Tude

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Notation **S, G,** n refers to downloadable file n placed on my website www.sheynin.de which is being diligently copied by Google, Oscar Sheynin, Home. I omit the unnecessary adjective in mathematical expectation.

Tude

D. F. Arago

Tyho Brahe, Kepler, Laplace

Tycho Brahe

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Foreword

It is strange indeed that Arago failed to mention that Kepler was able to refute the Ptolemaic system of the world by issuing from Tycho's observations. The three wooden rulers that belonged to Copernicus (§ 6) were perhaps the remnants of an instrument (of a quadrant?).

[1] Tycho Brahe, whom all the astronomers who succeeded him justly considered as the most exact observer of the pre-telescopic era, was born 13 December 1546 in Knudstrup, Scania, a province then subordinated to Denmark; his family belonged to the most ancient nobility of the kingdom.

According to the ridiculous ideas of that time, Brahe's father refused to instruct him even in Latin and it was because of the care of one of his maternal uncles that, without his family's knowledge[1], he was sent to school where his intelligence began to develop.

The solar eclipse of 1560 whose main phases almost exactly accorded with those stated in the ephemerides, extremely excited his imagination and contributed to the choice of his vocation. At the age of fourteen Tycho was sent to Leipzig for receiving a superficial instruction that in those remote years was thought to prepare a respectable member of the nobility for public service. There, without his tutor's knowledge, he gave himself up to studying mathematics and astronomy and spent all the money he got for entertainment to buy books and instruments.

In 1565, after his return to Copenhagen, people of his own estate were regarding him as an eccentric. Because of the disagreements caused by his relations with people unable to appreciate him, he again went to Germany where at that time many eminent astronomers including Guillaume IV, the Landgrave of Hesse-Cassel[2], whose friend he became had been living. Tycho diligently visited the main German observatories and, while passing Augsburg whose masters were enjoying a great reputation, he ordered many new instruments that served him to resolve important problems concerning the motions of the starry heaven.

[2] Upon returning to Copenhagen, Tycho began leading a secluded life. Owing to his observations of the New star of 1572 chancellor Oxe became his declared admirer and inspired King Frederick II of Denmark with the same feeling. Soon afterwards the king presented Tycho the small island Hven in the Sund strait between Elseneur and Copenhagen. In addition, the king granted him a pension of 500 écus, a fief in Norway and a canon's benefice valued at 2000 écus which would serve for maintaining an observatory built at the king's expense.

This rare generosity made eminent the small island of Hven. An observatory, since then destroyed, is existing eternally in the astronomers' memory under its name, Uraniborg. After the construction of the observatory was achieved, Tycho gradually equipped it with instruments manufactured under his care. That cost him not less than 100 000 thalers taken from his own fortune. The enormity of that sum will not astonish those who reads in Tycho's *Astronomiae instanratae mechanica* [1598, 1602] the description of the various instruments and their colossal dimensions (5 – 6 cubits or 2 – 2.5*m*) which he made use of one after another. All the new instruments had copper limbs divided with greatest care. The difficulties often encountered with these delicate constructions inspired Tycho to exclaim: *A good instrument is an Arabian wonder*! Nevertheless, he thought that by all his means taken together he had achieved precision up to 1/3, 1/4 and even 1/6 of a minute[3].

For ancient observers, measuring time was the most common obstacle. Tycho had been testing clepsydras and [various] clocks. In his first clocks, he used purified and well *revivifié* [losing its natural fluidity] mercury flowed out from a small opening but remaining in a conical vase at the same height. The weight of the flown out mercury should have indicated the time. Tycho also applied purified lead ground to very fine powder. But he said

To confess the truth, the ruse of Mercury possessing it [possessing the truth?], *mocks both astronomers and chemists, laughs at my efforts and Saturn*[4]*, not less a trickster although a friend of labour, does not favour me better than that which I had imposed on myself* [than mercury].

Tycho's collection of instruments included many clocks showing seconds, certainly without regulating pendulums, and, beyond the observatory, a copper clock also marking seconds; the diameter of its main wheel was two cubits or almost a meter and it had 1200 cogs.

[3] Uraniborg was built in 1580 and Tycho consecutively worked there for 17 years. He married a charming peasant's daughter called Christine who had borne him eight children. An intervention of the king himself became necessary for contracting that alliance because the entire nobility hindered it by alleging that Tycho was demeaning himself.

After Frederick II had died, during the minority of Christian IV, the nobles, already strongly irritated by Tycho, perhaps because of his success and immense reputation that he was enjoying in Europe, deprived him of his pension and benefice without which a simple private person evidently could not have taken care of the expenses incurred by maintaining the vast establishment created by him.

It was actually said that Tycho had no less than 20 – 30 collaborators either for observations or calculations. That injustice was mainly due to senator Walckendorp[5]. His name, says Laplace, like the names of all those who had abused their power for arresting the progress of the mind, should be scorned forever.

We ought to say that the hate which so unfortunately separated Walckendorp from Tycho was occasioned by a trivial cause. Danish writers reported that the senator, while being in Uraniborg with the young King Christian IV, impatiently tolerated the barking of two mastiffs presented to Tycho by King James VI [of Scotland] when visiting the observatory. Walckendorp kicked them whereas Tycho interceded on their behalf and an argument followed. Thus occurred the hostility with such fatal consequences for astronomy.

Uraniborg maintained a chemical laboratory where Tycho had been preparing medicines then freely distributing them to the poor. It was said that that circumstance annoyed the Copenhagen physicians who joined their howls to those of the nobility. I would have desired that, for the honour of the art of medicine, that story were fabricated.

[4] The great astronomer abandoned Hven with all his instruments[6] and the six children which were left him. He went to Denmark but was not permitted to decently establish himself there. Soon afterwards Tycho moved to Germany. [The Roman – German] Emperor Rudolph II ensured him a splendid position but he only enjoyed it for a short time.

24 October 1601, being 54 years old, Tycho died of retention of urine. He had already felt slight symptoms of that infirmity [?] sometimes previously and it is reported that he had once experienced serious danger while having a long stroll with the emperor since he thought it disrespectful to separate himself [for a minute] from his sovereign. If that story is true, Tycho Brahe ought to be ranked among the victims of etiquette[7].

[5] Something unusual is seen on the remaining portraits of Tycho. During his second voyage to Germany, he quarrelled in Rostock with one of his compatriots largely owing to some theorem in geometry. The quarrel carried them away and a duel followed during which Tycho lost a larger part of his nose. To cover the traces of that accident as much as possible he made a false nose of wax, or, according to other sources, of an alloy of gold and silver[8] and painters and engravers when reproducing the features of the great Danish astronomer believed themselves obliged to leave its obvious marks.

Why is it necessary to add to the description of a life so usefully devoted to the progress of science that Tycho, with respect to some problems did not elevate himself above the prejudices of his century and believed in alchemy and even astrology?[9] It is remarked with surprise that, for example, he thought it important to note that his subject, as far as it concerned the planet Mars, predicted that his face will be deformed which was realized during that duel.

The main argument on which Tycho based himself for making horoscopes somewhat plausible was very peculiar. He says:

The sun, the Moon and the stars are sufficient for our usage. It is really useless to attribute to the planets such a majestic route and subject them to such wonderful laws if they are not directly useful in their proper way, and so, that utility is the object of astrology.

And he maintained that the comets must possess some virtue, have some influence because nature does nothing in vain. It is really pitiful to indicate among Tycho's thoughts that strange idea that stars are capable of stimulating the powers of the planets. And we see that Tycho only gradually got rid of the nobility's prejudices and that he even hesitated to make public his observations of the New star of 1572 because, as he said, it was not proper for a man of his status to publish anything.

[6] We will not terminate this note without defending Tycho against the suspicion levelled at him by various authors that he was led to the unfortunate system of the world bearing his name being inspired by the jealousy of the work of Copernicus.

On the contrary, all Tycho's writings testify to his profound admiration for the astronomer from Thorn [Torun]. Upon receiving a present of three wooden [graduated?] rulers which Copernicus made use of for his observations, Tycho kept them in the most prominent place of his observatory and compiled an appropriate Latin verse distinguished by most proper enthusiasm. He framed and suspended it alongside the instrument that had belonged to the author of the *On the Revolution of Celestial Spheres* [1543, and here it is:]

The Earth did not produce such a genius for many centuries. […] *The giants of antiquity desiring to penetrate the sky, amassed mountains and placed Pelion on Ossa*[10]*. However, powerful owing to* [capable in accord with] *the force and weakness of the mind, they were unable to reach the celestial spheres. He, trusting the power of his genius, although weak of body, elevated himself with these trifling wooden pieces to the greatest heights of Olympus.* […] *The memory of such a person is inestimable even if it consists of frail pieces of wood*.

[7] Here is a list of works published by Tycho Brahe[11]. The most important of those written by the illustrious astronomer is *Astronomiae instauratae Progymnasmata* [1602]. It contains his main investigations and we believe it necessary to provide its critical analysis.

Among Tycho's works we should first of all place his discussion of the observations of the Sun and the ensuing tables. There, we see for the first time the consideration of atmospheric refraction whose [quantitative] values he discovered by means of his own observations. In spite of all the ingenuity in his methods, he was mistaken here since he maintained that refraction was non-existent at height [zenith distance] 45°. He was no less mistaken concerning the cause of that phenomenon believing that it was due to the vapours with which the atmosphere is usually filled rather than to the gaseous substances of which essentially consists the aerial envelope of our globe from the layers bordering the horizon to the zenith.

The third mistake brought about by his instruments was to suppose that the solar and lunar rays experience refraction differing from that of the star rays. Nevertheless, Tycho will forever enjoy the indisputable glory of being the first, together with the astronomer [Christoph] Rothmann, a collaborator of the Landgrave of Hesse-Cassel, of introducing refraction in the discussions of astronomical observations[12].

Considering the Moon, Tycho established that the Ptolemaic theory did not represent the observations; it was seen that there was a very perceptible inequality in the motion of that celestial body around the

Earth and especially during the octants when it deviates by about 36', positively in the first and fourth octants and negatively in the other two. That inequality, one of the greatest discoveries of modern astronomy, is called *variation*[13].

Tycho paid very special attention to periodic variations in the inclination of the lunar orbit with respect to the ecliptic and to a certain point assigned their laws. To him also we are indebted for valuable remarks about the perturbations experienced by the nodes of the lunar orbit in their general retrogradation, and still more for the determination of the parallaxes of our satellite. Although still corrupted by rather grave errors, they are much more precise than all those established by the predecessors of the astronomer from Uranisborg.

A dominant place in Tycho's work ought to be reserved for the determination of right ascensions and declinations of stars, or, in other words, for his efforts that led to the compilation of his celebrated catalogue. At the time of Tycho, for an astronomer deprived of telescopes the sunlight eclipsed the additional light of all the stars however many there could have been. Venus is sometimes seen by the naked eye even if the Sun is shining at the same time above the horizon. Under these circumstances, Venus might be compared to the Sun. As soon as night closes over, that planet can be compared with the stars. Stars are easily compared with each other[14]; their places got relative to the Sun or to the equinoxes whose position are known from previous observations of the shining star [of Venus].

That procedure is satisfactory if only the astronomer who applies it knows how to deal with all the errors to which that complicated method is exposed. Cardano, who thought to be the first one, applied a shocking solution that led him to a catalogue whose errors surpassed 1°40', greater than those with which the catalogues of Alphonso[15] and Copernicus are reproached.

[8] Tycho devoted seven years to these researches. Dominated by religious scruples resulting from false interpretations of the Bible, or by the desire to attach his name to a system of the world differing from that of Copernicus[16], Tycho supposed that the immovable Earth was at the centre of the world; that all the planets had the Sun as the centre of their motion; and that the Sun, followed by that cortège of the planets rotates about the Earth. It should not be thought that, when proposing his system, the celebrated Danish astronomer got rid of the epicycles which complicated the Ptolemaic system in such an unfortunate manner. Actually, it is seen in his works that Saturn's orbit, being concentric with that of the Sun, had two epicycles with Saturn moving around the second one.

Tycho thought that the stars were very near Saturn's orbit: It is *absurd*, he said, to believe in space devoid of stars and planets. He seems to me being ranked among those astronomers who, according to Copernicus had been considering a certain equality of the distribution of matter as a primordial law of the universe.

Aristotle believed that the comets were meteors engendered in our atmosphere, but Tycho proved by numerous observations of the comet of 1577 that it did not have an appreciable diurnal parallax so that it was much further from the Earth than the Moon. He discovered that other comets had no sensible traces of annual parallax, that consequently in the Copernican system they were much more remote than the former.

Moving freely in space, these celestial bodies cannot encounter the solid spheres which for a long time served for explaining planetary motions. It was Tycho, therefore, who had forever done away with those famous crystal spheres of the ancients which Purbach[17] revived with some theoretical amelioration.

Tycho's catalogue, his most real title for being forever remembered by scientists, had only comprised 777 stars. But he established the same number of right ascensions and declinations and it would be unjust to omit to remark that that was the result of immense work accomplished during many years at the observatory of the ever celebrated Uraniborg.

Notes

1. Hellman (1970) explains that the childless uncle took Tycho away from home and became his guardian. O. S.

2. William IV, 1532 – 1592. Arago compiled his biography, see the same volume of his *Oeuvres*. O. S.

3. Wesley (1978) devoted a paper to that subject without understanding the theory of errors. Anyway, Arago's estimate seems to be too pleasing, and there still remains a question: how did Tycho treat his observations when one or even more of his instruments had to be temporarily taken out of service? Its (their) removal from the pool could have led to a systematic shift in the mean measurement. O. S.

4. The name given to lead by the ancients. F. A.

5. Dreyer (1890/1963, pp. 216 – 217) states that Walckendorp did not even visit the observatory at that time. O. S.

6. Highly improbable. Hellman (1970) reports that, at that time, Tycho only took some of them. O. S.

7. Here is another story reported by contemporary authors.

On 13 October 1601 Tycho dined at Rosenberg's place and much had been drunk. Tycho felt the pressure in his bladder but preferred, as it is said, civility to health. Upon returning to his own place, he was unable to urinate and that indisposition [?] *continued and caused him a lot of pain. Insomnia, fever and delirium followed. Physicians were unable to compel him to eat. He quietly passed away on 24 October in the midst of consolations, prayers and tears of his nearest and dearest.*

The biographies do not say whether Tycho himself or his friends made the astrological remark that at the beginning of his illness the Moon was in opposition to Saturn and Mars occupied the same place in Taurus as at the moment of his birth. This is where we had been at the beginning of the 17th century. F. A.

Arago did not say anything about Rosenberg. O. S.

8. And copper. It is now all but established that Tycho died of copper poisoning (Hellman 1970). O. S.

9. A purely rhetoric question. O. S.

10. Pelion and Ossa are mountains in Greece. The former is essentially mentioned in Greek mythology. O. S.

11. I do not reproduce it. Tycho's works had been since published in full: *Opera omnia*, tt. 1 – 15. Copenhagen, 1913 – 1929. O. S.

12. Refraction, known to Ptolemy, is caused by the optical heterogeneity of the atmosphere. See *Great Sov. Enc.*, English edition, vol. 22, article *Refraction*. O. S.

13. Sédillot thought that a recently discovered manuscript had stated that that discovery should be attributed to Aboul Wefa. See an analysis of the ensued discussion by Biot and that learned orientalist in the *C. r. Acad. Sci*. F. A.

14. This is difficult to understand. O. S.

15. Alphonso X the Wise (1221 – 1284). O. S.

16. This alternative contradicts Arago's statement at the beginning of § 6. O. S.

17. The Austrian/German astronomer Georg von Purbach (1423 – 1461). He is also mentioned as Peuerbach etc. O. S.

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Tude

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Kepler

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Foreword

This essay is still interesting and I only remark on two points. First, Arago did not say anything about Kepler's methods of treating observations. True, neither did other commentators even in our time and I may therefore refer to my own paper (1993, § 5). Second, concerning Kepler's attitude to astrology, see also my more detailed account (1974, § 7). There also, on p. 107, I quote Kepler's mother accused of witchcraft (see Arago's § 7). She was finally acquitted after kneeling down in the presence of her judges and praying: let God "give a sign if I am a witch or monster". Did not Kepler himself, for all his piety, give his mother such a wonderful advice?

[1] The immortal Keplerian laws, the fruit of indomitable perseverance of a most fertile scientific genius of modern times, are not the only service which that prodigious man rendered astronomy. The deep imprints of his incomparable perspicacity are found everywhere; the views with which we are indebted to him were partly unacknowledged since the public got them mixed with systematic ideas[1]. It was thought opportune to battle it out with the application of hypotheses in any serious scientific investigation as though it was possible to imagine experiences of some value without assistance of hypotheses. Important is really to refuse to regard any theoretical idea as perfectly established unless and until being sanctioned by observations and calculations.

Kepler[2] kept true to that rule as far as possible. He never hesitated to abandon his dearest speculations once experience undermined them. The acute hardship experienced by him and his family obliged Kepler to publish at the request of book traders, so to say, daily and he became accustomed to think quite loftily and to initiate the public into everything dawned on him. Are there many among those called the wisest who would have supported such an ordeal? I do not claim, however, that Kepler's numerous works do not at all contain such concepts which the preceding considerations cannot excuse. But at least a mitigation of their eccentricity is most often found in the way of life that circumstances had imposed on the great astronomer and in the influence that the unparalleled difficulties experienced by his family should have exercised in his character.

The connection that I am attempting to establish here between the strained circumstances of Kepler's private life and the fruits of his imagination lose their paradoxical feature initially desired to be attributed to him after reading the biography of that restorer of modern astronomy[3] partly based on the work where Breitschwert analysed his unpublished manuscripts discovered in 1831[4].

[2] Johannes Kepler was born 27 December 1571 in Magstatt, a württembergian village in a mile from the imperial town of Weil in

Swabia. He was born two months premature, very feeble and of delicate appearance. His father, Heinrich Kepler, was a son of the mayor of Weil and his family, was very poor, with claims on nobility: in Rome the Emperor Sigismund conferred the title of *chevalier* on one of Kepler's ancestors. His mother, Catherine Guldenmann, daughter of an innkeeper from the neighbourhood of Weil, was not intellectually cultured, she was even unable to read or write. She spent her younger years with an aunt burnt for sorcery. Kepler's father participated in the war against the Belgians under Duke d'Albe.

Being six years old, Kepler was taken ill by smallpox; in 1577, just after escaping death, he was sent to a school in Leonberg. However, after his father returned home, he was totally ruined by the bankruptcy of a person for whom he had imprudently guaranteed. He then opened a cabaret in Elmerdingen, took his son from school and charged him with serving his clientele which Kepler did until the age of twelve. The person who was to distinguish to such an extent his name and his homeland began by being a waiter in a cabaret.

At thirteen, the young Kepler was taken ill by a severe illness and for several days it was thought that he will not survive. His father, when business did not prosper, enlisted in the Austrian army that fought the Turks and since then he was not heard of. His callous mother of a fault finding and cunning nature looked after the boy in a very bad way and dissipated the family's 4000 florins.

Johannes Kepler had two brothers of a character reminding their mother's. One was a worker in a tin foundry, the other a soldier, both really being hoodlums. The young boy only found comfort in his family from the tender friendship lavished upon him by his only sister, Margarete, married to a protestant pastor; as to the latter, he also sided with the enemies of the future astronomer.

[3] At first, Kepler had been employed as a worker in the fields but the young man, meagre and very feeble, was unable to endure the fatigue of labour and destined for theology. At eighteen, in 1589, he entered a seminary in Tübingen where he was taught at the expense of the state. At the examination that he was liable to take for the degree of Bachelor he did not gain the first place. That distinction was awarded to Johann-Hippolyte Brentius whose name, as I believe, is not included in any historical dictionary in spite of the poor efforts made by their authors who only mention real celebrities. This is not the last time that we will see in these [biographical] notices the decisions of the university authorities ruthlessly rejected by irrevocable judgement of time.

While still on a school bench, Kepler actively participated in disputes about the protestant theology; however, since his booklets contradicted the württembergian orthodoxy, they were declared unworthy of dissemination among the clergy (d'avancement dans l'Eglise).

Moestlin, who in 1584 was invited as professor of mathematics from Heidelberg to Tübingen, happily directed Kepler's mind otherwise. Kepler abandoned theology although without completely getting rid of a decided tendency for mysticism, a fruit of his first education.

[4] At that time he became acquainted with the work of Copernicus. Here are his own words:

Since I became able to appreciate the charm of philosophy I was ardently embracing all of its parts but I had not paid special attention to astronomy although I had easily succeeded to understand well enough all that was taught in school about it. I was educated at the expense of the Duke of Württemberg and when I saw my schoolfellows accepting positions in his service, I decided to accept the first post offered me.

That was the post of professor of astronomy. In 1593, Kepler, being 22 years old, was appointed professor of mathematics and morals at Gratz in Styria. He made his debut by publishing a calendar calculated according to the Gregorian reform.

[5] In 1600 serious religious persecutions began in Styrie and all protestant professors were expelled from the Grätz college. Kepler was not excluded although he was naturalized in a way by marrying, in 1597, a noble and very handsome woman, Barbara Muller, who already had two husbands. While marrying for the third time, she demanded on Kepler a proof of nobility and he felt obliged to obtain it in Wurtemberg. That union was not happy.

The same year Tycho invited Kepler to Prague to become his assistant, but the latter, having just arrived there, wrote to his friends:

Everything here is uncertain. Tycho is a person with whom it is impossible to live without being incessantly exposed to cruel insults. The salary is brilliant, but the cash box is empty and there are no payments.

His wife was compelled to demand each florin from Tycho but that humiliating dependence did not last long: Tycho died 24 October 1601. Kepler was immediately appointed court astronomer with a pension of 1500 florins, but money was still not paid up. He wrote: "I am wasting my time at the entrance to the royal treasury and begging". One circumstance consoled Kepler amid all these difficulties, and that was having from that moment Tycho's original observations at his free disposal and being able to search there for the secret of planetary motions.

In 1611 Kepler lost three children as well as his wife who first became epileptic, then mad. Among the problems he had to endure we ought to reckon the need of the emperor and of a crowd of other princes and the demand they made on the celebrated astronomer to compile them on all sorts of occasions.

[6] After Emperor Rodolphe [Rudolph II] died, his successor, Emperor Mathias invited Kepler to the Sejm [representative assembly] at Ratisbonne to help regulate the corrections of the calendar that the protestants refused calling it odious and, what was much worse at that time, papal. Although being in the sovereign retinue, Kepler had to earn his living by compiling small calendars containing prognostications; the arrears due him increased at that time to 12,000 ecus.

After defending the cause of the reform mentioned at the Sejm, Kepler was compelled to accept a chair of mathematics at a gymnasium in Linz. The same year he contracted a second marriage with the handsome Susanne Rettinger who had borne him seven children.

[7] His spiritual happiness did not last long. The catholic priests of Linz and the protestant priests of Württemberg simultaneously accused him of heresy and it proved very difficult for him to repel their attack. In 1615 a letter from his sister to Kepler implored the great man to help their mother accused of sorcery. The process [against her] lasted for more than five years. After unsuccessfully asking in writing the Duke of Württemberg to stop that incredible persecution, Kepler went on horseback from Linz to Stuttgart to appraise the effect of his personal solicitations.

Coming there, he found out that his mother aged 75 was accused of having been brought up to [respect] and taught the magical art by an aunt burnt in Weil as a sorceress [see § 2]; of having frequent talks with the devil; of being unable to shed tears; of causing the death of pigs in the neighbourhood where she strolled by night; and, finally, of never looking at the faces of those with whom she spoke, which, as it was said, was a habit among sorcerers.

She was also reproached for having engaged a gravedigger to get her the skull of her husband[5] which she wished, after trimming it with a silver ring and forming a goblet, to present to Kepler. That charge did not stick. The judges decided that the executioner should terrify the old woman by showing her one after the other the instruments of torture consecutively increasing pain and explaining the method of their action.

That terrible explication did take place; the old woman resisted all the threats and concluded by declaring that "Mid the torments I'll say I am a sorceress, but that'll all the same be a lie". Such courage led to success; Kepler's mother was released and died in August 1622.

Kepler returned to Linz, but his enemies insulted him so much calling him the son of a sorceress that he was compelled to leave Austria. Finally, at the instigation of the Jesuits, as it was said, he predicted the conferring of the Duchy of Mecklenburg to general Wallenstein. The illustrious astronomer did not, however, sufficiently encourage the decided inclination of the celebrated general to predictions drawn from the aspects of the celestial bodies, lost his favours and was replaced by the Italian astrologer Zéno.

Kepler vainly attempted to receive the payment of the arrears due him according to the treaty. His frequent travelling on horseback between Sagan and Ratisbonne[6] for obtaining what was justly owed him weakened his health and he died 15 November 1630 at the age of 59 years. He left 22 ecus, an outfit, two shirts and no books except 57 copies of his *Ephemerides* and sixteen of his *Tabulae Rudolphinae* [1627].

He himself compiled his epitaph; it was read out in the church of Saint Peter in Ratisbonne, and here is its translation [into French]:

I have measured the sky, I am now measuring the shadows of the Earth. Intelligence is celestial, here only repose shadows of bodies.

Dalberg, the coadjutor of Mayence and bishop of Ratisbonne, built a Doric order temple ten metres high dedicated to Kepler in a copse of the Ratisbonne botanical garden. Kepler's bust cast in bas-relief by a celebrated sculptor from Stuttgart recalls Voltaire's verse:

When a poor man in the grave is put
Rumour matters not, that word he hears not anymore.
Pope's shadow with royalty reposes,
The entire nation deifies him.
His name to immortality flies off,
While living was he persecuted.

[8] We will describe the ugly treatment that he had to endure during his lifetime adding that at the moment of his death the princes whom he served and with their caprices he even complied, owed him 29,000 florins. The gloomy details in Kepler's biography occupy a special place in the martyrology of science and they in any case allow me easier to touch the somewhat obscure parties in the great man's career.

Kepler, as it is stated, believed in horoscopes; it is more accurate to say that he compiled predictions at the instant demand of the sovereigns under whom he passed his life. However, he never explained himself regarding that subject as clearly as he did in his other publications. He said that

People are mistaken when they believe that it is the celestial bodies which determine things here below. These bodies are only sending us light; but, according to how are its rays arranged at the birth of an infant, his life is shaped in one or another form. If the arrangement is harmonious, he develops a good form of soul and that soul builds for itself a good dwelling. However, the strong always bear strong children, and the kind bear the kind[7].

I reject as still less intelligible what Kepler says about the influence of the celestial bodies on the soul of the world for arriving at a naïve recognition that

The philosophers extolling their sagacity should not have so mournfully blamed the daughter of astronomy; it is that daughter that feeds its mother. How small will actually be the number of scientists devoted to astronomy, if people would not have hoped to read future events in the sky!

Owing to the process instigated against his mother, Kepler wrote a great number of letters in which he spoke about sorcery as a phenomenon whose existence could not be denied. It is painful to read such opinions in his writings but who will dare to assure that these declarations were not at all dictated by the fear of indisposing the judges about to decide definitively his mother's fate? A bit of diplomacy is indeed excusable for a son pleading for his mother threatened by an auto-da-fé.

Kepler thought of popularizing the Copernican system by constructing, at the expense of the Grand Duke Frederick of Württemberg a sphere in which each celestial body was to be represented by a ball filled with alcoholic beverage bearing a relation to the body's intimate essence. The Sun will be filled with spirit;

Mercury, with ordinary brandy; Venus, with liquid honey [mead]; Mars, since it caused astronomers so much grief and did not wish to submit to calculation, with absinthe; Jupiter, with wine and Saturn with beer.

All that is certainly very childlike, but nevertheless it is not less sure that he regarded that idea seriously which is an argument proving that Kepler yielded to wild outbursts of imagination.

Kepler's character was robust and honourable. The love of truth without faint-heartedness was the basis of his esteem. Thus, he wrote: "I love Copernicus not only as a superior intelligence, but also as a free mind".

When the process of his mother was over, he had to leave Linz and Austria, and Jules de Medicis recommended him to the Republic of Venice which invited him to a professorship in Padua, but he answered:

I am a German by birth and by feeling, and am accustomed always to say imprudently the truth. I should not expose myself to be thrown in an autoda-fé as Giordano Bruno was.

As a sequel to the broken out condemnation of the work of Copernicus and of the booklet of the Carmelite Foscarini who attempted to prove that the passages from the Holy Writ should not be understood literally as they appeared to be presented[8], the congregation of the Index [librorum prohibitorum] prohibited Kepler's *Epitome Astronomiae Copernicanae* [1618 – 1622] in Italy and Tuscany. That was exactly at the time when Galileo vigorously defended himself from the inquisitors. The news about the condemnation of his own book greatly perplexed Kepler and he wrote to his correspondent Ramus: "Should I conclude that, if going to Italy, I could be seized?"

Incessantly occupied with the material needs of his family, he feared that the sale of the copies of his book left at Austrian book traders will be prohibited. And he added:

Should I regard the condemnation of my book as an indirect invitation [suggestion] *to quit teaching astronomy according to principles with which I have grown old without yet encountering opponents? I will rather leave Austria than agree to improperly contracting the boundaries of philosophical freedom*.

Kepler's troubles largely depended on the cruel circumstances in whose midst he had been living as well as on the vivacity of his imagination, certainly full of strong emotion which was sometimes the source of enjoying self-love. Witness what he describes in one of his letters about the traps set by eleven girls quite in love with, and desiring to marry him. Witness his prophetic words uttered after discovering the third law named after him:

The lots are drawn; I have written my book. It will be read at present or in the future, what does it matter to me? It can wait for its reader: did not God wait six thousand years for a contemplator of his works?

What is remarkable and proves the power of the soul with which Kepler was endowed, is that he executed the greatest and the most laborious work that science owes him forever, and at the time when his personal troubles and the calamities experienced by his homeland reached their peaks.

I am now briefly analysing his main contributions.

[9] **I. Prodromus dissertationum cosmographicarum continens Mysterium … (1596)[9]** [*Ges. Werke*, Bde 1, 8]

I will explain the meaning of the title of this first great work penned by Kepler. He reported about his works intended to link all that was done by Copernicus about the planetary distances and motions by regular laws. Kepler was persuaded that these laws existed and followed Plato's idea that, while creating the world, God must have done it geometrically.

For a long time his pertinent investigations remained fruitless; nevertheless, as he remarked, they engraved in his memory the distances and times of the celestial revolutions so that he became able to compile combinations which would not appear in his mind otherwise.

First of all, Kepler searched for a law connecting the distances considered separately but did not satisfactorily succeed. Then he wished to find a simple and uniform rule for passing from the time of the revolution of one planet to the same time of some other planet. He himself says:

Concerning that problem, *I abandoned myself extraordinarily to a daring premise. I assumed that in addition to the visible planets there are two other unknown because of their smallness, one of them between Mercury and Venus, the other one between Mars and Jupiter. That, however, did not lead me to my goal. Finally, I came to understand that the planetary system is directly connected by the number of planets and their distances with the regular bodies with which ancient geometers had been occupied. There are five such bodies*.

Regular bodies, as is generally known, are […].

This is the construction allowing the radius of one orbit to lead to the radii of all the others. A sphere whose radius is equal to that of Mercury's orbit circumscribes an octahedron and a sphere circumscribing that solid has radius equal to that of the Venus' orbit. A second sphere […].

Kepler did not find words to express his pleasure of discovering not only a regular connection between the planets but also the cause of their number. The distances obtained according to the progression of the circumscribed regular bodies were not precisely those provided by Copernicus in his great contribution, but Kepler seemed to have reasonably explained the discrepancies by the uncertainty of ancient determinations. Kepler's *Prodromus* was sent to Tycho who would have replied to the author in terms of admiration had it not been followed at once, said he [Kepler, see below], by a solar eclipse foreshadowing misfortunes. These last words prove that Kepler did not yet get rid of the prejudices of his time.

We find a chapter of the *Prodromus* where Kepler once more forcefully stresses the simplicity of celestial motions in the Copernican system and their intricate complication in the Ptolemaic and Tychonic systems. We see that at that time (1596), Kepler already was a decided Copernican.

Kepler did not restrict his efforts to deducing the planetary distances from the Sun by issuing from the concept of regular bodies, he also attempted to connect these distances with the time of the planetary revolutions by a mathematical law but without success. On that occasion he formulated a question: Does the Sun possess a soul endowed with a motive power that acts more forcefully on the neighbouring planets and less forcefully on the remote planets? Does not the Sun distribute motion like light? This is seen as the first feature of Kepler's ulterior discovery only accomplished many years later.

The main character of his genius was perseverance. It was not, as he himself said, by probing all the walls amid the obscurity of ignorance that he arrived at the brilliant doors of truth. I do not have to remark that the concept which he indicated so proudly cannot be nowadays defended since beyond Saturn there exist two new planets, Uranus and Neptune [and Pluto] and since a crowd of very small mobile [?] celestial bodies was discovered between Mars and Jupiter and because in addition the distances of the six previously known planets from the Sun are now perfectly well determined and they do not accord to those resulting from the consideration of the five regular bodies.

It is in the *Prodrome* that these remarkable words addressed to the opponents of Copernicus are to be found: "After striking iron, the edge of an axe will not chop wood anymore"[10].

[10] II. Ad Vitellionem paralipomena, 1604 [*Ges. Werke*, Bd. 2]

In this contribution, amid many eccentricities and ideas imprinted with all the prejudices of that time, many traits of a genius are present. In a very brief extract below the reader will easily see for himself the true and the false parts.

Light, according to Kepler, consists of a continuous outflow of matter from a luminous body and its velocity is infinite. It traverses, he says, dense and transparent bodies with more difficulty than the empty space. Opacity of bodies is attributed to the irregular arrangement of the intervals between the material molecules. Heat is a property of light, it has nothing material.

That contribution contains an explication of a fact mentioned by much more remote authors: the image of the Sun in a camera obscura seen at a certain distance appears circular even though its rays are introduced through a triangular opening whereas during an eclipse the Sun's image is presented in the form of a crescent. I believe that Maurolycus from Sicily earlier provided a similar demonstration.

Kepler adduces many remarks concerning Vitellion's tables of refraction of light passing from air to water. He clearly understands that refraction increased greater than the angles of incidence measured from the perpendicular, but he did not discover the experimental law that the ratio of the sines of the angles of incidence and of the refraction is constant attributed by some authors to Descartes who was the first to publish it, by others to the Dutchman Snellius[11]. When applying his findings and discussing the refraction of water, Kepler proved by an ingenious but rather complicated reasoning how the refraction ought to act when luminous rays pass from empty space to our atmosphere. He thus knew that refraction only disappears at the zenith rather than at [zenith distance] 45° as Tycho had imagined.

It is remarkable that from the zenith to 70° [at zenith distances from 0 to 20°] the empirical table compiled by Kepler did not differ from the veritable refraction by more than 9". In this contribution Kepler proved, at least up to the exactitude to which observations were then susceptible and contrary to Tycho and Rothmann, that the refraction of rays from all celestial bodies situated at the same height was the same and did not depend either on their distance or brightness. He also suspected that the refraction somewhat varies with the state of the air.

From his numerical results he deduced the comparative densities of air and water and found them to be as 1 to 1178; the veritable ratio is 1:773 [1:1293 at zero temperature and mean sea level]. He therefore "surmised" that the air had a certain weight and "stirred up physicists" against him, "<u>but the contemplation of nature led me to know that our atmosphere has weight</u>". We should note that these words preceded the pertinent work of Torricelli who was only born in 1608.

Kepler remarked on Vitellion's observations that the vertical dimensions of the Sun were diminished by refraction and he concluded, [although] much more secretively and delicately, that the Sun's disc should appear elliptic. In that same contribution we find a minute scientific discussion of the observations of refraction in 1596 made by the Dutch near Novaya Zemlya.

Kepler attributed the difference, now called *irradiation*, observed between the diameters of the parts of the Moon illuminated by the Sun and of the ash-grey to the dilatation of the retina[12]. He strengthened that explanation by referring to the apparent diminution of the diameter which proved, as he stated, a principle of opacity concerning that portion of its image which is projected on the Moon.

[11] In the second part of his contribution, Kepler abandoned himself to conjectures about problems that he was unable to solve then. He had believed, for example, that the Sun had the largest density in nature which the results of the magnificent Newtonian considerations irrevocably refuted.

Kepler was happier when maintaining that the mass of the Sun surpassed the total mass of all the planets. He thought that the Sun ought to be transparent so that we saw its interior although only its surface is commonly believed to be seen. There is some truth in that conjecture which is appreciated by those acquainted with modern results concerning the physical constitution of the Sun[13].

Kepler thought it necessary to remark that the lunar edge is more luminous than its centre and Galileo is known to have been occupied later by the same problem. Kepler imagined that the Moon was essentially the same as the Earth and can be inhabited. Note that these conjectures had appeared six years before Galileo's telescopic observations.

Kepler tells us that Moestlin explained the lunar ash-grey light in his theses defended in 1596.

Kepler's observations and conjectures concerning the scintillation of the stars and planets are cited in a note which I devoted to that phenomenon and it is unnecessary to repeat them here[14].

It is the *Astronomiae pars optica* where we find Kepler's opinion about the intrinsic nature of the comets and the optical phenomena that can cause the appearance of their straight or curved tails[15].

[12] Kepler discovered the cause of the reddish light reflected by the Moon during eclipses by the rays refracted by our atmosphere. They decrease the length of the conic shadow projected by the Earth then opposing the Sun. Very little was added since then to that which was special and satisfactory in his theory.

After establishing that during total eclipses of the Sun we see a crown of light around it, Kepler says that that phenomenon can be explained either by the solar, or lunar atmosphere. In spite of all the advantages of the telescope, we have not at all advanced our understanding of the aureoles since then[16].

According to calculation somewhat uncertain because of the inexactness of the tables, Kepler says, in 1464 Saturn should have been occulted by Jupiter. Observations were not made, but subsequent events were apparently a sufficient proof for him to say that that occultation did occur. I certainly do not have to say which opinion had that illustrious author supported.

Kepler indicated the means to deduce the longitudinal difference between two places by observing solar eclipses. That procedure is more difficult but much more exact than when observing lunar eclipses. The perpetual variations in the lunar parallax make the calculation of solar eclipses extremely laborious and delicate.

Kepler was the first to think of likening solar eclipses to those of the Moon. He imagined an observer on the Sun and calculated the entry of different regions of the Earth into the conic shadow projected by the Moon then opposing the Sun. That means, properly speaking, calculation of the terrestrial eclipse, and following this ingenious concept geometers were able to provide formulas for calculating solar eclipses almost as simple as those pertaining to lunar eclipses in their proper sense.

Maurolicus thought it impossible to consider the retina as the main organ of vision because the images of external objects must be reversed as also the vision. Kepler did not allow himself to be arrested by that difficulty and explained that, in spite of that reversal, we must see those objects as they really are. The discovery of the true theory of vision is therefore due to him.

Then Kepler explained the altered vision of the short-sighted by remarking that the luminous rays issuing from the diverse parts of an object are united before reaching the retina and form there an image of a certain size so that a point is represented by a surface. He added:

It follows that those who suffer from that vision defect see delicate (*déliés*) *and very remote objects <u>double</u> or <u>treble</u>; and what concerns me, I see not one single Moon, but ten or even more*.

Concerning these multiple images he seems to have felt that it was necessary that a hiatus would be borne in his eye[s] and attribute that to the ciliary motion[17].

In the same contribution Kepler remarked that the luminous part of the lunar crescent seems to have a greater diameter than that of its ash-grey part; or, as it is said in England, the new Moon *embraces* the old one. He provided a plausible explanation of that phenomenon.

Kepler was apparently the first to study the mechanism that enables to see distinctly variously distanced objects. He thought he had discovered that mechanism in the action of ciliary motion which either elongates or shortens the eye. That theory [?] which even today retains some followers is a subject of anatomic difficulties [efforts] about which we would not be able to insist here; we only remark that, formulating this problem and indicating one of its possible solutions Kepler really proved his genius[18].

Various observations made by Delaval in the previous century prove that the light coming to our eyes from coloured bodies is not only reflected from the exterior surface of the molecules, from which those bodies perhaps should have been formed as Newton supposed, but that that light penetrates into their interior and reflects from there. Kepler, however, derived a similar consequence from his experiments made more than half a century previously.

Kepler satisfactorily explained why do the Moon and the Sun appear greater at the horizon than at a certain height above it.

[13] III. De stella nova, 1606 [*Ges. Werke*, Bd. 1]

In that contribution, Kepler showed himself as an ardent Copernican. When discussing the objections to the Tychonic system he wrote:

How philosophers fail to see that they wish to extract a straw from the eye of Copernicus but do not catch sight of the log in Ptolemy's eye?

After providing a detailed historical account of the discovery of the New star in the Ophiuchus [Serpent bearer] and of theoretical considerations of scincillation, Kepler discussed observations made at different places and proved that that star had neither proper motion, nor annual parallax[19].

In this contribution, Kepler as though largely despised astrology, but after extensively refuting the critics of Pic de la Mirandole he defended the reality of the planets' influence on the Earth when they are situated in a certain manner. It is surprising to see, in particular, that Mercury had more power for causing tempests.

Tycho thought that the New star of 1572 was formed from matter issued by the Milky Way; although the star of 1604 was near that luminous zone, Kepler did not believe that its origin should be attributed in the same way because the Milky Way had not changed since the time of Ptolemy, – but how can we know it? What is certain, Kepler says, is that the appearance of that New star denies Aristotle's ideas about the incorruptibility of heaven.

Kepler examined whether that appearance had any connection with the conjunction of planets that had occurred a bit previously in his neighbourhood. Soon, however, refusing to discover the proper physical cause explaining the formation of the New star, he exclaims:

God, who is pleased to offer mankind proofs of His incessant care, wished to arrange the appearance of that star in the place and at the *time where and when it will not escape the investigations of the astronomers*.

In German, a locution had been current: *New star, new king*. And Kepler comments: "It is surprising that some ambitious [pretender] did not attempt to profit from that popular prejudice".

We will say nothing about Kepler's arguments concerning the New star in Cygnus that appeared in his time except noting that he combined there all that most extensive erudition could have provided him for demonstrating that it was not only variable but new.

Kepler attempted to prove that the year of Jesus Christ's birth was not precisely fixed and that the beginning of our era ought to be drawn back four or perhaps five years, so that the year 1606 would have become 1610 or 1611[20].

[14] IV. Astronomia nova … tradita commentariis de motibus Stellae Martis …, 1609 [*Ges. Werke*, Bd. 3**]**

In his first investigations aimed at perfecting the *Rudolphine Tables* [see § 21] Kepler did not yet dare to withdraw completely from the system of eccentrics and epicycles explained at length in [Ptolemy's] *Almagest* and adopted by Copernicus and Tycho. He only maintained, by a reasoning borrowed from metaphysics, or, if preferred to say so, physics, that the conjunctions should be referred to the position of the real rather than the mean Sun, as it was generally done previously. But the extremely laborious calculations continued over many years did not satisfy him: there still remained 5 – 6' which he wished to get rid of. It were these small errors which definitively led to the discovery of the real system of the world.

Kepler then dared to desert entirely the old system of uniform circular motion about an ideal eccentric point devoid of any matter and motion around epicycles. He supposed that the Sun was the centre of motion occurring along the outline of ellipses of which it occupied one of the focuses. For delivering that premise from its hypothetical character he executed prodigious calculations displaying tireless perseverance and unparalleled tenacity. He thus understood that his theory represented Tycho's remarkably precise set of observations of Mars.

To achieve his aim, it was sufficient to suppose that the Sun occupied a focus of the curve and that the velocity of the planet was such that the surfaces delimited by the orbit and the radius vectors drawn to its different points were equal if the corresponding intervals of time were also equal, or that these surfaces were proportional to those times of travel. Among the numerous observations from Uraniborg, which he had at his disposal, Kepler was compelled to choose intelligently those that could have served to solve various problems connected with his general aim and to invent incessantly ever new methods of calculating them.

This is how he discovered, for example, without adopting any hypotheses that all straight lines, the intersections of the planetary orbits with the plane of the ecliptic, passed through the Sun and that all the angles between these orbits and the terrestrial orbit were approximately the same and he thus refuted the *titubation* which his predecessors had introduced for explaining the change of the latitudes[21]. The calculation executed by Kepler, as we just mentioned, were very long and mostly very tedious since logarithms were not yet invented. This is how Bailly (1779 – 1782, t. 2, p. 52) describes them:

Kepler made unbelievable efforts; logarithms were not yet invented and calculations were then not as easy as they are now. Each of his calculations occupied 10 pages in folio and he repeated them 70 times which amounts to 700 pages. Calculators know how mistakes are made, how the work has to be repeated and appreciate the time that those pages demanded. That man was surprising. His genius was not at all repulsed by those minute and difficult investigations which did not at all exhaust it.

When beginning his work, Kepler had no illusions about the enormity of the task that he imposed on himself. He described how Rheticus, the distinguished disciple of Copernicus, had wished to reform astronomy, but, being surprised by the motion of Mars, was never able to explain it. He said:

Rheticus invoked his usual talent that was apparently disappointed by being interrupted, snatched its hair, raised it to the ceiling, let it fall to the ground and told it: Such is the motion of Mars.

That vision, reported by Kepler shows the measure of the difficulties that the problem he undertook to resolve presented him from all sides.

Regarding Kepler's satisfaction that he experienced after proving that the planets move along elliptical paths and follow the law of areas, I cannot wish anything better than to refer to the discourse addressed to the memory of the miserable Ramus. That celebrated professor of Collége de France, a victim of the St. Bartholomew's Day Massacre [1572], promised to abandon his chair and all the connected privileges to anyone who will describe celestial movements independently from any hypotheses. Kepler wrote:

You have done well to quit this life because otherwise you would have been obliged to cede me your chair since the conditions that you had imposed have all been accomplished in that contribution.

[15] We find there the ideas formed by Kepler about the physical causes of celestial motion. After recalling the time when it was written, we ought to recognize the depth and rare perspicacity of the author's genius. We will only offer a few quotations mostly borrowed from a historian of science[22].

Every corporeal substance, to the extent that it is corporeal, has been so made as to be suited to rest in every place in which it is put by itself, outside the sphere of influence of a kindred body.

The only natural motion is rectilinear rather than circular, as the astronomers claim. Those two propositions taken together almost constitute the principle of inertia adopted by all modern mechanicians.

Gravity is a mutual corporeal disposition among kindred bodies to unite or join together; thus, the earth attracts a stone much more than the stone seeks the earth.

If the moon and the earth were not each held back in its own circuit by an animate force or something else equivalent to it, the earth would ascent towards the moon by one fifty-fourth part of the interval, and the moon would descend towards the earth about fifty-three parts of the interval, and there they would be joined together; provided, that is, that the substance of each is of the same density.
If the earth should cease to attract its waters to itself, all the sea water would be lifted up, and would flow onto the body of the moon.

If the moon's power of attraction extends to the earth, the earth's power of attraction will be much more likely to extend to the moon and far beyond, and accordingly, that nothing that consists to any extent whatever of terrestrial material, carried up on high, ever escapes the grasp of this mighty power of attraction.
Nothing that consists of corporeal material is absolutely light. It is only comparatively lighter, because it is less dense, either by its own nature or through an influx of heat. By 'less dense' I do not just mean that which is porous and divided into many cavities, but in general that which, while occupying a place of the same magnitude as that occupied by some heavier body, contains a lesser quantity of corporeal material.

The motive force resides in the Sun and weakens with the distance from it[23].

The motion of light things also follows from their definition. For it should not be thought that they flee all the way to the surface of the world when they are carried upwards, or that they are not attracted by the earth. Rather they are less attracted than heavy bodies and are thus displaced by heavy bodies, whereupon they come to rest and are kept in their place by the earth.

Supposing that the Sun is that motive force of planetary motion, Kepler provided it with a rotation in the same direction as that of the planets, and that actually is what really exists as verified since the discovery of sunspots. Kepler, however, adduced many circumstances which later observations proved to be inexact.

[16] V. Dioptrics, 1611 and 1653 [*Ges. Werke*, Bd. 4]

It seems that for compiling a treatise on dioptrics it is necessary to know the law according to which light is refracted when passing from a rare to a dense medium or vice versa, the law, as we said above [see § 10], Descartes had first revealed to the scientific world.

And for small angles of incidence relative to the perpendicular the angle of refraction is almost proportional to it, so that Kepler made use of that approximate rule for studying the properties of lenses, either plane-pherical or with both surfaces belonging to spheres of the same radius. The formulas still applied today for calculating the focal distances of such lenses are due to him.

We find in that contribution that he was the first to think about telescopes consisting of a combination of two convex lenses whereas Galileo had always used telescopes with a concave ocular and convex objective. To him therefore we owe that combination which today constitutes astronomical telescopes, the only ones that can be advantageously applied in graduated instruments for measuring angles[24]. Kepler had not provided the rule for determining the magnification which consists of dividing the focal distance of the objective by that of the ocular, and its discovery was left to Huygens.

When publishing his *Dioptrics*, Kepler knew that Galileo had discovered the satellites of Jupiter and considering the short period of their revolution elicited the somewhat risky conclusion that the planet itself also ought to revolve with a very short period, certainly less than 24 hours, as he stated. That conjecture was only verified much later[25].

[17] VI. Nova stereometria doliorum vinariorum, 1615 [*Ges. Werke*, Bd. 9]

This is a purely geometric work in which Kepler examines in particular bodies produced by an ellipse rotating about its diverse axis. He also explains a procedure for measuring the volume of barrels.

[18] VII. Harmonices mundi, 1619 [*Ges. Werke*, Bd. 6]

That is the title of the contribution in which Kepler reported the discovery of his third law: the squares of the periods of revolution of two planets are as the cubes of their distances.

It was on 18 March 1618 that he came to think about comparing the [squares of the?] periods of revolution with the cubes of distances; however, because of an error in calculations he found that that law was not verified. On 15 May he repeated his calculations and the results conformed to the facts.

Kepler mentioned that at a certain moment he thought that a new error led him to an illusion, but, he added, after all possible verification it followed that the law represented the Tychonic observations so finely, that the discovery became certain. That discovery was regrettably accompanied by weird and, moreover, completely inadmissible ideas. The ratio that he found between motions and distances returned his mind back to the Pythagorean concepts of harmony. In the music of the celestial bodies, he said, Saturn and Jupiter were bass; Mars was the tenor; the Earth and Venus were altos; and Mercury was falsetto.

Another point equally spoiling that immortal work was the author's trust in astrological dreams. We see there, for example, that the air is always disturbed when the planets are in conjunction; that it rains when they are exactly at 60°, etc.

[19] VIII. De cometis, 1619 [*Ges. Werke*, Bd. 8**]**

While reading the three chapters comprising this work, it is astonishing to see that Kepler, the author of the laws of elliptical motion of planets about the Sun, insisted that the comets moved along straight lines. The observation of the paths of these celestial bodies, he said, did not merit much attention because they do not return. He made that remark, so unworthy of his genius, on the occasion of the comet of 1607, which had already returned three times (and twice afterwards). Nevertheless, he deduced from his mistaken system valuable consequences about the immense distance of that comet.

In the second part of that work entitled *Physiology of comets*, we find a passage unbelievably penned by such a great man:

Water, and especially salt water, engendered fish and the ether provided comets. The Creator did not wish to leave the immense dimension of the seas without inhabitants, and the same happened with celestial spaces.

The number of comets should be very considerable; if we see so little, it is because they do not come near the Earth and they easily dissipate.

In addition to this dreaming, the fruit of imagination unstoppable in its vagrant course, we find eligible and most scientific ideas as for example that the solar rays, when traversing a comet, incessantly move back its particles and thus form its tail[26].

Seneca, as testified by Ephorus, mentioned that a comet had separated itself in two parts, each following its own route. The Roman philosopher thought that that observation was deceptive[27]. Kepler treated that story extremely rigorously; we admit that all astronomers shared Seneca's opinion, but [and] this is what modern astronomers armed by telescopes were able to see: a separation of a single comet in two distinct celestial bodies going along differing routes. An assumption [?] of a genius should never be completely neglected.

The contribution that we are now discussing, although [?] appearing in 1619, included, especially in its last chapter, an imprint of astrological opinions of that time about the influence of comets from afar on the events in the sublunar world. I say *from afar* because Kepler thought that the plague can be brought about by a comet whose tail enveloped the earth. He expressed an idea whose falsity he was unable to prove having been ignorant about the essence of the comets' matter.

[20] IX. Epitome astronomiae Copernicanae, 1618, 1621, 1622 [*Ges. Werke,* Bd. 7**]**

That contribution consists of two volumes published in Linz. We will cite in a shortened form the material containing the views and scientific astronomical discoveries.

The Sun is a fixed star; it only seems greater than other celestial bodies because its distance is much less. It is known (by observation of sunspots) that it rotates about itself, and all the planets ought to rotate as well. Comets consist of matter susceptible of dilatation and condensation and of being transported far off by the action of solar rays. The radius of the starry sphere is at least two thousand times greater than the distance of Saturn[28].

Sunspots are either clouds or dense vapour rising from the Sun's entrails and consumed at its surface. The Sun rotates, and its attractive faculty is appropriately directed to different regions of the sky just as when turning a magnet around. When the Sun picks up a planet for attracting or repulsing it, the planet is forced to turn as well. The centre of the Sun is the centre of planetary motions; Copernicus is known to have placed the latter beyond the Sun.

Kepler attributed the light with which the Moon is encircled during total eclipses of the Sun to the Sun's atmosphere. He says that that atmosphere is visible some time after sunset and we believe that here

Kepler was the first to discover the zodiacal light[29]. He did not, however, say anything about its elongated form, and it is not therefore possible to attribute to him the observation that Childrey and Dominique Cassini seem to be unjustly deprived of.

[21] X. Tabulae Rudolphinae, 1627 [*Ges. Werke,* Bd. 10**]**

It was Tycho who began their compilation and Kepler completed them after 26 years of work. They are named after emperor Rodolphe who had been the protector of them both. In this contribution, we find the history of the discovery of logarithms; its examination without national prejudices detracts nothing at all from Neper's merit. He was incontestably their first inventor because priority rests upon published documents.

The explications adjoined to the *Tables* include first indications of the method of longitudes based on observing the distances between the Moon and stars.

The *Prutenic Tables* dedicated to Albert of Brandenburg, Duke of Prussia, were published in 1551 by Reinhold. They were founded on observations made by Copernicus and Ptolemy, and after Kepler published the *Tabulae Rudolphinae* based on Tychonic observations and his, Kepler's new theories, errors of many degrees were discovered in those tables.

[22] XI. Somnium, 1634 [*Ges. Werke*, Bd. 11/2**]**

That posthumous work published by Kepler's son [Ludwig] contains a description of astronomical phenomena as seen by an observer on the Moon. Some authors of later elementary treatises attempted to discuss celestial phenomena for observers on different planets thus providing a very useful exercise for beginners, but it is proper to say that the first idea about those imaginary displacements belongs to Kepler.

[23] Following are the other writings of Kepler; we provide a complete list of his other works, witnesses of the laborious life and indomitable perseverance of that illustrious astronomer[30].

Ephemerides, 1616 [*Ges. Werke,* Bd. 11/1**]**

Kepler's ephemerides had been appearing for the following years until 1628, but they were only published after the appropriate time. Kepler's son-in law, Bartschius, continued that work.

[24] Under the auspices of emperor Charles VI Hausch (1718) published a volume containing a part of Kepler's manuscripts. He was unable to get the necessary finances for presenting the promised second volume. In 1775, the Petersburg Academy acquired the 28 remaining notebooks of Kepler's unpublished manuscripts.

Notes

1. *Systematic* possibly meant *biased*. O. S.

2. I owe the description of Breitschwert's work to Humboldt's friendship, and I express him my greatest possible gratitude. F. A.

3. Why only restorer? O. S.

4. The only child who survived Kepler was [Ludwig] a doctor of medicine in Königsberg. He published his father's posthumous work entitled *Kepler's Dream about Lunar Astronomy* [*Somnium*]. F. A.

5. Her husband (Kepler's father) disappeared, see § 2, so that the "reproach" was meaningless. O. S.

6. The distance between these towns is hardly less than 300 *km*. O. S.

7. The last statement is certainly mistaken, as proved, for example, by Kepler's parents. O. S.

8. Kepler himself (1609, Introduction) stated the same. O. S.

9. Arago failed to mention the second, somewhat enlarged edition of that work. O. S.

10. An explanation is lacking here. O. S.

11. The ratio mentioned is determined by the appropriate velocities of light and the further history of that law is important. O. S.

12. That astronomical phenomenon is only an example of irradiation. O. S.

13. Visible is only the outer layers of the Sun, i. e. its atmosphere. O. S.

14. See Arago (1840). He did not mention his later and perhaps comprehensive contribution (1852), and these years, 1840 and 1852, therefore restrict the date of publication of Kepler's biography. O. S.

15. Kepler later returned to the explanation of the appearance of those tails, see § 19. O. S.

16. It is now possible to see the Sun's crown independently from the eclipses. O. S.

17. See also below. O. S.

18. Double vision also occurs when the eyes are misaligned. Sometimes, however, the brain can ignore one of the two images. O. S.

19. That statement only remained valid until much more precise observations became possible. O. S.

20. Kepler (1613) devoted a special publication to that subject. His estimate is at least approximately correct. O. S.

21. Polar motion, as it is now called, is indeed causing the change of terrestrial latitudes; Arago, however, discusses galactic latitudes. O. S.

22. Arago did not name that historian; anyway, all the following quotations in this section are from Kepler himself (1609/1992, pp. 55 – 58). O. S.

23. This is a paraphrase of Kepler's statement (1609/1992, pp. 377 – 378). O. S.

24. Euler is known to have proposed an objective consisting of two combined glasses and thus made a great contribution to astronomy. O. S.

25. That period is about 10.5 hours. O. S.

26. This explanation is still valid. O. S.

27. Ephorus lived several centuries *before* Seneca (and was a Greek rather than a Roman historian). O. S.

28. That means that the radius of the starry heaven was "at least" approximately 1/3 of a light year, an absolutely useless estimate. O. S.

29. The zodiacal light is the reflected light of the Sun. O. S.

30. I append Arago's list of other works of Kepler, certainly not comprehensive but likely indicating those more generally known at the time and I have corrected some titles according to the bibliography of Kepler's works. One work is included in the main text because

Arago supplied it with a commentary. All that is preceded by some translations of his writings which Arago commented on in his previous sections.

Some Personalities Mentioned by Kepler and Arago

Bartsch, Jakob, 1600? – 1633

Brentius, Johann, 1499 – 1590

Charles VI, Holy Roman emperor, 1685 – 1740

Childrey, Joshua, clergyman, Copernican, astrologer, 1623 – 1670

Dalberg, Karl Theodor, Archbishop; bishop of Regensburg not before 1805, 1744 – 1817

Foscanini, Antonio Paolo, a Carmelite, 1565 – 1616

Johann Frederick, Grand Duke of Württemberg, 1582 – 1628

Lottin de Laval, Victor

Maestlin (Möstlin), Michael, astronomer, mathematician, 1550 – 1631

Maurolicus, Franciscus, mathematician, 1494 – 1575

Mirandola, Giovanni Pico della, Count, Renaissance philosopher, 1463 – 1494

Ramus, Petrus, philosopher, 1515 – 1572

Reinhold, Albert of Braunschweig, Margrave

Reinhold, Erasmus, mathematician and astronomer, 1511 – 1553

Rheticus, Georg Joachim von Lauchen, mathematician, astronomer, theologian, physician, sole disciple of Copernicus, 1514 – 1574

Rudolph II, Roman – German emperor, 1552 – 1612.

Seneca, Roman Stoic philosopher, ca. 4 – 65

Sigismund von Luxemburg, Roman – German king, emperor of Holy Roman Empire (1433 – 1437), 1368 – 1437

Snell, Willebrord van Roijen, astronomer, mathematician, 1580 – 1626

Some place Names Mentioned by Arago

Leonberg, in Baden-Württemberg

Mayence, also Mainz

Ratisbonne = Regensburg

Sagan, seat of Silesian Dukes, now in Poland

Styria, in southeast Austria

Weil = Weil der Stadt

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J. Kepler

I adduce the list of Kepler's writings mentioned in Note 30. The Roman numerals, the same as in my main text, connect the translations with the original editions.

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II. *Optics*. Santa Fe, 2000.

III. *Über den Neuen Stern im Fuß des Schlangenträger*. Würzburg, 2006.

IV. *New Astronomy*. Cambridge, 1992.

VII. *Harmony of the World*. Philadelphia, 1997.

IX. *Epitome of Copernican Astronomy*, Books 4 and 5. In *Great Books of the Western World*, vol. 16, pp. 845 – 1004. Chicago, 1952.
X. Only the Preface: *Q. J. Roy. Astron. Soc.*, vol. 13, 1972, pp. 360 – 373.
XI. *Somnium*, in English. Madison – London, 1967.
Nova dissertatiuncula de fundamentis astrologiae …, 1602. *Ges. Werke*, Bd. 4. On the more certain foundation of astrology. *Proc. Amer. Phil. Soc.*, vol. 123, 1979, pp. 85 – 116.
De solis deliquio Epistola, 1605. *Ges. Werke*, Bd. 4.
De Jesu Christi Servatoris nostri vero anno natalitio. Heading: Sylva chronologica, 1606.*Ges. Werke*, Bd. 4.
See also De anno natali Christi; Bericht vom Geburtsjahr Christi, 1613. *Ges. Werke*, Bd. 5.
Bericht von dem im Jahre 1607 erschienenen Kometen, 1608. *Ges. Werke*, Bd. 4.
Phoenomenon singulare, seu Mercurius in Sole…, 1609. *Ges. Werke*, Bd. 4.
Dissertatio cum Nuncio sidereo… 1610. *Ges. Werke*, Bd. 4.
Narratio de observatis à se quatuor Jovis satellitibus erronibus …, 1610. *Ges. Werke*, Bd. 4.
Strena, seu de nive sexangulà, 1611. *Ges. Werke*, Bd. 4.
Eclogae chronicae …, 1615. *Ges. Werke*, Bd. 5.
Prognosticon, von aller … künfftigen Übelstands auf das 1618. und 1619. Jahre, 1619. German text: 1621. *Ges. Werke*, Bd. 11/2.
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Discurs von der grossen Conjunction … Saturni und Jovis…, 1623. *Ges. Werke*, Bd. 11/2.
Chilias logarithmorum, 1624. *Ges. Werke*, Bd. 9. Supplementum: 1625. *Ges. Werke*, Bd. 9.
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Admonitio ad astronomos…, 1629. *Ges. Werke*, Bd. 11/1.
Ad epistolam Jac. Bartschii responsio …[Joannes Keplerus Jacobo Bartschio], 1629. *Ges. Werke*, Bd. 11/1
Sportula genethliacis missa de Tabulam Rudolphi … *Ges. Werke*, Bd. 10.
The following item is not connected with Arago's text:
A thorough description of an extraordinary New star … 1604. *Vistas in Astronomy*, vol. 20, 1977, pp. 333 – 339.

Other Authors

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Tude

D. F. Arago

Laplace

Oeuvres, Notices biographiques, t. 3. Paris – Leipzig, 1855, pp. 456 – 515
Date of initial publication not indicated

[1] The Reporter of a Commission of the Chamber of Deputies charged in 1842 with examining a proposal made by the Minister of Public Education to publish at the state's expense Laplace's works, as I believed, traced a brief analysis of the main discoveries of our illustrious compatriot. Many people expressed the opinion, perhaps quite benevolently, that such an analysis did not rest hidden in the multitude of legislative documents but was published in the *Annuaire du Bureau des Longitudes*. I took occasion to develop it so as to be less unworthy for presenting the matter to the public. I present the entire scientific part of the writing forwarded to the Chamber of Deputies, the rest, as it seems, can be suppressed[1]. I will only retain a few lines of that report describing the aim of the proposed law (loi) and make known the dispositions adopted by the three branches of the state[2].

Laplace donated to France, to Europe and the scientific world three magnificent compositions: the Traité de Mécanique Céleste, the Exposition du Système du Monde and the Théorie analytique des probabilités. Today, in 1842, Paris libraries do not have the Théorie, and the copies of the Traité will soon be exhausted. We sorrowfully see that the time will come when, in the absence of the original edition, those devoted to studying transcendent mathematics will be compelled to order from Philadelphia, New York and Boston its English translation of that excellent contribution of our fellow countryman made by the skilful geometer Bowditch.

Let us hasten to say that these worries are unfounded. For the family of the illustrious geometer, a reprint of the Mécanique Céleste would have been an accomplishment of a sacred duty. Thus, Mrs Laplace, so legitimately, so deeply attentive to everything that can enhance the lustre of the name she bears, was not at all disturbed by financial considerations: a small estate near Pont-l'Evèque changed hands and scientific France will not be deprived of the satisfaction when enumerating astronomical wealth written in the national tongue.

The forthcoming reprint of Laplace's oeuvres is not less assured. Yielding both to filial feelings and moved by noble patriotism and enlightened enthusiasm that the most serious studies inspired by brilliant discoveries [made by his father], *General Laplace had been for a long time prepared to become the editor of the seven volumes which should immortalize the name of his father.*

It is most loftily glorious and really splendid to have that task remaining in the realm of private affairs. It remains for the government to preserve them from indifference, or from being forgotten; to offer the Oeuvres incessantly to the public, to disseminate them by all possible means so that they will finally foster common weal.

The Minister of Public Education undoubtedly knew all about these ideas when, on the occasion of the new necessary edition, he is asking to substitute [financially] *the great French nation for the family of the illustrious geometer. We support plainly and entirely that proposal; it issues from a national feeling and will not be opposed by anyone of us*.

[2] Actually, the Chamber of Deputies only examined and solved this sole question: *Are Laplace's works of such lofty and exceptional merit for their reprint to become an object of deliberation by the great powers of the state*? It was thought necessary not only to refer to the general reputation but to analyse carefully Laplace's brilliant discoveries for better to show the importance of the decision to be reached. Who could have later proposed a similar measure of pronouncing a decision before voting so honourably with respect to the memory of a great man, before minutely probing, measuring, appraising from all sides such monuments as the *Mécanique* and the *Exposition*?

I think that the task fulfilled on behalf of a Commission of one of the great branches of the state can worthily end this [Arago's] series of biographies of the main astronomers.

Marquis de Laplace, Peer of France, one of the forty members of the *Académie Français*, member of the Paris *Académie des Sciences* and the *Bureau des Longitudes*, corresponding member of all the great European academies and scientific societies, was born 28 March 1749 in Beaumont-en-Ange into a family of a simple peasant; he died 5 March 1827. The first two volumes of the *Méc. Cel*. were published in 1799, the next volumes in 1802, 1805, 1823 (vol. 5, books 11 and 12), 1826 (same volume, books 13 – 15) and 1825 (book 16). The *Théorie des probabilités* appeared in 1812. We will present the main astronomical discoveries contained in those immortal writings.

[3] Astronomy is a science in which the human mind can be most justifiably glorified. It owes that uncontested pre-eminence to its elevated aim, the greatness of its means of investigation, to the certitude, utility and unparalleled magnificence of its results. From the origin of societies the study of the course of celestial bodies has been incessantly attracting the attention of governments and people. Astronomy delighted many great leaders, – illustrious statesmen, writers, philosophers, eminent Greek and Roman orators. Nevertheless, if we are allowed to say so, astronomy really worthy of its name is quite a modern science and only dates from the 16th century. Three great and brilliant phases mark its progress.

In 1543, Copernicus firmly and boldly shattered the main part of the antique and venerable illusions of senses and pride with which many generations had filled the universe. The Earth ceased to be the centre, the pivot of all celestial motions and was modestly ranged among the planets. Its material importance among the set of bodies comprising our solar system was reduced almost to that of a grain of sand.

[4] Twenty eight years have passed since Copernicus had died holding in his weakening hands the first copy of the contribution that spread over Poland such blazing and pure glory. Then Wittenberg [Württemberg] witnessed the birth of a man destined to achieve a scientific revolution not less fruitful and even more difficult. That man was Kepler.

Endowed with two apparently excluding each other qualities, with a volcanic imagination and persistence which the most fastidious numerical calculations were unable to repulse, he surmised that the motions of heavenly bodies must be connected by simple, or in his own words, *harmonious* laws. And he undertook to discover them. A thousand fruitless attempts and numerical mistakes inseparable from such colossal work did not hinder him even for a moment from marching resolutely to his goal. Neglecting grief, Kepler devoted 22 years to that investigation, but what do 22 years of work really mean for that person who will become the legislator of the worlds; who will inscribe his name on ineffaceable treatises, on the frontispiece of an immortal code; and who could have exclaimed in the language of dithyrambs that not one dared attempt to edit:

The lots are drawn; I have written my book. It will be read at present or in the future, what does it matter to me? It can wait for its reader: did not God wait six thousand years for a contemplator of his works?[3]

Establish the physical cause capable of compelling the planets to move along closed curves; include in these forces the principle of conservation of the world rather than solid supports, the crystal spheres imagined by our ancestors; and extend the general principles of terrestrial body mechanics to the celestial bodies, – those were problems that still had to be solved after Kepler had published his discoveries.

[5] Clear outlines of these great problems had been visible here and there in the works of ancient and modern authors, from Lucretius and Plutarch to Kepler, Boulliaud and Borelli, but the merit of their solution ought to be attributed to Newton. That great man, following the example of many of his predecessors, provided celestial bodies with a tendency of rapprochement, of attraction, showed that the Keplerian laws were a mathematical manifestation of that force which extended to all the material molecules of the solar system and developed his brilliant discovery in a contribution that even today remains the most eminent production of human intelligence.

The heart is wrung when studying the history of sciences and seeing how such a magnificent intellectual movement took place without France's participation. And practical astronomy augmented our inferiority[4]. At first, the means of research were imprudently given over (furent donnés) to foreigners to the detriment of national completeness of knowledge and zeal.

Later, superior minds courageously but vainly struggled against the inability of our masters whereas at that time Bradley, being happier than those across the Channel, immortalized himself by discovering aberration and nutation. In 1740, among the admirable revolutions occurring in the astronomical science France's participation consisted in experimentally determining the flattening of the Earth and discovering the variations of gravity over the surface of our planet [Richer (1679)]. Those were two great subjects, but nevertheless our country could have rightly wished more: when France is not in the first rank, it loses her place. That rank, lost for a short time, was brilliantly returned owing to four geometers.

When Newton, providing his great discovery with a generality that the Keplerian laws did not possess, had imagined that not only the Sun attracted the planets, but that they also attracted one another, he thus inserted causes among celestial bodies that will inevitably corrupt every motion. Astronomers then became able to see at once that in any region of the sky, whether near or remote, the curves were unable to represent precisely the occurring phenomena according to the Keplerian laws; that the simple regular motions, which the imagination of the ancients had been pleased to provide to the celestial bodies, undergo perpetual numerous and considerable perturbations. To foresee many of these and assign them their directions and very seldom their numerical values, – that was the goal which Newton proposed to solve when compiling his *Mathematical Principles*.

In spite of his incomparable sagacity, that book only provided a sketch of planetary perturbations. And if that sublime outline did not become a comprehensive picture, it certainly cannot be imputed to lack of ardour or persistence: the efforts of that great philosopher had always been superhuman and problems which he did not at all solve were then unsolvable.

[6] When continental mathematicians had entered their careers, and attempted to base the Newtonian system on an unshakable basis and to perfect theoretically astronomical tables, they really discovered the difficulties that had repulsed Newton's genius. Five geometers, – Clairaut, Euler, D'Alembert, Lagrange and Laplace, – shared the world whose existence Newton had revealed. They explored it in every sense; penetrated regions thought to be inaccessible; reported countless phenomena not yet caught by observation; finally, and that was their imperishable glory, they attached all the most subtle and most mysterious celestial motions to a sole principle, to a unique law. Geometry thus proved to be daring to deal successfully with the future; the unfolding centuries scrupulously ratify the decision of science.

We will not discuss the magnificent work of Euler; on the contrary, we will concentrate on briefly analysing of the discoveries of his four French rivals[6].

If a celestial body, the Moon for example, is only attracted to the centre of the Earth, it will move precisely along an ellipse and strictly obey the Keplerian laws. Or, what is the same, obey the mechanical principles developed by Newton in the first chapters of his immortal writing. Activate now a second force, take into account the attraction of the Moon by the Sun, and we are now considering three bodies rather than two, and the Keplerian ellipse will only provide a rough idea about the motion of our satellite.

Here, solar attraction tends to augment, and actually augments the dimensions of the initial orbit, – there, on the contrary, it diminishes that orbit. At certain points the solar force acts in the same direction as the lunar motion, and increases its speed, but elsewhere the effect is opposite. In a word, the introduction of a third attracting body leads to greatest complications and all the appearances of disorder replace the simple regular march so obligingly restful for the mind.

Newton provided a complete solution of the problem of celestial motions in the case of two mutually attracting bodies, but he did not even touch analytically the infinitely more difficult problem of three bodies which became famous under that name. It determines the motion of a celestial body attracted by two others and was resolved for the first time by our fellow countryman Clairaut[7]. From that moment onward dates the important progress already achieved in the previous century towards perfecting the lunar tables.

[7] The most splendid astronomical discovery of antiquity is that of the precession of the equinoxes and its honour belongs to Hipparchus. He perfectly clearly reported all the consequences of that phenomenon two of which are most particularly privileged to attract the public's attention. It follows [first] that not always the same groups of stars, not the same constellations are seen in the sky during the same seasons. As the centuries go by, winter constellations become visible in summer and vice versa. And the pole does not constantly occupy the same place on the firmament. The rather brilliant star now quite justifiably called Polar, was rather remote […] and will again be […].

Concerning the troubles of explaining natural phenomena and following a wrong route, each precise observation presents the theoretician new complications[8]. As soon as Hipparchus discovered precession the seven crystal spheres enveloping the Earth became no longer sufficient for representing the phenomena. An eighth sphere was necessary to allow for a motion in which all the stars taken together were participating.

After depriving the Earth from its claimed immobility, Copernicus, on the contrary, very simply complied with the precession taking care of its minutest circumstances. He supposed that the Earth's axis of rotation did not remain exactly parallel to itself, and that after each complete revolution of our planet about the Sun it deviates by a small magnitude. In other words, instead of compelling the multitude of circumpolar stars to move with respect to the pole, he made the pole move with respect to the stars[9]. That hypothesis saved the mechanism of the world from its greatest complication introduced by the spirit of the system. A new Alphonse would have then be robbed of a cause for addressing his astronomical synod with his profound and so poorly interpreted words which history attributed to that king of Castile[10]; see the Note that I devoted to Alphonse X on p. 170 [of his, Arago's, present French collection].

If, as it is easy to see, Copernicus' concept ameliorated by Kepler had notably perfected the mechanism of the firmament, it still remained necessary to discover the motive force that, modifying each year the position of the axis of the world, compels it to describe an entire circle of almost 50° in diameter in about 26 thousand years.

Newton surmised that that force originated from the action of the Sun and the Moon on the matter that was elevated in the equatorial regions above the sphere whose centre coincided with that of the Earth and radius connecting that centre and one of its poles. He thus made the precession of the equinoxes depend on the flattening of the globe and declared that no precession would have existed for a spherical planet.

All that is correct but Newton did not establish it mathematically. That great man had [indirectly] introduced this harsh and just rule into philosophy: Do not consider anything unproven certain.

The demonstration of the Newtonian ideas about the precession was therefore a great discovery and it is D'Alembert to whom belongs its glory[11]. That illustrious geometer completely explained the general motion owing to which the axis of the terrestrial globe returns to the same stars after about 26 thousand years. He thus also connected the perturbation of precession discovered by Bradley with attraction.

The remarkable oscillation that the Earth's axis incessantly experiences during its progressive motion has therefore the period (about 18 years) exactly equal to the time for the intersection of the lunar orbit and the ecliptic to travel the 360° of an entire circumference.

[8] Geometers and astronomers have been quite understandably occupied with the form and physical constitution which the terrestrial globe had in remotest times as much as with the present state of those form and constitution. As soon as our compatriot Richer [1679] discovered that the same body of whatever nature weighs the less the nearer it is to the equinoctial [i. e., the equatorial] regions, it was generally noticed that the Earth, had it been initially fluid, should have become swollen there. In addition, Huygens and Newton calculated the difference between the greater and the lesser axis; that is, the excess of the equatorial diameter above the length of the line connecting the poles[12].

The calculation made by Huygens was based on hypothetical and entirely inadmissible properties of the attractive force whereas Newton founded it on a theorem that was necessary to prove. His theory had an even graver defect: he considered that the Earth was primitive, fluid and entirely homogeneous[13]. When desiring to resolve great problems, all such simplifications are abandoned; and, when so essentially distancing yourself from the natural physical conditions in order to elude difficulties of calculation, your results conform to an ideal world and are actually nothing but witty trifles.

For usefully applying mathematical analysis to determining the figure of the Earth, all hypotheses of homogeneity, all forced similarities of the forms of the superimposing layers with differing densities should be banned, and the case of a central solid kernel ought to be also examined. Such generality increases the difficulties tenfold, which did not arrest either Clairaut or D'Alembert. Owing to the efforts of these two mighty geometers and to some essential developments due to their immediate followers and the illustrious Legendre in particular, the theoretical determination of the figure of the Earth acquired all the desirable perfection. Nowadays, an excellent accordance reigns between the results of calculation and direct measurements. Initially, the Earth was therefore (donc) fluid; analysis leads us back to the first ages of our planet[14].

[9] Most Greek philosophers at the time of Alexandr the Great thought that comets were simple meteors engendered in our atmosphere. In the Middle Ages, without deeply inquiring about their nature, comets were seen as harbingers of sinister events. Regiomontanus and Tycho Brahe placed them above the Moon; Hevelius, Doerfel and others stated that the comets rotated about the

Sun and Newton established that they moved under the immediate influence of the attractive force of that celestial body rather than along straight lines and obeyed the Keplerian laws.

It remained to prove [by observation] that their orbits were indeed closed curves or that the Earth sees the same comet many times. That discovery was reserved for Halley; minutely collecting the circumstances of the appearances of all [at least] somewhat brilliant comets from the stories of historians and chronologists and astronomical annals, that ingenious scholar made it clear by a subtle and deep discussion that the comets of 1682, 1607 and 1531 were really successive appearances of one and the same body.

That identification inspired a consequence renounced by more than one astronomer: it became necessary to agree that the period of an entire revolution of the comet considerably varies up to 2 years in 76. Can such a great difference be a perturbation occasioned by the action of planets? The answer to that question should have compelled either to place the comets in the category of ordinary planets or to consider them forever removed from them.

Calculation was difficult but Clairaut discovered a method of accomplishing it. His success could have seemed uncertain: during 1758 he most courageously undertook to determine the epoch of the next appearance of the comet of 1682 and indicated the constellations and the stars it will meet [optically] on its route.

That was not of the sort of a long-term prediction which astrologers and other soothsayers previously so craftily concocted by making use of mortality tables so as never to be exposed as liars during their lifetime[15]. The event [foreseen by Clairaut] did occur which amounted to nothing less than to creating a new era for the cometary astronomy rather than letting it fall for a long time into disgrace.

Clairaut found by skilful and very long calculation that the action of Jupiter and Saturn should have retarded the comet's march and that its entire revolution, as compared with the previous occurrence, will be increased by 518 days by the attraction of Jupiter and 100 days, by Saturn; the total, 618 days, was more than a year and eight months.

Never did an astronomic question excite more vivid and legitimate curiosity. All classes of the society had been waiting for the announced reappearance with the same interest. A Saxon peasant, Palitszch, was the first to notice it. From that moment onward, thousand telescopes throughout Europe each night marked the points along the route of that comet traversing the constellations. And that route invariably, to the limits of the precision of calculation, coincided with what Clairaut had assigned it beforehand. The prediction of that illustrious geometer came at once true in terms of time and place.

Astronomy achieved a great and important victory at the same time destroyed, as it usually happens, a shameful and deep-rooted prejudice. Ever since it was established that reappearances of comets could be predicted and calculated, these celestial bodies definitively lost their ancient prestige. The most timid minds were now troubled by them no more than by equally calculable solar and lunar eclipses. Clairaut's works finally became even more popular than Bayle's ingenious and witty scientific reasoning.

[10] To the thoughtful mind the firmament does not offer anything more curious and strange than the coincidence of the mean angular [of the periods of] revolution and rotation of our satellite. Owing to it, the Moon always turns the same side to the Earth. Today, its visible hemisphere is precisely the same as had been seen by our remotest ancestors and it will be exactly the same for our posterity.

In this particular case, final causes which certain philosophers so unreservedly introduce to account for a great number of natural phenomena, could not be applied. How, indeed, to state [to explain] that men can be somewhat interested in invariably seeing the same lunar hemisphere without ever glimpsing the other one?[16] On the other hand, mathematics, when lacking a necessary connection between its [?] elements such as the perfect identity of translational and rotary motions of a given celestial body, is not in the least offended by ideas of probability[17]. However, there exist other no less extraordinary numerical coincidences: the identical orientation relative to the stars of the lunar equator and orbit; the exactly identical precessional motion of those two planes[18]. That set of singular phenomena discovered by G.-D. Cassini constitutes the mathematical code of what is called the *lunar libration*[19].

Libration still remained a vast and very unpleasant lacuna of physical astronomy at the time when Lagrange discovered its dependence on the figure of our satellite unobservable from the Earth and firmly connected it with the principle of universal gravitation. At the epoch when the Moon had been solidifying, owing to the terrestrial action it took a less regular form than it would have assumed in the absence of any alien attracting body in its neighbourhood. The lunar equator therefore became elliptic rather than circular which did not, however, hinder it from remaining everywhere swollen.

The pre-eminence of the equatorial diameter [the larger diameter] is directed towards the Earth and became four times larger than the other diameter. For an observer in space able to see it transversely the Moon is a body elongated towards the Earth as an unsuspended pendulum of sorts[20]. When a pendulum is moved away from the vertical, the action of gravity returns it; and similarly when the greater axis of the Moon leaves its usual direction, the Earth forces it back.

Thus is the strange phenomenon completely explained without resorting to equality [to identity of parameters] of miraculous sorts between two entirely independent motions, rotation and translation. We only observe one side of the Moon, and nowadays we also know that that is due to a calculable physical cause only seen by the mind's eye, to the elongation of one of the lunar diameters experienced by the Moon when it was passing from a liquid to a solid state under the attractive force of the Earth.

If there existed, from its origin, a small difference between the rotary motion and the revolution of the Moon, the attraction of the Earth would have caused them [their periods] to become rigorously equal. And that attraction was also sufficient for the disappearance of the slight lack of coincidence between the intersections of the equator and the lunar orbit with the plane of the ecliptic.

Lagrange's work, so capital in essence and no less remarkable in form, quite fortunately connected the laws of libration with the principles of universal gravitation. After reading it, everyone understood that the word *elegance* is applicable to mathematical memoirs.

[11] In our analysis, we were content to touch on the astronomical discoveries made by Clairaut, d'Alembert and Lagrange, but we will be a lit less concise when discussing the works of Laplace. After enumerating the forces, so numerous, that result from the mutual action of planets and satellites of our solar system, Newton, the great Newton, did not dare perceive the totality of their effects. In the midst of the maze of augmentations and diminutions of velocities, variations in the orbital forms, changes of distances and inclinations evidently resulting from the action of those forces, even the most scientific geometry [geometer] will be unable to find a solid and trustworthy common thread[21].

That extreme complication engendered a discouraging thought. So numerous and so variably positioned forces, so differing in intensities, seemed to be only able perpetually to maintain equilibrium by a miracle of sorts. Newton went as far as to suppose that the planetary system did not include in itself any elements ensuring its indefinite conservation. He believed that from time to time a mighty hand ought to intervene and repair the disorder. Euler, although advancing further in the knowledge of planetary perturbations, did not admit either that the solar system was constituted to last forever.

Never was a greater philosophical problem offered to the curiosity of man. Laplace tackled it boldly, persistently and fortunately. The deep investigations by that illustrious geometer continued for a long time established quite certainly that the planetary ellipses were perpetually variable; that the extremities of their greater diameters passed around the sky; that independently from an oscillatory motion the orbital planes experienced displacements owing to which their traces [their lines of intersection] with the plane of the terrestrial orbit were each year directed towards different stars. In the midst of that chaos there existed a magnitude remaining invariable or only subjected to small periodic changes: the greater axis of each orbit and therefore the period of revolution of each planet. That is the magnitude which according to the scientific prejudices of Newton and Euler should have been mostly variable.

[12] Universal gravitation suffices for the conservation of the solar system; it maintains the orbital forms and inclinations in a mean state about which the variations are slight and do not lead to disorder. The world offers harmony and perfection which Newton himself doubted. That occurs because of circumstances that calculation revealed to Laplace; being vaguely noted, they did not seem to exert such a great influence.

All the planets move in the same direction and the planes of their slightly elliptical orbits are little inclined to each other. However, substitute different conditions, and the stability of the world will become once more questionable, the result will likely be a horrible chaos[22].

Since the appearance of the contribution mentioned just below, the invariability of the greater axes of planetary orbits had been demonstrated still better, i. e. by further extending analytical approximations[23], but that fact still no less remains an admirable discovery made by the author of *Mécanique Céleste*. Citing the appropriate dates concerning such subjects is not a luxury of erudition. The memoir in which Laplace [initially] provided the results about the invariability of the mean motions, and therefore of the greater axes, was published in 1773, but only in 1784[24] he proved (déduisit) the stability from the other elements of the system, – of the small masses of planets, slight eccentricities of the orbits, and the identical directions of their circulation around the Sun.

[13] That discovery did not anymore allow, at least as it concerned our solar system, to consider the Newtonian attraction as a source of disorder; however, was it impossible that other forces had been admixing and producing gradually increasing perturbations feared by Newton and Euler? Positive facts seemed to justify those feelings. Ancient observations compared with modern findings reveal a continuous acceleration in the motions of the Moon and Jupiter and a not lesser diminution in the motion of Saturn. These variations lead to strangest consequences.

According to the presumed causes of these perturbations, to say that the velocity of a celestial body increases from one century to another means to declare in equivalent terms that it is approaching the centre of motion and vice versa. Thus, a singular fact emerges: our planetary system seems to be destined to lose Saturn, its most mysterious decoration, to see that planet accompanied by its ring and the seven satellites[25] getting gradually lost in the unknown regions which the eye armed with the most powerful telescopes will never penetrate.

Jupiter, that other globe, alongside which our planet is of so small consequence, seems to travel in the opposite direction to be absorbed by the incandescent matter of the Sun. Nothing doubtful or systematic [speculative?] is included in those sinister predictions; the incertitude can only concern the precise dates of those catastrophes[26]. It is known, however, that they are very remote so that the public is not interested either in scientific considerations or lively descriptions provided by certain poets on that subject.

It is not so with respect to scientific societies. There, the march of our planetary system to its ruin is sadly contemplated. The [Paris] Academy of Sciences attracted the attention of geometers of all nations to those menacing perturbations. Euler and Lagrange descended on the arena and never did their mathematical genius emit such a lively lustre. Still, the question remained indecisive and the fruitlessness of such efforts seemed only to leave place to resignation until the author of the *Mécanique Céleste* did not clearly show the laws of those great phenomena by issuing from two obscure nooks previously neglected by analytical theories. The variations of the velocities of Jupiter, Saturn and the Moon became the effects of evident physical causes and were returned to the category of usual periodic perturbations depending on gravity. Those threatening changes in the orbital dimensions were actually simple narrowly restricted oscillations and the material world finally found itself strengthened on its base by an all-powerful mathematical formula.

[14] I do not wish to quit this subject without at least naming those circumstances on which depend the unexplained for such a long time variations in the velocities of the Moon, Jupiter and Saturn.

The motion of the Earth around the Sun largely occurs along the contour of an ellipse whose form, owing to perturbations, is not always the same. The changes are periodic; sometimes the curve, while remaining an ellipse, approaches, at other times deviates ever more from a circumference. Beginning from the time of most ancient observations the eccentricity of the terrestrial orbit has been diminishing from year to year, but later, following the same laws, it will increase by the same amount.

Thus, Laplace proved that the mean velocity of the circulation of the Moon about the Earth was connected with the form of the terrestrial orbital ellipse; that the diminution of the eccentricity of that ellipse inevitably leads to the increase in the velocity of our satellite and vice versa. And, finally, that that cause was sufficient to account numerically for the acceleration of the Moon from the most remote days to our epoch[27]. And I hope that the inequalities in the velocities of Jupiter and Saturn will also be easy to comprehend.

Mathematical analysis did not arrive at representing in a finite form the value of the derangements of each planet experienced during its march by the action of all the other planets. Nowadays, the actual state of science provides those values in a form of infinite series whose terms rapidly decrease with their distance from the beginning of the series. And calculations ignore terms which correspond to magnitudes beneath [in absolute values] errors of observation. However, there exist cases in which the order [the numerical value] of the term is not sufficient to decide whether it is large or small: certain numerical relations between the initial elements of corrupting and corrupted planets can provide sensible values to ordinarily negligible terms. We encounter such a case when dealing with the perturbations of Saturn by Jupiter and vice versa.

There exists a simple commensurable relation between the mean velocities of these two large planets: the fivefold velocity of Saturn is almost equal to the twofold velocity of Jupiter. The terms being very small in the absence of that circumstance acquire considerable values [considerable meaning] and long-period inequalities in the motion of those celestial bodies appear which completely develop in more than 900 years and represent a marvel of all the oddities revealed by the observers.

Is it not surprising that the commensurability of the motions of those planets caused such an influential perturbation? To see that the definitive solution of an immense difficulty over which Euler's genius was unable to triumph, and which even questioned the sufficiency of the universal gravitation for explaining the phenomena of the firmament, – that that solution depended on the numerical relation, *five times the motion of Saturn almost equals twice the motion of Jupiter*? The delicacy of the concept and the result are equally worthy of admiration[28].

[15] We have explained how Laplace had proved that the solar system can only experience small periodic oscillations about a certain mean state. Let us see how he succeeded to determine the absolute orbital dimensions. How far is the Sun from the Earth? No other scientific question occupied man more than that one. Mathematically speaking, nothing can be simpler. It is sufficient, as in surveying a locality, to sight the inaccessible object from the ends of a given [a measured] base; all the rest is elementary calculation.

Regrettably, the distance to the Sun is great and measurable bases are comparatively very short and in such cases a smallest error of sighting exerts an enormous influence. At the beginning of the previous century Halley remarked that certain positions of Venus between the Earth and the Sun, or, to apply the usual term, that Venus' passage across the solar disc provides for every observatory an indirect means for fixing the ray of sight much superior in precision than the most perfect direct methods[29].

On those occasions, in 1761 and 1769, scientific expeditions were sent, apart from places in Europe, to the Rodrigues island [in the Indian ocean], the island of Saint-Domingue [in the Caribbean Sea], California and Pondicherry [in India] with French representatives being, respectively, Pingré, Fleurin, the Abbot Chappe, and Legentil.

At the same time, England sent Maskelyne to Saint Helena [in the Atlantic Ocean], Wales to the Hudson Bay, Mason to the Cape of Good Hope, Captain Cook to Tahiti, etc. No government hesitated to provide the [their] academies the means, however expensive, for suitably establishing their observers in most remote regions. Observations in the southern hemisphere, after being compared with those in Europe, and especially with those made by Father Hell in Wardhus, Lapland, brought about the result for the distance of the Sun since then entered in every astronomical and navigational treatise.

We have already remarked that the determination of the sought distance imperiously demanded a great base. Fine! But Laplace numerically resolved the same problem without having any base; he deduced the distance of the Sun by observations of the Moon made at one and the same place!

For our satellite, the Sun is the cause of perturbations which evidently depend on the distance of that immense globe illuminating the Earth. Who would not perceive that these perturbations diminish when the distance increases and vice versa? That, finally, the distance regulates the magnitude of the perturbations? Observations provide the numerical value of those perturbations; the theory, on the other hand, reveals the general mathematical relation connecting them with the solar distance and the known elements.

Having obtained that, the determination of the mean radius of the terrestrial orbit becomes a simplest algebraic operation. Such was the fortunate combination by whose means Laplace resolved the great and celebrated problem of the [Sun's] parallax, that is how the illustrious geometer established the mean distance of the Sun measured by the radius of the terrestrial globe, little differing from the value obtained after all the difficult and expensive voyages. According to the opinion of very competent judges, the indirect method should even be preferred[30].

[16] For our great geometer, the lunar motion occurred as a fertile mine. His penetrating mind was able to discover unknown treasures; with skill and patience equally worthy of admiration, he disengaged them from everything that had hidden them from ordinary eyes, and we will be excused for citing a new example.

The Earth retains the Moon in its course; it is flattened and does not therefore attract like a spherical body. It should therefore be something in the lunar motion, we almost say, in the lunar gait, an imprint of sorts of that flattening. Such was the first sketch of Laplace's thoughts.

It remained to decide, and that was especially difficult, whether these characteristic traits with which the Earth's flattening must provide lunar motion, were sufficiently sensible and apparent for avoiding to be mixed up with the errors of observation. And it was also necessary to find the general formula of that kind of perturbations for being able, as was the case of the solar parallax, to reveal the unknown.

Laplace's ardour and analytical power surmounted all obstacles. After the work that demanded infinite attention, the great geometer discovered two characteristic perturbations in the lunar motion dependent on the terrestrial flattening. The first influenced the motion of our satellite and was measurable, especially by an instrument known at the observatories as the transit telescope. The second perturbation acting almost in the north – south direction only manifested itself by observations with another instrument, the mural circle. These two inequalities, of very unequal values, measured by entirely different instruments, combined by the cause that produces them by most diverse analytical combinations, nevertheless lead to the same flattening.

That is certainly not the particular flattening corresponding to one or another region, observed in France, England, Italy, Lapland, North America, India, or near the Cape of Good Hope, since at different times and in differing places the Earth experienced considerable risings and the initial regularity of its curvature was notably corrupted. It is the Moon that renders the result of inestimable value and ought to provide and actually furnishes the general flattening of the globe, a kind of a mean of sorts of the various determinations obtained at enormous expenses and infinite work after great voyages by astronomers of all European nations.

My brief remarks will be largely borrowed from the author of the *Méc. Cél.*; they seem quite proper for showing in relief and throwing full light on the deep, unexpected and even paradoxical in those methods whose principal traits I have outlined.

What had Laplace applied for arriving at results of highest precision? On the one hand, mathematical formulas derived from the principle of universal attraction; on the other hand, certain irregularities observed in the returns of the Moon to the meridian. An observant geometer who never from his birth came out of his study; who saw the sky only across a narrow opening directed north and south with the main astronomical instruments moving along it in the vertical plane; who never revealed anything concerning the celestial bodies moving around above his head if it was not about mutual attraction following the Newtonian law, – that geometer nevertheless discovered by the power of analytical science that his humble and narrow abode belonged to a flattened ellipsoidal globe whose equatorial axis exceeded the polar axis, i. e. the axis of rotation, by 1/306. And being isolated and always immobile he also found his veritable distance from the Sun.

[17] We ought to return to D'Alembert, as I have reminded at the beginning of this essay, for finding a mathematical explication satisfying the phenomenon of the precession of the equinoxes;

however, our illustrious compatriot and Euler, who provided the solution twenty years later than D'Alembert, completely leaving aside certain physical circumstances which nevertheless should not have apparently been neglected without examination.

Laplace filled that gap; he proved that the sea, in spite of its fluidity, and the atmosphere, in spite of its currents, as though they formed solid masses adhered to the terrestrial spheroid, influence the motion of the Earth's axis, or the equator.

The axis around which our globe entirely turns each 24 hours, – does it always pierce the terrestrial spheroid at the same material points? Or, in other words, the poles of rotation from year to year corresponding to differing stars, – do they also move about the surface of the Earth? If the answer is in the affirmative, the equator is walking as the poles are; terrestrial latitudes vary, and over the centuries no region had been enjoying the same climate even in the mean. Most diverse climatic belts became, one after another, circumpolar. Adopt now the opposite premise, and everything acquires an admirable permanence.

The problem that I am formulating, one of the most essential in astronomy, cannot be resolved by observations only since ancient terrestrial latitudes are uncertain. Laplace [1809; *Méc. Cél.*, t. 5, 1825/1882, pp. 288 – 291] subjected it to analysis and the learned world found out from him that no cause connected with universal attraction should sensibly move that axis across the surface of the terrestrial spheroid[31].

Because of the mobility of water and the resistance which its oscillations engender, the sea, far from being an obstacle to a constant rotation of our globe around the same axis, returns on the contrary that axis to a permanent state. All the indications about the position of the axis of the world should be extended to the motion of the Earth's rotation, which is a veritable standard of time. The importance of that magnitude led Laplace to investigate numerically whether it can be changed by interior causes, such as earthquakes or volcanoes. I hardly need to say that the result was negative.

The admirable work of Lagrange on the libration of the Moon apparently exhausted that subject. However, something was left. The revolution of our satellite around the Earth is subjected to perturbations, to inequalities called *secular*, which were either unknown to, or neglected by Lagrange. In the long run, they place that celestial body (without allowing for entire circumferences) at a semi-circumference, at a circumference and a half, etc, from its position had not those inequalities existed.

[18] If the motion of rotation did not participate in those [did not experience those] perturbations, the Moon would have gradually presented us all parts of its surface. That event did not occur at all; the lunar hemisphere invisible at present will remain invisible forever. Laplace actually proved that the attraction of the Earth introduces secular inequalities of its revolutions in the rotation of the lunar spheroid. Such investigations present the power of mathematical analysis in all its lustre. It would have been really difficult for the synthesis [speculation?] to discover truths so deeply entangled with the complex actions of a multitude of forces.

Forgetting to place in the first rank the works of Laplace on perfecting the lunar tables will be unforgivable. Actually, that work was directly aimed at ensuring the rapidity of remote maritime communications and, what greatly surpasses all mercantile interests, the preservation of human life.

Owing to his unparalleled sagacity, unlimited perseverance and incessantly juvenile ardour communicated to his skilful collaborators, Laplace solved the celebrated problem of longitudes more completely than could have been dared to expect from the scientific viewpoint and more exactly than demanded by the nautical art in its latest refinements. Today, ships braving the winds and tempests have nothing at all to fear of being lost in the immense Ocean. A glimpse of the navigator's intelligent eye at the starry heaven will tell him, wherever he is and at any time, the [longitudinal] distance from the Paris meridian[32]. Laplace achieved an extreme perfection of the modern tables of the Moon and should by right be ranked among the benefactors of mankind.

[19] In the beginning of 1611 Galileo, as he thought, discovered a simple and rigorous solution of the [of that same] famous nautical problem by [observing] the eclipses of Jupiter's satellites. Active negotiations had then started, but failed, for introducing his new method on board of numerous Spanish and Dutch ships. The discussion revealed without doubt that an exact observation of those eclipses demanded powerful telescopes impossible to apply on a ship rocked by waves.

It seemed, however, that Galileo's method should at least preserve all its advantages on firm land with a promise of immense perfection of geography. These expectations also proved premature. The motion of Jupiter's satellites is not at all as simple as the immortal inventor of that method of longitudes supposed. Three generations of astronomers and geometers had to work persistently to disentangle the great perturbations involved. And finally for the tables of those small celestial bodies to acquire all the desired and necessary precision, Laplace had to illuminate them by the torch of his mathematical analysis. Nowadays, nautical ephemerides indicate five or ten years in advance the time when Jupiter's satellites ought to be eclipsed and returned and the pertinent calculations are not inferior in precision to direct observation.

In that group of satellites, considering them separately [from the planet], Laplace discovered perturbations similar to those experienced by the planets. The promptness of their revolution reveals in a very short time such changes for the development of which in the solar system centuries are needed.

Although the satellites have a barely appreciable diameter even when observing them in best telescopes, our illustrious compatriot determined their masses. Finally, he discovered simple and extremely remarkable relations called *Laplacean laws* between the motions and relative positions of those small celestial bodies; it is indeed natural that the name of such a great astronomer is entered in the firmament alongside Kepler's and the posterity will not renounce that designation. I cite two or three of these laws.

When adding together the mean longitude of the first satellite and the double longitude of the third one and subtracting from the sum the threefold mean longitude of the second, the result will be exactly equal to 180°.

Is not it really extraordinary that the three satellites initially placed at some distance from Jupiter in respective positions that had to remain invariably and rigorously in the abovementioned relation? Laplace answered that question by proving that that relation had not necessarily been initially rigorous. The mutual attraction of the satellites should have brought them about to the present state if at one single moment the distances and positions approximately satisfied that law.

That first law is equally valid in terms of synodical elements and it follows without doubt that the three first satellites of Jupiter cannot be eclipsed all at once. During recent and much celebrated observations certain astronomers for a short time did not notice any of the four satellites of that planet[33] which, however, does not at all mean that they were eclipsed. A satellite disappears when projected on the central part of the luminous disc of Jupiter, and again when it passes behind the opaque body of that planet[34].

And here is the second very simple law to which the mean motions of the same satellites are subjected:

When adding together the mean motion of the first satellite and the double mean motion of the third one, the sum will be exactly equal to the threefold mean motion of the second satellite[35].

These numerical relations, perfectly exact, would have been a most mysterious phenomenon of the system of the world had not Laplace proved that that law [those both laws?] could have only been initially approximate with the mutual attraction of the satellites being sufficient for rendering it rigorous. The illustrious geometer who always brought his studies to their final ramifications arrived at the [final] result:

The action of Jupiter coordinates the rotation of its satellites in such a way that, disregarding their secular perturbations, the period of rotation of the first satellite plus twice that magnitude for the third one provides a sum always equal to the threefold period of rotation of the second satellite.

[20] Because of deference, modesty and timidity having no plausible cause, during the previous century our mechanics had given over the monopoly of manufacturing astronomical instruments to the English. And, let us avow in plain terms, that when during the Herschel epoch fine observations had been made on the other side of the Channel, here, in France, it was impossible to follow, develop or even verify them.

Fortunately for the scientific honour of our nation mathematical analysis is also a powerful instrument. Laplace had proved so conclusively on a solemn occasion that, while remaining in his study, he foresaw and minutely announced what Herschel noted by means of greatest telescopes ever manufactured by man.

When Galileo, in the beginning of 1610, directed his recently constructed by himself very weak telescope to Saturn, he saw that that planet was not an ordinary globe, but he remained unable to perceive exactly its real form. The expression *tri-corps* [treble] with which Galileo summarized his thoughts even implied a completely erroneous idea. Our compatriot Roberval was inspired much better, but, not having minutely compared his hypothesis with observations, left to Huygens the honour of being considered the author of the true theory of the phenomena presented by that admirable planet.

Today, everyone knows that Saturn consists of a globe 900 times larger than the Earth and a ring. That ring does not touch the globe anywhere and is everywhere distant by 32 thousand kilometres. Observations provide its width, 48 thousand kilometres and its thickness is certainly less than 400 kilometres[36].

Except an obscure stripe extending along the entire ring and separating it in two parts of unequal width and dissimilar brightness, that strange colossal bridge lacking piles never presented [even] the most experienced and skilful observers either a spot or a bulge sufficient for deciding whether it remains immobile or is endowed with a rotational motion.

Laplace believed it unlikely that the ring was immobile since its parts only by adhesion resisted the continuous attractive action of the planet. Rotation can be seen, as he thought, as a principle of conservation and he determined its necessary velocity. His estimate was the same as deduced later by Herschel by extremely delicate observations!

Owing to the Sun's action, the two parts of the ring, situated at different distances from the planet, cannot avoid to experience differing precessional [?] motions. Their planes apparently should be ordinarily inclined to each other although observation shows that they are always entangled. There should therefore exist a cause capable of neutralizing that action. In a memoir published in February 1789, Laplace had discovered that that cause ought to have been Saturn's flattening produced by its rapid rotation whose existence Herschel announced in November of the same year[37]. Note how the mind's eye can sometimes supplement the most powerful telescopes and lead to astronomical discoveries of the first rank.

[21] Descend now from the sky to the Earth. Laplace's discoveries are here not less excellent, not less worthy of his genius. He attached the tides, that phenomenon which the ancients desperately called *the tomb of human curiosity*, to an analytic theory in which physical conditions entered for the first time[38]. And the calculators, to the immense advantage of navigation for our seaboard, are today venturing to predict many years in advance the time and height of the full tides feeling themselves not more uneasy about their results than it would have been in case of predicting the phases of an eclipse.

Among the diverse phenomena of flux and reflux and the attractive action of the Sun and the Moon on the liquid surface covering three quarters of our globe, there exists an essential and necessary connection from which Laplace, aided by 20 years of observations at Brest, calculated the mass of our satellite. Owing to the diligent and minute study of the fluctuations of the Ocean, scientists today know that 75 moons are necessary for providing gravity equivalent to that existing on our terrestrial globe.

We know only one means that can be added to the deep admiration which all attentive minds undoubtedly experience about theories capable of such consequences. We provide a historical citation; recall that in 1631 the illustrious Galileo, in his celebrated *Dialogue*, was so far from foreseeing the mathematical connections from which Laplace deduced such fine, evident and useful results that he accused of insufficiency (ineptie) Kepler's vague thought of attributing to the lunar attraction a certain part of the daily periodic movements of the sea flows.

Laplace did not stop at extending so widely and perfecting so essentially the mathematical theory of the tides. He also envisaged that phenomenon from an entirely new viewpoint: he was the first to consider the stability of the equilibrium of the sea. Systems of solid or liquid bodies are capable of two kinds of equilibrium that ought to be carefully distinguished. The first kind, the stable equilibrium, means that the system, slightly removed from its initial position, incessantly tends to return back. Under the unstable equilibrium, to the contrary, a very feeble fluctuation can in the long run cause an enormous displacement.

If the equilibrium of the flows were of that second kind, the waves born by the action of wind, earthquakes or abrupt movement of the sea bottom could have raised those flows in the past, can raise them in the future to the height of the highest mountains. The geologist will be satisfied to consider these prodigious oscillations as a rational explanation of a great number of phenomena, but the world will be, however, exposed to new terrible cataclysms.

We can rest assured: Laplace proved that the equilibrium of the Ocean was stable, although on the strict condition established in addition by invariable facts that the mean density of the liquid mass was inferior to the mean density of the Earth. At sea, everything remains in the same state; but substitute mercury instead of water, and stability will disappear, the liquid will frequently overflow its boundaries and flood the continents up to the snowy regions hidden in clouds.

Should not we remark how each analytic study made by Laplace subjects the universe and our globe to conditions of order and durability! It would have been impossible for the great geometer so successful in his investigation of the oceanic tides to miss altogether atmospheric tides, not to subject the generally held opinion concerning the influence of the Moon on the height of the barometer and other meteorological phenomena to the delicate and definitive trial of rigorous calculation.

Actually, Laplace devoted a chapter of his fine work to examining the fluctuations that the attractive force of the Moon can produce in our atmosphere[39]. As a result, the lunar flux at Paris as measured by the barometer occurred to be quite insensible. The value of that flux obtained by discussing a long series of observations did not surpass $0.02mm$, a magnitude inferior to what is possible to provide at the present state of meteorology.

That calculation could have appealed to the support by considerations which I had made use of when wishing to establish that, if the Moon does more or less modify the height of the barometer according to its different phases, it is not because of attraction.

[22] No one was more ingenious than Laplace in catching these relations and deep connections between apparently heterogeneous phenomena, no one was more skilfully able to draw important consequences from the unexpected rapprochements. For example, by means of certain observations of the Moon he refuted at the end of his life Buffon's and Bailly's theories fashionable for such a long time and achieved it with a single stroke of pen. According to those theories, the freezing of the Earth was inevitable and imminent. Never content with vague expressions, Laplace attempted to determine quantitatively that freezing announced by Buffon so eloquently but so groundlessly. Nothing is simpler and better woven and more demonstrative than the coupling of the deductions of that celebrated geometer.

A cooling body diminishes in size; according to the most elementary principles of mechanics, a rotating body, when being compressed, inevitably turns ever more rapidly. The day during each epoch was reckoned as the time of the Earth's rotation, so if the Earth is cooling, the day will be incessantly shortening. And there exists a method of discovering whether the duration of the day varies; examine each century, how large was the arc of the celestial sphere through which the Moon moved during the time that the astronomers of the respective epoch called a day, – during the time of the Earth's rotation around itself. The velocity of the Moon is actually independent from the duration of the rotation of our globe.

Assume now, as Laplace does, values as small as you wish in the known tables of the dilatation and contraction of solid bodies undergoing changes of temperature, then find in the Greek, Arab and modern astronomical annals the angular velocity of the Moon, and the great geometer will apply those data for invincibly proving that in two thousand years the mean temperature of our globe did not differ by a hundredth part of a degree centigrade.

Eloquence cannot at all resist the authority of similar arguments and the power of such numbers. Mathematics has always been an implacable adversary of scientific novels.

[23] The fall of bodies, had it not been a phenomenon seen each instant, would have justifiably excited in a highest measure human surprise. What, really, is more extraordinary than seeing an inert mass, i. e. a mass lacking volition, a mass that should not have any propensity to move in one direction rather than in another, precipitating to the Earth as soon as it is not anymore supported!

Nature engenders the weight of bodies by such concealed methods, so far beyond the reach of our senses and ordinary capability of human intelligence, that beginning with antiquity philosophers believed to be able to explain all except weight mechanically, by simple evolution of atoms. Descartes attempted that, what Leucippus, Democritus, Epicurus and their schools had thought impossible. He made the fall of terrestrial bodies depend on the action of a vortex of very fine matter circulating about our globe. The real perfection of the ingenious concept of our countryman achieved by the illustrious

Huygens remained, however, far from clarity and perfection, the characteristic properties of truth.

The ancients did not appreciate the sense, the extent of a greatest problem with which modern scientists are occupied. The latter see Newton coming out victoriously from a battle in which two of his immortal predecessors had failed, although without discovering the cause of gravity either.

Two given bodies approach each other, but Newton had not studied the nature of the force producing that effect. It exists and he called it *attraction*, but warned that for him that term did not imply any adopted idea about the manner of the physical action following which gravitation is born and exists.

The attractive force, once acknowledging it de facto, Newton followed it and studied its action on terrestrial phenomena, on revolutions of the Moon, planets, satellites, comets, and, as I have said above, indicated in that incomparable investigation the simple universal mathematical traits of the forces directing all celestial bodies comprising our solar system. The lively applause of the scientific world did not prevent the immortal author of the *Mathematical Principles* from hearing some isolated voices pronouncing words of an occult character about universal attraction. They made Newton and his most enthusiastic disciples abandon the restraint that they had thought to impose on themselves. […]

Newton never categorically described the manner of a possible birth of impulsion, a physical cause of the attractive capability of matter at least in our solar system. Today, however, we have serious reasons to suppose that, while writing the word *impulsion*, the great geometer thought about the systematic [speculative?] ideas of Varignon and Fatio de Duillier later discovered and perfected by Lesage. Those ideas were indeed communicated to him before any publications.

According to the ideas of Lesage, there exist corpuscules moving in certain regions of the space in all possible directions with excessive rapidity. The author named then *ultra-mondain* (ultra-temporal) with their set constituting the *fluide gravifique* (gravity-engendering fluid) although that designation can be applied to totalities of particles having no connection with each other. […]

If attraction is the result of the impulsion of a fluid, its action will need finite time for traversing the immense spaces separating celestial bodies. Suppose that the Sun suddenly disappeared, then the Earth, mathematically speaking, will still experience its attraction for some time. An opposite phenomenon will also occur after a sudden birth of a planet: some time will have to pass before our globe experiences its attractive force.

Many geometers of the previous century believed that attraction was not transmitted instantaneously from one body to another; they even thought that it propagated rather slowly. Daniel Bernoulli, for example, wished to describe how the greatest tide arrived at our shores a day and a half after syzygies, i. e., after the epoch when the Sun and the Moon are most favourably situated for producing that magnificent phenomenon. He supposed that the lunar action needed all that time (1½ days) for being transmitted to the sea. Such a weak speed cannot be reconciled with the mechanical explanation of gravity as described above. That explanation imperiously demands that the velocity of celestial bodies were insensible as compared with that of the gravity-engendering fluid.

Before discovering that the present diminution of the eccentricity of the terrestrial orbit was the real cause of the observed acceleration of the lunar motion, Laplace, for his part, examined whether that mysterious acceleration depended on the consecutive [in finite time] propagation of attraction. At first, the calculation rendered that supposition plausible. It indicated that a perturbation was then inevitably introduced in the motion of our satellite proportional to the square of the time passed from the beginning of any epoch and it was not at all necessary for numerically representing the results of astronomical calculations to attribute low velocity to attraction; a propagation eight million times more rapid than that of light will satisfy all phenomena.

Although the real cause of the acceleration of the Moon is now well known, Laplace's ingenious calculation still has its place in science. From the mathematical viewpoint, a perturbation depending on a consecutive propagation of attraction that that calculation provides really exists. The connection between velocity and perturbation is such that one of those two magnitudes leads to the numerical knowledge of the other. And, assuming a maximal value of perturbation conforming to observations corrected for the known acceleration, caused by a change of the eccentricity of the terrestrial orbit, the velocity of the attractive force is found to be 50*mln* times that of light.

Recalling that that number is the lower boundary and that the velocity of luminous rays is 308 [330] thousand *km/sec*, physicists claiming to have explained attraction by the impulsion of a fluid will see how prodigious are the velocities satisfying them.

[24] The reader will remark here once more with what sagacity Laplace knew how to catch phenomena suitable for elucidating most difficult problems of celestial physics, how successfully he examined them and suddenly called forth numerical consequences confounding the mind.

Like Newton, the author of the *Méc. Cél*. admitted that light consisted of excessively fine material molecules having velocity 308 thousand *km/sec* in vacuum. However, it is necessary to warn those who would like to refer to that imposing authority, that Laplace's main argument in favour of the system of emission was the possibility of subjecting it to simple and rigorous calculation whereas the wave theory presented and still offers immense difficulties to analysts.

It was natural that a geometer who had so elegantly connected the laws of simple refraction that light experiences in the atmosphere and of its double refraction in certain crystals to the attractive and repulsive forces, and to keep to that point of view until mathematically discovering the impossibility of arriving in the same way to plausible explanation of diffraction and polarization. Nevertheless, the trouble that Laplace always took to further as much as possible his researches to numerical deductions, allowed physicists, who will undertake a complete comparison of the two rival theories of light, to draw from the *Méc. Cél*. the data concerning many abundantly interesting and very exciting rapprochements.

Is light an emanation from the Sun? Is that celestial body sending each moment and in all directions a part of its own substance? Are its volume and mass gradually diminishing? Solar attraction of our globe will then become ever less considerable, the radius of the terrestrial orbit, on the contrary, will not fail to increase and the length of the year will increase correspondingly.

That is what results for the whole world after a first glance. Calculating analytically that problem and then going over to numerical applications aided by the results of the most precise observations of the duration of the year in various centuries, Laplace proved that two thousand years of constant emission of light did not diminish the Sun's mass more than by $2/10^6$ parts of its initial value.

Our illustrious compatriot never proposed anything vague or indecisive. His invariable goal was to explain some great natural phenomena according to the inflexible rules of mathematical analysis. No physician or geometer was more thoroughly on his guard against pedantry. No one was more afraid of scientific errors engendered by imagination unrestricted by the boundaries of facts, calculation and analogy.

[25] Once, only once did Laplace rush, like Kepler, Descartes, Leibniz and Buffon in the region of conjecture. His idea was then concerned with cosmogony, no less. All planets circulate about the Sun from west to east in planes forming small angles between them. The satellites move around their respective planets like the planets about the Sun, that is, from west to east. The planets and satellites with observable rotation also turn from west to east and, finally, the Sun [itself] rotates from west to east, so the total is 43 motions in the same direction. According to the theory of probability, there are more than $4\cdot10^9$ [ca. $8.8\cdot10^8$] chances against one that that coincidence is not due to randomness[41].

I think that Buffon was the first who attempted to account for that singularity of our solar system. "Wishing to abstain, when explaining phenomena, to issue from causes beyond nature"[42], the celebrated academician looked for a physical origin of what is common in the motions of so many celestial bodies, differing in size, forms, distances from the main centre of attraction.

[In 1745] he thought to have discovered that origin by making a triple assumption: a comet falls obliquely on the Sun; it engenders a torrent of fluid matter blowing before it; that matter, transported more or less far from the Sun according to diverse degrees of its lightness, condenses and forms all the known planets.

Buffon's bold hypothesis is a source of insurmountable difficulties. I will indicate in a few words the cosmogonic system that Laplace substituted instead of that illustrious natural scientist.

[26] According to Laplace, in remote times the Sun was a central kernel of an immense nebula having a very high temperature and spreading far beyond the region where Uranus is now moving. No planets existed at that time. The solar nebula rotated from west to east; when cooling, it could not have failed to experience a gradual condensation and from that time began to turn ever faster.

If the matter of the nebula initially spread to its equatorial region, to the boundary where the centrifugal force exactly counterbalanced the attractive action of the kernel, the molecules situated at that boundary would have separated themselves during the condensation from the rest of the atmospheric matter and form an equatorial zone, a ring turning separately with its initial velocity.

It can be perceived that similar separations took place at diverse epochs, that is, at differing distances from the kernel, in the superior layers of the nebula, and formed a succession of distinct rings contained in approximately the same plane and having differing velocities.

Admitting that, it is easily seen that an indefinite preservation of the rings should have demanded a very unlikely regularity of composition over all their circumferences. Each ring therefore broke up, one after another, in many masses rotating, as it is easy to understand, in the direction coinciding with that of the common revolution and, because of their fluidity, they acquired spheroidal forms.

Assume now that one of those spheroids could have seized all the other ones [de tous les autres] that originated from the same ring; for that to happen it is sufficient to attribute to it a mass surpassing the total mass of those others. In each planet being in a vaporous state about which we had spoken, the mind perceives a central kernel gradually increasing in mass and size, and an atmosphere which provides in its consecutive boundaries phenomena entirely similar to those that the proper solar atmosphere presented us. Here we see the birth of the satellites and the ring of Saturn.

[27] I sketched that system to indicate how a rotating nebula can be transformed in the long run into a central and very bright kernel (a sun) and a number of distinctive spheroidal planets removed one from another, all of them circulating around a central sun in the initial direction of the nebula; how these planets should also rotate in the same direction; how, finally, the satellites, after being formed, cannot fail to turn about themselves and around the planets that lead them in their own direction and their own circulation around the Sun.

Conforming to the principles of mechanics, we will find the forces with which the particles of the nebula were endowed, in the motions of rotation and revolution of the compact and distinct masses that those particles had engendered while agglomerating.

That, however, was only the first step. The initial rotation of the nebula was not at all connected with simple attractions, it apparently indicated the action of an impulsive primordial force.

On that point Laplace was far from sharing the almost general opinion of philosophers and geometers. "He did not believe that the mutual attraction of the initially immobile bodies should in the long run combine all those bodies being at rest around their common centre of gravity"[43]. On the contrary, Laplace maintained that three bodies lacking motions, [each of the] two much more massive than the third one, only agglomerate in exceptional cases. Generally, those two combine whereas the third one circulates around the common centre of gravity[44].

Attraction thus becomes the cause of the kind of motions which only impulsion seems to be able to engender. Actually, it can be thought that, while expounding that part of his system, Laplace had before his eyes and wished to refute the words that Rousseau had put into the mouth of a vicar from Savoy [France]:

Newton discovered the law of attraction, but attraction by itself will soon reduce the universe to an immobile mass. For the celestial bodies to describe their curves it was necessary to join to it a projectile force. Descartes tells us what physical law forces his vortexes to turn; Newton showed the hand that hurls the planets along the tangents of their orbits.

[28] According to Laplace's cosmogonic ideas, the comets did not at all initially belong to our system; they were not formed at the expense of the immense solar nebula's matter. They should be considered as small vagrant nebulae deflected from their initial path by the attractive force of the Sun. Those of them which penetrated into the great nebula at the epoch of its condensation and of the formation of the planets, fell spirally towards the Sun and, owing to their action, should have more or less pushed the planes of planetary orbits from that of the solar equator with which they would have otherwise coincided.

As to the zodiacal light, that stumbling-block on which so many dreams were shattered, it consists of parts more volatile than those of the initial nebula. These [parts or] molecules, not combined in the equatorial zones, consecutively abandoned the plane of the solar equator, continued to circulate with their primordial distances and original velocities persisting.

The existence of that extreme rare matter in the region occupied by the Earth or only by Venus seemed incompatible with the laws of mechanics. However, that [difficulty only occurs] when mentally connecting the zodiacal matter by an immediate and close dependence with the solar photosphere properly speaking, we imprint it with an angular rotation equal to that of the photosphere by means of which its entire revolution only demands 25½ days.

Laplace distrustfully [with reserve] presented his conjectures about the formation of the solar system as it should have been done for considerations not based on calculation and observation[45]. Perhaps we should regret that they were not better elaborated, especially in the part connected with the separation of the matter into distinct rings; perhaps it is disappointing that the illustrious author had not sufficiently explained the initial physical state, the molecular state of the nebula at whose expense the Sun, the planets and satellites of our system had been formed; perhaps we ought to regret, in particular, that Laplace thought it possible to discuss flippantly the possibility, evident for him, of circulation resulting from the action of simple attracting forces, etc.

In spite of these gaps, Laplace's ideas are no less the only ones which, by their greatness, coherence and mathematical character can be really considered as forming a physical cosmogony; the only ones that today are mightily supported by the results of recent astronomical studies of the nebulae of any size and form scattered over the firmament.

[29] In our analysis, we thought to be obliged to concentrate all attention to the *Mécanique Céleste* [although] the *Exposition du Système du Monde* and the *Théorie Analytique des Probabilités* demand to be no less elaborated[46].

The *Exposition* is the *Méc. Cél*. stripped of the great attire of analytical formulas by which each astronomer ought to be indispensably guided if, as Plato expressed it, he desires to find out the numbers governing the material universe. It is from the *Exposition* that those remote from mathematics will extract exact ideas sufficient for the mind about the methods to which physical astronomy owes its surprising progress.

That nobly simple work characterized by excellence in expression and scrupulous correctness ends by a sketch of the history of astronomy nowadays unanimously held to be one of the best monuments of the French language. It is often regretted that Caesar in his immortal *Commentaries* only described his own campaigns; Laplace's astronomical commentaries, however, go back to the origin of societies. Work undertaken during all ages to extract new truths from the firmament is analysed justifiably, clearly and deeply; a genius impartially appreciated geniuses. Laplace always remains at the peak of that great mission and his contribution will be respectfully read as long as the torch of science is spreading some gleam.

The calculus of probability restricted in proper bounds must equally interest mathematicians, experimenters and statesmen. Beginning with the now very remote time [with 1654] when Pascal and Fermat formulated its first principles[47], it rendered and daily renders eminent service. It is this calculus which after regulating the best applications of the tables of population and mortality, teaches us how to extract precise and useful consequences out of all those numbers ordinarily so poorly interpreted.

Only it can justifiably regulate the insurance premiums, the contributions necessary for joining tontines, deductions paid to pension funds, only it regulates annuities, discounts etc. It is the calculus of probability that definitively suppressed the craftily arranged lottery [of France] with all its shameful traps for greed and ignorance[48].

Laplace treated these and many more complex problems with his usual superiority. In one word, the *Théorie* is worthy of the author of the *Méc. Cél*. A philosopher, whose name recalls immortal discoveries, told his listeners fascinated by olden and generally adopted reputations:

Consider thoroughly that in scientific matters the authority of a thousand is less valuable than the humblest reasoning of a single person.

[30] Two centuries have passed without lowering the value of those words of Galileo, without concealing their verity. Therefore, instead of providing a long list of illustrious admirers of the three excellent works of Laplace, we preferred, so to say, to explain some of the great verities that geometry introduced there. Nevertheless, we will not carry the rigour to its extreme, the less so since chance handed us some unpublished letters of a man of genius whose nature endowed him with a rare faculty of catching at once the culminating points of subjects. I will be allowed me to publish two or three brief and characteristic appreciations of the *Méc. Cél*. and the *Théorie*.

On 27 Vendémiaire [the first autumnal month] an X[50], after receiving a volume of the *Méc. Cél*., General Bonaparte wrote to Laplace: "The first six months during which I will have time at my disposal, will be spent to read your fine work". It seems to us that those words, *the first six months*, take away the imprint of a banal thank you from that answer and include a justifiable appreciation of the importance and difficulty of its subject.

On 5 Frimaire [the last autumnal month] an XI, the reading of some chapters of the volume that Laplace had dedicated to him, became for the general

A new occasion to be saddened that the force of circumstances directed me [him] *to a career that removes me* [him] *away from science*[51]. *At least I sincerely desire that future generations, while reading the Méc. Cél., will not forget the esteem and friendship that I feel* [he feels] *for its author*.

On 17 Prairial [the last vernal month] an XIII the general who became emperor wrote from Milan: "The *Méc. Cél*. apparently attempts to provide our century with a new lustre".

Finally, on 12 August 1812, upon receiving the *Théorie*, Napoleon wrote from Vitebsk [Belarus] the letter reprinted here word for word:

There was a time when I thought to read with interest your Traité du calcul des probabilités [!]. *Today, I ought to restrict myself by testifying to you my satisfaction which I feel each time you provide new works that perfect and develop the main science and contribute to the nation's glory. The advancement and perfection of mathematics are connected with the prosperity of the state*.

I am finishing the task that I imposed on myself. I will be excused for describing in all detail the main discoveries that philosophy, astronomy and navigation owe to our geometers. It seems to me that, when retracing that glorious past, I showed our contemporaries all the scope of their duties for the state. Actually, we mostly recall the old saying, *noblesse oblige*, in connection with nations.

Notes

1. So where did Arago discover that matter? O. S.

2. No explanation offered. O. S.

3. Powell explains that Arago thought about the Keplerian laws. O. S.

4. No explanation offered; at the very least, Arago could not have thought about the then only emerging theory of errors. O. S.

5. Powell briefly describes the French participation, i. e., the first meridian arc measurements, and mentions Richer, see beginning of § 8 of the main text. O. S.

6. Perhaps I will be asked, why did I rank Lagrange among French geometers. Here, briefly, is my answer. The man called Lagrange Tournier whose both names were as French as it can only be imagined; whose maternal grandfather was Gros and paternal great grandfather, a French officer born in Paris; who always wrote in French and was acquiring high merit in our country during almost 30 years [that he had been living here]; who, although born in Turin, ought to be considered a Frenchman. F. A.

7. Powell also mentions Euler and D'Alembert. O. S.

8. Similar statements were due to Descartes (1637/1982, p. 48) and Bradley in 1750 while he was reporting his discovery of nutation, see Rigaud (1832/1972, p. 48):

Experiments become the more necessary the further we advance in our knowledge.

As we advance in the means of making more nice inquiries, new points generally offer themselves that demand our attention. O. S.

9. Certainly not *instead,* but *in addition*. O. S.

10. Here are these words: "If God had asked my advice when he created the world, I should have managed things much better". O. S.

11. Powell notes that Arago "imperfectly represented Newton's labours" and describes them. O. S.

12. Powell notes that Hooke preceded Newton and Huygens in stating that the Earth had a spheroidal form and that Newton began thinking about that fact in 1667 or 1668. O. S.

13. Powell describes Newton's assumptions and Maclaurin's proof of his statement. In connection with the former Todhunter (1873/1962, vol. 2, p. 34) referred to Laplace (1784). O. S.

14. By direct measurements Arago meant meridian arc measurements and (only for establishing the flattening) pendulum observations. The *excellent accordance* is certainly wrong as testified by the history of those measurements/observations. And local gravity anomalies have always been a serious additional impediment.

Powell explains the essence of the Clairaut theorem on the figure of the Earth. O. S.

15. Mortality tables have nothing to do with life expectancy of an individual. O. S.

16. Is there really any such connection between final causes and human interest? O. S.

17. No explanation provided. O. S.

18. This is somewhat careless since an equator is not a plane. O. S.

19. Libration of the Moon is its apparent periodic pendulum-like oscillation. O. S.

20. Todhunter (1873/1962, vol. 2, p. 34) refers to Laplace (1784). O. S.

21. As proved by Laplace, that thread is the universal attraction. O. S.

22. Powell describes how Laplace had proved that invariability and comments on the later findings of Lagrange and Leverrier. O. S.

23. See the excellent memoirs of Lagrange and Poisson. F. A.

24. I have not found any memoir published by Laplace either in 1773 or thereabouts; the other one is indeed Laplace (1784), see Todhunter (1873/1962, vol. 2, p. 34). Powell describes that work. O. S.

25. It is now known that Saturn has no less than 82 satellites. O. S.

26. See, however, below. O. S.

27. Powell notes that Adams and Plana had somewhat corrected Laplace's findings. O. S.

28. Powell describes that subject in detail. He did not, however, connect his note with the main text (did not insert an asterisk). O. S.

29. Powell notes that in 1663 Gregory preceded Halley. O. S.

30. Powell provides the pertinent results of Mayer, Laplace and Encke. O. S.

31. Polar motion, as it is now called, does exist, but it certainly is not considerable in Arago's sense. O. S.

32. In 1884, the prime meridian was chosen to pass through Greenwich. O. S.

Powell describes later work on lunar tables and mentions Burckhardt and Hanson. O. S.

33. Four satellites were initially discovered; now, Jupiter is known to have not less than 11. O. S.

34. The second case is apparently an eclipse. O. S.

35. Powell notes that the "second law" is a corollary of the first and discusses them in detail. O. S.

36. Nowadays, these figures ought to be corrected. Saturn is 770 times larger than the Earth; it has four rings distant from the planet itself by ca. 140 – 70*km* and their mean thickness greatly varies from 10*cm* to ca. 10*km*. The rings certainly rotate. O. S.

37. I have not found that memoir. O. S.

38. Although Arago does not mention Laplace's predecessors, Euler, Maclaurin and Daniel Bernoulli shared the prize of the Paris Academy of Sciences for solving its problem concerning the tides. And it was impossible to study them without accounting for *physical conditions*. In § 23 below Arago critically and briefly mentions Daniel Bernoulli. On the other hand, Wolf (1860) had not discussed that point. O. S.

39. See Laplace (1827) reprinted in the Supplement to t. 5 of the *Méc. Cél*., pp. 489 – 505. O. S.

40. Here and somewhat below I deleted Arago's description of the arguments of some of Newton's opponents. However, he failed to mention that Euler had comparatively late abandoned Descartes' vortexes and acknowledged *Newtonianism*. See Wolf (1860, p. 188) and Todhunter (1873/1962, vol. 2, pp. 138 – 139) who describes Euler's memoir presented in 1775 and published in 1787 in which there are no vortexes but universal attraction did not enter either. O. S.

41. Satellites with retrograde motion are now known to exist. Then, Arago's conclusion taken by itself is meaningless since all possible combinations of the 43 motions were equally unlikely. O. S.

42. No reference provided. Then, the first was not Buffon, but Daniel Bernoulli whose pertinent contribution crowned by the Paris Academy of Sciences appeared in 1735. O. S.

43. No reference provided. O. S.

44. Common for all three bodies, or for the two only? O. S.

45. Powell refers to Laplace's *Exposition*, Note 7. Arago wrongly stated (beginning of § 29) that he "concentrated all attention" on the *Méc. Cél.* O. S.

46. Arago did not mention Laplace's *Essai philosophique* (1814) which has the same relation to his *Théorie* as the *Exposition* to the *Méc. Cél*. Totally lacking mathematical formulas, it is difficult, especially for a layman, to grasp the meaning of some of its places. Then, the appearance of Quetelet's vividly written although superficial writings pushed the *Essai* into the background. O. S.

47. Pascal and Fermat did not formulate any general principles; they only actually applied the notion of expectation. O. S.

48. Laplace's passionate appeal (1819) to suppress the Lottery of France was unsuccessful. O. S.

49. Already in the 1830s the statistical method (and elements of probability) penetrated surgery. The same fact applied to meteorology even from the end of the 18th century, and Arago himself studied the influence of the Moon on terrestrial phenomena from 1833 onward. This proves that he treated Laplace's probability superficially. O. S.

50. The calendar of the French revolution pinpointed the beginning of their new epoch as 22 September 1792. On 1 January 1806 France returned to the generally adopted calendar. O. S.

51. Had it been otherwise, Europe including Russia and France in particular would not have lost a few million people in wars, most of them unjust. On a greatly larger scale, it would have been incomparably better had Stalin graduated from his seminary and become a priest. According to official statements, he was expelled for revolutionary activities but rumour had it that the real reason was his frequenting casinos and whorehouses. O. S.

Some Personalities Mentioned by Arago

Alphonso, King of Castille, 1040 - 1109

Bailly, Jean-Sylvain, 1736 – 1793, astronomer

Bayle, Pierre, 1647 – 1706, philosopher

Boulliaud, Ismael, 1605 – 1679, perceived that attraction was inversely proportional to the square of the pertinent distance

Borelli, Giovanni Alfonso, 1608 – 1679, physicist, mathematician, physiologist

Chappe, Jean-Batiste d'Auteroche, 1728 – 1769

Cook, James, 1728 – 1779

Doerfel, Georg Samuel, 1643 – 1688, theologian and astronomer

Encke, Johann Franz, 1791 – 1865, astronomer

Fatio de Duillier, Nicolas, 1664 – 1753, mathematician

Fleurien, Charles Claret de, 1738 – 1810, explorer, hydrographer, Minister of Navy. Did Arago indeed refer to him?

Gregory, James, 1638 – 1675, mathematician, astronomer

Hell, Maximilian, 1720 – 1792

Laval, Lottin de, wrote many books around 1830; I did not establish a book on optics

Le Gentil, Guillaume Joseph Hyacinthe Jean-Baptiste, 1725 – 1792, astronomer

Le Sage, Georges-Louis, 1724 – 1803, physicist

Leverrier, Urbain Jean Joseph, 1811 – 1877, astronomer

Mairan, Jean Jacques d'Orbous, 1678 – 1771, physicist, mathematician

Mason, Charles, 1728 – 1786, astronomer, geodesist

Mayer, Tobias, 1723 – 1762, astronomer, mathemaician

Pingré, Alexandre Guy, 1711 – 1796, astronomer

Plana, Giovanni Antonio Amedeo, 1781 – 1864, astronomer, mathematician

Powell, Baden, 1796 – 1860, mathematician, astronomer
Regiomontanus (Iohann Müller), 1436 – 1476, astronomer
Richer, Jean, 1630 – 1696, astronomer
Roberval, Gilles Personne de, 1602 – 1675, philosopher, mathematican
Rousseau, Jean-Jacques, 1712 – 1778, philosopher, composer

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Tude

Wolf, R. (1860), Daniel Bernoulli von Basel, 1700 – 1782. *Biographien zur Kulturgeschichte der Schweiz*, 3. Cyclus. Zürich, pp. 105 – 202. **S, G,** 39.

Jeff Loveland

Bufffon, the certainty of sunrise
and the probabilistic reductio ad absurdum

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In 1777 the naturalist Georges-Louis Leclerc de Buffon published an *Essai d'arithmétique morale* in the fourth *Supplément* to his multivolume chef-d'œuvre, the *Histoire naturelle*. In one section of the *Essai* he attempted to calculate the probability that the sun would continue to rise after having been observed to rise *n* days in a row. By performing a thought experiment in which a newly created adult observed the sun's movement over several days, he came up with odds of 2^n or 2^{n-1} to 1. This special case of the long-standing problem of measuring inductive certainty would end up being ridiculed in the 19th and 20th centuries by J. M. Keynes and others, but Buffon was not the only 18th century probabilist to treat it with seriousness. Applying the Bayes theorem and using a thought experiment prefiguring Buffon's to a remarkable degree, Richard Price came up with odds of 2^{n+1} or 2^n to 1 in his Appendix to the Bayes *Essay* in 1764.

Some years later Laplace (1774; 1814, p. xvii) calculated the odds as $n + 1$ to 1 by his own rule of succession. Unlike Price or Laplace, Buffon offered little explanation for his mathematical results. Accordingly, scholars have judged them *quite arbitrary*.(Todhunter 1865, p. 344) and mysteriously erroneous (Pearson 1978, pp. 193 – 194, 660) and even wondered if they were not merely miscopied from Price's text (Zabell 1988, pp. 175 – 177). Zabell (1997, p. 368) seems to abandon this theory and anticipates elements of my own argument.

Here I will argue that Buffon's odds for continued sunrise are understandable if erroneous and were not in all likelihood copied from Price. So numerous are the possible pre-1764 sources for Buffon's analysis that it is difficult to determine how exactly it developed, but its innovations were clearly modest with respect to philosophical and mathematical traditions that were already well established by the 1750s.

This is Buffon's argument (1777, pp. 52 – 53, my translation) followed by Price's for comparison [not reproduced]:

Let us imagine a man who had never seen or heard anything and examine how belief and doubt would be produced in his mind. Suppose he is struck for the first time by the sight of the sun; he sees it shine in the heavens, then decline and finally disappear. What can he conclude from this ? Nothing except that he has seen the sun, that he has seen it follow a certain route and that he no longer sees it. But this star reappears and disappears again the next day, This second viewing is a first experience which will make him hope to see the sun again and he begins to believe it could return, though he remains very doubtful. The sun reappears again and this third vision constitutes a ssecond experience which diminishes his doubt even as it increases the probability of a third return. A third experience increases this probability to the point where he no longer much doubts that the sun will return a fourth time … each day produces a probability and [their] *sum … creates physical certainty … We can thus always* *express his certainty numerically … and the same will be true of all other effects of nature. For example, if we wish to reduce the age of the earth and our experience to 6000 years, … the sun has risen for us … 2,190,000 times and … the probabilities of the rising the following day increase as the sequence* 1, 2, 4, 8, … or $2^{n–1}$,..

Buffon’s point was to establish a standard for physical certainty, different in his view from both mathematical certainty (where doubt is null) and moral certainty (where doubt is acceptable to the level of probabilities of 1/10,000 as suggested later by Buffon 1777, pp. 55 – 59). During the Enlightenment there were many other efforts to distinguish physical and moral from mathematical certainty, some of them quantitative. Iike Price’s very similar one, Buffon’s account is distinctive in featuring three elements not seen together in other previous discussions of the levels of human certainty. First, it uses a thought experiment involving an adult just created or *awakened* so as to lack all experience yet harbour potential for mature knowledge. Second, it depends on our expectations about sunrise. Lastly, it concludes that the odds for continued sunrise are 2^n to 1 within a factor of 2. But if discussion of non-mathematical certainty before Buffon’s or Price’s fail to conjoin all three of these elements, it is not hard to see where Buffon might have picked up each one individually in the philosophical literature of the century ending in the 1750s.

1. Adults without experience

The history of thought experiments involving adults without experience is intertwined with the history of sensationalism in the Enlightenment, for the purpose of imagining such adults was to show how knowledge originated in experience. For philosophers averse to far-fetched hypotheses real or imagined infants provided an alternative for illustrating sensationalism, but reconstructions of infanrs’ thoughts could be dismissed as implausible in their own right. Locke, the Enlightenment’s premier sensationalist, was wary of *conjecture concerning things not very capable of examination* (Locke 1690, vol. 1, p. 184). …

Locke does not deal with experience-free adults of the sort Hume (1748, p. 42) evoked:

Suppose a person, though endowed with the strongest facilities of reason and reflection, to be brought on a sudden into the world, indeed, immediately observe a continual succession of objects, and one event following another, but he would not be able to discover anything farther.

In France, thought experiments had flourished since at least Descartes’ time … Buffon himself (1749c, pp. 31 – 32; 1749a, pp. 364 – 370) reverted to the conceit twice in the opening volumes of the *Hist. natur*., first to prove that certain divisions of nature are imposed by experience, then to show generally how knowledge arises from sensory experience. This second thought experiment is pertinent to his analysis of sunrise in that its subject, an adult man without experience, is struck by the sun the first time he sees it and fears that it is gone forever after the first sunset. The *Essai* would merely prolong and quantify this early *experiment*.

Between 1751 and 1760 Diderot, Condillac and Charles Bonnet published their own thought experiments concerning adults without experience, all seeking to illustrate the preponderant role of the senses in shaping thought (Zabell 1997, p. 368). The accounts of Diderot and Condillac deserve special mention for they shed light on the matter of originality in such experiments. After presenting a series of thought experiments evolving blind and otherwise sensorially unusual people in his *Lettre sur les aveugles*, 1749, Diderot proposed considering each human sense in isolation (1751, p. 188). In 1754 Condillac published his book-length thought experiment in which a statue constructed internally like a human being is exposed to sensory stimuli to test their effects on her knowledge and thoughts.

2. The certainty of sunrise

Long before Buffon's *Essai* people evoked and analysed human knowledge of observed regularities such as the sun's movements. On an anthropological level it seems clear that the regularity of the sun's daily path is salient in human experience so much that the miracle of a stopped or straying sun is a common one in mythology worldwide. Conversely, the sun's constancy was cited even before Christianity as an indication of design in the universe, for example in Cicero (45 – 44 BC, Bk , NNo 15 – 17, pp. 52 – 53).

Sunrise as a specific aspect of the sun's regularity came to be focused on in the Middle Ages. In medieval discussions of the contingency of future events, the example of our apparent foreknowledge of sunrise was common. …

Pascal (1670, p. 400 – 401) used the example of our knowledge of sunrise to point out the fallibility of knowledge in general. Leibniz (1714, p. 707) cited it to illustrate the difference between the multitudes'*irrational* expectations and the *rational* expectations of the astronomer. On a more constructive note 'sGravesande (1724, pp.xl, liii),and Hume (1739, p. 124) used the *paradox* of sunrise to suggest that there might be a trustworthy middle ground between demonstration and ignorance. He (1724, p. liii, my [Loveland's] translation).even dramatized the possibility of uncertainty regarding sunrise in a way recalling later texts by Buffon, Condillac and Price:

Poor humans (?), *how deplorable your fate would be … if, upon seeing the sunset, you had to fear an eternal night*.

For our purpose, the most interesting discussion of the certainty of sunrise is Condillac (1754, pp. 292 – 293): his statue reacts to the sun over a number of days. Like Buffon's experienceless men, his statue focuses attention on the sun, worries that it is gone forever once it has set, and gradually grows confident of its continued returns as he observes it on subsequent days ,,,

His is the same narrative used by Buffon and Price some ten and twenty years later. The only major commonality between Price's and Buffon's texts that is not present in Condillac's is the mathematical one. Like others evoking the probability of sunrise before 1764, Condillac made no effort to quantify it.

Condillac was little-known in Britain in the 18th century and his *Traité* was not translated into English until 1930. For these reasons it seems unlikely that Price took his narrative from Condillac's similar one. Their common approaches are almost certainly a coincidence, and not a surprising one in an age concerned with certainty and experience and teeming with thought experiments and hypothetical adults in a state of mental *tabula rasa*. Buffon, on the other hand, could have read this section of the *Traité* although nothing proves it, but the example of Price shows that he did not need Condillac to inspire his thought experiment. In any case, his 1749 narration of an experienceless man confronting sunrise anticipates much of his own later account.

3. Quantifying certainty

As mentioned above, no author before Price and Buffon seems to have attempted to quantify the probability of sunrise, yet Buffon and Price both came up with odds similar to $2^{n+1} - 1$ to 1 to express their imagined subjects' confidence in another sunrise after *n* repetitions. To specify the exact odds given by Price or Buffon is difficult since both were ambiguous in presenting their results. Price hesitated between odds $2^{n+1} - 1$ and $2^{n-1} - 1$ to 1, see Price (pp. 405, 409 – 410) and Buffon (1777, pp. 52 – 53, 58 – 59) between odds of 2^n and 2^{n-1} to 1. Since many early probabilists rounded $x^y - 1$ to x^y the missing subtrahend in Buffon's results need not concern us. The only major difference is a factor of 2.

How Price arrived at his odds is clear A few pages earlier he applied the Bayes *first rule* to the special case of an outcome observed without fail over *n* trials and came up with odds of $2^{n+1} - 1$ to 1 to express the chance that the unknown probability *x* of sunrise exceeds 1/2. In modern terminology (Bayes and Price used Newtonian geometrical language) we have the sought probability as the ratio of integrals of x^n over [1/2, 1] and [0,1] equal to $(2^{n+1} - 1)/2^{n+1}$.

Yet when applying this result to the matter of sunrise, Price decided to neglect day number 1, apparently because in his conception of the thought experiment his subject observed the sun but not sunrise on his first day of existence. His subjects' first sunrise, in other words, took place on day number 2. Since Price was interested in calculating the probability of sunrise (*returns* of the sun) not the probability that the sun would be seen anywhere in the sky, his imagined man offered odds of $2^n - 1$ to 1 on the mistaken assumption that Price had not already removed the factor of 2 corresponding to the irrelevant first day of the thought experiment (Zabell 1988, pp. 176 – 177). This is a possibility but Buffon had a simpler way of getting his odds.

Unlike Price, Buffon offered almost no clues as to his reasoning. He (1777, p. 53) merely stated:

To count from the second day the sun rose, the probabilities of its rising the following day increase as the sequence 1, 2, 4, …, *or* 2^{n-1}.

Elsewhere in the *Essai* he appears to have assumed that the odds of continuation of an outcome observed *n* times without fail were 2^n, not 2^{n-1} to 1, a discrepancy suggesting that he too was ambivalent about the importance of the first day in the thought experiment. Nowhere however did he defend his results any more thoroughly than in the sentence above, whence the frequent charge that they are obscure and arbitrary. Perhaps he merely copied his formula from Price's text, but if Buffon was aware of Price's sophisticated Bayesian reasoning, why act as if his results were common-sensitive? At the least, failing attribution, he might have alluded to a proof too cumbersome to be reproduced. But he evidently considered his procedure transparent enough to require no justification, whether formal proof, namedropping, or proof by intimidation (?). Could he have had a intuitive shortcut for obtaining his odds? His work on probabilistic astronomy in the 1740s suggests that he did.

Buffon (1749, written in the early 1740s) advanced the notorious hypothesis according to which the earth and the other planets had been turned out of the sun by a passing comet. To make this idea credible he adapted an old argument for showing that the solar system was not organized randomly but rather in virtue of a dominant cause. Newton had made the same argument non-quantitatively, but Buffon was more likely inspired by Daniel Bernoulli's recent prize-winning application of probability to that question (1734).

Somewhat like Bernoulli Buffon calculated the odds that the six known planets would turn in the same direction because of chance alone … Unlike Bernoulli he forgot to specify that these odds only held if the sun's rotational direction was taken as a starting point, but this characteristic sloppiness need not concern us. What is interesting about the argument is its logic and the conclusions Buffon drew from it. On first glance the argument has nothing to do with the rule of succession Buffon later explored in calculating the probability of sunrise. In this example, Buffon like Bernoulli was simply engaging in a *reduction ad absurdum* of the premise that the planets' orbital directions were equipossible and determined by chance. While problematic in its application this kind of argument is not easy to understand. One assumes that a given outcome has a probability $1/p$ of being produced by chance alone, one notes that this outcome has been observed in all of n trials or cases, one calculates the probability of this having happened as $(1/p)^n$ and one uses this vanishingly small quantity as evidence that the premise is wrong (Bru 1988b, pp. 75 – 76).

In its pre-quantitative form, the argument can be traced back to at least classical antiquity, where Cicero (45 – 44BC, bk 2, NNo 15 – 17, pp. 52 – 53) for example cited the *ordered patterning* of the universe as proof that it could not have arisen by chance. Transformed by theologians into proof of God's providence, the argument became canonical in the medieval Christian church. In the late 17th century it acquired numerical precision, thanks to the development of quantified probability theory. But only in the 18th century was the newly *reductio ad absurdum* applied to observed regularities rather than merely hypothetical cases.

In 1710 [1712] John Arbuthnot published a paper based on the fact that according to records London had produced more boys than girls in every one of the last 82 years. Under the hypothesis of equiprobability the probability of this happening was $1/2^{82}$, a number too small. Arbuthnot reasoned that providence not chance had to be responsible for that effect. Soon afterward 'sGravesande (1774, pp. 220 – 248) refined and incorporated Arbuthnot's argument using a similar *reductio ad absurdum.* Informally quantitative, astronomers measuring the earth's shape in the early 18th century applied the same logic to prove that any errors in individual measurements would not, in all likelihood push the overall result in the same direction (Bru 1988a, pp. 223 – 235). In 1746, throwing himself into the longstanding debate over the origin of monsters (congenitally malformed beings) Mairan used the probabilistic *reductio ad* *absurdum* to prove that the four extra toes and fingers of a recently described human could not have developed by chance transposition from a defunct companion foetus. Crudely estimating as $1/10^{20}$ the probability that a *migrating* digit would correctly attach itself to the receiving foetus along with all the requisite connecting issues, Mairan (pp. 60 – 63) came up with odds of 10^{80} to 1 against the production of this monster bv *chance*. Maupertuis (1752, pp. 307 – 310) too applied probability to polydactyly arguing that the recurrence of six-digitism in certain families could not be due to chance. As a member of the Acad. Roy. des Sciences and a close friend of Maupertuis, Buffon was certainly familiar with these latter two arguments.

In principle, the probabilistic *reductio ad absurdum* served only to rule out the hypothesis of pure chance, but authors often used it to argue for the preponderant influence of favourite causes, whether these be providence (Arbuthnot and 'sGravesande) or genetic inheritance (Maupertuis). A more serious over-inference can be found in Buffon's work. The odds $(1/p)^n$ produced as the *coup de grâce* of the *reductio ad absurdum* show just one thing, namely that the premise is probably faulty. As a product of a proof by *reductio ad absurdum* they have no bearing on anything once their counterfactual premise is revealed to be erroneous. As his study of the planets' orbits shows, however, Buffon was not willing to limit this probability's meaning to merely disproving the hypothesis of equipossibility. Having calculated odds of 64 to 1 that chance alone could not be responsible for regularities in the planets' orbits, Buffon (1749, p. 134) concluded:

There is already 64 to bet against 1 that the planets would not have had this movement in the same direction unless the same cause produced it.

In other words, not content with the usual dubious leap from a hypothesis of blind chance to one of overall causality Buffon used the numerical byproduct of his *reductio ad absurdum* to assign a probability to his hypothesis of a single cause! To judge by his reasoning he seems to have believed that if a regularity with a prior probability of $1/p$ is observed over all of n consecutive trials the odds are $p^n - 1$ to 1 that a definite cause is behind the regularity. But if these are the odds that a cause is behind the regularity, it follows that the odds that the regularity will continue are the same. Applied to the problem of the probability of sunrise, this mode of analysis gives rounded odds of 2^n to 1 that the sun will continue to rise after n days, or alternatively 2^{n-1} to 1 if the first day is neglected for lacking a sunrise. These are exactly the figures Buffon gives in the *Essai*.

Perhaps Buffon was misled by the resemblance of his problem to other, more familiar ones in probability. In the well-established theory of witnesses, for example, it was routine to estimate the probability of past events as a function of the number and quality of corroborating witnesses. … … Filleau de la Chaise (1672) analysed the probability of testimony using the example of the Great London Fire of 1666 … The first terms of his sequence 1, 2, 100, 1000, ∞ may derive from some intuitive approximation of Buffon's reasoning. Other theorists of the juridical value of witnesses carried their own versions of this sequence of odds. All such sequences verge on being rules of succession since they not only give the probability of past events but also suggest the probability that additional future witnesses will continue to corroborate these events. Probabilists who assigned witnesses prior numerical value for reliability arrived at results looking even more like Buffon's. Typically they supposed that witnesses had a probability of being unreliable of $1/x$ and then calculated the probability of a fact witnessed by n witnesses as $1 - 1/x^n$. It is tempting to rationalize the certainty of sunrises as deriving from a series of n daily witnesses about whom we know nothing. That is, whose prior reliability can be taken as 1/2, but this logic is faulty since sunrise, the event witnessed, is not really singular. Nor does Buffon's narrative indicate that he was thinking in these terms. Still, his familiarity with solutions of this kind may gave lulled him into accepting the similar byproduct of his *reductio ad absurdum* as an evaluation of a real probability.

Buffon was not the first to invent a rule of succession linking past happenings with expectation. In addition to the indirectly predictive examples above, calculations of posterior probability by Jacob Bernoulli, De Moivre and others were frequently tantamount to rules of succession in limited contexts. Yet Buffon's rule, like Price's roughly contemporaneous one, stands out from its predecessors through its simplicity and generality. Bufon's mathematics may have been erroneous but he pursued it to the limit eventually arriving at a versatile precise rule for predicting the probability of events' future occurrence as a function of the regularity in past observation. His rule deserves remembering not just for the subtlety of its mathematical error (a lesson to everyone using *reductio ad absurdum*) but also as a counterpoint to Bayes' simple rule and as testimony to Buffon's constructive, sometimes sloppy scientific drive (Roger 1989, pp. 560 – 561), a drive perhaps heightened here by the opportunity probability offered for outstripping his old enemy, D'Alembert who discussed future cotingents.

4. Conclusion

It remains possible that Buffon borrowed from Price's treatment of the probability of sunrise just as it remaais possible that Price based his own treatment on Condillac's earlier one .As shown above, though such hypotheses are not necessary to account for the similarities of these philosophers' analyses. In any case, Bayes' *Essay* was apparently unknown in France before 1778 as Condillac's <u>Traité</u> was in Britain throughout the century (Bru 1998b, p. 77). Nor is it likely that Buffon, Price, Condillac or Hume got the idea of examining sunrise through the eyes of an experienceless adult rom a common lost source. On the French side, it is significant that no one mentioned any pre-1749 precedent for the similar thought experiments of Diderot, Buffon and Conillac in the quarrels over plagiarism of the 1750s. Citing a common predecessor (Hume, for example) would have ben a perfect means of discrediting a rival's claims to originality, yet none of the combatants did so., presumably because of their ignorance of any clear-cut antecedent. More generally, the evolution of discussions of the problem of sunrise from passing allusions in the 17th century to more intricate and eventually quantitative thought experiments in the mid-18th century suggests gradual development, not wholesale invention followed by simple copying. Intellectual borrowing certainly took place, especially within linguistic and geographic communities. Price's calculation of the probability of sunrise was a response to Hume's ruminations on the same subject (Zabell 1997, p. 368). Many of Buffon's friends , notably Diderot, Cramer, Maupertuis and Helvetius (Bru 1988b, p. 73) anticipated elements of his 1777 analysis of the probability of sunrise in their own writings of the mid-18th century, an indication, perhaps of influence and feedback. So prevalent however were thought experiments involving adults without experience in the mid-18th century and so natural was sunrise as a means of exploring the topical issue of non-mathematical certainty that relations of influence are hardly needed to explain similarities in the texts evoked here.

Though Buffon's *Essai* was only published in 1777, the section on sunrise may have been written before Price's similar treatment appeared in 1764. Much of the *Essai* consists of minimally edited reprints of material Buffon wrote in the 1730s and 1749s (Milliken 1965, pp. 180 – 181n), but some scholars have speculated that he edited and added to these older materials around 1760 to produce a coherent synthesis of his views of mathematics, probability, methodology and psychology (Gouraud 1848, p. 54n). Beyond its general plausibility, the main basis for his claim is a footnote in the *Essai* which quotes and replies to a 1762 letter from Daniel Bernoulli to Buffon (p. 57n). In addition, the volume's index relates the *Essai* to a text published by Ch. Paneckoucke in 1765 (Watts 1969, pp. 103 – 104). Finally, the *Essai* (pp. 55 – 58) proposes the probability of a 56 year old man dying in the next 24 hours as a standard for moral certainty [uncertainty]. Buffon, perhaps significantly, turned 56 in late 1763. On the other hand, his account of an *Adam* reacting to sunrise appears integrally connected with his interests in the 1740s. Not only do his calculations regarding the probability of sunrise mirror his and Bernoulli's regarding regularities in the planets' orbits, the whole reason for his calculations is to quantify the distinction between mathematical, physical and moral certainty, a distinction Buffon stressed most elsewhere, (1749c) and the earlier volumes of the *Hist. natur*. Certainly it is striking how much of the *Essai* is attached to Buffon's concerns from the 1730s and 1740s and how little it seems to have to do with the texts he was writing in the 1760s and 1770s. Perhaps the abovementioned references to texts of the 1760s in the index and footnotes should be interpreted as that the *Essai* was essentially completed *before* these related documents came to Buffon's attention.

In any case, by 1749 Buffon was familiar with fairly standard calculations for refuting certain hypotheses of equipossibility, and before the Bayes theorem came to anyone's attention, he had already concocted his own implicit rule of succession by erroneously giving positive value to the numerical byproduct of a probabilistic *reductio ad absurdum.* His only early discussion of his rule of succession neglects to apply it to future trials, presumably because there were no planets whose orbital direction remained undetermined, but it is hard to imagine he would not have made the leap from his law of causality to a true law of succession in a more appropriate context. Happily, an ideal context presented itself in his investigations of methodology, undertaken most conspicuously in the 1740s. Interested in the kind of certainty implied by repeated events and convinced that this certainty required probabilistic analysis by at least 1749 (Buffon 1749c, p. 62), Buffon would naturally have been drawn to the philosophically familiar *paradox* of sunrise. Already before 1749 he constructed two brief thought experiments based on adults without experience. The same mode of analysis would likely have suggested itself for a discussion of human knowledge of sunrise. Applying the common-sense probabilistic reasoning (1749b) Bufffon would have come up with the odds for continued sunrise given in the *Essai*. This, in all likelihood, is the origin oh his *arbitrary* calculations regarding the probability of sunrise.

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Notes

1. Laplace considered events which occur only in some trials. O. S.

2. The sum of these probabilities can exceed unity. The same remark applies to another case below. O. S.

3. See commentaries by Shoesmith (1985; 1987) and David & Edwards (2001, pp. 9 – 11). O. S.

4. This is too difficult to understand. O. S.

David H. A., Edwards A. W. F. (2001), *Annotated readings in the history of statistics*. New York.

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I left out much material which belonged to philosophy. The translator, if not named, is Loveland. The letters which sometimes accompany the year of publication are not always given and the bibliography is dated. Even the reprint of 1954 of Buffon's *Essai* is not included and the first names of the authors are unnecessarily shown. Loveland did not notice the close connection between the *probabilistic reductio ad absurdum* with the statistical null hypothesis so that it was possible to refer not to Cicero, but to Aristotle.

Methods for promoting research in the exact sciences, pp. 179 – 193

Carnegie Instn of Washington
Yearbook No. 3 for 1904, 1905

The Institution asked the opinion of several scientists about those methods. The reply received from S. Newcomb was sent out to them for comment. Newcomb's reply and comments follow.

Letter of S. Newcomb, May 12, 1904. Washington, D. C.

The following is a brief summary of views which I have at various times expressed to the officers of the Carnegie Instn or made known to the public. They embody my well-matured opinion as to the method by which the Carnegie Instn can most effectively promote research in the exact sciences. I begin by setting forth the main features of the situation.

1. The 19th century has been industrially piling up a vast mass of astronomical, meteorological, magnetic and sociological observations and data. This accumulation is going on without end and at great expense to every civilized country.

The problem of working out the best results from these observations is one which is not being effectively grappled with. The best methods of attacking the problem are little known to investigators in general, being scarcely developed in a systematic form. The result is that what has been done toward results consists largely in piecemeal efforts by individuals, frequently leading to no well-established results.

Another feature of the situation is the gradual extension of the principles of exact science into the biological and sociological field. It is through this extension rather than through addingto the already accumulated mass of facts, that progress is most to be hoped for in the future.

2. A consideration which I wish most respectfully to urge upon the Institution is the great advantage which comes from mutual discussion and attrition between men engaged in contiguous fields of work. My own work would have been much more effective could I have enjoyed this advantage more fully, and I am profoundly impressed by the waste of labour shown in an important fraction of current scientific researches through the authors not being acquainted with the best methods of work.

3. Under these conditions it still seems to me, as it has almost from the day the Institution was founded, that the most effective way in which it can promote research in exact science is by organization of an institute or bureau of exact science in general. If I had only my special field in view, I might simply suggest an astronomical institute; but it seems to me that this would be too restricted to get the best and most desirable results. I can not but feel it most important that exact methods should be extended into other branches of science than astronomy.

In defining the field of work in such a bureau or institution a division of physical and natural science into three great fields may well be borne in mind. One of these fields is that of the old-fashioned natural science, which is concerned very largely with morphology[1], physiology and vital processes which do not admit of reduction to mathematical forms.

Another field is that of purely experimental science. The third field which really needs development is that of observation, which I propose shall be now occupied. The work required is, in brief, the development of mathematical methods and their application to the great mass of existing observations. Doubtless suggestions as to experiment would frequently come in. These would be carried out by others.

4. The organization. The first requirement for the organization as a managing head in whom the Institution has entire confidence, who should be required to devote all his available energy to the work, and, in doing so, should act as the agent of, and be regarded as doing the work of the Carnegie Instn. He should be supplied with such office, appliances, and assistants as are necessary to commence work in that branch of the field with which he feels himself most conversant, beginning work on a small scale, to be enlarged and extended into neighbouring fields as success became assured. The opposite faults of beginning on too large a scale and of making no provision for possible expansion should both be avoided.

5. The head of the institute should be aided by a council comprising the leading experts best qualified to advise as to the various departments of work. This council might be international, and if the work of the institute is sufficiently expanded to justify it, should hold an annual meeting.

To secure the advantages of mutual consultation attrition, and cooperation, it may eventually be desirable that the work the Institution has already undertaken or is now promoting in the various branches of exact science should be merged with the proposed institute.

6. The institute should be started on a very modest scale. The case is one in which everything depends on correct methods from the beginning. By the adoption of these, results may be reached at small expense which, without them, would never be reached with any amount of labour. It seems to me that ten or fifteen thousand dollars would be ample for the expenses of the first year, as the number of employees who could be successfully put to work would be small. The principal appliances required would be books, but I think that three or four office rooms would suffice for all the purposes of the first year or two.

The expenses of subsequent years would depend upon the expansion which is found desirable to give to the work.

Appended hereto are letters on the subject from Prof. H. H. Turner of Oxford, and Lord Rayleigh, to each of whom I presented the question of the desirableness of working up the great mass of observations alluded to.

Letter of H. H. Turner,
Nov. 25, 1993. Univ. Observatory, Oxford

I have delayed answering your letter for a few days, not from any lack of sympathy with its general purport or doubt as to the value, the immense value, which such a scheme as you suggest would have, but because I wished to think whether I could contribute anything of possible importance to the discussion of details. The result has, however, not been very encouraging, and I must not delay longer a reply on the main point.

I imagine you will not find anyone to doubt the necessity of a far more extended discussion of results. In the days of Newton perhaps observations were scarcer than theories, and it was advisable to set them going. But, once set going, inertia had come into play, here as elsewhere, and observations of all kinds are churning out masses of observations which no one is attempting to deal with. There is no doubt whatever that it is a crying necessity that we should organize the discussion of the masses of accumulated material. The necessity extends beyond astronomy, to meteorology certainly, to natural history perhaps, though here the observations (metrical) are needed, as in astronomy in Newton's time.

How, then, to set to work to improve matters? I have no better plan than yours. Perhaps I should approach the subject from rather a different point of view. I should start with the proposition that the amount of critical discussion (discussion of any value) of results obtained is likely to depend roughly on the number of men of first-rate ability who can be enlisted into the service. For making observations a moderate ability may suffice, but there is no doubt about the ability required for discussing them and directing future programmes. Well, then, I fear it must come to this: That we want more positions of eminence, well paid or honoured or both, such as the leading professorships. When Schuster[2] gave his address, which you quote with approval, Dr. W. N. Shaw (head of our Meteorological office) remarked that meteorology had never had any professorships at the universities. (Is this also true in the United States?) and I think the remark went very near to a sufficient explanation of the lack of adequate discussion of results. You can get heaps of people to measure rainfall, but who is to think about the results? It is more thinkers we want.

Hence my proposition comes to this. Either

One. Endow more really first-class posts, such as will attract good men. It is no use getting youngsters into the science unless there is some prospect for them.

Two. Or look about for means for drawing into the work of discussion occupants of existing positions of repute who are now either wasting their time accumulating little-needed observations or are prevented from doing such work by the lack of machinery (i. e., of funds for getting computing done) for there is a good deal of computing attached to most discussions of masses of observations.

One could accordingly meet the present need in variety of ways. When you were over here[3] I was speaking of a calculating bureau (and you seemed to approve). This would follow from the second part of No. 2. If a man (like Sampson or Durham[4]) knew that he could get computing done pretty easily if he could arrange the details, he might be rendered efficient when otherwise his way would be blocked. The relief might be compared to that afforded in the matter of printing and publication which our societies have afforded and which the American observatories are finding in their bulletins and circulars. Before printing was easy much good work must have been lost.

But this is only one way of meeting the need and is practically included in your method, which includes, indeed (if I understand you rightly), all the elements I have sketched. At the head of your suggested organization you could scarcely fail to have at least one first-rate man which so far meets my point 1. You virtually meet the first part of 2 by establishing, instead of a new observatory to multiply observations, an organization of a new kind, which will set a good example to others, and the rest of 2 I have already considered. I have written truly my thoughts as they occur, and hope this letter is not too long and rambling. One cannot help, when these inspiring letters talking of new projects come from over the water, building a few castles in the air. One of my castles is a really critical astronomical journal for discussing the work of others rather than publishing our own. To some extent *Vierteljahresschrift des astron. Ges.* does this, but we could do with an English journal of the kind, and a better one. If you get your way perhaps this journal might be tacked on to the scheme.

Letter of Karl Pearson. June 14, 1904. Univ. College, London

I have put together a few suggestions that occur to me, principally based on my own personal experience, but I do not wish them to be considered in any way as dogmatic statements, only as impressions.

One. I agree absolutely with prof. Newcomb's first statement that the 19th century has industrially piled together a vast mass of astronomical, physical [meteorological!] and biological data and that very little use has hitherto been made of this material. The reason for this I take to be that a man of mediocre ability can observe and collect facts, but that it takes the exceptional man of great logical power and control of method to draw legitimate conclusions from them.

Two. Differing probably from Prof. Newcomb, I hold that at least 50% of the observations made and the data collected are worthless, and no man, however able, could deduce any result from them at all. In engineers' language we need to scrap about 50% of the products of 19th century science[5]. The scientific journals teem with papers which are of no real value at all. They record observations which cannot be made of service by anyone, however able, because they have not been undertaken with a due regard to the safeguards which a man takes who makes observations with the view of testing a theory of his own. In other cases the collector or observer is hopelessly ignorant of the conditions under which alone accurate work can be done. He piles up observations and data because he sees other men doing it and because that is supposed to be scientific research.

Three. I have had to deal to a great extent with the observations and data of other men in my statistical laboratory, to which applications are always being made for aid in the interpretation of observations. I think I might help to illustrate my point by citing a few actual experiences.

Meteorological statistics. We have here a large work in progress. The data are enormous, but without any system. Examination shows that in Europe and America the returns are often untrustworthy. There is no standardization of method, or time, or of quantity observed. Important stations are omitted or dropped for years, and where a well-organized plan for a quarter of the expense and labour would have led to definite results, the existing chaotic mass of data will only provide probabilities and suggestions. Any man with ideas on the subject of meteorology would after a little experience discard existing material and start afresh, or else waste his best years in trying to reduce material to a common measure which is really a hopeless s task.

Medical statistics. These are made by each medical man and each hospital on a separate plan and without any idea, as a rule, of the points which it is needful to observe that logical conclusions may be drawn. This is especially the case in inheritance of disease tendencies. Further, immense masses of material are wasted because one or other essential factor has escaped record in one or other series.

We have had to report recently on cancer statistics, lunacy statistics and inoculation for enteric fever statistics. Only moderately definite conclusions can be drawn, because the material has usually been collected without insight into the conditions requisite for drawing definite statistical results.

Physical measurements. The same applies here in perhaps a less degree, but still quite definitely. Observations on the strength of materials exist in immense quantities. These are largely of no value because the experimenters have had no clear prior idea of the points they wanted to elucidate. Further, this applies to a whole mass of physical observations which have been made without sufficient mathematical knowledge to realize the difficulties of the problem.

The failure on this account of physicists like Wertheim, Savant and Kupffer[6] in the first half of the 19th century is quite paralleled in recent work by men whom for obvious reasons it is better to leave unnamed.

Biological and sociological observations. These are of the lowest grade of value in too many cases. Even where the observers have begun to realize that exact science is creeping into the biological and sociological fields, they have not understood that a thorough training in the new methods was an essential preliminary for effective work, even for the collection of material. They have rushed to measure or count any living form they could hit on without having planned *ab initio* the conceptions and ideas that their observations were intended to illustrate. I doubt whether even a small proportion of the biometric data being accumulated in Europe and America could by any amount of ingenuity be made to provide valuable results, and the man capable of making it yield them would be better employed in collecting and reducing his own material.

It will be seen from the above results that I personally cannot form a very high expectation of the amount of results of first-class value which would be obtainable by forming an institute to deal with existing masses of observations.

Four. Nevertheless, if we reject 50% of existing observations as worthless, if we frankly scrap them, I still think something of service might be done with the remainder under certain conditions.

If the right man were available. This is the chief difficulty. He must be a man of wide appreciation of many branches of science, otherwise a special man will be wanted for each branch, astronomy, meteorology, physics, medical science, sociology etc. Even where the money forthcoming for that multiplicity of workers, I doubt whether the men themselves are to be found. If Prof. Newcomb's institute is carried out, the right man for director will be a man of very exceptional attainments, falling little short of scientific genius. I doubt if one man of this type cold be procured. It is certain that several could not.

The right man must have been rightly trained. He is to be occupied in drawing logical conclusions from other persons' observations and data. He must therefore in the first place be an adept in scientific method; he must be a first-class mathematician, statistician and a trained calculator and computator.

The right man must be rightly supported. He must have a competent staff of workers under him, and be to a considerable extent a man of affairs. He will have to reject after examination whole masses of observations and data as unsuitable, and his proceedings will be questioned and criticized. Unless he is a man of weight and tact, he will soon be in an impossible position relative to the mediocre observers whose data he is to manipulate. For example, he proposes to deal with the weights of the human viscera in health and disease.

He collects all the available data but issues his report and conclusions silently passing by the measurements of some well-known physician or hospital because they have been made in a manner which renders them of no scientific value. The result would be certainly controversy, possibly uproar, and the director of the institute should have to fight a series of pitched battles before his reputation as a censor and official scrapper was finally established beyond dispute. He might survive this initial state of affairs if he had the support of the best scientific minds in the country. But unless he was a strong man he would take the easier course and simply add another long series of reports on all existing material to the already overvoluminous scientific literature of the day.

The right man will be the man who has the courage to scrap and to do it relentlessly. Science wants immensely the courageous pruner today. But his is not an enviable task, and the Carnegie Trustees would have to support their man pretty steadily to enable him to be effectual. He will be sure to make some mistakes, and these will be at once seized on and trumpeted abroad. If we supposed that the above three conditions can be fulfilled, may we not question whether the man pictured would not be of such calibre that he would do far better work for science if he were allowed to use other people's observations where he chose and to observe and collect himself where he found them defective or incapable of throwing light on the branches of science he was peculiarly interested in?

In other words, the director would be reduced to an ordinary scientific worker placed in one sense under very favourable conditions, in another under unfavourable conditions. He would have ample material and support, but he would differ from an academic teacher in having no school wherein he might train his subordinates in his methods.

Five. On the whole, I doubt whether the founding of an institute to scrap and codify existing observations and scientific material is feasible if desirable. I am inclined to think that more might be done by a *Statisical and Computating Institute*. This institute should have a competent director and a highly trained staff. It should be prepared to report on any data or material submitted to it at a moderate fee. This fee might be remitted on the recommendation of the director, or a committee, in the case of first-class work from a man of scientific repute but small means.

It would have to be retained, however, to prevent a flood of worthless material being sent in to be reduced. The institute might also offer advice on the collection of material, on observational method and on statistical treatment, again charging a slight fee to prevent the institute being used as a source for providing research work for those who were too idle or too dull to discover such work for themselves. Besides, private individuals, learned societies, astronomical, meteorological, or biological, might and probable soon would use the institute to carry out special investigations on the value of material already amassed in some one or other branch of their special sciences. Finally, Government departments would very soon fall into the habit of asking for reports on the special material of their own spheres. The like course would be taken by local bodies in the case of demographic and other statistical material. I think that such an institute would be of great service and perhaps as far as possible fulfil the functions which Prof. Newcomb proposes, without that great amount of friction that a direct inquiry into the value of material collected by men, many of whom would still be holding scientific posts, would certainly involve.

Of course, one is far too apt to judge matters from one’s own little corner of the field of science. We have had a statistical laboratory established for some little time, and we find that an increasing number of workers send us their data for suggestion and report. To such an extent has this become current that we shall probably have either to institute a fee to check the flow of material or else decline to examine such work as we are only an academic department doing our own teaching and research work and without public support of any kind. Still our own small experience may be useful on the other side of the Atlantic, and we have found that a multiplicity of workers, physical and biological, want assistance, and further that public bodies and government departments seek statistical and calculating aid also. If Prof. Newcomb’s ideas were carried out first on material which was actually placed before the institute for report, then the action of scientific societies and public bodies would soon give the foundation an established position, from which possibly the more serious business of codifying and scrapping existing accumulations of observations and data could ultimately be carried out without too great friction and controversy.

Letter of Lord Rayleigh, Nov. 20, 1903.
Roy. Instn of Gr. Brit.

I am in complete sympathy with the views expressed in your letter and have indeed sometimes expressed myself in a similar sense. But my experience is far less than yours. I sincerely hope that you may succeed in organizing such an establishment as you indicate.

Letter of G. H. Darwin, no date. Newnham Grange, Cambridge

I sympathize very warmly with Prof. Newxomb’s plan for developing the Carnegie Instn and think that it may have a great future. I have been trying to picture to myself how it would work out, and I see that while the gain in some subjects would be great and immediate, in others it would be only collateral.

Scientific observations may be roughly classified in two groups, which however graduate into one another. I can best illustrate my meaning by examples. The subject of the tides seems to belong to the group which would reap immediate advantage. Observations are now published in the most diverse places and are not properly coordinated. A critical collection of tidal results would be a heavy task and would be of much value. There is nothing in this subject which corresponds to probable error in astronomy, for the defects depend on human frailty. It would require a first-rate man to classify and reject observations according to the internal evidence afforded by them. When such a collection was made, generalizations would follow, and the value of the conclusions would probably be great,

Meteorology and many other subjects fall into this group. The distinguishing feature is that we know exactly what to observe, that the mass of material is already enormous and that it is impossible to have too much matter, provided that it is coordinated.

The second kind of research to which I have referred is intermediate between observation and experiment. The subject of observation is to some degree indeterminate and it depends on the investigator what he shall observe. I cannot think of a very good example at the moment, but I may perhaps illustrate my meaning by supposing that we were investigating the laws governing the drifting of sand and the formation of sand dunes. It must be obvious that this is a subject of great agricultural importance in many parts of the world. Now, it would be almost useless merely to collect maps and photographs. There must be a guiding mind, forming theories to be proved or disproved by observation. The investigation might be expensive and troublesome, but it is essentially the work of an individual.

In this sort of case I should not look for any great gain from the proposed institution, except that it would afford a fixed position, with good pay, to men of ability. The exception is important, and it brings us to the point raised by Prof. Turner, viz, that the search for men is more difficult and more important than the search for facts. I hope that you will not regard this long letter as wide of the point, and in conclusion I desire to express my warm approbation of the scheme.

Letter of Arthur Schuster, Aug. 18, 1904.
Kent House, Victoria Park, Manchester

I will take Prof. Newcomb's s various points in order

One. There can be absolutely no doubt on the correctness of Prof. Newcomb's view regarding the piling up of a vast mass of observations, which has been made an object in itself, instead of being a means to an end, and hence a proper discussion has not been able to keep up with the accumulation of undigested figures. The efforts of individuals to discuss results have often been hampered by want of assistance or of funds, and in many cases have been doomed to failure owing to the fact that the men trained to observe are very often not particularly well fitted to draw conclusions. It would be easy to find examples of the waste of labour which has resulted from incompetent work in the planning of the methods of reduction[7].

Two. Here I also agree with Prof. Newcomb, and I would like to add another feature of the present situation which stands in the way of the discussion of great problems on a broad basis. The vast mass of accumulating material has rendered it necessary to have a special journal almost for each special branch of a subject. Thus we have a journal dealing with solar physics, and another with terrestrial magnetism, etc[8].

The mathematician and physicist who is probably most capable of dealing with the problems of solar physics and terrestrial magnetism often never sees thee journals. If he does he will get bewildered by the mass of detail which is put before him and often by technical terms which he does not understand.

What is required here is some intermediate agent whose business it should be on the one hand to place before the man of general science the main results of observations which want discussing and on the other hand before the observer the main facts and measurements which the theoretical student requires for his work.

The efforts which have been made to remedy this recognized difficulty by the publication of abstracts have, in my opinion, proved failures. To write efficiently an abstract which should give the pith of a paper in a form that can be utilized requires a very intimate knowledge of the subject. In a subject requiring special skill and training this cannot be expected from those who at present undertake work of this kind, nor is the frame of mind of the reader who takes up one of these journals of abstracts and endeavours to assimilate in half an hour the ideas of 150 different workers on 150 different subjects such as to make it likely that his thoughts will be usefully fertilized. A much more useful plan would be to have periodical reports dealing with the progress of the subject, but here again all will depend on how far it would be possible to get men who thoroughly understand the subject to write these reports.

It is doubtful to my mind whether the best results ever can be obtained by an observer who has not full grasp of what his observations will be used for. But, dealing with the question from a practical point of view, we must recognize that there are many men who can take excellent observations without any special power of discussing them, and it would be a pity not to make use of such men provided we can convince them of the limitation of their powers.

Three. An institute or bureau of exact science, according to Prof. Newcomb’s scheme, would in my opinion, prove useful, as it might in each subject find the best methods of coordinating facts and reducing observations. But the organization of the bureau would have to adapt itself to the different requirements of the different subjects. These requirements probably vary from time to time. In particular stages of a subject publication of a list of papers may be what is required, and in every case we must guard against stereotyping any one particular method of procedure. The abstracts which, as above mentioned, I found useless in my own subject might be very effective in others.

It would be, as Prof. Turner points out, a very material gain if there were a body of men whose special duty consisted in discussing observations and drawing attention to those matters where observation is most required. I consider the subjects included in Prof. Newcomb’s third *field* as requiring most attention at the present moment.

The bureau should, in my opinion, not only have power to initiate reductions, but should also be able to assist other workers in cases where its council approves of the proposed method. I may mention an example from my own experience. I have engaged during the last two years, at my on expense, an assistant to do certain reductions of sunspot observations by a method which, I believe, will give useful results in many branches of cosmic physics. It would have been advisable in any case that the first set of reductions by this method should have been carried out under my own supervision, but supposing the results arrived at to be valuable and the method to commend itself to competent judges, it would be quite beyond the powers of any individual to extend the calculations to include other phenomena such as prominences or magnetic disturbances, which can be brought into connection with sunspots. The bureau, with funds at its disposal, and a committee of directors who could judge of the value of any proposed piece of work, might prevent a block in the advance of science which is at present possible for want of a proper organization.

Four and five. I quite agree that everything must depend on the nomination of a managing head, although an advisory committee will probably be necessary and it can only be through the organizing powers of a man who is at any rate thoroughly qualified in one branch of science that the work can succeed.

Six. I also agree that the Institute should be started on a modest scale. If it is desired that the council should be international, I would suggest that the International Association of Academies[9] should be asked to nominate a certain number of its members. As this association has been founded for the purpose of international cooperation, it seems desirable to strengthen it as far as possible and to avoid the multiplication of other international organizations. I do not, however, wish to express an opinion at present on the desirability of starting the bureau at once on an international basis. It might be better to secure greater elasticity by leaving it, in the first instance, to be an American institution. If desirable, it will always be easy in a few years' time to ask the International association of Academies to nominate members on its council.

I am sorry there has been so much delay in sending you this reply, but, as I have already informed you, I was unusually busy when your letter reached me.

Letter of Edward C. Pickering, July 27, 1994.
Harvard College Observatory. Cambridge, Mass.

The plan in general meets with my hearty approval. There is no doubt that a proper discussion of existing observations is very much needed. This should be followed by suitable observations to supply the wants thus rendered evident.

To select subjects for the proposed institution a permanent council might be needed, but when a subject was chosen, specialists in that department of science should be employed, who would spend several days together arranging the details of the work. According to my experience, a discussion of generalities by a committee with no means at their disposal is unsatisfactory and the results are of little value. A number of experts, however, having an appropriation which they could expend on work with which they were entirely familiar could get much better results, than any one person alone. The officer in charge of the proposed institution, with his corps of computers, could readily carry out the plan of work recommended consulting the committee when difficulties arose, or calling other meetings as required. A large part of the laborious work involved in discussing an extensive series of observations in any department of science could be done to great advantage by such a permanent computing bureau.

It is often impossible to transplant a man of genius in other surroundings without greatly diminishing the value of his work, and it is better to improve his existing conditions rather than try to make him adopt new ones. On the other hand, he is often unable to discuss his own results or supervise large routine computations as well as one who devotes his life to such work. My views on this subject are given more fully in a pamphlet entitled *The endowment of astronomical research No. 2*, which will be distributed in a few days.

Notes

1. Morphology studies the form and texture of animals and plants and its scope is very wide. It is also a branch of linguistics.

2. In 1898 – 1908 Schuster published papers devoted to the treatment of astronomical, meteorological and magnetic observations. See his Letter below.

3. Newcomb rather often visited Europe and met there with many scientists.

4. In 1913 and 1918 Sampson published papers on the theory of errors but I cannot say anything about Durham.

5. Apparently 50% of *observations*.

6. I only found Kupffer, see below.

7. Reduction can be understood as a preliminary study of data. In astronomy, reduction of observations means their correction for refraction, nutation etc. and their transfer to a certain epoch.

8. In May 1999 there were tens of thousands of scientific journals (Prokhorov 1999, p. 893). The circle of readers of many of them was certainly small.

9. This Association existed in 1899 – 1913. Strangely enough, as it followed from Schuster's letter, its members were individuals rather than academies.

The scientists mentioned

A. Ya. Kupffer (1791 – 1865), physicist

E. C. Pickering (1846 – 1919), physicist, astronomer, director iof observatory. Extremely influential.

H. H. Turner (1861 – 1930), astronomer, seismologist, foreign member of Paris Academy of Sciences.

I do not know whether the Carnegie Instn took any measures in line with the proposals made. I am utterly dissatisfied with the discussion: the authors of the Letters passed over in silence the existng since the 17th century journals which were at least partly devoted to the publication of abstracts; only Turner mentioned one such periodical. Mikhailov (1975) indicated many such journals and, in turn, I mention the *Jahrbuch über die Fortschritte der Mathematik* (1868 – 1942, 66 volumes).Many authors indicated that the current of observations was unimaginable (for example, Lueder (1812, p. 9) and De Morgan (1915, vol. 1, p. 85).

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Guido Rauscher, Oscar Sheynin, Claus Wittich

The Correspondence between E. E. Slutsky and V. I. Bortkevich

Finansy i Biznes, No**.** 4, 2007, pp. 139 – 154

The published Russian text of this Correspondence contained many formulas which I am now unable to reproduce. The present text became less interesting but, as I really think, is still useful. O. S.

We publish the extant letters of the correspondence between Evgeny Evgenievich Slutsky (1880 – 1948) and Vladislav Iosifovich Bortkevich, or Ladislaus von Bortkiewicz (1868 – 1931) that constitutes a part of the latter's posthumous archive kept at the Manuskript & Musik Abteilung of the Library of Uppsala University, Kapsel 7 (Sweden) and recently discovered by Guido Rauscher. Slutsky partly, and Bortkiewicz completely adhered to the (Russian) system of spelling drastically changed in 1917 – 1918. It is perhaps noteworthy that there are no extant letters written by Slutsky from Moscow after mid-1926, – when the political situation in Russia began to worsen drastically and his own circumstances became precarious (Sheynin 1999/39, p. 129). True, he did continue to correspond with Western colleagues such as Ragnar Frisch, see Chipman (2004/42).

Bortkiewicz' letters are obviously drafts. Their reading is difficult and we were unable to decipher some words; such cases are denoted by [?]. Then, he crossed out many lines, sometimes not clearly enough and in many cases did not write out words completely. Some words and expressions in Slutsky's letters are underlined (here italicized), but there are cases when this was done very crudely, most likely by Bortkiewicz, and we have underlined rather than italicized the relevant words and expressions.

Among other topics, Slutsky dwelt on logical and philosophical issues connected with statistics, and it is opportune to note (Chetverikov 1959/32, p. 272/ 2005, p. 158) that in the mid-1940s he

even with some irritation refused to discuss purely logical concepts although he had been unable to disregard the then topical criticism levelled by Fisher against the problem of calculating the probabilities of hypotheses (*of the Bayes theorem*)*.*

In his letters, first from Kiev, then from Moscow, Slutsky invariably indicated his address: Nesterovskaia St. 17, flat 8, and Mashkov St. 17/15 (by N. S. Chetverikov, Chuprov's closest student), respectively.

Bortkiewicz is known to have corresponded with Slutsky since the latter (Letter No. 3) had adopted a term suggested by his colleague. That they exchanged letters from time to time was not, however, ascertained, and only recently Bortkiewicz' correspondence with Ptucha and Chetverikov came to light (Bortkevich & Chuprov 2005/12, p. 10), also see Note 25. Actually, although having lived in Germany for 30 years (and about seven years before 1897), Bortkiewicz had retained ties with Russia (Sheynin 2001/40, p. 228; Bortkiewicz & Chuprov 2005/12, pp. 9 – 12).

Here is an excerpt from a letter of Chuprov to Bortkiewicz of 13.2.1923 from Dresden (Ibidem, p. 250) which apparently led to the correspondence between the latter and Slutsky:

I have recently received a letter from Ptucha. […] *I also received a letter from Evg. Evg. Slutsky, again from Kiev. He had been in Moscow, attended the stat. conference, and obtained there my address from Chetv. He tells me, among other things, that a mathematician from Central Asia* [Bortkiewicz remarked here: Romanovsky] *read out a report in which he arrived in a similar way at some of my results which I had published in Biometrika. Amusing! It would be good if you will be able to send him some of your reprints, and especially Homogenität etc. He has again returned to math. stat. Delivers a course and is working himself in that field. Laments the absence of recent literature. It would be possible to send them through his relative N. Wolodkewitsch, Parkstrasse 4* [?] *Berlin-Südende*[1].

In the sequel, many formulas are missing. They are included in the Russian text of this contribution (Борткевич – Слуцкий) and it was too difficult to insert them here also.

Letter No. 1. Slutsky – Bortkevich. Kiev, 20.7.1923

Dear Vladislav Iosifovich!

I received two of your works, *Homogenität* [1918] and *Die Variationsbreite*, 1st part [1921] and hasten to thank you. You have thus rendered a real good deed for me. In Kiev, at the Institute for National Economy, I am delivering lectures on theoretical statistics and you know from M. V. Ptukha how much the most recent literature is absent here.

I would like to hope that, should I ask you to send me reprints of your future works, I will not abuse your kindness too much. Incidentally, being very interested in this subject, it is very important for me to have your paper *Das Laplacesche Ergänzungsglied und Eggenbergers Grenzberichtigung* [1920]. Choosing some parameters, I myself have recently managed to discover an expansion of the hypergeometric series. Regrettably, I do not know whether it is new.

I am sending you a reprint of my paper in *Vestnik Statistiki*; please accept it as a token of gratitude and profound respect from the sincerely devoted to you E. Slutsky. 17.7.1923

Allow me to thank you once more. You are unable to imagine how did you gladden me by sending your reprints.

Letter No. 2. Bortkevich – Slutsky. Berlin, 31.7.1923

Dear Evgeny Evgenievich!

I thank you very much for the reprint of your report [1922] and the letter [Letter No. 1] of the 20th inst. I fully agree with you in that the theory of probability should be constructed as a branch of pure mathematics quite independently from the logical problems connected with the concept of probability in its proper sense, but I do not deny that much can be expected from a change of the name. Your construction seems to adjoin that of F. A. Lange (*Logische Studien*

[2nd edition, Iserlohn, 1894]) who issued from the concept of disjunctive opinion (*Disjunktionsurteil*).

It was a pleasure to ascertain that you deny the identification of probability with limiting frequency. On that point you will find something in my review [1923] of Keynes' *Treatise on Probability* which I have sent you yesterday together with three other reprints. I regret that I am compelled to ask you to mail me back, when opportunity offers, that review and the paper on Laplace – Eggenberger [1920]. To a certain extent, *Variationsbreite und mittl. Fehler* [1921] can serve as an ersatz of the second part of the paper *Variationsbreite beim Gaußschen Fehlergesetz* [1922]. […]

Owing to disagreements between the publisher and the printing house, I did not receive its reprints at all. At one time, I have sent M. V. Ptukha my paper *Der mittl. Fehler der zum Quadrat erhobenen Divergenz-Koeffizienten* [1918]. I regret that only one of its reprints is still available. There, in footnotes on pp. 108 – 110, you will find remarks of a fundamental essence which will possibly somewhat interest you. Two books have recently appeared: Czuber (1923) and Urban (1923).

If you happen to see M. V. Ptukha, please thank him on my behalf for the four copies of his mortality table for the Ukraine which he had sent me.

Your problem and its solution are very enjoyable. I do not know whether anyone had considered it previously. Until now, I have not yet made out the initial formulas since I did not have time for properly grasping them.

I think that Prof. Mises will not refuse to publish a paper about that problem if only you will send it to him.

I apply Mises' address.

Letter No. 3. Slutsky – Bortkevich. Kiev, 25.9.1923

Dear Vladislav Iosifovich!

Please excuse me for delaying the answer to your kind and cordial letter, but these latest weeks I was head over heels in work, and wished to write without haste. And, in addition, I received your letter only about four weeks ago upon returning to Kiev from the village where I had passed a part of the summer.

I can not say how thankful I am to you for sending me your papers about Laplace – Eggenberg[er], Helmert, Keynes and *Variationsbreite* [1920; 1922; 1923; 1921]. For me, everything is extremely interesting. I am now leaving for three weeks on a scientific trip to Moscow, for working in libraries. After returning, I will first of all write out the necessary excerpts from those papers which you asked me to return (Laplace – Eggenb. and Keynes) and immediately mail them to you.

I like very much your term, *disjunctive calculus*; it never crossed my mind, but now it seems so natural! The name is of course of lesser rank which does not mean unimportant. Not without reason so much events had happened only owing to a single iota. *Name* is a great cause, as a mystic and metaphysician would have it. This, however, is probably music for the future, although that transformation of the probability calculus, about which I used to dream, is perhaps not so far off. Perhaps I will yet see your penned and published *Disjunktionsrechnung* (disjunctive calculus).

Concerning the problem about which I wrote to you, I will thankfully avail myself of your kind advice, – to send Mises for publication my manuscript as soon as I manage to process it. This summer I performed three thousand trials under differing conditions for each thousand. I was curious to investigate small bodies shaken up all together and then forming themselves in a ring, – curious to find out whether their size and shape influence the equal probability of their possible arrangements.

I used a round box with a dome-shaped elevation at its bottom, so that my pea pods had to arrange themselves in a ring. Before making the experiment, I thought that the difference in shape and [and/or?] size will be of no consequence, but I was somewhat mistaken. I have taken quite different pods: two very small and round, two rather larger and absolutely flat, two still larger, oblong and rounded, and four almost spherical, and the deviations from theory were quite definite (two experiments with a thousand of such pods). But the third thousand with pods of *approximately* the same shape although differing in size much more than those dice with which probabilistic experiments had been made, that thousand showed a remarkable coincidence with the theory.

In general, I think that in my experiment, as I arranged it, the shape and the size of small bodies ought to influence its results much less than under other conditions about which I managed to find out. Incidentally, I never heard nor read about the use of automatic self-registering devices in *experiments in the theory of probability*. And it seems that if such experiments can be scientifically important, they should be performed independently from the investigator's patience. Judging by myself, I say that the experiments severely tempt it.

I shook my box and out of boredom invented a machine in my mind that could have replaced me. It seems that I succeeded (certainly, in a sketchy way): a machine which can be able to shake and calculate. Apparently, for the Bufon [Buffon] problem it will not be difficult either to construct such a self-registering device.

As to the theory of my problem, I did not want to burden you with possibly quite uninteresting considerations and had not written out the derivation of the formulas. Now, I also fear to drag out my letter too much; however, since you have apparently decided, as I understood, to know how I arrived at my formulas, I venture to describe briefly the idea of the derivation (not in detail which would be too long).

Please excuse me for being too diffuse and be assured of my most sincere devotion. Profoundly respecting you E. Slutsky

Letter No. 4. Slutsky – Bortkevich. Kiev, 24.2.1924

Dear Vladislav Iosifovich!

At long last I was able to send you those two papers (about Keynes and Laplace – Eggenberger) which you have asked me to return. Once more I thank you most cordially, but please do not reproach me for delaying them for so long. By no means could I have mailed them earlier.

With the same registered book-post I have sent you my works published during that time: 1) [1923a], 2 copies; 2) [1923b], 5 copies, and the same in Ukrainian from the *Izvestia Vseukrainsk. Akad. Nauk*; 3) [1923c], 1 copy; 4) [1923d], 1 copy. The last-mentioned paper summarizes the last but one and concludes it by issuing from unpublished materials. I wrote No. 3 at the request of my friend, Prof. L. Yasnopolsky, as an appendix to his work on money circulation during the revolutionary epoch. I had to compile it more hastily than I wished, and it is therefore longer than necessary.

With the return of M. V. Ptukha from Germany there came a breath of lively Western impressions, a few more threads re-establishing the earlier torn fabric of contacts. Books are appearing, sets of journals for the last ten years are ordered. In a few months we will thus become to a certain extent Europeans.

Keynes interested me very much. When reading your paper, it was extremely pleasant to feel at one with you. However, concerning a detail, I would not have reproached him for *überraschend engherzige Auslegung des Ausdrucks Form* [for a sudden unimportant interpretation of the expression form; Bortkiewicz (1923, p. 6)].

I would wish to talk to you about a subject which interests me for a long time, although I did not have enough time for going in it as deep as it was necessary. Even when writing a review of Kaufman [1915 – 1916], I have expressed the idea that, since each method is based on some theory, the statistical method is based on applying either statistical theory, or some other theoretical science.

Then, I have chosen the first alternative. Now, after thinking about your considerations in *Die Iterationen* [1917], I do not feel my choice shaken, and the more I think about it, the firmer I become convinced in that conclusion. Allow me to issue from your objections to the expression *statistical physics* etc. You point out (p. 4) that physicists use the appellation *statistical* when considering subjects which are not concerned with an *actual* count of elements. But is this essential? A triangle remains a triangle both when we find and apply it in empirical reality, and when we study it in extra-empirical *reality*. A physicist is engaged in physics when arranging an experiment, and when solving an abstract problem formulated by tentative *suppose we have* (masses, forces, electrons etc). The logical essence is obvious: the essence of the thing (*Wesen*) is independent from the modality of existence as well as from our considerations or assumptions about it.

Then, when the physicist says: Let us suppose that *n* molecules having such-and-such velocities are given in some volume, it means that we suppose that counting would have provided *n* units of observations having a definite distribution of the size of a certain indicator. If actual count is a statistical operation, then an assumed count is the same, only indeed assumed, just as imaginary murder or theft are murder or theft, only indeed imaginary. […]

The subdivisions of theoretical statistics are given by other indicators which can be conjugated with the constitutional indicator and its logical derivatives leaving indefinite the species of the elements composing the totality. Those will be *arrangement, time and case*, in that order. We arrive thus at this subdivision:

Statistics

1. Sylleptik	2. Stochastik
1.1. Sylleptik in its proper sense	1.2. Sylleptical kinematic (?)
1.1.1. Horistik 1.1.2. Syntagmatik	

For me, it is unclear whether it will not be better to restrict the meaning of the term *Sylleptik* to its proper sense and I do not know what appellation can be devised for *sylleptical kinematic*. (I have chosen this term as an *ad hoc* expression, the first one to come to mind.) I would resolutely oppose *Bevölkerungssylleptik* [Sylleptik of population] since it does not have the needed logical purity except the case in which Bevölkerung is understood as a *terminus technicus* like *population* is for British statisticians. That, however, is hardly good enough either, because *population*, as they understand it, has no bearing on time. I do not deal with subdivision of *Stochastik*. In the logical sense, it is definitively dissociated from the calculus of probability; and, when agreeing with your appellation, *disjunctive calculus*, it is terminologically dissociated as well.

The concepts of statistical method, statistical technique, applied statistics (of population, fixed stars etc.) then follow quite naturally from the concept of statistics as a theoretical science, which, as such, justifies a special method, and, together with a number of applied subjects, substantiates a special technique etc.

For me, the most unclear point is, how to justify the demarcation between Sylleptik and *Mengenlehre* [set theory]. You mention this as something quite certain, but, regrettably, I can not say so about myself. I will be much obliged to you for at least hinting at this aspect.

One more consideration. If my point of view is rejected, I will insist that the usual statistics (without applying stochastic viewpoints) be called applied Sylleptik. This, as it seems, is the only logical attitude towards terminology. However, since the term *statistics* becomes discarded, it can be used for providing a common appellation to Sylleptik and Stochastik and thus preserving a habit. And we have thus come to the same conclusion.

Be assured of my profound respect and devotion. Your E. Slutsky

PS. I will thankfully avail myself of your advice about sending Prof. Mises my paper on the probabilities of cyclic permutations of pairs of identical elements. The manuscript is quite ready, but it is necessary to wait a little yet.

Letter No. 5. Slutsky – Bortkiewicz. Kiev, 24.7.1925

Dear Vladislav Iosifovich!

Allow me to thank you whole-heartedly for the sent reprints *Zweck und Struktur einer Preisindexzahl* I, II, III [1924]. This summer, I intend to study them with great interest. Although it does not excuse my belated response to your kindness, I ought to say that all this spring and summer up to this day I feverishly worked on a rather lengthy paper in probability theory, *Über stochastische Asymptoten und Grenzwerte* [1925]. I obtained some new results, to say nothing about treating a series of problems from the standpoint of the concept of limit in the stochastic sense which seems to be interesting. As to the st.[ochastic] limit, I found later that the priority belongs to Castelli, but apparently no one had previously formulated the notion of *st. asymptote*.

Be assured of my absolute respect and devotion. E. Slutsky

Letter No. 6. Slutsky – Bortkevich. Kiev, 31.12.1925

Dear Vladislav Iosifovich!

I apologize for sending you so tardily my latest work (*On the law of large numbers* [1925]), but to a large extent this was occasioned by outside considerations. I have read your letter with heartfelt joy since I cherish each rare contact with you.

It became impossible to correct in any way my work that had then been published, but please note that, when criticizing your views which I have allowed myself, I was mostly issuing not from the *Krit. Betr*. [1894 – 1896], but from the *Iterationen* [1917]. There, as it seemed to me, a certain standpoint was expressed absolutely distinctly. However, in such profound problems it is unimaginably difficult to find a completely adequate formulation; and I readily admit that certain nuances have escaped me.

Be assured of my absolute respect and devotion. E. Slutsky

Letter No. 7, Slutsky – Bortkevich. Moscow, 16.5.1926

Dear Vladislav Iosifovich!

Your letter did not reach me in Kiev and was sent to me to Moscow. I have now moved there because of *some discord with the Ukrainian language*. I would like to hope that you will generously excuse me my such belated answer. In a new place, with a range of new duties, it was difficult to make up my mind, then urgent work had come up etc. I am a consultant of the *Conjuncture Institute* and work there together with N. S. Chetverikov, and am living in his place until getting the promised apartment. And I also had to *take consulting duties in the Gosplan*. <u>I do not teach</u>. The situation and state are very unusual and felt as something transitional. Only Allah knows what will actually happen.

In spite of the barely hope lately left, it is difficult and painful to write about A. A. Chuprov's death. Chetverikov had certainly informed you already how our statistical family had endured it, and what we suppose to do. I was not close to A. A., but I cannot forget his subtle delicacy and indispensable readiness to render help in scientific work. With utmost thanks I remember his attitude towards my work *Über stochastische Asymptoten* … [1925] whose first and brief sketch I had sent him in 1923 asking his advice about publication.

Indeed, I was then completely cut off from foreign literature. Without his insistent advice, I would have hardly brought it up to that more complete version in which it is now being printed. However, only now, looking over Chuprov's letters, I see clearly how unimaginably did he delicately avoid in his critical remarks any hints at possible extensions and widening of the subject. He did not want to touch anything that should have been suggesting itself so as not to prompt me about something to which I should have come myself.

I have been recently repeatedly thinking about our disagreement and especially about the proofs of my paper in *Metron* [1925] which will appear in their next issue. There, Chapter 1 somewhat more briefly covers approximately the same range of problems which is discusses in my Russian paper *On the law of large numbers* [1925] […].

I was unable to make any essential changes since it was impossible for me to study anew the Poisson text which could have altered my point of view. With respect to the second point of our discord, namely, about my understanding of your concept of *mean probability of invariable composition* [see Letter 5], I could have written much more, but am extremely afraid of abusing your attention. I will only say that the problem seems to have two sides. a) How extensive is that, which you have established concerning the cases depending on the *mean prob. of invar. comp.*, and on the mean prob., as I call it, of arbitrary composition. And b) Can we find an indication that you are taking into account the *mean prob. of an arbitr. comp.* in your text itself.

In autumn, *Chuprov* had written me in his last letter that he *does not agree with me*, that he thinks that the problem is solved by the expression of the mean square [?]. In part, this remark is absolutely correct, but, since being covered by item (a), it misses my criticism. Indeed, the *mean prob. of an invariable composition* in its proper sense and the *mean prob. of an arbitr. comp*. have much in common, especially when compared with the *mean probability in its proper sense* [Durchschnittswahrscheinlichkeit im eigentlichen Sinn]. The point is, however, that, according to my belief, it seems to be absolutely impossible to find either in the *Krit. Betr.* [1894 – 1896] or *Iterationen* [1917] that you have foreseen and allowed for the case under my consideration. In both these sources there are places which objectively contradict that. I allow myself to note at least this [1917, pp. 54 – 55]:

[Poisson, however, thought to construct such a probability-theoretic pattern which would have corresponded with real events, namely, with irregular changes of random causes. The pattern of mean probability of invariable composition, however, is its *exact antithesis* since here the participating values of the probabilities (p_k) *enter the mean in established proportions*.]

Feststehende Proportionen [established proportions] indeed means that the selected values p_1, p_2, … are somehow restricted. The logical sense of the phrase absolutely rules out the idea that the numerical values of the consecutive magnitudes in the series p_1, p_2, … are absolutely arbitrary. It is impossible indeed to say *feststehende Proportionen* with respect to a series the numerical values of whose terms would have changed without any rule, or, for example, according to the rule

1/10, 1/10, …, 1/10 (m times); 1/2, 1/2, …, 1/2 (m^2 times);
1/10, 1/10, …, 1/10 (m^4 times); 1/2, 1/2, …, 1/2 (m^8 times) etc.

And I allow myself to think that the problem about the objective sense of the text, of *what* is objectively inserted into it and what any objective researcher can find there, leaves no doubt. For me, the quoted place from the *Iter*. seems decisive. Excuse my categorical expressions if I am mistaken, if you decide that I had missed something. I always readily admit my mistakes both in such cases and in print.

I am now sending you two of my works. One of them is that which you had formerly helped me so kindly to place in v. [von] Mises' journal, its turn had only now drawn up [1926]. The other one dates back even to 1915. Its proofs, sent to me during the war, did not reach me, and I have only now obtained five copies. I am sending you one of them. In that work, as it seems to me, I was able to add something essential after [Irwing] Fisher, Edgeworth and Pareto. I do not know when I will be able, if at all, to return to those subjects. It is the more annoying since I have shelved almost ready manuscripts … but everything depends on that *almost*.

Having made a short trip to Kiev in Eastertime, I brought back my reprints, and am now able to send everything possible to E. S. Altschul about whom you have written me. I will do it with great pleasure, will have to excuse myself for being unable to send everything.

Sincerely devoted to you E. Slutsky

Letter No. 8. Slutsky – Bortkevich. Moscow, 19.5.1926

Dear Vladislav Iosifovich!

I am allowing myself to add some considerations to my previous letter since I wish very much to attempt to ascertain my idea to you as best I can, leaving aside Poisson and in general all the history of the problem.

Be assured of my absolute respect and sincere devotion. E. Slutsky

Letter No. 9, Bortkevich – Slutsky. Berlin, 4.6.1926

Dear Evgeny Evgenievich!

I have received both your letters of May 16 and 19 [NNo. 7 and 8]. Concerning the difference between the mean prob. in the proper sense and of invar. composition, I am keeping to my previous opinion and do not see there any contradictions. Indeed, I discuss that in connection with the problem of the variance of a statistical series, and is interesting insofar as in the first case the measure of the variance, i. e. the sum of the squared deviations of the number of the occurrences [of the studied event] from its exp[ectation] [here, Bortkevich crossed out *square of the mean square error*] = *npq* whereas in the second case …

I do not know either when I will manage to read your paper [1915], although the problem there considered interests me. I have recently returned to it, but studied it in a much less intricate formulation. I am very glad that you were able to place one of your researches in Mises' journal. He would like to publish a note on the late Chuprov not longer than one page (two columns) [here, Bortkevich crossed out the following: I allowed myself to name you since I thought that you will be able quite successfully] and would be very grateful to you for that. I hope that you will not refuse, and, according to Mises' wish, will deliver him your manuscript in the *nearest* future. Bresciani will write [an obituary?] in the *Giornale degli Ec*.

A friendship lasting 30 years connected me with the late A. A., and for me each meeting with him was a festive occasion. It is difficult to reconcile myself with the notion that he is gone. Thank Chetverikov.

Bortkevich attached his rough calculations concerning Slutsky's letter. They had to do with estimating variance. In a covering text he mentioned his unpublished manuscript of 1914.

Letter No. 10, Slutsky – Bortkevich. Moscow, 14.6.1926

Dear Vladislav Iosifovich!

I will consider it my duty to write about Chuprov for Mises. I am informing him that my paper [1926b] will be ready not later than in two weeks.

I read your remarks about our discussion with utmost interest, but do not wish to abuse your attention anymore. Perhaps some time we will be able to meet and talk about many topics. I wish very much that that will happen.

If, however, your interests and occupations will eventually turn to the subject of my paper in the *Giornale* [1915], and you will scan it, you will certainly not refuse to write me a few words. I would have now written the end of this paper in an essentially different way. A supplement suggests itself. Namely, for defining uniquely the function of utility (up to an additive constant), it is not necessary to demand …

I have already read Nik. Serg. Chetverikov's copy of your paper with utmost interest. My copy did not yet reach me, but I will certainly get it, and thank you cordially for sending it.

Devoted to you Evgeny Slutsky

Letter No. 11, Slutsky – Bortkevich. Frankfurt/Main, 29.9.1928

Dear Vladislav Iosifovich!

After the Bologna congress and a short trip over Italy, I came to Germany for passing here about three weeks. In Frankfurt I found out that I am able, if you will not consider me conceited, to congratulate you from the bottom of my heart on your sixtieth birthday and to convey my very best wishes.

I would be very glad if you allow me to visit you when I come to Berlin. This happens, as I think, in the middle of next week, Wednesday or Thursday (October 3rd or 4th). For me, it would be delightful.

Profoundly respecting you and sincerely devoted to you Evgeny Slutsky

Notes

1. Mikhail Vasilievich Ptukha (1884 – 1961), demographer and historian of statistics. On Chetverikov see Note 18.

Nikolai Nikolaevich Volodkevich, or Nikolaus Wolodkewitsch, born 1888, was a brother of Slutsky's wife, Iulia Nikolaevna. He remained in Germany, and in 1932 earned a doctorate in physics at the Technical University of Darmstadt and later worked in the field of food technology and testing (in Turkey for a period in the 1930s, then again in Germany). German publications in his name appeared at least until 1959.

2. In 1923, Slutsky published two papers in that periodical and in 1924 he sent Bortkiewicz reprints of both of them (Letter 4). Here, however, he was obviously bearing in mind his article (report) of 1922 [15], see Letter No. 2.

3. Later Slutsky (1926a [22]) published the solution of this problem which he also discussed in Letter 2. For his expression *law of small numbers* (a few lines below), introduced by Bortkiewicz and then in vogue, read *Poisson distribution*.

4. In his published paper (1926a [22]), Slutsky named the biologist who prompted him to solve the described problem. His name (in German) was M. W. Tschernojarow, but the first who had considered the same problem was, as Slutsky believed, S. Navaschin who had offered its solution in 1912, in a paper published by the Imperial Academy of Sciences (Petersburg). Slutsky, however, expressed reasonable doubts about the result of his predecessor. Slutsky's formulas from his letter to Bortkiewicz are repeated in his paper of 1926, but formula (3), which is there numbered (16), see p. 153, is corrected as is, rather insignificantly (see same page of the paper) his table.

Vilenkin (1969 [28], p. 165 – 169; 1971, pp. 127 – 130; 1972, pp. 94 – 96) solved a particular case of this problem for *m* = 0. After simple calculations, his answer for *s* = 6, given in another form, provide the same figure as Slutsky's table did.

5. These remarks specified the notion of equally possible favourable cases. In one of them Bortkiewicz noted that "uniform randomness" can be absent in an urn problem with the tickets being extracted and returned back.

6. We can only mention Ptucha (1928 [31]).

7. See Slutsky (1922/1960 [15], p. 20) where he used this term (disjunctive calculus) and referred to Bortkiewicz.

8. The celebrated Buffon problem of 1777. A needle falls upon a set of parallel lines equally distant one from another; required was the probability of its intersection with one of the lines. This problem decisively introduced geometric probability into the theory of probability.

9. Nikolai Alekseevich Kablukov (1849 – 1919), zemstvo statistician and economist, Professor at Moscow University, Editor of *Statistichesky Vestnik.*

10. Edmund Husserl (1859 – 1938), a German philosopher, founder of the philosophical school of phenomenology.

11. It was Bortkiewicz (1917 [2], pp. 4 – 5) who (unsuccessfully) proposed the terms *Sylleptik*, *Horistik* and *Syntagmatik*, deriving them from the Greek. It is now generally known that, after referring to Jakob Bernoulli, he also reintroduced *Stochastik*. Already Wallis, in 1685, had applied the expression *stochastic* (iterative) *process* and Prevost & Lhuilier, in 1799, had used it in a probability-theoretic context (Sheynin 2017 [41a], p. 60, Note 1).

12. The Russian term is theory of *probabilities*; here, however, Slutsky used the singular number.

13. *Modo Bernoulliano* was an expression coined by Romanovsky in 1922 (Sheynin 1990 [37], pp. 50 – 51). Slutsky himself (1925a [20], pp. 2 – 3, Note 3) mentioned Romanovsky in connection with the notion of *stochastic limit* (see above). There also, on his next pages, he explained the difference between it and the concept of limit in analysis and quoted a relevant although not altogether distinct (as he himself remarked) statement by Poisson. However, it was Laplace who expressly noted that difference in 1786 and, less definitely, in the beginning of Chapter 3 of his *Théorie analytique* (Molina 1930 [48], p. 386).

Slutsky (1925a [20], p. 14) also explained that he adopted the term *stochastic asymptote* since the pertinent notion resembled the concept of asymptote in analysis as describing the behaviour of two functions.

14. See Bortkiewicz (1894 – 1896 [1], 1894, p. 650). There also, on the next page, he introduced *mean probability in the strict sense*, see Letter 7.

15. The German paper and the Russian contribution were apparently Slutsky (1925b [21]) and Slutsky (1925a [20]) respectively.

16. In the sequel, Slutsky explained the meaning of the first two symbols whereas the last one, as the reader will see, can actually be left without explanation. For this reason, after unsuccessfully scanning Chuprov (1918 – 1919 [34]) and Chuprov (1918 – 1919 and 1921 [35]), we prematurely abandoned here our attempt at finding it.

17. Slutsky did not master the Ukrainian language, which by a compulsory decree of the time was stipulated for all the lectures offered in academic institutions of that republic (Chetverikov 1959/1975 [32], c. 268; 2005 [32], p. 154).

18. Nikolai Sergeevich Chetverikov (1885 – 1973), Chuprov's student especially close to him. Worked in agricultural statistics, and on index numbers. Spent four years (apparently in 1931 – 1935) in prison as a *saboteur* and in 1937 or 1938 was subjected to new repressive measures (in any case, was banned from living in big cities) (Anonymous 1995 [27]).

19. This statement somewhat contradicts the previous description of Chuprov's advice. Slutsky (1925b/1960 [21], p. 26, Note 2) had also publicly expressed his gratitude to Chuprov. There also (p. 27, Note 2) he favourably noted that Chuprov (contrary to Markov's opinion!) applied the term *random quantity* (as it is called in Russian) "as the basis of the whole construction of theoretical statistics".

20. Slutsky (1915/14) is of course the paper on rational consumer behaviour on which Slutsky's fame in economic theory is based. It was published in Italian in one of the few European economic journals open at the time for contributions with mathematical content. In this work Slutsky developed further some ideas from his 1910 master thesis, as well as earlier contributions by Francis Y. Edgeworth (1845 – 1926) and Vilfredo Pareto (1848 – 1923). Slutsky's main achievement was to prove mathematically that under certain assumptions the consumer's reaction to a price change (*price-effect*) can be separated into two independent and additive effects: (a) an *income-effect*, related to the level of consumption and (b) a *substitution effect*, pertaining to changes in the structure of consumption. The so-called *Slutsky Decomposition* has become an integral part of every economics syllabus today.

Owing to its appearance in Italy in the middle of WW-I, the essay remained unnoticed at the time – even the author, as this letters shows, received reprints only in 1926, and then only five. While one of these rare items went to Bortkiewicz, Slutsky sent another one almost simultaneously to Ragnar Frisch (1895-1973), the Norwegian economist (in 1969 the first laureate of the Nobel Prize in Economics) with whom he corresponded between 1925 and 1937 (this copy was recently found among Frisch's papers in Oslo). Although both recipients were pioneers of mathematical economics, it took another ten years before Slutsky's merits were finally recognized by various European and US-American scholars, who had derived the same results partially independently and who all were significantly involved in the further development of modern consumer theory – among them Sir John Richard Hicks (1904 –1989) and Henry Schultz (1893 – 1938). Even then, the first translation of Slutsky's paper into English did not appear until the early 1950s (see Slutsky 1952/25a), and the first Russian translation had to wait another decade (see Slutsky 1963/25b). The story of the discovery and impact of Slutsky's paper in Western economic literature during the 1930s is related in Chipmen and Lenfant (1999/51) and (2002/52).

Slutsky's master thesis, *Theory of Marginal Utility* (in Russian) is kept at the manuscript section, V. I. Vernadsky National Library (Kiev), Fond I, No. 44850. Its Ukrainian translation appeared in Kiev in 2006. There, on p. 56, Slutsky's letter of 27 March 1919 to the Rector of the Kiev Commercial Institute is reprinted stating that he submitted his article in English.

21. Eugen S. Altschul (1887 – 1959), a scholar of Latvian origin. Chuprov (1922/1960 [36], p. 424) mentioned him in passing in one of his reviews. In 1925 Altschul was living in Berlin and his main occupation was somehow connected with banking (Bortkiewicz & Chuprov 2005/[12], Letter 199). In 1926, in a conversation, Chuprov (Letter 211) favourably referred to Altschul the statistician.

Altschul had remained in Germany after his studies in Freiburg, Leipzig and Strassburg and a 1912 doctorate. After a long period of work in property administration, banks (see Chuprov's remark above) and economic journalism, in Berlin in 1923 – 1926 (where Bortkiewicz might have known him), Altschul was in mid-1926 called to head the newly-founded Frankfurt *Gesellschaft für Konjunkturforschung*, where from 1927 he also taught conjunctural research methods at the university. Slutsky may thus have been asked to provide information about the Moscow Conjunctural Institute. Altschul was dismissed from his Frankfurt appointments after the Nazi seizure of power in 1933, emigrated to England in the same year (William Beveridge helped him to a research appointment at LSE) and then to the US, where he worked until 1939 at the National Bureau of Economic Research (supported by Wesley Mitchell, whose *Business Cycles* he had translated and published in German in 1931) and later taught at various universities, including U. of Minnesota and the University of Kansas-City, Missouri. He died in 1959 in Kansas-City. See Hagemann & Krohn (1999 [45], Bd. 1, pp. 4 – 7).

22. More precisely, npq is the variance not of a "statistical series", but of the number of occurrences of the studied random event having constant probability p of its occurrence in a single trial, $q = 1 – p$, and n is the number of independent trials in the series. David (2001 [43], p. 227) noted that Fisher (1918 [44], p. 399) had introduced the term *variance* in its modern sense and Bortkiewicz was possibly one of the first to use its translation (*dispersia*) in Russian.

Bortkiewicz discussed the subject-matter of this part of his letter not only in 1894 – 1896, but also in his contribution (1917 [2], §2.2). Concerning Bortkiewicz' notation g_λ (below), Slutsky (1925a [20], p. 20) explained that it was the number of times that the probability p_λ was attached to the occurrence of the studied random event. Bortkiewicz called the sum of the terms $p_\lambda g_\lambda$ the mean probability of a constant composition.

23. The few last lines (after the words *measure of variance,* which were thus left senseless) were obviously deleted.

24. Slutsky (1926b [23]) is his obituary of Chuprov that Mises had indeed published.

25. We are unable to say in what connection C. Bresciani (Bresciani-Turroni) is mentioned here. In 1908, he objected to Gini (Bortkiewicz & Chuprov 2005 [12], Letter 88) who denied the law of small numbers. He then translated into Italian at least one of Bortkiewicz' manuscripts on the same subject (Letter 91) which appeared in Gini's *Giornale* in 1909. Later, he thought of reviewing Chuprov's *Очерки* (*Essays on the Theory of Statistics*, 1909 and 1910; posthumous edition, 1959), see Letter 123 of 1913, and, finally, in 1925 he helped Chuprov to obtain a visa for travelling to Italy (Letter 210).

26. We can only say that in 1924 – 1927 Chetverikov corresponded with Bortkiewicz and, in September 1926 (Bortkiewicz & Chuprov 2005 [12], Note 178.2) informed him that Maria Smit (a notorious hard-liner) became the leading figure at the *Vestnik Statistiki* periodical, and he added: "The conclusions are obvious". In other words: the era of obscurantism had *in general* set in. A Black Sun had risen, as Mikhail Sholokhov wrote somewhere on quite another occasion.

27. See Slutsky (1927 [24]).

28. In Bologna, Slutsky participated in the work of the Congress of Mathematicians, see Chetverikov (1959 [40], pp. 269 – 270/2005, pp. 155 – 156 and Note 9 on pp. 163 – 164). Seneta (1992 [50], p. 30) published an English translation of a letter written by Slutsky to his wife during the sittings of the Congress. There, as also Chetverikov reported, he described his encounter with Cantelli concerning the authorship of the strict law of large numbers. One of us (O. S.) had received the text of this letter from Chetverikov and sent it to Seneta (Sheynin 1993 [38]).

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Е. Е. Слуцкий, E. E. Slutsky

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Tude

Aleksei Vladimirovich Postnikov

The Second World War: prehistory, events, lessons
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Abstract. On the basis of publications of the last years, especially those by Alexey Vladimirovich Solovyov and family archive of the author representing correspondence with V. M. Konstantinov in the years of his stay in camps of GULAG (Chita and Khabarovsk), and also communication with him after his release, some facts which aren't covered in official documents of investigations are restored. The conclusion is drawn on need of further studying of activity of scientists, engineers and technicians for prison's special organizations ("sharazhkhi") and their contribution to a general victory of our country in the Great Patriotic War of 1941 – 1945. The author offers to consider officially such employees as Veterans of the Great Patriotic War.

First of all, the author heartily thanks all those who studied the dramatic destiny of V. M. Konstantinov, and in the first place, the remarkable enthusiast Aleksei Vladimirovich Soloviev from Chita whose work formed the basis of this paper.

Our original sources were the unavailable to others materials of the family archive (mostly of the correspondence of M. A. Postnikova and V. M. Konstantinov, 1944 – 1956), and the stories of the father. They sometimes essentially differed from other written sources, and in particular, from the evidence of the investigation which A. V. Soloviev had used.

V. M. K. was born in 1903 to the family of a Japanese scholar, from 1919 professor at the Irkutsk University, Oriental department, historical -philological faculty of humanities. M. M. Konstantinov (1882 – 1938). V. M. used to tell us the following romantic story. According to the family legend, the grandfather of M. M. was a shaman (witch doctor) who left the taiga at the age of sixteen, and at 38 became professor at Petersburg University. Then followed that story of his love for Countess Stroganov from the coast-dwellers.

In spite of her father, that love brought about a happy marriage, the source of a talented family of Siberian intellectuals. Their representatives distinguished themselves in humanities and geological and geographical sciences.

M. M. K. graduated from a teachers' seminary in Irkutsk, then from the Oriental faculty of Petersburg University. From 1903 he participated in the social-democratic movement as a Menshevik. After the February revolution of 1917 he was one of the leaders of the Mensheviks in Irkutsk, editor of two newspapers. In autumn of 1919 he was elected chairman of the Irkutsk city Duma, filled various positions in Soviet establishments, headed the editorial-publishing department of the Far Eastern secretariat of Comintern, participated in the publication of a journal. From 1925 he was dean of the Far Eastern faculty of Moscow Institute for Oriental studies[1].

The family of V. M. K. was remarkable in that all the children from their most early age spoke with their parents in Russian, English and Japanese. And V. M., in addition to this unique possibility of studying languages, was endowed with surprising musical and linguistic aptitude. By the age of 30 he mastered six languages (Japanese, Chinese, English, German, Spanish and French).

Incidentally, in 1936, with the beginning of the civil war in Spain, those responsible ordered him to study Spanish in two months. In addition, he freely translated texts from Latin and ancient Greek.

After graduating from the Irkutsk Commercial School he studied at the Oriental department of the Eastern Siberian University, graduated from the Musical School (violin).Then, until 1921, he participated in the war of the Far Eastern Republic against the Japanese interventionists. After the Civil War ended he studied in the Moscow conservatoire, but his friends from the All-Union Communist Party (Bolsheviks) took into account his militant past and knowledge of Japanese and insistently recommended him to enter the Moscow Institute of Oriental Studies.

After graduating from the Japanese Department, Diplomatic Faculty, of that Institute he was at once sent to Japan. There, he worked at the Soviet embassy and continued his scientific work, graduated from a special university in Tokio as a specialist *in ancient Japanese and history of Japan*.

The Direction General of Intelligence (GRU) connected him with the Richard Sorge (1895 – 1944) group. During his short arrivals in the Soviet Union he studied in the Frunze Academy of the General Staff of the Army, special faculty (Intelligence).

In addition, also by the order of those responsible, he attended a correspondence course in aviation technical equipment at the Leningrad Polytechnic Institute. In 1933, V. M. was transferred to Moscow, and in 1935 was appointed assistant chief of the Oriental branch of the Administration of the General Staff of the army (GRU)[2].

In 1933, the head of the Administration, Jan Karlovich Bersin, real name Peteris Janovich Kiusis, 1890 – 1938, decided to send Sorge to Japan. He charged V. M. K. to familiarise Sorge with the peculiarities of life and work in the Land of the Rising Sun. Konstantinov secretly met the future legendary intelligence agent in Moscow, Harbin and Paris, passed him his experience and knowledge.

In 1933 Sorge married a graduate of stage art Ekaterina Maksimova[3]. V. M. K. used to say that, because of absolute secrecy of that marriage he was the only one in GRU who was charged with delivering E. M. Sorge's moneys as well as his letters in German. He translated them to her since she did not know that language.

In 1935 V. M. again reconnoitred in Tokio as First Secretary of the Soviet embassy[4]. He worked and studied in the academy until his arrest on 20 Aug. 1938. He spent two years under horrible investigation during which (according to the protocol of the investigation) he was subjected to *measures of physical pressure*.

Even during his investigation he translated a Japanese document which mentioned the dates of the German attack of the USSR. This fact was taken into account when his case was considered by NKVD [forerunner of KGB]. On 24 June 1941 he was sentenced to 20 years of detention.

During the war he worked in special organisations for prisoners (sharashkas) in Chita (1941 – 1942). There, he shared the same cell with A. L. Kletnev [see end of the document], then in Khabarovsk, decoded Japanese secret military documents.

In February 1946 V. M. K. retracted his testimony of 1938 when being subjected to measures of physical pressure. In July his term of detention was shortened by five years[5].

Remaining a prisoner, Konstantinov essentially contributed to the preparation of documents for the trial of Japanese military perpetrators (Dec. 1949). In Nov. 1952 he was freed before the appointed time but had to remain in the Khabarovsk territory for a few years as a reviewer of Japanese and Chinese documents. In 1956 he was demobilised because of illness.

He came to Moscow as a junior scientific worker at the Academic Institute for Oriental studies. Until the end of his life (1967) he worked there and defended his doctor thesis written on the basis of a translation and study of a Japanese source of the 18th century (*Dreams about Russia*). He discovered it in the Lenin State Library.

It is a manuscript chronicle of Japanese sailors who spent eight years in Russia after a storm cast them ashore in Kamchatka. Initially he defended a candidate thesis, but on the proposal of academician I. I. Konrad (1891 – 1970) it was unanimously admitted as a doctor thesis.

Konstantinov died on the fourth day after returning from his first (after rehabilitation) trip to his beloved Japan. During his last years he was preparing a commented translation of one more Japanese source of the 18th century.

In his letters to M. A, Postnikova (1944 – 1950) V. M. informed her about his repeated appeals to the highest authorities with a request to revise his case, but received no answer.

In 1950 A. L. Kletnev, his comrade, teacher and friend, was deported to Khabarovsk. Here is a passage from a letter of V. M. to Postnikova No, 58 of 15 March;

My old and best friend came here. I have not seen him for almost eight years. You do not know him, he was an instructor at the Academy. I helped him to settle down, then he fell ill. He is twelve years older than I am, and I try to help him. … I was unable to snatch time and write to you. Do not be angry.

The meeting of these old friends was apparently a real celebration of their preserved spirit with tears in their eyes. After returning to Moscow, V. M. told us a little bout Kletnev, but certainly without naming him. The most surprising in his story was that, while being in Khabarovsk labour camp, V. M. K. knew about a remarkable incident in the life of that Oriental scholar, intelligence agent and, at the same time, a repressed worker of GRU.

The essence of that incident, the details of whose documental evidence became known only recently owing to the studies of Aleksei Vladimirovich Soloviev, was the following.

During battles from 16 Aug. to 17 Sept. 1945 Kletnev was a member of an operational group of the local KGB. He was estimating the value of captured Japanese intelligence and counter-intelligence documents and revealed more than 700 intelligence agents in China and USSR[7].

V. M. told us that, upon learning this, he became sure that Kletnev will be freed as soon as he returned from Manzhoulia. This however did not happen, and after five years these comrades met and in spite of their inhuman conditions of life in GULAG continued their merciless intellectual struggle against the enemies of their fatherland.

We and M. A. Postnikova escaped the destiny of relatives of *enemies of the people* since the marriage of V. M. with M. A. was not and could not have been registered. Indeed, at that time V. M. had an official family (wife, Elena Aleksandrovna Konstantinova, a translator and author of Russian – Japanese dictionaries, and two daughters, Maia and Irina). According to the custom of the day, that family had cruelly suffered; wife did ten years in labour camps, and daughters lived in misery in special establishments opened for children of enemies of the people.

At that time, in spite of the terrible risk for an officer of the Red Army and mother of a juvenile son, M. A. Postnikova tried to find the address of the labour camp where V. M. did time, After all, she found it and in 1944 began corresponding with him.

During the last years of his detention I joined that correspondence with great pleasure, imported my ideas and feelings with a witty and subtle man. For me, he officially remained Uncle Vladimir. Long before his arrival in Moscow, when Mother told me the truth, I guessed that he was my father. Only after rehabilitation on 26 May 1956,

V. M. K. became fully free[8]. He returned to Moscow with his *prison wife* from 1943, Serafima Konstantinova. She did much to complete the work and immortalise the memory of the prematurely died V. M. K. And during the short period of our personal contact (1954 – 1957) he gave me very much, especially in improving my knowledge of history and English,

I describe now some facts from our correspondence and talks with V. M. K. They were not reflected in the materials which were available to A. V. Soloviev or other authors. I think that my information will assist future studies of his life and activities.

An official cover for V. M. K. as a military intelligence agent was his service in the Soviet embassy in Japan. This cover certainly did not mean that he never engaged in intelligence activities. Obviously because of the need to preserve top state secrets he was unable to tell much about it but in his talks with me he mentioned some incidents.

The first incident was connected with Soviet preparation to resist Japan in Manzhouli and Mongolia. On 12 March 1936 USSR and Mongolia signed a Protocol on mutual aid, and in 1937 units of the Red Army were deployed in Mongolia. In the summer of 1938 Soviet and Japanese units clashed for a fortnight near lake Khasan. The USSR was victorious.

On the eve and during that conflict, as V. M. K. testifies, he engaged in intelligence work in those two countries as a practical training for the Frunze military academy. His purely European appearance did not hinder V. M. K. since, according to his legend, he was an Ayn from Hokkaido who look like Europeans.

As dismal irony had it, he was arrested as a Japanese spy almost at once after returning from that task. This arrest prevented him to bury his father or even to read the obituaries in the Soviet main newspapers. In his letters to

M. A. Postnikova from the labour camp he asked her to send him these newspapers (as he thought, during June or July 1938) with the obituaries.

Mikhail Mikhailovich apparently did not know about the tragic fate of his son, but his mother, Iulitta Nikolaevna, née Nikolaeva, fully experienced the sorrow of a mother of a wrongly condemned son. V. M. K. wrote to Postnikova that his mother died in 1943. In concluding my essay about Father I allow myself to quote, after A. V. Soloviev, the renown Oriental scholar, Academician of the Russian academy of natural sciences, doctor of historical sciences, professor I. A. Latyshev.

Recalling the names of Japanese scholars of the senior generation who had worked in the Academic Institute of Oriental Studies in the 1950s, I consider it my duty to mention, at least briefly, the remarkable expert of Japanese history and culture, Vladimir Mikhailovich Konstantinov. He came to the Institute in the second half of the 1950s. …

In the beginning of the 1930s V. M. had full possibility to become a star of the first magnitude of the Soviet Japanese scholarship alongside Konrad and Nevsky. Indeed, only a few of our compatriots had a chance to study in their youth in a prestigious Japanese university and fully master Japanese.

Destiny, as it seemed, carried V. M. on its spread wings. Living a few years in Japan as a worker in the office of the military attaché at the Soviet embassy, he at the same time completed a course in a prestigious Japanese university. Butt in the ill-starred 1938 the wings of destiny proved treacherously unreliable. For no reason the mighty hand of the terror smote one of the most experienced experts on Japan.

V. M. came to that Institute in 1956 after 18 years of life in Siberian labour camps. He entered the unfamiliar milieu of scientific workers without losing interest in his speciality with a great store of accumulated knowledge and a passionate wish to plunge into scientific work for which he had all the premises.

He was impeccably bred, a gentle, kind and charming man.

I emphasized this phrase since I myself and an overwhelming majority of people who knew him in the first place isolated exactly these humane qualities in him.

Our conference takes place during the seventeenth anniversary of the Great Victory in the war against fascist Germany and militaristic Japan, and I consider it necessary to restore definitively historical justice with respect to those scientists, engineers and technicians who, in the most trying conditions of sharashkas of various types, in every possible way assisted this victory. Even posthumously they deserve the right to be called participants of the Great Patriotic War.

Notes

1. Article Mikhail Mikhailovich Konstantinov (1882 – 1938), journalist, historian, member of Irkutsk city Duma 23.09.1919 – 20.02.1920.

Irkipedia.ru/content/konstantinov_mihail_mihailovich See *Pervye istoriki Oktiabrskoi revoliutsii i grazdanskoi voiny v Sibiri*. (First historians of October revolution and civil war in Siberia). Novosibirsk, 1988.

2. Alekseev M. A. et al. *Enziklopedia voennoi razvedki* (Enc. military intelligence), *1918 – 1945*. Moscow 2012, p. 426.

3. She was repressed after Sorge's disclosure in 1942. Died in exile 3 July 1943 in a village in Krasnoiarsk territory.

4. Lurie V. M. et al, *GRU: Dela i Luidi* (GRU: Pursuits and participants). Compiled by A. I. Kolpakidi. Petersburg – Moscow, 2002, p. 408.

5. Ts. A. FSB (Central archive, Federal Security Service). Delo P-23744, vol. 3, list 453. According to the publication of A. V. Soloviev.

6. Family archive of Postnikovs.

7. See 5, list 488.

8. See 5, list 549.

9. Latishev I. *Iaponia, Iaponzy i Iaponovedy* (Japan, Japanese and Japan Scholars). Moscow, 2001, p. 13.

readr.ru>igor-latishev-yaponia …yaponovedy.html?

I. Kruglikova

The Russian Liberation Committee (RLC) organized in London in 1919 had the purpose to inform the western society about the events taking place in Russia. The RLC managed to receive the latest news from Russia and spread them among the British by publishing several periodicals in English. However, this was a complicated task.

The RLC was established in London in Febr. 1919 on the initiative of M. I. Rostovtzeff, a Russian academician in exile, and A. V. Tyrkova-Williams, a Russian writer and political activist of the Cadet [constitutional democrats] party. The former was chairman, and the latter, the secretary of RLC. Moreover, prominent political activists, predominantly cadets, were admitted members
(P. Milukov, P. Struve, N. Nabokov).

The major goal of the RLC was to inform the British society about the real situation in Russia at that time [1, L. 18; apparently List, page],

To contribute to the revival of Russia, and to raise the prestige of Russia abroad.

The RLC was financially supported by the Russian entrepreneur N. X. Denisov. However, since the spring of 1919 the RLC started to receive a subsidy from the government of Admiral A. V. Kolchak in Omsk. The Russian Telegraph Agency was sending the latest news from Omsk for publication in England. Informing the British about events of the Civil War in Russia became the main activity of the RLC. It produced leaflets in English, distributed them to the members of the parliament in London and to British politicians.

More than 50 newsletters were issued during three years of RLC work. Having established a link with the governing centres of the Russian Liberation (Omsk, Ekaterinodar, Helsingfors), the RLC began to report daily to the British press the information received by telegraph from various regions of Russia. In those newsletters the real situation in Russia was described in detail. For example, difficult conditions of cultural workers, the persecution of church officials and scholars etc.

Thus, issue No. 7 in April 1919 published the information about the bishops of Perm who were recently tortured to death by the Bolsheviks [1]. Furthermore, in June 1919 the RLC began to publish the newspaper *Rassvet* (Dawn) which was to reach the north of Russia in 10 – 20 days after release [2]. The first issue appeared on June 10 1919 due to the connection with the government of Admiral Kolchak and a subsidy for publication.

Nevertheless only two issues of that newspaper were published because of the complexity of transportation under the conditions of the Civil War.

Several articles in the published newsletters should be mentioned: an essay of Milukov, *Russia and England*;

G. Williams, *Why Soviet Russia is starving*, and Rostovtzeff, , *Proletarian culture* which was directed against the activities of Lunacharsky, the first Commissar of education and a cousin of Rostovtzeff.

In addition to the newsletters of the RLC a weekly *The New Russia* was issued since Jan. 1919. V. D. Nabokov and Milukov were the editors of the journal. Rostovtzeff and Tyrkova-Williams actively participated as well as Tyrkova's husband , a British journalist G. Williams. Just as in the newsletters, materials about the latest events in Russia were published on a regular basis. This journal appeared as an international platform for eyewitnesses who arrived in London directly from Russia. It published articles written by K. D. and Nabokov.

Rostovtzeff, Tyrkova-Williams, an editor and prominent political activist Milukov wrote articles about Russian culture [3]. By early 1920 financial difficulties significantly worsened the activities of the RLC [4]. The monthly *Russian life* was first published in London in 1920 immediately after the closing of *New Russia* as its successor. Articles on the economic situation in Russia, on the Red terror, prisons, violence and devastation were published there. In addition, there was a separate section devoted to the conditions of the *intelligentsia* in Russia. This was one of the first magazines which published the news of the poet Alexandr Blok's death (in Aug. 1921) and the death of the poet N. Gumilev (published in Sept. 1921) [4].

Thus, during three years of the work of the RLC (1919 – 1921) it performed the important task of informing the British about the events which took place in Russia by publishing newsletters and journals in English.

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4. Kazmina O. Russkie v Anglii. Russkaia emigratsia v kontekste russko-angliiskikh sviazei v pervoi polovine XX veka (Russians in England. Russian emigration in the context of Russian – English connections in the first half of the 20th century). Moscow, 1997, pp. 36 – 42. [No title of this source is given.] **5.** Skifsky roman (Skythian novel). Moscow, 1997, pp. 123 – 144. [Apparently, same source as in Ref. 4.]

I published a Russian translation of Rostovtzeff's *Proletarian culture* (**S, G,** 51).It pictures the misanthropic policy of the crazy Lunacharsky and in particular provides a long quote from a schoolteacher's story in the journal *Russkaia zhisn* (Russian Life) of 19 May 1919. She described the horrible life of children in a boarding school and their extremely high mortality (exact figure apparently remained unknown).

In 1919, Aleksandr Chuprov wrote a letter to his correspondent (family name unknown) from the RLC, see Code Add54437 in the British Library. He was a *nevozvrashchenets* (a person who left Russia before the Bolshevik coup d'Etat of 1917 and does not return) who lived mostly in Germany.

Chuprov commented on the half-hearted intervention and stated that it should be swift and decisive, otherwise useless. Moreover, *we* ourselves must be able to create a [new] state. It seems that he did not believe in such an ability.

Tude

Burton H. Camp

Karl Pearson and mathematical statistics

J. Amer. Stat. Assoc., vol. 28, 1933, pp. 395 – 401

The retirement of Karl Person as professor at the University of London and director of the Galton Laboratory marks the culmination of a most notable chapter in the development of statistics. From many parts of the world men and women have come to his laboratory to listen to his lectures and to conduct their own researches in his stimulating presence. His editorship of *Biometrika* has made for that journal its position of prime importance as repository for contributions to theoretical statistics.

Before reviewing Pearson's mathematical work it is necessary to pay respect to his personal qualities as a teacher and a scholar. It would be impossible for one who has been in close touch with him not to feel compelled to do this, and in addition these qualities have an important bearing on a proper interpretation of his writings. First of all he is friendly. This is probably not appreciated to the degree to which it is true by those who have been only his readers, for there is much in what he has written that is caustic. His critics have been dealt with in severe and able language. Sometimes it has been obvious that this has been well deserved, when they saw only a little of what he meant and gave publicity to palpably incorrect interpretations or to naïve criticisms of his views. But sometimes it has not been deserved, or at all events not obviously deserved, and then of course it reflected adversely on its author, but it does not follow, as some may have supposed, that he is given to shallow judgement or that he is unkind.

Rather, if I may apply an Americanism to so staunch a Briton, he is quick on the trigger. I once had a cowboy friend at Harvard who used to say that Cambridge was all right, but as for him, he preferred a country where there was *just a little smell of gunpowder in the air, not enough to make it disagreeable, but just enough to make everybody polite, one to the other*. He would have loved the Galton Laboratory when Professor Pearson was about. And this is the point of the story: the rest of us loved it too, for with the brilliant mind and its masterful repartee lives as warm and kind a heart as a teacher ever had. It cannot be said of him as of some that he is so engrossed in things scholarly as to leave out the human touch. Indeed, strange as it may seem, something which is almost the reverse of that is true: although one cannot be in his presence without recognizing that here is a distinguished person, one wants to be in his presence not because he is distinguished but because he is lovable. Every year at the Laboratory a reunion is held of his former associates and pupils as are near enough to come. What impresses the stranger most about these meetings is that these persons seem to have come to do honour not so much to the philosopher as to the friend.

At his laboratory there was truly an association of scholars. Although local students were working for degrees, for the most part those who had come were working simply for the development of science. Professor Pearson was not only the acting head of his laboratory, but was vital in every one of his activities. Anthropologists, biologists, sociologists, psychologists, mathematicians and others were there together, each working on his own problems, and once, frequently twice, every day, Professor Pearson sat down with each individual and thought through his work with him. He was indeed so very helpful it was even embarrassing for it was not always easy to show progress in research twice a day.

Pearson is indefatigable. He arrived at the laboratory early in the morning before others were admitted and left after others were excluded. He hurried through lunch and beat his staff back to the books. He did not attend the British Empire Exposition in 1924. It was only a ten minutes' ride from his office but he said he did not have the time. He was even then, at age 67, working at home late at night. He was taking a month's so-called vacation in August but carrying his work with him, and coming back to London one or twice a week.

He is painstaking in two important respects. First, his mathematics is essentially rigorous. I was somewhat surprised to find that this was so, for coming from a background of training in analysis and having read most of his papers, I had the feeling that his mathematics might be a bit on the hop, skip and jump order, but I found that although his writings did not always mention the fine points, still they were in his mind and really had been taken care of. Secondly, his computation, though naturally accurate, was always thoroughly checked, and he has insisted on similar care among his associates. Much of Pearson's theoretical work will of course ultimately be rewritten, perhaps several times, but the voluminous tables which he and his staff have compiled will for the most part never be recomputed. It is a comfort to know that they are trustworthy.

The problem of computing a truly reliable table is not the simple one which those who have not done it commonly suppose, and a prodigious amount of work, both of routine and of theoretical nature, has been done at the Galton Laboratory on tables. The following tables at least should be mentioned:

Tracts for Computers; *Tables for Statisticians and Biometricians*, 2 volumes; *Table of Twenty Place Logarithms*, *Tables of the incomplete Gamma Function*.

In connection with the construction of the latter much theoretical work was done on the problem of interpolation, see also the following by Seimatsu Narumi, one of Pearson' pupils: Some formulae in the theory of interpolation of many independent variables, *Tôhoku math. J.*, vol. 18, pp. 309 – 321.

This account of Pearson's scientific activities will have to be restricted almost exclusively to the mathematical part, but, although probably his eminence is due primarily to his success as a mathematician, his contributions to other sciences have been very important indeed. It is difficult to do justice even to his mathematics without incursions into various other fields, as will be evident from some of the titles to be cited below. This is especially true of his papers in the *Draper's Company Memoirs*. The record of his work is scattered through many volumes. His writings in *Biometrika* alone total about 1500 pages, not including papers under joint authorship and others obviously done under his immediate supervision. He has written no book on mathematical statistics. Many wish that he would do so, for his writings have a clearness of exposition hard to match and he has at his command a great wealth of illustrative material. Possibly now, after his retirement from the laboratory, this hope of his friends may be considered more favourably.

One of his most important early papers on statistics was Skew variation in homogeneous material, *Phil. Trans. Roy. Soc*., vol. A186, 1895, pp. 343 – 415. This contains a complete exposition of his now well known frequency curves (the fundamental types). Other frequency curves have been suggested such as the so-called Gram-Charlier series of Hermite's polynomials which had been tabulated by Pearson in the guise of tetrahoric functions and various generalisations of both types. For a time there was much discussion as to which sort of frequency curves was the most valuable. This was rather regrettable. Both the Pearson and Charlier types spring from natural assumptions and both are valuable aids in analysis. Although it is a striking fact that almost every natural frequency distribution can be fitted by one of Pearson's curves or by a few terms of the Charlier series, it does not follow that either of these systems comprises in some hidden sense a natural law, and prolonged argument as to which gives the better fit would not appear to be justified on that ground. Certain of Pearson's curves are of course coming into prominence now in another connection, namely as the theoretical forms which are satisfied by the sampling distribution of certain statistical parameters.

Pearson's discovery of the chi-square test of significance was published in the *Phil. Mag*. in 1900 with tables, vol. 50, pp. 157 – 175. The theory as then announced was essentially sound and has been of great value. As pointed out by Fisher and others that theory would better be modified if used otherwise than in the ideal case, that is, in the case where the universe sampled is supposed known. This modification turns out to be quite simple fortunately, and, as clearly stated by Irwin in the *J. Roy. Stat. Soc*., vol. 92, 1929, p. 264, it is not absolutely necessary. It is a matter of precisely what question in probability one wishes to solve. It should also be pointed out that by using too fine a division, Pearson at first carried some of the implications of his theory to an unwarranted extreme.

The theory of sampling runs through many volumes of *Biometrika*. When this theory was developed the samples were supposed fairly large and for the most part the discussion had to do with the discovery of formulae for the standard deviations of various statistics, a very important matter which is basic to the whole theory of sampling. Pearson was not at that time interested in the modern question of small samples and again he sought usually a solution for the ideal case when the universe sampled was supposed known. Again it is true that the modern improvements are often made possible by shifting the questions in probability from the questions whose solution was sought by Pearson to similar but not exactly identical ones whose solution for small samples is easier to obtain. These early papers of his on sampling are marked by a thoroughness and completeness that have not been fully appreciated. Together they form an admirable text on the foundations of the subject. Latterly he has contributed to the small sample theory. This he thinks of as valuable but no so valuable as it sometimes appears. It should not, he (1931) thinks, be swallowed whole:

Experimental work of a very useful kind has been started to discover how far the present mathematical theory of small samples can be extended to other than a single type of parent-population. But it is too early yet to be dogmatic as to the limits within which the application of such theory is valid. In particular I hold that the so-called z test as usually applied to small samples, especially when it is used to measure the probability or improbability of identity in the constants of small correlated samples, really requires further consideration.

The idea involved in the coefficient of correlation was initially due to Galton, and it was originally called Galton's function, but Pearson's work on the development of this theory has been so important that the coefficient is now commonly known as his. The following papers should be mentioned here:

Regression, heredity and panmixia, *Phil. Trans. Roy. Soc*., vol. A187, 1896, pp. 253 – 318; On the influence of natural selection on the variability and correlation of organs, same journal, vol. A200, 1903, pp. 1 – 66; Novel properties of partial and multiple correlations. *Biometrika*, vol. 11, 1915 – 1917, pp. 231 – 238.

Pearson has investigated also other measures of interrelation such as On he coefficient of contingency and its relation to average and normal correlation, *Drapers Co. Res. Mem.*, Biometric ser., vol. 1, 1904. These other coefficients are not so valuable as the coefficient of correlation, however, and the same is true of various coefficients advocated by others, and Pearson has been forced to spend a good deal of labour in proving this.

His tetrachoric *r* is theoretically the best measure of interrelation in a fourfold table, being in fact the very *r* of that normal surface which precisely fits the table. For many years it suffered in popularity because of the difficulty in its computation. That difficulty is now completely removed with the publication in 1931 of his second set of Tables (cf. also *Biometrika*, vols. 11, 19 and 22). The problem of polychoric *r* is still in a less satisfactory state (cf. an article by K. and E. S. Pearson, *Biometrika*, vol. 14, pp. 127 – 157) and it is especially because of this fact that the coefficient of contingency is used, but the latter is an unsatisfactory substitute, partly because it does not depend on the order in which the columns (or rows) of the correlation table are arranged. In this connection it is pertinent to note that at an early date Pearson recognised the error in dealing with a merely ordered series as if it were measured, by the method of assigning to it arbitrary numbers, and emphasized as the only scientific basis of measurement the method of graduation by means of a normal curve. This method lies at the foundation of much of the technique of the psychologist and the educationist and the use of the Kelly-Wood table and others.

Pearson has been much interested in the history of statistics and is an avid reader of the early masters of the theory of probability, Bernoulli, Laplace and others. It was by a brilliant inference that he found a rare appendix to a volume of De Moivre which showed that

De Moivre and not Gauss or Laplace was the real author of the normal law, in the sense that De Moivre first gave the relation between the exponential function and the point binomial of probability theory.

The above paragraphs have to do with Pearson's thoughts on some matters that are familiar to all of us. For the rest it is perhaps sufficient to pick out from a large number half a dozen subjects with brief references for each, merely to indicate the variety of his interest in mathematical statistics:

Probability that two samples belong to the same population. *Biometrika*, vols. 8, 10, 24, 25; hypergeometric series, simple and double, *Biometrika*, vol. 16 (cf. also Romanovsky in vol. 17); bivariate surfaces, *Biometrika*, vol. 17 (cf. also Rhodes in vol. 14, Narumi in vol 15); properties of Student's *z*, *Biometrika*, vol. 23; ranked individuals and ranked variations, vols. 23 and 24.

His earlier work in the fields of engineering and of mathematical astronomy is also important, but would not particularly interest readers of this *Journal*.

Pearson has given much of his energy to the study of eugenics and anthropology, and although these are not our primary interest, they are too interesting to omit altogether. To quote from the *University College Magazine*:

In the field of Eugenics, he has ever stressed the importance of the careful collection of information before any valid theories can be formed. "The treasury of human inheritance" which has been published in a number of parts, represents the first and still the only attempt in England to provide material on an adequate scale for the study of human genetics. His contributions to the scientific study of physical anthropology have been perhaps as great as those of any other man. A recognition of their value was shown in 1932 when the Rudolph Virchow Medal was presented to him, the only anthropologist not a German to have received this honour. His contributions to medical knowledge were also recognized when he was made an Honorary Fellow of the Royal Society of Medicine, a very unusual honour for a layman, while he is the only man outside the insurance world to be a member of the Actuaries Club. The year 1930 saw the completion of the third and last volume of a great labour of love, "The life and letters [*letters and labours*] *of Francis Galton". Those who glance at even a portion of it will begin to understand not only what Galton was, but what Karl Pearson has been and is.*

Pearson's scientific achievements is thus another excellent illustration of the old truth that progress in both mathematics and practical science is specially fostered when they are permitted to interact the one on the other. The modern mathematical theory of statistics apparently owes its existence to the need for solving practical problems in the theory of inheritance, and much of modern biometry would not exist if this study had not elicited the interest of a mathematician. At this moment a committee of the American Statistical Association is at work on the problem how best to nurture in this country the development of mathematical statistics and how to supply mathematical tools to the so-called practical statistician. It would appear that the story of Pearson might give the best possible solution, namely the founding for scholars in this country of a laboratory similar to his, with a mathematician of his promise who will study all their problems with them. If the latter objective appears too difficult to realise, it affords for that very reason a striking commentary on what he has accomplished.

Professor Pearson retires after 42 years of service at University College and 24 years at the head of the Galton Laboratory. This position having been transferred to him by Sir Francis Galoton two years before his death in 1911. Pearson's position is now being shared by his son, Egon Pearson, who is head of the department of statistics at University College, and by R. A. Fisher, who is Galton Professor of Eugenics and in charge of the Galton Laboratory.

The description of De Moivre's *Letter* of 1733 is inadequate. First, he effectively if not formally derived the normal law in the general case. Indeed, the title of his Letter includes the words $(a + b)^n$. In the text itself Camp derived that law in the case of the *point* binomial but noted that it is not difficult to generalize the derivation. Second, Pearson was not the first who discovered that *Letter* (Sheynin O., *Theory of probability*. … Berlin, 2017, **S, G,** 10, p. 66).

And the author's bibliographic information is simply substandard.

Israel and the pithecanthropes

In English and Russian. Английский и русский тексты

In 1977 or 1978 Israeli pilots shot down 5 soviet air fighters without losses. The Politbureau officially led by Brezhnev unofficially by Kremlin's grey Cardinal Suslov, decided to annihilate Israel with population. Criminally stupid advisors told: only moral indignation will follow from the West. Soviet U-boat with 5 nukes in the Mediterranean awaited signal.

But Israeli Muslims will die as well. Millions of Muslims in neighbouring countries and population in Southern regions of Soviet Union will slowly die as well.

Hundreds of millions of Muslims around the world will unite, declare Gazavat. First step: stop oil from entering the West. Europe forced to break off all relations.

Muslim republics at home will wake up … Muslim Caucasus ignites aided and abetted by Turkey… Disturbances in Tatarstan and Bashkiria. Soviet embassies and consulates around the world will be destroyed, personnel killed. Soviet citizens abroad in great danger. Future horrible.

Events in Czechoslovakia compelled the Soviet pithecanthropes to forget Israel (and save the world).

Source: Facebook, *On vipolnit liuboi prikaz* (commander of U-Boat will follow any order).

В 1977 или 1978 г. израильские лётчики без потерь сбили пять советских истребителей. Политбюро с Брежневым во главе, но фактически во власти серого кардинала Кремля, Суслова, решило уничтожить Израиль водородными бомбами, Советская подлодка в Средиземном море ожидала только приказа …

Был же какой-то прогноз ответных действий Запада, и решили от идиотского ума**:** кроме морального осуждения боятся нечего! Но погибло бы более миллиона арабских граждан Израиля, постепенно вымерло бы мусульманское население окрестных стран, да и некоторые районы Сов. Союза сильно пострадали бы.

Сотни миллионов мусульман *соединились* бы в Священной войне, газавате, Сов. Союзу, Были бы разгромлены советские посольства и консульства по всему миру, их сотрудники убиты. Советские граждане за рубежом оказались бы в страшной опасности

… И прекратилась бы продажа нефти на Запад, и Европа была бы вынуждена прервать все отношения с Советским союзом. А внутри страны, при поддержке Турции, запылал бы мусульманский Кавказ (не говоря о Татарстане и Башкирии). Проснулась бы Средняя Азия … Последствия были бы страшными …

События в Чехословакии вынудили советских питекантропов забыть об Израиле (и спасти мир).

Источник: Facebook, *Он выполнит любой приказ* (о командире советской подлодки).

History of mathematics:

Tude

some thoughts about the general situation

Silesian Stat. Rev., No. 16/22, 2018, pp. 127 – 156

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

1. Introduction

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

Below, in § 2, I consider these circumstances and apply appropriate examples of mistakes made by some authors; in my previous publication (2017) I collected mistakes committed by more than 140 authors.

Why are authors guilty?

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

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

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

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

Their statement is universally true.

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

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

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

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

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

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

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

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

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

1) Many reviewers just do not understand their duties.

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

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

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

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

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

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

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

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

In Germany, Bortkiewicz was called the *Pope of statistics*. *The publishers have stopped asking* [him] *to review their books* [because of his deep and impartial reviews] (Woytinsky 1961, pp. 451 – 452). And many weak works had probably never appeared since their authors were afraid of his response.

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

The sledgehammer law

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

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

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

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

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

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

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

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

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

Conclusion

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

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

Mikhailov A. I. (1975, in Russian), Abstracting journal. *Great Sov. Enc*., third edition, vol. 22, pp. 53 – 54. This source is available in English, in the same vol. 22.

Shaw N., Austin E. (1926), *Manual of Meteorology*, vol. 1. Cambridge, 1942.

Sheynin O. (1990, in Russian), *Alexandr A. Chuprov. Life, Work, Correspondence*.V&R Unipress, 1991.

--- (2017), *Black Book of History of Probability and Statistics*. Berlin. **S, G,** 80.

Truesdell C. (1984), *An Idiot's Fugitive Essays on Science*. New York. This is a reprint of many essays and reviews of classical works published over many years. Idiot, as Truesdell explains, is derived from Greek and properly denotes a non-specialist , but I do not understand why did he thus call himself.

Woytinsky W. S. (1961), *Stormy Passage*. New York.

2. Examples

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

J. Bertrand

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

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

$$p_1^m p_2^n \div p_2^m p_1^n$$

n = 499,609,

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

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

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

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

Bertrand J. (1888), *Calcul des probabilités*. New York, 1970, 1972.

De Montessus R. (1903), Un paradoxe du calcul des probabilités. *Nouv. Annales Math.*, sér. 4, t. 3, pp. 21 – 31.

Sheynin O. (1994), Bertrand's work on probability. *Arch. Hist. Ex. Sci.*, vol. 48, pp. 155 – 199.

F. W. Bessel

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

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

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

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

And here are excerpts from Gauss' correspondence.

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

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

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

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

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

--- (1838), Untersuchung über die Wahrscheinlichkeit der Beobachtungsfehler. Ibidem, Bd. 2, pp. 372 – 391.

--- (1843), Sir William Herschel. Ibidem, Bd. 3, pp. 468 – 478.

--- (1845), Übervölkerung. Ibidem, Bd. 3, pp. 387 – 407.

--- (1876), *Abhandlungen*, Bde 1 – 3. Leipzig.

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

Vladislav Bortkevich, Ladislaus von Bortkiewicz

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

For several decades his law of small numbers, LLN (1898) remained the talk of the town although it only repeated the results of

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

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

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

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

For his biography see Sheynin (2012).

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

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

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

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

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

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

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

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

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

--- (2012), L. von Bortkiewicz: a scientific biography. *Dzieje matematyki Polskiej*. Wrozlaw, pp. 249 – 266. Editor, W. Wieslaw.

Whitaker Lucy (1914), On the Poisson law of small numbers. *Biometrika*,
vol. 10, pp. 36 – 71.

Winkler W. (1931), Ladislaus von Bortkiewicz als Statistiker. *Schmollers Jahrbuch f. Gesetzgebung, Verwaltung u. Volkswirtschaft im Deutschen Reich*, 55. Jg., pp. 1025 – 1033.

P. L. Chebyshev

Novikov (2002, p. 330):

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

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

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

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

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

Chebyshev (pp. 152 – 154) investigated the cancellation of a random fraction, but Bernstein (1928/1964, p. 219) refuted his result (Sheynin 2017, p. 225). On that problem and on the stochastic number theory see Postnikov (1974).

The published text of the *Lectures* contains more than a hundred mathematical mistakes. Ermolaeva (1987) discovered their more detailed text but had not explained what was new there as compared with the Liapunov text. It remains unimaginably difficult to read it.

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

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

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

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

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

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

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

Sheynin O. (1994), Chebyshev's lectures on the theory of probability. *Arch. Hist. Ex. Sci*., vol. 46, pp. 321 – 340.
--- (2017), *Theory of Probability*. *Historical Essay*. Berlin. **S, G,** 10.
Solzhenitsyn A. (2013), *Dvesti let Vmeste* (Together for Two Hundred Years),
pt. 2. Moscow.

A. A. Chuprov

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Later, Chuprov repeatedly returned to the Lexian theory and finally abandoned it in 1921. In Letter 151 of 20 January of that year he (Bortkevich & Chuprov 2005) expressed his desire to *do away absolutely with it* (Bortkiewicz categorically disagreed.) And in a letter of 30 January to Gulkevich he (2017, p. 250) indicated that the [Lexian] *theory of stability is essentially based on a mathematical misunderstanding*.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

--- (1914, in Russian), The law of large numbers in contemporary science. In Ondar (1977/1981, pp. 164 – 181).
--- (1922a, review), E. Zizek (1921), *Grundriß der Statistik*. München – Leipzig. *Nordisk Statistisk Tidskrift*, Bd. 1, 1922, pp. 329 – 340.
--- (1922b), Das Gesetz der großen Zahlen und der stochastisch-statistische Standpunkt in der modernen Wissenschaft. Ibidem, Bd. 1, No. 1, pp. 39 – 67.
--- (1923), On the mathematical expectation of the moments of frequency distributions in the case of correlated observations. *Metron*, t. 2, No. 3,
pp. 461 – 493; No. 4, pp. 646 – 683.
--- (1960), *Voprosy Statistiki* (Issues in Statistics). Moscow.
--- (2009), *Pisma* (Letters to) *K. N. Gulkevich, 1919 – 1921*. Berlin. Publication by G. Kratz, O. Sheynin, K. Wittich. **S, G,** 28
Louis P. C. A. (1825), *Recherches anatomico-pathologiques sur la phtisie*. Paris.
Markov A. A. (1910, in Russian), Letter to V. A. Steklov. *Nauchnoe Nasledstvo*, vol. 17. Leningrad, 1991.
--- (1911, in Russian), On the basic principles of the calculus of probability etc. In Ondar (1977/1981, pp. 149 – 153).
Ondar Kh. O., Editor (1977, in Russian), *The Correspondence between A. A. Markov and A. A. Chprov* etc. New York, 1981.
Romanovsky V. P. (1926), On the distribution of the arithmetic mean in series of independent trials. *Izvestia Akad. Nauk SSSR*, ser. 6, vol. 20, No. 12, pp. 1087 – 1106.
--- (1938), *Matematicheskaia Statistika*. Moscow – Leningrad.
Slutsky E. E. (1925, in Russian), On the law of large numbers. *Vestnik Statistiki*, № 7 – 9, pp. 1 – 55.
--- (1926), A. A. Tschuprov. *Z. angew. Math. Mech.*, Bd. 6, pp. 337 – 338.
Sheynin O. (1990, in Russian), *Alexandr A. Chuprov: Life, Work, Correspondence.* V&R Unipress, 2011.
--- (2017), *Theory of Probability. Historical Essay*. Berlin. **S, G,** 10.
--- (2014, in Russian), On the history of university statistics. *Silesian Stat. Rev.,* No. 14 (18), 2016, pp. 7 – 25.

M. J. A. N. Condorcet

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

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

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

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

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

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

Condorcet M. J. A. N. (no date), Eloge d'Huygens. *Oeuvr*., t. 2. Paris, 1847,
pp. 54 – 72.

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

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

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

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

A. A. Cournot

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

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

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

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

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

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

Fries J. F. (1842), *Versuch einer Kritik der Prinzipien der Wahrscheinlichkeitsrechnung*. Braunschweig. *Sämtl. Schriften*, Bd. 14, pp. 1 – 236. Aalen, 1974.
Krüger L. (1987), The slow rise of probabilism etc. In L. Krüger et al, Editors, *Probabilistic Revolution*, vol. 1. Cambridge (Mass.) − London, pp. 59 – 89.
Thierry M. (1994), La valeur objective du calcul des probabilités selon Cournot. *Math. inf. sci. hum.*, No. 127, pp. 5 – 17.
--- (1995), Probabilité et philosophie des mathématiques chez Cournot. *Rev. hist. math*., t. 1, No. 1, pp. 111 – 138.

A. De Morgan

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

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

I. Ekeland

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

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

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

R. A. Fisher

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

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

Fisher R. A. (1925), *Statistical Methods for Research Workers*. In author's *Statistical Methods* (1973), *Experimental Design and Scientific Inference*. Oxford, 1990.
--- (1939), "Student". *Annals Eug.*, vol. 9, pp. 1 – 9.
--- (1951), Statistics. In *Scientific Thought in the 20th Century*. Editor A. E. Heath. London, pp. 31 – 55.
Kolmogorov A. N. (1947, in Russian), The role of Russian science in the development of the theory of probability. *Uchenye Zapiski Mosk. Gos. Univ*., No. 91, pp. 53 – 64. **S, G,** 7.
Sheynin O. (2010), Karl Pearson. A centenary and a half after his birth. *Math. Scientist*, vol. 35, pp. 1 – 9.

A. T. Fomenko

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

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

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

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

Efremov Yu. N., Pavlovskaia E. D. (1987, in Russian), The dating of the *Almagest* by the proper motion of the stars. *Doklady Akademii Nauk SSSR*, vol. 294, № 2, pp. 310 – 313.
--- (1989, in Russian), Same title. *Istoriko-Astronomicheskie Issledovania*,
vol. 21, pp. 175 – 192.
Fomenko A. T., Kalashnikov V. V., Nosovsky G. V. (1989), When was Ptolemy's star catalogue … compiled in reality? *Acta Applicandae Mathematicae*, vol. 17, pp. 203 – 229.
Nosovsky G. V., Fomenko A. T. (2004), *Tsar Slavian* (The tsar of the Slavs). Petersburg.
Novikov S. P. (1997, in Russian), Mathematics and history, *Priroda*, No. 2, pp. 70 – 74. **S, G,** 78.

B. V. Gnedenko

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

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

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

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

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

E. J. Gumbel

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

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

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

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

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

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

A. Hald

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

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

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

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

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

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

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

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

A. Ya. Khinchin

Khinchin's invasion of statistical physics (1943) was unfortunate.

Novikov (2002, p. 334) testified that

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

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

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

See also *Gnedenko*.

Khinchin A. Ya. (1937, in Russian), The theory of probability in pre-revolutionary Russia and in the Soviet Union. *Front Nauki i Techniki*, № 7, pp. 36 – 46. **S, G,** 7.
--- (1943, in Russian), *Mathematical Foundations of Statistical Mechanics*. New York, 1949.
Novikov S. P. (2002, in Russian), The second half of the 20th century and its result etc. *Istoriko-Matematicheskie Issledovania*, vol. 7 (42), pp. 326 – 356.

A. N. Kolmogorov

Kolmogorov (Anonymous 1954, p. 47):

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

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

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

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

The notion of mathematical statistics is a grotesque phenomenon.

Kolmogorov (1947, p. 56) maintained that

Chebyshev was the first to appreciate clearly and use the full power of the concepts of random variable and its expectation.

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

Anonymous (1954, in Russian), Account of the All-Union Conference on problems of statistics. *Vestnik Statistiki*, № 5, pp. 39 – 95.
Anscombe F. J. (1967), Topics in the investigation of linear relations […]. *J. Roy. Stat. Soc.*, vol. B29, pp. 1 – 52.
Gnedenko B. V., Sheynin O. (1978, in Russian), Theory of probability. A chapter in *Mathematics of the 19th Century*, vol. 1. Basel, 1992 and 2001,
pp. 212 – 288. Editors, A. N. Kolmogorov & A. P. Youshkevich.
Kolmogorov A. N. (1947, in Russian), The role of Russian science in the development of the theory of probability. *Uchenye Zapiski Mosk. Gos. Univ.*, No. 91, pp. 53 – 64. **S, G,** 7.
Pontriagin L. S. (1980, in Russian), On mathematics and the quality of teaching it. *Kommunist,* № 14, pp. 99 − 112.

P. S. Laplace

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

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

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

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

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

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

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

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

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

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

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

Fourier J. B. J. (1831, in French), Historical Eloge of the Marquis De Laplace. *Lond., Edinb. and Dublin Phil. Mag.*, ser. 2, vol. 6, 1829, pp. 370 – 381.
Gnedenko B. V., Sheynin O. (1978, in Russian), Theory of probability. A chapter in *Mathematics of the 19th Century*, vol. 1. Basel, 1992, 2001, pp. 211 – 288. Editors, A. N. Kolmogorov, A. P. Youshkevich.
Kant I. (1755), *Allgemeine Naturgeschichte und Theorie des Himmels* etc. *Ges. Schriften*, Abt. 1, Bd. 1. Berlin, 1910, pp. 215 – 358.
Laplace P. S. (1796), *Exposition du système de monde*. *Oeuvr. Compl.*, t. 6. Paris, 1884. Reprint of the edition of 1835.
--- (1812), *Théorie analytique des probabilités. Oeuvr. Compl.*, t. 7. Paris, 1886.
--- (1814, in French), *Philosophical Essay on Probabilities*. New York, 1995. Translated by A. Dale.
--- (1819), Sur l'application du calcul des probabilités aux observations etc. *Oeuvr. Compl.*, t. 14. Paris, 1912, pp. 301 – 304.
Lévy P. (1925), *Calcul des probabilités*. Paris.
Poisson S.-D. (1812). *Nouv. Bull. des Sciences Soc. Philomatique de Paris*, t. 3 pp. 160 – 163.
Sheynin O. (2006, in Russian), On the relations between Chebyshev and Markov. *Istoriko-Matematicheskie Issledovania*, vol. 11 (46), pp. 148 – 157.

G. W. Leibniz

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

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

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

A. M. Liapunov

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

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

Bernstein S. N. (1945, in Russian), On Chebyshev's work on the theory of probability. *Sobranie Sochineniy* (Coll. Works), vol. 4. Moscow, 1964, pp. 409 – 433. **S, G,** 6.
Liapunov A. M. (1895, in Russian), P. L. Chebyshev. In P. L. Chebyshev. *Izbrannye Matematicheskie Trudy* (Sel. Math. Works). Moscow – Leningrad, 1946, pp. 9 − 21. **S, G,** 36.

A. A. Markov

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

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

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

He thus destroyed his own defence of the method. At the end of his life Markov's health seriously deteriorated and the general situation in Russia became horrible which most essentially additionally affected his work. However, he hardly recognized Pearson, never mentioned Yule or Student and the references in the posthumous edition of his *Treatise* (1924) were the same as in the previous edition of 1913 but no one stated that Markov had time to prepare this edition for publication. Finally, Markov somehow decided that he transferred probability to the realm of pure science. See Sheynin (2006).

Many authors had remarked that Markov was very rude and sometimes unjust. Here is the clearest statement to this effect

(Chirikov & Sheynin 1994, letter of 24 Oct. 1915 from K. A. Andreev to P. A. Nekrasov):

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

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

Chirikov M. V., Sheynin O. (1994, in Russian), The correspondence between
P. A. Nekrasov and K. A. Andreev. *Istoriko-Matematicheskie Issledovania*, vol. 3pp. 124 – 147.

Linnik Yu. V. et al (1951, in Russian), Sketch of the work of Markov in number theory and theory of probability. In Markov (1951, pp. 614 – 640). Partly translated: **S, G,** 5.

Markov A. A. (1899, in Russian), The law of large numbers and the method of least squares. In Markov (1951, pp. 230 – 251).

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

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

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

Sheynin O. (2006), Markov's work on the treatment of observations. *Hist. Scientiarum*, vol. 16, pp. 80 – 95.

Youshkevich A. A. (1974), Markov. *Dict. Scient. Biogr*., vol. 9, pp. 124 – 130.

J. Neyman

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

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

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

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

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

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

Kh. O. Ondar

Tude

I knew him well. He hardly read any foreign language and his mathematics was poor, but he was a *nazmen* (supported by authorities since he belonged to a national minority) and a highly trusted citizen. Indeed, he lived in a student hostel of Moscow University in the same room with a few foreign students. He defended his candidate dissertation being supervised (apparently, mightily assisted) by Gnedenko. At least one of his papers (1970) and some of the comments in Ondar (1977) were way above his head. In that latter work, I (Sheynin 1990/2011, pp. 103 – 108) have discovered about 90 mathematical mistakes and most of them had been transferred to its translation of 1981. Ondar had thus treated his archival source as a bull in a china shop, and the damage done by him will remain for a very long time.

Ondar Kh. O. (1970, in Russian), V. A. Steklov's paper on the theory of probability. *Istoria i Metodologia Estestvennych Nauk*, vol. 9, pp. 262 – 264.
--- (1977, in Russian), *The Correspondence between A. A. Markov and A. A. Chuprov on the Theory of Probability and Math. Statistics*. New York, 1981. Ondar was Editor of Russian edition.
Sheynin O. (1990, in Russian), *Aleksandr A. Chuprov. Life, Work, Correspondence*. V&R Unipress, 2011.

S.-D. Poisson

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

Bortkiewicz (1894 – 1896, p. 661) formulated a wrong conclusion:

The difference between objective and subjective probability is unjustified since each probability presumes some knowledge, and some ignorance and is therefore necessarily subjective.

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

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

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

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

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

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

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

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

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

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

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

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

Sheynin O. (1978), Poisson's work in probability. *Arch. Hist. Ex. Sci.*, vol. 18, pp. 245 – 300.

--- (2002), Sampling without replacement. *Intern. Z. f. Geschichte u. Ethik d. Naturwissenschaften, Techn. u. Med*., Bd. 10, pp. 181 – 187.

--- (2012), Poisson and statistics. *Math. Scientist*, vol. 37, pp. 149 – 150.

--- (2013), Poisson et la statistique. In *Poisson. Les mathématiques au service de la science*. Palaiseau. Editor Yvette Kosmann-Schwarzbach, pp. 357 – 366.

T. M. Porter

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

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

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

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

--- (2003), Statistics and physical theories. In Mary Jo Nye, Editor, *Modern Phys. and Math. Sciences.* Cambridge, pp. 488 – 504.

--- (2004a), Karl Pearson's Utopia of scientific education etc. In R. Seising et al, Editors, *Form, Number, Order etc. Festschrift for Ivo Schneider* etc. Stuttgart, pp. 339 – 352.

--- (2004b), *Karl Pearson* etc. Princeton − Oxford. My review: *Hist. Scientiarum*, vol. 16, 2006, pp. 206 – 209.

S. M. Stigler

See downloadable file 31 on my website www.sheynin.de which is being diligently copied by Google: Oscar Sheynin, Home.