Studies

in the History of Statistics and Probability

Vol. 20

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Berlin
2020

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Notation
I am the author of items I, II, V and VI

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Notation

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

I omit the unnecessary adjective in the term *mathematical expectation*

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

I

Newton and the Classical Theory of Probability

Arch. Hist. Ex. Sci., vol. 7, 1971, pp. 217 – 243

To the memory of my mother,
Sophia Sheynin (1900 – 1970)

Summary

Probabilistic ideas and methods from NEWTON'S writings are discussed in § 1: definition of probability, probabilistic method in chronology, his ideas and method in the theory of errors and his probabilistic reasoning on the system of the world.

NEWTON's predecessors and contemporaries and his influence upon later scholars are dealt with in § 2. The section ends with LAPLACE, whose determinism is seen as a development of the NEWTONIAN determinism.

An Addendum is devoted to LAMBERT'S reasoning on randomness, to MAXWELL's thoughts and to the influence of DARWIN on statistics.

Abbreviations: PT abridged = *Phil. Trans. Roy. Soc. 1865 – 1900 abridged*. London, 1809.

Todhunter = I. TODHIUNTER, *History of the mathematical theory of probability*. Cambridge, 1865; New York, 1949, 1965.

1. Probabilistic Ideas and Methods in Newton's Writings

NEWTON left no direct contributions either to the theory of probability, or its applications in games of chance[1], demographic statistics (or political arithmetic in general) or the theory of errors. However, probabilistic ideas and methods enter his writings.

1.1. Definition of Probability. The so-called classical, prior probability of an event (p), which had been actually introduced even before the 17th century and by Newton, see below, is

$$p = \frac{m}{n}, \qquad (1)$$

where m is the number of chances (of cases) whereby the event can happen, and n is the number of all possible chances (cases), and all the chances are supposed equally possible.

The posterior, statistical definition of probability of an event

$$p = \frac{\mu}{\upsilon}, \qquad (2)$$

where μ is the number of observed occurrences of the event in ν independent trials, had also been in general usage, at least beyond the classical problems of games of, chance.

Definition (1) can only be applied when *m* and *n* are known which is a severe limitation. Then all the *n* cases ought to be equally possible (equally probable!) so that we find ourselves in a vicious circle. And, for good measure, (1) is not a definition but a formula for calculation. The second argument also applies to definition (2). Se also § 2.3.3.

The concept of probability (2) is foreshadowed in GRAUNT'S classical study[2] and extensively used by HALLEY[3]. Lastly, geometric probabilities, i. e. prior probabilities (1) with *m* and *n* being areas of corresponding figures, had been in use as well as an extension of the case of a finite number of outcomes. The first to use geometric probability in a published work was possibly HALLEY. In the memoir just mentioned (1693) he deduced probabilistic formulas analytically and then, possibly following ancient mathematicians, repeated his reasoning geometrically even in the three-dimensional case. Nevertheless, he only considered figures which corresponded to natural numbers whereas this restriction had not existed in Newton's earlier manuscript written sometime between 1664 and 1666[4].

Newton does not mention Huygens, but WHITESIDE believes that Newton discussed his *De Ratiociniis*[5].

If a ball *randomly* falls on a circle divided in two parts whose areas are as $2:\sqrt{5}$, then, as immediately follows from Newton's reasoning, these numbers are the terms of formula (1). NEWTON showed that the geometric probability was capable of treating irrational proportions of chances.

Then NEWTON discussed the casting of a non-regular die but only stated that

It [still] *may bee found how much one cast is more easily gotten than another*.

WHITESIDE reasonably believes that NEWTON

Clearly opts for a frequency theory of probability, that is, the absolute probabilities are not given a priori but are to be determined as the asymptotic limit of the numerical probabilities observed over a succession of occurrences of a state.

I disagree with the *asymptotic limit* which is only associated with R. MISES[6]. But WHITESIDE'S opinion about NEWTON'S recommendation of using probability (2) seems proper. Thus it seems that in both examples NEWTON is concerned with generalizing the concept of probability.

The second example was followed by HAM (or by one of the earlier anonymous translators of HUYGENS[7]) who inserted the irregular die absent in the original text, then by SIMPSON[8] who was the first who analytically calculated the sought probabilities.

The irregular die later appeared in the contributions of other authors[9].

1.2. Probabilistic Method in Chronology. NEWTON systematically used two methods for verifying the chronology of ancient events. One of these was the classical method of computing the dates of various astronomical phenomena put on record by ancient chronologists. The other one, the first method mathematical in the proper sense which was used in chronology and original with NEWTON, was based on probability and both methods are applied in

NEWTON's posthumously published book[10] although I only discuss the latter:

The Greek Chronologers … have made the kings of their several Cities … to reign about 35 or 40 years a-piece, one with another; which is a length so much beyond the course of nature, as is not to be credited. For by the ordinary course of nature Kings Reign, one with another, about 18 or 20 years a-piece: and if in some instances they Reign, one with another, five or six years longer, in others they Reign as much shorter: 18 or 20 years is a medium.

The length of reigns as derived by NEWTON from ancient chronology are the mean lengths for the given dynasties, and apparently NEWTON was unable to correct these lengths by allowing for the correlation of the lengths of reign of consecutive *kings*. The last interval (18 to 20 years) derived by NEWT0N from trustworthy sources is essentially a statistical estimate of the corresponding expectation. NEWTON used the same idea which underlies the law of large numbers: a random variable (the time interval) should be approximately equal to its expectation.

The rejection of a 35 to 40 years' length of reign on the basis of a twice shorter expectation is quite reasonable. But of course NEWTON did not possess a general statistical rule for the rejection of excessive time intervals.

NEWTON'S reasoning was not forgotten: he was quoted by CONDORCET[11], then by ELLIS[12] and, lastly, by PEARSON[13]. The last-mentioned provided two alternative methods (one of them is used in the mathematical theory of life insurance and described by C. F. TRUSTAM to whom a separate part of PEARSON's article is due) of developing NEWTON'S probabilistic idea, though owing to the lack of relevant statistical data neither could be successfully employed (PEARSON'S own remark).

The whole story of NEWTON'S work on chronology and of the scientific war between him and the French chronologists which began with a pirate French edition of some of NEWTON'S chronological calculations and NEWTON'S subsequent remarks[14] is described by MANUEL[15], who, however, leaves the probabilistic argument hardly noticed. So here is MANUEL (p. 34):

The bombshell Newton hurled into the staid world of the chronologists largely depended for its effect upon the scientific prestige which he had won elsewhere.

MANUEL (p. 35) also implies that the astronomical method in chronology is largely due to NEWTON.

1.3. Random and Systematic Errors. In one of his letters[18] NEWTON explained the occurrence of discrepancies between different experiments:

There may be many various circumstances which may conduce to it (to the discrepancy); *such as are not only the different figures of prisms, but also the different refractive power of glasses, the different diameters of the sun at divers times of the year, and the little errors that may happen in measuring lines and angles, or in placing the prism at the window; though … I took care to do these things as exactly as I could.*

In the same letter (pp. 339 – 341) NEWTON notices the change of the length of the sun's spectrum with the brightness of the sky. Being satisfied, however, with a crude estimate, he did not conduct special experiments. (For one thing, there was no numerical measure of the brightness of sky at his disposal.)

In his astronomical correspondence NEWTON[19] predicted the dependence of the vertical refraction on the air temperature and suggested corresponding corrections to observed altitudes.

In other words, NEWTON clearly understood the inevitability of random and systematic errors but did not explicitly distinguish between them.

1.4. Design of Experiments. NEWTON'S *Experimentum Crucis*[20] was the proof that the observed disproportion of the length and breadth of the sun's spectrum cannot be explained by unevenness in the glass or other irregularities. This became evident after NEWTON used a combination of two reciprocally located prisms.
The experiment is described in the *Lectiones opticae* (part 1, sect. 1)[21], and NEWTON repeatedly referred to it in his writings, especially in connection with the ensuing discussion in which NEWTON'S achievements were questioned. Strictly speaking, NEWTON'S proof is of a physical, not statistical nature and therefore bears no direct connection with the theory of errors. However, it is relevant to the general idea of the design of experiments.

NEWTON'S writings abound with reasoning on the design of experiments. In his *Lectiones*[22] NEWTON describes the measurement of refractions *ex Aere in quaelibet Media* and notes that his mode of measurement

Is easy and least prone to errors, especially if the prism's angle is large and accurately known, the quadrant large and accurate and the observations (registrations) *are made far from the prism where it is easier to distinguish the greatly dilated colours.*

The boundaries between different colours of the sun's Spectrum were independently recorded by NEWTON and his *friend*[23]. The discrepancies between these records were small, and NEWTON obviously relied on his experiment.

In his astronomical correspondence NEWTON[24] expressed his desire to receive unaltered, *naked* results of observations. One reason for this was to secure for himself the possibility of subsequent alterations in computations such as may be needed by a more advanced theory. The other reason was to avoid possible mistakes in computations undetectable if made by others.

NEWTON also discussed the design of experiments in his various letters[25]. Of some interest is the reasoning in his *Lectiones*[26] where he actually followed the practice of ancient astronomers of selecting optimal conditions for observation, for observing the position of the planets when they change their visible direction of motion.

A short remark on the insignificant effect of small errors under definite conditions is also contained in NEWTON'S Particular answer to Linus's objections[27].

1.5. Probabilistic Ideas in Astronomy. In the General scholium to his *Mathematical principles* ...[28] NEWTON asserted that the

Most beautiful system of the sun, planets, and comets, could only proceed from the councel and dominion of an intelligent and powerful Being.

This idea is formulated more definitely in his treatise on *Optics*[29]:

Whence is it that planets move all one and the same way in orbs concentrick, while comets move all manner of ways in orbs very excentric.

Then, in Query 31 (p. 261):

Now by the help of these principles, all material things have seem to have been composed of the hard and solid particles … variously associated in the first creation, by the counsel of an intelligent Agent. For it became Him who created them, to set them in order.

And, on the next page (p. 262):

Blind Fate could never make all the planets move one and the same way in orbs concentrick, some inconsiderable irregularities excepted, which may have risen from the mutual actions of comets and planets upon one another, and which will be apt to increase, till this system wants a reformation. Such a wonderful uniformity in the planetary system must be allowed the effect of choice. And so must the uniformity in the bodies of animals.

A reasoning follows (p. 263) which may well have occurred in LAPLACE:

As in Mathematicks, so in Natural Philosophy, the investigation of difficult things by the method of analysis, ought ever to precede the method of composition. This analysis consists in making experiments and observations, and in drawing general conclusions from them by induction, and admitting of no objections against the conclusions, but such as are taken from experiments or other certain truths. For hypotheses are not to be regarded in Natural Philosophy[30]. *And although the arguing from experiments and observations by induction be no demonstration of general conclusions; yet it is the best way of arguing which the nature of things admits of, and may be looked upon as so much the stronger, by how much the induction is more general*.

To this reasoning, and to the reformation of the system of the world, I return in § 2. I also add that if both directions of circulation are supposed equally probable and if these directions are, for different planets, independent, the probability that all the six planets known to NEWTON should circulate in the same direction is $(1/2)^6 = 1/64$. If, finally, the existence of *concentrick orbs* is supposed independent from the direction of circulation of the planets, the probability of the simultaneous occurrence of both phenomena would not be higher than $(1/64)^2 < 1/4000$.

An additional source of information about NEWTON'S reasoning is a letter from W. DERHAM to CONDUITT dated July 18, 1733[31]. DERHAM wrote of a

Peculiar sort of Proof of God wch Sr Is: mentioned in some discource wch he and I had soon after I published my Astro-Theology. He said there were 3 things in the Motions of the Heavenly Bodies, that were plain evidences of Omnipotence and wise Counsel. 1. That the Motion imprest upon those Globes was Lateral, or in a Direction perpendicular to their Radii, not along them or parallel with them.

2. That the Motions of them tend the same way. 3. That their orbits have all the same inclination.

Probabilistic reasoning accompanied by calculations connected with the *wonderful uniformity in the planetary system* were pursued by DANIEL BERNOULLI and LAPLACE[32]. Neither referred to NEWTON although both undoubtedly knew his point of view.

2. Newton's Probabilistic Influence

Almost the only pertinent reference to NEWTON is in PEARSON'S[33]: the development of

Newton's idea of an omnipresent activating deity, who maintains mean statistical values, formed the foundation of statistical development through Derham, Süssmilch, Niewentyt, Price to Quetelet and Florence Nightingale.

And again:

De Moivre expanded the Newtonian theology and directed statistics into the new channel down which it flowed for nearly a century. The causes which led De Moivre to his Approximatio[34] or Bayes to his theorem were more theological and sociological than purely mathematical, and until one recognises that the post-Newtonian English mathematicians were more influenced by Newton's theology than by his mathematics, the history of science in the 18th century, in particular that of the scientists who were members of the Royal Society, must remain obscure.

That this reference to NEWTON is extremely important is obvious, but to whom PEARSON referred when speaking about the *deity, who maintains mean statistical values*? This idea certainly occurs in DE MOIVRE (see below) and runs through all the works of SÜSSMILCH, but *mean statistical values* just do not occur in NEWTON? In 1971 E. S. Pearson answered my question on this point:

From reading [the manuscript of K. Pearson (1978)] *I think I understand what K. P. meant* […]. *He has stepped ahead of where Newton had got to, by stating that the laws which give evidence of Design, appear in the stability of the mean values of observations.*

2.1. Newton's Predecessors. What is crucial in NEWTON'S system of the world is his determinism, of which the LAPLACIAN determinism is a further development. Most certainly NEWTON did have predecessors. However, a really significant feature of NEWTON'S determinism as opposed to the determinism of his predecessors is the causality of NEWTON'S system of the world[35].

The concept of randomness was not foreign to ancient philosophers. And a direct connection of necessity and randomness is a distinct feature in EPICURUS and LUCRETIUS[36]. But starting with ARISTOTLE[37], it was generally accepted that random behaviour was peculiar to the earthly domain, remote from perfection, and that as a whole the world is ruled determinately. At the same time the accursed problem of the free will arose. This was mentioned already by LUCRETIUS (Ibidem), although without satisfactory explanation, and, in post-classical times, by EULER[38], JAKOB BERNOULLI (see below), and LAPLACE, then by QUETELET (see below).

KEPLER also held that the world was determinate and strived to establish the general deterministic laws of nature. Precisely the same climate of opinion was characteristic of NEWTON and his contemporaries. W. DERHAM (1657 – 1735), who was mentioned above, held[39] that the numerous examples of regularities observed in nature could be explained only by an act of creation or, to put it otherwise, by determinate laws:

Should we be so besotted by the devil, and blinded by our lusts, as to attribute one of the best contrived (by the deity) *pieces of workmanship* (i.e., man) *to blind chance, or unguided matter and motion, or any such sottish, wretched, atheistic stuff*?

Or (p. 194), quoting GALEN, the Roman physician: the order of teeth in man

Must needs be the work of some wise, provident being; not chance, nor a fortuitous concourse of atoms.

The whole *Astro-Theology*[40] of the same author was also conceived as a demonstration of the being of God. And among other references DERHAM repeatedly refers to CICERO'S *De Natura deorum*, apparently considering him as one of his predecessors.

All of this in my opinion additionally testifies that not NEWTON was the originator of determinism but, on the contrary, NEWTON’S *Principia* had occurred at the right time and was mathematically applied to supplement the general, prevalent idea of a determinate world (see also below). And because of NEWTON’S general influence, and, of course, because of the causality of his determinism, this prevalent idea became associated with NEWTON.

2.2.1. Arbuthnot and De Moivre. JOHN ARBUTHNOT (1667 – 1735) was the first author to calculate (as NEWTON implicitly did prior to him, see § 1.5) the odds of randomness versus necessity in a problem later to become classical, about the probability of an observed extreme pattern under a hypothesis of insufficient reason[41].

Had the probabilities of the births of both sexes been equal, ARBUTHNOT argued, the predominance of the births of boys would not have been observed in London 82 years in succession. He rejected the hypothesis of a uniform distribution (equal probabilities, as he understood it) of the birth of both sexes, not in favour of, say, a binomial distribution (unequal probabilities, as he could have understood it), since his conclusion was consistent with BERNOULLI trials with a p slightly higher than 1/2, but in favour of a determinate predominance of the birth of boys. Thus, like NEWTON, he contraposed the uniform distribution with determinism at large (with *design*).

FREUDENTHAL[42] holds that this article of ARBUTHNOT was the first paper in mathematical statistics. This is hardly so because one of N. BERNOULLI’S works, at least partly belonging to mathematical statistics, was published even before[43] and NEWTON’S reasoning on the planetary system (though not a separate paper), if formalized, also of course belongs to mathematical statistics.

FREUDENTHAL (Ibidem) and KRUSKAL[44] pointed out some methodological difficulties inherent in studies such as those of ARBUTHNOT (in particular, the impossibility of increasing the number of statistical observations). Borel[45] also expressed doubts. He thought that in cosmogony, as in studies related to the problem of the origin of life on earth, probabilistic reasoning would not at present be fruitful.

Of course, since some satellites were found to move in the direction opposite to that of all the planets and other satellites, the *wonderful uniformity in the planetary system* had been somewhat disturbed.

Now I notice two more known examples of such considerations: the *Jeans miracle* (a large amount of heat can pass from a cold body to a warmer one only by miracle) and *the miracle des singes dactylo-graphes* (a purely random reproduction of a literary chef-d'oeuvre is possible only by miracle)[46]. The last example, however, had already been known in the 17th century[47]:

It would be folly to play twenty sous … against a kingdom on the condition that we could gain the stake only if an infant arranging at hazard the letters from a printing-office, should compose all at once the first twenty lines of Virgil's Aeneid.

The same example, although in a simple form, occurred later in D'ALEMBERT and LAPLACE: a man of sense would scarcely doubt that letters forming the word *Constantinopolitanensibus*, or arranged in alphabetical order, are arranged so on purpose[48].

I conclude with a quotation from A. A. COURNOT[49] which shows that in a sense NEWTON'S thoughts which were qualitative are of course safer than those of ARBUTHNOT:

Independamment de la probabilité mathématique … il y a des probabilités non réductibles à une énumération de chances, qui motivent pour nous une foule de jugements, et même les jugements les plus importants; qui tiennent principalement à l'idée que nous avons de la simplicité des lois de la nature, de l'ordre et de l'enchaînement rationnel des phénomènes, et qu'on pourrait à ce titre qualifier de probabilités philosophiques. Le sentiment confus de ces probabilités existe chez tous les hommes raisonnables; lorsqu'il devient distinct, ou qu'il s'applique à des sujets délicats, il n'appartient qu'aux intelligences cultivées, ou même il peut constituer un attribut du génie. Il fournit les bases d'un système de critique philosophique entrevu dans les plus anciennes écoles, qui réprime ou concilie le scepticisme et le dogmatisme, mais qu'il ne faut pas, sous peine d'aberrations étranges, faire rentrer dans le domaine des applications de la probabilité mathématique.

This is by no means all there is in COURNOT'S works, which merit special research. It is really remarkable that, at least until recently, the greatest achievements in science were those based on rather simple hypotheses and error-burdened observational series. As EINSTEIN put it,

Der Herr Gott (= nature) *ist raffiniert, aber boshaft ist er nicht*.

2.1.2. DE MOIVRE. He openly acknowledged his adherence to the NEWTONIAN (determinate) philosophy. In the *Dedication* of the first edition of his *Doctrine of chances*[50] to NEWTON DE MOIVRE declared his intention of giving a

Method of calculating the Effects of Chance … and thereby fixing certain Rules, for estimating how far some sorts of Events may rather *be owing to Design* (= to determinate causes) *than Chance … so as to excite in others a desire of … learning from your* (= NEWTON'S) *philosophy how to collect, by a just Calculation, the Evidences of exquisite Wisdom and Design, which appear in the Phenomena of Nature throughout the Universe*.

Essentially, this is a declaration of the goals of the theory of probability, which DE MOIVREe evidently considered a mathematical discipline with direct applications to exact sciences and demography. Possibly because of his personal hardships DE MOIVRE himself applied probability only to games of chance and annuities on lives, but he repeatedly confirmed his point of view in his *Approximatio*.

There DE MOIVRE found *evidence of exquisite wisdom and design* in demography. Referring to N. BERNOULLI, he notices that the births of both sexes in London are to each other as 18:17 and that with a total of *n* births annually the study of relative frequencies of births is tantamount to the study of *n* castings of a 35-faced die. More important, DE MOIVRE (p. 253) insists that

The facilities of production of the two sexes or, if you will, the Form of the die is the Effect of Intelligence and Design.

What, then, did DE MOIVRE consider to be random? No concept of a random quantity, even of a heuristic kind occurs in his writings. Consider the example of the just described binomial distribution which, as it is seen, he supposed to be a feature of a determinate phenomenon, and also his Remark 1 (p. 250), which appears after some numerical calculations connected with the “integral De Moivre – Laplace limit theorem”. They suggest that he would have thought that a frequency curve was a curve of a determinate phenomenon in which only empirical deviations from that curve were random. Here is that Remark:

Chance very little disturbs the Events which in their natural Institution were designed to happen or fail, according to some determinate (sic!) *law.*

This was perhaps one of the reasons why the definition of a random quantity as “dependent on chance” and possessing a certain law of distribution had not appeared in LAPLACE (see below).

Lastly I note DE MOIVRE’S obscure reasoning on *chance in atheistical writings* (p. 253):

Chance, as we understand it, supposes the Existence of things, and their general known Properties: that a number of Dice, for instance, being thrown, each of them shall settle upon one or other of its Bases. After which, the Probability of an assigned Chance, that is of some particular disposition of the Dice, becomes as proper a subject of Investigation as any other quantity or Ratio can be.

But Chance, in atheistic writings or discourse, is a sound utterly insignificant: It imports no determination to any mode of Existence; nor indeed to Existence itself, more than to non-existence; it can neither be defined nor understood: nor can any Proposition concerning it be either affirmed or denied, excepting this one, “That it is a mere word.”

I return to this passage below, but note here that a comparison of different fragments of the *Approximatio* suggests that DE MOIVRE used the word *chance* in two different ways. Possibly this was the reason that BAYES stated that by chance he meant *the same as probability*[51].

2.2. Bentley. In 1692 R. BENTLEY (1662 – 1742), the future eminent theologian and member of the Royal Society, delivered a series of sermons, which were the first BOYLE lectures designed to prove Christian religion against infidels. Some of these sermons were necessarily devoted to astronomy, and BENTLEY had to use the newest NEWTONIAN ideas and approached NEWTON to clarify some of his doubts. Thus it happened that BENTLEY became the first populariser of NEWTON.

NEWTON favourably responded to BENTLEY, answered his letters and somewhat explained his reasoning on gravitation. But my goal is to describe BENTLEY'S understanding of chance[52]:

In the Atheistic Hypothesis of the World's production, Fortuitous and Mechanical must be the self-same thing. Because Fortune is no real entity nor physical essence, but a mere relative signification, denoting only this; That such a thing said to fall out by Fortune, was really effected by material and necessary causes; but the Person, with regard to whom it is called Fortuitous, was ignorant of those Causes or their tendencies, and did not design nor foresee such an effect …

But thus to affirm, that the World was made fortuitously, is as much as to say, That before the World was made, there was some Intelligent Agent or Spectator; who designing to do something else, or expecting that something would be done with the Materials of the World, there were some occult and unknown motions and tendencies in Matter, which mechanically formed the World beside his design or expectation. Now the Atheists, we may presume, will be loth to assert a fortuitous Formation in this proper sense and meaning; whereby they will make Understanding to be older than Heaven and Earth.

Then BENTLEY explains his own point of view:

All events called Casual, among inanimate Bodies, are … produced according to the determinate figures and textures and motions of those Bodies; with this negation only, That those … Bodies are not conscious of their own operations.

It is true that the most authoritative representative of atheism in those times, HOBBES[53], held that by definition fortuitous means something whose necessary causes are unknown. But this was also the point of view of JAKOB BERNOULLI and LAPLACE and could be traced at least to THOMAS AQUINAS (see below).

What is more, it seems that BENTLEY'S criticisms were directed against what was later called *subjective idealism*, usually ascribed to BENTLEY'S compatriot and junior contemporary G. BERKELEY, also notable for his struggle against atheism and materialism. In at least one of his works[54] we find the same attribution of casualty to atheists.

Now, however, I am primarily concerned not with BENTLEY[55] or BERKELEY but with DE MOIVRE, and am satisfied to conclude that it seems rather reasonable that he being also influenced by NEWTON, denounced casualty as opposed to NEWTON'S determinism, and that seeing no casualty in nature he also equated it with ignorance of necessary causes. And this, as I noticed before, was in my opinion the reason that DE MOIVRE did not consider a binomial distribution (§ 2.1.2) as a distribution of a random quantity. Nor, except for the uniform distribution (binomial distribution with $p = 1/2$), did he consider any distributions as such. Thus in his works, as in ARBUTHNOT'S memoir, the uniformly distributed random quantity is opposed to determinate quantities in general[56].

The uniform distribution was the only used in natural science prior to DARWIN and BOLTZMANN, see FREUDENTHAL[57], who relates its reason

To the old controversy whether the world has been created … by chance or by reason. The latter party held that by chance all things are equally probable, and so chance can only create a chaos.

I supplement FREUDENTHAL by holding that the uniform distribution was opposed to determinism at large. But other distributions, notably the triangular (SIMPSON, 1756 and and 1757) and the normal ones, were used in the theory of errors (which however is not a natural scientific discipline) long before DARWIN and BOLTZMANN. As to NEWTON'S irregular die (§ 1.1), this was an important methodological example though not applied to natural science.

I return to DARWIN and the theory of errors in the *Addendum*.

LAMBERT[58] also mentioned the case of equal probabilities:

Die gleiche Möglichkeit aber gründet sich, auch bey der weisesten Einrichtung des Laufs der Dinge in der Welt, auf die Menge einzelner Ursachen, die bey den Glücksspielen … jede nach ihren eigenen Gesetzen so zusammentreffen, dass sie eben so leichte den einen Fall als den andern hervorbringen, und bey Fortsetzung des Spieles einander compensiren[59]. Dadurch aber kommt jeder Fall desto häufiger vor, je wahrscheinlicher er an sich ist.

And elsewhere he[60] stated that

Bey Voraussetzung des blindes Zufalles eine durchaus gleiche Möglichkeit aller Fälle annimmt.

Obviously considering the blind chance incogitable, he even compared it with $\sqrt{-1}$.

Without mentioning equal possibilities but possibly bearing them in mind, EULER[61]repeatedly stated that the *pur hazard* could not have been able to create the world. The most interesting in this respect is letter 44 from vol. 1[62]:

Cependant les Athées ont la hardiesse de soutenir que les yeux aussi bien que le monde tout entier, ne sont que l'ouvrage d'un pur hazard.

Pur hazard is the same as *blinder Zufall*. This is obvious, but I quote LAMBERT[63]:

Nennen wir diese Reihe einen durchaus oder absoluten blinden Zufall, blindes Ungefähr, einen Casum purum, le pur hazard etc.

Had LAMBERT an intention of including an English equivalent, he would possibly have had nothing against using DERHAM'S *blind chance* or *blind Fate*.

2.3. Laplace

2.3.1. Laplacian Determinism.

Une intelligence qui, pour un instant donné, connaitrait toutes les forces dont la nature est animée et la situation respective des êtres qui la composent …

This passage[64] is quoted *ad nauseam* and led to the appearance of the special term, *Laplacian determinism,* in which randomness was only allowed out of ignorance. Indeed, on p. viii LAPLACE continues:

La courbe décrite par un simple molécule d'air ou de vapeurs est réglé d'une manière aussi certaine que les orbites planétaires: il n'y a de différence entre elles que celle qu'y met notre ignorance.

One is tempted to explain this determinism by LAPLACE'S own achievements in astronomy: he succeeded to explain almost all the known motions of celestial bodies of the solar system by a single law, the law of gravitation. Furthermore, he proved (or thought he proved) the stability of the solar system, thereby refuting NEWTON'S idea[65] of a deity needed to execute *reformations*.

Even at the very beginning of his career LAPLACE made essentially the same remark as quoted above on the *état de l'univers*[66] and therefore

Determinism and a strictly causal view of change in nature leaving no room for arbitrariness or lawless intervention, were in fact the metaphysical presuppositions with which he began his career.

This is the remark of R. HAHN[67] who also quotes from the 1773 memoir and notices that the *source of inspiration* for LAPLACE was CONDORCET.

LAPLACIAN determinism is not LAPLACIAN at all, as shown in § 2.2. In particular, the relation of randomness to ignorance was the prevalent idea of thinkers beginning at least with THOMAS AQUINAS[68], and in the theory of probability the first to advocate this idea was JAKOB BERNOULLI[69]. He distinguished randomness (*contingens liberum*) *quod ab arbitrio creaturae rationalis … dependet* and randomness proper (*contingens fortuitum et casuale*) *quod à casu vel fortuna dependet* and asserted that random events depend on *potentia remota, non proxima*, whose action is either impossible or difficult to consider. Thus, *contingency mainly depends on our knowledge* (p. 213).

One of the corollaries of the *Laplacian determinism* had possibly been the absence of a formalized conception of a random quantity in his works (as also was the case with DE MOIVRE). Repeatedly introducing various laws of distribution, noticing their general properties[70], calculating their convolutions[71] etc., LAPLACE nevertheless each time kept himself within the bounds of his specific problems and therefore left behind him no general discussion of these laws *per se*.

As a sideline, I notice that LAPLACE, frequently used the so-called BAYESIAN conception, which allows the estimation of the behaviour of a random quantity by its prior distribution. Sometimes he considered that an unknown but constant parameter possessed a prior distribution, thereby equated it with a random quantity[72].

This is also seen in LAMBERT (see the Addendum) and possibly in other scholars and at that time it obviously did not lead to confusion. Similarly DANIEL BERNOULLI did not distinguish a random quantity and its expectation, although no one noticed it[73].

Lastly LAPLACE extensively used what I shall call statistical determinism, or stability of mean statistical values. It was introduced by GRAUNT and others (in § 1.2 I noticed a glimpse of this idea in NEWTON) and mathematically treated by JAKOB BERNOULLI (the law of large numbers), but LAPLACE was the first to use the stability of mean values for the solution of mathematically rather difficult problems (in demography and natural science). What is more, he[74] definitely noticed this statistical determinism in voluntary acts, thus anticipating QUETELET: a government-sponsored lottery is just another tax:

On dit encore que cet impôt est volontaire. Sans doute il est volontaire pour chaque individu; mais, pour l'ensemble des individus, il est nécessaire; comme les mariages (!), *les naissances et tous les effets variables sont nécessaires, et les mêmes à peu près, chaque année, lorsqu'ils sont en grand nombre; en sorte que le revenu de la loterie est au moins aussi constant que les produits de l'agriculture*.

He provided moral (and even fiscal) arguments for the suppression of the lottery, as for example:

Qu'on se repelle ce qui a été dit mille fois contre l'immoralité de ce jeu et sur les maux qu'il occasionne

or that a *grand objet* is sacrificed in favour of *petites considérations fiscales.*

2.3.2. Induction. LAPLACE'S classical achievements in astronomy had been possible precisely because he applied the theory of probability to estimate the statistical significance of observations collected sometimes over many centuries. Thus, groping for laws and successfully overcoming tremendous difficulties, he then proved them deductively. LAPLACE describes this general method in a series of his memoirs and also in his *Essai philosophique* (1814/1995, p. 112):

Analysis and natural philosophy owe their most important discoveries to this fecund method which is called <u>induction</u>. Newton was indebted to it for his binomial theorem and the principle of universal gravitation.

But induction alone is not sufficient (p. 113):

It is always necessary to corroborate it with proofs or by conclusive experiments.

This, then, is what I relate with the appropriate passage from Newton in § 1.. And it was Jakob Bernoulli's goal to elevate the inductive, posterior probability (2) to the level of the deductive, prior probability (1) which led him to his law of large numbers.

2.3.3. Theory of Probability. Like his predecessors (e. g., DE MOIVRE and even LEIBNIZ) LAPLACE defines probability (1) starting from equipossibility, which is presumed[74a]

Lorsque rien ne porte à croire que l'un de ces cas (chances) *doit arriver plutôt que les autres, ce qui les rend, pour nous, également possibles*.

Or, LEIBNIZ[75],

C’est l’axiome, aequalibus aequalia, pour les suppositions egales il faut avoir des considerations egales.

That such a definition of probability as (1) is unclear was possibly widely known although not stated until this century, when, for example, MARKOV[76], while introducing the (Laplacian) definition of equipossibility, added that the definition is either insufficiently clear or incomplete but that it is hardly possible to improve it. And so it certainly was, within the classical theory. POINCARÉ, then SMOLUCHOVSKI and BOREL[77] also mentioned earlier that the definition of probability is non-strict. And KHINCHIN[78] remarked that

Each author invariably reasoned about equipossible and favourable chances, but endeavoured to leave this unpleasant subject as soon as possible.

But to the absence of causes. It occurs in LAPLACE’S various memoirs and testifies not to his subjectivism but rather to his use of a definite statistical method, an anticipation of the method of the null hypothesis. Studying various observational series, LAPLACE sometimes rejected the absence of causes. So, for example, when studying the diurnal variation of the atmospheric pressure and having at his disposal its morning and evening measurements for 400 days, LAPLACE calculated the difference (q) between the sums of these measurements and proceeds thus[78a]:

Pour déterminer avec quelle probabilité cette cause (of $q \neq 0$) *est indiquée, concevons que cette cause n’existe point, et que la différence observée q résulte des causes perturbatrices accidentelles et des erreurs des observations*.

After calculating this probability, LAPLACE decided in favour of the opposite hypothesis that the observed q was statistically significant. Such was his general but not sufficiently formalized method. Testing hypotheses, the master did not leave definitions of a significance level, of errors of the first and second kind, etc.

3. Addendum

3.1. Lambert: Randomness. Without repeating what was said about the random quantity it is worth noting that, contrary to the theory of probability at large, in the theory of errors frequency curves had been understood as laws of random phenomena (LAMBERT, SIMPSON, then LAPLACE and GAUSS). And precisely GAUSS[79] first formalized the notion of such curves. The development of the notion of the random quantity (and equipossibility) in the beginning of the 20th century is connected with POINCARÉ, SMOLUCHOWSKI, MISES and BOREL. I will describe neither these nor still later developments, but I feel it necessary to say a few words about LAMBERT[80]. Blind chance

Schließt alles, was in den Reihen der Dinge und ihrer Ordnung, Auswahl, Zusammenhang … heißt, schlechthin aus (§ 315)
and is contrary to *absolute oder fatale Nothwendigkeit* (§ 317).

However, absence of *Ordnung* in these *Reihen* is not yet sufficient evidence of randomness (§ 316) and (same section)

Bey dem Zufall die Existenz der Unordnung desto wahrscheinlicher je mehr an sich schon Unordnungen möglich sind, als Ordnungen.

Then (§ 318),

Umgeachtet bey dem blinden Zufall ebenfalls Ordnung in den Dingen seyn kann, die Ordnung dennoch am unwahrscheinlichsten ist, weil unzählig mal mehr Unordnungen als Ordnungen möglich sind.

Four lines of digits 3.14159 … seem to be random and so are the digits of square roots extracted out of incomplete squares (§ 319); how should random series be distinguished from those formed by necessary laws (§ 322)?

The probability of the hundredth digit in the development of $\sqrt{2}$ to equal five is 1/10 (§ 323); and (§ 327) *locale Ordnung oder Ordnung der Stelle nach* should be distinguished from *gesetzliche oder regelmäßige Ordnung, oder Ordnung im Zusammenhänge*.

Thus, LAMBERT formulated important problems and interesting considerations. Most interesting, it seems, are his thoughts (§ 316) about the relation of disorder and randomness, which are qualitatively linked to modern ideas[81].

The calculation of probability in § 323 means that LAMBERT, as also BAYES and LAPLACE (§ 2.3.1), and as POINCARE[82], did not distinguish unknown constants from random quantities and thus to a certain extent depreciated his own goal, which was precisely to distinguish between randomness and order. (A modern discussion of this problem is, for examples, in NEYMANN[83].)
But then, even the posing of this problem was extremely important. If all of LAMBERT'S works are considered, he is to be credited as the first follower of LEIBNIZ who aimed to include the calculus of probability in a general system of logic[84]. LAMBERT'S achievements in the theory of errors are briefly discussed in my article[85].

3.2. A Probabilistic World. I concluded the classical theory of probability with LAPLACE. A new stage of its development and, especially, of its use in natural science was reached in the 19th century.

It is true that in the 18th century physics we already see a glimpse of probabilistic ideas and methods (DANIEL BERNOULLI, LOMONOSOV, BOSCOVICH), but the real breakthrough came with MAXWELL and BOLTZMANN. In statistics, says MAXWELL[86],

We meet with a new kind of regularity, the regularity of averages, which we can depend upon quite sufficiently for all practical purposes, but which can make no claim to that character of absolute precision which belongs to the laws of abstract dynamics.

The laws of abstract dynamics (= LAPLACIAN determinism) were later subjected to criticisms in that the impossibility of precise knowledge of initial conditions implies that no absolute precision could be ever contemplated[87].

SCHRÖDINGER[88] gave a short and accurate description of the introduction of the statistical method in natural science at large:

In the course of the last sixty or eighty years, statistical methods and the calculus of probability have entered one branch of science after another. Independently, to all appearance, they acquired more or less rapidly a central position in biology, physics, chemistry, meteorology, astronomy, let alone such political sciences as national economy etc. At first, that may have seemed incidental: a new theoretical device had become available and was used wherever it *could be helpful, just as the microscope, the electric current … or integral equations. But in the case of statistics, it was more than this kind of coincidence.*

On its first appearance the new weapon was mostly accompanied by an excuse: it was only to remedy our shortcoming, our ignorance of details or our inability to cope with vast observational material. … But … the attitude changes … the individual case is entirely devoid of interest. … The working of the statistical mechanism itself is what we are really interested in. … The first scientific man aware of the vital role of statistics was C. Darwin. His theory hinges on the law of big (!) *numbers.*

Mentioning physics, SCHRÖDINGER obviously means, in the first place, thermodynamics. As to DARWIN, the role of his theory in establishing mathematical statistics is clearly noticed by the founders of *Biometrika*:

A very few years ago, begins the editorial to the first volume of this periodical[89],

All those problems which depend for their solution on a study of differences between individual members of a race or species, were neglected. … The starting point of Darwin's theory of evolution is precisely the existence of those differences. … The first step in an enquiry into the possible effect of a selective process upon any character of a race must be an estimate of the frequency with which individuals, exhibiting any given degree of abnormality with respect to that character, occur. …

These, and many other problems, involve the collection of statistical data on a large scale.

This editorial is preceded by a picture, on a separate plate, of DARWIN'S statue at Oxford, complete with a caption *Ignoramus, in hoc signo laboremus.* A second editorial follows[90]:

The problem of evolution is a problem in statistics. … We must turn to the mathematics of large numbers, to the theory of mass phenomena, to interpret safely our observations. …

May we not ask how it came about that the founder of our modern theory of descent made so little appeal to statistics? … The characteristic bent of C. Darwin's mind led him to establish the theory of descent without mathematical conceptions; even so Faraday's mind worked in the case of electro-magnetism. But as every idea of Faraday allows of mathematical definition, and demands mathematical analysis … so every idea of Darwin, variation, natural selection ... seems at once to fit itself to mathematical definition and to demand statistical analysis. Nor was the statistical conception itself entirely wanting in Darwin's work (an example follows). *That Darwin's mind did not work easily in mathematical lines is, perhaps, best evidenced in … a letter of 1857. … But that he realised the importance of the statistical method for his investigations is evidenced not only by this very passage, but by several others ...*[91]

The biologist, the mathematician and the statistician have hitherto had widely differentiated field of work. … The day will come … when we shall find mathematicians who are competent biologists, and biologists who are competent mathematicians.

Extremely interesting as these considerations are, the authors could have added that after DARWIN the ancient – NEWTONIAN – LAPLACIAN determinism, as far as biology was concerned, came to an end. The uniformity in the bodies of animals (see NEWTON'S reasoning in § 1.4) was no longer allowed to be the effect of choice.

Acknowledgement is due to Dr. M. V. CHIRIKOV for information about the book of BYRNE (note 68), to Professor R. HAHN for offprints of his works and to Professor W. KRUSKAL for comments on the first version of this paper.

Notes

1. NEWTON was hardly interested in games of chance, either practically or theoretically. Only for completeness it is worth mentioning that in 1693 SAMUEL PEPYS (1633 – 1703), a member of the Royal Society and its president at the time NEWTON'S *Principia* was first published, while designing to solve a problem in a game of chance entered into a correspondence with NEWTON (I. NEWTON, *Correspondence (1688 – 1694)*. Cambridge, 1961). NEWTON had solved that problem but neither it itself nor NEWTON's solution warrant special attention. As FLORENCE N. DAVID put it:

Newton's interpretation [of the problem] *would have been solved by any of the French statisticians*[mathematicians] *discussing gaming problems between 1650 and 1660. One has indeed the feeling that Galileo would have made short work of it by the method of exhaustive enumeration.*

(F. N. DAVID, Newton, Pepys and Dyse: a historical note. *Annals of sci.*, vol. 13, 1957 (1959). pp. 137 – 147, see p. 140.)

And on p. 146:

The letters [of NEWTON] *are certainly pleasing in their clear logic … but mathematically they are disappointing and leave one with the feeling that the master could have accomplished more had he been so inclined.*

See also E. D. SCHELL, S. Pepys, I. Newton and probability. *Amer. Statistician*, vol. 14, No. 4, 1960, pp. 27 – 30, who notices that NEWTON sees *no philosophical difficulties in applying probability to the outcome of a single cast*.

2. J. GRAUNT, *Natural and political observations made upon the bills of mortality* (1662), Baltimore, 1939.

3. E. HALLEY, An estimate of the degree of mortality of mankind etc. (1693.) PT abridged, vol. 3, pp. 483 – 491, 510 – 511.

4. *Math. Papers*, vol. 1 (1664 – 1666). Editor, D. T. WHITESIDE. Cambridge, 1967, pp. 58 – 61.

5. I am inclined to support this comment. This tract (1657) of HUYGENS appeared as an appendix to Fr. A. SCHOOTEN's *Exercitationum mathematicarum*, see C. HUYGENS, *Oeuvr. Compl.*, t. 14. La Haye, 1920, pp. 49 – 91. In the same volume are nine appendices to this tract, written by HUYGENS before 1688.

The *Ratiociniis* appeared in English; I have seen the anonymously published fourth edition *Of the laws of chance, or, a method of calculation of the hazards of game* etc. Revised by JOHN HAM. London, 1738.

6. ELLIS and COURNOT are named by MISES as his predecessors. See the Einleitung to his *Wahrscheinlichkeitsrechnung und ihre Anwendung in der Statistik und theoretischen Physik*. Leipzig – Wien, 1931.

Following is a quotation from A. A. COURNOT, *Exposition de la théorie des chances et des probabilités*. Paris, 1843 (Préface, p. iii) [also 1984. Editor, B. Bru. **S, G,** 54]:

De même la théorie des probabilités a pour objet certains rapports numériques qui prendraient des valeurs fixes et complétement déterminées, si l'on pouvait répéter à l'infini les épreuves des mêmes hasards, et qui, pour un nombre fini d'épreuves, oscillent entre des limites d'autant plus resserrées, d'autant plus voisines des valeurs finales, que le nombre des épreuves est plus grand.

Other relevant sections of this work are §§ 238 and 240, art. 5.

7. *Of the laws of chance*, p. 49.

8. T. SIMPSON, *The nature and laws of chance*. London 1740. See problem xxvii on p. 67: *In a parallelopipedon, whose sides are to one another in the ratio of* *a*:*b*:*c*; *To find at how many throws any one may undertake that any given Plane*, *viz*. *ab*, *may arise*.

9. Grundlagen der Wahrscheinlichkeitsrechnung (1919). See his *Selected papers*, vol. 2. Providence, 1964, pp. 57 – 105 (p. 58). Mentioning this example without referring to the exact source, A. YA. KHINCHIN called it

The celebrated example, *due to Mises unsurpassed in validity and simplicity of argumentation*.

See p. 95 of the first part of his article (posthumously published by B. V. GNEDENKO), The frequentist theory of R. Mises and modern ideas of the theory of probability (in Russian.) *Voprosy filosofii*, 1961, No. 1, pp. 92 – 102 and No. 2, pp. 77 – 89. [My translation: *Science in context*, vol. 17, 2004, pp. 391 – 422.]

The irregular die appears also in J. H. LAMBERT, although only as an example of unequal probabilities of different events. See *Phänomenologie oder Lehre von dem Schein*, § 152 (in his *Neues Organon*, Bd. 2. Leipzig, 1764, pp 217 – 435). I return to this source later.

10. *Chronology of ancient kingdoms amended*. London, 1728. See p. 52. [London, 1770.]

11. See TODHUNTER, § 746.

12. R. L. ELLIS, On a question in the theory of probability (1844). In *Mathematical and other writings of R. L. Ellis*. Cambridge, 1863, pp. 173 – 179 (see p. 179).

13. K. PEARSON, Biometry and chronology. *Biometrika*, vol. 20A, pts. 3 – 4, 1928, pp. 241 – 262, 424.

14. Remarks on the Observations made on a chronological index of I. Newton, translated into French by the *Observator*, and published at Paris (1725). *PT abridged*, vol. 7, pp. 89 – 93.

15. F. E. MANUEL, *I. Newton historian*. Cambridge (Mass), 1963.

16, 17. Not needed.

18. *Lectiones opticae* (1669 – 1671). In: *Opera quae extant omnia*, vol. 3. London, 1782, pp. 250 – 437. A Russian translation, edited by S. I. VAVILOV, appeared in 1946.

19. Answer to letter of Mr. Lucas. (1676.) *PT abridged*, vol. 2, pp. 338 – 343. See p. 341.

Almost all NEWTON'S optical memoirs and letters published during his lifetime are collected in *Newton's papers and letters on natural philosophy*. Editor, I. B. COHEN assisted by R. E. SCHOFIELD. Cambridge 1958.

Regarding random and systematic errors, Whiteside commented in a private letter:

Newton in fact (*but not in explicit statement*) *had a precise understanding of the difference between random and structurally inbuilt errors. He was certainly, himself, absorbed by the second type of inbuilt error, and his many theoretical models of differing types of physical, optical and astronomical phenomena were all consciously contrived so that the structural errors should be minimized. At the same time, he did, in his astronomical practice, also make suitable adjustments for random errors in observation.*

20. I. NEWTON, *Correspondence*, vol. 4 (1694 – 1709). Cambridge, 1967. See pp. 67 and 86.

21. See for example NEWTON'S Letter containing his new theory of light and colours (1672). *PT abridged*, vol. 1, pp. 678 – 688.

22. See part 1, sect. 2, § 38, p. 283. The quotation in main text is a translation from Russian (see note 18).

23. I. NEWTON, An hypothesis explaining the properties of light, discoursed of in my several papers. (1757). In: *Papers and letters* ..., pp. 178 – 199; see p. 192.

24. *Correspondence*, vol. 4, pp. 67 and 134.

25. *Correspondence*, vol. 1 (1661 – 1675). Cambridge, 1959, pp. 136 – 139, 407 – 411, 421 – 425. NEWTON'S work on the effect of errors and aberrations of optical systems is outside my goal, and only mention his works on this subject: Letter dated May 4, 1672. *PT abridged*, vol. 1, pp. 712 – 714; Answer to some considerations on his (NEWTON'S) doctrine of light and colours (1672). Ibidem, vol. 2, pp. 13 – 29 (see pp. 14 – 15); On the number of colours and the necessity of mixing them all for the production of white (1673). Ibidem, pp. 91 – 93. Then, his letters in

Correspondence, vol. 1, pp. 247 – 253, 269 – 271; vol. 2 (1676—1687). Cambridge, 1960, pp. 254 – 260.

26. *Lectiones opticae*, part 1, sect. 2, § 33, p. 278. The translation is again from Russian.

27. I. NEWTON, Particular answer to Linus's objections. (1676). *PT abridged*, vol. 2, pp. 276 – 280. See p. 279.

28. *Mathematical principles of natural philosophy*. Cambridge, 1934. (A revised reissue of the 1729 edition with a historical and explanatory appendix by FLORIAN CAJORI.) The quotation is from p. 544.

29. *Optics, or a treatise of the reflexions, refractions, inflexions and colours of light*. (*Opera quae extant omnia*, vol. 4. London, 1782, pp. 1 – 264. The date of the original publication of this edition of the *Optics* is not stated, but there is an *Advertisement* by NEWTON, dated 1717, so that this is likely a reissue of the 1718 edition mentioned in the *British Museum Catalogue of Printed Books*.) The quotation is from part 1, book 3, qu. 28 (p. 237).

30. The generally known place concerning hypotheses is in the General Scholium to the *Principia* (see p. 547 of the edition mentioned in note 28):

I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses. (*Hypotheses non fingo*.)

CAJORI (pp. 671 – 676) appends a lengthy explanation of how actually to understand this phrase.

31. F. E. MANUEL, *A portrait of Newton*. Cambridge (Mass), 1968, p. 127. The *Astro-Theology* to which I return below was first published in 1715.

32. A short account is in TODHUNTER, §§ 394 – 397 and 987.

33. K. PEARSON, Abraham De Moivre. *Nature*, vol. 117, No. 2946, 1926, pp. 551 – 552. This is a discussion with R. C. ARCHIBALD. For another reference see note 13.

34. The memoir in which DE MOIVRE proved the "De Moivre-Laplace limit theorems". DE MOIVRE inserted his English translation of this memoir in the *Doctrine of chances* (first published in 1718) in its 1738 edition and in the posthumous edition of 1756 (reprint: New York, 1967) in which that translation (enlarged) occupies pp. 243 – 254.

35. M. BORN, *Natural philosophy of cause and chance*. Oxford, 1949.

36. CARUS (TITUS) LUCRETIUS, *De rerum natura*, vol. 1. London, 1886. See book 2, lines 216 – 224, 251 – 262, 292 – 293.

37. E.g., *De Partibus Animalium*, I 1, 641b 15.

38. *Lettres à une princesse d'Allemagne*, t. 2 (1768), Letter 113. See p. 269 of EULER'S *Opera omnia*, ser. 3, vol. 11. Turici, 1960.

39. *Physico-Theology or a demonstration of the being and attributes of God from his works of creation*. Preached in 1711 – 1712. London, 1768 (the 13th edition). See p. 313.

A letter of DERHAM to NEWTON (1714) is referred to in D. BREWSTER, *Memoirs of the life of Newton*, vols. 1 – 2. London, 1865. See p. 520 of vol. 2. In this letter DERHAM asks NEWTON to honour his promise of giving *castigations* for the third impression of the *Physico-Theology*.

40. *Astrotheology or a demonstration of the being and attributes of God from a survey of the heavens*. Fourth edition, much corrected. London, 1721.

41. An argument for divine Providence, taken from the constant regularity observed in the births of both sexes (1712). In M. G. KENDALL, R. L. PLACKETT, editors (1977), *Studies in history of statistics and probability*, vol. 2. London, pp. 30 – 34.

42. H. FREUDENTAL, 250 years of mathematical statistics. In: *Quantitative methods in pharmacology*. Editor, H. DE JONGE. Amsterdam, 1961, pp. xi – xx. See p. xi.

43. *Specimina artis conjectandi, ad quaestiones iuris applicata*. Basle, 1709 and 1711. See TODHUNTER, §§ 338 –341. BERNOULLI used the continuous uniform distribution and an order statistic. Elsewhere (On the early history of the law of large numbers. *Biometrika*, vol. 55, No. 3, 1968, pp. 459 – 467) I have erroneously ascribed this distribution to DE MOIVRE.

The appearance of the exponential function of the e^{-xx} type, although implicit, is also due to N. BERNOULLI (letter to P. R. MONTMORT dated Jan. 23, 1713, published by the latter in his anonymous *Essay d'analyse sur les jeux de hazard*. Paris, 1713, pp. 388 – 393).

44. W. KRUSKAL, Significance, tests of. *Intern. Enc. of the Social Sciences*, vol. 14, 1968, pp. 238 – 250.

45. E. BOREL, *Probabilité et certitude*. Paris, 1956, § 56:

Des observations analogues pourraient être faites au sujet des tentatives que l'on pourrait imaginer, d'appliquer le calcul des probabilités aux problémes cosmogoniques. Dans ce domaine également, il ne semble pas que les résultats que nous avons exposés puissent être actuellement d'un grand secours.

46. E. BOREL, *Probabilité et certitude*, § 55 and *Le hasard*. Paris, 1914, § 112.

47. *Logique de Port-Royal*. (1662). An anonymous work of A. ARNAULD & P. NICOLE. Quotation from English translation: *Logic, or the art of thinking: being the Port-Royal* logic. Edinburgh – London, 1850. See chapt. 16, p. 360.

48. TODHUNTER, § 493.

49. *Exposition de la théorie des chances* etc., § 240, art. 8.

50. *Doctrine of chances*, 1718. Lacking in the second edition, this *Dedication* is appended to the third edition (p. 329). All the following quotations are also from this third edition.

51. An essay towards solving a problem in the doctrine of chances (part 1) (1763). *Biometrika*, vol. 45, No. 3 – 4, 1958, pp. 296 – 315. See p. 299.

52. The letters of NEWTON to BENTLEY are published in NEWION'S *Papers and letters on natural philosophy*. The same book contains an article on NEWTON and BENTLEY by P. MILLER (pp. 271 – 278) and two of BENTLEY'S sermons originally published in 1693. The quotations from BENTLEY are from this source (pp. 316 – 318). The entire existing correspondence between BENTLEY and NEWTON is published in NEWTON'S *Correspondence*, vol. 3 (1688 – 1694). Cambridge, 1961.

53. T. HOBBES, *Elementorum philosophiae*, sectio prima (*De Corpore*), pars secunda, caput 10, § 5. See his *Opera philosophica*. Amstelodami, 1668, p. 69 of the *Elem. Philos*, separate paging.

54. *Three dialogues between Hylas and Philonous* (1713). In: *The works of G. Berkeley*, vol. 2. Editors, A. A. LUCE & T. E. JESSOP. London, 1949. See *Second dialogue*, p. 213.

55. COTES highly praised BENTLEY in his preface to the second edition of the *Principia* (see the revised edition of NEWTON'S *Mathematical principles*, 1934):

Newton's distinguished work will be the safest protection against the attacks of atheists. … This was felt long ago and first surprisingly demonstrated in … discourses by R. Bentley. … For many years an intimate friend of the celebrated author, …. he cared both for the reputation of his friend and for the advancement of the sciences … he persuaded … the splendid man … to grant him permission for the appearance of this new edition … at his expence and under his supervision. He assigned to me the not unwelcome task of looking after the corrections.

56. It seems, though, that the case of the continuous uniform distribution, which also appears in DE MOIVRE (see my article mentioned in note 43), is somewhat different.

57. *250 years of mathematical statistics*, p. xii.

58. *Phänomenologie*, § 152 (see note 9).

59. POINCARÉ later developed this seemingly natural outstanding idea.

60. *Anlage zur Architectonic*, Bd. 1. Riga, 1771. See §§ 314 and 324.

61. *Lettres à une princesse*, t. 1, 1768, letter 21 and t. 2, letter 113.
The general pattern of EULER'S philosophy is discussed, for example, by W. W. KOTEK, Les tendances matérialistes dans les opinions de L. Euler sur le monde. (In Russian.) *Actes du XIe congrès international d'histoire des sciences 1965*. Wroclaw, 1967, vol. 3, pp. 221 – 224.

62. *Opera omnia*, ser. 3, vol. 11, p. 100.

63. *Anlage zur Architectonic*, Bd. 1, § 311.

64. *Essai philosophique sur les probabilités*. (1814). *Philosophical Essay on Probabilities*. New York, 1995. Translator A. I. Dale.

65. See quotation from p. 262 of NEWTON's *Optics* in § 1.4.

66. P. S. LAPLACE, Recherches sur l'intégration des équations différentielles aux différences finies, et sur leur usage dans la théorie des hasards (1773, published 1776). *Oeuvr. Compl.*, t. 8, Paris, 1891, pp. 69 – 197. See p. 144.

67. R. HAHN, Laplace's first formulation of scientific determinism in 1773. *Actes du XIe eongrès international d'histoire des sciences 1965*. Wroclaw, 1967, vol. 2, pp. 167 – 1 71. See also HAHN, Laplace as a Newtonian scientist. Booklet, Will. Andrews Clark memorial library (Univ. of Calif., Los Angeles, 1967).

68. M. G. KENDALL, The beginnings of a probability calculus. *Biometrika*, vol. 43, No. 1 – 2, 1956, pp. 1 – 14. E. F. BYRNE, *Probability and opinion. A study in the medieval presuppositions of post-medieval theories of probability*. The Hague, 1968, extensively discussed the role of THOMAS AQUINAS as the originator of the medieval ideas on probability and their relation to some of the modern ideas.

69. *Ars conjectandi*, pars quarta, caput 1, p. 212. Bruxelles, 1968. (A photographic reprint of the original 1713 edition.)

70. Mémoire sur la probabilité des causes par les événements. (1774). *Oeuvr. Compl*. t. 8. pp. 27 – 65.

71. Mémoire sur l'inclinaison moyenne des orbites des comètes etc. (1773, published 1776). Ibidem, pp. 279 – 321.

72. E. g., Mémoire sur les probabilités (1778, published 1781). Ibidem, t. 9. Paris, 1893, pp. 383 – 485. In § 19 LAPLACE finds the probability that the possibility (considered constant) of the birth of a boy is higher than 1/2. For a description of the demographic works of LAPLACE see TODHUNTER §§ 902 and 1026.

73. Neither does TODHUNTER (§§ 410 and 417) offer any such comment.

74. Sur la suppression de la loterie. (1819). *Oeuvr. Compl.*, t. 14. Paris, 1912, pp. 375 – 378.

74a. *Théorie analytique*, chapitre 1, p. 181. See also his Recherches sur l'intégration des équations différentlelles (1773, published 1776). *Oeuvr. Compl.* t. 8, pp. 69 – 197, see § 25.

75. *Nouveaux essais sur l'entendement humain. Neue Abhandlungen über den menschlichen Verstand.* (Bilingual edition.) Bd. 2. Frankfurt/Main, 1961. See book 2, chapter 16, p. 514.

76. A. A. MARKOV, *Ischislenie veroiatnostei* (*Calculus of probability*). Moscow, 1924, 4th edition, chapter 1, § 1, p. 2.

77. H. POINCARÉ, *La science et l'hypothèse* (1902). See his *Foundations of science*. Lancaster, Pa. 1946, pp. 9 – 197, Science and hypothesis (p. 156).

M. SMOLUCHOWSKI, *Experimentell nachweisbare, der üblichen Thermodynamik widersprechende Molekularphänomene*. (1912). *Oeuvres*, t. 2. Cracovie – Paris, 1927, pp. 226 – 251, § 2 of the article.

E. BOREL, *Le hasard*, § 6.

78. *The frequentist theory of Mises*, part 1, p. 94.

78a. *Théorie analytique*, chapt. 5, § 25, p. 356.

79. *Theoria motus* etc. (1809). *Werke*, Bd. 7. Gotha, 1871. See § 175; Theoria combinationis observationum etc, part 1. See also § 4.2 of my article mentioned in note 43.

80. *Anlage zur Architectonic*, Bd. 1, §§ 314 – 344. See also notes 9 and 58 and my article on Lambert in this collection.

81. E.g., A. N. KOLMOGOROV, On the logical foundations of the information theory, 1969 (in Russian). *Sel. Works*, vol. 2. Dordrecht, 1992, pp. 515 – 519.

82. *Science and hypothesis*. See p. 159 of his *Foundations of science*.

83. J. NEYMANN, L'estimation statistique traitée comme un problème classique de probabilité (1938). See his *Selection of early statistical papers.* Cambridge, 1967, pp. 332 – 353. NEYMANN used a quite similar example (p. 337).

84. B. RUSSELL, *A critical exposition of the philosophy of Leibniz*. London, 1900,1937; L. C0UTURAT, *La logique de Leibniz.* Paris, 1901.

85. O. B. SHEYNIN, Origin of the theory of errors. *Nature*, vol. 211, No. 5052, 1966, pp. 1003 – 1004.

86. Molecules. (*Nature*, vol. 8). See his *Scientific papers*, vol. 2. Paris, 1927, pp. 361 – 378. Quotation is from p. 374. Throughout his writings MAXWELL repeatedly refers to BOSCOVICH. For a description of BOSCOVICH's philosophical outlook see e. g. A. M. GODYTSKY-TSVIRKO, *Nauchnye idei Boshkovicha* (Scientific ideas of Boscovich). Moscow, 1959.

D. M. IVANOVIC, Molecules, stars and Boscovic's law. *Atti del Convegno internazionale celebrativo del 250 anniversario della nascita di R. J. Boscovich etc. 1962*. Milano, 1963, pp. 243 – 250, notices a glimpse of the statistical method in Boscovich's *Philosophiae naturalis theoria*, § 481.

L. L. WHYTE, Boscovich's atomism. In R. J. Boscovich. *Studies of his life and work*. Editor, L. L. WHYTE. London, 1961, pp. 102 – 126, see p. 111, offers a possible explanation for the lack of a developed kinetic theory of heat motions in BOSCOVICH (and, for that matter, in the 18th century in general):

Though Boscovich was in principle concerned with all possible arrangements and modes of interaction of puncto (= atoms) *he concentrated his attention on those properties which appeared to him simplest and, though interested in the theory of probability, he did not consider applying statistical methods to random motions.*

More important though could have been the general determinism inherent in BOSCOVICH's philosophy.

The same source contains a contribution on BOSCOVICH's work in the error theory: C. EISENHART, Boscovich and the combination of observations (pp. 200 – 213). Its shorter version appeared in *Actes du symposium international Boscovich 1961*. Beograd, 1962, pp. 19 – 25.

87. See for example the first few pages of H. POINCARÉ, Science et méthode (1908). (Science and method. In: *Foundations of science*, pp. 359 – 546.)

Without mentioning the exact source, N. WIENER, *The human use of human beings. Cybernetics and society*. New York, 1956, p. 8) notices that

There was, actually, an important statistical reservation implicit in Newton's work, though the 18th century … ignored it. No physical measurements are ever precise; and what we have to say about a machine or other dynamical system really concerned not what we must expect when the initial positions and momenta are given with perfect accuracy (which never occurs) but what we are to expect when they are given with attainable accuracy. … we know not the complete initial conditions, but something about their distribution.

88. E. SCHRÖDINGER, The statistical law in nature. *Nature*, vol. 153, No. 3893, 1944, pp. 704 – 705.

89. *Biometrika*. A journal for the statistical study of biological problems. Edited in consultation with Fr. GALTON by W. F. R. WELDON, K. PEARSON and C. B. DAVENPORT. See vol. 1, pt. 1, 1901 – 1902. The scope of Biometrika, pp. 1 – 2.

90. *The spirit of Biometrika*, pp. 3 – 6.

91. Several quotations from DARWIN are given, notably this:

I have no faith in anything short of actual measurement und the Rule of three.

Afterword

Some new sources are included in the Bibliography appended to Sheynin (2017). Then, see Lambert's collected works in the *Afterword* to my paper about him in this collection. I have not seen the English translation of the book of Hobbes (note 53): *Elements of philosophy*; in his *English works*, vol. 1, 1839.

To note 6. Bayes also was Mises' predecessor, see Sheynin (2017, § 5.2). **To § 2.1.1. The first to introduce** philosophical probabilities was Fries (1842). **Poisson**. I have not mentioned him and ought to say that he introduced the notions of distribution function and random variable, advocated studies of the significance of empirical discrepancies and studied subjective probability (Ibidem, Chapter 8).

Low probabilities. I mentioned them in few places, but have an important comment. Arbuthnot (§ 2.1.1), for example, thought that a low probability of an event proves that it does not happen. The main additional point here is that many possible outcomes can have the same low probability but some of them are remarkable which indicates their extreme rarity. The separation of events into ordinary and remarkable can however be difficult.

Fries J. F. (1842), *Versuch einer Kritik der Principien der Wahrscheinlichkeits-rechnung*. Braunschweig. *Sämtliche Schriften*, Bd. 14. Aalen, 1974.
Sheynin O. (2017), *Theory of probability. Historical essay*. Berlin. **S, G,** 10.

II

J. H. Lambert's work on probability

Arch. Hist. Ex. Sci., vol. 3, 1971, pp. 244 – 256

Summary

JOHANN HEINRICH **LAMBERT** (1728 – 1777) worked in different spheres of mathematics and its applications, including optics, map projections, and geodesy, and also in astronomy. In the theory of probability proper he is the author of a small article and of a few posthumously published notes, and his work in this sphere seems rather slight, but his philosophical reasoning in regard to probability are very interesting.

LAMBERT also published works on the then most important applications of probability: demographic statistics and the theory of errors. His contribution to the former, while essentially a commentary on the contributions of Daniel BERNOULLI, nevertheless presents a methodical elaboration of the mathematical foundation of this discipline. As to the theory of errors, LAMBERT should be credited as the main predecessor of GAUSS.

My §§ 1 – 3 are devoted to LAMBERT'S work in the theory of probability, demographic statistics; and the theory of errors. There is no complete edition of LAMBERT'S works [10], and his numerous articles are scattered in various periodicals and not readily available. For this reason I do not claim to offer a complete description, but the main deductions briefly stated above and elaborated in the paper itself seem to be established beyond reasonable doubt.

1. Theory of Probability

LAMBERT [22] calculated the probability of the realization of "random" (unscientific) predictions, and thought that believers of such predictions are superstitious. Formalizing the problem, he formulates it thus: n letters are enclosed in n addressed envelopes (all addresses different), one letter to each envelope, with an equal probability for every (subsequent) letter to be enclosed in each of the (still empty) envelopes. To find the probability that just r letters ($0 \le r \le n$) shall be inserted correctly.

N. BERNOULLI, EULER, DE MOIVRE and LAPLACE attacked an equivalent problem for n =13 (the problem of *treize* or *rencontre* see [35]). DE MOIVRE [30, p. 116] and then EULER [6] arrived at a formula for the probability of the rencontre of $r \ge 1$ cards:

$$\lim \text{Prob} = 1 - 1/e,\ n \to \infty. \qquad (1)$$

In 1819 THOMAS YOUNG, 1773 – 1829, a naturalist and a historian, considered an equivalent problem about the coincidence of words in two different languages and derived a general formula for the probability when the number of coincidences was not less than r:

$$\lim \text{Prob} = \frac{1}{er!},\ n \to \infty$$

from which (1) at once followed and which, naturally, is the formula of the POISSON distribution with unit parameter [11].

LAMBERT partly devoted his writings [16] and [17] to the introduction of subjective probability. He [16, § 76, p. 500] introduced probability as a quantitatively expressed fraction of certitude. In [17] chapter 5 is devoted to the theory of probability. Here LAMBERT explained the meaning of *probable*, of prior and posterior probabilities, introduced inverse probability and discussed the probability of testimonies of historical events. Leaving aside the evaluation of LAMBERT'S general philosophical achievements in mathematics [34], I notice that in substantiating the concept of probability he followed an ancient tradition which can be traced back at least to THOMAS AQUINAS and which manifested itself in J. BERNOULLI and LAPLACE (relation of randomness to ignorance). I also notice (and agree with) the general opinion that LAMBERT should be credited as the first follower of LEIBNIZ, who aimed to include the calculus of probability into a general system of logic.

Voluminous literature is devoted to the concept of subjective probability. I quote from a lesser known article [12]:

An estimation of the probability of any definite opinion which could really be only true or false, only possesses a temporary and subjective sense, conveys our attitude. But a subjective probability of a certain event considered chosen from a series of events occurring under reiterating conditions already possesses an objective sense.

This, nevertheless, is far from being the last word and new ideas continue to be introduced [13]. I [33, § 3.1] treated LAMBERT'S philosophical reasoning on randomness and probability. In a word, his contributions [17, § 152; 21, §§ 314 to 344] contain a qualitative link with later works of POINCARÉ which explained the occurrence of equipossibility and the uniform distribution in common cases and with some modern ideas on the relation between the theory of probability and information theory [13].

An interesting feature in LAMBERT'S work is the problem of randomness which he [21, § 311] supposes to arise out of ignorance:

Das <u>Nichtwissen</u> der Gründe, welches bey uns sehr häufig vorkömmt, und so auch das nicht vorhersehen des Erfolges, macht, dass wir die Worte eines <u>Ungefährs</u> oder eines <u>Zufalls</u> dabey gebrauchen. Aus diesem Grunde, da wir erst angeführten Ausdruck als eine Definition ansehen, beut uns die symbolische Möglichkeit auch Mittel an, zu dieser Definition das Definitum zu bilden, und so nennen wir diese Reihe einen … <u>absoluten blinden Zufall</u>.

And, again [21, § 324]:

Da die Berechnung der Wahrscheinlichkeit nur da vorkömmt, wo wir den Erfolg nicht voraus wissen können, so ist es in dieser Absicht gleich viel, ob wir denselben wegen der gleichen Möglichkeit, oder wegen des Nichtwissens der Gesetze nicht voraus wissen.

This statement is in accord with the theory of information (an equal probability of two possible events means lack of information). First of all, however, the reader will recall the principle of insufficient reason (which Keynes named principle of indifference instead).

Then, LAMBERT [33] considered infinite sequences of digits and posed the problem of distinguishing between sequences governed by chance and derived in accordance with a certain law. He certainly did not define of a chance sequence although mentions [21, § 324] that *Bey dem blinden Zufalle wird die gleiche Möglichkeit aller Fälle vorausgesetzt*, so that in infinite sequences the frequencies of occurrences of all the different digits are equal to one another. This, however, is also the case with, e. g., the infinite sequence of the digits of √2 [21, § 323], which means that the problem of distinguishing the two types of sequences was left unsolved.

Of course, equipossibility is not the only way in which chance manifests itself. As to the probability of the occurrence of different digits in the development of irrational numbers, I emphasise that such problems were posed and solved only in the 20th century (E. BOREL, 1909; A. YA. KHINCHIN, 1936).

LAMBERT'S philosophic reasoning on probability was hardly mentioned by any later scholar, in particular, by MISES, who originated a profound study of random sequences.

LAMBERT repeatedly reasoned about probability in his popular contribution [15], see also [20]. Like NEWTON, DERHAM and DANIEL BERNOULLI, he wished to prove that the regularities observed in the solar system were not produced by randomness. From a formal point of view, their attempt constituted a calculation of the odds of an occurrence of a series of independent events each of which had an equal probability of happening or failing. The probability of regularities (e. g., that all the six known planets and their satellites circulate in one and the same direction) produced by such simultaneous occurrences occurred too low, and LAMBERT, following NEWTON [33], thought that such regularities had been produced by the Creator.

The calculations involved are quite elementary; nothing new emerges either in method or in ideas; and until LAPLACE'S time no further progress accrued to astronomy from the theory of probability. Only for the sake of completeness it is worth mentioning that LAMBERT left three notes devoted to games of chance. They were published posthumously [25].

2. Demographic Statistics

It originated in the 17th century and led to the formulation and solution of important problems in probability.

LAMBERT [23] deduced an empirical law of mortality (§ 9), studied the mean and probable durations of life (§ 36), formalized the termination of marriages by deaths (§ 53), studied the distribution of the number of children in families (§ 108) and of the number of marriages in various age-groups of the population (§ 113), and, lastly, considered deaths of children from small-pox (§ 125).

His empirical law of mortality is

$$y = a(1-x/c)^2 - C(e^{-x/p} - e^{-x/q}),\ y(0) = 10{,}000, \qquad (2)$$

where x is the age and y, the corresponding number of survivors. The calculation of the parameters was not explained. LAMBERT chose the functions by analogy with those used to describe the outflow of water from a cylinder (the first term) and thermal processes (the other terms; see also [19, § 58]. D. BERNOULLI studied the outflow of water from cylinders in his *Hydrodynamica*, part 3 (1738), but it seems that a quadric does not appear there in this connection. It would not be difficult to deduce a differential equation, whose solution will be that quadric. More interesting, however, is that (2) is formed by curves which later became known as PEARSONian curves, types ix and x.

In his correspondence LAMBERT [24, pp. 365 – 368; Letter 33, dated Dec. 6, 1776] explained that his law (2) is empirical with no theoretical background. What is more, LAMBERT did not use (2) in his further exposition. Instead (§ 44), he employed the equation

$$y=ax^q,\ a > 0,\ q > 1,$$

and hastened to prove that the probable duration of life does not coincide with its mean duration. In this he obviously differed with D'ALEMBERT who thought that the existence of two different durations of life is a great objection to the theory of probability. D'ALEMBERT'S objection

Is as reasonable as an objection to the theory of mechanics would be on the ground that the centre of gravity of an area does not necessarily fall on an assigned line which bisects the area [35, § 505].

D. BERNOULLI [3] studied the termination of marriages, being guided by his own simultaneously published memoir [2] in which he considered urn problems. An urn with $2n$ strips of paper, n of them white and n black, is given. Strips of each colour are numbered from 1 through n so that each two strips different in colour but having the same number constitute a pair. His problem consisted in finding out the number of paired strips remaining in the urn after $(2n - r)$ random extractions without replacement (after the urn contained only r strips out of the original number, $2n$). BERNOULLI applied this urn problem in the case when strips of one of the colours are for some reason extracted more often than the other strips, and solved a number of problems pertaining to the duration of marriages (numbers of surviving marriages, widows, widowers etc.) for identical and different mortalities of men and women.

Similar problems constitute the subject matter of the relevant part of [23], but nothing except formalized descriptions of the problems emerge. Evidently LAMBERT thought that his problems should be solved purely statistically, and from this point of view nothing more could have been done. (It seems that EULER held to the same point of view.) Only after the establishment of state statistical agencies and the development of mathematical statistics it became possible to get trustworthy statistical data and estimate the reliability of the deduced results.

When studying the distribution of the number of children in marriages, LAMBERT proceeded from statistical data on 612 families with up to and including 14 children in each. He somehow managed to adjust his data and, although considering his work only a *Beyspiel* rather than a *Muster*, provided hardly any explanation of his method.

There were 104 childless families, 149 families with one child each etc. In the adjusted results there were 906 families with 3480 children in all (1518 children in the data), the mean ratios of children per family were, respectively, $k_2 = 3.84$ and $k_1 = 2.48$ with $k_2/k_1=1.55$ and the adjusted number of children per family up to and including 18.

All these figures are given by LAMBERT himself, and he states also that it was necessary to increase the number of children by one half and that those adjusted numbers are *um* 2/3 *näher beysammen.* I can only add that in demography systematic errors are more dangerous than random, so, evidently, the increase in children in the adjusted results should be explained by the wish to eliminate these systematic errors (deaths of children) although I do not understand his *um* 2/3 *näher beysammen.* I also notice that with $x \geq 2$ the adjusted values may be approximated by an arc of a circumference

$$(x - 17.9)^2 + (y - 16.2)^2 = 17.82.$$

LAMBERT repeatedly fitted curves and straight lines to empirical data, and I note one more example [23, § 67 and further] where he considered mortality data for London, 1753 – 1758, but see also § 3 below.

In § 68 LAMBERT states that the statistical treatment of data must include detection of irregularities; in § 69 he accepts certain qualitative features for a mortality law and among them an asymptotic approach of the mortality curve to the *x*-axis. The probability of a person attaining an age of 130 years happened to be equal to $1/10^8 > 0$ and such, as it seems, is also the modern practice of demographic and life-insurance calculations.

In § 70 and further LAMBERT selects a parabolic curve of the fifth degree to represent the law of mortality for age $x \geq 45$ and justly observes that mortality of people with life annuities is essentially different from that of the whole population. Strictly speaking he does not here adjust his data, since the parabolic curve passes through all of his empirical points.

In different cases LAMBERT selects essentially different mortality laws, which prompts me to think that he considered his contribution as a methodological commentary on the works of D. BERNOULLI.

And as far as deaths of children from small-pox is concerned (about which I have not yet said anything) this is particularly true: this part of LAMBERT'S contribution is indeed essentially a commentary on D. BERNOULLI'S memoir [l]. But then, LAMBERT, along with BERNOULLI and EULER (whom he for some reason does not mention, not even referring to [7]), should undoubtedly be credited as the originator of methodical foundations of mathematical demography [5, 27, 28].

3. Theory of Errors

3.1. General. Except for my preliminary publication [31], LAMBERT'S contributions to the theory of errors have remained almost unnoticed. It is true that GALLE [8] stated, without adducing any evidence, that GAUSS arrived at the idea of least squares while reading LAMBERT and that GAUSS himself [9] named LAMBERT among others as his predecessor. I shall now try to prove that LAMBERT should be given precedence over GAUSS as the originator of the theory of errors.

Elements of that theory occur in the work of GALILEI, who distinctly expressed its main propositions (1632, Third day) are:

inevitability of errors; equal probability of positive and negative errors; higher probability of lesser errors; concentration of the greater portion of observations in the vicinity of the true value of the observed constant; necessity of rejecting outliers. He also suggested a definite principle of treatment of indirect observations.

In 1756 – 1757 T. SIMPSON, by actually introducing random variables, proved that for two distributions the arithmetic mean was preferable to a single observations. At the same time he considered the first continuous frequency curve in the theory of errors. And even in 1722 R. COTES and, after him, a number of astronomers (MAUPERTUIS, BOUGUER) studied the influence of errors in triangulation upon the accuracy of calculated functions (lengths of meridian arcs).

Here is R. Cotes, Aestimatio errorum in mixta mathesi per variationes partium trianguli plani et sphaerici (1722). *Opera misc.* London, 1768, pp. 10 – 58. He (Ibidem) also offered a rule for the adjustment of direct observations qualitatively based on an analogy of the arithmetical mean with the centre of gravity of a system of points.

Lastly, redundant simultaneous linear algebraic equations, especially in two unknowns (the parameters of the earth's ellipsoid of rotation) had been repeatedly solved although the solutions were only qualitatively justified by stochastic considerations [32].

Such was the state of the theory of errors before the work of LAMBERT, who, from 1760 onward, devoted many pages of his writings to this theory.

3.2. Lambert's Photometria [14]. In §§ 271 – 306 LAMBERT described the properties of observational errors: (1) the absolute values of errors are finite; (2) the number of errors of a given absolute value decreases with the increase of this value and (3) the probability of errors of both signs are equal. Referring to J. BERNOULLI'S *A. C.*, LAMBERT remarked (§ 281) that the number of errors of opposite signs tend to equality with the increase in the number of observations.

He then classified errors according to their origin (§ 282), proved the necessity of rejecting the extreme observation (§§ 287 – 291), estimated the precision of observations (§ 294) using the difference of the arithmetic mean of all observations (x_0) from that of all but the most deviating observation (x_1) with $|\Delta x|/x_0$ chosen as the measure of precision.

Then, for an unspecified continuous frequency curve, LAMBERT formulated the problem of introducing a *Mittel* (a statistic) which with a maximum probability deviated the least from the real value of the observed constant (§§ 295 – 296); introduced the principle of maximum likelihood, again without specifying the appropriate curve (§ 303) which however was unimodal and conformed to the properties of *usual* random errors; and deduced the likelihood equation, stating, however, that in most cases the maximum likelihood estimate will not differ from the arithmetic mean (§ 306).

Deduction of the real value of an observed constant is a characteristic expression (and the aim) of LAMBERT and of the classical error theory in general. Only within the framework of mathematical statistics the estimation of corresponding parameters of the laws of distribution has been mostly considered (Sheynin 2007).

Thus, already in [14], LAMBERT'S achievements considerably exceeded those of GALILEI. Nevertheless, the relevant sections [14, 271 – 306] were omitted from the German translation of this work (*OSTWALD'S Klassiker* series No. 31 – 33, 1892) and, moreover, the translator asserted that that portion contained nothing of interest.

I shall now describe the subject matter of these sections in more detail. The rejection of the extreme observation is substantiated: let *a, b, c, ..., m, n,* be the errors of observation (assumed positive and *n*, the maximal error). Then the rejection of *n* decreases the error of the arithmetic mean and is advisable if (an important additional condition!) the corresponding observation is considerably separated from the rest. This reasoning was only a qualitative approach to rejection.

Consider now a frequency curve $\varphi(x - x_0)$ with a single mode x_0. The condition of maximal probability of a series of observations is

$$\varphi(x_1 - x_0)\varphi(x_2 - x_0) \ldots \varphi(x_n - x_0) = \max. \quad (3)$$

LAMBERT, however, issued *ex theoria combinationum et permutationum*:

$$\mathrm{PN}^n\mathrm{QM}^m\mathrm{RL}^l\mathrm{SK}^k = \max.$$

Here, PN, QM, … are the ordinates of the frequency curve and *n*, *m*, … are the numbers of the corresponding observations.

Condition (3) is arrived by calculating the number of ways *N* in which the observations P, Q, … occur *n, m*, … times respectively:

$$N = \frac{(n+m+...)!}{n!m!...}\mathrm{PN}^n\mathrm{QM}^m...$$

Permutations of $n + m + \ldots$ elements with recurrences are allowed here. In (3), the numerical factor is unnecessary, and, moreover, it is sufficient to change the observations infinitesimally to have $n = m = \ldots = 1$.

The ordinates PN, QM etc. are called (§ 300) the true numbers of observations (*vices verae*), which shows that LAMBERT (just like LAGRANGE somewhat later) introduced a continuous frequency curve but used concepts peculiar to the discrete case. This intermediate point of view vanished only in the works of LAPLACE and D. BERNOULLI (1778). (In contrast with LAMBERT, BERNOULLI, to whom the second introduction of the maximum likelihood principle is due, clearly contraposed this principle with the principle of the arithmetic mean.)

In § 304 LAMBERT equates to zero the logarithmic differential of (3) and introduces subtangents but does not say that the differentiation should be made with respect to the unknown mode x_0, a defect not really important for curves of the $\varphi(x - x_o)$ type.

3. 3. Lambert's Subsequent Work. In 1765, he partly repeated himself. He [18,§ 320] called the arithmetic mean *allerdings das sicherste* if only errors of both signs were equally possible and added [19, § 3] that with the increase in the number of observations this mean tended to the observed constant. This, the limit property of consistency, is true for linear estimates in general.

Then (§ 4) LAMBERT asserted that the observation deviating most from the true value is also deviating most from the arithmetic mean and vice versa. Lastly [18, § 441], he noted that the use of the mean is based on its maximum probability, which was incompatible with his own deduction of the maximum likelihood principle (§ 3.2).

All these assertions seem to be based on [18, §§ 443 – 445], where LAMBERT attempted to estimate the error of the arithmetic mean. He assumed that observations were situated within necessary boundaries and compared that mean with the mid-point between the boundaries but did not solve his problem or estimate the error of the mean. Indeed, without assuming a law of distribution his problem remained indefinite.

I also notice in [18] a classification of errors (§ 311), a description of their properties (§ 434), and even an experimental check of these properties (§§ 435 – 436); a remark on the different influence a given error can exert according to circumstances (§ 322); a qualification remark about the necessity of calculating the arithmetic mean from direct observations, not their functions (§ 322); and, finally, a deduction of the law of distribution of errors (§§ 429 – 430).

LAMBERT mentions errors due to the imperfection of optical instruments and human vision and singles out systematic errors tolerated because they are negligible, for instance, those caused by assuming the path of light to be a straight line.

LAMBERT'S experimental check of the properties of errors was based on numerous transfers of a segment with a compass. The experiment is of course primitive, but that LAMBERT did perform a check is a fact interesting in itself.

On the other hand, LAMBERT'S deduction of the law of distribution of errors in pointing a geodetic instrument is in contrast with principles of empirical proof. Here is his deduction. A point which *das Auge nicht mehr unterscheidet* is considered as though seen through a magnifying glass and is assumed to be a circle, since there is *kein Grund* for it to be of an angled form (*eckicht*). If that point ADBE is covered by the hair of the cross-hairs, it, the hair, might have as many positions as there are points in the diameter AB. The possibility of each position of a vertical chord DE corresponds to the number of points in DE, and the half-length of DE will be the possibility of the ensuing error. The distribution curve thus takes the shape of a semi-circumference.

LAMBERT goes on to study the errors of angle measurements by using two circumferences and then, starting from his distribution curve, arrives at the main properties of errors (see § 3.2). I doubt that any experiments were made for deriving that distribution, and, as far as natural objects serve as targets for pointing, the main error occurs because these targets are mostly illuminated by the Sun from aside.

Note, however, that LAPLACE [26] later derived a certain frequency law by assuming a proposition only since there were no reasons for etc.

LAMBERT'S formal result is that the "possibility" of an error x in pointing an instrument is

$$p = \frac{2}{\pi r^2}\sqrt{r^2 - x^2}, \ |x| < r \text{ and } p = 0 \text{ otherwise},$$

but r remains unspecified.

As in 1760, LAMBERT estimates the precision of observations by comparing the arithmetic means x_0 and x_1. Similarly he compares the slopes of empirical straight lines [18, § 333;19, §§ 4, 22, 25 – 42] in the case of indirect observations. It is the formulation of the problem of estimating precision which is most interesting in his work. Before GAUSS introduced the mean square error as an estimate of precision, the treatment of observations had been accomplished without a sufficient estimation of their precision. In particular, it seems that no one (including LAMBERT himself) noticed that estimates of precision (for example, his own estimates) should be normed so as to correspond to the number of observations, thus making possible comparisons of series of observations with different numbers of observations. But then, LAMBERT is to be credited for the first numerical rules of estimating precision and practising them without fail.

In this connection I should also note DELAMBRE'S book [4], in which normed estimates of precision are used repeatedly (for instance, on pp. 59 and 235). This book was written some time between 1818 (a reference to a book published in 1818 is contained on p. 258) and 1822, when DELAMBRE died. Thus DELAMBRE was the first to introduce normed estimates of precision at about the same time as GAUSS did.

LAMBERT [19] described the solution of redundant simultaneous linear algebraic equations. Like his contemporaries, he [32] clearly understood the similarity of treating direct and indirect observations. This is proved by the similarity in estimating precision (see above) and by the coincidence in terms: in both cases the estimate(s) of the unknown constant(s) is (are) called *Mittel*.

GAUSS and LEGENDRE, in 1806, upheld this unified viewpoint.

LAMBERT [19, § 20] fitted straight lines to sets of points or observations. He divided these observations into two groups (with lesser and greater abscissas), with equal or unequal (§ 24) numbers of observations in each. He then calculated the centre of gravity of each group and assumed that the straight line should pass through them.

Likewise he fitted curves by dividing observations into several groups. He reasonably supposed (§ 66) that an adjusting curve was more representative of the empirical data than a polynomial curve drawn through every given point.

In many instances LAMBERT considered cases in which there existed functional dependences between variables, but his ideas of using centres of gravity and of constructing adjusting curves are now used in correlation analysis.

As to the solution of redundant simultaneous equations, it should be noticed that LAMBERT, just as before in 1760, enunciated the minimax principle (minimization of the maximum residual, the minimum being sought among all possible solutions, see [18, § 420], confessing, however, that he did not know how to use this principle *auf eine allgemeine Art, imd ohne viele Umwege*. In a rudimentary form this principle should be credited to EULER [31, 32]. It was then developed by LAPLACE and CAUCHY; now it is used in decision theory and the theory of games.

The term "theory of errors" (*Theorie der Fehler*) is due to LAMBERT, who used it at first in the *Vorberichte* to Bd. 1 of the *Beyträge,* although without elaborating it and then in [18, § 321]. Here he defined the goals of this theory: to find the *Verhältnis zwischen den Fehlern, ihren Folgen, den Umstanden der Ausmessung und Güte des Instruments*. The *Theorie der Folgen* was defined independently as the study of errors of functions of observed quantities.

LAMBERT also introduces [19, § 1] the *Zuverlässigkeit* of observations and names the main problems of the treatment of observations: deduction of the true values of observed constants and estimation of the precision of observations.

He devotes [18, §§ 340 – 426] to the *Theorie der Folgen*. Using differential calculus, he arrives at most advantageous types of standard geodetic figures. He does not refer to Cotes but repeatedly mentions [29], which is unknown to me.

The general outline of the goals of all his *Theorie's* taken together is rather successful. And there is a point for singling out the *Theorie der Folgen* if, actually following LAMBERT, we are to understand it as the determinate part of a unified theory of the treatment of observations, whereas the *Theorie der Fehler* proper is its stochastic part.

I shall only add that obviously neither LAPLACE nor GAUSS used the term *theory of errors.* This may mean that that term was not put into general use until the 19$^{\text{th}}$ century. Note, however, that Bessel began applying the new term in 1820.

The emergence of error theory in LAMBERT's work as a separate scientific discipline and its division can be explained by his obvious desire to classify science, plainly seen in his philosophical writings [16;17 etc.].

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Afterword

Many new pertinent sources have appeared and are listed in the Bibliography appended to Sheynin (2017). Not included there is Lambert's works (1965 – 2020). On p. 83 of that book I mentioned Lambert's study of the influence of the Moon on air pressure and that Daniel Bernoulli encouraged him. Also there, on p. 96, Note 12, I quoted a letter from E. S. Pearson who had explained the absence of Lambert in K. Pearson (1978) by his father's old age which restricted his investigation to Condorcet, D'Alembert, Lagrange and Laplace. I disagree and believe that at the time when K. P. delivered his lectures Lambert remained barely known.

Then, contrary to the stated at the end of §1, Cournot (1851, § 33, Note) and Chuprov (1909/1959, p. 188) did discuss Lambert's philosophical reasoning on probability.

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III

J. B. J. Fourier

Historical Eloge of the Marquis De Laplace

Lond., Edinb. and Dublin Phil. Mag., ser. 2, vol. 6, 1829, pp. 370 – 381. Read 1829
At the time, this journal was called *Phil. Mag.* (ser. 2). Translation also published in *Edinb. J. of Sci.*, vol. 1, No. 11, 1829, pp. 193 – 207. Original French text published in *Mém. Acad. Roy. Sci. Inst. de France*, t. 10, 1831, pp. LXXX – CII.

[1] The name of Laplace has been heard in every part of the world where the sciences are honoured; but his memory could not receive a more worthy homage than the unanimous tribute of the admiration and sorrow of that illustrious body who shared in his labours and in his glory. He consecrated his life to the study of the grandest objects which can occupy the human mind.

The wonders of the heavens, – the lofty questions of natural philosophy, – the ingenious and profound combinations of mathematical analysis, – all the laws of the universe have been presented to his thoughts during more than sixty years, and his efforts have been crowned with immortal discoveries.

From the time of his first studies it was remarked that he possessed a prodigious memory: all the occupations of the mind were easy to him. He acquired rapidly a very extensive knowledge of the ancient languages, and he cultivated different branches of literature. Every thing interests rising genius; every thing is capable of revealing it. His earliest success was in theological studies[1]; and he treated with talent and with extraordinary sagacity the most controversial questions.

We do not know by what fortunate event Laplace passed from the study of scholastics to that of higher geometry. This last science, which scarcely admits of a divided attention, attracted and fixed his thoughts. Henceforth he abandoned himself without reserve to the impulse of his genius, and he was impressed with the conviction that a residence in the capital had now become necessary. D'Alembert was then in the zenith of his fame. It was he who informed the court of Turin that its Royal Academy possessed a geometer of the first order, Lagrange, who, without this noble testimony to his merits, might have remained long unknown. D'Alembert had announced to the king of Prussia that there was only one man in Europe who could replace at Berlin the illustrious Euler, who, having been recalled by the Russian government, had consented to return to St. Petersburg[2]. I find in the unpublished letters possessed by the Institute of France the details of this glorious negotiation which fixed the residence of Lagrange at Berlin.

[2] It was about the same time that Laplace began that long career which was destined to become so illustrious. He waited upon D'Alembert preceded by numerous recommendations which might have been considered as very powerful. But his attempts were vain, for he was not even introduced. He then addressed to him, whose suffrage he solicited, a very remarkable letter on the general principles of mechanics, of which M. Laplace has frequently quoted to me different fragments.

It was impossible that a geometer like D'Alembert could fail to be struck with the singular profoundness of this composition. On the same day he invited the author of the letter, and thus addressed him:

You see, Sir, that I hold recommendations as of very little value; you have no occasion for them. You have made yourself better known, this is sufficient for me. You are entitled to my support.

In a few days he succeeded in getting Laplace nominated Professor of Mathematics in the Military School of Paris[3]. From that moment, devoted wholly to the science which he had chosen, he gave to all his labours a fixed direction from which he never deviated. For the unchangeable purpose of his mind has always been the principal feature of his genius. He already trenched upon the known limits of mathematical analysis; he was versed in the most ingenious and powerful parts of this science and there was none more capable than he of extending its domains.

He had solved a leading question in theoretical astronomy. He formed the project of consecrating his efforts to this sublime science. He was destined to perfect it, and was able to embrace it in all its extent. He thought deeply upon this glorious purpose, and he spent all his life in accomplishing it with a perseverance of which the history of the sciences presents perhaps no other example[4].

The immensity of the subject flattered the just pride of his genius. He undertook to compose the *Almagest* of his age. This memorial he has left us under the name of *Mécanique Céleste*, and his immortal work surpasses that of Ptolemy as much as the modern analysis surpasses the *Elements* of Euclid[5].

[3] Time, which is the only just dispenser of literary glory and which sinks into oblivion contemporary mediocrity, perpetuates also [only?] the remembrance of great works. They alone convey to posterity the character of each succeeding age. The name of Laplace will thus live for ever, but I hasten to add that enlightened and impartial history will never separate his memory from that of the other successors of Newton. It will conjoin the illustrious names of D'Alembert, Clairaut, Euler, Lagrange, and Laplace. I confine myself at present to the mere mention of the great geometers whom the sciences have lost and whose researches had for their common object the perfection of physical astronomy. In order to give a just idea of their works it would be necessary to compare them, but the limits of a discourse liked this oblige me to reserve a part of this discussion for the collection of our *Memoirs*.

Next to Euler, Lagrange contributed most to the foundation of mathematical analysis. In the writings of these two great geometers it has become a distinct science, the only one of the mathematical theories of which we can say that it is completely and rigorously demonstrated[6]. Among all these theories it alone is sufficient for its own purposes, while it illustrates all the rest. And it is so necessary to them that without its aid they must have remained very imperfect.

Lagrange was destined to invent and to extend all the sciences of calculation. In whatever condition fortune had placed him, whether prince or peasant, he would have been a great geometer. This he would have become necessarily and without any effort, which cannot be said even of the most celebrated individuals who have excelled in this science.

If Lagrange had been the contemporary of Archimedes and Conon[7], he would have divided with them the glory of their most memorable discoveries. At Alexandria he would have been the rival of Diophantus. The distinctive mark of his genius consists in the unity and grandeur of his views. He attached himself wholly to a simple though just and highly elevated thought. His principal work, the *Mécanique Analytique*, might be called *Philosophical Mechanics*, for it refers all the laws of equilibrium and motion to a single principle. And, what is not less admirable, it submits them to a single method of calculation of which he himself was the inventor. All his mathematical compositions are remarkable by their singular elegance, by symmetry of form, and generality of method, and if we may so express it, by the perfection of his analytical style.

Lagrange was no less a philosopher than a great geometer. He has proved this in the whole course of his life, by the moderation of his desires, by his immovable attachment to the general interests of humanity[8], by the noble simplicity of his manners, and the elevation of his character, and by the justness and profoundness of his scientific labours.

[4] Laplace had received from nature all that force of genius which a great enterprise required. Not only has he united in his *Almagest* of the eighteenth century all that mathematical and physical sciences had already invented, and which formed the foundation of astronomy, but he has added to this science capital discoveries of his own which had escaped all his predecessors. He has resolved, either by his own methods or by those of which Euler and Lagrange had pointed out the principles, questions the most important, and certainly the most difficult of all those which had been considered before his time. His perseverance triumphed over any obstacle. When his first efforts were not successful, he renewed them under the most ingenious and diversified forms.

In the motion of the moon, for example, there had been observed an acceleration, the cause of which philosophers were unable to discover. It had been ascribed to the resistance of an ethereal medium in which the celestial bodies moved. If this had been the cause, the same cause affecting the orbits of the planets would have tended continually to disturb their primitive harmony. These stars (!) would have been constantly disturbed in their course and would have finally been precipitated upon the mass of the sun. It would have required the creating power to have been exerted anew in preventing or repairing the immense disorder which the lapse of time would have caused[9].

This cosmological question is undoubtedly the greatest which human intelligence can propose: it is now resolved. The first researches of Laplace on the immutability of the dimensions of the solar system, and his explanation of the secular equation of the moon have led to this solution.

He at first inquired if the acceleration of the moon's motion could be explained by supposing that the action of gravity was not instantaneous but subject to a successive transmission like that of light. By this means he succeeded in discovering its true cause. A new investigation then gave a better direction to his genius. On the 19th March 1787 he communicated to the Academy of Sciences a precise and unexpected solution of this great difficulty. He proved in the clearest manner that the observed acceleration is a necessary effect of universal gravitation.

This great discovery threw a new light on the most important points of the system of the world. The same theory, indeed, proved to him that, if the action of gravitation on the stars was not instantaneous, we must suppose that it propagates itself more than fifty millions of times faster than light whose velocity is well known to be 70,000 leagues in a second[1].

Hence he concluded from his theory of the lunar motions that the medium in which the stars revolve does not oppose any sensible resistance to the motions of the planets; for this cause would particularly affect the motion of the moon whereas it produces no perceptible effect.

The discussion of the motions of this planet is pregnant with remarkable consequences. We may conclude from it, for example, that the motion of rotation of the earth about its axis is invariable. The length of the day has not varied the 100th part of a second for 2000 years[11]. It is remarkable that an astronomer need not go out of his observatory to measure the distance of the earth from the sun. It would be sufficient to observe carefully the variations of the lunar motions and from this he would deduce with certainty the distance required.

A still more striking consequence is that which relates to the figure of the earth. For the form even of the terrestrial globe is impressed on certain inequalities of the lunar orbit. These inequalities would have not taken place if the earth had been a perfect sphere. We may determine the compression at the poles of the globe by the observation of the lunar motions alone, and the results hence deduced agree with the real measures which have been obtained by the great trigonometrical surveys at the equator, in the northern regions, in India, and in different countries.

It is to Laplace that we especially owe this astonishing perfection of modern theories. I cannot undertake to recount at present the series of his labours and [or] the discoveries to which they have led. The simple enumeration of them, however rapid it may be, would exceed the limits which I am obliged to prescribe to myself. Beside these researches on the secular equation of the moon, and the no less important and difficult discovery of the cause of the great inequalities of Jupiter and Saturn, we may mention those admirable theorems on the libration of the satellites of Jupiter[12]. To these we may add his analytical inquiries respecting the tides, a subject which he had pursued to an immense extent.

There is scarcely a point of physical astronomy of any importance that he did not study with the most profound attention, and he submitted to calculation most of the physical conditions which his predecessors had omitted. In the question already so complex, of the form and rotatory motion of the earth, he has considered the influence of the waters distributed between the continents, the compression of the interior strata, and the secular diminution of the dimensions of the globe.

[5] Among all these researches we must particularly distinguish those which relate to the stability of great phenomena for no object is more worthy of the meditation of philosophers. Hence it follows that those causes, either accidental or constant, which disturb the equilibrium of the ocean are subject to limits which cannot be passed. The specific gravity of the sea being much less than that of the solid globe, it follows that the oscillations of the ocean are always comprehended between very narrow limits; which would not have happened if the fluid spread over the globe had been much heavier.

Nature in general keeps in reserve conservative forces which are always present, and act the instant the disturbance commences, and with a force increasing with the necessity of calling in their assistance. This preservative power is found in every part of the universe. The form of the great planetary orbits and their inclinations vary in the course of ages, but these changes have their limits. The principal dimensions subsist, and this immense assemblage of celestial bodies oscillates round a mean condition of the system, towards which it is always drawn back. Every thing is arranged for order, perpetuity and harmony[13]. In the primitive and liquid state of the terrestrial globe the heaviest materials are placed near the centre, and this condition determines the stability of seas.

Whatever may be the physical cause of the formation of the planets[14], it has impressed on all these bodies a projectile motion in one direction round an immense globe[15]. And from this the solar system derives its stability. Order is here kept up by the power of the central mass. It is not therefore left, as Newton himself and Euler had conjectured, to an adventitious force to repair or prevent the disturbance which time may have caused. It is the law of gravitation itself which regulates all things, which is sufficient for all things, and which everywhere maintains variety and order. Having once emanated from Supreme Wisdom, it presides from the beginning of time and renders impossible every kind of disorder. Newton and Euler were not acquainted with all the perfections of the universe.

Whenever any doubt has been raised respecting the accuracy of the Newtonian law, and whenever any foreign cause has been proposed to explain apparent irregularities, the original law has always been verified after the most profound examination. The more accurate that astronomical observations have become, the more comfortable have they been to theory. Of all geometers Laplace is the one who had examined most profoundly these great questions.

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

[6] The same character appears in his researches on the analysis of probabilities, a science quite modern and of immense extent; whose object, often misunderstood, has given rise to the most erroneous interpretations, but whose application will one day embrace every department of human knowledge, a fortunate supplement to the imperfection of our nature[16].

This art originated from a fine and fertile ides of Pascal's: it was cultivated from its origin by Fermat and Huygens. A philosophical geometer, James Bernoulli, was its principal founder. A singularly happy discovery of Stirling[17], the researches of Euler and particularly an ingenious and important idea due to Lagrange have perfected this doctrine. It has been illustrated by the objections even of D'Alembert and by the philosophical views of Condorcet. Laplace had united and fixed the principles of it.

In his hands it has become a new science, submitted to a single analytical method[18] and of prodigious extent. Fertile in useful applications, it will one day throw a brilliant light over all the branches of natural philosophy. If we may here be permitted to express a personal opinion, we may add, that the solution of one of the principal questions, that which the illustrious author has treated in the 18th chapter [it does not exist] of his work, does not appear to us exact. But, taken all in all, this work is one of the most precious monuments of his genius.

[7] After having mentioned such brilliant discoveries, it would be useless to add that Laplace belonged to all the great Academies of Europe. I might also and perhaps ought to mention the high political dignities with which he was invested, but such an enumeration would only have an indirect reference to the object of this discourse. It is the great geometer whose memory we now celebrate. We have separated the immortal author of the *Mécanique Céleste* from all accidental facts which concern neither his glory nor his genius. Of what importance indeed is it to posterity who [which] will have so many other details to forget, to learn whether or not Laplace was for a short time the minister of a great nation.

What is of importance are the eternal truths which he discovered; the immutable laws of the stability of the world and not the rank which he occupied for a few years in the conservative senate. What is of importance and perhaps still more so even than his discoveries, is the example which he has left to all those who love the sciences, and the recollection of that incomparable perseverance which has sustained, directed, and crowned so many glorious efforts.

I shall omit, therefore, all the accidental circumstances and peculiarities which have no connection with the perfection of his works. But I will mention that in the first body in the state the memory of Laplace was celebrated by an eloquent and friendly voice, which important services rendered to the historical sciences, to literature, and toi the state, have for a long time illustrated (Le Marquis Pastoret)[19].

I shall particularly mention that literary solemnity which attracts the attention of the capital. The French Academy, uniting its suffrages to the acclamations of the country, considered that it would require a new glory by crowning (M. Royer-Collard) the triumphs of eloquence and of political virtue.

At the same time it chose to reply to the successor of Laplace, an illustrious academician (M. Le Comte Daru), with more than one claim, who united in literature, in history and in the public administration, every species of talent.

[8] Laplace enjoyed an advantage which fortune does not always grant to great men. From his earliest youth he was justly appreciated by his illustrious friends. We have now before us unpublished letters which exhibit all the zeal of D'Alembert to introduce him into the Military School of France, and to prepare for him, if it had been necessary, a better establishment at Berlin. The president Bochard de Saron[20] caused his first works to be printed. All the testimonies of friendship which have been given to him recall great labours and great discoveries; but nothing could contribute more to the progress of the physical sciences than his relations with the illustrious Lavoisier, whose name, consecrated in the history of science, has [had] become an eternal object of our sorrow and esteem[21].

These two celebrated men united their efforts. They undertook and finished very extensive researches in order to measure one of the most important elements of the physical theory of heat. About the same time they also made a long series of experiments on the dilatation of solid substances. The works of Newton sufficiently show us the value which this great geometer attaches to the special study of the physical sciences.

Laplace is of all his successors the one who has made the greatest use of his experimental method[22]; he was almost as great a natural philosopher as he was a geometer. His researches on refractions, on capillary attraction, on barometric measurements, on the statical properties of electricity, on the velocity of sound, on molecular action, and on the properties of gases, testify that there was nothing in the investigation of nature to which he was a stranger. He was particularly anxious about the perfection of instruments, and he caused to be constructed, at his own expense, by a celebrated artist, a very valuable astronomical instrument, which he gave to the Observatory of France.

All kinds of phenomena were perfectly well known to him. He was connected by an old friendship with two celebrated chemists, whose discoveries have extended the boundaries of the arts and of chemical theory. History will unite the names of Berthollet and Chaptal to that of Laplace. It was his happiness to reunite them. And their meetings always had for their object and for their results the increase of those branches of knowledge which are the most important and the most difficult to acquire.

The gardens of Berthollet at his house at Arcueil were not separated from those of Laplace. Great recollections and great sorrows have rendered this spot illustrious. It was there that Laplace received celebrated foreigners, men of powerful minds, from whom science had either obtained or expected some benefit, but especially those whom a sincere zeal attached to the sanctuary of the sciences. The one had begun their career the others were about too finish it. He received them with extreme politeness. He went even so far that he led those who did not know the extent of his genius to believe that he might himself draw some advantage from their conversation.

[9] In alluding to the mathematical works of Laplace, we have particularly noticed the depth of his researches, and the importance of his discoveries, but his works are distinguished also by another character which all readers have appreciated. I mean the literary merit of his compositions. That which is entitled *Système du Monde* is remarkable for the elegant simplicity of its style and the purity of its language.

There had previously been no example of this kind of composition. But we should form a very incorrect idea of the work were we to expect to acquire a knowledge of the phenomena of the heavens in such productions. The suppression of the symbols of the language of calculation cannot contribute to its perspicuity, and [or] render the perusal of it more easy[23]. The work is a perfectly regular exposition of the results of profound study: it is an ingenious epitome of the principal discoveries. The precision of its style, the choice of methods, the greatness of the subject, give a singular interest to this vast picture. But its real utility is to recall to geometers those theorems whose demonstrations were already known to them. It is properly speaking the contents of a mathematical treatise.

The purely historical works of Laplace have a different object. They present to geometers with admirable talent the progress of the human mind in the invention of the sciences. The most abstract theories have indeed an innate beauty of expression. It is this which strikes us in several of the treatises of Descartes and in some of the pages of Galileo, of Newton and Lagrange. Novelty of views, elevation of thought, and their connection with the grand objects of nature, fix the attention and fill the mind. It is sufficient that the style be pure and have a noble simplicity. It is this kind of literature that Laplace has chosen and it is certain that he has attained in it the first rank. If he writes the history of great astronomical discoveries, he becomes a model of elegance and precision. No leading fact ever escapes him, the expression is never obscure or ambiguous. Whatever he calls great is great in reality. Whatever he omits does not deserve to be cited.

[10] Laplace retained to a very advanced age that extraordinary memory which he had exhibited from his earliest years. A precious gift which, though it is not genius, is that which serves to acquire and preserve it. He had not cultivated the fine arts but he appreciated them. He was fond of Italian music and of the poetry of Racine, and he often took delight in quoting from memory different passages of this great poet. The works of Raphael adorned his apartments and they were found beside the portraits of Descartes, Francis Vieta, Newton, Galileo and Euler.

Laplace had always accustomed himself to a very light diet, and he diminished the quantity of it continually and even to an excessive degree. His very delicate sight required constant care and he succeeded in preserving it without any alteration. These cares about himself had only one object, that of reserving all his time and all his strength for the labours of his mind. He lived for the sciences and the sciences have rendered his memory immortal.

He had contracted the habit of excessive application to study, so injurious to health, though so necessary to profound inquiries, but he did not experience from it any inconvenience till during the two last years of is life.

At the commencement of the disease by which he was cut off, there was observed with alarm a moment of delirium. The sciences still occupied his mind. He spoke with unwonted ardour of the motions of the planets and afterwards of a physical experiment, which he said was a capital one. And he announced to the persons whom he believed to be present that he would soon discuss these questions in the Academy. Hi strength gradually failed. His physician (M. Magendie) who deserved all his confidence, both from his superior talents and the care which friendship alone could have inspired, watched near his bed; and Bouvard, his fellow-labourer and his friend, never left him for a single moment[25].

Surrounded with a beloved family, under the eyes of a wife whose tenderness had assisted in supporting the necessary ills of life, whose amenity and elegance had shown him the value of domestic happiness, he received from his son, the present Marquis de Laplace, the strongest proofs of the warmest affection.

He evinced his deep gratitude for the marks of interest which the King and the Dauphin had repeatedly exhibited. Those who were present at his last moments reminded him of his titles to glory and of his most brilliant discoveries. He replied:

What we know is little, and what we are ignorant of is immense.

This was at least the meaning of his last words which were articulated with difficulty. We have often heard him express the same thought, and almost in the same terms. He grew weaker and weaker but without suffering pain. His last hour had arrived. The powerful genius which had for a long time animated him, separated from its mortal coil, and returned to the heavens.

The name of Laplace honoured one of our provinces already so fertile in great men, ancient Normandy. He was born on the 23rd March 1749, and he died in the 78th year of his age, on the 5th May 1827, at nine o'clock in the morning[26]. Shall I remind you of that gloomy sadness which brooded over this placed like a cloud when the fatal intelligence was announced to you? It was on the day and even at the hour of your usual meetings. Each of you preserved a mournful silence, each felt the sad blow with which the sciences were struck. All eyes were fixed on that place which he had so long occupied. One thought only filled your minds, every [any] other meditation became impossible. You separated under the influence of a unanimous resolution, and for this single time your usual labours were interrupted[27].

It is doubtless great, it is glorious, it is worthy of a powerful nation to decree high honours to the memory of its celebrated men. In the country of Newton the ministers of state desired that the mortal remains of this great man should be solemnly deposited among the tombs of its monarchs. France and Europe have offered to the memory of Laplace an expression of their sorrow, less pompous no doubt, but perhaps more touching and more sincere.

He has received an unusual homage: he has received it from his countrymen in the bosom of a learned body, who could alone appreciate all his genius. The voice of science in tears was heard in every part of the world where philosophy had penetrated. We have now before us an extensive correspondence from every part of Germany, England, Italy, and New Holland, from the English possessions in India, and from the two Americas, and we find in it the same expressions of admiration and sorrow. This universal grief of the sciences so nobly and so freely expressed has in it no less truth than the funeral pomp of the Westminster Abbey.

Permit me, before closing this discourse, to repeat a reflection which presented itself when I was enumerating in this place the great discoveries of Herschel[29], but which applies more directly to Laplace. Your successors will see accomplished those great phenomena whose laws he has discovered. They will observe in the lunar motions the changes which he had predicted, and of which he was alone able to assign the cause. The continued observation of the satellites of Jupiter will perpetuate the memory of the inventor of the theorems which regulate their course. The great inequalities of Jupiter and Saturn pursuing their long periods, and giving to these planets new situations will recall without ceasing one of the most astonishing discoveries. These are the titles to true glory which nothing can extinguish. The spectacle of the heavens will be changed, but in these distant epochs the glory of the inventor will ever subsist. The traces of his genius bear the stamp of immortality.

I have thus presented to you some feature of an illustrious life consecrated to the glory of the sciences. May your recollection supply the defects of accents so feeble! May the voices of the nation, may that of the world at large be raised to celebrate the benefactors of nations – the only homage worthy of those who, like Laplace, have been able to extend the domains of thought – to attest to man the dignity of his being by unveiling to his eyes all the majesty of the heavens!

Notes

1. These studies are hardly known.

2. Euler's return to Russia: see Youshkevich (1968, p. 108).

3. In the beginning of § 8 this school is called *Military School of France.*

4. Fourier forgot Euler.

5. The comparison with the *Almagest* is marred by the rejection of the Ptolemaic system of the world.

6. Cauchy later justified the foundations of mathematical analysis much more thoroughly.

7. Conon from Samosa was an eminent ancient Greek astronomer but not of the same rank as Archimedes.

8. Regrettably, no details provided. I can only add that Lagrange had written an *Essai d'arithmétique politique sur a la premiers besoins de l'interieur de la République* only published in t. 6 of his *Oeuvres*, 1831, see Pearson (1978, pp. 628 – 633). Then, his very short *Essai d'arithmétique politique* is in t. 14 of his *Oeuvres* (1892, pp. 608 – 614).

9. Acceleration of the moon's motion. Laplace examined and rejected the suggestion that it was occasioned by the deceleration of the rate of the Earth's diurnal rotation (Morando 1995, p. 132).

10. Velocity of light: 70,000 *leagues* = 70·1,000·4.83 = 338,000 *km* (*/sec*), erroneous more than by 10%.

11. The present estimate of that lengthening: 0.02 *sec* in two thousand years.

12. Only four satellites of Jupiter were known then, now their number is no less than 79.

13. In cosmogonic terms, the stability of the solar system is only proved for a short time.

14. No theory of the birth of the planets is yet generally acknowledged.

15. Already in 1797 William Herschel discovered that two satellites of Uranus were moving in the other direction.

16. Here is my comment on Laplace's theory of probability. First, it is primarily used for studying mass random events. Then, his predecessors treated it as a branch of pure mathematics but Laplace resolutely directed it to the realm of applied mathematics. At least three times he separated himself from (pure) mathematicians (Sheynin 2017, p. 111). He thus became able to make fundamental discoveries in natural science but later the theory of probability had to be constructed anew. A special circumstance is that his *Méch. Cél*. made extremely difficult reading (see also note 23). And now the forgotten Bayes: he greatly influenced and still influences statistics and numerically estimated the precision of the inverse law of large numbers (given, statistical probability, required, theoretical probability of an event). See also note 17.

17. De Moivre discovered that formula about the same time as Stirling, although the latter indicated to him the exact value of its numerical factor. Lagrange (see just below) foreshadowed the introduction of characteristic functions. On D'Alembert see Sheynin (2017, p. 75). Wolf (1860) published interesting quotations from Daniel Bernoulli's letters to Euler which concerned D'Alembert. It transpired that D. B. only gradually recognized D'Alembert's merits but that some of his criticisms were just.

18. The single analytic method is apparently the application of the central limit theorem, rigorously proved only by Liapunov and Markov, not even by Chebyshev.

19. Fourier mentioned three names: Claude Emanuel Joseph Pierre de Pastoret (1755 – 1840), politician and writer; Pierre Antoine Noel Bruno Daru (1767 – 1829), statesman, historian, poet. Pierre Paul Royer-Collard (1768 – 1825), statesman, philosopher. I do not know why Fourier called Daru the successor of Laplace. De Pastoret read out an Eloge on Laplace (Laplace, *Oeuvr. Compl.*, t. 14. Paris, pp. 388 –).

20. Bochart de Saron (1730 – 1794, beheaded), lawyer, mathematician, astronomer. President of the Paris parliament. He published Laplace's first contributions, at least partly at his own expense.

21. Lavoisier (1743 – 1794, beheaded). Did the scientific community try to save him? I can only say that each scientist was likely mortally afraid to stick his neck out. And E. S. Pearson, in a separate note (K. Pearson 1978, p. 635) quoted the prosecutor at the Lavoisier trial: *The Republic has no need in scientists or chemists*.

22. Galileo and Huygens should be mentioned as well.

23. There are no formulas in his *Essai philosophique* of 1814 and some places are hardy understandable.

24. See General comment.

25. Alexis Bouvard (1767 – 1843) was a tireless calculator of astronomical observations.

26. Laplace died on 5 *March* 1826.

27. The Academy did not sit at the day of Laplace's death, just as it happened in Petersburg at the day of Euler's death.

28. New Holland was the European name for mainland Australia. *Two Americas* is not definite enough. Strangely, Fourier does not mention the Petersburg Academy although Laplace was its honorary member since 1802.

29. Fourier read the Eloge on William Herschel on 7 June 1824. I have not seen it.

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General Comment

The French Academy of which Laplace became member (§ 7) was established in 1635 for studying the French language. The *Institut de France* is an umbrella for five academies and a multitude of other scientific establishments.

Morando (1995) provided the almost present-day description of Laplace's achievements in astronomy. What no author and even more certainly the authors of eulogies ever mention are the shortcomings of their heroes, but see Sheynin (2017, pp. 117 – 119 and 121 – 122). The main points there are, first, his barely useful theory of errors. He based it on the central limit theorem which he introduced all but arbitrarily and which required a large number of observations lacking in geodesy and, with a large number of them in astronomy, but therefore hardly obeying a single frequency law with constant parameters. Moreover, he never admitted the superiority of the Gaussian theory of errors.

Second, in his book of 1796 he ascribed the eccentricities of the planetary orbits to local irregularities in the bodies of those planets whereas Newton showed that they depended of the velocity of the planet's motion. Moreover, even in 1835 he had not corrected himself and I strongly suspect that he tried thus to conceal his mistake which no one apparently indicated.

One more circumstance ought to be discussed: Fourier did not mention either Poisson or Gauss. Laplace was the main source of inspiration for Poisson and at the very least Fourier should have noted it. In 1881, the centenary of Poisson's birth was not observed and *some circles* still hold a negative opinion about him (Bru 1981, p. 75), but this happened much later and was occasioned by later causes. Then, Bru (p. 69) stated that Fourier had thought of compiling a tract on the theory of probability and thus of eclipsing Poisson, but his death in 1830 prevented him. Still, this only hints at explaining Fourier's attitude.

Now, Gauss. French scientists including Poisson (but not Laplace) to their own detriment never cited Gauss which was caused by Gauss's peculiar and unfriendly treatment of Legendre over the invention of least squares (Sheynin 2017, pp. 130 and 138 – 140), so even in 1829 Fourier apparently followed suit.

I have now found out that the original French text of the *Eloge* published in 1831 is much more detailed.

IV

Jacob Bernoulli

On the Law of Large Numbers

Ars Conjectandi. Basileae, 1713, pt. 4

Contents

Preliminary explanation

Since 1713 the *Ars Conjectani* (AC) has appeared in a German translation whereas its Part 4 was translated into Russian and French (and, in an horrible unsatisfactory way, into English), see the references. The German translation, especially insofar as mathematical reasoning is concerned, is rather far from the original; the Russian text also somewhat deviates from Bernoulli; finally, the French translation is perhaps almost faultless in this sense, but the translator made several mathematical mistakes.

I do not read Latin and had to begin from the Russian text, but I invariably checked my work against the two other translations and the several English passages from the AC which Shafer (1978) had provided, as well as against the original with the help of a Latin dictionary. I am really thankful to Claus Wittich (Geneve) who kindly went over my own text and made valuable suggestions and corrections. I am confident that the final result is good enough and in any case better than any translation mentioned above, but any remaining shortcomings and/or mistakes are my own.

A few words about Markov are in order. He initiated, and then edited the 1913 Russian translation. The same year he put out the third, the jubilee edition, as he called it, of his treatise (see References) and supplied it with Bernoulli's portrait. Again in 1913, he initiated a special sitting of the Imperial [Petersburg] Academy of Sciences devoted to Bernoulli's work in probability and, along with two other mathematicians, delivered a report there, first published in 1914, reprinted in Bernoulli (1986) and available in an English translation (Ondar 1977/1981, pp. 158 –163).

Later, in the posthumous edition of his treatise (1924), Markov improved Bernoulli's estimates (§ 2.4), as Pearson did at about the same time and, perhaps as an indirect result of his study of the AC, inserted there many interesting historical comments.

I had previously privately printed the same translation, see **S, G,** 8, but now I am not satisfied by it.

FOREWORD

1. The *Art of Conjecturing* and Its Contents

Jacob Bernoulli (1654 – 1705) was a most eminent mathematician, mechanician and physicist. His AC (1713) was published posthumously with a Foreword by his nephew, Nicolaus Bernoulli (English translation: David (1962, pp. 133 – 135); French translation, Jacob Bernoulli (1987, pp. 11 – 12)). It is not amiss to add that N. B. (1709) published his dissertation on the application of the art of conjecturing to jurisprudence where he not only picked up some hints included in the manuscript of his late uncle, but borrowed whole passages both from it and even from the *Meditationes*, never meant for publication (Kohli 1975b, p. 541).

The *Meditationes* is Bernoulli's diary. It covers approximately the years 1684 – 1690 and is important first and foremost because it contains a fragmentary proof of the law of large numbers (LLN) to which Bernoulli indirectly referred at the end of Chapter 4 of Part 4 of the AC. Other points of interest in the *Meditationes* are that he (1975,

p. 47) noted that the probability (in this case, statistical probability) of a visitation of a plague in a given year was equal to the ratio of the number of these visitations during a long period of time to the number of years in that period. Then, Bernoulli (p. 46, marginal note) wrote out the imprint of a review published in 1666 of Graunt's book (1662) which he possibly had not seen; he had not referred to it either in the *Meditationes* itself or in the AC. And, lastly, at about the same time Bernoulli (p. 43) considered the probability that an older man can outlive a young one (cf. Item 4 in Chapter 2, Part 4 of the AC). All this, even apart from the proof of the LLN, goes to show that already then he thought about applying statistical probability.

Part 1 of the AC is a reprint of Huygens' tract (1657) complete with vast and valuable commentaries. Nevertheless, this form testifies that Bernoulli was unable to complete his contribution. Also in Part 1 Bernoulli (pp. 22 – 28 of the German translation), while considering a game of dice, compiled a table which enabled him to calculate the coefficients of x^m in in the development of $(x + x^2 + \ldots + x^5 + x^6)^6$ for small values of n. Part 2 dealt with combinatorial analysis and it was there that the author introduced the *Bernoulli numbers*. Part 3 was devoted to application of the "previous" to drawing of lots and games of dice.

Parts 1 and 3 contain interesting problems: the study of random sums for the uniform and the binomial distributions; a similar investigation of the sum of a random number of terms for a particular discrete distribution; a derivation of the distribution of the first order statistic for the discrete uniform distribution; and the calculation of probabilities appearing in sampling without replacement. The author's analytical methods included combinatorial analysis and calculation of expectations of winning in each set of finite and infinite games and their subsequent summing.

Finally, Part 4 contained the LLN. There also we find a not quite formal "classical" definition of probability (a notion which he had not applied when formulating that law), a reasoning, in Chapter 2, on the aims of the art of conjecturing (determination, as precisely as possible, of probabilities for choosing the best solutions of problems, apparently in civil life) and elements of stochastic logic. Strangely enough, the title of Part 4 mentioned the completely lacking applications of the "previous doctrine" whereas his main theorem (the LLN) was not cited at all. This again testifies that Bernoulli had not completed his work. He did state, however (Chapter 4) that his LLN provided moral certainty which was sufficient for civil life and at the end of Chapter 2 he even maintained that judges must have firm instructions about what exactly constituted it.

Moral certainty had first appeared about 1400 (Franklin 2001, p. 69), but it was Descartes (1644, p. 323) who put it into circulation (above all apparently bearing in mind jurisprudence!). Huygens (Sheynin 1977, pp. 251 – 252) believed that proofs in physics were only probable and should be checked by appropriate corollaries and that common sense ought to determine the required degree of certainty of judgements in civil life. This latter statement seems much more reasonable than Bernoulli's rigid demand.

Bernoulli apparently considered the art of conjecturing as a mathematical discipline based on probability as a measure of certainty and on expectation which included (the not yet formally introduced) addition and multiplication theorems and crowned by the LLN.

2. The Art of Conjecturing, Part 4

2.1 Randomness and Necessity. Apparently not wishing to encroach upon theology, Bernoulli (beginning of Chapter 1) refused to discuss the notion of randomness. Then, again in the same chapter, he offered a subjective explanation of the "contingent" but actually corrected himself at the beginning of Chapter 4 where he explained randomness by the action of numerous complicated causes. Finally, the last lines of his book contain a statement to the effect that some kind of necessity was present even in random things (but left too little room for it). He referred to Plato who had taught that after a countless number of centuries everything returned to its initial state. Bernoulli likely thought about the archaic notion of the Great Year whose end will cause the end of the world with the planets and stars returning to their positions at the moment of creation. Without justification, he widened the boundaries of applicability of his law and his example was, furthermore, too complicated. It is noteworthy that Kepler (1596) believed that the end of the world was unlikely. In this, the first edition of this book, his reasoning was difficult to understand but later he substantiated his conclusion by stating, in essence, like Oresme (1966, p. 247) did before him, that two [randomly chosen] numbers were "probably" incommensurable.

Bernoulli borrowed his example of finding a buried treasure from Aristotle (end of Chapter 1) but, unlike him, only indirectly connected it with randomness. The later understanding of randomness began with Maxwell and especially Poincaré, who linked it with (among other interpretations) with the case in which slight causes (digging the earth somewhere near) would lead to considerable effects (the treasure remained buried) and numerous complicated causes (here, he repeated Bernoulli). Poincaré also sensibly reasoned on the interrelations between randomness and necessity. On randomness see Sheynin (2014); new ideas took root late in the 20$^{\text{th}}$ century.

2.2. Stochastic Assumptions and Arguments. Bernoulli examined these in Chapters 2 and 3, but did not return to them anymore; he possibly thought of applying them in the unwritten pages of his book. The mathematical aspect of his considerations consisted in the use of the addition and the multiplication theorems for combining various arguments.

Unusual was the non-additivity of the deduced [probabilities] of the events under discussion. Here is one of his examples (Chapter 3, Item 7):

"Something" possesses 2/3 of certainty but its opposite has 3/4 of certainty. Both possibilities are probable and their probabilities are as 8:9. Koopman (1940) resumed, in our time, the study of non-additive probabilities whose sources can be found in the medieval doctrine of probabilism that considered the opinion of each theologian as probable. Franklin (2001, p. 74) traced the origin of probabilism to the year 1577, or, in any case (p. 83), to 1611. Nevertheless, similar pronouncements on probabilities of opinion go back to John of Salisbury (the 12th century) and even to Cicero (Garber & Zabell 1979, p. 46).

I note a "general rule or axiom" concerning the application of arguments (pp. 234 and 236): out of two possibilities, the safer, the more reliable, etc. should be chosen.

On the subject of this subsection see Shafer (1978) and Halperin (1988).

Bernoulli derived many formulas which I had not copied. I believe that no one had or will ever apply them, but they are inserted in any full translations of the *Ars* and certainly in Bernoulli (1975).

2.3. Arnauld and Leibniz. Antoine Arnauld (1612 – 1694) was an extremely well known religious figure and philosopher, the main author of the influential treatise Arnauld & Nicole (1662). In Chapter 4 Bernoulli praised Arnauld and approved his reasoning on using posterior knowledge and at the end of Chapter 3 Bernoulli borrowed Arnauld's example (1662, pp. 328 – 329) of the criminal notary. Other points of interest are Arnauld's confidence in moral certainty and his discussion of the application of arguments. It might be reasonably assumed that Arnauld was Bernoulli's "non-mathematical" predecessor.

In 1703, Bernoulli informed Leibniz about the progress in his work (Kohli 1975, p. 509). He had been compiling it for many years with repeated interruptions caused by his "innate laziness" and worsening of health; the book still lacked its "most important part", the application of the art of conjecturing to civil life; nevertheless, he, Bernoulli, had already shown his brother [Johann] the solution of a "difficult problem, special in its own way" that justified the applications of the art of conjecturing.

Most important both in that letter and in the following correspondence of 1703 – 1705 (Ibidem, pp. 510 – 512) was the subject of statistical probabilities. Leibniz never agreed that observations could secure moral certainty, but his arguments were hardly convincing. Thus, he in essence repeated the statement of Arnauld & Nicole (1662/1992, pp. 304 and 317) that the finite (the mind; therefore, observations) could not always grasp the infinite (for example, God, but also, as Leibniz stated, any phenomenon depending on innumerable circumstances).

Leibniz' views were possibly caused by his understanding of randomness as something "whose complete proof exceeds any human mind" (manuscript, 1686/1960, p. 288). His heuristic statement does not contradict a modern approach to randomness founded on complexity and he was also right in the sense that statistical determinations cannot definitively corroborate a hypothesis.

In his letter of 3 Dec. 1703 Leibniz (Gini 1946, p. 405) also maintained that the allowance for all the circumstances was more important than subtle calculations, and Bortkiewicz (1923, p. 12) put on record Keynes' (1921) favourable attitude towards this point of view and indicated the appropriate opinion of Mill (1843/1886, p. 353), who had sharply contrasted the consideration of circumstances with "elaborate application" of probability and declared that the “neglect of this obvious reflection” made probability “the real opprobrium of mathematics”. Bortkiewicz agreed that mathematicians had been sometimes guilty of such neglect, which, however, had nothing to do with the calculus of probability. In his Chapter 4, Bernoulli touched on medical statistics and, for my part, I note that its progress is accompanied by the discovery of new circumstances so that stochastic calculations ought to be made repeatedly. Thus, in the mid-19th century, amputation of a limb made under the newly introduced anaesthesia sometimes led to death from bronchitis (Sheynin 1982, p. 262) and the benefits of that procedure had to be critically considered. Circumstances and calculations should not be contrasted.

Bernoulli paid due attention to Leibniz’ criticism; more than a half of Chapter 4 of the AC in essence coincided with the respective passages from his letters to Leibniz (whom he did not mention by name).

In 1714, in a letter to one of his correspondents, Leibniz (Kohli 1975, p. 512) softened his doubts about the application of statistical probabilities and for some reason added that the late Jacob Bernoulli had “cultivated” the [theory of probability] in accordance with his, Leibniz’ “exhortations”.

On the correspondence between the two scholars see also Sylla (1998).

2.4. The Law of Large Numbers

2.4.1. The Prehistory. The LLN has its prehistory. It was thought, long before Bernoulli, that the number of successes in n “Bernoulli” trials with probability p was approximately equal to

$$\mu = np. \tag{1}$$

Cardano (Ore 1963, pp. 152 – 154 and 196), for example, applied this formula in calculations connected with games of dice. When compiling his mortality table, Halley (1694) assumed that “irregularities” in his data would have disappeared had he much more observations at his disposal. His idea can be interpreted as a statement on the increase in precision of formula (3), see below, with n; it is likely, however, that these irregularities were occasioned by systematic corruptions.

A second approach to the LLN took shape in astronomy not later than during Kepler’s lifetime when the arithmetic mean became the universal estimator of the constant sought.

Similar but less justified statements concerning sums of magnitudes corrupted by random errors had also appeared. Thus, Kepler (Sheynin 1973, p. 120) remarked that the total weight of a large number of metal money of the same coinage did not depend on the inaccuracy in the weight of the separate coins. Then, De Witt (Sheynin 1977, p. 214) stated that the then existing custom of buying annuities upon many (n) young and apparently healthy lives secured profit “without hazard or risk”. The expectation of a gain $\mathrm{E}x_i$ from each such transaction was obviously positive; if constant, the buyer could expect a total gain of *n*E*x*. There also apparently existed a practice of an indirect participation of (petty?) punters in many games at once. An any case (Sheynin 1977, p. 236), both De Moivre and Montmort mentioned in passing that some persons bet on the outcomes of games. The LLN has then been known, but not to such punters, and that practice could have existed from much earlier times.

2.4.2. Jakob Bernoulli. Before going on to prove his LLN, Bernoulli (Chapter 4) explained that the theoretical "number of cases" was often unknown, but what was impossible to obtain beforehand, might at least be determined afterwards, i.e., by numerous observations. In essence, Bernoulli proved a proposition that, beginning with Poisson, is being called the LLN.

Let *r* and *s* be natural numbers, $t = r + s$, *n*, a large natural number, ν= nt, the number of [independent] trials (De Moivre (1712) was the first to mention independence) in each of which the studied event occurs with [probability] *r*/*t*, μ – the number of the occurrences of the event (of the successes). Then Bernoulli proved without applying mathematical analysis that

$$P(|\frac{\mu}{\nu}-\frac{r}{t}|\le\frac{1}{t})\ge 1-\frac{1}{1+c} \qquad (2)$$

and estimated the value of ν necessary for achieving a given $c > 0$. In a weaker form Bernoulli's finding meant that

$$\lim P(|\frac{\mu}{\nu}-\frac{r}{t}|<\varepsilon)=1, \nu\to\infty \qquad (3)$$

where, as in formula (1), *r*/*t* was the theoretical, and μ/ν, the statistical probability.

Markov (1924, pp. 44 – 52) improved Bernoulli's estimate mainly by specifying his intermediate inequalities, and Pearson (1925), by applying the Stirling formula, achieved a practically complete coincidence of the Bernoulli result with the estimate that makes use of the normal distribution as the limiting case of the binomial law; Markov did not use that formula apparently because Bernoulli had not known it, but then, on p. 55ff, he applied it without any connection with his previous reasoning.

In addition, Pearson (p. 202) considered Bernoulli's estimate of the necessary number of trials in formula (2) "crude" and leading to the ruin of those who would apply it but had not found a single word appreciating the result achieved. On the contrary, he inadmissibly compared the Bernoulli law with the wrong Ptolemaic system of the world.

The very fact described by formulas (2) and (3) was, however, extremely important for the development of probability and statistics, and, anyway, should we deny the importance of existence theorems? For modern descriptions of Bernoulli's LLN see Prokhorov (Bernoulli 1986) and Hald (1990, Chapter 16; 2003).

And so, the LLN established a correspondence between the two probabilities. Bernoulli (Chapter 4) had indeed attempted to ascertain whether or not the statistical probability had its "asymptote"– whether there existed such a degree of certainty, which observations, no matter how numerous, would never be able to reach. Or, in my own words, whether there existed such positive numbers β and δ < 1, that

$$\lim P(|\frac{\mu}{\nu}-\frac{r}{t}|<\beta)=1-\delta, \nu\to\infty .$$

He answered his question in the negative: no, such numbers did not exist and he thus established, within the boundaries of stochastic knowledge, a relation between deductive and inductive methods and combined statistics with the art of conjecturing.

Throughout Part 4, Bernoulli considered the derivation of the statistical probability of an event given its theoretical probability and this most clearly emerges in the formulation of his *Main Proposition* in Chapter 5. However, both in the last lines of that chapter and in Chapter 4 he mentioned the inverse problem actually alleging that he had solved it as well. I return to this point in § 2.4.3.

2.4.3. Remarks on Later Events. De Moivre (1756, p. 251) followed Bernoulli. Without any trace of hesitation, he claimed to have solved both the direct and the "converse" problems; he had expressed less clearly the same idea in 1738, in the previous edition of his book. De Moivre's mistake largely exonerates Bernoulli, so that Keynes (1921, p. 402) wrongfully stressed that the latter "proves the direct theorem only". It was Bayes who perceived that the two problems were different. He was the first to determine precisely the theoretical probability given the appropriate statistical data and for this reason I (Sheynin 2003) suggested that Bayes had completed the construction of the first version of probability theory. This, however, does not diminish the great merit of Bernoulli in spite of the much more precise results of De Moivre (for one of the problems) and Bayes.

I do not discuss Nicolaus Bernoulli's version of the LLN, which he described in one of his letters of 1713 to Montmort (1713, pp. 280 – 285); see Youshkevich (1986) and Hald (1990, § 17.3; 2003). I myself (Sheynin 1970, p. 232; lacking in the original publication of 1968) noted that N.B. was the first to introduce, although indirectly, the normal distribution.

2.4.4. Alleged Difficulties in Application. Strangely enough, for a long time statisticians had not recognized the fundamental importance of the LLN. Haushofer (1872, pp. 107 – 108) declared that statistics, since it was based on induction [only partly], had no "intrinsic connections" with mathematics which is based on deduction [consequently, neither with probability]. A most noted German statistician, Knapp (1872, pp. 116 – 117), expressed a strange idea: the LLN was hardly useful since statisticians always made only one observation, as when counting the inhabitants of a city. And even later on, Maciejewski (1911, p. 96) introduced a "statistical law of large numbers" in place of the Bernoulli proposition that had allegedly impeded the development of statistics. His own law qualitatively asserted that statistical indicators exhibited ever lesser fluctuations as the number of observations increased.

All such statements definitely concerned the Poisson law as well (European statisticians then hardly knew about the Chebyshev form of the law) and Maciejewski's opinion likely represented the prevailing attitude of statisticians. Here, indeed, is what Bortkiewicz (1917, pp. 56 – 57) thought: the expression *law of large numbers* ought to be used only for denoting a "quite general" fact, unconnected with any definite stochastic pattern, of a higher or lower degree of stability of statistical indicators under constant or slightly changing conditions and given a large number of trials. Even Romanovsky (1961, p. 127) kept to a similar view and stressed the natural-scientific essence of the law and called it physical.

The text of Part 4 of the Art of Conjecturing follows.

The Art of Conjecturing, Part 4 showing The Use and Application of the Previous Doctrine to Civil, Moral and Economic Affairs

Chapter 1. Some Preliminary Remarks about Certainty, Probability,Necessity and Fortuity of Things

Certainty of some thing is considered either objectively and in itself and means none other than its real existence at present or in the future; or subjectively, depending on us, and consists in the measure of our knowledge of this existence. Everything that exists or originates under the sun, – the past, the present, or the future, – always has in itself and objectively the highest extent of certainty. This is clear with regard to events of the present or the past; because, just by their existence or past existence, they cannot be non-existing or not having existed previously. Neither can you have doubts about [the events of] the future, which, likewise, on the strength of Divine foresight or predetermination, if not in accord with some inevitable necessity, cannot fail to occur in the future. Because, if that, which is destined to happen, is not certain to occur, it becomes impossible to understand how can the praise of the omniscience and omnipotence of the greatest Creator remain steadfast. But how can this certainty of the future be coordinated with fortuity or freedom [independence] of secondary causes? Let others argue about it; we, however, will not touch something alien to our aims.

Certainty of things, considered with respect to us, is not the same for all things, but varies diversely and occurs now greater, now lesser. Something, about which we know, either by revelation, intellect, perception, by experience, autopsia [direct observation; by one's own eyes] or otherwise, that we cannot in any way doubt its existence or realization in the future, has the complete and absolute certainty. To anything else our mind assigns a less perfect measure [of certainty], either higher or lower depending on whether there are more or less probabilities convincing us of its existence at present, in the past or the future.

As to *probability*, this is the degree of certainty, and it differs from the latter as a part from the whole. Namely, if the integral and absolute certainty, which we designate by letter α or by unity 1, will be thought to consist, for example, of five probabilities, as though of five parts, three of which favour the existence or realization of some event, with the other ones, however, being against it, we will say that this event has 3/5α, or 3/5, of certainty.

Therefore, the event that has a greater part of certainty than the other ones is called *more probable,* although actually, according to the usual word usage, we only call probable that, whose probability markedly exceeds a half of certainty. I say *markedly* because a thing, whose probability is roughly equal to a half of certainty, is called *doubtful* or indefinite[1.1]. Thus, a thing having 1/5 of certainty is more probable than that which has 1/10, although actually neither is probable.

Possible is that which has at least a low degree of certainty whereas the impossible has either no, or an infinitely small certainty. Thus, something is possible if it has 1/20 or 1/30 of certainty.

Morally certain is that whose probability is almost equal to complete certainty so that the difference is insensible. On the contrary, *morally impossible* is that which has only as much probability as the morally certain lacks for becoming totally certain. Thus, if morally certain is that which has 999/1000 of certainty, then something only having 1/1000 of certainty will be morally impossible.

Necessary is that, which cannot fail to exist at present, in the future or past, owing exactly to necessity, either *physical* (thus, fire will necessarily consume; a triangle will have three angles summing up to two right angles; a full moon, if in a node, will necessarily be accompanied by a [lunar] eclipse), – or *hypothetical*, according to which all that exists, or had existed, or is supposed to exist, cannot fail to exist (in this sense it is necessary that Petrus, about whom I know and accept that he is writing, is indeed writing), – or, finally, according to the necessity of a condition or agreement (thus, a gambler scoring a six with a die is necessarily reckoned the winner if the gamblers have agreed that winning is connected with throwing a six).

Contingent (both *free*, if it depends on the free will of a reasonable creature, and *fortuitous* and *casual*, if it depends on fortune or chance) is that which can either exist or not exist at present, in the past or future, – clearly because of remote rather than immediate forces. Indeed, neither does contingency always exclude necessity up to secondary causes. I shall explain this by illustrations.

It is absolutely doubtless that, given a certain position of a die, [its] velocity and distance from the board at the moment when it leaves the thrower's hand, it cannot fall otherwise than it actually does. Just the same, under a certain present composition of the air, and given the masses, positions, motions, directions, and velocities of the winds, vapours and clouds, as well as the mechanical laws governing the interactions of all that, the weather tomorrow cannot be different from that which it will actually be. So these phenomena take place owing to their immediate causes with no lesser necessity than the phenomena of the eclipses follow from the movement of the heavenly bodies. And still, usually only the eclipses are ranked among necessary phenomena whereas the fall of a die and the future weather are thought to be contingent.

The sole reason for this is that what is supposed to be known for determining future actions, and what indeed is such in nature, is not enough known. And, even had it been sufficiently known, geometric and physical knowledge is inadequately developed for subjecting such phenomena to calculation in the same way as eclipses can be calculated beforehand and predicted by means of known astronomical principles. And, for the same reason, before astronomy achieved such perfection, the eclipses themselves had to be reckoned as future chance events to not a lesser extent than the two other [mentioned] phenomena.

It follows that what seems to be contingent to one person at a certain moment, will be thought necessary to someone else (or even to the same person) at another time after the [appropriate] causes become known. And so, contingency mainly depends on our knowledge since we do not see any contradiction with the non-existence of the event at present or in the future, although here and now, owing to an immediate but unknown to us cause, it is either necessarily realized, or ought to occur.

Not everything which brings us well-being or harm is called *happiness* or *misfortune* {*Fortuna prospera*, un B*onheur*, ein Glück & *Fortuna adversa, un Malheur,* ein Unglück}, but only that which with a higher, or at least with the same probability would have possibly failed to occur. Therefore, happiness or misfortune are the greater, the lower was the probability of the well-being or harm that has actually occurred. Thus, exceptionally happy is the man who finds a buried treasure while digging the ground because this does not happen even once in a thousand cases. If twenty deserters, one of whom will be put to death by hanging as an example for the others, cast lots as to who remains living, those nineteen who drew the more favourable lot are not really called happy; but the twentieth who cast the horrible lot is most miserable. [In the same way,] your friend who came out unharmed from a battle in which [only] a small part of the combatants were killed should not be called happy, unless you will perhaps think it necessary to do so because of the special fortune of preserving life.

Chapter 2. On Arguments and Conjecture. On the Art of Conjecturing. On the Grounds for Conjecturing. Some General Pertinent Axioms

Regarding that which is certainly known and beyond doubt, we say that we *know or understand* [it]; concerning all the rest, – we only *conjecture or opine*.

To make *conjectures* about something is the same as to measure its probability. Therefore, the art of *conjecturing or stochastics* {*ars conjectandi sive stochastice*}**[2.1]** is defined as the art of measuring the probability of things as exactly as possible, to be able always to choose what will be found the best, the more satisfactory, serene and reasonable for our judgements and actions. This alone supports all the wisdom of the philosopher and the prudence of the politician.

Probabilities are estimated both by the *number* and the *weight of the* *arguments* which somehow prove or indicate that a certain thing is, was, or will be. As to *the weight*, I understand it to be the force of the proof.

Arguments themselves are either *intrinsic*, in every-day speech artificial, elicited in accordance with considerations of the cause, the effect, of the person, connection, indication or of other circumstances which seem to have some relation to the thing under proof; or *external* and not artificial, derived from people's authority and testimony. An example: Titius is found killed in the street. Maevius is charged with murder. The accusing arguments are: 1) He is known to have hated Titius (an argument from a *cause*, since this very hate could have incited to murder). 2) When questioned, he turned pale and answered timidly (this is an argument from the *effect* since it is possible that the pallor and fright were caused by his being conscious of the evil deed perpetrated). 3) Blood-stained cold steel is found in Maevius' house (this is an *indication)*. 4) The same day that Titius was killed, Maevius had been walking the same road (this is *circumstance* of place and time). 5) Finally, Cajus maintains that the day before Titius was killed, he had quarrelled with Maevius (this is a *testimony*).

However, before getting down to our problem, – to indicating how should we apply these arguments for conjecturing to measure probabilities, – it is helpful to put forth some general rules or axioms which are dictated to any sensible man by usual common sense and which the more reasonable men always observe in everyday life.

1) *In such things in which it is possible to achieve complete certainty, there is no place for conjectures*. Futile would have been an astronomer, who, knowing that two or three [lunar] eclipses occur yearly, desires to forecast, on such grounds, whether or not there will be an eclipse during a full moon. Indeed, he could have found out the truth by reliable calculation. Just the same, if a thief says at his questioning that he sold the stolen thing to Sempronius, the judge who wants to conjecture about the probability of that statement by looking at the expression of the thief's face and listening to the tone of his voice, or by contemplating the quality of the stolen thing, or by some other circumstances, will act stupidly, because Sempronius, from whom everything can certainly and easily be elicited, is available.

2) *It is not sufficient to weigh one or another argument; it is necessary to investigate all such which can be brought to our knowledge and will seem suitable in some respect for proving the thing*. Suppose that three ships leave the harbour. After some time it is reported that one of them had suffered shipwreck and is lost. Conjectures are made: which of them? If only paying attention to the number of the ships, I shall conclude that each of them could have met with the misfortune in an equal manner. But since I remember that one of them was comparatively old and decrepit, badly rigged with masts and sails, and steered by a young and inexperienced helmsman, I believe that, in all probability, it was this ship that got lost rather than one of the others.

3) *We ought to consider not only the arguments which prove a thing, but also all those which can lead to a contrary conclusion, so that, after duly discussing the former and the latter, it will become*

clear which of them has more weight. It is asked, with respect to a friend very long absent from his fatherland, may we declare him dead[2.2]? The following arguments favour an answer in the affirmative: During the entire twenty years, in spite of all efforts, we have been unable to find out anything about him; the lives of travellers are exposed to very many dangers from which those remaining at home are exempted; therefore, perhaps his life came to an end in the waves; perhaps he was killed en route or in battle; perhaps he died of an illness or from some [other] cause in a place where no one knew him. Then, has he been living, he would have reached an age which only a few attain even in their homeland; and he would have written even from the furthest shores of India because he knew that an inheritance was expected for him at home. And so on in the same vein.

Nevertheless, we should not rest content with these arguments but rather oppose them by the following supporting the contrary. He is known to have been thoughtless; wrote letters reluctantly; did not value friends. Perhaps Barbarians held him captive so that he was unable to write, or perhaps he did write sometimes from India, but the letters got lost either because of the carelessness of those carrying them, or during shipwrecks. And, to cap it all, many people are known to have returned unharmed after having been absent even longer.

4) *For judging about universalities remote and universal arguments are sufficient; however, for forming conjectures about particular things, we ought also to join to them more close and special arguments if only these are available*. Thus, if it is asked, in general, how much more probable is it for a twenty-year-old youth to outlive an aged man of 60 rather than the other way round, we have nothing to take into consideration other than the distinction between the generations and ages. But if the question concerns two definite persons, the youth Petrus and the old man Paulus, we also ought to pay attention to their complexion, and to the care that each of them takes over his health. Because if Petrus is in poor health, indulges in passion, and lives intemperately, Paulus, although much older, may still hope, with every reason, to live longer.

5) *Under uncertain and dubious circumstances we ought to suspend our actions until more light is thrown. If, however, the necessity of action brooks no delay, we must always choose among two possibilities that one which seems more suitable, safe, reasonable, or at least more probable* [2.3]*, even if none of them is actually such.* Thus, if a fire has broken out and you can only save yourself by jumping from the top of the roof or from some lower floor, it is better to choose the latter as being less dangerous, although neither alternative is quite safe or free from the danger of injury.

6) *That which is in some cases helpful and never harmful ought to be preferred to that which is never either helpful or harmful. In our vernacular it is said Hilfft es nicht, so schadt es nicht* [Even if it does not help, it does not harm]. This proposition follows from the previous [considerations], because that which can be helpful is more satisfactory, reliable and desirable than that which under the same conditions cannot [be helpful].

7) *Human actions should not be assigned a value according to their outcomes* because sometimes the most reckless actions are accompanied by the best success, whereas, on the contrary, the most reasonable [may] lead to the worst results. In agreement with this, the Poet says: “May success be wanting, I wish, for him who would judge facts by their outcomes” [Ovidius, *Epistulae Heroidum* II, “Phyllis Demophoonti”, line 85]. Thus, someone who intends to throw at once three sixes with three dice, should be considered reckless even if winning by chance. On the contrary, we [ought to] note the false judgement of the crowd which considers a man the more prominent, the more fortunate he is, and for which even a successful and fruitful crime is mostly a virtue. Once more Owen (*Epigr*[*ammatum*] *lib*[*er*] *sing*[*ularis*, 1607], § 216)[2.4] gracefully says:

Although just now Ancus is believed to be a fool, it is argued that he is wise because the poorly conceived turned out successful [for him]. If something reasonably thought-out fails, even Cato will be judged a fool by the crowd.

8) *In our judgements, we ought to beware of attributing to things more than is due to them, ought not to consider something which is only more probable than the other as absolutely certain, nor to impose the same opinion on others*. [This is] because the trust attributed to things ought to be in a proper proportion to the degree of certainty possessed by each thing, and be less in the same ratio as its probability itself is. In vernacular, this is expressed as

Man muss ein jedes in seinem Werth und Unwerth beruhen lassen [Let each thing be determined by its value or worthlessness.]

9) *However, since complete certitude can only seldom be attained, necessity and custom desire that that, which is only morally certain, be considered as absolutely certain.* Therefore, it would be helpful if the authorities determine certain boundaries for moral certainty, – if, for example, it would be defined whether 99/100 of certainty be sufficient for resolving something, or whether 999/1000 be needed, so that a judge, unable to show preference to either side, will always have firm indications to conform with when pronouncing a sentence.

Anyone having knowledge of life can compile many more similar axioms, but, lacking an appropriate occasion, we can hardly remember all of them.

Chapter 3. On Arguments of Different Kinds and on How Their Weights Are Estimated for Calculating the Probabilities of Things

He who considers various arguments by which our opinions and conjectures are formed will note a threefold distinction between them since *some of them necessarily exist and contingently provide evidence; others exist contingently and necessarily provide evidence; finally, the third ones both exist and provide evidence contingently.*

I explain these differences by examples. For a long time, my brother does not write me anything. I doubt whether to blame his laziness or his business pursuits, and fear that he may even have died. Here, there are threefold arguments for explaining the ceasing of the correspondence: *laziness, death, pursuits.* The first of these exists for sure (according to hypothetical necessity, since I know and accept that my brother is lazy), but proves true [provides evidence] only contingently because laziness possibly would not have hindered him from writing. The second one contingently exists (because my brother could still be alive), but proves true without question because a dead man cannot write. The third one both exists and provides evidence contingently because my brother can have business pursuits or not, and if he has them, they need not be such that prevent him from writing.

Another example. I suppose that, according to the conditions of a game, a gambler wins if he throws seven points with two dice, and I wish to guess his hope of winning. Here, the argument for winning is the throwing of seven points. It necessarily indicates the winning (owing, indeed, to the agreement between the gamblers), but it only exists contingently, because, in addition to the seven points, another number of them can occur.

Excepting this difference between the arguments, another distinction can also be noted since some of them are pure, the other ones, *mixed*. I call an argument *pure* if in some cases it proves a thing in such a manner that on other occasions it does not prove anything positively. A *mixed* argument, however is such that in certain cases it thus proves a thing that on other occasions it proves the contrary in the same manner.

An example. Someone in a quarrelling crowd was cut with a sword; and, as trustworthy people who saw the incident from a distance testify, the perpetrator was dressed in a black cloak[3.1]. If Gracchus was among those quarrelling together with three others, all of them in black tunics, this tunic will be an argument in favour of Gracchus having committed the murder.

However, this argument will be mixed since in one case it proves his guilt, and, in three other cases, it demonstrates his innocence. Indeed, the murder was perpetrated either by him, or by one of the other three, with the latter instance being impossible without exonerating Gracchus. If, however, during the subsequent questioning Gracchus turned pale, the paleness of his face will be a pure argument because it demonstrates his guilt if occasioned by disturbed conscience. On the contrary, it would not prove his innocence had it been called forth by something else, since it is possible that he turned pale owing to another cause but still was himself the perpetrator of the murder [the murderer].

The above makes it clear that the force of proof peculiar to some argument depends on the multitude of cases in which it can exist or not exist, provide evidence or not, or even provide evidence to the opposite of the thing. Therefore, the degree of certainty, or the probability engendered by this argument, can be deduced by considering these cases in accordance with the doctrine given in Part 1 [of this book] in exactly the same way as the fate of gamblers in games of chance is usually investigated.

And so, first, let an argument exist contingently and provide evidence necessarily. If some argument *both exists and indicates contingently*, …

Then, if several arguments are collected for proving one and the same thing, the force provided by the totality of all the arguments is estimated in the following way …

If, in addition to the arguments leading to the proof of a thing, there exist other pure arguments favouring the opposite, the arguments of both kinds ought to be weighed separately …

It might happen that something has 2/3 of certainty whereas its opposite has 3/4 so that each of these contraries will be probable although the first of them is less probable than the opposite; namely, their ratio will be as 2/3 to 3/4 or as 8 to 9.

I cannot conceal here that I foresee many obstacles in special applications of these rules that can often lead to shameful mistakes if caution is not observed when distinguishing between the arguments. Indeed, sometimes such arguments can seem to differ which actually compose one and the same argument, and to the contrary: differing arguments can be accepted as a single argument. Sometimes an argument includes such premises which absolutely refute the opposite, etc. As an explanation, I only adduce one or two illustrations. In the example above concerning Gracchus, I assume that the trustworthy people who saw those quarrelling also noted that the perpetrator was red-haired and that Gracchus together with two of the others were distinguished by hair of that colour, but that no one of the latter was dressed in a black toga. In that case, if someone would have desired to compare the probabilities of Gracchus' guilt and innocence by the indications that Gracchus and three others were dressed in black, and also, that, again in addition to him, two others were notable for their red hair, and found that they are in a composite ratio of 1:3 and 1:2, or in the ratio of 1 to 6; and if he were to conclude that Gracchus is by far more likely to be innocent than to be the perpetrator of the murder, he would certainly have collated the matter in a most inept fashion. Actually, there are no two arguments here but only one and the same, resulting from two simultaneous circumstances, the colour of the dress and of the hair. Since both these circumstances are only conjoined in the case of Gracchus, they certainly demonstrate that no one else except him could have been the perpetrator.

Another example. It becomes doubtful whether a written document is fraudulently antedated. An argument to the opposite could be that the document was signed by the hand of a notary public, i.e., by an official and sworn person, with regard to whom it is unlikely that he might have permitted himself any fraud. Indeed, he would have been unable to do so without greatly endangering his honour and well-being; in addition, even from among 50 notaries hardly one would have dared to commit such a vile action. The following arguments could be in favour of an answer in the affirmative: This notary is very ill-famed; and could have expected greatest benefits from the fraud; and especially that he had testified to something having no probability, as for example that someone had lent 10,000 gold coins to another person, whereas, according to everyone's estimation, all his property then barely amounted to 100.

Here, if considering separately the argument from the character of the signatory, the probability that the document is authentic may be valued as 49/50 of certainty. When, however, weighing the arguments favouring the opposite, it would be necessary to conclude that it is hardly possible that the document is not forged so that the fraud committed in the document is of course morally certain, that is, has 999/1000 of certainty. However, we should not conclude that the probabilities of authenticity and fraud are in the ratio of 49/50 to 999/1000, or almost of equality. Because, if I believe that the notary is dishonourable, I am therefore assuming that he does not belong to the 49 honest notaries detesting deception but that he is indeed the fiftieth who has no scruples of fulfilling his duties faithlessly. This consideration completely destroys all the power of that argument, which in other cases could have been able to prove that a document is authentic.

Chapter 4. On a Two-Fold Method of Investigating the Number of Cases. What Ought To Be Thought about Something Established by Experience. A Special Problem Proposed in This Case, etc.

It was shown in the previous chapter how, – given the number of cases in which arguments in favour of some thing can exist or fail to exist, can provide evidence or not, or even prove the opposite, – the force of what they prove, and the probabilities of things proportional to these forces, can be derived and estimated by calculation. We thus see that for correctly conjecturing about some thing, nothing else is required than both precisely calculating the number of cases and finding out how much more easily can some of them occur than the others. Here, however, we apparently meet with an obstacle since this only extremely seldom succeeds, and hardly ever anywhere except in games of chance which their first inventors, desiring to make them fair, took pains to establish in such a way that the number of cases involving winning or losing were determined with certainty and known and the cases themselves occurred with the same facility.

However, for most of other matters, depending either on the production of nature or the free will of people, this does not take place at all. Thus, for example, the number of cases is known in [a game of] dice. For each die there are manifestly as many cases as faces, and all of them are equally inclined [to turn up], since, owing to the similitude [congruence] of the faces and the uniform weight [density] of the die, there is no reason for one of them to turn up more easily than another[4.1].

This would have happened if the forms of the faces were dissimilar or if one part of the die consisted of a heavier substance than the other one. In the same way, the number of cases for drawing a white or a black ticket from an urn is known, and known [also] is that [the drawings of] all of them are equally possible. Indeed, the number of tickets of both these kinds is evidently determined and known, and no reason is seen for one of them to appear more easily than any other.

But, who from among the mortals will be able to determine, for example, the number of diseases, that is, the same number of cases which at each age invade the innumerable parts of the human body and can bring about our death; and how much easier one disease (for example, the plague) can kill a man than another one (for example, dropsy or, dropsy than fever), so that we would be able to conjecture about the future state of life or death? And who will count the innumerable cases of changes to which the air is subjected each day to form a conjecture about its state in a month, to say nothing about a year? Again, who knows the nature of the human mind or the admirable fabric of our body shrewdly enough for daring to determine the cases in which one or another participant can gain victory or be ruined in games completely or partly depending on acumen or agility of body?

Since this and the like depends on absolutely hidden causes, and, in addition, owing to the innumerable variety of their combinations always escapes our diligence, it would be an obvious folly to wish to find something out in this manner. Here, however, another way for attaining the desired is really opening for us. And, what we are not given to derive a priori, we at least can obtain a posteriori, that is, can extract it from a repeated observation of the results of similar examples. Because it should be assumed that each phenomenon can occur and not occur in the same number of cases in which, under similar circumstances, it was previously observed to happen and not to happen. If, for example, it was formerly noted that, among the observed three hundred men of the same age and complexion as Titius now is and has, two hundred died after ten years with the others still remaining alive, we may conclude with sufficient confidence that Titius also has twice as many cases for paying his debt to nature during the next ten years than for crossing this border. Again, if someone will consider the atmosphere for many previous years and note how many times it was fine or rainy; or, will be very often present at a game of two participants and observe how many times either was the winner, he will thus discover the ratio of the number of cases in which the same event will probably happen or not also in the future under circumstances similar to those previously existing.

This empirical method of determining the number of cases by experiment is not new or unusual. Because the celebrated author of *L'art de penser*, a man of great intellect and acumen[4.2], prescribes the like in Chapter 12 and in the next ones of the last part [of that book], and the same is also constantly observed in everyday life. Then, neither can anyone fail to note also that it is not enough to take one or another observation for such reasoning about an event, but that a large number of them are needed. Because, even the most stupid person, all by himself and without any preliminary instruction, is guided by some natural instinct (which is extremely miraculous) and feels sure that the more such observations are taken into account, the less is the danger of straying from the goal.

Although this is known by nature to everyone, its proof, derived from scientific principles, is not at all usual and we ought therefore to expound it here. However, I would have estimated it as a small merit had I only proved that of which no one is ignorant. Namely, it remains to investigate something that no one had perhaps until now run across even in his thoughts. It certainly remains to inquire whether, when the number of observations thus increases, the probability of attaining the real ratio between the number of cases, in which some event can occur or not, continually augments so that it finally exceeds any given degree of certitude. Or [to the contrary], the problem has, so to say, an asymptote; i.e., that there exists such a degree of certainty which can never be exceeded no matter how the observations be multiplied, so that, for example, it is never possible to obtain more than a half, or than 2/3, or 3/4 of certainty in deriving the real ratio of cases.

To make clear my desire by illustration, I suppose that without your knowledge three thousand white pebbles[4.3] and two thousand black ones are hidden in an urn, and that, to determine [the ratio of] their numbers by experiment, you draw one pebble after another (but each time return the drawn pebble before extracting the next one so that their number in the urn will not decrease), and note how many times is a white pebble drawn, and how many times a black one. It is required to know whether you are able to do it so many times that it will become ten, a hundred, a thousand, etc., times more probable (i.e., become at last morally certain) that the number of the white and the black pebbles which you extracted will be in the same ratio, of 3 to 2, as the number of pebbles themselves, or cases, than in any other different ratio. To tell the truth, if this failed to happen, it would be necessary to question our attempt at experimentally determining the number of cases. If, however, this is attained and we thus finally obtain moral certainty (in the next chapter I shall show that this is indeed so), then we determine the number of cases a posteriori almost as though it was known to us a priori. In social life, where the morally certain, according to Proposition 9 of Chapter 2, is assumed as quite certain, this is undoubtedly quite sufficient for scientifically directing our conjectures about any contingent thing in a no lesser way than in games of chance. Because, if we replace an urn for example by air or by a human body, which contain in themselves sources of various changes or diseases just as the urn contains pebbles, we will be able to determine by observation in exactly the same way how much easier can one or another event occur in these things.

To avoid false understanding, it ought to be noted that the ratio between the numbers of cases which we desire to determine experimentally is accepted not as precise and strict (because this point of view would have led to a contrary result and the probability of determining the real ratio would have been the lower the more observations we would have taken)[4.4], but that this ratio be accepted with a certain latitude, that is, contained between two limits [boundaries] which can be taken as close as you like. Indeed, if in the example just provided concerning pebbles, we will assume two ratios, 301/200 and 299/200, or 3001/2000 and 2999/2000, etc., one of which is very near but greater, and the other one very near but smaller than 3/2, it will be shown that, setting any probability, it can be made more probable that the ratio derived from many observations will be contained within these limits of 3/2 rather than outside.

This, then, is the problem that I decided to make here public after having known its solution for twenty years. Its novelty and the greatest utility joined with an equal difficulty can attach more weight and value to all the other chapters of this doctrine [of the *ars conjectandi*]. However, before exposing its solution I shall defend myself in a few words from the objections to these propositions levelled by some scholars.

1. First, it was objected that the ratio of pebbles is one thing, whereas the ratio of diseases or changes in the air is something else. The number of the former is definite but the number of the latter is indefinite and vague. I answer this by saying that they both, in comparison to our knowledge, are equally indefinite and vague. However, we can imagine anything that is such in itself and in accordance with its nature, not better than a thing created and at the same time not created by the Author of nature because everything done by God is determined thereby.

2. Second, it is objected that the number of pebbles is finite and that of diseases etc. is infinite. Answer. Rather immense than infinite. But let us assume that it is indeed infinite. Even between two infinities a definite ratio is known to be possible and to be expressed by finite numbers either precisely or at least with any desired approximation. Thus, the ratio of each circumference to [its] diameter is definite. [True,] it is not precisely expressed otherwise than by an infinitely continued Ludolphus' cyclic number. However, Archimedes, Metius and Ludolphus himself[4.5] restricted that ratio within limits [boundaries] sufficiently close to each other for practice. Therefore, nothing hinders a ratio of two infinities approximately expressed by finite numbers to be determined by a finite number of experiments either.

3. Third, it is said that the number of diseases does not remain constant but that new diseases occur every day. Answer. We are unable to deny that diseases can multiply in the course of time; and he who desires to conclude from present-day observations about the times of our antediluvian forefathers will undoubtedly deviate enormously from the truth. But nothing follows from this except that sometimes we ought to resume observations just as it would be necessary to resume observations with the pebbles if it is assumed that their number in the urn is variable.

Chapter 5. Solution of the Previous Problem

To explicate the long demonstration as briefly and clearly as possible, I will attempt to reduce everything to abstract mathematics, eliciting from it the following lemmas after which all the rest will only consist in their mere application.

Lemma 1. Suppose that a series of any quantity of numbers 0, 1, 2, 3, 4, etc., follow, beginning with zero, in the natural order and let the extreme and maximal of them be $r + s$, some intermediate, be r, and the two nearest to it on either side, $r + 1$ and $r - 1$. If this series be continued until its extreme term becomes equal to some multiple of the number $r + s$, that is, until it is equal to $nr + ns$, the intermediate number r and those neighbouring it, $r + 1$ and $r - 1$, will be augmented in the same ratio, so that nr, $nr + n$ and $nr - n$ will appear instead, and the series itself

$$0, 1, 2, 3, 4, \ldots, r - 1, r, r + 1, \ldots, r + s$$

will change becoming

$0, 1, 2, 3, 4, \ldots, nr - n, \ldots, nr, \ldots, nr + n, \ldots, nr + ns.$

With an increasing n both the number of the terms situated between the intermediate nr and one of the limiting terms, $nr + n$ or $nr - n$, and the number of those terms which extend from these limits to the extreme terms $nr + ns$ or 0 will thus increase. However (no matter how large will n be assumed), the number of terms following after the larger limit $nr + n$ will never be more than $s - 1$ times greater than, and the number of terms preceding the lesser limit $nr - n$ will never be more than $r - 1$ times greater than the number of them situated between the intermediate nr and one of the limits, $nr + n$ or $nr - n$. Because, after subtraction, it is clear that between the greater limit and the extreme term $nr + ns$ there are $ns - n$ intermediate terms, and between the lesser limit and the other extreme term 0 there are $nr - n$ intermediate terms, and n terms between the intermediate and each of the limits. However, $(ns - n):n = (s - 1):1$ and $(nr - n):n = (r - 1):1$. It therefore follows, etc.

Lemma 2. A binomial $r + s$ raised to any integral power is expressed by terms whose number exceeds by 1 the number of unities in the exponent.

Since a square [of a binomial] consists of three terms, a cube has 4, a fourth power has 5 terms, etc., as is known.

Lemma 3. For any power of this binomial (at least for an exponent equal to the binomial $r + s = t$, or to its multiple, for example, to $nr + ns = nt$), a certain term M will be maximal if the number of terms preceding and following it are in the ratio of s to r; or, which is the same, if the exponents of letters r and s in this term are in the ratio of the magnitudes r and s themselves. The term nearer to it from either side is greater than the more distant term on the same side; however, the same term M is in a lesser ratio to the nearer term than the nearer term to the more distant one if the numbers of intermediate terms are the same.

Dem[*onstration*]. 1. Geometers know well enough that the binomial $r + s$ raised to the power nt, that is, $(r + s)^{nt}$, is expressed by such a series:

$$r^m + \frac{nt}{1} r^{m-1}s + \frac{nt(nt-1)}{1\cdot 2} r^{m-2}s^2 + \ldots + \frac{nt}{1} rs^{m-1} + s^m.$$

[…] Since the number of all the terms except M is, according to Lemma 1, $nt = nr + ns$, and, as assumed, the numbers of the terms preceding and following M are as s to r, these numbers are ns and nr respectively. Therefore, in accordance with the law of the series, then term M will be

$$\frac{nt(nt-1)(nt-2)\ldots(nr+1)}{1\cdot 2\cdot 3\cdot \ldots \cdot ns} r^{nr}s^{ns} \text{or } \frac{nt(nt-1)(nt-2)\ldots(ns+1)}{1\cdot 2\cdot 3\cdot \ldots \cdot nr} r^{nr}s^{ns}$$

call it (5.1),

and in the same way the terms nearest to it on *the left* and t*he right* are

$$\frac{nt(nt-1)(nt-2)...(nr+2)}{1\cdot 2\cdot 3\cdot ...\cdot(ns-1)}r^{nr+1}s^{ns-1} \text{and } \frac{nt(nt-1)(nt-2)...(ns+2)}{1\cdot 2\cdot 3\cdot ...\cdot(nr-1)}r^{nr-1}s^{ns+1}$$

and in the same way the next ones on *the left* and *the right* are […].

After a preliminary suitable cancellation of common multipliers from both the coefficients and the powers themselves, it becomes clear that the term M is to its nearest on the left as $(nr + 1)s$ to nrs; this latter to the next one, as $(nr + 2)s$ to $(ns - 1)r$, etc., and also that the term M is to its nearest on the right as $(ns + 1)r$ to nsr, this latter to the next one, as $(ns + 2)r$ to $(nr - 1)s$, etc. But

$(nr + 1)s > nrs$, and $(nr + 2)s > nsr - r$, etc.

Also,

$(ns + 1)r > nsr$ and $(ns + 2)r > nrs - s$, etc.

It follows that the term M is greater than either of the nearest terms on either side which [in turn] are greater than the more remote terms on the same side, etc. QED.

2. The ratio $(nr + 1)/ns$, as is clear, is less than the ratio $(nr + 2)/(ns - 1)$. Therefore, after multiplying [them] by one and the same ratio s/r, the ratio

$$\frac{(nr+1)s}{nsr} < \frac{(nr+2)s}{(ns-1)r}.$$

Just the same, it is evident that the ratio

.

$$\frac{(ns+1)}{nr} < \frac{ns+2}{nr-1}.$$

Consequently, after multiplying [this inequality] by one and the same ratio r/s, also

$$\frac{(ns+1)r}{nrs} < \frac{(ns+2)r}{(nr-1)s}.$$

But the ratio

$$\frac{(nr+1)s}{nsr}$$

is equal to the ratio of the term M to its nearest term on the left and the ratio[5.1]

$$\frac{(nr+2)s}{(ns-1)r}$$

is the same as M has to the next one. And the ratio

$$\frac{(ns+1)r}{nrs}$$

is that of the term M to its nearest term on the right, and

$$\frac{(ns+2)r}{(nr-1)s}$$

is the ratio of that term to the next one. What was just shown may in the same way be also applied to all the other terms.

Therefore, the maximal term M is in a lesser ratio to the nearer term on either side than (if the intervals between the terms are the same) the nearer term is to the more distant one on the same side. QED.

Lemma 4. The number n in a binomial raised to the power nt can be taken so great that the ratio of the maximal term M to [any of the] two others, L and Λ distant from it by n terms on the left and on the right [respectively], would be greater than any given ratio.

Dem[*onstration*]. Since in the previous Lemma the maximal term M was found to be equal to (5.1) the terms on the left and on the right, L and Λ, in accordance with the law of the [formation of the] series (adding n to the last multiplier in the numerators of the coefficients, and subtracting n from the last multiplier in their denominators, adding the same n to the power of one of the letters r and s, and subtracting it from the power of the other letter), will be

$$\frac{nt(nt-1)(nt-2)...(nr+n+1)}{1\cdot 2\cdot 3\cdot ...\cdot (ns-n)} r^{nr+n}s^{ns-n}$$

and $$\frac{nt(nt-1)(nt-2)...(ns+n+1)}{1\cdot 2\cdot 3\cdot ...\cdot (nr-n)} r^{nr-n}s^{ns+n}.$$

And after a suitable cancellation of common multipliers,

$$\frac{M}{L} = \frac{(nr+n)(nr+n-1)...(nr+1)s^n}{(ns-n+1)(ns-n+2)...nsr^n},$$

$$\frac{M}{\Lambda} = \frac{(ns+n)(ns+n-1)...(ns+1)r^n}{(nr-n+1)(nr-n+2)...nrs^n},$$

or

$$\frac{M}{L} = \frac{(nrs+ns)(nrs+ns-s)...(nrs+s)}{(nrs-nr+r)(nrs-nr+2r)...nrs},$$

$$\frac{M}{\Lambda}=\frac{(nrs+nr)(nrs+nr-r)...(nrs+r)}{(nrs-ns+s)(nrs-ns+2s)...nrs}.$$

However, when n is assumed infinite, these ratios will [also] be infinitely large, because then the numbers 1, 2, 3 etc. will vanish as compared with n, and the numbers themselves $nr \pm n \mp 1$, $nr \pm n \mp 2$, etc., and $ns \pm n \mp 1$, $ns \pm n \mp 2$, etc. will have the same value as $nr \pm n$ and $ns \pm n$ [respectively], so that, after dividing [both parts of both last fractions] by n,

$$\frac{M}{L}=\frac{(rs+s)(rs+s)...rs}{(rs-r)(rs-r)...rs},\ \frac{M}{\Lambda}=\frac{(rs+r)(rs+r)...rs}{(rs-s)(rs-s)...rs}.$$

It is clear that these ratios are composed of as many ratios $[(rs + s)/(rs - r)]$ or $[(rs + r)/(rs - s)]$ as there are multipliers whose number is n, that is, infinite since the difference between the first multipliers $nr + n$ or $ns + n$, and the last ones, $nr + 1$ or $ns + 1$, is $n - 1$. These ratios [M/L and M/Λ] will therefore be equal to $[(rs + s)/(rs - r)]$ or $[(rs + r)/(rs - s)]$ raised to an infinite power and therefore simply infinite. If you doubt this conclusion, imagine infinity [of ratios] in a continued proportion with their ratio being as $rs+s$ to $rs - r$ or $rs + r$ to $rs - s$. The first ratio will be to the third as the square; to the fourth, as a cube; to the fifth, as the fourth [power], etc. Finally, the first ratio will be to the last one as infinite powers of the ratio $[(rs + s)/(rs - r)]$ or $[(rs + r)/(rs - s)]$. It is known, however, that the ratio of the first [ratio] to the last one is infinitely large since the last one = 0 (see Coroll. to Prop[osition] 6 of our [*Tractatus de*] *Seriebus Infinitis* [etc.][5.2]). It is therefore clear that infinite powers of the ratio $[(rs + s)/(rs - r)]$ or $[(rs + r)/(rs - s)]$ are infinite. It is thus shown that the ratio of the maximal term M to [any of the] two others, L and Λ, exceeds any assigned ratio. QED.

Lemma 5. Assuming the same as above, it is possible to imagine such a large number n, that the sum of all the terms from the intermediate and maximal M to both the [to any of the] terms L and Λ inclusive, is to the sum of all the other terms exterior to the limits L and Λ, in a ratio greater than any given ratio.

Dem[*onstration*]. Let the terms between the maximal M and the limiting term L on the left be designated thus: the second one from the maximal[5.3], F, the third one, G, the fourth one, H, etc.; and the second one beyond L, P, the third one, Q, the fourth one, R, etc. Since according to the second part of Lemma 3

$M/F < L/P$, $F/G < P/Q$, $G/H < Q/R$, etc.

and (Lemma 4), for an infinite n, M/L is also infinite, and

$$M/L,\ F/P,\ G/Q,\ H/R, \text{ etc.} \qquad (5.2)$$

are certainly infinite just as

$$\frac{F+G+H+...}{P+Q+R+...}$$

is. That is, the sum of the terms between the maximal term M and the limit L is infinitely greater than the sum of the same number of terms beyond and nearest to L. And since according to Lemma 1 the number of all the terms outside L is not more than $s-1$ times (i. e., not more than a finite number of times) greater than the number of terms between this limit and the maximal term M, and the terms themselves, in accordance with the first part of Lemma 3, become the smaller the further they are from the limit, the sum of all the terms between M and L (even without considering M) will be infinitely greater than the sum of all the terms beyond L. In a similar way it is shown that the sum of all the terms between M and Λ is infinitely greater than the sum of all the terms beyond Λ (whose number, according to Lemma 1, is not more than $r-1$ times greater than the number of the former).

Therefore, finally, the sum of all the terms situated between the limits L and Λ (the maximal term may be excluded) will be infinitely greater than the sum of all the terms beyond these limits. Consequently, this statement persists all the more if the maximal term is included [in the first sum], QED.

Explanatory Comment. Those, who are not acquainted with inquiries involving infinity may object to Lemmas 4 and 5 in the following way: Although, if n is infinite, the multiples of the magnitudes expressing the ratios M/L and M/Λ, that is, $nr \pm n \mp 1$, $nr \pm n \mp 2$, etc., and $ns \pm n \mp 1$, $ns \pm n \mp 2$, etc. have the same value as $nr \pm n$ and $ns \pm n$ since numbers 1, 2, 3… vanish with respect to each multiplier, it can still happen that, taken together and multiplied one by another, they increase to infinity (because the number of multipliers is infinite) and will infinitely decrease, that is, make finite, the infinite powers of the ratios $[(rs+s)/(rs-r)]$ or $[(rs+r)/(rs-s)]$.

I cannot obviate these scruples better than by showing now a method of deriving a finite number n, or a finite power of a binomial, for which the sum of the terms between the limits L and Λ has a larger ratio to the sum of the terms beyond them than any no matter how great given ratio, which I designate by letter c. Once this is shown, the objection will necessarily fall down.

To this end, I choose some ratio [greater than unity], less, however, than the ratio $[(rs+s)/(rs-r)]$ (for the terms on the left), for example, the ratio $[(rs+s)/rs]$ or $(r+1)/r$, and multiply it by itself so many times (m times) that the product becomes equal or exceeds the ratio of $c(s-1)$ to 1; that is, until

$$[(r+1)^m/r^m] \geq c(s-1).$$

When will this happen can be advantageously investigated by means of logarithms. Because, taking logarithms, we obtain

$$m\text{Log}(r+1) - m\text{Log}r \geq \text{Log}[c(s-1)]$$

and, after dividing, we find at once that

$$m \geq \frac{\text{Log}[c(s-1)]}{\text{Log}(r+1)-\text{Log}r}.$$

I continue in the following way. With regard to a series of fractions or multipliers

$$\frac{nrs+ns}{nrs-nr+r}, \frac{nrs+ns-s}{nrs-nr+2r}, \ldots, \frac{nrs+s}{nrs}$$

from which, according to Lemma 4, the ratio M/L is obtained by multiplying them one by another, it may be remarked that, although the separate fractions are less than the fraction $[(rs + s)/(rs - r)]$, they approach it the nearer the larger is the assumed n. Therefore, one of them will sooner or later become equal to the ratio $[(rs + s)/rs] = [(r + 1)/r]$ itself. It should be therefore found out how great n ought to be taken for the fraction whose ordinal number is m to become equal to $[(r + 1)/r]$ itself. But (as it is seen from the law of the formation of the series) the fraction of ordinal number m is

$$\frac{nrs+ns-ms+s}{nrs-nr+mr}.$$

Equating it to $[(r + 1)/r]$, we obtain

$$n = m + \frac{ms-s}{r+1} \text{ so that } nt = mt + \frac{mst-st}{r+1}.$$

I maintain that if this is the power to which the binomial $(r + s)$ is raised, the maximal term M will be more than $c(s - 1)$ times greater than the limit L. Indeed, for the assumed value of n the fraction of ordinal number m will be equal to $[(r + 1)/r]$, and the fraction $[(r + 1)/r]$, being multiplied by itself m times, that is [the fraction] $(r + 1)^m/r^m$, is (as constructed) equal or greater than $c(s - 1)$.

Therefore, this fraction [of ordinal number m] multiplied by all the previous fractions will all the more exceed $c(s - 1)$ since all these are greater than $[(r + 1)/r]$. Consequently, the product, being multiplied by all the following [fractions], will all the more exceed $c(s - 1)$ because each of these is at least greater than unity. But the product of all the fractions expresses the ratio of the term M to term L and it is therefore absolutely clear that the term M exceeds the limit L over $c(s - 1)$ times.

But, see (5.2), it follows that the second term after the maximal term M exceeds the second term after the limit L more than $c(s - 1)$ times, that the third term [after M] still more exceeds the third term [after L], etc. Therefore, finally, the sum of all the terms between the maximal M and the limit L will exceed the sum of the same number of maximal terms situated beyond this limit more than $c(s-1)$ times, and more than c times the same sum taken $(s-1)$ times.

Consequently, it is still more evident that it exceeds more than c times the sum of all the terms situated beyond the limit L whose number is not more than $s-1$ times greater [than the number of terms between M and L].

I proceed in the same way with regard to the terms on the right. I take the ratio

$$\frac{s+1}{s} < \frac{rs+r}{rs-s},$$

assume that

$$\frac{(s+1)^m}{s^m} \geq c(r-1)$$

and determine

$$m \geq \frac{\mathrm{Log}[c(r-1)]}{\mathrm{Log}(s+1)-\mathrm{Log}s}.$$

Then, among the series of fractions

$$\frac{nrs+nr}{nrs-ns+s}, \frac{nrs+nr-r}{nrs-ns+rs}, \ldots, \frac{nrs+r}{nrs}$$

included in the ratio M/Λ, I assume that the fraction having ordinal number m, namely,

$$\frac{nrs+nr-mr+r}{nrs-ns+ms},$$

is equal to $(s+1)/s$. I derive therefrom

$$n = m + \frac{mr-r}{s+1} \text{ so that } nt = mt + \frac{mrt-rt}{s+1}.$$

After this, it will be shown just as before that the maximal term M of the binomial $r+s$ raised to this power will be more than $c(s-1)$ times greater than the limit Λ, and also, consequently, that the sum of all the terms between the maximal M and the limit L will be more than c times greater than the sum of all the terms beyond this limit whose number is not more than $r-1$ times greater [than the number of terms between M and Λ]. And so we finally conclude that, upon raising the binomial $r+s$ to the power equal to the greater of two numbers,

$$mt + \frac{mst-st}{r+1},\ mt + \frac{nrt-rt}{s+1}.$$

the sum of all the terms included between the limits L and Λ will exceed more than c times the sum of all the other terms extending on either side beyond these limits. The finite power possessing the desired property is thus discovered, QED.

The Main Proposition. Now follows the proposition itself for whose sake all the previous was stated and whose demonstration ensues solely from the application of the preliminary lemmas to the present undertaking. To avoid tediousness, I name the cases in which some event can happen fecund or fertile; and sterile those in which the same event does not occur. In the same way, I name the experiments fecund or fertile if some fertile case appears in them and infertile or sterile when we observe something sterile.

Let the number of fertile cases be to the number of sterile cases precisely or approximately as r to s; or to the number of all the cases as r to $r + s$, or as r to t so that this ratio is contained between the limits $(r + 1)/t$ and $(r – 1)/t$. It is required to show that it is possible to take such a number of experiments that it will be in any number of times (for example, in c times) more likely that the number of fertile observations will occur between these limits rather than beyond them, that is, that the ratio of the number of fertile observations to the number of all of them will be not greater than $(r + 1)/t$ and not less than $(r – 1)/t$.

Dem[*onstration*]. Suppose that the number of the available observations is nt. It is required to determine the expectation, or probability that all of them without exception will be fecund; that all of them will be such with one, with two, 3, 4, etc. being sterile. Since, according to the assumption, there are t cases in each observation, r of them fecund and s sterile, and because separate cases of one observation can be combined with separate cases of another one, and then again combined with separate cases of the third, the fourth, etc., it is easy to see that the Rule attached to the end of the notes of Proposition 12[5.4] of Part 1 [of this book] and its second corollary containing the general formula by whose means the expectation of the lack of sterile observations, $r^m{:}t^m$; of the expectations of one, two, three etc. sterile observations

$$\frac{nt}{1}r^{m-1}s:t^m,\frac{nt(nt-1)}{1\cdot 2}r^{m-2}s^2:t^m,\frac{nt(nt-1)(nt-2)}{1\cdot 2\cdot 3}r^{m-3}s^3:t^m,...$$

are here suitable.

Therefore (after rejecting the common term t^{nt}) it becomes clear that the degrees of probability, or the number of cases in which it can happen that all the experiments are fecund, or all excepting one sterile, excepting two, 3, 4, etc. sterile, are expressed, respectively, by

$$r^m,\frac{nt}{1}r^{m-1}s:\frac{nt(nt-1)}{1\cdot 2}r^{m-2}s^2,\frac{nt(nt-1)(nt-2)}{1\cdot 2\cdot 3}r^{m-3}s^3,...$$

that is, by the terms themselves of the binomial raised to the power of *nt*, which were just studied in our lemmas. All the rest is now manifest. Namely, it follows from the nature of the series that the number of cases, which add *nr* fecund to *ns* sterile observations, is indeed [corresponds to] the maximal term *M* since, according to Lemma 3, *ns* terms precede, and *nr* terms succeed it. In the same way, the number of cases in which there occurred either $nr + n$ or $nr – n$ fecund observations with the others being sterile, are expressed by the terms *L* and Λ, *n* terms apart on either side from the maximal term *M*. Consequently, the total number of cases in which there are not more than $nr + n$, and not less than $nr – n$ fecund observations, is expressed by the sum of the terms situated between the limits and Λ. The total number of the other cases in which there occur either more or less fecund observations is expressed by the sum of the other terms beyond the limits *L* and [or] Λ. The power of the binomial may be taken so great that, according to Lemmas 4 and 5, the sum of the terms between the limits L and Λ inclusive is more than *c* times greater than the sum of all the other terms exceeding these limits. It is thus possible to take so many observations, that the number of cases in which the ratio of the number of fecund observations to the number of all of them does not exceed the limits

$$\frac{nr+n}{nt} \text{ and } \frac{nr-n}{nt} \text{ or } \frac{r+1}{t} \text{ and } \frac{r-1}{t},$$

is greater than *c* times the number of the other cases. That is, it will become greater than *c* times more probable that the ratio of the number of fecund observations to the number of all of them is contained between the limits $(r + 1)/t$ and $(r – 1)/t$ rather than beyond them. *Quod demonstrandum erat.*

When applying this to separate numerical examples, it is self-evident that the greater, in the same ratio, we assume the numbers *r*, *s* and *t*, the narrower can be made the boundaries $(r + 1)/t$ and $(r – 1)/t$ of the ratio r/t. Therefore, if the ratio of the number of cases r/s that should be determined by observation is, for ex[ample], one and a half, I take for *r* and *s* not 3 and 2, but 30 and 20, or 300 and 200, etc. It is sufficient to assume $r = 30$, $s = 20$ and $t = 50$ for the limits to become $(r + 1)/t = 31/50$ and $(t – 1)/t = 29/50$.

Suppose in addition that $c = 1000$. Then, according to what was prescribed in the Explanatory Comment, it will occur that, for the terms on the left and on the right respectively[5.5],

$$m > \frac{\mathrm{Log}[c(s-1)]}{\mathrm{Log}(r+1)-\mathrm{Log}r} = \frac{42,787,536}{142,405} < 301,$$

$$nt = mt + \frac{mst-st}{r+1} < 24,728,$$

$$m > \frac{\mathrm{Log}[c(s-1)]}{\mathrm{Log}(s+1)-\mathrm{Log}s} = \frac{44,623,980}{211,893} < 211,$$

$$nt = mt + \frac{mrt - rt}{s+1} = 25,550.$$

From which, as it was demonstrated there, it will follow that, having made 25,550 experiments, it will be more than a thousand times more likely that the ratio of the number of obtained fertile observations to their total number is contained within the limits 31/50 and 29/50 rather than beyond them. And in the same way, assuming c = 10,000 or 100,000 etc., we will find that the same is more than ten thousand times more probable if 31,258 experiments will be made; and more than a hundred thousand times if 36,966 experiments will be made; and so on until infinity, always adding 5708 other experiments to the 25, 550 of them. This, finally, causes the apparently singular corollary: if observations of all events be continued for the entire infinity (with probability finally turning into complete certitude), it will be noticed that everything in the world is governed by precise ratios and a constant law of changes, so that even in things to the highest degree casual and fortuitous we would be compelled to admit as though some necessity and, I may say, fate[5.6]. I do not know whether Plato himself had this in mind in his doctrine on the restoration of all things according to which everything will revert after an innumerable number of centuries to its previous state.

Notes

1.1. This remark conforms to information theory.

2.1. It was Bortkiewicz (1917, p. x) who noticed the new word in the *Ars Conjectandi* and put it into scientific circulation, although Prevost & Lhuilier (1799, p. 3) had preceded him. The *Oxford English Dictionary* included this word, which had already appeared in ancient Greece (Hagstroem 1940), with a reference to a source published in 1662.

2.2. Although an astrologer, Kepler (1610, §115; p. 238 in 1941) simply refused to answer definitely the same question. Times had changed! Bernoulli resumed this discussion in his Chapter 3.

2.3. The application of stochastic reasoning to one single case conforms to modern ideas.

2.4. John Owen (1563 – 1622). Haussner (Bernoulli 1713, German transl., p. 311) saw five editions of his *Epigrams*.

3.1. A few lines below I write *black tunic*, and, at the end of the chapter, *black toga*. Bernoulli himself applied three different nouns.

4.1. This is the very old principle of indifference. It can be perceived, for example, in the use of the arithmetic mean in astronomy since Kepler's time.

4.2. Arnauld was the main author of *L'art de penser* (Arnauld & Nicole 1662).

4.3. Bernoulli wrote *stones*; the German translation mentioned *small stones* (Steinchen).

4.4. The maximal term of the binomial $(r + s)^n$ is approximately equal to $1/\sqrt{2\pi nrs}$ and therefore decreases with an increasing n as $1/\sqrt{n}$, see e.g. Feller (1950, §3 of Chapter 6).

4.5. Adriaan Metius (1571 – 1635); Ludolph van Ceulen (1540 – 1610).

5.1. A misprint in this ratio was corrected without comment in all the translations.

5.2. Separate parts of Bernoulli's *Tractatus de Seriebus Infinitis* appeared in 1689 – 1704, and, for the first time as a single entity, in 1713 together with the *Ars Conjectandi*.

5.3. The "second" (repeated in the same sense in the Explanatory Comment below) is unusual: Bernoulli actually had in mind the term immediately neighbouring *M*. Cf.: February is the second month of the year, not the second after January. A similar remark is of course valid with respect to the "third" and the "fourth".

5.4. Bernoulli wrongly referred to Proposition 13. Haussner (Bernoulli 1713, German transl., p. 262) corrected him without comment.

5.5. The excessive number of significant digits below was the result of a venerable but misleading habit.

5.6. Bernoulli obviously had in mind the archaic notion of the Great Year ("innumerable number of centuries").

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V

Corrections and Short Notes on My Papers

Arch. Hist. Ex. Sci., vol. 28, No. 2, 1983, pp. 171 – 195

I have published thirteen papers in this Archive and some correction of almost all of them is overdue. In what follows, I take up these articles, one by one, note the mistakes and misprints, and point out references which should have been noticed before. It is almost self-explanatory that any review of the literature which appeared meanwhile is out of the question. I append indexes of names mentioned and an index of subjects.

Notation. Roman numerals denote my papers. A reference such as [ii; 17] indicates reference [17] from paper [ii]. Except for expressions "Replace [3] by [6]", [l], [2] etc. stand for items [1], [2] etc. Finally, l. 3b or etc. denotes line 3 from the bottom of the page while an arrow means "see (also) my contribution."

I. Newton and the Classical Theory of Probability

this *Archive*, vol. 7, No. 3, 1971, pp. 217 – 243

1. p. 218, middle. HALLEY and geometric probabilities → [x, p. 228].

2. p. 221, l. 8. Arc measurements: I mean meridian are measurements.

3. p. 224, middle. The proof is not given by NEWTON. In a letter dated 1972 Professor D. T. WHITESIDE commented:

The proof does exist in an unpublished MS and is more elementary than yours, viz. … *your immediately following inference that Newton's phrase 'motus regressus' is an 'astronomical expression' I cannot admit*. [Perhaps not only an astron. expression].

Regarding my § 1.3, WHITESIDE noted:

NEWTON in fact (but not in explicit statement) ... had a precise understanding of the difference between random and structurally 'inbuilt' errors. He was certainly, himself, absorbed by the second type of 'inbuilt' error, and his many theoretical models of differing types of physical, optical and astronomical phenomena were all consciously contrived so that these structural errors should be minimized. At the same time, he did, in his astronomical practice, also make suitable adjustment for 'random' errors in observation.

4. p. 227, just above § 2.1. Mean statistical values: In a letter dated 1971 Professor E. S. PEARSON opined:

From reading [the *Lectures on the history of statistics*], *I think I understand what K. P.* [PEARSON] *meant when he referred to Newton's view of the deity, who maintains mean statistical values ...; he has stepped ahead of where Newton had got to, by stating that the laws which give evidence of Design, appear in the Stability of the mean values of observations, i.e.* [he] *supposed Newton was perhaps unconsciously thinking what De Moivre put into words.*

5. p. 228, § 2.2.1. ARBUTHNOT → [iv, 95].

6. p. 231, § 2.3, l. 3. BOYLE's lectures: BOYLE'S will is published in his *Works* [5, p clxvii].
7. p. 238, § 3.1. LAMBERT → [ii, pp. 245 - 246].
8. pp. 240 – 241. Statistical method in biology → [xiii, § 5.8.1].
9. p. 241, § 3.3. ARISTOTLE → [vii, § 2.2].

II. J. H. Lambert's Work in Probability

this *Archive,* vol. 7, No. 3, 1971, pp. 244 – 256

10. p. 246, l. 4b. LAMBERT'S reasoning: COURNOT (1851, § 33 note) and then CHUPROV [xi; 61, p. 188] noticed it. Note its relation with the concept of a normal number.
11. p. 248, 1. 2b. Number of children in adjusted data: LAMBERT likely allowed for those who died in childhood.
12. I cite another passage from Professor PEARSON'S letter (see item 4):

Curiously, I find no reference to Lambert in these lectures [on the history of statistics]. *It was not because his writings were in German of which my father was an excellent scholar. I suppose, however, that he selected the names of the personalities he would study from a limited number of sources, e.g., Todhunter, and that these did not include Lambert's name.* [TODHUNTER did refer to LAMBERT, but failed to describe his work.] *Of course, K. P.* [PEARSON] *was over 70 by the time his history lectures passed the year 1750, and no doubt his exploration was limiting itself to the four Frenchmen, Condorcet, D'Alembert, La Grange and Laplace.*

III. On the Mathematical Treatment of Observations by L. Euler

this *Archive,* vol. 9, No. 1, 1972

13. p. 48, 1. 3 after formula (4). Weighing of observations: LIIOYD studied estimators with weights depending only on the order of the corresponding observations.
14. p. 48, l. 2b, 1. 6. Posterior estimators: the first to use them was SHORT, not PINGRÉ [iii, p. 48, note 17].

IV. Finite Random Sums etc.

this *Archive,* vol. 9, No. 4/5, 1973, pp. 275 – 305

15. p. 276, l. 2b. Dice with unequal numbers of faces not considered after MONTMORT: this should not imply that the behaviour of random sums was not studied in the most general case. And astragali deserve mention.
16. p. 287, l. 3b. Independence of events (observations) → [vi, p. 112, 1. 3b; viii, p. 172, l. 10 before § 3; ix, p. 11, lower half of].
17. p. 293, footnote. Discontinuity factors → [ix, p. 1, note l].
18. p. 294, 1. 9b. LAPLACE'S understanding of continuity of functions: he followed the generally accepted views of his time.

V. R. J. Boscovich's Work on Probability

this *Archive*, vol. 9, No. 4/5, 1973, pp. 306 – 324

19. p. 306, § 1.1.3. For a description of BOSCOVICH'S geodetic work see CUBRANIC [6].
20. p. 307, § 1.1, title. Arc measurements: see item 2.
21. p. 310, lower half. Adjustment of observations by LAPLACE → [ix, p. 41, footnote].
22. p. 312, solution of equations (1.3.2) → [xii, p. 33].

23. p. 312, l. 6 et seq. Number of exactly satisfied equations: Before one of BOSCOVICH'S restrictions (sum of errors equals zero) was allowed for, system (1.3.2) involved four unknowns. Its solution led to three equations that should be satisfied exactly. This fact corroborates GAUSS'S conclusion [v, p. 311, 1. 2b] which I previously did not believe in. Even before my paper [v] appeared in print, I had informed Professor R. L. PLACKETT of GAUSS'S "error" and he incorporated my wrong remark in his own article [ix; 63, p. 242]. Professor S. M. STIGLER who pointed out my mistake (letter dated 1981) added:

Gauss's last sentence [v, p. 312, 11. 1 – 2] *puzzles me* (*what does "before" refer to* ?) *but your suggested change* (Ibidem, 11. 8 – 9) *seems wrong to me.*

24. p. 316, 1. 4. Replace (a_{ij}) by (a_{ji}).

VI. Mathematical Treatment of Astronomical Observations etc.

this *Archive*, vol. 11, No. 2 – 3, 1973, pp. 97 – 126

25. p. 98, § 2.1. I should have included a subsection on LEVI BEN GERSON [13, 14, 23].

26. p. 108, § 3.3. Falsification of observations: BABBAGE published a short account [2] exposing various methods for falsifying observations.

27. p. 115, a passage from BRADLEY. I note a similar opinion of DESCARTES [7, p. 48]: *Je remarquais, touchant les expériences, qu'elles sont d'autant plus nécessaires qu'on est plus avancé en connoissance.*

28. p. 119, 11. 2 – 3 under Table 2. *Mittlerer Betrag recht und schlecht*: The original Latin expression *ex aequo et bone* means *in fairness and justice*. This correction is due to Professor N. SWERDLOW who expressed his view in a letter to Professor W. KRUSKAL. The letter dated 1979 contains a phrase *feel free to send copies around*. SWERDLOW disagrees with my interpretation of KEPLER'S treatment of observations [vi, p. 119]. He rather agrees with J. J. FILLIBEN. Rejecting the most deviating observation and assuming double weight for the middlemost observation, the latter arrived at the same estimate as KEPLER (and myself). However, EISENHART [9, p. 356], who quoted'Filliben and referred to Professor O. GINGERICH, proved that the problem is rather complicated. [Cicero stated that that Latin expression implied: *rather than according to the letter of the law,* and I infer that the arithmetic mean became then (or somewhat earlier) *the letter of the law.* In more detail see Sheynin (2017, p. 32).]

29. p. 119, lower half. An English translation of BODINI'S book was published in 1606 and reprinted in 1962 in Cambridge (Mass.). The relevant portion of the book occupies pp. 781 – 792 of the reprint.

30. p. 120, first passage from KEPLER → [ix, p. 49, note 12].

31. p. 120, last passage from KEPLER. Statistical procedures connected with coining money deserve special attention [31, pp. 79 and 81; 29].

32. That the errors of TYCHONIAN observations did not exceed 8' (p. 120) was my misunderstanding; the precision of these observations, as KEPLER stated, enabled him to distinguish errors of 8'.

VII. On the Prehistory of the Theory of Probability

this *Archive*, vol. 12, No. 2, 1974, pp. 97 – 141

33. p. 97, note 2a. SCHILLER on randomness → [xiii, p. 330, note 15].
34. p. 102, 1.1. SAMBURSKY quotes SIMPLICIUS: In a reprinted version of his article [x; 117], he amended his reference to SIMPLICIUS. Elsewhere [24, pp. 55 and 97 – 98] he supplied related information on LEUCIPPUS, ALEXANDER APHRODISIENSIS and CHRYSIPPUS.
35. p. 104, l. 3. Chance and production of females: according to VAN BRACKEL [x; 103, p. 125] ARISTOTLE considered phenomena with logical or subjective probability less than 0.6 – 0.9 to be accidental. VAN BRACKEL'S criticism (Ibidem, p. 120) of my article [vii] or at least of its first sections seems to be correct.
36. p. 105, l. 8. A moral law of large numbers: cf. RABINOVITCH [22].
37. p. 107, middle. Ordeals were cooked-up frauds: this was the opinion of TYLOR [32]. He asserted that the rate of "success" was 50%.
38. p. 109, note 55. Quantification of qualitative characteristics → [X, p. 217, end of footnote].
39. p. 111, passage from CANTOR. JOAN GADOL (see my note 59 on p. 110) rather than I noticed this passage.
40. p. 112, 1.6. Complexity of games of chance. An appropriate example is the game of the bowl, the principal game of hazard among the northern tribes [of Indians] [26, pp. 85 – 87]. See also LONGFELLOW'S *Hiawatha*, chap. xvi.
41. p. 113, end of note 69. Accusation of gambling: see also BUFFON [viii; 9, pp. 67 – 69].
42. p. 114, middle. KEPLER'S expression *unmathematische Glückspielmethode.* SCHNEIDER [25, p. 56, note 32] maintains that this expression (a translation from the original Latin) is *unglücklich und irreführend.* Discussing successive approximations in algebra, WALLIS [35, p. 254] used the term *Stochastick Process.*
43. p. 130, l. lb. Section 8.2.1 should be 8.1.2.
44. p. 131, l. 2. All planets return to their position at the moment of creation: according to ancient belief, this event would have brought about the end of the world [15, p. 440; 34].
45. p. 135, § 9.2, 1.2. An argument about randomness and necessity: the first such argument is due to NICOLAUS BERNOULLI and DE MOIVRE rather than to KANT [iv, p. 303].
46. p. 137, text between formulas (1) and (2). The reference to KEPLER should be § 8.1.2 (cf. item 43); as to CARDANO, the reference should be to his *Book on games of chance* [vii, note 57].
47. p. 138, 1. 3b, 1. 3. Limit theorems and. the paradox of the heap → [viii, p. 162, footnote].

VIII. P. S. Laplace's Work on Probability

this *Archive,* vol. 16, No. 2, 1976, pp. 137 – 187

48. p. 139, just before § 2. New reference: I failed to mention that LAPLACE published a short announcement on the forthcoming publication of his *Théor. anal. prob.* (*Comm. temps* pour 1815 (1812), pp. 215 – 221).
49. p. 141, 1.3 before § 2.2. Change [32] for [17].
50. p. 159, the population of France: BERTILLON [3] published the *Résultats sommaires des recensements* in European countries. On p. 30 he presented the figures for France throughout the 19th century.
51. p. 161, l. 1. Include missing reference [viii; 45].
52. p. 172, 1. 2b before § 3.1. Replace "independently" by "independent".
53. p. 17.2 A line which should have been 6b is missing. It reads: *of his scientific career* ([39], p. 144; [32], [33]).
54. p. 173. Note * should have been placed before the first table.
55. p. 173, § 3.2, 1. 2. Replace ([39], p. 145 and [43], p. 132; [79], p. 296) by ([39], p. 145 and [44], p. 296; [79], p. 132, note 146).
56. p. 177, l. 3b. The epigraph referred to is that to this article [viii].
57. p. 175, 1. 4b. Mean interval between molecules: G1LLISP1E [12, p. 438] noted the introduction of this concept by POISSON.
58. p. 175, note *. Introduction of delta function → [xi, pp. 250 – 252].
59. p. 179, § 4.2. I should have referred to my earlier contribution [iv, pp. 300 – 301] where I compared LAPLACE's main work with a monumental maze.
60. p. 184, Note on HUYGENS → [x, § 4].
61. p. 186, ref. [42]. This should have been to LAPLACE'S Sur les comètes [xii, 95].
62. p. 187. Ref. [80] is missing. This is my contribution [ix].
63. p. 187, ref. [81]. The volume of the *Istoriko-Matematicheskie Issledovania* is 20 rather than 2.

IX. Laplace's Theory of Errors

this *Archive*, vol. 17, No. 1, 1977, pp. 1 – 61

64. p. 2, 1.2 above § 2. Delete 'my' from 'my earlier investigation'.
65. p. 5. Unnumbered equation after formula (2.2.2): it should be

$$\varphi(x) = \frac{m}{2}\exp^{-m(x-v)}.$$

66. p. 5, formula (2.2.3). In LAPLACE'S original memoir (not in the *Oeuvr. compl.)* the denominator in the formula was 3.
67. p. 39, l. 3. Measuring angles of a triangulation → [xii, p. 50, note 46].
68. p. 45, note 11. LAPLACE'S azimuths: their use in the adjustment of triangulation presupposes the knowledge of the deviations of the vertical.
69. p. 50, ll. 4 – 2 above § 11. The end of the phrase must read: *associated with the estimation of the precision of observations* (*for example, with the study of the stochastic behaviour of L*) *become extremely complicated for integer m* > 1 *as compared with* etc.

X. Early History of the Theory of Probability

this *Archive*, vol. 17, No. 3, 1977, pp. 201 – 259

70. p. 210, after l. 2b, ULPIANUS's table: SENTEMANN [27, p. 252] maintained that ULPIANUS based his table on moral and legal considerations rather than on statistical data.
71. p. 212, just before § 2.3.3. Insurance societies: BABBAGE [l] described the conditions for life insurance stipulated by the main insurance societies of the day.
72. p. 212, note 2. Compound interest adversely influenced life insurance: HEYDE & SENETA [xiii; 73, p. 37] refute this thesis.
73. p. 217, just before § 2.4.2. New reference: PTOUKHA [21] studied the work of PETTY, GRAUNT and HALLEY.
74. p. 223, middle of. Quotations from the *Logique de Port-Royal*: these are from pp. 355 and 372.
75. p. 229, end of note 1. T. PAINE and his pension programme: In a letter dated 1977 Professor W. KRUSKAL remarked:

There were many pieces of then-published evidence that Paine might have used; the puzzles are (1) *why he did not*, (2) *why his many critics did not pick at that weak spot* (*weak both methodologically and in terms of estimated cost for the proposed pension program*), *and* (3) *why Richard Price wasn't consulted to set Paine straight*.

76. p. 240,11 3b and 6b. The work of HUYGENS. In a letter dated 1981 Dr. O. REIERSOL objected to my phrase Huygens *only describes expectation* and noted that I did not give any indication of how HUYGENS proved most of his propositions.
[On this p. 240 I wrote that HUYGENS directly calculated the expectations *sought*.] REIERSOL argued that HUYGENS proved theorems about, rather than described, expectation [exactly so] and that he, REIERSQL [x, 85], offered a possible explanation of HUYGENS's method. A recent study of HUYGENS' s work in probability is FREUDENTHAL [11].
77. p. 241, formula (4.1.l). Its correct explanation is due to REIERSOL himself[x; 85]. He informed me of this fact in a letter dated 1980.
78. p. 241,11. 5 – 6. Interpretation of HUYGENS's problem. in the same letter REIERSOL objects to the interpretation of this problem, as solved by HUYGENS, in terms of conditional probabilities.
79. I have referred to games of chance indirectly [x, pp. 222 – 223, first few lines of § 2.4.4 and p. 223, note 1].

XI. S. D. Poisson's Work in Probability

this *Archive*, vol. 18, No. 3, 1978, pp. 245 – 300

80. p. 271, note 25, l. 2b. Mean interval between molecules: see item 57.
81. p. 279, formula (5.2.2. 3). As given by POISSON himself, the coefficient is $2/\sqrt{\pi}$ (not $2/\pi$).
82. p. 289, § 7.1, 1. 3. Change §3.2.2 to § 3.3.2.
83. I long ago decided to restrict my research in probability theory to events which happened before the middle of the 19th century (with, possibly, occasional forays into alien territory).

XII. C. F. Gauss und the Theory of Errors

this *Archive*, vol. 20, No. 1, 1979, pp. 21 – 72

84. p. 33, footnote 17. GAUSS and linear programming: see item 23.

85. p. 38, § 4.4. The death of the probable error: astronomers used the probable error until recently and may be still use it once in a while. It is therefore opportune to quote L. STRUVE [30, see *Thesen* on the last (unnumbered) page]:

Statt des wahrscheinlichen Fehlers sollte allgemein der mittlere Fehler benutzt werden.

Note another of his *Theses* (Ibidem):

Die Anwendung sechs- und siebenstelliger Logarithmentafeln in den Schulen sollte verboten werden.

86. p. 44, just before § 5.7. Substantiation of least squares: In a letter dated 1979 Professor W. KRUSKAL writes:

Despite the comments of … and others, I am not wholly persuaded that Gauss published a full statement of what you call the third substantiation, that is (*roughly speaking*) *least squares as minimum variance estimation, given linearity and unbiasedness of estimator.* [See Sheynin (2012).]

87. p. 48, l. 2b. Role of MARKOV: NEYMAN [20, p. 228] subsequently admitted *the confusion* to which he

Unwittingly contributed by attributing to Markoff the basic theorem an least squares.

88. p. 51. Observations in triangulation: one of the leading Soviet statisticians, the late Professor L. N. BOLSHEV, attempted to study this topic from the vantage point of modern statistics. I do not know whether he had time to complete his research, but at least we discussed the historical aspect of the problem. BOLSHEV thought that GAUSS was in favour of attaining a (formal) equality of the variances of the observed angles or directions. I disagreed and later formulated my opinion in this article [xii, p. 51] but my highest respect for BOLSHEV'S scientific expertise obliges me to report his point of view. For the same reason I make known a finding of his which I was unable to confirm: one of the geodetic volumes of GAUSS'S *Werke*, BOLSHEV asserted, contained an example of the chi-squared distribution. [See Sheynin (1988).]

BOLSHEV read my papers and told me he understood them (no‘ doubt, a polite reference to errors and ambiguities) and considered them useful; moreover, he corrected a few of my early MSS. For what unsubstantiated evidence is worth, I recall BOLSHEV' s words:

It was after reading your article [vii] *that I came to understand* [the statistical aspect of] *the work of Kepler*.

89. p. 54, § 6.5. GAUSS as the master of experimental science: I should have referred to HERMANN [16].

90. p. 56, § 7.1, l. 3b,1.1. Replace [47] by [46].

91. p. 61, l. 2b. Introduction of word *statistics* into English: see also HILTS [xiii; 73a, pp. 24 – 25].

92. p. 67, Acknowledgements. Replace D. H. L. HARTER by Dr. H. L. HARTER.

93. Article as a whole: At the time, I could not have known about other contributions on the same subject [28, 33] published somewhat before mine. I did not refer to one of Dr. C. EISENHART'S unassuming contributions [8] (which I mentioned elsewhere [ix; 42]). I did not then realize that

(1) EISENHART was one of the first Western authors to present a correct view of GAUSS'S work in statistics.
(2) The large number of references in my paper [xii] made any omission unduly significant.

EISENHART also published a biography of GAUSS [ix; 43]. It was all but unavailable and at the time I had not seen it since 1975 and did not dare to refer to it once more. It is now reprinted [10].

It was not my intention to discuss GAUSS'S linear model. On my p. 441 referred in this connection to IDELSON [xii; 79, chap. 5] and SEAL [xii; 123] and I can additionally mention HELMERT [xii; 75]. I always thought that such authors as HELMERT and IDELSON had adequately described all GAUSS'S achievements in this direction though of course not in the language of modern statistics.

XIII. On the History of the Statistical Method in Biology

this Archive, vol. 22, No. 4, 1980, pp. 323 – 371

94. p. 329, 1. 1b. *Recherches statistiques* etc: vol. 5 appeared in 1844, fourteen years after FOURIER died.
95. p. 336, § 3.2.1, 11. 5 – 6. Chance as ignorance of causes: LAMARCK repeated this statement elsewhere [xiii; 91, p. 329].
96. p. 343, note 38, 1. 3. Replace "who most possibly also decided" by "who likely took a too resolute point of view".
97. p. 361, note 71, 11.2 – 1b. HERSCHEL referred to SWIFT: so he did [17, p. 63, footnote]: the philosopher's

Plan for writing books has a close parallel in the theories of the production of animals … by natural selection. BAER was not the only one to present a narrow view on evolution! [SWIFT borrowed his *philosopher* from RAYMOND LULLY (13^{th} – 14^{th} century).]
98. p. 361, § 5.8.1. The Biometric school: cf. MACKENZIE [18; 19, pp. 82 – 91]. This source contains chapters on GALTON and K. PEARSON, and on the development of the statistical theory.
99. p. 363, 1. 2. Reference to PEARSON: replace [107, p. 321] by [106, p. 321].

XIV. On the History of Medical Statistics

this *Archive*, vol. 26, No. 3, 1982, pp. 241 – 286

100. § 1.2, end of note 4, Addendum: MENDELSOHN [19a, p. 204] testified to the fact that *medical probabilities* became acknowledged, at least for some time:

Diese Art von Wahrscheinlichkeit, da wir das Verhältnis der Fälle selbst, erst durch einen wahrscheinlichen Schluss suchen müssen, nennet Rüdiger [*De sensu veri & falsi*] *die medicinische Wahrscheinlichkeit, weil man in der Heilungskunst aus dem Verhältnisse derer, die an einer gegebenen Krankheit gestorben, oder durch ein gewisses Arzneymittel genesen sind, zu der Zahl derjenigen bey welchen dieses nicht erfolgt ist, auf die Wahrscheinlichkeit in einzelnen vorkommenden Fällen schließt.*

A. RÜDIGER'S (RIDIGERI'S) book was published in 1741 in Leipzig; see *Brit. Mus. Cat.*

Acknowledgements. Professor S. M. STIGLER advised me to compile an Index, and items 13, 18, 23, 65 and 66 are due to him. Professor K.-R. BIERMANN noticed a mistake now pointed out in

item 90. Professors G. COHEN, W. KRUSKAL, E. SENETA and S. M. STIGLER have sent me xerox copies of some sources to which I refer in these Corrections.

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Index of Names

Here, as also in the Subject Index below, Roman numerals‘denote my papets. Ref. xiv stands for my article On the history of medical statistics in this *Archive*, and entries such as xiv 1.2, 5 mean §§ 1.2 and 5 of this article. In other cases, Arabic numerals denote pages.

Index of Subjects

Linguistics

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Mathematics

VI

History of Medical Statistics

Arch. hist. ex. sci., vol. 26, 1982, pp. 241 – 286

1. Introduction

1.1. The Statistical Method. I [129, § 1] have defined the statistical method in experimental science as a method of reasoning based on the mathematical treatment of numerical data. Now, I subdivide the history of the method into three stages. During the first one, statements based on observed statistical regularities were put on record [125, § 6.2]. In actual fact, the very origin of medicine came about in this manner [19, Proemium, p. 19]:

Careful men noted what generally answered the better, and then began ta prescribe the same for their patients. Thus sprang up the Art of medicine.

Hippocrates's aphorisms or the assertions of FRACASTORI [41, book 1, p. 36. and book 2, p. 41][1] or even the work of RAMAZZINI (§2. 2) may serve as relevant examples.

The second stage is distinguished by the availability of statistical data. Scholars had then arrived at important conclusions either by means of simple stochastic ideas and methods, or even directly, as before. The work of GRAUNT (which marked the beginning of this stage in medicine) and. the proof, in the middle of the 19th century, that cholera *poison* diffuses in (unpurified) drinking water (§ 7.3.2)[2] are two good cases in point respectively.

The third stage, which dates back to the end of the same century, is the one during which inferences were, and are, checked by quantitative criteria.

The statistical method is not clearly separated from the experimental method. I am not concerned with the latter. In passing, I note that in medicine it originated with HARVEY and that, in the 18th century, LIND [69] and WATSON [149] applied it for the study of preventive measures against scurvy and inoculation of small pox, respectively.

I think that, beginning with its second stage, the statistical method pertains to mathematics. Indeed, the second stage of the method constitutes the prehistory of exploratory data analysis, a branch of mathematical statistics which has seen a [mathematical] renaissance in the 1960's and 1970's [l, p. 97].

1.2. The Statistical Method Established in Medicine. Is it possible to reconcile the individual approach to a given patient with an abstract statistical point of view? In 1835, during a discussion at the Paris Academy of Sciences, POISSON [128, p. 285] mentioned this problem and contended that in respect to the use of statistics medicine did not differ from other sciences.

In any case, the statistical method did gnaw its way into medicine. First, demography essentially used the method. GRAUNT and PETTY were pioneers of both demography and. medical statistics.[3] Late in the 17th century LEIBNIZ busied himself with demography [127, p. 225] but his five manuscripts remained unpublished until 1866. LEIBNIZ did not collect statistical data, but at least he urged practitioners to record their observations. He also proposed to compile an encyclopaedia of medical sciences (see note 9) and recommended the establishment of a special *Collegium Sanitatis* to supervise shops, bakeries etc.

At the close of the 17th century, HALLEY compiled a mortality table for a stationary population and explained how to estimate populations from data on births and deaths. Eminent scholars of the 18th century (DANIEL BERNOULLI, LAMBERT, EULER) studied laws of mortality, birth rates and sickness. Their works belong to the history of probability theory and of medicine. Beginning with the researches of SÜSSMILCH population statistics exists as a separate scientific discipline. He devoted about fifty pages of his main work [141] to statistics of sickness and mortality. He held that mortality from any given disease is stable (Bd. 2, p. 408)[4]; he proposed to standardize the names of diseases (p. 424) and advocated the inoculation of smallpox (p. 440).

Second, the range of application of the statistical method greatly widened after the emergence, in the middle of the 19th century, of public hygiene (the predecessor of ecology) and epidemiology[5], which were (and are) close linked with population statistics (and with each other).

Third, at about the same time (the middle of the l9th century) surgery and obstetrics, branches of medicine proper, yielded to the statistical method. Fourth and last, in 1825 P. LOUIS introduced what is called the numerical method of studying symptoms of various diseases. His proposals amounted to the use of the second stage of the statistical method in its simplest form (without stochastic considerations). Discussions on the numerical method lasted for at least a few decades. With all the shortcomings of his method, LOUIS greatly contributed to the application of the statistical method in medicine.

1.3. The Aim of this Paper. The main sections here are devoted to the second stage of the statistical method, i.e. to the period from the second half of the 17th and, approximately, to the second half of the 19th century. I restrict my account to medical statistics in a narrow sense of the expression. Thus, I present a barest possible outline of the history of public hygiene, and I do not treat obstetrics because the available literature pertains rather to physical anthropology, e. g., SIMPSON'S statistical inquiries into the influence of the duration of' labour on proportions of mothers lost, infantile deaths and stillbirths [135] and the duration of human pregnancy [136]. Of course, I do not repeat my investigation of the relevant works of DANIEL BERNOULLI, D'ALEMBERT and LAMBERT [123; 124] or of the scholars mentioned at the beginning of § 1.2 [127].

There is no general literature on my subject except for studies [82; 67; 116] devoted to the history of public hygiene. The main achievement of this paper is a systematic description of its subject, medical statistics before and including the middle of the 19th century. I also describe, for the first time, the work of SEIDEL (§§ 7.4.2 – 7.4.3) which contains ideas and methods pertinent to mathematical statistics.

2. Remarks on the First Stage of the Statistical Method

2.1. Astrology. At least until the middle of the 17th century scholars of the highest calibre believed in, and practised, astrology. There were at least two different points of view in regard to the manner in which heavenly bodies influenced earth (and man in particular). For some savants, this astrological influence was a fatal drive, for others, only a general tendency. KEPLER resolutely held on to the latter opinion and even considered himself the founder of astrology as a science [125; 57].

But he had predecessors, and R. BACON was one of them. BACON maintained that astrology might discover general tendencies [28, p. 378] and *predict human behaviour statistically* (p. 382). It seems likely that physicians of old adhered to one or the other traditional belief. I present a few examples concerning *correlationists* and offer a short comment.

Astrological almanacs, insofar as they mentioned epidemics, sometimes spoke of tendencies. This was the case with the astrological study of the plague for the year 1502 [143, vol. 5, p. 161]. And, of course, there was also the correlative link between the moon and one or another disease. Thus, in a posthumous publication of 1627 SPIEGEL [143, vol. 7, p. 125][6] held that epilepsy was apt (!) to come on the time of the new moon.

A prudent, though no less faulty approach of some scholars consisted in the introduction of intermediate [correlative] causes. Thus, IBN SINA (AVICENNA) [54, 1.2.2.1.8] thought that remarkable astronomical phenomena brought about changes in meteorological conditions which in their turn influenced health. A physician in the early 18th century [78, p. 183] asserted that bleeding occurs when the resistance (!) of the atmosphere is least and (p. 187) that alterations in the weight and pressure of the atmosphere may influence crises in acute diseases.

He somehow connected the changes in the atmosphere with the influence of the moon and he even spoke of determining the share which the alterations may have in them (in the diseases?). Important questions which I did not answer are:

(1) (How) did the fatalists allow for the variations between men in their response to disease and treatment?

(2) (How) did ancient and medieval philosophers (THOMAS AQUINAS) take medicine into account in their general studies of the influence of heaven upon earth? In their explanation of randomness? Cf. GALEN [125, pp. 119 – 120].

(3) What did KEPLER think about the links between astrology and medicine?

See also my preceding article [125, § 7].

2.2. Occupational diseases. I devote this subsection to the classical study due to RAMAZZINI [115]. Presenting his account, he recorded typical, i.e. statistically prevalent cases. At least in one instance (chapter 37, p. 434) he compared two relative frequencies of a disease:

J'ai observé que les Religieuses sont attaquées de hernies plus fréquemment que les autres femmes; ce qu'il faut attribuer à leurs chants trop violens, aussi bien que celles qui arrivent aux Moines.

Elsewhere (chapter 4, p. 56) RAMAZZINI obviously recognized statistical considerations:

Il s'est élevé, il y a quelques années, un procès … entre un habitant de Final … & un commercant. ... Ce dernier avoit, à Final, un vaste laboratoire où il fabriquoit le sublimé. L'habitant apella le commercant en Justice, le pressant de changer son laboratoire de lieu, parce qu'il incommodoit tout le voisinage par les vapeurs du vitriol. Pour appuyer son accusation, il avoit une attestation d'un Médecin … & un nécrologe du Curé, qui démontroient qu'il périssoit chaque année plus de monde dans ce bourg, & sur-tout dans le voisinage du laboratoire, que dans les lieux d'alentour, Le Médecin attestoit, que le marasme, & les maladies de poitrine sur-tout, tuoient presque tous ceux qui étoient voisins du laboratoire, & il en attribuoit la cause aux vapeurs du vitriol…. Enfin, les Juges renvoyerent le marchand absous, & déclarèrent innocent de vitriol. … Je laisse aux Naturalistes à juger, si ce Juris consulte ne s'est pas trompé.

Regrettably, RAMAZZINI did not adduce the relevant figures. His example bears a direct relation to the history of public hygiene.

RAMAZZINI also formulated his opinion on the sickness of physicians and the general appearance of mathematicians (Suppl., chapter 14, p. 174). He might have known about ten, or even twenty of the former, but he was hardly acquainted with more than a few of the latter. Here is what he wrote:

(1) During epidemics physicians are affected rather seldom.

As I see it, … this is not because of their prudence, but rather due to their experience and cheerful mood when they return home with a lined purse. … Physicians endure hardships only when nobody else endures them.

(2) *Almost every mathematician … is unworldly, lethargic, suffers from drowsiness, and is utterly impractical. The organs of mathematicians and their whole bodies inevitably become numb*[7].

The rise of public hygiene in the 19th century led to the resumption of the study of occupational diseases.

3. Prehistory of the Numerical Method

3.1. Mead. In the very beginning of the 18th century MEAD [77, p. x] expressed his rather naive hope that

In a short time mathematical learning will be the distinguishing mark of a physician from a quack. … He who wants this necessary qualification, will be as ridiculous as one without Greek or Latin.

He [78, p. cliii] also maintained that

Medicine still deals so much in conjecture[8]*, that it hardly deserves the name of a science. Whether this be owing to the nature of the art, as being incapable of sure principles; or rather, to the artists …*

3.2. D'Alembert. MEAD did not mention statistics at all, but his considerations belong to the prehistory of the numerical method (§ 4). The same is partly true in regard to D'ALEMBERT'S thoughts [29, p. 163]:

La médecine systématique me parait un vrai fléau du genre humain. Des observations bien multipliées, bien détaillées, bien rapprochées les unes des autres, voilâ à quoi les raisonnemens en médecine devraient se réduire.

D'ALEMBERT did not content himself with this rather one-sided assertion. He (Ibidem and p. 167) also contended that

(1) A physician is *un Aveugle armé d'un. bâton. … Il léve son bâton sans savoir où il frappe; s'il attrape la Maladie, il tue la Maladie; s'il attrape la Nature, il tue la Nature*.

(2) *Le médecin le plus digne d'être consulté, était celui qui croyait le moins à la médecine*.

D'ALEMBERT raised objections to the basic principles of probability theory. Being sometimes altogether wrong, his criticisms revealed the need to sharpen some propositions of the theory and the methodology of its application. He specifically attacked DANIEL BERNOULLI'S study of the inoculation of small pox (§ 7.2.1). At the same time he [29, p. 175] included medicine among the sciences in which the [mathematical] *art de conjecturer est nécessaire*.

I think that because of D'ALEMBERT's pronouncements in mathematics and medicine some scholars regarded him as a bull trying to put the china shop of science in order.

3.3. Black. Late in the 18th century BLACK advocated the application of mathematics and statistics in medicine. Thus, he [8, p. 38] *strenuously* recommended *Medical Arithmetick, as a guide and compass through the labyrinth of Therapeuticks.* He (p. 56) also proposed

To spread out and to review, in one general Chart, the enormous host of diseases which disgorge their virulence over the earth, and, with frightful rapacity, wage incessant hostilities with mankind. By this means we shall be warned to make the best disposition and preparation for defence[9].

BLACK (pp. 65 – 68) compiled a

Medical catalogue of all the principal diseases and casualties by which the Human Species are destroyed or annoyed.

He also appended a Statistical table to his book, a *Chart of all the fatal diseases and casualties in London, during … 1701 – 1776*[10].

BLACK (p. 235) blamed physicians for their ignorance of statistics:

Except for the few high priests … the rest of the Esculapian train are nearly as ignorant as the ancients.

For his own part, he (pp. 414 (pp. 414 – 430) noticed certain shortcomings in the compilation of statistical data on the birth-rate, mortality and sickness in London.

BLACK'S thoughts on medical observations were self contradictory. On the one hand, he (p. 394) attacked *the empiricism of licensed murderers* and maintained [7, p. 430] that

Dans les écrits même désAuteurs qui jouissent d'une grande réputation, les faits nouveaux & les observations originales de quelque utilité sont extrêmement rares.

He (Ibidem, p. 424) even set off observations against theory:

Les Médecins-Théoricièns au lieu de marcher pas-à-pas dans la recherche de la vérité, ont essayé de voler. Ils ont cru qu'il étoit *nécessaire de rendre compte de tous les phénomènes & d'expliquer toutes les difficultés d'une manière philosophique & méthodique*.

(BLACK adduced an example to the effect that scholastic arguments were no substitute for action.)

On the other hand, he (p. 394) stated:

Multipliez les observations est le cri général. … Pour combattre avec plus de succès les maladies [he mentioned fifteen diseases including cancer] *& la mort, il ne nous manque aujourd'hui que des rémèdes, des rémèdes, & encore des rémèdes*.

3.4. Condorcet. He [25, p. 542] stressed that observations

Suivies et multiplies … peuvent nous apprendre de vérités utiles sur le rapport de notre régime, de nos habitudes, de notre constitution organique, et de ses dérangements, avec nos facultés intellectuelles, nos passions et notre constitution morale. …je ne m'attacherai point à prouver la nécessité de suivre ces observations dans la vue de prévénir ou de guérir les difformités naturelles, et les maladies réputées incurables; d'arrêter les contagions, ou de prévoir et de dissiper les causes des épidémies[11].

3.5. Pinel. According to PINEL [99, p. 3]

Il faut prendre pour guide en médecine la méthode [of attentive observation of' each object] *qui réussit constamment dans toutes les parties de l'histoire naturelle …*

He (p. 402) remarked that

Une expérience, pour être authentique et concluante, … doit être faite sur un grand nombre de malades asservis à des régles générales et dirigés suivant un ordre déterminé. Elle doit être aussi établie sur une succession régulière d'observations constatées avec un soin extrême et répetées. … Enfin, elle doit rapporter également les événemens favorables comme ceux qui sont contraires[12].

Lastly (p 406), stochastic considerations are necessary for a comparison of two competing methods of medical treatment.

PINEL devoted the concluding pages of his book to a statistical study of the treatment of mental patients at his hospital and contended (p. 424):

Il est nécessaire d'y appliquer les notions élémentaires du calcul des probabilités, ce qui n'a été fait encore que pour l'hospice de la Salpêtrière[13].

He calculated statistical probabilities for the recovery of various groups of cases and compared these probabilities with each other. PINEL largely repeated his considerations and reprinted his statistical data in another contribution [100]. He (p. 169) again mentioned probability theory:

Medicine doit être fondée sur la théorie des probabilités … sur laquelle doivent désormais porter les méthodes de traitement des maladies, si on veut les établir sur un fondement solide.

In spite of PINEL's references to the theory of probability, his method did not differ from that later introduced by LOUIS (§ 4).

4. The Numerical Method

4.1. Louis. He and his numerical method of reasoning based on the comparison of statistical estimates with each other, occupy a special place in the history of medical statistics. LOUIS [74, p. xvii] contended that

Dans une suite d'observations, les données d'un problème à plusieürs inconnues dont il faut trouver la valeur; et comme en mathématiques cette valeur ne change pas avec les personnes qui s'occupent de la solution du problème, on doit nécessairement aussi, en médecine, obtenir des résultats identiques de l'analyse des mêmes observations. … Mais quels résultats obtenir de la considération de faits douteux, incomplets ou faux? (p. xviii).

The numerical method amounted to the collection and ordering of numerical facts without stochastic considerations. I do not think that LOUIS ever introduced a formal definition of his method.

DAVIDOV [32, p. 84] was the first mathematician to comment favourably on it. LOUIS (p. 72) studied the frequency of the occurrence of certain symptoms or other features of illnesses, e. g., their duration (p. 185). Sometimes he based his deductions on a small number of observations [75, pp. 85 – 86], but he noted that the situation would be better with the accumulation of data. Moreover, he remarked:

Si done il y a moyen de recueillir l'expérience des siècles en thérapeutique, ce ne peut être qu'en employant la méthode numérique[14].

In one instance he (Ibidem, p. 75) advised to use a statistical procedure, a random distribution of patients, for the comparison of competing methods of treatment[15]. Inevitable errors of observation, he (pp. 76 and 111) thought, would largely compensate each other. Probably relying upon this belief, LOUIS never estimated the trustworthiness of his observations. He had many predecessors among both scholars in general (D'ALEMBE'RT, CONDORCET, §§ 3.2 and 3.4) and physicians (BLACK, PINEL, §§ 3.3 and 3.5).

LOUIS'S method came to be wide known. A special Société médicalé d'observation under his permanent chairmanship was established in Paris. It published three volumes of memoirs (in 1837,1844 and 1856), the last two of which I managed to see.

4.2. Discussions. In 1835, the Paris Academy of Sciences discussed the possibility of applying probability theory to medicine[16]. The savants did not achieve unanimity [43, p. xi]. Thus, DOUBLE [35, p. 281] maintained:

La méthode éminemment propre aux progrès de [therapeutique appliquée] *c'est 1'analyse logique et non point l'analyse numérique.*

In 1837, the Paris Académie Royale de Médecine debated the application of the numerical method [44, pp 44 – 52]. This was only the beginning: the method remained the talk of the town for a few decades. I describe the opinion of its resolute opponents (§§ 4.2.1 – 4.2.3), discuss the view of its partisans (§§ 4.2.4 – 4.2.5) and consider the standpoint of those believing in stochastic considerations (§§ 4.2.6 – 4.2.7). I conclude with an estimate of the numerical method due to an author of this century (§ 4.2.8).

4.2.1. D'Amador. Generalizing his report at the Paris Académie Royale de Médecme, D'AMADOR published a book [30] on the application of statistics in medicine. He did not recognize the theory of probability at all. Referring to PASCAL (?), D'ALEMBERT and POISSON (?), D'AMADOR contended that the logical foundation of the theory was doubtful (p. 114) and that its application *aux faits réels du monde physique et moral* [was] *ou inutile ou illusoire* (p. 15). As far as medicine was concerned, probability provided solutions

Ou nuisibles, ou insufiisantes, ou trompeuses. son importation en médecine est anti-scientifique, abolissant la véritable observation … (p 31).

I adduce a few more passages just to describe the common belief of those times.

(1) *La probabilité des mathématiciens … n 'est guère que la théorie du hasard. Invoquer la probabilité c'est done invoquer le hasard; c'est renoncera à toute certitude médicale, à toute règle rationnelle tirée des faits propres de la science. … La médecine ne sera plus un art, mais une loterie* (p. 14).

(2) *Le vaisseau que je monte périra-t-il ou non ? Le calcul ne me dit rien sur ce point essential … À quoi me servira cette connaissance* [of the probability for the loss of the ship]? (p. 24).

(3) All these probabilities of success and failure

Varient dans chaque hôpital, à chaque série des expériences. …Que faire de toutes ces probabilités en conflit? (p. 29).

D'AMADOR (p. 12) also attacked LOUIS, wrongly supposing him to advocate the use of the theory of probability:

Il existe actuellement une école qui place les nombres au dessus de toute chose; qui proclame le calcul des probabilités la seule règle de certitude possible en médecine.

On the positive side, D'AMADOR recommended use of analogy and induction. All discoveries allegedly due to *les numéristes*, D'AMADOR (p. 62) attributed to other physicians and to the application of induction. Indeed (pp. 42 and 52), induction is based on analogy whereas the numerical method presupposes a non-existing identity of cases.

4.2.2. Comte. In 1838, he [129, § 3.3.3] put on record his attitude against the use of statistics in medicine, denouncing it as *une profonde dégénération directe de l'art médical*. He specifically maintained that the existence of biological variations made comparisons between competing treatments impossible.

4.2.3. Bernard. He formulated a few cautious theses. Scientific laws, he [4, p. 217] contended, were determinate (in those days no one thought otherwise) whereas statistics provided only probable results. At best, it only turns our attention to one or another fact, but it is unable to lead to a veritable law. Besides, statistics might *guider le prognostic du médecin* (p. 221), but could not help in a, particular case (p. 219).

From my point of view, BERNARD'S book just did not live up to its promising title. And one of BERNARD'S assertions (p. 220) was at least doubtful:

La statistique ne saurait enfanter que les sciences conjecturales; elle ne produira jamais les sciences actives et expérimentales.
However (p. 221), *La médecine est encore presque partout conjecturale*.

4.2.4. Simpson. In many of 'his works he (§ 6.1) essentially used the statistical method. He [133] also came out in support of the method and criticized its opponents. Mentioning GAVARRET (§ 4.3.1) SIMPSON (p. 315) noted that the extent of [*normal*] oscillations in statistical data was *easily ascertainable*. Nevertheless, adducing numerous examples of the use of statistics, he himself did not analyse them in the sense just described and his article (and the statistical side of his work in general) indeed followed the numerical method[17].

SIMPSON mentioned QUETELET (p. 316), DOUBLE (p. 330; § 4.2.1), and even LAPLACE (Ibidem)[18] and I think that his failure to follow the advice of GAVARRET stemmed from the disuse of stochastic criteria by QUETELET. Note that medical statistics possessed some means for verifying its conclusions. For example, SIMPSON (pp. 318, 319 and 325 – 326) proved the existence of certain regularities by discovering a monotonous change of the corresponding functions. In the last instance which belonged to mortality of lithotomy he detected regularity by arranging the initial data into suitable groups. See also my § 6.1.2.

4.2.5. Pirogov. He [10, p. 125; 103, p. 5] several times favourably referred to the numerical method. Mentioning syphilis, the stone disease and amputations, he [101, p. 125] also remarked, without adducing any references:

Surgeons used the statistical method even before [LOUIS] *for the determination of symptoms of diseases and indications for certain ways of treatment and operations*.

I describe the work of PIROGOV in § 6.2.

4.2.6. Bouillaud. Nous avons compté les jours de la maladie, le nombre des pulsations des artères et du coeur, le nombre des inspirations etc., … nous avons mesuré la témpérature … la densité du sang. nous avons ausculté et percuté.

This is BOUILLAUD [14, p. 385]. No wonder he [13, p. 160] was dissatisfied with physicians who *oublient trop ce principe: Numero, mensurâ et pondere deus fecit mundum*. BOUILLAUD [14, p. 390] was a supporter of the statistical method in its broad sense:

Par quelle étrange et déplorable inspiration, des hommes auxquels on ne saurait refuser, sous tant d'autres rapports, une haute portée intellectuelle, prenant en quelque sorte une académie de médecine pour une académie de jeux, ont ils comparé à un jeu de hazard. … Le calcul des probabilités appliqué a des faits médicaux dont une longue experience, et non le hazard, nous, a enseigné les lois?

Comment les mêmes hommes ont-ils mis en opposition la méthode numérique et la méthode de l'induction? *Comme si une alliance éternelle ne devait pas toujours régner entre ces deux méthodes* …

BOUILLAUD [13, p. 112] characterized his time by

L'art de faire en quelque sorte la statistique des observations recueillies en nombre plus ou moins considérable.

In this connection he (Ibidem, pp. 222 and 288) stressed the importance of the theory of probability[19]. Elsewhere he (p. 218) expressed the same idea in more detail:

La statistique médicale n'est malheureusement encore qu'à son berceau, et l'avenir lui réserve de grands développemens. Toutefois, elle a déjà été appliquée avec quelque succès à l'examen de diverses questions médicales d'un haut intérét et si un plus grand nombre de bons observateurs avaient le temps, la patience et le zèle nécessaire à ce mode de recherches, on verrait bientôt se dissiper comme de vains fantômes une faule d'assertions, dont quelques-unes exercent un si funeste empire sur la pratique elle-même et partant sur la vie des hommes.

BOUILLAUD (Ibidem) reasonably concluded

Que le calcul approximatif ou des probabilités est presque toujours le seul dont nous puissions faire usage, quand il s'agit de généraliser un résultat.

4.2.7. Gavarret. Before taking to medicine he had graduated from the Ecole Polytechnique where he studied under POISSON. He published a book [43], the first of its kind, devoted to the application of the theory of probability to medicine, and sincerely acknowledged POISSON'S influence (p. xiii):

Ce n'est qu'après avoir long-temps médité les leçons et les écrits de l'illustre géomètre, que nous sommes parvenu à saisir toute l'étendue de cette question … de régulariser l'application de la méthode expérimentale (!) *à l'art de guérir.*

GAVARRET (p. 189) contended that medical knowledge was [often] based on *assertions vagues et confuses* and pointed out (p. 50)

La funeste habitude, trop longtemps consacrée en médecine, de confier les observations à la mémoire, aura de son côté amené ce fàcheux résultat, que les faits extraordinaires paraissant se multiplier en raison de l'impression produite sur l'esprit, des auteurs auront pris la règle, ce qui n'était réellement que l'exception[20].

Indeed (p. 60), everyone used statistical experience in one or another way, so wouldn't it be better to compile the data thoroughly? Having taken this stand, GAVARRET did not disown the numerical method at all, but he definitely noted its shortcomings. Discussions of the method, GAVARRET (p. x) asserted, were held

Uniquement de savoir si on remplacerait, par des rapports numériques les mots souvent, rarement, … etc. La méthode numérique, considérée sous ce point de vue rétréci, ne pouvait s'étendre au delà d'une simple réforme dans le langage, mais il était impossible d'y voir une question de méthode scientifique et de philosophie générale[21].

This opinion seems too severe: PE'ITY, the cofounder of political arithmetic, also attempted to introduce numerical measure instead of vague words. I consider GAVARRET'S recommendations concerning the use of stochastic methods in § 4.3.1.

4.2.8. Greenwood. He [46, p. 139] contended that

Some heart-breaking therapeutic disappointments in the history of tuberculosis and cancer would have been avoided if the method of Louis had been not merely praised but generally used during the last fifty years[22].

He (p. 133) also lamented over possibilities which LOUIS himself allegedly did not explore:

One wonders what might have been the future of clinical statistics if Louis had secured the collaboration of … Poisson.

But the real point was that LOUIS evidently did not lend an attentive ear to POISSON's one-time student, GAVARRET.

4.3. Elements of Mathematical Statistics. I describe here the work which pertains to mathematical statistics. I discuss the grouping of observations in § 7.2. Then, in §§ 7.1 and 7.3.3 I mention the application of empirical formulas.

4.3.1. Gavarret. He [43] introduced two formulas necessary for the application of probability theory.

(l) Denote the unknown probability for the appearance of a random event in a binomial trial by p. If the event appeared m times in μ trials then (p. 256)

$$P(|p-\frac{\mathrm{m}}{\mu}|\le u\sqrt{\frac{2mn}{\mu^3}})=1-\frac{2}{\pi}\int\exp(-t^2)dt. \qquad (1)$$

Evidently, (1) corresponds to the DE MOIVRE – LAPLACE integral limit theorem, a theorem of probability theory proper. Referring to POISSON, GAVARRET supposed that, as a rule, the confidence coefficient in therapeutics should be equal to 0.9953[23].

(2) Let a random event happen m times in a series of μ trials, and m_1 times in a series of μ_1 trials. Then, if no *cause perturbatrice* (see below) was involved,

$$|\frac{m}{\mu}-\frac{m_1}{\mu_1}|\le 2\sqrt{\frac{2mn}{\mu^3}+\frac{2m_1n_1}{\mu_1^3}},\ n=\mu-m,\ n_1=\mu_1-m_1. \qquad (2)$$

This inequality is a simple corollary to a formula due to POISSON [128, p. 279, with an obvious mistake] for binomial trials with variable probabilities. However, POISSON clearly pointed out the restrictions superimposed on the nature of variations in probabilities whereas GAVARRET (pp. 65 and 265) vaguely mentioned *un ensemble permanent de causes possibles*[24].

GAVARRET adduced examples on the use of formula (2) and, in particular, on the comparison of competing methods of treatment. He (p. 194) also added an advice on the check of the null hypothesis:

Le premier travail d'un observateur qui constate une différence dans les résultats de deux longues séries d'observations, consiste donc à chercher si l'anomalie n'est qu'apparente, ou si elle est réelle et accuse l'intervention d'une cause perturbatrice; il devra ensuite … chercher à déterminer cette cause.

Thus, GAVARRET's main achievement was the introduction of the principle of the null hypothesis and of its check into medicine (and natural science in general)[25].

GAVARRET (p. x) did not consider himself the only partisan of the statistical method:

Quelques hommes distingués luttaient avec persévérance pour faire adopter l'emploi de la statistique en médecine. C'était, disaient-ils, le seul moyen de recueillir l'expérience des siècles en thérapeutique.

4.3.2. Guy. Referring to CELSUS (§ 1.1), he [48, p. 40] stressed the importance of statistics for activities connected with public health:

If we consider the health of large masses of men placed under different circumstances, and acted on by different influences, it is to the numerical method that we must look for accurate information as to the effect of these circumstances.

GUY did not mean the method due to LOUIS. Elsewhere he [50, p. 803] offered a definition of the numerical or statistical method and he preferred the use of the former adjective because statistics might be wrongly confused with Staatswissenschaft. He (Ibidem, p 801) also claimed that medicine had a special relation to statistics:

There is no science which has not sooner or later discovered the absolute necessity of resorting to figures as measures and standards of comparison; nor is there any sufficient reason why physiology and medicine should claim exemption. On the contrary, they belong in an especial manner to the class of sciences which may hope to derive the greatest benefit from the use of numbers.

And (p. 802):

Without statistics a science bears to true science the same sort of relation which tradition bears to history[26].

GUY attempted to prove the importance of extreme rather than mean values in medicine by remarking [48, p. 42] that the action of poison can manifest itself only during a certain interval of time; most essential, he explained, was the knowledge of the extreme points of this interval. See § 6.2.2 for a description of PIROGOV's use of minimal mortality in surgery[27].

4.3.3. Bienaymé. He was a mathematician and a statistician. He [6] formulated general recommendations on the application of the statistical method in medicine. It seems [52, pp. 103 – 104] that BIENAYMÉ founded his considerations on the use of the central limit theorem and one quantitative statistical test. His contribution, essentially ahead of its time, was too concise and remained scarcely noticed for more than a century.

4.3.4. Davidov. He, to whom I referred in § 4.1, was also a mathematician. He took interest in the theory of probability and was partly responsible for CHEBYSHEV'S attention to the theory [126, p. 182].

DAVIDOV [32] stressed the need to estimate the plausibility of statistical deductions and recommended the use of formulas of the GAVARRET'S type (§ 4.3.1). He (p. 66) believed that

Vague ideas on probability and an inexact distinction between subjective and objective probabilities are among the main obstacles against the speedy development of practical medicine.

DAVIDOV evidently underestimated the importance of trustworthy statistics: exactly its absence most impeded the application of the statistical method. Russian physicians came to recognize the need to use stochastic methods as a result of DAVIDOV'S efforts [86].

4.3.5. Parkes. He is the author of a manual on hygiene [87] prepared, according to its full title, especially for the use in the army. He explained the use of GAVARRET'S formula (4.3.1.2) and noted (p. 481) that

The group-building ... can only be done by the most subtle and logical minds [a remark which might have been made in regard to statistics in general]. *The dividing character must be* [absolutely] *definite*. … *The dividing character must be precise enough*.

Probably he was the first to consider grouping in medical statistics though certainly not the first to use it (§ 7.2.1).

PARKES (p. 484) also described *the method by* (?) *successive means*. Denote *n* observations by $x_1, x_2, \ldots, x_n$. Then the successive means will be

$$\frac{x_1 + x_2}{2}, \ \frac{x_1 + x_2 + x_3}{3}, \ldots$$

PARKES recommended to calculate the successive means in both the direct and inverse order. *The degree of uncertainty* of observations, he continued, will then be the *mean variation between the … means*. PARKES did not claim this method for himself. The attempt to determine the uncertainty of observations was important insofar as the study of the stability of statistical series had properly started with LEXIS (in 1879). Successive means are a particular case of moving averages. I doubt whether anyone followed PARKES's recommendations and I do not understand the second part of his explanation.

4.3.6. Further Work. In 1874, two authors, HIRSCHBERG and PESKOV, published writings devoted to the application of statistics in medicine. The former [53] described the elements of probability theory and explained the use of GAVARRET'S formulas. The latter [89, p. 89] contended that

Medical statistics should have at its disposal mean values as accurate as those used in meteorology[28] *to enable the construction of lines of equal sickness, mortality etc. and thus to discover the laws of sickness*.

The geographical distribution of diseases was studied in those days within the framework of public hygiene (§ 5) but PESKOV, as it seems, was the first to mention lines of equal sickness (mortality).

The work of SEIDEL is also relevant to my subject. It is very important and I describe it in §§ 7.4.2 and 7.4.3.

5. Public Hygiene

It originated in the middle of the 19th century[29]. From its very beginning, the new scientific discipline stood in need of a statistical backbone (cf. GUY'S opinion in § 4.3.2). Thus, LEVY [68] considered such global problems as the influence of external agents (atmosphere, water, climate) on man, and the choice of suitable type of clothes and appropriate food, i.e. the very problems that belong to hygiene to this day and demand the use of statistics.

BOUDIN [ll] collected vast statistical data on seasonal periodicities in crime and suicide[30], on fertility of man and animals, geographical distribution of diseases, zoogeography and on many other topics. He described periodical phenomena using graphs constructed in polar coordinates with ordinal numbers of weeks as the arguments of the corresponding points (vol. 1, pp. 32 – 33).

In 1851 the first international sanitary conference discussed measures to prevent the spread of transmissible diseases and several subsequent meetings took place before 1875, i. e. before the advent of the bacteriological era Though 1851 is generally regarded as the beginning of international [movement for] public health [116, p. 290], no real success was then achieved (Ibidem, pp. 290 – 293).

From 1855 onward, international statistical congresses debated statistical problems of public health[31]. Meeting in Paris, the Congress [26, pp. 121 – 132 and 336 – 340] discussed studies of settlements, mostly from the point of view of preventing cholera epidemics from spreading; see also § 7.3.4.

In 1857, the Congress [26, pp. 39 – 81 and 359 – 389] examined the statistique des établissements et des associations

Destinés à venir en aide aux malades et aux infirmes, ainsi que des résultats de l'organisation sanitaire.

The next meeting of the Congress (in 1860) adopted proposals for uniform hospital statistics and sanitary statistics in general [26, pp. 173 – 183, 247 and 264]; see my § 6.1.2. The Congress also recommended an international registration of epidemics. In 1863, the Congress considered problems of vitality and mortality of the civil population and military personnel [26, vol. 2, pp. 227 – 272, 494 – 499 and 549 – 560].

Lastly, in 1872, the Congress adopted a special terminology. It [26, t. 2, p. 161] resolved that the aim of *Statistique anthropologique* was the description of the *état physique de la population* and that its branches were *Statistique somatologique* (which had to do with *la vigueur physique et l'état général de santé de la population*), *Statistique nosologique* (influence of disease on population), *Statistique hygiènique*, and *Statistique du service médical*[32].

Late in the 19th century ERISMANN [36, vol. 1, p 7] remarked on the role of statistics for public hygiene:

Achievements of no small importance … have been made lately in the deepening and dissemination among the medical profession of the understanding that statistics must needs be the foundation of all our sanitary activities, … and the cornerstone of specific studies in public hygiene.

The second volume of ERISMANN's work contained a lengthy appendix devoted to sanitary statistics. Here he (p. 3) noted that, using statistical data, physicians often came to wrong conclusions, so that until recently famous practitioners did not recognize medical statistics. He (p. 7) reasonably concluded that statistical data ought to be trustworthy.

He discussed the methodology of data compilation, referred to a number of mathematicians and statisticians and described the opinion of DAVIDOV regarding quantitative tests (§ 4.3.4)[33].

5.1. Statistics of Settlements. The study of settlements and especially large cities had become a most important object of public hygiene of the time. The Industrial Revolution in England, which commenced in the middle of the 18th century, caused the urban population to grow rapidly. By the middle of the next (19th century, its poorer sections had found themselves in horrible (DICKENSIAN) living conditions. The thoughts of the founders of political arithmetic, GRAUNT and PETI'Y, as well as of statisticians of the 18th century (SÜSSMILCH) on the social value of man had gone out of fashion.

CHADWICK clearly depicted the sanitary state of the British nation. He crammed his book [20] with descriptions of specific facts accompanied by relevant figures and adduced statistical tables. Thus (pp. 228 – 231), he published a table of *Comparative chances of life in different classes* [of society]. According to this table, among children of Liverpool gentry and professional persons only two out of three lived to the age of five years, and only three out of five lived until twenty.

CHADWICK devoted a section of his book to the estimation of damages due to the disregard of sanitary measures. For example, referring to the Rev. G. LEWIS, he (p. 272) noted that the [typhoid?] fever bill in Dundee for 1833 – 1839 amounted to £ 175,000.

Later PETTENKOFER published a thorough and much better known sanitary study of Munich [93]. He estimated the financial loss ensuing from such diseases as typhoid fever thus proving the case for public hygiene. The city council adopted PETTENKOFER's recommendations and mortality from typhoid fever fell from 0.15 % in 1871 – 1875 to 0.08 % in the next five years [94, p. 12]. The number of fever cases likely diminished just as well.

I end by quoting FARR's telling conclusion [38, p. 148] on the general role of sanitary conditions (ca. 1857).

Any deaths in a people exceeding 17 in 1000 annually are unnatural deaths. If the people were shot, drowned, burnt, poisoned by strychnine, their deaths would not be more unnatural than the deaths wrought clandestinely by disease in excess of seventeen deaths in 1000 living.

He corrected himself remarking that 17 deaths in 1000 were also far too many because sanitary conditions were unsatisfactory even in localities in which mortality did not exceed 1.7%[34].

6. Surgery

One of the first branches of medicine which began to use statistics was surgery. Thus, CIVIALE [22, p. xix] even contended that

C'est par l'application de la loi des grands nombres qu'ont été résolues les principales questions relatives aux luxations, aux fractures, aux amputations (he mentioned eight more items)[35]. It seems that C1VIALE exaggerated the role of the law of large numbers; besides, the *principales questions* are of an everlasting nature and each new generation of physicians must consider them anew. CIVIALE regrettably adduced no references.

Later SIMPSON [133, p. 331] noted that, as far as surgery was concerned, a perfect and undoubted diagnosis was comparatively easier to arrive at than in *some* [other] *departments of the physician's study*. Therefore, SIMPSON argued, it was easier to apply the statistical method in surgery.

For his part, PIROGOV [103, p. 6] maintained that that method was absolutely concordant with the spirit of surgery. See also § 4.2.5.

6.1. Amputations. PHILLIPS [98] was probably the first to conduct a statistical study of'amputations. Collecting results of 640 amputations in several European countries, he determined the relevant mortality (23%) and concluded that physicians unjustly disregarded the danger of the operation. Both SIMPSON [133, p. 320] and PIROGOV (§ 6.2.2) confirmed the former existence of this general misbelief.

PHILLIPS'S work was not comprehensive at all. He discussed amputations in general and did not study mortality from complications.

6.1.1. Anaesthesia. It earned its place of honour only because of statistics. SIMPSON, for one, from the very beginning understood the need to follow the statistical path [134, p. 93]:

Some eagerly and stoutly doubted, in toto, the possibility of making operations painless; and many who admitted its possibility, denied altogether its propriety on the alleged ground of its increasing, the general subsequent dangers of the patient. … I became convinced that there was only one method of arriving at the truth, viz. by instituting a statistical investigation, upon as large a scale as possible.

SIMPSON continued (Ibidem, footnote):

In my letter of application [asking for data], *I stated, that the effects, whether favourable or unfavourable, of anaesthesia upon the ultimate recoveries of patients from surgical operations is still a matter of much doubt and uncertainty. We have as yet had no proper collection of data to ascertain whether the mortality of operations has been increased or not by patients being placed under the influence of ether. … In order to determine as far as possible this important point, I have been induced to undertake the statistical investigation. …*

Amputations have been selected for this purpose … because they are … nearly and everywhere alike, and because the general average mortality accompanying most of the greater amputations [without anaesthesia] *is already known* [from published inquiries].

SIMPSON [134, p. 102] also answered criticisms levelled against him (by whom?):

The data I have adduced … have been objected to on the ground that they are collected from too many different hospitals, and too many/different sources. But, on the contrary, I believe all our highest statistical authorities will hold that this very circumstance renders them more, instead of less, trustworthy.

Elsewhere SIMPSON [133, p. 327] contended that general averages excluded the influence of runs of successes or failures. He undoubtedly used heterogeneous data. Thus, some of his figures corresponded to the period 1794 – 1839 whereas the aim of his study demanded that he compare the use of ether only with the least dangerous (most recent) amputations without anaesthesia.

SIMPSON'S initial data were not comprehensive also insofar as he was unable to determine mortality from hospital gangrene and similar complications which often obscured the action of all the other factors taken together. Then, he obviously had no possibility of separating other causes of death after amputation as, e.g., pneumonia.

Even a few decades later HODGES complained of the lack of relevant statistical data; LISTER [72, p. 156] quoted him as saying

As regards deaths from ether, I make no doubt many occur which are never reported [as such], *for the simple reason that the death … occurs some hours later* [after amputation] *from bronchitis*[36].

PIROGOV [102] studied amputations (and operations in general) under anaesthesia at about the same time as SIMPSON. He compiled data and used them in the same way as SIMPSON. Note however that PIROGOV was the first to use anaesthesia in the field.

PIROGOV [102, pp. 7 – 8; 103, p. 191] contended that in some instances anaesthesia increased the rate of mortality. But even then (§ 6.l) he noted that the data were not trustworthy and that amputations without anaesthesia were partly performed in small hospitals.

6.1.2. Hospitalism. This term was likely coined by SIMPSON, or at least it is the title of a contribution [137] in which he studied the influence of hospital conditions on amputation cases. According to his data which covered 2,098 patients (p. 303), mortality from amputations performed at home (usually under unfavourable conditions) amounted to 10.8%[37]. In hospitals morta1ity was immensely higher (p. 338) and increased with the number of beds[38].

As SIMPSON (p. 341) supposed, the real reason for the latter fact was the related worsening of ventilation and decrease of air space per patient. He [137, p 399] noted that *a small hospital if overcrowcled, becomes as insalubrious as a large hospital under one roof*[39].

S1MPSON discussed the causes which led to the performance of amputations at home. Thus (p. 332), many physicians were afraid to refer serious cases to hospitals though the patients themselves often knew nothing about the danger which 1ay in wait for them in medical institutions. In any case, SIMPSON made a special point (p. 372) of the fact that home amputations had concerned a relatively higher per cent of traumatic (more serious) cases.

In 1860 the International Statistical Congress adopted FLORENCE NIGHTINGALE'S *Proposals for uniform plan of hospital statistics* [26, p. 247; 83, p. 159]. She [83, p 171] separated statistics of surgical operations and recommended (p. 173) to introduce a common nomenclature of complications. Poor conditions in a surgical hospital, she (pp 5 and 10) contended, led to complications:

(l) *Perhaps the most delicate test of sanitary condition in hospitals is afforded by the progress and termination of surgical cases after operation, together with the complications which they present. … The origin and spread of fever in a hospital, or the appearance and spread of hospital gangrene, erysipelas, and pyaemia generally are much better tests of the defective sanitary state of a hospital than its mortality returns*[40].

(2) *There is no such thing as inevitable infection.*

FLORENCE NIGHTINGALE had good reason indeed to begin her book with a very special statement:

It may be a strange principle to enunciate as the very first requirements in a Hospital that it should do the sick no harm. … The actual mortality in hospitals, especially in those of large crowded cities, is very much higher than any calculation founded on the mortality of the same class of diseases out of hospital would lead us to expect.

For a description of NIGHTINGALE's statistical work see KOPF [64].

6.1.3. Antiseptics. The introduction of (LISTERIAN) methods of antiseptics had transformed surgery to such an extent that no statistical study of the advantages of the new practice seemed necessary. In 1878 a Munich physician, I. N. NUSSBAUM, published an essay on LISTERISM. He [70, vol. l, p. xx] wrote:

Formerly. almost all patients in whom bones were injured, were attacked by pyaemia. For example, of 17 cases of amputation 11 died from this cause. … 80 per cent of all wounds and ulcers were attacked [by hospital gangrene]. … *Almost every wound was attacked with erysipelas. Now, No pyaemia. No hospital gangrene. No erysipelas*[41].

According to LISTER himse1f [71, p. 129],

Before the antiseptic period, [there were] *16 deaths in 35 cases* [amputations]. … *During the antiseptic period, 6 deaths in 40 cases. … These numbers are, no doubt, too small for a satisfactory statistical comparison; but, when the details are considered, they are highly valuable.*

LISTER adduced non-statistical considerations regarding his data. He could have remarked (as the editors of his *Collected papers* did [70, vol. 1, p. xx]) that before the introduction of antiseptics *surgical intervention was … limited more or less entirely to operations necessary for the saving of life.*

Even in 1870 LISTER [71, p. 124] noted that there was

A striking evidence that the emanations from foul discharges … constitute the great source of mischief' in a surgical hospital.

Referring to statistical data (which he did not publish) he (Ibidem, footnote) added:

The ground-floor wards were, on the average, most liable to pyaemia, whoever might be the surgeon in charge; … those on the floor immediately above come next in this respect[42].

6.2. Pirogov. I mentioned PIROGOV in §§ 6, 6.1 and 6.1.2 in connection with statistics of amputation, comparison of conditions in small and large hospitals and the use of the statistical method in surgery.

6.2.1. Use of Statistics. PIROGOV [107, p. 5] called himself *ein eifriger und aufrichtiger Verehrer der medicinischen Statistik.* Though individuality was important even in surgery, PIROGOV [106, p. 7] believed that it could be studied statistically [103, p. 5][43].

PIROGOV [107, p. 685] singled out an important reason for the unreliability of statistical data:

Die Statistik nur dann sicher ist, wenn sie keinen anticipirenden Zweck hat und die persönlichen Interessen dabei nicht im Spiele sind.

In this connection he [105, p. 31] accused even die *berühmtesten Hospitalärzte* and noted [106, pp. 9 and 68] that unsuccessful cases were not always recorded. But the main point was that under military conditions surgical statistics was just *nicht zuverlässig* [110, p. 297]. He obviously took into account inevitable displacements and evacuation of large numbers of wounded (and sick) personnel. PIROGOV [108, p. 438] also noticed a particular source of mistakes in statistical data or its interpretation. Comparing results of early and late amputations, he argued, one must understand that the former are not always performed *early* while the latter are really above their reputation: surgeons postponed amputations in the hope of preserving the limb and in some cases they obviously succeeded. In other instances report cards were easy to tamper with [110, pp. 427 and 456].

Still, PIROGOV (Ibidem, p 529) expected much of statistics:

Wenn wir gewillt sind alte, sich immer wiederholende Fragen der Kriegschirurgie mit Hülfe der Statistik zu entscheiden, so ist … ein besonderes Institut von Specialisten erforderlich, welche verpflichtet sind auf den Verbandplätzen und, in den Hospitälern … persönlich zugegen zu sein.

His programme was of course unrealistic. But in a modest way, PIROGOV [104, p. 382] elsewhere suggested that surgeons themselves should compile statistical data, observing the fate of the wounded right from the operating room to the bitter [or happy] end.

As the years went by, PIROGOV became more sceptical of statistics. In the second (Russian) edition of his book [108, p. 20] he writes:

Even a slightest oversight, inaccuracy or arbitrariness makes [the data] *far less reliable than figures founded only on a general impression with which one is left after a mere but sensible observation of cases*.

In the first (German) edition he [107 p. 6] had written:

In Ermangelung einer sicheren [Statistik] *will ich also lieber gar keine, sondern eben nur die Resultate eines solchen Eindruckes in dieser Schrift mittheilen*. [In 1849] I [108, p. 20] … *did not yet imagine myself all the blind alleys into which figures sometimes lead*[44].

Only a gifted physician (as PIROGOV) could have allowed himself this mode of action if one is to understand its description literally. The rule for the ordinary man is, to trust only plausib1e data, to rely only on statistics of sensible observations. By (following) *blind alleys* PIRQGOV probably meant an unwarranted confidence in the calculated rates of mortality from various injuries and operations.

6.2.2. Conservative Treatment and the Death-Rate. One of the main problems which confronted PIROGOV and demanded the application of statistics was the estimation of the conservative treatment of fractures and bullet wounds of the limbs versus their amputation. In 1847, compiling statistics of amputations, PIROGOV [106, p. 66] for the first time questioned their inevitability. Later he [107, p. 690] expressed his views quite definitely:

Bei dieser Unbestimmtheit des Quantums von beiderseitigem Risico schwankt man in der comparativen chirurgischen Statistik fortwährend zwischen zwei Extremen: bald setzt man zu wenig Risico *auf Rechnung der Amputation, bald auf die der conservativen Kur zu viel*[45]. *Die alte, nicht statistische Schule überschätzte übrigens den Werth des Lebens. … Die mit der Amputation selbst verbundene Lebensgefahr hielt Sie für zu geringfügig, um sie in die Wagschale zu legen. … Wir leben ofifenbar in einer Übergangsperiode. Die geheiligten Grundsätze der alten Schule, deren Ansichten im ersten Decennium dieses Jahrhunderts vorherrschten, sind durch die Statistik erschüttert – dass muss man ihr lassen, mit neuen Grundsätzen hat sie aber die alten nicht ersetzt, was auch unmöglich ist, so lange die kriegschirurgischen Statistiker nicht nach einem bestimmten und für alle Nationen festgestellten Plane handeln*.

The single plan advocated by P1ROGOV was in line with QUETELET's lifelong efforts to standardize population statistics. PIROGOV also formulated a natural test for the advisability of the conservative method. He [10, p. 525] proposed to compare the minimal death-rate peculiar to this method with that of amputations.

Thus, PIROGOV stood in need of minimal (or, maybe, mean) rates of mortality from various injuries of the limbs for the two competing methods of medical treatment. He [l06, p. 2] supposed that, for a given moment,

Jede Krankheit und jede chirurgische Operation in Bezug auf Nichtgelingen und tödlichen Ausgang ihr festes und bestimmtes Verhältniss hat[46]. *Dies Verhältnis hängt ab von der continuirlichen Einwirkung der äußeren Bedingungen auf die verschiedenen Krankheitsformen, von der Natur der Krankheit, von der Individualität der Kranken, so wie von der Art des traumatischen Eingriffs der mit jeder Operation verbunden ist. Der Einfluss des Arztes aber, die verschiedenen Curmethoden und die mechanische Fertigkeit spielen eine so secundäre Rolle, dass sie nur ein in der großen Masse kaum bemerkbares Schwanken der Zahlenverhältnisse hervorrufen*.

PIROGOV [104, p. 382; 107, p 5] repeatedly professed his confidence in the stability of death rates. Did he think of mean, or minimal, rates? Before 1879 he did not elaborate, but later m life he mentioned the minimal rate [1l0, p. 297]; see also above.

According to PIROGQV [103 p. 5], even

Individual peculiarities are subject to statistical regularities … so that the influence of the patient himself on the course and treatment of his illness can be determined only by statistical considerations.

(See also § 6.2.1.) Therefore, only changes in external conditions (especially in times of war) can throw the figures of mortality into utter confusion. However, under perfect external conditions death-rates may be considered as random magnitudes, i.e. quantities with stable probabilities of their various values. One may well assume then that with probability 0.95 (say) the value of a certain death-rate is not less than some number c ($0 < c < 1$).

This is my explanation of PIROGOV'S choice of the minimal rates of mortality. However, he did not resolutely change from mean to minimal mortality: even in his last contributions there are quite a few places [109, p. 80;'110, pp. 440, 476 and 524] where he discussed unspecified mortality. Moreover, he noted that the (mean or minimal?) death-rates were stable only in some instances.

To ensure meaningful statistics, PIROGOV strove to secure stability of death-rates and he began to distinguish amputations of each third of each limb [108, p. 439; 110, p. 442]. SIMPSON [133, p. 331] did the same even in 1847[47].

7. Epidemiology

Continuing the HIPPOCRATIC and ARISTOTELIAN tradition, SYDENHAM [125, § 6.2] and physicians of his time connected epidemics with the *constitution* of the locality; see § 8. GREENWOOD [45, p. 9] did not agree that the constitution impressed *a common character upon the illness of an epoch*. His opinion, which of course contradicted the ancient tradition, was scarcely true; see § 7.3.1.

Modern epidemiology uses determinate or stochastic models to foretell the progress of epidemics [3, chap. 9]. The aims of scholars in the l8th and 19th centuries were different (§§ 7.2 – 7.4), but a study conducted by FARR belonged to the modern type (§ 7.1). For this reason I ventured to describe it although his study did not belong to medicine.

Statistical studies of smallpox had commenced in the middle of the 18th century (§ 7.2) and in about a hundred years from then studies of cholera (§ 7.3) and typhoid fever (§ 7.4) had ensued.

A figurative picture of a later period, of the second half of the 19th century, belongs to GREENWOOD [45, p. 19]:

We devoted ourselves as a profession to the task of tracking down bacilli, to segregating the infected and presumably infective members of our herd and to immunising the remainder; the herd ceased to be a herd, it was an aggregation of individuals, what was found to be true, or believed to be true, of individuals was assumed to be true of the herd.

Does this mean that the advent of bacteriology had checked the development of the statistical branch of epidemiology?

7.1. The First Modern Study. In 1866 FARR published a letter in a newspaper in connection with the cattle plague which then invaded England. BROWNLEE [15] reprinted that letter and provided related information. Denote the number of attacks of the plague during four weeks by Δ. According to FARR, the third differences of log nat Δ were constant, so that

$$\Delta = \exp(a + bt + ct^2 + dt^3) = C\exp[dt(t + m)^2 + n],\ C > 0$$

(d is negative because $\Delta > 0$ as $t \to \infty$). Of course, FARR had no possibility of adducing any formulas in his letter.

FARR’s calculated values of Δ did not agree with actual figures[48] but at least he correctly predicted a rapid decline of the epidemic, an event in whose occurrence no one then had dared to believe. FARR produced certain arguments in favour of the decline: individuals who did not fall victims of an attack, he argued, were less prone to the disease and will hardly be ever affected, while its violence diminished *as the epidemic poisons … lose part of the force of infection in every body through which they pass*[49].

The effects of slaughtering in the later weeks of the epidemic obviously worsened FARR'S prognosis. BROWNLEE (p. 252) quoted G. H. EVANS who maintained that FARR himself was (or, rather, became) aware of this circumstance.

In his main letter FARR noted that he made (similar?) studies of the visitations of cholera and diphtheria which took place in 1849 and 1857 – 1859, respectively.

7.2. Smallpox

7.2.1. Inoculation. Inoculation of smallpox in England dates back to the 1720's [60] and, in France, to the middle of the 18th century [62, p. 283]. The practice of inoculation involved various statistical problems. JURIN [60, pp. 3 and 30] remarked on the need to estimate the efficiency of inoculation and the ensuing danger both to those inoculated and the population at large. He (p. 17) noted that the number of inoculated persons in England had reached 474 of whom nine (1.9%) died[50]. However, he argued, at least in several cases death had resulted from causes other than inoculation. Comparing the danger of this procedure with that of natural smallpox (mortality 16.1%)[51] JURIN concluded that inoculation was beneficial.

As to the danger of inoculation to the general population, JURIN [60, p. 30] dismissed it in comparison with the risk of contracting smallpox in the natural way, there being about 20,000 cases of the disease per year. He also noted (p. 5) that even natural smallpox can attack the same person twice, but he was reasonable enough to disregard such highly improbable events.

JURIN's table of the results of inoculation (p. 17) is an early example of group-building in medical statistics. His table showed the general number of those inoculated with figures entered for each of the observed outcomes of the procedure (Had the smallpox ...; Had an imperfect smallpox ...; No effect; Suspected to have died of inoculation). JURIN subdivided all his figures into age-groups.

The danger of inoculation did after all cause certain measures to be adopted. During 1728 – 1740 inoculation was not practised in England at all [27, vol. 2, p. 489]. In 1763 the French parliament provisionally banned that practice *dans l'enceinte des villes & des faux bourgs* [23, p 249]. In England, in 1807 the exclusion of inoculated persons from communication with others had become the law of the land [10, p. 344; 27, vol. 2, p. 609].

DANIEL BERNOULLI [124, pp. 114 – 116] formulated simple statistical hypotheses on the progress of, and mortality from smallpox epidemics and worked out. and solved a differential equation determining the relative number of persons not attacked by the disease. Supposing that inoculation protected against smallpox but was fatal in a small number of cases, BERNOULLI noticed that inoculation lengthened the mean duration of life by about two years. He therefore resolutely came out in favour of that practice. He did not consider the danger of inoculation to the general population[52].

Nevertheless, his work (1766) is justly considered a classic.

D'ALEMBERT criticized his study. His thoughts were reasonable and, moreover, important for the development of mathematical statistics, see TODHUNTER [144, Chap. 13] who, as usual, accurately adduced all the references and I myself have given a short record [124, p. 130] of the subject.

A rough estimate of the overall influence of inoculation on the mortality of the population is due to BLANE [10]. Remarking that the greatly diminished [during 1706 – 1818] general death-rate was *not quite a just scale*, he [10, p. 337] nevertheless asserted that inoculation seems to have added to the mortality.

GUY [51, p. 205] considered other periods, viz., 1710 – 1719, 1740 – 1749 and 1790 – 1799, and arrived at an exactly opposite conclusion. Mortality figures for those times are available only for London, not for England, and this circumstance seems to prevent the possibility of a trustworthy analysis. Besides, the extracts from the London bills of mortality as used by BLANE possibly contained errors. In any case, CREIGHTON [27, vol. 2, pp 531, 535 and. 568] adduced extracts for the same years and in a few instances those extracts differed from BLANE'S.

But the main difficulty for any analysis stems from the large errors in the bills themselves; according to OGLE [85, p. 451]

It is necessary to correct the bills in the eighteenth century by an addition of from 39 to 44 per cent of the recorded burials [and] *it is necessary in the nineteenth century, or at any rate from 1832 onwards, to make a much larger correction*.

One may well assume that the data on mortality from smallpox were not accurate either.

Even this is not the end of the story, at least with respect to the last period studied by BLANE (1804 – 1818). CREIGHTON [27, vol. 2, p. 586] testified that, during the 1820's, *The original mode of inoculation … was far from being supplanted by its rival* [by vaccination].

KARN devoted a portion of her contribution [62] to the history of smallpox and inoculation. She (p. 290) noted that in actual fact the main problems posed by the practice of inoculation had been left unanswered[53].

7.2.2. Vaccination. A new era and, as it seems, the final one, in the battle against smallpox commenced with the introduction of (the JENNERIAN) vaccination. Having examined a large number of cases, JENNER [55] proved that cowpox ensured immunity against smallpox for a good few dozens of years and conducted a special study of vaccination [57, p. 146]:

Upwards of six thousand persons have now been inoculated with the virus of Cow Pox and the far greater part of them have since been inoculated with that of Small Pox, and exposed to its infection in every rational way that could be devised, without effect.

JENNER [56, p. 91] asserted that cowpox

Is to be attributed to matter conveyed to the animal … from the horse. One of the arguments which he adduced in favour of his opinion was

The total absence of the disease in Ireland and Scotland, where the men servants are not employed in the dairies.

In one instance JENNER [55, p. 49] appealed to an imaginary randomization of subjects.

SIMON [13, vol. 1, p. 230] pointed out a particular statistical problem connected with vaccination:

Did the extensive use of degenerated lymph, he asked, lead to *too frequent* impermanence of protection against post-vaccinal smallpox? *It is chiefly from* [the yet imperfect] *national statistics that the answer must be sought*.

I do not know how this and other practical problems were eventually solved, but it seems that always nothing short of total success was demanded of any version of the new procedure, a principle which doubtless made comparisons of competing versions easier.

7.3. Cholera. In 1831 European physicians had begun to take (timid) measures against cholera [132, p. 169]. In 1832 (p. 832)

It was well understood that foul linen, bedding and clothes were a most certain means of carrying the poison. This was precisely the old experience of plague. The theory that the poison of cholera was conveyed in the drinking water, of which illustrations were collected in 1849 and 1854, was not applied.

The *Reserches* [40, 1823, table 8] contained a qualitative description of the drinking water and (in table 9) information on its physical and chemic composition. The authors even ranked the waters of numerous rivulets according to their healthfulness. At the same time they said nothing about the contamination (or possible contamination) of water by organic substances.

But how did the spread of cholera epidemics depend on local geographical etc. features? Physicians hotly debated this problem for some thirty years. About 1818 [90, p. vi] J. JAMESON [37, p. 166] contended that

There is abundant proof that in high, dry, and generally salubrious spots [cholera] *was both less frequent in its appearance and less general and fatal in its attacks than in those that were low and manifestly unwholesome.*

The wording of his conclusion was obviously lame, but see § 7.3.3.

7.3.1. Pettenkofer. He [90, p. vi] expressed his high opinion of JAMESON and other British physicians who worked in India during 1817 – 1819:

Alle bis jetzt cursirenden Ansichten über die Art der Entstehung und Verbreitung der epidemischen Cholera bereits in den Beobachtungen und Bemerkungen der englischen Aerzte enthalten sind.

His book contained extracts from the German edition of JAMESON'S *Report on epidemic cholera*.

PETTENKOFER was the most distinctive scholar among those who studied cholera before KOCH. In his earlier years (1854) he [96, Bd. 5, p. 379] was

Noch ein sehrgläubiger, wenn auch kein unbedingter Contagionist. In later life he [91, p. 329] repeatedly stressed that no cholera epidemic was possible at a certain moment *wenn der Ort keine lokale Disposition besitzt*. In this connection PETTENKOFER attached special importance to the level of subsoil water and the rate of its change[54]. SEIDEL [118, p. 176], who noted this opinion, adduced no references and I did not find the relevant place.

In 1886 PETTENKOFER [96, Bd. 5, p. 354] mentioned KOCH but did not budge (p. 381):

Vom Einfluss bestimmter Fluss- und Drainage- und Regengebiete habe ich nun immer mehr Belege gefunden, hingegen von der Infection Gesunder durch die Dünndarmausleerungen Cholerakranker gar keine[55].

In 1886 – 1887 PETTENKOFER [96] published a monstrous survey of writings devoted to cholera and adduced a large number of graphs and statistical tables. He was unable to analyse his data, but he repeated. his early pronouncements [96, Bd. 6, p. 78]: *bei den Choleraepidemien Ort und Zeit eine entscheidende Rolle spielen*.

In 1892, being 74 years old, PE'ITENKOFER conducted an experiment on himself by drinking some water infected with comma bacilli [97]. Escaping with a stomach disorder, he considered this fact as an additional argument in favour of his theory.

WINSLOW [151, p 335] offered a modern estimate of PETTENKOEER's views.

7.3.2. Snow. Searching for an understanding of the sudden extensions peculiar, to cholera SNOW [139, pp. 58 – 59] compared mortality from cholera in 1832 and the nature of water supply in various districts of London at that period. But his main study (Ibidem, pp. 74 – 86) concerned the epidemic of 1849 and a particular metropolitan district with a population of about 500,000. Two companies supplied the water which either contained the sewage of London (former case) or was quite free from such impurity (latter instance). Mortality per 10,000 houses amounted to 315 deaths in the former case, 37 deaths in the latter instance (p. 86).

SNOW (p. 47) also delimited a small outbreak of cholera in 1854 in a certain district of London as being due to the water from one of the local wells[56]. FARR [38, p. 143] cautiously remarked that

Subsequent [after 1854] *investigation by a committee … placed it beyond a doubt that the mortality of cholera in London was augmented by the impure water*.

PETTENKOFER did not agree with SNOW. He [90, p. 40] maintained that

(1) *Alle Bemühungen, in der allgemeinen Luft-Beschaffenheit und im Trinkwasser eine Ursache zu finden, haben bis heute nur negative Resultate gegeben.*

(2) *Das Wasser als einen allgemeinen Verbreitungsweg zu betrachten, widerspricht den Thatsachen, welche an andern Orten und in andern Epidemien gesammelt worden sind. In München wurde der Verlauf der Epidemie ebenso genau, wie in London auf den Einfluss verschiedenen Trinkwassers, aber mit völlig negativem Resultate untersucht* [91, p. 353].

(3) SNOW'S theory agreed with facts only in some instances [96, Bd. 5, p. 383]; facts concerning cholera in Munich during 1873 – 1874 contradicted his theory, but

Die contagionistische und Trinkwassertheorie zerschellen jämmerlich an diesem Felsen (Ibidem, Bd. 6, p. 79).

PETTENKOFER also produced a special argument against SNOW. In 1866, as he [95, p. 31][57], contended, [H.] LETHEBY

Questioned … the correctness of the facts and pointed out that … the water mains [of the two London companies] *have been gradually entangled*.

PETTENKOFER adduced no reference and did not repeat LETHEBY's (?) statement in his later works. Neither did PARKIN mention his compatriot. Maintaining that cholera invariably presented a more malignant form at the commencement of an epidemic and a milder form towards its termination, PARKIN [88, p. 215] concluded:

This infective water theory … is unsound, illogical and false.

And he simply brushed aside SNOW's statistical data explaining them away as *merely … a coincidence*. His is a strange point of view, and his attitude seems all the more surprising in the light of his own previous pronouncement made in 1833 [145]:

The cause of cholera is a noxious matter or poison which is generated in the earth, … this finds its way into springs … the water from such springs should be fltered through charcoal.

SIMON [130, pp. 416 – 417] supported SNOW confirming his theory by new statistical data. In his later years SIMON [132, p. 263] noted that *For immediate practical use*, [SNOW's] *broad verdict in itself was abundantly enough*.

ROSEN [116 p. 183] arrived at the same conclusion:

Contagionist and non-contagionist viewpoints alternated in the public favour, and during the early decades of the nineteenth century the latter position had achieved dominance. [Living organisms] *played practically no part in the sanitary movement of the mid-nineteenth century*[58].

Considering epidemiology in general, PESKOV [89, p. 10] contended, perhaps too resolutely:

The development of medical statistics had begun exactly after mankind had to convince itself too clearly and too bitterly of the utter helplessness of medicine against such of its evils as cholera, [typhoid?] *fever etc*.

7.3.3. Farr. In 1849 he [38, pp. 343 – 345] offered a formula connecting mortality from cholera in London with the height of the districts above the Thames. Being satisfied with reasonably small discrepancies between calculated and actual values of mortality, he did not use any formal rule for the estimation of the precision of his fit.

FARR also noted that mortality depended on the number of the river terrace on which the relevant district was situated. Just as in the previous case, he did not estimate the overall accuracy of his fit. The data on later epidemics either in London itself or elsewhere did not. corroborate FARR's conclusions [131, vol. 1, pp. 105 – 106; 96, Bd. 5, p. 395].

In 1862 FARR [38, p. 386] estimated the increase of deaths of cholera with age, again resting content with a comparison of actual and calculated values In 1854 he [38, p. 356] conducted a more general study which belongs to the prehistory of rank correlation, and qualitatively concluded:

After arranging the districts [in London] *in the order of mortality ..., in the order of the density of population, … it was found that the variations of density had some connexion with the mortality, that wealth und poverty exercised more influence, that unclean water was pernicious* (!), *and that there was a certain relation between the diminution of the mortality of cholera and the elevation of the ground on which the people lived*.

7.3.4. International Statistical Congress. In 1855, [J.- D.] THOLOZAN delivered a report on the statistics of epidemics at the Congress. He [26, p. 337] advocated the need to establish

Quelques principes généraux relatifs à la marche des maladies epidémiques et aux moyens d'en diminuer les ravages.

One may well wonder, THOLOZAN continued, that the statistics of epidemics

N'ait point été, au Congrès de Bruxelles [in 1853], *l'objet d'une discussion, d'une élaboration, d'une conclusion particulière*.

He also recommended a programme for the study of settlements (cities) which had suffered from epidemics. It included registration of *phénomènes météorologiques qui ont précédé et accompagné l' invasion*.

In 1872 the Congress debated statistical problems of cholera and syphilis. The report of the first section ran as follows [26, vol. 1, p. 45, separate paging for proceedings of each section]:

L'expérience corfirme chaque jour davantage la théorie scientifique, d'apres laquelle le choléra provient de l'importation et du développement dans une localité d'un agent spécifique special appelé miasme cholérique. …

La statistique montre … que le développement du choléra est intimement lié à la présence de certaines conditions locales et passagères.

The authors of the report obviously had in mind the theory due to PETTENKOFER. A resolution [26, vol. 2, pp. 126 – 127] adopted by a subsection of the same section stressed its utmost importance and asked the general assembly to check the theory statistically[59].

One of the participants, ERICHSEN, (p. 154) noted that

Il n'y a aucun doute que dans beaucoup de cas le rapport entre le choléra et l'eau souterraine sera constaté. …

At the same time he (p. 153) contended:

Les travaux de vingt ans ont donné des résultats qui détruisent plutôt qu'ils ne confirment la théorie de Pettenkofer.

7.4. Typhoid fever

7.4.1. Buhl. PETTENKOFER'S theory which concerned cholera stimulated studies of typhoid fever. BUHL [16] collected data for 1857 – 1864 on the quantity of precipitation, level of subsoil water and deaths from fever in Munich. He concluded that at least the second factor influenced mortality. But he (p. 7) doubted the action of precipitation since *die jährliche Gesammtmenge … durchaus keine Congruenz mit der jährlichen Typhusmortalität beurkundet*.

SEIDEL used BUHL'S data to conduct a more thorough research (§§ 7.4.2 and 7.4.3). He examined the number of monthly cases of the sickness which he supposed to equal (I might add, proportional) to the (known) mortality for the next month.

7.4.2. Seidel (1865). In his first contribution SEIDEL [117] compared the number of fever cases with the level of subsoil water measured downward from the surface of the earth. Denote the mean monthly value of the latter and the number of fever cases per month by x_i and y_i respectively (i = l, 2, …,108). In a preliminary version of his calculations SEIDEL introduced differences

$$u_i = x_i - \bar{x},\ v_i = y_i - \bar{y} \qquad (1)$$

where $\bar{x}$ and $\bar{y}$ were the mean values of x and y respectively. Then SEIDEL excluded the influence of year cycles. Suppose, for sake of simplicity, that the observations he used commenced in the beginning of a certain year (1857). Then x_1, x_{13}, x_{25}, … will denote the January levels of water in the subsoil for 1857, 1858, 1859, … The mean of these levels, i. e., the influence of year cycles for January, should be subtracted from x_1. And similar corrections should be applied to the number of fever cases. In general, SEIDEL changed from (1) to

$$X_i = x_i - \hat{x}_i,\ Y_i = y_i - \hat{y}_i. \qquad (2)$$

Here, in my own notation, $\hat{x}_i$ and $\hat{y}_i$ are the influences of year cycles. Thus, for example,

$$\hat{y}_1 = (y_1 + y_{13} + y_{25} + ...)/9,\ \hat{x}_{14} = (x_2 + x_{14} + x_{26} + ...)/9$$

where 9 is the number of years in the period studied.

Counting coincidences and non-coincidences of the signs of differences (l) SEIDEL found out that there were l.67 times more of the former than of the latter[60]. After the elimination of the influence of the year cycles the corresponding ratio was 2.13.

Using the well-known normal approximation to the binomial distribution, he then maintained that his results were not accidental and that (p. 230) a high level of subsoil water involved a low rate of fever and vice versa. Note that SEIDEL (Ibidem) explained the increase of the ratio studied in the second version of his calculations by successful elimination of the yearly cycles of mortality *die mit dem Gange des Grundwassers nichts zu thun haben.*

Here (and also below) SEIDEL studied correlation[61], but introduced no quantitative measures. I think that this fact once again proves the profoundness and originality of the reasoning of GALTON, the founder of the correlation theory. Indeed, over the centuries many a scholar noticed correlative relations between various quantities while during the second half of the 19th century even geophysicists, i.e. people really knowledgeable in mathematics, studied such dependences statistically. And still, no one before GALTON had thought of placing correlation under the rule of mathematics.

As to SEIDEL, he was able to estimate quantitatively the significance of the correlative relation between two (and even three; § 7.4.3) variables. However, he did so in a roundabout way connected with a loss of information.

SEIDEL's article also contained a straightforward example of rank correlation. Arranging the studied years in the order of, first, decreasing mortality, and, second, decreasing mean yearly values of *x*, SEIDEL (p. 235) contended that the correspondence between the two sequences

1857, 1858, 1856, 1863, 1864, 1862, 1859, 1860, 1861
1857, 1858, 1863, 1864, 1862, 1856, 1859, 1861, 1860

was *frappant*. Readers can calculate the SPEARMAN'S coefficient, rho, and thus to check his conclusion.

7.4.3. Seidel (1866). In his companion paper SE1DEL [118] first made a similar study in regard to precipitation. He carried out separate calculations for each three years and, moreover, took into account statistical probabilities for the appearance of each sign in each of the two sequences. Again, applying the normal approximation to the binomial distribution (and the multiplication theorem), SEIDEL concluded that there was a real connection between precipitation and the number of fever cases. He also studied the relation between all three variables (*x*, the level of subsoil water, y, the precipitation, and *z*, the number of fever cases). Denote the corresponding differences (§ 7.4.2) by X_i, Y_i and Z_i.

The signs of numbers X_i, and Y_i, coincided 56 times, and, in 46 cases, the sign of Z_i coincided with the common sign of X_i and Y_i. Once more SEIDEL proved this result to be significant and also noticed the correlation between factors *X* and Y.

According to SEIDEL, a small (large) amount of rain and/or snow and a low (high) level of subsoil water was correlated with an increase (decrease) in fever cases. Referring to PETTENKOFER, SEIDEL (§ 7.3.1) asserted that mortality from cholera depended also on the rate of the fall or rise of subsoil water. Studying fever, he did not allow for this factor at all, thus sparing himself considerable additional difficulties.

SEIDEL's contributions [117; 118] are not listed either in his obituary[62] or in general bibliographic sources.

7.4.4. Further Work. JESSEN [58] corroborated SEIDEL'S conclusion. Dividing the same period into intervals during which the level of subsoil water was higher (lower) than usual, he used GAVARRET's formula (4 3.1.2) to prove that the difference between mortality in the two intervals was significant. He tacitly (and quite unnecessarily) assumed that the intervals must be of the same duration.

SOYKA [140] presented graphs showing the dependence of mortality from typhoid fever in various German cities on the level of subsoil water but he did not study his data analytically. SOYKA's graphs seem too convincing to be true, and WINSLOW [151, p. 330], for one, explained the depicted regularities by accidental coincidence**63**.

VIRCHOW [148] examined mortality in Berlin. He (p. 330) maintained that

Der Gang der Sterblichkeit [he really meant infantile mortality from typhoid fever, see his pp. 330—338] *in hohem Maasse entspricht dem Gange des Grundwassers und den Wasserständen der Spree, so jedoch, dass das Steigen des Wassers mit der Periode des Sinkens der Todesfälle und das Sinken des Wassers mit der Periode des häufigeren Sterbens züsammenfällt*.

VIRCHOW (p. 338) repeated his assertion in the shortest possible way: *Trockene Jahre sind Typhusjahre*. However, he did not prove his conclusion.

8. Appendix: Meteorology and Medicine

SYDENHAM [142, p. 42] supposed that the violence of an epidemic disease and the peculiarities of its spread depended on the general state of the weather. Describing the connection of mortality with the weather BLANE [9, pp. 131 and 135] continued this ancient tradition.

BOYLE [18, p. 303] noticed the connection between weather and sickness:

In 1666, John Locke … with the encouragement of Boyle, … began a weather register. [He (p. 304)] *avidly collected figures of the vital occurrences of European cities*.

During 1699 – 1703 LOCKE [34, pp. 300 – 301] attempted to correlate weather *with information gleaned from a wide survey in social medicine*.

Efforts to determine the influence of weather on man had properly begun in the 19th century**64**. GUERRY [47] collected meteorological data and information on the number of patients admitted into hospitals, on the number of marriages, births deaths and suicides.

SMITH [138] Compared meteorological data (air temperature and moisture, direction of wind) with the number of cases admitted at the London Fever Hospital, their sex, occupation, etc. Like GUERRY, he was unable to do anything with the information compiled.

It was found impossible to include, SMITH complained in the Preface, *some researches of a statistical nature which it was at first intended to incorporate in the work***65**.

\ QUETELET [113] collected data on deaths in various age groups, air temperature, moisture and pressure, etc. Considering a large number of factors, he was unable to use his information to any reasonable degree. He (p. 30) did note, however, that mortality depended on the variations of the daily temperatures.

In an earlier contribution QUETELET [112] studied the distribution of births and deaths by months.

GUY [49] showed. the progress of mortality, sickness and of several meteorological variables during the year 1842 by arranging, separately, the quarters of the year in the decreasing order of each quantity. He (p. 135) concluded:

It appears that there is no relation … between the mortality and any single condition of the air, but that the sickness follows the exact order of the temperature and dew-point, varying directly as each of them.

Taken together, the writings of various authors which I have described up to this point did not amount to the origin of a new scientific discipline. The conception of medical climatology had occurred in the middle of the 19th century, or even later, perhaps in the 1880's. In § 5 I mentioned the works of BOUDIN and LEVY which were published in 1857 and 1862, respectively, and which had a direct relevance to the rise of medical climatology. And during 1877 – 1880 LOMBARD [73] discussed applications of meteorology to medicine, the geographical distribution of diseases, and the influence of climate on the life and health of man. Needless to say, all these subjects were directly connected with statistics. LOMBARD dedicated his work

A la mémoire vénérée de mes maitres Andral [a French physician] *et Louis* (!) et de mes amis Sir James Clark[66] et Quetelet.

Acknowledgement. Professor W. KRUSKAL turned my attention to DEBUS'S contribution [33], see § 4.1.

Notes

1. In the latter instance FRACASTORI maintained: *Derartige Fieber befallen vornehmlich Kinder, seltener Erwachsene, am spärlichsten Greise*. He also adduced qualitative statistical considerations in respect to the foot-and-mouth disease in cattle (book l, p. 33).

2. An example which proves that *for the community as a whole, there is nothing so extravagantly expensive as ignorance.* The author of this passage [121, p. v] was concerned with meteorology, but the lack of statistical data in any branch of science or public life and/or the reluctance to study them were just as expensive, or even disastrous.

3. Those responsible felt the lack of statistical data on epidemics of plague and *sweating sickness* even in the 16th century [18, p. 285]. Comments on GRAUNT'S table of mortality (contained in his *Natural and political observations made upon the bills of mortality*) continue to appear to this very day [63, pp. 519 – 520]. PETTY's contribution to medical statistics is not readily seen, but it is thought [127, p. 221] that he at least helped GRAUNT to compile his table.

4. JACOB BERNOULLI'S considerations which precede the proof of his law of large numbers [5, chap. 4 of pt. 4] are highly relevant. The probability of a certain throw of a die was known beforehand, he noted, but no one was able to determine the [stable!] probability of one or another disease, of rain, etc. However, BERNOULLI continued, probabilities of such events might be calculated inductively. In the sequel, he repeated his example concerning diseases. Thus, to say the least, BERNOULLI would not have objected to the application of his law of large numbers to statistical studies of various diseases.

5. Elements of the latter date back to the second half of the 17th century and GRAUNT is sometimes called the father of modern epidemiology [45, p. 10]. Note that GRAUNT's achievements in medicine impressed even his contemporaries [61] and that F. BACON, who published an essay on mortality which *proceeds from decay and the atrophy of old age* [2, p. 217] did not utter a single word about the need for relevant statistical studies.

6. Even in 1857 BOUDIN [11, t. l, p. 8] testified that *La théorie des* [correlative or fatalistic] *influences lunaires sur les maladies compte encore un bon nombre de partisans*.

I adduce one more passage just to show that an insufficiently precise expression (a not infrequent occurrence) makes it impossible to understand its meaning [8, p. 132]:

In tropical climates, the moon is observed to have considerable influence on febrile paroxism, and crisis.

Considerable' in each case (this is a fatal drive), or in a considerable number of cases (a tendency)? The moon does after all influence man and DARWIN [31, p. 248] explained this fact without any recourse to astrology:

In the lunar or weekly recurrent periods of some of our functions we apparently still retain traces of our primordial birthplace, a shore washed by the tides.

7. The Supplement was not included in the *Essai* [115] and I translated these passages from the Russian translation (1961) of RAMAZZINI. Note RAMAZZINI'S lenient approach toward smoking and chewing tobacco. *On ne doit blâmer,* he maintained (chap. 16, p. 192), *que l'usage immodéré* [of tobacco], *ou à contre-temps*.

8. CELSUS whom I referred to in § 1.1 argued [19, Book 2, § 6, p. 115] that *the art of medicine is conjectural* adding though that signs are deceptive i*n scarcely one out of a thousand* (!) *cases*.

9. LEIBNIZ [127, p. 224] advocated the compilation of *Staatstafeln* and contended (p. 225) that the *zusammensetzung der bereits vorhandenen wissenschafften, Erfindungen, Experimenten und guther gedancken* will bring under control *vielen Kranckheiten.*

Compilations of this kind may be related to the corresponding activities in the field of *Staatswissenschaft*. Some scholars, as for example LEIBNIZ, were partisans of both this Wissenschaft and political arithmetic, the predecessor of statistics (Ibidem, § 2.4.4 and p. 255).

10. GRAETZER [44, pp. 21 – 23 and 28] described and reprinted a portion of the Berlin bills of mortality published by J. D. GOHL (1665 – 1731) in 1721. He even called GOHL the founder of medical statistics. GRAETZER also put on record information about another scholar, J. C. KUNDMANN (1684 – 1751), who worked in the same field and published his main writing in 1737.

11. Note that CONDORCET (p. 536) recommended an international *plan général d'observations* in meteorology including observations made at sea and in balloons. I mention him in § 5 (note 29) in connection with public hygiene.

12. Suppression of unsuccessful cases, PINEL pointed out elsewhere [100, p. 169, footnote], brought about *un aveugle empyrisme.*

13. PINEL (p. 406) wrongly attributed the *Ars conjectandi* [5] to DANIEL BERNOULLI.

14. Note a related passage [75, p. 82]: la science, *j'entends la vraie science, n'étant que le résumé des faits particuliers*. But LOUIS hardly meant his own statement in the literal sense.

15. In 1648 VAN HELMONT [33, p. 27] rhetorically advocated the same procedure.

16. See § 1.2 for the relevant opinion of POISSON.

17. Many physicians probably practised the method just out of common sense. Thus, PANUM [42] studied an epidemic of measles by compiling scrupulously all the relevant data.

18. For some reason he did not refer to LOUIS.

19. He referred to LAPLACE more than once and he (p. 288) repeated the latter's assertion [66, p. viii] that probability *est relative en partie à notre ignorance, en partie à nos connaissances sur les causes dont il s'agit*.

20. KEPLER offered a similar remark in connection with a sudden fulfilment of a [fraudulent] forecast [125, p. 114].

21. He also maintained (p. xiv) that the members of the Paris Académie Royale de Médecine *n'avaient qu'une idée fort imparfaite de l'emploi du calcul en médecine*.

22. METCHNIKOV [79, chap. 12] pointed out the great importance of the statistical method in the study of cancer and other diseases.

23. In his example which pertained to jurisprudence POISSON chose 0.9853 [128, p. 288].

24. Cf. POISSON'S definition of randomness [128, p. 248]:

L'ensemble des causes qui concourent à la production d'un événement sans influer sur la grandeur de sa chance est ce qu'on doit entendre par le hasard.

GUY [50, p. 806] repeated GAVARRET'S remark in a slightly different French (!) wording: *l'invariabilité de l'ensemble des causes possibles*, etc.

25. ARBUTHNOT'S memoir with its idea of the same motion appeared in 1712. It

indirectly led to a most important development of the theory of probability, but more than. a century had passed before POISSON derived formulas specifically intended for the check of the null hypothesis. Yet even he did not apply them to natural science.
26. GUY mentioned the faulty treatment of syphilitic cases with mercury which took place in the absence of relevant statistical data. His example does not directly contradict a remark due to PIROGOV (§ 4.2.5).
27. Being a physician, GUY (1810 – 1885) was engaged in forensic medicine and in 1868 he published a book on this subject. During 1852 – 1856 GUY edited the Journal of the (Royal) Statistical Society. In 1869 –1872 and 1873 – 1875 he held the offices of the vice-president and president of the Society, respectively. GUY was also a fellow of the Royal Society and, during 1876 – 1877, served as its vice-president (*Dict. Nat. Biogr*., vol. 23, 1890, pp. 392 – 393). See also FITZ PATRICK [39, pp. 190 – 192]. I mention GUY in §§ 4.3.1, 7.2.1 and 8.
28. QUETELET [114, p. 322] compared the accuracy ascertained in astronomical and physical observations with that secured by statistical inquiries. Geomagnetic forces were known up to the third, or even fourth, decimal place, the relative error of weighing small bodies (weight about a kilogram) was of the order of 10^{-6} etc. but statisticians were *bien loin encore* from conducting censuses whose errors did not exceed one or two units (people) in 10,000. Records of births, deaths and marriages, QUETELET continued, *atteint à peu près le degré d'exactitude désirable*, a statement which I do not quite believe.

He could have well remarked that the general situation in medical statistics was much worse than in the field of censuses. Quetelet (Ibidem, pp. 266 – 267) also held that *toutes les sciences d'observation, à leur début, ont subi les mêmes phases; c'étaient des arts* ... and that statistics still had to acquire a scientific skeleton.
29. LEIBNIZ formulated recommendations on public hygiene (§ 1.2). CONDORCET [24, pp. 544 and 552] described the aims of *mathématique sociale* (as he preferred to call political arithmetic) and mentioned the study of the influence of temperature, climate, properties of soil, food and general habits on the ratio of men and women, birth-rate, mortality and number of marriages. Condorcet (Ibidem, p. 549) also paid attention to the relation between political arithmetic and the mathematical treatment of observations. He reasonably supposed that *La théorie des valeurs moyennes doit être considérée comme un préliminaire de la mathématique sociale*.
30. Of course, QUETELET also worked in this field [111, p. 51ff]. By that time both demography and medical statistics had recognized the problem of suicide. Indeed, in 1820 LAMARCK [65, p. 226] argued that *l'individu qui se suicide est alors malade* but in 1825 CASPER [17, pp. 3 – 95] studied that problem from a statistical viewpoint. He published data on suicides in Prussia (pp 13 and 48), estimated the number of suicide cases among the drowned (p. 20), pointed out errors in statistical returns on suicides (p. 26) and compared the number of suicides in Berlin with the weather (p. 34). CASPER (p. xi) did not fail to stress the importance of his subject:

Statt verklingender Moralpredigten … habe ich sprechende Thatsachen angeführt, die ich der Aufmerksamkeit der hohen Regierungen dringend empfehle.
31. These congresses took place from 1853 to 1876. Being too official, they did not conform to the spirit of the time and were ultimately discontinued. In 1885, the International Statistical Institute was established in their stead. SPERK [26, vol. 2, p. 158], a participant of the Petersburg Congress, noted that, over the years, the congresses had treated medical statistics *d'une façon très-superficielle*. During the discussions, as he explained, medicine was not duly separated from *questions entièrement étrangères*. For my part, I suspect that the real reason for the superficiality was the just mentioned bureaucratic nature of the congresses.

According to official language [26], the congresses preferred to consider themselves as a permanent body. Referring to them (see for example my next lines) I shall therefore use the definite article.
32. Nowadays sanitary (medical) statistics is divided into two branches, viz., statistics of health and statistics of the public health service (*Great Sov. Enc*., third edition, vol. 24/1, 1976, pp. 1314 – 1315, art. *Sanitary Statistics*). [This edition was translated into English.]

33. ERISMANN was a student of PETTENKOFER [36, vol. 1, p. 7] whose work on prevention of cholera epidemics I describe in § 7.3.1. PETTENKOFER originated the experimental method in hygiene, a fact which I leave aside.
34. A more or less comprehensive study of public hygiene should include the description of works on disease and mortality of the population, and of institutionalized populations (military, prison, and hospital populations). Except for the so-called hospitalism which I discuss in connection with surgery (§ 6.1.2), I do not treat these subjects.
35. Note, however, the beginning of this passage: *Pour ne citer ici que des faits de chirurgie.* CIVIALE (p. 549) compiled data on the prevalence of the stone disease in various European countries and in Buenos Aires, minutely studied cases of the disease in Paris subdividing them in a number of groups (pp. 613 – 630) etc.
36. SIMPSON (pp. 75 – 85) discussed objections against anaesthesia based on religious grounds and returned to the same subject in regard to childbirth (chap. 7). Any innovation, he noted, mentioning vaccination (pp. 75ff), was given a hostile reception. WHITE [150, vol. 2, pp. 55 – 63] offered an instructive relevant study.
37. SIMPSON (p. 345) also remarked that the mortality of women in childbirth was 0.5 and 3 – 4% respectively. Drawing on independent sources, FLORENCE NIGHTINGALE [84, p. 11] corroborated the former conclusion, but adduced a somewhat different figure for hospitals, viz., 1.1 – 2.1%.

Simpson adduced separate data on amputation of thigh, leg, and forearm and noted [134, p. 105] that the results of the amputation of thigh were more remarkable.

Of special interest is SIMPSON's study of the dependence of mortality from amputations on the number of these operations (not more than 5 – not less than 12) performed by the practitioner. The ratio of the mortalities after those mentioned amputations amounted to 0.55 – 0.59.
38. PIROGOV [102, p. 191] arrived at the same conclusion even before SIMPSON, but he did not prove it:

Je me suis convaincu par expérience, combien les résultats sont différents entre les operations faites dans les petits établissements cliniques, et les opérations exécutées dans les grands hôpitaux; et même, combien la différence est grande dans les résultats obtenues par les opérations dans les diférents hôpitaux de la même ville, exécutées,dans des conditions exactement semblables en apparence.

An important feature, he continued (p. 192) was

La constitution générale d'un hôpital. Cette constitution générale, comme étant la consequence de l'organisation d'un hôpital, de son installation, de sa situation, et enfin aussi souvent de certaines maladies que l'on traite particulièrement dans tel ou tel hôpital.

In 1863 SIMON [131, vol. 2, p. 137] maintained that the *death-rate of the large general hospitals in large towns is twice as great as the death-rates of small hospitals in small towns.* A fellow of the Royal Society, Sir JOHN SIMON (1816 – 1904) devoted life-long efforts to sanitary reforms. His achievements in the sanitary science outweighed his outstanding career in the field of surgery (*Enc. Brit.*, vol. 20, 1965, pp. 695 – 696). 1 mention him in §§ 7.2.2 and 7.3.2.
39. Cf. V1RCHOW's remark [147, p. 21):

Nicht die Grösse und Ausdehnung eines Spitales ist das Gefährliche, sondern die Luftverderbnis. Was die Patres von Regensburg bereits von 600 Jahren sagten, ist noch heute vollkommen wahr.

I mention Virchow once more in § 7.4.4 in connection with his study of typhoid fever. His *Ges. Abh.* [146] contain seven articles on statistics of sickness and mortality published during 1849 – 1875, and a note on military hygiene (1870).
40. Even at the beginning of the century BLANE [9, p. 140] asserted: *A large mortality may even be considered as a presumption of an hospital being well conducted.* [If the hospital admitted only serious cases.]
41. However, METCHNIKOV [79, p. 43] put on record the existence of criticisms of Listerian methods pronounced by SIMPSON (of all men! see note 36) and SPENS. The latter *s'appuyait sur les données statistiques qu'il avait rassemblées pour tâcher d'anéantir une fois pour toutes la méthode antiseptique.* METCHNIKOV gave no references.
42. The lack of antiseptics led to the spread of puerperal fever (not a surgical disease) in maternity wards. SEMMELWEIS [119] attacked this problem

statistically. In the first department of the hospital under his investigation, the mean maternal mortality for 1841 – 1846 amounted to 9.9 % with variations from 6.8 to 15.8%; in the second department it was 3.4% (from 2.3 to 7.5%) (p. 13); cf. note 37. Mortality of the newly born was also significantly higher in the first department (p. 27). And, on top of all this, some ill women in childbirth from the first department, but not the second, were being transferred to another hospital so that the real difference between the departments in regard to deaths was even more striking.

On many pages SEMMELWEIS described his search for the cause of high mortality in the first department. At last (p. 38) be discovered the reason. Medical students who worked there, without taking any precautions, reported regularly at their lying-in wards straight from dissecting rooms. And there were no students in the second department at all. Note that SEMMELWEIS's data just did not demand any special methods of treatment and his common-sense conclusion was unquestionable.

43. Cf. CIVIALE's opinion [22, p. xviii]: *Les individualités ne sont ni plus variables, ni plus capricieuses que le sort qui amène telle carte ou tel coup de dés*.

44. The texts of the two sources [107; 108] do not coincide; the passage in English lacking in the first German edition [107] is my translation from the second (Russian) edition [108].

45. I see no practical difference between these cases.

46. Cf. my note 4 in § 1.2

47. l note PIROGOV's famous pronouncement [110 p. 295]: *Der Krieg ist eine traumatische Epidemie*. This is yet another proof of his belief in the existence of statistical regularities, this time in the number of killed and wounded.

48. It follows that FARR could have rounded off his figures. He likely used data which related only to four periods, but he did have figures for every week at his disposal; see another of his letters published at about the same time [15; p. 250].

49. A good example of argumentation offered during the pre-bacteriological era.

50. In 1721 or thereabouts NETTLETON [81] inoculated 62 persons of whom only one (the last one) died, though possibly not from the contacted smallpox. NETTLETON concluded, somewhat vaguely, that the danger of death from inoculation did not exceed 1/62. This estimate is almost arbitrary.

51. I do not understand why he did not allow for the fact that a percentage of the population escaped smallpox altogether, a fact which he himself noticed in an earlier contribution [59].

52. BERNOULLI also studied (although in a purely mathematical way) the ratio of male and. female births [122]. His junior contemporary, J. H. LAMBERT, attempted to determine infant mortality from smallpox (1765, *Anmerkungen über die Sterblichkeit* etc., § 125) and the distribution of families by the number of their children and in general made an important contribution to mathematical demography [123, pp. 247—249].

53. JURIN, who possibly was the first to formulate these problems, gave an incomplete answer to one of them (see above).

54. PETTENKOFER [91, p. 344] also pointed out the relation between stagnant subsoil (?) water and malaria.

55. At the same time he [90, p. 294] recommended disinfecting dubious spots *wo Fremde möglicherweise ihre Excremente ... deponiren könnten*.

56. Almost at the same time as SNOW, BUDD contended that the cause of

Malignant Cholera [was] *a living organism and that these organisms were disseminated through society*, (l) *in the air* …, (2) *in contact with articles of food, and* (3) *and principally, in the drinking-water of infected places* [15a, pp. 5 – 6, see also p. 19, footnote].

57. I refer to the Russian edition (Petersburg, 1885), the only one which I managed to see.

58. Witness the experience of sanitary improvements in a small city [120, p. 14]:

The total deaths from Cholera m Exeter, in the two epidemics of 1832 and 1849 amounted to 445; of these 402 … took place with the concomitants of bad drainage and a deficient [in quantity] *water-supply; while with, in great measure, an absence of these conditions, the complementary number of 43 … only occurred. Can any more convincing statement be offered of the beneficial influence of sanitary improvements*?

59. Detailed statistical investigations of cholera outbreaks in small localities had been made long before this meeting of the Congress [76; 21] but they were evidently not up to the purpose; for one thing, they did not even contain information on subsoil water.
60. He allowed for an occurrence of a zero by splitting the case in half. Thus, he counted (0, 1) as a *ha1f-coincidence* and, at the same time, as a *half-non-coincidence*.
61. In this and the next subsection I use modern terms *correlative* and *correlation*.
62. *Astron. Nachr*., Bd. 141, No. 3379, 1896, pp. 319 – 320. A pupil of BESSEL, SEIDEL (1821 – 1896) was an astronomer, a mathematician and an optician. He devised a well-known version of the method of successive approximations for the solution of systems of linear algebraic equations, a fact not mentioned in the *Astron. Nachr*.
63. As to PETTENKOFER, referring to his own experience, he stated [92, p. 31] that epidemics. of typhoid fever occurred also during periods of high levels of subsoil water. Taking into account the opinion of other authors (see §§ 7. 4.2 – 7.4.3 and below), it may be desirable to conduct a special investigation of the subject. Of course, other factors, such as the air temperature arid moisture, insofar as they influence the life of bacilli of typhoid fever, should also be allowed for.
64. I have quoted PETTENKOFER (§ 7.3.1) who contended that cholera epidemics occurred in a given district only if it was *predisposed* accordingly (and, I would specify his idea, if certain meteorological conditions prevailed). SEIDEL and other authors conducted studies on the dependence of typhoid fever on either meteorological, or both meteorological and geological factors (§ 7.4).
65. MURCHISON [80] published further statistical data pertaining to the same hospital.
It has been my humble intention, he declared (p. v), … *to follow the example of Louis* (§ 4.1).
66. A physician (1788 – 1870) who accumulated observations on the effect of climate on chronic diseases (*Dict. Nat. Biogr*., vol. 10, 1887, pp. 401 – 402).

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Abbreviation. J. (*Roy.*) *Stat. Soc* = JRSS
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