Studies in the History of Statistics and Probability

Collected Translations

P. R. Montmort, Nic. Bernoulli,
P. S. Laplace, S.-D. Poisson and others

vol. 4

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Introduction by the Compiler

This collection includes translations of some classical work. Before inserting general comments on separate items, I will say a few words about Laplace and Poisson in connection with the method of least squares (MLSq) and theory of errors. Laplace offered his own version of that theory and barely referred to Gauss. Just as Legendre (and Poisson after him) he applied the inaccurate term *MLSq of errors*; actually, of the residual free terms of the observational (of the *conditional*, as Laplace called them) equations.

For Poisson, Gauss as though never treated observations, which greatly diminished the value of his pertinent works. He was apparently unable to see beyond Legendre's hurt pride although Delambre [vi, Note 11] formulated a sober opinion about this issue.

I (1999) have discussed Gauss' early application of the MLSq which some authors are denying, and now I am adding two more relevant points. First, Gauss' letter to Olbers of 24 Jan 1812 mentions such applications made in 1799 and 1802 written down but since then lost (Plackett 1972/1977, p. 284). Second, concerning notification of colleagues, I note that Olbers testified that Gauss had indeed told him about the MLSq in 1802. True, he only went public in 1816, four years after Gauss had asked him about it, but in 1812 – 1815 he had not published anything suitable for inserting such a statement, see the *Catalogue of Scientific Papers* of the Royal Society.

I am using later notation C_m^n and n!.

Plackett R. L. (1972), The discovery of the method of least squares. *Biometrika*, vol. 59, pp. 239 – 251; M. G. Kendall & R. L. Plackett, Editors (1977), *Studies in the History of Statistics and Probability*, vol. 2. London, pp. 279 – 291.

Sheynin O. (1999), The discovery of the principle of least squares. *Hist. Scientiarum*, vol. 8, pp. 249 – 264.

[**i**] The author provides much information about Jakob Bernoulli's early years and shows his brother Johann's mean attitude towards him.

[ii] Nothing is known for certain about the author. A *student V. Mrocek* "edited" Markov's mimeographed lectures of 1903 in differential calculus, see Markov (1951, p. 708), – perhaps took them down. The second part of the paper below should have appeared in the same source complete with Bibliography, as Mrocek stated, but had hardly ever appeared. The Editor was Bukharin, a leading political figure, arrested in January 1937 and executed in March 1938, and anyone somehow associated with him could have then be shot (or sent to a labour camp) just in case, cf. Sheynin (1998).

Perhaps the author was not a mathematician; his § 7 is hardly satisfactory. Another serious shortcoming is that Mrocek made many non-mathematical mistakes; apparently, he wrote in great haste. Finally, some of his pronouncements are simply unjustified statements in the spirit of the repulsive and vulgar Soviet variety of Marxism, in itself a highly biased teaching. Finally, much more is known nowadays than in 1934; thus, I (1977) have discussed the history of insurance of property and life.

Nevertheless, I translated Mrocek's paper because it is one of the first writings attempting to connect probability with social and economic factors and because it presents a good negative example of Soviet sociological studies. Concerning the former, the reader will certainly recall Pearson (1978); and there was yet another deserving contribution (Hessen ca. 1931). In his last section Mrocek severely and ignorantly criticized the textbook Khotimsky et al (1932). The latter published an article (1936) on the history of probability (still suffering from sociological vulgarity) and perished in 1937 or 1938 (Kolman 1982, p. 132). He was a mathematical statistician of Chuprov's calibre and his death was a tragic loss.

[iv] In the theory of probability Leibniz (Sheynin 1977, pp. 222 – 227 and 255) is best known as Jakob Bernoulli's correspondent. He also left five manuscripts devoted to Staatswissenschaft (University Statistics) and political arithmetic first published in 1866. One of them is this, [iv]. My accompanying Notes are critical, but Leibniz apparently had not prepared it for publication and, moreover, it is surprising that he had found time for political arithmetic. Note also that Leibniz obviously considered his manuscript as (his only?) popular scientific writing.

In his other manuscripts Leibniz recommended to compile *Staatstafeln* and compare those describing different periods or states; advised to compile medical reference books and establish a *Collegium Sanitatis* which should have also carried out meteorological and magnetic observations and formulated recommendations for agriculture.

[v] Süssmilch, see Pfanzagl & Sheynin (1997), is mostly remembered for the vast materials he had compiled and for originating moral statistics. He (1758) also indicated the need to study the dependence of mortality on climate and geographical features and indicated that poverty and ignorance fostered the spread of epidemics. His cooperation with Euler proved fruitful for both of them.

[vi] Idelson was one of the first to discuss the theory of errors from the viewpoint of mathematical statistics. I note that he had not commented on Laplace's belief in the almost universal validity of the central limit theorem.

[vii] Montmort deserves to be better known as an influential scholar although somewhat less important than Nic. Bernoulli or De Moivre.

[viii] The author (1778 – 1870) was an adventurer, a military man and a high functionary, until 1851 charged with the *Statistique général de France* (cf. the beginning of his § 6). The first chapters are interesting in that Jonnès is very specific when discussing the aims of various statistical tasks. However, in my Notes I indicate the deficiencies in his exposition. In general, he greatly overestimates ancient statistical work and at least in several cases his deliberations are superficial.

[ix] Bernoulli appended the list of the number of births (or rather baptisms). It coincides with the list published by Arbuthnot (1712) although the number of girls in 1687 became 7114 instead of 7214. The yearly number of baptisms in London reached (and exceeded) 14,000 only once, in 1683, but at the very beginning of the period under consideration it was less than 10 thousand and even still less (six – eight thousand in 1644 – 1660).

In 1709, Bernoulli (1687 – 1759) published a dissertation on the application of the art of conjecturing to jurisprudence, still only existing in its original Latin. It certainly fostered the dissemination of stochastic ideas and was mathematically interesting (Todhunter 1865, pp. 195 – 196).

In a tiny note I (1970, p. 232) have shown that Bernoulli had actually come to the normal distribution. Denote

$$p = m/(m+f),\ q = f/(m+f),\ p+q=1,\ s = 0(\sqrt{n}).$$

Then his formula can be written as

$$P\left(\frac{|\mu - np|}{\sqrt{npq}} \le s\right) \approx 1 - \exp(-\frac{s^2}{2}).$$

Since then Hald (1984; 1990, pp. 264 – 267; 280 – 285; 1998, pp. 16 – 17) had studied Bernoulli's result but did not connect it either an integral or a local limit theorem. Indeed, *s* is restricted and the factor $\sqrt{2/\pi}$ needed in the local theorem is lacking. Nevertheless, Youshkevich (1986) reported that at his request three (!) unnamed mathematicians, issuing from Hald's description, had concluded that Bernoulli came close to the local theorem.

[x] This one of the first memoirs which Laplace devoted to probability. It shows that he had barely abandoned his general views.

[xiii] See below my comments on [xix]. Here, I only add that, unlike his predecessors, Laplace had freely applied various approximations which became a tradition. The theory of probability owes its return to rigour to Chebyshev, Markov and Liapunov.

[xv – xviii] These notes show Laplace from an unusual angle. Regrettably, we do not know whether his speeches were followed by any discussion.

[xix] My own comments include harsh criticism. Here is a curious statement (Laplace 1796/1884, p. 504; Sheynin 2011, p. 43):

Had the Solar system been formed perfectly orderly, the orbits of the bodies composing it would have been circles whose planes coincided with the plane of the Solar equator. We can perceive however that the countless variations that should have existed in the temperatures and densities of the diverse parts of these grand masses gave rise to the eccentricities of their orbits and the deviations of their movement from the plane of that equator.

The causes mentioned by Laplace were hardly external, and the main relevant explanation of randomness, deviation from the laws of nature, persisted. Leaving aside the planes of the planetary orbits, I question his opinion concerning eccentricities. Newton theoretically proved that the Keplerian laws of planetary motion resulted from his law of universal gravitation and that the eccentricity of the orbit of a given planet is determined by the planet's initial velocity.

So it really seems that Laplace was mistaken. He certainly studied Newton, although a bit later, in t. 1 of his *Traité de Méc. Cél.* (1798/1878, Livre 2, chapters 3 and 4) but did not correct anything in the later editions of the *Exposition*. Witness finally Fourier's comment on the *Exposition* (1829, p. 379): it *is an ingenious epitome of the principal discoveries*. And on the same page, discussing Laplace's *historical works* (to whose province the *Exposition* belonged):

If he writes the history of great astronomical discoveries, he becomes a model of elegance and precision. No leading fact ever escapes him. […] *Whatever he omits does not deserve to be cited.*

Laplace's version of the theory of errors essentially depended on the existence of a large number of normally distributed observational errors and was therefore unsuccessful. He should have acknowledged the Gaussian demand for studying the treatment of a small number of observations and to restrict therefore the importance of his own results. Instead, he insisted on his own approach and virtually neglected Gauss. Later French scientists including Poisson followed suit, especially since they had been much too much offended by the Legendre – Gauss propriety strife, and even the most eminent mathematicians (or at least those of them who had not studied attentively the treatment of observations) became confused. When proceeding to prove the central limit theorem, Chebyshev remarked that it leads to theMLSq!

Laplace collected his earlier memoirs on probability in one contribution which cannot, however, be regarded as a single whole. He never thought about solving similar problems in a similar way (and his *Essai* (1814) was not a masterpiece of scientific-popular literature. Then, many authors complained that Laplace had described his reasoning too concisely. Here, for example, is what Bowditch (Todhunter 1865, p. 478), the translator of Laplace's *Traité de mécanique céleste* into English, sorrowfully remarked:

Whenever I meet in La Place with the words 'Thus it plainly appears' I am sure that hours, and perhaps days of hard study will alone enable me to discover how it plainly appears.

This can also be said about the *Théorie analytique*. Then, Laplace was *extremely careless in his reasoning and in carrying out formal transformations* (Gnedenko & Sheynin 1978/1992, p. 224 with examples attached). And here is Laplace's careless opinion (1814/1995, p. 81) about mortality tables: *There is a very simple way of constructing* [them] *from the registers of births and deaths*. But the main point is to study the plausibility of these registers, to single out possible corruptions and exceptional circumstances etc. Then, the boundaries of the constructed mortality table have to be determined both in time and territory.

Laplace had not even heuristically introduced the notion of *random variable* and was therefore unable to study densities or characteristic functions as mathematical objects. His theory of probability remained an applied mathematical discipline unyielding to development which necessitated its construction anew. It is opportune to note that Boltzmann did not mention him at all. And now I quote Fourier (1829, pp. 375 – 376):

We cannot affirm that it was his destiny to create a science entirely new, like Galileo and Archimedes; to give to mathematical doctrines principles original and of immense extent, like Descartes, Newton and Leibniz; or, like Newton, to be the first to transport himself into the heavens, and to extend to all the universe the terrestrial dynamics of Galileo: but Laplace was born to perfect everything, to exhaust everything, and to drive back every limit, in order to solve what might have appeared incapable of solution. He would have completed the science of the heavens, if that science could have been completed.

I believe that the first version of the theory of probability was completed by Bayes (Sheynin 2010) rather than Laplace.

Laplace introduced partial differential equations and, effectively, stochastic processes into probability, and non-rigorously proved several versions of the central limit theorem by applying characteristic functions and the inversion formula. In the not yet existing mathematical statistics Laplace investigated the statistical significance of the results of observation, introduced the method of statistical simulation, studied his version of sampling and extended the applicability of the Bayesian approach to statistical problems. He knew the Dirichlet formula (even in a generalized version), introduced the Dirac delta-function and integrals of complex-valued functions.

[xx] Poisson kept to the usual contemporaneous distinction between *possible* and *probable*, and I also note that Laplace certainly had not rigorously proved the central limit theorem which Poisson actually mentioned at the end of his review. Poisson (1837, §§ 110 and 111) had returned to Laplace's problem about the inclinations of celestial bodies.

[xxii] This review shows that Poisson had not then been knowledgeable about probability. I myself (1976) had described the contents of the separate chapters of Laplace's *Théorie* and I see that Poisson had indeed missed some important points, see also my Notes to [xxi]. Poisson was also careless; he did not even mention the last chapter of Laplace's contribution.

I

R. Wolf

Jacob Bernoulli from Basel, 1654 – 1705

Biographien zur Kulturgeschichte der Schweiz,
1. Cyclus. Zürich, 1858, pp. 133 – 166

[1] On 27 December 1654, old style, Margaretha Schönauer, wife of councillor Nicolaus Bernoulli, gave birth in Basel to a son baptized Jacob. He became the first of the seven Bernoullis[1] who, without there being any other such example in history, for more than a century cultivated mathematical sciences so perfectly that a Newton, and a Leibniz, and later a D'Alembert and an Euler must regard them as their equals; that the scientific societies had been really owing them interest; that even now each mathematician discovers their footprints almost at each step and only mention their names with deep respect; and, yes, that Switzerland also became worthy abroad in matters of intellect just like it happened previously owing to body strength, courage and loyalty.

Jacob Bernoulli was meant to be a theologian. He attended school, then the university of his home city, learned the languages of antiquity and in 1671 became Master of Philosophy. As stipulated, he then continued to study further. At the same time, however, mathematical disciplines, which he had accidentally noticed when considering some geometrical figures, irresistibly attracted him. He was only able to study them in his spare time, without any guiding, and almost without aids since his father wished him to follow strictly the previously chosen course of studies.

Nevertheless, being 17 years old, he already solved the rather difficult chronological problem posed by Schwenter: to determine the year of the Julian calendar's period given the solar cycle of 28 years, the Metonic period of 19 years and [financial] indiction cycle of 15 years. Then he began mostly pursuing astronomy in general and, in accordance with the custom of the time, chose an emblem, showing himself driving the solar chariot with an inscription *Invito patre sidera verso*[2].

[2] In 1676 Bernoulli passed his examinations in theology and on 20 August went travelling across Switzerland and France. At first, on 27 August, he arrived in Geneva and stayed there for seven quarters. He described his life there in travelling notes still in possession of the respected Professor Rudolf Merian in Basel, and there he (Peter Merian 1846) wrote in particular:

On 6 October I came to Mr. Waldkirch to instruct his children in exchange for board and continued to perform that duty until departure, three hours daily. I taught his blind daughter complete courses in logic and physics and partly Matthiae's history [later translation, 1841] *and Woleb's compendium* [1626]*, taught her to write and to sing various spiritual songs*[3]*. For some time I have also* *instructed* […, noblemen from Schaffhausen] *in geography, physics, and German and a German nobleman* […] *in Latin.*

In addition, during my stay in Geneva I had 18 times lectured on various events, three times dispensed the chalice at Holy Communions and twice publicly opposed Turretin.

Just like the Frenchmen who are everywhere pigs [?], *they keep the city in a very dirty condition. When someone walks through the allées, nose turned to the sky, he must beware of being baptized at night from above. They have to thank the north-easter that prevents the air to be infected.*

Water of good quality is greatly lacking; they only have three regular wells, one of them in an obscure and sombre place (bourg de four), another near the city hall and the third one by the gymnasium, but the water there is bad, so they fetch it from the Rhône. That water is repulsive because of the public toilets found here and there along the river. Men and women go there when necessary and call it going on the Rhône. It can be easily imagined that sometimes a lump will be concealed in the drink. For my part, I drank wine that did not taste bad.

Ordinary houses are built mostly for comfort rather than delicacy. A stone spiral staircase leads from below towards the top. It sometimes serves 12 – 15 apartments, three or four to a storey. Otherwise, it is swinish. They do not know sideboards, pictures, spacious halls (Luftsälen), candlesticks, gratings under the staircase for wiping off the footwear. While sitting at the table, they could really throw gnawed off bones over the shoulder. Usually, just like in the entire France, there are no stoves here and people warm themselves by the kitchen fire; from the front, legs get roasted, but the back freezes. Walls are not panelled, they either show the bare stone or are papered. There are no quilts, only bare mattresses.

Near the St. Peter cathedral there is an auditorium in which lectures on law and philosophy are read. Across is the theological auditorium for services in German, Italian, and, during the winter, in French. Both are badly equipped and I would have wished them to have our Basel geese coop [instead], *it would then be better.*

The cemetery is beyond the city, behind the Plainpalais. It is enclosed in a square by four walls and old and young are thrown there into graves like dogs, without song and music, without lux, crux et Deus[4]*. The Genevans do not celebrate any holidays, do not know the Holy Week or Christmas, the New Year etc.*

The only exception is the Escalade on 12 December when they recall their corporeal liberation from the Savoyards' yoke in 1602[5]*. They should have thanked the Lord much more for the spiritual liberation from Satan's power by a marvellous humanization of our Saviour, by his bitter suffering and death. Their Escalade celebration is more a holiday of gluttony and hard drinking, of defying the Savoyards by getting blind drunk rather than of devotion to God. Even the poorest citizen is not poor enough for abstaining and one of the citizens was able to give a capon in exchange.*

On 8 May 1678 Bernoulli departed from Geneva to take over an offered position with the

Marquis de Lostanges residing in his estate in Nede, Limousin, for instructing his only son for some time and afterwards travelling with him. In exchange, I was promised free board [and lodging] *and 15 pistoles yearly*.

He was disappointed.

And I was to find out how the Frenchmen were keeping their promise. Apart from an only son, as I was informed, there were three children, two sons and a mignone whom I had to instruct, and not only in Latin and German, but had to teach them to read and write. Instead of going travelling with them after a short while, I saw that they were just children and will not be separated from their mother for six years. Again, each Sunday I had to read them a sermon and pray with them daily, morning and evening.

That position did not please him, and he only stayed there for a little more than a year, gave during that time sermons in French and constructed two gnomons in the court of the mansion.

After being in Nede for 13 months and getting 12 louis-d'ors of the Marquise, I wished to leave that back of beyond as soon as possible and to seek fortune in Bordeaux.

He arrived there on 10 July 1679 and stayed quite agreeable for six months at the home of a Protestant lawyer, teaching his son in exchange for board and lodging. On the contrary, the manners of Frenchmen (of eigentlichen Franzosen) did not please him. Thus, for example,

The young and the old all over France have four meals daily. In the morning, they do not go out of their place without breakfast and a glass of wine, just like our drunkards do. They have little household or kitchen appliances, no knives or spoons, and both nobleman and peasant gobble up soup with their fingers.

[3] On 16 February 1680 Jacob went from Bordeaux to Paris, stayed there for seven weeks, then returned to Basel via Strasbourg safely arriving there 20 May of the same year. Soon after that appeared the noteworthy comet of 1680. It was viewed with trepidation by the superstitious and, on the contrary, with highest interest by him himself. The olden fear of comets reached its highest level, then had to lower (Wolf, Jahrgang 1857). From 4 December 1680 to 17 February 1681 Bernoulli determined a series of the comet's positions although "owing to the lack of suitable instruments, only by the naked eye and a cord" and attempted to attach them to a theory that he devised at the same time.

So was his first contribution (1681a) compiled. There, he considered comets as satellites of a [of an unknown] planet situated far beyond Saturn. Having adopted that hypothesis, he calculated the period of the comet of 1680 as being equal to 38 years and 147 days.

We will see this very comet again in its perigee on 27 May 1719 (provided that we are still living) and actually at 1°12' of the Scales.

Then he reasonably added:

If my prediction coincides with the outcome, my principles can at once sweeten you; if not, they can be arbitrary.

It seems that Bernoulli was rather free of the cometary superstitions of his time but that he did not wish to oppose them sharply:

I thought of concluding here because of the fear of being reproached for teaching that comets were bodies created at the beginning and destined to appear at definite times as though I wished to contradict the clergymen who understood the comets as signs of wrath of God. And therefore I must reject such strained opinions by explaining that they never follow from my principles; it can really be that the wise Creator, who foresaw everything and according to whose will everything occurs, arranged and ordered the motion of comets so that they only then appear when He wants to announce to us His punishment. Or, on the other hand, that such signs He wishes to announce only then when the comet according to His ordered and arranged course should not be lowered to its perigee.

And here is [here begins] the conclusion, perhaps funny, but now quite distasteful:

A prediction for the old womenfolk, for the devoutly faithful, the laymen and numerous animals, or for the jovial men who are glad to have something to laugh about.

[4] Soon after completing that work, which, in spite of its small extent introduced him to the scientific world, on 27 April 1681, Bernoulli began travelling once more, this time having a definite intention to establish scientific acquaintances which to his regret he had neglected [to think about] during his first travel.

At first he went to Amsterdam via Mainz and stayed there for a long time so as to give two contributions (1682; 1683) to publishers[6]. The first of these, in Latin, was an extension of his earliest work and due to it he became really well known and commented on[7]. On 11 May 1682 it was announced in the *Journal des Sçavans (Savants)* and prompted La Montre, a professor of mathematics at the Collège de France, to publish there a note on 25 May entitled *Démonstration physique de la fausseté du système des comètes proposé dans le dernier Journal*. He wrote, in part, that

At first, Bernoulli's system seems ingenious, but nevertheless it is so contrary to the laws of nature, that it can be doubted whether that author was serious. It is easily seen that his suppositions are unworthy of that mathematician.

Neither did Montucla (1799 – 1802) mention Bernoulli quite worthily, but I cannot agree with that opinion. Although it cannot be denied that Dörfl (Dörffel) (1681) had a more fortunate idea [about the same subject], Bernoulli's contribution has nevertheless advanced the state of cometary science of his time since he considered comets as periodic heavenly bodies and attempted to calculate their return. A few years later he naturally laid down other principles, but in that second edition he remarkably made a larger sacrifice to the existing superstitions: he saved the nucleus of the comet, but not its tail.

Bernoulli's second contribution (1683) partly dealt with the weight of the air and partly with that of the finer matter in whose pressure on bodies he thought to have found the cause of their cohesive relations. In February 1685 it was reviewed in the *Journal des Savants* and in connection with that paper Bernoulli remarked that he had given that [contribution] to Wettstein *who, in turn, promised me* [Bernoulli] *Boyle's Opera, Wallis (1670) and Guericke (1672).*

[5] After staying in Holland for about two months, Bernoulli travelled through Belgium to England where he became acquainted with Flamsteed in Greenwich, attended a conference of the Royal Society, then returned to his home town via Hamburg and Frankfurt and safely arrived there 26 October 1682. Except for a more extensive travel across Switzerland the following year, he never quitted Basel for a long time. Instead of an offered position as preacher in a reformed community of Strasbourg or at Heidelberg University, he settled down in Basel and married Judith Stupan who delighted him by a daughter and son. Contrary to his father's wish, that son preferred art to science[8].

Bernoulli's public reports on mathematics and physics accompanied by experiments had been very successful and quite new for Basel; Fontenelle (1706) remarked that *his method of philosophising, the only reasonable, was nevertheless so late to emerge*. In 1687, after the death of Peter Megerlin, the chair of mathematics at Basel became vacant and was faithfully entrusted to him although no one at the time could have suspected that it will be held by him and his family for a full century and soon become the highlight of the entire university.

And so, Fontenelle continued, his new talent appeared, the gift of instructing,

capable of attaining highest knowledge and leading others to that level, so that more intelligence was sometimes needed for stepping down from it than for continuing higher up[9]. *Lecturing with extreme clarity and soon achieving great* [scientific] *progress, Bernoulli attracted to Basel many foreign listeners*[10].

In 1684, as soon as Bernoulli's external circumstances were properly dealt with, and he became able to devote undisturbed studies to his favourite sciences, Leibniz published in the *Acta Eruditorum* a specimen of his differential calculus. It was incomprehensible for most mathematicians[11] but for our Bernoulli a hint was sufficient. With depth and excellence distinguishing most of his works, he slowly but confidently penetrated Leibniz' secrets, although his letter of 1687 to the latter asking further explanations did not find the addressee at home[12]. In 1690, after returning from a great travel over Germany and Italy, Leibniz finally answered, but he did not need help anymore. He even adopted the new calculus so well, that soon afterwards he became able to publish a sketch of the differential and integral calculus in the *Acta Eruditorum*. There, Bernoulli developed general rules for [dealing with] tangents, rectifications, quadratures etc and applied them to parabolas, logarithmic spirals, loxodromes etc.

His brother Johann, thirteen years younger, whom after returning home he with unprecedented success, as could well be said, introduced into mathematics, did not remain behind him so that Leibniz felt himself obliged to explain that the new calculus belonged to both of them as much as to himself[13].

[6] And now discoveries followed one after another: the problems concerning the isochrone, the brachistochrone and catenary, the isoperimetric problems etc. were nobly treated, although that work was somewhat overshadowed by the fierceness of Johann Bernoulli's competition. Their glory rose so rapidly, that in 1699, when the eight foreign members of the Paris Academy were first appointed, both brothers were included[14], and in 1701 the same happened when, as recommended by Leibniz, the Berlin Academy was established.

Neither did the later authors of works on the history of mathematics, when treating the discovery of the differential calculus, fail to praise deservedly both of them. Savérien (1775) even burst out extraordinarily honourably but almost strongly that

Neither did the English, the German, the French, nor their authors understand at all the value of their discoveries. To Switzerland belongs the glory of producing two rare men, the brothers Bernoulli, who perceived the pertinent scope.

It would lead us too far to discuss all the separate contributions with which Jacob Bernoulli promoted mathematics, to deal with all the disputes in which he confidently and calmly participated, unlike his opponents whose behaviour often entirely lacked those qualities. It is generally sufficient to mention the richness of his *Opera* (1744) which shows us that he more or less studied all branches of pure and applied mathematics. For example, he annotated Descartes (1695); in physics, he considered caustics and oscillations; in astronomy, the problems of the shortest twilight [?] and of determination of longitudes at sea, etc.

However, he preferred to investigate the theory of series that, in particular, led him to the *Bernoulli numbers*[15]; the theory of combinations and its application to the theory of probability; to develop further the differential and integral calculus, which for example owes him the first integration of a differential equation, and its applications to the theory of curves and certain related problems in mechanics.

We will only follow him by a somewhat more detailed discussion of two of his fields of work, isoperimetry, where we will find him in a bitter contest with his brother Johann, and the theory of probability, where we will see his nephew Nicolaus I on his side instead of his son, see Note 8.

[7] As mentioned above, Jacob Bernoulli initiated his younger brother Johann into mathematics with exceptional success. After reading with him the most important mathematical authors and introducing him into the new field traced by Leibniz that he then wished to pick up, both had been at first most harmoniously working together, Jacob appreciating the impatient views and skill of his younger brother, and Johann recognizing the calmness and depth of his elder brother.

However, time and time again Johann's immeasurable ambition had wakened up and made it impossible for him not only to consider properly his elder brother and former teacher, a circumstance at which Jacob perhaps hinted now and then; it also led him to attempt at any price to elevate himself above Jacob. The latter naturally became colder and more reserved towards him, and when, in 1695, Johann departed to Groningen and their personal contacts ended, they became more and more alienated one from another and finally the abovementioned battle erupted between them.

In June 1696 Johann formulated a problem for mathematicians: To determine until the end of that year the line of quickest descent somewhat later called brachistochrone. At first, Jacob did not wish to occupy himself with that problem in earnest since, with regard to the problems published by him, Johann lately behaved the same way.

Only when Leibniz informed him on 13 September that he had solved the new problem and invited him to study it as well, did Jacob really take it up and solved it in a few days, at least before 6 October, that is, long before the initial deadline. And he had witnesses, Samuel Battier and Jakob Hermann. However, since Leibniz had informed him that the deadline was extended to Easter 1697 and asked him not to publish his possible solution until then, and since he had been working on the isoperimetric problem, Jacob did not hurry so as to present both problems at once.

By the end of 1696 everything was prepared for publication, and the only question was, whether he, for the time being, sends to Leipzig [to the *Acta Eruditorum*] only the result of his investigation of the brachistochrone, or also the appropriate analysis. He decided to take the middle ground and, on the one hand, to convince Johann that his brother had really discovered the solution, not simply guessed it, and, on the other hand, to prevent outsiders from appropriating the course of his study.

Then, however, he received a programme [no reference provided] published by Johann on 1 January 1697 in which he personally was challenged to solve the problem by a malicious allusion to one of his previous works. And now he could not hesitate anymore and that same January sent his solution to Leipzig adding a new problem:

Curves of equal length connect two points situated on the x-axis. Determine that curve the arbitrary powers of whose ordinates form a curve with the maximal area between it and the x-axis[16].

At first, he sent it to his brother Johann and promised him 50 reichsthaler on behalf of a friend (who was astonished by Jacob's own solution) should he submit a proper solution before the end of the year. When Johann saw that communication as well as Jacob's solution of the problem concerning the brachistochrone published in the *Acta Eruditorum* in May, he did not hesitate to take up the gauntlet. He believed to conclude the work in a short time and already in June 1697 wrote about it partly to Leibniz, see their correspondence (1745, p. 414) and partly to Basnage, see Basnage (compiled 1687 – 1709) and also Johann Bernoulli (1742, t. 1, p. 194).

[8] After complimenting Newton, Leibniz and l'Hospital on their solution of the brachistochrone problem, he referred to his brother:

He presumed (because, he said, Leibniz had asked him and he does not anymore wish to evade the task of studying it) that that problem occupied him for a long time and demanded much effort. Actually, he had believed from long ago, as Galileo did, that our curve was a circumference[17] *which surprises me since that kind of problems does not at all demand either much work or long or difficult calculations. Ordinarily, they are algebraic and at least I may say that as soon as that problem was proposed to me, I solved it.*

Yes, I solved it not by chance, as someone convinces himself, but, as challenged, by deliberate intention. Leibniz and Newton will say the same because both had discovered the solution as soon as they saw the problem. Be that as it may, my brother had finally found the proper solution by exactly the same method or slightly different from that applied by Leibniz who was long ago pleased to inform me about it in his relevant letters.

In addition, I find that Leibniz reasoned more succinctly without all those detours to similarities which my brother had applied for supporting his own solution[18]. *My brother was then the fourth of those from the three great nations of Germany, England and France, each of whom competed with me in such a fine research and arrived at the same result. That marvellous agreement can therefore prove the high quality of our methods for those who have no time to examine them and who without comprehension wish to refute them.*

Then Johann comes to the new problem of his brother:

No matter how difficult these problems seem to be, I do not shirk from studying them the very moment I see them. But look now how successful I am: Instead of the three months given me for exploring the ford, and instead of the rest of the year for discovering its solution, I have only spent three minutes to attempt, to begin and penetrate deeply all the mystery[19]. *And much more than that, since I provided a solution a thousand times more general than demanded*[20].

He concludes:

Finally, I have already sent my solutions to Leibniz and asked him to be our judge; indeed, it is justified and necessary for the unknown person promising a reward to hand it over to the judge and that is what, as I expect, an honourable man will not refuse to do. Once that is achieved, Leibniz will publish my solutions and at the same time pronounce his verdict on whether they are valid or not.

However, I am also assuring that it is not the desire to win but only the interest of the poor for whom I earmark that money which obliges me to take pains. I would be ashamed to receive the money for something that gave me so little trouble and only demanded time for writing this explanation. Even if it occupied me somewhat, money is not the means for recompense the mind in such cases. The noble ardour felt when studying these sciences is much beyond all money and the least discovery costs more than all the riches.

On 15 October 1697 Johann sent a similar letter to Varignon and communicated his alleged solution (JS[21] 2 Dec. 1697; Joh. B. *Opera omnia* 1742, t. 1, p. 206). As soon as Jacob saw it, he explained (JS 7 Febr. 1698; Jac. B. *Opera* 1744, t. 1, p. 214) that it could not be correct and offered:

1. To discover by a justified analysis what led his brother to the solution published in that periodical.

2. Be that as it may, to show the faulty conclusions if that is desirable for publication.

3. To provide the veritable solution of the problem in all its parts.

He obliged himself, should anyone wish to set prizes on those three points, “to lose as much, or twice, or thrice as much if unable to achieve them, respectively”.

After Johann (JS 21 April 1698; Jac. B. *Opera* 1744, t. 1, p. 215) acknowledged a somewhat barely noticeable and slight error made due to haste, Jacob (JS 26 May 1698; Jac. B. *Opera* 1744, t. 1, p. 220) simply advised his brother to check the supposed solution once more. Johann (JS 23 June 1698; Jac. B. *Opera* 1744, t. 1, p. 221) insisted that his method was correct and explained that he had something better to accomplish than to check his solutions once more.

Jacob meanwhile had sharply criticized his brother's solution in a letter of 26 June 1698 to Varignon (JS 4 Aug. 1698; Jac. B. *Opera* 1744, t. 1, p. 222) and reproached him for accidentally arriving at a particular correct answer issuing "from a faulty hypothesis by a faulty reasoning". Neither was Jacob satisfied by that refusal and stated (JS 4 Aug. 1698; Jac. B. *Opera* 1744, t. 1, p. 230) that he never believed that his

Brother possessed the veritable method for solving the isoperimetric problem, and now I doubt [he doubted] *it more than ever before owing to the difficulty that he encounters in checking his solutions. Because, finally, why refuse to do something done recently if not lacking trust in his own method? If it only takes him three minutes, as he says, to attempt, to begin and penetrate deeply all the mystery, then it seems that the reviewing of what was discovered will not be advantageous to him. And even if he spends twice that time, that is, six minutes, for that check, will it so much diminish the number of his new discoveries*?

Then Jacob once more invited Johann to examine again at least a certain part of his solution, to explain it not only to Leibniz, but also to Newton, l'Hospital and in general to all those whom all mathematicians recognize as judges and only to ask them to postpone their decision until he completely reviews his brother's solutions.

[9] And now Johann flared up. His answer of 22 Aug. 1698 (JS 8 Dec. 1698; Jac. B. *Opera* 1744, t. 1, p. 231) showered his brother with accusations such as, for example,

My brother acknowledges that he had not yet at all seen my analysis but nevertheless strangely refutes it. At first he fabricates an analysis and wrongly attributes it to me; he reasons unboundedly, invents absurdities, contradictions, nonsense. He is not looking for advantages, he is obstinate, imputes me everything as consequences of my imagined analysis. Throughout his letter he certainly refers to it with an inconceivable self-assurance as belonging to me. What audacity! What impudence! To wish to impute me entirely an analysis that is not my at all, that I myself forbid and disapprove of!

Johann concludes somewhat calmer:

Anyway, I am very glad that he finally really wishes to accept Leibniz as an arbiter and I am also content with the Marquis de l'Hospital and Newton. If rather accepting that suggestion, he will be able to evade many very useless debates. It was a long time ago that I sent Leibniz all my solutions for keeping together with my analysis and methods, both direct and indirect, which he approved and much praised, very far from finding those faults whose correction will encounter the truth[22].

And I am inviting my brother to send Leibniz also and at once his own method, solution and analysis. Leibniz will publish all at the same *time for our readers and for all of our judges for them to be able to confront, examine and judge them. Let us stop now and let my brother be silent until our solutions and methods appear. I will not accept anything more from him than at least that he delivers his solutions and methods to Leibniz and that they be published together with mine, and in the same place. Justice also demands his unknown friend to give over the prise to someone of our judges and he will do it if he is a decent honourable man. I have already said, and am saying it once more, that I am not soliciting anything. Indeed, the poor will claim the money*.

Jacob did not answer at once, and so it all remained as it was for more than a year except that the 50 reichsthaler were sent to Varignon (1745, p. 572). Only 6 May 1700 Jacob (1700) passed a candid message to his brother[23], and here is how it began:

Bearing in mind the squabble mutually carried on for some time, dearest brother, I fear that it can worsen our reputation in the eyes of many not because, when considering such a difficult subject, I regard our disagreement as insulting us (even friends, to say nothing about brothers, may without damaging the bonds of their friendship have differing views), but since we could have stopped quarrelling long ago and since our readers (or at least the readers of one of us) had almost always picked up from us more boasting and gossip than honesty and reliability, I have reasonably took care to avert that suspicion from us.

I thought I should brotherly start you up and caution you at the same time to deal with them a little more candidly, abandon any indecision and achieve that [honesty and reliability] *so that finally the truth is provided for the public and its general instruction and benefit looked after in such a way that, on the one hand, science is advanced and, on the other hand, neither of us loses the glory of discovery (which is, as our good Leibniz stated somewhere, the most honourable reward for work that can in future spur us and others). And so that now all will occur the more properly and each of us recognizes what part in that subject is due him, I consider it advisable to recall and enumerate briefly everything not less known to you than to me over which we had been arguing*.

Then Jacob worthily and calmly, although being confident about his legitimate cause, described what is already known to us about the emergence and course of those arguments and invited Johann to publish his method. In concluding, he provided his own solution of isoperimetric problems although without analysis and criticized both Johann's solutions.

Johann bitterly complained to Leibniz about that letter[24], sent him its copy supplied with marginal comments and implored him to stand by. He still thought that his previously sent solutions, although not coinciding with Jacob's, were good enough and sent them 1 February 1701 with an analysis in a sealed package to Varignon for depositing it at the Paris Academy not to be opened without his consent. He (JS 21 Febr. 1701; Jac. B. *Opera* 1744, t. 1, p. 377) once more gave full vent to his bile, as for example:

I am reserving for another time, should it become necessary, [the right] *to reveal other contradictions, errors and blunders in the* *writings of the author of those problems, even with respect to the main axioms of geometry, without however wishing at all to corrupt the beauty of his other mathematical discoveries possible to achieve by justified reasoning. Even the greatest men can stumble, and he is all the more pardonable who does not wish to blame others, especially if not wrongly*.

[10] Jacob however did not answer those compliments anymore but in March 1701 he published his analysis together with a disputation (1701)[25] as soon as becoming aware of Johann's deposit at an impartial judge, and that actually ended the tiresome strife. It could be thought that Johann will now attack the work of his brother or at least made at once public his own deposited work, but nothing happened. He was beaten and kept himself completely silent.

He recognized the mistake he made and the excellence of his brother's work[26] but lacked the generosity to acknowledge that publicly. He hesitated under hollow excuses until, hoping after Jacob had died, that he did not have to fear anything anymore since no one else could have penetrated the subject deeply enough and appreciate the difference between their methods[27].

He was not mistaken, he was really left unchallenged until at least in 1748 he felt it advantageous to admit that[28]

I have [he had] *looked over my long ago forgotten solutions anew and, after examining them again as attentively as possible, I finally recognize that I was actually mistaken in a certain way. Aspiration for truth compels me to acknowledge frankly what I had not noticed before and to recognize it with all the less shame since that is expected from an honest person and since the public will be grateful owing to the new discoveries it offers me the occasion to communicate.*

Had I not reviewed my manuscripts which much contributed to the advance of fine geometry, those discoveries could have remained forever buried there.

At the same time, he published a new solution which was no more than a remake of his brother's, a fact that he deemed best not to mention. It is so painful to see two otherwise exceptional men of great merit, two brothers, violently struggling for years that we thought it necessary to deal with that subject in detail because, in spite of all of its dark sides, it still formed a high spot in the scientific life of both of them.

Jacob's decided victory, a victory over an opponent powerful by himself and supported by Leibniz, was perhaps the greatest ever achieved in the purely intellectual field. It should not be forgotten, that their battle had been going on in the highest parts of mathematics, that already the acumen of Johann's works dazzled his contemporaries; that later mathematicians have also regarded Jacob's work as a wonder of discovery and depth and even thought that, considering his lifetime, no more difficult problems were then solved; and that no geometer of that time had publicly ventured to intervene in their battle or even to try to solve those problems although they were formulated for everyone.

Bossut (1804, Tl. 2, p. 181) correctly stressed that

All advantageous combined to excite ardour of competition: the novelty of the subject, the need to surmount serious difficulties, and the possibility of enriching geometry[29].

[11] The celebrated Laplace (1814/1995, p. 118) stated, after indicating the merits of Huygens, Hudde, Witt and Halley in furthering the theory of probability first attacked by Pascal and Fermat, that

About the same time James Bernoulli put various problems in probability to the mathematicians, problems whose solutions he [himself] *later gave. Finally he wrote his classic work entitled Ars Conjectandi, which only appeared seven years after his death in 1706* [eight years; died in 1705]. *The science of probabilities is much more deeply examined in this book than in that of Huygens; the author gives there a general theory of combinations and of series and applies it to several difficult problems in chances. This book is moreover remarkable for the accuracy and ingenuity of its insight, for its use of the binomial formula in this kind of questions, and for the proof of the following theorem:* [Laplace provides his own formulation of Bernoulli's law of large numbers and continues][30]

This theorem is extremely useful in discovering the laws and causes of phenomena from observations. Bernoulli attached, with reason, great importance to his proof which he said he had mulled over for twenty years.

After this testimony of the most competent judge about the importance of Jacob Bernoulli's work in the field of probability, it is sufficient to add a few relevant historical notes and add somewhat more details about his merit.

The name of his nephew Nicolaus I is so intimately connected with that of his great teacher. Jacob Bernoulli had already earlier dealt now and then with probability and, for example, already in 1685 formulated a pertinent problem (JS; *Opera* 1744, p. 207)[31], but only in the last years of his life he had been working on his main study definitely intending to compile his own systematic theory. That occurred exactly when his nephew became happy to be introduced by him into mathematics and had to some extent breathed in the flowery scent issuing from Jacob's work[32].

Jacob was regrettably unable to bring it to its desired goal and perhaps the *Ars Conjectandi* would have been forgotten forever to the great evil for the sciences had not Nicolaus developed it as is mainly shown by his own exceptional contribution (1709) which earned him the degree of Doctor of Laws and created a new branch of applied mathematics.

He showed there, in particular, how certain legal propositions, previously considered arbitrarily and quite differently in various countries, can be led to scientific principles. For example, in his Chapter 3 he recommended that a missing person be declared dead and his property given over without indemnity to his relatives if he is absent so long that the probability of his death becomes twice higher than that he is still alive. That means that, according to mortality tables, 2/3 of people of his age have died[33].

[12] The appearance of Montmort (1708)[34] also led to new studies and when, on 17 March 1710, Nicolaus' uncle Johann sent Montmort, with whom he had already been in correspondence for a long time, remarks about his book [Montmort 1713, pp. 283 – 298], Nicolaus also appended his own *Remarques* (pp. 299 – 303) which Montmort received with much interest.

And so was a scientific correspondence with that respected mathematician initiated also for Nicolaus. Montmort regarded the letters of his new friend so valuable, that he published all their correspondence [Montmort (1713)] with the following complimentary words [pp. XXV – XXVI]:

It is not necessary for me to praise those letters since they recommend themselves. It will be seen that in that field there can be nothing better and I hope that geometers will be grateful to me for sacrificing my author's vanity to the love of the public and the perfection of sciences by inserting these letters into my book.

In those letters and the replies to them many new and very difficult studies not mentioned in the body of my book will be found.

We see in those letters that, for example, in 1711 Nicolaus had published in the *Journal des Savants* his solutions of the problem *On the Lorraine lottery* formulated by Montmort; that he provided Montmort with many hints for revising the [future] edition of his book; that he used his stay in London (see Note 32) in particular for ascertaining from the bulletins of births for 1629 – 1710 that 18 boys were born there for 17 girls[35]. And, first of all, that while living and working together (see Note 32) Montmort learned how to estimate all magnitudes [?] since soon after their separation, on 20 August 1713, he [Montmort 1713, p. 400] wrote Nicolaus:

You are a terrible man; I believed that for going ahead I did not need to begin at once, but I see well enough my mistake. I am now far behind you and am compelled to apply all my ambition to follow you from afar. If I were jealous because of estimating you too highly, I would have liked you less; but not at all, Sir, your superiority and great talent only increase my attachment to, and if only I dare to use that term, my sincere friendly feelings about you.

[13] After all the above it will be hardly ventured to state that Jacob Bernoulli could have left for publication his unfinished *Ars Conjectandi* in better hands than those of his nephew Nicolaus. It is only to be regretted that he himself did not leave any such provision. Nevertheless, Nicolaus did not lose such publication from sight, and it was not his fault, as we will see, that he did not quite attain the desired. At first he thought, as it follows from a letter of 26 Febr. 1711 [Montmort 1713, pp. 308 – 314, see pp. 313 – 314], to accept Montmort's offer to look after the printing in Paris and said that he had written about it to Jacob's son Nicolaus. He added:

It is a great loss that the fourth part of that treatise that should have been the main part, was not concluded. It is not even commenced and only contains five chapters where only general things are treated. What is the most remarkable is the last chapter [the fifth][36] *where he provided the solution of a very curious problem that he even preferred to the quadrature of a circle, that is, To find how many observations should be made to achieve the desired degree of probability, and at the same time he proved that by repeated observations the ratio between* *the number of cases leading to a certain event and those not leading to it can be precisely discovered*[37].

That indication of the manuscript's value drew the attention of Jacob's heirs, but they preferred however to sell it to a bookshop in Basel, and Nicolaus, in a letter of 25 Oct. 1712 from London to Leibniz, regretted that the printing in his absence had begun without professional supervision. He also made known his intention to work after returning back at the extension of Jacob's manuscript if only the heirs will entrust him the pertinent materials (Papiers). When, however, he arrived in Basel the printing was too much advanced so that it was impossible to think about fulfilling his intention and he restricted his participation by adding an explanation of the circumstances[38]:

At last, here is the art of conjecture, the posthumous treatise of my uncle which has been so long awaited. The Brothers Thurnisius, thinking to do a public service, have acquired the manuscript from the author's executors, and have printed it at their own expense. The author wanted to make known in civil life the usefulness of that part of mathematics which is directed towards the measurement of probabilities. We have already seen in the memoirs of the Academy of Sciences of 1705 [Fontenelle (1706)][39] *and in the Scientific Abstracts of Paris of the year 1706* [*Journal des Savants*, Saurin (1706)], *by what method and up to what point the author has fulfilled the task which he set himself.*

He has divided his work into four parts. The first contains the treatise of the illustrious Huygens [1657] *with notes, in which one finds the first elements of the art of conjecture. The second part is comprised of the theory of permutations and combinations, theory so necessary for the calculation of probabilities and the use of which he explains in the third part for solution of games of chance. In the fourth part he undertook to apply the principles previously developed to civil, moral and economic affairs. But, held back for a long time by ill-health, and at last prevented by death itself, he was obliged to leave it imperfect.*

The editors would have liked the brother of the author (John – F. N. D.[apparently Johann]*), so capable of achieving this work, to have taken over completing it, but they knew that he had undertaken so much that they did not even ask him about it. As they knew that in an inaugural dissertation* [1709] *I have given some trial to this theory as applied to law, they asked me to undertake the completion. But my absence on travels did not allow me to do this. On my return they asked me again and I think I ought to mention why I did not. I was too young and inexperienced to know how to complete it. I did not feel I had enough initiative and I was afraid not only that I would not hold the attention of the reader but even that by risking the possibility of adding trivial and ordinary things I would do wrong to the rest of the work. The printing of this treatise being already fairly advanced, I advised the printers to give it to the public as the author left it*[40].

However, as it is necessary that so useful a thing as the application of probabilities to economic and political affairs should not be forgotten, we beg the illustrious author [Montmort (1708)] *and the* *celebrated De Moivre who wrote a little time ago some excellent fragments of this art* [De Moivre (1712)], *to set themselves to this work and to consecrate to it a little of the time that they set aside for the public good*[41]. *We hope especially that the generalisations given by the author in the five chapters of the last part will offer to the readers the principles of application important for the solution of particular problems.*

This is all I have to say on this treatise. The editors have added to it the theorems on infinite series which the author made the subject of five dissertations and which are out of print. It was for this reason that they have reprinted them at the end of this work. The affinity of the subject-matter has made us also add the paper written in French by the author entitled *Letter to a friend on chances in the game of tennis*[42].

[14] To end my essay it still remains necessary to add something about Jacob Bernoulli's activity in his home town and his last days. Meier von Knonau, see Note 1, describes the former:

During the civil unrest that occurred in Basel owing to the serious abuses in the public management, he provided in 1691 a memorandum about the practice (of cunningly acquiring positions) at the university, about the invasion of incompetent applicants for acquiring better paid chairs, appointment of uneducated elected representatives (supervisors of theology (Kirchenwesen) and educational system), of improper holidays, privileges enjoyed by professors of the philosophical faculty by occupying profitable vacant positions, etc.

The academic senate suspended him from the regency for a year. He explained that he never called himself the author of that memorandum, defended its contents and remarked that his misdemeanour consisted in that he turned to the investigation office established by the state. But he also indicated that if the senate will not improve the situation, the government or the citizens could arrange changes in the university which will not perhaps be pleasing to its staff.

It can be obviously regretted that Jacob Bernoulli's memorandum (1691/1993), that so clearly described the main evils from which the university suffered and will still suffer for a long time, had almost no other result except to quarrel him with his colleagues. However, it seems that they soon began to have second thoughts and found their behaviour towards him reprehensible. That probably was connected with the decision entered in 1692 into the records of the regency to destroy the pages concerning Jacob Bernoulli's affair[43].

It is hardly necessary to say that, owing to the incessant mental activity coupled with his slowness and depth and demanding many hours of work at night, and to the sedentary way of life essentially connected with his scientific efforts, his strength had to be exhausted early. The tiresome long-standing battle with his brother contributed to that process and harmed him the more the more he attempted to endure it while keeping outwardly calm.

Already in 1702 he complained to Leibniz that for many years he had been suffering from irritability and gout. In summer 1704 he went on a health cure to Baden [Baden-Baden] without however, as it seems, any considerable success and did not fail to understand that his life was drawing to an end. Indeed, this follows from his last letter to Leibniz of 3 June 1705 (Bernoulli Jakob 1993, pp. 149 – 151) which he prophetically ended by stating:

If rumour has it correctly, my brother will definitely return to Basel, although to take over not the Greek [chair] *(since he himself is analfabetos) but rather my position about which he, perhaps not unreasonably, feels that I will soon die and abandon it*[44].

And actually a severe fever strengthened his other ever repeating and increasing suffering. Being still fully conscious, he summoned his family and died on 16 August 1705 deeply mourned by his relatives, fellow citizen and the entire scientific world. According to his repeatedly expressed and pressing request, an ever again generating itself logarithmic spiral with the inscription *Eadem mutate resurgo* (Having changed, I am resurrected as I was previously) was carved on his tombstone. He thus reminded posterity not only about one of his most beautiful works, but also about his faith in the immortality of memory.

Notes

1. Two Jacobs, two Johanns, two Nicolauses and a Daniel, and a third Johann, a second Daniel and a Christoph can also be added. For preventing any confusion between the many learned members of that celebrated family it would be proper to give room to a genealogical note for the compilation of which the praiseworthily known by his technological contributions, his *Vademecum* etc Prof. Christoph Bernoulli (Basel), the son of Daniel II, had generously informed me that Jakob Bernoulli (1598 – 1634) was a merchant from an outstanding Antwerpen family who fled to Frankfurt in 1839 and then moved to Basel because of Alba's religious persecutions and in 1622 acquired civil rights there. From his son,

a) Nicolaus (1623 – 1708), a councillor in Basel, the following offspring can be mentioned:

b) **Jacob I** (1654 – 1705), the son of (a), professor of mathematics in Basel, discoverer of the logarithmic spiral, reviser of the theory of probability, etc, and teacher of Johann I (d) and Nicolaus I (e). Fontenelle (1706) is the author of an eulogy on him.

c) Nicolaus, painter, son of (a).

d) **Johann I** (1667 – 1748), son of (a), professor of mathematics in Groningen and Basel, teacher of l'Hospital, Euler and others; the first reviser of exponential magnitudes etc, Leibniz' correspondent and advocate. [Forney] in the Memoirs of the Berlin Academy (1747) and [Fouchy] in the Memoirs of the Paris Academy of Sciences (1748) are the authors of eulogies on him. [These dates are either of actual publication, or of the *für*, the *pour*.]

e) Nicolaus I (1687 – 1759), son of (c), professor of mathematics in Padua, later professor of law in Basel, editor of the posthumous papers of Jacob I.

f) Nicolaus II (1695 – 1726), son of (d), professor of law in Bern, then academician in Petersburg. An eulogy on him is Anonymous (1729).

g) **Daniel I** (1700 – 1782), son of (d), academician in Petersburg, later professor of physics in Basel, the author of *Hydrodynamics* published in 1738. The authors of eulogies on him are Condorcet (1785) and Daniel II (1783). [Daniel II had also published a German translation of Condorcet (1785) with corrections and comments, see Condorcet (1785) in the appended Bibliography. O. S.]

h) Johann II (1710 – 1790), son of (d), professor of mathematics in Basel.

i) Johann III (1744 – 1807), son of (h). Director of astronomical observatory in Berlin, later of the mathematical class of the Academy there.

k) Daniel II (1751 [1757?] – 1834), son of (h), professor of physics in Basel.

l) Jakob II (1759 – 1789), son of (h), academician in Petersburg. An eulogy on him is Anonymous (1793).

Johann I and Daniel I will be separately described in the next volumes of my collections, the other members of the Bernoulli family will be at least mentioned on occasion in more detail. For the current work about Jacob I and Nicolaus I, in addition to Fontenelle (1706) and Battier (1705) with 44 funeral odes appended to it the papers of Lacroix (1811), Meyer von Knonau [apparently: his article on Jacob I or Nicolaus I] in Ersch & Gruber (1818 – 1889) should also be taken into account. Then, the works of Montucla (1799 – 1802), Bossut (1802) and others and naturally my speech at the bicentenary of Jacob's birth (1855). R. W.

Jakob Bernoulli (1598 – 1634) mentioned in the beginning of this Note certainly was not persecuted by Alba who died in 1582. O. S.

2. Fontenelle (1706) translates this [into French] as *I am among the celestial bodies in spite of my father*. R. W.

3. Wolf outlines the life of that daughter, Elisabeth, who, although blind, became well educated. He referred to the pertinent discussion in the *Journal des Savants*, 1680, to the correction published there in 1685 by Jacob Bernoulli and to Schalch (year?, Bd. 2, pp. 191 – 196). O. S.

4. Translation: *In a simple way, without cross or God.* O. S.

5. Actually, in 1602 the Savoy troops attempted to storm the city, but were beaten. O. S.

6. In 1744 Gabriel Cramer published both these contributions in Geneva [in Jacob Bernoulli's *Opera*] as well as Jacob Bernoulli's other dissertations and communications to the *Journal des Savants*, *Acta Eruditorum*, etc, although not the *Ars Conjectandi*, and some of his posthumous papers and also his [in his] *Opera* in two volumes in quarto dedicated to Nicolaus I. In 1719 Weidler published the first contribution (1682) once more, and Lalande (1803) somewhat sharply stated that "It seems that Weidler was not yet as great an astronomer as he became later". R. W.

7. The German publication seems to have been distributed mostly in the neighbourhood and, for example, it remained entirely unknown to Lalande (1803). R. W.

8. Johann III Bernoulli (1777), after imagining the portraits of the learned friends of his family hanging in Basel in the house of his father, tells us that

The portrait of the Marquis l'Hospital was thought to be a good copy of the one drawn by Rigault and perhaps retouched by that great painter himself. I think that I heard that, being not at all the worst likeness made at the time, it was painted by the only son of Jaques Bernoulli, a councillor of the state who died a few years ago. He was devoted to painting but did not paint for a long time. His father destined him for science, and his cousin Nicolaus [I – R. W.] *became a painter. Their mentality did not conform to the main intentions of their fathers, and the sons often remained alien to them.*

Jacob [I] also named his son after his grandfather [after his own father] Nicolaus. R. W.

9. Each of those responsible for elections should even now imagine this statement pronounced already 150 years ago […] as though written in golden letters. Regrettably, it is ever again been neglected […]. However, *Exempla sunt odiosa* (odious) and I will therefore better keep silent. [Wolf's own gaps. O. S.] R. W.

10. It can be added in passing that, according to Leu & Sons (1747 – 1765), Jacob Bernoulli had also put out an *Übersetzung des andern Teils der Stimm* [Stimme?] *Gottes* (a translation of the other/the next? Part of Richard Baxter from Dutch, Basel, 1686) and that he was also praised for his not unfortunate poetical trials in Latin, German and French. One of them was published (1681b). R. W.

11. I am not dealing with the repeated and mostly passionately carried out priority strife between Newton and Leibniz about the discovery of the differential calculus since it did not essentially affect Jacob Bernoulli. However, I refer to the relevant contributions of Gerhardt, Biot, Sloman (1857) and others and to what I will briefly say in one of the next volumes of my collections as far as it concerned Johann Bernoulli. R. W.

12. The previously entirely unknown, so to say, correspondence between Leibniz and Jacob Bernoulli was included in Gerhardt (1855 – 1856, Bd. 3) [in Leibniz; Gerhardt was editor]. That volume also contains Leibniz' correspondence with Nicolaus I and Johann Bernoulli as also a new and supplemented edition of his correspondence with Johann as published previously (Lausanne, 1745, tt. 1 – 2) where many letters were corrupted. Although I only came across that volume a short time before concluding my present essay, I have attempted to make use of it as much as possible. R. W.

13. 21 March 1694 Leibniz wrote Johann Bernoulli: "Vestra enim non minus haec methodus, quam mea est". R. W.

14. Wolf mentions other foreign members of the Paris Academy up to Albert [Johann Albrecht] Euler and notes that "perhaps no other country and at least no other country as small as Switzerland" had so many of its sons appointed to that membership and that only Switzerland possessed a family with a foreign membership there for almost a century. O. S.

15. Bernoulli numbers are

1/6, 1/30, 1/42, 1/30, 5/66, …

and tend to a in the equality

$$\sum_{n=1}^{x} x^n = \frac{1}{2n+1} x^{2n+1} + \ldots \pm ax.$$

Jacob Bernoulli calculated the first five of them; later, they became important for the advanced theory of series. However, only De Moivre and Euler discovered the general law for calculating them, see Raabe (1848) and Staudt (1844). R. W.

The Bernoulli numbers are taken as

$B_n = 1, -1/2, 1/6, -1/30, \ldots$, the coefficients of the series $\sum_{n=0}^{\infty} B_n \frac{t^n}{n!}$ O. S.

16. I have formulated that problem in my own wording. O. S.

17. Here, Johann based himself on a note written by his brother:

Curva pag. 269 proposita videtur esse circulus fig. 5 cujus centrum est in intersectione horizontalis per punctum A transeuntis et alterius rectae ipsam rectam A B ad angulos rectos bisecantis.

Obviously, Jacob had written it when first reading his brother's problem and put it in the appropriate volume of the *Acta Eruditorum* which he later sent to l'Hospital without thinking about it anymore. L'Hopsital indiscreetly appended it to a letter to Johann remarking nevertheless that "You will however please me to say nothing". Johann did not pay any attention to that remark and at once sent the note together with a gloating comment to Leibniz who in his previous letters had also sent Johann fragments of writings in Jacob's hand.

It is thus seen that during the battle with his brother Jacob had reasonably became suspicious about many of his previous friends which in particular for a long time hindered his correspondence with Leibniz. The latter, although repeatedly asking Johann to restrain himself, was still not entirely impartial. On 15 November 1702 Jacob (Bernoulli Jacob 1993, pp. 100 – 104) bluntly wrote him that he, Leibniz, did not make use of his position to suppress the strife in embryo. R. W.

18. It is opportune to note that Jacob had deliberately chosen such a form to conceal the course of his thoughts. R. W.

19. That estimation of time intervals should not at all be understood literally. Johann Bernoulli also said in his biography that he and his brother unriddled Leibniz' secret in a few days whereas it actually demanded *many years*, see § 6 above. R. W.

In § 6 Wolf mentioned this without providing any documentation; moreover, he only had Jacob in mind. Later, Wolf (1859, p. 71n) stated that he had mentioned that biography as published by Roques (1750). O. S.

20. Instead of the ordinates of the second curve being equal to a given power of the corresponding ordinates of the first one, as was formulated in the initial problem, Johann stated that the former were *some given function* of the latter. R. W.

21. Here, and many times lower, JS means *Journal des Savants*. O. S.

22. Johann had indeed sent his first solution to Leibniz in June 1797 [perhaps 1697] and his revised second one a year later. Leibniz' approval possibly contributed to his being so sure about that subject. However, Leibniz had either considered it wrongly, or deceived himself, as we will see at once, by Johann's *Raisonnement*. R. W.

23. Probably according to Johann's desire the *Epistola* [see title of Jac. B. (1700)] was not included together with the reprints of his brother's contribution. Bossut was very interested in the strife and dealt with it in detail (Bossut 1802), then had it [its description] reprinted in the *Journal de Physique* for September 1792. R. W.

24. In particular, he wrote:

Tota fere conflata est ex calumniis, mendaciis et falsis pro more suo suspicionibus; nonnullis in locis me laudare videtur, sed aspero veneno latente acerrimo,

see p. 640 of their correspondence. R. W.

25. Wolf remarked that that *analysis* (1701) was dedicated to the "incomparable" l'Hospital, Leibniz, Newton and Nicolas Fatio de Duilleri and reported that same year in the May issue of the *Acta Eruditorum* and later in the author's *Opera.* O. S.

26. Jacob had examined, just as it was necessary, three elements of the curve; Johann, on the contrary, only two which was sufficient, for example, for the brachistochrone, but let him down in the general case although in some particular instances, when one condition being satisfied the second one was also necessarily complied with, he arrived at the correct result. For more details see letters and contributions JS 4 Aug. 1698; *Opera* t. 1, p. 222; Jacob Bernoulli (1700; 1701) as well as Giesel (1857). R. W.

Wolf's explanation is not sufficient. Johann dealt with a differential equation of the second rather than of the third degree (Hoffmann 1970, p. 48). O. S.

27. *Mém. de Paris* (1706); Jacob Bernoulli *Opera*, t. 1, p. 424. Johann's solution deposited at Paris was only opened on 17 April 1706, after the death of Jacob. R. W.

28. *Mém. de Paris* (1718); Johann Bernoulli (*Opera*, t. 2, p. 235). In the Introduction, Johann says that a suspicion was voiced that he had intentionally published his work mentioned in Note 26 after Jacob's death but that the [real] reason for his hesitation was described by Fontenelle (1706). However, I had vainly searched there for such reasons which would have been still valid after March 1701 and must therefore share that suspicion.

Yes, fear overwhelmed Johann when early in 1701 he came to hear rumours that Jacob wished to bring his analysis to Paris in person and be present at the opening and checking of his, Johann's, deposited packet, see the correspondence of Leibniz and Johann, pp. 654 and 659, and it seems to strengthen that suspicion. R. W.

29. It is remarkable and deserving to be mentioned that later the field of isoperimetry was also for the time being studied by Swiss mathematicians. In the analytical direction Bernoullis followed Euler (from Basel) and in the synthetic direction, they were led by Lhuilier (Geneva) and Steiner (Bern). R. W.

30. I think I may mention here the six series of trials I had studied (Wolf 1849 – 1853). R. W. [1849 – 1851– O. S.]

31. Here it is. A and B play with a die on the condition that the winner will be he who first tosses 1. They both, in turn, toss once, then twice each, at first A, then B, then thrice each etc. Or, A tosses once, B tosses twice, A tosses three times, etc. It is required to determine the ratio of their expectations (sort). See Jacob Bernoulli *Opera*, p. 207.

No one provided the answer, and Jacob himself published it in the *Acta Eruditorum* in January 1691 somewhat changing the conditions of the game. Similar problems are studied in his *Ars Conjectandi*, in the Supplement to its part 1. R. W. [In the commentary to the solution of Huygens' Additional problem No. 1. O. S.]

32. Nicolaus I was born 10 October 1687, early turned with special liking to mathematics, cf. Note 7. Already in 1704 he earned his Master degree defending his thesis (1704), later included in Jacob Bernoulli's *Opera*, under the chairmanship of his uncle [Jacob]. He then studied the law without, however, deserting mathematics as testified by his paper of 1708 *Regula generalis inveniendi aequationes, per quas alia quaepiam data, modo reducibilis sit, dividi potest* prompted by Newton's *Arithmetica Universalis* [1707; reprinted many times, translated into English many times and into French in 1802]. Nicolaus' uncle Johann sent the *Regula* to Leibniz, see their correspondence, pp. 827 – 835, and in an enlarged version in the previous edition of their correspondence, t. 2, pp. 179 – 209.

Then, his mathematical pursuits are also seen in his dissertation of 1709 [see beginning of § 12], in his letters to Montmort written in 1710 – 1713, to Leibniz in 1712 – 1716 and to Euler in 1742 – 1743, see Note 11 and Fuss (1843). Nicolaus also compiled a number of scattered contributions; in the summer of 1712 he travelled via Holland to England where he became acquainted with Newton, De Moivre and others and probably for this reason he was elected in 1714 Fellow of the Royal Society; previously, in 1713 he became member of the Berlin Academy, and later, in 1724, of the Academy of Bologna.

Newton presented Nicolaus a copy of his *Analysis* (1711), still kept at the Basel Library, De Moivre gave him his *Animadversiones* (1704) also to be found there with many marginal comments made by Nicolaus and his *Mensura sortis* (1712). At the end of the year [of 1712] Nicolaus went through Brussels to Paris where Montmort received him most obligingly and later took him to his estates where they were diligently occupying themselves for three months with their favourite science.

Nicolaus was also welcomed at the Paris scientific world which is unquestionably testified by the names of those meeting him there; and he was indeed also admitted into the high life: the Duchess de Angoulème, well acquainted with Montmort, affectionately received him. She died at an old age on 12 August 1713 [Montmort (1708/1713, p. 395].

Exactly a year after his departure, Nicolaus came back to Basel and first of all became busy with the printing of his uncle Jacob's posthumous papers. After Hermann left Padua, Leibniz recommended him in 1716 to the vacant post, but he did not stay there for a long time and in 1722 took over the professorship of logic in Basel for which he applied by publishing his *Theses* (1722).

In 1731 he became professor of law and remained in that position until his death on 29 November 1759. Having been deeply respected by his colleagues, he was four times appointed rector. His post and other [related] duties had left him no spare time for serious mathematical work as he already in 1742 with regret informed Euler. Otherwise, bearing in mind his keen perception, he would have certainly excelled in such studies. According to Leu & Sons (1747 – 1765), he published shorter contributions already in 1719 and 1720 in the *Acta Eruditorum* and in tt. 7 and 9 of their *Supplements*, in tt. 20, 24, 29 and 33 of the *Giorn. de lett.* [*Giornale de letterati d'Italia*], in No. 341 of the *Philosophical Transactions of the Roy. Soc*. [1717] and in the Paris *Mémoires* for 1711. Then, his published contributions to jurisprudence are his *Theses* (1711; 1720; 1722; 1731). Finally a volume in quarto of his manuscripts, it being a medley of studies and fragments on geometry, mechanics, astronomy etc, is kept at the Basel Library. R. W.

On Bernoulli's correspondence with Montmort see Henny (1975). O. S.

33. Wolf describes an episode in which that Nicolaus's recommendation became harmful to him himself. O. S.

34. The Basel Library keeps a copy of that book with the following lines inscribed by Bernoulli:

Francisce Christ, Amice mi/Tuas mihi doctissimas/De sorte dedicas theses,/Mihique sic das symbolum/Amoris erga me tui./En offero munusculum/Tibi vicissim, et hoc erit/Amoris erga te mei/Animique grati symbolum. R. W.

35. That statement should be essentially supplemented and corrected. First, Bernoulli had copied his statistical data from Arbuthnot (1712/1970, pp. 201 – 202) who, in turn, referred to "Observations […] of the births in London" (actually, of the baptisms there). Second, Bernoulli achieved much more (and Wolf's description above of his correspondence with Montmort is not adequate at all). In 1713 he (Montmort 1708/1713, pp. 280 – 285) studied Arbuthnot's data and indirectly derived the normal distribution thus anticipating De Moivre's note of 1733. See my general comment on [ix].

Finally, bearing in mind Wolf's description below of Bernoulli's participation in publishing his uncle's *Ars Conjectandi*, I ought to refer to Kohli (1975b, p. 541): not only did N. B. pick up in his thesis of 1709 some hints included in the manuscript of the *Ars*, he also borrowed separate passages both from it and from Jacob's *Meditatione* (Diary) never meant for publication. The stochastic part of that source is published: Bernoulli Jacob (1975, pp. 21 – 89). Also see Yushkevich (1986 and Kohli (1975a). O. S.

36. That statement partly contradicts the previous phrase. O. S.

37. The coincidence of Nicolaus' judgement with Laplace's statement above is certainly most interesting. R. W.

Note the use of *probability* and *favourable/unfavourable cases* in the same phrase. O. S.

38. Bernoulli Jacob (1713/1975, p. 108). I am inserting the translation from David (1962, pp. 133 – 134). Neither Wolf, nor she had translated that Introduction in full. O. S.

39. Bearing in mind Fontenelle's aim, Hermann informed him about the posthumous manuscript [of the *Ars Conjectandi*]. R. W.

40. This explanation does not completely agree with Wolf's description above; moreover, it would seem that Nicolaus did not really wish to "complete" the *Ars Conjectandi* as it indirectly follows from Note 35. O. S.

41. Montmort (1708/1713, p. XIII) felt himself unable to do anything of the sort: he should have studied the application of probability to political, economic and moral problems, but did not know "where to find the theories based on factual information which would allow me [him] to pursue" that research; translated by David (1962, p. 150).

42. See Bernoulli Jacob (1713) in Bibliography. O. S.

43. According to the verbal information from councillor Peter Merian in Basel. R. W.

44. The chair of Greek language in Basel was indeed offered to Johann, who, answering the pressing request of his father-in-law, resigned from his position in Groningen and on 18 August 1705 departed from there to Basel with all his family. On 23 August, while passing Amsterdam, he received information about Jacob's death. R. W.

Information about Scientists and Others Mentioned by Wolf

Baxter, Richard, 1615 – 1691, theologian

Bossut, Charles, 1730 – 1814

Cramer, Gabriel, 1704 – 1752, mathematician

Euler, Johann Albrecht, 1734 – 1800, mathematician. Son of Leonhard Euler

Fatio de Duillier, Jean Christophe, 1656 – 1720, mathematician. Jakob Bernoulli corresponded with him, see Bernoulli Jakob (1993)

Flamsteed, John, 1646 – 1719, astronomer

Gerhardt, Carl Immanuel, 1816 – 1899, mathematician

Hermann, Jakob, 1678 – 1733, mathematician

Hudde, Johannes, 1628 – 1704, mathematician

La Montre, Jean Joseph, 17th century, mathematician

L'Hospital, Guillaume François Antoine, 1661 – 1704, mathematician

Megerlin, Peter, 1623 – 1686, mathematician

Meton, – V century, Greek astronomer. Metonic period covers all the changes of the Moon

Rigault, Rigaud, Hyacinthe, 1659 – 1743, painter

Schwenter, Daniel, 1585 – 1636, orientalist and mathematician

Steiner, Jacob, 1796 – 1863, mathematician

Turretin, Jean Alphonse, 1671 – 1737, theologian and historian

Varignon, Pierre, 1654 – 1722, mathematician

Waldkirch, Esther Elisabeth, born 1660. See beginning of § 2

Weidler, Johann Friedrich, 1691 – 1755, mathematician, astronomer, lawyer

Witt, Jan de, 1625 – 1672, mathematician, statesman

Woleb (Wolleb), Johann, 1640 – 1675, professor of music, later of physics in Basel

Wolf, Rudolf, 1816 – 1893, astronomer. Author of much important work on history of science in Switzerland, see Bibliography

Geographical Names

Limousin, region in central France

Schaffhausen, capital of a canton of the same name in Switzerland

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II

V. R. Mrocek

The Origin and Development of the Theory of Probability

Trudy Inst. Istorii i Techniki, ser. 1, No. 2, 1934, pp. 45 – 60

1. Introduction

The origin of the main concepts of the theory of probability, like most foundations of our science, is lost in the darkness of the bygone times.

Thus Grave (1924) begins his short historical essay in Chapter 2 of his popular book. The history of the calculations of probabilities is connected with the empirical search for methods of enabling capitals to increase, which was not however allowed by the canon dogmas of feudalism with its closed economies of separate estates. In the beginning, the canonical doctrine utterly prohibited interest, gain and [the corresponding] increase of capital. Production for the market rather than for consumption had been compelled to look for roundabout ways, sometimes very witty. […]

During the era when municipal economy had been developing (the 13th century), the main theoretician of the catholic church, Thomas Aquinas, 1227 – 1274[1] studied the issues of commerce in his *Summa Theologiae* (about 1250) and considered profit as payment for labour. In isolated cases, under conditions of risk, he also tolerated increases in the initial price, especially in the intercity and international commerce. In Thomas' words, *the creditor may, without committing a sin, arrange matters with the debtor about being compensated for loss* (damnum emergens).

This opened a wide field for speculations of any kind. *Anyone became able to levy interest with clear conscience* (Kulisher 1918, p. 221). Thus began the medieval bacchanalia founded on *risk* and *loss*: the usual rate stated when concluding agreements was *each two months, one mark for each 10 marks* (60% yearly). It became necessary to yield further, i. e., to allow the levy of interest although lowering its rate. In the 14th century[2], it was allowed to take 43 1/3% for consumer credit. For commercial crediting, Venice levied 25 – 50%, not yearly but for the duration of a voyage, and the same applied to Florence. This often amounted to 250% yearly. An ordinance of 1311 allowed levies of 20% [on deals concluded] at fairs in Champagne.

Having begun with theoretical concessions, the Church passed on to usury. Monasteries, convents, city churches, monastic orders became in essence bank institutions accumulating the moneys obtained from the believers either as gifts or for keeping. In Bruges and Novgorod as well as in other places churches kept wares and balances and the buying and selling went on before the altars […].

Under feudalism, *risk* and *loss* in general did not yield to qualitative [quantitative] definition; each instance was *difficult* in itself. For sea voyages, the situation was especially complicated: the number of chances favouring a successful shipping of commodities had to be calculated[3]. Similar to dicing, then spread over the whole world, the case in which only one chance among all the possible ones was favourable, was called *difficult*, azari, from the Arabic asar[4]; hence, hazard. Sea operations were indeed as hazardous as gaming and it was necessary to find methods for making them profitable allowing for the *risk* and *loss* in accord with the canonical dogma. Gradually, *chances* of operations (*chance*, a face of a die) or cases (*casus*, the fall of a die) began to be calculated and a special branch of mathematics, the theory of probability, appeared. *La Grande Enc*., t. 27, p. 720 (end of the 19th century) clearly stated that *Le calcul des probabilités a pur but de mesurer les chances d'arrivée des événements due au hasard*[5].

2. Marine Insurance

Already in the 11th century commercial sea operations required legal regulations. A number of statutes appeared, at first connected with separate harbours, under a common title *Ordo et Consuetudo Maris*, see the details in one of the next issues of the *Arkhiv Inst. Istorii Nauki i Techniki*.

In the 12th century marine traders introduced promissory notes (*Wechsel*, exchange) transformed in the next century into bills of exchange. In the 14th century the latter were officially recognized, and many cities began discounting them and the bill of exchange passed on to trading in general. After the promissory notes came the marine loans (*foenus nauticum*) on the security of either the ship or the freight. This was one of the methods for sidestepping the canonical doctrine.

Finally, the joint-stock risk replaced the individual risk, or the fear of losing both ship and freight: marine insurance societies had appeared. They already calculated (roughly, of course) the probability of ship-wrecks and capture of ships by pirates. For shipping, the cost of insuring distant voyages was higher. Although the Atlantic impressed special fear [?], and a sea trip from Pisa to Bruges was considerably shorter than a voyage down the Rhein and by land, the insurance premium in the former case amounted to 12 – 15% because of the additional risk as sea as compared with 6 – 8% in the latter instance.

The first marine insurance societies appeared in the 14th century in Italy, Amsterdam and Bruges; in the next century, they acquired charters (in 1435, the Barcelona *ordinance*, and in 1498, the Genoi *statute*). In the 16th century, marine insurance was introduced in Florence, Naples, Spain, Portugal and Holland. The Florentine banks insured commodities and moneys for 10 – 15% *a tutto periglio di mare e di genti, di foco e di corsali*. Concerning the attitude towards calculating chances, the *Grande Enc.* (t. 4, p. 331) stated:

Toute compagnie d'assurance doit être considérée comme jouant au jeu de hasard avec le public qui est infiniment plus riche qu'elle[6].

3. Calculating the Chances of a Gambler

We already find the first published indication in the 15th century. In 1477, in Venice, Benvenuto d'Imola published Dante's *Divina Comedia* with his comments and considered there the frequency of the occurrence of the faces of a die. It is there that the term *azari* was used to designate a case having only one chance. A more interesting problem about an interrupted game is contained in the celebrated work of the monk Fra Luca Paciuoli [Paciolo, Paccioli, Luca di Borgo] *Summa Arithmetica* … of 1494. Cardano (*Practica* [*Arithmetice*], cap. 61, De extraordinariis et ludis), Fr. Peverone, ca. 1550, and Tartaglia in 1556 (*Generale Trattato*, I, entitled *Error di Fra Luco del Borgo*) followed suit.

In the 16th century it became necessary to study all the possible combinations which pushed the summing of series into the foreground. Already Cardano offered a table of chances based on the calculation of the cubes of numbers. Further on we find sums in Stiffel's well-known *Arithmetica Integra* of 1544. It contained a comparison of arithmetical and geometric series which Bürgi about 50 years later [in 1620] developed into a method of taking logarithms; a calculation of the powers of numbers by using the binomial, for example, $12^4 = (10 + 2)^4$; the triangular table of the binomial coefficients perfected later by Pascal; and, finally, various summings.

Buteo (*Logistica*, in 1559) and Harriot (*Artis Analyticae Praxis*, written about 1600, published only posthumously in 1631) calculated combinations. In 1612 – 1619, Faulhaber and Remmelin published rules for summing arithmetical series. Fermat (letter to Roberval of 16 Dec. 1636) described a method for summing any powers of integers and in 1634 Herigone (*Cursus mathematicus*, t. 2, p. 102) offered the formula for calculating C_r^n.

In 1653 Pascal informed his friends about his manuscript *Traité du triangle arithmétique* only published in 1665. He applied his triangle for solving the problem of points which the Chevalier de Méré had proposed to him next year [in 1654]. Pascal's correspondence with de Méré and Fermat showed that both he and Fermat, although by different methods, independently from each other solved this problem already formulated by Cardano in 1539 and Tartaglio in 1556. Historians of mathematics usually exaggerate this episode claiming that it constituted an exceptional event and considering that the *theory of probability* was born in 1654[7]. Actually, this was a direct continuation of the work of their predecessors as is seen from Pascal's words:

Usage du triangle arithmétique pour déterminer les parties qu'on doit faire entre deux joueurs qui jouent en plusieurs parties.

As we shall see now, the abovementioned correspondence only published in 1679 did not influence the development of the insurance institutions or the running of the joint-stock companies.

Huygens in 1657, Leibniz in 1666, Frenicle in 1676, Wallis in 1685 and Spinoza in 1687 had also been engaged in issues of combinatorial analysis. Many authors solved isolated problems calculating the chances of gamblers or describing previous achievements. Thus, Caramuel (*Mathesis biceps*, tt. 1 – 2, 1670) discussed the works of Ramon Lull, the Danish astronomer Longomontanus and Huygens. Saveur in 1679, Jakob Bernoulli in 1685 and Johann Bernoulli in 1690 studied isolated issues.

Is it possible to say that in those times, in the 17th century, a mathematical theory, or at least a method for solving the problems about chances was created? The most fervent advocates of the leading role of science [as compared with social and economic issues] are compelled to admit that the matter did not go beyond the solution of separate problems. Thus, Gouraud (1848, p. 13) says with respect to Huygens' *Ratiociniis in ludo aleae* of 1657 which served as a basis for all the subsequent quests for more than 60 years: *Plusieurs problèmes analogues à ceux du Chevalier de Méré résolus sans généralité, il est vrai*. Todhunter (1865, p. 21) very scornfully regards Gouraud's compilation filled with *very pretentious considerations*[8] but lacking a single formula. He himself is inclined to consider Pascal and Fermat as the founders of the theory though he hesitates about who of them is to take precedence and he also has to admit that for half a century after 1654 the theory of probability *advanced but little*. Concerning the significance of Pascal's work Grave (1924, p. 31) says:

For all the witticism of this solution, it suffers from the shortcoming that it is not seen how to solve other, more complicated problems about the sharing of stakes in an interrupted game.

Huygens and other writers used the formulas of the usual and weighted arithmetic mean of two numbers. The concept of *expectation* was thus established by the mid-17th century although for a long time it was not designated by any term. In the 18th century Jakob Bernoulli introduced the term *gambler's fate* quite corresponding to reality and to the essence of the matter[9]. But in the 19th century the bourgeois hypocrisy replaced it by *espérance mathématique* (Laplace 1812) or *mathematische Hoffnung* (with a shade of hope), or *mathematical expectation*. This is seen in the explanation provided by the *Enc. Sci. math*. (t. 1/4, p. 39):

Pour évaluer d'une façon objective l'attente d'un gain (ou d'une perte), attaché à la réalisation d'un événement incertain, on a introduit la notion de l'espérance mathématique.

Thus, the psychology of the game was masked by an allegedly objective mathematical calculation. At the same time, the new term suggested to the masses that this was a scientific expectation having nothing to do with fatalities, luck, chance etc [exactly so].

In the 17th century, the calculation of chances began to take shape in astronomy, in the works of Kepler and Galileo[10]. Such attempts were not however repeated until [Daniel] Bernoulli and Laplace. So what had compelled mathematicians of the 17th century, not gamblers at all, to consider issues as though connected with dicing? A definition included into our modern courses provides an answer:

An enterprise where a change in the capitals of its participants without any change of the total capital, is possible, is called a game[11]. And such games had indeed been developed then.

4. Joint-stock Companies, Banks and Exchanges in the 16th and 17th Centuries

The outcome of the heroic era of great geographical discoveries; the class shifts in the 16th and the beginning of the 17th century; the colonial policy pursued by the European countries; the flourishing of commercial operations together with the liberation of most states from the tyranny of the Romish church, were caused by, and at the same time gave a great impulse to European capitalism. Medieval guilds made way for the companies of the 16th century competing one with another until the 17^{th} century when united joint-stock societies were established in each country by merging. However, they did not yet need mathematical theories. As Mephistopheles says: *Krieg, Handel und Piraterie, − dreieinig sind sie, nicht zu trennen*. Kulisher (1918, p. 390) stresses that

A vast majority of the companies established in the 17^{th} and 18^{th} centuries had been existing precariously although many of them practised piracy.

Banks and exchanges provided a similar picture of activities founded not on scientific research but on methods sufficiently far from science. The work of medieval Italian money-changers was characterized by an oath (the city of Lucca, 1111):

To abstain in the future from deception, theft and forgery (*nec furtum faciant nec treccamentum aut falsitatem*).

The oath was engraved on a marble stone in the porch of the cathedral, but it seems that such oaths were easily forgotten … The money-changer sat at a table (*banco*) covered by a green cloth, hence the designation of the institution, and, later, of the person, *bancherius*.

Genuine banks of the modern type accepting moneys for keeping and change spread over Europe in the beginning of the 17^{th} century, and exchanges developed at the same time. The leather bag for keeping money, *bursa*, became a generic nickname for the Bruges merchants, van der Burse. This *noble clan* possessed a large house overlooking a square and soon gave the occasion for calling the merchants' meeting before the house *Bursa*.

The new institution, the exchange, appeared in Antwerp, Lyons, Amsterdam, Venice, Hamburg and London. Speculation went on not only in shares, but in all kinds of commodities as well. In 1619, an Italian author, Skaccia [transliteration uncertain] wrote that *By means of short letters the bankers transform fictitious money of account into real gold*. However, at the end of the century, in 1688, the Portugal Jew De La Vega warned: *Profit at the exchange is morning dew, a soap bubble, which disappears on the spot*. And the London street, Change Alley, where the exchange was situated, had been candidly called *robbers' den*. And so, neither the joint-stock societies, nor the banks, nor the exchanges stood in need of probability. Their demands on the theory only appeared in the 19^{th} century when methods of scientific gain at least founded on scientific grounds superseded downright robbery.

In the 17^{th} century, insurance societies developed slowly, beginning with marine insurance. In England, fire insurance then originated. The situation somewhat improved in the 18^{th} century (Copenhagen, Stockholm, Berlin, Paris) although only in its second half. Both Catholic and Protestant authors then adhered to the same opinion about lightning bolts and storms: *God sends all these phenomena as punishment for the sins of mankind* … Because of similar religious prejudices life insurance developed at the same slow rate.

The first societies originated in England; there, in the 18^{th} century, the bourgeoisie freed itself from many beliefs and turned to capitalism in a considerably greater measure than on the Continent. Thus, the *Amicable Society* (1706), the *Royal Exchange* and *London Assurance*

(1721) had appeared. France ventured to follow only in 1787, but the society there established was brought to an end in 1793, and only in 1819 a government decree allowed to organize the *Compagnie d'assurances générales sur la vie*. In Russia, a *Society concerning Cases of Death* was established during the reign of the Empress Ekaterina II.

Masked life insurance based on calculations of the gamblers' chances appeared in the 17th century. A Neapolitan banker Lorenzo Tonti established a society whose members deposited a certain sum of moneys into a fund with the interest being paid out to those still living. After the death of its last member, the fund had to become state property. The first *tontines* appeared in Italian and German cities; in 1653 Tonti proposed a project of a tontine to Cardinal Mazarin, but the [French] parliament did not approve it. Tontines were however introduced in France and England by the end of the 18th century. A modified tontine named after Lafarge existed in Paris from 1759 to 1889.

The craving for games of chance, or, more precisely, for quick winnings, so widely spread among mankind, was made use of when state lotteries came into being. The first such lottery was set up in Genoi in the beginning of the 17th century, and in the 18th century lotteries spread over France, Germany and Austria. Mathematical calculations concerning the Genoise lottery were simple although too difficult for laymen. There are 90 numbers, and 5 of them win at each series of drawings. The probability that a number wins was $p = 5/90$; for a bet on two numbers the probability was $(5/90)\cdot(4/89)$. For betting on 3, 4 or 5 numbers the respective probabilities were much lower in accordance with the formula[12]

$$p = \frac{5(5-1)(5-2)...(5-i+1)}{90(90-1)(90-2)...(90-i+1)},\ i = 1,2,...,5.$$

For stake M and betting on 1 number, the gambler's fate [expectation] is $-M/6$ [?] and a similar conclusion holds for all the other cases: his fate is indeed negative and the lottery is not fair. In a game of roulette the banker's expectation is always positive: the gamblers only get 36/37 of the fair payout.

6. Statistics

Let us now pass on to the new branch which prompted the development of most important mathematical methods, to statistics[13]. Already in the 11th century we find first information about the movement of property, commodities and population in various countries of Western Europe. In 1086 William the Conqueror brought together the data concerning England in the celebrated *Domesday Book*. In Russia, cadastral books were being compiled from 1266. After the Reformation, the churches began to register births and marriages, and in England such records were being published from 1592. And, again in England, from 1603 the movement of the population had been continuously recorded. At the end of the 17th century Colbert [in France] originated statistics of commerce. In England, at the same time, official price-lists were being published.

Capitalism then developing in Europe stood in need of combining isolated economic data and of creating methods for their application. Financial businessmen whose speculations put in danger the lives of isolated people as well as the existence of institutions also attempted to discover grounds for some predictions and to ensure their operations. The two first bulwarks of the bourgeoisie of the 17th century, Holland and England, made the first experiments in this direction.

In 1662, John Graunt, an owner of a haberdashery, issued a book, in which he treated the pertinent statistical data and offered a first attempt at calculating probable mortality at different ages. He already deduced the sex ratio at birth, 1.068[14], based on 32 years of observation. His investigations resulted in his election to the [just established] Royal Society.

In Holland, two eminent representatives of the bourgeoisie, the Amsterdam Burgomaster Johann Hudde, and the Grand Pensionary Johann de Witt, studied life annuities. The latter's booklet appeared in 1671, shortly before his tragic death. At the same time, a versatile Englishman who engaged in medicine, mathematics, land surveying, music and shipbuilding, as well as being a practical worker in economics; who made a fortune and was knighted, went further on in justifying the new branch of mathematics. He was William Petty, the author of several writings appearing in 1662 – 1692 and especially, in 1690, of *Political Arithmetic* which made him famous. In his Preface Petty says:

I have taken the course […] *to express my self in Terms of Number, Weight, or Measure; to use only Arguments of Sense, and to consider only such causes as have visible Foundations in Nature leaving those that depend upon the mutable Minds, Opinions, Appetites, and Passions of particular Men, to the Consideration of others* […].

Graunt and Petty are the founders of scientific statistics. The third person who developed their methods was Edmund Halley, the celebrated astronomer, the Southern Tycho, as his contemporaries called him. Halley studied the bills of mortality for London [?] and Breslau and in 1694 compiled his table of mortality. For a long time his method served as a basis for calculations concerning life insurance[15]. After Halley the following statisticians published mortality tables […].

In France, Deparcieux (1746) treated the data pertaining to three tontines (of 1689, 1696, 1734) by another method.

In the 18th century the application of statistical tables developed on the basis of mortality statistics met with sharp objections as soon as the same method was applied to cover other aspects of the life of states. The Dane Anchersen (1741) published a book which originated statecraft (Staatswissenschaft, University Statistics)[16]. Religious moralists [?] attacked his followers calling them *slaves of tables* and *representatives of the humble statistics*. Only in the 19th century did statistical tables earn a sound position. In 1853, the first International Statistical Congress took place in Brussels.

7. Mathematical Research in the 18th Century

My aim does not include a study of separate authors or their investigations. I offered [am offering] an essay analysing the appropriate events and expounding the social and economic motives causing the development of mathematical methods and models, and I shall restrict my attention to a brief exposition here also.

A jolly and witty nobleman of Louis XIV, Pierre Rémond de Montmort, a student of Malebranche and a correspondent of Leibniz, engaged in a general study of games of chance and the chances of winnings under the latter's influence. He (1708) dwelt on a number of games applying combinatorial analysis and infinite series, as for example

$$1/6 + 2/6^2 + 3/6^3 + \ldots$$

and offered a historical essay *depuis l'origine jusqu'à son temps*, that is, certainly from the time of Pascal.

Then, after the revocation of the Edict of Nantes, Abraham De Moivre, a Frenchman and a Huguenot, at the age of 18 [some three years later] fled from France to England. Being 40 years old and engaged as tutor by the family of the Duke of Devonshire, he became interested in the *Principia* whose copy Newton had presented to the Duke. When attempting to read it, De Moivre was surprised to find out that his knowledge was insufficient. A persistent man, he began [resumed] studying mathematics and attained great success. In 1712 he published his first work [in probability], *De mensura sortis* […].

Following Halley, De Moivre offered the first formulation of the so-called law of mortality stating that the number of those living decreased by a straight line[17]. […] Subsequent studies (Gompertz in 1825 and Makeham in 1860) based on more detailed statistical materials led to the introduction of a curve whose middle part approached the De Moivre straight line. […] Later a large number of authors proposed other *laws*, – actually, only generalizations of a purely empirical nature.

In 1730 Stirling offered a formula for calculating the product of natural numbers. […] De Moivre deduced the same formula even earlier […] and in 1738[18] applied it for proving the Bernoulli theorem. […]

Montmort's book of 1708 caused a lively correspondence and a number of other writings. In 1709, Niklaus Bernoulli, following his late uncle, Jakob, attempted to apply the theory of chances to legal issues. In 1713, appeared a second edition of Montmort's book. It contained his correspondence with Johann and Niklaus Bernoulli.

The same year (1713) there appeared Jakob Bernoulli's posthumous *Ars Conjectandi*. There, he (Chapter 2 of pt. 4) says[19]:

Regarding that which is certainly known and beyond doubt, we say that we <u>*know*</u> *or* <u>*understand*</u> [it]; *concerning all the rest, we only* <u>*conjecture*</u> *or* <u>*opine*</u>*. To make* <u>*conjectures*</u> *about something is the same as to measure its probability. Therefore, the art of* <u>*conjecturing*</u> *or* <u>*stochastics*</u> *is defined as the art of measuring the probability of things as exactly as possible, to be able always to choose what will be found the best, the more satisfactory, serene and reasonable for our judgements and actions. This alone supports all the wisdom of the philosopher and the prudence of the politician.*

[…] In Chapter 1 Bernoulli introduces *certainty* and its part, *probability* as well as the *necessary* and the *contingent* accompanied by an apt remark: *Contingency does not always exclude necessity up to secondary causes.* […]

Bernoulli calculates probabilities [chances] by simple algebraic methods which is not the important part of his work [?]. But then, in Chapter 4 of pt 4 he questions in its title: *What ought to be thought about something established by experience*? And further (pp. 29 – 30):

Even the most stupid person, all by himself and without any preliminary instruction, being guided by some natural instinct (which is extremely miraculous) feels sure that the more such observations are taken into account, the less is the danger of straying from the goal. […] *It remains to investigate something that no one had perhaps until now run across even in his thoughts. It certainly remains to inquire whether, when the number of observations thus increases, the probability of attaining the real ratio between the number of cases in which some event can occur or not, continually augments so that it finally exceeds any given degree of certitude. Or* [to the contrary] *the problem has, so to say, an asymptote, i. e., that there exists such a degree of certainty which can never be exceeded no matter how the observations be multiplied.*

And on p. 31:

This, then, is the problem that I decided to make here public after having known its solution for twenty years[20].

[…] The author's proof by means of the binomial, series and logarithms is now abandoned[21]. […] His philosophical views are expressed in the last words of his book:

If observations of all events be continued for the entire infinity (with probability finally turning into complete certitude), it will be noticed that everything in the world is governed by precise ratios and a constant law of changes, so that even in things to the highest degree casual and fortuitous we would be compelled to admit as though some necessity and, I may say, fate. I do not know whether Plato himself had this in mind in his doctrine on the restoration of all things according to which after an innumerable number of centuries everything will revert to its previous state[22].

The halo surrounding Jakob Bernoulli's book (1713) in the 20th century and the enthusiastic opinions expressed about it in the 19th century[23] compel the historian to regard it especially attentively. In itself, the principle of mass observations was in the possession of all interested scientists of his time. Already Cardano formulated the so-called principle of large numbers in the mid-16th century and the development of statistical observations and insurance in Holland and England was based on *large numbers*.

Thus, only the mathematical shell of the principle constituted Bernoulli's innovation. Picked up, improved and modified by subsequent workers in this field, it was completed in the mid-19th century. As to applications, Basel and entire Switzerland were in those times remote from the advanced economy of the Northern countries and France. […]

The achievements of the 18th century apparently include the development of insurance, the creation of the elements of statistics and the publication of a large number of solved or formulated problems, but an integral theory of probability was lacking. If the 17th century was the era of initial accumulation, the 18th century was the period of secondary accumulation. Only after the social revolution of the end of the 18th, and the beginning of the 19th century, the bourgeoisie, having come to power, gave the mathematicians the opportunity to place the new calculus at its service. I shall consider the history of that period in a further work.

8. The History of the Theory of Probability

Apart from the two abovementioned books of Gouraud (1848) and Todhunter (1865), absolutely different with respect to their contents and manner of exposition, and similar only in that they lack any indication about the social and economic issues which originated the new calculus, there are no more books on the history of probability before the 19th century.

Cantor, in his fundamental work, included pertinent isolated paragraphs (1900 – 1908, volumes 2, 3 and 4). Cajori (1893/1919, pp. 377 – 383) devoted six pages to the period from Laplace to 1912. Still less (two pages) are in Smith (1958, vol. 2, pp. 528 – 530) who *covers* the period from the 15th to the 20th century. And the *Enc. Math. Sci.*, also mentioned above, does not have, in its pertinent chapter, even a single subsection on the history of the calculus of probability. Tropfke (1924, pp. 63 – 74) offers similar fragmentary information. Helen Walker (1929) provides an important description of the 19th and 20th centuries but she restricts her attention to the earlier period by offering only brief indications (pp. 4 – 13) in accord with Todhunter. Finally, Czuber (1899) almost exclusively treats the 19th century with only a few lines about the mathematical works in probability appearing before Laplace.

Given this situation, it is not surprising that various absurdities can be found in some textbooks. […] I shall have to return to the other mistakes of Khotimsky et al in the second part of my work. […] A detailed bibliography will be appended at the end of my investigation which is to appear in one of the next issues of this source.

Notes

1. The date of his birth is possibly 1225 or 1226. O. S.

2. In many instances the author only mentions centuries rather than shorter intervals. O. S.

3. At best, chances had been crudely estimated. O. S.

4. David (1962, p. 34n) mentions another possible explanation, and Kendall (1956/1970, p. 21n), in spite of d'Imola (see § 3), does not agree with deriving *difficult* from *d'asar*.

5. This passage describes a much later period and is therefore irrelevant. O. S.

6. Pity the poor companies! O. S.

7. Pascal and Fermat directed the attention of mathematicians to a new field and actually introduced the expectation (of a random variable). O. S.

8. *Lacking formulas* seems to be correct, but *very pretentious* … is an invention. Todhunter many times mentioned Gouraud, but not on p. 21. O. S.

9. Huygens mentioned the *valeur de ma chance* whereas Jakob Bernoulli (1713/1975, p. 110) translated this expression into Latin as *expectation mea dicenda est valere*. Below, the author discusses the introduction of the term *expectation* in a most vulgar manner. Laplace (1812/1886, p. 189) added the adjective *mathematical* to distinguish it from the then topical *moral expectation*. His innovation took root in the French and Russian literature, but it is high time to get rid of it. O. S.

10. Galileo and Kepler had to treat direct and indirect measurements rather than calculate chances. See Sheynin (1993). O. S.

11. A passage concerning games of chance would have been better, but still irrelevant. Dicing and games of chance in general were and still are useful in the methodical sense. O. S.

12. The formula below (as well as the solution of a particular case just above) is wrong. See, for example, Maistrov (1967/1974, pp. 101 – 103). O. S.

13. I do not touch on the statistical work carried out in the ancient world. V. M.

14. According to Graunt, the sex ratio at birth was 14:13 (= 1.077). And the author did not adequately describe Graunt's achievements. O. S.

15. In 1699 Stansfeld established in London a *Society of Assurance for Widows and Orphans*. V. M.

16. In 1727, Kirilov prepared an atlas of tables describing Russia, but it was only published in 1831. O. S.

17. De Moivre introduced the uniform distribution as the law of mortality for ages beginning with 12 years. O. S.

18. The correct date is 1733 rather than 1738. Then, De Moivre derived the Stirling formula not *even earlier* than, but at the same time as Stirling himself. O. S.

19. I quote from my translation of that part 4 (Bernoulli 2005). In 2006, Edith Dudly Sylla translated the entire *Ars Conjectandi*, but her work is beneath criticism (Sheynin 2006). O. S.

20. Some authors translate the last sentence as … *after pondering about it for twenty years*. O. S.

21. No, not abandoned but essentially improved, mostly by applying the Stirling formula still unknown to Bernoulli. O. S.

22. Bernoulli expressed his no less important philosophical views in chapter 4 of pt 4, see above. O. S.

23. Thus, Gouraud (1848): *Un des monuments les plus importants de l'histoire des mathématiques*. Todhunter and Moritz Cantor were more restrained. V. M.

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III

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The Tercentenary of Population Statistics

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1. Introduction

In most cases it is very easy to ascertain the anniversary of some man of science by issuing from the date of his birth or death, but it is difficult indeed to establish an anniversary of a science. It is usually impossible to trace precisely enough its beginnings often shrouded by the mist of bygone centuries. Happily, this can not be said about demography the date of whose birth can be stated quite exactly: January 1662. It was then that the Englishman John Graunt published a book which should unconditionally be considered as the first scientific investigation in population statistics. We therefore have all the grounds to think that January 1962 was a prominent date, the tercentenary of demography as a science.

2. Biographical Information about Graunt

John Graunt was born 24 April 1620 in London into the family of a haberdasher. His father Henry was a Hampshire man. After moving to London, he started a shop called *Seven Stars* in Birchin Lane. He had eight children, and there are reasons to suppose that John was the eldest: he was born when his father was only [?] 28 years old.

John did not receive any special education. We only know that in the mornings, before shop-time, he studied Latin and French. And it is also known that his father apprenticed him to a haberdasher and that he, during all the time of his commercial activities, had been engaged in selling haberdashery. It might be thought that after the death of his father he took over his business. As a merchant, Graunt became rather famous in the business circles of London. He was reputed to be honest and was elected arbitrator between parties to various disputes. Graunt's contemporaries highly praised his intellect, unusual wit and ability to keep up an interesting conversation. His business went on so successfully that he became able to buy expensive paintings and his superb picture-gallery was among the best ones.

Graunt apparently enjoyed a very interesting circle of cultured friends. One of these was the then celebrated miniaturist Samuel Cooper; the others were John Hales, William Petty and many other prominent citizens of London. Graunt's friendship with Petty should be especially noted. He apparently became already acquainted with the latter in his young years; they probably met at the house of one of their countrymen (both were of Hampshire stock). During the first period of their friendship, the rich and influential Graunt in every possible way helped Petty who was three years younger and did not yet have means or sufficient social connections. In any case, Graunt, then aged 30, is known to have actively assisted Petty, aged 27, in becoming professor of music at Gresham College[1].

In addition to commerce, Graunt found much time for social work. He was captain, then major of the *Trained Band* [of militia] and for two years member of the London municipal council. In 1666 he was even appointed commissioner for the water-supply of London and invited to head New River Co. managing this supply. This fact gave some authors occasion for accusing Graunt of turning off the cocks during the night before the Fire of London of 2 September 1666. However, the historian Maitland refuted this allegation by proving that Graunt had only become commissioner on 25 September 1666, 23 days after the Fire.

In those years, special bills of mortality had been published in London. They even date back to the 16^{th} century, but they only began to appear regularly in the early 17^{th} century. Each Tuesday, the relevant data were extracted from the separate parish registers; on the next day the weekly summaries appeared and on Thursdays these were sent out to subscribers. A yearly bill had been published on the Thursday before each Christmas.

It seems that the number of the subscribers was not small, otherwise the bills would not have been published. Many inhabitants of London, mostly those from the wealthier, willingly glanced them over, being, however, interested in them for purely practical purposes. Rich people read the bills to find out whether mortality from plague had increased and attempted to leave London at the first indication of an epidemic. In addition, merchants and producers sought answers to questions such as, Should they buy up commodities; and, How high are the chances of selling their wares.

In 1660, the stormy period of civil war, which lasted in England for almost 20 years, ended. A new period of English history began with the Restoration, with the enthroning of Charles II. It might be thought that exactly by that time John Graunt, the flourishing 40-years-old London merchant and public figure, originated a brilliant idea: Would it not be possible to consider the bills of mortality as an object of scientific research? Exactly in 1660 – 1661, when his business was in a fine state, he evidently began to have enough time at his disposal and to engage in treating the rich initial statistical material.

Indeed, his main calculations concern the data up to 1658 which gives grounds for assuming that Graunt began writing his book even in 1659 and gave it the same year to the publisher. His *Epistle Dedicatory* is dated 25 January 1662, just before the *Observations* appeared, and the book was apparently put out a few days later. Indeed, already on 5 February 50 of its copies were submitted on the author's behalf to the *Philosophical Society* at Gresham College then being incorporated as the *Royal Society*. And, in another few days, Charles II himself asked the Royal Society to admit Graunt to their body, to the academy of sciences of England.

The Society very attentively regarded the Royal proposal. Only a week had to pass before, on 12 February, a special commission comprising six men including Petty was set up for appraising the scientific significance of Graunt's contribution. We would think that Petty thoroughly explained all the importance of the *Observations* to the other members. The report of the committee did not survive, but in a fortnight, on 26 February 1662, as its result, Graunt was elected Fellow of the Royal Society. His merits were thus all at once recognized by his contemporaries and the year 1662 became the beginning of his fame.

During all that year Graunt did not make any reports and apparently neither had he been engaged in developing his writing. But it is known that he, somewhat suddenly, became occupied with the efficiency of rearing carp and salmon in ponds. On 19 August 1663 Graunt reported on this issue at the Royal Society. He discussed the number of carps and their distribution by size and thus applied statistical methods to pisciculture[2]. It seems however that Graunt nevertheless actively participated in the work of the Society because on 30 November 1664 he was elected to its council and remained there until 11 April 1666.

During those years Graunt's fame rose and his book was reissued several times (in 1664 and twice in 1665); during 1662 – 1665 Graunt's merits were being widely recognized and he remained on the summit of his celebrity and material well-being. However, already the next year his circumstances sharply changed. In September 1666 the Fire of London destroyed all his property; he became insolvent and found himself in dire poverty. His friend Petty, then getting on successfully, tried to help Graunt by appointing him his London agent. Petty lived then in Ireland and he even insisted that Graunt moved there, but the latter never left London. In his youth, Graunt assisted Petty, but in their old age these roles were switched. Petty's help did not, however, save Graunt. And it is even possible that he was refusing it. Graunt's circumstances also worsened because of his renouncing the Church of England and becoming a Roman Catholic[3].

From 1666 Graunt ceased to participate in the activities of the Royal Society and later he apparently withdrew from every occupation. On 18 April 1674 he died of jaundice in his own house[4] in London being six days short of 54. He was buried on 22 April in a church in Fleet Street. Petty was present at the funeral and contemporaries testified that he had keenly felt the death of his friend.

Except for the *Observations* Graunt did not leave any published writings although it is known that his scientific activity was not restricted to that contribution. During his last years he wrote *Observations on the Advance of Excise* and a work devoted to religious issues. Both these manuscripts remain unpublished[5]. It should be thought that they have no relation to demography, and that, even if appearing in print, they will not increase Graunt's scientific merits which are high in this very branch of knowledge.

3. Graunt's Work

Graunt's book (1662) was the first scientific investigation of demographic issues in the history of mankind. Already its title is interesting in that it stresses the need to study not only biological and geographical factors of mortality, but, for that matter, its social and economic causes. It is essential to note that a theological interpretation of facts is barely present in the *Observations*, so that on the whole it is of a secular, and we might even say [?], of a sociological nature.

Graunt was the first to guess that the material of population statistics can be an object of scientific analysis and the basis for important deductions. And he did not at all consider such a study as a game with figures, he understood that statistical analysis can really benefit the people. In his Preface he wrote:

Finding some truths, and not commonly-believed opinions, to arise from my Meditations upon those neglected Papers [the bills of mortality], *I proceeded further to consider what benefit the knowledge of the same would bring to the world* […].

Graunt admits that he is not a professional scientist. We already know that he discovered demography while being engaged in commerce, but this fact does not in the least detract from his merits. A professional would have been unable even to arrive at his discovery because no such science had existed before him. And we should not be surprised that a merchant and a social figure rather than, for example, a biologist or a physician originated population statistics; a successful businessman was more likely to have enough time at his disposal for making such a discovery.

It is also understandable that demography was indeed born in England. By those times, England was already the most developed industrial country pushing past Holland, France and Spain. It was in that country which liberated itself from under the watch of the Catholic Church and had comparatively developed printing facilities, that it became possible to publish weekly bills which attracted Graunt's attention. And we can not fail to note the influence of the epidemics of plague which devastated the densely populated London with an especial force. A considerable part of the city population perished as a result of each epidemic, and this gloomy fact was one of the causes explaining the publication of the bills of mortality in London. The combination of all these circumstances had indeed led to the origin of demography in England in the mid-17th century.

It is important to note that Graunt did not restrict his efforts to a simple comparison of various numerical data. His main merit is that he, as though rising above the isolated facts, understood that some regularity, a conformity with some laws, characterizes their totality taken as a whole. Graunt did not directly mention the action of the [still unknown] law of large numbers, but he almost felt it[6] when formulating his demographic conclusions. Here they are.

1) The numbers of men and women are roughly the same. For us, this is a platitude, but in those times there existed the most wrong impressions on that point. Physicians, for example, maintained that among their patients there were twice as many women as men so that the opinion that in the general population women considerably outnumbered men was widely spread. And polygamy existing in Muslim nations gave additional grounds for believing that the number of women largely exceeded the number of men. Graunt, after counting the number of those who died during 1628 – 1661, concluded that the Christian religion, forbidding polygamy, better conformed to the laws of nature than Islam.

2) The sex ratio at birth is 14 boys:13 girls which means an excess of the male births of 1/13 = 7.7%. Graunt understood that, since he dealt with a large number of observations, such an excess could not have been the result of the action of some random factors. He correctly interpreted this excess as a definite regularity. Nowadays it is

expressed by a somewhat lesser number, but the discrepancy should be explained by the fact that in Graunt's time boys had been baptized more often than girls[7].

3) Mortality in London is higher than the birth rate, but the number of its inhabitants is increasing, which is only possible because of a strong influx of population from the countryside. Incidentally, this fact gave grounds for him to conclude that the rural areas were disproportionately represented in Parliament.

4) While studying the influence of the plague epidemics, Graunt ascertained that in London the ensuing loss of life was being made up in two years.

5) He (p. 320) indicated that

London […] *is perhaps a head too big for the Body, and possibly too strong*: […] *this head grows three times as fast as the Body unto which it belongs*.

6) Mortality in towns is higher than in the countryside.

It is important to note that Graunt (p. 397) understood in a wide sense the significance of population statistics which he originated. He thought that the knowledge of the population and of its distribution was necessary for governing the country and directing commerce and industry in accord with the requirements of the population:

I conclude, That a clear knowledge of all these particulars, and many more, whereat I have shot but at rovers, is necessary in order to good, certain and easie Government, and even to balance Parties and Factions both in Church and State.

In the *Observations*, we also see attempts at a social and economic analysis of statistical material. Thus, he (p. 396) indicated that

It would appear, how small a part of the People work upon necessary Labours and callings, viz., how many Women and Children do just nothing, only learning to spend what others get[8].

He showed a remarkable gift for statistical investigations; he skilfully dealt with the initial data, critically analyzed them, determined the boundaries for the possibility of comparing them, etc.

Graunt was the first to construct a mortality table [a life table] which is his especially great merit. References to the Roman praetorian prefect Ulpianus, who is considered as the author of the first such table, are unconvincing. We are not sure what exactly do the data of his table represent: the mean duration of life for men of various ages, or the consecutive remainders of the debt (assumed to equal unity) yet to be paid by the annuitants, see Trennery (1926, p. 150) as quoted by Dublin et al (1949, p. 31). And neither the origin of Ulpianus' figures nor their justification is known. Graunt, on the other hand, provided the two main columns of the mortality table, d_x and l_x, and explained his proposed methods of calculation.

When compiling this table, Graunt was in a very difficult position. His data were the lists of the died only distributed by the causes of death without any information about their ages. In addition, the registration of those causes was extremely imperfect: they included, for example, headache, chill, teeth, fright and misfortune and had been determined by hardly competent persons.

These difficulties did not, however, confuse Graunt, and he made use of the indications about the causes of death for roughly ascertaining the age structure at death. He based himself on the totals of death for 1629 – 1636 and 1647 – 1658. Although his main table has data for 1659 and 1660 as well, he did not include them in his analysis and restricted his calculations to the 20 years during which the total number of deaths, the basis for his further work, was 229,250.

It is extremely interesting that Graunt had in essence grouped all the 81 causes of death. He isolated those who died of children's diseases (71,124); epidemics; chronic diseases; and accidents which enabled him to approach the issue of the age structure at death.

Graunt thought that all the children's diseases happened at ages of up to 4 or 5 years, but he understood that children had died not only of these specific diseases. He therefore set up a new group of causes of death, diseases affecting both children and adults (smallpox, measles and intestinal worms), and having been responsible for 12,210 deaths. Graunt then assumed that a half of this number were children up to 6 years of age but excluded 16 thousand deaths from plague considering the plague epidemics as a *perturbative* factor. As a result, he showed that those who died before reaching 6 years of age constituted 36% of the total[9]:

$$\frac{71{,}124+12{,}210/2}{229{,}250-16{,}384}=0.36.$$

Another indication of the age at death was an entry *died of old age*. The pertinent number was 15,757 or 7% of the total, 229,250. And he (p. 352) wrote:

Only the question is, *What number of years the Searchers call Aged, which I conceive must be the same that David*[10] *calls so, viz. 70*. […] *It follows from hence, That if in any other Country more than seven of the 100 live beyond 70, such Country is to be esteemed more healthful than this of our city*.

Graunt, however, refused to consider only 70 years as *aged*; he issued from a far lesser age, 56, which he therefore assumed as the beginning of old age. At the same time, he decreased the portion of those who died of old age from 7 to 6% but did not justify this decision.

He had no other possibilities for connecting the causes of death with age, but he formulated for himself a problem of great importance: To determine the order of extinction of a generation; that is, to calculate the column l_x. At first, he needed the column d_x, and he acted thus (p. 386):

Whereas we have found, that of 100 quick Conceptions about 36 of them die before they be six years old, and that perhaps but one surviveth 76; having seven decades between six and 76, we sought six mean proportional numbers between 64, the remainder, living at six years, and the one, which survives 76, and find, that the numbers following are practically near enough to the truth; for men do not die in exact proportions, nor in Fractions.

He then provided these numbers:

Of an hundred there die within the first six years […] *36; the next ten years, or Decad*, […] 24 [and then 15, 9, 6, 4, 3, 2, and 1].

On these grounds he (Ibidem) compiled a table describing the order of the extinction of a generation:

From whence it follows, that of the said 100 conceived, there remain alive at six years end […] *64. At sixteen years end* […] *40.* [The figures 25, 16, 10, 6, 3, 1 and 0 at 86 follow.]

Various assumptions were made about the kind of mean proportional numbers on which Graunt had underpinned his column l_x for ages beginning with 6 years. The British statistician and demographer Greenwood (1928) thought that Graunt had applied a geometric progression with ratio 0.62 and first term 64. The Soviet statistician and demographer Ptoukha (1938) decided that Graunt's progression had ratio 0.63 whereas Willcox (1939, p. xii) suggested that its value was 5/8. In accord with these hypotheses we obtain the following series of numbers for survivors of various ages [not shown in translation].

It is most likely that Graunt, when determining these numbers, assumed that during each next decade the relative number of those dying was the same as during the first six years of life. He (see above) established that 36% die during the first six years of life, and 64% survive. Assuming that the next decade the same portion of the survivors will die, he obtained 40.96 (= 64 – 0.36·64), or, after rounding off, 40.

For the next 10 years he got, in the same way, 25.6 (= 40 – 0.36·40), or, approximately, 25, then 16. For the two decades after that Graunt's numbers are obtained with the [same] coefficient of survival, 0.64. Only for the ages exceeding 56 he abandons this proportion. Supposing that the curve of survival begins to decrease sharply, he assumes for 66 years 3 rather than 4, which should have been taken had the proportion persisted. That is, he supposes that half of the survivors will die during the decade 56 – 66. For the next decade he assumes an even sharper decrease of the curve, by factor 3 rather than by 2, so that only one lives to age 76.

It is natural that Graunt's numbers badly reflected reality. Whereas he rather closely approached it for children's mortality[11], he was absolutely wrong in all the other cases. He made a serious error in assuming a proportional decrease in the number of survivors. It was of course a great mistake to believe that within the interval from 6 to 56 years the coefficient of mortality persisted for any age. The change of this coefficient with age can be roughly shown in the following way. [The author provides two graphs of the coefficients of mortality against age; the first one, according to Graunt and the second, basing himself on Russel (1948, p. 464) and Greenwood (1936, pp. 676 – 677). The graphs sharply differ; the broken line of the second one goes downward until ca. age 10, then rises steeply.]

Graunt greatly exaggerated the level of mortality for the ages from 6 to 56 years[12]; consequently, in accord with his mortality table, the mean duration of life was only equal to 18.2 years. We can only regret that he did not calculate it himself; otherwise, he would have likely seen that the principle of proportionality, which he assumed as his basis, led to figures far from reality[13]. However, irrespective of his numerical results, Graunt's main merit is that he was the first to put forward the idea of a curve [?] of survivorship and virtually calculated it. The business of his followers was to correct his calculations, but, anyway, he pioneered such studies and no one can deny it.

Graunt did not restrict his work by determining the numbers of survivors but applied them for various calculations. He considered them as a reflection of the age structure of the population and appropriately computed the number of *fighting men*. Having previously determined that the male population of London numbered 199 thousand, and believing that those from 16 to 56 constitute 34% of the total population (he subtracted 6% of those who survived 56 from the 40% surviving 16), he calculated 34% of 199 thousand which equalled 70 [67,7] thousand . This was only 18% of the total population, a manifestly underestimated portion. His mistake directly resulted from his erroneous idea that it was possible to apply a mortality table for describing the age structure of the population.

Graunt's estimates of the population of London were probably not quite precise, but in any case they destroyed the then prevalent and absolutely wrong notion that the inhabitants numbered 2 *mln*. Only by the mid-19th century, or almost two hundred years later, did the London population reach this figure.

In concluding, we again emphasize that it was he rather than someone else who laid the foundation of population statistics. Almost everyone who studied his contribution agrees with this. Thus, the American demographer Dublin writes[14]:

Graunt was one of those remarkable men who stepped out of the sphere of their everyday duties to enrich the world by surprisingly new ideas and directions of study.

Willcox (1939, p. xiii) as though continues:

Graunt paved the way both for the subsequent discoveries of the uniformity of many social and volitional phenomena (for example, marriages, suicides and crime) and for the study of this uniformity, its nature and boundaries. Thus, he, more than any more man, was the founder of statistics.

4. Graunt and Petty

These names usually stand side by side. Both are deservedly called the founders of political arithmetic although not everyone shares this opinion. Some thought that Graunt, a London merchant lacking education, was unable to write such a remarkable book. And soon after his death a rumour went around that it was not Graunt at all who wrote the *Observations* but Petty, and that Petty had agreed, out of friendly feelings, that it be attributed to Graunt[15].

The rumour turned out to be persistent and gradually gained many supporters. The celebrated astronomer and political arithmetician Halley; the famous historian Macaulay; and many other authorities upheld the idea of Petty's authorship. This fact had compelled Hull to study the issue and later Greenwood and Willcox also considered it. All three agree in that the author of the *Observations* was Graunt and not Petty. We do not intend to dwell on this problem, but still desire to formulate several appropriate points.

No one denies that Petty was an exceptionally gifted person, not only an economist, a statistician and demographer, but also a physician, an anatomist, a musician, poet, political figure, businessman, inventor, teacher and mathematician. Given such a versatile talent, it could have been thought that he had also written the *Observations*. We are nevertheless convinced that such an assumption is groundless. Petty's descendants, entitled Lords Landsdownes, most vigorously come out in favour of his authorship. One of them writes, for example, that Graunt was

Possessed of all the virtues, a man of marked integrity, a good friend, an excellent haberdasher, but [...] *I can not believe that he wrote the* [...] *Observations*.

Petty's contemporaries who were charmed by his rare talent and spread the rumour of his authorship considered Graunt as *a dwarf mounted on an elephant*. Hull, however, justifiably indicates that Petty's friend Southwell, the author of this comparison, was not a very good judge of scientific work and was strongly impressed by Petty's ability to solve easily linear equations *in two variables*[16].

In turn, the advocates of Graunt's authorship put forward, in particular, the argument that it was not Petty's ambitious nature to give away a part of his fame to anyone, even to a friend. And we ought to add that it was not in Graunt's nature either to appropriate anyone else's fame. All authors agree that he was morally impeccable. That any career considerations were alien to him is clearly testified by his breaking away from the predominant religion and joining Socinianism, a doctrine upholding free will, liberty of conscience, spread of education, etc. Could such a man strike a vulgar bargain with his conscience and appropriate his friend's fame? We believe that he could not.

In addition, I point out that in 1660 – 1661, when the *Observations* were being written, Petty had been preoccupied with energetic social and political activities and had absolutely no time for calm scientific investigations. Graunt, however, was in a different situation. At that time, he was perfectly well provided for and had all the possibilities for studying the bills of mortality, which he received, during the long winter evenings.

This certainly does not mean that Petty had not participated in writing the *Observations* at all. It is quite possible that he helped Graunt with respect to some issues; Willcox even thinks that Petty suggested the very idea of a mortality table to Graunt. Petty's original mind certainly assisted Graunt in outlining some directions of his research, but, in spite of his help and participation, Graunt is still the author of the *Observations*.

5. Conclusion

Three centuries have passed since Graunt's *Observations* had appeared and during that time the world has changed beyond recognition. Social, economic and mathematical sciences essentially progressed, and demography has consequently developed its scientific tools and greatly enlarged out knowledge of the regularities of social life. In Graunt's time, the term *statistics* and *demography* were unknown. The first appeared in a book written by Achenwall 87 years later, in 1749 [who described the *so-called statistics*][17], and the second, after almost 200 years, in a contribution by Achille Guillard (1855).

Süssmilch justifiably compares Graunt with Columbus. But, whereas the latter never suspected that he had discovered a new continent, Graunt distinctly understood that he treated materials which before him had only been applied for unimportant and minor purposes. Nevertheless, he did not perceive the greatness of the science that he discovered, and in this respect he resembles Columbus who thought that the land he opened up was a part of Asia. The entire mankind should remember that exactly 300 years ago, on the banks of the Thames, in foggy London, the clear thought of John Graunt laid the first brick of the majestic building of modern population statistics.

Notes

1. Benjamin (1978) doubted whether Graunt had indeed assisted Petty on this occasion. O. S.

2. Hull (Graunt 1899, vol. 1, p. 432) quoted Graunt's Note of 1663:

There were […] *taken out of this pond* [some 870] *carps of about nine inches in length, some more, some less* […].

Both statements (about rearing carp and some statistical methods) are quite wrong. O. S.

3. Graunt also became a Socinian (a Unitarian), see § 4, a very special branch of Christianity. O. S.

4. *Dire poverty* and *his own house*? O. S.

5. I am not sure whether these second *Observations* are still unpublished. O. S.

6. It was quite impossible for Graunt *almost* to *feel* the law of large numbers. He believed in the stability of statistical ratios. O. S.

7. Graunt had to issue from christenings rather than from unregistered births. And, anyway (Graunt 1899, Chapter 3, § 44), in 1650 – 1660 *not half of the people of England were convinced of the need of Baptizing* newborn babies. In §§ 45 – 47 Graunt lists other reasons for people to abstain from that procedure. O. S.

8. The author should have chosen the very next lines: *how many are meer* [mere] *Voluptuaries* … O. S.

9. Hull (Graunt 1899, vol. 1, p. 349) referred to Graunt's Table on p. 406 in which the number 16,384 had appeared. O. S.

10. It seems that the proper reference should have been to Moses (*Psalms* 90:10). O. S.

11. In the mid-19th century, according to Greenwood's interpolations (1941 – 1943/1970, p. 78) based on Farr's data, the number of children dying before the age of six constituted 32% (Graunt, 36%), and he indicated that there were no good medical reason for holding that the conditions of child life in London had essentially changed during the two past centuries. Westergaard also agreed that Graunt had calculated the level of children mortality approximately correctly. B. U.

12. John (1884, p. 163) of course wrongly stated that Graunt had offered a rather correct picture of the order of extinction. B. U.

13. Huygens was the first to calculate the mean duration of life in accord with Graunt. Some modern authors (in particular, Dublin) justifiably consider his result as curiosity pure and simple. B. U. In 1669, in correspondence with his brother Lodewijk, Huygens considered the issue of mortality and, to say the least, was thus the first to apply probability beyond the field of games of chance (Sheynin 1977, pp. 247 – 249). O. S.

14. Both this and the next passage are translated back from Russian. O. S.

15. I (Sheynin 1977, p. 220n) noted Petty's phrase (1684, Address to Lord Brounker): *I have also* (*like the Author of the Observations*) *Dedicated this Discourse to* […] *the Duke of Newcastle*. In the same paper, I considered the work of Graunt and Petty and, on p. 219, quoted Petty's statements showing him as a philosopher of science, congenial in some respects with Leibniz, his junior contemporary. O. S.

16. The author based these last ten lines on Greenwood (1928, p. 86). O. S.
17. The word statistics first appeared in an Italian book of 1539 (Kendall 1960). O. S.

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The author included additional items. In turn, I am adding a subsequent contribution Glass (1964) with a detailed description of Graunt's life.

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IV

G. W. Leibniz

Some New Considerations about Human Life

Essay de quelques raisonnemens nouveau sur la vie humaine.
Manuscript 1680 – 1683, first published 1866.
In author's *Hauptschriften zur Versicherungs- und Finanzmathemetik*.
Berlin, 2000, with a German translation, pp. 428 – 445.
Editors, E. Knobloch, J.-M. Graf von der Schulenburg

[1] Application of This Research

This research can be essentially applied in politics; first, for estimating the power of a state and the number of its inhabitants by issuing from the number of deaths in the registers of deaths which are usually compiled at the end of each year; second, for estimating the mean life of a person; and finally for fixing a fair price of life annuities which are very beneficial for the state as the late Grand Pensionary De Witt explained in a contribution on that subject.

[2] On Indication and Its Estimation

Since all these considerations are based on reasonable indications (apparence), it should be first of all explained what is an indication and how should it be estimated. I say, however, that indication is just the degree of probability. For example, a die which is used in games has six quite equal faces and the indication of each is the same. This means that there is no reason to say that it falls rather on 1, or 2, …, or 6. However, throw two dice at once, and add the number of points on both. There will be a stronger indication that the sum is 7 rather than 12. Indeed, the former is thrice stronger than the latter since there is only one way to achieve 12 points, by throwing 6 and 6, but there are three equally possible ways to get 7 points, 6 and 1, 5 and 2, and 4 and 3[1].

[3] A Rule for Finding Mean Indications Which We Should Choose in Case of Uncertainty

When many indications are given and a mean indication is looked for to choose something in cases of uncertainty, here is how we should proceed. Suppose for example that it is required to estimate the value of some inheritance, a house or some other property. The custom of the peasants in Braunschweig – Lüneburg sanctioned by usage is to compose three groups of valuers (3 Schürzen, as they are called). Each consists of a certain number of men which agree on some value and declare it on common behalf. For example, the first group says that the value of a property is 80 écus, the second, 92, and the third, 98. For establishing a mean value [the arithmetic mean] is chosen[2].

That procedure, although of peasant origin, is based on demonstrative reasoning. Each group has the same authority […] and only [the arithmetic mean] should be chosen. Therefrom we formulate this rule: Having many equally possible indications, choose [the arithmetic mean].

[4] The Usual Boundary of Human Life Is 80 Years When Neglecting a Small Number of Those Who Overstep It

Concerning human life, I suppose, according to the Scripture and experience, that its usual maximal duration is 80[3] years. This means that people pass not more than 80 years but do not pass 81 years. Some people call this latter number the largest threshold since it is equal to 9 times 9. The small number of those who overstep that age should be neglected.

[5] We Disregard Particular Considerations But Apply Them in Special Cases

We should bear in mind that there are two kinds of considerations which can be applied for estimating [the duration of] human life. One is less certain, more particular and depends on experience. The other is more general, more proper for calculating and mostly depends on reasoning. Concerning the first kind, some believe that men are livelier than women; that more children die from smallpox and other diseases than youngsters and it can also be thought that proportionally more die in large cities than in the countryside, that the same is true for different professions, and that there are countries where people ordinarily live up to 100 years or more.

However, since these particular considerations are too discrepant, we disregard all of them apart from instances in which we need to apply our general considerations to some special case[4].

[6] A Fundamental Premise: 81 Newborn Babies Die Out Uniformly: For 81 Years, One of Them Dies Each Year

And so, neglecting the robustness, sex, profession, nation and other circumstances which can be added here if needed, and considering in general that all people are equally lively and that all the years of human life are equally fatal for human nature, – here is how we should proceed.

Consider 81 recently born babies and assume that all of them must die during the following 81 years since this is what we have presumed. And since we also have supposed that all the years of human life are equally fatal, they die out during that period in a uniform manner, that is, one of them dies each year. Finally, since we have premised that all of them were equally lively, it will be as though by drawing lots that one of them will be the first, the second, … to die since all of them have the same indication of dying.

[7] A Rigorous Demonstration That the Mean Duration of Human Life Is 40 Years and That a Life Annuity Bought for a Recently Born Baby[5] Should Be Estimated As a Pension for 40 Years

Now we will easily determine the mean duration of human life. For any of those babies in its particular case there are as much indications to say that it will die during its first, its second, its third year, as there are for any other year until the 81st. If it dies during its first year it will not reach any year; their number is zero. If it dies during its second year, it achieves one year etc since we disregard the fractions or parts of years. Finally, if it dies during its 81st year, its age or the number of its years is 80. And so, we have 81 possible ages or estimations equally indicative of human life, i. e., the years 0, 1, 2, …, 80.

For calculating the mean estimation we should [choose the arithmetic mean] which is 40. We can therefore say that 40 years is the duration of mean human life. It follows that a life annuity for a recently born baby should be considered as a temporary pension for 40 years after which it expires. We can thus estimate the present value of a pension, that is, estimate for how much can it be bought at present when allowing for the rebate which I had described elsewhere and do not repeat here.

[8] The Rule for Finding the Mean and Presumed Life for a Person of a Certain Age To Remain Probably Living[6] and Therefore the Value of a Life Annuity Which He Buys

The same way that we have determined the mean future or presumed life of a newborn baby we can also calculate it for another person of any age. For example, a one year old baby can live either zero years (if it dies before reaching the [end of the present] year of his life) or 1, 2, …, 79 years. There are therefore 80 equally reasonable estimators of its remaining life and [the arithmetic mean] is 39 + 1/2 or rather (neglecting the fraction) 39 years for an infant who reached one year. The same way a child who reached 10 years still has 0, or 1, or 2, …, or 70 years to live and [the arithmetic mean] is 35.

[9] A Shorter Rule for Determining the Same

It is rather tiresome to calculate the sums of all those numbers, so here is a very short rule providing the same result. It is required to find how long a child of 10 years will probably live; that is, to find the mean duration of his remaining life. In a few words, 10 years which it reached is subtracted from 80 thus obtaining the maximal remaining life. Take its half, 35, which will be the required number.

Here is the proof of this rule by issuing from the preceding calculation. It was required to find the sum of all the numbers taken in their natural order from 1 to 70 and divide it by 71. But that sum is a half of the number which occurs when 70 is multiplied by 71 (and the sum of the numbers from 1 to 79 is half of 79 multiplied by 80 and the sum of the numbers from 1 to 12 is a half of 12 multiplied by 13 etc which is easy to check). And that sum, a half of 70 multiplied by 71 should be divided by 71. Multiplication and division by the same number 71 destroy [cancel] each other so that only a half of 70, i. e. 35, remains.

We can therefore reasonably suppose that a child who reached 10 years will live 35 years more and a life annuity bought for it should be estimated as a temporary pension for 35 years. For a young man of 20 years it is 30 years and for men aged 30, 40, 50, 60 and 70 years, 25, 20, 15, 10 and 5 years respectively.

[Leibniz appended a table showing that the probably remaining years of life are 40(1/2)37 1/2 and 35(2 1/2)0 for ages 0(1)5 and 10(5)80 years.]

[10] Proportion of People Dying at Each Age. For Example, It Can Be Judged That There Will Die Almost 1/36 of Those Reaching 45 Years

Let us turn to the number of people. We have established that 81 recently born babies will die uniformly for the next 81 year or that one will die each year until all of them die out. It follows that out of those 81 babies who did not reach one year, one dies during that year. Next year there will only be 80, each one year old, out of which one more dies, then only 79 are left, each two years old out of which one more dies etc.

The same happens if we have many groups of 81 people , i. e., an arbitrary number of groups. Obviously each year there will die 1/81 part of infants not yet reaching one year of age, 1/80 of those who reached one year, 1/79 of those who reached two years etc, 1/71 of 10 years old children, 1/61 of people 20 years old and, generally, when subtracting the age, for example, 30, from 81, the difference will be 51, so that 1/51 of those aged 30 will die and finally those aged 80 will all die during a year as stipulated by our hypothesis.

[Leibniz appended a table showing that from those aged 0, 1, 2, 5, 10, 15, …, 75, 80 years, one out of 81, one out of 80, 1/79, 1/76, 1/71, 1/66, …, 1/6, 1/1 will die during a year.]

Therefore, if you know the number of people of some age, for example, of those 50 years old, divide it by the denominator of the corresponding part, by 31. This means that out of 10,000 men aged 50 next year there will die 322. And the larger the number, the less notable will be the error other things being equal.

[11] Supposition: Taken in a Multitude, the General Number of People, and Even of People of Each Age Remains Almost the Same As in the Previous Year

We can introduce one more assumption, namely, that human fecundity is always the same and equals mortality so that the number of people remains almost the same and even that this year there will be the same number of children aged 1 year, 2 years, 10 years, of people aged 20, 30 etc years as there were a year ago. We therefore see that the multitude of people only changes notably due to some particular and extraordinary accidents but that at least from year to year the difference is not really sensible.

I recognize that, according to the natural conditions, people will always rapidly multiply for compensating a large number of countries not yet sufficiently cultivated[7], but people ruin themselves in so many ways by their disorder apart from visitations of widespread diseases so that their number does not much increase.

[12] Reasonable Proportions of the Number of Living of Each Age. For Example, Out of 3321 People about 2 Are 20 Years Old for 1 of 50 Years

Here is this proportion. There is 1 person of 80 years, 2 of 79 years, 3 of 78, …, 32 of 50 years, 41 of 40, 51 of 30, 61 of 20, 66 of 15, 71 of 10, 76 of 5, 79 of 2, 80 babies 1 year old, and 81 recently born who did not yet reach 1 year.

The number of people in general and of each age in particular can only subsist if there exists this proportion coupled with the proposition established above about ages at death. This year therefore one of each age will die so that from the 2 living at age 79 one will reach 80. […] From the 3 aged 78 two will reach 79 […]. However, 81 babies will be born for replacing those 81 who die […] and thus the same number of people of each age subsists forever.

[13] It Follows That Almost the Same Number of People Die

in Each Age. For Example, This Year Die 100 People Aged 20 And the Same Number of People Aged 50 Will Die Excepting Some Particular Cases Such As Those Concerning Little Children

Therefore, if a 100 of ten-year-old children die, 100 people of 20 years, 100 of 30 years, and in general the same number of each age will also die. This should not be surprising because although old men are naturally more inclined to die their number is proportionally smaller since many young men die on the way to old age. And an unequal number of deaths of young and therefore vigorous people and old men can only occur when young and old are equally numerous.

However, the number of young men is larger as much as their liveliness is so that one compensates the other and there are as many deaths among the few old men and among the multitude of the young. All this also conforms to the supposition made above that, as explained, all the years of life are equally fatal for human nature. We may certainly make many exceptions since usually there die much more little and therefore weak children than people of other ages.

Apart from such particulars, we should nevertheless consider that ordinarily there are more than 81 baptisms for each 3321 [= 81·41] people, or more than one for 41 people but the surplus of baptisms and deaths can be disregarded: if more are born than I have proposed, then also more than I proposed will be mowed down, and it is not necessary to consider them.

[14] Each Year Dies about a Fortieth Part of People. About As Many Should Be Born and Perhaps a Little More To Maintain the Number of People

We can conclude from the above that each year there dies about 1/40 of those living since for one man alive at 80 years there are two of 79, three of 78, …, 79 of 2 years old, 80 of one year and 81 recently born babies. Their sum, 1 + 2 + 3 + … + 79 + 80 + 81 = 3321. One person of each age dies so that 81 die or 81 out of 3321 […] or one out of 41. In other words, about a fortieth part of people dies each year. Although derived a priori and only by reasoning, it quite conforms to experience.

Indeed, as was remarked, in large cities and in somewhat unhealthy places there dies a thirtieth part, but in some places with best air only a fiftieth part. We may assume a reasonable mean, i. e., 1/40[8]. And that number is the same as the mean duration of human life, as 40 years according to our demonstration.

We can also remark concerning the proportion of men of each age as established above that the number of young men aged 20 years is almost twice larger than those of 50 and it is easy to compare similarly the other indicated numbers.

[15] Nine, Or Ten Times More Children Can Naturally Be Born Than Born At Present[9]

When supposing also that there are as many women as men, we may estimate how many women are there of 15 – 44 years of age, of childbearing age. They amount to 705 out of 3321 women, a proportion not very different from 3:10 as was established by the London register of deaths. We see now that barely 1/10 or 1/9 of those women become pregnant each year since these 870 women barely have 80, 90 or 100 children yearly. Polygamy is not a proper solution for multiplication apart from countries in which the number of women greatly exceeds the number of men. Such countries, however, possibly do not exist in Europe.

Notes

1. There are 6 possible ways rather than 3; for example, not only 6 and 1, but 1 and 6 as well.

2. Leibniz once more referred to this practice in his book (1765/1961, p. 515).

3. See Psalm 90:10: *Our days may come to seventy years or eighty if our strength endures*. Note also that Leibniz, perhaps being carried away by deductive reasoning, had disregarded both Graunt's classical contribution and De Witt's suppositions and conclusions although he referred to the latter in § 1. In § 15 Leibniz mentioned the London register of deaths but not definitely enough.

4. It is too difficult thus to separate considerations, and for example, contrary to Leibniz (see his *fundamental premise* in § 6 and other cases), infant mortality should have been considered as one of the former.

5. Hardly anyone had ever bought life annuities for recently born babies!

6. Leibniz had not introduced probable life as Huygens did in his correspondence of 1669.

7. This statement is doubtful, cf. Laplace's pertinent pronouncement in [xv].

8. That mean was hardly reasonable. Leibniz should have said that, lacking information, he assumed a fortieth.

9. This conclusion is only valid if *Naturellement* in the original French title is understood as being theoretical. Practically speaking, Leibniz' conclusion is useless. The number 870 which appears at the end of this section is a mystery. Then, 790 corresponds to § 12, but 705:3321 = 0.21 (and 870:3321 = 0.26) which is not near enough to 3:10.

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V

J. P. Süssmilch

The Divine Order of the Changes of the Human Species As Demonstrated by Births, Deaths and Propagation, Introduction

Die Göttliche Ordnung in den Veränderungen des menschlichen Geschlechts aus der Geburt, dem Tode, und der Fortpflanzung erwiesen, Vorrede[1]

Benevolent and impartial reader,

This spring it will exactly be 20 years since I had for the first time ventured to give the printer my considerations about the order of the divine wisdom and goodness clearly shown in births, propagation and deaths of people. I was brought to this work by tracking with greatest pleasure and admiration the Divine Providence and by perfecting the rules formulated, repeated and confirmed by Graunt, Petty, King, Arbuthnot, Derham, Nieuwentyt and others.

I dared to go further than my predecessors by availing myself with the registers of the Royal Prussian provinces. I even had to get involved in various political considerations by applying the rules of the wisest divine order to human behaviour. All this frightened me especially since it was impossible to abide unerringly in such a scarcely inhabited country.

However, the public shamed the mistrust in my conclusions expressed in scientific journals and I was also welcomed from beyond Germany, from Holland, England, Switzerland, Denmark and Sweden. This reassured me to collect gradually new testimonies and registers concerning the formulated rules. And in a few years after that the Royal Academy of Sciences [in Berlin] honourably admitted me to its membership without my applying for it[2]. Its late President Maupertuis encouraged me to choose my deliberations as a subject of academic memoirs and thus gradually to perfect, and make them certain.

That was a good advice. I followed it and very often read out lectures about those matters at the Academy. Then, I have obtained the works of Kerseboom, the materials of the incessantly diligent Struyck, the comments of Short as well as the fine work of Deparcieux and finally the brief exposition of all those considerations compiled by the skilful Wargentin in Sweden, read out by him at the Swedish Academy and enriched by his own elegant demonstrations.

This ever more made me capable of filling in the gaps in my work, of correcting mistakes and perfecting the main rules by raising the probability of my assumptions as high as it was possible in such matters. And after that I ventured to obtain with great effort not only all our possible provincial registers but even to ask the superintendant [high-ranking clergyman] and preachers of our land for assistance and most of them met my request. Thus I became able to extend essentially the first edition of my book. It was sold out and, moreover, many foreign scientists had requested me to put out a new edition. And so I resolved to comply but had no time for completely remaking my work although understanding that that ought to be done.

My professional duties had left me too little time whereas much should have been recasted. Finally I decided to busy myself with this work in those often mournful hours of this war[3] and thus to fill up the short periods of my idle time. I began working but had soon encountered difficulties since coherence of ideas was essentially required. Meanwhile I continued my work but often had to abandon it for weeks on end and I praise the divine goodness since in spite of these hindrances after three years of protracted work I was finally able to give its first part to the printer and thus to satisfy the desire of so many patrons of my work.

And indeed because of these circumstances I may hopefully require and permit myself to request a just verdict from the friends of these considerations. I understand that this second edition still can not be free from error since this kind of work does not tolerate faultlessness. Much effort and many new materials are still needed. Although I provided more than was contained in either my first edition or accomplished by anyone else, many section are still incomplete. The portion discussing the order of propagation of both sexes seems to be almost adequately based on registers, but many oriental materials are nevertheless lacking the more so with regard to their agreement with western documents which I have no sufficient reason for doubting.

The splendid order of the ages at death still especially demands important additional studies. We have almost enough registers from cities, but too little from small towns and still less from the countryside. I was only able to collect a small number of registers from rural parishes. Preachers working there can usefully contribute to this task and I am asking those who enjoy this work to collect materials and benevolently send them to me or after my death to scientific monthlies. Since I will undoubtedly make avoidable mistakes I request my readers to treat them without leniency but rather indicate them graciously. I will correct them some time or other.

My highly esteemed friend and colleague, Professor Euler, the worthiest Director of the mathematical class of the [Berlin] Royal Academy of Sciences, had most generously assisted me with calculations of the doubling of population[4] and in addition had most graciously undertaken the proof-reading. His manifested satisfaction and friendly but impartial verdict somewhat calmed me; meanwhile, however, I am asking my readers to inform me about noticed mistakes. If something is doubtful, I will really try to explain it.

A few years ago I objected to Justi[5] about the application of the laws of mortality and this will hopefully indicate that I am merely attempting to establish verity. Perhaps some readers, as it had already occurred, after glancing at this revision will however even stronger decide that I have been too much engaged in political considerations.

But can I really be accused of sinning since I do not suppress the truths which are necessarily connected with considerations about the order in the divine wisdom? Is it unbecoming for a theologian who I am that I attempted to derive the true politics and cleverness in the art of governing from the first basic law and command of the Creator, *Be fruitful and increase in number, fill the earth and subdue it* [Genesis 1:28], and indicated that no Regent can happily reign without always having the divine law before his eyes and reasonably following it? Can it be misinterpreted that in morality I have discovered new grounds for the real wisdom of Regents? And I have attempted to show that they should never abandon morality and good customs so that the population will not decrease since otherwise the divine commandment will be contradicted and at the same time the security, power, and happiness of the state and its subjects will weaken and its richness will diminish[6].

That I have attempted to save the Christian religion from the new and dangerous charges of Montesquieu[7] who enjoys a high status because of his erudition and wit and to reveal their groundlessness? Should not a theologian know what is going on around him in the world? Should I have neglected all this, and, as some quick-tempered, unjust and uninvited judges have decided, should not have I ventured in many respects too far? I ought to tell such malicious, envious and arrogant minds to their faces that I will listen to their verdict with all contempt it deserves and that I will be very glad if they leave my book unread.

I am sure that the robustness of the foundation which underlies that order of nature encouraged me about twenty years ago to begin this work, the necessary rescue and explanation of the all-important and all-consoling doctrine of divine government of the world. I have always kept this goal before my eyes and it especially relieved all my efforts. I therefore desire that God blesses these deliberations with some advantage for His glory and the wellbeing of the human society. The infinitely wise and good Creator, award me this favour!

Cölln[8] on the Spree, 30 March 1761

Notes

1. Translated from the edition of 1775 which was a reprint of the second edition of 1765, see Bibliography.

2. Süssmilch was elected mostly owing to his work in linguistics as a member of the Academy's class of philology.

3. He became a chaplain, thus his *professional duties*. Just below, Süssmilch mentioned the First (Prussian – Austrian) Silesian war.

4. Indeed, Euler actively participated in preparing the second edition of the *Divine Order* and was coauthor of at least one of its chapters (*On the rate of increase and doubling of population*). One of the authors' conclusion, viz., that population increases, roughly, in a geometric progression, was picked up by Malthus and is still adopted (with reservations).

5. This learned man had especially declared in *Göttingische* [*gelehrte*] *Anzeigen* that mortality in large and densely populated cities is weaker than among countrymen, that barely 1/60 dies [yearly]. He based this conclusion on the population of Vienna mostly consisting of servants, coachmen, lackeys, maidservants, travelling journeymen and the like going to and fro and therefore [allegedly] not to be considered permanent inhabitants. Moreover, they are engaged at ages in which the vital capacity is greatest and mortality least.

He desired to confirm that by the multitude of the inhabitants of Vienna which is much larger than it should have been according to the rules of mortality adopted by me and others. I attempted to prove the groundlessness of this application in a message printed in 1756 and sent to Justi. He did not, however, answer either in writing or in a printed form, or, later, when I had the honour to meet him personally. I understood this complete silence as an agreement with my arguments and my interpretation is the reason why I had not asked him about it. And neither did I therefore wish to mention this petty dispute in the new edition of my book since I thought that it was over.

However, when the printing of this first part was all but completed, I came across his excellent work on national economy published a year ago [see Bibliography] and saw there that my interpretation was wrong. He wholly retained his initial opinion, even without any reservations or proof which I had, however, convincingly asked. I can not hope, therefore, that a repeated persuasion can be more effective and leave it at that and I only wish to impart some misgivings.

If in a large city the total number of servants of both sex amounts to 50,000, they do not live family lives and many of them come and go so that the departed are always made up. As long as the families of the noble and the rich remain in the state of prosperity and luxury this number of servants should therefore be considered as a permanent crowd. And they, the servants, must yearly surrender their share to death whether they were born in the city or not.

Some of them are in their best ages since still being able to serve and their death rate is certainly lower, but they still ought to give away their definite contribution. Owing to the disorderly way of life of menials in large cities it is usually heavier than otherwise. In a table appended to the message mentioned above, I indicated the calculated rate of mortality in cities and towns. It showed that in large cities one person out of 96 aged 20 – 29, one out of 57, 43, 30 and 20 aged 30 – 39, 40 – 49, 50 – 59 and 60 – 69 must pay the debt to nature.

And servants of either sex so little differ [in this respect] from the masters and their wives as though they live wherever they wish. Death makes no exception and demands a definite part as determined by the Creator. It seems quite clear to me that in large cities the servants must also resign themselves to the law of mortality. If only we do not state that the servant, exactly when death wishes to mow him down, leaves the city to be entered in a death register elsewhere, and, again, that the vacant job is taken up by a quite healthy and vigorous servant who will only remain there until feeling the fear of death.

However, I stop here and only obligingly thank Justi for the benevolent and polite mention of my considerations about this issue. J. P. S.

It can be safely thought that in their old age servants really attempt to return to their former home ground. However, as a whole, the opinion of Süssmilch seems likely. On the life and work of Justi see Bachhaus (2008).

6. Multiplication of mankind was therefore a divine commandment and Süssmilch believed that Regents must foster marriages and take care of their subjects, condemned wars and excessive luxury, declared that the welfare of the poor was to the advantage of the state and in the self-interest of the rich. His pertinent appeals brought him into continual strife with municipal (Berlin) authorities and ministers of the state (Prussia).

7. Montesquieu is known to have denied divine providence and believed that protestanism suites republics the best, catolicism, monarchies, and Islam, despotisms. In the eyes of Süssmilch and like-minded men he was a great heretic.

8. The present Neukölln, a district of Berlin.

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VI

N. I. Idelson

Introduction: The History of the Problem

N. I. Idelson, *Sposob Naimenshikh Kvadratov i Teoria Matematicheskoi Obrabotki Nabluideniy* (MLSq and the Theory of Mathematical Treatment of Observations). Moscow, 1947, pp. 7 – 17

1. Before the method of least squres (MLSq) was justified, observational data had been usually combined so as to obtain from them the best approximate value of the unknown in the following way. Suppose that the observations provided magnitudes l_i for the unknown sought, X, multiplied by given coefficients a_i. In other words, the problem consisted in combining n necessarily approximate magnitudes of the form

$$a_i x \approx l_i. \quad (1)$$

According to the old method, it was required to change the signs in these equations[1] so that the coefficients will be positive and then to assume that

$$X = \sum l_i / \sum a_i. \quad (2)$$

It was Cotes, 1682 – 1716 (1722) who substantiated this method. Each observed l_i is corrupted by an unavoidable random error ε_i; its influence on the sought value, X, will evidently be the less the larger is the positive coefficient a_i. Cotes therefore considered a_i as the *mass* or the *weight* of the corresponding observation providing the approximate value l_i/a_i for the unknown. He marked off these separate values on a straight line beginning at a common origin and placed masses a_i at the end points of the appropriate segments. By definition, the resulting value of the unknown was assumed to be the centre of gravity of all the masses a_i. Multiplying each abscissa by a_i and dividing the product by the sum of the masses, Cotes indeed obtained the expression (2)[2].

Laplace called this the usual method and contrasted it with the new MLSq. However, we shall even see the formula (2) considerably later, when expounding the Cauchy method. The main difficulty of applying the usual method was encountered when passing to systems of approximate equalities of the type of (1) with many unknowns. The Cotes reasoning was evidently unable to lead to intelligible rules for combining the observed magnitudes l_i when their number exceeded that of the unknowns.

However, considerably long series of planetary observations had been already compiled in astronomy in the 18th century, and no one actually knew what to do with them when extracting from this vast material corrections for only six elements of the elliptical movements of each of the main planets. Similar problems had been encountered in geodesy after the great French expeditions to Lapland and Peru for measuring a degree of arcs of the meridian had returned home.

2. In 1806 [in 1805] Legendre, 1752 – 1833 (1805, pp. 72 – 80) offered a simple and elegant solution of the problem. On a few pages which can serve as a specimen of completeness and clearness[3] he provided practical workers with the method of combining observations preserved without any changes to this day. Legendre says that when having to extract from observations results as precise as possible, the matter is usually reduced to solving a system of linear equations whose number exceeds that of the unknowns. The observed values do not exactly satisfy these equations and non-removable errors remain in the free terms. Some arbitrariness in distributing the errors among separate unknowns is in such cases unavoidable. Legendre wrote[4]: […]

He then shows how it is possible to obtain in each problem the same number of *final equations* as there are unknowns. Indeed, this possibility does away with the main shortcoming of the usual method. For systems with one unknown of the type of (1) Legendre's solution constructed in accord with his principle

$$\sum(a_iX - l_i)^2 = \min$$

immediately led to the value

$$X = [al]/[aa]$$

where, in general [in Gauss' notation], $[ab] = a_1b_1 + a_2b_2 + \ldots + a_nb_n$. It was of course possible to derive the expression (3) in accord with Cotes, if, however, assuming that his *masses* were proportional to a_i^2. If all the coefficients are unities, solutions (2) and (3) coincide providing one and the same value of the unknown equal to the mean of all the observed magnitudes l_i.

3. A few years after the appearance of Legendre's memoir, Gauss, 1777 – 1855 (1809) developed its algebraic aspect to high perfection. Legendre's final, or, as they are now usually called, normal equations, are symmetrical. The coefficients of the unknowns in the rows coincide with the corresponding coefficients in the columns; the determinant of the system is positive (we exclude, once and for all, those special cases in which it can vanish).

The solution of such systems of linear equations by consecutively eliminating the unknowns is equivalent to transforming some quadratic function of the unknowns to its canonical form in which it only contains the squares but not the products of the unknowns. The essence of the Gauss algorithm (of the rule for performing the consecutive operations) indeed consists in such a transformation of a quadratic function corresponding to the Legendre system of normal equations and serving as an expression for the sum of the squares of the errors brought to its minimal value. While working out this algorithm Gauss introduced notation which is being applied to this day and became an integral part of any exposition of the MLSq, see […].

4. Gauss' *Theoria motus* appeared in 1809. There, he offered methods for determining the six elements of the elliptical motion when three or more observations were available. For that matter, his methods were only insignificantly modified during the further development of the science and technique of calculations.

An entire section of this book, *Determination of an Orbit Corresponding as Precisely As Possible to an Arbitrary Large Number of Observations* (§§ 172 – 189), concerned the very problem which, as mentioned above, was so important for astronomy and became especially hot after the discovery, at the beginning of the 19th century, of the first minor planets.

There, Gauss offered a probability-theoretic justification of the MLSq. Its essence, when restricting the exposition to systems in one unknown of the type of (1), consisted in the following. The best approximate value of the unknown is indeed the value (3) obtained by the Legendre rule. Best because it possesses a higher probability than any other linear combination of the observed values.

After introducing this new condition about the highest probability, and under some other assumptions, Gauss established, first of all, that the appearance in each observation of a random error whose value was obtained within an infinitely small interval $[\varepsilon, \varepsilon + d\varepsilon]$ is proportional to the product of the differential $d\varepsilon$ by the function

$$f(\varepsilon) = (h/\sqrt{\pi})\exp(-h^2\varepsilon^2) \quad (4)$$

where h is a constant, also of a stochastic origin, which Gauss called the measure of precision of the given series of observations (assumed to be of equal precision).

However, Gauss showed (and this was the most important part of his discovery) the following. Let u be the resulting random error of determining X in accord with the Legendre rule, i. e., of the value (3). The probability that the value of this error is contained within the interval $[u, u + du]$ is equal to the product of du by the function of the same type as (4), – by

$$f_1(u) = (H/\sqrt{\pi})\exp(-H^2u^2),\ H = h\sqrt{[aa]}. \quad (5a, b)$$

The constant H is called the measure of precision of the resulting value X, and $[aa]$, its weight. The measure of precision of the result thus increases by $\sqrt{[aa]}$ as compared with that of the separate observations, and it is not difficult to show that this increase is the largest possible for any linear combination of given a_i. On the other hand, the function $f_1(u)$ evidently reaches its maximal value at $u = 0$, that is, when X is assumed to be indeed equal to the Legendre value (3). In addition, this maximal value

$$f_1(0) = (H/\sqrt{\pi}) = h\sqrt{[aa]}/\sqrt{\pi} \quad (6)$$

is proportional to H. It therefore occurs (if assuming Gauss' additional conditions) that the MLSq leads to such a value of the unknown which at the same time possesses both maximal weight and maximal probability (the Gauss theorem; see its detailed proof in my main text).

If all the coefficients a_i are unities, the measure of precision of the most probable value, which is here the mean of the observed l_i, increases $\sqrt{n}$ times as compared with that of each separate observation. This naturally attaches a new meaning to the rule of the arithmetic mean.

In those days Gauss found the philosophical, or, as he wrote later, the *metaphysical* justification of the requirement of maximal probability of the result (which is of course absolutely hypothetic and prior [not based on experience]) in an axiom according to which exactly the arithmetic mean of equally precise observations made under the same conditions is always assumed as the most probable value of the unknown. True, we saw that it was introduced both in the old method due to Cotes and in the new Legendre method. But (Poincaré 1896/1912, p. 185),

To say that this rule is admitted by all the world does not mean justifying it, because all the world perhaps does not sufficiently imagine what is a law of error.

5. After explicating his stochastic solution, Gauss (1809, § 186) stated that the rule about the minimal value of the sum of the squares of the differences between the observed and the calculated values of the unknowns can also be derived from more simple considerations unconnected with the theory of probability. And here he in essence only somewhat developed and modified the known to us Legendre's reasoning. And Gauss also included the following phrase:

On the other hand, our principle, which we have made use of since the year 1795, has lately been described by Legendre in […] *where several other properties of this principle were explained. They have been here omitted for the sake of brevity*[5].

There is no doubt at all that Gauss had indeed knew the MLSq from the age of 18 (from 1795); documents and correspondence convince of that. Moreover, when receiving Legendre's book, Gauss wrote to Olbers 30 July 1806 (W-8, p. 139):

It seems to be my fate to compete with Legendre in almost all my theoretical work. So it is in the higher arithmetic, in the researches on transcendental functions connected with the rectification of the ellipse, in the fundamentals of geometry, and now here again. Thus, for example, the principle I have used since 1794, that the sum of squares must be minimized for the best representation of several magnitudes which can not be given exactly, is also used in Legendre's work and is most thoroughly developed.

At the same time, however, it can not be doubted that Gauss' words [*our principle*] should have greatly pained Legendre. The latter had indeed frankly wrote Gauss about it[6]. The situation became somewhat complicated because already in 1803 Gauss publicly used the same phrase, also concerning Legendre, with respect to a very important theorem from the theory of numbers. All this could have aroused certain rumours in European academies and especially in Paris. Thus, Laplace, when sending in 1810 his just appeared memoirs on the theory of probability[7] to Gauss (28 years his junior), wrote in a covering letter of 1811 (W-10, p. 380):

In his work on elliptical movement M. Gauss says that he was conversant with it [with the MLSq] *before M. Le Gendre has published it, I would greatly like to know whether before this publication anything was printed in Germany concerning this method and I request M. Gauss to have the kindness to inform me about it.*

In the concluding part of his detailed answer, which is very valuable for us, Gauss referred to several astronomers and informed Laplace that he had applied this method from 1795; that among his papers was a note of 1798 where he had written about its approach to the theory of probability; that he had applied it especially often *from the year 1802* and since then used it, *as might be said, every day in my* [in his] *astronomical calculations on the new planets*. […] Gauss thus ended his letter:

I had no idea that Mr. Legendre would have been capable of attaching so much value to an idea so simple that, rather than being astonished that it had not been thought of a hundred years ago, he should feel annoyed at my saying that I have used it before he did[8].

6. The first edition of Laplace's, 1749 – 1827, *Théorie analytique des probabilités* appeared in 1812[9]. This immense volume is as though a synthesis of all his work on the theory of probability. The immediate cause for compiling this treatise was however his fundamental discovery which he only made about 1808 – 1809 [published in 1809]. It is interesting to hear what the French mathematician Bienaymé (1853, pp. 311 – 312) said about this:

Aussi Laplace avait-il senti sur-le-champ l'importance de sa découverte. A peine l'eut-il faite, qu'il l'apporte devant cette compagnie, et qu'il annonce qu'il va publier un Traité des probabilités. De 1770 à 1809, pendant près de quarante ans, Laplace avait donné des Mémoires nombreux sur les probabilités; mais, quelque intérêt qu'il y eût dans ces Mémoires, il n'avait pas voulu les rédiger en théorie générale. Aussitôt qu'il a reconnu la propriété des fonctions de probabilités, il voit clairement que c'est un principe qui régit presque toutes les applications, et il compose sa théorie.

[*Laplace had indeed discerned at once the importance of his discovery, immediately announced it* [to the Paris Academy of Sciences] *and stated that he will publish a treatise on probabilities*[10]. *From 1770 to 1809, during almost 40 years, Laplace had been presenting numerous memoirs on probability, but whatever was their importance, he did not wish to combine them into a general theory. But as soon as he established the properties of the functions of probabilities, he clearly saw that it was the principle governing almost all applications and compiled its theory*.]

I explicate the essence of Laplace's discovery in the main text. With respect to the best combination of approximate equalities of the type of (1) his result consisted in the following. Assume that the best approximate value of the unknown x_0 is a linear function of the observed value l_i so that

$$x_0 = \alpha_1 l_1 + \alpha_2 l_2 + \ldots + \alpha_n l_n \qquad (7)$$

where $\alpha_1, \alpha_2, \ldots, \alpha_n$ are the yet undefined constant coefficients.

Without introducing any suppositions about the type of the function $f(\varepsilon)$ which determined the probability of a random error of the separate observations, and only assuming that the number of the observations increases indefinitely, we may state (under some additional conditions imposed on the coefficients α_i) that, The probability P_n that the random error of the expression (7) is confined between 0 and any arbitrary u tends to the limit

$$P_{n\to\infty} = \frac{K}{\sqrt{\pi}}\int_0^u \exp(-K^2u^2)du, \; K = \frac{h}{\sqrt{[aa]}} \qquad (8; 9)$$

where h is, as it was previously, the measure of precision of the given series of observations. This indeed is the Laplace *limit theorem* as applied to the theory of combining the equalities (1). See a detailed exposition in the main text.

The integral in (8) increases with K. Namely, if, after assigning an arbitrary value u, we demand that the limiting probability P takes the largest possible value, it will be necessary to determine the coefficients α_i by the condition $K = \max$, see main text.

Taking now into account that those coefficients are connected by the condition $[a\alpha] = 1$, which is easily derived from the totality of the equations (1) and (7), and determining the relative maximal value of K in accord with well-known rules, we find, first of all, that

$$\alpha_i = a_i/[aa]. \qquad (10)$$

Then, substituting these α_i in (9), we get

$$K_{\max} = h\sqrt{[aa]}\,. \qquad (11)$$

Comparing this with (5b), we see that the maximal value of Laplace's K equals Gauss' H which is, as I said above, the maximal possible. In addition, when substituting the expressions α_i in (7), this general linear expression for X as a function of α_i takes the Legendre's form (3).

Thus, deriving the absolute maximum of the limiting integral at any value of u, Laplace got a new stochastic justification of the MLSq; and we ought to emphasize as strongly as possible the difference between the fundamental notions assumed by Gauss and Laplace: Gauss thought about the maximal probability of some given value of X (which corresponded to the value $u = 0$) whereas Laplace required the maximal probability that X is confined between the boundaries $[x_0, x_0 + u]$ with u remaining arbitrary.

Especially important is the independence of Laplace's result from the law of distribution of the separate observations. He thus opened up a new era in mathematical statistics, in the theory of combining observations etc. And he (1812/1886, p. 354) indeed stressed this fact:

Mais, si l'on considère un grand nombre d'observations, ce qui a lieu le plus souvent dans les recherches astronomiques, ce choix devient indépendant de cette ici, et l'en a vu, dans ce qui précède, que *l'Analyse conduit alors directement aux résultats de la méthode des moindres carrés des erreurs des observations*.

[*However, when considering a large number of observations, which most often takes place in astronomical researches, that choice* [of the normal equations] *becomes independent from that law, and the previous shows that the analysis directly leads to the results of the MLSq of the observational errors*.][11]

At the same time, the *mysterious connection* (Bienaymé 1853, p. 313) between

$$f_2(u) = (K/\sqrt{\pi})\exp(-K^2u^2), \tag{12}$$

see formula (8), and the Gauss function $f_1(u)$, see (5a), is surprising: we see that they simply coincide when the constants K and H are chosen appropriately. The causes of this fact really surprise us when taking into account the fundamental difference between the main suppositions made by Gauss and Laplace. They lie in the deep and exceptional properties of the Gauss – Laplace law. These properties correspond to the so-called *Laplace – Chebyshev* limit theorem [the central limit theorem] and they were only recently completely revealed, see main text.

7. In one of his early memoirs Laplace (1774) turned his attention to an a priori possible form of expressing the probability of a random error. He assumed that it is equal to the product of $d\varepsilon$ by the function

$$f_3(u) = (k^2/2)\exp[-k^2|\varepsilon - \varepsilon'|] \tag{13}$$

where k^2 was a constant and $|\varepsilon - \varepsilon'|$, the numerical value of the difference between the given value of ε and the median of these values. In accord with (13) the maximal probability is possessed exactly by the value of ε coinciding with the median.

Since the first mention of functions of the type of $\exp(-h^2\varepsilon^2)$ only occurs in one of Laplace's later memoirs (1781/1893, p. 383), suggestions were made at present to call the law expressed by the formula (13) the Laplace first law, and that, described by (4), his second law. In spite of some obvious difficulties encountered in its application (the median of a sum is not equal to the sum of the medians etc.), the *first law* recently again attracted attention (Fréchet 1928; 1935).

8. Beginning at least in 1819, as is seen from his correspondence, Gauss became dissatisfied both with his own justification resulting from the prior requirement of maximal probability and with Laplace's substantiation assuming an unbounded increase in the number of observations. During the next decade he created an absolutely new method of combining observations.

His *large memoir* (1823 – 1828) in which he expounded his second method consists of three parts[12]. Pt 1 (§§ 1 – 22) includes the general theory, the method of eliminating the unknowns, and the determination of their weights. The second part (§§ 23 – 40) deals with the transformation of the quadratic form corresponding to the normal system with the determination of the weight of linear functions of the unknowns and includes the celebrated formula for the mean square error of unit weight. The third part, a Supplement, contains the essence of the adjustment of geodetic measurements by applying the method of conditional observations including its substantiation (§§ 1 – 22). It concludes by offering two examples, a numerical adjustment of two nets of triangulation (§§ 23 – 24) and Gauss himself is known to have observed one of them.

From the diverse conditions for combining observations with which we met above, Gauss leaves here only one: To combine them in such a way that the measure of precision of the results becomes maximal[13]. In this case all the difficulties connected either with his first method, or with the Laplace method fall away, but the practical result is the same in all three cases. Indeed, the proposition H = max (or, which is the same, the condition that the mean square error of the result is minimal) appears in the Gauss first method as a corollary of his other assumptions, and in the Laplace method it is the condition for combining observations. In all three cases the practical algorithm is therefore one and the same, the MLSq.

I discuss reasons why Gauss (1823, § 6) chose this criterion for combining observations in the main text and quote his later, very important letter of 1839 to Bessel. And I expound the Gauss method and its development by Markov who brought it to the highest logical and mathematical perfection[14]. Here, I restrict my exposition by a remark.

When determining the adjustment by the criterion H = max, we obtain the same values of the unknowns as provided by the Legendre principle, cf. (3), (7) and (10). But we can not prove that the condition of minimizing the sum of the squares of the errors directly follows therefrom. That criterion is of a stochastic nature whereas the condition is algebraic, and a transition from one to another does not exist. Therefore, we only establish, in each problem concerning adjustment, that the results are identical for both justifications of the solution. This is very clearly shown in Markov's treatise (1924)[15].

9. In the 1850s, a number of reports one way or another concerning the MLSq was made at the Paris Academy of Sciences. In those times, Cauchy (1789 – 1857), see our main text, and Leverrier (1811 – 1877) put forward methods roughly equivalent to the MLSq but simpler and more flexible especially when treating systems of a very large number of equations[16].

The problem was mainly formulated with respect to the possible values of the coefficients in the expressions (7) and to the estimation of the approximation at each step of the elimination of the unknowns. In the Cauchy method, these coefficients always equalled ± 1, and for the case of one unknown this restriction returns us to the Cotes formula (2). Leverrier (1855) proposed other versions. The Bienaymé memoir (1853), to which we have already referred, was written as a criticism of these suggestions. He stood for a strict Laplacean point of view and made a number of very important indications, see our main text.

10. The issue of the *law of large numbers* and the justification of the MLSq play a substantial role in the works of Chebyshev (1821 – 1894) and Markov (1856 – 1922)[17]. Without touching on Chebyshev's fundamental work in probability, we shall only point out that he published a number of memoirs devoted to interpolation by the MLSq[18]. They contain a transition from the classical applications of the method to more general and wider problems of *quadratic approximations*, i. e., to the construction of a function of a certain class (for example, of a polynomial) providing the best approximation to a given set of values of the approximated function on a given finite or infinite domain. The quality of the approximation is here estimated by demanding that the sum of the squares of the remaining errors [discrepancies] be minimal[19].

Above, I have sufficiently emphasized the significance of Markov's work on the principles of the MLSq. Now I note that he attached comparatively little importance to the law of distribution of the errors[20].

11. Does such a law really exist? And if it does, is it the Laplace – Gauss law, which finds its robust foundation in the general conditions of the Laplace limiting law, as Poincaré (1896/1912, pp. 143 – 144)[21] was the first to indicate? Or, on the contrary, is this law, theoretically speaking, inadequately justified, as Jeffreys (1938), the leading British specialist in theoretical geophysics, is stating? Newcomb (1886), a most competent astronomer, claimed that the Laplace – Gauss law can not in principle represent the distribution of the errors in large series (of a thousand or several thousand) observations.

If so, how should this law be corrected? In such a way that it remains as a first approximation and is only subjected to a *perturbative influence* of factors incompatible with the conditions of the Laplace limit theorem? Poisson (1824 – 1829) initiated this approach but only the Swedish astronomer Charlier (1906) completed such a construction[22]. Or, on the contrary, should the Laplace – Gauss law be abandoned so that we ought to return to the initial principles of the theory of probability and search for an expression of the law of distribution of random errors from the general patterns of mathematical statistics, as Jeffreys and his school are doing? Such are the problems of the modern theory of mathematical treatment of observations. In my main text, I provide a brief report on this subject.

12. The methodology of expounding the MLSq is continuously changing. It had been extending and developing since Legendre by Gauss and Laplace and up to our days. Thus, the prominent French mathematician and astronomer Andoyer (1923) completely [!] developed it by the deep methods of the theory of quadratic forms and Kolmogorov (1946) showed that the entire exposition of the MLSq can be essentially simplified by applying linear vector algebra and making use of its main notions (for example, of orthogonality).

This approach which we will briefly describe in the main text, allowed Kolmogorov to achieve great compactness and transparency in the derivation of all the main formulas of the Gauss algorithm. In addition, his contribution contains some new findings concerning the estimation of the reliability of the results provided by the MLSq when assuming that the random errors of observations obeyed the normal Laplace – Gauss law. I discuss these new results in my main text.

Notes

1. This is quite unnecessary. O. S.

2. The author did not say anything about the previous methods of adjusting *indirect* observations. His description of the Cotes recommendation is due to Laplace (1812/1886, pp. 351 – 352). Concerning Cotes see my general comments on [xiii].

3. Legendre did not distinguish between errors and residual free terms of the initial equations and he all but stated that his method provides shortest possible intervals for the extreme [residuals]. Actually, this is the definition of the minimax method! O. S.

4. See translation in Hald (1998, p. 119). O. S.

5. Translation here and in a few more cases below is due to Plackett (1972). O. S.

6. This letter of 31 May 1809 (W-8, p. 138) was preserved in Gauss' papers. It ends thus:

You have treasures enough of your own, Sir, to have no need to envy anyone; and I am perfectly satisfied, besides, that I have reason to complain of the expression only and by no means of the intention.

Twelve years later, when the second part of Legendre's work (1820) on the cometary orbits appeared, a page was glued to it and there an anonymous author who hid himself under the letter *N* (Mr N***), sarcastically (and rather naively repeating Legendre's words from his letter of 1809) spoke about Gauss and the incidents connected with Legendre's priority. Gauss never publicly answered any of these attacks. Much later, 3 Dec. 1931 (W-8, p. 138), he wrote to Schumacher:

This [a public statement by him or his friends] *would amount to recognizing that my announcement in the <u>Theoria motus</u> that I had used this method many times since 1794 is in need of justification, and with that I shall never agree*. N. I.

7. Laplace had published a relevant memoir (with a supplement) in 1810, and another one, in 1811. O. S.

8. Gauss' letter to Laplace of 30 Jan. 1812 (W-10, pp. 371 – 374). It is entirely concerned with the theory of probability, a fact caused by his receiving two of Laplace's memoirs of 1810 [see Note 7]. Gauss begins by thanking Laplace for sending them; then he tells Laplace about a problem in probability with which he dealt about 12 years ago without finding its satisfactory solution and adds:

Perhaps you will care to study it for a few moments: in this case I am sure that you will find a more complete solution.

A Leningrad professor, Kuzmin (1928) solved it only in our time. The letter in its entirety is extremely interesting. The record of 1798 which he mentioned had indeed been preserved in his diary (W-10, p. 533). 17 June 1798 he wrote: *Calculus probabilitatis contra La Place defensus*. N. I. See Note 11 below. O. S.

9. I can certainly only agree with Charlier (1906):

I know as a result of my own experience that the study of the Laplace theory of errors requires long reflection and much time. N. I.

10. That Laplace began compiling his treatise was known well enough. Gauss [for example] mentions this in his letter to him of 30 Jan. 1812. N. I.

11. In the same place Laplace, after expounding the essence of the Legendre rule, says:

Mais on doit à M. Gauss la justice d'observer qu'il avait eu, plusieurs années avant cette publication la même idée dont il faisait un usage habituel, et qu'il avait communiquée à plusieurs astronomes.

[*However, concerning Gauss justice demands to say that he arrived at the same idea many years prior to that publication and that he usually applied it and communicated it to many astronomers*.]

Nevertheless, it was Delambre (1810, p. 393) who exonerated Gauss:

Gauss y fait usage de la méthode des carrés dont la somme est un minimum. Il ajoute, qu'il est en possession de cette méthode depuis 14 ans; mais il reconnaît les droits de M. Legendre qui l'a publiée le premier dans son mémoire sur les comètes.

12. Gauss' own abstracts of these parts appeared in 1821, 1823 and 1826 respectively. N. I.

13. In § 6 of the *Theor. Comb*. Gauss compared observations with *a game of chance in which one can only fear a loss*. He had used the word *jactura* which

Newcomb (1886) translated as *evil and worth of erroneous results*. N. I. The author did not say that Gauss abandoned here the idea of a single law of error. O. S.

14. This is a legend (Sheynin 1989, pp. 345 – 346 and 348 – 350). On p. 345n I quoted Idelson (!) who had stated that Markov's exposition of the MLSq was ponderous. O. S.

15. Markov (1924, p. 386): *The same approximate magnitudes can be determined from a rather simple system of equations*. Here, he is in essence discussing the transition from the Gauss method to the normal equations, i. e. to the Legendre method. Also see his p. 459 in connection with solving a problem which I discuss in the main text. N. I.

16. All the Cauchy memoirs concerning his method and the theory of probability can be found in his collected works (1900, pp. 36, 63, 87, 94, 114 and 125). Especially important is the application of characteristic functions (pp. 97 and 105), see our main text. Also there is his not quite convincing answer to Bienaymé's criticisms. N. I.

17. Chebyshev was not really interested in the latter subject (Sheynin 1994, § 5). With respect to Markov cf. Note 14. O. S.

18. Chebyshev's main memoirs are (1859; 1864; 1875). He introduced orthogonal polynomials by whose means his expansion (Chebyshev 1855) is achieved and considered them up to 1887, see Chebyshev (1887, especially § 5). I do not examine the Chebyshev method and only refer to some authors who explicated it and to its applications. N. I.

19. Quadratic approximations constitute an important and vast section of modern analysis, see for example Goncharov (1934, Chapter 3). N. I.

20. See Markov (1924, pp. 341 – 344). In addition to Chapter 7 of this treatise, extremely important is his memoir (1899). N. I.

21. The reference should have been to Poincaré's §§ 143 – 144. However, Laplace himself and Bessel (to name only them) have precedence over Poincaré, see however end of Note 13. A few lines below, the author refers to Newcomb, but even Bessel noted that the errors of Bradley's observations did not quite obey the normal distribution (Sheynin 2000, pp. 79 – 80). O. S.

22. Some of Charlier's formulas complete with notation coincide with those of Poisson. However, the series called after Gram and Charlier definitely occurred in Chebyshev's memoir (1887). N. I.

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VII

Anonymous (P. R. Montmort)

Essay on an Analysis of Games of Chance, Preface

Anonymous, *Essay d'analyse sur les jeux de hazard*. Paris, 1708, 1713.
P. R. Montmort, New York 1980. Preface

[1] Even a long time ago, geometers had been boasting to be able to discover in natural sciences all the truths accessible to the human mind. And it is certain that the marvellous alloy between geometry and physics[1] achieved during the last 50 years compelled the public to recognize that what the geometers are saying about the advantage of geometry is not groundless. But what glory will cover their science if it will also be able to serve for regulating judgements and the behaviour of men in ordinary life!

The eldest of the Bernoulli brothers [Jacob], both so well known in the scientific world, did not think it impossible to bring geometry to that point. He attempted to provide rules for judging probabilities of future events whose knowledge is concealed either in games [of chance] or other events in life in which only fortuity is involved. His contribution [1713] will be entitled *Ars Conjectandi*, The Art of Conjecturing. Premature death prevented him from completing it.

Both Fontenelle (1706) and Saurin (1706) briefly analysed this book, and here is its structure as stated by them. Bernoulli separated it in four parts and in the first three he solved various problems in games of chance. Many new things will be found there about infinite series and combinations and arrangements as well as the solution of the five problems proposed to the geometers a long time ago by Huygens. In the fourth part Bernoulli applied the methods provided in the first three for solving various moral, political and civil problems[2].

We do not know at all of which games that author determined the solution or which political or moral matters did he wish to elucidate. No matter how astonishing was his plan we may believe that the learned author had perfectly accomplished it. Bernoulli much exceeded others by wishing to command respect and belonged to the small number of rare people able to invent, and I am convinced that he wished to carry out everything promised by the title of his book.

[2] Nothing retards the progress of sciences and profoundly hinders the discovery of hidden truths as strongly as disbelief in our own capabilities. Many things seem impossible mostly because we do not apply all the resources of our mind. Many friends of mine had long ago induced me to find out whether algebra was able to determine the advantage of the banker in the game of *pharaon*. I would have never dared undertake that research since I knew that the number of the various possible arrangements of the 52 cards more than a hundred thousand million times exceeds the number of grains of sand which can be contained in our globe. And, considering that immense number, I thought it impossible to separate the arrangements favourable for the banker from those that are contrary or indifferent to him. I would have still remained thus prejudiced had not a few years ago the late

Bernoulli's success induced me to study the different chances in that game.

I was luckier than I dared to hope because, in addition to the general solution sought, I found out the approaches for solving infinitely many similar and even much more difficult problems. I knew that it was possible to go much further in the world of games of chance which no one had still entered and flattered myself with hope of gathering there a rich harvest of equally curious and new truths. This knowledge suggested me the idea to get to the bottom of the matter and led me to desire for compensating somewhat the public for the possible loss of Bernoulli's excellent work. Various reflections had confirmed my intention.

[3] The feebleness of the human mind is especially revealed in games of chance and induces men to superstition. Nothing is as usual as seeing that gamblers attribute their failures to those who approached them or to other circumstances not less indifferent to the events in games. There are those who consider it necessary to play with [previously] winning packs of cards believing that some luck is attached to them. Others, on the contrary, choose only the losing cards since they think that, having lost many times, the cards will be less likely to lose again, as though the past can decide something for the future[3].

Then, again, there are those who prefer certain places and days; and we can also see gamblers who only agree to shuffle the cards when they are arranged in a certain way and are sure to lose if deviating from this restriction. Finally, most people seek their advantage where it is lacking or rather neglect it altogether. Almost the same might be said about the behaviour of people in all their everyday actions in which fortuity plays some part.

The same prejudices are governing them, imagination masters their activities and blinds their fears and expectations. They often abandon a small and certain benefit recklessly pursuing a greater boon whose acquisition is all but impossible. Again, often because of excessive mistrust they give up considerable and well justified expectations to keep a benefit whose value is out of any proportion to the neglected. The general explanation of these prejudices and mistakes is that most people attribute the distribution of the good and the evil and in essence all the events of this world to a fatal power acting without order or rules. They believe it better to abandon themselves to that blind divinity called Fortune than to force it to become favourable by following the rules of prudence which they believe are only imagined.

[4] I think it therefore useful not only for gamblers but for people in general to understand that chance has rules which can be known and that otherwise they are daily led to err with unpleasant consequences to be more reasonably ascribed to themselves rather than to destiny accused by them. To prove this, I may report infinitely many examples taken either from games [of chance] or other events of life depending on fortuity.

It is clear that people are not at all sufficiently using their minds for obtaining what they even most ardently desire and do not at all exert enough efforts for depriving Fortune of what can be stolen from her by the rules of prudence.

I think that this subject can excite the curiosity even of those who are least of all interested in abstract knowledge. People naturally like to see clearly what is done, even independently from any interest. They will undoubtedly play more readily when knowing at each moment the expectation of winning or the risk of loss to which they are exposed. They will more calmly meet the events in games, will better imagine the absurdity of the incessant complaints which most gamblers allow themselves about commonest occasions contrary to them.

[5] By itself, an exact knowledge of chances in a game is not sufficient for winning, but at least it can help gamblers to choose the best next move in doubtful situations, and, what is very important, to understand how disadvantageous for them are the conditions of some games, daily introduced by stinginess and idleness.

For my part, I believe that, had the gamblers known that, staking a *louis* of thirteen *livres* at *pharaon* for a card which passed three times, so that the pack will have no more than twelve cards left, is just the same as presenting the banker 1 *livre* 1 *sou* and 8 *derniers*. Only a few [diehards] will try their luck at such a disadvantage. Most often the behaviour of people determines their good or unlucky fortune, and wise men leave to chance as little as possible.

We can not divine the future, but in games of chance and often in other circumstances of life we can always exactly find out how much more probable is the arrival of some thing in a certain way rather than in any other manner! And since these are the boundaries of our knowledge, we should at least attempt to reach them.

Everyone knows that, for approaching the truth but lacking the evidence, we ought to look for the likelihood. However, we do not at all sufficiently know which likelihoods are higher or lower. For a correct judgement the mind ought to distinguish all those degrees; it often occurs that an uncertain thing is nevertheless certainly and even evidently likely, and more likely than all the rest ones. It seems that as yet the possibility of providing infallible rules for calculating the differences existing between various probabilities is not at all sufficiently understood.

[6] I am here attempting to compile an essay on this new art by applying it to a new subject which until now remained quite obscure and does not seem to be capable of any precision. I believe that it is more proper than anything else to subject to analysis that marvellous art, the key to all exact sciences. It was neglected apparently only because the scope of its application was not at all sufficiently seen. Indeed, instead of applying algebra and analysis for discovering constant and immutable relations between numbers and figures as it was done until now, here they will find out the connections between probabilities of uncertain things which have nothing fixed and seem strongly opposed to the spirit of geometry and apparently remain in some way above its rules. It is this that led the illustrious de Fontenelle (1706) to a reasonable feeling:

It is not so glorious for the spirit of geometry to rule in physics as to govern moral things, so casual, so complicated and changeable. The more does the subject oppose geometry and rebel, the more honourable it is to curb the new field.

[7] I divided this Treatise in four parts. The first one includes a complete theory of combinations; in the second, I solve various problems about current card games. At first, I examine those depending on pure chance (*pharaon*, *basett*, *lansquenet* and *treize* [thirteen]) and determine the gamblers' advantage or disadvantage in all their possible circumstances. Geometers will find here all the desirable and possible generality and the gamblers will discover novelties, very special and important for them.

I restricted my attention to those four games not wishing the book to become too large and I have preferred them because they are more commonly played and seemed to me the most curious of all games. The rest of part 2 contains solutions of various problems about *hombre* [a version of *omber*], *piquet, imperial, brelan* etc. Owing to causes mentioned on pp. 157 – 161, I was unable to treat these games as comprehensively as the previously mentioned[4].

In the third part the reader will find the solution of all problems which are possible to propose about *quinquenove*, the game of three dice and hazard [craps is its simplified version]. The first two are the only dice game played in France and the last one is only known in England. Then I provide rules for playing as perfectly as possible in an ingeniously invented game as well as in two card games, *le her*[5] and *tontine*. The person who taught me this [invented?] game was unable to name it, so, to make up for this deficiency I called it the *game of hope*. There also I solved some sufficiently easy problems about *trictrac* [a version of tricktrack]. One of them can be somewhat useful for gamblers. I conclude this third part by considering very general problems concerning dice and adduce tables which can be useful to gamblers. Three problems are also added as examples; they describe the games of the first raffle, of three raffles, and the game which the Baron de la Hontan described in the second volume of his *Voyages* [1740] and which, as he says, is generally played by the savages in Canada[6]. Its name, game *of noyauk* [stones of fruit] is unattractive.

All these dice games considered in pt 3 are disadvantageous for the banker whereas such card games as *pharaon, basset, lansquenet* and *treize* are considerably profitable for him. It should be thought that the inventors of these games did not at all pretend to render them entirely fair; or, which seems more likely, that they did not at all sufficiently understand the essence of their inventions and were unable to distribute the chances well enough. Most conditions of those games are so damaging for the gamblers that we can justifiably state that, for them, to win fairly or to lose without being undoubtedly duped, is impossible.

Although I had in mind the pleasure of geometers rather then the benefit of gamblers, and although in my opinion those who lose time by playing really deserve to loose money, I did not at all neglect to discover the gamblers' advantage or disadvantage or to remark how to reform the games rendering them perfectly fair.

[8] In the fourth part, I solve the five problems proposed by Huygens and add many more, some of which seem curious and perhaps difficult enough, and I conclude by proposing, like Huygens did, four sufficiently singular problems[7]. However, I apparently ought to warn the geometers inquisitive to enquire about their solution that those problems are not less difficult than the most awkward problems of the integral calculus. Those who regard them as arithmetical will see that, perhaps demanding less knowledge of geometry, they require more knack and certainly much more exactitude and circumspection.

Had I proposed to follow exactly Bernoulli's project, I would have added a fifth part and made use there of the methods contained in the first four parts by applying them to political, economic and moral issues. What hindered me was the embarrassment of finding the hypotheses for applying them to trustworthy facts and leading and helping me in my researches.

However, I am not at all entirely satisfied with the achieved and think that it will be better to return to this work later or to leave the glory to someone abler rather than saying something either generally known or inexact and not up to the readers' expectations or the beauty of the subject. I restrict my account to briefly remarking about the relation existing between this matter and games and the views which should be adopted for elucidating it.

[9] Strictly speaking, nothing depends on chance. When studying nature, we soon become convinced in that its Author acted in a general and uniform manner characterized by wisdom and infinite prescience. For attaching an idea in conformity to true philosophy to that word, fortuity, we should suppose that everything is regulated according to definite laws whose array most often remains unknown. Things depending on chance are those whose natural causes are hidden from us. According to this definition, we may say that human life is a game ruled by chance.

We should discern more precisely that the analysis of the geometers, and mainly that applied here, is proper for partly dispersing the mystery apparently shrouding future things in civil life. To achieve this, it ought to be indicated that some games are only ruled by chance, others, partly by chance and partly by the gamblers' skill, and that, just as well, there are things in life whose success entirely depends on chance and others in which a large part is played by the behaviour of men.

In general, concerning everything decided by us, our deliberations should be reduced, just as in games, to comparing the number of cases for the occurrence and non-occurrence of a certain event. Or, in the language of geometers, to examining whether the expected multiplied by the degree of probability for getting it is at least equal to our stake, to the advance necessarily paid in labour, cash, credit, etc.

It follows that the same rules of analysis which served us for determining the decisions of the gamblers and the manner in which they ought to play can also guide us in establishing the proper degree of our expectations in various enterprises, should teach how to behave for ensuring the greatest possible advantage. It is clear, for example, that the same method which served us for determining when to renounce the due counters in *hombre* while expecting a *volle*, – that same method can be applied, although with more difficulties, under which circumstances of life should we sacrifice a small benefit for obtaining a larger boon.

[10] To continue this comparison, it should be remarked that the same causes that prevent us from solving all the possible problems about games do not allow us to solve those about civil life. They, these causes, are of two kinds; the first is the uncertainty about the decisions made by those whose actions ought to regulate the events in our enterprises. A shock experienced by a body decides its path and velocity since the laws of transferring movement are fixed and invariable, but the causes and various motives compelling people to act in one way rather than another can not assure us about the consequences. Often they do not understand their own interests; and even otherwise, people often do not pursue them. Caprice leads them much oftener than reason and it can always be only guessed what the free will of people decides.

The second cause of our ignorance of future things results from the narrowness of the boundaries of our mind. All the knowledge that presupposes the existence of a very large number of ratios is beyond its power. And in many games and in most situations of life there are so many comparisons which we ought to make, that it is barely possible to exhaust them. To determine the value of a [throw of] a die for the two gamblers in *trictrac*; of the next move in *piquet*; to establish whether a knight or a bishop is more, and how much more advantageous in chess[8], – such are the problems whose solution I think is impossible for us.

The same, and for the same reason, holds for most problems in moral and politics. For example, to determine whether under such-and-such circumstances I should consider more attentively a recommendation of a relative or a request from a number of friends; whether some kind of trade is advantageous or harmful for a nation; how successful should be a negotiation or a military enterprise, etc.

[11] Insurance policies, which are so common among merchants, mostly in the Republics[9], do not always enrich the insurers, and, since men's prudence is not sufficient for surely penetrating the future, the ablest British politicians are suffering daily losses from those large bets made there about the events of war. With a sober mind, well knowing the facts and especially the secret mainsprings which set in motion and move the affairs we can discover likely enough the best decision in those bets. However, it is impossible to determine it by issuing from the exact ratio of two numbers.

The assistance that the human mind can get from geometry is the virtue called prudence whose rules are uncertain. Against a small number of truths and trustworthy principles of politics and morals there are infinitely many obscurities impenetrable for the human mind.

People, who familiarize themselves with the kind of logic applied in this treatise, acquire the habit of likely distinguishing the true; they only agree with the evident. They will be better prepared to discern the various degrees of probability accompanying diverse decisions possible in moral things or civil life and to avoid errors in judging them.

[12] Let people think what they wish, but it is certain that that power and that sobriety of mind acquired when researching abstract truths extends to perceptible truths, and, so to say, to practice. Analysis is an instrument that serves everyone who knows the proper manner of using it. All truths are connected; we can spend some time attempting to apply that power [of the analysis] to our exact notions about numbers and figures, but it will be more successfully applied to less exact knowledge which can be the object of our mind. Those who had been better versed in metaphysics, physics and perhaps even medicine and morals were excellent geometers. Those, who can not be persuaded by reasoning, will be convinced in the utility of geometry by experience.

For concluding the parallel between problems in games and questions which can be proposed about economics and political and moral matters, we ought to note that in both cases there exists a kind of problems which can be solved when following these two rules: Restrict the proposed question to a small number of assumptions established for trustworthy facts; and, neglect all the circumstances in which the free will of men, that perpetual hindrance to any knowledge, can play some part.

We should suppose that in the fourth part of his book Bernoulli took into account these rules, and that, when keeping to both these restrictions, we can certainly treat many issues in politics and morals with all exactness of geometrical truths.

[13] It is this that Halley (1694) had admiringly done. There, the learned Englishman determined the degrees of mortality of mankind. His note is full of curious matters and the reader would have been delighted by some appropriate extracts, but my Preface is perhaps already too long and I will only report what the author had treated very subtly. I bear in mind a method for determining the grounds for regulating annuities.

Halley compiled a Table for ages 1(5)70 showing how advantageous was for Englishmen the decision of Roy Guillaume[10] to pay 14% yearly, or almost 1/7 of the advanced sum, as a life annuity. According to the Table, a person aged 10 should only receive 1/13; aged 36, 1/11; and, finally, 10% was only due to people aged 43 – 44 years [or more]. Halley generalized this idea and examined the grounds for regulating a life annuity on two or more lives of differing ages. His memoir exhausted this issue.

There are several other similar aspects [of that problem] luckily enough although less exactly treated by Petty (1690), but much more of the same essence can be considered with the same success and benefit for the public[11].

[14] Now I feel myself obliged to mention two illustrious geometers to whom I owe my first views about the subject now treated. In 1654 Pascal resolved the problem:

Two gamblers play a fair game until a certain number of points [is won by one of them]; *they are supposed to have differing numbers of* *points, and it is required to determine how they should share the stakes if wishing to quit without finishing the game.*

A solution of this problem can be seen in his very short posthumous book, *Triangle arithmétique* (1665). This great man who gave much thought to properties of numbers[12] discovered many applications of that triangle to the problem of points and to combinations. The Chevalier de Méré proposed that problem as well as a few others about dice games to Pascal. They were sufficiently easy as, for example, to determine in how many throws we can get a certain raffle. That Chevalier, a clever man rather than a geometer, solved these dice problems, but neither he, nor Roberval[13] was able to tackle the problem of points.

Pascal proposed it to Fermat with whom he corresponded as a friend and geometer, with the man who as a geometer was not inferior to Descartes. Fermat solved that problem in a way different from Pascal's; he went even further and ascertained that his method held for any number of gamblers. Pascal did not believe that and, in a letter included with some others on the same subject in the latter's posthumous works (1679), attempted to convince Fermat that his method, adequate for two gamblers, was not proper for a larger number of them. That source does not have Fermat's answer, but he was certainly right; his method is incontestable and extends to any number of gamblers.

[15] A bit later Huygens, that famous geometer who enriched all parts of mathematics by so many excellent discoveries, had heard about these problems and undertook to solve them by an analytical method[14] which as a rule allowed him to go further than all the rest of them. He included these problems in a small treatise at the end of van Schooten's *Exercitationes Geometricae*. Although Huygens did not attempt to determine the best decisions for gamblers in any card or dice game and restricted his account to the easiest part of the subject, almost to Pascal's problems, we see, as he wrote to Schooten, that he highly esteemed what he did in that small work:

There is nothing more glorious in the art that we are applying in this Treatise, than to be able to provide rules for matters which depend on chance, seem to be studied by no one and which therefore eluded human reasoning. […] *I am sure that those capable to judge matters will see in this work that its subject is more important than it seems to be, that it lays the foundation of a marvellous and very subtle theory and that Diophant's researches which only aimed at abstract properties of numbers are simpler and less agreeable than those which can be proposed on this subject*[15].

Huygens ended his treatise by inviting geometers to study the five problems which no one, as far as I know, had yet solved[16]. He supplied the answers to three of them though without any analysis or demonstration, and did not at all adduce the solution of the other two.

[16] I have mainly composed this Treatise for geometers; since scientists are not usually gamblers, I thought myself duty bound to explain in detail the games considered here and attempted to describe each necessary circumstance. At first I supposed to elucidate in plain language the solution of some of the easiest problems, such as those included in pt. 4. Then, however, I was compelled to abandon this plan and therefore to avoid compiling an infinitely long book no one will be patient enough to follow.

Algebra briefly expresses a large number of ideas and simplifies a cursory inspection of the relations between the considered things. I think that, not wishing to write a large book I should have by no means renounced that advantage. I have been only explaining my subject in this [other] manner at the end of each problem and in the corollaries and remarks adduced after their solution, so that everyone and even the gamblers will understand me. Authors only write for being read and I attempted to simplify the reading of this work since I prefer to satisfy easily the reader rather than to be esteemed by mediocre minds who only admire that which demands their great effort and seems to be beyond the boundaries of their intelligence.

It can be established that I remained very far from the region which I believe difficult and mostly from such which ought to throw their light on many truths. But I also know that the benefit of a mathematical book consists less in the discovered truths than in the tendency it provides for the mind towards discovering similar novelties.

This disposition is acquired much easier when finding out what the author had already discovered, when following his each step so that I believe that I should not at all be ashamed of describing everything in detail or even of explaining everything. It is sufficient to leave no difficulties remaining insurmountable after enough application. Finally, I do not intend to protect the reader from the labour of invention and thus I ensure him a pleasure of sorts.

Notes

1. See Youshkevich (1970). Novikov (2002) described the present situation and the essence of his paper is reflected in its title.

2. Neither Montmort, nor the authors he mentioned above could have known what really later appeared in pt 4 of the *Ars Conjectandi*.

3. Such thoughts opposed the existence of a *fatal power acting without order or rule* (see below). Cf. also Bertrand's remark (1888, p. XXII): the roulette *n'a ni conscience, ni mémoire*.

4. Those were not games of pure chance. Then, the choice of best decisions was sometimes too difficult and could have strongly depended on prejudice.

5. Concerning the strategic game *le her* see Hald (1990, pp. 314 – 322). The modern theory of games studies such games by means of the minimax principle. However, already Nicolas Bernoulli indicated that gamblers ought to keep to mixed strategies. Montmort published his letter in the main text of his treatise.

A tontine (named after the Italian banker Laurens Tonti) was a group of annuitants. Acting as a single body, it distributed its total yearly interest between its still living members so that who lived long enough received large moneys. It was thought that tontines hampered the development of *usual* life insurance and in addition the members of a tontine necessarily hated each other. As a result, tontines had not been socially acceptable. I can not describe the *game* called tontine.

6. Here is David's (1962, p. 149) translation of Montmort's quotation which he included in the first edition of his treatise. It is from p. 113 of vol. 2 of the book of Baron Hontan:

It is played with eight nuts [not stones of fruit?] *black on one side and white on the other. The nuts are thrown in the air. If the number of black is odd, he who has thrown the nuts wins the other gambler's stake. If they are all black or all white he wins double stakes, and outside these two cases he loses his stake.*

Schoolcraft (1845, pp. 85 – 87) remarked that *the principal game of hazard among the northern tribes* [of Indians] was very complicated. See also Longfellow's *Hiawatha*, chapter 16.

7. See Todhunter (1865, pp. 105 – 106 and 110 – 111). Montmort himself solved the first problem; the third was *le her*, see Note 5, the fourth problem concerned a game only partly depending on chance.

8. No general answer is here possible.

9. In those times life insurance had been most of all developing in the Netherlands (the *Republics*). Below, Montmort apparently mentioned the war of the Spanish succession. For Englishmen, bets (gageurs) on the events in that war were certainly immoral.

10. I have no information about Roy Guillaume.

11. This means that Halley did not, after all, exhaust his issue (see above).

12. *Properties of numbers* as studied by Pascal and Diophant (see § 15 below) were quite different. Huygens obviously underestimated Diophant.

13. Gilles P. de Roberval, 1602 – 1675.

14. Huygens (like Pascal and Fermat) introduced expectation (of a random variable). He studied games in which expectations varied from set to set, and this compelled him to apply difference equations. Probabilities, on the other hand, would have remained constant.

15. Montmort had apparently translated this passage (into French) from the Latin of 1657 which somewhat differed from its later French text.

16. See Hald (1990, § 6.3). Moreover, Montmort himself (see his § 1) noted that Jakob Bernoulli had solved those problems.

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VIII

A. Moreau de Jonnès

Elements of Statistics, Chapters 1 and 2

Eléments de statistique. Paris, 1847

Chapter 1. Definition and Object of Statistics. Its Origin and Dissemination

[1] Statistics is a science of social facts expressed in numerical terms. Its object is a profound knowledge of the society considered in its elements, its economics, situation and movements. Numbers are its language, not less essential for it than figures are for geometry or signs for algebra. Statistics incessantly deals with numbers which provide it the characteristic feature of precision and certitude of the exact sciences.

Works which appear in its name without pursuing its object and lacking its language do not at all belong to it since they are beyond the conditions of its existence. Statistical contributions without numbers or with such that are not at all enumerating social facts do not merit their misappropriated name. The same holds for moral and intellectual statistics since it is futile to wish the submission of the mind or its passions to calculation or to computation of the movements of the soul and the events of human intelligence[1].

Statistics is a science of facts just like history, geography and natural sciences. Like astronomy and geodesy it is a science of numerical facts. It resembles history in that it also collects present and past facts. However, there is also an essential difference; not restricting itself to the alien events of people's life, it strives to penetrate their civil and inward life, to reveal the mysterious elements of the society's economy. Contrary to history which almost always concentrates the interest of its accounts on battles and conquests[2], statistics mostly occupies itself with the blessings of peace.

Geography only connects with statistics by the works which it borrows and appropriates from the new science. The former describes countries, the latter analyzes societies. One recounts or discusses; the other calculates and studies. And it is not at all possible to be less similar.

Among all the sciences, political economy is most closely connected with statistics. In guiding the administrative and political powers by the light of a high motive, both aim at improving social conditions. However, the first is a transcendent science which daringly soars the loftiest region of speculative systems, whereas the second is only a science of facts which enumerate by swift numbers the needs of the population, its daily progress and each particular successful for, or fatal to its destiny. Both are at a disadvantage of being unpopular although they devote all their efforts to the interests of the people. This is an irremediable misfortune since it is occasioned by the established scientific order and therefore obliged to use its language.

Political economy proceeds by abstractions but statistics, like the exact sciences, only speaks by numerical signs. Nevertheless, among the branches of human knowledge there only is a very small umber of those which do not resort to the service of statistics, do not consider it as an auxiliary. History gets from it luminous numbers which indicate the reality of things or their delusiveness and borrows calculations to establish after 25 or 30 centuries Herodotus' veracity, the exactness of Thucydides and the errors of Diodorus.

Geography is indebted to statistics for its best materials, for those, which being formed by rigorously defined terms, avoid the versatility of human judgement and are not altered by the influence of time or place. Finally, it continuously demands from statistics numerical facts and calculations which serve as a basis for its theories and justify its deductions.

Statistics is incessantly applied to all social transactions either directly by great actions or by barely noticeable deeds concerning details. In their private life, statistics studies people from their first hours and considers them as unities added at first to the general number of births. It returns to a man for perhaps half a century in the lists of censuses. At 20 years, he is entered in the military ranks but more likely among the married. He will be present in the classification of professions, so numerous and diverse. A place for him is assigned among those having political capacities up to the state's distinguished men. And then he is entered in the column of deaths in which everyone is listed for the last time and human vanity is suppressed. However, how many times before that catastrophe did he reappear in the numbers? Before jurymen, in elections to the parliament, and in his vote which sometimes tilts the balance of justice or of the destiny of the state.

Does he possess land? Manufactures? Disposes labour and riches on a large scale and becomes the foundation of numbers expressing agricultural or industrial production and everything else accompanying his fortune? But perhaps he is a poor proletarian. Statistics studies whether the price of foodstuffs he needs is in equilibrium with his earnings. It explains to him the advantage of accumulating money instead of squandering it. It throws light on the charity establishments which should assist people in distress.

Statistics certainly can not act, but it is able to reveal and, happily, for our time this is almost the same. Long ago, the popular cry was, If only the king had known! Nowadays, the authorities know everything. Fifteen years ago in some orphanages the mortality of foundlings amounted to 25%. Statistics denounced that crime and now their mortality is more than twice lower. Without this science, the situation would have been ignored. Almost for a hundred years there existed orphanages where death had been carrying off a quarter[3] of those wretched creatures entrusted to such disastrous charities.

For the public life of people statistics is no less necessary than for their private life. It is by its works and investigations that the main interests of the state are elucidated, thoroughly studied and become known. Numbers provide the best arguments, they are the most decisive witnesses which daily appear at the Royal Council, at

Parliament and the Academy. The lack of this means of governing characterizes ignorance and barbarism of an epoch, of a state or administration.

In France, there was no statistics at all under Louis XIII or Louis XV, no statistics under the Directoire or during the Restoration. However, during the reigns of Louis XIV and Napoleon statistics had been cultivated, honoured and considered as an official, administrative and political science[4]. The revolution of 1830 rendered it the right to serve the state. The same phases of good and bad fortunes are found in the entire history of statistics covering 40 centuries. The Egyptians, the Greeks and the Romans applied it for assisting the development of their marvellous civilization. On the contrary, the Middle Ages destroyed its institutions.

Long after the renaissance of sciences and arts some European people beginning with the Swedes recognized the possible advantage of its application. However, the progress was slight since statistics remained a science of learned men, purely speculative and lacking application to public affairs[5], or, rather, it had been denied by the people who considered it a fiscal invention as well as by sovereigns who feared that it revealed the secrets of their cabinets.

[2] The example of France, England and Prussia began to dissipate these vain fears and from then onwards its progress has been assured, at least in states where the affection for the public weal is not a deception. All good minds recognize that statistics is absolutely necessary for statesmen, publicists, economists and historians

1) To establish in all their elements the population of a country, the source of its power, its richness and glory.

2) To ameliorate the territory after having explored and made known its fertility, communications, means of defence, healthiness of its rural areas and towns.

3) To regulate, according to assured sources, the exercise of civil and political rights acquired by all the sacrifices made by the generation soon to disappear.

4) To fix and distribute the conscription which maintains the army and secures independence.

5) To establish fair taxes for satisfying the needs of the state.

6) To determine the quantity and value of agricultural and industrial production, which incessantly renews the public fortune.

7) To appreciate the developments of commerce and study the conditions unfavourable for its prosperity.

8) To extend or restrain the juridical repressive actions, the vigilant guardian of the social order.

9) To trace the progress of public education. This should improve the people by enlightening them.

10) To guide the administration through its innumerable measures which govern the charity and repressive establishments in the interests of the inferior classes[6].

Finally, to explain by new or more exact truths many other objects which daily appear and thus to influence public opinion and fill up parliamentary discussions and formulate problems whose solution is not possible without statistics.

These numerous and powerful interests certainly do not only belong to our century. They always existed everywhere and at all times, and for satisfying their requirements all the civilized people from the earliest antiquity had been compelled to turn to statistical operations. Actually, the history of the first societies of our globe proves to us that such operations had been practised in both extremities of Asia and extended across the ocean to the New World. In spite of innumerable testimonies of that remote origin, statistics is stubbornly considered as a new science and even that it was born in Germany in the mid-18th century by the learned Göttingen professor Godefried [Gottfried] Achenwall who discovered it in 1748.

This argument is justified by the name which he imposed on it and under which it is nowadays known in the entire Europe. It is a strange confusion to date the origin of sciences at the epoch during which they were named[7]. Political economy was also thus named by Quesnay and his disciples, but did it only exist for 60 or 80 years, as though many philosophers and statesmen of Greece and Rome had not been eminent economists?

Technology existed even before the Deluge (Genesis 4:22) and the special name it received in our days does not at all allow us to appropriate this invention. A long time ago geology had been a mystic cosmogony enshrouded in symbols and obscured by darkness. During the 18th century those scientists who had been cultivating it were frightened by Galileo's fate and cautiously called it the Theory of the Earth. Its present name boldly announces that it wishes, like Prometheus, to reveal the secret of the origin of things. In any case, its object did not change at all and it is the same science under a new name.

[3] The same happened to statistics. It appeared during the first ages of the world and found a place in the most ancient book, the Pentateuch, under the expressive name, *Numbers*[8]. For three or four thousand years people performed useful operations in different regions of the globe but did not try to accompany them by a general name which would have indicated their common aim. Finally, in England, in 1669 [in 1690], without knowing or at least without mentioning his memorable forerunners, [Petty] reproduced the name (dénomination) imposed by the Hebrews, or rather that which they borrowed from the Egyptians, as well as their other knowledge[9].

From then onwards Europe has adopted the name *political arithmetic* for explaining them [the *useful operations*] and began to cultivate it. However, we should admit that it was not a professorial science anymore, poorly suited for the authorities. The learned Bushing [Büsching] whose zeal turned him to statistics had asked Friedrich II for some numbers for his works but the King replied that he did not prevent him from publishing what he had procured, but will not give him anything at all.

The revolution involved France in economic studies thus directing the minds to applied mathematics and popularizing statistics and allowing it to penetrate the corridors of power. Thus, the name *statistics*, only a century old and then forgotten was pulled out. The society had been reconstructed on other foundations, by other materials, and it became necessary to submit to calculation the effects of that daring experience as well as the newly obtained forms.

Statistics rendered that service and, as though revived, became a political science associated with governing the state. However, after examining what was established before, and what is done now, it is impossible to fail to recognize that in its aims and methods of executing its duties it remained the same throughout the principal nations of the globe since the earliest antiquity[10].

Was not, indeed, the document compiled by the emperor Augustus, who died 1830 years ago, presented by the successor [Tiberius] to the Roman Senate and explicated by him in public lectures, – was it not general statistics, and, even concerning its registered goals, the most comprehensive ever undertaken? It showed

The state of the Empire's riches, the number of its citizens and allies bearing arms, its fleet, taxes and other items of the public revenue, ordinary expenses and recompenses of the people. Augustus had written all that by his own hand (Tacitus, *Annal*., Bk 2, 11, see Suétone, in Tib., 21). [The author quotes the last sentence of Tacitus in Latin: *Quae cuncta suâ manu praescripserat Augustus*.]

No one ought to overlook that that was not a kingdom of modern Europe squeezed into narrow confines and only peopled by a few million inhabitants. The Roman Empire then had a territory of 412 *mln* hectares [4.12 *mln* km^2], eight time more than France today. Concerning population, special researches allow us to increase it up to 83 *mln* inhabitants, free men or slaves, which is almost equal to the population of the French Empire with its dependencies as counted in 1810.

It is surprising to find out that the ruler of the known world had been so determined and had talent enough for executing the statistics of his immense territory. But perhaps even more marvellous is that he understood with profound perspicacity its considerable usefulness for governing his empire. Among the long sequence of kings which had been ruling France for 1400 years only two out of 78, Louis XIV and Napoleon, had the same idea as Augustus whereas England had no such kings.

[4] Almost at the same time [?], during the year 2042 BC, the prince who ruled at the other extremity of the ancient world, the Emperor of China, Yu, had accomplished the compilation of the statistics of his vast state. According to the first sacred book of that country, the *Chowking*, engraved in its entirety on public monuments for preventing alterations of its text, that sovereign separated the Chinese territory in provinces and, when executing statistics, determined the arrangement that provided the perfection of work, superiority of the end product and the rate of taxes (Gaubil).

In our Europe, so interconnected by its civilization, there is one single state whose provinces can be properly ranged according to statistical data which makes known the pre-eminence of its products. This proves that our knowledge of the most essential things does not advance as rapidly as usually imagined. Only France positively knows the quantity and values of its ordinary agricultural products in separate provinces. This year it lets known the industrial products over a quarter of its territory but it is still far from achieving this latter enterprise exposed to many chances.

Another Asiatic people who are all but placed among our forefathers very successively cultivate statistics for more than a thousand years. The Arabs, when they captured Spain, charged their scientists to compile its statistics. In 721, El Samali, the Viceroy of the Peninsula, sent the Caliph a detailed Table of the country, its shores, rivers, towns, population, and revenues (Conde). Among the works of Arab authors there is a multitude of numerical data which proves that the Moors perfectly well [?] knew the number of the inhabitants of each town, the number of manufactories of each kind, of their workers, of books in the libraries and other notions which we feel happy to obtain in our modern societies.

It is known that in the 8th century people having creative capabilities to calculate, those to whom we owe our numerical characters[11], compiled the statistics of Spain, i. e. at the time when Charlemagne, the greatest sovereign of Christian Europe, was unable to write. We also understand that the Chinese, who were geometers, astronomers, chemists, and for three or four thousand years possessed sciences and industries which we only acquired a few generations ago, had compiled the statistics of their vast empire. Europe then had still been a region of savages.

Long before our era the Chinese had the surveying compass, gun powder, fireworks, balloons, hydraulics [irrigation?], shorthand, enamelled pottery, porcelain. They fabricated glass, spun and weaved flax and silk, they had five types of grain and six species of domestic animals. But first of all, they had free labour and civil equality and capable men had been admitted to political posts.

Here, however, is a human race separated from the Old World from the beginning of things. [To Europe,] they appeared all of a sudden with their liberal arts, perfect agriculture, with surprising industry and inventions which did not owe anything to our hemisphere. These first two people of that new [for this story] race, the Mexicans and the Peruvians, possessed extensive and diverse statistical notions and applied them in the usual way to the needs of their countries and the policy of their governments. Says the historian Herrera:

Montezuma [II]*, the Emperor of Mexico, had a hundred large towns, capitals of so many provinces, from which he received the taxes and where he had governors and held garrisons. He knew perfectly well, added Cortés in his first letter to Charles V, the state of the finances of his Empire and together with many other things represented it in distinct and intelligible characters in the progress registers* (registres points).

At the other extremity of America, that vast continent which occupies one of the two hemispheres of the globe and extends, so to say, from one pole to the other, were the Peruvians, squeezed between the high chain of the Andes and the great ocean with no communication with any civilized people until Pizarro discovered and subjugated their empire. That new country possessed a statistics as complex as the best we have today. And still, the only means to write or calculate known to that people was the *Quipos*, i. e., the somehow joined strings of differing colours and knots.

Garcilasso de la Vega and other historians of that conquest report that the Peruvians applied those strings for performing and keeping most complicated and most extensive accounts. Thus they stated the number of births and deaths; enumerated men able to bear arms in each province, munitions, stocks of foodstuffs and other elements of civil and military administration. Such numerical details are only collected in some states of the 19th-century Europe.

[5] Those examples and many others which we will provide elsewhere incontestably prove that statistics had existed from times immemorial but remained an unnamed science like political economy, zoology, geology and so many other branches of human knowledge of the first rank. Since all through the centuries it has been a public necessity for every country, the principal civilized people of the globe practised its operations during three or four thousand years. Nevertheless, we should recognize that it had almost always been empirically applied for answering the requirements of the moment without defining it, or restricting its scope or classifying the objects it covered according to their similarity, or studying which method it ought to follow, and without answering the following questions:

What kind of operations constitute its investigations; which means should it apply for numerically establishing each social fact important for the interests of the country; which arrangements and methods of joining tabular numerical terms most evidently render the certitude of facts; which trials can distinguish proper numbers in the data from defective or fraudulent numbers; what advantages are provided to statistics by introducing the language of numbers and numerical analysis in civil, administrative and political transactions; what kind of errors are mixed up with the true results and how to defend ourselves from the ensuing mistakes; what obstacles are occasioned in statistical work a) by ignorance which hinders less when depreciates its results rather than pretends to assist the work; b) by indifference whose repose is troubled by requirements; c) by interests afraid of its light; d) by the feelings of the system which wrongly appreciates its estimates; e) and by a thousand fortuitous circumstances which are opposed to the success of its operations or at least render them laborious and difficult.

The solution of these questions provides the essential elements of our science and it can be reasonably thought that they, the questions, had likely been thoroughly examined long ago and that, if the antiquity did not deal with them, at least our inquisitive century had studied them. A grave mistake! These questions were not even posed and until now almost always statistics is considered as a science that intuitively reveals itself to its partisans rather than recognised as a branch of knowledge which, like the other branches, can only be mastered by studying, practice and instruction.

People wrongly consider its origin; it is incompletely defined; the system of its operations is not described; its methods had not been subjected to enlightened criticism; and finally, its scattered elements were not yet joined, enumerated or rationally grouped as required by the laws of logic.

[6] Official duty instructs us to fill in, at least as it is in our power, these gaps harmful for the progress and application of statistics. We devote our next pages to satisfying this requirement and draw on our 40 years of statistical work while serving the country as stipulated by the orders of public authorities. In this work [of 40 years] we proposed

1) To warn young statisticians against the uncertainty of the path which they ought to follow in their first assignments.

2) To stimulate those who, living in some smallest town or even in a rural commune, and having at their disposal the local archives, registers of civil status, market price-lists and other documents whose numbers are worthy of interest, – those who nevertheless believe themselves unable to accomplish any statistical work.

3) To appeal to the departments for their assistance in present and future statistical research by allowing to use such sources as depositaries of old manuscripts containing numerical information about a multitude of important and curious objects, especially meteorological observations, Tables of wages in remote times, expenses of education in colleges, assurances, arrangements of lease [of land], former prices of transportation and the duration of the voyages, wages of labourers and artisans at different epochs and many other particular statistical matters which can not be studied otherwise.

4) To guard the publicists against numbers of unknown origin, events engendered by circumstances and statistical compilations insulting both science and truth and published in view of a mercantile gain.

5) To prove how unanimous the most enlightened European governments are now patronizing statistics and are usually applying its works for guiding their administrative and political operations.

6) And finally, to maintain the hope that statistics ever more merits its successes and the honour of its participation in state affairs not only by the higher rectitude of its numbers, but also by the elevated essence of its works which should be inspired by the attachment to the public weal and effectively contribute to the amelioration of the lot of humankind.

Chapter 2. Classification of Statistics

[7] The great European states have such vast territories, so numerous populations and belong to the civilization which renders their societies so complex, that the execution of their statistics is very difficult. It is not so at all for secondary states such as Belgium or the Kingdom of Sardinia only equal to five or six our départements. Indeed, in explorations of such nature the obstacles become more serious with the increase of the numbers to be researched and established.

It is therefore a false idea as recently expounded to compare the statistics of those states done on a small scale with that of France with a territory of 0.53 *mln* km^2 and a population of 35 *mln* inhabitants. We should not flatter ourselves with hope for covering that immense area without guiding our work by a powerful method such as analysis and rational classification such as a systematic separation of the objects.

Industry remains unsuccessful when wishing to accomplish everything all at once and it flourishes when labour is divided and itself specialized for each of its branches. The same is true for statistics. It fails when attempts to attain immediate success. A century ago the intendants of Louis XIV and [later] Napoleon's prefects had suffered a set-back for the same reason. They only accomplished partial and scattered statistics without any connections between them and therefore incapable of providing general results. They embarrassed all France which actually was proposed by their aim.

Our time will profit from those two unsuccessful experiences and by being taught that at first a plan of a simplest possible statistics should be drawn up. Then the work should be carried out in consecutive parts [parties; by consecutively studying the necessary subjects] with an appeal to all quarters for necessary materials. That method is equally suited for the statistics of an empire, a département or a province. Applying it with perseverance, we executed the statistics of France which remained impossible for so long.

[8] That system of work is so natural and logical that no one has mentioned its use; it seems that everyone believes that no other can be adopted. However, this was its first application, and it is quite contrary to what is done in England or in France during the reign of Louis XIV. According to the new system, the different parts of the statistics follow in the order that establishes the connections which logically exist between its diverse objects (sujets). Each of these constitutes an item and comprehensively treats some matter separated and subdivided according to the requirements demanded by its volume, elementary composition and lucidity.

Now, we briefly sketch the separation of statistics according to this method. **1.** Territory. **2.** Population. **3.** Agriculture. **4.** Industry. **5** and **6.** Home and foreign trade. **7.** Navigation. **8.** Colonies. **9.** Public administration. **10.** Finances. **11.** Military forces. **12.** Justice. **13.** Public instruction[12].

[9] The territory is the native soil with its associations, the fatherland with its affections, the estate with its powerful interests, the agricultural domain with the labour which is the people's fortune. And still, this first element of a country is not thoroughly or completely known. With a great deal of trouble we found out the area of France's territory. For exactly fixing that term we should wait for the conclusion of our cadastral survey. At the times of Charles IX and Louis XIV our territory had been exaggerated by 50 and 25% respectively. Uncertainty still equals some hundred [square] leagues [1 *league* = 4 *km*]; in England it is many [square] miles and Russia is an empire the errors of whose estimated territory are of the order of its territory.

For determining the area of a country, very delicate and very numerous scientific operations are necessary. They demand deep knowledge acquired by many people. Astronomers are needed for tracing a meridian and fixing the directions to the benchmarks, and geodesists, for executing the main triangulation and determining the altitude of the relief[13]. Then, many surveyors for measuring the areas of properties and filling in the intervals in the triangular network, and finally for backing it all up, agents, draughtsmen, inspectors, couriers, wardens, managers. All these form such an expensive administration that many European countries do not anymore have the means to pay for, or even organize great enterprises of that kind.

And still, many other operations are needed for describing the physical state of a country. Levelling for constructing railways and laying out irrigational systems; determining the volume and rapidity of water currents for regulating their regimes; exploring the country for mapping the minerals; drilling boreholes and tapping water for domestic use, watering plants, ensuring the work of machines, etc. Then, long and numerous meteorological observations for finding out the power of the elements of climate and its action on agricultural products and public health.

Statistics carefully compiles the numerical data provided by those operations, classifies them in analytical tables which make known

1) The physical state of the geographical regions: their [the ridges'?] direction, their borders, heights, mountains, rivers, and geological structures of the various kinds of the terrain.

2) The climate: mean and extreme temperatures; the rainfall that waters plains and mountains; air pressure and other meteorological elements.

3) The physical division of the territory: the areas of mountainous regions, plains, valleys, arable lands, pastures and forests.

4) The political and administrative division, former and present.

France is the European state whose territorial statistics is the most advanced. In some years it will hopefully become complete and satisfactory. Among the recent advances we should praise the general map compiled by the military depositary and the geological map which we owe to the knowledge and perseverance of Elie de Blaumont and Dufrenoy. We ought to appreciate these magnificent maps all the more since they are still the only ones. The kingdoms which are playing a great part in contemporary history did not yet accomplish any of those investigations, which constitute the necessary foundation of the amelioration required by public prosperity.

[10] Population is the soul of a country, its strength, power, richness and glory, – if well and successfully governed. Without satisfying this rare and difficult condition, the more is the population growing, the more it is distressed of which Ireland is a vivid example.

Being an object of all social interests, the population is the basis of statistical operations and the expression measuring their results. The inhabitants of a country should be counted for establishing what they ought to obtain from the land for their subsistence and for determining the forces by which they will oppose their enemies. And should we suppose that the first known censuses had occurred 40 centuries ago, and is it evident that in those times there had only been a relevant Egyptian tradition whose origin is lost in the mist of time.

It does not suffice for the public economy only to know the number of inhabitants; it is also important to reveal the distinct parts making up that multitude, their shares, the movements which act on it, and especially the conditions of its consecutive renewal, its increase or decrease. For coming to know these particulars statistics studies

1) The population, its more or less remote former and present states and compares them.
2) The home movements of the population: births, deaths, marriages in towns and rural areas and the country as a whole.
3) The civil status of individuals: unmarried, married, widowers and widows, babies born in and out of wedlock.
4) The sex ratio at birth, at death, during life, of the widowed, all this for each civil status.
5) The age structure of the living and dead.
6) The ordinary mortality owing to usual and epidemic diseases and accidental and violent.
7) The mean yearly increase in the number of inhabitants.
8) The former and recent difference between the original races, creeds and social conditions.
9) The individual political capacity in accordance with the requirements imposed by law.
10) The nature and value of [land] property distributed by the categories of the owners according to the essence of the property.

Even today much is lacking in the statistical data of the most advanced people of Europe, always something is wanting. In France, that is the age and the profession of individuals; in England, their civil status and even the sex of individuals is not provided. In Portugal, the stoves are counted rather than people; Spain allowed half a century to pass without censuses of the population. In France before the revolution registration of births, deaths and marriages had been the duty of the church, and only 57 years ago it was turned over to municipal administrations. In other Catholic countries civil acts are still tucked away in vestries. In England, only seven years ago that greatly important public service had been transferred from the priests and religious dissident communities and assigned to a special administration charged with the compilation of acts in each locality and the concentration of the data on the movements of the population.

These divergences should not surprise us. Very long ago, under the Roman dominion, an imperial edict, which prescribed a census or any other measure of public utility, sufficed for extending their execution over 50 provinces each of them as great as our modern kingdoms, and, taken together, constituted the entire civilized world [of Europe]. However, during the Middle Ages Europe became parcelled out by the feudal power in a multitude of territories of sovereign princes and governed as though by good will but actually by caprices and arbitrary and violent wishes of the noblemen, owners of both the land and its inhabitants.

The monarchies formed by conquering all those tiny states had been unable to do away with the innumerable diversities[14] with a language unintelligible to the others. These monarchies, although the needs of their people had been the same, were not at all similar to each other, which could not have been otherwise. The rivalry and incessant wars inspired them with a perpetual aversion to everything done by their neighbours, and besides their arrogance spurned the most advantageous ameliorations such as a decimal system for the coins, unification of weights and measures, triangulation of territory, its administrative division into approximately equal units, cadastral surveying, censuses, statistical and geodetic operations and many other measures beneficial for the society.

Nevertheless, a long peaceful period allowed many governments to understand better the interests of their people and for some years a successful progress was achieved, especially in England, Prussia and many parts of Germany. Regrettably, however, we ought to say that the states of Southern Europe remained stationary, alien to the application of science as much as they are ignorant of its benefits.

[11] Agriculture is the main interest of the people, but, owing to an inconceivable fatality, it is the least known and the most neglected. In France, the registration of agricultural fertility had been vainly demanded for more than two and a half centuries. A relevant plan was imagined and prepared by (par) Louis XIV and Napoleon. Three times during the best epochs of governing the country, its execution had begun, but always proved unsuccessful owing to the method of evaluating everything at once, both blindingly and obstinately. They thought it possible to derive the volume of production for the entire kingdom by issuing either from the gross yield of a square mile, from the number of ploughs or, rather, by supposing that with 6521 communes covered by cadastral surveying the other 30,730 will not differ from them at all. These inductive methods were due to Vauban, Lavoisier and Chaptal respectively[15].

It is certainly not by applying similar conjectures that the agricultural production of France was estimated in its general statistics. An official investigation of the 37,300 communes was executed. It established the volume of the rural production and its value. That gigantic enterprise required six years of work and persistently demanded a classification whose lucidity enlightened the great mass of materials. For achieving, if at all possible, that important goal, those responsible studied immense collections of official numbers which had existed then and exist now and established

1) The area occupied by each kind of culture.

2) Its seeding in volume and value.

3) Its yearly production, total and per hectare.

4) The value and the price of that production per département and total.

5) The consumption of the agricultural products per locality and inhabitant, and for the entire kingdom.

6) The trade in these products, home and foreign.

Also consecutively examined were

1) Cereals totally, and their kinds separately.

2) Wines and vodka.

3) Various cultures: alimentary, industrial and horticultural.

4) Pastures: natural meadows, artificial meadows, unploughed land.

5) Woods and forests belonging to the crown, to the state, to individuals.

6) Finally, the agricultural domain in general in its actual state and formerly, at different memorable epochs in the history of the country.

The second part of the investigation treated the bred domestic animals. They were numbered by species, sex, age and locality, their value, profitableness, number of, and price for those slaughtered for consumption, their gross and net weight, quantity of meat of each variety consumed by an inhabitant in each district (arrondissement) and département of the kingdom.

That immense work concluded by a general recapitulation of the various branches of the production and the relevant profits during a mean year. The final result is the numerical total agricultural richness of the kingdom, a main fact to be studied for many generations of economists and statisticians. Its completion was only possible by successfully carrying out a long and difficult investigation to which we all are obliged. The coherent classification applied in that persevering work can not be appreciated by comparison since it still is the only one of its kind in Europe. The execution of that work proved the possibility of determining by rational operations the agricultural production of a country with a territory of 0.53 $mln\ km^2$. It is an example whose utility is apparently recognized and admitted by eminent statesmen of countries best prepared for carrying out such enterprises.

[12] Industry, that king of our century whom science certainly still did not honour either as a historical event or as a statistical object. All that has been said until now about its production and the relevant estimates of quantities and values in England and France are more or less brave conjectures. This certainly means that a classification of the branches which comprise that immensely interesting subject is lacking or at least no traces of that procedure are left.

We know well enough how remote is the reality from speculative plans neither restricted nor depressed by the innumerable obstacles encountered in practice when researching the truth. Nevertheless, while accomplishing those grand investigations connected with the statistics of France, the participants had arrived at the statistics of industry. Today, after concluding a half of these operations, we can present a classification of a wide scope sanctioned by their results. Here it is.

Industry is separated in two structures quite different in importance but having similar aims, production of everything answering the real or imagined needs of the society:

Manufactories and mining industry; handicraft

Both are distributed by regions, départements, districts and communes so that actually this is the industrial geography of the country. Then, they are grouped and numbered according to the nature of their products. For example, all the coal mines of a département form a single item; all the smelting houses, another item. All the mills spinning flax, cotton and wool, are appropriately joined, etc. This is veritably the statistics of industry. All its parts are separated in three sections according to the nature of the treated elements:

Mineral; vegetable; animal products

Each series [?] enumerates the manufactured or mined products from the simple to the complicated. Thus, the land and the products resulting from its mining are the first ones; then follow the metals according to the volume of labour required by their various transformations. In the second and the third sections the tissues occupy the last places.

Each article of each branch of industry undergoes two series of numerical research: value and quantity. The values are those of the patents, locations, primary materials and end products. The quantities concern both the primary materials and end products accompanied by their partial and total prices.

In addition to these special indications pertaining to each establishment and comprising the statistics of production, there is the inventory of the forces which dispose everything: the number of workers by sex, age, daily wages of each, as well as the industrial movables: motors, water mills, windmills, horse-driven mills, steam engines, animals; as furnaces, blast-furnaces, forges; as lathes, generators etc.

This recapitulation indicates industrial products in all details:

1) By district, département and region.

2) By products mined or manufactured.

3) By series of products the elements of whose production are similar or the results analogous.

Unlike agricultural products, the industrial products are not restricted to a domain of natural things; on the contrary, being aided by the inventive talent of our century, they traverse the boundless regions of human intelligence. Nothing is more difficult than tracing a logical classification closely covering and interconnecting them in the order of their affinity without ever underestimating the need to remain within the possibility of administrative execution of the investigation.

[13] Home trade constitutes the greatest movement of public wealth which can exist in a country. Banks, taxes, the value itself of the money in circulation are only insignificant as compared with that immense capital in kind, infinitely diversified in origin and form. That trade aims at satisfying all the real or imagined needs of the population beginning with daily sustenance and finishing with splendid spoils of luxury and mode.

All kinds of merchandise therefore perpetually circulate and their mass is everywhere proportional to the demand of the consumers with prices regulated by their available quantities. Wholesale and retail selling is going on in markets, indoor markets, shops and stores. Sold are

1) Home agricultural products.

2) Products of the manufacturing industry and handicraft less the commodities directly exported abroad but increased by those imported from abroad for consuming.

The necessary means for that trading are

1) Warehouses, fairs, Exchanges, banks, indoor markets, markets of every type.

2) Transport for cabotage and navigation through canals, streams and rivers; for highways, country roads and railways.

Formerly the nature and value of the objects of home trade had to be determined since toll was demanded at each step. Nowadays, the circulation of merchandise and the trade in them is free so that their quantities are not known exactly and neither is their value estimated comprehensively. The opposing difficulties are insurmountable.

If wishing to base the reckoning on transportation, an immense mistake will be caused by the mass of products of every nature sold on the spot, at the place of origin without any transportation allowing the establishment of its quantity. When choosing as that basis the agricultural and industrial products, we are led to miscalculate since their great part is consumed by the producers themselves, is not sold, and does not enter the home trade. When adopting consumption as the initial point the same cause will lead to the same result, and we are thus unable to find out the home commercial movement either by the statistics of transportation, or production, or consumption although all such studies are indispensable for our aim.

Even that is not all. Such essential work is yet only done in France. Other countries do not even have any statistics of handicraft which is necessary for a general investigation of the home trade. We see that nothing is ready for that enterprise and that much time will have to pass before it becomes possible to contemplate it. It is therefore unnecessary to study here how the home trade should be classified.

[14] Foreign trade does not encounter the same obstacles to its study and among all the branches of statistics it is the best known. Custom-houses encircle each state, levy duty on importing and even exporting merchandise of every kind and thus become active agents of the investigation. Established by the treasuries, they are serving science without wishing, and often without even imagining it.

Financial interest connected with their operations ensures exactitude although in many states their greed engenders a dangerous adversary: the smuggling that withdraws a part of the merchandise from government taxes and all scientific registration as well.

Foreign trade is naturally separated in two great sections, importation and exportation. In turn, these are also subdivided:

1) Merchandise, imported for consumption and exported, again for consumption, and provided by the land or industry of the country. It comprises the special trade of importation and exportation.

2) Merchandise imported and deposited in storehouses together with the exported but not provided by the land or industry of the country. It comprises the general trade of importation and exportation.

From the viewpoint of the origin and destination the special commerce is separated as follows.

1) Importation of colonial products and foreign merchandise.

2) Exportation of merchandise to colonies and foreign countries.
Another important division applicable to all the trade distinguishes the merchandise according to the nature of transportation:

1) Goods imported or exported by land.

2) Same, by sea.

However, the most important and the most luminous classification is that which shows importation and exportation

1) By countries of origin and destination.

2) By merchandise according to the nature and object of each.

In the first case each country of the globe is comparatively shown in particular annual Tables which indicate the transactions in quantity and value and also note the duties levied by the custom-houses. In the second case the numerical history of each agricultural and industrial merchandize is shown and accompanied by the variations of its importation and exportation experienced under the different regimes of the custom-houses to which it was subjected.

Those statistical tables are certainly the most interesting for studies by statisticians and merchants and it is evident that the most successful lessons can be thus easily obtained. The merchandise is methodically classified as follows.

1) Concerning importation: objects necessary for the industry. Main natural objects for consumption. Main manufactured objects for consumption.

2) Concerning exportation: main natural and manufactured products. Here, both the agricultural and industrial products are separated according to the trade with colonies and foreign countries.

When treating foreign trade in any respect, it is important to compare the numbers for a succession of years. Indeed, without collecting the testimony of the past and the present, statistics will only feebly explain and corroborate the present by the previous.

[15] Navigation. This branch of statistics only exists in, and is very important for the exploration of the states of Western and Southern Europe. It is not difficult to collect the data and regularly coordinate them. Navigation is here understood as concerning the merchant ships rather than the navy. Three objects comprise this subject, materials, personnel and the navigation itself.

1) Material is the entirety of the merchant ships whose condition at different epochs indicates their loss or advance. To be found out are the number of ships, their ages, ports of registration, the strength of their ordinary crews, the newly built, the extinctions, their yearly distribution by series of displacements from 1000 to 30 tons.

2) The personnel by ages, rank, service record, port of registration.

3) The annual movements, calling at and leaving ports, the numbers, displacement and crews of the ships sailing from the colonies or foreign countries or going there, and the same details except origin and destination for the high-sea and river cabotage and fishing. These movements ought to be general and cover as many consecutive years as possible. Other similar Tables will indicate the alterations in the navigation for each port.

All European maritime powers, even England, lack a historical statistics of their commercial navigation before the 13th and 14th centuries. This is possible to remedy and would be curious indeed.

[16] At first, **the colonies** had been the remote possessions of the European maritime powers destined to ensure them an exclusively advantageous trade. A century ago events had destroyed that system and changed the distribution of those overseas possessions. England acquired an enormous [?] number of them, France still retained some. Spain and Holland lost much, but what remained is worthy of envy whereas other European countries have nothing or very little.

Colonies are provinces separated from the metropolises and their administration is difficult and important. It is essential to explore them carefully so that they will possess statistics of good quality. A better knowledge of their transatlantic [?] possessions would have possibly prevented the discord between England and Spain. And, had France better known its colonies, it would have derived more benefit. If executed conscientiously and ably, colonial statistics should be therefore ranged among the most useful enterprises.

Each such statistics should form a single whole comprised of the same parts as the general statistics of the European countries except the trade whose classification ought to be somewhat modified owing to the complexity imposed by the proper interests of the metropolis and those connected with the colony in the degree of the extension experienced by the introduction of merchandise from foreign countries.

[17] Public administration forms a part of statistics which throws the brightest light on the daily discharge of duties by the authorities. It covers the institutions of public utility and classifies them in the following way.

1) Political establishments: voters[16], elections, jurymen, the Chamber of Deputies, the Chamber of Peers.

2) Financial establishments: the Bank of France, other banks, savings banks, pension funds, life insurance offices, other insurance institutions.

3) Welfare establishments: kindergartens, shelters, homes for foundlings, hospitals and hospices, mental hospitals, welfare offices, workrooms in convents, pawnshops.

4) Establishments of repression: départemental prisons, reformatories, agricultural colonies for young prisoners, poorhouses, detention centres, prisons for those sentenced to hard labour, colonies for the deported.

There is no collection of works embracing all these subjects for any European country except France which had recently published the statistics of its establishments of welfare and repression. It describes the situation and movements in them, mortality of people living there, the expenses of these establishments, the value of work done there, and curious details about the origin of the convicted, their ages, previous and present specialities, the crimes they committed, recidivism, the degree of their instruction etc[17].

The publication of these details efficiently contributes to the improvement of the situation in these public establishments and, for example, since hospital mortality is not anymore a gloomy secret, it does not stop lowering owing to generous care and efforts.

[18] Finances are as though nerves of modern life. In the excess and poor distribution of taxes they show an imminent cause of misery, bankruptcy and revolutions. Their statistics is known under the names of Budget and reports on the expenses, in parliamentary acts etc. However, they are overburdened by details which will be suppressed in a special publication. In addition, for compiling comprehensive tables it will be necessary, when studying anything, to estimate its quantity, then take into account the values and collect the data belonging to previous epochs [as well].

Financial statistics is naturally separated into three main sections:

1) Ordinary and extraordinary revenue.

2) Public expenses.

3) Registered and floating national debt.

The first chapter (!) enumerates all kinds of taxes as well as their yearly value, their distribution per locality and inhabitant. Under the second head all the expenses should be registered with their various destinations per ministries. Finally, the third section is a résumé of the movements of the national debt, its increase or decrease and its level at various epochs. Financial statistics should include studies of the moneys in circulation accompanied by a table of new emissions of coin, paper money and other assets.

[19] Military forces securing the state's independence are formed by two very different sections

1) Army.

2) Navy.

Considered in each is the personnel and its material with the means of its conservation and increase, expenses in peace and war. All this is constantly debated and studied down to minutest elements and there is no obstacle to uniting the well-known numbers, so this branch of statistics is certainly the least difficult if only the state does not conceal it all.

[20] The administration of **justice** presents one of the most interesting objects of statistics: the knowledge of the number of crimes and criminals, their nature, means of perpetration and the punishments inflicted on them. Beginning with 1825, France provides an example of such a curious study[18] which enables the calculation of the danger experienced by people and property in the war thrust on them by perversity, vice and misery. That continuing [from 1825] and progressively improved work is worthy of the highest appraisal and we can not do better than to refer to its yearly systematic classification of that very complicated matter.

[21] Public instruction allows us to expect the appearance of a more instructed generation, probably better than ours. This subject has the right to occupy a place among the most curious subjects of statistics. It yearly shows the sexes, the establishments (!), the nature of the institutions, the schools of the state, its colleges and academies, special, professional and other instruction. It concludes by studying institutions from the five classes of the Institut[19] and public libraries, museums and the periodic press, one of the most active means of public instruction, provided that it fulfils its mission.

[22] Capital cities. In our time, the centres of civilization are so mighty, the places of commerce so rich, and the populations of cities so numerous and condensed, that that subject ought to be treated separately and comprise a special chapter of statistics. Here, it is convenient to consider it as a separate entity and, without leaving its confines, to glance at the same subjects as those required by an empire and studied numerically. We will most certainly discover the means for such an investigation as though carrying out a study of a province [not an empire?].

[23] In concluding this chapter, I remark that the classification of the subjects is determined by the existence, discovery and unification of materials. By rearranging the preparatory operations it often occurs that, instead of beginning a statistics by long and difficult research of these subjects, we waste our time and expend our diligence and ardour by distressingly constructing their classification without knowing whether we will be able to treat them or whether the collected anticipated materials are sufficient.

We usually suppose to be perfectly knowledgeable about our subjects and the manner of work. After beginning it, preoccupation with this supposition will prove fruitless. It is more important and wise to postpone the ranking and separation of the subjects until deciding by profound examination which acquisitions we have made, what kind of developments can be accomplished, what subdivisions is it possible to adopt and how wide are the boundaries within which it should be necessary to restrict ourselves.

Notes

1. This is wrong, recall Süssmilch and Quetelet (and even Graunt) as well as Laplace (1814/1995, p. 62):

Let us apply to the political and moral sciences the method based on observations and the calculus, a method that has served us so successfully in the natural sciences.

2. Does the author agree? C. Schlözer (1827, p. 11) stated that it was necessary to establish *general historical principles*. A bit below the author remarked that statistics *calculates and studies*, but did not add … *causes and effects*. There also, it is unclear why discussions in geography are unlike those studies. Half a page below the author mentions geographical theories.

3. The author almost repeated himself.

4. *Napoleon, as the First Consul, fervently supported the Statistical Bureau* [of France]. *However, after becoming Emperor, he suspiciously regarded publication of statistical data. The Bureau fell into decay* (A. I. Chuprov 1910, p. 60).

5. That statistics *remained a science of learned men* etc is doubtful. In Sweden, a state that the author mentioned just above, the situation had certainly been different (Nordenmark 1929). The author's reservation which followed next does not entirely remedy his remark.

6. This list is not in accord with the author's Chapter 2.

7. Kendall (1960) remarked that the word *statistics* had appeared in Italy in 1589.

8. The numbers in that source were apparently provided not more precisely than to within 10. In one case which described the redemption of the first born the number was 22,273. The subsequent drawing of lots deserves a special mention (Sheynin 1998, pp. 192 – 193).

9. Did the author really think that the term *political arithmetic* had been known in antiquity?

10. This is definitely wrong. The aims had been incomparably narrower and the methods, certainly primitive. The document compiled by Augustus (see just below) was a *balance sheet of the Roman Empire* (Kendall 1960) and it is also remarkable since ancient science had been qualitative.

11. The introduction of our present system of numeration *had been one of the most important merits of the Baghdad school* (Rosenfeld & Youshkevitch 1970, p. 209). It occurred in the first half of the 9th century.

12. At the end of Chapter 2 the author added another item. This is one of quite a few instances of his carelessness.

13. Suffice it to say that the author was ignorant of such astronomical and geodetic work.

14. Here is the end of that sentence: *et l'on pourrait en citer, qui se composent de 60 provinces* etc. Suppose that *qui* should have been *que*, but the *provinces*?

15. The author understandably denies sampling which had not been recognized as a scientific method until the beginning of the 20th century. However, he could have recalled Laplace's sampling estimate (1786) of the population of France largely repeated in the *Théor. Anal. Prob*.

16. Voters became establishments …

17. This as well as the appropriate place in § 20 is extremely superficial. Did the author read Poisson (1837)?

18. The author refers to the *Comptes* (1825 and later).

19. The author meant the Institute of France which was comprised of five academies.

Brief Information about Mentioned Personalities

Augustus, 63 BC – 14 AD, Founder and first emperor of the Roman Empire
Blaumont J.-B. A. L. L. Elie de, French geologist, 1798 – 1874
Büsching A. F., 1724 – 1793, geographer, historian, educator, theologian
Chaptal J.-A., 1756 – 1832, French chemist and statesman
Charlemagne, Charles the Great, 742 or 748 – 814. From 800 emperor of Western Europe
Charles V, 1500 – 1558. King of Spain from 1516, Emperor from 1519
Charles IX, 1550 – 1574. King of France
Cortes Hernando de Monroy y Pizarro, 1485 – 1547. His expedition caused the fall of the Aztec Empire
Diodorus Siculus (Sicilian), wrote between 60 and 30 BC
Louis XIV, Louis the Great, Le Roi-Soleil (Sun King), 1638 – 1715
Montezuma II, 1466 – 1520, Ninth emperor of the Aztec Empire
Petit-Dufrenoy O.-P.-A., French geologist, 1792 – 1857
Pizarro Francisco González, 1471 or 1476 – 1541, conqueror of the Inca Empire
Vauban S. Le Prestre de, 1633 – 1707, military engineer
Yu the Great, ca. 2200 – 2100 BC, Chinese emperor

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IX

N. Bernoulli
Letter to P. R. Montmort, 23 January 1713

Montmort (1708/1713, pp. 388 – 394)

I am sending you a list of the babies of each sex born in London from 1629 to 1710 with my proofs of what I have written you[1] about the argument according to which it can be proved that it is miraculous that during 82 years in succession the numbers of babies of each sex born in London do not differ more from each other. It is impossible that during such a long time they had always been contained by chance within such narrow boundaries as indicated in the list showing these 82 years.

I state that [nevertheless] there is nothing to be surprised at and that there is a high probability for the numbers of boys and girls to be contained within still narrower boundaries. For proving this, I suppose that the number of all the babies born yearly in London is 14,000, 7200 boys and 6800 girls if only these numbers exactly follow the ratio 18:17 which expresses the facility of the birth of both sexes.

Since the number of boys is sometimes greater and sometimes smaller than 7200, let us assume a limit. For example, take the year 1703 when the number of girls was nearest to the number of boys, 7683 and 7765, or 6963 and 7037 if their sum is reduced to 14,000. The number of girls thus exceeded 6800 by 163, and the number of boys was equally less than 7200. And I will prove that it is quite possible to bet on the [yearly] number of boys not being either larger or smaller by 163 than 7200.

This means that the ratio of boys to girls will not be larger than 7363:6637, or smaller than 7037:6963. To prove this, let us imagine 14,000 dice with 35 faces each, 18 white and 17 black. You know that the terms of the binomial (18 + 17) raised to the power of 14,000 indicate all the possible cases for obtaining with those 14,000 dice any number of white faces. The first term shows all the cases for all the faces to be white; the second and the third terms, for obtaining 1 and 2 black and 13,999 and 13,998 white faces, etc.

The 6801-st term therefore expresses all the cases for getting exactly 6800 black and 7200 white faces; the 6638-th and the 6964-th terms, 6637 and 7363 white and 6963 and 7037 black faces. It is therefore required to determine the ratio between the sums of all the terms from the 6638-th to the 6964-th inclusive and of all the other terms, those not exceeding the 6638-th and larger than the 6964-th. However, all these terms are extremely large, and, for calculating this ratio, a special trick is needed. Here is how I am doing it.

Let the number of all the babies be n rather than 14,000, the facilities of the births of a boy and a girl be as $m:f$ and the limit l rather than 163. Denote also $p = n/(m + f)$ rather than 18:17, so that $n = mp + fp$. In our example, $mp = 7200$ and $np = 6800$. At first, I look for a very close approximation of the ratio of the $(fp + 1)$-st term to the $(fp - l + 1)$-st term. By the law of the course of these terms, the former is

$$C_n^3 \ldots \frac{n-fp+1}{fp} m^{n-fp} f^{fp},$$

and the latter,

$$C_n^3 \ldots \frac{n-fp+l+1}{fp-l} m^{n-fp+1} f^{fp-1},$$

and the ratio of the former to the latter is

$$\frac{n-fp+l}{fp-l+1}\frac{n-fp+l-1}{fp-l+2}\frac{n-fp+l-2}{fp-l+3}\ldots\frac{n-fp+1}{fp}\left(\frac{f}{m}\right)^l.$$

Replacing $(n-fp)$ by np [by mp], it becomes

$$\frac{mp+l}{fp-l+1}\frac{mp+l-1}{fp-l+2}\frac{mp+l-2}{fp-l+3}\ldots\frac{mp+1}{fp}\left(\frac{f}{m}\right)^l. \qquad (1)$$

Then I suppose that all the factors in the left side except the last one, $(f/m)^l$, are in a geometric progression and their logarithms, in an arithmetic progression. This supposition is very near to reality, especially if n is a large number. The sum of all these logarithms is therefore

$$\frac{l}{2}\left(\lg\frac{mp+l}{fp-l+1}+\lg\frac{mp+1}{fp}\right),$$

which is the sum of the logarithms of the first and the last factor multiplied by half the number of all the terms. Adding the logarithm of $(f/m)^l$ equal to $l\lg f/m$ we have

$$\frac{l}{2}\left(\lg\frac{mp+l}{fp-l+1}+\lg\frac{mp+1}{fp}\right)+l\lg\frac{f}{m}=$$

$$\frac{l}{2}\left(\lg\frac{mp+l}{fp-l+1}+\lg\frac{mp+1}{fp}+\lg\frac{fp}{mp}\right). \qquad (2)$$

And this is the logarithm of the ratio sought. The ratio itself is therefore

$$\left(\frac{mp+l}{fp-l+1}\frac{mp+1}{fp}\frac{fp}{mp}\right)^{l/2}. \qquad (3)$$

If a better approximation is needed, we can separate that sequence of factors

$$\frac{mp+l}{fp-l+1}\frac{mp+l-1}{fp-l+2}\frac{mp+l-2}{fp-l+3}\ldots$$

into many parts and suppose that the factors of each are in a geometric progression, but this is not necessary since the values determined in accord to these suppositions will very little differ one from another and even although the first supposition leads to a slightly larger ratio than it is, that excess will be much less considerable as compared with what I will neglect further.

If I consider now the terms fp and $(fp - l)$ which immediately precede the terms $(fp + 1)$ and $(fp - l - 1)$, their ratio will be

$$\frac{mp+l+1}{fp-l}\frac{mp+l}{fp-l+1}\frac{mp+l-1}{fp-l+2}\ldots\frac{mp+2}{fp-1}\left(\frac{f}{m}\right)^{l},$$

which is larger than (1) or

$$\left(\frac{mp+l}{fp-l+1}\frac{mp+1}{fp}\frac{fp}{mp}\right)^{l/2}$$

since each factor of the first sequence is larger than the corresponding factor of the second. Just the same, the ratio of the terms $(fp - 1)$ and $(fp - l - 1)$ is larger than the ratio of fp and $(fp - l)$; $(fp - 2):(fp - l - 2)$ larger than $(fp - 1):(fp - l - 1)$ etc until we reach the first term.

This is why, if we separate all the terms preceding the term number $(fp + 1)$ in classes each containing l terms, and, beginning with the term fp, calculate the ratio of the first term of the first class to the first term of the second class, it will be larger than the ratio of $(fp + 1):(fp - l + 1)$ and the ratio of the second term of the first class to the second term of the second class will be still larger, etc. And so, the ratio of all the terms of the first class taken together to all the terms of the second class taken together will be larger than the ratio of $(fp + 1)$ to $(fp - l + 1)$. The ratio of all the terms of the second class to all the terms of the third class will be still larger; and the ratio of all the terms of the third class to all the terms of the fourth class will be yet larger, that is, larger than (3).

Call this expression q and denote by s the sum of the terms of the first class. Then the sum of the terms of the second class will be smaller than s/q, the sum of the terms of the third class smaller than s/q^2, of the fourth class, s/q^3 etc. The sum of all the classes, be their number infinite, except the first one will be smaller than

$$s/q + s/q^2 + \ldots,$$

that is, smaller than $s/(q - 1)$.

It follows that the ratio of the sum of the first class, i. e., of all the terms between those of numbers $(fp + 1)$ and $(fp - l + 1)$ including also the latter, and the sum of all the preceding terms is larger than

$$\left(\frac{mp+l}{fp-l+1}\frac{mp+1}{fp}\frac{fp}{mp}\right)^{l/2}-1.$$

Therefore, replacing *f* by *m* and *m* by *f*, we see that the ratio of the sum of all the terms between $(fp + 1)$ and $(fp + l + 1)$ including the latter and the sum of all the subsequent terms including the last one is larger than

$$\left(\frac{fp+l}{mp-l+1}\frac{fp+1}{fp}\frac{mp}{fp}\right)^{l/2}-1. \qquad (4)$$

And finally the ratio of the sum of all the terms between those $(fp - l + 1)$ and $(fp + l + 1)$, including them both but even without the middlemost term $(fp + 1)$ and the sum of all other terms is larger than the smaller of the two magnitudes (3) and (4), QED.

Let us now apply this to our example in which $n = 14{,}000$, $mp = 7200$, $fp = 6800$, and $l = 163$ and we will find that (4) is equal to

$$\frac{163}{2}[\lg\frac{7363}{6638}+\lg\frac{7201}{7200}+\lg\frac{6800}{7200}]=$$

$$163/2[0.0450176 + 0.0000603 - 0.0248236] = 1.6507254.$$

The number having this logarithm is $44_{74/100}$; replacing *mp* by *fp* and *fp* by *mp* we will find

$$\frac{l}{2}\left(\lg\frac{fp+l}{mp-l+1}+\lg\frac{fp+l}{fp}+\lg\frac{mp}{fp}\right)=$$

$$\frac{163}{2}[\lg\frac{6953}{7038}+\lg\frac{6801}{6800}+\lg\frac{7200}{6800}]=$$

$$163/2[-0.0046529+0.0000639+0.0248236] = 1.6491199.$$

The number having this logarithm is $44_{58/100}$ and I conclude that the ratio of the probability that among the 14,000 babies the number of boys will not be either larger than 7363 or smaller than 7037 and the probability of the contrary is at least higher than $44_{58/100}$. And it will be advantageous to bet that the number of boys will not be beyond those limits more than three times in 82 years.

However, when examining the list of babies born in London during 82 years, we find that the number of boys 11 times exceeded 7363, – in 1629, 39, 42, 46, 49, 51, 59, 60, 61, 69, 76. You will also easily determine that it is possible to bet more than 226 against 1 on the number of boys not exceeding those limits 11 times in 82 years. You will also note that, when assuming a limit greater than 163, but smaller than the largest contained in the list, I will determine a probability much higher than 43:1 that each year the number of babies of both sexes is contained rather within than beyond it.

It is not therefore surprising at all that, just as I wished to demonstrate, the numbers of babies of both sexes are not more different from each other. I recall that my late uncle had proved a similar statement in his *Ars Conjectandi* now in the press in Basel, that when wishing to discover something by often repeated trials, the numbers of cases in which a certain event can occur or not, we can increase the number of observations so that finally the probability that we have discovered the real ratio existing between the numbers of those cases will be higher than any given probability. When his book appears, we will see whether in such matters I have found as good an approximation as he did.

I have the pleasure of remaining with perfect esteem your etc.

Note

1. See Bernoulli's letter to Montmort of 11 Oct. 1712 (Montmort 1708/1713, p. 374).

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X

P. S. Laplace

Application of the Preceding Studies to the Analysis of Chances

Application des recherches précédents à l'analyse des hasards,
this being a section (pp. 144 – 153) of
Recherches sur l'intégration des équations différentielles finies et sur leur usage dans la théorie des hasards (1776).
Œuvr. Compl., t. 8. Paris, 1891, pp. 69 – 197

The present state of the system of nature is evidently the sequel to its preceding moment. And, if we imagine an intelligence that embraces all the relations between the beings in this universe existing at a given instant, it will be able to determine, for any time past or future, the respective positions, the movements and generally the connections between those beings[1].

Physical astronomy which, of all branches of our knowledge, shows the greatest respect to the human mind, offers an idea, although imperfectly, of what such intelligence would have been. The simplicity of the laws which move the celestial bodies and the relations of their masses and distances allow the analysis to follow up in a certain measure their movements. It suffices geometry to observe the position and velocity of the system of those great bodies at some moment for determining their state in the past and future centuries.

Man has therefore the advantage of calculating by applying the power of his instruments and a small number of relations. However, the ignorance of the different and complicated causes which jointly produce the events coupled with the imperfection of the analysis prevents us from pronouncing our conclusions about very many phenomena with the same certainty.

It follows that for us there exist uncertain, more or less probable matters. It is impossible to comprehend them and we are therefore attempting to make this up by determining their different degrees of likelihood and are thus indebted to the feebleness of our mind for a most delicate and most ingenious mathematical theory, the science of chances or probabilities[2].

Before going any further, we ought to fix the meaning of these words, *chance* and *probability*[3]. We regard a thing as an effect of chance if it does not offer our eyes anything regular or a sketch [of itself] and in addition if we are ignorant of the causes which produced it. In itself, therefore, chance is not real; it is only a term proper to denote our ignorance about the manner in which the various parts of a phenomenon are related to each other and to the rest of the nature.

The notion of probability includes this ignorance. If we are sure that one of the two events which can not coexist necessarily occurs, but do not see any reason for one of them to arrive rather than the other, the existence and non-existence of each is equally probable. And if one of the three events mutually excluding each other necessarily occurs and we see no reason for any of them to arrive rather than another, their existence is equally probable, but the non-existence of each is more probable than its existence in the ratio of 2:1 since out of three equally probable cases there are two favouring the non-existence and only one contrary to it.

If the number of possible cases remains the same, the probability of an event increases with the number of favourable cases; on the contrary, if the number of favourable cases remains the same that probability decreases as the number of possible cases increases. Probability is therefore directly proportional to the number of favourable cases and inversely proportional to the number of all the possible cases.

The probability of the existence of an event is only the ratio of the number of favourable cases to all the possible cases if we see no reason for one of the cases to arrive rather than another one. It is therefore represented by a fraction whose numerator is the number of the favourable cases, and the denominator, of all the possible cases. Similarly, the probability of the non-existence of an event is the ratio of the contrary cases to all the possible cases and should therefore be expressed by a fraction whose numerator is the number of the contrary cases and the denominator, the number of all the possible cases. It follows that the probability of the event's existence joined with the probability of its non-existence makes up a sum equal to 1 which therefore expresses entire confidence since an event should evidently either arrive or fail.

Then, a thing necessarily occurs if all the possible cases favour it, and the fraction expressing its probability is therefore itself 1. Certitude can thus be represented by unity, and probability, by a fraction of certitude. It can approach unity ever closer and even differ from it less than by any given magnitude, but it can never become larger. The goal of the theory of chances is to determine these fractions, and it thus supplements the uncertainty of our knowledge in the best possible way.

Certitude and probability, as we defined them, are obviously comparable and can be subjected to a rigorous analysis. However, suppose that all the possible cases favour an event or, alternatively, that it is found that many of them are contrary to it, then these instances [possibilities] are absolutely incomparable and it is impossible to say that the first is twice or three times greater than the second since truth is indivisible.

Here we arrive at the same thing that is encountered in all physical and mathematical sciences. We measure the intensity of light, the different degrees of the temperature of a body, their (?) forces, their resistances, etc. In all these studies the objects of analysis are the physical causes of our sensations rather than the sensations themselves.

The probability of events serve for determining the expectation or the fear of those who are interested in their existence and it is from this viewpoint that the science of chances is one of those most useful for civil life. The word *expectation* has various meanings. Ordinarily it expresses the state of the human mind when some blessing should happen owing to certain suppositions which are only likely. In the theory of chances expectation is the product of the expected sum by the probability of obtaining it. For distinguishing the two meanings, I call the former *moral*, the latter, *mathematical* expectation[4].

Suppose that n people have an equal probability of getting a sum a, and that that sum should certainly belong to one of them. The total probability of that event is 1, or certitude and the probability of each of those people getting it is evidently $1/n$ and the mathematical expectation, a/n. This is also the sum which should be given to [each of] them if they wish to share the entire sum without running the risk connected with the event.

If one of those people has a double probability as compared with each of the others, his mathematical expectation and therefore the sum that should be given him in the former case will consequently be also twice larger. Indeed, if $(n + 1)$ people have the same probability to obtain the sum a, then their probability of getting it is $1/(n + 1)$, and the mathematical expectation, $a/(n + 1)$. If now one of them gives up his claims and his expectation to A, then A acquires a double probability, and a double expectation, $2a/(n + 1)$. When sharing the total sum he should get twice more than each of the others.

We see that mathematical expectation is just the partial sum to be obtained when those people do not wish to run the risk connected with the event and when supposing that the share of the expected sum is proportional to the probability of obtaining it. Actually, it is the only equitable way to share the sum when leaving aside all alien considerations since equal degrees of probability lead to equal rights to the expected sum.

Moral expectation, just like the mathematical expectation, depends on the expected sum and the probability of obtaining it, but it is not always proportional to the product of these two magnitudes. Regulated by a thousand variable circumstances, it is hardly ever possible to define and still less subject to analysis. True, these circumstances only increase or decrease the advantages provided by the expected sum, so we may regard moral expectation as the product of that advantage by the probability of obtaining it, but we should distinguish the relative and the absolute value of the expected blessing. The latter is completely independent from the needs and other reasons for desiring that blessing whereas the former increases according to various motives.

We are unable to offer any determinate method for appreciating that relative value. Here, however, is a very ingenious rule proposed by the illustrious Daniel Bernoulli (1738): the relative value of a very small sum is proportional to its absolute value divided by the entire fortune of the interested person. That rule is not, however, general but it can serve us in a great many number of circumstances which is all that can be desired in that matter.

Most of what is written about chances apparently confuses moral expectation and probability with mathematical expectation and probability or at least regulates one by another. It was thus desired to extend the provided theories, which was impossible since then they become obscure and barely satisfy the minds accustomed to the rigorous clarity of geometry. D'Alembert proposed very shrewd objections to them and thus turned the attention of geometers to this matter. When a large number of circumstances are involved, the absurdity to which the results of the calculus of probability leads those authors, becomes realized as also, consequently, the need to distinguish in these matters the mathematical and the moral. That calculus is indebted to him for being now based on clear principles and restricted to its veritable limits[5].

Permit me to digress about the difficulties to which the analysis of chances is apparently susceptible. The probability of uncertain things and the expectations connected with their existence are, as I said, the two objects of that analysis. The distinction established above between the moral and the mathematical expectations answers, as it seems to me, all the possible objections to the latter, so let us therefore examine those which are levelled against the former.

When studying the probability of events, we evidently start from the principle according to which the probability is the number of favourable cases divided by all the possible cases. The difficulty can only concern the supposition of an equal possibility of two unequally possible cases and we ought to agree that the application of the calculus of probability to objects of civil life made until now is subjected to that difficulty.

I suppose, for example, that the coin used in the game *heads* or *tails* has a tendency to land on one side rather than on the other but that the gamblers do not know which side is favoured. It is evident that they can equally bet on the arrival of each. We may therefore suppose, as it is done usually, that at the first throw both outcomes are equally probable. This supposition is not however permitted anymore, for example, if one of the gamblers bets on the appearance of *heads* in two throws. The unequal possibilities of *heads* and *tails* ought to be taken into consideration although we do not know which side appears more frequently.

Indeed, an inequality always favours the gambler who bets on the *heads* not to appear in two throws since the probability of that event is higher than in the case in which the equal probability of both outcomes does not exist. Among the cases *heads, heads*; *heads, tails*; *tails, heads*; and *tails, tails* the first and the last are more probable than the other ones. Suppose that the probabilities of the two outcomes of a throw are $(1 \pm w)/2$ [and $0 < w < 1$]. Then *heads, heads* will have probability $(1 \pm 2w + w^2)/4$. Having no reason to prefer one of the possibilities rather than the other, both probabilities should be added and the sum divided by two. So the probability of *heads, heads* (and *tails, tails*) will be $(1 + w^2)/4$. A similar calculation results in probability $(1 - w^2)/4$ for either of the two other possibilities, QED[6].

What is stated about *heads* or *tails* can be applied to dice games and generally to all games in which the different events are prone to physical inequality. I have sufficiently developed this remark elsewhere (1774), and here I only note that, although not knowing which of those events is more probable, we can almost always remarkably determine to whom of the gamblers is this inequality advantageous.

The theory of chances also supposes that if *heads* and *tails* are equally possible, the same will hold for all the combinations *heads, heads, heads*; *tails, heads, tails*; … Many philosophers think that this supposition is not exactly true and that the combinations in which an event occurs many times in succession are less probable than the others. However, we then ought to suppose that past events somehow influence those that should occur later, which is not admissible at all. Actually, in the ordinary course of nature the events are intermixed, but only since, as it seems to me, because the combinations with such events are much more numerous. Here, however, is an apparently difficult problem which it is proper to discuss.

If *heads* appears for example 20 times in succession we are strongly tempted to believe that it did not happen by chance, whereas, if *heads* and *tails* are somehow intermixed, we certainly do not look for a cause. So why are these two cases different if one of them is not physically less probable than the other?

I answer in general that, when we note a symmetry, we always admit the effect of a cause acting orderly and thus deliberate according to probabilities. Indeed, symmetry is produced either by chance or a regular cause, and the latter supposition is more probable. Let $1/m$ be the probability of the former, and $1/n$, of the latter. Then (1774) the existence of that cause has probability

$$\frac{1/n}{1/m+1/n}=\frac{1}{1+n/m}$$

and we see that the larger is m as compared with n, the higher is the probability of the latter supposition. It is not since a symmetric event is less possible than the others, but because we can bet much more that it was due to an orderly acting cause than to chance and therefore try to discover that cause.

Here is a very simple example explaining this remark. Printed letters lying on a table form the word INFINITESIMAL[7]. The reason that leads us to believe that that arrangement was not accidental can not be due to its being physically less possible than the others. Indeed, had not that word be used in any language, it would have been neither more, nor less possible but we will not then suspect an act of some particular cause. However, we do use that word and it is incomparably more probable that someone had disposed those letters, that that arrangement was not due to chance.

I return now to my subject. The uncertainty of human knowledge concerns either the events or their causes. If we are sure, for example, that an urn only contains white and black tickets in a given proportion and the probability of extracting a white ticket by chance is required, the event is uncertain, but the cause on which depends the probability of its existence, i. e., the ratio of the white and black tickets, is known. In the following problem the event is known but the cause is not.

An urn supposedly contains a given number of white and black tickets. A white ticket is drawn and it is required to determine the probability that the proportion of the white and black tickets is p:q. The event is known but the cause is not.

We can reduce all problems depending on the theory of chances to these two classes. Actually, there exist a very large number of problems in which both the cause and the event seem unknown, for example:

An urn is equally supposed to contain 2, 3, …, or n white and black tickets. Determine the probability that two extracted tickets will be white.

The proportion of the white and black tickets, their total number and the event which should follow, are all unknown. However, here we ought to consider as the cause of the event the equal probability of all the numbers from 2 to *n* and the indifference of the colours of the tickets[8]. The problem therefore belongs to those in which the cause is known but the event is not.

I did not at all intend to provide here a complete treatise on the theory of chances and am satisfied to apply the previous researches to the solution of many problems of that theory. I even restrict my attention to those of them in which the cause is known and it is required to determine the events and I (1774) have earlier considered the case in which it was proposed to establish the events given the causes [read: … the causes given the events].

Notes

1. Laplace's later statement (1814/1995, p. 2) is more generally known. He did not, however, admit an existence of any such intelligence. Anyway, there exist unstable and even chaotic movements and the utmost importance of the latter has been recently recognized. Maupertuis (1756, p. 300) and Boscovich (1758, § 385) had formulated similar statements earlier (Boscovich: calculations will then be possible *to infinity on either side*) but disclaimed such a possibility.

2. Actually, the existence of the theory of probability is due to the need for studying the regularities of chance in mass trials (observations). Laplace (1814/1995, p. 3) left a related statement: *Probability is relative in part to our ignorance and in part to our knowledge*.

3. Very much can be added about each of these terms. I myself have published two papers (1991; 2011) about the former and I noted Bayes' considerable merit as the main forerunner of Mises (2010).

4. Laplace (see below) understood moral expectation more generally than Daniel Bernoulli. Later, however, he (1812/1886, p. 189) restricted his attention to the Bernoulli's proposal and once more suggested to apply the term *mathematical expectation* for distinguishing it from the then topical new notion. This latter adjective, which took root in the French and Russian literature, had long ago become obsolete.

5. Laplace had certainly prettified D'Alembert (Sheynin 2009, § 6.1.2). For one thing, the latter was one of the philosophers who wrongly reasoned on the probability of symmetric events (see below).

6. Although admitted by Hald (1998, p. 192), this reasoning is certainly wrong. Laplace himself (1812/1886, p. 411) stated the opposite: the probability of (*heads, heads* or *tails, tails*) is higher than the probability of the two other possible outcomes and it is advisable to bet on the former compound event.

7. Laplace repeated this statement, first in his *Leçons de mathématiques* of 1795 delivered at the Ecole normale, published in 1812 and reprinted in his *Oeuvr. Compl.*, t. 14. Paris, 1912, see p. 163, then in his *Essay* (1814/1995, p. 9), but replaced *infinitesimal* by *Constantinople*. His example was due to D'Alembert (1767) who had wrongly discussed it.

8. Indifference of the colours was not stipulated. However (Laplace, for example 1814/1995, p. 116), hypotheses should not be considered to be true and ought to be continually corrected by new observations.

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XI

P. S. Laplace

Lectures in Mathematics. Lecture 10 (Fragments)[1] on Probabilities

Laplace P. S. (1812), Leçons de mathématiques
données à l'Ecole normale en 1795.
Œuvr. Compl., t. 14. Paris, 1912, pp. 10 – 177.
Dixième séance. Sur les probabilités, pp. 146 – 177

Page 146. For following the programme in mathematics which I myself sketched, I should have additionally discussed the differential and integral calculus involving finite and infinitely small differences (calcul différentiel et integral aux différences soit finies, soit infiniment petite); mechanics; astronomy; and the theory of probability. The short duration [of studies] at the Ecole normale does not permit that at all. However, concerning mechanics and astronomy, I proposed to compensate the impossible lectures by publishing a treatise called *Exposition* … [1796] in which, independently from the analysis, I will present the sequence of discoveries made until now about the system of the world. Today, I devote the last lecture to the theory of probability, interesting both in itself and owing to its numerous relations with the most useful social matters.

All events, even those which because of their insignificance seem to be independent from the great laws of the universe, are their corollaries as necessary as the rotation of the Sun. Being ignorant of the ties which join them with the entire system of nature, we believe that they depend on final causes or chance depending on their regular or apparently confused appearance and succession. However, these imaginary causes have been retreating one after the other with the boundaries of our knowledge and finally disappear in the face of sober philosophy which only perceives them as an expression of our ignorance of their veritable causes.

We will become convinced in this important result of the advance of enlightenment when recalling that in bygone times floods, great draughts, comets trailing very long tails, eclipses, northern lights, and generally all the extraordinary phenomena had been regarded as signs of celestial wrath [Laplace (1814/1995, p. 3)].

Page 157. When considering a very large number of events, the formulas to which we come are composed of so many terms and factors, that their numerical calculation becomes impractical. It is therefore necessary to be able to transform those formulas in converging series. I provided a suitable method based on transforming functions of very large numbers into definite integrals and application of rapidly converging series[2]. And it is remarkable that the integrands are generating functions of the functions expressed by the integrals. The theories of generating functions and the approximations of functions of very large numbers can be thus considered as two branches of the same calculus which I named the *Calculus of generating functions*.

The boundaries of the probability of the results and the causes indicated by a large number of events as well as the laws according to which that probability approaches its boundaries as the events multiply can be easily determined when applying that calculus. This research, the most delicate in the theory of chances, merits the attention of geometers by the analysis that it requires, and of the philosophers by showing how the regularity becomes established even in matters which seem to be entirely given up to chance and how we reveal in them hidden although constant causes on which that regularity depends[3].

Page 161. *When observations or experiences are multiplied indefinitely their mean result converges to a fixed term* [number]. *Therefore, when choosing an arbitrary small interval extending over both sides of that term, the probability that the mean result becomes finally contained within that interval will differ from certitude less than by any assignable magnitude.*

That term is verity itself provided that positive and negative errors have the same facility[4]. *And generally it is the abscissa of the curve of the facility of errors corresponding to the centre of gravity of that curve's area*[5] *if only the origin of coordinates coincides with the origin of the errors.*

The mean result of a large number of future observations will thus almost coincide with that of a large number of already made observations.

Pages 168 – 169. It is noted that there are more women than men in spite of more boys being born than girls. In countries with a constant population[6] its ratio to yearly births equals the number of years in the mean life. That duration is therefore larger for women than for men either because of their constitution or since they are exposed to lesser dangers.

The mean life will obviously increase with wars becoming rarer, with better and more generally extended welfare, and if man will somehow render our land [environment] healthier and get rid of some diseases and decrease their danger.

This last-mentioned circumstance happened with smallpox, one of the most destructive scourge of mankind. Ingeniously applying the calculus of probability, Daniel Bernoulli [1766] discovered that inoculation sensibly increases the mean life even if supposing that one person out of 200 dies from that procedure. Inoculation is therefore undoubtedly beneficial for the state. However, who wishes to be inoculated, ought to compare the very low but immediate danger of dying with a very much higher but protracted danger of dying from natural smallpox.

Although the former does not exist for the state which only considers masses of citizen, it is not so for individuals. Nevertheless, properly accomplished inoculation kills so little people and the ravages of natural smallpox are so great, that the interest of an individual joins the interest of the state in adopting inoculation. The head of a family[7], whose attachment to his children increases with them [with their number? their ages?], certainly should not waver when subjecting them to an operation which delivers all of them from worry and the danger of such a cruel disease and ensures him the fruit of his care and their education. I do not therefore hesitate to advise this salutary practice of inoculation and I regard it as one of the most advantageous results obtained by medicine through experience.

Pages 169 – 170. Games are as much immoral as these institutions [annuities, tontines] are beneficial for the morals by favouring the peaceful tendencies of nature in the greatest possible measure. And the capitals being insignificant and remaining idle in the hands of each individual become productive, nourish the business going on in the large establishments which receive them, and, owing to the bulk of the joined capital ensure a certain benefit if the capital is properly understood (conçus) and wisely managed.

There is nothing inconvenient and similar to what we have remarked about even the most fair games in which losses are more sensitive than gains. On the contrary, the capital provides the means for changing from surplus to sure future resources. The government should therefore encourage these establishments and show regard to their vicissitudes since the hope they provide concerns remote future so that they can only prosper when being delivered from any unease about their continuance.

Pages 172 – 173. When wishing to correct one or many already approximately known elements [unknowns] by a set of a large number of observations, we compile *conditional equations*, as they are called. The analytical expression of each observation is a function of the elements. Substitute their approximate values together with their [unknown] corrections in each observation, expand the obtained expressions in series neglecting the insignificant squares and products of the corrections and equate the series to the observations which they represent. Thus we get conditional equations connecting the corrections of the elements.

Each observation provides such an equation. Had the observations been exact, it would have sufficed to obtain as many of them as there are elements. However, since observations are always corrupted by errors, we consider a large number of observations so that their errors will almost compensate each other in the mean results.

The observer ought to choose the most favourable circumstances for determining the elements [for observation] whereas the art of the calculator consists in combining the conditional equations provided by the observations in the most advantageous way and thus in reducing their number to the number of the elements. All possible combinations come to multiplying each equation by a particular factor and summing up those products. This will provide the first final equation[8] relative to the applied factors. A second system of factors will lead to a second final equation etc. until as many of them are compiled as there are elements.

The most precise correction will be evidently found if the system of factors is chosen in such a manner that for each element the mean error to be feared in either direction is minimal. The *mean error* should be understood as the sum of the products of each error to be feared by its probability. The investigation of that minimal value, one of the most useful in the theory of probability, requires singular analytical tricks. I will only say that here we are led to a remarkable result: the most advantageous way to combine the conditional equations consists in bringing the sum of the squares of the observational errors to a minimum which will provide as many final equations as there are corrections sought[9].

Pages 173 – 174. Let us imagine a hundred men gathered indiscriminately and asked to decide *whether the Sun daily rotates about the Earth*. We have good reason to believe that a majority will decide affirmatively and this conclusion will become still more probable when a thousand or ten thousand men are gathered instead of a hundred. Simple common sense allows us to infer that it is extremely important as a public matter that instruction be much extended and that the national representation [the parliament] be consisted of an elite of fair and enlightened men.

Truth, justice, humanism, – such are the eternal principles of the social order which should only rest on earnest relations of man with his fellow creatures and nature. They are also as necessary for maintaining that order as the universal gravitation for the existence of the physical order. The most dangerous error is the belief that we may sometimes deviate from those principles and deceive or enslave people for their own good fortune. Fatal experience had proved that those sacred principles can never be violated with impunity.

Notes

1. Greatly extended, this Lecture 10 formed Laplace's *Essay* (1814). I am only translating those pieces which he had not included in that later contribution.

2. I had not seen the paper Kurdiumova (1972) which possibly discusses that method. The Editors referred the readers to Laplace's *Oeuvres Complètes*, tt. 9, 10 and 12.

3. Laplace thus indirectly formulated one of his aims.

4. See my paper Sheynin (2007).

5. Many authors had been using this loose expression.

6. Did Laplace know any such country?

7. Did Mother have any say? In 1848, Buniakovsky (Sheynin 1991, pp. 216 – 217), when discussing the dread of cholera, had also failed to mention Mother.

8. Laplace's final equation was not necessarily a normal equation as understood nowadays.

9. The last lines could have only appeared in 1811 or 1812, after the publication of two papers by Laplace (or during their preparation).

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XII

P. S. Laplace

Statement about the *Théorie analytique des probabilités*, either forthcoming or just published

Conn. des Tem[p]s, 1812 for 1815, pp. 215 – 221

1. We can not better describe that work than by copying the announcement made by the author himself at its beginning[1](au commencement).

I propose to provide an analysis and formulate the principles necessary for solving problems about probabilities. That analysis is comprised of two theories which I have been discussing for 30 years in the *Mémoires de l'Académie des Sciences*, of the *Theory of generating functions* and the *Theory of approximating functions of very large numbers*. They are treated in Book 1 in which I describe them even more generally than in those *Mémoires*. Their rapprochement clearly shows that the latter is only an extension of the former and that they can be considered as two branches of the same *calculus of generating functions*, as I named it, which is the foundation of my *Théorie des probabilités* treated in Book 2.

The problems concerning events occasioned by chance most often easily lead to linear difference and partial difference equations and the first branch of the calculus of generating functions provides the most general method of integrating this kind of equations. However, when a large number of the considered events is available, the expressions to which we arrive consist of so many terms and factors that their numerical calculation becomes impractical. It is therefore necessary to have a method for transforming them in convergent series. This is what the second branch of the calculus of generating functions achieves the advantageously the more necessary this method becomes.

2. [This section is also the *Introduction* to the first edition of the *Théorie analytique*.] I aim at presenting here the methods and general results of the theory of probability and I especially treat the most delicate, most difficult and at the same time the most useful issues of this theory. I try my best to determine the probabilities of causes and of the results indicated by a large number of events and look for the laws according to which those probabilities approach their limits as the events are multiplied.

This study merits the attention of geometers owing to the analysis that it requires. It is here that the theory of approximating functions of large numbers finds its most important applications. Then, this study interests the observers by indicating the means that should be chosen of the results of their observations and the probability of errors which are still to be feared. Finally, it merits the attention of philosophers by showing how regularity becomes established even in matters which seem to be entirely given up to chance and by revealing the hidden but constant causes on which that regularity depends. It is this regularity of the mean results of a large number of events on which such various establishments, as annuities, tontines and life insurance, are founded. The problems relative to them [?] such as smallpox vaccination or decisions of electoral assemblies do not offer any difficulties when my theory is applied. Here, I restrict my attention to resolving the most general, so that the importance of these matters for civil life, the moral issues which complicate them and the numerous observations which they imply require a special contribution.

When considering the analytical methods which were already born owing to the theory of probability and those which will additionally appear; the fairness of the principles which serve as its foundation; the rigorous and delicate logic required by its application for solving various problems; the establishments of public utility which rest on this same theory; if finally noting that it provides opinions as surely as possible which can guide our judgements and teaches us to guard ourselves against illusions which often bewilder us even in those matters which could not be subjected to analysis, – when taking all this into account, you will see that there certainly does not exist any other science either worthier of our deliberations or whose results are more useful than the theory of probability.

Its birth was due to two French geometers of the 17th century, of the period so fruitfully producing great men and great discoveries, and perhaps honouring the human mind higher than any other century. Pascal and Fermat proposed and solved some problems in probability and Huygens brought together these solutions and extended them in a small treatise. After him the Bernoullis[2], Montmort, De Moivre and many celebrated geometers of the recent period considered that subject more generally.

3. The work that we are announcing includes everything important done in that branch of human knowledge which the author had perfected either by the generality of his analysis or by the novelty and difficulty of the problems which he solved. Among these numerous problems those which concern the means which ought to be chosen of the results of observations as well as probabilities of phenomena, of their causes and of future events derived from those observed, – all those problems should particularly draw the attention of geometers.

After describing how often had the observations taught the analysts by making them feel the need to rectify their approximations and how did it happen on its own by considering probabilities of large periodic secular inequalities in celestial motions, the author continues:

It is seen now how necessary it is to study attentively the indications of nature when they result from a large number of observations even if they seem to avoid explanation by known means. Therefore, I invite astronomers to follow with a particular attention the secular lunar inequality which depends on the sum of the longitude of the Moon's perigee and twice the longitude of its nodes and which is already very likely indicated by observations. If the sequence of observations continues to verify it, geometers will be compelled to turn once more to the lunar theory and allow for the possible difference between the northern and southern hemispheres of the Earth. That difference seems to be mostly dependent on it.

And we may say that nature itself assists with perfecting the theories resting on the universal law of attraction which in my opinion is one of the most convincing proof of the verity of that admirable principle.

4. [The following passage repeated in the author's *Essai* (1814/1995, pp. 60 – 61) discusses the discovery of causes acting on the *organic matter* and *animal magnetism*. Then Laplace stated that the existence of the *inexplicable*, whose denial is *unphilosophical*, can become obvious if its existence is proved by observations.]

5. The same analysis can be extended into various results of medicine and political economy and even into the influence of moral causes. Indeed, the action of these causes when repeated many times provides their results the same regularity as that of physical causes.

6. [Repeated on p. 56 of the *Essai* is this phrase:]

One of the most remarkable phenomenon in the system of the world is that the planets and their satellites move almost circularly in the same direction and almost in the same plane whereas the comets move in very eccentric orbits indifferently in both directions and anyhow inclined to the ecliptic.

7. Count Laplace analysed the probability of the existence of that singular phenomenon if supposed to be due to chance. He found that that probability is an extremely small fraction and decided that that phenomenon indicated a particular cause with a probability higher than that of most historical facts about which no doubt is permitted. In his *Exposition* (1796) he showed that that cause can only be the solar atmosphere initially extending beyond the planetary orbits and that the cooling down and the attraction of the sun successively contracted it.

8. [On pp. 57 – 58 of the *Essai* Laplace, repeating his text of 1812, discussed the origin of stars and mentioned Michell's examination (1767) of *the particular disposition of some stars.* He described Herschel's findings (Dale, the translator of the *Essai*, provided the relevant references) who concluded that the Sun had *formerly* been *surrounded by a vast atmosphere* (of nebulous matter). Only in 1812 Laplace noted that Herschel and he himself made this inference *by opposite routes*.]

9. Herschel's fine researches justly deserve their due. We modify them in respect of his opinion about the cause of the rotation of the sun and the stars. A cluster of initially immovable molecules can not by contraction, as he seems to believe, produce a star capable of rotation. In his *Mécanique Céleste* Count Laplace showed that if, after being united, all the molecules begin to form a body possessing such a capability, the axis of rotation will necessarily be the line perpendicular to the invariable plane of maximal areas and passing through the centre of gravity of the entire mass. The rotation ought to be such that the sum of the projections on that plane of the areas circumscribed by each molecule invariably remains as it was initially. Therefore, that rotation will not exist if all the molecules were initially at rest.

My *Exposition* shows that that permanence of the areas maintains the uniformity of the earth's rotation and of the duration of the day which, in spite of the winds, the ocean currents and all the interior convulsions of the globe, had not varied from the time of Hipparchus even by $0.^s01$. However, in a nebula with numerous nuclei nothing prevents the resulting stars from rotating in different directions. Indeed, is it not true, as many celebrated philosophers have suggested, that universal attraction can not produce any permanent motion for a system of bodies initially at rest and that they should unite in the course of time around their common centre of gravity.

Herschel's research once more ensures him the right to be recognized by astronomers as did all his important discoveries since long ago. And one of the main ones was the discovery of Uranus and its six satellites which he was able to see through his telescope. Only two of them have been discerned by other observers. It is desirable that that celebrated astronomer publishes the observations he certainly made for establishing the existence of these celestial objects and determining their motion.

Notes

1. Laplace had repeated some parts of this text elsewhere and I have therefore subdivided it into sections and indicated the relevant sources. Sections lacking such indications were also mostly borrowed from somewhere as Laplace stated in his first lines (and confirmed by inverted commas omitted in the translation).

2. On p. 118 of the *Essai* (1814/1995) Laplace appropriately named Jakob (James, in the translation) and Nicolas Bernoulli. The translation of this source contains commentaries and a Bibliography which includes the works of Herschel mentioned below.

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XIII

P. S. Laplace

Théorie analytique des probabilités, four last sections of Chapter 4

21. Suppose now that there are two elements, z and z_1, whose corrections should be determined by a set of a large number of observations. Form conditional equations[1] just like in § 20; they can be written in this general way:

$$\varepsilon_i = p_i z + q_i z_1 - \alpha_i.$$

Here, as before, ε_i is the error of the $(i + 1)$-st observation. When multiplying these equations by $m, m_1, \dots, m_{s-1}$ and adding up the products we get the first final equation[2]

$$\Sigma m_i \varepsilon_i = z\Sigma m_i p_i + z_1 \Sigma m_i q_i - \Sigma m_i \alpha_i.$$

Multiply then the same equations by $n, n_1, \dots, n_{s-1}$ and add up the products to obtain the second final equation

$$\Sigma n_i \varepsilon_i = z\Sigma n_i p_i + z_1 \Sigma n_i q_i - \Sigma n_i \alpha_i.$$

The symbol Σ, just like in § 20, extends over the values of i from 0 to $s - 1$. When supposing that both functions, $\Sigma m_i \varepsilon_i$ and $\Sigma n_i \varepsilon_i$, which we denote by (1) and (2), disappear, these two final equations lead to the corrections z and z_1. However, these corrections are susceptible to errors just as is the introduced supposition. Let us therefore imagine that those functions are not zero but l and l_1 and denote by u and u_1 the errors of the corrections z and z_1 determined as above. The final equations will become

$$l = u\Sigma m_i p_i + u_1 \Sigma m_i q_i, \; l_1 = u\Sigma n_i p_i + u_1 \Sigma n_i q_i.$$

Now we ought to determine the factors $m, m_1, \dots, n, n_1, \dots$ so that the mean error to be feared in each element becomes minimal. To this end we consider the product

$$\int\varphi(x/a)\exp[-(mv + nv_1)x\sqrt{-1}\,] \int\varphi(x/a)\exp[-(m_1 v + n_1 v_1)x\sqrt{-1}\,] \dots$$
$$\int\varphi(x/a)\exp[-(m_{s-1}v + n_{s-1}v_1)x\sqrt{-1}\,]$$

in which the symbol $\int$ covers all the values of x from $-a$ to a and $\varphi(x/a)$, as in § 20, is the probability of both errors, x and $-x$. After joining the two exponential functions corresponding to x and $-x$, the previous function becomes

$$2\int\varphi(x/a)\cos[(mv + nv_1)x]\cdot 2\int\varphi(x/a)\cos[(m_1 v + n_1 v_1)x] \dots\cdot$$
$$2\int\varphi(x/a)\cos[(m_{s-1}v + n_{s-1}v_1)x],$$

where the symbol ∫ extends over all the values of x from 0 to a. According to supposition, both x and a are divided into infinitely many parts of unit length.

The term not depending on these exponential functions in the product of the previous function by $\exp[-(lv_i - l_1v_i)\sqrt{-1}]$ is the probability that function (1) is equal to l and that at the same time the function (2) is equal to l_1. That probability is therefore

$$(1/4\pi^2)\iint dvdv_1\exp[-(lv + l_1v_i)\sqrt{-1}][2\int\varphi(x/a)\cos[(mv + nv_1)x] \ldots \times$$

$$2\int\varphi(x/a)\cos[(m_{s-1}v + n_{s-1}v_1)x],$$

where the integrals are taken over $[-\pi, \pi]$.

And now, wholly following the analysis of § 20, we will find that the previous function is almost exactly reduced to

$$\frac{1}{4\pi^2}\int\int dvdv_1\exp[-(lv + l_1v_1)\sqrt{-1}] -$$

$$\frac{k''}{k}a^2(v^2\sum m_i^2 + 2vv_i\sum m_in_i + v_i^2\sum n_i^2),$$

$$k = 2\int dz\varphi(z),\ k'' = \int z^2dz\varphi(z).$$

And it is also seen that both integrals can be taken over $(-\infty, \infty)$.

Assume now that

$$t = av + av_1(\Sigma m_in_i/\Sigma m_i^2) + kl\sqrt{-1}/(2k''a\Sigma m_i^2),$$

$$t_1 = av_1 - \frac{k}{2k''a}\frac{[l\sum m_in_i - l_1\sum m_i^2]\sqrt{-1}}{\sum m_i^2\sum n_i^2 - [\sum m_i\sum n_i]^2},$$

and let

$$E = \Sigma m_i^2\Sigma n_i^2 - (\Sigma m_i\Sigma n_i)^2,$$

then the double integral becomes

$$\exp[-\frac{k}{4k''a^2E}(l^2\sum n_i^2 - 2ll_1\sum m_in_i + l_1^2\sum m_i^2)]\times$$

$$\int\int\frac{1}{4\pi^2a^2}dtdt_1\exp[-\frac{k''t^2}{k}\sum m_i^2 - \frac{k''t_1^2E}{km_i^2}].$$

Supposing that these integrals are also extended over $(-\infty, \infty)$, we arrive at

$$\frac{k}{4k''\pi a^2\sqrt{E}}\exp[-\frac{k}{4k''a^2}\frac{l^2\sum n_i^2 - 2ll_1\sum m_i n_i + l_1^2\sum m_i^2}{E}]. \quad (3)$$

To determine the probability that the values of l and l_1 are contained within given boundaries, we will now multiply this magnitude by $dldl_1$ and integrate it in those boundaries. Denoting the result by X, we see that the probability sought is $\iint Xdldl_1$. However, for getting the probability that the errors u and u_1 of the corrections of the elements are contained within given boundaries we should replace l and l_1 in the obtained integral by their values expressed through u and u_1. And, when differentiating the expressions for l and l_1 but considering l_1 constant, we will have

$$dl = du\Sigma m_i p_i + du_1\Sigma m_i q_i,\ 0 = du\Sigma n_i p_i + du_1\Sigma n_i q_i,$$
$$dl = du(\Sigma m_i p_i \Sigma n_i q_i - \Sigma n_i p_i \Sigma m_i q_i) \div \Sigma n_i q_i.$$

If then we differentiate the expression for l_1 but consider u constant, we will obtain

$$dl_1 = du_1\Sigma n_i q_i,\ dldl_1 = (\Sigma m_i p_i \Sigma n_i q_i - \Sigma n_i p_i \Sigma m_i q_i)dudu_1.$$

Introduce now

$$F = \Sigma n_i^2(\Sigma m_i p_i)^2 - 2\Sigma m_i n_i \Sigma m_i p_i \Sigma n_i p_i + \Sigma m_i^2(\Sigma n_i p_i)^2,$$

$$G = \Sigma n_i^2 \Sigma m_i p_i \Sigma m_i q_i + \Sigma m_i^2 \Sigma n_i p_i \Sigma n_i q_i -$$
$$\Sigma m_i n_i \Sigma n_i p_i \Sigma m_i q_i + \Sigma m_i p_i \Sigma n_i q_i,$$

$$H = \Sigma n_i^2(\Sigma m_i q_i)^2 - 2\Sigma m_i n_i \Sigma m_i q_i \Sigma n_i q_i + \Sigma m_i^2(\Sigma n_i q_i)^2,$$

$$I = \Sigma m_i p_i \Sigma n_i q_i - \Sigma n_i p_i \Sigma m_i q_i$$

and the function (3) becomes

$$\frac{k}{4k''\pi a^2\sqrt{E}}\exp[-\frac{1}{4k''a^2}\frac{k(Fu^2 + 2Guu_1 + Hu_1^2)}{E}].$$

Integrate this expression with respect to u_1 over $(-\infty, \infty)$. If

$$t = [u_1 + (Gu/H)]\sqrt{kH/4k''} \div a\sqrt{E}$$

when calculating the integral with respect to t over $(-\infty, \infty)$, and considering only the variation of u_1, we will have

$$\int\sqrt{\frac{k}{4k''\pi}}\frac{du}{a\sqrt{H}}\exp[-\frac{ku^2}{4k''a^2}\frac{FH - G^2}{EH}].$$

But $(FH - G^2)/E = I^2$ so that that integral becomes

$$\int \frac{1}{\sqrt{H}} \frac{du}{a} \sqrt{\frac{k}{4k''\pi}} \exp[-\frac{k}{4k''} \frac{I^2u^2}{a^2H}].$$

According to § 20 the mean error to be feared in either direction in the correction of the first element[3] is obtained by multiplying the integrand by $\pm u$ and integrating over $[0, \infty)$. The error in either direction will be

$$\pm a\sqrt{H} \div I\sqrt{k\pi/k''}.$$

Let us now determine such factors m_i and n_i that the mean error will become minimal. When only varying m_i, we have

$$d\log(\sqrt{H/I}) = (dm_i/I)[-p_i\Sigma n_iq_i + q_i\Sigma n_ip_i] + (dm_i/H)[q_i\Sigma n_i^2\Sigma m_iq_i - n_i\Sigma m_iq_i\Sigma n_iq_i - q_i\Sigma m_in_i\Sigma n_iq_i + m_i(\Sigma n_iq_i)^2].$$

It is easy to see that the differential will disappear if the coefficients of dm_i are

$$m_i = \mu p_i,\ n_i = \mu q_i,$$

where μ is an arbitrary coefficient independent from i so that m_i and n_i can be natural numbers.

According to the previous supposition, the differential of $\sqrt{H/I}$ with respect to m_i [the appropriate derivation] is reduced to zero. We can similarly reduce to zero the differential of the same magnitude with respect to n_i. Our supposition thus leads to a minimal mean error to be feared in the correction of the first element. The same method, when replacing H by F, will evidently lead to the minimal mean error to be feared in the second element. The correction of the first element thus becomes

$$z = \frac{\sum q_i^2 \sum p_i\alpha_i - \sum p_i q_i \sum q_i\alpha_i}{\sum p_i^2 \sum q_i^2 - (\sum p_i q_i)^2}$$

[z_1 is obtained from z by interchanging p and q].

It is easily seen that this method holds for any number of elements sought by the MLSq of observational errors, that is, by the minimal value of

$$\Sigma(p_iz + q_iz_1 - \alpha_i)^2.$$

It follows that this method holds for any number of elements sought since the previous analysis can be extended to include the general case. According to § 20 we can suppose that

$$a\sqrt{\frac{k''}{k\pi}} = \sqrt{\frac{\sum \varepsilon_i^2}{2s\pi}}$$

where $\varepsilon, \varepsilon_1, \ldots$ are the residual free terms of the conditional equations left after the inclusion of the least squares corrections. The mean error to be feared in the first element will be

$$\pm\sqrt{\frac{\sum\varepsilon_i^2}{2s\pi}}\sqrt{\sum q_i^2} \div \sqrt{\sum p_i^2 \sum q_i^2 - (\sum p_i q_i)^2}. \qquad (4)$$

The mean error of the second element [is obtained by replacing q by p in the numerator]. It is therefore evident that the first element is determined better or worse than the second according to Σq_i^2 being smaller or larger than Σp_i^2. If the first r conditional equations do not at all contain q, and the last $(s - r)$ do not at all contain p, then $\Sigma p_i q_i = 0$ and the previous formulas coincide with the similar formula of § 20.

It is also possible to determine the mean error to be feared in each element calculated by least squares whatever is their number if only the number of observations is large. Let $z, z_1, z_2, z_3, \ldots$ be the corrections of the elements. We represent the conditional equations in the general case in the form

$$\varepsilon_i = p_i z + q_i z_1 + r_i z_2 + t_i z_3 + \ldots - \alpha_i.$$

For one single element that mean error is [see (4)]

$$\pm\sqrt{\frac{\sum\varepsilon_i^2}{2s\pi}} \div \sqrt{\sum p_i^2}. \qquad (5)$$

For two elements that error of the first element is obtained by replacing Σp_i^2 by $\Sigma p_i^2 - (\Sigma p_i q_i)^2/\Sigma q_i^2$ in formula (5) which leads to formula (4). For three elements the same magnitude is determined by (4) when replacing Σp_i^2 by $\Sigma p_i^2 - (\Sigma p_i r_i)^2/\Sigma r_i^2$, $\Sigma p_i q_i$, by $\Sigma p_i q_i - (\Sigma p_i r_i \Sigma q_i r_i)/\Sigma r_i^2$ and Σq_i^2, by $\Sigma q_i^2 - (\Sigma q_i r_i)^2/\Sigma r_i^2$. […]

Continuing in the same way, we can determine that magnitude given any number of elements. […] We are thus led to a simple method of comparing the precision of different astronomical tables [catalogues]. It can always be supposed that all of them are represented in the same form and therefore only differ by the assumed epochs, mean motions [of the celestial bodies] and coefficients of their arguments. If, for example, one of the tables includes an argument lacking in all the others, we can evidently assume that in those cases it is zero.

When comparing these tables with all the suitable observations and correcting them accordingly, then, as shown above, the sum of the squares of the remaining errors in the thus corrected tables will be minimal. Tables closest to obeying that condition deserve to be preferred. It follows that, when comparing different tables containing a large number of observations, opinions about their precision should be advantageous for those in which the sum of the squares of the errors is minimal[4].

22. Above, we supposed that the facilities of positive errors were equal to those of negative errors. Now, we consider the general case in which those possibilities can differ. Let a be the interval within which the errors of each observation can be contained. Suppose that it is divided into infinitely many, $n + n_1$, equal parts of unit length. Here, n and n_1 are the numbers of such parts corresponding to the negative and positive errors. Erect perpendiculars in each point of the interval *a;* they will represent the probabilities of errors and denote now the obtained ordinates corresponding to errors x by $\varphi[x/(n + n_1)]$. Then consider the sum

$$\varphi(-\frac{n}{n+n_1})\exp(-qnv\sqrt{-1}) + \varphi(-\frac{n-1}{n+n_1})\exp[-q(n-1)v\sqrt{-1}] + ... +$$
$$\varphi(-\frac{1}{n+n_1})\exp(-qv\sqrt{-1}) + \varphi(\frac{0}{n+n_1}) + \varphi(\frac{1}{n+n_1})\exp(qv\sqrt{-1}) + ... +$$
$$\varphi(\frac{n_1-1}{n+n_1})\exp[q(n_1-1)v\sqrt{-1}] + \varphi(\frac{n_1}{n+n_1})\exp(qn_1v\sqrt{-1})$$

which we represent as

$$\int\varphi(\frac{x}{n+n_1})\exp(qxv\sqrt{-1}),$$

where the symbol $\int$ extends over all the values of x in $[-n, n_1]$. According to § 21 the term independent from $\exp(v\sqrt{-1})$ and its powers in the expansion of

$$\exp[-(l+\mu)v\sqrt{-1}]\int\varphi(\frac{x}{n+n_1})\exp(qxv\sqrt{-1})\times$$
$$\int\varphi(\frac{x}{n+n_1})\exp(q_1xv\sqrt{-1})...\int\varphi(\frac{x}{n})\exp(q_{s-1}xv\sqrt{-1}) \qquad (6)$$

is the probability that the function

$$q\varepsilon + q_1\varepsilon_1 + + \dots + q_{s-1}\varepsilon_{s-1} \qquad (7)$$

equals $l + \mu$. It therefore takes the value

$$\frac{1}{2\pi}\int dv\exp(-lv\sqrt{-1})\exp(-\mu v\sqrt{-1})\int\varphi(\frac{x}{n+n_1})\exp(qxv\sqrt{-1})\dots \quad (8)$$

with the integral (l'intégrale) taken over $[-\pi, \pi]$. The logarithm of the function (6) with $l = 0$ is

$$-\mu v\sqrt{-1} + \log[\int\varphi(\frac{x}{n+n_1})\exp(qxv\sqrt{-1})] + ...$$

If n and n_1 are infinite and if

$x/(n + n_1) = x'$, $1/(n + n_1) = dx'$, $k = \int dx'\varphi(x')$,

$k' = \int x'dx'\varphi(x')$, $k'' = \int x'^2 dx'\varphi(x')$, …,

with the integrals extending over $[-n/(n+n_1), n_1/(n+n_1)]$, then

$$\int \varphi(\frac{x}{n+n_1})\exp(qxv\sqrt{-1}) = (n+n_1)k \times$$
$$[1+\frac{k'}{k}q(n+n_1)v\sqrt{-1}-\frac{k''}{2k}q^2(n+n_1)^2v^2+...].$$

The error of each observation ought to be contained within the boundaries $-n$ and n_1 and the probability that this indeed occurs is

$$\int\varphi[x/(n+n_1)] = (n+n_1)k,$$

and should be unity. It is therefore easy to conclude that the logarithm of the function (6) with) $l = 0$ is

$$[\frac{k'}{k}\sum q_i - \mu_1](n+n_1)v\sqrt{-1}-\frac{kk''-k'^2}{2k^2}(n+n_1)^2v^2\sum q_i^2]+...$$

with $\mu_1 = \mu/(n+n_1)$ and the sums covering all the values of i from 0 to $(s-1)$. The first power of v disappears if $\mu_1 = (k'/k)\Sigma q_i$ and if we only consider its square, which is possible according to the previous since s is a very large number, we will have for the logarithm of (6) with $l = 0$ [only the second term of the previous sum].

When going over from logarithms to numbers this function becomes

$$\exp[-\frac{kk''-k'^2}{2k^2}\sum q_i^2(n+n_1)^2v^2]$$

and the integral (8) will be

$$\frac{1}{2\pi}\int dv\exp(-lv\sqrt{-1})\exp[-\frac{kk''-k'^2}{2k^2}(n+n_1)v^2\sum q_i^2].$$

Let

$$l = (n+n_1)r\sqrt{\sum q_i^2},$$
$$t = \sqrt{\frac{(kk''-k'^2)\sum q_i^2}{2k^2}}(n+n_1)v - \frac{r\sqrt{-1}}{2}\sqrt{\frac{2k^2}{kk''-k'^2}}.$$

The variation of l is unity, therefore

$$1 = (n+n_1)dr\sqrt{\sum q_i^2}$$

and the previous integral, after calculating it over t from $-\infty$ to ∞, becomes[5]

$$\frac{kdr}{\sqrt{2(kk''-k'^2)\pi}}\exp[-\frac{k^2r^2}{2(kk''-k'^2)}].$$

And so, the probability that the function (7) is contained within boundaries

$$\frac{ak'}{k}\sum q_i \pm ar\sqrt{\sum q_i^2} \qquad (9)$$

is

$$\frac{2}{\sqrt{\pi}}\int \frac{kdr}{\sqrt{2(kk''-k'^2)}}\exp[-\frac{k^2r^2}{2(kk''-k'^2)}]$$

where the integral is taken from $r = 0$.

The magnitude (ak'/k), see (9), is the abscissa whose ordinate passes through the centre of gravity of the area of the curve of probability of the error of each observation. The product of that abscissa by Σq_i is therefore the mean result to which linear functions of errors are invariably tending[6]. If $1 = q = q_1 = \ldots$, that function becomes the sum of the errors and Σq_i will be equal to s. Divide the sum of the errors by s; the obtained mean error will invariably tend to the abscissa of the centre of gravity so that when the number of observations indefinitely [unboundedly] increases, the probability that it is contained within an arbitrarily small interval on both sides of the centre of gravity will only differ from certainty less than by any assigned magnitude.

23. We will now study the mean result which numerous and not yet made observations should most advantageously indicate and establish the law of probabilities of that result[7]. Consider the mean result of already made observations whose deviations from it are known. Suppose that we have s observations of one and the same class, A, $A + q$, $A + q_1$, …, i. e., having the same law of error.

Magnitudes q, q_1, q_2, … are positive and increase which is always attainable by a suitable arrangement of the observations. Denote also the probability of error z in each observation by $\varphi(z)$ and suppose that the true result is $A + x$. The observational errors will be $-x$, $q - x$, $q_1 - x$ etc. The probability of the simultaneous existence of all these errors is the product of their respective probabilities,

$$\varphi(-x)\ \varphi(q-x)\cdot\varphi(q_1-x)\ \ldots$$

Then, x can take infinitely many values; when considering them as so many causes of the observed phenomenon, the probability of each will be (§ 1)

$$\frac{dx\varphi(-x)\varphi(q-x)\varphi(q_1-x)\ldots}{\int dx\varphi(-x)\varphi(q-x)\varphi(q_1-x)\ldots}$$

where the integral in the denominator is taken over all the possible values of x. Denote this denominator by $1/H$ and represent the curve of probabilities of the values of x with ordinate y corresponding to its abscissa x as

$$y = H\varphi(-x)\,\varphi(q-x)\,\varphi(q_1-x)\,\ldots$$

The mean result should be assumed as the value leading the error to be feared to its minimum. Each error, both positive and negative, should be regarded as a disadvantage, as a real loss in a game, and the mean loss is obtained by calculating the sum of the products of each by its probability. Therefore, the mean value of the error to be feared, l, is the sum of the products of each error taken without its sign by its probability. Let us determine now the abscissa corresponding to the minimal value of that sum. To this end, we choose the nearest end of the previous curve as the origin of the abscissas and denote the coordinates of the curve's points by x' and y'.

Suppose we should choose the value l and that the real result is x', then, until $x' < l$, the error of l without taking into account its sign is $l - x'$. Now, y' is the probability that x' is the real result and that the sum of the errors to be feared without considering their signs will therefore be $\int(l - x')y'dx'$ for all $x'< l$, so that the integral is taken over $[0, l]$. In a similar way, the sum of errors to be feared multiplied by their probabilities for all $x' > l$ is the integral $\int(x' - l)y'dx'$, taken from $x' = l$ to the abscissa of the remote end of the curve. The whole sum of such errors multiplied by their respective probabilities is therefore

$$\int(l - x')y'dx' + \int(x' - l)y'dx'.$$

The differential of this function with respect to l is

$$dl\int y'dx' - dl\int y'dx'.$$

Indeed, when differentiating the first integral we should first differentiate the value of l in the integrand and then add the increment occurring when the limit of the integral varies and takes the value $l + dl$. This increment is $(l - x')y'dx'$ at $x' = l$ and therefore vanishes so that the differential of the first integral is $dl\int y'dx'$. Similarily, the differential of the second integral is $-\,dl\int y'dx'$ and, for the abscissa l, corresponding to the minimal value of the error to be feared, their sum is zero.

Therefore, for that abscissa,

$$\int y'dx' = \int y'dx',$$

with the integrals extending over $[0, l]$ and from $x' = l$ to the extreme value of x'. It follows that the abscissa for which the mean error to be feared is minimal, divides the area of the curve in two equal parts. The deviations of the real result from the established point [abscissa] are equally probable in both directions and that point can therefore be also called *the mean in probability*. Illustrious geometers have assumed the point rendering the observed result [results] most probable as the mean and have therefore chosen the abscissa corresponding to the maximal ordinate of the curve[8]. However, the mean which we have chosen is evidently indicated by the theory of probability.

When $\varphi(x)$ is represented as an exponential function exp $[-\psi(x^2)]$ so that it will correspond to positive and negative errors alike, we will have

$$y = H\exp[-\psi(x^2) - \psi(x-q)^2 - \psi(x-q_1)^2 - \ldots]. \quad (10)$$

Assume now that $x = a + z$ and expand the exponent of e in powers of z, then

$$y = H\exp[-M - 2Nz - Pz^2 - Qz^3 - \ldots].$$

Here

$$M = \psi(a^2) + \psi(a-q)^2 + \psi(a-q_1)^2 + \ldots,$$
$$N = a\psi'(a^2) + (a-q)\psi'(a-q)^2 + (a-q_1)\psi'(a-q_1)^2 + \ldots, \quad (11)$$
$$P = \psi'(a^2) + \psi'(a-q)^2 + \psi'(a-q_1)^2 + 2a^2\psi''(a^2) + 2(a-q)^2\psi''(a-q)^2 + 2(a-q_1)^2\psi''(a-q_1)^2 + \ldots,$$

$\psi'(t)$ is the coefficient of dt in the differential of $\psi(t)$, $\psi''(t)$, its coefficient in the differential of $\psi'(t)$ etc.

Suppose that the number s of the observations is very large and determine a from the equation $N = 0$ which ensures the maximal value of y. We will have

$$y = H\exp[-M - Pz^2 - Qz^3 - \ldots],$$

where $M, P, Q, \ldots$ are of the order of s. And if z is very small, of the order of $1/\sqrt{s}$, Qz^3 will have the same order so that the $\exp(-Qz^3 - \ldots)$ can be considered to be unity. For z contained between 0 and $r/\sqrt{s}$ we can therefore assume[9] that

$$y = H\exp[-M - Pz^2]. \quad (12)$$

Beyond that interval, when z is of the order of $s^{-m/2}$ with $m < 1$ [$0 < m < 1$], Pz^2 will have order s^{1-m} and, like y, $\exp(-Pz^2)$ becomes insensible. We can therefore assume that equation (12) holds for all the extension of the curve. The value of a is determined by the condition $N = 0$ [see (11)], i. e., it is the abscissa x corresponding to the ordinate dividing the curve's area in equal parts. All that area represents certainty or unity, so that

$$(1/H) = \int dz\exp(-M - Pz^2),$$

with the integral taken over $(-\infty, \infty)$ and therefore

$$H = e^M\sqrt{P}/\sqrt{\pi}.$$

If a is the mean result of the observations, the mean error to be feared in excess and deficiency is $\pm \int zydz$, with the integral taken over $[0, \infty]$, and that error is equal to $\pm 1/2\sqrt{\pi P}$. However, the complete ignorance of whether $\exp[-\psi(x^2)]$ is the law of observational errors does not allow us to compile the equation $N = 0$ [see (11)]. And so, the values $q, q_1, \ldots$ do not ensure any *posterior* knowledge of the mean result a of the observations and we should therefore keep to the most advantageous value determined *beforehand* and provided, as we have seen, by the MLSq of errors.

Let us find the function $\psi(x^2)$, which will continually lead to the arithmetical means adopted by the observers. Assume that the first i observations out of s coincide, as do the last $(s - i)$ ones. Then the equation $N = 0$ [see (11)] becomes

$$0 = ia\psi'(a^2) + (s - i)(a - q)\psi'(a - q)^2,$$

and the rule of the arithmetic means leads to

$$a = [(s - i)/s]q.$$

The previous equation therefore becomes

$$\psi'\{[(s - i)/s]^2q^2\} = \psi'[(i^2/s^2)q^2].$$

It should be valid for any i/s and q so that $\psi'(t)$ is independent from t and is equal to $\psi'(t) = k$, a constant. Integration leads to

$$\psi(t) = kt - L,$$

where L is an arbitrary constant. Therefore

$$\exp [-\psi(x^2)] = \exp(L - kx^2).$$

This, then, is the only function always leading to the rule of the arithmetic means. The constant L should be determined by the condition that the integral $\int dx\exp(L - kx^2)$ taken over $(-\infty, \infty)$ is equal to unity since it is certain that the observational error ought to be contained within those boundaries. Thus,

$$e^L = \sqrt{k/\pi}$$

and the probability of error x is

$$\sqrt{k/\pi}\exp(-kx^2).$$

Actually, this expression leads to infinite boundaries of the errors which is not to be admitted. However, owing to the rapidity of the decrease of exponential functions of such kind with the increase of x, we can assume that k is sufficiently large for the probabilities of inadmissible errors to become insensible[10] and to be thus considered as zeros.

For the general expression (10) the preceding law of error provides

$$y = \sqrt{sk/\pi}\,\exp(-\,ksu^2).$$

Determine now H so that the whole integral $\int ydx$ equals unity and assume that

$$x = (\Sigma q_i/s) + u.$$

The ordinate dividing the area of the curve in two equal parts corresponds to $u = 0$ so that

$$x = (\Sigma q_i/s).$$

We thus define the value x, which should be chosen as the mean result of the observations, and it is this value which leads to the rule of arithmetical means. The previous law of error of each observation thus certainly provides the same results as the indicated rule does and it is obviously the only law possessing such a property.

When assuming this law, the probability of error ε_i of the $(i + 1)$-st observation is

$$\sqrt{k/\pi}\,\exp(-\,k\varepsilon_i^2).$$

In § 20 we saw that, if z is the correction of an element, this observation will provide a conditional equation

$$\varepsilon_i = p_iz - \alpha_i.$$

The probability of the value $p_iz - \alpha_i$ is therefore

$$\sqrt{k/\pi}\,\exp[-\,k(p_iz - \alpha_i)^2],$$

and the probability of the simultaneous existence of the s values $(pz - \alpha)$, $(p_1z - \alpha_1)$, …, $(p_{s-1}z - \alpha_{s-1})$ will be[11]

$$(\sqrt{k/\pi}\,)^{s-1}\exp[-\,k\Sigma(p_iz - \alpha_i)^2].$$

It varies with z and we will find the probability of some of its certain value z by multiplying the obtained magnitude by dz and dividing the product by its integral over $(-\,\infty, \infty)$. Let

$$z = \frac{\sum p_i\alpha_i}{\sum p_i^2} + u\,, \qquad (13)$$

then its probability becomes

$$du\sqrt{\frac{k\sum p_i^2}{\pi}}\exp[-ku^2\sum p_i^2].$$

Construct a curve extending from $u = -\infty$ to $u = \infty$, with its ordinates being the coefficients of du, and abscissas, those u, and it can be considered as the curve of probabilities of the errors u corrupting the result [i. e., the right side of formula (13) without the correctional term]. The ordinate which divides the area of the curve in two equal parts corresponds to value $u = 0$ and z is therefore equal to [the same].

This, therefore, is the result that should be chosen and it coincides with that of the MLSq of observational errors. The previous law of error of each observation thus also leads to it. That MLSq becomes necessary when we should choose the mean of many observations each of them provided by a large number of observations of different classes.

Suppose that the same element is provided by the mean results 1) A, of s observations of the fist class; 2) $A + q$, of s_1 observations of the second class; 3) $A + q_1$, of s_2 observations of the third class etc. If the true element is represented by $A + x$, the error of the first mean is $-x$.

Let[12]

$$\beta = \sqrt{\frac{k}{k''}}\frac{\sqrt{\sum p_i^2}}{2a}.$$

When the mean result is provided by the MLSq [the probability of its error is]

$$\sqrt{\frac{k}{k''}}\frac{\sum p_i}{2a\sqrt{s}}$$

and when applying the ordinary method [of means], the probability of that error, according to § 20, is

$$(\beta/\sqrt{\pi})\exp[-\beta^2x^2].$$

The error of the result of s_1 observations is $q - x$; in this case, denote by β_1 what was called β, then the probability of that error is

$$(\beta_1/\sqrt{\pi})\exp[-(\beta_1^2(x-q)^2].$$

Just the same, the probability of the error of $q_1 - x$ in the result of s_2 observations will be

$$(\beta_2/\sqrt{\pi})\exp[-(\beta_2^2(x-q_1)^2] \text{ etc.}$$

The product of all these probabilities is the probability that $-x$, $q - x$, $q_1 - x$, … [really] are the errors of the mean results of s, s_1, s_2, … observations. Multiply this product by dx and integrate the result over

$(-\infty, \infty)$. We will thus get the probability that the mean results of those observations exceed the mean result of s observations by q, q_1, …
If now we calculate that integral over some definite boundaries we will obtain under the mentioned condition the probability that the error of the first result is contained within those boundaries. Divide this probability by the probability of the condition itself to get the probability that the error of the first result is contained within given boundaries since [now] it is certain that that condition really takes place.

That probability is therefore [the ratio of two coinciding integrals]

$$\int dx \exp[-\beta^2x^2 - \beta_1^2(x-q)^2 - \beta_2^2(x-q_1)^2 - \dots],$$

with the integral in the numerator calculated over the given boundaries and in the denominator, over $(-\infty, \infty)$. We have

$$\beta^2x^2 + \beta_1^2(x-q)^2 + \beta_2^2(x-q_1)^2 + \dots = (\beta^2 + \beta_1^2 + \beta_2^2 + \dots)x^2 - 2x(\beta_1^2q + \beta_2^2q_1 + \dots) + \beta_1^2q^2 + \beta_2^2q_1^2 + \dots$$

Let

$$x = [(\beta_1^2q + \beta_2^2q_1 + \dots) \div (\beta^2 + \beta_1^2 + \beta_2^2 + \dots)] + t, \qquad (14)$$

and the probability above will be [the quotient of two coinciding integrals]

$$\int dt \exp[-(\beta^2 + \beta_1^2 + \beta_2^2 + \dots)]t^2,$$

with the integral in the numerator calculated over the given boundaries, and in the denominator, over $(-\infty, \infty)$. This latter is

$$\frac{\sqrt{\pi}}{\sqrt{\beta^2 + \beta'^2 + \beta''^2 + \dots}}.$$

Denote

$$t_1 = t\sqrt{\beta^2 + \beta'^2 + \beta''^2 + \dots}$$

and the previous probability will be

$$(1/\sqrt{\pi})\int dt_1 \exp(-t_1^2).$$

The most probable value of t_1 corresponds to $t_1 = 0$ (!) so that the most probable value of x corresponds to the value $t = 0$. And so, the correction of the first result to which all the s, s_1, s_2, … observations are leading with the highest probability is [the expression (14) without the t]. Added to the result A, it provides the result to be chosen

$$\frac{A\beta^2 + (A+q)\beta'^2 + (A+q_1)\beta''^2 + ...}{\beta^2 + \beta'^2 + \beta''^2 + ...}$$

whereas the previous correction reduces the function

$$\beta^2 x^2 + \beta'^2(x - q)^2 + \beta''^2(x - q_1)^2 + \dots$$

to its minimal value.

As we have seen now, the maximal ordinate of the curve of probabilities of the first result is $\beta/\sqrt{\pi}$. For that curve of the second result, $\beta'/\sqrt{\pi}$ etc. The mean to be chosen leads to the minimal value of the sum of the squares of the errors of each result multiplied by the maximal ordinates of the respective curves of probabilities. And the principle of minimal squares of errors becomes necessary if the mean of the results each provided by a large number of observations should be calculated.

24. We saw that, at least when a large number of observations necessary for determining the elements is available, it is most advantageous to combine the conditional equations for determining the final linear equations by the MLSq. However, when going over to the least sum of other powers of the errors or to their quite different functions the final equations will not be linear anymore and remain unsolved.

Nevertheless, there exists a case deserving special attention since it determines the system for which the maximal error without considering its sign is less than for any other system. This is the case of minimal [sum of] infinite and even powers of the errors. When dealing with the correction z of only one single element, represent, as above, the conditional equations in the form

$$\varepsilon_i = p_i z - \alpha_i,$$

where i changes from 0 to $s - 1$ and s is the number of the observations. The sum of the errors raised to the power of $2n$ is $\Sigma(\alpha_i - p_i z)^{2n}$, where the symbol Σ is extended over all the values of i.

We can assume that all the p_i are positive; indeed, if one of them is negative, it will become positive after a permissible change of the signs of both terms of the respective binomial raised to the power of $2n$. And, when supposing that the magnitudes $(\alpha - pz)$, $(\alpha_1 - p_1 z)$, $(\alpha_2 - p_2 z)$, … are arranged so that p, p_1, p_2, … are positive and increase, and if $2n$ is infinite, the maximal term of the sum $\Sigma(\alpha_i - p_i z)^{2n}$ will be evidently equal to the entire sum, at least if there is no either single, or numerous other terms equal to it, although this circumstance should not occur in the case of a minimal sum.

Actually, if only one single term, for example $(\alpha_i - p_i z)$, is maximal without considering its sign, it can be decreased by a suitable change of z and since the sum $\Sigma(\alpha_i - p_i z)^{2n}$ will also decrease, it was not minimal. Moreover, if $(\alpha_i - p_i z)$ and $(\alpha_j - p_j z)$ are two maximal terms equal to each other without considering their signs, they should have contrary signs. And if the sum

$$(\alpha_i - p_i z)^{2n} + (\alpha_j - p_j z)^{2n}$$

becomes minimal, its differential

$$-\,2ndz[p_i(\alpha_i - p_i z)^{2n-1} + p_j(\alpha_j - p_j z)^{2n-1}]$$

should vanish. With an infinite n this can only occur if those two terms have contrary signs and differ by an infinitely small magnitude. If three terms are maximal and equal to each other without considering their signs, their signs evidently can not coincide.

Consider now the sequence

$$(\alpha_{s-1} - p_{s-1}z),\ (\alpha_{s-2} - p_{s-2}z),\ (\alpha_{s-3} - p_{s-3}z),\ \ldots,\ (\alpha - pz),\ (-\,\alpha + pz),\ \ldots,$$
$$(-\,\alpha_{s-3} + p_{s-3}z),\ (-\,\alpha_{s-2} + p_{s-2}z),\ (-\,\alpha_{s-1} + p_{s-1}z). \qquad (15)$$

Suppose that $z = -\,\infty$. Then the first term will be larger than the other ones and when z increases it will remain larger until becoming equal to one of them. And then, when z continues to increase that new term will become larger than all the rest ones and remain larger than the following terms. For determining that term we form the sequence of quotients

$$\frac{\alpha_{s-1} - \alpha_{s-2}}{p_{s-1} - p_{s-2}},\ \frac{\alpha_{s-1} - \alpha_{s-3}}{p_{s-1} - p_{s-3}}, \ldots, \frac{\alpha_{s-1} - \alpha}{p_{s-1} - p},$$

$$\frac{\alpha_{s-1} + \alpha}{p_{s-1} + p}, \ldots, \frac{\alpha_{s-1} + \alpha_{s-1}}{p_{s-1} + p_{s-1}}.$$

Let $[(\alpha_{s-1} - \alpha_r)/(p_{s-1} - p_r)]$ be the minimal quotient, now, however, allowing for the signs: consider that a larger negative magnitude is smaller than a lesser negative magnitude. If there are many smallest and equal quotients we choose that which concerns the term remotest from the first one in the system (15). This term will remain maximal until, when z increases, it becomes equal to one of the subsequent terms which will then become maximal.

To determine that term form a new sequence of quotients

$$\frac{\alpha_r - \alpha_{r-1}}{p_r - p_{r-1}},\ \frac{\alpha_r - \alpha_{r-2}}{p_r - p_{r-2}}, \ldots,\ \frac{\alpha_r - \alpha}{p_r - p},\ \frac{\alpha_r + \alpha}{p_r + p} \ldots,$$

The term of the sequence (15), to which the least of these quotients corresponds will be that new term. And so we should continue in the same way until one of the two terms which become equal to each other and maximal will be situated in the first half of the sequence (15) and the other, in the second half. Let these two terms be $(\alpha_i - p_i z)$ and $-\,(\alpha_j - p_j z)$. The value of z to which corresponds the least of the maximal errors without considering their signs will be

$$z = (\alpha_i + \alpha_j)/(p_i + p_j).$$

When there are many elements, the conditional equations which determine their corrections include many unknowns[13] and the investigation of the system of corrections for which the maximal error without considering their signs is less than in any other system becomes more complicated. I studied this general case in Book 3 of the *Mécanique Céleste* [ca. 1804/1878, Chapter 5, § 39] and here I only remark that, just as in the case of one single unknown, the sum of the observational errors raised to the power of $2n$ is minimal when $2n$ is infinite. It easily follows that in the system under consideration the number of equal and maximal errors without paying attention to their signs should by unity exceed the number of the elements being corrected.

The results corresponding to a large value of $2n$ should understandably little differ from those to which leads an infinite $2n$. And it is not even necessary for $2n$ to be very large. I know from many instances that even when $2n \le 2$ (!) the results little differ from what is provided by the system of the minimal maximal errors and this is a new advantage of the MLSq of observational errors.

For a long time geometers have been applying the arithmetic mean of their observations and, to determine the desirable elements, they have been choosing the most favourable circumstances so that the observational errors corrupted the values of those elements as little as possible. If I am not mistaken, Cotes was the first who offered the general rule for determining an element proportionally to the influence of many given observations.

Consider each observation as a function of the element, and its error as an infinitely small differential, and it will equal the differential of that function with respect to that element. The larger is the coefficient of the differential of the element, the smaller should that element be varied so that the product of its variation by the coefficient will be equal to the observational error. This coefficient will thus express the influence of the observation on the value of the element. Cotes had thus represented all the values of the element provided by each observation by parts of an infinite straight line having a common origin. Let us now imagine weights proportional to the influence of the corresponding observations on their other ends. The distance from the common origin to the general centre of gravity of all those weights will be the value chosen for the element.

I return now to the equation of § 20

$$\varepsilon_i = p_i z - \alpha_i$$

where ε_i is the error of the $(i + 1)$-st observation, z is the correction of an approximately known element and p_i, which we can always suppose positive, expresses the influence of the corresponding observation. The value of z derived from the observations is α_i/p_i and the Cotes rule is reduced to multiplying it by p_i, summing all such products and dividing the sum by the sum of all p_i:

$$z = \sum\alpha_i/\sum p_i. \tag{16}$$

This is the correction which had been adopted by the observers until the application of the MLSq of observational errors. However, after that excellent geometer we do not see anyone using this rule before Euler. It seems to me that [in 1749] he was the first, in his first memoir on Jupiter and Saturn, to apply conditional equations for determining the elements of the elliptical movements of these two planets. Almost at the same time [in 1750] Tobie [Tobias] Mayer applied them in his fine investigations of the libration of the Moon and then for compiling lunar tables.

From that time onward the best astronomers had been applying that method and the success of their tables thus compiled justified its advantage. When only one element is determined this method does not involve any embarrassment; otherwise, however, it is required to form as many final equations as there are elements by combining a multitude of conditional equations and then to determine the corrections sought by [successive] elimination. But which method of combining those conditional equations is the most advantageous?

It is here that the observers abandoned themselves to arbitrary guesswork leading them to differing results although deriving them from the very same observations. For avoiding such groping, Legendre conceived [in 1805] a simple idea to consider the sum of the squares of observational errors and to render it minimal which immediately leads to as many final equations as there are elements to be corrected. That learned geometer was the first to publish the indicated method, but we ought to acknowledge that Gauss had been invariably applying the same idea for many years before Legendre's publication and communicated it to many astronomers.

In his *Theory of Motion* (1809) Gauss attempted to coordinate this method with the theory of probability by showing that the very law of observational errors, which generally leads to the adopted rule of the arithmetic mean of many observations, at the same time provides the rule of least squares of observational errors, and this is seen in § 23. However, since nothing proved that the first of these rules leads to the most advantageous results, the same uncertainty existed with respect to the second one. The investigation of the most advantageous method of forming the final equations is certainly one of the most useful applications of the theory of probability, and its significance for physics and astronomy turned my attention to it.

And I have considered how all the methods of combining the conditional equations for deriving a final equation are reduced to multiplying them by factors which disappear for those equations which we do not apply[14] and to summing up all those products. This indeed leads to the first final equation. A second system of factors leads to the second final equation etc until there will be as many of these as there are elements to be corrected.

Nevertheless, the systems of factors should be chosen in a way that ensures for each element the minimal value of the mean error to be feared in either direction. The mean error is the sum of the products of each error by its probability. With a small number of observations the choice of these systems depends on the law of error of each. However, when considering a large number of observations, which is indeed generally occurring in astronomical studies[15], that choice becomes independent from the indicated law. The previous deliberations show that the analysis then immediately leads to the results of the MLSq of observational errors. This means that that method, which at first did not ensure anything except the derivation of the final equations without any arbitrariness at the same time provides the most precise corrections, at least if only linear final equations are being applied. This condition is necessary when a large number of observations is considered at once, since otherwise the [consecutive] elimination of the unknowns [from the equations] and their determination become impracticable.

Notes

1. Those equations are called *observational*.

2. As noted by David (2001, p. 222), Gauss (1822), when applying the MLSq, called them *normal*.

3. Laplace's *element* really meant an *unknown* whose approximate value was necessary to correct. *Laplace freely used non-standard analysis by treating definite integrals as sums* (Bru 1981, p. 57).

4. It seems that Laplace had thus recommended the construction of the tables almost anew and in any case he had not even mentioned systematic errors. Newcomb (Sheynin 2002, p. 146) accomplished a Herculean task of combining the catalogues of the main observatories the world over.

5. This is difficult to understand since the previous integral did not at all contain t.

6. As noted by Molina (1930, p. 386), Laplace (1786/1894, p. 308) had remarked that approximations in the theory of probability differed from the *usual* which ensure the result.

7. Laplace had not really established that law (which was hardly possible).

8. Those *illustrious geometers* were Lambert, in 1760 (Sheynin 1966; 1971, p. 251), Daniel Bernoulli, in 1778 (Sheynin 2007, p. 293) and Gauss (1809). Gauss, however, joined the condition of maximal probability and the rule of the arithmetic mean rather than applying it all by itself. A few lines above and several times in the sequel Laplace applied a loose expression, *area of a curve*.

9. Laplace did not specify that r.

10. A large k (a small variance) can be certainly presumed although only in the stated restricted sense.

11. The coefficient of the exponential function should be ks rather than k.

12. Laplace had not directly specified β.

13. I can only understand the *unknown* as an elegant variation of *element*, but in any case the phrase is awkward.

14. This brings back the memory of the Boscovich method of adjusting observations (and linear programming).

15. A large number of observations is not likely to obey the same law of distribution of their errors. And the number of observations in geodesy is not large at all.

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XIV

P. S. Laplace

On the Application of the Calculus of Probability to Observations and Especially to [Trigonometric] Levelling

Sur l'application du calcul des probabilités aux observations
et spécialement aux opérations du nivellement (1819).
Œuvr. Compl., t. 14. Paris, 1912, pp. 301 – 304

The extensive triangulation carried out for measuring the Earth includes the careful observation of the zenith distances of [the adjacent] surveying signals either for reducing the measurements [of the angles] to the horizon or for determining the relative heights of various stations[1]. Vertical refraction strongly influences those heights and they become very uncertain owing to its variability.

Here, I propose to estimate the probability of errors to which they are susceptible. The theory of [vertical] refraction indicates that as long as the atmosphere is constant it is an aliquot part of the celestial arc contained between zeniths of the observer and the observed surveying signal. To determine that refraction it is thus sufficient to multiply the mentioned arc by a factor which does not change if the atmosphere remains invariably constant but which necessarily alters all the time due to the continuous changes in the temperature and density of the air.

A large number of observations can provide the mean value of that factor and the law of the probabilities of its variation[2]. I issue from Delambre's observations (Méchain & Delambre 1807) and determine the probability of error in the height of Paris above the sea level under the supposition that Dunkerque and Paris are connected by a chain of 25 equilateral triangles which means that each side of those triangles is about 20,000 *m* long[3].

That height can be obtained by various methods, but we should prefer as the most advantageous that which provides a most rapidly decreasing law of the probabilities of error. Its investigation is a simple corollary of the analysis that I have carried out concerning all such issues. My result is that the odds are 9:1 that the error of the Paris height above the sea level does not exceed 8 *m*[4].

The method applied by Delambre for deciding that that height transferred by a chain of almost the same number of triangles is somewhat less precise. The main point is however that it was the large length of the sides of many of his triangles which caused the uncertainty of his result and it is not sufficiently probable that that uncertainty by no means amounts to 16 or 18 *m*.

Equally probable errors essentially decrease with the stations being closer to each other and, when desiring to obtain precise levelling, this indispensable condition ought to be achieved. Large triangles, quite proper for measuring terrestrial degrees, are not at all suitable for measuring heights, and these two types of measurements should be separated. However, the error caused by measuring zenith distances increases with the number of stations and becomes comparable to that depending on the variability of the vertical refraction.

This circumstance induced me to investigate the law of probabilities of the observational results in case of numerous sources of errors. Such is the case in most astronomical results since we observe celestial objects by two instruments, the meridional circle and a theodolite, both susceptible to errors whose laws of probabilities can not be supposed identical. The analysis that I provided in the *Théorie analytique des probabilités* is easily applicable to this case for any number of the sources of error and establishes the most advantageous results and the laws of probabilities of the errors to which they are susceptible[5]. For applying that analysis to levelling the law of probabilities of the errors caused by the astronomical [the vertical] refraction should be known. And I have just indicated that its results are determined by the great triangulations along the meridian. And we also ought to know the law of probabilities of the errors of the zenith distances. No appropriate observations are available, but we will little deviate from reality by supposing that it is the same as the law for the horizontal angles derived from the errors of the sums of the three angles of each triangulation triangle[6].

By issuing from these laws, I found that, when dividing the distance from Dunkerque to Paris by stations 1200 *m* apart you can bet 1000 to 1 that the error of the height of Paris above sea level does not exceed 0.4 *m*. This error decreases with the stations spaced nearer to each other, but the precision thus attained does not compensate the duration of the work required.

The conditional equations which are compiled for deriving the astronomical elements indirectly include the errors of both instruments serving for the determination of the star places. Those errors influence the various coefficients of each equation. The system of the most advantageous factors by which those equations should be multiplied for deriving as many final equations as there are elements to be determined by joining the calculated products will not be anymore a system of coefficients of the elements in each conditional equation. Analysis led me to the general expression for this system of factors and therefore to the result with a less probable error to be feared than the error of the same magnitude inherent in any other system. The same analysis established the laws of probabilities of the errors of these results.

The derived formulas include as many constants as there are sources of errors and they depend on the laws of probabilities of those errors. In case of only one source I provided in my theory of probability a means for estimating the constant by compiling the sum of the squares of the residual free terms of each conditional equations left there after the substitution of the calculated values of the elements. In the general case a similar method determines the value of those constants whichever their number and this concludes the application of the calculus of probability to the results of observation.

I conclude by a remark which seems important to me. The small incertitude left in the values of those constants when the number of observations is not very large renders those probabilities determined by the analysis somewhat uncertain. However, almost always it suffices to know whether the probability of the errors of the obtained results is contained within narrow boundaries and extremely (extrêmement) tends to unity. If not, suffice it to find out how many more observations should be made for attaining a probability ensuring the virtue of the results beyond any reasonable doubt.

Analytical formulas of probability perfectly accomplish this aim and from that viewpoint they can be considered as a necessary complement of the scientific method based on studying a set of a large number of observations susceptible to error. And so, if the error to be feared in the height of Paris above the sea level derived from large triangles measured along the meridian is decreased from 18 *m* to 15 it is not less true that this height is uncertain and that it should be determined by more precise methods.

At the same time the analytical formulas concerning such triangles from the base measured near Perpignan to Formentera indicate that it is possible to bet 1,700,000 to 1 on the error of the corresponding arc of the meridian more than 460 *km* long not to exceed 60 *m*. This should dissipate the fear of an incertitude possibly inspired by the lack of a comparison base on the Spanish side [of the arc]. We can still be reassured in this respect even if the probability of an error equal or larger than 60 *m* exceeds the fraction established by the formulas and reaches 1:1,000,000.

Notes

1. Precise (spirit) levelling is accomplished by horizontal lines of sight achieved by optical levelling instruments. It came into general use in the mid-19th century. Theodolites of later design provided angles reduced to the horizontal plane.

2. Empirical determination of the vertical refraction is only possible if at all for restricted homogeneous regions and a definite time of day.

3. Such a chain, if laid out along a straight line, will be 260 *km* long. The distance from Dunkerque to Paris is 295 *km*. These cities have almost the same longitude.

4. The mean height of Paris above the mean sea level (not just *above the sea* as the author wrote) is 30 *m*. Laplace did not specify whether he meant the mean height.

5. Laplace (ca. 1819) had indeed studied the joint action of two (easily generalized to a larger number) sources of error. However, he restricted his study to the case of normal distributions and moreover did not provide anything new (Sheynin 1977, pp. 46 – 47; Hald 1998, pp. 430 – 431).

6. Laplace apparently thought that both laws were normal (with differing variances).

7. With the advent of invar wires (early beginning of the 20th century) two identically measured bases at the ends of a triangulation chain had been considered necessary rather than an *ordinary* and a comparison bases. When adjusting a chain, it became possible to disregard the comparatively insignificant errors of those bases.

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XV

P. S. Laplace

Statement on the Export of Grain Made in the Chambre des Pairs in 1814

Archives Parlementaires 1787 – 1860. Paris, 1868, pp. 470 – 471

Gentlemen, I am asking the Chamber to hear patiently these reflections suggested to me by the report of its commission on the important bill with which we are dealing now. I entirely agree with the principles of the reporter on the free export of grain and I think that, as proved by the successful results of the Edicts of 1764 and 1774, the benefits provided by that trade for the soil of France has the same effect as the overflow of the Nile on the soil of Egypt.

The following consideration will additionally support those principles. According to the law of nature common for all species of the organized beings, the population of mankind is known to tend incessantly to increase and reach the level of subsistence[1]. However, when arriving exactly at this condition people become miserable and no-one will have anything except the absolutely necessary. A least draught will kill millions as it frequently happens in China and India.

It is therefore important for the general well-being that the possibilities always exceed the barely needs of the people. And this is admirably achieved by the grain trade which augments the reproduction of grain and extends prosperity into the most numerous classes of the society so that they will be able to subsist during sterile years by sacrificing their surpluses. Actually, the population is less considerable but more active and especially more happy. If I am not mistaken, this is one of the greatest advantages of the European societies and it is extremely useful to maintain and even to extend that advantage by a free exchange of the agricultural produce.

However, if pressing circumstances compel us to restrict these natural rights of property both Chambers especially constituted for maintaining all rights should diminish the proposed obstacles to exercising them as much as prudence permits it. This is the aim of the report of your commission. The reasons provided there for suppressing the tax on exporting grain from the bill seem reasonable. However, I can not share its opinion about the suspension of that export by the government if it decides that that measure is necessary.

It seems to me that such a possibility will totally destroy that trade which most of all needs security. Some years ago many merchants made losses because of a sudden suspension provoked by imaginary fears. Recalling this fact, speculators will undoubtedly shy away if the law will not take care to reassure them in that regard. This bill is only a concession to prejudices and popular fear. The separation of the départements in many classes seems to be a corollary of that plan. Two classes will possibly be enough.

We will be able to judge better if, as is desirable, the bill [itself] separates the départements. This will also be advantageous by assuring the merchants against troubles caused by sudden changes in such separations. And the necessary security will be provided by an addition at the end of § 11 stating that once a governmental regulation is published, it will not experience any changes.

This is how I propose to modify the bill presented to you. I regard my suggestion as a means for attaining some day an unbounded freedom of the grain trade demanded by the best authors of political economy and confirmed by the advantages enjoyed for a long time in Tuscany [Italy]. Due to the progress in enlightenment our provinces are not anymore alien to each other in this respect. Let us hope that the same will happen to all European nations.

The Chamber resolved to publish the report of Count Laplace.

Note

1. Cf. Laplace (1814), *Philosophical Essay on Probability*. New York, 1995, p. 85.

XVI

P. S. Laplace

On the Execution of the Cadastral Surveying

Sur l'exécution du cadastre
Read in 1817 in the Chambre des Paires, published 1868.
Oeuvres Compl., t. 14. Paris, 1912, pp. 372 – 374

Gentlemen, the following reflections about the execution of the cadastral surveying especially concerns the government, but it seems to me that an operation with expenses reaching 100 *mln* deserves to attract for a few minutes the Chamber's attention and I also believe that a statement from this tribune will be better heard.

I do not at all consider whether by means other than that surveying it is possible to obtain with sufficient precision and more rapidly an evenly matched land-tax. Cadastral surveying is good in itself and it is now too advanced for being abandoned. I only desire to indicate proper measures for its improvement.

It is its topographical part that requires most time and expenses. There is only one method of compiling a precise plan of a kingdom [of mapping it precisely], but it was not regrettably followed in the cadastral surveying. It consists of tracing two main mutually perpendicular lines directed, respectively, to north and south and east and west[1]. All the territory to be measured is covered by a network of large triangles connected to those lines; then each is separated into secondary triangles etc and thus the work descends to fixing the boundaries of the communes. The errors of local measurements are restricted by circumscribing triangles and the carelessness of the land surveyors is revealed and rectified. This is a system of operations fine in details and perfect as a whole.

France has all the desired means for carrying out that system, – most capable scientists to direct it; and a corps of well educated engineers-geographers to execute the work in the best possible way and to whom artillery and supper officers can be joined. Cadastral surveying will offer those officers the most favourable occasion to exercise for the operations which they will have to carry out during wartime. And this is the way in which Prussia extends the topographic work of our engineers on the other side of the Rhine; it can not follow a better example.

One of the fundamental lines which I mentioned above is already traversing France from Dunkerque to Perpignan; a perpendicular directed from Strasbourg to Brest has begun to be measured. The former was traced with an extreme precision and was continued on the other side of the Pyrenees to the island of Formentera in the Mediterranean. Owing to the well-informed care of the Minister of the Interior about the progress of science that line will also extend to the north [by turning north] until Yarmouth.

By following the described method, Colonel Mudge[2] as ably as thoroughly compiles plans [maps] of both England and Scotland. Together with French scientists he should lengthen our meridian [arc measurement] by joining his work to it. The actual length of this great arc amounts to ca. 1/7 of the distance from the pole to the equator. The latitudes of its extreme and many intermediate points are measured and so are the corresponding lengths of second pendulums. This work throws vivid light on the figure of the Earth and on the inequalities of its [meridional] degrees and gravity. The finest of its kind executed until now, it serves as the base of the decimal metric system of weights and measures whose general adoption will be a great blessing for the governments. It is a lucky complement of our admirable number system and, just like it, equally suitable to all peoples; it was just adopted by the kingdom of Netherlands.

In France, only a few seconds, please, sometimes thwarted by the authorities, a successful struggle is going on against the obstacles placed by the force of habit which opposes even the most useful innovations. But later that force joined with reason will maintain the metric system and assure these humane institutions (?) an eternal duration.

I would wish our ministers to take my plan[3] into serious consideration. It is possible to adapt to it the accomplished portion of the surveying and to execute it without delay or increased expenses. In our present peaceful state it will perhaps be even possible to permit a large number of engineers-geographers to participate in this work in which we barely see foreigners and to execute the survey more rapidly and with less expenses.

And a commission chosen by the government for interpreting this issue will collect necessary information. It will examine how justified are the reproaches about negligence and inability levelled against many workers in the field and it will indicate measures to accelerate and perfect the surveying.

The compilation of the great Map of France offers an example which other nations will be quick to follow. Therefore, we should not become inferior, should not retreat even a step back while they advance. Maintain the glory of our sciences and fine arts. Sweet and pacifying, it possesses the precious advantage of increasing without diminishing the glory of foreigners or the interests of any people and provides new enjoyment to all.

Notes

1. Below, Laplace mentions these lines once more. It follows that *tracing* them was not needed, and even less so since the lay-out of triangulation depends on the country.

2. William Mudge (1762 – 1820).

3. So where did Laplace describe his *plan*?

XVII

P. S. Laplace

On the Suppression of the Lottery

Sur la suppression de la loterie. Read in 1819 in the Chambre des Pairs, published 1868. *Oeuvr. Compl.*, t. 14. Paris, 1912, pp. 375 – 378

Gentlemen, the state of our finances allows to decrease taxes. The bill presented to us decreases direct contributions and the deductions from salaries, but is this measure the most advantageous? I have the honour to submit to the Chamber the following reflections about this issue. [...]

I think that it is much more useful to suppress the tax of the lottery. Recall what had been said a thousand times against the immorality of that game and the evil that it causes. The banker gets the greatest benefit and most gamblers, the least luck, not from other games, but from the lottery. The gambler's disadvantage, both physical and moral, much exceeds the disadvantage caused by other public games[1] barely tolerated to prevent larger evil.

In those other games the banker only deducts in advance[2] 1/40 of the stake; in the lottery, the government deducts 1/3. Bet 18 *fr* on one of the five winning numbers and the stake is reduced to 15 *fr*. It is reduced by 1/3 and 1/2 when betting on two and three numbers and much more when four numbers have to win, and this is the physical disadvantage of that game. However, those losses, insensible for the rich, are very sensible for the greatest number of participants, and this is their moral loss.

The poor, excited by the desire of a better destiny and seduced by hopes whose unlikelihood they are unable to appreciate, expose their necessaries to that game. They clutch at the combinations which promise them a large benefit although it is seen at once how many of them are unfavourable. Thus, everything concurs to render this game disadvantageous and prompts us to suppress it by law. We would applaud a man who diverts his listeners from the lottery by passionately describing the crimes and misery, the bankruptcies and suicides which it breeds.

Let us therefore hasten to abolish a game so contrary to the morality and so disadvantageous for the gamblers that the police do not allow it in many public games which they feel themselves compelled to tolerate. It is remarked that the tickets of foreign lotteries are creeping in. However, governmental supervision can impede this process or at least render the tickets so rare that they will not at all reach the population of the kingdom's interior. It is possible to state that with some vigilance the stakes in those lotteries will not amount to 1/50 of those in the actual lottery of France.

It is also remarked that for each individual that [factual] tax is voluntary[3]. Yes, but for a multitude of individuals it is necessary just as marriages, births and all variable effects are necessary and almost the same from year to year when considering large numbers. The revenue from the lottery is at least as constant as agricultural products are.

That tax is the one requiring most expenses involved in collecting it; it burdens the people much more than it provides the government since the stakes do not return to a hundredth of the gamblers. Moreover, publicity pays special attention to the gains and this becomes a new cause of incitement to that pernicious game. Therefore, although the lottery only contributes 10 or 12 *mln* to the public treasury, the tax levied on a large, and, besides, the poorest part of the population, amounts to 40 or 50 *mln*.

How much false reasoning, illusions and prejudices does the lottery hatch! It corrupts both the mind and the morality of people whereas the legislator mostly ought to bear in mind their moral education. He should sacrifice petty fiscal considerations to this great aim but I also maintain that that sacrifice will not in the least diminish our finances since here, as everywhere, what is good in itself is at the same time profitable. When becoming more industrious and more at their ease, people will more readily pay taxes and consume more so that the treasury will recover by indirect contributions more than it looses by the suppression of the lottery.

Grace to the noble Peer[4], the founder of the savings bank! That establishment, so favourable for morality and industry, diminishes the profits of the lottery which is one of its advantages. Let the government encourage similar establishments by whose means, when sacrificing a small part of his income, he [the head of a family] assures the existence of himself and family against the time when he will not be anymore able to cope with its needs. As much as the lottery is immoral, thus much are those establishments wholesome for morality by favouring the most pacifying tendencies of nature.

They should be respected in the face of the vicissitudes of the public fortune since the expectations they present concern remote future; they can only prosper when delivered from all anxiety about their duration. This is an advantage of which the lucky form of the system of our governing can assure them. So let us also encourage the associations whose members mutually guarantee their property against accidents by proportionally supporting the burden of that guarantee. However, the establishments based on illusions of ignorance and greed should be rigorously banned. No benefit can compensate their evil effect. And we ought to regret greatly that the suppression of the lottery considered as a tribute rendered to morality was not placed at the head of the list of the taxes to be reduced.

Notes

1. *Jeu public*: as might be thought, a game regularly played in registered casinos. Below, the same term apparently concerned such casinos.

2. The *deduction in advance* seems to concern the banker's benefit due to the unfairness of a game. Poisson (1837, § 22) stated that the game thirty-and-forty only provided the banker less than 0.011 (< 1/91) of the stake but that the rapidity of the game ensured his benefit. He also stated that, owing to the moneys involved in it, that game was more harmful than the lottery.

Then, after indicating that the government deducts 1/3 of the banker's profit, Laplace adds information based on lacking calculations. Cournot (1843, § 55) provided the following figures for the banker's benefit: 1/6, 1/3 and 22/25 of the stake when the gambler is betting on 1, 2 and 4 numbers out of the 5 winning.

3. Petty (1662/1899, p. 64) stated that lotteries were *properly a Tax upon unfortunate self-conceited fools*. Several later authors left similar statements.

4. Duke De la Rochefoucauld – Editors of the *Oeuvr. Compl*. The Duke (1747 – 1827) established a savings-bank in a province. The first such bank appeared in Paris in 1818 (*Grand Dict. Universel Larousse*, t. 3, p. 92). Laplace borrowed a few lines at the end of his statement from his *Essai* (1814/1995, pp. 89 – 90) somewhat changing the earlier wording.

5. The lottery was only suppressed in 1829, and only in some départements, then, in 1836, in the entire kingdom, but after some time allowed once more (*La Grand Enc.*, t. 22, pp. 584 – 585, article Loterie). Poisson (1837, § 22) mentioned that the lottery was *luckily suppressed by a recent law*.

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XVIII

P. S. Laplace

On the Manner in Which the Decision of Jury Panels Is Formed

Sur la manière dont se forme la décision du jury.
Read in the Chambre des Pairs in 1821, published 1868.
Oeuvr. Compl., t. 14. Paris, 1912, pp. 379 – 381

Gentlemen, the manner in which the jury panel forms its decision is certainly the most important element of its constitution. From its introduction, that manner had been greatly inconvenient and startled sober minds. When 5 jurymen out of 12 declare that an event is not established, the law reasonably expresses doubt and attempts to dissipate it by an intervention of the judges of an assize court. It does not see a sufficient motive for convicting by a simple majority of 7 votes out of 12 and looks for a confirmation of such a motive in the decision of judges.

This is fair and conforms to the doctrine of probabilities which is basically only common sense reduced to a calculus [Laplace 1814/1995, p. 124]. The latter provides the power for arriving at consequences that the former is unable to formulate independently. However, when the judges decide by a majority of 3 votes out of 5, far from confirming the motive for conviction, they weaken it. Is it not evident that, since the judgement was considered indeterminate, the new decision made it even less sufficient? And is it not contrary to common sense and humanism to convict an accused in such cases? [7/12 = 0.58, 3/5 = 0.60.]

Even more: sometimes the jurymen, being uncertain about the culpability of the accused and wishing to pass the judgement to an assize court, arbitrarily form a majority of 7 votes against 5. In such cases their decision is fictitious and should be regarded null and void. [However,] the 5 judges of the assize court will deliberate and if 3 votes against 2 are favourable for the accused, he is [nevertheless] condemned.

I do not know whether judiciary annals of any people offer another example of a conviction pronounced by a minority vote[1]. It is therefore important to abolish promptly this great inconvenience. But it is said that the bill concerning that issue and presented to you perverts the institution of jurymen by preferring its majority over the majority of the assize court. This, however, only occurs in the interests of the accused when an attempt is made to find in the decision of the judges a new motive for strengthening the decision of the jurymen, insufficient in the eyes of the law, for condemning him. It is this insufficiency which, according to the bill, annuls the decision of the jurymen and which the assize court does not confirm but rather weakens.

It is also remarked that an arbitrary division of 7 jurymen against 5 will become oftener when they are not restrained by fear of the accused being condemned by a minority vote in the assize court. We do not know the rate of cases in which a simple majority [7:5] of the jurymen is just an agreement. On this point we have no observations without which that rate will be either exaggerated or diminished in the interests of the intended cause. Still less do we know how the bill will influence this rate. What we certainly know is that it is urgent to suppress a greatest possible abuse when an accused is condemned by a minority vote. The legislator ought to take into account the jurymen's sense of duty felt when dealing with the life of fellow humans. Many jurymen had told me about such cases in which they easily persuaded the panel to check deeply the culpability of the accused. Even in the case in which the law does not prescribe an intervention of the assize court, is it possible to fear that the jurymen will not discuss with all necessary care the question submitted for them to decide?

In many countries the law requires the jurymen to deliberate until reaching unanimity and thus to compel them to study their cases. Here, however, a new inconvenience presents itself. The obstinacy of the jurymen, their temperament and habits, and a thousand other causes alien to the judgement sometimes influence injuriously their decision so that the opinion of a minority of the jurymen prevails. Let us say that everything in this world has its inconveniences and advantages. The difficulty of a correct choice and introduction of useful innovations consists in their proper appreciation.

Let us only change our laws after an extreme circumspection but promptly introduce improvements evidently indicated by common sense and humanism. It is objected, finally, that the approval of the bill will sanction the intervention of judges which seems to act contrary to the institution of the jury panels. However, when improving an existing law, the legislator never forbids the possibility of revising it as a whole or of introducing changes judged to be advantageous by experience and deep examination.

Such an examination especially demands long and sober reflection when dealing with the important law concerning jury panels. In the presented bill we should only see an urgent correction of a grave abuse which could daily compromise innocence. From this very viewpoint I had made the same proposals more than four years ago and I urge to adopt them now.

Note

1. Laplace referred to his opinion pronounced in Supplement 1 to the *Théor. Anal. Prob*. (1816/1886, p. 529). There, he provided an example: 7 jurymen and 2 judges condemn the accused and 5 jurymen and 3 judges absolve him; 9 > 8 and he is condemned although the decision of the higher instance weakened the case against the accused.

Cournot (1843, § 217) noted that in 1825 – 1830 convictions by a simple majority only held if confirmed by a majority of the 5 magistrates of an assize court. There also, he indirectly agreed that the practice of delivering agreed decisions did exist *during the previous legislation*. Commenting in the edition of 1984 on this statement, Bernard Bru noted that in 1838 Cournot pronounced an opposite opinion.

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XIX

S.-D. Poisson

Speech at the Funeral of the Marquis de Laplace

Discours prononcé aux obseques de M. le Marquis de Laplace.
Conn. des tem[p]s pour 1830, 1827, pp. 19 – 22 of the second paging

Gentlemen, it is becoming that the centenary of Newton's death was marked by the demise of one of his most illustrious successors whom England and France had often called the French Newton thus expressing at once the glory of those two nations! Undoubtedly this is not the time for attempting to diminish our profound sorrow. However, if we consider the whole century separating these two great events, what an admirable spectacle is presented to us by the advance of the sciences, by their tendency to the mathematical spirit which is the true philosophy, and especially by the height which physical astronomy has reached by combining the most sublime analysis with the most exact observations!

When tracing this immense summary of discoveries with an able hand it is impossible to see without surprise that all its parts are explained by the genius of one and the same man[1] whose loss alas! we are bemoaning. He experimented together with his friend Lavoisier which would have sufficed for a reputation of a physicist of the first rank; he was closely connected with Berthollet and between them there existed a community of ideas which bore fruit both in the *Statique chimique* [1803] and the *Exposition* [1796]. He contributed to all the sciences and they all respected him. Indeed, those among the most celebrated interpreters of all its branches, Haüy[2], Berthollet, Cuvier, Biot, Humboldt, considered it as an honour to dedicate to him their contributions.

Newton enclosed in a single idea the constant laws governing matter and, what is not less worthy of admiration, indicated most of the consequences of his principle which time and diligent observations should have allowed us to reveal. But how was it possible to follow further along the path anticipated by a genius who seemed to tower higher than humanity by appreciating completely the phenomena and perfectly comparing them with experience which constitutes the astronomy of our time!

For achieving this goal the works of Euler, Clairaut, D'Alembert, Lagrange and Laplace were required. And today Laplace's *Mécanique Céleste* is a complete development of the *Math. Princíples of Natural Philosophy*. Each of these contributions is only signed by a single author, but is the fruit of profound meditations of many generations.

I can not name Lagrange without your recollection, Gentlemen, of how his name and the name of Laplace were often pronounced together and how they had been joined in the world's opinion as representing the summit of intelligence. For a long time scientific Europe had been seeing how a memoir of one of them was succeeded by the other's work on the same subject. And the Bureau of Longitudes, on whose behalf I am now speaking, will retain forever the memory of that memorable meeting when they both came to communicate works on the same theory, one of the most important in physical astronomy. The scope of the problems which occupied those superior men was so vast, that they were able to proceed from entirely different points of view, sometimes even without exhausting the issue.

And their geniuses differed as has been remarked by everyone who studied their work. Be it the libration of the Moon or a problem concerning numbers, Lagrange most often seemed to see in them only mathematics and he therefore set high store by the elegance of his formulas and generality of his methods. On the contrary, for Laplace mathematical analysis was an instrument which he adapted for most various applications, but he always subordinated it to a special method suited to the very essence of the problem at hand.

Posterity will perhaps consider one of them as a great geometer and the other as a great philosopher who had attempted to cognize nature by highest geometry. This is how Laplace had indeed provided for us the theory of capillary action, determined the degrees of probability of [the results of] various methods of treating a large number of observations. He determined the laws of the tides in spite of the large number of arbitrary elements on which they depended and expressed them by especially exact formulas representing observations separated by more than a hundred years.

Laplace discovered the cause and measure of the secular equation of the Moon and the secular inequalities of Saturn and Jupiter, of the two problems with which geometers had been mostly occupied since the former Academy of Sciences had several times unsuccessfully proposed them: those tasks had invariably resisted the efforts made. Among the numerous periodic inequalities of the Moon Laplace distinguished those which depend on the solar parallax and he revealed inequalities caused by the Earth's flattening. Without going out of his observatory, an astronomer can now actually determine the figure of our planet and our distance from the Sun by observing the lunar motion.

Finally, to restrict this enumeration of the admirable results, in which I included those most pleasing his imagination, I am adding that the particular inclination of his mind allowed him to disentangle such complicated laws of motion of Jupiter's satellites. The difficulty of this problem was caused by a unique circumstance which exists in the system of the world but Laplace perspicaciously picked it up: it was connected with the motion of the first three satellites [cf. Laplace (1814/1995, p. 113)].

These works had been appearing without interruption almost to his 60-th year. Had we not known that fecundity is a perennial and essential feature of genius, we would have been surprised by their number and variety. We should also say that it had been his friend Bouvard[3] who accomplished the numerical calculations, which absorbed a considerable part of such a precious time. Laplace's formulas became the basis of Delambre's astronomical tables[4]. He was Laplace's friend as well, and his both capacities were mentioned at his funeral.

It was D'Alembert who directed Laplace's first steps of his scientific career and who was quick to see Laplace as a geometer whom he will soon have to emulate. Although entering the Academy at the age of 24, Laplace earlier discovered an essential fact, the invariability of the mean distances of the planets from the Sun, and besides, he published many important memoirs. The Bureau of Longitudes heard out the reading of his last work, and, so to say, the tone of his last voice. Even this year, just fifteen days before becoming ill, Laplace communicated to us a memoir on the oscillations of the atmosphere since published in the *Connaissance des Tem[p]s*. The printing of a new edition of the historical part of his *Système du monde*[5] has begun. He prepared a first supplement to vol. 5 of the *Mécanique Céleste*, the fruit of his last years, and volume 6 of the *Mémoires* of the Academy of Sciences to be published forthwith contains one more of his memoirs worthy of ending a long series of his works with which he had enriched all our files and which originated in 1772.

This passionate affection for sciences was his life and it was only extinguished with its end. Who will now provide an impulsion to sciences which they had been getting from the activity of his spirit and the cordiality of his soul? Where will those who cultivate sciences find such pleasant approval, such noble encouragement? Musing over the welcome he bestowed upon me in my youth; over the signs of vivid friendship with which he so often lavished on me; over the communications of his thoughts which cleared up my mind about various issues, – in this last parting, musing over all this, I am quite unable to express all the love which I feel for, or how much I am obliged to him.

Addition by Editors

Laplace died in Paris on 5 March 1827, at 9 o'clock in the morning. He was born 23 March 1749 in Beaumont-en-Auge near Caen where he first studied. Monday, the day of his death, the Academy of Sciences assembled as usual, but decided to forgo its session on that day. When Euler died, the Petersburg Academy had provided that same example.

In 1783 Laplace succeeded Bezout as the examiner at the Corps royale de l'artillerie. He married in 1788 and left a son, the inheritor of his title of Peer of France and a colonel of artillery who is occupied by calculating chances as can be seen in the *Fourth Supplement* to the *Théorie analytique des probabilités*[6].

Notes

1. This is certainly wrong. Below, Poisson had himself remarked that the *Méc. Cél.* was *the fruit of profound meditation of many generations*.

2. Rene-Just Haüy (1743 – 1822), a crystallographer and mineralogist.

3. Alexis Bouvard (1767 – 1843), a tireless calculator. He made all the *detailed* calculations in the *Méc. Cél.* His unsuccessful tables of the motion of Uranus prompted the (successful) search of a further planet (Neptune). See Alexander (1970).

4. His tables appeared in Lalande's *Astronomie* (1792), then separately in 1806.

5. That part (Chapter 5 of the *Exposition*) was first separately published in 1821.

6. That Supplement (Laplace 1812/1886, pp. 617 – 645) of 1825 did not mention anyone at all.

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XX

S.-D. Poisson

Review of P. S. Laplace, On the Approximation of Formulas being Functions of Very Large Numbers and on Their Application to Probability

Sur les approximations des formules qui sont fonctions de très grands nombres et sur leurs application aux probabilités (1810).
Œuvr. Compl., t. 12. Paris, 1898, pp. 301 – 345.
Nouv. Bull. des Sciences. Soc. Philomatique de Paris,
t. 2, No. 35, 1811, pp. 132 – 136

Research contained in this memoir supplements the author's earlier memoirs (1781, 1785, 1786). Solution of problems concerning probabilities often leads to formulas including very large numbers and numerical calculation becomes impossible. Analytic formulas contain general solutions of proposed problems but in each case we are prevented from arriving at a numerical value of the probability sought.

It was necessary to derive a means for applying those formulas, and this is what Laplace had done in his preceding memoirs. One of them (1785) provides a general method for reducing functions of large numbers in series converging the more rapidly the larger are those numbers so that those series are the more convenient the more are they necessary. However, in some problems the required probability is only equal to a part of a function of large numbers while its other part is independent from it. This circumstance leads to a new difficulty whose elimination is the main goal of the latest memoir.

Here, we restrict our explication to discussing his most remarkable results; concerning the extremely delicate analysis leading to those results we are unable to provide [here] a satisfactory idea and refer readers to the memoir itself. The author first proposed to determine the probability that the sum of the inclinations of some number of planetary or cometary orbits to the ecliptic is contained within given boundaries when supposing that all the inclinations between 0 and two right angles are equally possible. His first solution repeats the one obtained in 1781. The probability sought is

$$\frac{1}{n!h^n}[(s+e_2)^n - n(s+e_2-h)^n + C_n^2(s+e_2-2h)^n - \\ C_n^3(s+e_2-3h)^n + \ldots - (s-e_1)^n + n(s-e_1-h)^n - \\ C_n^2(s-e_1-2h)^n + C_n^3(s-e_1-3h)^n - \ldots]. \qquad (1)$$

Here, $(s + e_2)$ and $(s - e_1)$ are the given boundaries[1], h is a semicircle, n is the number of the inclinations. Each of the two obtained series should be continued until the term raised to the power of n is not positive anymore and all the other terms neglected. The expression (1) is always finite whichever are the boundaries $(s + e_2)$ and $(s - e_1)$.

Laplace applies this formula to the planets discovered until now, 10 in number, not including the Earth[2]. At the beginning of 1801 the sum of their inclinations to the ecliptic was $90^{grad}.4187$ [$100^{grad} = 90°$] so that for $h = 200^{grad}$, $s + e_2 = 91^{grad}.4187$, $s - e_1 = 0$, $n = 10$, and formula (1), if only all the inclinations are equally possible, provides the probability that the sum of these 10 inclinations should be contained in the interval [0, $91^{grad}.4187$]. This result lets us know the degree of likelihood of that hypothesis. Formula (1) leads to

$$(1/10!)\,(91.4187/200)^{10} = 1.0972/10^{10}.$$

The probability of the contrary event is $(1 - 1.0972/10^{10})$ which does not sensibly differ from certitude. We ought to conclude that the adopted hypothesis is quite unlikely. And so, in the beginning an unknown cause led to the proximity of the planetary orbits to the ecliptic and it is absurd to attribute to chance the smallness of those inclinations. It is evidently easy to calculate the numerical values provided by formula (1) when n is not very large. However, when desiring to apply that formula to cometary orbits and to take into account all 97 of those observed until now, calculation becomes impossible and the formula, useless.

Laplace had therefore provided a second solution according to which the probability sought was expressed by a series

$$(2/\sqrt{\pi})[\int dx \exp(-x^2) - (1/20n)\exp(-x^2)(3x - 2x^3) + \ldots]$$

where π and e are […]. The boundaries of the mean inclination are supposed to be $(1/2)K \pm rK/(2\sqrt{n})$, where K is a right angle, $x^2 = (3/2)r^2$ and the lower limit of the integral is x. When n is very large as in the case of cometary orbits ($n = 97$) that series rapidly converges.

The mean inclination of their orbits to the plane of the ecliptic is $51^{grad}.87663$. Therefore, $K = 100$, $rk/2\sqrt{97} = 1^{grad}.87663$ and $x = 0.452731$. The mean inclination should be contained between $50^{grad} \pm 1^{grad}.87663$. Calculation provides probability 0.4913, almost equal to 1/2. The probability of the contrary event, of the mean inclination being beyond those boundaries, is also 1/2. We have no reason to think that a primitive cause had influenced the inclinations, and the hypothesis of an equal facility of the inclinations can be admitted without any unlikelihood.

After comparing these two solutions and achieving a coincidence of their results, Laplace arrived at this remarkable equation

$$\frac{1}{n!2^n}[(n + r\sqrt{n})^n - n(n + r\sqrt{n} - 2)^n + C_n^2(n + r\sqrt{n} - 4)^n -$$
$$C_n^3(n + r\sqrt{n} - 6)^n + \ldots] = \frac{1}{2} + \sqrt{\frac{3}{2\pi}}\int dr \exp(-\frac{3r^2}{2}).$$

Here, n is supposed to be a very large natural number and the formula is only approximate since magnitudes of the order of $1/n$ were disregarded. The value of r is arbitrary, positive or negative, and equal to the lower limit of integration. Just as above, the series in the left side should be continued until the term raised to the power of *n* is not positive anymore; all the other terms are neglected.

It is permissible to differentiate or integrate that equation with respect to *r* any number of times and thus to form a sequence of other equations which, just like the previous one, are only valid when *n* is very large. Laplace arrived at this equation in an indirect way and a method directly leading to it is desirable. He provided much which we are regrettably unable to indicate here. On one occasion he remarked that the left side of that equation was a function of *n* and *r* which by its form should satisfy two partial differential equations by passing from the finite to the infinitely small. By approximately integrating them Laplace calculated this function anew. Another method of such calculations was based on a reciprocal passage from imaginary to real results which he (1809) had applied previously. Now he remarked:

It is similar to a passage from natural to negative numbers and fractions by whose means geometers have been led by induction to many important theorems. When applied cautiously, as was their wont, it becomes a fruitful method of discovery and it ever more proves the generality of analysis.

The preceding problem about the inclinations of the orbits is the same as that concerning the determination of the probability for the sum of errors of *n* observations to be contained within given boundaries provided that all the errors from 0 to some *h* are equally possible. The formulas which we cited were therefore immediately applicable for determining that probability. However, Laplace had also considered the general problem in which those errors were not equally possible and a given function expressing the law of their facilities was given. Whichever was that law, for the case of a large number of observations he discovered that the probability that the length of the interval within which the mean error was contained shortened with the increase in the number of observations.

That error therefore continuously tended to a fixed term [number] common for both boundaries. Imagine that the law of those facilities of the errors is represented by a curve, then that fixed term is in general the abscissa corresponding to the ordinate of the centre of gravity of that curve with the origin of the abscissas coinciding with the zero error. When positive and negative errors are equally possible, that curve is symmetric with respect to the axis of ordinates and the abscissa of its centre of gravity is zero.

The mean error then converges to zero and the mean result obtained from the set of the observations at the same time converges to reality. By multiplying the observations indefinitely, we increase the probability that in either direction that mean result differs from reality by an arbitrarily small magnitude. That probability, whose value Laplace had determined for an arbitrary number of observations, therefore ever closer approaches certitude and finally, in the case of an infinitely many observations, coincides with it.

Notes

1. More naturally: $(s - e_1)$ and $(s + e_2)$.

2. Cournot (1843, § 146) listed 11 planets including the Earth, Uranus and the four first discovered minor planets. Poisson himself (1837, § 110) applied formula (1) for similar calculations, but he borrowed it from Laplace's *Théorie analytique des probabilités*.

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XXI

S.-D. Poisson

Review of P. S. Laplace, On generating Functions, Definite Integrals and Their Application to Probabilities[1]

Sur les fonctions génératrices, les intégrales définies
et leur application aux probabilités.
Nouv. Bull. des Sciences. Société philomatique de Paris,
t. 2, No. 49, 1811, pp. 360 – 364

Laplace first explicated general considerations about the theories of generating functions and functions of large numbers. He remarked that these theories are the two reciprocal branches of the same *calculus of generating functions*, as he named it. [Here, Poisson mentions earlier work by d'Arbogast, Euler and Kramp.]

Laplace described their veritable theory and eliminated everything possibly paradoxical. Interpolation of series; similarities between powers and differences[2]; and many other parts of mathematical analysis naturally belong to that calculus. However, its greatest advantage is that it forms the basis of the calculus of probability and provides the methods necessary for evaluating probabilities. After these general observations Laplace occupies himself with studying the values of many definite integrals. […]

It is seen that Laplace's method is a new example of passing from real to imaginary magnitudes by induction which he often applies in his previous memoirs as a method of discovery. It is however important to confirm the results thus obtained by a direct method, and this is what we will do on another occasion about the integrals cited above.

The memoir under review contains solutions of three problems on probabilities. I repeat their formulation and indicate, as far as possible, the analysis applied for their solution.

[*The* f*irst problem.*] *Consider gamblers A and B of equal ability. At first B has r counters whereas A has infinitely many. B obtains a counter from A when winning a set, and gives him a counter when losing it. The game continues until A wins all of B's counters. Let r be a large number. It is required to determine the number of sets after which either even money can be bet, or the odds will be 2:1, 3:2, etc that A wins the game*[3].

Laplace first proves that the probability that the game should end [in a finite number of sets] is unity, or certitude. Then he asks for the probability that it ends in a number of sets not exceeding x. That probability is a function of x and r and is expressed by a partial difference equation of second order which Laplace provides immediately after formulating his problem.

He expresses that value [the value of the probability sought] by a definite integral. If x and r are very large, it is transformed into one of those considered previously. For betting equal money the value thus obtained is equated to 1/2. Laplace solved the obtained equation; for r = 100 he found out that it is disadvantageous/advantageous to bet on

23,780/23,781 sets. In general, by equating that probability to $m/(m + n)$, he determined the number of sets for advantageously betting m against n that the game is ended.

The second problem. Consider two urns, A and B, each containing n balls with the same number of white and black balls in the total (in the 2n). Suppose that a ball is drawn at the same time from each urn, that these balls are interchanged and put back and that this procedure is repeated r times. After each drawing of the two balls the urns are shaken so that the balls will be better mixed. It is required to determine the probability that after these r operations urn A will contain x white balls.

That probability is a function of x and r. By a delicate analysis Laplace finds out all the presented chances expressed by a partial difference equation of second order. In case of very large values of x it is transformed into a partial differential equation. Complete integrals of such equations, which really are of the second order, have only one single arbitrary function. Laplace remarks that that problem offers the first example of applying such kind of equations in the calculus of probability.

He determines the complete integral of that equation in a finite form by means of a definite integral, then derives the arbitrary function which it contains by issuing from the initial state of the urns supposed to be known. This step requires a remarkable expansion of the integral whose details are here impossible to analyse[4].

The third problem concerns the mean to be chosen of the results of observations. All its importance, especially for treating astronomical observations, is evident. The problem is here solved for the first time by a direct and general manner provided only that the number of observations is very large. The ordinary method is to choose that mean for which the sum of the observational errors disappears. By issuing from one of his previous memoirs Laplace determined the probability of [the error of] that result whichever is the law of the faculties of those errors, see [xx].

Here, however, he considers that probability from a more general viewpoint: not the sum of errors should disappear, but the sum of each multiplied by indeterminate constants. Then he calculates these constants so that the error of the thus determined result is as small as possible. His analysis led him to the result obtained by the *MLSq* of errors which is being applied by many geometers. However, its advantage was not yet shown. Now Laplace proved that it provides the minimal value of the error to be feared in the result.

And it also has another advantage which makes it preferable to the ordinary method. Actually, by comparing the results of both methods for the same problem, i. e., by determining the probabilities that their errors are contained within given boundaries, Laplace proved that for the same probabilities the boundaries for the MLSq are closer to each other; inversely, for the same boundaries the probability provided by that method is higher. Finally, the author considers the case in which the same result should be obtained by a large number of observations of differing kinds and shows that the MLSq of errors is necessary for choosing the mean of the results of such kinds of observations[5].

Notes

1. The source as stated by the Editors (?) was a report soon to be published. Actually, Laplace (1811) greatly extended it and it appeared under a different title.

2. Similarities and interpolation barely belong to one and the same theory.

3. The gambler's ruin is an important problem and many authors have studied it, see Sheynin (1994, p. 167). The first of them was Pascal who formulated its elementary version. In 1888, overlooked by commentators, Bertrand had considered several relevant problems including Laplace's version, see Sheynin (Ibidem, p. 170).

4. Laplace's second problem deserves a detailed comment and I largely repeat my previous discussion (Sheynin 1976, pp. 149 – 151). Already Daniel Bernoulli (1770) solved Laplace's problem No. 2. The same problem was solved by Lagrange (1777/1869, pp. 249 – 251), Malfatti (Todhunter 1865, pp. 434 – 438).

Laplace worked out a partial difference equation and *mutilated it most unsparingly* (Todhunter 1865, p. 558) obtaining a partial differential equation

$$u'_{r/n} = 2u + 2\mu u'_\mu + u''_{\mu\mu},\ x = (n + \mu\sqrt{n})/2$$

and expressed its solution in terms of functions related to the [Chebyshev –] Hermite polynomials (Molina 1930, p. 385). Hald (1998, p. 339) showed, however, that Todhunter's criticism was unfounded.

Later Markov (1915) somewhat generalized this problem by considering the cases of $n \to \infty$, $r/n \to \infty$, and $n \to \infty$, r/n = const and Steklov (1915) proved the existence and uniqueness of the solution of Laplace's differential equation with appropriate initial conditions added whereas Hald (2002) described the history of those polynomials. Hostinský (1932, p. 50) connected Laplace's equation with the Brownian motion and thus with the appearance of a random process (Molina 1936).

Like Bernoulli, Laplace discovered that in the limit, and even in the case of several urns, the expected (as he specified on p. 306) numbers of white balls became approximately equal to one another in each of them. He also remarked that this conclusion did not depend on the initial distribution of the balls. Finally, in his *Essai* (1814/1995, p. 42), Laplace added that nothing changed if new urns, again with arbitrary distributions of balls, were placed in among the original urns. He declared, apparently too optimistically, that

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

Laplace had thus thought that his urn problem described many important natural processes. Anyway, the Ehrenfests' celebrated model is nothing but that very problem due to Daniel Bernoulli (above).

5. See [xiv, Note 5].

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Addition to Note 4

Here is a description of Markov's paper as provided by O. V. Sarmanov on pp. 672 – 673 of Markov's *Izbrannye Trudy*.

Markov considered (Daniel Bernoulli –) Laplace's problem more generally. There were n_1 and n_2 balls in the urns and, in all, $(n_1 + n_2)p$ were white and $(n_1 + n_2)q$, black, $p + q = 1$, and he denoted the probability that after r cyclic transpositions of the balls the first urn will contain x white balls by $z_{x,r}$.

Markov issued from a partial difference equation connecting $z_{x,r+1}$ with $z_{x,r}$ and $z_{x-1,r}$ but it was Sarmanov who demonstrated it. Then Sarmanov stated that Markov's finding supposed that

$$r(\frac{1}{n_1}+\frac{1}{n_2}) \to \infty \text{ as } n_1,\ n_2 \to \infty$$

or that that product remained constant and equal to 2ρ.

In both cases he considered the random variable

$$\mu = (x - np) \div \sqrt{\frac{2pqn_1n_2}{n_1+n_2}}$$

and indicated that in the first case its moments approached the moments of a normal distribution, and in the second case, of a normal distribution with a perturbative factor expanded into a series of Chebyshev – Hermitian polynomials (which Laplace called after Laplace, Chebyshev and Hermite) $\varphi_1(\mu)$, $\varphi_2(\mu)$, …

$$1 + L_1e^{-2\rho}\varphi_1(\mu) + L_2e^{-4\rho}\varphi_2(\mu) + \dots$$

Finally, Sarmanov noted that Bernstein (1927/1946, pp. 127 – 130) had considered a particular case of that problem.

For large n_1, n_2 and r with given n_1 and n_2 the approximate expected number of white balls in the first urn will be, as Bernstein proved,

$$\mathrm{E}x_r \approx (n_1 + n_2)\exp[-r(n_1 + n_2)].$$

Bernstein S. N. (1927 Russ), *Teoria Veroiatnostei* (Theory of Probability). Moscow – Leningrad, 1946.

XXII

S.-D. Poisson

Review of P. S. Laplace, *Théorie analytique des probabilités*

Nouv. Bull. des Sciences.
Soc. Philomatique de Paris, année 5, t. 3, 1812, pp. 160 – 163

In this treatise Laplace combined his earlier memoirs on probability including the two recently published (1810; 1811). A complete treatise on the theory of chances has resulted; we can find there uniform and general methods for solving related problems and an application of these methods to most important issues. We will briefly indicate the course followed by the author and the sequence of problems which he treated.

Laplace's work is separated in two parts. In **the first one** he described the analytical methods which he applied in the calculus of probability, reduced to a single general method entirely due to him, and called *calculus of generating functions*. That calculus is separated in two branches; one of them encloses the known theory of generating functions, and the other, inverse with respect to the former, includes methods for expressing functions of large numbers by definite integrals, then expanding them in converging series.

In that first part we find important remarks about the metaphysics [philosophy] of the differential calculus, the passage from finite to infinitely small magnitudes, about the use of discontinuous functions in the calculus of partial difference equations and, finally, about a type of induction which Euler and he himself had applied many times[1] and which had discovered for them the values of various definite integrals.

The second part contains the general theory of probability and in particular an application of the calculus of generating functions to the most important issues of that theory. Laplace reduced the number of general principles on which that theory is founded to four[2]. The exposition and demonstration of those principles is the aim of **Chapter 1**.

In the **second chapter** he treats the probability of events consisting of simple events with known probabilities. The simplest problem of that kind and the first that he solved here consisted in calculating the chances in a lottery. Then he determined how many drawings are needed for betting even money on the extraction of all of its tickets. For a very large number of those tickets this problem provides the first application of formulas concerning functions of large numbers. Among the other problems here we note the celebrated problem of points first solved by Pascal and Fermat. Laplace offered a general solution for any number of gamblers with known relative abilities also considering a special circumstance never before entering calculations[3]. Here also is a complete solution of the problem about the inclination of planetary orbits with respect to the ecliptic. It occurred that from the very beginning almost certainly all the inclinations from 0 to 100 grads [to 90°] were not equally possible and that on the contrary, an unknown cause determined very small inclinations as observed by astronomers.

In **the next chapter** Laplace discusses the laws of probability resulting from an indefinite [infinite] multiplication of events. He proves that in a long sequence of trials the possibilities of many simple events, only one of which ought to arrive at once, are proportional to the number of times that each event had occurred. If, for example, an urn has an unknown number of white and black balls and after a very large number of drawings *a* white and *b* black balls have arrived, it is highly probable that the balls of those colours contained in the urn are in the ratio of a:b. Laplace provided an expression of this probability which approaches certainty the closer, the more considerable was the number of drawings. Although the result is in itself very simple and seems to be naturally supposed, it is one of the most delicate points in the theory of chances[4]. Other problems solved here are remarkable since their solution depends of the application of partial differential equations.

Laplace's research of the mean to be chosen of a large number of observations, constitute **the fourth chapter** of his work. He proves that the MLSq provides the minimal error to be feared in the mean result of a large number of observations. He also provides the most probable expression of that minimal error. This chapter is especially interesting for astronomers since there they will find the surest means for comparing the respective merits of their tables[5] and the principles which ought to direct them when compiling the conditional equations for correcting the elements.

The fifth chapter deals with the application of the calculus of probability to examine phenomena and their causes. It ends by solving a curious and difficult problem never resolved before:

A floor is divided into small rectangular cells by a net of mutually perpendicular lines. Determine the probability that a needle randomly thrown on the floor will rest on a joint of those cells[6].

The sixth chapter concerns probabilities of causes and future events as determined by observed events. The general problem solved there, of which the other ones are only particular cases, is this:

An observed event is composed of simple events of the same kind and its possibility is unknown. Determine the probability that that possibility is contained within given boundaries.

The formula which includes the solution of this problem is applied to the births observed in the main cities of Europe. Laplace concludes that the dominance of male over female births can not be attributed to chance and that on the contrary it results from an unknown cause. The ratio of these births derived from a large number of observations is expressed by 22/21 but in Paris it seems to be less and only equals 25/24. Laplace calculated the probability that that anomaly, since it was too large, was not an effect of chance and decided that the observed difference between Paris and other large European cities is due to an unknown cause and very likely discovered it[7]. Also here he determined the probability of results based on mortality tables and finally calculated the population of a considerable empire by issuing from yearly births. Applying his results to France, Laplace found that its population numbered 42,500,000[8] and showed that more than a 1000 can be bet against 1 on the error of that estimate to be less than $0.5{\cdot}10^{-6}$.

The seventh chapter treats the influence of unknown inequalities possibly existing between chances which we suppose to be perfectly equal. He proves that that influence is always favourable to a repetition of the same event. Thus, in a game of *heads* or *tails*, if the coin tends to rest on one side rather than on the other, it is always beneficial to bet on the similarity of the throws although the more probable side remains absolutely unknown to the gamblers.

In **the next two chapters** Laplace studies the most important issues of political arithmetic, such as the mean life, marriages and other associations, mortality tables, advantages depending on the probabilities of future events and those [provided by] establishments based on probabilities of life. One of the most interesting results is the increase of mean life which happens if smallpox is completely got rid of by the use of the [Jennerian] vaccine: the increase amounts to more than three years if, however, the resulting increase of the population will not at all be arrested by insufficient subsistence[9].

Finally, **the last chapter** treats *moral expectation*. For determining it Laplace adopts the Daniel Bernoulli rule which supposes that the advantage resulting from some gain is inversely proportional to the already possessed fortune[10].

Notes

1. The best known relevant place is Laplace's *Essay* (1814/1995, Chapter *On various approaches to certainty*). There, he discussed induction, analogy and hypotheses and noted mistakes made by distinguished authors who had proceeded by incomplete induction. Fermat (p. 113) thus reasoned on a problem in number theory, but remarked that his conclusion (later refuted by Euler) was not really proved. On p. 112 Laplace stated that the plausibility of inductive conclusions increased with the number of observed confirmations. See also Bottazzini (1981/1986, p. 132) about the application of induction by many authors including Laplace when passing over to the complex domain. Poisson [xxi] mentions definite instances of Laplace's application of induction.

2. Laplace had not properly isolated them from his context. Later he (1814/1995, pp. 6 – 14) formulated 10 principles.

3. Actually, the third chapter (§§ 10 and 11) includes the problem about the gambler's ruin with one of the gamblers having an unbounded capital and the so-called Waldegrave problem solved earlier by Montmort, Nicolas Bernoulli and De Moivre (Todhunter 1865, pp. 122, 123 and 139).

4. See my comments to [xxi, Note 4].

5. See, however, [xiii, Note 4].

6. Laplace had not indicated and Poisson apparently did not know that Buffon had introduced that problem (and, definitely, geometric probability) although he considered a set of parallel lines rather than cells. Then, Poisson did not mention Laplace's remark about the ensuing possibility of a statistical determination of the number π.

7. Laplace (p. 392) explained the difference by foundlings, mostly girls, having been left in Paris by people from beyond the city. Neither he, nor his commentators said anything about the possible occurrence of the same phenomenon elsewhere.

8. Poisson was mistaken, but only because Laplace did not formulate his conclusion understandably. On p. 399 he indicated a certain figure provided that the yearly births amounted to 1 *mln*, and on p. 401 he provided another figure (mentioned by Poisson) corresponding to 1.5 *mln* yearly births. Later he (1814/1995, p. 40) repeated the former figure, 28.353 thousand people, but, due to the barely known number of yearly births, only as a tentative estimate.

9. Later Laplace (1814/1995, p. 83) mentioned both the *inestimable discovery of the vaccine* by Jenner, *one of the greatest benefactors of mankind* and the previous method of inoculation and its study by Daniel Bernoulli.

10. Poisson overlooked Laplace's Chapter 11 devoted to studying the testimonies of witnesses and verdicts of law courts.

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XXIII

S.-D. Poisson

A Note on the Probability of the Mean Results of Observations

Note sur la probabilité du résultat moyen des observations.
Bull. Sci. Math. et Phys. Férussac's Bull Universel Sci. et Industrie,
Sect. math., astron., phys. et chim., t. 13, 1830, pp. 266 – 271

We are obliged to Laplace for determining that probability provided that there are very many observations whichever is their law of possibilities. His formula does not depend on that law if only the errors equal in magnitude and having contrary signs are equally possible. It does not presume that that unknown law remains constant during the entire series of observations, as I have shown in my latest memoir (1829). This allows its application to observations made by different observers and instruments. Here is that formula which its illustrious author provided and in addition applied it to most interesting examples in his *Théorie analytique*.

Denote by A some measured thing, by x its unknown value, by a_1, $a_2, a_3, ..., a_n$ its equal or unequal observed values and let their sum be s, and m, the mean of all these values, then

$$s = a_1 + a_2 + a_3 + ... + a_n, \; m = s/n.$$

Subtracting m from the observed values one by one, we find their deviations from the mean. In general, they are very small and are usually applied for judging the quality of the observations.

Denote by h half the sum of their squares

$$2h = (a_1 - m)^2 + (a_2 - m)^2 + \ldots + (a_n - m)^2. \qquad (1)$$

There exists a certain probability p that the error to be feared when m is supposed to be the value of A is contained within the boundaries $\pm 2\alpha\sqrt{h/n}$. Or, in other words, the probability that the difference $x - m$ will not exceed those boundaries. Here, α is an arbitrary numerical coefficient on which p depends. And if n is a large number, and magnitudes of the order of $1/n$ are disregarded, the value of p will be

$$p = \frac{2}{\sqrt{\pi}} \int_0^{\alpha} \exp(-t^2) dt \,. \qquad (2)$$

Here, e […], and π […]. At the end of his book Kramp (1799) provided a table of numerical values of that integral. It shows that as α increases the probability p rapidly approaches certitude and does not differ from it, for example, by 0.0005 even when the value of 2α is inconsiderable, such as $2\alpha = 5$. When desiring to have $p = 1/2$, we should choose α almost equal to 0.4764. And so, it is possible to bet equal money that the error to be feared in the mean result of a large number n of observations is contained within the boundaries $\pm (0.9528/n)\sqrt{h}$.

This value of h is derived from the equation (1), which can also be written in an equivalent form

$$2h = (1/n)s - (1/n^2)\sigma, \qquad (3)$$

where σ is the sum of the squares of the n observational values[1]. The proof of the equation (2) is based on the method which Laplace had provided for reducing the integrals of exponential functions of very large numbers to convergent series. It was demonstrated for the case of one single and directly measured unknown but, without considering that method anew, it can be generalized on a linear function of two or more unknowns, each measured a very large number of times. Indeed, if the law of the possibilities of the errors of each unknown is given, the law of the errors of that function can be immediately derived. To show this, let A, A_1 and C be some things whose unknown values are x, x_1 and z. Suppose that they are connected by the equation

$$z = kx + k_1x_1,$$

where k and k_1 are known coefficients. A и A_1 are directly measured a very large number of times n and n_1. Let m and m_1 be the means of the observed values and h and h_1, the halfsums of the squares of their deviations and denote for the sake of brevity

$$\beta = 2\alpha\sqrt{h}/n,\ \beta_1 = 2\alpha\sqrt{h_1}/n_1.$$

Then there will be probability p provided by formula (1) that the differences $x - m$ and $x_1 - m_1$ are contained within boundaries $\pm\beta$ and $\pm\beta_1$.

Denote also

$$\gamma = \sqrt{k^2\beta^2 + k_1^2\beta_1^2}$$

then there will also be the same probability that the error to be feared in the value $km + k_1m_1$ of C is contained within the boundaries $\pm\gamma$, or that the difference $z - km - k_1m_1$ will not in either direction exceed γ. Indeed, let

$$x = m + v,\ x_1 = m_1 + v_1,$$

then the infinitely low probabilities of the errors v and v_1 are known to be

$$\frac{n}{2\sqrt{\pi h}}\exp\left(-\frac{n^2v^2}{4h}\right)dv,\ \frac{n_1}{2\sqrt{\pi h_1}}\exp\left(-\frac{n_1^2v_1^2}{4h_1}\right)dv_1$$

where terms of the order of $1/n$ and $1/n_1$ are disregarded. In addition, we discard at once the terms of the order of $1/\sqrt{n}$ and $1/\sqrt{n_1}$ which only include odd powers of v or v_1 and therefore disappear from the final result. It will be precise to magnitudes of the order of squares and products of those fractions.

The probability that the two errors, v and v_1, take place at the same time is the product of the two previous probabilities, or, after taking account of the values of β and β_1,

$$\frac{\alpha^2}{\pi\beta\beta_1}\exp[-\alpha^2(\frac{v^2}{\beta^2}+\frac{v_1^2}{\beta_1^2})]dvdv_1.$$

Therefore, if

$$kv + k_1v_1 = u, \qquad (4)$$

and integrating the preceding expression over all the values of v and v_1 satisfying that equation, we obtain the infinitely low probability that the error of

$$C = km + k_1m_1 \qquad (5)$$

is exactly equal to u. And if

$$v = [u/(k + k_1) + k_1\theta,\ v_1 = [u/(k + k_1)] - k\theta,$$

expression (4) will take place for arbitrary values of the variable θ.

We should therefore integrate with respect to θ over $(-\infty, \infty)$. Substituting u and θ instead of v and v_1 we will have, according to the known rule about the transformation of double integrals, $dvdv_1 = dud\theta$,

$$\frac{\alpha^2du}{\pi\beta\beta_1}[\int_{-\infty}^{\infty}\exp[-\frac{\alpha^2\gamma^2\theta^2}{\beta^2\beta_1^2}-\frac{2\alpha^2(k_1\beta_1^2-k\beta^2)u\theta}{\beta^2\beta_1^2(k+k_1)}]d\theta\times$$

$$\exp(-\frac{\alpha^2(\beta^2+\beta_1^2)u^2}{\beta^2\beta_1^2(k+k_1)^2})$$

for the probability of the error u where γ is the same as above. However, according to a known formula, the integral with respect to θ is

$$\frac{\beta\beta_1\sqrt{\pi}}{\alpha\gamma}\exp(-\frac{\alpha^2(k_1\beta_1^2-k\beta^2)^2u^2}{\beta^2\beta_1^2\gamma^2(k+k_1)^2})$$

so that that probability becomes equal to

$$(\alpha du/\gamma\sqrt{\pi})\exp(-\alpha^2u^2/\gamma^2).$$

Therefore, the probability that the error to be feared in the value (5) of C is contained within the boundaries ± γ will be

$$q = \frac{\alpha}{\gamma\sqrt{\pi}} \int_{-\gamma}^{\gamma} \exp(-\frac{\alpha^2 u^2}{\gamma^2}) du.$$

Let $(\alpha u/\gamma) = t$, $(\alpha du/\gamma) = dt$. Then, finally,

$$q = \frac{1}{\sqrt{\pi}} \int_{-\alpha}^{\alpha} \exp(-t^2) dt = \frac{2}{\sqrt{\pi}} \int_{0}^{\alpha} \exp(-t^2) dt,$$

i. e., $q = p$, QED[2]. This result can be generalized on a linear function of three or more unknowns in agreement with the rule announced without proof by Fourier (1829) for an arbitrary function.

Although formula (2) is generally independent from the law of possibilities of errors, I (1824) had an occasion to note the existence of a singular case in which this is not so and the boundaries of the error to be feared in the mean result do not ever approach with a definite probability the real value of the unknown as the number of observations increases[3]. In this exceptional case which can be ignored in practice the rule about the function of many unknowns does not hold either.

Notes

1. Poisson neither proved nor applied this formula.

2. Poisson apparently supposed that the two random variables were distributed according to the same normal law, and proved that their linear function had the same distribution. He thus actually proved that the normal law was stable (although he issued from a large number of observations) which was known to Gauss and Laplace (and which Bessel proved in 1838).

3. Poisson (1824, § 4) had considered the so-called Cauchy distribution. He did not provide an exact reference to Fourier and anyway his statement about Fourier was apparently wrong.

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XXIV

S.-D. Poisson

Speech at the Funeral of Legendre

Discours prononcé aux funérailles de M. Legendre.
J. f. die reine u. angew. Math., Bd. 10, 1833, pp. 360 – 363

Gentlemen, when losing one of our colleagues most advanced in age our grief is alleviated by the thought that he did not suffer during his last minutes, and, being weakened by a long life, died away without sorrow. Today, this consolation is lacking. The illness that terminated Legendre's days at the age of 81[1] had been protracted and painful but he courageously endured his suffering without any illusions about its fatal outcome, and, which should have caused more mental anguish, resigned to leave the happiness of his home life, the care and the expectations surrounding him. Our colleague often expressed the wish that, when talking about him, only his work, which had actually been his life, should be discussed. And in this homage, which I render on behalf of the Academy of Sciences[2] and the Bureau of Longitudes, to an illustrious geometer, the doyen of science whose loss is mourned by the scientific world, I will scrupulously keep to his desire. Being used to study his works, the task imposed on me is not difficult to carry out, and, when addressing you, Gentlemen, I fear to go into details in which you only find eulogistic references [?].

Legendre began his scientific career with one of his fine memoirs. In a short time he analysed the important problem of the attraction of spheroids which Newton and Maclaurin had already synthetically treated. Without fearing that that great analyst [apparently, Newton] had exhausted this issue, Legendre chose it as the subject of his first studies. He was successful, and the applied expansion into a series generated theorems. They were later developed but are still providing a basis for the general theory to which we have ascended.

The work of the new geometer became necessary in 1783, when sciences have lost Euler and D'Alembert, and opened to him[3] the doors of the Paris Academy of Sciences, so famous in Europe. His second memoir was devoted to a problem of no lesser importance connected with the first one. He offered the first and only known until now direct determination of the figure of a homogeneous fluid planet[4]. Soon he extended his research to the general case of planets consisting of heterogeneous layers. During the same period Legendre read out in the Academy a memoir on the calculus of partial differences and described many methods of integrating and applying them to various examples.

After participating in an astronomical operation aiming to connect the meridians of Paris and Greenwich, Legendre was led to occupy himself with problems in trigonometry, and sciences gained a very useful theorem about the area of almost spherical triangles [1806] such as those traced on the Earth's surface. The *Mémoires* of the First class of the Institut de France also include his other researches about spheroidal triangles which were preceded by a joint, with Delambre, publication [1799] on calculating meridian arcs.

The Berlin Academy of Sciences proposed a prise problem about the motion of a projectile in the air. Legendre took part and won the prise. When additionally mentioning that our colleague was the author of a method of calculating cometary orbits; that the sciences of observation are obliged to him for a rule called the MLSq of errors which Laplace later proved to possess all possible advantage concerning precision of the results; if recalling his numerous studies of the two types of definite integrals which he called after Euler; if I say that, for example, for almost 40 years he participated in calculating the great, still unpublished logarithmic tables under the guidance of Prony[5]; and if I finally mention Legendre's *Eléments de Géometrié* [1794] where he was the first to indicate a kind of equalities until then disregarded but necessary for completing the demonstrations known from the time of Euclid, – if adding all this together, you will undoubtedly agree, Gentlemen, that it entirely justifies Legendre's elevated scientific rank.

And I still have not, however, mentioned two branches of research which he had preferred, to which he had so many times returned during his long career and by which he ended it with two great relevant contributions and thus compiling everything he had done and all that we know as doctrines of the theory of numbers and of the Eulerian integrals. Problems about the properties of numbers, being remote from any applications, are indeed powerfully influencing mathematicians. They present extreme difficulties which Legendre had often overcome by following the two great geometers he most admired, Euler and Lagrange.

The treatise on elliptical functions includes their numerical tables which he calculated and which in themselves meant an immense task. For a long time no one else had been occupied by that theory until Abel and [C. G. J.] Jacobi had not proved, in the beginning of their scientific work that after Euler and Legendre it was still possible to make fundamental discoveries in their beloved science. You will not forget, Gentlemen, how lucky he felt, how he abandoned himself to it, how effusively he exposited it. That science in which those two young rivals followed him, – he spoke about it as though it was a quite new creation.

Nevertheless, Legendre did not remain behind their work. Moreover, although being almost 80 years old, in less than a year he published the third volume of his treatise on elliptic functions that contained all their discoveries [in that other field] and the developments which he was able to add. His satisfaction of discovering two successors worthy of himself had not lasted long: science lost Abel [in 1829] soon after he became known.

Common to Legendre and many preceding geometers was that their work only died with their life. The latest volume of our *Mémoires* includes another memoir of Legendre about a difficult problem in the theory of numbers. And not long before he became terminally ill he got the most recent observations of comets with short periods for applying and perfecting his methods. This is quite worthy of remarking and it is also consolatory indeed to see that when our physical strength abandons us, our intellectual power is still quite vigorous for occupying ourselves with difficult speculations. History of science provides many such examples. At an age almost the same as that reached by Legendre, Lagrange died while publishing a second edition of his *Mécanique analytique*, twice larger than the first one. Laplace died while completing the fifth volume of his *Mécanique Céleste*[6], and Euler, while having almost completed calculations of the buoyant force of balloons, of the invention then interesting the public and scientists.

So that was an enumeration of the contributions of all the kinds which had been made without any interruption during the entire life of a celebrated geometer whose loss is added to those which the *Institut* [*de France*] had experienced this latest year. Less than in a year Cuvier was stolen from natural, and Legendre, from mathematical sciences. The cruelly fair death has at the same time hit both classes of our Academy.

Notes

1. Adrien-Marie Legendre, 1752 – 1833.

2. In the absence of Arago who was charged with examinations in Metz. S.-D. P.

3. The Academy included four more of our colleagues, De Cassini, De Jassieu, Desfontaines and Tassier. S.-D. P.

Of our (parmi nous) certainly did not mean *still living*, see the *Information* below. O. S.

4. Later classical work by Poincaré and Liapunov (stability of the figures of equilibrium of a rotating liquid) can be mentioned.

5. In 1793, Prony with numerous calculators had begun compiling logarithmic and trigonometric tables to 14 – 29 decimal points which does not mean that their precision was the same and which was hardly necessary. Only their excerpt was published in 1891.

6. That volume was published in 1825, about two years before Laplace died.

Information about Scientists Mentioned by Poisson

J. D. Comte de Cassini, 1748 – 1832, cartographer, astronomer

R. L. Desfontaines, 1750 – 1833, botanist

A. L. de Jussieu, 1748 – 1836, botanist

G. C. F. M. R. de Prony, 1755 – 1839, mathematician and engineer

H. A. Tessier, 1745 – 1837, professor of agriculture and commerce

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