J. B. J. Fourier

General enumeration of questions which should be treated in the Mémoires statistiques sur la ville de Paris …

Recherches statistiques sur la ville de Paris et la département de la Seine anée 1823. Paris, t. 2. Deuxième edition, 1834, pp. VII – XII

I. Topography

1.1. Physical description

Geographical situation, extent, height above sea level, position of principal locations with respect to the (?) meridian and the perpendicular to it.

Configuration of the land, river routes, alluvial deposits, natural forms of the terrain, accidental forms, contour lines, heights of remarkable points

Rivers, tributaries, their sources, speed of the flowing water, yearly variations, lowest level of waters

Floods, extraordinary subsistence of water

Nature of soil, fossils, mineral water, enumeration of mineral substances and their use, quarries, catacombs

State of the atmosphere, air temperature, regions of low air pressure (lieux profonds), barometric observations, hygrometry

Winds, clouds, rain, snow, hail, dew, northern lights, magnetic effects

Flora, native and cultivated plants

1.2. Political description

Administrative, ecclesiastic; legal, military delimitation, diverse provinces

Consecutive and actual territory, (frontier?) posts, previous division of territory

Main roads, bridges, stretches of hauling, ferries, local and rural roads, channels, springs, sewers, culverts, distribution of waters, public and private use, navigation

II. Population

2.1. General state of population

Total population, its division

Relations to territory, distinction with regard to sex, marriage, housing. Number of fires

General comparison of results

Procedures to be followed when taking censuses

Earlier counts

2.2 Yearly movement

Yearly births, deaths, marriages, children born out of wedlock, and abandoned children

Causes of deaths, common diseases

Vaccine [vaccination?]

Study of the movement of population during the previous century

Comparison of results

2.3. Buildings, streets, localities

Designation of streets, places, crossroads, quays, bridges, passages, yards, enclosures

Number of houses, manner of numbering the houses, stairs, halls, slaughterhouses, city road systems

Guardhouses

Palaces, law courts (magistrates?) [tribunaux], churches, catacombs, monuments, hospitals, hospices, colleges, public establishments, prisons, barracks, theatres, large hotels, institutions (?), boarding establishments, furnished hotels

2.4. Professions and circumstances of situation

Political rights, electors, eligibility

Enumeration of occupations and circumstances

Number of people who profess various cults

Frenchmen with no fixed abode, military men, foreigners

Number and distinction of domestic workers

III. Civil institutions

3.1. Government

Public functions, see official annuals

3.2. Administration

Administration, see official annuals

Public documents

Administration of postal and telegraphic service

Games (of chance), lotteries (which are also games of chance)

Healthiness

Illumination (of towns)

Fires, accidents, public security, road systems

Cemeteries

Theatres

Day nurseries [bureau des nourrices]

Hackney carriages

Various objects

3.3. Legal order

Functions and occupations in the legal agencies, see official annuals

Civil courts

Courts of commerce

Justices of the Peace

Insolvencies, bankruptcies

Criminal courts; number and distinction of causes (of crime)

Prisons, remand homes, detention centres, expropriations, convictions, executions

3.4. Status of the military. Law enforcement agencies

The military

National guard

Gendarmery

Recruitment, random selection of recruits [taille], causes of reform, results of previous conscriptions

3.5. Religious establishments

3.6. Administration of public relief

Hospitals, special hospitals, hospices

Homes for old people, help at home, workhouses, relief workshops, free schools, number of the needy, (their) division by sex, occupation, age, place of birth

Comparison of the results of many years

Mendicancy

Abandoned children

Administration of hospitals and hospices

General expenses, number and fares for travelling (or) movement (?), military diseases

3.7. Arts, sciences, education

Academies, universities, colleges, conservatoires, institutions, boarding schools, schools

Number of trained children and their distinction by age, sex, occupation, kind of training, progress of elementary education

Museums, libraries, botanical gardens

Rooms of natural history, mineralogical collections

Physical rooms, rooms of medals

Collections of specimen [modèles], conservatoires of arts

Various collections (of?) newspapers, periodic editions

Productions of fine art

Literary societies

History, morals (manners), customs, antiquities

Archives, inscriptions, historical monuments

3.8. Various results

State of consumption, various foodstuffs, other objects of usual use, fare for travelling to workplace

Corn in reserve

Comparison of consumption with population

Wages of domestic workers

Interest on money, discounts, banks

Objects liable to excise duties, right of entry, kinds and quantity of raw materials [matières]

Pawn offices, savings banks

Tontines

Distinction of population, heads of families, immovable and movable property; functions (?), employment, industry, activity, needs

IV. Agriculture

4.1. Sources and products

Extent of agriculture, territorial division

Proceeds, rural employment

Woolly-haired, horned animals, horses, pigs, birds, bees

Epizootics

Veterinary schools, agricultural establishments

4.2. Estimation of produce

Sowing, work, expenses, yields, leases, rent (of farms), products of nature, evaluation in money

Subsistence of farmers

Forage

Preservation of harvest

Agricultural housing and constructions

V. Industry and commerce

5.1. Manufactures and factories

Enumeration of them; aims, places, owners, kinds and quantity of used raw materials [matières], wherefrom they came, their price

Number and distinction of workers, fares for travelling (to work), various expenses (whose?)

Quantity of products, places to which they are sent and used up

Progress or variations of establishments

General comparison of results; division of manufactures and factories from differing viewpoints; progress, variations

Factories, enumeration, description

5.2. Commerce

General enumeration, distinction, evaluation, import and export, consumption, navigation

Comparison of general results

Public establishments; fairs, markets, progress, variation, history

5.3. Handicrafts

Enumeration, distinction, number of inhabitants (!) in each of the main occupations

Distinction by sex, age, kind of work; owners [maîtres], workers and their distinction, apprentices

Application of various occupations

General remarks, comparison of occupations with respect to various viewpoints

Public establishments

VI. Finances

6.1. Property and rights of the state

Enumeration, distinction, evaluation

Administration

6.2. Contributions

Direct: distinction, evaluation; receipt (of taxes); land registry; general results

Indirect: distinction, evaluation; receipt (of duties etc.), general results

6.3. Revenue of towns

Enumeration, distinction, evaluation

Receipt, use of

Commentary

1. Some items are unclear and there are a few repetitions

2. Fourier pays due attention to the comparison of the results in time (not in space although it was perhaps possible to compare Paris with London).

3. Strangely enough, up to the 20th century population statistics did not pay attention to diseases or even to epidemics. For Fourier, cholera and smallpox had not anymore been a horrible threat, but other epidemic diseases were still rampant. Nevertheless, he mentioned epizootics (§ 4.1) but not epidemics.

4. Understandably, Fourier (§ 3.8) was interested in the wages of domestic workers (home help), but not of the workers in factories or field.

5. Fourier included mineralogical collections (§ 3.7) in statistics, and so did J. B. J. Delambre in 1819 (Sheynin 2017, p. 174). So when were they replaced by mineral resources?

6. How did Fourier follow his pattern in his monumental *Recherches statistiques*? I doubt that they were ever thoroughly analysed and have no answer.

7. Schlözer (1804), Moreau de Jonnès (1847) and some inferences. Schlözer was an eminent scholar but his book is hardly satisfactory and I think that he had not highly valued it. See Introduction to its translation. In particular, his pithy saying

History is statistics flowing and statistics is history standing still is senseless since (as even Leibniz advocated) statistical data ought to be compared in time and space. Note however, that, unlike some of his followers, Schlözer had not considered his saying as a definition of statistics.

What had nevertheless transpired, and was definitely stated by Moreau de Jonnès in his chapter 2, was the need to classify the objects of statistics according to a definite pattern. He himself studied the statistics of France under the following heads:

territory – population – agriculture – industry – home and foreign trade – navigation – colonies – public administration – finances – military forces – justice – public institutions – main cities

What had also become clear was that reasonable classification of data and its study and relations of statistics with other sciences was (sometimes perhaps intuitively) understood as the theory of statistics.

Schlözer A. L. (1804), *Theorie der Statistik*. Göttingen. **S, G,** 86.

Moreau de Jonnès (1847), *Eléments de statistique*. Paris. **S, G,** 58 (only chapters 1 and 2).

Cluster of anniversaries, pt. 2

Oscar Sheynin

Abstract

Pascal's treatise on the arithmetic triangle described the separate findings made mostly in the 16^{th} century. It showed how to apply the triangle to the theory of figurate numbers and combinatorics. Bayes completed the first version of the theory of probability and possibly considered that it belonged to pure science. Laplace's *Essai* was a barely successful popular treatise but it included interesting side issues such as the natural scientific study of moral sciences, psychology, and final causes. De Morgan was the first to note the normal distribution in De Moivre but was considered a logician rather than a mathematician. For 150 years Todhunter's history of probability has remained a necessary and useful source of information.

1. Introduction

As a continuation of my previous efforts (2014), I describe noteworthy mathematical contributions whose anniversaries occur in 2014 and 2015.

2. Blaise Pascal

Pascal (1623 – 1662) is best known for his correspondence of 1654 with Fermat which originated the theory of probability. He was also the author of the *Treatise on the Arithmetical Triangle* (1665, posthumous) written in 1654. Pascal included in his treatise separate tracts describing the use of the triangle in the theory of figurate numbers, the theory of combinations, the solution of the classical problem of points in games of chance, and the derivation of the powers of binomials. The main commentator on that work is Edwards (2002).

Pascal (1665) became extremely influential in spite of the earlier findings of many scholars (Al-Tusi, in 1265 and Apianus, Stifel and Tartaglia in the 16^{th} century) who, taken together, had described the entire content of the Pascal (1665), except for the problem of points. Another predecessor, Levi ben Gerson, had introduced the method of mathematical induction, but it was Pascal who made it known. Then, Hald (1990, p. 49) noted that Pascal, this time without precursors, solved a partial difference equation. About 1655 Pascal turned from mathematics to religion, which possibly explains some methodical imperfections of Pascal (1665).

3. Thomas Bayes

See Sheynin (2003), (2010) for a description of his achievements and emphasized that he completed the first version of the theory of probability by studying the precision of the inverse law of large numbers. Some authors who failed to note that study are: Meyer (1879, Chapter 7, § 91), Chebyshev (1936, p. 192) and Laplace (1812,

p. 285 of the 1886 version). They provided the formula of the inverse law of large numbers, but of these only Meyer mentioned Bayes, although neither he, Chebyshev nor Laplace noted its lower precision. Bayes, but not his result, was also mentioned later by Laplace (1814, p. 120 of the translation of 1995).

Meyer (1879) originated from his lectures of 1849 – 1857 and was translated by an eminent scholar, E. Czuber. Likewise, the lectures of Chebyshev (1936), as written down by his student Liapunov, constituted the text of his book. Chebyshev lectured on probability from 1860.

Todhunter (1865, p. 295) noted the existence of Bayes' pertinent companion memoir of 1765, whereas Dale (1999) somehow managed to ignore that memoir, as he also did in the bibliography that he appended to his translation of Laplace's *Essai* (see Laplace (1814)).

My second and last point is that Bayes seems to have regarded the theory of probability as a branch of pure science. Laplace, however, had pointed probability in the applied direction leading to one of many similar remarks made by Poisson (1837, § 84): "There exists a very high probability that these unknown chances very little differ from the ratios …". Chebyshev and his students, Markov and Liapunov, turned probability towards pure science, but for a long time their views had been hardly listened to. Much later, Kolmogorov's breakthrough met with fierce opposition (Doob 1989): "Some mathematicians sneered that … perhaps probability needed rigor, but surely not *rigor mortis* …"

But what about Jakob Bernoulli and De Moivre? The new science certainly needed to justify itself, and Bernoulli stated that, unlike his great theorem, "the most important part" of his *Ars Conjectandi*, "the application of the art of conjecturing to civil, moral and economic issues", was not yet written. See his letter to Leibniz of 3 Oct. 1703 (Kohli 1975, p. 509).

De Moivre defined the aim of the doctrine of chances as "estimating how far some sort of Events may rather be owing to Design than Chance". See his dedication of the first edition of his book to Newton (De Moivre 1756, p. 329). Although rigorously demonstrating his theorems, De Moivre thus thought that his probability was a general applied scientific discipline.

4. Pierre-Simon Laplace

In Sheynin (2014), the author described Laplace's work but only said a few words about his *Essai* (Laplace 1814). Now, I dwell in some detail on Andrew I. Dale's translation from its last edition of 1825 published during Laplace's lifetime. Dale meticulously noted the differences between the editions of 1814 and 1825, added many commentaries of his own and lengthy pertinent quotations from various authors. Also added is a bibliography of about 250 items often restricted to first editions, and a glossary but no indices.

Dale made some mistakes. Nicolas Bernoulli did not edit the *Ars Conjectandi* of his late uncle; the dates of some sources in his bibliography are wrong; commentaries on the Petersburg problem or the Daniel Bernoulli – Laplace urn model do not mention modern developments; and, finally, his explanation of the terms *repeating circle* and *triangulation* prove that he is ignorant of geodesy.

The *Essai* originated from a lecture of 1795 at the *Ecole Normale* and remained a source for educated readers, especially mathematicians and astronomers, the more so since formulas were only described by words which made many pages hardly readable. Barely dwelling on subjects treated in the *Théorie analytique*, I describe other topics pertaining to probability, statistics and other fields of knowledge.

Laplace discussed his achievements in the natural sciences and astronomy in particular. Suffice it to say that he proved that the stability of the Solar system was durable, and completed the explanation of the motion of its bodies by the law of universal gravitation.

Laplace (1814, see p. 4 of the translation of 1995) stated that the aim of the calculus of probability was to determine the probability of various events, a view which seems similar to that of De Moivre (see my § 1). Then, on pp. 6 – 14, Laplace lists 10 *general principles* of the calculus, – principles, never theorems. Thus, the simplest forms of the addition and multiplication *theorems* are lacking. Next follow analytical methods of the calculus, hardly understandable owing to the lack of formulas. The theoretical content of the book contains only one more topic: the treatment of observations. Laplace (Ibidem, p. 121) stated that Legendre and Gauss had introduced the principle of least squares, without a single additional word about Gauss! In 1823 Gauss had published his main memoir on the combination of observations, but Laplace (Ibidem, p. 45) missed the apparent opportunity to improve his statement about the weight of the mean of observations. He hardly thought about systematic effects when actually stating that the worth of an observational result is determined by random errors as stated in his opinion on p. 46, repeated almost word for word on p. 65.

I shall now take up other topics in the Laplace's *Essay* (Ibidem).

"Final causes always disappear on a deeper examination" (Laplace, Ibidem, p. 41). This is what happened in population statistics, although not straightforwardly. The main proponent of final causes in that field was Süssmilch. He compiled one of the chapters of the second edition of his *Göttliche Ordnung* (Divine Order!) (Süssmilch 1761) together with Euler, and its entire context testifies that the deeply religious Euler shared Süssmilch's views about the Divine laws of population.

However, the Divine command to multiply and subdue the Earth encountered great difficulties. In 1740, Struyck (Pearson 1978, p. 337) "apparently" thought that, "while his Creator would not approve of starvation for thinning humanity, he would have no objection to plague or war". Indeed (Pearson quotes Struyck), "in this manner the number of mankind remains nearly stationary". This contrasts with Malthus's presumption.

Buffon never mentioned the Divine command although he managed to explain "scientifically" (Pearson's derisive comment (Pearson 1978, p. 190)) the 930 years of Methusaleh's age. Graunt left only one though not really important pertinent remark in his *Observations* of 1662 (Petty 1899b, p. 369) about the year 1660: “As if God almighty had caused” [its] “healthfulness and fruitfulness …”, and Halley, in both his papers of 1693, stuck to statistics. For Graunt’s classic see Petty (Ibidem, pp. 317 – 435).

Laplace, however, thought about astronomy. Mostly he refuted Newton’s statement (1704, Query 31) about regular divine reformation of the Solar system. Currently, the existence of final causes is studied in connection with the Big Bang.

The celebrated Pascal wager stated: If God does not exist, you may lead a life of sin; otherwise, however, you will lose eternity. Laplace (1814, see pp. 71 – 72 of the translation of 1995) was unsatisfied with that conclusion but only substantiated his opinion by introducing some mysterious witnesses who, “in the name of God, exaggerated their promises beyond all bounds” and thus destroyed their own evidence. Laplace was not an out-and-out unbeliever, but he is known to have said to Napoleon that he had explained celestial motions without introducing the *hypothesis* of God’s existence. Arnauld & Nicole (1662, see p. 334 of the translation of 1992) formulated a similar statement.

Laplace (1814, see p. 100 of the translation of 1995) turned to psychology claiming to having invented this term (although in 1732 Chr. Wolff published in Leipzig a book called *Psychologia empirica*) and argued that one should study its problems by the method “that has been used for observations of external senses”. On p. 107 he specifically stated that “the study of mathematics” and “the knowledge of probabilities” can “shed a great light on psychology”. In 1860, Fechner, working in the spirit of Laplace’s opinion, initiated psychophysics (Sheynin 2004).

Somewhat strangely Laplace (1814, see pp. 70 – 71 of the translation of 1995) denied *miraculous* cures rather than explaining them as extraordinary psychological events. To psychology belongs Laplace’s remark (p. 110) that “our belief depends on our habits” and that (p. 111) “the exaggeration of probabilities by the passions is a psychological principle”. Poisson (1837, § 60) extended these thoughts by examining deductions made from classical physical experiments and he quoted Hume as also did Hald (1998, p. 127): in 1739, Hume argued that it was “ridiculous to say that the next sunrise is only probable”. Thus originated the classical problem of the next sunrise!

“Let us apply to the political and moral sciences the method based on observation and the calculus …” (Laplace 1814, see p. 62 of the translation of 1995).

I doubt whether there exists a clear definition of *moral sciences*; perhaps they should be understood as studying human acts. *Moral statistics,* which emerged with Süssmilch and especially Quetelet, served as a tool for studying the moral sciences, or more specifically, acts of free will (crime, suicide, marriage, and so on). Its scope has been gradually widening, but I cannot agree with Landau & Lazersfeld (1978, p. 827, right-hand column) who equate it with sociology. I rather view moral sciences and therefore moral statistics as constituting a part of sociology.

In his *Essai*, Laplace (Ibidem) discussed testimonies, administration of justice, and tables of mortality. Dale noted that Pearson (1978, p. 694) had severely criticized the childish description of the method of compiling those tables (p. 81). On p. 102 Laplace reasonably remarked that "publicity of crimes is not without danger". Then, Laplace (p. 89) compared "free people" to an "association whose members mutually protect their property" and went on to praise "institutions based on the probabilities of human life". So far, so good, but perhaps up to the mid-19th-century insurance had been close to depending on fraud.

In a letter of 1742 Daniel Bernoulli (Fuss 1843b, p. 496 of the 1968 version) stated that mathematics (possibly probability) could be applied in politics. An entirely new science would thus originate, he continued, if only as many observations were made in politics as in physics. Bernoulli also mentioned that Maupertuis had agreed with him.

In his attack on Poisson, Poinsot (see Poisson 1836, p. 380) violently opposed Laplace's advice but unwisely cited another of Laplace's statements to the effect that the theory of probability was very delicate. A bit later, all the (French) schools of thought united in ridiculing Poisson (1837) for stochastically studying the administration of justice, – "naturally, without reading a single line" of that source (see Bru (2013, p. 355)).

Laplace's celebrated statement (1814, see p. 2 of the translation of 1995) to the effect that randomness will disappear in the presence of an all-powerful mind, could not have been upheld because of the existence of unstable motions, to say nothing about the recently discovered phenomenon of chaos. It is needless to add that Laplace did not reject randomness.

Laplace had used an excessive and thus misleading number of digits, as in the barometer "rises to 1.0563 millimetres …" (Laplace Ibidem, p. 56). Gauss calculated his measured angles to within 0.001 arc seconds, Karl Pearson also retained unnecessary digits, and at least once Fisher followed suit. (See a discussion of this subject in *Science*, vol. 84, 1936, pp. 289 – 290, 437, 483 – 484 and 574 – 575.)

5. Augustus De Morgan

Augustus De Morgan (1806 – 1871) was an eminent logician and was also believed to be a mathematician. In 1838 and 1845 he published popular essays on probability. Rice (2003) described De Morgan's merits in furthering actuarial science and his work on applying probability to logic, but concluded that De Morgan had later moved away to philosophy.

I am concerned with De Morgan (1864). He was the first to notice the appearance of the normal distribution in De Moivre's work, although only in the edition of 1738 of the *Doctrine of Chances* rather than in De Moivre's privately distributed note of 1733. Again, De Morgan provided the first attempt to generalize the law of error: he multiplied the exponential function by a polynomial ($p + qx^2 + rx^4 + \ldots$). Regrettably, there is more to say.

Here is De Morgan's statement (see De Morgan 1864, note on p. 421) see also (Sheynin 1995, p. 179): "The negative probability may no doubt be an index of the removal from possibility of the circumstances, or of the alteration of data which must take place before possibility begins. But I have not yet seen a problem in which such interpretation was worth looking for. I have, however, stumbled upon the necessity of interpretation at the other end of the scale: as in a problem in which the chance of an event happening turns out to be 21/2; meaning that under the given hypothesis the event must happen twice, with an even chance of happening a third time".

Also, in the beginning of the 1850s Boole had informed De Morgan that his solution of a problem was wrong since it involved a probability of 4/3 (see Rice 2003, p. 303)).

Worse is to follow! In a letter of 1842 to John Herschel (see Sophia De Morgan 1882, p. 147), whose answer I did not find, Augustus De Morgan declared that $\sin \infty = \cos \infty = 0$, $\sec \infty$ and $\operatorname{cosec} \infty$ are likely zero, and $\tan \infty = \cot \infty = \mp\sqrt{-1}$. The first two equations recall Leibniz's reasoning about the sum of an infinite set of terms 1, – 1, … being equal to 1/2, the statement about the secant and cosecant are surprising and the last equalities are a mystery.

6. Isaac Todhunter

Kendall (1963) had briefly described Isaac Todhunter's (1820–1884) life and work. Apart from an unfinished *Treatise on Elasticity* completed by Pearson, Todhunter left books devoted to the history of three branches of mathematics (calculus of variations; theories of attraction and the figure of the Earth; and probability). He also compiled many noteworthy textbooks, in particular ten out of the 14 volumes provided for the use by the India Civil Service's examiners.

I am familiar with Todhunter's book (Todhunter 1865). Kendall correctly stated that Todhunter was "so meticulous in his attention to detail and so blind to the broad currents of his subject", but that his *History of Probability* "has stood for nearly a hundred [now, for 150] years without an imitator or a rival, and we are all indebted to it".

Attention to detail (see just above) includes description of the work of lesser known authors about whom we would be barely able to find any information. One such author is the eminent E. Waring whose main achievements were in algebra and number theory. It is of interest that Waring (see Todhunter 1865, p. 618)), without however justifying his method, also applied probability for ascertaining the number of imaginary roots of equations.

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Bortkiewicz' Alleged Discovery: the Law of Small Numbers

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Abstract

Ladislaus von Bortkiewicz (1868 – 1931) published his law of small numbers (LSN) in 1898. The name of that law was unfortunate; moreover, lacking any mathematical expression, it was only a principle. Many commentators described it, but my paper is the first ever attempt to examine it thoroughly, and I argue that Kolmogorov's unsubstantiated denial of its worth is correct. For a few decades the law had been held in great respect and thus deserves to be studied.

Introduction

I begin with a short description of statistics in the second half of the 19th century and the beginning of the 20th century and introduce my main heroes; in conclusion, I describe here the preparation of Bortkiewicz' booklet on the LSN, quote his definitions of that law and discuss its name. Debates around the LSN took place in the early 20th century, and it is opportune to mention that by that time the Continental direction of statistics became established, and that Bortkiewicz believed that his law strengthened the Lexian theory, or, in other words, essentially contributed to that direction. Actually, however, he was gravely mistaken; the LSN, never expressed in a quantitative, mathematical way, was deservedly forgotten, but it certainly turned general attention both to the Poisson distribution and to the Lexian theory.

1. Statistics in the Second Half of the 19th Century

The most eminent statistician of that period until his death in 1874 was Quetelet (Sheynin 1986; 2001a, § 3). His field of work was population and moral statistics; he did not try to apply the statistical method in biology. In that latter direction he could have preceded the British biometricians, but his religious feelings prevented him from studying Darwin whom he never mentioned.

Quetelet had introduced elements of probability theory into his moral statistics (inclinations to marriage and crime), and after his death German statisticians, without understanding that a statistical indicator did not apply to any given individual, rejected his approach as well as his alleged denial of free will. The same happened with Quetelet's belief in stability of crime under invariable social conditions (his forgotten reservation). However, a correct understanding of the dialectic of randomness and necessity together with the Poisson form of the law of large numbers would have dispelled that conclusion (if formulated in terms of mean values).

Coupled with the general refusal to accept any probabilistic pattern excepting (not at all universally) Bernoulli trials, the situation became deplorable (Sheynin 2001a, § 5.3). The problem of testing the invariability of statistical indicators (naturally extended to cover those concerning vital statistics) became topical. Here is how Chuprov's former student (see §1.5) described the situation:

Our (younger) generation of statisticians is hardly able to imagine that mire in which the statistical theory had gotten into after the collapse of the Queteletian system, or the way out of it which only Lexis and Bortkiewicz have managed to discover[1].

1.1. Emile Dormoy. The first to advance along the new road was the French actuary Dormoy (1874; 1878), but even French statisticians had not at the time noticed his theory, his discoverer happened to be Lexis (Chuprov 1959, p. 236). To specify: they barely participated in the development of the Continental direction of statistics (Keynes 1973, p. 431). Later Chuprov argued that the Lexian theory of dispersion should be called after Dormoy and Lexis (Chuprov 1926, p. 198, in Swedish; 1960, p. 228, in Russian; 2004, p. 78); however, Lexis achieved much more, and in addition it was his work that had been furthered by Bortkiewicz and Chuprov.

Bortkiewicz described the work of Dormoy and ranked him far below Lexis (Bortkiewicz 1930). In particular, he strongly opposed Dormoy who had decided that man, at least in large numbers, was subject to the "laws of fatality" (Bortkiewicz 1930, p. 44). I do not agree with him, nor do I understand how can the Lexian theory or his general views deny Dormoy's conclusion[2].

1.2. Wilhelm Lexis (1837 – 1914). He studied law, mathematics and natural sciences, but eventually turned over to social sciences and economics. He taught at several universities and became actively engaged in editorial work. From 1875 onward Lexis seriously contributed to population statistics, attempting to base it on stochastic considerations and thus advanced to the first rank of theoretical statisticians (Lexis 1903).

Bortkiewicz published a long review of Lexis (1903) intended for non-mathematical readers and described the latter's investigation of the stability of the sex ratio at birth, his statistical achievements in general, and his theory of stability of statistical series (Bortkiewicz 1904a). Much later he devoted two more papers to Lexis (Bortkiewicz 1915a; 1915b). The first one was the text of his oration on the occasion of Lexis' jubilee; Lexis, however, died soon afterwards. The second paper, which appeared in the *Bulletin* of the International Statistical Institute, was an obituary, and there, strangely, the author had in essence said nothing about Lexis the statistician. Still, in the

[1] "Unsere (jungere) Generation der Statistiker kann sich kaum jener Sumpf vorstellen, in welchen die statistische Theorie nach dem Zusammenbruch des Queteletschen Systems hineingeraten war und der Ausweg aus welchem damals nur bei Lexis und Bortkiewicz gefunden werden konnte." (Anderson 1963, p. 531).

[2] The only source describing Dormoy's life and work, which I was able to establish, was mentioned by Chuprov (1959, p. 236): A. Paolini, an article in the *Archivio di Statistica* for 1878, and it proved unavailable. Chuprov had not given the title of Paolini's article.

first case, disregarding biometricians, he credited his teacher with a "new founding of a theory of statistics"(Bortkiewicz 1915a, p. 119).

Finally, Bortkiewicz stated that Lexis' most important merit was not the introduction of Q, of his measure of stability of a statistical series, but the discovery that [assuming independent trials] it was never less than unity and depended on the extension of the "field of observation" (Bortkiewicz 1930, p. 40). I choose to say that his most important innovation was the introduction of a more general random variable into statistics.

1.3. Ladislaus von Bortkiewicz (1868 – 1931). He was born into a distinguished Polish family in Petersburg and graduated there as a lawyer but became interested in statistics and economics and achieved worldwide recognition in both these fields. Since 1890 Bortkiewicz published serious work on population statistics, worked under the direction of Lexis in Göttingen and defended there his doctoral thesis. His German was perfect; it probably had been spoken at home and been the main language in his *gymnasium*. Most of his publications are in that language.

In 1901, on Lexis' recommendation he was appointed Professor at Berlin University, and there, in Berlin, he lived all his remaining life becoming ordinary professor in 1920. His style was ponderous, his readership tiny, partly because German statisticians (and economists) had then been opposed to mathematics. Many authors deservedly praised Bortkiewicz for his scientific work. Thus, he was called *The statistical Pope* (Woytinsky 1961, pp. 452 – 453), and Schumacher explained Bortkiewicz' attitude towards science by a quotation from the Bible (Exodus 20:3): *You shall have no other gods before me* (Schumacher 1931, p. 573)[3].

Bortkiewicz (and Chuprov) furthered the Lexian theory by determining the expectation and variance of its measure of stability, Q, a problem Lexis himself had not even hinted at, and Chuprov had also essentially specified (and greatly restricted the usefulness of) the conclusions of the theory.

The spelling of his name changed from Bortkevich (in Russian) to Bortkiewicz (in German).

1.4. Aleksandr Aleksandrovich Chuprov (1874 – 1926)[4] Born in provincial Russia as a son of an eminent "non-mathematical" statistician, he became a mathematician with an eye to applying it to social sciences. He taught statistics in Petersburg and became Professor after defending his second thesis in 1908; the first one he defended in Germany in 1902. Under the influence of Markov with whom he corresponded for several years, Chuprov really turned to his initial goal although even much earlier he expressed himself as a partisan of Lexis and Bortkiewicz (Chuprov 1905). True, there also he wrongly stated that Bortkiewicz had rigorously justified the LSN (Chuprov 1905, p. 467).

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[3] For his biography see Gumbel (1968) and my own paper based on archival sources (Sheynin 2001b). An almost complete bibliography of his works is in Bortkevich & Chuprov (2005). Much information about Bortkiewicz, also based on archival sources, is in my book Sheynin (2006).

[4] See Sheynin (1990/1996 and 2011).

Emigrating in 1917, Chuprov finally settled in Leipzig (Germany) as an independent researcher and died after a long illness in Geneva having lived there for a short while as a guest of an old friend.

Chuprov prepared many gifted statisticians. One of them was Oskar Anderson, a Russian German who emigrated in 1920 and became the leading statistician first in Bulgaria, then in (West) Germany. For about 30 years Chuprov corresponded with Bortkiewicz. I published their extant letters in their original Russian (Bortkevich & Chuprov 2005).

1.5. The Two Branches of Statistics. Lexis became the founder of what became called the Continental direction of statistics, whose forerunners were Bienaymé and even Poisson (Heyde & Seneta 1977, p. 49). In England, the periodical *Biometrika* appeared in 1902 with a subtitle *Journal for the Statistical Study of Biological problems*. Its first editors were Weldon (who died in 1906), Pearson and Davenport "in consultation with Galton". Pearson became the head of the *Biometric school.*

For a long time the two branches of statistics had been developing almost independently; moreover, the contributions published in *Biometrika*, for all their importance, were being dismissed on the Continent since they were usually of an empirical nature lacking stochastic support, see Sheynin (1996, pp. 120 – 122/2011, pp. 149 – 150). In particular, I have quoted there Chuprov and Kolmogorov (who described the traits of the *Biometric* school):

Not "Lexis against Pearson" but "Pearson cleansed by Lexis and Lexis enriched by Pearson" should be the slogan of those, who are not satisfied by the soulless empiricism of the post-Queteletian statistics and strive for constructing its rational theory[5]

Notions of the logical structure of the theory of probability, which underlies all the methods of mathematical statistics, remained at the level of eighteenth century (Kolmogorov 2002, p. 68).

Some essential findings of the Continental direction had been independently discovered in England; thus, there exists a connection between the application of Q^2 and the chi-square method and analysis of variance (Bauer 1955). And it is opportune to mention Chuprov, whose important results only recently became sufficiently known (Seneta 1987).

The two last-mentioned commentators had not, however, aimed at a comprehensive study of the merging of the two branches of statistics into a single entity, but, anyway, the LSN had not helped in that process. For that matter, Bortkiewicz, contrary to Chuprov, had not recognized any merits of the Biometric school (Bortkiewicz 1915c).

2. Stability of Statistical Series (Lexis)

[5] "Nicht 'Lexis gegen Pearson', sondern 'Pearson durch Lexis geläutert, Lexis durch Pearson bereichert' sollte gegenwärtig die Parole derer lauten, die, von der geistlosen Empirie der nachqueteletischen Statistik unbefriedigt, sich nach einer rationellen Theorie der Statistik sehnen". (Chuprov 1918 – 1919, 1919, pp. 132 – 133).

In his main contribution on statistical series, Lexis considered various types of statistical series (Lexis 1879). For my purpose, it is sufficient to mention series whose terms corresponded to a variable probability of the occurrence of the event studied. In other words, he abandoned the assumption of a random variable with a constant binomial distribution, – abandoned Bernoulli trials.

Suppose (my notation here almost coincides with Bortkiewicz' of 1898) that the observed proportions of successes in σ sets of trials, the result of each trial being based on *n* observations, are

$$p'_1,\ p'_2, ..., p'_\sigma$$

corresponding to the *true* probabilities p_i. Their variance can be estimated indirectly:

$$\varepsilon_1{}^2 = \frac{\overline{p}'\overline{q}'}{n}, \overline{q}' = 1 - \overline{p}'$$

where $\overline{p}'$ is the mean of $p'_1,\ p'_2, ..., p'_\sigma$, whereas the direct estimate of the variance is

$$\varepsilon_2{}^2 = \frac{\sum_{i=1}^{\sigma} (p'_i - \overline{p}')^2}{\sigma - 1}.$$

Now, Lexis introduced a measure of the stability of a series, the *coefficient of dispersion*,

$$Q = \varepsilon_2/\varepsilon_1,$$

perhaps choosing the letter Q in honour of Quetelet. He called stability supernormal, normal or subnormal for $Q < 1$, $Q = 1$ and $Q > 1$ correspondingly. In the third case, as Lexis stated, the probabilities p_i underlying the different terms of the series were different; in the first case, the terms had to be somehow interdependent, whereas Bernoulli trials (independence of terms and constant probability p_i of the event studied) had only taken place if $Q = 1$. Bortkiewicz, however, noted (without supplying a reference) that Lexis had not discovered any supernormally stable statistical series (Bortkiewicz (1904a, p. 240), and Lexis had indeed restricted his attention to subnormal stability (Lexis 1879, § 10).

His conclusion about the three possible values of Q, based on common sense, seemed correct, but, mostly as a result of Chuprov's later and quite forgotten work, hardly anything was left from his theory (Chuprov 1918 – 1919; 1922b; 1926). Nevertheless, Lexis became the founder of what became called the Continental direction of statistics, – the study of population statistics by means of stochastic patterns, – whose forerunners were Bienaymé and even Poisson (Heyde & Seneta 1977, p. 49).

But how, in Lexis' opinion, did the probability vary? No universal answer was of course possible; nevertheless, he could have been more definite on that point. As it occurred, he thought that the variations followed a normal law (Lexis 1876, pp. 220 – 221 and 238), but then he admitted less restrictive conditions (evenness of the appropriate density function, – which is a later term) and noted that it was senseless to introduce more specific demands (Lexis 1877, § 23). Finally, he discussed "irregular waves" of variability (Lexis 1879, § 23). Bortkiewicz had not commented on this point. At the same time, Lexis made a common mistake by believing that the relation between the mean square error and the probable error remained constant (and equal to its value for the normal law) irrespective of the relevant distribution.

Concerning his first-mentioned pattern of variability, Lexis could have possibly attempted to apply somehow Newcomb's introduction of a mixture of normal distributions with randomly appearing different variances and zero parameters of location as an adequate law of error for long series of astronomical observations (Newcomb 1886; Sheynin 2002, p. 149). True, his suggestion was hardly practical since it demanded additional calculations and a subjective choice of the variances, of the number of terms in the mixture and of the probabilities with which each of these laws occurred, but at least it was possible for Lexis to heuristically support his research by that innovation. Apparently, however, neither he, nor Bortkiewicz had known about it.

3. The Law of Small Numbers (Bortkiewicz 1898)

3.1. Its Appearance, Definition and Name. Bortkiewicz had been preparing his publication for at least two years[6]. During that period Chuprov the mathematician helped him with his mathematics and advised Bortkiewicz to refer to Poisson[7].

Bortkiewicz twice defined the LSN:

> It turned out that the fluctuations found in the investigated series almost entirely corresponded to the predictions of the theory, which is precisely what constitutes the law of small numbers[8].

> … we may well call the fact, that small numbers of events (out of a very large numbers of observations) are subject to, or tend toward a definite norm of fluctuation, the law of small numbers[9].

These definitions describe a principle rather than a law.

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[6] See the first letters in (Bortkevich & Chuprov 2005).

[7] Letter No. 2 dated 1896, ibidem.

[8] "Es ergab sich, dass die bei den untersuchten Reihen gefundenen Schwankungen den Voraussagungen der Theorie fast vollständig entsprechen, worin eben das Gesetz der kleinen Zahlen besteht." (Bortkiewicz 1898, pp. VI).

[9] "die Tatsache, dass kleine Ereigniszahlen (bei sehr großen Beobachtungszahlen) einer bestimmten Norm der Schwankungen unterworfen sind bezw. nach einer solchen tendieren, das Gesetz der kleinen Zahlen wohl benannt werden". (Bortkiewicz 1898, p. 36).

Many authors, beginning with Chuprov and Markov, objected to the name itself, *Law of small numbers*. Chuprov called it "tempting but deceptive" (Bortkevich & Chuprov Letter No. 2 dated 1896) and Markov "once more demanded" its change (Ibidem, Letter No. 27 dated 1897). Much later, after Bortkiewicz' death, authors of several obituaries suggested another name, *Law of rare events*, e. g. Gumbel (1931, p. 232), whereas Mises earlier recommended a more suitable but hardly practical term, *Law of large numbers for the case of small expectation* [of the studied event] (Mises 1964, p. 108n). He had not repeated this remark in his obituary published in a rare source (Mises 1932).

In the same letter of 1897 (above), Bortkiewicz indicated that his attempt to publish his booklet in Russian by the Petersburg Academy of Sciences had failed owing to its expected appearance in German. There also, he described his talk with Markov. I quoted him and I only repeat now that Markov

Considered the mathematical calculations [apparently, in a preliminary version of the booklet] correct, but did not dare pronounce his opinion concerning the work's scientific value since he believed that it belonged to statistics. (Sheynin 1996, p. 42/2011, p. 60).

3.2. Bortkiewicz (1898): Its General Contents. The booklet contained an Introduction, three chapters and three appendices. In Chapter 1 he introduced the Poisson limit theorem and explained related material applied in Chapter 3. Chapter 2 was devoted to checking the agreement of the Poisson formula with statistical returns in cases of rare events (suicides and fatal accidents, including the study of deadly horse-kicks, so beloved by commentators). Modern authors confirmed that the agreement was "remarkably good" (Quine & Seneta 1987, p. 173). I examine Chapter 3 separately.

In Appendix 1 Bortkiewicz derived the first few moments of the binomial distribution in his own way using only a few of them. In Letter No. 7 dated 1896 (Bortkevich & Chuprov 2005), he explained to Chuprov that "now" he consented "to Markov's demand, without, however, resorting to generating functions and successive differentiation". The rejected (and now standard) method was likely comparatively new; anyway, Bortkiewicz could have well applied it in addition to his own, the more so since he liberally used power series and integrals in his Chapter 1.

He had been avoiding advanced mathematical tools. Much later he stated that the rejected method "was similar to solving the equation $2x - 3 = 5$ by determinants" [which was quite impossible!] (Bortkiewicz 1917, p. III). Concerning economics, Schumpeter argued that that attitude prevented Bortkiewicz from rivalling such scholars as Edgeworth (Schumpeter 1932, p. 339).

In Appendix 2 Bortkiewicz discussed *solidary trials*, but only in later contributions did he name his predecessors, Bienaymé and Cournot[10], and neither had he mentioned his own paper (1894 – 1896).

[10] See Heyde & Seneta (1977, § 3.1) and Cournot (1843, § 117).

Such, as he explained, were trials, or events, connecting several people at once (one of his examples: a group of travellers)[11]. Chuprov and, later, another author, without mentioning Bortkiewicz, indicated the other version of solidarity, – the negative correlation of trials, see Chuprov (1959, p. 234) and Geiringer (1942, p. 58).

Bortkiewicz explained the new case by drawing each time several balls at once from randomly selected urns with differing content. He derived a formula which somehow showed that solidarity led to $Q > 1$[12]. Much later Bortkiewicz applied the case of solidary trials to counter Markov's criticisms. (Bortkiewicz 1923, pp. 17 – 18). It would have been better to discuss solidarity in the main text rather than in an appendix.

Appendix 3 is Bortkiewicz' table of the Poisson distribution with four significant digits. Soper discovered there rounding-off errors whereas its author not really properly blamed his sister for this shortcoming[13], see Soper (1914) and B&C, Letter No. 138 dated 1914. The Poisson distribution had been noticed previously. Cournot recommended to apply it in actuarial calculations and Newcomb, in 1860, actually applied it for determining the probability that stars, uniformly scattered over the sky, can be situated near to each other (Cournot 1843, § 182; Sheynin 1984, pp. 163 – 164). Nevertheless, it was Bortkiewicz who made the Poisson distribution generally known.

3.3. Bortkiewicz (1898, Chapter 3). Some formulas of § 13 of this chapter as well as some other expressions in subsequent sections contain *n*, the constant number of trials but he did not tell the reader that it meant the number of trials applied to calculate any term of the

[11] Solidary action had been known in the treatment of observations as systematic errors (much later term) even to Ptolemy. Gauss thought that two functions with partly common observed arguments were not independent, and Kapteyn, in 1912, without mentioning him, even introduced the appropriate (but unnoticed) correlation coefficient (Sheynin 1984, pp. 187 – 189).

Another development in the same field concerning systematic errors was heuristically similar to applying the coefficient of dispersion (Helmert 1872, p. 274). The mean square error of measurement in triangulation can be computed during *station adjustment*, and after computing all the *conditional equations* corresponding to the chain. Such errors were present if the second estimate was larger.

[12] A modern derivation is due to Geiringer (1942).

[13] The unmarried Helene von Bortkiewicz. In 1935 she visited Aline Walras, the daughter of the late economist Léon Walras with whom Ladislaus von Bortkiewicz had been in correspondence (published by Jaffé in 1965). In one of her letters of 1935 to Jaffé Aline described that visit. Helene had been subjected to the "horrors" of the Russian revolution, but then [in 1918] with "great difficulties" managed to join her brother in Berlin (Potier & Walker 2004, p. 88). The Germans, as Aline continues, suffer "de la misère"; Helene herself is drawing a small pension and is "prudent when speaking about Hitler". "He is not as malicious as is thought, and there will be no war. He should not be considered an ogre! He is a lamb!"

I can only add that Ladislaus was a member of the German Democratic Party, but had not been at all interested in internal policy, see Tönnies (1932/1998, p. 319) and Schumacher (1931, p. 576).

statistical series studied. Bortkiewicz had indeed said so, but only later, and only two commentators noted this point[14].

Bortkiewicz' main formula (unnumbered, on p. 31) of Chapter 3 is

$$Q = \sqrt{1 + (n-1)c^2}$$

where c is a constant and Bortkiewicz naturally noted that Q decreased with n.

Several remarks are needed. First, the case of $Q < 1$, which was included in the Lexian theory[15], is here impossible since his Q differed from the Lexian coefficient, see below.

Later Bortkiewicz indirectly explained that in 1898 his main aim was to isolate the possible changes in the probability underlying a (number of) series (Bortkiewicz 1923, p. 15). Yes, he had isolated the influence of these changes (Quine & Seneta 1987), but, as it follows, had to abandon the case $Q < 1$. Second, and more important, it occurred that Q described not the desired magnitude, but rather the changes in n. Chuprov noticed this fact but only referred to Lexis (Chuprov 1959, p. 277). Bortkiewicz himself (1904a, p. 239) later stated that it did not at all follow

That we ought to keep to small numbers and prepare our statistical data accordingly. On the contrary, for the most part it is of greater statistical interest to ascertain the physical component of fluctuations which, with moderate numbers, remain blurred[16].

That component makes it possible to decide whether the underlying probability mentioned had changed.

Third, Bortkiewicz also introduced the Lexian coefficient denoting it Q' and stated, on p. 35, that it was approximately equal to Q. Later he noted that $EQ' = Q$ (Bortkiewicz 1904b, p. 833). Actually, as was readily seen from his formulas, an equality of that type held only separately for the appropriate numerators and denominators. Now, Q' was a fraction, and it was again readily seen that its numerator and denominator were mutually dependent. In such cases, as follows from a remark by Chuprov, the equality above does not necessarily hold (Chuprov 1916, p. 1791/2004, p. 40).

Bortkiewicz only admitted that the equality was not "fully rigourous" (Bortkiewicz 1918, p. 125n). This was an understatement:

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[14] See Bortkiewicz (1904b, p. 833), Newbold (1927, pp.492) and Bauer (1955, his formula (1)).

[15] Bortkiewicz remarked that the case should not be overlooked, that he arrived here at some "rather interesting results" and promised to acquaint Chuprov with them (Bortkevich & Chuprov 2005, Letter No. 135 dated 1914). I am unable to say anything else.

[16] "Dass man sich an die kleineren Zahlen halten und dementsprechend sich das statistische Material zurechtlegen soll. Es wird im Gegenteil meist eine größere materiell-statistisches Interesse haben, die physische Schwankungskomponente, die bei mäßigen Zahlen verschleiert bleibt, festzustellen." (Bortkiewicz 1904a, p. 239).

Chuprov subsequently devoted a paper to calculating the expectation of a ratio of two mutually dependent variables, and referred to Pearson's appropriate approximate formula (Chuprov 1922a), see Pearson (1897; 1910).

To repeat: 1) Bortkiewicz had only explained the meaning of *n* in a later contribution and, anyway, the coefficient *Q* did not describe the behaviour of the magnitude under study. 2) He had to abandon the case $Q < 1$. 3) Contrary to his statement, his coefficient Q' differed from the Lexian *Q*. Chapter 3 was not therefore satisfactory.

4. Discussions about the LSN

Chuprov listed four possible interpretations of the LSN, but the main point was the difference between its being the application of the Poisson theorem or a strengthening of the Lexian theory (Chuprov 1959, pp. 284 – 285). Then, in a letter to Markov of ca. 1916, Chuprov wrote that Bortkiewicz had been avoiding any discussion of the subject, and, in particular, did not comment on his (Chuprov's) statement above (Sheynin 1996, p. 68/2011, pp. 91 – 92).

More is contained in Bortkiewicz' Letters NNo. 93, 101 and 106 dated 1909 – 1911 (Bortkevich & Chuprov 2005). In the first of these, he only stated that the LSN ought to be understood as "the agreement between formula and reality". In the second one Bortkiewicz emphasized that his views had not changed since 1898 and that he really had in mind a small number of occurrences of the studied event rather than its low probability. He also remarked that "Strange as it is, we find it ever more difficult to agree about the general significance and understanding of the l. of sm. numbers". And, in the third letter: "It is wrong to infer that I understand the low value of *p* [probability] as decisive". Did this mean that he was prepared to abandon the Poisson theorem (and the first two chapters of his booklet)? Anyway, he stated that his law

Appears after all as the outcome of an extension of those Lexian investigations, and, in relation to theory, perhaps represents their conclusion[17].

Much later Bortkiewicz forcefully confirmed that his LSN was closely connected with the Lexian theory, – and unjustly denied the negative binomial distribution (Bortkiewicz 1915c, p. 256).

Markov was the first to criticize the LSN, at first privately, then publicly stating that a large *Q* was hardly possible when small numbers were involved[18]. Bortkiewicz himself later expressed the same idea but did not attach any importance to it (Bortkiewicz 1923, p. 17). Then, Bortkiewicz, even earlier than 1916, refused to agree that *Q* ought to be *shelved* (Bortkewich & Chuprov 2005, Letter No. 135 dated 1914).The context did not imply the denial of the LSM; I cannot

[17] "Erscheint nun als Ergebnis einer Weiterführung jener Lexis'schen Untersuchungen und bildet in theoretischer Beziehung vielleicht gar einen Abschluss derselben." (Bortkiewicz 1898, § 18, p. 38).

[18] See Ondar (1977, Letters NNo. 71 and 84 to Chuprov dated 1916) and Markov (1916).

explain Chuprov's suggestion, but this disagreement is rather interesting[19].

5. Conclusion

Many other authors had later expressed their opinions, directly or tacitly, about the LSN. Romanovsky, who later became a leading statistician and head of the statistical school in Tashkent approvingly mentioned by Kolmogorov, called the LSN "the main statistical law" (Romanovsky 1924, vol. 17, p. 15). Among other authors who praised the LSN I name Gumbel (1931; 1968) and Mises (1932).

This support was not, however, unanimous. Czuber several times mentioned Bortkiewicz's booklet but did not say anything about it (Czuber 1921). Anderson not quite resolutely questioned the practical importance of the law, and, much later, Bauer, who stated that his research had appeared owing to Anderson's wish, did not mention it at all, see Anderson (1961, p. 531) and Bauer (1955). Neither did Mises although he described the Lexian theory (Mises 1928; 1972). In 1932 (see above), being an author of an obituary, he possibly was too generous.

It was Kolmogorov who became the first to state bluntly that the LSN was just a name given by statisticians to the Poisson limit theorem, but he did not elaborate (Kolmogorov 1954). My own verdict is that the LSN had indeed turned attention both to the Poisson theorem, and to the Lexian theory, but proved to be hardly useful otherwise.

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Abbreviation: JNÖS = *Jahrbücher für Nationalökonomie und Statistik*

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[19] In Letter 6 of 1896 to Chuprov, Bortkiewicz admitted that his exposition of the LSN was not quite satisfactory (in what respect?) but that he will possibly publish some corrections, see translation in Sheynin (1996, pp. 41 – 42). He did return to his LSN, but failed to correct it. I have also included Bortkiewicz' remarks to the effect that Markov did not really understand anything in statistics except its purely mathematical substance (Sheynin 1996, pp. 42 – 43).

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Romanovsky's Correspondence with K. Pearson and R. A. Fisher

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Abstract

Eight letters from Romanovsky to Pearson (1924 – 1925) and 23 letters between him and Fisher (1929 – 1938) are published for the first time. The letters to Pearson were occasioned by Romanovsky's manuscript sent to *Biometrika* (it appeared there in 1925), by his attempts to continue publishing there and the overlapping of their findings. Among the topics discussed in Romanovsky's correspondence with Fisher were the latter's books, his *t*-statistics and field experiments. In a letter from Paris Romanovsky asked Fisher to help a Russian emigrant scientist. Letters 18 and 19 are already published (Bennett 1990, pp. 200 – 202).

1. Introduction

Vsevolod Ivanovich Romanovsky (1879 – 1954) was an outstanding mathematician and statistician. I refer to his works but in some cases the reader should consult the complete bibliography of his writings in Bogoliubov & Matvievskaia (1997), abbreviated as B&M. These authors described his life and work, and, in particular (p. 85), mentioned his acquaintance with Karl Pearson (which I am unable to confirm) and Ronald Aylmer Fisher. For my part, I make public his correspondence with these most eminent statisticians and specify or add the relevant bibliographic information[1] but omit unimportant everyday details. Two letters from Romanovsky's correspondence, both written in March 1931 on the role of prior distributions in formulas of the Bayesian type, are already published (Bennett 1990, pp. 200 – 202). The reader will notice that Romanovsky's English was very imperfect but understandable and that his terminology is now dated, see Note 8.

As a preliminary, I supplement B&M by a few words. In 1923, Chuprov became interested in Romanovsky's work. Sometimes revealing there "rather large mistakes", he nevertheless at once made him out as a prominent scientist and entered into correspondence with him (Sheynin 1996, pp. 50 – 53/2011, pp. 70 – 73), cf. the beginning of Letter 23. I also noted (1996, pp. 40n and 96/2011, p. 166, Note 7.13, and p. 121) that, at the beginning of his scientific career, Romanovsky had certainly overrated the Bortkiewicz law of small numbers and stressed the natural-scientific essence of the law of large numbers and invariably called it a physical law [21, p. 18; 22, book 1, p. 127].

Romanovsky's ties with Western statisticians were not at all restricted to correspondence. He published many important papers in Europe one of which [11] served as a point of departure for E. S. Pearson and Neyman. Even after several decades, the former did not forget to testify to this (E. S. Pearson 1966). In 1939, on the occasion of his jubilee, the Central Asian University in Tashkent, where Romanovsky was working since its establishment in 1918 until his death, published a collection of papers written by many most eminent

Soviet and foreign mathematicians and statisticians in his honour (Zbornik 1939).

Then, Romanovsky published four reviews of Fisher's books. He [14] described in detail Fisher's *Statistical methods* (1934) justly calling it a "remarkable phenomenon" (p. 127) and indicated that it was already translated into Russian and published "as a manuscript in a small number of copies", cf. Letter 28. Nevertheless, an ordinary Russian edition appeared only in 1958 and even then the (state-owned) publisher accompanied it by a critical comment (p. 5). There, he accused Fisher of a "bourgeois narrow-mindedness and formality of views", disregard of the qualitative side of social phenomena etc. For that matter, in Russia, a combined work of mathematicians and sociologists was unheard of at the time.

In [13] Romanovsky indicated that the work of Fisher and his associates was based on experimentation of many years and predicted the importance of their main ideas. Soon Romanovsky [16] described Fisher's new book (1935), *Design of experiments*, and concluded that it deserved "greatest attention" (p. 125). It was not, however, translated. Finally, Romanovsky [19] reviewed the tables Fisher & Yates (1938). He called them valuable but added that they should be published in Russian in a revised and supplemented form. This, however, had not happened either.

From about 1927 the general situation in Russia, and certainly in statistics as well, sharply deteriorated. In particular, it became dangerous to cite Pearson favourably (Sheynin 1998)[2]. And it seems that even Fisher became suspicious. In addition to the above, I indicate an editorial note to Romanovsky's paper [9], see its p. 224:

The editorial staff does not share either the main suppositions of Fisher, who belongs to the Anglo-American empiricists' school, or Romanovsky's attitude to Fisher's constructions ...

I stress that, although the Anglo-American statistical school had indeed been empirical to a large extent[3], the only occasion for such an attack could have been the general directive to deny all the *bourgeois*. Thus, Maria Smit (1927/1930, pp. 8 – 9) absurdly accused Romanovsky (and L. K. Lakhtin) of considering random variables with permanent laws of distribution. That, she declared, contradicted both the spirit of Darwinism and the Engels dialectic … I (Sheynin 1998) have already cited that worthy troglodite. Now, I might add that she had no inkling of the difficulties connected with studies of such general densities (or, for that matter, of mathematics at all) and that she stated, in equally bad Russian, that "Engels' opinion retains its validness".

Even in 1938 Romanovsly [18, p. 17] nevertheless called Pearson the head of contemporary mathematical statistics; or, more precisely [17, p. 49], its co-creator (together with Galton). In the second case he added that "we also ought to name Fisher, Charlier and Chuprov"[4]. Fisher's subsequent findings advanced him to the very first place in statistics.

After World War II the Soviet authorities launched a new attack against mathematical statistics. In 1948, a Second All-Union Statistical Conference took place in Tashkent (Vtoroe 1948) and Romanovsky naturally became chairman of its organizing committee (B&M, p. 92). In his report, Kolmogorov (1948, p. 220) mentioned the "great" work done by Romanovsky and his school, but another speaker (Sarymsakov 1948, p. 222) blamed his teacher for "following in the Anglo-American direction". Moreover, the conference (Vtoroe 1948, p. 314) carried a resolution that indicated, without naming anyone, that there existed "servility and cringing to all foreign" and approvingly put on record that Romanovsky had admitted his previous ideological mistakes. The resolution also "decisively" condemned Nemchinov, with whom Romanovsky had been in correspondence (B&M, p. 93), for his active opposition to [the notorious humbug] Lyssenko.

The conference had a sudden consequence. Romanovsky published an unfortunate manual of error theory [20]. Like many other mathematicians and statisticians, he was not sufficiently acquainted with that subject[5] and no wonder that Chebotarev (1951) expressed reasonable criticism. However, he also recalled that Lenin had called Pearson a Machian and an enemy of materialism and he attacked Romanovsky (and the historian of astronomy Idelson) from the ideological viewpoint. Finally, Chebotarev formulated a few absurd remarks. Thus (p. 8), Romanovsky, like Mach and Pearson, only attempted to describe phenomena whereas Marx had established that the world should be changed rather than described …

Answering that lackey, Romanovsky [21, pp. 17 – 18] stated that Pearson's mathematics should not be lumped together with his philosophy, and that he, Romanovsky [18], supported the constructions of the Biometric school by a stochastic base and thus amended them[6]. Chebotarev (1953), however, repeated his accusations, although later on, likely being compelled by a somewhat improved general political situation, he (1958, pp. 571 and 586) began to recognize Romanovsky[7].

The correspondence below shows that Fisher held a high opinion of Romanovsky: not only did he describe his own work to his Russian colleague, he also expressed his desire to see Romanovsky's writings, even if in Russian (Letter 29). And Pearson published five of Romanovsky's papers in *Biometrika* and he certainly had to correct the English in each of these. Romanovsky's political inclinations are also felt in his correspondence: the GPU (more correctly, OGPU, the forerunner of the KGB), as he wrote (Letter 11), was "the most dreadfull and mightful organization in the present Russia". And my own experience, on my own scale, tells me that for him the correspondence should have certainly been a vent for fresh air.

After the notorious Conference of 1948 the correspondence ended. Perhaps Romanovsky was compelled to brake his relations with the West the more so since at that very time he became deputy of the Uzbek Supreme Soviet, as he informed Fisher.

I have translated B&M (**S, G,** 91) whish naturally has much more about Romanovsky. I omitted there the name of the Responsible Editor S. S. Demidov since he actually was a figurehead.

2. Romanovsky's letters to Pearson

Letter No.1, 18.12.1924

I send you with this letter a paper on the distribution of the means and standard deviations in samples of an arbitrary number from normal populations with one or two arguments[8]. I think that it can interest the readers of *Biometrika* for it contains the complete and rigid solution of some problems which are partly not completely solved and partly new, as I know. I beg you to pay attention to the following places of my work:

P. 7, formula (21): the generating function of the moments of stand. dev. for one variable, and formula (22) – their general expression.

P. 10, formula (33): the exact value of the mean error of stand. dev. in samples of number s. On the p. 11 I give a short table for $s = 2$ to 30 of the true values of probable error of st. dev.[9] and of approximate values, published in Your *Tables*.

P. 18, formula (44): the generating functions of the moments of means in samples of number s from normal population with two arguments; p. 19, form. (52): their general expression.

P. 28, form. (81): the generating function of the moments of the products of st. deviations in samples from the population with two variables.

P. 31, form. (87): their general expression.

P. 35, form. (101) and (102): the equation of the surface of distribution of st. deviations for the same case.

Many other results I do not mention. I have received them by a method which I discovered some months ago and which I have hold for new till now. When I had finished my work and some other investigations with these method I have received from Prof. Tchuproff some his papers and among them his note (1924) on the book of Mr. Soper (1922) which I do not know, for I could not get it till now. From this note of Prof. Tchuproff I knew that my method in some essential points is contained in the Mr. Soper's book. I do not know the content of this interesting book and can only suppose as far as I know it from the note of Prof. Tchuproff that my method differs from the method of Mr. Soper in some directions (for example, in a symbolical calculus of moments and in its applications to continuous distributions), which are not unimportant. However, I do not pretend much, I state only that I had come independently to the same fundamental ideas as Mr. Soper, and that I developped them in some new directions. I hesitated how to include in a Post-scriptum all these remarks but finally I resolved to omitt them. If you accepte my paper for *Biometrika* (it would be very desirable for me and important), and if You find necessary to accompany it with some remarks on its relation with the Mr. Soper's method, I beg You very much to denote my independence from Mr. Soper.

Now I have finished another research on the product-moments of the form (in samples from normal population) $\bar{\mu}^{h}_{11}, \bar{\mu}^{k}_{20}, \bar{\mu}^{l}_{02}$
where [10]

$\bar{\mu}_{11} = (1/s)\sum (x - \bar{x})(y - \bar{y})^2$; $\bar{x} = (1/s)\sum x$;

$\bar{y} = (1/s)\sum y$; $\bar{\mu}_{20} = (1/s)\sum (x - \bar{x})^2$; $\bar{\mu}_{02} = (1/s)\sum (y - \bar{y})^2$

and on the equation of distribution of the quantities $\bar{\mu}_{11}$, $\bar{\mu}_{20}$, and $\bar{\mu}_{02}$. I have found the generating functions of these moments and the equation of distribution of these three quantities. The particular result of the last is the exact value of mean error of coefficient of correlation. It is following:

$$\sigma_{\bar{r}}^2 = \frac{1-r^2}{s-1}[F(1;\,1;\,(s+1)/2;\,r^2) + \frac{(s-1)^2 r^2}{(s+1)(1-r^2)} F(1;\,1;\,(s+3)/2;\,r^2) - \frac{4r^2\Gamma^4(s/2)}{(s-1)(1-r^2)\Gamma^4[(s-1)/2]} F^2[1/2;\,1/2;\,(s+1)/2;\,r^2].$$

Here F denotes hypergeometrical function, $\bar{r}$ is coef. of corr. for samples of number s and r – coef. of corr. of the general population. From this formula it is not difficult to find the ordinary used first approximation $= \sigma_{\bar{r}}^2(1 - r^2)^2/(s - 1)$, and the approximations of the higher orders.

I prepare now a paper containing the exposition of these results and I shall send it to You if You do not refuse to accept it. I am very glad that my paper on the moments of a hypergeometrical series will be printed in Your journal. If you find that some corrections and alterations in my paper must be made, I beg You to make them. I add to the remark on the $\sigma_{\bar{r}}$ that

$$\{\Gamma^4(s/2)/\Gamma^4[(s-1)/2]\} = (s^2/4)\,[1 - (3/s) + (5/2s^2) - (1/8s^4) + \dots].$$

Letter No. 2, 9.1.1925

I am returning you the proofs of my paper with many thanks for your corrections. The proofs are excellent and I have found almost nothing to correct in them. I am late with them because they were retained some days in our university befor to be handed to me.

Letter No. 3, 5.5.1925

I am very obliged for your very interesting letter. The problems you write me on are difficult and attracting and I do not see for the moment how to solve them (I mean the distribution of $\sqrt{\beta_1}$ and β_2, ${}_x\mu_3$, ${}_x\mu_4$, etc, $r_{\sigma_x\sigma_y}$ etc being much easier)[11].

The methods of R. A. Fisher or of generating functions seem to be of little use in these problems. I have a method of obtaining very various classes of moments of an arbitrary distribution of two variates, but, applied to your problems, it would involve infinite series of very complicated nature and it would be very difficult to prove the convergence of these series. I am very sorry that for some months I shall not be able to work on statistics, for I am writing now a text-book of analysis[12] for an editor in order to have necessary money for the voyage in England which I hope to do in the beginning of 1926.

For the same reason I could not rework my paper on the distribution of standard deviations very thoroughly[13]. Yet I have much shortened it and added to it my results on the coefficient of correlation. Now I send you this rewritten paper. I hope that it will be more satisfactory and beg you to publish it in *Biometrika.*

Letter No. 4, 2.6.1925

I am very sorry that there is some clashing in our work and that my paper cannot be published in *Biometrika.* I shall try to publish it in *Metron* or in *Nordisk statistisk tidskrift.*

I would be very glad if you added to your paper that many results contained therein are reached by me by a different method and independently from you[14]. Thus I can indicate your equations (v), (viii), (x), (xiv), (xxiii), (xliii), (xliv) and (xlv) which are also in my paper besides some other results which I do not find in your paper (the general formulae of the product-moments $\overline{\mu}_{20}$, $\overline{\mu}_{02}$, $\overline{\mu}_{11}$, of σ_x, σ_y, $\overline{\mu}$ and of correlation coefficient, their generating functions, the equation of distribution of $\overline{\mu}_{20}$, $\overline{\mu}_{02}$ and $\overline{\mu}_{11}$ etc). Perhaps it would be of interest to indicate the mean error of correlation coefficient in my form, which is (in your notations) [here Romanovsky rewrites the same formula from his Letter 1 although in a somewhat different notation. O. S.] and which is different from your equation (xxv).

I thank you very much for sending me your very interesting paper[15].

Letter No. 5, 1.9.1925

I must beg you to excuse me that I answer your letter of the 15-th June only now. I was not in Tashkend.

I am preparing now a paper on the distributions of $\overline{\mu}_{11}$ and $\overline{\rho}_{xy} = \overline{r\sigma}_y / \overline{\sigma}_x$ which I hope soon to send you for *Biometrika.*

This summer I have received some results of purely mathematical and some of statistical character. For example, I have demonstrated that the mean of some variate in samples of number s from a population with any distribution of this variate tends to be normal for $s \to \infty$[16], if some restrictions are laid on the growth of the moments of the variate. Then, I have discovered some interesting relations between the moments of any distribution and the coefficients of its taylorian expansion. I have also constructed an example of non-linear correlation whose correlation coefficient can be made as near to unity as you desire. If these results can be interesting for *Biometrika* I can send you short notes on them.

I have, endly, systematized several general theorems on the distributions [such as "given the distribution $\varphi(x_1; x_2; \ldots; x_n)$, to find the distribution of any functions of $x_1, x_2, \ldots, x_n$"; "given the distribution of x, y, z in a general population, to find the distribution of any functions of the samples of number s from this population" and so on][17]. I consider only continuous distributions.

Letter No. 6, 2.10.1925

I am very sorry that it may seem to you that I have acted incorrectly in regard to you, Fisher and others publishing my notes in the *Comptes rendus*. I have not pretended in them that my results are new and it is not written there. I have only written that "le but de cette note est d'indiquer une méthode nouvelle pour la recherche" etc[18] and I think that my method is indeed new. This does not contest Mr. Fisher in his note published in the *Comptes rendus* (1925) and containing the solution of an integral equation which I have received in one of my notes and could not to solve. Besides my aim in publishing the notes was not the claim of priority or of newness but to indicate a method which can be of use in many similar questions. I am very sorry that, trying to be short as possible, I have omitted all indications of the results already known and of their authors. But I think that this cannot lead anywhom knowing the modern state of statistics in mistake.

I can add to my explanations that my two notes on mathematical statistics were sent together in February befor I have received your letter with your paper (1925) which has reached me at the end of May or the beginning of June (I cannot remember exactly).

It is a very great grief for me that, as you write, I cannot be a contributor to *Biometrika* – the best journal of the theoretical statistics – and still greater one to have lost your good esteem of me. I shall be very obliged to you if you write me how you accept my explanations[19].

Letter No. 7, 5.11.1925

I think that I misunderstood the rules of *Biometrika*: I thought that the quite short abstracts from the contributions to it can be published elsewhere (such is the custum in many mathematical journals). I beg you to excuse me of this misunderstanding and of sending you my paper on the distribution of the regression coefficient: I see now that it could not be printed in *Biometrika*[20].

In order to clear up the matter wholly I beg you very much to write me if you could to accept my other contributions which are and will not be published nowhere or if you refuse in general my contributions to *Biometrika.*

The Statistical Cabinet of the Law faculty of our university through an agent of our Government will purchase an exemplar of *Biometrika* for its library. The Faculty begs you not to refuse to send the journal to the Cabinet or to whom will indicate the agent.

3. Romanovsky's correspondence with Fisher

Letter No. 8. Romanovsky – Fisher 9.10.1929

I would be very glad to see you and to visit the Rothamsted station[21]

Letter No. 9. Fisher – Romanovsky 10.10.1929

… perhaps you can visit us on Monday, October 14th. And if it suits you stay with us for a while.

Letter No. 10. Romanovsky – Fisher 18.10.1929

… I shall visit you again on Monday if you do not object it. I shall be glad to see you and all your friends … again.

Letter No. 11. Romanovsky – Fisher 28.10.1929

There is [here] in Paris a friend of mine who was some years ago a lecturer of polytical economy at the University of Tashkend and now, as an emigrant, allready two years, lives in Paris. He is an able scientist who has published two books[22]… These books were much praised as original and novel in views and working out of data. They are written, I must add, quite not in an orthodoxal Marxian manner. The name of the author is Alexander [Petrovich] Demidoff. He is now 36 years old and is a bearer of quota immigration visa for U.S.A. In Paris he was earning his, his wife and his little daughter's life serving at a bank (very little gaining, I must add) and in leisure minutes he was working in the libraries of Paris on a big problem: the present economical state of England, its development and its future. His views, as far as I can judge, are very interesting and in some points original.

Now I come to the aim of this letter. Mr. Demidoff lives in very difficult conditions and has no prospects to finish and publish his work just mentioned. Perhaps, you and Prof. Hotelling[23] could help him to receive the Rock[e]feller's stipend for a year in order he could quietely work on his problem? He will send you a prospect of his work and I ask you and Prof. Hotelling to read it and to write to Mr. Demidoff if he can hope to obtain a help and how he can act further for this aim. You and Prof. Hotelling have many american friends and if you find it to be possible you can help him very much.

There is yet a very important point. If you and Prof. Hotelling resolve to help to Mr. Demidoff, please do not remember at all my name, for it can end with my emprisonment by GPU (Chief Political Administration of Soviet Russia, the most dreadfull and mightfull organisation in the present Russia). It is a crime, and a very heavy one, from its point of view my endeavouring to help an emigrant, although there is no politics in my action but only the desire to help to an able scientist who can do much important work being placed in good conditions. Act if thus as if you knew only Mr. Demidoff's prospect and his book, which, I hope, he will also send you.

To morrow I go to Berlin and from there to Moscow. My best remembrance from my voyage abroad is and will be Rothamsted Experimental Station and the men I had known there. … Please do not write to me in Russia on Mr Demidoff and read this letter to Prof. Hotelling.

Letter No. 12. Romanovsky – Fisher 22.12.1929

Romanovsky begins with season's greetings and continues:

I have received your Christmas card and your last [latest] paper also and bring you my thanks for them. Write me, if you please, the name of the author and the title of the book on statistics of enginiring I have seen at you: I shall purchase it for me. I beg you also to write me a short description of the scheme of the increasing of precision of plot experiments I have seen in your laboratory at the Rothamsted Station.

I have forgotten it and some agronomical researchers here are very interested with it.

Letter No. 13. Fisher – Romanovsky 6.1.1930

The title of the book you refer to is [Fry (1928)]. I am not sure what you refer to about Plot experimentation. You have my book[24] and various papers on the subject. There is in my laboratory a diagram illustrating the logical position of the three principles of plot experimentation[25]…

Letter No. 14. Romanovsky – Fisher 22.3.1930

I have received some papers of you and your friends and perused them with great pleasure. Twenty years ago I was much occupied with the theory of the prime numbers and published some papers on them. Then, having no table of the prime numbers under my disposition, I prepared it myself, up to 2000, with the same principle as it is constructed in your note on the sieve of Eratosthenes [Fisher (1929a)]. Thus it procured me much pleasure to see your note and to know that you also have not escaped the fascinating power of the prime numbers – one of the most wonderful things in the world.

I am much occupied in our university and have still no time to study your and Craig's papers on the theory of moments[26]. I shall do it in summer. Some rare hours of leisure I have spent on the investigation of a class of integral equations which I have found in connection with further development of Markoff's chains (you have a note on them)[27]. These equations seem to be novel and I have developped their theory analogous to that of Fredholm. I shall do a communication on this theory and on further generalisations of Markoff's chains this summer at the Congress of the Mathematicians of USSR which will take place in Kharkov.

Just in this moment I have received a letter from the Organizing Committee of the Congress (I am one of its members) and there stands that many foreign mathematicians (Borel, Hadamard, Lichtenstein, Levi-Civita, Blaschke, Cartan, Denjoy, Montel, Mandelbroit and others) will take place at the Congress and read communications. Perhaps you should also come and read on your researches in the math. statistics? It would be splendid to meet you at this Congress!

Letter No. 15. Fisher – Romanovsky 11.4.1930

I am afraid I cannot manage the trip to Kharkoff in June next as I seem to have in other ways a very busy year in front of me. Many thanks for the suggestion. I hope you will have a successful meeting.

I am glad to hear of the new class of integral equations; it is a subject that I admire from a distance[28]. The combinatorial procedure for evaluating the higher moments of algebraic statistics may, however, be intimately of interest in this regard. It was a long while before I could see the reason for all the simplifications which the method introduces. Indeed it is still a mystery to me why the algebraic coefficients corresponding to the "patterns" should be so simple.

I worked out the other day the coefficients corresponding to the three symbolic figures … [Fig. 1] which are all that are wanted (in the case of a normal population) for anything like the 4th semi-invariant of the distribution of k_4, such as (with two variates) any 4th order semi-invariant of the simultaneous distribution of k_{40}, k_{31}, k_{22}, k_{13}, k_{o4}[29]. Well, the patterns have eight rows each, and the number of separations of eight parts is very large, so that it was very heavy work before I had the coefficients; but when all is done they are simply

$$n(n + 1) (n^4 – 8n^3 + 21n^2 – 14n + 4)/(n – 1)^3 (n – 2)^3 (n – 3)^3,$$

$$n^2 (n + 1)^2 (n – 2) (n – 3)/(n – 1)^3 (n – 2)^3 (n – 3)^3,$$

$$n (n + 1) (n^4 – 9n^3 + 23n^2 – 11n + 4)/(n – 1)^3 (n – 2)^3 (n – 3)^3.$$

So that letting N_1, N_2, N_3 stand for these three expressions the 4th semi-invariant of k_4 is simply

$$4·12^3 (9N_1 + 8N_2 + 36N_3)k_2^8$$

and, for example, for the 4th semi-invariant of k_{22} in the bivariate problem, we have only to subdivide the numerical factors by supposing that the four rods which meet at each point are two black and two red, and enumerating the number of ways of linking them up with 0, 2, 4, 6, or 8 black-red junctions (as opposed to black-black or red-red junctions which must be equal in number and supply the factors k_{20}, k_{02}). Thus in every problem the algebraic coefficients are the same, and they are so simple that one feels that one ought to be able to write them down by inspection of the pattern, or of its symbolical diagram.

I am glad you liked the sieve. I feel that Eratosthenes has been too long exposed to the patronising remarks of his critics!

Letter No. 16. Romanovsky – Fisher 28.10.1930

I am very thankful for the copies of your works and of your collaborators and assistents. They are regularly received here and are very interesting and important for me, especially since I am more closely connected with the Cotton Research Institute organised here, in Tashkend. The works of the Rothamsted Experimental Station and your methods for field experiment are of much aid for me and I am propagating them very zealously.

Much time is lost in performing my professional duties and so I am almost unable to write on my personal researches. I am much advanced in the investigation of phenomena connected in chains and depending from random (Markoff's chains as I name them) and the results are very interesting from the point of time series. My intention is to write a memoir on these results but all my time I am spending in new researches: it is not very pleasant to lose it in writing down acquired results. It would be splendid if I could to spend all my time only in the quiet work in libraries like I did it past year in Berlin, Paris and especially in London (The British Museum is the most beautiful and comfortable library).

What are you working on?

Letter No. 17. Fisher – Romanovsky 14.11.1930

I am very glad to hear that my reprints have been safely received, and I shall be much interested to see more of your own researches as they are published. I have long intended to gather together the most important mathematical researches of recent years in a book on Mathematical Statistics, but so far I have not found the time to make any real progress with this task.

I am very glad you found the Library of the British Museum convenient to your work, and hope that you may again have an opportunity to visit us, and carry out the more substantial researches you have in mind.

My family is well. I hope Mrs Romanovsky and your daughter are also in good health[30].

Letter No. 20. Romanovsky – Fisher 22.12.1931

Season's greetings.

Letter No. 21. Fisher – Romanovsky 5.1.1932

"Belated" season's greetings. He continues: I sincerely hope that your country may in time reap the rewards of the great efforts and sacrifices which are being made.

Letter No. 22. Romanovsky – Fisher 19.1.1934

I am very glad to congratulate you with the professorship in the London University. Your field of activity is now widening and I hope it will be to the benefit of the science and yourself.

I would be very content if you send me the prospects or the plans of the researches of the laboratories which are now under your guidance. It interests me very much as also all what concerns the organisation of your laboratories.

Our Physico-Mathematical Research Institute is developping steadily and I hope very soon to send you the proofs thereof: the offprints of papers, mine and of my collaborators, made in the Institute.

Are you now living in London or, as before, in Harpenden?

Letter No. 23. Fisher – Romanovsky 5.2.1934

I am very glad to have your letter, and to see your handwriting again. I am glad to hear of the Physico-Mathematical Research Institute in Tashkent. I have recently been seeing some of the indirect effects of your activity in the improvement of methods of experimentation in Cotton trials. I suppose the new Institute will be concerned with the technology of cotton spinning.

My new department will, I am afraid, only be slowly organised. I want to give Students of Eugenics working here an opportunity to acquaint themselves thoroughly with modern genetical knowledge in animal material. I find I have a nice animal house, and have been engaged since I have been here in getting adequate equipment for the photographic studio, and now for the Laboratory. All the equipment here was very old and bad. I hope later to have a Biological Assistant but he is not yet appointed, and at present I have only two Biological Voluntary workers.

The department of Statistics has been separated from the Galton Laboratory[31], which saves me from having to organise the Statistical teaching, but has the bad effect that Students have not always confidence enough to ask my advice on Statistical points when they need it. I have been lecturing on the Logic of Experimentation, and also on Quantitative Inheritance, and a very good class chiefly of members of the staff have been coming to the lectures, but I am afraid I am not an experienced lecturer and the preparation of the lectures has taken more time than I ought to give.

I shall continue to live in Harpenden, as the new Laboratories are conveniently accessible from there, and I hope some day to welcome you, or perhaps a Student from your University in the Galton Laboratory.

Letter No. 24. Romanovsky – Fisher 4.12.1935

I would be very obliged to you if you indicated me how are established two approximate formulae, p. 221 of your *Statistical methods* …[32] I am also puzzled why you use, in the analysis of variance,

$$z = 1/2\ln(s_1^2/s_2^2)$$

instead of s_1^2/s_2^2. Many thanks in advance for the answers.

Season's greetings follow.

Letter No. 25. Fisher – Romanovsky 20.12.1935

So far as I remember I obtained the approximation on page[33] for the test of significance of z where both n_1 and n_2 are large by obtaining the moments of the distribution of z, or rather its cumulants, from its characteristic function. I forget the details, but clearly the factor $[(1/n_1) - (1/n_2)]$ is a simple allowance for the third moment, while the first term is derived from the normal distribution.

I had a good many reasons for using z instead of some function of it in the test of significance in the analysis of variance. One important reason was that in order to make a compact table it is necessary that the test value should be well interpolated by what I call asymptotic interpolation using the reciprocals of the numbers of degrees of freedom and this is more true of z than of any other simple function. A second point is that half the tabulation is saved by the fact that reversing the sign of z and interchanging n_1 and n_2 we have the 5% and 1% points at the opposite ends of the distribution. Finally, the close analogy between interclass and intra class correlations is paralleled by that of the values of z obtained from r by the same transformation. The advantages of this transformation I have set out in the book.

Please accept my kind wishes for yourself and family during the coming year. I am sending a copy of a recent book of mine, which may, I hope, interest you.

Letter No. 26. Romanovsky – Fisher 23.1.1936

Many thanks for your excellent and very interesting book [Fisher (1935)]. I shall read it and write a note on it like one I have written on your *Methods for Research Workers* and sent you some time ago. Have you received it[34]?

In some days I shall send you my last [latest] memoir [15].

Letter No. 27. Fisher – Romanovsky 1.2.1937

I am very obliged for the cuttings of the two reviews of the *Design of Experiments* which you were good enough to send me. I am having them translated into English.

Letter No. 28. Romanovsky – Fisher 15.10.1937

One of my pupils, V. Peregoodoff, … has translated in Russian your *Design of Experiments*[35]. The translation will soonly be published and it is intended to accompany it with your portrait in the frontispice. V. Peregoodoff does not dare to beg you to send him it and asked me to write to you. I do it with great pleasure for I appreciate your book very highly. We all shall be very thankfull to you. …

I have read a conference on your book in the Society of Naturalists at our university and now prepare it for publishing.

My time is now very occupied (I am now dean of the physico – mathematical faculty of our university) and I work very little in statistical research and I publish still less. But I hope to publish soonly some of my last [latest] researches in the theory of probabilities and in the math. statistics. At the end of this year will be printed my book [22], a big volume containing much of the recent researches, with demonstrations, more a mathematical work than practical. I shall be glad to send you an exemplar.

Letter No. 29. Fisher – Romanovsky 1.11.1937

I am delighted to learn that one of your pupils has translated my *Design of Experiments*, and, naturally, wish the greatest success to this publication. Nevertheless your request for a photograph does somewhat embarrass me, for the following reason.

I understand that the Soviet Government does not legally recognise the copyright laws of other countries, although, in fact, they make arrangements with the publishers who possess these copyrights. I do not think my publishers, … have been approached, or have given permission for this translation, and in these circumstances I cannot myself cooperate in what they may regard as an infringement of their rights.

I have reason to believe that, if the Department concerned, approached [the publishers] with an offer of no great magnitude, even though payable only in internal currency, they would be satisfied with this formal acknowledgement of their rights, and would at my request not stand in the way of what may be a valuable publication. Would you, or Mr Peregoodoff, take the matter up with the Russian authorities, in which case I should be happy to cooperate.

I am glad to hear that your services in University organisation are now being recognised, even though the additional work may withdraw your time from mathematical statistics. I should very much indeed like to possess a copy of your book when it is published.

Letter No. 30. Romanovsky – Fisher 14.10.1938

I have received from your editors your *Tables*[36]. Many thanks for this valuable presentation. Of course, I shall write and publish a review in some [in one?] of our journals, for I appreciate very much your new statistical tables. I hope to do it as soon as I can (my duties have increased very much: I am now elected as a member of the Supreme Council [Supreme Soviet] (Parliament) of our republic).

Notes

1. Romanovsky's letters to Pearson are kept at Special Collections, Library Services, University College London (Pearson papers 831/3); his correspondence with Fisher is in the Barr Smith Library, University of Adelaide. I myself (Sheynin 1996, pp. 50 – 53/2011, pp. 70 - 73) published two letters from Chuprov to Romanovsky (1923 and 1925) as well as Chuprov's later letters concerning him. The Archive of the Russian Academy of Sciences (*Fond* 173, inventory 1, *delo* 17, No. 1) keeps Romanovsky's letter to Markov of 2.11.1916. Taking into account Markov's criticism, Romanovsky revised the proof of one of his theorems, enlarged on his considerations and expressed his desire to publish his manuscript in Petrograd (Petersburg). Markov's answer is not known but the paper in question appeared many years later [12].

2. B&M (pp. 98 – 101) describe Romanovsky's ideas [1] on scientific progress and social phenomena. When mentioning his admiration of Mendel and eugenics, they justly remark that his conclusions were still possible [to publish] in the beginning of the 1920s.

3. Romanovsky (Ibidem, pp. 225 – 226) attributed to Fisher the Mises concept of probability. At the very least, Fisher was indeed an empiricist.

4. Earlier Romanovsky [7, p. 1088] called Chuprov "the greatest Russian statistician".

5. Recall, for example, Fisher (1934, p. 23) who wrongly declared that the method of least squares was a corollary of the principle of maximum likelihood.

6. Kolmogorov (1947, p. 63) favourably cited [18] as well as the Western school of statistics.

7. This book (Chebotarev 1958) was written on the level of the mid-19th century with some elements of linear algebra and mathematical statistics having been added. On p. 579 we find that the Ptolemy system of the world "for 14 centuries held mankind in ideological captivity".

8. In present-day terminology, one-dimensional and bivariate populations. The expression "equation of distribution" (below in this Letter) is also dated. Romanovsky again mentions the same manuscript in Letters 3 and 4. It did not appear in *Biometrika*, but Chuprov (Sheynin 1996, p. 50/2011, p. 70) later communicated its modified version to *Metron*. Indeed, Romanovsky shortened it and added some new material, see Letter 3. The additions concerned the issue described below in this Letter (see Letter 4).

9. Probable errors calculated for a sample are random variables and do not therefore possess true values.

10. Instead of x and y read x_i and y_i respectively.

11. The Pearson article (1925, p. 181) contains only the last two symbols; they pertained to coefficients of correlation.

12. Such a textbook appeared only in 1939.

13. See beginning of Letter 1.

14. Pearson (1925, p. 199) had indeed indicated:

Writing without knowledge of the papers in Biometrika … and naturally without knowledge of my present paper, Professor Romanovsky had reached, dealing only with the algebraic side, many of the published results and certain additional ones. While willing to publish the latter, the present cost of printing prohibited the reproduction of much work already published or about to be published in this Journal. … I sent him a proof of this paper and asked him to cable if he were willing that I should add under the title his name to my own. … He [his Letter 4?] *is satisfied with the statement that many results contained in the present and earlier papers have also been obtained by him quite independently and by a different method. I trust for the sake of his additional results that his paper may shortly be published elsewhere.*

And here is Romanovsky's *Remark* [4, p. 208] translated from its Russian version:

After completing this article, I received from Prof. Pearson the proofs of his paper (1925). … It contains some results of my present article derived by means of an utterly different method.

15. The article Pearson (1925), also see Note 14 and Letter 6.
16. The expression "the mean … tends to be normal" is unfortunate. Concerning the indicated findings see [18]. Romanovsky's discovery of the relation between the moments and the Taylorian series is unknown to me; see however Delsarte (1930) where Romanovsky is not cited.
17. See [22, book 2, pp. 47 – 50].
18. Romanovsky quoted these few words from one of his papers [6].
19. Also see Letter 7. In addition to [10], Romanovsky later published two more papers in *Biometrika*, in 1933 and 1936. His first articles there appeared in 1923 and 1924. In the second of these he [3] studied a generalized system of the Pearsonian curves and Pearson added there his remarks.
20. The manuscript was published in Russia [8].
21. The experimental station near Harpenden. Fisher worked there as statistician for 14 years, from 1919 to 1933.
22. The American *National Union Catalog pre-1956 Imprints* mentions three books by Demidov (including those cited by Romanovsky) published in Russia and one more which appeared in Paris in 1931.
23. Harold Hotelling (1895 – 1973), an American statistician and economist. Corresponded with Fisher from about 1927 and worked for a few months in 1929 at Rothamsted.
24. Evidently an earlier edition of Fisher (1934).
25. Fisher wrote out five terms: 1) Replication; 2) Random distribution; 3) Local control; 4) Validity of estimate; of error; 5) Diminution of error; but he numbered only the three first ones. He also indicated by arrows the directions 1 – 2, 1 – 3, 1 – , 1 – 5, and 2 – 4.
26. Apparently Fisher (1929b; 1930) and Craig (1930) if only Fisher (1930) was already published and available.
27. The expression *Markov chains* dates back to Bernstein (1926, §16) who called them *chaines de A. Markoff*. Romanovsky used it in 1929 and 1930. I did not find Fisher's note mentioned by Romanovsky.
28. See however Letter 6.
29. Here is Fisher's marginal note: "There seems to be 34 such bivariate formulae". The so-called Fisherian *t*-statistics k_r $(x_1; x_2; …; x_n)$, r = 1, 2, … are the most general homogeneous polynomials of degree r with mean values Ek_r equal to the r-th cumulants of the appropriate sample distribution. Kendall (1963/1970, p. 442) called Fisher (1929b) "the most remarkable paper he ever wrote" and testified on his next page that Fisher "was never able to explain … to me how he thought of these results". Fisher's letter to Romanovsky is interesting, in particular, in this connection, but it only partly explains his ideas. In his paper (1929b) he investigates, for the same purpose, the partitions of the numbers r (he applies the term *separation* in his letter). Even Wilks (1962, §8.2c) refers readers to special literature on these statistics. Also see [22, book 1, pp. 88 – 99]. Finally, I note that Fisher had not

sufficiently explained his figures either. He mentioned points and junctions without distinguishing between them, see below. He adduced other figures in his paper (1929b) with no explanation at all.

30. Romanovsky's daughter died in 1925 (B&M, p. 85). The next two letters are those already published, see § 1.

31. The Galton Laboratory of National Eugenics was established in 1907 at London University. In 1933 Fisher replaced Pearson in the new faculty of (mathematical) statistics, but it was E. S. Pearson who taught statistics. Fisher was left with eugenics and biometry and in actual fact (not as he wrote to Romanovsky) that situation disappointed him (Bartlett 1978, p. 353).

32. Read Fisher (1935, p. 221). The formula below is the known Fisher z-transformation which he introduced in 1925 in *Biometrika*; r is the sample coefficient of correlation. In his book of 1935 Fisher introduced the formula on p. 200 and indicated its merits on p. 207. In Letter 25 he put forward additional pertinent considerations and Romanovsky [14, p. 126] apparently agreed with him. The quotient s_1^2/s_2^2 (in standard notation) is indeed in general usage in analysis of variance but

$$(1 + r)/(1 - r) \neq s_1^2/s_2^2.$$

Later on Romanovsky [22, book 2, p. 21] applied 1/2 $\ln(s_1^2/s_2^2)$ in the same analysis.

33. A blank in the original text.

34. See § 1 for the reviews mentioned in Letter 27.

35. See § 1.

36. Fisher & Yates (1938).

Acknowledgements. The libraries mentioned in Note 1 kindly permitted me to publish Romanovsky's correspondence. His letters to Pearson have appeared in a preliminary manner in a microfiche collection (mostly of translations) Bernstein et al (1998, pp. 233 – 239), but the publisher has no copyright to it.

I am grateful to Professors Herbert A. David and J. Pfanzagl for indicating some statistical sources and to Dr. A. L. Dmitriev for sending me photostat copies of some of Romanovsky's papers.

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Abbreviation

M = Moscow; L = Leningrad; R = in Russian

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4. On the moments of standard deviations and of correlation coefficient in samples from normal population. *Metron*, vol. 5, 1925, pp. 3 – 46.

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On the History of University Statistics

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I describe the early development of university statistics or *Staatswissenschaft*, briefly sketch its changing state in the second half of the 19th century and note that it still in existence, although in a changed way. I also describe the work of Karl Fedorovich Gherman (Carl Theodor Hermann) (1809) and a booklet by Christian von Schlözer (1827), the son of A. L. Schlözer.

1. G. Achenwall created the Göttingen school of Staatswissenschaft. It determined the climate, geographical situation, political structure and economics of separate states but did not study the relations between quantitative indications. Achenwall recommended to carry out censuses without which he (1761/1779, p. 187) nevertheless thought it possible to obtain a *probable estimate* of the population by issuing from data on births (on baptisms) and deaths.

Achenwall followed the founder of the Staatswissenschaft, Hermann Conring, and was the first to expound systematically this discipline, and, moreover, in German rather than in Latin, the Conring's language. In his opinion (1752; Introduction) Staatswissenschaft actually denoted politics. And it is appropriate to mention that in a letter of 1742 Daniel Bernoulli (Fuss 1843/1968, t. 2, p. 496) stated that *mathematics can be also rightfully applied in politics*. Citing Maupertuis' approval, he continued: *An entirely new science will emerge if only as many observations will be made in politics as in physics*. But did he understand politics just as Achewall (and Gherman, see below) did later? As Laplace did? He (1814/1995, p. 62) urged that *the method based on observation and calculus* should be applied *to the political and moral sciences*.

But who exactly ought to accomplish this task? Statisticians had (have?) been unwilling to allow mathematicians a free hand. Indeed,

An ablest mathematician can judge matters belonging to agriculture as artlessly as a child (A. L. Schlözer 1804, p. 63). And, more to the point, Chuprov (1922, p. 143): *Only mathematically armed statisticians can defeat mathematicians playing at statistics*.

Achenwall (1749, p. 1) also appropriately defined *the so-called statistics* as the *Staatswissenschaft* of separate states and thus left an indirect definition of statistics:

In any case, statistics is not a subject that can be understood at once by an empty pate. It belongs to a well digested philosophy, it demands a thorough knowledge of European state and natural history *taken together with a multitude of concepts and principles, and an ability to comprehend fairly well very different articles of the constitutions of present-day kingdoms* [*Reiche*].

Achenwall's student A. L. Schlözer (1804, p. 86) figuratively stated that *History is statistics flowing, and statistics is history standing still.* Obodovsky (1839, p. 48) suggested a similar maxim: *Statistics is to history as painting is to poetry*. For those keeping to *Staatswissenschaft* Schlözer's pithy saying became the definition of statistics which, contrary to his opinion, was thus not compelled to study causal connections in society or discuss possible consequences of innovations. Furthermore, the much needed comparison of data in space and time was left out.

Knies (1850, p. 24) quoted unnamed German authors who had believed, in 1806 and 1807, that the issues of statistics ought to be the national spirit, love of freedom, the talent and the characteristics of the great and ordinary people of a given state. This critic had to do with the limitations of mathematics in general. Note that Leibniz (§ 4) did not mention such concepts.

Here, however, is an ancient example of uniting description with numbers:

Moses (Numbers 13: 17 – 20), who sent out spies to the land of Canaan, wished to find out *Whether the people who dwell in it are strong or weak, whether they are few or many*, – wished to know both numbers (roughly) and moral strength.

Tabular statistics which described separate states by numerical tables appeared in Anchersen (1741) and perhaps could have served as a connecting link between words and numbers, but Achenwall had *experienced a public attack* against the first edition of that book (published in 1749 under a previous title) by Anchersen. *Tabular* statisticians continued to be scorned, they were called *Tabellenfabrikanten* and *Tabellenknechte* (slaves of tables) (Knies 1850, p. 23). In 1734, I. K. Kirilov (Ploshko and Eliseeva 1990, pp. 65 – 66) compiled a tabular description of Russia, but it was only published in 1831.

A. L. Schlözer (1804, pp. 41 and 90) twice mentioned that subject. In the first instance he stated that the concocted tables provide *Unwahrheiten* (why did he single out those tables?) but then positively mentioned the tabular method. Anyway, statistical tables have retained their importance, witness for example astronomical yearbooks or the still celebrated *Recherches* … edited by Fourier (1821 – 1829).

By the end of the 19th century the scope of *Staatswissenschaft* narrowed, although it still exists, at least in Germany, in a new form: it includes numerical data and studies causes and effects and it is the application of the statistical method to various disciplines and a given state, but statistics, in its modern sense, owed its origin to political arithmetic founded by Petty and Graunt.

They studied population, economics, and commerce and discussed the appropriate causes and connections by means of elementary stochastic considerations. Petty called the new discipline *political arithmetic* and its aims were to study from a socio-economic point of view states and separate cities (or regions) by means of (rather unreliable) statistical data on population, industry, agriculture, commerce etc.

2. Gherman, Hermann (1809). The title of his booklet mentioned statistics, and he applied this term time and time again, but he really meant Staatswissenschaft. Indeed, the subject of statistics is the state (p. 57). Gherman several times specifies this statement, and even largely repeats himself. Here is the gist of his declarations.

Statistics differs from geography, history, civil law, economics and politics in its usual sense, but all these sciences support, or can support statistics and provide it with their materials (pp. 39 and 50 – 54). Elsewhere Gherman (pp. 19 – 20) adds a queer explanation: *Politicians known as economists* … Then, history is mostly interested in great upheavals and their causes (p. 52) and Schlözer's pithy saying is only an intricate play on words (p. 48). I suspect, however, that Gherman wrongly considered that saying as a definition of statistics.

Note that Quetelet (1846, p. 275) thought that other sciences were alien to statistics which was only true in a strict sense. I would say that those sciences (for example, geography) apply the statistical method.

Statistics has to do with people living in a state but not to those dwelling in loose societies (pp. 35 and 52) and it is knowledge rather than a science since it considers deeds but not concepts (p. 35). At the same time, however, Gherman (Introduction on unnumbered pages) insists that only a perfect theory can make statistics [transfer it into] the foundation of all political sciences. Here, as in many other instances, he follows Schlözer (1804), but many later statisticians stressed that statistics did not yet have its own theory.

I believe that in those times *theory of statistics* really meant a system, a suitable arrangement of statistics.

For his part, Obodovsky (1839, p. 2) declared that theory is important for statistics just as the soul is important for the body; that it ought to distinguish, estimate, collect and arrange statistical data; that no one is anymore doubting that the theory should constitute the main and essential part of statistical courses. He (p. 102) also noted that for many statisticians the material part had been the main component of statistics and the mass of statistical data increased boundlessly and it was impossible to arrange the collected data in a system. Referring to Lueder (1817), he added that statistics became a target for mockery.

Indeed, Lueder (1817, p. v) formulated his aim as destroying statistics and politics which is tightly connected with it and likened statistics with astrology (p. ix). His statements were understandably forgotten.

Then (Gherman, pp. 36 and 37), statistics considers everything that noticeably influences the wellbeing of a state at a given calm period. It does not judge, praise or blame (p. 37) and useless secrecy harms it (p. 105). Secrecy dominated statistics which had only been tolerated (p. 19). (The past tense is certainly wrong.) Cf. A. L. Schlözer (1804, pp. 51 and 52): the possibility of collecting and publishing statistical data, incompatible with despotism, is a litmus test of civil freedom.

It is usually thought that statistics only includes numbers (p. 16), but non-numerical information is also needed about the enlightenment and education, about the work of the government, foreign affairs and legislation (p. 17). However, readers feel themselves *almost choked* by the great amount of statistical materials and calculations which still do not provide genuine knowledge (p. 22). Only well arranged and compiled [numerical] tables can glorify statistics (p. 52).

Gherman pays much attention to ensuring a real picture of reality and Ploshko & Eliseeva (1990) who provide some more information on Gherman note that elsewhere he discussed grouping of populations, means and relative magnitudes. On p. 87 they also state that in 1821 Gherman and another professor of the Petersburg university were removed from teaching for *insulting religion and the existing order*. Indeed, Gherman (1809, p. 3) stated that in Western Europe theologians formerly acquired so much power that they had hindered scientific progress.

3. Leibniz. In the beginning of the 1680s he compiled several manuscripts on political arithmetic and Staatswissenschaft which were only published in mid-19th century. Now, they are available in his collected writings on insurance and finance mathematics (2000). In one of those manuscripts he (1680 – 1683/2000, pp. 442 and 443) adopted unfounded premises about population statistics including a simply fantastic statement: the birth rate can be nine or ten times higher than it is.

In his manuscripts devoted to Staatswissenschaft, Leibniz had recommended the compilation of state tables containing information useful for the state and the comparison of those of them which pertained to different states or times, as A. L. Schlözer (1804, p. 32) later stated; the compilation of medical sourcebooks of observations made by physicians, of their recommendations and aphorisms; and the establishment of sanitary commissions to be entrusted them with unimaginably wide tasks. He mentioned inspection of shops and bakeries, registration of the changes in the weather, fruit and vegetable yields, prices of foodstuffs, magnetic observations and, the main goal, recording of diseases and accidents affecting humans and cattle.

Leibniz (1682) also compiled a list of 56 questions (actually, of 58 since he made two mistakes in numbering them). He left them in an extremely raw and disordered state and a few are even incomprehensible. Their main topics were population statistics in a wide sense; money circulation; cost of living; morbidity. Incidentally, for some strange reason population statistics at least up to the 20th century had shunned medical problems. I am listing those questions in English although for two of them I only quote their German and hardly understandable translations from the original Latin..

1. The numerical strength of the population

2. The ratio of men to women which determines to what extent is celibacy compatible with it.

3. The ratio of married and unmarried.

4. How many women are fertile.

5. How many men can bear arms.

6. The strength of each age of the population.

7. Which age groups are more prone to diseases.

8. How many children live to become adult.

9. How long is the mean duration of human life.

10. How long is the presumable duration of life for people of a given age group.

11. How much does a life annuity cost.

12. How salubrious are the localities.

13. Which diseases are predominant, when and where.

14. How are the diseases mutating from one into another.

15. What is the ratio of the forces of diseases.

16. And especially of the chronic and acute diseases.

17. Comparison of rural areas and towns of medium size with cities.

18. Which localities or years are more or less fruitful.

19. The relations of the ways of life (if the respective numbers of deaths are known).

20. The distribution of the numerical strengths of populations in various localities.

21. Comparison of the numbers of deaths and births.

22. Increase or decrease of the population.

23. The knowledge of the geometric area, of the aspects and disposition of each locality and of their parts mostly restricted by natural boundaries.

24. How large is the crop capacity of various meadow plants.

25. How many herd animals can be kept given a certain amount of hay etc.

26. How large is the mean crop capacity of a fertile field or arable land in about seven years.

27. In what ratio and why does the cost of things increase or decrease.

28. What land is better suited for which use.

29. About the ratio of the cost of gold to silver or other metals.

30. What is the amount of a day labour of a man in each locality and how much can a man of the lowest rank acceptably earn.

31. How much can he save after a year.

31 (bis). How much of each thing is consumed.

32. To what extent are people from without earning their income here, and we from them.

33. To what extent do we need them and they need us.

34. About the barely understood real ratio (wahre Verhältnis) of money.

35. On the inherent real value of fields and arable land as well as of other things.

36. About the present use value of houses, commodities etc. and its distinction from the real value.

37. About these distinctions from year to year.

38. How much money is there in hand. This is usually judged quite mistakenly.

39. How many men are there of each speciality (Beruf) and how large are the ratios of their numerical strengths.

40. How large ought to be those ratios.

41. About a gradual reduction of things to their real ratios.

42. About the improvement of land (Äckern) by draining marshes.

43. About decreasing the flooding by preparing the ground for many ponds in dry localities and in the mountains so that torrents will less flood the plains.

44. Flöße, flößen, damit es, soweit es geschehen kann, verringert wird.

45. Carting of timber from remote localities and how to achieve this.

46. About transplanting separate trees along streets.

47. An exact description of all arts and specialities.

48. Address bureaus.

49. Registration of all changes due to deaths, baptisms, marriages etc.

50. The history of diseases in each locality.

50 (bis). About old and new loads of metals.

51. Daß an verschiedenen Orten auf öffentliche Kosten die Menschen, wenn auch langsam, Wasserleitungen in die Berge führen, in der Hoffnung auf Metalle. Städte oder ganze Gegenden werden zusammenwirken.

52. On sowing clover.

53. On planting potato.

54. Put anonymous communications in publicly installed boxes.

55. The significance of a usual and a valuable man.

56. Documents and books. Nothing should be issued without permission because many desire it. Kalenderangelegenheiten. [Something to do with choosing a calendar.]

4. Subsequent history. The scope of Staatswissenschaft gradually narrowed. After economics became separated (Adam Smith), geography, meteorology and biology followed suit. And the study of causes and consequences simply had to begin. A. L. Schlözer (1804, pp. 85 – 86), for example, rhetorically asked, why did the population of Spain only number 12 *mln*.

The climate of opinion had however, been different, see Delambre (1819, p. LXVII), France; Anonymous (1839), England, the statement of the established London Statistical Society; and Russia (Gherman, § 2, indirectly). But still, life demanded such studies:

Absurd restrictions [about investigating causes and consequences] *have been necessarily disregarded in … numerous papers* [in the *Journal of the London Statistical Society*] (Woolhouse 1873, p. 39).

Wagner (1867, p. 423) bluntly stated that numbers were necessary and on p. 428 mentioned the *Süssmilch – Quetelet direction, or the school of statistics proper*, which did not at all belong to Staatswissenschaft. Zahn (1926, pp. 870 – 871) noted that a quantitative direction had appeared in the university statistics (when?) although opposed by the partisans of previous notions. He concluded that statistical materials had ousted remarkable features and political arithmetic had gained the upper hand, that statistics had adopted its *classical form*:

Accordingly, nowadays statistics appears as a doctrine (Lehre) *of mass occurrences in human societies and social life* (Gattungsleben) *of nations and especially of the regularities and order which become there noticeable*.

Remarkable features of a nation insistently sought out by university statisticians had been excluded from statistics as were the notions mentioned by Kries (§ 1). Even A. L. Schlözer (1804, p. 11) only thought about the *moral state of the population*, but certainly did not say anything about its measurement. Kries' statement (1850, Introduction) that statistics is a branch of Staatswissenschaft ceased to be true. Note that neither Schlözer, nor Gherman applied the term *Staatswissenschaft*.

Many early authors thought that statistics was a science. Butte (1808) proclaimed it in the title of his book; Schlözer (1804, p. 58) stated that without order and system statistics was not a science; he possibly thought that that unattained condition was sufficient. Even Gherman (§ 2) by separating statistics from some sciences contradicted himself and recognized it as a science as well.

The notion of the theory of statistics apparently changed with time so that Cournot (1843, § 105) declared that statistics ought to have (e. g., did not yet have) its own theory, rules and principles. Much later Chuprov (1905, p. 422) remarked that statistics had no generally accepted principles for corroborating its conclusions or the expediency of its methods.

A queer episode followed: Chuprov (1909) called his book *Essays on the Theory of Statistics* but (p. 20 of the edition of 1959) agreed with a German author that *a clear and rigorous justification of the statistical science was urgently needed*. And, just below, Chuprov mentioned the *lack of a clear theoretical foundation*.

Issuing from Schlözer's opinion, I think that statistics fortified by its theory, i. e., resting on mathematical statistics and the theory of probability, is a science. However, I replace here mathematical statistics by its theoretical analogue, which means that I complement the former by the collection of data and their preliminary investigation.

Zahn said nothing about the application of the theory of probability to statistics, but German statisticians are known to have been sharply opposing it for many decades. Haushofer (1872, pp. 107 – 108) declared that statistics, since it was based on induction, had no *intrinsic connections* with mathematics based on deduction. Knapp (1872, pp. 116 – 117) stated that the law of large numbers was barely needed since statisticians always made only one observation, as when counting the population of a city.

Maciejewski (1911, p. 96) introduced a statistical law of large numbers instead of the Bernoulli proposition that allegedly impeded the development of statistics. His own law qualitatively asserted that statistical indicators exhibited ever lesser fluctuations as the number of observations increased and his opinion likely represented the prevailing attitude of statisticians. Bortkiewicz (1917, pp. 56 – 57) thought that the law of large numbers ought to denote a *quite general* fact, unconnected with any stochastic pattern, of a degree of stability of statistical indicators under constant or slightly changing conditions and a large number of trials. Even Romanovsky (1912, p. 22; 1924, pt. 1, p. 15; 1961, p. 127) kept to a similar view.

The situation changed slowly although even Woolhouse (1837,

p. 37), for example, stated that statistical investigations are closely linked with the theory of probability. Chuprov published two German contributions (1905; 1906), but their impact was barely felt and Wolff (1913, p. 31) stated that Chuprov was hardly a statistician. Even later Kaufman (1922) denied all the new ideas and methods of mathematical statistics. Sampling only became definitively accepted at about the same time although a century ago Lueder (1812, p. 9) had noted the appearance of *legions of numerical data and statistical tables filled with numbers*.

But to return to Staatswissenschaft. A noteworthy statement was due to Chuprov (1922, p. 339):

The worthy creation of the German university statistics certainly should not be returned from its grave where it had been dozing for a century in its previous state. It will arise rejuvenated and smartened up, but, under its contemporary look it will display its previous face which it had at the times of Achenwall and Schlözer.

As an independent science, statistics will become a systematic description in time and space of the remarkable features of the various clearly delimited social forms. … Numerically described mass phenomena will occupy the forefront without however attaining absolute dominance. Otherwise statistics will not at all be able to establish itself as an independent science.

Although Chuprov himself had studied the application of the statistical method in natural sciences, he did not mention this topic here. A. L. Schlözer (1804, p. 21), however, noted that in France the term *statistical meteorology* had already appeared. He apparently referred to Lamarck (1802, title and p. 300).

The regeneration of Staatswissenschaft, its partly transfer to political arithmetic had apparently occurred in the mid-19th century. It exists nowadays, and at least in Germany it is taught at some universities. Statistics is obviously needed in natural sciences, in national economy and by governments. Perhaps in spite of Chuprov's opinion numerical data do not appear in the forefront of Staatswissenschaft considered as a whole, − considered as *an application of the statistical method to the various aspects of the life of states*.

5.1. Christian von Schlözer and his booklet. According to Hugelmann (1890), Christian von Schlözer (1774 – 1831), son of A. L. Schlözer, became Doctor of Law at the age of 22 and, five years later, Professor at Moscow University. He returned to Germany as Professor Emeritus a few years before his death and was an *Extraordinarius* at Bonn.

The scientific library of Moscow University keeps 13 of his books, mostly in Latin and German, but he is barely known. I describe his booklet (1827), his own translation from its original French edition of 1823. On the title page of that booklet he described himself as a

Professor Emeritus of natural economy and diplomacy at the Moscow Imperial University, an Honourable Member of the Kremlin Armoury and an honourable, a full, or a corresponding member of various scientific societies in Moscow, Petersburg, Mitau (Jelgava)*, Königsberg and Göttingen*.

Below, I translate the Introduction to his booklet. Its main text consists of two parts, *Theory of Statistics*, which testifies to the author's understanding of the essence of statistics, and *Theory, or Philosophy of History*. There is also an *Appendix*, a reprint of a part of his report (1822). At the end of the Introduction Schlözer indicated that he had enriched the theory of statistics with *many new ideas* and stated that history has its own theory as well. He did not justify his former claim.

Schlözer himself remarked that his booklet will only become useful after a report (I would say, after reports) about the sketched topics. In essence, he said as much on his title page: *For application at my lectures*. Indeed, the topics in both parts of the booklet are listed haphazardly, and only a shortest comment on them can be useful. I only note that

1) The description of climatic belts is obviously unfortunate. In particular, he should have compared, in the first place, not the Old and the New World, but the northern and the southern hemispheres (not forgetting Australia and especially its animals). Then, C. Schlözer evidently did not know that Humboldt, in 1817, had introduced isotherms and continued to mention *mathematical climates*.

2) Schlözer said absolutely nothing about the visitations of small pox and cholera which had been devastating both Europe, and at least parts of the New World and Asia. He could have also mentioned the classical memoir of Daniel Bernoulli on prevention of small pox.

5.2. Translation of the Introduction to C. Schlözer's booklet (1827)

Bearing in mind the sciences whose contours I am now offering to my respected listeners, I can only add a few remarks. In accordance with its notion, in Germany, the theory of statistics has for a long time been generally known. However, only 20 years ago my celebrated father had formulated its entire significance[1]. In his opinion, the theory of statistics rather than *practical statistics*, or *statistics in its proper sense*, as it is usually thought, should be recognized as the most important part of that science.

Gradually, entire Germany had adopted his system, and most universities there offer special lectures on the theory of statistics. My father was apparently in the right when he stated[2] that by means of this science a thinking young man can imagine the statistics of any given nation and as though create it for himself. Or, in other words, can achieve a clear idea about its might, culture, richness, constitution, and government.

If the theory of statistics is not alien to him, he will most easily do without any reports[3], only by reading statistical works or even by using statistical sources. On the other hand, it is also true that without the support of a *general theory*, even the widest and most precise information about isolated statistical facts invariably provides *a crude, incomplete and disordered* knowledge.

However, if my celebrated father was able to *improve the method of studying statistics*, and especially its theory, I, for my part, attempted to *enrich* the theory of statistics with many new ideas. At least to excuse, if not justify my impudence, I may refer to an utterance of my father himself (A. L. Schlözer 1804, p. 125): *We are* [a gap in the text; 40, as stated by Schlözer the elder] *years younger and will not be dim-witted when going further* [than Achenwall did].

And, under similar circumstances, I have already obtained a soothing and encouraging experience when, 22 years ago, I compiled a reference book on natural economy [apparently, the book of 1805 – 1807]. I courageously renounced all the mistaken propositions then received by the best German authors and teachers of politics. Nevertheless, my book, *from the very moment of its first appearance*, had suddenly been most favourably accepted and until now is being applied as a manual in Russia and Germany, probably in Poland[4] as well. Otherwise, had I blindly followed the old theory, my manual would have been forgotten long ago.

Finally, let it be allowed to mention as a justification[5] one more, last fact. Almost all of my views with which I have attempted to enrich the theory of statistics have been expounded three years ago in my scientific reports and met in the best possible way by the Moscow scientific community. These reports, which I do not regard as useless, to a certain extent expound my new views on which [whose?] recent tabular booklets[6] are partly based.

The second science which I am here describing is *the theory or philosophy of history*. For most of my listeners this science will seem entirely new, and so it is with regard to its repute with which I impart it, if not to its subject. Even during the first instruction it resembled a very useful and deep booklet of my late father (1779)[7]. What I am now offering as the theory or philosophy of history is just *a sequel of that booklet*. In other words, my theory is a *preparatory course on history for adults*.

Following my father, my booklet aims at *laying the foundation of a certain method* for studying history, i. e., for presenting on a few pages the subject of this science in a general way. Then, like my father, I am offering you a large number of considerations, apparently simple and superficial, but which actually are the fruit of long and diligent studies. In any case, readers can argue that the events expounded in history had been occasioned by blind *chance*, that history has no invariable or definite subject; that each isolated fact as though constitutes a whole; that it is therefore impossible to establish *general historical principles* for applying them to isolated facts. I ought to fear that such objections will mostly be voiced by those who are narrow-minded to such an extent that they believe that history is only concerned with *battles, conquests, personalities, dates and changes of power*, that nothing except a good memory is therefore needed for a sound mastery of that noble science. Each such objection will be absolutely groundless.

Indeed, the so-called chance does not at all possess the significance usually attributed to it. I think that a battle can be lost because of an unforeseen random occasion, for example, when a sudden storm blinds one of the armies. However, *blind chance* had never destroyed an entire state after its long or even fleeting well-being, and never had any other state become rich, cultured and mighty by chance.

Actually, the change in the destiny of peoples is indeed based on eternal and invariable moral laws just as the changes in the material world are governed by physical laws. Our restricted mind is sometimes unable to study all the various latent conditions which stipulate the application of either kind of laws[8]. However, experts often allow themselves to derive close connections in politics and the theory of statistics[9] and reduce causes and effects to laws when only blind chance is apparently suspected.

Some other considerations are connected with those mentioned above. If special and random differences are disregarded, we invariably note essentially the same. We always have the same needs and inclinations and we therefore often reveal remarkable similarities in the spiritual and secular organizations and institutions of nations most remote from each other.

And exactly this feature of similarity constitutes the first topic of my theory of history as described in its first section (A). On the contrary, in section (B) I mostly turn my attention to the already existing large national associations and states. Institutions and facts are there much more diverse than in case of populations living in childlike conditions. And as soon as such populations become considerably more numerous and start advancing in culture, richness and might, they also begin to deviate from their exact similarity.

In many respects peoples become distinguished from one another and develop as though by following different ways. I hope that my readers and listeners will not therefore reproach me for being inconsistent since in section (B) I consider *differences* whereas in (A) I only establish resemblances and similarities. This is occasioned by the essence of the matter. There surely was a period (I am offering just one example) when the Romans had in some aspects been just like other Italiotes[10], but they gradually constituted a nation as indicated to some extent in history.

The same occurred with many other peoples. Recall that children are more alike than adults although those latter who attained a higher social standing again less differ from each other. And still there exists some similarity in the destinies of the larger associations of people, some common connection between them. The same causes that bring about the fall of one state or the increase in the richness, culture and might of another one determine the destiny of each state and they are usually more or less the same. Assisted by our science, my respected listeners will therefore be able to reveal the relations existing between separate nations about which they never had any inkling.

This study also protects us from impulsive judgement to which we are too often led by insufficient information about isolated subjects such as the origin and kinship of nations, linguistics, establishment and development of states, origin and advances in culture, causes of the increase in richness and political might of states. I therefore flatter myself with hope that in both indicated main directions my theory of history will be worthy of attention and approval of my listeners. Exactly the theory of statistics will allow you, when studying the theory of history, not only to consider subtly the history of any people or state, but, as my late father had put it, to compile the history without attending any special lectures.

As to the sources which provided the materials for my theory, I ought to refer preferably to two contributions[11], one of them by my late father (1772), and the other one, by the illustrious Adelung (1806). Most of my views are the fruits of my own historical and ethnographic studies. In this respect, the 30 years of my life in Russia inestimably benefited me. It provided me with a possibility of clearly, and sometimes to a certain extent with my own eyes seeing people of various nationalities existing under all possible conditions from the primitive state up to the highest form of civilization and, moreover, belonging to every level of social life.

One more remark. This booklet was initially intended for the students, the sons of the gentry, of the Moscow boarding school at the Imperial University. Its real usefulness can only be derived in reports on the topics themselves which could have been only mentioned here. Indeed, even in this Introduction it would have been necessary to provide various explanations by pertinent examples which I will certainly do [in my future reports]. Concerning some other topics, for example, classification of various occupations and trades, no further explanations are required. I have added these latter topics for the sake of comprehensiveness so as to indicate all the objects demanding the attention of the nation's government, if, as I stated in the second edition of my book on national economy[12], it intends to be informed *about the richness of its subjects and the various productive and consuming classes as precisely as the father of a family, be it rich or poor, wishes to assess the situation in his household.*

5.3. A few words about the main sections of that booklet. I only translate the main headlines.

[Section 1.] Theory of statistics

Preliminary notions

The main objects of the theory of statistics

A. The main forces of the state

1. People
2. Land
3. Riches

B. Unification of forces or the constitution. General remarks

C. Management of the forces of the state, or the government

1. Managerial branch
2. Branches indirectly aiming at maintaining security of the state
3. The branch of the management of all kinds of objects

[Section 2.] Theory or philosophy of history

A. Preliminary notions about any historical study, preferably about the primitive state of peoples

B. Preliminary notions about the history of an entirely formed small or large state unity

5.4. Appendix

As noted above, in my report (1822) I have openly indicated the insufficiency of Staatswissenschaft or of statistics as a science in its previous sense. I may be so bold as to flatter myself that, according to the unanimous opinion of my Russian readers my report was sufficiently clear and comprehensive and therefore easily understandable even by those less accustomed to the Latin language. And I think that it is expedient to repeat literally its part concerning the problems discussed here, the more so since in accord with its essence my report cannot become known in Germany. [That part of the report is reproduced (in Latin).]

Translated by the author from the French edition of this book (Moscow, 1823) and somewhat enlarged by him.

Notes to § 5.2

1. *I turn statistics over and mostly discuss its theory* (A. L. Schlözer 1804, p. 91). [Above, he (1804) indicated that the German universities only teach the theory of statistics as an introduction to *practical statistics*. O. S.]

2. *Thus, by applying this science beginners can easier than by means of the previous method learn the art of studying the statistics of some state and, so to say, of creating it* (Ibidem).

3. The last phrase also concerns practical statistics. Witness the excellent words of the programme of the lectures of this year at Breslau (p. 4):

Etenim res in scholis traditae et quae dictare conscribique possunt partem tautummodo faciunt disciplinae Academicae, ex libris interdum certius petendam etc.

And, if truth be said, the *quantities and lists of commodities,* which had previously been thought to be the essential component of practical statistics, are so unconvincing in a report. Just the same, it is so unimportant for listeners to write them down, or, which is the same, they require the sacrifice of the mostly *insufficient precious* time for rewriting them especially if considering in addition that only 30 years ago lecturers on many sciences had been attempting to overload their talks by empty quotations.

On the contrary, the topics of the theory of statistics are *notions, considerations, opinions,* and the expression *singularum rerum pondus* etc. mentioned in that programme is extremely suitable for our science just as reports quite agree with philosophical, physical and other sciences.

4. I justified it in various generally available booklets written on occasions.

5. Above, Schlözer only aimed at excusing his *impudence*. O. S.

6. Incidentally, a very flattering anonymous review of the two of my booklets in the *Leipz. lit. Z.*, Jg. 1825, and another one no less flattering compiled by renown authors in the local *Göttingische gelehrte Anzeigen* for the same year prove how well I was able to attain my goal of enriching the theory of statistics as indicated above.

7. See Niemeyer's *Pädagogik* and a brief biography of my father in *Zeitgenossen*. Note that many adults could have beneficially read his booklet. It was translated into almost all the accomplished European languages, and some of them twice, for example into French and Russian and had appeared in Germany in seven or eight editions. [See Niemeyer (1796). The journal *Zeitgenossen* had been published in Leipzig in 1816 – 1841. O. S.]

8. I recall hearing a very shrewd remark at a lecture of the learned and witty Lichtenberg and it is quite suitable for interpreting that fact, but perhaps I reproduce it only approximately:

We can calculate the motions of planets, but no mathematician has yet been able to foresee the outcome of a die cast which is based on the eternal laws of gravity to the same extent as are those motions. Indeed, the arrangement of the die [on our palm] and its relation with one or another muscle are unknown to us.

[G. Chr. Lichtenberg (1742 – 1799), an experimental physicist and satirist. I do not see anything shrewd in his remark. O. S.]

9. Actually, even my celebrated father (A. L. Schlözer 1804, p. 86) had called history itself *statistics flowing*. However, for studying any given statistics we need *the theory of statistics*. It is for this reason that my readers and listeners will not be surprised when discovering at the end of my theory of history a certain similarity between the topics whose study requires the attention of an investigator of the history of a given state and some topics of the theory of statistics.

Clearly, however, when previously information had been lacking, it was impossible to study separate facts as it is done today. Now, everything is therefore seen wider and in less precise masses.

10. Italiotes: pre-Roman (!) Greek-speaking inhabitants of the Italian peninsula between Naples and Sicily. O. S.

11. Understandably, I mean the learned and celebrated Adelung from Dresden. [Joh. Chr. Adelung (1732 – 1896), a philologist. O. S.] Both contributions are full of new and radiant ideas about ancient history, methods of ethnographic studies etc. My statement mostly concerns A. L. Schlözer. Apart from other places, see the remarks on his pages 211, 212, 222, 263, 271, 273, 275 and 306.

It even seems that the excellent Adelung had borrowed some of Schlözer's ideas which had not at all been as widely known as they should have been, and which, moreover, are nowadays almost forgotten. For example, we still hear superficial chatter about Scythians and Sarmatians although even 60 years ago my father (1772) had shown that (just as in the case of Negroes, Siberians, Indians et al) these names are *empty*. They should not now be mentioned in reasonable historical contributions.

Without yet being acquainted with some of Adelung's original ideas, I have expounded them in a competitive paper on the present situation of the history of ancient Russia. My paper had indeed won the prize of the Moscow Imperial Society for Russian History and Antiquities. I was unable to read Adelung's book since it only appeared when I had been compiling my paper and even outstanding German books had often only reached Moscow a year or a few years later.

My celebrated father had also participated in that competition although I did not know it. His characteristic style at once revealed his authorship, but he did not win the prize since, in an outburst of low spirits, instead of answering the question he attempted to prove that it was inadmissible (which was not altogether true). And he therefore selected for himself the motto *Ignorare malo, quam commenta credere* [I prefer to ignore rather than to trust comments (?)]. I had not at all been suspected of being an author of one of the competitive papers, otherwise, owing to various reasons and even because of a [negative] opinion about my father, I would not have won the prize.

I have written and presented my paper in German, but it was published in Russian. It also became necessary to remake it and submit it in Latin since its topic attracted readers of history and all the investigators of the development of Slavonic nations certainly more numerous in present-day Europe. An official announcement and a testimonial about my paper are in the *Göttingische gelehrte Anzeigen* for 1808.

12. I can only name the book C. Schlözer (1805 – 1807). O. S.

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I. M. Ch. Bartels

Several statements concerning N. I. Lobachevsky

V. F. Kagan, *Lobachevsky*, 1944. Moscow – Leningrad, 1948, in Russian

I translate several passages written by Bartels (professor at Kazan university from 1808 and at Dorpat, Derpt,Youriev, Tallinn from 1820) and quoted by Kagan. They concerned Lobachevsky and, to a lesser extent, Ivan Mikhailovich Simonov (1794 – 1855). Astronomer Simonov became professor at Kazan in 1816, rector of that university in 1846 until ?, and corresponding member of the Petersburg Academy. On Bartels see Depman (1950) and Biermann (1975).

1. *Kagan, pp. 31 – 32.* My lectures much pleasured me since most of my listeners had attained good success.

This was Bartel's report to Razumovsky, the custodian of the Kazan educational region. Following Bulich (1887, pt. 1, pp. 246 – 247), Kagan quotes Bartels' report, written in the beginning of his work at Kazhan, in Razumovsky's own translation from Latin which he submitted to the Minister of people's education.

[Simonov and Lobachevsky], and especially Lobachevsky, attained such success that even in any European university they would have been excellent students. I flatter myself with hope that, if continuing to perfect themselves, they will occupy important places in mathematical circles. I adduce at least one example of Lobachevsky's skill.

I arrange my lectures in such a way that the students are at the same time listeners and teachers. Before ending my course I have therefore charged Lobachevsky to offer [the students] under my guidance an extensive and difficult problem about rotation. Following Lagrange, I had already worked it out for myself in an understandable way. At the same time I ordered Simonov to write down four steps of my teaching to communicate them to the other listeners.

However, after the last lecture, disregarding these [Simonov's] notes, Lobachevsky gave me the solution of that such intricate problem on a few quarto pages. Academician Vishnevsdky was then present and became unexpectedly delighted by this small specimen of the knowledge of our students.

As a consequence of that opinion [of Vishnevsky] Lobachevsky received thanks from the Minister. V. K.

2. *Kagan, pp. 32 – 33.* Bartels began teaching by following textbooks which occupied as though an intermediate position between manuals for high school and universities. This is what he himself wrote (Bulich 1887, vol. 1, pp. 241 – 242): V. K.

For first-year students I chose the Cagnoli [1786] trigonometry. It assisted me in explaining the preliminary notions about the theory of series and differential calculus and at the same time it ensured thorough information about both trigonometries. However important is that book, I considered it necessary to change fully its exposition in many places and to keep to it only in general.

A very pleasant experiment which I have thus made on my listeners showed me that my method was wholly advisable. I was able to move even their weakest so that they can rather easily solve almost all trigonometric problems. They also mastered differential calculus and the theory of series to such an extent that became able to apply their knowledge to logarithmic functions. There was [naturally] a great difference between those students and those who had time to digest what I had explained them.

For the second year students I expounded number theory according to Gauss (naturally, only some chapters) and, in much detail, differential calculus to prepare them for the third year when I shall turn over to analytic geometry and mechanics.

Cagnoli (1786) is undoubtedly a rich and instructive course in trigonometry. It thoroughly sets forth the main properties of trigonometric functions and their application to geodetic and astronomical calculations. The elementary principles of differential calculus are added up to study small variations. As usual for the 18th century, the methods are not irreproachable but this book is useful for general mathematical education.

Bartel taught analysis according to Lacroix (1797), doubtlessly the best at the time course in elementary and higher mathematics. Then he went to Euler and compelled the advanced students to read serious and difficult mathematical compositions, for example, Laplace's *Exposition du système du monde*. It is somewhat puzzling however that Bartels postponed analytic geometry to the third year. V. K.

I wonder whether Bartels or any student (especially Simonov) noticed Laplace's unforgivable mistake: just like Kepler and Kant, he ascribed the eccentricities of planetary orbits to differences in the temperatures and densities of the planets whereas Newton had proved that they were occasioned by the velocities of the motion of the planets. O. S.

3. *Kagan, p. 38.* When being already in Dorpat, Bartels (1833, p. IX) described his Kazan students in a short autobiography which preceded his course in analysis.

To my great joy, in Kazan, although there were not many students, I discovered their unusual interest in mathematical sciences. In my lectures in mathematical analysis I could have expected to have at least twenty students. Gradually I built up a small mathematical school from which many fine teachers had emerged for Russian gymnasiums and universities, and especially for the Kazan educational region.

And here is how Bartels contrasted the previous Dorpat university with Kazan: V. K.

Here, I found much more students, but among them there were much less lovers of mathematics, and in my lectures I had to restrict myself mostly to elementary mathematics.

4. *Kagan, pp. 47 – 48.* On 10 June 1812 Bartels reported to the Council of the [Kazan] university. I found its Latin text in an unpublished book (Vasiliev, *Zhizn i nauchnoe delo Lobachevskogo*, Life and Scientific Pursuits of Lobachevsky). V. K.:

At the beginning of this academic year I took upon myself the guidance of a deep study by Masters Lobachevsky and Simonov and a report about their work. I am submitting this report all the more willingly since I am happy about the success of my efforts. In my private lessons, I explained them a large [apparently, *the greatest*; the Russian expression is ambiguous] part of the first, and an essential part of the second volume of Laplace [of the *Méc. Cel.*]. Our Masters had not only studied this material with a remarkable studiousness, they attempted to progress wherever possible all by themselves. …

Although Simonov has well progressed in mathematics, Lobachevsky excels him, especially in the higher chapters of mathematics. His communication which he elaborated all by himself excluding the work of the glorious Laplace shows that he had not only studied the material contained there but supported it by his own ideas. That short communication of our outstanding mathematician, who in due time will not fail to earn a glorious name, includes indications which are hardly suitable to be described here.

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O. Sheynin

On A. N. Kolmogorov's letters to V. P. Efroimzon

Introduction

1. Vladimir Pavlovich Efroimson (E.), 1908 – 1989, was a geneticist, Doctor of biological sciences and co-founder of national genetics. In 1929 he was expelled from a university for defending Chetverikov, a most prominent geneticist, and I have not seen anywhere that he had ever graduated from a university.

In 1932, he did three years for participating in the Free philosophical society. I did not establish it, but anything *free* was an anathema! In 1949 – 1955 he did time once more for allegedly slandering the soldiers of the Red Army in the aftermath of the war. And then E. experienced great difficulties when applying for a post. The usual true cause of his new difficulties (to put it mildly) in the post-war period was his ardent denunciation of Lysenko, Stalin's battering ram for subduing the entire science. In the above, I used the entry on E. in vol. 10, 2001, of the (*Kratkaia*?) *elektronnaia evreiskaia enz.* (Short (?) Electronic Jewish Enc.). It seems to be translated in the Internet.

E.'s study of Lysenko and Lysenkoism was published in pieces in all four yearly issues of *Voprosy Istorii Estestvoznania i Tekhniki* in 1989. There, E. (No. 3, p. 102 note) added, regrettably without substantiation, that genetics was rooted out in Nazi Germany.

Fisher (1948) also attacked Lysenko, and in addition I quote the opinion of Kolman (1982, pp. 213 – 214):

I was disgusted since his opponent, the official Vavilov school at the Academy of Agricultural Sciences, had for a long time prevented him from practically proving his innovatory ideas, slighted him since he, a provincial agronomist-breeder lacking higher education, invaded the sanctum sanctorum of those pontiffs of science. And I was delighted by the enthusiasm with which he developed his concepts.

At the beginning he sincerely believed in being right and ardently upheld his ideas, but, after gaining authority and having felt power, he did not mind anymore to apply administrative, forceful methods of struggling with his convinced enemies. Who knows whether he himself had not repeatedly participated in hounding them to death or that he did not "doctor" his experiments if they had not confirmed his theory.

In December 1985, during a premiere of a film documentary about Vavilov, the leading Soviet geneticist and a member of the Royal Society, E. spoke out without permission, not mincing his words:

Vavilov did not die [in the labour camp], *he croaked like a stray dog from hunger and cold.*

Oh, yes! Vavilov was guilty since he impulsively promised that very soon genetics will achieve grand practical results.

E. also stated that the Soviet Union was a *land of slaves governed by nomenklatura thugs* (shpana).

E.'s main works are (all in Russian): *Genetics of Genius* (Genetika i genialnost), 1998. Apart from its constitutive writing it contains

Pedagogic Genetics (Pedagogicheskaia genetika), 1998 which was written in 1974 – 1977, and a paper *Origin of altruism* first published in 1971 in an adapted form. Apparently not included was *Genetika etiki i estetiki* (*Genetics of ethics and aesthetic*) (1995). I also mention the included manuscript *Preconditions of Genius* and a manuscript on the history of Jews (not included). Not included either was *Vvedenie v medizinskuyu genetiku* (Intro. into Med. Genetics). The proof of E.'s early Russian contribution *Genetics of Silkworm* was scattered perhaps of some infringement on dialectical Marxism.

And here is a quotation from *Preconditions* … (part 1, end of chapter 4) which hints at Israel, cf. Note 7:

Even a small country of, say, five million inhabitants, but having developed and realized 10% of its potential geniuses and talented men, will after 50 years leave behind a country of a hundred times more inhabitants which left barriers for the development and realization of its potential geniuses.

I glanced at *Genetics and Genius* in the Internet and E.'s statements described in my Notes are from that source that lacked paging.

2. Kolmogorov's letters likely contain something barely known about his work with school students and his interest in psychoses. Incidentally, I think that he mentioned Stalin's psychosis or psychoses in one of the places blackened by the Archive. And now a reservation: Pontriagin (1980), that anti-Semite supreme alongside Vinogradov and Shafarevich, justly stated that Kolmogorov's recommendations concerning school students in general were sky-high above reality.

In a mildly form the chair of mathematics at the Plekhanov Institute in Moscow where I worked remarked that the graduates of the Kolmogorov boarding school were not attuned to applied mathematics.

Yes, geniality is fraught with inconvenience for ordinary people, and I myself, experienced Kolmogorov's impatience on a tiny scale (Gnedenko and Sheynin 1978). I noted the appearance of the Dirac delta-function in Laplace, but Kolmogorov, the main editor of the source, struck out my discovery since it made no sense in the language of generalized functions (although was still noteworthy!).

Concerning § 15 I note that Kolmogorov was a Russian (but certainly not a Soviet) patriot. As a hardly needed illustration I recall a chair of mathematics in Berlin, a Russian, telling me that Kolmogorov once swam in cold water and commented afterwards: *We are Russians, not Germans* or words to that effect. See also Letter 2, end of Item 3, and Note 15. It seems that at heart Kolmogorov was devoted to socialism *with a human face*.

The very fact of Kolmogorov's correspondence with E. is noteworthy. He also talked with E. over the telephone (§ 15, P. S.) and mentioned their future meeting (did it occur?). It remains unknown which of his contributions had E. sent Kolmogorov. But anyway, it was likely a draft.

3. But where are E.'s letters? Here is an edited text of my tiny publication (*Math. Intelligencer*, vol. 39, No. 4, 2017, p. 46):

Where are Kolmogorov's posthumous papers?

In a worthwhile tradition, the Archive of the Russian Academy of Sciences (RAN) collects and keeps the posthumous papers of its late members. Kolmogorov died in 1987, so I asked the Academy for permission to look at his papers. I found that RAN did not have them. Staff at their Archive advised me to inquire at the Archive of Moscow University where Kolmogorov had been a staff professor. I had twice inquired there but received no answer and asked the Presidium of RAN. An anonymous representative from the Class of Mathematical Sciences answered in writing that nothing was known about Kolmogorov's papers. Period! They obviously did not dare say anything more.

A colleague told me that Albert Shyraev, professor at Moscow University, perhaps keeps those papers. Twice I wrote to him but received no answer.

Shyraev (albertsh@mi.ras.ru)! He hurriedly published a paper (1989) which described Kolmogorov's merits in mathematics complete with a list of his publications. After its extremely superficial examination I found two omissions; in addition, translations of his works were not mentioned. But the main point is that Shyraev is unscrupulous. Novikov (1997) explained how irresponsibly he managed to promote that crazy Fomenko and in his § 3 washed his hands of the business: *Allow me to keep silent about Shyraev's role*.

Another episode is insignificant as compared with the above but just as disgusting. In 2001, the yearly journal *Istoriko-Matematicheskie Issledovania* published a paper by Yu. V. Chaikovsky who, without even a trace of justification, invented the Jacob Bernoulli – Cardano law of large numbers. I was member of the editorial board, did not know anything beforehand and resigned. The Editor, S. S. Demidov, *explained*: Shyraev recommended the manuscript. Such an obliging person is really needed, and he is now President of the International Academy of History of Science. That scientific body had however degenerated and is hardly needed at all.

Quite recently I searched for Kolmogorov's papers anew and it really seems that the situation had not changed. Furthermore, I found out that the financial circumstances of the Archive of RAN are horrible so that even their inestimable treasures are in danger.

Kolmogorov wrote both You, Yours and you, yours and I left it at that. A few sentences were grammatically wrong and I corrected them. Then, some words were also grammatically wrong and I italicized them in translation which sometimes seems curious.

Both letters from Kolmogorov are kept by the Archive of RAN, Fond 2024, Inventory 1, Delo 354, pp. 1 – 11

Letter 1, 10 Dec. 1977

Dear colleague, I am sending you my remarks to ensure at once the possibility for you to decide to what extent we are fellow-travellers and not to form exaggerated assumptions. I found much interesting in your manuscript and hope to find *notlittle* (ne malo) at our meeting.

1. About the genius of great men

At the beginning of the previous century France needed one single emperor. It is very difficult to estimate how many candidates potentially fit for filling that post were among the officers promoted by the revolution. Similarly, it is unclear what measure of "genius" in military leadership, management and personal courage needed Joan of Arc for accomplishing her mission. The information which you found in the *Larousse* dictionary[1] about her constitution is very interesting. It is naturally connected with her disposition to have hallucinations. Then follow the milieu and the sense of her exceptional mission. The war [apparently, the German – Soviet war of 1941 – 1945] showed us that in an appropriate situation courage reaching utmost limits is not so exceptional.

2. General and specific endowment

I was extremely surprised and, I would say, saddened by what you had stated about that subject on pp. 45 – 47. A "titanic purposefulness" without a proper point of application seems to me *some what* (chem to) abnormal and quite *un desirable* (ne zhelatelno). Then, to discern in proper time and cultivate special gifts is not at all simple and is [even] central for the system of upbringing.

3. To take care of the children of talented men and geniuses is naturally the duty of their parents[1].

If a talent is really inherited, it usually does not vanish. More important is the problem about the inborn and acquired components of talent. How strong is the former if talent had not revealed itself in previous generations because of its "polygene" feature. This problem interests us when planning a system of upbringing and education.

4. Geniuses, talent and psychoses

When assuming that the manic depressive psychosis[1] and schizophrenia are the two main psychoses it would be natural to turn our attention on both. As it seems, this is indeed happening in the literature. I know well enough the data on Moscow mathematicians. Quite pronounced manic depressive psychosis **text blackened by the Archive**. Schizophrenics among us are much oftener. You certainly know better, but the situation with musicians is possibly the same.

5. Gout

For me, this subject was new and as far as I understand, your great work of four years was inserted just there [devoted …]. I do not dare criticize your final conclusions, but I note that the method of statistical comparisons with a constant frequency of 0.4% seems to me unfounded[1].

The frequency of gout in various social strata and times is apparently sharply different so that comparisons should be made between homogeneous groups. I have not statistically studied it, but after simply following our classical literature it apparently becomes possible to establish that the gout was most widely spread among the Russian nobility of the nineteenth century[1]. And the frequency of gout among talented men and geniuses, who belonged to that nobility, ought to be compared to that particular frequency.

It is possible to approach such comparisons by selecting a random sample of those families which had not revealed special talents and studying the archives of their remembrances and letters.

6. Uric acid

Is it possible to check directly the correlation between its concentration and mental activity? Otherwise the hypothesis remains not too convincing[1].

7. Four mechanisms

One of them is constructed on a single example of Joan of Arc. The second one on three examples (Lincoln, Anderson and Prof. Nikolsky[1]). I would not name them in a general reasoning. But the existence of the "schizoid" type of talented men and geniuses seems doubtless.

Are not the chemical and hormonal explanations too categorical? The example of enormous quantities of drunk coffee (Napoleon and others) does not seem fortunate. It will be then too easy to become the emperor of France.

I got the impression that you regret the impossibility of stimulating talent and genius just by inserting uric acid. And *there fore* (iz za) you recommend much more complicated methods of stimulation.

8. Kolmogorov left out this number

9. Selection of talent

In the narrow field of mathematicians I may be considered a specialist. The boarding school which I head provides *notbad* (ne plokhie) results. We have to select at age fifteen. Had we better possibilities of establishing summer camps for teenagers 13 or 14 years old and of selecting from them after becoming closely acquainted with each , we would have preferred this lesser age. But still 13 years seems doubtful. And I will certainly advise not to bother with those of 12 years.

<u>Specific</u> abilities which we need are apparently formed later. Most members of mathematical study groups for those of 12 years later scatter. Girls occupy there the first place but already at 15 years of age most of them lose interest in mathematics.

This certainly does not mean that ability is not needed for studying mathematics. Suitable training should begin earlier. But this general quick-wittedness and ingenuity can be successfully developed even by fishing, birdwatching, playing games etc. Gifted "wild" boys, if being interested in mathematics at age 14, can already at 19 publish their own scientific work. **Text blackened**.

In music serious training of receptivity and technique should certainly begin earlier, and still earlier for circus performers and sportsmen since sport became professional[1].

10. Speeding up development

Freedom is *undoutedly* (nesomneno) better, therefore we certainly should not prohibit external school-leaving examinations. I think however that both parents and teenagers should be warned that that method is dubious. And in any case no preparatory summer schools for external examinations ought to be established.

I have a rather considerable personal experience of work with child prodigies and in particular of their considerable frustration which happens sometimes. I tell them that, in the gymnasium, the greatest mathematician of our century, Hilbert, as he himself said, did not hurry too much to study mathematics since being sure that in due time he will become an excellent mathematician[1].

11. Tests

The ban on tests is now lifted. Our Academy of Pedagogic Sciences applies them and, in appropriate instances, recommends tests. They certainly reign supreme in the Anglo-American world. Last year I attended an International Congress on Mathematical Education in Karlsruhe, Germany. And I can confirm that in our field, in Germany and France, tests play a most modest role. They strongly criticize the English system of selection of 10 – 12 year old children for entering "grammar schools" which lead to universities.

In France and Germany I feel myself at home, but I little know America [the USA] on the occasion of having *nocommand* (iz za nevladenia) of English[1]. But still I suspect that you exaggerate the value of the MERIT programme[1]. In America [in the USA] everything at once assumes an immense scale and is skilfully advertised. But did this programme become the main method of promoting gifted youths? I will ascertain this as far as it concerns student-mathematicians by asking my American colleagues. For the present, I would be grateful for indicating the materials which are at your disposal.

12. Early childhood

Here, I quite sympathise with you. Allowing for all the conditional character of the IQ, a publication of the data based on that indicator would have been useful[1]. I heard that considerable measures to ensure the mothers a possibility of remaining home with children during the first years of their life are, or are being implemented in Hungary. If you speak out on this subjects you need to inquire and secure precise information.

13. Cutting down the size of school classes

It seems that *some thing* (chto to) is done in that direction once more in Hungary. In France, until recently, classes were separated in two groups, as it is done here with respect to foreign languages[1]. Gabi had recently abolished this system but, instead, curtailed the size of classes to 30 school students. Regrettably, 40 students are already planned here for many years ahead. It would be very important to achieve changes here.

14. Editing the proposals

For justifying your proposals (§ 16 [where is it?]) very little is needed from genetics and age-specific psychology. It would be reasonable to restrict the appropriate document by the necessary only.

15. Patriotic motifs and the criticism of capitalism

In a paper and in the respective [future] address such passages are extremely unfortunate. "Plutocracy" which "was compelled" to allow the democratic forces to enter its milieu etc. are fantastic [expressions] and will favourably impress no one.

With deep respect [signature follows]

PS. I found the book of Volotskoy. It occurred that I meant exactly it when speaking with you over the telephone. It begins by a short introduction by P. M. Zinoviev which is however less substantial than I thought before seeing it. But the last, the twelfth chapter seems interesting. There, in accord with Kretschmer[1], the cyclic and the schizoid characters are described by fluctuations between the two appropriate poles with a third epileptoidnic polarity. The latter did not apparently find a wide response.

After glancing at your example of cyclic geniuses and talented men I began to think that schizoids are also found there. Is it so? I have recently reread Oscar Wilde and was astonished that the gout was apparently extremely usual in the circles of the English society which he described.

And my suspicion that here we have to do with what is called nonsense correlation had essentially strengthened.

Letter 2. 19 Jan. 1978

Highly respected colleague,

I begin with what offended you in my first letter. I should have apparently avoided any irony when speaking about the uselessness of some passages in your *proiect* (proэkt of an appeal to high instances. But my aim was quite serious: to caution you against a mistake. An excessive ideological zeal in such documents impresses our leading circles in quite the opposite direction: it provokes mistrust.

But somewhat later about the outlook of some or other appeals to the top people. At first I would like to appear as your *assistent* (pomoshnik) in in the search for truth.

1. I see no need to charge my collaborators with verifying your card indices etc. Let us issue from assuming that they were compiled conscientiously. But any statistician would have turned your attention to the danger of what is called "nonsense correlation". In any

publication you should prevent beforehand such objections rather than "give way to despair" because of my fault finding. I did not suggest to begin in earnest the story of the prevalence of gout among the Russian nobility. However, such studies, for example, of various layers of the English society had been probably accomplished long ago.

But you shouldn't appeal to the public without discussing such issues of the methodology of the statistical approach to the business at hand.

I became interested in the data about the professors of the Michigan University. But here also a suspicion of nonsense correlation concerning age appears at once. It is curious that the "prevalence of investigative interest" offers exactly the least correlation with the uric acid. But I suspect that the American researchers themselves had foreseen such an objection and somehow warded it off.

Generally speaking, I note however that you had convinced me in that the role of the ill-starred uric acid is similar to the part of caffeine. It was new to me that this issue was widely illuminated in the literature before you. The appearance of an essay in Russian about the correlation of the prevalence of gout with endowments and the stimulating action of the uric acid, for example in the journal *Priroda,* would certainly be very desirable.

2. Any essay about the correlation of endowments with psychoses should touch not only manic depressive psychosis but schizophrenia as well. You write that there are many examples among artists and men of letters. Do not **text blackened by the Archive** belong to them? I shall not yet enumerate real and gifted schizophrenics and schizoids in the younger generation, but there are many of them.

Among mathematicians of the 20th century beyond our country I name L. E. J. Brouwer, the founder of mathematical intuitionalism and a topologist. Poincaré ad Hilbert would have deserved attention. It is curious that Brouwer and **text blackened** … open the long list of the representatives of mathematical logic. This is already a detail which definitely seems not to be random. In general, a vast literature apparently exists about talented men and geniuses among schizophenics. Do you belong there **text blackened** …

For realization, the talents of schizophrenics naturally need prolonged remissions. The course of the illness is cyclic. As far as I know, such courses do not give grounds for confusing it with the manic depressive psychosis to which a cyclic course is predominantly ascribed.

I note in passing that Ivanovs, in the *Dostoevsky Clan* [Volotskoy (1934)] were undoubtedly schizophrenics and schizoids. Volotskoy's opinion that schizoid *sidebyside* (na riadu) with prevalent "epileptoid" features were a specific feature of Dostoevsky himself as well, does not seem to me that nonsensical. I only met with the concept of "epileptoids" in Volotskoy's book and in Zinoviev's introduction to it. I do not know whether it was widely recognized. But in any case a fine essay in Russian on correlations of endowments with psychoses would have been desirable.

3. I intend to ask Neyman, the head of American statisticians, about the results of the programme of revealing talented men by means of

tests and about subsequent support for them. Regrettably, correspondence is slow. I do not wait an answer (?) in the near future.

I am grateful to you for sending me a copy [copies] from the journal *Amerika*[1]. They had not convinced me that the notorious programme worth sixty million had occupied such a central place like you imagine in the activities directed at promoting talented men in the USA. It is curious that you require dozens of billions.

For the time being this is all that I can offer as assistance to your inquiries. In your last letter you strengthen still more your horrible forecasts (in six or seven years the USSR will lose its rank of a superpower). I think that this is fantastic. I know well enough the deficiencies of our system and even the danger of their aggravation (see the latest decision about the school[1]) but happily our competitors and in particular our main competitor, the USA, have their own deficiencies. When we meet I can tell you about my estimates of the future and the observations on which they are founded. Now, however, I formulate something about my mood which will hardly change because of our meeting.

A. I have written about considering definitely unfortunate any mention of studies of psychoses, uric acid etc. along with proposals for pre-school and school upbringing.

B. Proposals about pre-school upbringing with justification of the importance of its individual character ought to come, in the first place, from psychologists. Perhaps it will be possible to cooperate, for example, with Zenkov[1] and his collaborators. But argumentation by means of genetics can also play a certain part. It will be really essential to base ourselves on the achievements of the socialist countries of Mid-Europe [Central Europe] which had apparently overcome us.

C. It is beneficial to propagandise tests. But I do not see any definite programme of promoting talented men founded exactly on tests. Ideal are certainly studies open for all when selection is accomplished all by itself: the lazier themselves will scatter. And in many directions we are not so far from such an ideal. But if a competition with appraisal is indeed unavoidable, tests will be useful, although only in a secondary role.

Thus, higher institutions entrance examinations: a test for elimination is certainly unfit (negodny). A serious written worksheet followed by an interview is needed. Therefore I imagine that your entire concept of an all-embracing “testing” and education of specialists “testologists” is mistaken. For example, tests of mathematics will certainly be better when compiled by able mathematicians somewhat acquainted with that task.

E. [D is missed.] Proposals about curtailing the size of classes and about special work with school students of the higher forms are certainly very important. But I think that they should be put forward independently; references to genetics can rather hinder. Details also during personal meeting.

For me, talks with you will be interesting, but I thought it beneficial to disappoint you beforehand by establishing definite bounds for the matter about which we agree. In the issues in which I myself have at least *some thing* (kakoi to) like a small weight (school students of higher forms) I would not see much benefit from my support of your all-embracing proposals. I would rather lessen my capability of doing *some thing* (koe chto) useful.

Yours (signature)

The Archive appended Kolmogorov's postal address (in a building for the staff of Moscow University).

Notes

1. *Larousse*: a multi-volume encyclopaedic dictionary. E. (=Efroimson) and apparently Kolmogorov thought about its ten-volume edition of 1960 – 1964 with an additional volume in 1968. E.: Joan of Arc's behaviour was determined by her Morris syndrome (in inborn disturbance of the gender development). In his *Genetics and Genius* E. considers in detail various syndromes and stimulation by gout, see Notes 3 and 5.

2. Item 3 was not separated from the context. And here is the difference between talent and genius (E., without mentioning any source): a genius creates what he is obliged, a talented man creates what he can.

3. The manic depressive psychosis: in its manic phase the mental process is accelerated. E. apparently also mentioned schizoids and epileptoids. The former are submerged in their inner world with a preponderance of abstract thinking, the latter: explosiveness untidiness.

4. The method of statistical comparison is likely the rank correlation.

5. Here is N. I. Nekrasov, *Who Is Happy in Russia*: A house-serf boasts that he earned his gout by drinking much expensive wine, so that in this respect he is a nobleman. Gout was thought to be caused by gluttony, heavy drinking and various excesses.

E. described in detail how the victims of gout almost became the vehicle for the history of society. In particular, he mentioned Boris Godunov as an outstanding statesman who had nothing to do with the assassination of Prince Dmitry. Historians knew it but were afraid to oppose widespread calumny.

6. Uric acid is structurally very similar to caffeine and the stimulation of the brain by gout can elevate its activity to the level of talent and geniality (E., partly supporting himself by a source of 1955).

7. Anderson, likely the physicist and Nobel-prize winner Carl David Anderson (1905 – 1991). E. thought that Lincoln, Anderson (and de Gaulle) had the Marfan syndrome (a form of gigantism). Kolmogorov (title of § 7) mentioned four mechanisms, E. mentioned four conditions determined by the society which are necessary but not sufficient for the appearance of geniuses. In the first place, as he thought, they emerge after the breakdown of caste, class and other restraints. Here is Novikov (1997, p. 72), about the crazy A. T. Fomenko who curtailed the chronology of civilization by one and a half thousand years:

As it appears, the 75 year old Nikolsky was mightily attracted by the new theory and communicated his manuscript for publication.

Nikolsky was an academician and an eminent mathematician and Novikov certainly had not hinted at any psychosis. The moral atmosphere which reigned in the Soviet Academy is shown by Fomenko's carrier: he was elevated to the very top and managed the science of the land.

8. Professional sport did not officially exist in the Soviet Union, but leading sportsmen's way of life had been *professional* except for payment.

9. Geniuses are exceptions and ought to be studied individually. Gauss behaved quite differently.

10. At least Kolmogorov easily read English literature apparently including fiction.

11. Quoting another author, E. stated that the IQ was a most important instrument which prevented science from degenerating into a system of castes. It is certainly difficult to compile a test, and a tested person could have better or worse answered another question. In his *Pedagogic Genetics* E. said that the IQ test was banned since in the first place the needed people were those devoted to the authorities rather than the clever ones.

12. A class was separated if its students studied different languages, say German (the prevalent language before 1945) and French or English. I have not found Gabi who was apparently a French (?) high official in the educational field.

13. Ernst Kretschmer (1888 – 1964), a German psychiatrist and psychologist.

14. The journal *Amerika* had been published monthly in the USA in 1946 – 1948 and 1956 – 1994. Its circulation in the Soviet Union, perhaps except 1946, was restricted to the utmost and sometimes banned. Stalin, who had been suspecting his own shadow, could not have decided otherwise. Something was possibly done to slower correspondence with the *capitalist surrounding*, see just above.

Also a bit above Kolmogorov discussed the national American scholarship programme MERIT. It was initiated in 1955 and is managed by a privately founded corporation. I found a description of its work but still have no answer to Kolmogorov's question.

15. The decision of the highest Party and government organs of 22 Dec. 1977. It required a strengthening of the ideological direction but neither mathematics, natural sciences, or foreign languages were mentioned there and had to suffer.

The Soviet Union had existed only a bit longer than E. thought so that Kolmogorov was wrong. The perestroika was doomed to fail since the entire mighty nomenklatura was rotten and most if not every constituent republic demanded real independence from Russia proper. Brezhnnev's claim that there appeared a new entity, a Soviet man, was proved damnably wrong.

16. Psychologist Leonid Vladimirovich Zenkov. He died 27 Nov. 1977, but Kolmogorov obviously had not yet known it.

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Early history of the theory of probability

Arch. Hist. Ex. Sci., vol. 17, 1977, pp. 201 – 259

1. Introduction

The theory of probability originated in the period from 1654 (correspondence between Pascal and Fermat) to 1713 (posthumous publication of Jakob Bernoulli's *Ars Conjectandi*). My paper is devoted to this period. I included § 2, the relevant history of games of chance although not lotteries[1], jurisprudence, insurance of life and property as well as political arithmetic and demographic statistics, see also [93].

I do not mention Newton. His philosophy extensively though indirectly influenced probability and, for example, turned the attention of De Moivre to probability stronger than games of chance [9, pp. 230 – 231], but his work is beyond my framework. Newton also deserves credit for achievements in probability (Ibidem, pp. 217 – 227).

2. Origin of stochastic ideas and notions in science and society

2.1. Games of chance. They promoted the general, partly intuitive ides of stochastic properties evinced by mean outcomes [93, p. 114] and (p. 113) served to prove that certain events in nature were designed rather than produced by chance.

In games of chance Pascal, Fermat, then Huygens were confronted with problems whose solutions gave rise to stochastic theory, see also [69][2]. In their efforts to assess the possibilities of that emerging theory as well as their own competence scholars directed their attention to various problems in games of chance, see for example § 4.2.2.

Leibniz [93, p. 115] even proposed to use games of chance as models for studying the *Erfindungskunst* and, I would add, for originating a statistical decision theory. All this is understandable. At that time, games of chance and possibly only they, could have provided models for posing natural and properly formulated stochastic problems. Their studies were also in the social order of the day.

The possibility of other applications for probability was contemplated by Huygens (§ 4.1), but there was not even a hint of anything beyond games of chance in his treatise [48]. On the other hand, they do not occur in the work of De Witt (§ 2.3.3) or Halley (§ 2.4.5). Nevertheless, Huygens' treatise merely represented the infantile stage of probability and the *Ars Conjectandi* was conceived to include its applications to *civil, moral and economic affairs*, I may say that the emergence of probability theory was not entirely due to games of chance.

Games of chance proved fruitful for De Moivre and later authors and they are methodologically important even now. Kendall [54, p. 26] remarked on their early history:

By the end of the 15th century the foundations of a doctrine of chance was being laid. The necessary conceptualization of the perfect die and the equal frequency of occurrence of each face are explicit.

Why then, he asks, had not the theory of probability emerge those times? He (p. 30)[3] concludes after listing several possible reasons:

It is in basic attitudes towards the phenomenal world, in religious and moral teachings and barriers, that I incline to seek for an explanation of the delay.

It is also possible to mention the lack of practical requirements, cf. § 2.3.

Why no treatise resembling Huygens [48] had appeared a century earlier? Because the 17th rather than the 16th century marked the beginning of modernity both in society and science when scientific communities began to be influential and scientific correspondence essentially expanded. All this midwifed the development of probability (§§ 3 and 4). As to religious and moral obstacles in the path of probability, it would be more proper to mention hindrances to a philosophical apprehension of randomness and probability. They, these hindrances, were occasioned by the general state of philosophy which, even in the 17th century, did not completely abandon the obsolete Aristotelian picture of the world.

A venerable problem in games of chance formulated in 1380 or even earlier [73, p. 414] was that of dividing the stakes (problem of points). A game between two gamblers is to continue until one of them scores n points. For some reason it is interrupted on score $a:b$ ($a, b < n$)[4]. The division of stakes had been considered by Cardano, Tartaglia and Peveroni[5]. In 1558 Peveroni [54, p. 27] gave a correct answer for problem (10; 8, 9) and only a blunder prevented him for proving a correct answer for the game (10; 7:9).

2.2. Jurisprudence. It seems that, beginning from about the second half of the 17th century, the importance of civil suits considerably increased and the practice and possibly theory of legal proceedings (both criminal and civil) started to employ stochastic estimates of proof more or less openly and therefore to disseminate stochastic ideas and notions.

Thus, Leibniz [93, p. 109] testified to the existence of an elementary scale of stochastic proofs in jurisprudence and Descartes [23, pp. 323 – 324] introduced the

Certitude morale, suffisante pour régler nos moeurs, ou aussi grande que celle des choses dont nous n'avons point coustume de douter touchant la conduite de la vie, bien que nous sachions qu'il se peut faire, absolument parlant, qu'elles soient fausses. ...

L'autre sort de certitude est lors que nous pensons qu'il n'est aucument possible que la chose soit autre que nous la jugeons. ... Cette certitude s'estend à tout ce qui est démonstre dans la Mathématique.

Descartes had not mentioned jurisprudence but here is Thomas Aquinas [93, p. 108]:

In the business affairs of men we must be content with a certain conjectural probability.

Chapter 15 of pt. 1 of the *Port-Royal* [2] contains an example of applying this certitude in a legal case[6] later borrowed by Jakob Bernoulli, see chapter 3 of pt. 4 of [3]. For his part, Leibniz thought of applying moral expectation in theology. The extant contents of his manuscript [57] mentions a chapter *Demonstratio probabilitatis infinitae, seu certitudinis moralis …*

But what about natural science? At least he [111, p. 169] stated that there exist

Drei Grade der Sicherheit in Urteilen: die logische Gewissheit, die nur eine logische Wahrscheinlichkeit ist, die physische Wahrscheinlichkeit. Ein Beispiel ... der dritten: Der Südwind ist regnerische, welche meistens wahr sind, obwohl sie nicht selten fehlgehen.

And elsewhere [58, p. 453, 457]:

Scientia est notitia certa ... Opinio est notitia probabilis ... Certitudo est claritas veritatis.

And Montmort [69, p. xiii] mentioned the difficulty of applying probability to *sujets politiques, aeconomiques ou moraux*:

Ce qui m'en empêché, c'est l'embarras où je me suis trouvé de faire des hypothèse, qui étant appuyées sur des faits certains.

Moral expectation (Daniel Bernoulli, in 1738) is an example of such a hypothesis. During 1654 – 1713 only one contribution was devoted to the application of probability to the law (Nicolas Bernoulli [4]). He **1.** Issuing from Graunt's table (§ 2.4.3), calculates the mean duration of life for various ages. **2.** Recommends to use it for computing the value of annuities and estimate the probability of the death of obscure absentees[7]. **3.** Calculates the expectation of losses in marine insurance. 4. And of gains in the Genoese lottery. **5.** And the probability of truth of testimonies. **6.** Most important: he calculates the expectancy of the last survivor of a group of men [96, § 340]. Assuming a continuous uniform distribution of deaths, Nicolas calculated the expectation of the appropriate order statistics. He was the first to use, in a published work, both that kind of distributions and an order statistics[8]. His work became known to Condorcet[9], Laplace and Poisson.

2.3. Insurance of property and life

2.3.1. Insurance of property. It had existed since ancient times [84, p. 40]:

Two thousand years B. C. ... participants of trade caravans in the Near East concluded agreements to share damages incurred en route due to robbery, theft or loss. And, according to the Talmud, similar agreements were concluded in Palestine and Syria …

The author also mentioned agreements between merchants active in marine commerce on the shores of the Persian Gulf, in Phoenicia and ancient Greece while the Solon law refers to companies among whose participants existed agreements to share damages in marine commerce and sea robbery, as the author added.

These agreements lacked stochastic ideas and notions, had not even embraced any system of initially established insurance payments. Such a system apparently originated in European feudal guilds, and in Japan a similar system existed even in the 12th century [7].

Now, marine insurance. Possibly until the 19th century [13, pp. 349 – 350]

L'assurance maritime a été et devait être la première forme de l'ssurance. …

A *certaine organisation* for such insurance originated in the 14th century while during the 15th century *l'assurance* [maritime] *fait déjà l'objet de dispositions législatives importantes*.

But the forms of marine insurance were not conducive to stochastic reasoning. One of these forms was [25, p. 4] *l'assurance par forme de gageure*. And [13, p. 349]

On rencontre … dans les textes du Digeste la stipulation suivante: "Je stipule que vous me donnerez 100, si tel navire n'arrive pas d'Asie".

Another source [25, p. 6]:

Il seroit odieux qu'on se mit dans le cas de desirer la perte d'un vaisseau … dans la plûpart des Places de Commerce, les Assurances par gageure ont été prohibées.

Nevertheless [13, p. 349] this form of insurance

Ait reparu à différentes époques avec une fâcheuse persistance.

In the 16th century (p. 351)

L'assurance [maritime] *dégénéra vite en operation de jeu du caractère le plus aléatoire*.

Another form of marine insurance was the so-called *bottomry*[10], perfectly legal but also primitive. It implied a mortgage on a vessel with a stipulation that the repayment of loan is conditional on the safe arrival of that vessel. The interest on bottomry was considerably higher than on loans in general [84, pp. 68 – 70; 45, pp. 127 – 131].

On the brighter side I quote a particular statement and a noteworthy description of what was possibly the birth of the modern form of marine insurance. The former, regrettably unjustified, is to the effect that marine insurance almost gave rise to the notion of probability (or expectation?) of a random event [13, p. 349]:

Au moyen âge, pour la première fois, on a compris que la risqué est une réalitéque l'on peut séparer idéalement du tout dont elle fait partie pour lui assigner sa valeur propre.

This description [45, pp. 141 – 142] refers to the speech of the Lord Keeper Bacon (father of Sir Francis) in 1558:

Doth not the wise merchant in any adventure of danger, give part to have the rest assured?

The same author quotes from the first English Statute on assurance (Publicke Acte No. 12 (1601), an Acte conc'ninge matters of Assurances amongst Marchantes[11]):

Whereas it hathe ben the Plicie of this Realme by all good measures to comforte and encourage the Merchante, therebie to advance and increase the general wealth of the Realme, her Majesties Customes and the strengthe of shippinge, which consideracon is nowe the more requisite because Trade and Traffique is not at this psente soe open as at other tymes it hath bene.

And whereas it hathe bene tyme out of mynde an usage amongst merchants, both of this realme and of forraine nacyons, when they make any great adventure (specialie into remote partes) to give some consideracion of money to other psons (which commonlie are in noe small number) to have from them assurance made of their goodes, merchandizes, ships and things adventured, … whiche course of dealing is commonly termed a policie of assurance.

The main text of the Act is devoted to the legal aspect of disputes on policies of assurance.

2.3.2. Life assurance. A policy on human life [is] *defined as any instrument by which the payment of money is assured on death or the happening of any contingency dependent on human life, or any instrument evidencing a contract that is subject to payment of premiums for a term dependent on human life*[12].

The second form of life insurance is a life annuity offered either individually or to a group of men. For example, mutual insurance in tontine associations[13] secures annuities to its members which increase with the decrease of the number of members still alive. There also existed other forms of mutual insurance (usually man and wife) with a constant annuity payable until the death of the last survivor[14].

The same author continues (p. 1094):

The payments of certain benefits on death against certain periodical subscriptions are to be found in the Roman collegia (*artisans' associations*).

This statement does not directly contradict the opinion of another author [13, p. 348]:

La prevue absolue que les Romains n'ont pas connu l'assurance, c'est qu'on ne trouve pas un seul mot relatif à ce contrat dans les écrits de leurs jurisconsultes.

In any case, the system of periodical subscriptions seems to have faded out of existence in later centuries. Thus [84, p. 61] in the 13th century Danish guilds provided insurance based on subsequent distribution of damages (e. g., from shipwrecks or captivity with ensuing payment of ransom). Allowance for ransom money is actually an insurance premium.

During Middle Ages (Ibidem) mutual guild insurance covered most diversified cases, including those *directly related to the personality* of guild members. A definite example (p. 62): in 1284 one of the English guilds paid allowances in cases of incurable diseases or blindness. Discussing insurance against accidents and sickness another author [41, p. 74], without specifying the system of subscriptions, stated:

Whereas we [in England] *began to be busy in this direction about the middle of the sixteenth century* (1560), *Italy practised this civilising art of insurance as early as the end of the 12th century*.

Life insurance had obviously spread beyond the limits of separate guilds[15]. Thus, an anonymous French author of the 16th century [45, p. 228] testifies:

Pilgrims going to the Holy Sepulchre of Jerusalem, or on other distant voyages, may effect insurance for their redemption. ... Another kind of insurance is made by other nations upon the life of men, in case of their decease upon their voyage. ... Which are all stipulations forbidden.

It seems that the main reason for forbidding insurance of life was its developing connection with gambling[16].

Thus, quoting many sources, Emerigon [25, p. 198] says:

Ces sortes d'Asssurances ne sont pas des Assurances proprement dites; ce sont de veritables gageurs. ... Ces gageurs ... sont prohibées *en Hollande, & en plusieurs autres pays. Depuis longtemps elles avient prohibées en France.*

The Amsterdam Ordinance of 1598 [45, p. 229] *expressly* [prohibited] *insurance of life of any person and likewise Wagers upon any voyage.*

Similar prohibitions (Ibidem) were contained in the Rotterdam ordinances of 1604 and 1635, in the marine ordinance of Louis XIV (1681) and in a series of Netherlands' ordinances issued in 1570 – 1635, while in the Statutes of Genoa for 1588 [54, p. 32] insurance of life was forbidden *sine licentia Senatus*. Some of the passages quoted above suggest that insurance of life could have well originated from the semi-legal and odious marine insurance *par forme de gageure* (§ 2.3.1)**[17]**.

Numerous prohibitions possibly hindered the advancement of life insurance in the second form since it seems that no legal prohibition ever applied to it (tontines excluded)**[18]** which existed even in ancient Rome [45, p. 224]:

The Roman lawyers, at least about the time of the division of the Empire, found it necessary to consider and frame a table by which annuities could be valued so as to meet the requirements of the Falcidian law, which prevented the testator from leaving more than three quarters of his property to any others than legally constituted heirs. ... One of the most eminent commentators on the Justinian Code, the Praetorian Praefect Ulpianus (170 – 228), gave a table of the estimated present worth of ... life annuities.

This table is available in many sources. Ulpianus showed life expectations against age. In the opinion of Hendriks (pp. 224 – 225) he may have obtained these expectancies either

From inquiries on the results of like annuity engagements or from returns of the number of deaths occurring within a given time at various ages. The former method would seem to have been the most likely to be available, but the other was quite within the bounds of possibility as the foundation of an approximate computation, for there is ample record of a kind of registration or ephemeris of deaths having been observed by the ancients.

On the contrary, Greenwood [38; 39, p. 67] asserts that Ulpianus did not base his table on any statistical data. But even in this case (and bearing in mind that Ulpianus' expectation of life does not necessarily coincide with its present meaning) his table at least methodologically constituted the highest achievement of demographic statistics until the 17th century (see also § 2.4.1). Regrettably, owing to the general conditions of the development of society and science, this table was forgotten.

In modern times annuities are known at least from the 14th century**[19]**. Referring to a number of Dutch sources published in 1670 – 1671, Hendriks [45, p. 112] concludes that in Holland, both long before, and at that time, the price of annuities normally did not depend on the age of the annuitant. This practice seems to imply that it was borne out by high mortality both in childhood and old age.

That annuities were purchased in the name of children and even infants is testified by Hudde [47, t. 7, pp. 95 – 96][20] who compiled statistics showing the age of annuitants

Sur les têtes desquelles des contrats de rentes viagères ont été vendus par le gouvernement des Provinces unies en 1586, 1587, 1588, 1589 et 1590.

A very numerous group constituted annuitants aged from two to seven years. Hudde also shows the duration of life of each annuitant. On the other hand, prices of annuities sold in Holland in 1672 and 1673 did not depend on the age of annuitants [16, p. 1205]. It is another question whether and to what extent the need to calculate the price of annuities stimulated research of mortality, see also § 2.3.3.

As to tontines, it seems that they were neither socially acceptable nor widespread [45, p. 116]:

In England, we have such strong prejudices against Tontine Associations, based perhaps on the assumed rationale that they are too selfish and speculative to be encouraged.

The same feeling prevailed in France [30, p. 617]:

Le Ministre de l'Intérieur … a désiré que l'Académie des Sciences choisit … une Commission chargée d'examiner les articles qui règlent les intérèts respectifs des actionnaires [of a proposed tontine].

Tontines, as the author (Fourier, p. 619), continues,

Exercent deux penchants funestes: l'un est la disposition à attendre du hasard ce qui devoit être le fruit d'une industrie profitable à tous, ou le résultat ordinaire des institutions; l'autre est le désir d'augmenter ses jouissances personelles en s'isolant du reste de la société.

A supplementary consideration follows (pp. 625 – 626): the government asked the Academy

Sur le projet de l'établissement de la Caisse dite de Lafarge[21]*, proposa un avis contraire à ce projet. Nous avons trouvé dans nos Archives le Rapport de la Commission chargée de l'examen de cette question. Il a été adopte 1790: il est signé de MM. de Laplace, rapporteur, Vandermonde, Coulomb, Lagrange et Condorcet*.

And here is the obvious conclusion (p. 630):

L'Académie ne peut que refuser son approbation à un établissement irrégulier, contraire aux vues du Gouvernement, et même aux intentions des auteurs du project [because, p. 629] *le placement des capitaux en tontine est beaucoup moins favourable que le simple contrat de rente viagère. L'Académie approuve le Rapport et en adopte les conclusions*.

Nevertheless, tontines did exist in the 17th century. In France, they were established in 1689 and 1696 [46, pp. 206 and 209; 95, p. 405], in Holland in 1671 [94, p. 226]. The down payment in the former depended on the age of the annuitant [95, p. 405; 4, p. 35] which necessitated consideration of the law of mortality. In turn, tontines provided data on mortality[22] although unsuitable for studies of the population at large. But still, Hendriks [45, p. 116] supposed that their data were used for compiling first tables of mortality.

Societies which offered life insurance in the second form came into being in the 18th century [10, p. 100]:

Zwar kann eine 1669 in England unter dem Namen der <u>*Society of Assurance for widows and orphans*</u> *... ins Leben gerufene Gesellschaft als Lebensversichungsanstalt im heutigen Sinne bezeichnet werden.*

The author then justifies his opinion. Another author**[23]** states that the first established (in 1706) was the *Amicable Society* and that before the 18th century insurance in the second form mostly covered temporary contingencies and only over short periods of time. He adds:

The premiums were very high, but this was in part necessary for two reasons. First, the insurers had no sufficient data upon which to estimate the risk they incurred; and secondly, the transactions were probably not numerous enough to secure anything like a regular average in the occurrence of claims.

This apparently explains, at least partly, why life insurance did not play any important role in promoting stochastic ideas and notions. Mrocek [70, p. 50] concludes the same, though for another reason:

Neither joint-stock societies, nor banks, not stock exchanges stood in need of probability. Their demands for probability only appeared in the 19th century when scientific gain superseded methods of downright robbery.

There is ample evidence of cheating, if not of robbery, in life insurance both in the 18th and 19th centuries**[25]**. However, the first two reasons seem important as well. They amount to saying that until the 19th century life insurance in the second form was not sufficiently developed**[26]**.

2.3.3. De Witt's memorandum [100]. There is a special feature common to any kind of insurance: statistical data and methods of actuarial calculations could have well constituted a commercial or even state secret. This is evident at least with respect to De Witt, a prominent mathematician and statesman**[27]**.

In his memorandum addressed to the members of the government of the *Provinces unies***[28]** De Witt strove to substantiate the possibility of raising the price of life annuities sold by the state. Assuming definite suppositions about the law of mortality and a 4%, or rather a $(\sqrt{1.04}-1)\,100\%$ yearly discount, De Witt [45, p. 234]

Mathematically ... proved that ... the life annuity should be sold at 16 years' purchase.

De Witt's main suppositions about mortality are [45, p. 234]

The likelihood of dying in a given year or half year ... from 53 to 63 years of his [the annuitant's] *age, taken inclusively, does not exceed more than in the proportion of 3 to 2 the likelihood of dying in a given year or half year during the ... rigorous period of life* [from 3 to 53]. *... During age 63 – 73 this chance cannot be estimated at more than double ... and as triple ... during the 7 following years.*

The count of half years begins at age 3. In an appendix De Witt [100, pp. 23 – 24; 45, pp. 117 – 118] adds:

Since the proof of the foregoing demonstration I have had very carefully extracted from the registers of your Lordships some thousands of cases of persons upon whose lives annuities have ben purchased.

Examining *considerably more than a hundred different classes, each class consisting of about one hundred persons*, De Witt found that

For young lives each of these classes always produced to the annuitants … a value of more than sixteen florins of capital arising from one florin of annual rent. … Thus, one finds with wonderment, that in practice, when the purchaser of several life annuities comes to divide his capital … upon several young lives, upon ten, twenty, or more, this annuitant may be assured, without hazard or risk of the enjoyment of an equivalent in more than sixteen times the rent which he purchases.

Noting that it is impossible to check the correctness of De Witt's use of his data, Hendriks [45, p. 117] is satisfied that

Experimental observations and collection of the indications of mortality at various ages, and in different classes of lives, were the principles in which the acuteness of De Witt and Hudde [who officially declared his agreement with the principles of De Witt's calculation] *recognized the only true foundations for their labours*.

De Witt's statement about the effect of combining several annuities is related to the law of large numbers[29]. It is regrettable that his calculations are lost. Indeed, how had he arranged annuitants into classes? Now, De Witt's exposition [26, p. 66] is

Très obscure, on l'a ordinairement mal comprise [an example of M. Cantor's misunderstanding follows]. *De Witt parle du risque de mourir sans faire ressortir expressément que ce risque se rapporte toujours à un enfant ayant 3 ans; au contraire il s'exprime de manière à faire croire que le risque de mourir entre x et x + 1 ans accomplis signifie ce risque pour une personne ayant actuellement x ans. De plus, il fait deux différentes suppositions sur les nombres des décès semestriels … Dans l'exposition … il suppose ces nombres proportionnels à 1, 3/2, 2, 3, mais dans l'application de la méthode il introduit, sans un mot d'explication, l'hypothèse* [of numbers 1, 2/3, 1/2 and 1/3].

To prove this fact Eneström adduces tables of mortality compiled under each of these hypotheses and concludes (p. 68):

En examinant ces deux tables, on voit …que la dernière … indique une mortalité beaucoup plus grande que la première. On pourrait donc conjecturer que De Witt, après s'être servi de la première hypothèse pour calculer la valeur de la rente viagère, eût trouvé cette valeur trop grande, et que, pour cette raison, il choisissait une autre hypothèse [the second one] *qui permettrait de vendre la rente à un prix moins élevé*[30].

The simplest way to detect the discrepancy of De Witt's reasoning is to note that the chance to die during, say, the interval No. 3, is in addition, the chance to survive intervals NNo. 1 and 2. The chances of death should diminish as in hypothesis 2 and De Witt's calculation of the value of life annuities actually corresponds to this hypothesis because what calculation requires is precisely these chances of death.

The mean duration of life and the value of life annuities according to both hypotheses are 46 and 33.2 years and 18.8 and 16.0 years purchase correspondingly. Thus it was the mean duration of life rather than the value of annuities which compelled De Witt to change the initial hypothesis. Alternatively, both he and Hudde could have overlooked the discrepancy altogether.

Be that as it may, hypothesis 2 led to an expectation of life which conformed with the data given by Struyck [94, p. 212]: in the mean, purchasers of annuities sold in Holland in 1672 lived to enjoy approximately 32 annual payments. Struyck's data related to 1698 annuitants, men and women, and he gave figures for those under and above twenty for each sex separately. Thus, for men (women) above twenty the mean expectation of life was 21 3/4 and 24 1/2 years. De Witt, however, assumed a single value of annuities of any age.

In 1671, in a letter to Hudde, De Witt [45, p. 109] formulated the problem of calculating the value of annuities on several lives[31]. Suppose that eight persons have lived $x_1, x_2, \ldots, x_8$, years, $x_1 < x_2 < \ldots < x_8$. (Actually, his is a numerical example.) Then combinations of the type (x_i, x_j), $i, j = 1, 2, \ldots, 7, 8$ and $i < j$ will constitute all possible cases of the duration of lives of two persons out of the eight. In one case out of $C_8^2 = 28$ the last survivor will live to age x_2, in two cases, to age x_3 etc. Thus the law of longevity of the last survivor is obtained. Similar calculations with (x_i, x_j, x_k), $i, j, k = 1, 2, 7, 8$ and $i < j < k$ are made for the case of three lives etc. De Witt had thus determined the distribution of one of the order statistics (of the maximal term of a sample)[32].

The same correspondence explains why De Witt's memorandum for a long time remained a closely guarded secret. On 2 Aug. 1671 De Witt thus begins his letter to Hudde [45, pp. 101 – 102]:

I have properly understood the estimation of the value of life annuities upon one life computed [by whom?] *from the life and death of 96 persons. ... I leave you to consider whether ... you do not judge it to be useful for the public that this estimate be absolutely hidden ... for the advantage of the State finances.*

But the existence of De Witt's memorandum had become known to contemporary scholars and at least one of them, Leibniz, had a copy. Jakob Bernoulli not less than twice unsuccessfully asked him to send it for a time [33]. Can it be that Leibniz received that copy under an obligation to show it to no one?

2.4. Political arithmetic and demographic statistics

2.4.1. Ancient history. Statistical data had ben collected in ancient times and later (Fedorovitch [27, pp. 7 – 21][33]. In China, about 2238 B. C., a geographical description of the territory and an estimation of the population had been attempted. Censuses, including compilation of tables of movement of the population as well as land registers were possibly known in Egypt from the 35th century B. C. and the first positively established census taken there, but only of the warrior caste, occurred in the 16th century B. C. The Old Testament describes enumerations of the people of Israel and Moses had to know the exact (comparatively small) number of people, little more than 22 thousand (Sheynin 1998, § 5.1.2).

In Sparta, Lycurgus (800 – 730 B. C., a lawgiver) distributed land among his subjects which meant that he had more or less known the population.

In Athens, registers of births and deaths were kept; censuses were carried out under Pericles and Solon. Freeborn citizens were entered in special lists as registered at birth and after reaching ages 18 and 20. Traditional offerings to gods on the occasion of birth and death facilitated the registration of the movement of population. In Rome, lists of citizens able to bear arms and Romans enjoying full rights, i. e., males aged 20 and 30 respectively, were maintained. And Ulpianus (§ 2.3.2) compiled a table of life expectancies for different ages. Aristotle's *Politica* contained a comprehensive description of Greek states and cities.

Much can be found in Chapter 2 of Moreau de Jonnès (1847). Thus, he describes the plans of the Roman emperor Augustus implemented by his successor, emperor Tiberius, and notes that in France, similar plans had only Napoleon and Louis XIV but no one in England (see however § 2.4.2). This subject deserves a special research. In general [27, p. 15],

It is hardly possible to agree with those who maintain that statistics [I would say, Staatswissenschft, university statistics] *as a science was completely unknown to the ancients and that it was only founded by Conring and Achenwall.*

Fedorovitch (p. 17) refers to Conring who named Aristotle, Strabo and Ptolemy as cofounders of Staatswissenschaft.

I supplement this statement [55, p. 45]:

Statistics as we now understand it did not commence until the 17th century and then not in the field of 'statistics' [= not in the Staatswissenschaft] *but in that of political arithmetic. The feudal state of the Middle Ages was just not interested in statistics* (*in our sense*).

And (p. 46), even in 15th century Italy for all its achievements in accountancy and mathematics,

Counting was by complete enumeration and still tended to be a record of a situation rather than a basis for estimation and prediction in an expanding economy.

Statistical data collected for example in Rome or Athens (see also § 2.3.2) could have assisted in the emergence of some elements of political arithmetic. However, the general laws of the development of ancient civilization had no need for demographic statistics as emerged in the 17th and 18th centuries.

2.4.2. William Petty. He, the founder of the classical political economy, introduced the term *political arithmetic*. While describing states and cities from a socioeconomic viewpoint, Petty [79, p. 244] rejected the use of *comparative and superlative Words*, and rather expressed himself *in Terms of Number, Weight, or Measure*; urged to use only *Arguments of Sense* and consider *only such Causes as have visible Foundations in Nature*[34].

Petty [81, vol. 1, pp. 171 – 172] even proposed to establish a *Register generall of people, plantations & trade of England*. He thought of collecting the accounts of all the

Births, Mariages, Burialls … of the Herths and Houses … as also of the People by their Age, Sex, Trade, Titles and Office.

The scope of this *Register* was to be *wider than that of our existing General Register Office* [39, p. 61].

Strictly speaking, neither Petty, nor apparently his followers ever introduced a definition of political arithmetic But, without violating Petty's thoughts it is possible to say that the aims of this new scientific discipline were to study states and cities from a socioeconomic viewpoint by means of (rather unreliable) statistical data on population, agriculture, commerce, manufactures. Thus, Petty [80, p. 108] estimates the wealth of England by determining the value of

Housing, Shipping, Stock of the Kingdom yielding but 15 Millions of proceed, is worth 250 M then the people who yield 25 are worth 416 2/3.

It is reasonable to object to equating population with wealth, but his calculation conformed to the principles of statistics in that the labour of each man and woman was estimated on the same footing.

At least to the middle of the 19th century the most interesting problems of political arithmetic belonged to demographic statistics which then came to the fore. And within demographic statistics most important from my point of view were problems in mortality because of their application to life insurance, and more specifically, to the institution of annuities which emerged independently from political arithmetic (§§ 2.3.2 and 2.3.3). The founders of political arithmetic (see also § 2.4.3) did not foresee the importance of life insurance either for society or mathematics (probability)[35].

I offer only a few comments on the work of Petty in political arithmetic; [39] is more detailed, but here is Strotz (1978, p. 188):

Petty's essays on political arithmetic were econometric in methodological framework even from the modern point of view.

Among the Petty papers at least 30 belong to political arithmetic. One manuscript [81, vol. 2, pp. 10 – 15, quotation from p. 15], see also letter of 1687 to Southwell [82, pp. 318 – 322], is devoted to algebra which

Came out of Arabia by the Moores into Spaine and from thence hither, and WP hath applied it to other than purely mathematical matters, viz., to policy by the name of Politicall arithmetick, by reducing many terms of matter to termes of number, weight and measure , in order to be handled Mathematically.

Algebra (p. 10) is *a kind of Logick*; in algebra (p. 14)

(1) The Algorithme is the Tooles. (2) The stock of axiomes is the Materialls (3) The practice and a good head is the workmanship. (4) The finding out abstruse truths in the work, and out of a few truths to draw out infinite true conc[lusions] *and to preserve the method of numbering unconfounded is the exc*[ell]*ency.*

This subordination of algebra to logic as also Petty's meditations on *Fundamentall questions* show him as a philosopher of science possibly congenial in some respects with Leibniz, his junior contemporary. Indeed, among these Questions [81, vol. 2, pp. 39 – 42] are such as

What is a common measure of Time, Space Weight, & motion? What number of Elementall sounds or letters will … make a speech or language? How to give names to names, and how to adde and subtract sensate & to ballance the weight and power of words; which is Logick & reason[36].

The title of the second chapter of the *Questions* (p. 40) is *What rules of Marriages are best for procreation*? Here, as also in other manuscripts, Petty advocates improvements of biological conditions for the multiplication of mankind. And in his correspondence in 1685 [82, pp. 148 and 154] we find:

(1) It is for the honour of God and the advantage of mankind that the world should be fully and speedily peopled, ad that objections against the same be deferred till a thousand years hence (!).

That the more people there are in any Country, the greater is the value of each of them.

(2) Till we see the Earth peopled (*as perhaps 3/4 is not*), we may doubt (*that the whole Earth; and the fixed stars too, was made for the use of man*); *and not knowing to what other use it was designed, may stumble into the Error of Its having been made by Chance, and not by the designe of an infinite wisdome*[37] …

So Petty did after all use the word *infinite*! He did not forget to add (p. 15) that the

King of England hath a greater share of the unpeopled Earth … than most other Princes; wherefrom when the whole shall bee peopled Hee will have a greater share than he hath now.

Multiplication of mankind continued to be the favourite subject of scholars of the 18th century (e. g., of Süssmilch)[38].

Two more sources from Petty, though unrelated to political arithmetic, deserve to be discussed. The first [81, vol. 2, pp. 8 – 9] is obviously a programme for educating young men. It mentions algebra (and in this connection some scholars from Viète to Wallis), geometry, chess, games of chance and even hunting and fishing).

The second [77, p. 64] is a passage largely repeated by Condorcet and Laplace [91, p. 320]:

A Lottery is properly a Tax upon unfortunate self-conceited fools. … Now because the world abounds with this kinde of fools, it is not fit that every man that will, may cheat every man that wold be cheated; but it is rather ordained, that the Sovereign should have the Guardianship of these fools … (and monopolise lotteries?).

Lotteries should be carried out *but for small Leavies*.

2.4.3. John Graunt. He is the co-founder of political arithmetic. For a long time his *Observations* [36] were attributed to Petty. However [76, vol. 1, p. lii] Petty

Perhaps suggested the subject of inquiry, … probably assisted with comments upon medical and other questions here and there …, procured [some] *figures … and may have revised, or even written the Conclusion*[39].

Graunt was able to use such fragmentary statistical data as existed in his time and to arrive at general quantitative estimates of the population of London and England and of the influence of various diseases on mortality, see also § 2.4.5. Thus (Chapter 8), the difference between London's male and female populations equalled about 1/13th of the latter. Graunt remarked: comparatively more men die violent deaths, travel and remain unmarried, such as

Fellows of colleges, and apprentices above eighteen etc. ... the said ... difference bringeth the business but to such a pass, that every woman may have a husband, without the allowance of polygamy.

In Chapter 11 Graunt estimates the population of London. Here is one of his methods (pp. 68 – 69):

I guessed that in 100 yards square there might be about 54 families. … There are 220 such squares *within the Walls* [11,880 families]. But *forasmuch as there dy within the walls about 3200 per Annum, and in the whole about 13000* … the whole population of London consists of 47,520 families.

Graunt's celebrated table of mortality is compiled from bills of mortality in the same chapter (p. 69). Information about the age at death was almost completely lacking in these bills, but Graunt's statistical insight and ingenuity enabled him to compile this table by drawing on his own conclusions about mortality from various diseases, children's diseases included. Graunt's own explanation of the method he used (p. 69) is generally known and the opinion of Willcox [36, p. x] particularly applies to it:

To the trained reader Graunt writes statistical music; Petty is like a child playing with a new musical toy which occasionally yields a bit of harmony[40].

And, on p. xiii:

Graunt is memorable mainly because he discovered the uniformity and predictability of many biological phenomena taken in the mass. ... Thus he, more than any more man, was the founder of statistics.

In this context, *uniformity and predictability* mean that final results of statistical inquiries (such as the mortality table) could be used for a certain number of years to come, a fact implicitly supposed by Graunt (see below).

Willcox [36, p. xi] opines that the table itself is due to Petty

Who incidentally and characteristically ignored Graunt's theory that seven percent survived seventy[41] *assuming instead, without reason, that one percent survived seventy-six and not one percent eighty-six.*

As I understand him, Willcox also supposes that Petty calculated a constant chance (p) of dying by decades from the equation

$$64(1-p)^7 = 1 \qquad (2.4.3.1)$$

in which 64 and 1 are the numbers of people alive at ages 6 and 76, respectively.

In 1937, Ptoukha [83, p. 71] reconstructed Graunt's table assuming

$$1-p = (64-1)/100 = 0.63 \approx 3/8.$$

However, this method of determining p is of course arbitrary.

Greenwood [39, p. 79] noticed that, according to Graunt, later age mortality is enormously higher than according to Halley. Granted. But why does Grenwood infer that *this shot did not find the bull's eye*? Graunt must be credited for inventing the table if not for its accuracy (unattainable in his time).

Graunt's table, as he himself said [36, p. 70], enables one to estimate the number of fighting men. Other eventual uses of the inquiry as a whole are mentioned on pp. 78 – 79, in the *Conclusion*: the

Art of Governing, and the true Politiques, is how to preserve the Subject in Peace, and Plenty. ... Now, the Foundation, or Elements of this honest harmless Policy is to understand the Land, and the hands of the territory to be governed. ...

It is no less necessary to know how many People there be of each Sex, State, Age, Religion, Trade, Rank, or Degree, etc. by the knowledge whereof Trade, and Governement may be made more certain, and Regular. ... Whether the knowledge thereof be necessary to many, or fit for others, than the Sovereign, and his chief Ministers, I leave to consideration.

The last remark shows that even Graunt (or Petty or Leibniz, see § 2.4.4) did not yet recognize the importance of statistics for a broad circle of educated citizens.

I conclude with three estimates of Graunt's work. The first is found in Huygens' letter dated 1662 [47, t 4, p. 149], the others [83, p. 67]; [34, pp. 13 and 14] belong almost to our time.

(1) *Le discours de Grant* (!) *est très digne de considération et me plait fort, il raisonne bien et nettement et j'admire comment il s'est avisè de tirer toutes ces conséquences hors de ces simples observations, qui jusqu'a luy ne semblent avoir servi de rien. dans ce pais icy l'on n'en fait point, quoy qu'il seroit a souhaiter qu'on eust cette curiositè et que la chose soit assez aisée, principalement dans la ville d'Amsterdam, qui est tout divisée en quartiers, et dans chascun il y a des prefects qui sçavent le nombre des personnes et tout ce qui s'y passe.*

(2) The *trois plus grands mérites de Graunt devant la science statistique* are: **1°** *il fut le premier qui établit d'après des matériaux statistiques les lois empiriques qui sont propres aux phénomènes collectifs atypiques*; **2°** *il montra la manière pratique avec laquelle on peut et on doit utiliser les données statistiques après les avoir soumises à une analyse critique*; **3°** *il établit la première table de mortalité.*

(3) *The concept of a life table was an outstanding innovation and it lay ready for Halley's use. ... Graunt's work created the subject of demography.* (It) *contributed to statistics in general.*

Even if Graunt did not actually create the subject of demography completely alone, he at least published the first quantitative demographic research.

2.4.4. Leibniz. As to probability proper, Leibniz should be credited with findings mainly in the field of games of chance; they have been described in a series of articles by K.-R. Biermann and K.- R. Biermann & Margot Faak.

Besides this, Leibniz claimed there was need for a probability logic [93, p. 115]. Lastly, the correspondence between him and Jakob Bernoulli was also extremely valuable. Part 4 of the *Ars conjectandi* was written with a mind to Leibniz' opinion.

Bernoulli had confided to Leibniz his ideas about using statistical probability on a par with theoretical probability. Leibniz, at least initially, disagreed. He [93, p. 138] may have been prepared to weigh delicate subjective opinions and probabilities rather than enumerate successful and unsuccessful trials.

Now I state more definitely that, according to Leibniz [109, p. 288] and his letter to Bernoulli of 3 Dec 1703 [33, p. 405],

(l) *Deren* [*zufällige Dingen* are meant] *vollkommener Beweis jeden endlichen Verstand überschreitet.*

(2) *Was von unendlich vielen Umständen abhängt, nicht durch endlich viele Versuche bestimmt werden kann.*

This reservation was possibly borrowed from the *Port-Royal* [2, p. 372]:

(1) *Il est de la nature d' un esprit fini de ne pouvoir comprendre l'infini.*

(2) *Ce serait un défaut de raison de s'imaginer que notre esprit étant fini, il put comprendre jusq'aù peut aller puissance de Dieu, qui est infinie.*

In any case, it proved pessimistic: science generally, and mathematical statistics in particular, has merely to do with transitions from finite to infinite. But of course a final confirmation of facts or hypotheses by statistical data is theoretically impossible and in this sense Leibniz was absolutely right.

My last comment is that, in accord with his general philosophical point of view, Leibniz likely believed in deduction rather than in induction; see however the discussion of his contribution [62] below. Only in 1714, in a letter to one of his correspondents [112, p. 570] he seemed to recognize the principle of posterior estimation of probability:

On estimé encore les vraisemblances a posteriori, par l'expérience, et on y doit avoir reqours au défaut des raisons a priori: par exemple, il est également vraisemblable que enfant qui doit naistre soit garcon ou fille, parce que le nombre des garcons et des filles se trouve à peu près egal dans ce Monde. L'on peut dire que ce qui se fait le plus ou le moins est aussi le plus ou le moins faisable dans l'etat present des choses, mettant toutes les considérations ensemble qui doivent concourir à la production d'un fait.

Considering the development of the theory of probability Leibniz also says, just before the quoted passage:

Feu M. Bernoulli a cultivé cette matière sur mes exhortations which was a fabrication pure and simple. In a letter dated 3 Oct. 1703 Bernoulli wrote to Leibniz [33, p. 404]:

Ich möchte gerne wissen, großer Meister, von wem Du erfahren hast, dass ich mich mit der Lehre von der Abschätzung der Wahrscheinlichkeiten beschäftigt habe.

A discussion of Leibniz' work on the subject is beyond my purpose. Readers may look up commentators, Biermann and Biermann & Faak included.

Now I turn to political arithmetic. This new discipline caught Leibniz' imagination, the more so because his and Petty's attitudes towards philosophy of science were similar (§ 2.4.2). Leibniz' writings on political arithmetic [60] – [64], none published during his lifetime, had been collected by Klopp [59] who maintains (p. xxxviii) that the first three of them *in die achtziger Jahre fallen* and that his Essay [63] was called forth by Petty's *Political Arithmetick* about which Leibniz also *hat eine Menge einzelner Bemerkungen und kleiner Aufsätze niedergeschrieben*.

Klopp (p. xxxvii) quotes one such remark which he found *auf einem losen kleinen Blätte*:

Il faut reduire toutes les sciences en Figures et en Formules, car, plusieurs choses ne pouvant estre exprimées par figures (si non analogiquement ce qui n'est pas scientifique), pourront estre au moins asujetties aux formules qui tiennent lieu de figures, et servent à arrester l'imagination.

Leibniz advocated the compilation of Staatstafeln [60, p. 303]:

Ich kenne Staats-Tafeln eine schriftliche kurze Verfassung des Kerns aller zu der Landes-Regierung gehörigen Nachrichtungen, so ein gewisses Land insonderheit betreffen, mit solchen Vortheil eingerichtet, dass der Hohe Landes-Herr alles darin leicht finden, was er bey jeder Begebenheit zu betrachten, und sich dessen als eines der bequämsten instrumente zu einer löblichen selbst-Regierung bedienen könne.

He noted the benefit of comparing such tables with each other [61, p. 316]:

Sonderlich aber würde großen Nuzen haben die comparation unterschiedener Lande, orthe unter einer herrschaft, und unterschiedener Zeiten eines Landes gegen einander. ... Daraus dann allerhand reale verbeßerungen erfolgen würden[4]".

Leibniz (Ibidem, p. 317) also mentions *special- und general-Registraturen*, the latter possibly being the same, or of the same nature, as the *Staatstafeln*. He urges (Ibidem, p. 319) the need to follow the example of theologians and lawyers:

Die Theologi haben Harmonias confessionum; die juristen haben Diflerentias variorum jurium ... weit nüzlicher würde sein eine Harmoni und collation in Regierungssachen, dadurch der Herrschaft und gemeinen Wesen viel nuz schaffen welches alles zu diesem Registraturen Amt eigentlich gehöhret.

Thus Leibniz recommends establishment of a special Amt[4§] and a list of fifty-six questions [64] seems to correspond to a programme, or a part of a programme, for its statistical inquiries. Among these questions are such as

1. Numerus hominum

4. Quot foeminae aptae ad generandum

21. Comparatio mortium et nativitatum

26. Quanta sit agri frugiferi media fecunditas intra 7 circiter annos

47. Descriptio exacta omnium artium et vitae professionum[44]

Leibniz [62] pays special attention to medicine. In his opinion (p. 321)

Der juristen insgemein zu viel, der Medicorum aber zu wenig seyen.

Considering also peoples' *Blindheit,* when (p. 322)

Es geht den meisten mit der gesundheit wie mit der seeligkeit, deren keines sie achten, bis sie von der späthen reue übereilet werden.

Following his general line of thought, Leibniz (p. 321) advises a

Zusammensetzung der bereits vorhandenen wissenschafften, Erfindungen, Experimenten und guther gedancken,
which would bring under control *vielen Krankheiten*.

He also urges practitioners to record their observations (p. 325):

Wenn jeder practicus nur einen einigen richtigen Aphorismum zu den Hippocraticis oder andern bereits bekandten gefüget hatte, man jezo weit kommen sein würde. Ich nenne aber aphorismum nicht eine jede thesin, sondern diejenigen säze so nicht durch die vernunfft erhellen, noch von selbsten sich verstehen, sondern aus der erfahrung vermittelst fleißiger beobachtung entdecket werden. Wiewol derjenige so ein schöhnes theorema oder vernunfftschluß, dessen man sich nicht leicht versehen solte, durch scharfes nachsinnen a priori oder aus betrachtung der Ursachen ausgefunden hätte, so durch die erfahrung hernach richtig befunden würde, wegen solcher seiner scharfsinnigkeit nicht weniger als jener wegen seiner fleißigen aufmerksamkeit zu loben und zu belohnen.

The last lines are related to the general problem of comparing deductive and inductive methods. On the whole, Leibniz' contribution [62] is in line with the ideas of political arithmeticians concerning the betterment of human life and multiplication of mankind (see § 2.4.2).

A special point made in Leibniz' work [62] is a concrete proposal (p. 322) to establish a *Collegium Sanitatis* for supervising shops, bakeries etc. Moreover (p. 323),

Die Acta und Archiva des Collegii Sanitatis köndten und müsten unter andern in sich halten, was in gesundheitssachen, und damit verwandten Dingen von Zeiten zu Zeiten passiret, und sonderlich wie in diesen und benachbarten orthen das wetter sich gewechselt, ..., wie sich das gewicht der lufft auch des magnets declinationen und inclinationen geändert, und was dergleichen durch die neuen istrumenta, nehmlich Thermometra, Hydroscopia, Anemia, Barometra und gewiße Compasse zu entdecken. Ferner wie diese oder jene orth von früchten und obst gerathen, was die victualien für einen preiß gehabt, für allen Dingen aber was für Krankheiten und Zufälle unter Mensch und Vieh regiret, da dann die Symptomata, auch juvantia und *nocentia* [wholesome and harmful] *sammt allen umbständen aufs genaueste zu beschreiben*.

I do not think any such Collegium ever existed! In his *Essay* Leibniz [63, p. 328] supposes that the

Bornes ordinaires de la vie humaine sçavoir 80 ans, comptant pour rien le petit nombre de ceux qui le passent

and that (p. 329)

81 enfans nouvellement nés mourront uniformement, c'est à dire à un par année dans les 81 ans suivans.

He (pp. 330 – 332) calculates the *moyenne longueur de la vie humaine* both for new born babies and people of any age, necessary, as he notes, for estimating the value of life annuities. Assuming one more supposition (p. 334), viz.,

Que la fecundité des hommes est aussi tousjours la même et tellement égale à leur mortalité,

he notes (Ibidem) that the *multitude des hommes ne change pas notablement, si non par quelques accidens particuliers et extraordinaires*.

Il s'en suit par là [p. 335] *que si 100 enfans de dix ans meurent, il mourront aussi 100 personnes de 20 ans, et 100 personnes de 30 ans, et généralement autant d'un aage que d'un autre ... car si les viellards sont plus sujets naturellement à mourir, leur nombre aussi est plus petit à proportion*.

One more conclusion (p. 336) is that

Il meurt à peu près la quarantième partie des hommes par an.

This conforms to experience, *quoyque on l'ait trouvé a priori et par le seul raisonnement*.

I should also remark that Leibniz (p. 327) introduces *apparence* which *n'est autre chose que le degré de la probabilité*; for an ordinary die it is the same for each face.

There also exists *une Apparence Moyenne* (expectation). An example of calculating it follows (Ibidem):

Supposons ... qu 'il s'agissé de sçavoir la valeur de quelque heritage, maison ou autre bien, qu'on doit estimer …

The value sought, Leibniz explains, is determined by *trois bandes d'estimateurs*, see [63] where the whole passage is repeated, the only essential difference being the change from *Apparence Moyenne* to *Prostapherèse*. Thus Leibniz seems to leave the term probability beyond the realm of mathematics, although he does not say so.

Now I discuss the opening lines of the *Essay* (p. 326):

Cette recherche peut avoir un usage considérable dans la politique: l'un pour juger de la force d'un éstat, et du nombre des personnes vivantes par le nombre des morts qui se voit dans les listes des mortuaires[45]*, qu'on a coustume de dresser sur la fin de chaque année; l'autre pour estimer la longueur moyenne de la vie d'une personne a fin de donner une juste valeur aux rentes à vie, qui sont d'une grande utilité dans l'éstat comme feu Monsieur le pensionnaire de Wit a fair voir*.

See also § 2.4.2 where I emphasize the importance of the institution of annuities.

The *Essay* with its important if simple conclusions remained unpublished and did not influence such scholars as Halley (or, perhaps Huygens). In particular, the possibility of estimating populations overlooked by Graunt and understood by Leibniz was (independently) pointed out by Halley (§ 2.4.5).

2.4.5. Edmond Halley. In 1693 Halley published a memoir [44] which played an outstanding role in the foundation of demographic statistics[46]. Drawing on incomplete and inaccurate data on mortality in various age groups he arrived at his main result, a mortality table, or, more correctly, at a table of survivors for a stationary population.

Being unsatisfied with his initial data because of its irregularity, Halley attributed it to chance (p. 5):

Yet that (irregularity) *seems rather to be owing to chance, as are also the irregularities in the series of age, which would rectify themselves, were the number of years* (of observation) *much more considerable*.

Moreover, using additional data on births, Halley rectified these irregularities. However, adjustment of data in demography mainly aims at detecting (and excluding) systematic influences rather than chance effects[47]. But Halley seems to be the first to correct the observed frequencies of events. As in the case of De Witt (§ 2.3.3) Halley's statement is related to the general idea which underlies the law of large numbers.

Halley used his table for some stochastic considerations, in particular for calculating probabilities concerning the lives of two men. One of his problems is this: calculate the probability that two men, aged 18 and 35 correspondingly, will both remain alive after eight years; that they will both die; that only one of them will remain alive (two cases). Calculating these probabilities, Halley used the multiplication theorem for independent events. For example, the answer to the first question is

$$P = \frac{50 \cdot 73}{610 \cdot 490} \qquad (2.4.4.1)$$

(50 = 610 – 560, 73 = 490 – 417).

It seems that Halley was not satisfied with such analytic procedures. Possibly imitating ancient mathematicians, he repeated his considerations using the geometrical method, even in the three-dimensional case with each number from (2.4.4.1) and from similar expressions for other probabilities corresponding to the length of a side of a certain rectangle.

Discussing "chances" $50 \cdot 73$ and $610 \cdot 490$ Halley did not introduce probabilities 50/610 or 73/490. Neither did he introduce geometric probabilities: the lengths of the sides of the rectangles were represented by integers. However, Halley offered a geometric illustration of classical chances.

He also pointed out (pp. 6 – 7) that the solution of such problems as described above makes it possible to calculate the value of annuities on two or three lives[48], the

Proportion of men able to hear arms, the different degrees of mortality, or rather vitality, in all ages,
and the probable duration of life (the term itself is not introduced). Then (p. 8),

By what has been said, the price of insurance of lives ought to be regulated.

What he actually means is the comparison and possible adjustment of the price of insurance (of annuity?) for men of various ages.

Adding up the numbers of survivors at each age shown in his table, Halley achieved his most important success: he thus calculated the whole studied population. Exactly this calculation mainly impressed Halley's contemporaries [8, pp. 1 – 2]: at a time when censuses were completely unknown, he had shown the way to estimate populations from data on births and deaths[49]. See also § 2.4.4.

Halley's table greatly influenced De Moivre. Having turned his attention to annuities on lives De Moivre [22, p. 262] consulted Halley's memoir and

Found that the Decrements of Life, for considerable Intervals of Time, were in Arithmetic Progression[50].

Thus the uniform distribution was properly introduced into probability[51]. In the course of time, however, it proved too primitive and was superseded by other distributions. However, at the hands of such masters as De Moivre the use of even this primitive distribution led to the formulation and solution of important stochastic problems.

2.4.6. Caspar Neumann. The data which Halley used were collected by Neumann (1648 – 1715) in Breslau where [40], p. 206, footnote l], beginning from 1542,

Die Namen der Getauften, Getrauten und Gestorbenen in dem Kirchenbuche zu Maria Magdalena, und 1569 zu Elisabeth were entered *in ein Buch*[52].

According to this source (p. 10), after spending three years at Jena University, Neumann *wurde 1670 zum Magister der Philosophie promovirt*. A *Prediger* since 1673 (p. 12), Neumann corresponded with Leibniz freom 1689 (p. 13). In 1706, on Leibniz' recommendation, Neumann was elected member of the *Kgl. Societät der Wissenschaften* in Berlin.

He (p. 204)
... war, wie es scheint. in Deutschland der Erste, welcher über die Zahlenverhältnisse der jährlichen Geburten und Todesfälle, zunächst innerhalb der Grenzen seines Wohnorts, zusammen-hängende Beobachtungen anstellte und zu allgemeinen Schlüssen benützte.

Neumann's main work seems to be lost (p. 207; see also below):

Als die Herausgabe des ersten Bandes des der Miscellanea Berolinensia ... vorbereitet wurde, ward zwar Neumann von Leibniz zu einem Beitrage aufgefordert, doch schickte er damals nichts ein. Erst in J. 1713 übersandte er ... eine Abhandlung De methodo periodica in Obss, meteorologicis adhibenda. ... (Der zweite Band erschien lange nach seinem Tode 1723). Jedenfalls war Schlesien lange vor seiner Einverbindung in den preußischen Staat durch ... Neumann in der Berliner Königlichen Societät ... würdig vertreten.

C. Wolff, Neumann's pupil and friend (p. 208),

Selbst hat bei jeder Gelegenheit dankbar bekannt, wie viel er … Neumann verdankte, doch ohne gerade seine Methode zu rühmen.

Guhrauer appends eleven letters exchanged between Leibniz and Neumann. In one undated letter (p. 265) Neumann remarks that he *vil Jahre lang* meteorological observations *begriffen gewesen*. In another letter dated 1707 (p. 267) he discusses the influence of the moon on the weather:

Das aber der Monde mit der Luft ihren Veränderungen einige Verwandnüß habe, muthmaße ich, sei auch schon bei den Hebräern geglaubt werden.

Die Observationes meteorologicae erfordern eine gewisse Theorie, Neumann says in yet another letter of the same year (p. 269), *ohne welche imand anders sich schwer zum observiren schicken wird. Ich könte aber schon damit dinen. Was bißher von solchen Dingen geschriben ist; oder auch die Parisisthe Societät in ihren letzteren Actis hat …, das ist alles vil zu wenig.*

In the last letter dated 1713 (pp. 272 – 273) Neumann mentions his writing on meteorology:

Habe mir aber gegen das Ende des abgewichenen Jahres die Ehre genommen … einen kleinen Discours de methodo periodica … beigeleget. Weil nun dises leztere inein besonderes an Ew. Excellence haltendes paquet eingeschlossen gewesen, und vielleicht in Berlin möchte sein ligen gebliben; ich aber doch nicht gern wolte, dass dise wenige Arbeit verlohren gehen, oder in fremde Hände gerathen solte.
A small essay on Neumann written by F. Cohn is in the book by Graetzer[35][53]. Cohn does not substantiate his attribution to Neumann of an attempt (p. 27)

Durch statistische Ermittelungen zu erproben, ob den wirklich ein Zusammenhang zwischen Geburt und Tod der Menschen und gewissen kabbalistischen Zahlen oder dem Stande der Planeten nachweisbar sei.

Possibly Cohn had in mind a letter of Neumann to Leibniz dated 1689 [40, pp. 263 – 264] where Neumann says:

Als nehme ich mir endlich die Freyheit, einige Abschrift von den bisher gemachten Reflexionibus über Leben und Tod bei denen in Breslau geborenen und gestorbenen zu überreichen, wiewohl das, das gegenwärtige zu Ende laufende 89ste Jahr noch nicht hat können beigefüget werden. Noch zur Zeit kann man freilich nicht sehen, was eigentlich der Nutzen davon sein werde[54]*. Sollte aber Gott das Leben so lange fristen, dass man die Rechnungen etzlicher Jahre zusammen bringen könnte, oder auch jemand in einer andern Stadt dergleichen Observationes machen … so würden als denn schöne Anmerkungen göttlicher Providenz über unser Leben und Tod, Erhaltung und Vermehrung der Welt, und dergleichen mehr können gemacht, auch vielerlei Aberglaube desto besser aus der Erfahrung widergelegt werden. Ich beklage sehr oft dass itzund fast die ganze gelehrte Welt in regno Naturae sich auf Experimenta leget und Observationes schreibt, aber kein Mensch dergleichen in regno gratiae oder in der Theologia zu thun gedenket.*

For his part, Leibniz, in a letter to Justell dated 1692 [65, p. 279] testified:

Mons. Neuman (!) … *a fait des bonnes remarques sur les mortuaires et baptêmes de la ville* (Breslau), *qu'on m'a communiquées. Entre autres il observe que les contes des années climacteriques ne se verifient point.*

Most likely Neumann rejected the *contes* as a result of a common sense analysis of data. Still, for his time, this is something well worth remembering.

A letter from Halley to Neumann on mortality dated 1692 is published in the original Latin in Halley's *Correspondence* [44a, pp. 88 – 89]. A letter from Neumann to Halley dated 1694 kept at the Royal Society (Ibidem, p. 35) was published by Graetzer [35, p. 42].

3. Pascal and Fermat

This section is mostly devoted to the correspondence of Pascal and Fermat which was initiated, as it seems, by the former. Pascal's attention had been attracted to games of chance by De Méré, a man of the world well versed in them although not a gambler by vocation [73. p. 409].

The extant part of the correspondence between Pascal and Fermat [29, pp. 288 – 314 and 450 – 452] includes eight letters; two of them, written in 1660, do not directly bear on my subject. The other six letters were written in 1654 and games of chance are discussed in four of them. It is these letters that I describe below. I also mention in passing the fifth letter of the year (Fermat – Pascal, 29 Aug. 1654).

Meusnier (2009) stated that five letters are missing and that one letter from Fermat was actually written by someone else.

3.1. Fermat – Pascal, letter without date [29, pp. 288 –289]. Gambler A tries to accomplish a certain score with a single die by throwing it eight times in succession. Suppose now that A is to throw the die only seven times. What part of the stake is due him as compensation?

Par mon principe, says Fermat, obviously answering a letter from Pascal, A is to receive 1/6 of the stake. If A will throw the die only six times he is to receive additionally 1/6 of the remainder, i. e. $1/6 \cdot 5/6 = 5/36$ of the stake etc. Finally, if, for example, the first three throws prove unsuccessful, A's compensation for one of the remaining throws is still 1/6 of the stake.

The probability of scoring k points ($k = 1, 2, …, 5, 6$) in eight throws exactly once is less than the probability of the same event happening in seven throws; generally, in n throws

$$p_n = \frac{n}{5}(5/6)^n,\ p_8 < p_7.$$

For this reason and, of course, bearing in mind the general context of Fermat's letter, I interpret the condition of the problem as scoring k points at least once.

Denote the probability of success in each throw by p ($p = 1/6$). Then the probability of success in two throws is

$$p + p(1-p) = 2p - p^2 \qquad (3.1.1)$$

and in three throws $3p - 3p^2 + p^3$. Thus we arrive at the general formula of inclusion and exclusion known to De Moivre and Simpson [90, pp. 278 – 279].

Fermat actually used the *classical* definition of probability and, also, the concept of expectation of a random event. The essence of Fermat's *principe*, as I see it, was the calculation of expected gains (or losses). But neither he nor Pascal formally introduced probability or expectation, nor did they introduce any formal term for expectation. Lastly, expressions like (3.1.1) involving "pure" probabilities possibly remained foreign to both of them.

3.2. Pascal – Fermat, letter of 29 July 1654 [29, pp. 289 – 298]. It begins by discussing the problem of points (3; 2:1), with notation as in § 2.1 above. A wins the fourth set (and the whole game as well) with probability 1/2, therefore, argues Pascal, a half of the stake is already due him. If, however, A loses the fourth set, the score equalizes so that A is to receive additionally 1/2·1/2 of the. stake. The total share of the stake due A is thus 3/4.

3.2.1. Random walks. Freudenthal [31, chap. 7] interpreted this problem, or rather the problem (5; 4:3) in the following way.

Point M_1 with coordinates (4, 3) is the initial situation. As the game goes on, new positions of that point are described by a transitional matrix π. Since the maximal number of sets is 2, the end position of M_1 will be described by matrix π^2.

It is also possible to interpret this problem in terms of conditional probabilities. Denote the event of A's winning the third set by A_1, the contrary event by A_2, and the event of A's winning the game as a whole by A. Then

$$p(A_1) = p(A_2) = 1/2,\ p(A/A_1) = 1,\ p(A/A_2) = 1/2,$$

$$p(A) = \sum_{i=1}^{2} p(A/A_i)p(A_i) = \frac{3}{4}.$$

[See §4.2.2 in which I comment on a similar formula.]

3.2.2. Binomial coefficients. Pascal's letter also contains a rule for sharing the stakes and a remark concerning a game of dice. He considers the problem of points for games of the type (n; a:0), a= 1, 2,..., n – 1. Although he does not say so, this is sufficient because games (n; a:b) with $a > b$ are equivalent to games [(n – b); (a – b):0].

Pascal begins with game (5; 1:0) and notes that the *valeur* of the first set (A's expected gain due him after the first set over and above his own stake) is

$$\frac{{}_{1/2}C_8^2}{{}_{1/2}C_8^4 + C_8^5 + C_8^6 + C_8^7 + C_8^8} = \frac{35}{128}. \qquad (3.2.2.1)$$

Indeed, the maximum number of remaining sets is eight, so that the value of the first set (in terms of both stakes) is

$$\frac{C_8^0 + C_8^1 + C_8^2 + C_8^3 + C_8^4}{2^8} - \frac{1}{2} = \frac{1/2 C_8^4}{2^8} = \frac{35}{256}$$

(the second term in the left-hand side is the prior probability of *A*'s winning the game).

For the case (n; 1:0) the value of the first set in Pascal's sense is

$$p_1 = \frac{C_{2n-2}^{n-1}}{2^{2n-2}}. \quad (3.2.2.2)$$

With no explanation Pascal included a table of values of each set for games (n; 1:0) and $n = 1, 2,..., 5, 6$. In particular, the value of the second set (again in Pascal's sense) is

$$p_2 = \frac{C_{2n-3}^{n-2}}{2^{2n-3}}. \quad (3.2.2.3)$$

Pascal had not given formulas (3.2.2.2) or (3.2.2.3) though he noticed that

$$p_1 = \frac{1 \cdot 3 \cdot 5 \cdot ... \cdot (2n-3)}{2 \cdot 4 \cdot 6 \cdot ... \cdot (2n-2)}$$

and that $p_2 = p_1$.

Thus, in calculating values of various sets (probabilities), Pascal used sums of binomial coefficients. However, in his letters he had not referred to the arithmetic triangle. His treatise on this triangle [74] was published posthumously and Fermat mentioned it in his letter of 29 Aug. 1654 [29, pp. 307 – 310]. Possibly Pascal had sent him a copy of this treatise (still unpublished or at least not yet normally published). He had not remarked that sums of binomial coefficients can be used irrespective of the score although he did just this in his treatise (§ 3.6).

3.2.3. Small differences between probabilities. As to Pascal's comment on games of dice, he refers to an error committed by De Méré who supposed that the probability of scoring six points in four throws of a die is equal to the probability of scoring twelve points in 24 throws of two dice. The probability of these events are

$p_1 = 1 - (5/6)^4 \approx 0.5177$, $p_2 = 1 - (35/36)^{24} \approx 0.4913$.

De Méré knew, obviously from experience, that $p_2 < 1/2$ and declared: *L'Arithmétique se démentoit*. However, noticing the minuteness of the difference between the two events Ore

[73, pp. 411 – 412] and Van der Waerden [97] do not believe in explanations based on experience. Ore supposes that De Méré knew two formulas for calculating p_2, the correct one above and its rather crude substitute[55] and that De Méré's comment was provoked by the difference between the respective results[56].

Can a minute difference between probabilities be detected empirically? It seems that it was possible for professional gamblers by paying attention only to the results of the appropriate throws, see also note 56. However, apart from them there possibly was a kind of gamblers cum businessmen who preferred safe ways of fortune-making to taking chances. One such way would be to participate in many games whose probability of winning was (even if slightly) higher than a half.

Considering that marine insurance (§ 2.3.1) and life insurance (§ 2.3.2) were partly connected with betting and, also that men of substance purchased life annuities on a number of young lives simultaneously (§ 2.3.3), why could bets not have been made on the results of many games? Obviously, the probabilities of various outcomes in these games had to be estimated beforehand, but this was probably accomplished by small "reconnaissance" bets.

In a few of his problems De Moivre [21, problem 70 and others] introduces both gamblers and *Standers by*. Who were these bystanders, unnecessary from the mathematical point of view? Late comers unable to find a place for themselves or those in strained circumstances afraid to lay the usual stake? Hardly would De Moivre introduce such characters. Were they not after all (petty?) businessmen laying bets on many games at once? I have also found *Spectateurs* in one of Montmort's problems [69, p. 169]: *Pierre, Paul et Jacques jouent au Brelan. Deux des Spectateurs ... disputant ...*

A second indirect fact in my favour is that a small difference of 0.0160 between two probabilities was detected, possibly by observation, in the 18th century [21, p. iii]:

When the Play of the Royal Oak was in use, the Odds against any particular Point of the Ball were One and Thirty to One. This intitled the Adventurers, in case they were winners, to have thirty two Stakes returned ... instead of which they (received) *but Eight and Twenty ... The Master of the Ball maintained that they had no reason to complain; since he would undertake that any particular point of the Ball should come up in Two and Twenty Throws; of this he would offer to lay a Wager.*

The probability of gain by the Master of the Ball was

$$\boldsymbol{p}_{22} = 1 - (31/32)^{22} = 0.5004$$

while for 21 throws

$$p_{21} = 1 - (31/32)^{21} = 0.4844.$$

The Master did not undertake to reduce the number of throws to 21; he possibly knew that $p_{21} < 1/2$.

Finally, I note that in discussing the problem of small differences between probabilities Kendall [54, p. 29] concludes that

Relative chances were all reached on the basis of intuition or trial and error in the games played up to the middle of the seventeenth century.

3.3. Pascal – Fermat, letter dated 24 August 1654 [29, pp. 300 – 307]. Pascal discusses Fermat's method (*votre méthode, qui procéde par les combinaisons*) of solving the problem of points and maintains, referring to Roberval, that it could not be applied in the general case of three or more gamblers. (Actually, Roberval objected to this method even in the case of two gamblers, but Pascal noticed that in that case his objection was unwarranted.)

Fermat's method, as described by Pascal, consists in separately enumerating combinations that lead to wins by each gambler and dividing the stakes in the ratio thus obtained. For problem (n; $(n-2):(n-3)$) the maximal number of sets is four, but the game can also end after two sets. Would it be correct then, asked Roberval, to enumerate combinations occurring in four sets? Even if the game ended in two sets, answered Pascal, it could be supposed to continue fictitiously for two sets more. He could have added that exactly this was his own tacit assumption (see § 3.2.2). For four sets the 16 possible combinations of wins are *aaaa* (all four sets won by A), *aaab* (three sets won by A and one set by B), ..., *bbbb*. In all, the letter *a* is contained no less than twice in 11 combinations and letter *b* no less than thrice in 5. Therefore, the stakes should be divided between A and B in the ratio of 11/5.

Consider now the case of three gamblers. Applying the same method, Pascal finds that in some instances the game is won by two gamblers. Therefore he recommends to return to his own *méthode générale*. For game (n;$(n-1):(n-2):(n-2)$) the stakes should be divided as 17:5:5, Pascal adds without proof.

Consider various combinations occurring in two (not three) sets of this game. Obviously, (l) A wins with probability 5/9, and (2) the score equalizes with probability 2/9. Thus the share of A is

$$5/9 + 2/9 \cdot 1/3 = 17/27$$

etc. However, the term *méthode générale* gives rise to doubt: first, Pascal had not used this term before; second, when calculating the values of different sets (§ 3.2.2) he used another method.

And here I note a disappointing error on Pascal's part [29, p. 301]: if a game of two gamblers continues through four sets the total number of possible combinations is, he says, 42. Actually, it is 24. The end result is the same, which would not be the case in general.

3.4. Fermat – Pascal, letter dated 25 September 1654 [29, pp. 310 – 314]. Fermat defends his combinatorial method. First, the case of two gamblers winning simultaneously can be reconciled with common sense if the priority of winning is additionally considered. Second, the probabilities of wins by each gambler could be separately calculated by the combinatorial method for various durations of the game. Thus in Pascal's example (n;$(n-1):(n-2):(n-2)$), A wins the whole game with probabilities 1/3, 2/9 and 2/27 for durations of one, two and three sets respectively. The total probability of his winning is 17/27.

In passing, Fermat noticed that the total number of combinations in n sets (n = 2 or 3) is 3^n. Most likely he realized that the total number of combinations for m gamblers and any natural n would be m^n.

For the case of three gamblers combinations can be enumerated in Fermat's method by using expressions of the type

$$(a + b + c)^n \qquad (3.4.1)$$

where n is now the maximal number of remaining sets. Thus, again for Pascal's example, n = 3 and combination *abb* will occur thrice. Now these three occurrences are *abb, bab, bba* so that only two of them (the first ones) are favourable for A etc.

Denote the number of sets to be won by A, B and C by X, Y and Z respectively,

$$X+Y+Z = n,$$

then the use of expressions such as (3.4.l) resembles the use of "triple" generating functions

$$1/3^n(a+b+c)^n$$

of distribution for the triplet $\{X, Y, Z\}$ and l/3 is the probability of each set being won by each gambler.

3.5. Another problem posed by Pascal. In a letter to Huygens Carcavi [47, t. 1, pp. 492 – 494] describes the following problem which Pascal proposed to Fermat. Throwing three dice, A undertakes to score 11 points, while B, under the same condition, undertakes to score 14 points. Each success counts for one point and the game is to last until one of the gamblers is 12 points ahead of his partner. Required is the ratio of chances of the gamblers.

Fermat gave a correct answer: the chances of A and B are in a ratio of 1156:1 approximately. Indeed, in any set these chances are 27 and 15 respectively so that the required ratio is $(27/15)^{12} \approx 1157$.

Pascal also gave the correct answer by calculating 27^{12} and 15^{12}. He did not bother to cancel three out of the initial ratio.

Huygens [47, t. 1, pp. 505 – 507] soon solved this problem as well. And it seems to be the problem in which probabilities rather than expectations were first sought. See also § 4.1.

3.6. Pascal's treatise [74]. I begin by quoting two passages (pp. 478 and 482).

(l) *Pour entendre les règles des parties la première chose qu'il faut considérer, est que l'argent que les joueurs ont mis au jeu ne leur appartient plus, car ils en ont quitté la propriété; mais ils ont reçu en revanche le droit d'attendre ce que le hasard peut leur en donner*[57]. ... *Le règlement de ce qui doit leur appartenir doit être tellement proportionné à ce qu'ils avoient droit d'espèrer de la fortune, que chacun d'eux trouve entièrement égal de prendre ce qu'on lui assigne,* *ou de continuer l'aventure du jeu: et cette juste distribution s'appelle le parti*.

(2) *Il ne faut proprement avoir égard qu'au nombre des parties qui restent à gagner à l'un et à l'autre, et non pas au nombre de celles qu'ils ont gagnées*.

Pascal goes on to mention three methods to *faire des parties*. One of them is just a straightforward use of expectations. *Il y en a deux autre*, adds Pascal, *l'une par le triangle arithmétique*, et *l'autre par les combinaisons*. It would have been more correct to distinguish between the criterion (expectation) and the methods of calculating probabilities needed to use this criterion.

Here in contrast with his correspondence (§ 3.2.2) Pascal not only estimates the value of various sets (pp. 493 – 498) but also uses his arithmetic triangle (pp. 488 – 489) for calculating sums of binomial coefficients in the general case ($n; a:b$). If the tabular form of defining a set of numbers is recognized on a par with the analytical form, then, for the binomial distribution with $p = q$, Pascal's method is equivalent to the method of generating functions. For this reason I am inclined to begin the prehistory of these functions with Pascal.

Notice, however, that the tabular form of defining binomial coefficients prevented Pascal from generalizing his method of dividing the stakes to include the case of three gamblers. Such generalization would have led to the coincidence of the methods of Pascal and Fermat (see end of § 3.4).

3.7. Aleae geometria. In l654 Pascal communicated a letter [75, pp. 101 – 103] *A la très illustre Académie Parisienne de science* (the forerunner of the official Academy) informing it about his desire to write a number of tracts. One of them was to be devoted to geometry of chance:

…Joignant la rigueur des démonstrations de la science (*matheseos* is the Latin original) *à l'incertitude du hasard et conciliant ces choses en apparence contraire, elle peut, tirant son nom des deux, s'arroger à bon droit ce titre stupéfiant: La Géométrie du hasard.*

Pascal's wish did not come true but its mere existence proves that he wanted to explicate elements of the theory of probability. An interesting attempt to guess the subject-matter of Pascal's proposed tract is due to Rényi [86]. I can agree with him about the subject-matter, but not with the year (1654) which he proposes as the date of Pascal's work or, rather, his unknown letters to Fermat. In any case, in his letter to the Academy Pascal mentions only one problem, that of dividing the stakes; no other problem is even hinted at.

Another shortcoming of Rényi's attempt is his lack of a reference to Aristotle, who [93, § 2. 2] had connected randomness with non-fulfilment (or non-existence) of goal. Aristotle's point of view would have hindered the development of probability had it not been tacitly rejected both by Huygens and Jakob Bernoulli (or at least not upheld by them). As to Pascal, the reasoning on philosophical problems of probability put into his mouth by Rényi seems hollow just because he does not mention the Philosopher.

4. Huygens

I discuss Huygens' main work in probability in § 4.1. His correspondence and manuscripts which contain interesting achievements pertaining to probability are described in § 4.2, while § 4.3 is devoted to moral certainty as explicated by Huygens[58].

4.1. Huygens' main work [48]. It is prefaced by a letter written by Huygens to Van Schooten (pp. 57 – 58), in which Huygens prophetically remarks that the study of games of chance is not a simple *jeu d'esprit* and that it *jette les fondements d'une spéculation fort intéressante et profonde.* However, continues Huygens, studying these games

Quelques-uns de plus Célèbres Mathématiciens de toute la France ... ont ... caché leurs méthodes (more accurately, did not publish their work). ... *Il m'est impossible d'affirmer que nous sommes partis d'un même première principe.* (Precisely this was the case). *Mais ... j'ai constaté en bien de cas que mes solutions ne différent nullement des leurs*.

The criterion Huygens used was the expectation of a random event; the essence of his treatise consisted in explicating this concept and its use in studies of games of chance.

The first three propositions of his treatise are devoted to the expected gain in a game of chance for cases of two or three equally probable gains and for a case of two unequally probable gains. Neither here nor in his correspondence does Huygens introduce a special term for this concept. Thus in proposition iii he only describes expectation:

Avoir p chances d'obtenir a et q d'obtenir b, les chances étant équivalentes, me vaut

$$(pa + qb) \div (p + q).$$

Neither Pascal nor Fermat introduced any term for expectation (§ 3.1). In the following six propositions Huygens discusses the problem of points in cases of two or three gamblers. Obviously he did not know either the elegant combinatorial method due to Fermat (§ 3.3) or the method of binomial coefficients due to Pascal (§ 3.2.2) and appropriate for the case of two gamblers. What Huygens actually did was to solve the problem of points for a number of sets of initial conditions by a direct calculation of expectations, and his proposition ix includes a corresponding table of results thus obtained.

The last five propositions are devoted to problems connected with a game of dice. I describe two such problems.

(1) [4.1(1)]. In how many throws should a gambler undertake to score twelve points with two dice.

(2) [4.1(2)]. *A* undertakes to score 7 points with two dice, and *B* undertakes to score 6 points. They throw the dice alternately with *B* beginning the game. Required is the ratio of their chances[59].

The natural modern solution[58] of the first problem is achieved by using the generating function

$$\frac{1}{36} + \frac{35}{36}x.$$

Describing Huygens' solution Reiersol [85] interprets it as the use of formula

$$E[E(X/Y)] = EX$$

in which, as I understand it, the first E corresponds to taking the mean among y's.

The treatise ends by formulating five additional problems. Here they are; the first and third were due to Fermat and the last one, to Pascal.

I. ***A*** undertakes to score 6 points in a throw of two dice while *B* undertakes to score 7 points. *A* begins with one throw, then *B* has two throws after which both in turn have two throws. Required is the ratio of the chances of these gamblers.

II. There are 12 counters, 8 black and 4 white. *A, B and C* playing in turn draw these counters [with or without retuning them?] one by one. The gambler who first draws a white counter wins the game. Required is the ratio of chances of these gamblers.

III. A pack contains 40 cards. Four of them are drawn. Required is the ratio of chances that cards of all suits are or are not thus drawn.

IV. A gambler draws 7 counters out of the 12 mentioned in problem II [same question as in II]. Required is the ratio of chances that among these 7 counters 3 will or will not be white.

V. *A* and *B* undertake to score 14 and 11 points respectively in a throw of 3 dice. They have 12 counters each and the winner receives one counter from his partner. Required is the ratio of chances that they be ruined[60].

4.2. Huygens' correspondence and manuscripts. He repeatedly returned to probability in his correspondence; besides that he left manuscripts on the same subject, possibly written in connection with his correspondence. These manuscripts are now published as appendices to his main treatise [47, t. 14, pp. 92 – 179][61].

4.2.1. The Year 1656. In 1656 Carcavi [47, t. 1, pp. 418 – 419] sent Fermat Huygens' solution of the problem of points. And Huygens also offered an additional problem on the same subject or possibly the same problem with another set of initial conditions (Ibidem, p. 432). Fermat solved that additional problem to the complete satisfaction of Huygens (Ibidem, p. 442):

J'ay veu par la solution que M. de Fermat à faite de mon Problème qu'il a la méthode universelle pour trouver tout ce qui appartient à cette matière.

Then, in a letter to Carcavi, Fermat (Ibidem, pp. 433 – 434) proposed (to Huygens?) five problems complete with answers but without indicating the method of their solution. Huygens published Problems No. 1 and 4 in his treatise as I and III.

The remaining three problems are

(2) *A*, who begins the game, undertakes to score 6 points in 2 throws of 2 dice while B undertakes to score 7 points. Required is the ratio of the chances of their winning.

(3) A and B draw in turn one card at a time from a pack of 52 cards until the first heart appears; he who draws it is the winner. Required is the ratio of the chances of their winning.

(5) 12 cards are drawn out of a pack of 36 cards. Required is the ratio of the chances that 3 aces occur or do not occur among the drawn cards.

Huygens solved these problems.

4.2.2. The Year 1665. In 1665 Huygens returned to probability in his correspondence with Hudde. At first [47], t. 5, pp. 305 – 311] they discussed three problems, II, IV and a problem pertaining to the game of *croix ou pile*.

Soon Hudde (Ibidem, pp. 348 – 351) generalized the last problem:

A has one white and two black counters; B has a number of counters of both these colours. Each gambler draws a counter out of his own stock and replaces it. When drawing a black counter the gambler stakes a ducat; when drawing a white counter he receives all the stakes and A begins. Required is the ratio of white and black counters in B's stock for a just game.

The solution of II is contained in a manuscript of 1665 [47, t. 14, pp. 96 – 101] published as Appendix 2 to Huygens' main treatise. Hudde [47, t. 5, p. 307] solved this problem assuming that the counters are drawn without replacement. He commented:

Il arrive au tirage des fèves a Hoorn et en Frise, lors de l'élection du Magistrat.

He got

$$x:y:z=232:159:104.$$

The correct answer (Jakob Bernoulli [3, part 1, p. 65] is

$$x:y:z= 77:53:35\ (= 231:159:105).$$

IV. Huygens solved it in the same manuscript as Problem II. He used both conditional probabilities and the formula (in standard notation)

$$P(B) = \sum_{i=1}^{n} P(A_i)P(B/A_i).$$

[This formula is known as **Laplace's** seventh Principle of his *Essai philosophique* (formulated in words).]

4.2.3. The year 1669 (mortality). While corresponding with his brother Lodewijk, (Christiaan) Huygens turned to stochastic problems in mortality. Thus probability was first applied beyond the realm of games of chance. The correspondence was most possibly initiated by Lodewijk and occasioned by the publication of Graunt's mortality table (§ 2.4.3). Becoming interested in calculating the expected duration of life for people of various ages, Lodewijk [47, t. 6, p. 483] wrote to his brother:

Selon mon calcul vous vivres environ jusqu'à l'aage 56 ans et demij (Christiaan lived 66 years plus). *Et moij jusqu'à 55*.

He obviously based himself on his own calculations (Ibidem, pp. 515 – 518). Lodewijk also calculated the mean duration of life for men aged 6, 16, 26, etc. years, at the ages which enter Graunt's table.

For Christiaan, then forty, the mean duration of life would be 57.1 years, rather than *56 ans et demij*. Indeed, six persons died from age 36 to age 46. Dividing the interval [36; 46] in five equal parts, I arrive at three points (42; 44; 46) to the right of point 40, i. e. the points (moments of death) that I have introduced. I was unable to find the date of Lodewijk's birth, hence unable to check the estimate of the duration of his own life.

Christiaan (Ibidem, pp. 524 – 525) warned that

Il ne s'ensuit pas que les 18 ans et 2 mois (the mean duration of life for a new born baby according to Graunt's table) … *soit l'aage de chaque personne creée ou conçeue* … and that (p. 528) *il est beaucoup plus apparent qu'il* (*l'enfant conçeu*) *mourra devant ce terme*.

Continuing the correspondence, Christiaan (Ibidem, pp. 531 – 532) introduced the probable duration of life though not the term itself: *Combien il reste raisonnablement a vivre* for a man of a given age, he asks. He shows that this duration can be determined by a graphic procedure. Once more (Ibidem, p. 537) he explains the essence of the probable duration of life and indicates its possible use:

Ce sont donc deux choses différentes que l'espérance ou la valeur de l'aage futur d'une personne et l'aage auquel il y a égale apparence qu'il parviendra ou ne parviendra pas. Le premier est pour règler les rentes a vie, et l'autre pour les gageures[62].

The graphic procedure for determining the probable duration of life was based on a graph [47, t. 6, plate inserted between pp. 530 and 531] which Huygens constructed by drawing a continuous curve through empirical points given by Graunt's table. This graph corresponded to a curve

$$y = 1 - F(x)$$

where $F(x)$ was the distribution function with an unusual interval of admissible probabilities [0; 100].

Now I consider two problems which Huygens (Ibidem, p. 528) formulated and partly solved in this correspondence.

(l) What is the expected period of time during which neither spouse would die? Or, continues Huygens,

Si on m'avoit promis 100 francs au bout de chasque an qu'ils vivront ensemble pour combien seroit il juste qu'on rachetast cette obligation?

(2) What is the expected period of time during which

(a) Forty persons aged 46 would die out?

(b) Two persons aged 16 would both die?

Huygens supposes that problem (1) does not essentially differ from the last mentioned.

Following is his solution of the last problem (Ibidem, pp. 526 – 531). According to Graunt's table each of the two men has

15 chances to live an average of five years more,

9 chances to live an average of fifteen years more, etc.

Suppose, says Huygens, that they both draw tickets with the duration of their lives written on them. If *A*, the one to die sooner, draws a ticket with five years, t*hen B*, the survivor, will live not less than five years. More precisely, his 15 chances to live 5 years change into 71/2 chances to live 5 years and another 71/2 chances to live 8 years. Other chances remain constant and *B*'s duration of life turns out to be 20.8 [20.3] years.

Now if *A* draws a ticket with 15 years, then etc.

Huygens (p. 530) notes that the same method can be applied to find the duration of *A*'s life. Thus, he used conditional expectations of life. He also contemplated to use an order statistic and was able to calculate its expectation for a discrete distribution.

Huygens did not solve problem 2b and of course calculations for 40 men were simply impossible. However, he could have well assumed that the last survivor would live almost until age 86, i.e. almost until the last possible age according to Graunt.

In connection with a problem similar to 2a Huygens (p. 538) arrived at a wrong conclusion: assuming an equal probability of death during each year from 46 to 56, he also believed that during the first years mortality would be higher

A cause que le nombre des personnes (of the studied group) *est plus grand alors qu'après que la mort en a ostè quelques uns*.

Possibly he assumed a constant ratio between the numbers of dead and still alive which is the case in stationary populations. No such assumption is valid for his problem: for a distribution continuous and uniform in some interval, *n* order statistics divide it into (n + 1) approximately equal parts and the yearly number of deaths should remain approximately constant.

4.2.4. Work during 1676 – 1688. In 1676 and again in 1679 and 1688 Huygens [47, t. 14, pp. l56 – l79] returned to games of chance. In 1676 he solved a problem similar to the one described in his main treatise [48]; in 1679 he studied a game called bassette, in 1688 he investigated another game later called Waldegraav's game [96, p 122].

Here is a description of that game. *A* plays a set with *B*. The loser deposits a ducat while the winner goes on to play with *C*, the last gambler, etc. The one who wins twice in succession wins the game and gets all the stakes. Huygens considered a few versions of this game, but, as noticed by Korteweg, he made mistakes. More important, his solution was not sufficiently explained, and in this sense it is much worse than a clear solution due to De Moivre [20, pp. 237 – 243; 21, pp. 132 – 159].

4.2.5. Huygens' analytical methods. In the words of Korteweg [47, t. 14, p. 20],

Il était facile à ses successeurs immédiats (Jakob Bernoulli?) … *de dépasser sur plusieurs points importants l'oeuvre de Huygens, au moyen de l'application de l'analyse combinatoire. Et il faut ajouter que ses prédécesseurs, Fermat et Pascal, se servient de même avec* *avantage* (*mais comme nous le sçavons à l'insu de Huygens*) *de cette analyse pour la résolution de quelques problèmes de jeu.*

However, there is yet another reason why Huygens' solutions are more complex than necessary, i. e. his use of expectations rather than probabilities.

In one of his problems the expected gains in various drawings differ while the probabilities of each outcome are constant throughout. Even if Huygens noticed this (at the time or later) he possibly had no desire to better his solution. After publishing his treatise [48] and solving a few important problems in mortality, he did not see any new applications of probability and lost interest in it[63]. This of course is only a conjecture but in any case the *fondements une spéculation fort intéressante et profonde* (§ 4.1) have been laid by Jakob Bernoulli.

And precisely because Huygens used variable expected gains he was unable to avoid equations in finite differences. Commenting on Huygens'solution of the problem mentioned above, Korteweg [47, t. 14, p. 135] noticed that

Toutes ces équations (used by Huygens) *se réduisent à des cas particuliers de l'équation*

$$x_m = m\Delta - 1/2x_{m-1} - 1/2x_{m+1}$$

(Δ is just 1 ducat).

Thus the name of Huygens should be connected with the history of equations in finite differences.

4.3. Moral certainty. This concept was introduced by Descartes and in the *Logique des Port-Royal* (§ 2.2). Huygens, for his part, introduced moral certainty into natural science[64]. He first applied it in one of his letters dated 1673 [47, t. 7, pp. 298 – 300]:

La cause de la pompe et du siphon est avec une très grande vraisemblance attribuée à la pesanteur de l'air et a son ressort. Parce que cette action de la pesanteur de l'air se manifeste dans cent expériences. ... Dans les choses de physique il n'y a pas d'autres démonstration[s] que dans le dechiffrement d'une lettre. Ou ayant fait des suppositions sur quelques légeres conjectures, si l'on trouve qu'elles se verifient en suite, de sorte que suivant ces suppositions de lettres on trouve des paroles bien suivies dans la lettre, on tient d'une certitude très grande que les suppositions sont vraies, quoy qu'il n'y ait pas autrement de démonstration, et qu'il ne soit pas impossible qu'on n'en puisse y avoir d'autres plus veritables.

En matière de physique il n'y a pas de démonstrations certaines, et ... on ne peut sçavoir les causes que par les effects en faisant des suppositions fondees sur quelques experiènces ou phénomènes connus, et essayant ensuite si d'autre effects s'accordent avec ces mêsmes suppositions ... D'autant plus qu'on trouvera de phénomènes conformes a l'hypothèse, d'autant plus vraisemblable la doit on tenir.

Se souvenant pourtant tousjours qu'on n'a point démonstration de sa veritè, et qu'il peut s'offrir tel autre phénomène qui estant incompatible avec nostre supposè principe le detruise absolument. Cependant ce manque démonstration dans les choses de physique ne doit pas nous faire conclure que tout y est également incertain, mais il *faut avoir égard au degrè de vraisemblance qu'on y trouve selon le nombre des expériences qui conspirent a nous confirmer dans ce que nous avons supposè. … En examinant et pesant bien ce degrè de vraisemblance que l'on a trouvè dans quelque chose, l'on peut en tirer grande utilitè, parce qu'on prévoit par les choses connues les effects qui raisonnablement doivent suivre, lorsqu'on appliquera certaines matières d'une manière nouvelle, ou que l'on fera telle chose pour obtenir tel eflect.*

Similar assertions are contained in the preface to the *Traite de la lumière* [50]: in physics

Les Principes se vérifient par les conclusions qu'on en tire … Sçavoir lors que les choses, qu'on a démontrées par ces Principes supposez, se raportent parfaitement aux phénomènes que l'expérience a fait remarquer; sur tout quand il y en a grand nombre, et encore principalement quand on se forme et prévoit des phénomènes nouveaux, qui doivent suivre des hypothèses qu'on employe, et qu'on trouve qu'en cela l'effet repond à notre attente. Que si toutes ces preuves de la vraisemblance se rencontrent dans ce que je me suis proposé de traiter, comme il me semble qu'elles sont, ce doit êstre une bien grande confirmation du succes de ma recherche.

In a letter dated 1691, Huygens [47, t. 10, p. 739] mentioned Descartes:

Je ne suis pas tout a fait pour le Criterium de des Cartes. Parce que dans la géométrie mêsme on s'imagine souvent de comprendre très clairement des choses qui sont fausses. Il a reste donc tous jours a sçavoir si l'on a compris clairement et distinctement, ce que est assez douteux dans des longues démonstrations. Et de la naissent les paralogismes. Je serais donc plus pour les divers degréz de vraisemblance, laquelle dans certaines rencontres est si grande que d'êstre quelque fois comme 10^{11} *et plus contre un*[**65**], *que le vray ou le faux d'une proposition, et qu'en de certaines choses cela va comme à l'infini.*

Thus Huygens seems to dismiss probabilities of the order of $p = (1 + 10^{11})^{-1} = 10^{-11}$. He does not explain the origin of his estimate, and it is doubtful whether he ever used this or any other estimate as a criterion, but I notice that, according to Borel [9, p. 27], $p = 10^{-6}$ is insignificant *on the human scale* and $p = 10^{-15}$ is insignificant *on the terrestrial scale*.

In a small work of 1690 [51] Huygens maintained (p. 541; see also the Editor's comment on p. 532) that human judgement is only more or less probable and that the degree of certainty of judgements should be assessed by common sense. He repeated this idea in [52, p. 688], in a writing in which he upheld and even used the thesis about the plurality of inhabited worlds[**66**]:

.. *Il y a beaucoup de degrés de vraisemblance dont les uns sont plus proches de la vérité que les autres; c'est surtout dans l'évaluation de ces degrés qu'on doit faire preuve de bon sens.*

According to the prevalent concept of his time [93, pp. 132 – 135 and 140], Huygens [52, p. 700] did not believe in the chance origin of the world:

En effet … un sectateur de Démocrite, ou bien aussi de Descartes peut se faire fort d'expliquer tout les phénomènes Terrestres que les phénomènes célestes de manière à n'avoir besoin que d'atomes et de leurs mouvements jamais de pareils objets n'ont pu être le résultat du mouvement déréglé et fortuit de corpuscules, puisque l'on constate que tout y est parfaitement accomodé à de certaines fins.

Addendum to §§ 2.2, 2.3.2, 2.3.3 and 2.4.4

The time which passed after this article was sent to the Editor proved unusually eventful. First, volume 3 of Jakob Bernoulli's *Werke* [101] arrived in Moscow. Expecting this source for quite a while, I never expected it to contain reprints of the works of Nicolas Bernoulli [4] and De Witt [100] complete with relevant commentaries as well as a number of contributions on the history of probability. In particular, Kohli & Van der Waerden [107] have mentioned a lesser known and possibly comprehensive contribution of Du Pasquier [114] on the history of tontines. I have now inserted two passages from this contribution in my § 2.3.2.

Second, Van Brakel, who commented on the work of De Witt (§ 2.3.3) has proved [103, pp. 130 – 131, note] that, after all, this work had become available in the next two or three generations after its first appearance. Van Brakel[67] also mentioned some sources not known to me before, notably the *Mémoires* [113]. These contain articles on De Witt, Huygens, Hudde, Struyck and Kerseboom as well as texts of *Certificats de rente viagère* dating back to 1228 and 1229, see § 2.3.2. Third, volume 2 of *Studies* [117] has appeared. Among other reprints it contains those of articles written by Seal [115] and Lazarsfeld [108] about which I did not know. Agreeing with Greenwood (§ 2.3.2), Seal does not attach any great importance to Ulpianus' table. Seal also refers to two general sources on the history of insurance [104], [118]. That which I managed to see [104] is well worth reading; however, I do not hold myself guilty of omitting any essential information on the subject.

I take up (1) the commentary [106] on Nicolas Bernouli's dissertation (§ 2.2); (2) Lazarsfeld's comment on the history of statistics (§ 2.4.4).

(1) Kohli (p. 541) remarks that

Der geistige Vater dieses [N. B.'s] *Werkes ist eindeutig Jakob. Ganze Abschnitte sowohl aus dem Tagebuch* [102] *als auch aus der Ars Conjectandi hat Niklaus wörtlich übernommen. An andern Stellen wurden Fragestellungen und bloße Andeutungen Jakobs aufgegriffen und weiterverarbeitet.*

The *Tagebuch* was not even meant for publication. Kohli (Ibidem) also provides a translation of a passage from the *Praefatio* to Nicolas Bernoulli's work. Jakob Bernoulli, N. B. testifies,

…Hat mir … die Veranlassung gegeben … den Gebrauch der Mutmaßungskunst in Fragen des Rechtes zu wählen. … Ich sehe dass mit Hilfe der Mutkunst viele äußerst wichtige Fragen, die fast täglich vor Gericht behandelt werden, entschieden werden können, besonders solche, welche Leibrenten oder die Toterklärung von Verschollenen betreffen.

(2) Lazarsfeld (p. 219) studies the battle between political arithmetic and the German university statistics (= Staatswissen-schaft):

The battle was won, in Germany as well as elsewhere, by the political arithmeticians. From the beginning of the 19th century onwards, they also monopolized the title of statisticians. Whatever was left of the former activities of university statisticians was thereafter considered a part of political science.

But why did university statistics originate and develop in Germany? Posing this question, Lazarsfeld (p. 221) compares Petty with Conring:

The Englishman, citizen of an empire, looked for causal relations between quantitative variables. The German, subject of one of 300 small principalities, tried to derive systematically the best set of categories by which a state could be characterized.

International law, Lazarsfeld (p. 223) continues, *started* [in Germany] *a few miles from everyone's house or place of business. No wonder then that it was a spirit of systematically cataloguing what existed, rather than the making of new discoveries, that made for academic prestige.*

The author then introduces Leibniz as a junior colleague of Conring and erroneously maintains (p. 226, note 29) that

Political arithmetic is about the only topic of contemporary knowledge on which Leibniz himself did not write.

For my part, searching for political arithmetic in Leibniz' writings, I have omitted noticing, or perhaps even had been afraid of remarking, on the *Staatswissenschaftliche* aspect of his studies [60 – 62]. Now I maintain that Leibniz had been both a political arithmetician and a university statistician at the same time but that even he failed to root political arithmetic in Germany.

Acknowledgement. Professor J. Cohen and Dr. W. Romberg have helped me to obtain necessary literature.

Notes

2. Lotteries are games of chance. Calculations of the probabilities of sequences in extracted numbers; of extracting all numbers of a lottery in a given number of drawing at least once etc. are sufficiently interesting , but such problems were not investigated until the mid-18th century.

But I ought to mention the famous Genoese lottery which imitated the yearly city elections, the selection of five candidates from a hundred by lots. Not later than from the beginning of the 17th century bets were made on their results and local bankers offered a twenty thousand-fold payoff to anyone guessing all the five successful candidates [11, p. 336]. Stochastic calculations concerning that lottery are due to Nicolas Bernoulli [4; 7], see also § 2.2.

2. Especially interesting in [69] was the game called *le her*. Both Montmort himself (p. 278) and Nicolas Bernoulli (p. 334) were unable to study it properly since the minimax method [32, p. 158] was required.

3. Kendall connects these reasons (superstition) with the psychology of gamblers which can, however, enter in a different way [14; 15]. Psychological subjective probabilities differ from objective statistical probabilities. On superstitions see [69, p. vi – viii] and a special chapter in Laplace's *Essay philosophique*.

4. I shall use notation (*n*; *a*:*b*) to describe the initial conditions of the problem.

5. An article devoted to G. F. Peverone (1509 – 1559) is in *Atti Torin Accad.*, t. 17, 1882, pp. 320 – 324, as stated by Lancaster (1968, p. 22). Hald (1990, p. 36) also mentions him.

6. In Chapter 16 expectation was introduced. The authors declared that it should govern everyday behaviour (moral certainty is no longer mentioned) and agreed with the posthumously published Pascal's wager [32, p. 154; 143, p. 63 – 72], with benefit accrued by believing in God.

7. Bernoulli suggested that such absentees should be only declared dead when the probability of that event becomes twice higher than the probability of their still being alive. Condorcet [105] considered the ratio of risks of loss of the absentee's property either by him himself or by his heirs.

Absentees are mentioned in the Roman law [67, p. 145]:

Une loi de Julien décida que la captivité ne serait plus une cause de dissolution du mariage et que l'incertitude même de l'existence de l'époux prisonnier ne pourrait permettre à son conjoint de se remarier qu'après cinq ans a partir du jour de la captivité.

It seems that legislation of this kind was always based on common sense, even in the 19th century (Ibidem):

Le c[ode] *civ. a divisé l'absence en trois periods. 1. La presumption d'absence. ... le doute sur l'existence de l'absent est très léger. 2. La declaration d'absence ... la présomption de mort l'emporte sur la présomption d'existence ... 3. L'envoi en possession definitive. Avec le temps la présomption de mort se fortifie et se change presque en cértitude.*

8. The works of De Witt and Huygens (§§ 2.3.3 and 4.2.3) remained unpublished.

9. Condorcet [17, p. 498] even overestimated Nicolas' work:

Depuis l'ouvrage de N. Bernoulli, le calcul des probabilités est devenu l'objet des recherches des philosophes comme des travaux des mathématiciens.

But how about Jakob Bernoulli?

10. *Enc. Brit.*, vol. 4, 1965, p. 8. The author continues: *A similar contract creating a security interest in cargo is called respondentia.*

11. *Statutes of the realm*, vol. 4, pt. 2, pp. 978 – 979.

12. *Enc Brit.*, vol. 13, 1965. *Life insurance* (p. 1091).

13. A name derived from that of the inventor, the Italian Laurens Tonti [46].

14. If primitive forms of early life insurance are disregarded (possibly an unnecessary restriction), life annuities will chronologically constitute the first form of life insurance, and this is how I shall call them. Insurance against death will then be the second form of insurance.

15. A similar development took place in Japan. Considering insurance of both life and property, Noguchi [72, pp. 238, 242] maintains:

Wie in Europa im Mittelalter die Gilden Träger der Versicherung waren, so hat sich in Japan schon über tausend Jahre früher derselbe Gedanke und dieselbe Organisation gegenseitiger Hilfe durchgesetzt. ... Nun sind die meisten Forscher in Europa der Meinung, das Versicherungswesen habe sich, was die Entstehung des Gedankens betrifft, aus dem Gildenwesen entwickelt, während die Entstehung der Form auf die Seeversicherung zurückgehe. Genau das gleiche ist auch in Japan der Fall.

16. See also the remark of Huygens (§ 4.2.3).

17. *Wide research concedes that Life Insurance came into its own not by a front-door entrance but by the marine insurance porthole.*

[72, p. 78]. The text of O'Donnell's abundant and fascinating non-mathematical source which I did not explore, carries no references to the appended list of literature.

18. More precisely [114, pp. 484 – 485],

Eine papstliche Bulle von 1423 erklärt schließlich [after about a century of prohibition] *den Rentenkauf für erlaubt.*

19. *Der älteste reine Leibrentenvertrag wurde im Jahre 1308 … abgeschlossen* [114, p. 484]. *Certificats de rente viagère* dating back to 1228 and 1229 are published. One example of an annuity on life regards Benvenuto Cellini [12, p. 423; 1]:

I sacrificed my pay for his portrait … and he arranged that he should keep my money at 15 per cent during my natural life.

20. Hudde will be mentioned time and time again. There exists a general description [42] of his mathematical works.

21. *Grand Larousse Enc.*, t. 7, 1962, p. 542:

J. Lafarge pout établir … une casse d'épargne, la célèbre tontine Lafarge, dont le succès fut éphémère, mais qui servit de modèle aux premières cases d'épargne.

22. Seal used data of the early tontines to study the laws of mortality and maintained that the Dutch annuity patterns of the 16th century were really tontines. Hudde's letter to which Seal and myself refer is written in vernacular and I am unable to check this statement. Drawing on Deparcieux, Seal also used data on mortality of French monks during 1607 – 1669.

23. *Enc. Brit.*, vol. 13, 1881, p. 180.

24. In the opinion of Chaufton [13, p. 351] the first successful life insurance society was established in England in 1720.

25. *A number of minor companies became defunct during the period of speculative financial schemes which resulted in the crisis of 1720. … Speculation was common throughout the century … between 1800 and 1870 some 500 new offices were established. … Some were of definitely fraudulent intent. Enc. Brit*, vol. 13, 1965, p. 1094.

26. Without discussing the history of insurance Bienaymé remarked that compound interest adversely influences the activities of insurance societies. Even a small loss incurred during the initial period of their work will not be compensated by a later gain of the same order. He concluded that success in insurance is only possible when transactions are sufficiently numerous.

27. Hendriks [45, pp. 253 – 255] collected utterances of various scholars about De Witt. I add from the correspondence of Huygens [47, t. 2, pp. 411 – 412] dated 1659:

Il est bien sçavant en la Géométrie et en l'algebre et s'y exerce tousjours non obstant les grandes affaires qu'il a sur le bras.

A modern biography of De Witt is [87].

28. Or at least to *Noble and mighty Lords* of the state [45, p. 232], from an English translation of [100]. On p. 257 Hendriks says that he discovered [100] in Resolutions of the *States of Holland and West Friesland of 1671* (he mentions only this English title). This source likely contains a reprinted version of [100]. In any case Hendriks mentions a misprint in calculations which does not occur in [100] which I had seen.

29. In this instance it is the Poisson form of the law of large numbers.

30. Eneström obviously did not know that Hendriks [45, p. 246] had pointed out the actual assumption of De Witt. However, Hendriks did not say that De Witt changed his initial hypothesis.

31. This problem is possibly due to Hudde, at least De Witt acknowledges a letter from him on the same problem but does not comment. See also below.

32. Relevant achievements of Huygens (§ 4.2.3) also remained unpublished. In another letter, also in 1671 [45, p. 102], De Witt informed Hudde that the

Number which you have recently proposed respecting the ratio between the chances of two players at quinque novem agrees literally with the one I had already calculated.

Montmort [69, p. 173] described a version of that game not devoid of interest.

33. This is a well documented source. See also John [53, pp. 17 – 34], Meitzen [68, §§ 2 – 4] and Elsner [24].

34. In a letter of 1685 Petty [82, pp. 157 – 159] censured Pascal for using

Many words, phrases and sentences … which have no certain, sensible signification and therefore cannot beget any clear notion, sense or science in the Reader.

The Editor seems to prove that Petty bears in mind Pascal's *Différence entre l'esprit de Géométrie et l'esprit de finesse*. In another letter of 1667 Petty [81, vol. 2, p. 22]

Beg[s] *leave of the world to decline the words Infinite, Eternall, Incomprehensible when* [speaking] *of Almighty God*. They *are not soe fitt for Ratiocination, but rather for Adoration, they do not cleare or brighten our understanding.*

Still, people use words and I [93, p. 109 note 55] noticed recent attempts to quantify qualitative description of men's social behaviour.

35. However, Leibniz, though not a co-founder of political arithmetic, regarded insurance of life and property as a highly important social institution [110; 116].

36. Cf. Petty [78, p. 15]:

1. Place is the Image or Fancy of Matter or Matter considered. 2. Quantity, the Fancy of Place. … 5. Situation, several Places considered together. 6. Figure is Quantity and Situation considered together. … 9. Time, the Image of Motion.

On pp. 82 – 88 Petty alleges that

(1) The likelihoods of reaching 70 years of age for those aged 16 and a [$a < 16$] *is to the likelihood of the converse event are as* $\sqrt{16} : \sqrt{a}$.

(2) The likelihood of A aged a dying before B aged b [$a, b > 16$] *is to the likelihood of the convers event as* $\sqrt{a}:\sqrt{b}$.

He does not refer to Graunt whose table of mortality (§ 2.4.3) contains nothing to corroborate these conclusions. But then, Petty illustrates his *laws* by examples mostly pertaining to men aged 16, 26 and 36 years, i. e., to ages which enter Graunt's table.

37. Pearson (1978) described the work of English statisticians of the 17th and 18th centuries and noted that the growth of the population in Europe had troubled them. Then, noting the horrible epidemics (of cholera and smallpox) Pearson (p. 337) concluded, contrary to the well-known Biblical command: *It must be the Creator's will that* [the population] *should remain stationary*.

For utterances on the chance origin of the world see [93, pp. 134 and 140].

38. Is it possible that early eugenists beginning with Galton saw any connection between them and Süssmilch if not Petty? D. Mackensie in a private communication informs me that the answer seems to be negative.

39. If so, Petty will be co-author. But here is **my own finding**; I quote from Petty's Address to Lord Brounker [78]:

I have also (*like the author of those* <u>*Observations*</u> [on the bills of mortality]) *Dedicated this Discourse to … the Duke of Newcastle.*

40. As Hull [76, vol. 1, p. lii] noticed, Petty sought to consider even the number of sea-fish and wild-fowl at the end of every thousand years since the Flood. No wonder his statistical estimates were often wrong; even so, it was he who first advocated the use of the new toy and (see above), suggested the subject of Graunt's inquiry. For a vivid characteristic of Petty see also Greenwood [37, p. 80; 39, p. 73].

41. On this occasion Graunt's statistical music was written in a rather disorderly fashion; this supposition is found elsewhere [36, p. 32]. As to Petty's authorship, see also § 2.4.2 for a description of a related (and unfounded) study due to him.

42. Unification of national statistical data, a problem tackled by statisticians in the second half of the 19th century, proved extremely difficult.

43. A pertinent remark is due to Couturat [18, p. 522]: In 1704 Leibniz *pensa … à fonder une Société des Sciences … à Dresde*. One of the aims of this society in his opinion would have been to *dresser des statistiques démographiques*. See also Biedermann [5, p. 457].

44. The questions just quoted (and some others) seem to be intelligible even for those who do not read Latin, myself included. But a thorough study of Leibniz' questionnaire is still warranted. *Feci quad potui, faciant meliora potentes*!

45. Leibniz did not say anything more about calculating populations.

46. *Der Tag, an welchem E. Halley seine Abhandlung ... vortrug, darf als der Geburtstag der statistischen Wissenschaft bezeichnet werden.*

This opinion [18, p. 1] is an overestimation: it seems impossible to speak about the *Geburtstag* of statistical science without mentioning Petty and Graunt.

47. Thus, discussing mortality from various diseases, Graunt first and foremost strove to correct systematic influences: he reasonably supposed that the death-rate from syphilis was grossly underestimated because those who died of it were usually *returned of ulcers etc*. His book [36, p. 39] also contains a curious passage on freaks of chance:

The rickets were never more numerous than now, and ... they are still increasing; for anno 1649, there were but 190 (cases of death of rickets), *next year 260; next after that 329 and so forwards, with some little starting backwards in some years.*

48. A small appendage to the main memoir begins thus (Ibidem, p. 19):

What I gave you in my former Discourse on these bills, was chiefly designed for the computation of the Values of Annuities on Lives.

This addition is mainly devoted to political arithmetic and contains pronouncements such as (p. 21)

The Strength and Glory of a King being in the multitude of his Subjects etc. … *Celibacy ought to be legally discouraged. ... And those who have numerous Families of Children to be countenanced and encouraged by such Laws as the Justrium Liberorum among the Romans. But especially, by an effectual Care to provide for the Subsistence of the Poor, by finding them Employments.*

In 1693 Halley [44a, p. 232] with a reference to *Biog. Brit*. 1757, a source I did not see,

Produced a paper wherein he shewed a Method of computing the Value of Annuitys for one two. or three lives ... which was ordered to be printed in the Transactions.

However, no additional paper is mentioned in the *List of Halley's published writings* (Ibidem).

49. Almost a century after the publication of Halley's memoir T. Paine, in his *Rights of Men*, presented arguments for national welfare activities. Requiring an estimate of those above fifty, Paine [56], p. 106]

Several times counted the persons (he) *met in the streets of London ... and* (had) generally found that the average is about one in sixteen or seventeen (who are older than fifty).

Commenting on this estimate and referring to a number of sources Kruskal & Pieters [56] suggest that the proportion of those above fifty should have been 17% or, possibly, any percent between 13 and 20. It is extremely interesting that Halley's table, although compiled for a different population (and time), would have furnished a figure of 18%! Thus Paine could have arrived at a rather trustworthy result just by using an old table, a classical table, I would add.

50. In another place [21, p. x] De Moivre returns to Halley his

Very hearty Thanks for Instructive Notions readily imparted ... during an uninterrupted Friendship of five und Twenty years.

A few lines describing the friendship between the two scholars are due to Helen M. Walker [98, p. 356].

51. The first introduction of the same distribution by Nicolas Bernoulli (§ 2.2) remained unnoticed. At least, no one referred to him in this connection.

52. A related example is provided without substantiation by Elsner [24, p. 136]:

Thomas Cromwell, entfernt verwandt mit ... Oliver Cromwell, der Lordkanzlcr von Heinrich Vlll (1509 – 1547), *befiehlt in England die systematische Aufzeichnung von Geburten und Todesfällen in Kirchenbüchern; in der Mark Brandenburg schreibt dies kurz danach die Visitations- und Consistorialordnung von 1573 bindend vor.*

53. Graetzer (pp. 33 – 37) also appends a letter from Neumann to Justell, Regis Magnae Brittanicae Bibliothecario, written in 1692. There Neumann informs his correspondent about plans to conduct magnetic observations.

54. Did Neumann know anything about Graunt or Petty? lt is not clear whether the *Reflexionibus* was ever published. Elsner [24, p. 138] names the titles of two of Neumann's works sent by the latter to Halley via Leibniz: *Schöne Anmerkungen* *göttlicher Providcnz über unser Leben* and *Reflexionen über Leben und Tod bei denen in Breslau Geborenen und Gestorbenen*.

For that matter, had Neumann or Halley known about the correspondence of Pascal and Fermat?

55. Quite consistently, Ore adds that the latter formulas above were generally known; otherwise, he argues, Pascal would have mentioned them. This argument is not really convincing; in letters to Fermat those formulas did not deserve mention.

56. In Galileo's time gamblers detected a difference of probabilities equal to 0.0385. Since David [19, p. 66] assumes that this difference is equal to 1/108 I shall adduce the whole argument. The comparison is between the probabilities of scoring 10 or 11 points with three dice on the one hand and 9 or 12 points on the other. In itself, the first outcome (call it A) has probability p(A)= 27/216 and the second outcome (B) has p(B) = 25/216 and Δp = 1/108. Nevertheless, disregarding all other possible outcomes, gamblers were able to compare p(A/A or B) with p(B/A or B) so that the difference sought will be 0.0385.

57. Montmort [69, p. 73] repeated these lines in a somewhat different wording.

58. In part 1 of his *Horologium oscillatorum* [49] Huygens considers various errors of pendulum clocks. Research of this kind belongs to the prehistory of the design of experiments [89].

59. In 1656, in his correspondence [47], t. 1, pp. 426 – 427], without ever explaining the method of solution, Huygens solved a few simple problems of the same kind.

J'attends avec impatience ce qu'en dira Monsieur Fermat, wrote Huygens, *pendant quoy vous me permettrez de tenir cachée la solution*.

Huygens also solved a similar problem and, for that matter, by a similar method, in a manuscript of 1676 [47, t. 14, pp. 156 – 163].

60. I am not reprinting Huygens' solutions of problems which are available in §§ 4.1 and 4.2 of my original text. And Pascal (§ 3.5) did not mention ruin.

61. Appendix 1 dated 1656 and possibly written in connection with Huygens' correspondence with Carcavi was devoted to the problem of points for the case of three gamblers. The other appendices belong to 1665 – 1688.

Korteweg, the Editor of t. 14 of the *Oeuvres complètes*, had in most cases supplied these dates.

62. The conclusion [99] that Huygens recommended to use the probable duration of life rather than its mean duration is thus mistaken. See § 2.3.2 for a connection between life insurance and betting. Note that both brothers (pp. 524 – 526 and 484 – 485) had previously mentioned betting on lives of men.

63. Huygens could expect nothing new in the work of De Witt [100]. Still, it seems curious that he did not mention this work in his correspondence all the more since he certainly had not known what exactly did it contain.

64. Its introduction into probability is due to Jakob Bernoulli who proved that the relative frequency of the occurrence of a random event in the case he discussed is in a sense morally certain to coincide with the corresponding probability. Chapter 3 of pt. 4 of the *Ars Conjectandi* was devoted to arguments applied for assigning probabilities to random events, and his conclusions, rather than Huygens' reasoning (below), ought to be borne in mind in the first place.

65. Huygens wrote out the eleven zeros.

66. Editor's note on p. 534 of the appropriate volume of the *Oeuvr. Compl.*:

L'improbabilité de la thèse que parmi tous les corps célestes un seul, la terre, serait habité, lui paraît extrêmement grande.

67. Van Brakel criticized my article [93] noting that my account often remains in a raw condition since I did not distinguish between the various concepts of probability. The distinction he wants to see is not discussed in the national mathematical literature and I had always restricted myself so as to avoid a *terra incognita*.

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