. This is also corroborated by the superb coincidence of the results of three bases repeatedly measured by the Brunners apparatus belonging to the Geodetic Institute [opened in 1886 in Potsdam].

[6] In May 1888 the direct leadership of the trigonometric department of the Survey of the Land passed to another person since colonel Schreiber became chief of that Survey. In April 1893, according to his wish, he was transferred to the reserve after becoming lieutenant general in 1890. He came to Hanover and devoted himself to studies. A fruit of his free time was the mentioned construction of the conformal double projection. That time regrettably became ever more shortened by illness.

From 1878 to 1903 I enjoyed a repeated possibility of meeting personally that excellent geodesist, of being gladdened by the hospitality of his house and of wondering about the originality and profundity of his thoughts. His charming frankness and obvious integrity brightened up the hours of our meetings, ensured special value for them and left in my memory one of the most precious gifts which had brought me to the measurement of the Earth¹³.

Notes

1. Apparently Th. Wittstein. See for example his contribution (1867).
2. Difficult to understand this reference.
3. Only the sides? Incidentally, Helmert had not mentioned braced quadrilaterals (or other geodetic figures) although he himself had studied them previously (Sheynin 1995, p. 77).
4. See also Gauss' *Werke*, Bd. 9, pp. 278 – 281. Concerning observations of angles and directions see Bradford (1948). True, I am not familiar with that paper.
5. This was not good enough (if reported correctly). The limb should have been turned after each set of observations. For ten (say) sets, turned by 18°. It follows that the number of sets should be known beforehand.
6. This is difficult to understand. The same is true about several places at the end of § 3, for example: precision was increased since the limb was not turned, after which there followed an opposite correct statement. It seems that Helmert had inattentively compiled this paper. (I do not think that he was ignorant.)
7. How exactly did Schreiber improve on Gauss?
8. This is difficult to understand.
9. Helmert did not explain this *interesting example*.
10. Curiously enough Helmert did not mention his own previous study of the same problem (Sheynin 1995, p. 78). For that matter, his investigation also concerned adjustment of geodetic nets and the replacement of chains of geodetic figures by geodetics (Ibidem, pp. 81 – 82).
11. Cf. Eisenhart (1964, p. 24): the existence of the memoirs Gauss (1823; 1828) *Seems to be virtually unknown to all users [of the MLSq] except students of advanced mathematical statistics*.
12. Who introduced these additional terms?
13. Helmert began to study this subject in 1906.

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XIII

G. P. Matvievskaia

On V. I. Romanovsky paper [xiv]

Istoriko-Matematicheskie Issledovania, vol. 2 (37), 1997, pp. 66 – 67

All his life Vsevolod Ivanovich Romanovsky (1876 – 1954), one of the most eminent national specialists in the theory of probability and mathematical statistics, had been connected with Central Asia. He was born in Verny (now, Almaty), lived as a child in Tashkent and graduated from the local school. He was mathematically educated in the Petersburg University, and, in his contributions, had been keeping to the traditions of the Petersburg mathematical school.

After graduating and passing his master examinations, Romanovsky for some time taught at that Tashkent gymnasium. Then, in 1911, he became a docent and later professor of the Warsaw University. In the beginning of WWI he, together with the University, was evacuated to Rostov-Don. All those years he had spent his vacations in Tashkent and directly participated in the cultural life of that city.

Local intellectuals had for a long time discussed the need of establishing a higher educational institution in Central Asia, or, for the time being, of a People's university of the type which well proved itself in various cities. Romanovsky had taken an active part in the compilation of a plan of such a university, and later, as the circumstances turned out, became one of its organizers.

The complicated situation during the civil war made trips from Rostov-Don to Tashkent and back too difficult and in 1918 Romanovsky found himself cut off from Rostov and did not return there.

I am appending a forgotten paper of Romanovsky on the principles of the arrangement of the Tashkent University. It was published in 1918 in a local periodical which was soon discontinued. His paper is interesting first of all as a document showing the situation of people's education in pre-revolutionary Russia. It also testifies that Romanovsky was sincerely interested in the development of education and science in Central Asia. He had been thus interested all his life and totally surrendered himself to teaching and science. The Tashkent school of the theory of probability and mathematical statistics which he had created has much contributed to the development of national mathematics.

Bogoliubov A. N., Matvievskaia G. P. (1997, Russian), *Vsevolod Ivanovich Romanovsky, 1879 – 1954*. Translated by Oscar Sheynin. Berlin, 2018. **S, G**, 91.

XIV

V. I. Romanovsky

On Some Goals of the Proposed University in Tashkent

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[1] Turkistan (Turkestan) is a country of ancient high culture, rich and peculiar, full of widest and excellent opportunities. It saw the riches and splendour during the time of Timur who ruled Persia from Samarkand, victoriously marched across India from North to South and successfully battled with China. Those riches and that splendour had been based on fortuity, on the military power and state mind of the great conqueror and soon disappeared after his death.

However, a new flourishing will appear instead. It will only wither away together with human intellect since it will be based on that intellect, will be necessitated, supported and developed by it. For humanity, mind in conjunction with nature is the greatest and the most beneficial union.

All the future of Turkistan, of the Earth and its nations, lies in that union. And the first step to achieve such a union for the benefit of that Territory is being prepared by the proposed establishment of the Turkistan University in Tashkent. I devote the rest of my paper to some considerations about the goals of that future university which will be connected with the material development of Turkistan. That will be the immediate and most urgent aim to which each inhabitant of Turkistan ought to strive for.

The war had already involved in its ruinous orbit almost the whole planet. It painfully and convincingly revealed the great significance of positive science for the life of people. In war, technology and industry born by mechanics, physics, chemistry and biological sciences, no less than military art by itself, play an extremely important role.

Improved rifles with clips, machine guns, trench mortars, quick-firing guns of the light field artillery, mortars of the 42 *cm* calibre, airships, airplanes and submarines, choky gazes and explosives of horrible force, all these means of destruction applied in a contemporary war as well as an uncountable set of medicaments, means and devices applied in innumerable infirmaries and hospitals which serve the same war, all that was born by pure science set on a technical and industrial base.

They would have been impossible without the perfection which was attained by mathematics and exact and experimental sciences. We, all of us, see and painfully feel what a mighty and dangerous enemy is

Germany. For more than half a century it cultivated scientific research coupled with industry and technology; that Germany where both the ruling section of the population and the leaders of industry and commerce, to say nothing about the representatives of pure science had recognized and put into practice widely formulated and properly organized scientific researches for the well-being and preservation of the nation.

[2] At the beginning of the war Germany had surpassed in armament any other nation. Its industry was ahead of the industry of any country. In innumerable ways, often very essential, fateful and scary, even the most advanced and cultured states of the Earth found themselves in an industrial and technical dependence from Germany. The war had disclosed the significance of science for all countries of Entente. Deadly danger had been revealed not only for Russia but for France and England as well.

It was occasioned by the backwardness of their industry as compared with Germany and almost wholly occasioned by an insufficient understanding of the importance of science for contemporary humanity and a clumsy use of its possibilities. To see the extent, to which Russia became Germany's slave and how had it threatened suffice it to recall the statistical tables and diagrams published in [the newspaper] *Russkoe Slovo* in the very beginning of the war. Had not our allies rapidly freed themselves from the German industrial dominant influence, and not helped Russia, we would have long ago been put out of action and subdued.

For the same reason already in the beginning of 1915 in France, and especially in England, there originated a wide reformatory movement which was initiated and supported by all of their outstanding scientists and public figures. It aimed at a proper organization of scientific research and scientific education on the level with the goals and requirements of modernity. In England, for example, that movement led to the establishment of the National Physical Laboratory, the Imperial College of Science and Technology [at the University of London], the British Science Guild, the Council for the Development of Scientific and Industrial Research¹ etc. At the same time there appeared many societies and councils which aimed at elaborating further measures for a planned development of scientific and industrial research answering modern state and national problems.

Science plays the main role in the peaceful life of nations and even a more essential role than in the period of military conflicts since in peace the field of its application is infinitely wider and those applications are infinitely more diverse. We have entered such an epoch of development when the scientific and industrial success of the nations will determine their fate. In our time scientific discoveries duly applied on an industrial basis create or destroy entire branches of

industry and at the same time lead wide social groups to prosperity and progress or decay, misery and degradation.

As an example of such an overturn I can indicate the synthetic preparation of indigo which had been previously extracted as a vegetable paint. This was the invention of the German chemist Bayer in 1880. Twenty years later, after numerous experiments and essential expenses (more than 10 *mln* roubles) an aniline and soda factory in Baden² began to produce indigo in unlimited quantities. The indigo industry in England was therefore done away with. In a similar way the synthetic production of the alizarin paint at the same factory by means of coal tar destroyed the French madder industry which had provided profit to the tune of 100 *mln* francs yearly.

[3] It seems therefore evident that for each nation a proper organization of scientific education and scientific investigations is a problem of life and death. Germany had understood it long ago and the USA followed suit better than any other country. There, in the USA, many millions are being spent for the establishment and support of most various scientific institutes and laboratories, for the organization of an incessant and rational interaction of science and life in the widest sense right up to housekeeping and kitchen as seen by a number of measures taken by the American Bureau of Measures³ for thoroughly rationalizing housekeeping.

For estimating all this activity in the US suffice it to recall the Carnegie Institution of Washington with its sections of research in experimental evolution, botany, embryology, biology of sea animals, geomagnetism, geophysics, economics and sociology etc. with its multimillion budget and numerous (up to 50) laboratories for scientific investigations with a budget of 200 thousand to a million roubles each.

I provide one more fact. Only for January and February of this, 1917, year, and only large donations (from 30 thousand to 1,630,220 dollars) to universities and scientific organizations of the USA amounted to about 5.5 *mln* dollars. So should not we attribute a large part of the tragic events which our ignorant and poor Russia is now experiencing to the influence of our lack of education, our industrial backwardness which follow our previous conscious or unconscious disregard of science?

Everyone agrees that an economic rebirth of Russia and its further existence are impossible without its intensified industrial development. This, in turn, is impossible without the development of science, of proper national scientific education and scientific research. We may safely say that the future of Russia lies in its universities and polytechnic schools. Of course, we also need a proper and firm legal order of the state and public life. This condition, however, is needed

for the existence of each state but it is certainly insufficient for its flourishing. Below, I presume that it is fulfilled.

[4] The future of Turkistan which is a part of Russia's future is also intimately connected with the high school. For the inhabitants of Turkistan the care about their country is their personal, local aim and, at the same time, their aim as citizens of Russia. It is particularly necessary, and as thoroughly and as best as possible, to decide, how to organize a higher school in Tashkent and formulate its aims. Keeping within my power, I will set out the pertinent considerations although only those which, as stated above, have to do with the material development of the territory⁴.

In the future, the well-being and the power of nations and countries will be certainly even more dependent on their industrial development. Therefore, to achieve the well-being of Turkistan it is necessary first of all to see to its industrial and economic development. Wide and various technical forces and a comprehensive study of the natural wealth of the country are needed. And, again, that wealth can only be made useful by technique and industry. It follows that we need agronomists, civil engineers, mining engineers and mechanics (applied mathematicians), hydraulic engineers, electricians, etc. And, to educate them, teachers are needed, professors and their assistants working in technical departments of the high school. These teachers belong to two types: representatives of general disciplines (mathematics, theoretical mechanics, physics, chemistry, mineralogy, geology, botany, zoology etc.) and of special disciplines, i. e., of various technical disciplines which are learned at some departments of the high school. The education of teachers of one or another type is a most important aim of a university or polytechnic school or various special high schools.

To base soundly the industrial development of Turkistan, the future high school in this country should especially bear in mind this problem. Turkistan should have its own teachers who became familiar with the needs of the Territory and were scientifically educated in the same place where they will work. This remark ought to be especially accounted for when educating teachers of special technical sciences.

[5] It is intended to open a university in Turkistan, but a university of a new type, with technical departments. Given favourable conditions, such a university can widen the problem of preparing teachers of both types. However, universities and polytechnic schools also require researchers to develop their disciplines and discover novelties.

Teacher and researcher are not always united in the same person. Gauss was a genius but he disliked teaching and avoided it. Newton read lectures only about a fortnight yearly and mostly expounded his own discoveries. Science is obliged to both for great discoveries but

none of them had direct followers, they had not left a school, as it is called. Conversely, a perfect teacher can be not creatively fruitful. The university should have research chairs for scientists to belong to it and investigate without the obligation to read lectures. Therefore, changing the usual Russian arrangement of a university professor indispensably uniting teaching and investigation in his work, it is necessary to ensure free devotion to research for those who are not inclined to teaching but show a talent for investigation.

In Russia, there are research chairs but they are concentrated in one central institution isolated from the universities: in the Academy of Sciences, in Petrograd. The academicians are known to have no obligation to read lectures or to teach in any other way and are working in such conditions which ensure free scientific research. But one such institution is not enough for the enormous Russia. Similar institutions although on a lesser scale are also needed and they can be successfully represented by research chairs in universities. Such petty analogues of the Academy of Sciences will be somewhat advantageous: they will be nearer to local needs without being torn off from the universities. Note that at present it is supposed to establish such chairs in British universities. This arrangement, even if not immediate but decided in principle, will provide yet another point of beneficial novelty for the Turkistan University.

[6] Another problem is closely connected with that of creating teachers and enlisting researchers, the study of the Territory, both purely scientific and industrial. Such a study is obviously needed for a sound and proper life of the inhabitants. In addition, young men who are intending to teach or investigate in a university will prepare themselves to that work in laboratories, museums, during expeditions, in experimental fields and plantations, in factories and enterprises. That preparation will be extremely important and useful for them and their future work for the welfare of Turkistan⁵.

Connections with reality should never be broken off and most fruitful scientific preparation will always be that which, satisfying the necessary special conditions of the appropriate discipline, goes on by studying as much as possible concrete vital problems. Real science was never and cannot ever be broken off from the urgent problems of time and place⁶.

Therefore, to repeat, the Turkistan University ought to consider the theoretical and applied study of the Territory as one of its most important goals. The idea of connecting technical departments or faculties in that University to the purely scientific usual faculties, just as it is done in English and American universities, is fortunate. If a technical faculty with various departments (civil engineering, mechanical, hydraulic technical, mining and agronomic) is connected with its mathematical and natural-scientific departments, a centre of

power will be formed. Its influence on the material and spiritual development of Turkistan cannot be even approximately estimated.

Pure and applied sciences, unusually valuably for both, will then develop when being intimately close to each other. The technical sciences will find themselves always near the source from which follows their very existence, follow all their discoveries and applications. Indeed, the progress of industry is impossible without the progress of pure sciences. Technology is based on the abstract work of theoreticians, on their discoveries which often seem to have no connections with real life. Thus, all the currently greatly developing electrical engineering is based on the laws of induction and electrolysis discovered by Faraday, and the wireless telegraphy, on the purely theoretic investigations of Maxwell about electromagnetic waves.

On the other hand, abstract and pure sciences will be invariably informed about the needs and aspirations of technology and industry and this proximity will freshen and vitalize their problems. Science, cut off from life can easily sink into scholastic sketchiness, whereas, when being near to reality, it discovers ever new fruitful fields of research.

I indicated three aims of the future Turkistan University: preparation of teachers and researchers and study of the Territory. These are the usual aims of modern universities and polytechnic schools. I have also indicated a condition which is favourable to the highest extent for solving those aims, i. e., the unification of a university and a polytechnic school in one single institution.

[7] I am now turning to a wider and more profound problem which modernity opens to science and technology in their state activity and which the new type of the high school will be best suited to solve. Its solution would have been impossible either for a university or polytechnic school by themselves. Until now, even in the most advanced countries (Germany, USA, England) scientific, industrial and technical investigation had been carried out according to isolated personal plans with a large component of fortuity in the formulation of problems and questions. This circumstance led to uncoordinated work, often to vainly spent efforts or useless, insignificant or already accomplished research. The progress of science and technology was based on separate independent efforts.

The ever more complicated life, and, at the same time, the closer and more profound connections between its different parts prevent isolation or fortuity in the manifestation of various vital forces. For a further successful development of life an ever better coordination and cooperation of its forces are needed. The same should characterize the activity of scientists on one side and industrialists, technicians and merchants on the other side.

The elaboration and realization of the interaction of pure and applied science, the organization of research in a planned connection with the industrial life of Turkistan is naturally the business of the Turkistan University. This is its great aim in addition to those discussed above and usually fulfilled by universities. The University should provide pure scientists and practitioners and it also ought to organize both of those, elaborate plans for their work which interacts to a certain extent and outline the urgent and most important aims.

But how to achieve such an organization and what is its essence? These are fundamental problems of state importance, but this is not the place and neither the time to solve them. To provide an idea about the approaches to their solution I briefly describe the measures suggested by the US National Research Council for attaining a balanced development of science and industry. These measures are expounded in a report of Professor Hale, the chairman of its organizational committee, published in the *New York Times* in 1916 and reprinted the same year in *Nature* on 28 September, new style.

The aim of that Council is to urge on the existing state educational and instructive, industrial and other organizations to assist jointly the study of the phenomena of nature, to intensify the application of scientific research for the development of the American industry, strengthen the means of national defence by scientific methods and in general to develop such applications of science which ensure national welfare and security.

The Council consists of outstanding American researchers and engineers who represent the army and fleet, of the Smithsonian Institution (in Washington which aims at assisting scientific work in ethnography, astronomy and geomagnetism), various national scientific societies, educational institutions, research units, scientific laboratories, industrial and technical (?) enterprises.

The Council plans [to create] two types of research committees: central committees being in charge of various branches of science and local committees in universities, colleges and other participating research institutions. Here are some items from the plan of actions outlined by the Council and approved by the Council of the National Academy.

It is necessary to establish qualifications for those researchers who are supported by the Council and a plan of investigations which should be fulfilled by the participating state educational and research establishments and industrial research units. These qualifications and plans should be compiled in agreement with that general plan which will be elaborated by the proposed (now, apparently created since 1916) State Council of National Defence. The various special committees ought to submit reports indicating important problems and favourable possibilities for research in different branches of science.

It is necessary to collaborate with educational institutions by helping them to obtain large donations and ensure more favourable conditions for training the students in the methods and spirit of research; to collaborate with research units and other societies which wish to ensure a more productive use of the means earmarked for investigations. Finally, to outline the need to support laboratories destined for strengthening the means for national defence and intended to ensure the independence of the country from foreign sources of supply which can be severed in war.

[8] This is a brief list of the aims of the National Research Council. There also exist plans of various scientists and industrialists who have similar aims and many similar plans can be cited from English and French literature. Quite a number of points there as well as in the described list should be changed when applied to Turkistan which is only a Territory rather than a vast state like the US or the British Empire. But the essence of the proposed measures consists in the creation of a central organisation for uniting and directing scientific and industrial research for the welfare of the country and it is wholly applicable to Turkistan.

Such an organisation obviously cannot be created apart from the Turkistan University. Most probably that even if the idea about the creation of such an organization comes from some central institution of Russia, it will be implemented under the guidance of the University and by it. It is also obvious that we cannot do without such an organization although it is difficult to hope that it will be established in the near future. And that organization will intensify many times over the importance of the university for the Territory.

I have listed the most important aims of the future of the Turkistan University connected with the material and economic development of the Territory and depending on exact, natural-scientific and on technical sciences which are based on them. These aims are great and wonderful, their solution promises a glorious future both for the University and the Territory. Properly formulated and carried out in a planned way they can be successfully fulfilled. It is only necessary always to connect them with the definite vital problems of the Territory, not to tear them away from real life and coordinate them with the available forces and resources, and the development of the University and the Territory will be ensured.

Scientific investigations in any field, pure or applied, are the main form of public service which should be assisted by any means, says Professor Hale in the report mentioned above. They, as well as the institutions connected with them, will never be depreciated, never fall into decay, if rooted in the real life of the time and place.

The problems discussed above do not at all exhaust the activities of the physical & mathematical faculty or of the technical faculty which

is closely, and I would say, vitally connected with it. They have other aims as well which I have omitted only because I restricted my description by the problem belonging to the material side of life.

[9] To indicate some of those additional aims I mention that the physical & mathematical faculty ought to be concerned about the development of pure science for its own sake. It should prepare teachers of the secondary school which is a very important aim of a university but we should confess that it is very poorly fulfilled by Russian universities. Finally, the faculty ought to take care of elevating the general level of scientific education of the society and therefore popularize science.

I left aside other possible faculties of the university: historical-philological, Eastern languages, the law and the medical faculty. Their general and special importance for Turkistan will be undoubtedly great, they will find a widest and perfect field for investigations and make great and beneficial discoveries. A philologist, a historian and a lawyer will find an almost inexhaustible field of research in those cultures which are still with us or existed here in the past.

How vast are for example the prospects for orientalists in this Territory which had been only touched by scientific investigations. Almost a subtropical climate with all the transitions from lowland deserts to high mountainous regions, abundance of natural medical resources. An infinite set of peculiar problems, both theoretical and practical, is promised for a physician. Our renowned scientist Woeikov⁷ wrote in his paper published in *Vestnik Evropy*, apparently in 1914:

There is no other territory where a man can accomplish more cultural work than in Turkistan.

However, I am a mathematician and do not discuss the aims of other faculties and only allow myself to remark the following. The tragic and distressing conditions of the time through which Russia is now living, advance to the forefront the need of material and economic revival of the country, and of Turkistan in particular. After the regulation of the political and legal life this should become the immediate problem. It is necessary to think right now about the immediate and most vital of needs. First of all we ought to worry about the material well-being and development of the Territory, to take necessary and purely practical measures and then turn to measures designed for its remotest future.

Among the latter, as I believe, there should be the establishment of the Turkistan University with a physical & mathematical and a technical faculty. Indeed, in our time it is the field of their sciences that is the base on which the welfare of nations and countries is erected. And only when the material life in the Territory will return to normal, and means and prosperity will appear, only then it will

become possible to open the other faculties of the Turkistan University. However, until means for them are not available and they can only be opened to the detriment of the physical & mathematical and technical faculty, their existence will have the imprint of utopianism and impracticality which are inadmissible where institutions of state importance are established.

Notes

1. I have only found this Council in India.
2. A historical province in Germany.
3. I have not found this Bureau.
4. Romanovsky used capital letter T for denoting Turkistan as a country, but he was not consistent. In some cases I replaced t by T.
5. Laboratories, museums ... Where were they in Turkistan?
6. Mathematics is not necessarily connected with reality. Romanovsky himself mentioned abstract research made by Faraday and Maxwell (below).
7. Aleksandr Ivanovich Woeikov (Voeikov), meteorologist, climatologist, geographer.

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N. S. Chetverikov

A few words about the work of V. I. Romanovsky

Vestnik Statistiki, No. 9 – 12, 1922, pp. 42 – 44

As Romanovsky stated, his works, which are published in this issue, are chapters from a more extensive contribution. These reports [works] are not intended for a wide circle of readers, not because of their mathematical form (which only requires the knowledge of algebra and the main theorems of probability theory) but rather because of the rigour and abstruseness of the applied method. They require some preparation and a special habit of thought, so let it be allowed to premise them a few lines for justifying that ardent interest which they excited among theoretical statisticians.

A fresh current is beginning noticeably to break through the statistical theory and to outline a new phase of its history. The stormy development of the statistical methodology accompanied by its penetration into the very citadel of the so-called exact sciences, of physics and mechanics¹, calls forth a striving for securing the conquered field and summons up fresh energy for new victories.

Statisticians attempt to clear up *the theory of their theory*², to realize the peculiarity and universality³ of their notions, of their Weltanschauung. They are already probing the laws for wide generalizations which should solder together, into a single whole, the yet uncoordinated chapters of theoretical statistics. All this requires the introduction of implacable rigour and clarity into the methods of constructing and formulating the initial propositions.

Nowadays, the matter deals not anymore with the replacement of the practical methods which had been discovered by groping around, no! The matter concerns an introduction of complete rigour⁴ and precision in the very mathematical basis of the statistical methodology.

Until now, the two most prominent statistical schools, the English and the Continental, only slightly cooperated with each other. Each had been developing its own beloved problems. German statisticians headed by Lexis had concentrated their efforts on the theory of stability of statistical series and supported themselves by the work of French mathematicians⁵. This school is poor in concrete studies; its main attention was directed on the abstract theoretical development of guiding sketches (mostly urn problems) and notions. The development of an applied methodology had been slow, more attention was devoted

to the fundamentals of the entire edifice, rigour of demonstration, harmony of the axiomatics [of the premises?].

The history of the statistical thinking in England had turned out quite differently. The main stimulus for development was provided by the problems of variability and heredity as bequeathed by Darwin⁶. Along with the luxurious flourishing of the methods of study (how to count?) we see here a theoretical development of the mathematical side of biological problems, a genuine theory of the problems themselves (how to imagine a phenomenon? to formulate questions?). Thus appeared the theories of the curves of distribution; the expansion of complicated curves; the theories of correlation and contingencies; the theory of the precision of statistical means and other indicators; and many other subjects. And, along with all that, the theory of moments which serves as a common basis for it.

It will be grossly unfair to reproach the English school for a slighting treatment of the concepts and schemes of probability theory and statistical logic. The works of Pearson contain many indications about the initial problems and notions (urn problems, elementary causal chains etc.) from which there had developed the theory of the curves of distribution and the correlation theory. Only the peculiar manner of exposition which avoids any ornaments, anything *superfluous* which does not directly bear on the studied problem or on the methods of its solution, only that can give rise (to repeat: can groundlessly give rise) to contrast the English who *superficially and formally describe*, and the German scientists who penetrate the *inner structure* of phenomena⁷.

The richness of the English methodological thought, which is conditioned by its close and incessant dependence on studies, was nevertheless achieved at a high price. All the initial notions and constructions were embodied in concrete images of empirical totalities, whereas the abstract mathematical essence of the studied magnitudes and especially of the methods slipped through the investigator's attention. When problems became more sophisticated and the requirements on the rigour of their solution were heightened, the defects of the English school became vividly felt, led to mistakes, confused controversies and even hampered the further development of the theory. This especially concerns the study of the probable errors of statistical indicators and the estimation of the precision of approximate equalities.

It is indicative that the English are usually applying analysis of infinitesimals and approximately transfer the achieved results on the calculus of finite empirical totalities⁸. The Continental school, however, oftener applies more cumbersome but better justified algebra. Today the development of statistics leads to the need for a

synthesis of the positive features of both schools. Such is the vital problem of our epoch.

The new current possesses a mighty weapon, the method of mathematical expectations. Its might consists in the strict separation of the theorems, notions and magnitudes which belong to statistical variables and to their limit values.

Apart from the usual definition of the expectation which is provided in the courses on the theory of probability, [...], this notion can be interpreted by statistics alone as the limit of the mean value of a statistical variable when its underlying totality increases unboundedly (and the conditions for the appearance of the studied phenomenon remain invariable).

The mathematical situation therefore distinctly differs depending on whether we consider finite totalities and values of variables (empirical material can only belong here) or the limit relations which are the beacons of theoretical thought. This peculiarity of the method of moments completely clarifies the solution of two most important problems of the statistical theory: **1.** The discovery of the limit of some indicator. **2.** The discovery of the precision of the value of that limit given a concrete [empirical] material⁹.

A reconstruction of the fundamentals of the English achievements in the spirit of the best traditions of the German (!) school opens up a boundless perspective for the future.

That work had begun in Petersburg more than ten years ago and is now continuing in Dresden [by Chuprov in both places] and in far Tashkent, a city isolated from the scientific life of the West¹⁰. Similar ideas are born, analogous goals are formulated, the same methods are being applied there. This is why Romanovsky's reports, in spite of the exceedingly abstract manner of their exposition, are actually topical and vitally important for statistics. Their import is needed for the development of our science. Such investigations are similar to the electric current which puts lathes into motion and gives us light for life and work.

Attachment

I seize the opportunity to append a list of the latest works of Chuprov [...] ¹¹

Notes

1. Statistics did not penetrate mechanics (chaotic motion) but Chetverikov certainly had not known anything about this phenomenon, so what did he bear in mind?
2. The theory of statistics had properly emerged in the works of Fisher and Student (Gosset), and the author's expression is unfortunate.
3. These notions are opposed to each other.
4. Rigour is a notion changing in time and *complete rigour* is a doubtful expression.

5. Cramér (1953) noted that at the beginning of the 20th century only French and Russian authors had been rigorously treating the theory of probability. The only Russian author which he could have thought about was Markov.

6. Bernstein (1922; 1924) published interesting papers on this subject, but even in Russia they are little known.

7. To whom had Chetverikov referred?

8. See Note 5.

9. Bienaymé and Chebyshev are properly called the founders of the method of mathematical expectations. In Germany, its partisans are Bohlmann and Bortkiewicz but its complete development is due to Russian statisticians-mathematicians (Markov, Chuprov and O. N. Anderson). The English had ignored it but fell back upon its imperfect imitation, the so-called *elementary proofs* of some general propositions in the theory of probable errors, sample investigations and curvilinear correlation. I ought to indicate, however, that Chuprov's main contribution [1918 – 1919; 1921] had appeared in *Biometrika* and his corrections of the constructions of Pearson and his students were at once acknowledged by the latter. N. C.

The *complete development* of the method of math. expectations (which Chetverikov also called method of expectation instead of sticking to one name) seems doubtful, cf. Note 4. I have discussed that method in my comments on [vi]. Some information about the English school is in Sheynin (2011, chapter 5). The merging of the two schools had not actually occurred (Sheynin 2017, § 15.3). Chuprov's *corrections* were likely made in 1919. He privately sent them to Pearson (Sheynin 2011, p. 75) who had not mentioned him. O. S.

10. This is hardly true. Romanovsky corresponded with Pearson and Fisher (Sheynin 2008) and in 1925 went abroad on a scientific journey, see Bogoliubov et al (1997). Foreign literature, however, was likely difficult to come by.

11. See the bibliography of Chuprov's works in Sheynin (2011).

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