Studies

in the History of Statistics and Probability

Vol. 7. Collected Translations

**Nik. Bernoulli, F. W. Bessel,
C. F. Gauss, H. C. Schumacher**
and about them,
O. Sheynin

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Introduction by Compiler

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General comments on some items

   [i] Kohli provided a useful and first ever commentary on N. Bernoulli's dissertation, but still a few of our Notes obliquely or otherwise criticize him. Thus, Kohli had not questioned N. B.'s statement on the rare birth of twins (Note 24) and indirectly called N. B. a cofounder of the theory of probability (Note 26).

   There exists a translation of N. B.'s dissertation by Richard J. Pulskamp and A. Berra available in the Internet. The Latin original was slavishly preserved there without any regard for English grammar (or common sense). Some phrases are impossible to understand and many are unbelievably long, up to 25 or even 40 lines. Kohli (who certainly did not know about that, possibly not yet existing translation) had quoted (in German) many excerpts from N. B., and, as a rule, we followed his own translation. Hald (1990) used an unpublished translation made in 1976 by T. Drucker who informed me that he is not anymore satisfied with his work but has no time for improving it.

   It is opportune to add that, although Latin has been generally praised and even called indispensable (e. g., for classifying plants and animals), Gauss complained that *The delicate* (spröde) *Latin language resists expression of thoughts in a simple and natural way* (his letter to Olbers of 14 April 1819).

   N. B. devoted his dissertation to his uncle, Johann Bernoulli, rather than to the memory of his other uncle, Jakob Bernoulli, whom he plagiarized. His letter to Montmort (1708/1713, pp. 388 – 394) of 23 Jan. 1713 ended by an ambiguous phrase: When the *Ars Conjectendi* appears in print, we will know … As though he did not quote it times and times again!

   [ii] Had Gauss already applied the method of least squares in the *Theoria motus*? I (2009, § 9.1.4) discussed this point and concluded that he did (which is not, however, accepted by all commentators). What remains completely unknown is whether Gauss changed the exposition of that method in the period between 1806 and 1807.

   Sheynin O. (2009), *Theory of Probability. Historical Essay.* Berlin. **S, G,** 10.
   [iv] In my Notes, I have made many critical remarks and can only conclude that Bessel had hardly read the *Ars Conjectandi* and that he was apparently satisfied to deliver a kitchen-sink talk.

   [v] Bessel read his report not earlier than in 1839 (Note 21). Bearing in mind both this report and Chapter 1 of his book [No. 322/135], I stress that he was also a metrologist. Regrettably, historians of science did not study his pertinent work. Mikhailov (1939, p. 200), however, called his work of 1825 – 1826 *classical*. One circumstance is unclear: how Bessel could have measured very small differences of the temperatures of the two bars (§ 16)?

   [vi] The booklet creates a distressing impression by poorly explained descriptions and astonishing mistakes (see my Notes). This is all the more regrettable since Anger was Bessel's student (Bruhns 1875, p. 562) and worked with him in 1827 – 1831 (Repsold 1920, p.

188). Anger himself called himself Doctor and professor in Danzig (title page of his booklet), so what did he teach?

However, he provided many details lacking in Repsold's detailed account, and, moreover, my translation is a warning for those numerous authors who refer almost to any published source without bothering to study it. Cf. Gauss (letter to Schumacher, 6 July 1840):

I reluctantly express myself in detail about the achievements attained by others […] *if not being entirely convinced in that I may really mention them approvingly*.

But he also recognized that *prior literary studies* […] were *not exactly to* his *taste*.

Some notation

Notation W-i means Gauss, *Werke*, Bd. i.

Notation [No. a] means that Bessel's paper *a* was included under that number in the list of his contributions compiled by R. Engelmann in Bessel's *Abhandlungen*, Bd. 3. Leipzig, 1876, pp. 490 – 504. Notation [No. a/b] additionally indicates that contribution *a* was reprinted in those *Abhandlungen* under number *b*.

Notation **S, G,** i means that an English translation of the appropriate paper is available on my cite www.sheynin.de which is being copied by Google, Oscar Sheynin, Home, in Document i.

I

K. Kohli

Commentary on Niklaus Bernoulli's Dissertation
De usu artis conjectandi in jure (1709)

Without the dissertation of his nephew Niklaus Bernoulli (N. B.), the publication of the works of Jakob Bernoulli (J. B.) would have been incomplete[1]. Its spiritual father was certainly J. B. Whole sections (Abschnitten) from his Diary (*Meditationes*) and the *Ars Conjectandi* (AC) are copied in the Dissertation word for word[2]. And N. B. had also picked up some problems and hints and remade problems posed by his uncle although it was often necessary to formulate them mathematically.

The Dissertation contains splendid investigations and often testifies to N. B.'s clear concept of the application of the probability theory[3] to ordinary life. But still, it is not as ripe as could have been expected from him bearing in mind his later letters of the 1720's to De Moivre and Montmort[4].

In his *Introduction* N. B. states:

I will discuss some theme out of mathematics produced by that divine knowledge the study of which I have joined so far with the study of law with GOD favouring. From the first years I have continued with conspicuous love with my most celebrated Uncles Jakob and Johann Bernoulli displaying a light for me in this knowledge. Jakob is now indeed enrolled in the heavenly chorus of the blessed, but to his own he bequeathed the treatise on the Art of Conjecture (unedited thus far but shortly, as we hope, to be brought into light). He has prompted me to choose this subject, i. e., the application of the Art of Conjecture to the law which I undertake with pleasure. I see that many of the most useful investigations, particularly about absent men to be considered dead, likewise life annuities &c occurring nearly daily in the court of justice, can be decided by this art.

The introductory **first chapter** is perhaps the most unfortunate (unglücklichste) of all of them. On three pages N. B. attempts to say something general about the art of conjecturing and quotes J. B. (1713, pt. 4, beginning of Ch. 2):

The art of conjecturing[5] 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.

N. B. then continues:

As becomes evident from this definition, the object of the art is indefinite and doubtful so that that object is unreliable. No full certainty is possible, although by assumptions one can determine how high the probability of an event is.

However, J. B. (Ibidem, end of Ch. 1) deeper indicates that he discusses not things doubtful per se, but those which in accord with our knowledge are not completely certain. But what does *probability* mean? Huygens (1657) did not mention it, and J. B. only introduced it in the last part of his AC[6]. The main notion of both these scholars was expectation which is indeed sufficient. The probability of an event can be understood as a special case of expectation[7]. A gambler can consider his win as a unity, and his loss as a zero and N. B. apparently thought exactly so, although did not express himself quite clearly.

Indeed, he began his dissertation by a long quote from J. B. (1713, pt. 4, Ch. 1), who had defined probability as a part of certainty, and went on:

The foundation of this entire Art upon which we ought to rely perpetually in assessing probability, consists in this general Rule which Huygens (1657) demonstrates in his elegant pamphlet, in Propositions 1, 2 & 3, and in my uncle in his Notes to these same Propositions.

[Kohli quotes the definition of expectation. Here is its end:] *The quotient provides the value of the expectation or the degree of the probability.*

I can only understand these last words, *the degree of probability*, in the sense that N. B., as stated above, considered probability as a special case of expectation. Then he quotes the remark of his uncle from the commentary on the Proposition 3 about the expectation being identical with the rule concerning mixtures when their prices are being determined and indicates that the general arithmetic mean and the centre of gravity of many weights are calculated the same way.

The latter comparison seems especially instructive since, so to say, the centre of gravity of all the probabilities[8] which leads to an equilibrium of weights is also calculated in the same way. For this reason jurists, in doubtful and obscure cases, tend to attain such an equilibrium. N. B. prefers to prove his statement on the basis of the Justinian *Corpus Juris Civilis*. He ends his first chapter by J. B.'s remark (commentary to Proposition 1 of the Huygens treatise): the expectation concerns the fear of something worst as well as the hope for something better.

In his **second chapter** N. B. estimates the duration of human life and thus lays the foundation for subsequent investigations:

… although the end of our life be the most uncertain, & the hour of death is known to no one except GOD, the highest and best, the ultimate giver of our life, who is able to take from us this his own gift at whatever time He Himself shall have pleased. Nothing remains for us other than that through conjecture to determine how many years up to this time a man will probably gain, or how much be the probability that he exceeds some given year or not &c.

I see however that there are many who will state that by the art of conjecturing it is almost impossible to determine the exact number of cases for something to occur. No mortal [N. B. continues by quoting J. B. (pt. 4, Ch. 4):] *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; or how much easier one disease (for example, the plague) can kill a man than another one (for example, dropsy; or the dropsy than fever), so that we will be able to conjecture about the future state of life or death.

After an intermediate remark about those things that depend on causes completely hidden and evading our experience, N. B. once more quotes J. B. (*Meditationes*, § 77; 1975, p. 46):

The matter reveals itself in another way in divinations & games which fate alone governs since here the expectation can be determined precisely and scientifically. Indeed, we accurately and clearly perceive the number of chances according to which profit or loss will follow infallibly. These chances manage themselves indifferently, and they will be equally likely to happen, or at least one shall be somewhat more probable than the other, so that we are able to define scientifically how much more probable.

Here, N. B. says, we determine the number of cases in another way. He quotes J. B.(from Ch. 4 once more) on the posterior estimation of the desired rather than on prior, when issuing from causes, and then turns to a marginal remark in the same § 77 of the *Meditationes*:

I can deviate less from the true value of the ratio [of cases] *when observing oftener than rarer.*

This excerpt from J. B., which N. B. had not indicated as such, supplements Huygens' statement about the *splendid* art of conjecturing[9]. Then N. B. goes on to calculate the order of the dying out [of a group of men] according to Graunt, which I have also discussed elsewhere (Kohli & van der Waerden 1975).

Out of a 100 new born babies only 64 are left after 6 years; only 40, after 16 years, […]. N. B. reports that he found these data in J. B. (1686, p. 283 in 1975) who, in turn, took them from the *J. des Sçavans*, No. 31, 1666[10]. Neither J. B., nor N. B. knew, however, that the Graunt table was based on considerations rather than observations.

Then N. B. shows in detail how to determine the mean expectation of life in years. He writes:

If one should be driven to estimate the lifetime of some new born baby, he will have to consider the following. This baby is included either among those 36 who die within the first six years, or among those 24 who die between the sixth and sixteenth year or … […] Therefore, there are 36 chances that he will die within the first six years, i. e., that he may probably survive up to three years (this half is chosen since on account of the lack of observations not extending into individual years it must be supposed that anyone is equally likely to die in the individual moments of these six years). […] There are another 24 chances that he will die between the sixth and sixteenth year, i. e. that he will probably live up to 11 years. […] Likewise another 15 chances […] By the general rule given in the preceding chapter the expectation of our baby is worth

$$\frac{36 \cdot 3 + 24 \cdot 11 + 15 \cdot 21 + 9 \cdot 31 + 6 \cdot 41 + 4 \cdot 51 + 3 \cdot 61 + 2 \cdot 71 + 1 \cdot 81}{100} =$$

$18\dfrac{11}{50}$ *years.*

In the same manner the life of a six-years old child will be

$$\frac{24\cdot5+15\cdot15+9\cdot25+6\cdot35+4\cdot45+3\cdot55+2\cdot65+1\cdot75}{100}=$$

$20\dfrac{25}{32}$ years.

Therefore, the expectations of people aged 16, 26, 36, 46, 56, 66 and 76 are $20_{1/4}$, $19_{2/5}$, $17_{1/2}$, 15, $11_{2/3}$, $8_{1/3}$ and 5 years.

N. B. notes that these numbers can be calculated easier when moving in the opposite direction and applies a formula which in modern notation is

$$e_x = \frac{1}{l_x}[\frac{1}{2}(l_x - l_{x+1}) + l_{x+1}(1+e_{x+1})].$$

Here l_x is the number of those who survived age *x,* and e_x – the mean expectation of the duration of life at that age. N. B. clearly distinguishes the mean expectation of life (although applies the terms expected, mean age, probable or most probable age) and the age at which there survive exactly 1/2 of a certain age group. The brothers Huygens understood the difference between the two time periods (Kohli & van der Waerden 1975)[11]. N. B. writes:

We say that the expectation of a new born baby or of someone who is 6, 16, &c years is worth 18₁₁/₅₀, 20₂₅/₃₂, or 19₂/₃ &c years or that it is equally probable that they have died within that age span rather than beyond, which means that among many men of the same age so many will go beyond the specified age as die within it. All this concerns the average or mean life.[…] The longest life is compensated by the untimely and anticipated death. This is what the Germans call <u>ein Jahr in das ander gerechnet</u>, and the French, <u>l'un portant l'autre.</u>

It would be contrary to say that the most probable age of this man & the mean age are, e. g., 20 years. […] Thus, a new born baby according to the former finding will expect 18₁₁/₅₀ years. However, it must be almost twice more probable that he will not lead his life to that age, for out of 100 new-born babies scarcely 37 survive after 18₁₁/₅₀ years. […] But if we wish to determine that time in which this new born baby will most probably die, we will only have to search within how many years a half of such babies dies. […]Within 6 years 36 die out of 100, within the next decade, 24. So within how many years 14 die? […] 5₅/₆ will be found. The sought time period is evidently 11₅/₆ years.

Huygens, De Witt and J. B. (in a letter to Leibniz of 2 Aug. 1704, see Kohl (1975)) had considered the next problem discussed by N. B. In this letter J. B. wrote:

In the same way you can show by an example what you are thinking about annuities for many lives.

Huygens, Hudde, De Witt and Halley had studied that problem.

N. B. goes on to determine the mean expectation of the life of the last survivor from a group of 2, 3 or more people of the same age or of different ages. However, before that, he says, we ought to solve the following problem. Given, a period of a years during which b people die and each dies equally easily at any moment. It is required to determine the number of years until which the last survivor will probably live. His answer: $ba/(b + 1)$, which is $a/2$ for one man, 2/3, 3/4, 4/5, … of a for groups of 2, 3, 4, …men.

This result[12] is very remarkable and quite new. N. B. proves it by dividing the period a into n equal intervals, determining the number of the possible distributions of deaths in each and calculates the limit as $n \to \infty$. He does not distinguish the index which runs from 1 to n from the number n itself and we have therefore denoted that index by v. As was usual in those times, N. B. non-rigorously passed to the limit.

Let us follow his solution. Separate a into an uncountable number of equal intervals or moments m whose number n is infinite. The last man to survive dies at moment v, the other die either at the same moment or earlier. Suppose that 0, 1, 2, … people are still living. This can happen in cases whose numbers are equal to the number of zeros, units, twos, … in v things, i. e., in 1, v, $v(v + 1)/2$, $v(v + 1)(v + 2)/6$, … cases. Therefore, the product of the number of the cases by the number of moments of the life expectation for the last survivor will be

$$1vm,\ v^2m,\ [v(v + 1)/2]vm,\ [v(v + 1)(v + 2)/6]vm,\ \ldots$$

The sum of all the products (with $v = 1, 2, …, n$) divided by the sum of all the cases will provide the expectation sought when the death is equally likely to happen at any moment

$$\frac{\sum vm}{n},\ \frac{\sum vvm}{n(n+1)/2},\ \frac{\sum[v(v+1)/2]vm}{n(n+1)(n+2)/6},\ \frac{\sum[v(v+1)(v+2)/6]vm}{n(n+1)(n+2)(n+3)/24}, \ldots$$

or, as $n \to \infty$,

$$\frac{\int vdv}{n}m,\ \frac{\int vvdv}{nn/2}m,\ \frac{\int(v^3/3)dv}{n^3/6}m,\ \frac{\int(v^4/6)dv}{n^4/24}m,\ \ldots$$

which is equal to

1/2, 2/3, 3/4, 4/5, … times $nm = a$.

N. B. also provides a very elegant geometrical proof. Construct a curve whose abscissa x corresponds to the time interval during which a given number of men will die, and the ordinate y, to the number of cases in which this happens. Then the distance between the centre of gravity of this curve and its apex,

$$\int xydy \div \int ydx$$

will be the number of the years sought.

In our case, the ordinate invariably equals the abscissa to the power of $(b - 1)$ where b is the number of men in the group. And for $y = x^{b-1}$ the most probable age of the last survivor will be

$$\int x^b dx \div \int x^{b-1} dx, \text{ or } ba/(b + 1) \text{ for } x = a.$$

The number of cases should naturally not be understood literally; it is only essential that the probabilities of the death of $(b - 1)$ men during time interval $[0, x]$ be proportional to x^{b-1}.

After proving this auxiliary theorem, N. B. is able to determine the life expectancy of the last survivor of two new born babies:

[36·36·4 + 2·36·24·11 + 24·24·12$_{2/3}$ + 2·60·15·21 + 15·15·22$_{2/3}$ +
2·75·9·31 + 9·9·32$_{2/3}$ + 2·84·6·41 + 6·6·42$_{2/3}$ +
2·90·4·51 + 4·4·52$_{2/3}$ + 2·94·3·61 + 3·3·62$_{2/3}$ +
2·97·2·71 + 2·2·72$_{2/3}$ + 2·99·1·81 + 1·1·82$_{2/3}$]: (100·100) =

274$_{4119/5000}$ years.

For two people aged 16 and 46 years, that life expectancy is 25$_{23/48}$ years. The same problem can also be solved for three or more people of any ages.

N. B. once more turns to his uncle's considerations. J. B. (1686) stated that a 16-years-old daughter of a 56-years-old man outlives him in 101 cases, whereas the opposite happens in 59 cases. N. B. provides the lacking calculation. If the father dies during the first decade, the daughter can also die during that period in 15 cases and in 25 cases will live 10 years longer. The expectation of the opposite case is then (the answer here and below is expressed in parts of certainty)

[15·1/2 +25·0]: 40 = 3/16.

If the father dies during the second decade, his corresponding expectation is

[15 + 9·1/2 + 16·0]:40 = 39/80.

Finally, if the father dies during the third decade, his expectation is

[24 + 6·1/2 + 10·0]:40 = 27/40.

The father's total expectation is [the sum of those three terms] 59/160 of certainty, and 101/160 of certainty is left for the daughter.

This calculation with parts of certainty, i. e., with probabilities, is unusual for N. B. as well; most often he provides the ratio of the expectations or of the number of cases. He ends this chapter by some

considerations about the available observations. He is dissatisfied by linearly interpolating the numbers in the Graunt table, but a friend provided him actual empirical data on [almost] two thousand men from a celebrated Swiss city. Regrettably, N. B. did not give these data, he only indicated the life expectancies for various age groups calculated and rounded off to years. [Kohli provides these expectancies.] The expectations derived from the Graunt table essentially differed, and N. B. concluded:

It is doubtful how to explain this difference. Perhaps the number of observations made was insufficient, for if ages of more men, e. g., of three, four, ten &c thousand had been observed, I could have deviated less from truth. Or (what I rather would have believed) that in our Switzerland, because of a more temperate life or a better constitution of the air, men are more often reaching a longer lifetime than in France, where by chance the observations given in the Chronicle of French scholars [J. de Sçavans, wrote Kohli] had been made. Or, rather some other thing and for the time being we will nevertheless cling in those prior observations to our hypothesis until we obtain better ones[13].

We can only regret that N. B. had hesitated to base his investigations on the private data from his friend and applied the published Graunt table. The order of the dying out of a group of men from that unnamed city can approximately be reconstructed by issuing from the rounded off life expectancies according to the formula for the number of survivors

$$x + 5 = x \frac{e_x + 3}{e_{x+5} - 2}.$$

The calculated numbers agree well with the Halley data for Breslau.

N. B. ends this second chapter by expressing his hope that the pastors in each town will *record more accurately* the ages at death. This, he adds, will enable better to estimate life expectancies and provide the foundation for applying the Lex Falcidia[14] to presume the death of a missing person and to determine the cost of life insurance and life annuities.

It ought to be clearly indicated that N. B. boldly investigated his problems and that their solution was mathematically irreproachable. His calculation of the life expectancy of the last survivor of a group of men especially surpassed the appropriate Dutch works, especially the determination of the expectancy of the life of the last survivor of a group of men. [See also Note 12. O. S.] In the sequel, N. B. applied the results of this second chapter to various legal problems.

In his **third chapter** N. B. discussed the declaration of death of missing persons. A man is called missing, he says after citing legal sources, if his whereabouts is unknown, so that it is unknown whether he is still alive. If, during a long time, there is no news either from him himself or from others, he may be declared dead at the request of his compatriots.

But how to understand the *long time*? After quoting many sources, N. B. shows that the opinions of jurists essentially differ from each other. Some believe that five years is sufficient, others think that a century is necessary. Johannes Bunz, in his dissertation which appeared in Basel in 1686, groundlessly suggested 30 years as a reasonable duration. N. B. himself decided that that duration is best determined by the data on deaths.

Probable is generally understood as something obviously exceeding 1/2. When requiring that the probability of death is twice higher than the contrary alternative, it will exceed a half of certainty by 1/6 of it [2/3 – 1/2 = 1/6]. For example, when is it possible to declare dead a missing new born baby? Or, otherwise, when 67 such babies out of a hundred will die? 60 die during 16 years, and 15 during the next ten years. By the rule of three 7 more will die during the next $4_{2/3}$ years[15]. The disappeared new born baby can only be declared dead after $20_{2/3}$ years.

N. B. provides the appropriate durations for people aged 6, 16, …, 76 years. In other respects he follows Bunz: after those durations the missing person is not taken into consideration and his belongings can be given to his heirs without requiring any security.

J. B. also left some notes about missing persons, see § 77b of his *Meditationes* (Problem 10) and his *AC* (pt 4, Ch. 2, Item 3). He asks whether a person missing for 20 years is more probably living than dead, and decides that this question can be answered after discussing the causes of the two possibilities. He does not, however, conclude which of these two is more probable, and the distance between his considerations and the clear conclusions made by N. B. is really large[16].

In his **fourth chapter** N. B. studies the purchase of hopes [expectations], at first in a very general way, then with regard to life annuities. Such purchases can also include catches of fish, hunter's bags or legacies. Differing and not studied is here the purchase of future things, such as ripening fruit, young cattle or yet an unborn baby of a slave woman. Indeed, the appropriate agreement will become null and void if the presumed event does not occur.

In the case of expectations, on the contrary, their cost should be paid even if the buyer is left empty-handed since here it is the purchase of a pure expectation independent from the result.

The just price is determined not by what will finally occur, but by an equilibrium of gain and loss at the conclusion of the agreement.

Thus, N. B. assigns such a price for fishing rights.

The value of the expectation will be determined if the number of fish captured during several previous years in that river is divided by the number of those years since the quotient will denote the number of fish which will probably be caught in this year. And so, a just price in this purchase will be that which otherwise must be paid for so many fish. However, the beneficial nature of the law should be accounted for if the buyer pays more than double that price or less than half of it. The circumstances of an agreement are always decisive.

When discussing life annuities, N. B. surveys the appropriate legal literature. Some jurists do not recognize at all any agreements about

them since, first, they are often declared unjust, and, second, because they essentially prompt crimes. Thus, a Florentine was poisoned after contracting such an agreement.

N. B. refutes the second argument by indicating the laws of inheritance. Concerning the first one he refers to many authors who nevertheless believe that such agreements are valid. He himself upholds the legality of agreements in which the equilibrium of the conditions for both buyer and seller is observed. In other words, in which they are both running the same danger of loss[17]. This condition is only fulfilled, as he adds, when the cost of the annuity is determined in accord with the probable life expectancy.

The jurists had not at all been unanimous about the ratio of the yearly annuity and its price and their suggestions differed from 1/6 to 1/12. N. B., however, argued that the price of an annuity should not be assigned without allowing for the buyer's age and health. He referred to likeminded jurists, for example, to Ulpian's reasoning about maintenances. Other jurists, on the contrary, did not establish any just price but rather left this problem to the discretion of a clever judge. N. B. himself had only partly realized his recommendation: he allowed for the age, but not for the health of the buyer[18].

N. B. did not elicit the Dutch investigations from oblivion but considered in detail the rule due to Molina who thought that the just price of a life annuity is equal to the moneys drawn during half the maximal duration of the buyer's life. N. B. disagreed. First, if, for example, a 60-year-old man can only live until 80, the assumption of his dying equally easy during each year does not hold. Second, a hundred ducats today is more than ten times ten ducats yearly.

After rejecting all these unjustified opinions, N. B. says, when concluding his survey:

We will relate the genuine method of estimating annuities. First, it is well known that the annual payments ought to exceed ordinary interests since the principal or the paid premium cannot be redeemed. Second, so much should be added to the interest that by the time of the buyer's death all the principal be exhausted.

Accordingly, N. B. derives a formula for calculating the price of life annuities for a man with a given duration n of life. A new born baby can expect to live $18_{11/50}$ years. At an interest of 5% the just price[19] will be $11_{1897/2433}$. N. B. also provides the results for men aged 16, 26, … years and easily calculates the price of life annuities for two lives and continues:

Certainly, […] *I perceive that the value of these annuities is incorrectly estimated by supposing the duration of the return to be so many years, as many as someone will probably be presumed to survive. Since the premiums do not increase in the same proportion with years, the just premium of the annuity bought for one life which will certainly expire within the decade, and, moreover, expire equally likely during each year of that period[20], should not be the same as the premium of a usual annuity for five years. It should be equal to the arithmetic mean of the prices of the annuities for one, two, &c years up to ten[21].*

However, this reasoning is wrong. If the buyer dies during the first decade[21], the annuity for the tenth year will not be paid, and only nine payments should be accounted for. Similarly, if the buyer dies during the second decade, the payments for the 10th, the 11th, …, the 19th year are only considered:

In order to determine the true premium of the life annuity, it is necessary to find the premium in individual years lasting until any man is able to survive and to multiply those premiums by the appropriate number of cases, and to divide the sum of all these products by the number of all the cases. To this end I compiled the following table of those premiums up to hundred years.

Issuing from this table for an annuity of *n* years, N. B. now derives the price of the life annuity providing 1000 monetary units yearly for buyers of different ages:

Age 0, 6, 16, 26, 36, 46, 56, 66, 76
Price of life annuity: 9.420, 10.680, 10.593,
        10.576, 10.164, 9.457, 8.148, 6.545, 4.568

As an example, he shows in detail the calculation for a 16-year-old buyer:

$[15 \cdot 4.558 + 9 \cdot 10.519 + 6 \cdot 14.179 + 4 \cdot 16.427 + 3 \cdot 17.806 + 2 \cdot 18.653 + 1 \cdot 19.173):40 = 423.720/40 = 10.593.$

And now N. B. attempts to confirm his results by comparing them with the prices published in Amsterdam in 1672 and 1673 and notes that the agreement is very good. I recall that those prices were much lower than those indicated by Hudde and De Witt.

There also exists another kind of life contracts, N. B. continues, which *has a great affinity with annuities and is still used today most of all by the Italians*. The father of a new born baby girl *receives from someone four or five times more than restores to him if the daughter arrives at the marriageable age but he retains the whole if she dies before this age.*

N. B. shows that that insurance provides too little: the father ought to get 5.457 times the possibly returned.

The **fifth chapter** is devoted to a problem in the law of inheritance. Already in the Roman law the testator was unable to will everything quite freely: by the Lex Falcidia, he was obliged to leave a certain part of the legacy to his heirs.at-law.

  Kaser (1955, pp. 630 – 633) indicated that

Only the Lex Falcidia (40 BC) safeguarded those heirs since it allowed the testator to will not more than 3/4 of the legacy; 1/4 should have been left for the heirs. If the testator had also burdened this quarter [by obligations], *all the legacy was decreased respectively.*

Calculations are based on the price of the legacy at the moment of the testator's death less the debts. When a part of the legacy is claimed, or if there are debts or possible heirs [apart from the heirs-at-law] *the price of the possible rights is estimated or secured by a guaranty.*

It was this last type of wills which depended on chance that interested N. B. How to estimate properly that price? He quotes an Aemil. Macer from the *Corpus Juris Civilis* (Code of Civil Law) who knew the Ulpian table (Kohli & van der Waerden 1975). This table prompted various interpretations and objections. N. B. partly took them into consideration, then submitted his own opinion by providing an example.

For a 24-year-old man Ulpian established that his duration of life was 28 years. According to N. B.'s table (Ch. 4), the price of an annuity of 10 [monetary units] for such a duration is 149, so this should have been that price.

Ulpian had probably regarded his data as a table of life expectancies and intended to apply it accordingly, just as N. B. did. But then his similar calculations in the previous chapter are wrong. In any case, N. B. noted that that table did not agree with reality, i. e., in this case, naturally with the Graunt table. He continued almost polemically, without imagining how badly his data were justified:

It is evident from Chapter 2, where we computed the probability of a human life, not according to the opinion of some medical quacks, physiognomists, palmists, foretellers, diviners who inspect entrails or similar deceivers of this nature, not from anyone about whom Titius surmises that the Romans have perhaps accepted the method of reckoning which is handed down by this law, but from observations made concerning the number of dead at whatever age.

Therefore, by this case of the Falcidian law I think that the ratio is best entered upon as a calculation if only such legacies should be estimated according to the value & premiums of life annuities.

Domat (1689 – 1694) refers to the *calculs qui ont été faits sur les expériences du nombre de personnes qui meurent à chaque àge* [to the treatment of data on the number of people dying at each age]. In one case, however, N. B. acts more properly. He determines the expectation of life whereas Domat calculated the number pf people living after a certain time.

N. B. illustrates the application of the Lex Falcidia by two examples. In the first, the deceased man left 3000 of which 800 went to Titius, 900, to Sempronius, and 100 annually to a six-year-boy Maevius. It followed from the previous chapter that the annuity was worth 1060 so that out of the 3000 the testator had distributed 2760. For obeying the Lex Falcidia 510 ought to be subtracted from that sum which means subtracting $147_{20/23}$ and $166_{7/23}$ from Titius and Sempronius and $181_{11/23}$ from the annuity of Maevius.

N. B. remarks that such calculations can be made not only with regard to the Lex Falcidia but also in other cases in which assumptions and conjectures about the duration of human life are made. Most jurists believe that in such cases one should follow the Lex Falcidia, as for example when discussing the letting of premises or leases made for the life of the leaseholder, or, *if someone harmed another one or killed him, and the harmed one or the heirs of the killed desire a valuation of the ceased services.*

In the **sixth chapter** N. B. discusses three examples of insurance. He took the first one concerning a shipwreck almost word for word

from J. B. (*Meditationes*, § 77b, Problem 6; its solution begins in § 87). N. B. easily solves this problem by the theorem on the addition of expectations[22].

The second example had to do with the interest in dealings with navigation. J. B. (*Meditationes*, § 77b) had considered this problem as well, but he only traced its solution. A merchant wishes to buy a commodity in A, to ship it to B and to resell it there. Is it possible to lend the necessary money provided that it will not be returned if the commodity is lost due to a shipwreck? Such a concession should naturally be balanced by a higher interest. The usual monthly interest [in such cases] apparently amounted to 1%; was it reasonable?

N. B. denotes the cost of the commodity by *a*, the monthly payment to the creditor by *x*, the time of travelling, by *n* months, by *p*, the number of successful arrivals of ships, and by *q*, the number of the opposite cases. The expectation of the creditor is then

$$[p(a + nx) + q \cdot 0] : (p + q)$$

which should coincide with the increase of his capital in the usual case, i. e., with $(a + nb)$. Therefore,

$$x = (qa + pnb + qnb) : pn$$

where *b* is the usual monthly interest.

N. B. provides numerical calculations in two examples.

The last problem of this chapter concerns life insurance. At present, annual premiums are usual, but in those times the premium was paid in cash at the conclusion of the agreement. N. B. begins by rejecting the objections which stated that humans should not be dealt with on a par with commodities. He pointed out that the matter dealt not with people, but with the probabilities of human life

Concerning games, wagers and lotteries is *the title of* **Chapter 7.** N. B. begins by stating that games and wagers are allowed as far as they are just and deal with worthy matters[23]. He praises the significance of the art of conjecturing, refers to Huygens and J. B. and considers in detail two examples: bets about electing five senators for managing the *more principal offices* of Genoa and the Netherland lottery.

J. B. had discussed the Genoas problem in his *Meditationes* (§ 89). N. B. took it over in a simplified form, although partly word for word, and provided numerical calculations. Thus, he was able to establish that the prizes were far too small as compared with the stakes so that the gamblers were cheated by their 2,094,053/5,019,168[th] part. Consequently, such bets are not allowed anymore, and *the merchants* (?) are demanded to return what they have received over and above the just price. Caramuel had established even earlier that the conditions of the wagers were unjust.

N. B. does not discuss these results in more detail since they are mistaken. Already in N. B.'s time lotteries had been adapted for drawing money from the public in a merry way. Thus, for a certain stake a lot can be extracted from an urn called Jar of Fortune. The

prize went to that person who had extracted an inscribed lot rather than some of the many uninscribed ones.

In the *Novelli Bernenses* of 15 March 1709 N. B. found information about a very special lottery with prizes being life annuities. There were 8000 lots priced at 250 florins each.

2 lots provided life annuities of 3000 florins; 4 lots, of 2000 florins; 4 lots, of 1000; 8 lots, of 500, 14, of 250; 30, of 150; 30, of 100; 1208, of 30

Each of the rest 6700 lots provided a life annuity of 15 florins, and the buyers of the first, and the last lot got an additional annuity of 150 florins. Thus, for 2,000,000 florins the public was promised life annuities totalling 170,040 florins. It was possible to change the annuities into 4% bonds; an annuity of 3000 florins could have been changed into bonds worth 35,250 florins, i. e., in the ratio 1:11 3/4.

N. B. concluded that the organizers of that lottery could have only expected a surplus of 2030 florins [2,000,000 − 170,040·11.75]. However, in chapter 4, when issuing from the Graunt table, N. B. established that 1:10 3/5 was more proper, that his calculations envisioned an interest of 5% whereas the usual interest was 3 or 4% and that, finally, the gamblers could have profited by the lack of taxes on life annuities.

In the **eighth chapter** N. B. once more discussed a problem in the law of inheritance. The Romans had established that, if the testator left a pregnant wife and a son, the son got 1/4 of the legacy at once. N. B. disagreed. Even triplets are so rare that they may be neglected. Then, one pregnant woman out of a thousand possibly gives birth to twins[24], but at least one pregnancy will miscarry. The expected number of babies will therefore be

$$[1·2 + 998·1 + 1·0]:1000 = 1.$$

Basing himself on this dubious, as I believe, argument, N. B. defended the opinion that a half of the legacy can without delay be awarded to the son. If, however, no living baby will be born, he ought to get the entire legacy; if several babies, he will get less respectively.

In the **ninth chapter** N. B. begins by two possibly dubious examples of applying the theory of probability to the administration of justice. They concern the credibility of the testimonies of witnesses and on the conclusiveness of the evidence. He believes that in testimonies, the credibility of witnesses must be examined before their questioning so that it makes sense to formulate a rule for measuring the credibility of a person and thus to determine the probability of his telling the truth or not. And here is this rule:

Divide the number of chances in which one is observed to speak truths, by the sum of these chances and the chances in which he was observed to lie, & you will have the degree of credibility. Or, if several men having been approved by credibility testify to the truth of that man, and others, just as well having been approved, accuse him, divide the number of the first group of men by the sum of both.

This rule is literally contained in J. B.'s *Meditationes* (§ 77, Problem 5). However, as he added apparently later, in arts and sciences its application is very restricted.

In his second example, N. B. issues from the evidence which indicates the guilt of an accused in one case out of three. Lack of evidence means that he is undoubtedly innocent, that his innocence has *worth* 1. For 1, 2, …, 10 evidences the worth of his innocence becomes equal to 2/3, 4/9, 8/27, …, $(2/3)^{10}$. This last number , N. B. says, means that the accused is morally almost certainly guilty. He believes that many other similar problems can be investigated the same way[25].

The last example is, on the contrary, unquestionable, but N. B. does not go further than J. B. did in his *Meditationes*, § 81, Problem 4. Suppose that a thing being intact is given to someone for temporary use and became damaged. Should its cost be compensated? J. B. as well as Pufendorf answers affirmatively. This seems harsh but it is just. In the same way it is possible to bet 100 talers to one when the expectation of winning is a hundred times higher.

Here, as also elsewhere, we sense that the founders of probability theory[26] must have felt pleasure when having been able to investigate and impeccably solve everyday problems occurring in the society by the doctrine of games (Lehre von den Spielen).

N. B. published an abridged account of his dissertation [in 1711] in the *Acta Eruditorum*, Suppl. t. 4, sect. 4, pp. 159 – 170, calling it *Specimina artis conjectandi ad questiones juris applicatae*.

Notes

1. N. B.'s dissertation was reprinted in the volume of J. B.'s contributions on the theory of probability.

2. As a rule, N. B. mentioned the AC when copying its passages, but kept silent when turning to the *Meditationes* never even meant for publication. Its part had nevertheless appeared in that same volume of J. B.'s contributions.

3. Only elements of the theory of probability had appeared by then. Interestingly though, N. B. was the first to apply the term *calculi probabilitatum* in his Preface to the AC, see p. 108 of that same volume. An English translation of the Preface (David 1962, pp. 133 – 135) does not apply this term. Kohli mentioned the theory of probability once more, when describing chapter 9 of N. B.'s dissertation.

4. In 1713, in his correspondence with Montmort (1713), N. B. described his invented Petersburg game and, while discussing the sex ratio at birth, actually introduced the normal law (Sheynin 2009a, § 3.3). De Moivre (1718/1756, pp. 252 – 253) criticized N.B.'s reasoning on randomness but called him *a very learned and good man*. I do not dwell on De Moivre's correspondence with N. B. in general since it did not concern probability.

5. Actually, *The art of conjecturing sive stochastice*. Bortkiewicz (1917, p. X) referred to J. B. and put this word into scientific circulation.

6. N. B. returned to the *number of cases* when proving his weak law of large numbers.

7. Bernstein (1946, p. 46) explained this point in detail. Let ξ equal 1 or 0 with probabilities p and q. Then $E\xi = 1p + 0q = p$.

8. Or, rather, the centre of gravity of all the possible values of the random variable whose expectation is being calculated. K. K.

9. This eulogistic statement was contained in Huygens' introductory letter to van Schooten, but the term *art of conjecturing* had not yet appeared.

10. Kohli referred to J. B. (1666) which was an obvious mistake. J. B. mentioned the *J. des Sçavans* on a margin of his book, see p. 46 of its 1975 edition.

11. See also Sheynin (1977, § 4.2.3).

12. N. B. was the first to apply a continuous (uniform) law of distribution in a published work. What he calculated was the expectation of the appropriate order statistics.

In 1669, in correspondence with his brother, Huygens (1888 – 1950, t. 6) discussed problems in mortality and life insurance and introduced the continuous uniform law of distribution. In one of his problems he wrongly assumed that the number of dying men from a given group decreases with time. However, under that law, order statistics divide a given period of time into approximately equal intervals. N. B. had made no such mistake. See Kohli & van der Waerden (1975) and Sheynin (1977, § 4.2.3). In that paper, we have also described the determination of the expected life of the last survivor of a group of men by Huygens.

13. Graunt, whom N. B. mentioned, had known nothing about that Chronicle (or, rather, about the *J. de Sçavans*).

14. See explanation in the description of the fifth chapter.

15. N. B. applied linear interpolation in his Ch. 2 as well, but then, also in Ch. 2, he expressed his dissatisfaction with this method when dealing with numbers in the Graunt table.

16. J. B. devoted his chapter 2 of pt. 4 to arguments and their significance. It should not have included any calculations.

17. The same danger of loss is not a sufficient condition, since the same expected losses were also needed.

18. It was too difficult to consider the health of the buyers.

19. Meaning: the just premium should be $11_{1807/2433}$ times the annuity.

20. While criticizing the Molina rule (see somewhat above), N. B. did not agree with this assumption. O. S. Possibly presumed (see above): N. B. meant expectation of life. K. K.

21. Apparently, death during the tenth (the twentieth, see below) year was meant.

22. N. B. had not applied any such theorem. For that matter, they had not yet been formulated.

23. Although repeatedly prohibited, there existed a revolting practice of betting on the safe arrival of ships (Sheynin 1977, p. 207).

24. Twins are known to be born once in about 80 – 85 births. In any case, N. B. did not justify his unbelievable estimate.

25. This reasoning is superficial. An evidence proving something with probability 1/3 is unworthy. Then, summing evidences is hardly justified; for one thing, they are possibly interdependent. Cournot (1843, § 225) mentions cases in the context of the administration of justice in which *fictitious independence between actually solidary* facts is presumed. And in § 222 he admits the possibility of deriving the trustworthiness of testimony in the same way as N. B. did (see above). In other words, both N. B. and Cournot issue from a uniform prior distribution, from ignorance, which is hardly reasonable.

26. N. B. should not be called a cofounder of the theory of probability.

Brief Information about Those Mentioned

Domat, Jean, 1625 – 1696, jurist, friend of Pascal
Falcidius, first century, a people's tribune
Hudde, Johannes, 1628 – 1704, mathematician
Macer, Aemilius, 1535 – 1600, Jesuit
Molina, Luis de, 1535 – 1600, a scholastic theologian and jurist
Pufendorf, Samuel Freiherr von, 1632 – 1694, jurist
Titius, Gottlieb Gerhard, 1661 – 1714, jurist
Ulpianus, Domitianus, died in 228, jurist
Witt, Jacob de, 1625 – 1672, mathematician, statesman

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II

C. F. Gauss

A Sketch of the Introduction
to the German text of the *Theoria motus* (an Excerpt)

Deutscher Entwurf der Einleitung zur *Theoria motus* (1807).
Werke, Bd. 12, 1929, pp. 156 – 162

During a few weeks after the discovery of Ceres its orbit became known only along an arc covering 3° of its geocentric motion, and after a year Ceres had to be searched for in a quite another part of the sky.

I first applied my method in October 1801, and, by using the result derived from it, Ceres was found during the first cloudless night exactly there, were it was looked for [on 7 Dec. 1801, by von Zach – Editor]. In a short while, the second, the third, and the fourth new planet provided a further possibility of checking the general applicability of my method.

Soon after the rediscovery of Ceres many eminent astronomers began to ask me insistently to publish my method. However, various hindrances, my wish to expound thoroughly this subject, and, finally, my hope that a further occupation with these works will bring the various parts of my method to a higher degree of perfection, generality and handiness, only now allowed me to satisfy the desire of those friends of mine. I flatter myself with hope that that delay will not cause their discontent.

During the passed time I had very much repeatedly changed my initial method, added a great deal and in many of its parts followed quite another ways. Little in common is left between my initial method of calculating the planetary orbits and that which I applied in this work. I certainly had not intended to offer a complete account of my investigations, but neither had I thought about completely excluding many of my previous methods, the less so since they concerned the solution of exceptionally interesting problems. On the contrary, along with the really easiest and most useful methods of solving the intended main problem, I collected everything, which, during considerably long calculations, I found remarkable and practically tested about the motion of the heavenly bodies. Nevertheless, I invariably describe my own (eigentümliche) investigations in more detail and touch on the known in so far as it is necessary for the completeness of the whole.

This work therefore naturally breaks down into two sections. The first one is devoted to the study of all the most interesting and most useful relations between the various magnitudes which describe the motion of the heavenly bodies around the sun according to the Keplerian laws. In addition, this study prompts many peculiar methods for deriving geocentric phenomena from the elements. Those phenomena result from the complicated (künstlich verwickelten) combination of the elements and it is therefore necessary first of all to

get confidently acquainted with all the separate tangles of that web, then dare to hope once more to take successfully apart the individual threads and unravel the whole into its initial separate parts.

In the second section, it will be so much easier to solve the inverse problem, namely, to derive the elements from the phenomena, since the greatest part of the necessary individual operations is already known from the first section, and the work mostly reduces to collecting, ordering and combining them in a common whole.

I have accompanied most problems by examples choosing them when possible from really occurred cases. Hopefully, they will prove the practical usefulness of the solutions and illustrate them. Because of the increased handiness, less proficient readers will also be able to acquaint themselves with the whole, and the number of the adherents of these calculations, which comprise one of the most important and most splendid branch of theoretical astronomy, will increase.

Editor's Remark

In the autumn of 1806 Gauss had begun working out his *Theoria motus*, and, approximately in April 1807 its German text was ready (see his letters to Olbers of 29 Sept. 1806 and 28 April 1807). He still had no publisher, and Olbers turned to the Hamburg bookseller Perthes. At first, Perthes declined, then stated that he was prepared to publish that work *in Latin* (see the letters of Olbers to Gauss of 21/22 April and 6/7 May 1807). Gauss agreed and began the translation at once (his letter to Olbers of 26 May 1807). In November 1807 the printing began, but the going was slow, and the work only ended in June 1809 (letter to Olbers of 27 July 1809). Only the sketch published here is left from the initial manuscript written in German.

<div align="right">Brendel</div>

III

F. W. Bessel

Letter to Professor Airy at Cambridge[1]

Abhandlungen, Bd. 3, 1876, pp. 462 – 465

Dear Sir, I am very glad to see by your kind letter of Aug. the 6th, that you are ready to undertake the solution of what I consider as the principal problem of practical Astronomy of the present time, viz., to construct most concise Catalogues of places of Planets observed since Bradley's time. I do not doubt but this undertaking duly executed, will grant to you the thanks of present and future Astronomers, in what measure it appears important to me, you may judge yourself by remembering that it was this very problem, which gave rise to my *Tabulae Regiomontanae*.

One half of the labour being made by these Tables, I thought proper to propose publicly the accomplishment of the remaining half; I am particularly obliged to you for having entered upon my proposal, and I shall readily comply with your desire to explain my views about this subject. It would be useless to enter here into the particularities of the computations; but I avail myself of the present opportunity to state my opinion respecting a matter of influence on the reduction of astronomical observations in general.

These Reductions depend upon Elements, the numerical values of which are derived from observations and accordingly always are liable to some incertainty (!). Such values have a particular claim to the attention of Astronomers. Every new inquiry increasing the weight of the result, issueing (!) from the combination of this and former inquiries, the remaining error probably will diminish continually; but this error never vanishing entirely, it will (generally speaking) be necessary to exhibit the result of a computation depending upon assumed values of certain Elements in a form open to further corrections. The true values of the Elements being designed by x, y, z, …, the assumed values by α, β, γ, … the general form of a reduced observation, accordingly, should be

$$O + a(x - \alpha) + b(y - \beta) + c(z - \gamma) + \ldots$$

where O is the computed result, and a, b, c, … are Coefficients, also computed.

In many cases, results exhibited in this form will be complicated with a great number of undetermined quantities. The Rightascension (!) of a Planet, for instance, would depend upon twice as many such quantities as Fundamental-stars have been compared (viz. the corrections of the assumed Rightascension for two Epochs) and upon two more for the Constants of Aberration and Nutation. Such a complication undoubtedly would not be convenient for use, and nothing will remain but either to diminish the number of undetermined

quantities by a *supposition* or else to leave to the future the care to compute the Observations a-new.

Taking the utility of the present reduction of the Observations of the Planets, made between Bradley's time and ours for granted, the latter alternative will be rejected; the former requires to *suppose equal* the corrections of the different Rightascensions contained in every one of the two Fundamental-catalogues, whereby only one undetermined quantity will be left for each of them. The number of undetermined quantities entering into the exhibition of the results, accordingly, will be reduced to *four*. But I am of opinion that even this diminished complication would be without real advantage.

If indeed *general* Corrections of the two Fundamental-catalogues for 1755 and 1820 will be indicated by future inquiries, their influence on every result may *then* be computed exactly as easily as by the *present* exhibition of Conditions; with respect to the Constants of Aberration and Nutation their possible errors will scarcely be of any moment if the Result is presented in the most suitable form.

If the observations are made at an Observatory furnished with large and well established instruments, the Planets will commonly be compared with stars culminating at every hour of the day, from the morning till after midnight: the Aberration and Rightascension being negative if a star culminates between 18^h and 6^h, positive, if it culminates between 6^h and 18^h, the Correction of the clock derived from all the observed stars will be affected in contrary directions by an error of the Constant of Aberration; whereby the influence remaining in the mean of all stars will be so much diminished that it will not be of any consequence in a computation founded already on a *supposition*, viz., that of the egality (!) of Errors of the different stars in every-one of the Fundamental-catalogues.

I am accordingly of opinion that the correction which may perhaps be applied in a future time to the Constant of Aberration deserves no notice in the present Reduction; but if thus reduced Observations are to be compared with the Tables, it is yet once necessary to know the Constant of Aberration, viz., for reducing the apparent place to the true, or vice versa. Here the influence of an error may not be omitted because it generally acts in one direction. It accordingly will be proper to present the mean result of every group of Observations without subducing (!) from it a supposed value of the Aberration; for the sake of convenience two Logarithms may be exhibited, which, being added to the Log. of the Constant of Aberration will give the required corrections for Longitude and Latitude.

The Influence of the Constant of Nutation on the Rightascension, viz.,

$$- 15".39537\sin\Omega + [6".68299\sin\alpha\sin\Omega - 8".97707\cos\alpha\cos\Omega]\text{tg}\delta$$

is composed of two parts, the first of which is common to all the heavenly objects; the second depending upon the place of the star or planet vanishes in the Aequator (!), and is of small amount if the object is near the Aequator. The future correction of this part may be omitted for reasons similar to those aledged (!) for the omission of the

correction of Aberration. The first part will be nearly without influence if the Longitude resulting from every group of Observations is reckoned not from the apparent, but from the mean aequinoctial (!) point.

The reduction of an observed Declination supposes as known, not only the Constants of Aberration and Nutation, but also the quantity of Refraction, which, though it is undoubtedly an Element of considerable difficulty, appears nevertheless to be settled at present with an approximation sufficient for the reduction of Observations made between Bradley's time and ours. Two Tables, one scarcely different from the other, have been the result of two highly complete sets of observations made expressly for the purpose; one about the middle of the last century, the other 70 years later; one with a Mural-quadrant, the other with a Meridian-circle; both affording every desirable control; both founded on a theory which leaves no doubt respecting the laws of the phaenomenon (!) and of the influence of barometrical and thermometrical variations.

It is not likely that the remaining error should be so great as to be really prejudicious (!) to the reduction of the afforesaid (!) Observations. Should it nevertheless appear desirable to represent a Result as not depending upon a certain Table of Refractions, it ought not to be overlooked that two undetermined quantities have an equal claim to our attention, viz. the Constant for the Normal-temperature for which the Table has been constructed and the expansion of the air produced by heat. The *absolute* height of the mercurial column of the Barometer may also be considered as dubious within the limits of nearly the same extent.

You know, dear Sir, that I have derived from Bradley's Observations all the Elements necessary for their reduction and that every-one of these Elements is in such a connexion with the other that it would be wrong to vary one of them without varying the other even in case the first variation should be a decided correction. The same being the case with my own Observations, both series, in my opinion, would be prejudiced by the application of Refractions different from mine.

Dr. Maskelyne's Observations require too to be reduced by the same Table; not only because they are made at the same place and with the same instrument as Bradley's, but especially because this Table has been applied by Mr. Olufsen by whose elaborate inquiry into the errors of the Greenwich Quadrant, the observations have regained what they lost by the wearing out of the Instrument. − With respect to the introduction of Conditions relative to the Constants of Aberration and Nutation in the Reduction of Declinations I only remark that both will entirely disappear out of the final results exhibited in the form recommended above.

You will perceive, dear Sir, by what I have said, that, were I to superintend the business, I would prefer to exhibit the Results without complicating them by the introduction of a single undetermined quantity. But permit me to add a few words about the method by which a continually increasing approximation to the true values of the Elements of Reduction will be obtained. Some of these Elements have

been the subject of repeated inquiries, every-one of which has afforded a new determination somewhat different from others extant. Proceeding in this manner, and *rightly* combining the results of earlier inquiries with those of a later date, we shall undoubtedly arrive, in some future time, at every desirable degree of approximation.

This combination of different results must always be preceded by an *impartial* and *cautious* discussion of the *weight* of each of them; which discussion accordingly should be considered as an essential part of the inquiry. The want, or rather to [rather the] insufficiency of it, may probably have effected that the result of a later inquiry has sometimes been looked upon as excluding that of earlier ones while the same rightly combined only would have produced a slight variation.

− In the present state of our knowledge of the Elements of Reduction their yet admissible errors are so narrowly limited that further corrections can only be expected from long continued observations made expressly for the purpose. The nearer the approximation is, the more difficult will be a further correction, and the less probable will be the supposition that the Result of every new inquiry will approximate yet more to the truth; − continual oscillations within the limits of unavoidable imperfections are, on the contrary, agreeing with the very nature of Results derived from observations[3].

On the other hand, convenience and uniformity of the astronomical calculations are lost by continual changes, while no real advantage indemnifies for this loss. − My opinion of this matter is accordingly that, as soon as an Element is known with an approximation sufficient for the Reduction of Observations then extant, its value should be considered as *fixed* for practical use as long as either the observations will have acquired a degree of accuracy high enough to represent as desirable a further Correction of the Element, or as subsequent inquiries will have increased so much the *weight* of its determination, that a correction appears indubitable.

I shall now proceed to the second part of the business. A group of observations having been reduced, it is required to deduce from the same one mean place of the observed Planet. This will be done by the help of the Tables of the Planet and of the Sun; an Ephemeris for every day within the limits of the time, filled by the observations, being computed by these Tables, the Rightascensions and Declinations contained in the same compared to the observed place of the Planet will give the error of the Tables, deduced from every single observation. The mean of all considered as the error of the Tables for the mean Epoch and applied with a contrary sign to the place computed for the nearest day will give the required mean place representing the whole group of Observations. The Tables by the help of which this mean place has been obtained, disappearing entirely out of the ultimate result, the choice of these Tables is quite arbitrary; previous corrections of one or the other of their Elements will neither be necessary nor convenient.

− The observed mean place of the Planet may be reduced to geocentric Longitude and Latitude and the former related to the mean aequinoctial-point by the subtraction of the Nutation taken always

from the same Table which constantly has been employed in the whole computation.

In this manner the business will be brought to the genuine end. By the exhibition of the mean result of every group of observations these will be reduced to their concisest (!) form which afterwards will completely replace the Observations themselves and afford easy and sure means continually to correct our knowledge of the motions of the Planets.

Some trouble will be spared to those who will undertake this correction by the exhibition of the heliocentric places of the Planet and of the Earth corresponding *exactly* to the geocentric place of the former computed for the time to which every group of Observations has been reduced. In case of an Opposition of a superior Planet the addition of one step more will also be convenient, viz., the exhibition of the time and place of the Opposition together with the dependency of both upon the assumed place of the Earth.

I have nothing more to add respecting the arduous task you are ready to undertake. Believe me dear Sir

Königsberg Novb. the 9th 1833. Your F. W. Bessel

Notes

1. This is a reprint rather than a translation. This letter shows (as others could have also showed) that Bessel corresponded with foreign astronomers, and it reveals the level of his knowledge of English. He (1876, p. XIII) was only able to study this language for two or three months. The use of capital letters (Observations, Declination etc.) seems to have been outmoded. And he apparently copied such expressions as *Meridian-circle* from German.

2. The *quantity of refraction* can only be settled by a table computed for Greenwich, for the same time of day in which Bradley had been observing, and for the same meteorological conditions. Olbers, for example, in a letter to Bessel of 2 Nov. 1817, noted that the anomalies of refraction depended on the location of the observatory.

3. This conclusion has been shared by many, and perhaps by all observers, see Sheynin (1994, pp. 263 – 265). Even Bayes, in a letter of ca. 1756 (Dale 2003, pp. 263 – 265) , noted that systematic errors (as well as some dependence between observations!) prevent absolute precision. Then, Encke, Gerling and Bessel himself had applied the term *Grenze der Sicherheit* (boundaries of reliability) in the same sense (Sheynin 1994, p. 266).

Brief Information about Those Mentioned

Gerling Christian Ludwig, 1788 – 1864, astronomer, geodesist. Student of Gauss

Olufsen Christian Friedrich Rottboll, 1802 – 1855, astronomer

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IV

F. W. Bessel

On the Calculus of Probability

Über Wahrscheinlichkeits-Rechnung. *Populäre Vorlesungen.*
Hrsg. H. C. Schumacher. Hamburg, 1848, pp. 387 – 407

[1] Since I intend to talk to the respected Physical Society[1] about the calculus of probability, I ought to presume such an interest in this subject which will be characteristic of an exception to a rule easily derived by experience: neither a calculus, nor even its result is suitable for an oral presentation. And this is what I really believe in.

If some kind of a mathematical contemplation is often involved in the entire extent of our knowledge, of the occurrences in everyday life, it is the mathematical study of probability. Evidently, we are not used to consider many matters from this viewpoint, but it is not difficult to prove that the very laws which govern the games of dice are essential in the real world and that we are often pushed over when expecting it least of all.

Our knowledge is separated in two parts based respectively on certainty and probability. *Certain* is only that which we actually observe or is derived from such observations by a sequence of correct, mostly mathematical conclusions. On the contrary, *probable* is that which becomes known to us by testimony or consequences from observations whose correctness and explicitness cannot be rigorously justified.

[2] The first part is vast. It includes the entire kingdom of mathematical truths, an uncountable quantity of facts offered by nature and events occurring before our eyes. The second part is however also large. It includes all the forthcoming events in the essence of whose laws we are unable to penetrate. Also included here are facts indicated by history, the outcomes of a roll of a die and the destiny of nations.

In everyday life, much of what is only *probable* is usually called *certain*, although only in cases in which the probability is very high. That there was a man called Julius Caesar is called *certain* since it is confirmed by many trustworthy witnesses and by the connection of his life with other events. Dubious and even unlikely is that there had been seven Roman kings since in this case the witnesses are less trustworthy and moreover because the intervention of other events casts even more doubts.

But still our information about Caesar and those kings are *of the same kind*. Our knowledge only differs in the measure of its strength[2]. It is so precarious concerning the seven kings that we do not dare believe in their existence whereas the information about Caesar is so robust that any doubts seem unreasonable. Strictly speaking, however, his former existence is only *much more* likely than that of the kings. The doubt about him is not more unreasonable than the hope to extract at random one single white ball from an urn also containing many

millions black balls. That doubt is therefore not really unreasonable but only very weak. In ordinary life such doubts are completely ignored, but stronger doubts occur oftener.

So where is the boundary, the level of probability for two events both called certain[3]? Be it possible to determine this level we will be able to assign correct places to each event and numerically establish which event is *more probable*. In history, however, and in all matters which cannot be reduced to numerical relations, it is difficult to establish the amounts of probability. Historical events can be dated, but no other number denoting their probability can be assigned them.

On the contrary, there exist very many things whose probability can be measured, and I will say something about the means which may be applied for this. The entire theory of probability[4] rests on what is usually called chance. Will a tossed coin fall on one side or on the other? The outcome, as we say, is the effect of chance.

[3] After some thought we easily realize that the motion of the coin is determined by some cause; *arbitrariness* cannot govern it just like chance cannot compel Jupiter to fall on the Sun. However, we also notice that a smallest change of the toss suffices for a change of its outcome. That change is so tiny that our senses are unable to perceive it and the same happens with each following toss. We cannot bring about or foresee any definite outcome and for us, then, the fall of a coin is subject to *chance*. This example provides the sense which we attach to that word. We always mention chance when unable to assess how an effect is connected with a previous cause, when we do not understand it, when there are so many causes that we are unable to separate them one from another and follow them up to the effect.

Who wishes to see an explanatory example of the notion of chance need not go too far: *each* event which we cannot fathom either by calculation or other inferences *is called* a chance event. It loses this name as soon as we become able to connect it with its causes. A storm that darkens the Sun is called a chance event, but an eclipse of the Sun by the Moon is not; we really know the causes of the latter but not of the former. Previously, however, eclipses had also been called chance events. Many chance events so called today will lose this characteristic and it is generally clear that the entire notion of chance event is relative.

When Newton had begun to illuminate the world, much of the incomprehensible left the dark kingdom of chance. A new Newton will reveal the causes of other matters and we may *imagine* a mind for which only a little remains for the chance. I do not maintain that that mind can be human, but if mankind sheds light on all the darkness, a more serious previous study of chance will be very interesting. Only thus we will be able to judge about the certainty of the investigated events which result from unknown causes but obey, according to experience, some definite laws.

[4] We are not concerned about the causes of things supposedly governed by chance and their essence is of no consequence for us. We have therefore looked for means to judge the so-called chance *in general* so as to apply it somehow in each case. Such a means has been found in the comparison of chance with games of dice [with

games of chance] and Jakob Bernoulli was the first who, in his *Ars conjectandi*, in 1713, paved the way and prompted various later mathematical investigations. The great work of Laplace which appeared some years ago (vor einigen Jahren) combined all of them[5].

We can imagine a die with an arbitrary number of faces. Suppose that one of them is black, and the other ones white. Then, obviously, the larger is the number of faces, the lower will the probability be of the appearance of black. For two faces, one of them black, and the other one white, the probabilities of both outcomes are apparently the same, and we may reckon on the appearance of each to the same degree. A gambler who pays 2 talers each time black appears and gets only 1 taler for white, will certainly lose after a long game

On the contrary, a die with three or more faces oftener rests on white than on black and the appearance of white is certainly more probable. For two faces the probability of each outcome is 1/2; for three faces, the probabilities are 2/3 for white and 1/3 for black etc. For a die with 12 faces, 7 of them white and 5, black, those probabilities are 7/12 and 5/12. Playing with such a die and gaining 5 talers in case of white, I ought to lose 7 talers in the opposite case. If I pay less, I will probably win; if more, I will probably lose since there is no reason (or at least it is thus assumed in the calculus of probability) for one face to appear rather than the other[6].

A larger number of white faces will therefore result in an oftener appearance of white. All this determines the measure of probability. Probability 1/2 refers to *exactly balanced* things and can just as well result in the appearance of one or of the other event. It is thus possible to maintain that a thing having probability 1/2 is probable or improbable; those things whose probability is even a bit lower than 1/2 are called improbable, and those whose probability is a bit higher, probable. The larger is the deviation of the probability of a thing from 1/2 the less or the more probable it is.

So here we have the means to judge precisely the probability or improbability of an event. However, its application usually leads to serious, and often unsurmountable difficulties since we often do not have the data on which our judgement is dependent. As stated above, the probabilities of white and black for a die with 7 white faces and 5 black ones are 7/12 and 5/12. Roll such a die many thousand times, and then the ratio of the occurrences of these faces will be 7:5, and the nearer to it the larger is the number of the rolls. When we do not know how many white and black faces the die has [how large is their ratio] it can be derived [from the experiment]. The result will be the more reliable, the more rolls are made. And so, there are two means for discovering the number of faces of a die[7]: either count them, or observe the result of the rolling.

[5] I hope that the respected Physical Society will excuse me for discussing all this somewhat extensively, since it was indeed necessary for stressing the true point of contact of the calculus of probability with occurring events. When considering this in a more general setting, the unknown numbers of white and black faces represent the favourable and unfavourable causes of some event. When counting the occurrences of that event we obtain the number of

the cases in which it happened and did not happen. The derived ratio of the numbers of white and black faces will therefore be the ratio of the number of cases in which we ought to expect, and not to expect, an occurrence of that event.

Suppose that a hundred times the height of the barometer was lower by half an inch than in the mean [of very many other observations] and that during that time there were 60 storms. The probability of a storm in such cases is 6/10; a storm is therefore probable even if its connection with the height of the barometer were unknown. And we therefore conclude that during 10 such observations a storm should be expected 6 times.

That such *definite* indications of probabilities are interesting and useful is obvious since most discovered rules are justified not by *certain* success but by higher or lower probabilities. Imagine that you are a skipper who knows by experience that a storm leads to some damage worth 100 talers, say. Then, when he does not sail today, he pays 50 talers for the demurrage. The barometer fell 1/2 inches, so should he pay these 50 talers or ignore the danger of a storm? I think that a vote will be divided; some will prefer the doubtful danger to a sure loss, others will rather pay the 50 talers and prevent the loss of the 100 talers in an unfavourable case.

The latter opinion is reasonable; the probability of a storm is 6/10 so that in 10 cases occurring in similar circumstances 6 storms are expected which means the loss of 600 talers, or 60 talers in the mean. Understandably, it is reasonable to avoid it by paying the 50 talers.

There are very many rules which ought to be based on similar considerations, but the situation is usually judged by a more or less unreliable estimation. This happens partly because the true justification of judgements is not developed sufficiently clearly, and partly since people do not bother to compile properly, by measure and number, the facts which can be provided by experience. True, the principle that the probabilities of the connections of two events are derivable by counting the observed cases can be applied too extensively. Nevertheless, I think that it is necessary to turn the attention to the fact that this powerful source of knowledge is too often neglected in ordinary life and that therefore the probability or improbability of events is doubtful. At the same time, however, orderly observations, i. e., actual counts of the favourable and unfavourable cases, can show whether there exist adequate grounds for deciding one way or the other.

[6] Mathematicians[8] have made a very significant step forward by discovering a means for determining by calculation the reliability with which we may reckon on an event found probable by observation. This reliability obviously heightens with the number of the observed cases. When we roll only a hundred times the abovementioned die with 7 white and 5 black faces we will be much less certain about the possibility that the ratio of white and black outcomes is very near to 7:5 than after 1000, 10,000 or 100,000 rolls. However, it is possible to calculate how *reliable* is that ratio as derived from a 100, a 1000, … rolls. This reliability heightens so much that the boundaries of the

probable error[9] will soon become so near to each other that the derived ratio will not noticeably differ from the probability anymore.

Observations, whose reliability is clearly determined, can only be obtained by applying this theory. And only during the last years we have learned how to derive much use from it, and I will hardly be mistaken when supposing that after a sequence of years the first chapter of each textbook on science based upon experience will be devoted to the application of the calculus of probability to the art of observation[10]. The data for such applications will surely not be available at once since it is easy to show that much which is today called observation hardly deserves this name. However, new observations require time, often very much time. In medicine, national economy and in similar matters in which the general rule is essentially corrupted by numerous chance events, it will only later be possible to understand reliably what exactly should be obtained for trusting the observed result. Unreliable is much of what in usual life is thought to follow from experience and judged and justified by everyday occurrences but what still is completely wrong. Thus, everyone says that a full moon changes the weather and believes that he personally had made many suitable observations, but there does not exist anything so unjustified than that statement as proved by actual counts covering 50 years[11].

Another example of credulity in believing events allegedly justified by observations seems still much more remarkable. In St. Malo, where the range of the tides is uncommonly large, it was generally agreed that deaths only occurred at the time of ebb. Over the centuries, it was possible to check this striking phenomenon whose existence was, however, never doubted. Finally, the Paris Academy of Sciences had sent a committee to check this remarkable fact on the spot. It occurred that people had been dying both at ebb and high tide and that, according to church registers, neither ebb nor high tide had during a hundred years influenced mortality.

I consider these examples quite remarkable. It is not necessary to go too far for finding similar and more important matters. Had people been led by the calculus of probability and invariably applied observations, we would have known that much of the believed was groundless. Moreover, in spite of hosts of chance occasions, we would have discerned many rules still completely unknown since they are not so clearly seen and do not manifest themselves.

[7] What I have told here in general, had already found very interesting applications to astronomical observations and investigations. Suppose, for example, that the zenith distance of a star is measured. The result will not be the desired magnitude, but invariably its approximate value. The more perfect is the instrument, the more attentive and able is the astronomer, the better will that approximation be. Still, we never arrive at a true value, since the instrument is always somewhat imperfect and since there are other imperfections caused by our senses even made more sensitive by most powerful magnification of the instruments. Then, the vibration of the air, [insufficient] illumination of the graduations [of the circle] and

uncountable possible small causes whose influence we are unable to calculate.

All this is revealed by the observations: repeat today's work tomorrow, and its result will be a bit different, and the day after tomorrow, different once more. At the time of the forefathers of astronomy such differences amounted to half a degree, at Tycho's time, a few minutes. Now, having such aids[12] as those in my observatory, it is possible to reckon with considerable reliability that observations made today and tomorrow will not differ more than by a second.

In spite of such precision, I, just as Tycho, cannot maintain that my observation provides the truth. I am nevertheless looking for the truth, so which of the two observations should I prefer? Obviously, both are equally wrong since there is no reason why one of them should be chosen. We therefore take the mean of all those made, and this rule can be rigorously justified, although the great Lambert had objected to it[13].

What we thus obtain is still not the truth, since it deviates from the truth by an unknown magnitude which probably is the smaller the larger is the number of observations and the more perfect are the aids. It is clearly seen without any calculations that a series of observations with larger and oftener deviations from their mean provides a less reliable result than another series with narrower boundaries of such deviations. Furthermore, the calculus of probability offers a means for more definitely determining that reliability. It shows how the worth of the observations should be established through those same deviations, it provides the boundaries within which an error is as probable as beyond them. [The distance between] these boundaries is called the *probable error* of an observation. Only it gives us the means to weigh one series of observations, and its result against another one, again with its result. According to this viewpoint, we do not anymore discuss *true* astronomical determinations, we only look for the *probable* and find out to which of the various determinations of the same thing we may assign the highest probability and which is therefore the best one.

When following these considerations further, we are led along the proper way to much more difficult cases in which, for example, we assess not the observations themselves, but the results of their entire series. Thus, for example, the path of a heavenly body is determined by three complete observations; when a hundred is made, the path of that body can be determined by any three of them. Since observations only approximate the truth, we obviously only arrive at an approximate path, and, furthermore, at a different one each time when a new set of three observations is chosen. So which path should we choose?

[8] The answer to this question is offered by the calculus of probability. It teaches us that among the possible uncountable (?) paths we ought to discover the one which has the highest probability. That calculus leaves no room for arbitrariness. Previously, before that theory [that calculus] was developed, the computer had to be satisfied to choose, in accord with his prudence and ability, a path more or less

conforming to the observations. Nowadays, he has complete power over choosing quite methodically the *best* path derivable from the observations. Moreover, he will be criticized if not arriving at the very best to which he could have freely approached.

In the first case, he thus certainly strengthens his reputation for ability, but not when he acts otherwise even if he manages to keep very near to observations. The *astronomer* will thus lose as much as *astronomy* wins, and we should not doubt that, owing to this invention (?), observations acquire quite different weights[14] and astronomy can advance more in *one* year than formerly in a decade.

It can be proved that a derivation of a result which should be preferred to any other based on the same observations as well as the determination of the uncertainty of its probability is always possible. However, it is not sufficient to prove that we have determined the most probable result from the available series of observations. Indeed, it does not follow that that result is probable per se. It can certainly deviate from the truth so that the most probable boundaries of that deviation should be provided for us to see clearly the measure of reliability.

Suppose that someone determined that the orbital period of a comet is 100 years with a probable uncertainty of 1/4 of a year, and that someone else determined that that period was 102 years with a probable error of 1 year. The choice between these determinations is not arbitrary anymore: the first one should unquestionably be preferred[15]. For example, among my first applications of such reasoning was my conclusion that the Olbers comet will most probably next appear on the 9th of February 1887 with a probable error of 101 days. The period during which its new occurrence should be expected can thus be immediately estimated.

Without such considerations the uncertainty of its occurrence would have been measured by many years, and anyone was then able to recognize openly a new investigation (?). Now, however, it is possible to derive a definite result from the available observations and any differing one will be worse. It is therefore obvious how reliable and stable became astronomy through the application of the calculus of probability.

[9] What happens with everything new had indeed happened to the applications of the calculus of probability. Many of those who had not penetrated into its spirit believe that it is unnecessary or even strange. Delambre, in his *Astronomie*[16], stated much of ill-considered about it and its English reviewers allowed themselves to mock at some Continental astronomers who had now been determining cometary orbits, the figure of the Earth, the distance of the Sun and whatever else according to *probability* rather than *truth*.

We may easily tolerate all that and would have a good reason to thank very much these English reviewers for teaching us how to determine the true cometary orbits etc. Indeed, we ought to be only satisfied with probability when denied the truth. Nothing else has been done nor could have been done. Yes, we have often called true what was only probable and even not the most highly probable. But, on the

other hand, no one ever thought of proving the Pythagorean proposition by probabilities since it can be, and was proved certainly.

I have somewhat extensively dealt with the application of that (?) reasoning to astronomy but would have rather considered other sciences more closely connected with everyday life. However, those sciences are not yet completely cultivated[17], and, in addition, I myself am too little informed about other things and do not venture into any such investigation. Nevertheless, any person tending to contemplate will have sufficient possibilities to note that what I said about astronomy was only an example and that the same, even if in another form, is true everywhere else.

Each science which passes from experience to theory begins with observations and learns from the calculus of probability how to observe and apply the observations and, finally, how to construct *the most probable* theory. In astronomy, for example, practice is a problem of that calculus, and theory, a problem of higher mechanics[18]. 150 years previously it was different, no one thought of either, but what science had amounted to in those times as compared with today? A chaos of phenomena[19], whereas nowadays they comprise a coherent whole whose separate parts are most closely connected by the mentioned (?) strong ties.

It is indeed instructive to consider the course which science had taken until our time. It did not at all arrive at knowledge by issuing from prior systems as it possibly was attempted in other fields[20]. On the contrary, it had invariably asked the observations for advice and was always on guard against admitting something not following from them into its propositions. And it certainly had arrived at its aims not by leaps but by slowest and surest steps. I wish all experimental sciences to proceed by such thoughtful steps, and I hope that the calculus of probability will soon provide them such an audible proper rhythm that any deviation from the proper course will offend both eye and ear.

Notes

1. Bessel actively participated in the work of the Physical section of the Königsberg Physical – Economic Society.

2. Bessel did not mention moral certainty which was discussed by Jakob Bernoulli but introduced into science much earlier (Sheynin 2009a, §§ 2.1.2, 2.2.2 and 3.2.2).

3. One of those events was apparently certain, but the other one only probable.

4. Only here did Bessel mention the *theory of probability*, in all other cases it was *calculus of probability*.

5. *Some years ago*: not less than nine (see Note 16). Then, Laplace had included the theory of probability into applied mathematics whereas his predecessors had regarded it as a branch of pure science. Again, Bessel had not mentioned Laplace's *Essai philosophique* … of 1814.

6. Only assumed as the very first approximation and even so, not always. Then, in information theory, probability 1/2 (see below) means complete ignorance.

7. Not the number of the faces, but the appropriate ratio.

8. Yes, mathematicians, beginning with Jakob Bernoulli, and for any chance event rather than for probable events.

9. Bessel only defined the probable error in § 7. In 1816, he himself introduced it into probability theory.

10. Bessel invariably mentions the calculus of probability instead of the theory of errors. Unlike Laplace or Gauss, he himself (1820, p. 166) picked up that term from Lambert.

11. Those *actual counts* are extremely dubious, see Sheynin (1984b, § 2) to which I am now adding that Lambert had studied that problem and Daniel Bernoulli urged him to go on with his investigation (Radelet de Grave et al 1979, p. 62). Bernoulli remarked that the possible influence of the Moon on the atmosphere can be revealed since its distance varies if only it influences the air the same way as the sea. However, the elasticity of the air and its insignificant inertia ought to be allowed for.

12. By aids Bessel meant astronomical instruments.

13. Lambert (1760, § 303) introduced the principle of maximum likelihood (although not the term itself) for continuous densities, but thought (§ 305) that the maximum likelihood estimator usually did not essentially differ from the arithmetic mean. The translator of Lambert's book excluded those sections from its German translation.

14. This is dubious. Weights of observations are not changed owing to calculations.

15. A superficial statement. First, Bessel completely ignored systematic errors; second, natural scientists hardly ever followed such simple indications, see especially Sheynin (2002).

16. Delambre published investigations of ancient, medieval and contemporary astronomy in 1817, 1819 and 1821 respectively, and, in 1827, an investigation of astronomy of the 18th century. According to the context of Bessel's lecture, he thought about Delambre's book of 1821 or 1827 which means that Bessel read his lecture not before 1821.

17. But is any science *completely* cultivated? In any case, Bessel should have mentioned medical, if not meteorological statistics and certainly population statistics.

18. Why *higher* rather than *celestial* mechanics? He himself (1876, written about 1846) described his own study of Laplace's *Mécanique Céleste*.

19. How about Kepler?

20. Bessel could have mentioned astrology hardly justified by observations but recognized, for example, by that same Kepler.

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V

F. W. Bessel

On measures and weights in general
and on the Prussian measure of length in particular[1].

Über Maß und Gewicht im allgemeinen
und das Preußische Längenmaß in besonderen.
In author's *Populäre Vorlesungen über wissenschaftliche Gegenstände*.
Hrsg. H. C. Schumacher. Hamburg, 1848, pp. 269 – 325.

[1] When a magnitude is measured, its ratio to another one is determined and it is this ratio that exhaustively describes the former if the latter, or the measure, is known. Such a description is indeed the aim of the measurement. When the measured magnitude is a line, a flat surface, a body or a weight, their measures are, again, a line, a flat surface, a body or a weight. And if we agree to choose the same measure in all similar cases, all of them will be understandable.

Each society recognized the need to adopt a *certain* measure for each of the four cases of measurements, and no level of culture had apparently ever been low enough to manage without such measures. In previous times, *arbitrariness* in the choice of measures coupled with the limited nature of social ties led to the introduction of different measures in each town and small region.

Many of such local measures had certainly disappeared with the expansion of those ties but the great number of the remaining can be estimated by the comparison of the Italian measures of the foot in the *Annuaire* of the French Bureau des Longitudes for 1832. It took into account, not completely, the measures applied in field measurements, but not in commerce, and still, 215 measures were listed[2].

The introduction of a certain measure is obviously the more successful, the more extensive becomes its region of application. The ties between neighbouring smaller societies had been inconvenient and difficult because of their different measures, and this circumstance must have been noticed very long ago. Nevertheless, those measures had hardly been often unified since apparently there always appeared some complications. A change of the existing measures invariably required changes in many appropriate customs, agreements and laws.

No society, for which a unification of measures was desirable, had therefore resolved to burden itself of its own free will. Moreover, that process was difficult; it was never possible to estimate whether the assumed benefit for the local ties will not disappear because of the losses for the external relations. Owing to these causes the differences between local measures had lasted for a long time even after the formation of a single country and only ended after legislation aiming at the common good abolished them.

That process apparently went on gradually with the introduction of separate generally valid regulations about, for example, the levying of taxes. The final goal, a complete unification of the measures in all

parts of a country, was already attained in most of the large European countries whereas the other ones have been approaching it.

During its revolutionary years, France had even attempted to introduce a single measure for all the civilized nations. The intended success was not achieved but some neighbouring countries had adopted the French measure[3].

However, a definite determination of a measure ought to precede its general introduction. The yearning for maintaining the existing order will be the least if the most commonly applied measure is chosen as the general and more elevated standard. Such a choice will be difficult to doubt, but, in itself, it does not secure the required definite determination of a measure. If the name of the measure is retained, some uncertainty will surely occur because of the imperfection of its initial embodiment and errors in its extant copies.

If that uncertainty is moderate and does not essentially harm commerce or industry, any such established measure ought to be considered of equal weight. Nothing new will thus be introduced but the uncertainty will not be preserved (and increased).

If an initial measure was established five hundred or a thousand years ago, its uncertainly could have ever more increased with time. Reversing this process will at least be contrary to the intention of changing the existing measures as less as possible. In addition, the initial measure will be rarely found if at all, and even when discovered the aim of its establishment and the state of the mechanical art in previous times will allow us to believe that it was prepared very roughly so that its uncertainty was not contained within a narrow interval.

[2] Copies, more perfectly prepared later, will perhaps provide more definiteness, but the uncertainty of the initial measure will persist. If measures of each of the four types of the measured magnitudes are established, then a lesser number of embodied measures will be needed. All the planes should be measured by a measure of length, and each method of measurement will depend on the application of this measure. The establishment of the measure of a (restricted) plane invariably depends on the measure of length; any other embodiment will be inapplicable.

It is otherwise with the measure of three-dimensional bodies although they can often be, and actually are measured by the measure of length. Indeed, in other cases, for example, concerning liquids, measurements can be made much easier when a certain vessel is chosen as a measure. Its capacity can be measured by a measure of length, by the foot, say, and will be expressed in cubic foot or in a certain part of a cubic foot. However, it can just as well be defined independently from the measure of length and it was thus defined at least in all cases which became known to us from previous times. And the measure of weight is itself a weight.

And so, three measures are needed, those of length, of liquids and grain, and weights. Their embodiments are necessary and serve as the base for establishing any system of measures which becomes quite definite when those embodiments exclude any ambiguity, becomes

invariable when resisting all the influences of time. Then it conforms to its intention the better the more accessible are its initial units.

Each type of measurement is traced back to the testimony of our senses and cannot therefore be completely precise. The degree of the attained approximation to the real values depends on the applied thoroughness and its assurance by more or less appropriate aids. It immediately follows that it is easier to measure less precisely rather than more precisely. In everyday life the highest precision is never achieved; for example, achieved not higher than is ensured by our senses without their artificial sharpening. It can really be quite indifferent whether a new house is larger or smaller than by $1/10,000^{th}$ of its intended size, or if the relative error of a load reaches 1/10,000.

It is therefore wasteful to develop the means of measurement as much as possible and thus to hinder usual work. Such attempts will only result in applying measures of unneeded precision. Bricklayers and carpenters will reasonably complain if ordered to apply, instead of roughly produced but satisfactory for their work wooden measures of the foot, a more thoroughly made expensive measure made of better material and precise to a hair's breadth.

However, we may also imagine measurements whose significance heightens with precision. They prompt us to bring the precision of the methods of measurement and of the applied measures up to the highest possible level by the most powerful sharpening of our senses. When such measurements are carried out not in everyday life, but only owing to scientific requirements, it is necessary that neither the applied measure, nor its embodiment leaves even a tiniest ambiguity.

A measurement only remains significant as long as the measure on which it was based is preserved. Inversely, a measure only achieves weight and significance through measurements depending on it. As long as the bricklayer and carpenter are measuring with a foot, it does not really matter whether that measure is quite defined or somewhat ambiguous, whether it remains quite invariable or changes its length with time by a few ten thousandths.

[3] The need for a reliable determination of a unit for measuring lengths became felt in France in 1734 when two meridian arc measurements were planned, one of them near the equator to be carried out by Bouguer and Condamine, the other, at the polar circle, by Maupertuis. Two identical copies of the toise, iron bars whose ends marked the distance, were produced. From that time, they had been considered the unit of the French measure of length. That unit was chosen to coincide with the generally applied measure of the same name so precisely, that the existing small differences will not be noticeable, that the thus newly introduced measure will not disrupt handicraft or industry.

One of those toises was later damaged in a shipwreck; the other one which had been applied near the equator, in Peru, and called the *Peru toise*, was safely brought back to Paris. Its length at 13° Réaumur became the unit of the French system of length. It was divided into 6 foot or 72 inches or 864 lines. As long as that original of the toise is preserved, or its length can be reproduced by copies, the significance of the result of the equatorial meridian arc measurement retains its full

significance but loses it as soon as that measure is lost. Means have therefore been devised for ensuring essential reliability of preserving the toise of Peru and for removing the causes of its damage. Until now, both aims have been attained.

In England, already the *Magna Charta* [1215] stipulated that the same measure ought to be applied in the entire kingdom. The measure of length is the yard. A brass bar produced at the time of Queen Elizabeth [I] and preserved at the Exchequer was preferred to the older, probably from the time of Henry VII, and preserved at the same place. It is considered as the standard yard and used for comparisons with the other yards which acquired a legal status by stamping.

However, these regulations proved so unsuccessful that the Parliament often had to turn its attention to measures and weights. A document prepared by Francis Baily, who was busy with producing a measuring bar for the Royal Astronomical Society, shows that gradually more than 200 laws having to do with measures had been introduced without eliminating essential uncertainty even in usual measurements. An investigation ordered in 1758 established that the ends of the yard kept at the Exchequer were neither flat nor parallel to each other and that therefore were not marking any definite measure of length. It also occurred that the other public standard of length preserved at the London Guildhall deviated from its stipulated length by 1/25 of an inch or by 1/900[4]. Many other legally recognized standards kept in different places of the kingdom essentially differed one from another.

The committee of the House of Commons which carried out this investigation determined the cause of this confusion that crept in the entire business of producing measures and weights: their manufacturers had often been unqualified and the means for checking their work were insufficient. For improving the situation mechanic Bird was asked to produce two brass bars with a cross-section an inch square and the length of the yard to be marked on a side of each by driven golden pins. Bird earned a good reputation by producing a mural quadrant for the Greenwich observatory and graduating it. Now, he recommended to the Parliament to preserve carefully one of those bars with *Standard Yard 1758* inscribed on it and to keep the other one at the Exchequer for common usage when checking copies of the yard.

During the next years a newly appointed committee [of the House of Commons] combined its proposals with those of the previous committee but recommended to produce a copy of the Standard Yard and preserve it on the premises of some public authority for use on special occasions. Such a copy was indeed produced in 1760, but the law whose text was compiled in accord with that proposal and twice read in the Parliament did not completely get through: the text was lost because of the prorogation of the Parliament.

The existing uncertainty in the true value of the yard lasted therefore unrelentingly. Only in 1814 the House of Commons once more appointed a commission and in 1824 a law established that the measure produced in 1760 with an inscription *Standard Yard 1760* in its present condition at 62° Fahrenheit was the true value of the yard.

And still the intended aim was not yet reached since an investigation of that yard, legally elevated to become its initial measure, made in 1834 by Baily, revealed that an unambiguous length cannot be got from it since both points [pins] determining it were not rounded nor did they have any other regular form, but were irregular to the highest extent.

This fact was explained not by their initial condition but by the damage of the points by various use made without proper precaution. The ensuing uncertainty was obviously not large enough for preventing applications of that standard in everyday life, but I have noted above that scientific use requires complete definiteness rather than a restriction of uncertainty within narrow bounds[5].

Scientific measurements have been made in England and its colonies, and I only mention the measurements of the length of a simple seconds pendulum, for which we are thankful to Kater, and of an arc of the meridian in England and of a much more extensive arc in India. General Roy had begun the first measurement and lieutenant colonel Mudge completed it. Colonel Lambton had begun the second measurement, and colonel Everest completed it. As far as I know, that arc will be extended to the north.

There was no legal and unambiguous measure so that whether just one measure had actually been applied can only be ascertained by privately owned and unused copies. When a completely unambiguous determination of the yard appears, it will become possible to compare the actually applied measures still existing and remaining in good condition with that yard and correct the concluded measurements. However, such later and always tentatively possible corrections, without which the essential effort and moneys will be more or less squandered, contradict the aims of an orderly system of measures.

I have dwelt on the history of the English measure of length since I consider it instructive. As a conclusion, I note that in 1824 the yard, elevated to the status of the initial measure, was lost when the building of the Parliament burned down. This, however, was not an unhappy event since the very first requirement of a measure, its complete definiteness, would have necessitated new investigations.

[4] I return now to the French legislation about measures. The revolution brought about an entirely new system of measures and weights, the so-called metric system introduced on 18 Germinal III by the law of the *National Convent*. It was entirely based on a new measure of length, the *meter*, and its multiplication and division by 10, the base of our number system, and its powers, 100, 1000, … A meter is the 1/10,000,000$^{\text{th}}$ part of a quadrant of the [Paris] meridian.

10, 100, 1000, 10,000 metres are called deca-, hecto-, kilo-, and myriametre respectively; 1/10, 1/100, 1/1000 of a meter, − deci-, centi- and millimetre. The unit of area, the are, is a decametre square; the unit of volume for wood, coal, etc., the stere, is a cube with meter square faces; the unit of liquids, the litre, a cube with decimetre square faces; the unit of weight, the gram, is the weight of pure water at its largest density (at about 4°C) filling a cube with centimetre square faces.

In a similar way, the multiples and the fractional parts of the are, ster, litre and gram were named. The monetary unit, the franc, weighed 5 gram, 9/10 of it silver and 1/10, copper, was divided into decimes and centimes. The day was just as well subjected to the decimal system: it had 10 hours, 100 minutes per hour, of 100 seconds each. The quadrant of a circle was divided into 100 grads, a grad contained 100 minutes of 100 seconds each. Even the calendar did not resist the revolution: it began with the vernal equinox and was divided into 12 months, each 30 days long, and 5 or 6 additional days[6].

As follows from the above, this system disregarded all the existing systems and chose its main measure not more or less arbitrary, as it happened previously, but in connection with some measurement of the Earth. The introduction of the metric system in another time would have presumably been more difficult. And it was introduced in spite of the thus certainly caused inconvenience for internal life. Still, only a part of the new names yielded to the previous designations.

The myriametre became lieue, and decametre, decimetre, centimetre, were replaced by perche [perch], palme [palm] and doigt [finger] respectively and these unchanged names of the previous measures thus obtained new values. I do not intend to investigate the difficulties caused by such a resolute change of the system of measures but I retain my viewpoint on its main idea, the replacement of an arbitrary measure by a so-called natural measure.

The metric system has two inherent features which we are not obliged to consider essentially connected with each other: the unit of the system was linked to the size of the Earth, and it was divided into decimal parts. Such a division generally shortens calculations, but at the same time introduces a disadvantage: the fractions 1/12, 1/6, 1/3, … cannot be precisely expressed as they are in the often encountered duodecimal system. That advantage would have been more essential had it been more difficult to decimalize those fractions.

Other systems of measures sometimes apply the decimal multiplication and division, but in this respect they are unequal to the metric system which applies the same procedures throughout. The decimal division of the day and of the quadrant of the circle had not for a long time replaced the usual system as was applied in France; for that matter, the division of the day, as it seems, had never been inculcated in the general public.

The idea of natural measure was not new; even Huygens, in the mid-17th century, recommended the length of the simple seconds pendulum as the measure of length. His proposal had been repeatedly supported and discussed during the introduction of the metric system but had to give way to the choice of the 1/10,000,000th part of the quadrant of the [Paris] meridian. The metric system was the first to *realize actually* the idea of a natural measure, and, moreover, so comprehensively and with such consequences that the partisans of that idea should have been completely satisfied. However, we will consider that idea from various sides and will only then be able to express our friendly or hostile opinion about it.

[5] *Each* measure is obviously equally easy and reliably applied for measurements since it only serves for establishing the *ratio* of two

magnitudes of the same kind. It does not acquire any advantage even when the ratio of the measured distance between two points on the surface of the Earth to the length of the entire quadrant of the meridian is expressed by a decimal fraction and becomes directly known.

Still less (?) desirable is a direct expression in decimal fractions of the ratio of areas, volumes or weights to the square or cube whose sides/edges are equal to the length of a quadrant of the meridian, or, in case of weights, to the weight of water contained in that cube. And so, there is no advantage either with respect to simplicity or reliability when applying one or another measure or in the form in which a measure directly provides the result of measurement. An advantage of a measure can only be justified by its *greater invariability*.

With regard to this property a measure offered by nature itself is unquestionably more advantageous than any other. So the question which I intend to discuss is, whether the metric system actually possesses or can possess that advantage to which its emergence seems to be due.

If nature produces a body which, in each of its occurrences, has the same size [one of whose dimensions has the same size], it will hardly be doubtful that, since the choice of a measure is arbitrary, that size will be thus chosen. And if all the sizes of that body are always the same, it will be a natural measure of volume. In addition, if that body always has the same density of its matter, its mass will provide a natural measure of weight.

However, we do not know any body which possesses all those three properties or even one of them, or such, by means of which we can directly measure or weigh. If, nevertheless, we wish to have a natural measure, we can only obtain it obliquely by deriving it from a measured object.

The length of a simple seconds pendulum can be such an object, and it recommends itself by its availability in any place on the Earth as well as by the relative ease of the operations required by its measurement. Its invariability depends on assuming a constancy of gravity at the point of measurement whose correctness was never doubted. True, new experience showing slow elevations of large parts of the Earth's surface compel us to question that assumption.

When wishing to choose that length as the base of a system of measurements, we ought to restrict its definition to a certain place, not even to a certain parallel since it is known that that length changes along them.

A quadrant of a meridian was preferred to the length of a pendulum since the latter's interpretation depends on time (on the period of oscillation of the pendulum) whereas the former is a measure of length without any further connections [complications]. The thus chosen measure becomes definite after a certain meridian is named; this is necessary since we are not convinced in that all meridians of the Earth are identical whereas the new meridian arc measurements decisively resist this assumption[7].

So which meridian we may choose not only as a measure, but as a natural measure? This can only be decided by measurements, but we never obtain any magnitude by measuring or observing it, we only get

to know it approximately. Therefore, measurement does not ensure the fulfilment of even the first requirement demanded from a measure, – the exclusion of any uncertainty.

[6] When introducing a certain measure corresponding to the results of measurement, – when introducing an embodiment of those results, – and adopting it for further usage, we thus sacrifice the natural measure. We will only get hold of a natural measure by measurement after learning how thus to reach a completely definite result. This, however, is impossible since each improvement of the methods of measurement only brings about a better approximation; imperfect possibilities of our senses will never lead to perfection[8].

Moreover, it is not only the inevitable imperfection of measurements that resists the attainment of a natural measure, be it the length of a pendulum or a quadrant of a meridian. The object of observation rarely and in this context even never, appears in its pure form. It is usually distorted by extraneous influences which should therefore be separated from direct observations before these will be able to provide the intended determination.

This requirement presumes a complete knowledge of everything that is entangled with the object of observation, but there are no means for becoming convinced in such knowledge. The history of the determination of the length of the simple seconds pendulum can illustrate this proposition.

Concerning the early, less satisfactory attempts to measure it, I will say without thinking too long, that Borda, one of the most astute experimenters of the previous century, had measured the length of the pendulum in Paris when the metric system was being introduced. He applied a method, whose elegance coupled with its masterly execution, allowed to believe that his measurement could have only minutely deviated from the true value. Later, however, Kater discovered another, no less witty method and superbly applied it for the same measurement in London. However, two causes influencing the results had escaped the keen perception of both.

They, the causes, could have, and had engendered errors which much exceeded the errors of observations proper. Laplace discovered one of those causes: the invariably insufficient sharpness of the edges on which the pendulum oscillates. He showed that the influence of this cause can be noticeable whereas previously it was disregarded.

The other cause manifested itself during measurement of the length of the pendulum in Königsberg. It occurred that the previously applied theory of the influence of the surrounding air overlooked an essential circumstance so that it was decided that that influence was twice less than actually[9].

The measurements themselves of Borda and Kater had been correct to about a few thousandths of a line, but those later discoveries revealed that their results were erroneous up to a few hundredths of a line. Now, the determination of the length of a pendulum can be freed from both those extraneous influences as well as from all the earlier known ones and no other causes of error had been discovered, but this fact does not convince us in their absence anymore than in the time of Borda.

Bearing in mind these remarks, it is easy to imagine the consequences of an immediate adoption by some country of the Huygens proposal to choose the length of a simple seconds pendulum as the unit of measure and to base on it a decisive revolution of its system of measures. That length would have been measured as perfectly as the art and aids of the time allowed it, and the derived magnitude fixed as the measure. Not long afterwards, after the discovery that the length of the pendulum increases with the distance of the place of observation from the equator (by about 21/4 lines after moving from the equator to a pole)[10], it will be noted that a measurement was only valid for that place and that the established measure did not possess the previously attributed property of being independent from the place of measurement.

This remark did not deprive the measure of being natural, but restricted it to a certain place. In addition, in Huygens' times the means of measuring the length of a pendulum had been so imperfect that an error of some tenths of a line was as probable as an error of some thousandths for Borda. If only a most favourable chance did not provide a correct early measurement, Borda would have shown that the previous result was not the intended natural measure.

Then, if the idea of natural measure was still upheld, the trust which Borda's splendid measurement inspired could have prompted to consider it as the discovered measure instead of the previously established. But the trust in the possession of a natural measure would have soon shattered: the later discovery of the two mentioned influences on the length of the pendulum would have compelled either to ignore these or to establish a measure anew. But only those will believe in its invariability, who cannot elevate their viewpoint from the condition of the experimental art existing in their time.

The illustrated variations of a natural measure derived from measurement ought to take place regardless of the measured object, ought to appear as well in the case of the meter derived from the quadrant of a meridian. Moreover, in this case the imperfection of the measurement is coupled with the indefiniteness of the measured object. It is impossible to measure a meridian from the equator to a pole, and since the knowledge of the figure of the meridian is lacking, a comparison of a measured arc with the quadrant of a meridian is impossible.

[7] There exists, however, a reason for the figure of the Earth on the whole probably to deviate inconsiderably from a spheroid formed by rotating an ellipse about its minor axis. Nevertheless, even if excluding from the existing arc measurements those that have lost their claim on reliability owing to the insufficiency of the means for their accomplishment or to other causes, the rest ten cannot at all be combined when assuming a spheroidal figure of the Earth. They indicate that the figure of the Earth is flattened in some places more, in other places, less.

The latest arc measurement in East Prussia made it probable that the actual figure of the Earth is to a regular surface approximately as the irregular surface of flowing water is to the surface of an even and calm

water. Separate deviations are therefore small and perhaps not exceed a few miles[11].

This nature of the figure of the Earth means that an arc measurement can only determine the curvature of a place of a body which does not possess any regular figure; that, in addition, any number of such measurements can only determine a spheroid as nearly situated to them taken together but certainly not to each place of the surface of the Earth.

Those irregularities in the figure of the Earth indeed engender indefiniteness of the lengths of the quadrant of its meridian. At least in the present condition of the astronomical art, this indefiniteness joined with the imperfection of the measurements by themselves is much larger than those to which the measurements of the length of a pendulum are liable. I think that they are ten times larger even when a measured arc of the meridian is only 100 miles long.

[8] The introduction of the meter led to the measurement of a great arc of the Paris meridian from Formentera[12] to Dunkerque more than 1/8 of its quadrant. This arc was advantageously situated: its middle latitude was almost 45° so that the derived length of a degree was very near to the eighth of the length of a mean degree or to 1/90 of the quadrant and almost independent from the flattening of the Earth. Its length was 57008.22 toises. Multiplied by 90, it provided the length of the quadrant and of the meter: $1/10,000,000^{th}$ of that was consequently 3 foot 11.296 lines, or 443.296 lines of the toise of Peru. This length was declared the legal length of the meter. A platinum bar was produced for its embodiment; at the temperature of melting ice its end surfaces should have marked that length, the length of the declared standard meter.

The above makes it clear that there are no grounds for believing that the thus established meter is the intended natural measure. The determination of the size and figure of the Earth will continue forever, its eagerness has increased and will compel us to abandon the aim of legally establishing the length of the meter as described above.

At present, we already have ten arc measurements, and all of them have an equal right in deriving the size and figure of the Earth[13]. I have found out their most probable result: the mean degree of the quadrant of a meridian is 57011.453 toises, about 31/4 toises longer than legally established. It follows that the length of an entire quadrant which we ought to regard now as its most probable value is not 10,000,000, but 10,000,000 and 565 metres. Its unavoidable variation, when keeping to the initial definition, i. e., to the meter being a $1/10,000,000^{th}$ part of a quadrant, will lead to internal contradiction: the fraction whose denominator differs from its numerator will still be unity. We should therefore abandon the initial definition and assume that the meter is established not by the length of a quadrant, but by its ratio to the toise. For a quadrant to be once more 10,000,000 metres long, the meter ought to be lengthened by 1/40 of the line.

However, for the new value of the meter to attain a weight greater than it had initially, we will have to sacrifice the unsuccessful idea of a natural measure. Indeed, it is impossible to doubt that each future arc measurement will again lead to another value of the meter. The

uncertainty remaining in its length after all the now existing arc measurements are coordinated is about the same as that, which follows from the change of the previous definition of the meter. It will decrease with the increase of the number of those measurements but no increase will be sufficient for it to disappear.

[9] I hope that by now my listeners are convinced in that it is impossible to possess a natural measure. I have remarked that its application for measurements cannot be easier or more reliable than in case of any other arbitrary measure. But if it is still doubtful that direct appearances of length measurements in the form of decimal fractions of the length of a quadrant cannot justify a preference of the meter to any other measure, I will add the following.

That doubt can be substantiated by easy calculations, but everyday life does not lead to them. In scientific measurements there occur instances when the knowledge of the ratio of a measured length to a quadrant is desirable, but calculations will be still necessary. Indeed, the adopted unity of that ratio, the desired round number of meters, is and will be lacking.

I believe to have some experience in scientific measurements and allow myself to indicate that I did not yet encounter a single instance in which the application of the French meter would have shortened calculations. All those, who recommended the introduction of a natural measure, attributed to it the advantage of its reconstruction in case of loss.

Actually, the knowledge of each previous measurement of a still existing magnitude leads back to the appropriate measure, but neither easier nor more reliably than it would have led to any other measure. The meter can be restored when knowing how many meters are contained in a quadrant, but not easier than any other measure given similar data. The described reconstruction of the meter can be supposed more reliable than the restoration of another measure only during the time when the tradition of the round number, ten million, still exists, whereas the tradition of a slightly less easily pronounced number disappeared. In other words, only during the time about which we assume beforehand, that the information on the present measurements is lost. I do not think that we ought to attach much significance to the period during which the knowledge of a measure was based on lost measurements.

I have shown that the so-called natural measure has no advantage over any other one either in the ease or reliability of its application to measurement, or in the form in which it represents the measurements, or in the ease or reliability of its reconstruction in case of loss. And since I do not know any other grounds for preferring it, I ought to decide that it really has no advantage over any other measure.

[10] For introducing a *real* natural measure we ought to refer anyone who requires a true measure not to its embodiment, but to nature itself. However, apart from today's impossibility of following this advice, its unavoidable consequence is that differing measures will become the bases for each new measurement and the errors of the measurement proper will be combined with the variations in the derivation of the measure. When desiring to illustrate this conclusion

by a definite example of a measure only defined by its relation to a quadrant of a meridian rather than by its embodiment, we may imagine, for example, two measurements of the length of a simple seconds pendulum, one made at the time when the meter was introduced, and the other one, nowadays. Even if they completely coincide, they actually still essentially differ by about 1/40 of the line. And later measurements, when the most probable length of a quadrant will be different again, and when complete coincidence still takes place, the length of the seconds pendulum will ever differ.

This occurrence too strongly contradicts the aim of introducing a measure as though it envisaged a direct definition of the meter by the quadrant. Since there are no advantages in introducing a measure of length having a definite relation to a length offered by nature, I ought to acknowledge as well that I cannot find any advantage in introducing measures of liquid or weight having simple relations to the cube of the unit of the measure of length and, respectively, to the weight of water filling that cube.

Measuring a liquid by the number of filled measures is much easier than geometrically measuring its volume, which is why only the former method of measuring is being applied. And it obviously makes no difference whether the measure is an easily pronounced part of the cube of the unit for measuring lengths or another part of it. For restoring the measure in case of loss it is certainly possible to measure geometrically its volume, but it will be just as possible if the measure were initially arbitrary or produced according to a certain intention.

More important than the measure for liquids and [its] more precise determination is the measure or unit of weight. I have mentioned that in the metric system this unit is the mass of the densest water filling a cube with faces 1 *cm* square. Later regulations in various countries stipulated that the unit of weight was dependent on the mass of water contained in a given volume.

However, none of these regulations have required that in each case the weight be derived from this interpretation. Suppose, for example, that a vessel is placed on a pan of a scales and water is poured in it and finally balances the scales with a weighted body on the second pan. So measure the volume of the water and calculate the weight of that body.

But still, those regulations require weighing by an embodying weight which is incomparably more expedient that referring to the explanation which accords with the business at hand not better than the introduced meter accorded with a quadrant. Neither do I doubt that, when, for example, a repeated and very precise weighing of a given volume of densest water provides a weight *differing* from the embodied weight, the appearing doubt will be resolved by preferring the latter[14]. In this case an interpretation referring to volume and water will be useless since anyway both interpretations more or less essentially contradict each other.

On this occasion I remark that the weighing of a given volume of densest water with a relative reliability of 1/10,000 is not at all easy and is probably not yet attained[15]. In addition, concerning the use of water it is possible to make a remark similar to that stated above about

the introduction of a length provided by nature for measuring lengths. And a restoration of a lost weight is as easy and reliable when turning to volume and water as it is when arbitrarily (?) weighing it by water.

Before I finally leave the problem of natural measure, I ought to say something else about reducing a magnitude given by a measure to that same measure. In each case it is obviously possible, if only that magnitude had not experienced any change after being measured. Its new measurement will express it through the same measure whereas the previous measure should now be considered unknown and thus the ratio of those two measures will be known. However, this ratio will not be always derived equally reliably but more reliable when the magnitudes can be measured by simpler and more precise methods; less reliable when measured by complicated and less precise methods or even when the measurement is more or less indefinite.

This statement can be interpreted by the measurement of a quadrant of a meridian, which requires extremely complicated operations. It was finally achieved by combining the measurements of meridian arcs situated in various geographical latitudes. The length of each arc, the pole altitudes of whose end points differ exactly by 1°, is only derived by many separate steps.

[11] The derivation of the length of a terrestrial arc first requires a measurement of a line [of a base] on the surface of the Earth which is the only operation in which a measure of length itself is applied. That line becomes a side of a triangle whose angles are measured by proper instruments and the other sides of the triangle are trigonometrically calculated. A second triangle is adjoined to the first one and its elements become known in a similar way, then a third triangle is added etc[16].

A chain of triangles extending from one point on the surface of the Earth to another on the same meridian is thus formed and the distance between them becomes known. To measure that distance directly without forming triangles will always be time-consuming and only possible if the measured line does not pass through hills or water [hardly ever possible].

The polar altitudes of both end points of the chain are measured astronomically and after comparing their difference with the now calculated distance it becomes known how long an arc should be to correspond exactly to 1° of latitude.

The thus concluded meridian arc measurements are the basis for our knowledge of the length of an entire quadrant. If these measurements should lead back to the applied measure, it will occur the more reliably the nearer in time is the actual application of that measure to the measurements from which the transition to the measure is done once more.

Most reliable the measure is again derived as long as the end points of the base are still preserved so that it can be measured anew. Less reliable, when those points have disappeared and we are therefore compelled to measure once more (?) another side of the triangulation. Indeed, in this case the uncertainty of the angle measurements are added to that of the base measurement. Still less reliable, when every point of the triangulation has disappeared and only the computed

length of a degree is left. The uncertainty of the astronomical observations is then also added. Although becoming ever smaller, it will always remain larger than the uncertainty of the other measurements[17]. Finally, least reliable, when only the length of a quadrant is left since it is only derived, and will be derived, from a combination of various arc measurements under the presumption of a regular figure of the meridian which is known to be only approximately true.

This description clearly shows how a measure is discovered the less satisfactory the further in time it is from the final result of measurement; how greatly impractical it is to issue from a later measurement as long as a previous is available. The preservation of a quadrant of a meridian is certainly less doubtful than that of the traces of those steps which led to the knowledge of its length. However, the great advantage in preserving those steps requires deliberation about means for achieving this goal to the highest possible extent. The most desirable is the preservation of the initial measure itself and then of its direct copies.

[12] I think that everything stated until now about studying measures should be sufficiently clear. I consider unjustified a preference of one measure over another and I only recognize one reason for replacing an existing measure: its replacement by a measure which will become more generally used.

On the contrary, I consider the fulfilment of three requirements essential. First, a measure should be *entirely unambiguous* so that each measurement based on it will only be uncertain due to its own imperfection rather than occasioned by an uncertainty of the measure. Then, the established measure will ensure the promised means among which a long-lasting construction of the standard itself is the only one which, provided that its intention was not inappropriate, ensures its unambiguity. The fulfilment of this requirement is aided by producing as precise and as long-lasting as possible copies kept in different places as well as by measurements based on the standard [on the established measure?]. Copies, however, restore the measure the less unambiguously the more complicated they are.

Finally, I regard essential that the establishment of a measure be accompanied by discovering means for producing its copies as perfectly and as easily as possible[18]. The fulfilment of these three requirements by each established measure with superb rigour, especially in the case of the measures of length and weight should be achieved if the art of investigating measures, without restricting it just to everyday needs, is to be put in order and preserved.

By now, I have entirely developed an opinion in accord with which I had tried, in 1835, to fulfil the instruction of the Royal Prussian government to regulate finally the Prussian measure of length. In 1816, a law was passed which declared that the length of the Prussian foot was a standard preserved at the Ministry for finance and trade. This standard was embodied by an iron bar a bit longer that 3 Prussian foot.

The length of 3 foot and its division into 36 inches and the division of the last inch into 12 lines was marked on that bar by strokes. Two

of them, located on one of the wide sides of the bar, perpendicularly intersected two parallel lines about 0.4 lines apart which ran along the entire length of the bar. The strokes marking inches were silver pins and those marking lines were on inlaid plates. The bar and its three copies to be preserved at appropriate places were produced by Pistor.

The intention formulated in the law which governed the work was, to produce a Prussian foot equal to 139.13 lines of the French foot so that the much more generally applied in Germany Rheinland foot will be as near [to the produced] as was possible by the existing uncertainty of the former. That law failed to ascertain some points which are required for an unambiguous description of the Prussian foot by its standard. It can be assumed doubtless that that foot is 1/3 of the distance between the end strokes of the scale as measured along the middle between the two parallel lines at temperature 16$1/4$°C which the toise of Peru ought to assume for being 6 French foot long.

On the contrary, I do not think that the third requirement, also unmentioned in the law, can remain without an unambiguous definition although its necessity became known already in 1816. And later Kater had indeed indicated that the bending of a bar on whose surface two points or strokes are made, and whose distance apart had to determine a measure of length should be much more carefully avoided than it was thought previously.

[13] The scale of points or strokes is not sufficient for achieving an unambiguous definition of a measure; it ought to be accompanied by an instruction establishing the condition in which the figure of the bar should be for representing the intended measure. The cause of this previously overlooked influence of the bending was that the middle line of the bar neither shortens nor lengthens, the location of its end surfaces perpendicular to that line does not change either, but the surface of the bar becomes either convex and it necessarily lengthens, or concave, then it shortens.

That influence on the bar with the same properties as our has, is so great that a playing card inserted between it and the plane on which it lies can already change the distance between its extreme strokes by many thousandths of a line. Even the bending caused by the bar's own weight when it rests on two points essentially changes this distance. My calculations showed that, when the bar rests on its ends, it shortens by 6$1/2$ thousandths of a line; that this shortening becomes smaller as the distance between the ends of the bar and its supports lengthened; and that the shortening disappears and becomes a lengthening when that distance is 7$3/4$ inches[19].

The lacking specification of the method of resting the bar during its application thus engenders an uncertainty about the existing definition of the Prussian foot which is larger than that which still remains when it is restored to its intended legal length. The latter uncertainty can be got rid of by a later legal arrangement, but not if the bar had permanently changed its length. This can easily occur as a result of an accident or by careless handling and it does not seem advisable to base the preservation of a standard on such shaky grounds.

Such uncertainty is peculiar to any similarly produced standard. It can be avoided if the definition of a measure depends on the distance

not between two points or strokes marked on its surface, but between its end planes. It will then not be difficult to produce such a rigid bar that neither its own weight nor an unintentionally preserved bending will actually change the distance between its end planes as measured along its unchanged middle line.

Such arrangements, the same as provided for the standards of the toise and the meter, are more suitable for their aim than the described above. Moreover, it has another no less essential advantage: the end points of a bar can be produced of such a hard matter and so reliably attached to it that their preservation will be incomparably better ensured than in case of necessarily very fine points or strokes on the surface of the bar. Again, equally precise copies of the standard can be produced much easier since a contact of planes can be achieved with an almost unlimited reliability exceeding the microscopic sight of the strokes.

These advantages of an endpoint measure leave no doubt in that the still necessary definite establishment of the Prussian foot should be attempted on such grounds rather than by a later assertion concerning a measure restricted by strokes. And it is necessary to continue to follow the legal intention of having the foot equal to 139.13 lines of the French foot[20].

[14] The new Prussian standard is a bar not anymore of iron, but of cast steel with a cross section 3/4 inch square. A bending exceeding the boundaries of elasticity of such a bar 3 foot long will require such an essential effort, that we should not at all fear its unintentional occurrence. Its end planes are frustum cones of reinforced sapphire whose longer bases are installed in the bar's interior and the shorter bases jut out a bit from their end planes. They are embedded in gold and the method of their fastening is such that the distance between their outer surfaces is protected against accidents which are possible during applications of the bar.

Their robust reliability also protects them against wear and damage and the gold protects them against rust. The distance between the outer surfaces of the sapphires along the axis of the bar at 16.°25C serves for determining *three Prussian foot*. An instruction about the method of supporting the bar during its applications is unnecessary since even its maximal shortening is insignificant and remains undetected by any measurements.

This bar was produced by Baumann in Berlin, and to this excellent master I am also thankful for all the other appliances which I used during my occupation with the Prussian measure of length. The aim of establishing the length of a measure determined by the distance between the sapphires of 3 foot or 417.39 French lines was achieved by applying suitable means to a thousandth of a line.

Great could have been the caution exercised in producing that measure, but it can be essentially increased during measurement. It is necessary to compare repeatedly and as precisely as possible the length of the bar expressed in the French measure with that standard. A series of such measurements showed that the bar was 417.38939 French lines long, by 0.00061 of those lines or by 0.00063 Prussian lines shorter than intended. Actually, it is really indifferent whether to

choose the established value of the Prussian foot or the still unknown value which will be a few ten thousandths shorter or longer. The length of the bar can therefore be declared exactly equal to 3 Prussian foot.

Chance can lead to this rather than to any other length approximate to within narrow boundaries, but this cannot be the reason for deviating from a pronounced intention. Remaining true to it, we gain the advantage of not daring without reason to disturb the clarity of law, and thus the bar was declared the basis of the Prussian measure of length:

The Standard of the Prussian Unit of Length, 1837

This bar at 16.°25C as measured along its axis is 0.00063 lines shorter than 3 foot.

The Royal Act of 10 March 1839 recognized it as the only one possessing that property[21]. And thus the Prussian foot was declared definitely and unambiguously. In accord with the above, its ratio to the French foot is

$$139.13:144 = 1:1.03500323 = 0.96618056$$

which allows to replace one of these measures by the other one. These measures had been compared with each other 48 times during 8 days. Their coincidence is so exact that from those 48 comparisons the mean error of the length of 3 foot was not larger than 1/4000 of a line, and the mean error of the mean result was only 1/27,000. The seventh significant digit of that ratio does not change even by one whole unit.

[15] In accord with the intention of this report any details may be left out, but I would like to hint in a few words why those measurements attained such a high precision which exceeded its usual boundaries. I mainly ascribe this fact to the avoidance of small differences of the temperature between the two compared measures which escaped notice by the thermometers. I have attained this goal by making all the measurements in a washtub filled with spirits of wine and immersing there both the measures and the appliance for measurement. Then, the latest arrangement was only founded on contacts of planes and all microscopic images were excluded. Also, the micrometer screws of that appliance were more rigorously investigated, and the appliance was faultlessly produced by Baumann, that talented artist, who invariably and willingly helped with everything.

The determination of the ratio of the two measures can be considered satisfactory indeed, but we must not forget that the applied French measure was not the toise of Peru, but its copy produced by Fortin in Paris and owned by the Königsberg observatory. Arago and Zahrtmann compared it with its original after which it acquired the greatest possible authenticity. The same length represented by that copy of the toise had been the basis for the measurement of the length

of the pendulum in Königsberg, Güldenstein[22] and Berlin as also for the arc measurement in Eastern Prussia [No. 322/135].

Two more equally authentic copies of the toise of Peru are kept in the rich collection of instruments of the state councillor Schumacher in Altona. I have compared them with the previously mentioned by means of the same Baumann appliance and found out that one of them also produced by Fortin was 0.0025 of a line longer, and the other one produced by Gambey 0.0049 of a line shorter.

It follows that the copies of the toise of Peru can be uncertain which is not really important for most applications, although often not to be considered insignificant. If the true value of the toise of Peru will be still more reliably known also abroad, the ratio of both measures will possibly change. As I have said, this remark refers to the Königsberg toise which can therefore become more reliably known by comparing it with the Prussian foot. After its legal establishment this will make no difference but I mentioned it so that it could be found out to what extent it can be related to the French measure with which many other measures had been compared and on which many scientific measurements are based.

The actual aim of my efforts concerning the Prussian measure is a systematic arrangement of rules which should lead along an easily understood way to the production of copies whose reliability satisfies even the most delicate scientific measurements. In my opinion, without following such rules the achievement of an unambiguous standard is impossible. I understand the importance of a precise measure as well as the previous difficulties or impossibility of obtaining it by issuing from too much experience, my own included, and I may therefore doubt that the rules directed to that aim which were got hold of in Prussia do not deserve attention.

An authentic copy of the Prussian measure ought to be a bar of lithe cast steel of which that measure was produced as well. Both have the same thickness and the same or almost the same length. Instead of the sapphire end planes fastened to the measure a copy is fitted with end planes of tempered steel[23]. After being firmly attached to the bar, they are ground and polished smoothly and are exactly perpendicular to the bar's axis. To prevent dust and rusting these end planes are covered by brass cylindrical caps pushed on the cylindrical ends of the bar which can be screwed or unscrewed from it.

Such a bar is being produced by Baumann. After its completion it will be compared with the measure and its length (at the temperature during the comparison) will be known in the Prussian measure. An inscription will be made:

(*The year.*) *This bar at temperature … as measured along the axis of its cylindrical ends is … lines longer/shorter than three Prussian foot*

This inscription will make it an authentic copy of the Prussian measure. For officially recognizing this fact it will be necessary to apply to the Royal Commission on Standards in Berlin and submit the

original comparison as stated on the inscription on the bar. The price of authentication is 60 Prussian talers.

[16] For estimating the advantage promised by these rules I ought to go into some detail about the comparison of a copy with the standard. It is done by means of an appliance equipped with two very delicate micrometers fixed on a mahogany prop together with a Repsold water level – probe[24]. The standard and the copy are brought in turn between those micrometers. Both bars are laid side by side on a trolley which can only move perpendicular to the line of the micrometers and only between two points, when the axis of either bar is brought on that line. The movement is stopped by a shock against the edges of the two screws each of which is situated in the intended position at each placement of the bars.

Consequently, the bars can very rapidly and without any supervision be brought one after another between the micrometers so that the influence of the observer's body warmth on them and on the appliance is decreased as much as possible. To exclude from the comparison of the bars the presumption of a completely correct position in the line of the micrometers it is necessary to repeat this procedure after turning them both around.

Each pair of comparisons made with changing some external circumstances required 15 minutes or somewhat more. The mean of the two comparisons, if only considering the errors of measurement, ensured a very near approximation seldom leaving an uncertainty of more than 2/10,000 of a line. However reliable is the appliance by itself and however delicate are its micrometers, these good qualities would have been barely beneficial if no means were found for ensuring a sufficiently equal temperature of both bars.

The difficulty of attaining this equality is only felt when the appliance is properly fitted out and very precise. A warming of a steel bar 3 foot long by (1/44)°C already changes its length by 1/10,000 of a line, and about (1/4)°C is required to change it by 1/1000 of a line. Therefore, if the measurement itself ensures a reliability of not less than 1/1000 of a line, it will hardly be difficult to equalize the temperatures of the bars and keep them equal. In this case, leaving them near each other for an hour will be sufficient and the proximity of the observer will not lead to any new difference between those temperatures. However, that procedure will be unsuccessful when the difference should be ten times less.

The different radiation of heat from or to the side of the room, opposite to that in which the appliance is held, generates, in my experience, much larger differences and the temperatures are equalized so slowly, that an occurrence of a new difference can be expected much more than that equalizing.

However, this difficulty can be eliminated, as was so successfully proved by my previous measurements, by immersing both bars in a liquid. True, the possibility of damaging the standard and/or the appliance will increase (although due care eliminates the danger). So, it was necessary to find a rule valid for an indefinitely long time.

In my opinion, it should impede an unfavourable influence of negligence or carelessness, and I had therefore thought of abandoning

the immersion of the bars in a liquid and of discovering another means. Obviously, it is now essential to produce copies of the same material and size as the standard and to process them the same way. Failing that, it will be impossible to keep both bars incessantly at the same temperature in spite of external disturbances and the never ceasing fluctuations of the temperature of the surrounding air.

I expected success by covering the appliance, that is, the micrometers, trolley and the bars, with a tight-fitting mahogany casing out of which only protruded the micrometers' heads and drums. That casing only had two openings for reading the thermometer which lay on the bars. However, when I experimented in my room, the relative lengths of the copy still fluctuated, often more than by 1/1000th of a line. A change of the placing of the appliance with respect to the window or the fireside, even after screening off the latter, did not help. Only when I moved the appliance into an unheated room in the basement of the observatory, carefully closed it and only entered from time to time for comparisons, did these comparisons occur according to my wish.

[17] None of the 14 full comparisons of a copy with the standard deviated from their mean by 2/10,000[th] of a line and only 4 deviated more than by 1/10,000[th]. And so a condition was found whose fulfilment is necessary for very reliable comparisons. To illustrate the size of 1/10,000[th] of a line, I indicate that it is about 1/300th of the mean thickness of a human [of a masculine?] hair.

The inscription on each copy shows its length in the Prussian measure at the temperature of its comparison with the standard rather than its directly measured deviation from it. For finding out that length it is necessary to know the length of the standard not only at its normal temperature (16.°25C), but at any other temperature, or its change with each of its degree.

So that nothing else can be desired in this respect, I produced my own appliance for determining the change in the length of the standard with temperature and found out that each degree centigrade changes it by 0.004375 of a Prussian line. If the owner of a copy assumes that its steel has the same coefficient of thermal expansion, he may use that result. However, it will be wrong for him to decide this beforehand, he should replace it in accord with his own experiments and find out all the means for applying the copy.

Copies of the Prussian measure have an essential advantage in that they had been directly compared with the standard rather than with an intermediate copy. Other countries, in which the study of measures is also regulated, had excluded their standards from usual applications and thus protected them from damage and wear. However, this seems to contradict their aim, and I have preferred to secure their unchanged long-lasting preservation by their proper construction. Actually, I do not see what can damage the sapphire end planes of a bar since there is no reason for them to come into contact with diamond, the only known harder body. And a steel bar 3/4th inch square cannot be permanently bent by careless handling. The method of preserving it if it is always duly covered, lays on the appliance for comparisons and

only once touched when turned about, decreases the danger of its damage by carelessness, and, in my opinion, eliminates it.

However, unavoidable accidents can always happen, and an additional protection against them is only possible by disseminating copies of good quality, and it is always desirable to preserve them in different places *without application*.

[18] After, owing to their proper construction and ordered efforts, the production of copies of good quality became obviously possible without presenting any difficulties, the standard and the appliance was brought from Königsberg to Berlin. Installed there in a best-appointed house and protected from fire in the best possible way, they were given over to the Royal Commission on Standards. They turned to Baumann, the same artist who had rendered such an excellent service to the entire business and should have been most deeply involved in the essence of all the equipment. They charged him with comparisons, and I cherish the hope that he will not experience anymore difficulties in satisfying the need that had been felt for a long time for a reliable measure of length. Even the most delicate scientific applications can, at least for the time being, be based on a measure whose three foot are uncertain not more than by 2/10,000[th] of a line, or whose unit's (?) uncertainty is less than 1/2,000,000[th] of a line. If, however, the necessary precision of measurement heightens, means will be found for satisfying them.

The simplest of all the measurements, the copying of a standard, can provide a reliability surpassing now, as it does, that of all the other contemporary measurements. With respect to the precision needed for any goal, the described rules have eliminated any uncertainty about the Prussian measure of length as well as about its copies. At the same time, the measures of length of two countries became identical. The Royal Danish government has established exactly the same length of their now adopted measure as that described above and, in addition, introduced completely similar rules of its dissemination by copies. I hope that state councillor Schumacher, who had directed and directs all this business, will soon inform us about its final completion. We will thus have standards of exactly equal precision in Copenhagen and Berlin.

Notes

1. Bessel published a paper of the same name [No. 344].

2. Italy only became a single country in 1870, but 215 measures of the foot, even 40 years previously, is difficult to imagine.

3. The goal of an international metric convention signed in Paris in 1875 was to ensure a unity of measures and to develop the metric system.

4. The yard was subdivided into 36 inches and its 1/900th part approximately equalled 1 *mm*.

5. But can complete definiteness be ever attained?

6. Instead of a simple indication of a leap year the new definition was not clearly connected with the year's number. The calendar was thus deprived of its decisive advantage. However, the new calendar was soon abandoned. F. W. B.

Abandoned in France and never introduced elsewhere. O. S.

7. Bessel [No.254/138] briefly mentioned Gauss' pertinent reasoning.

8. There are other causes as well, for example, the influence of external conditions, unavoidable in spite of Bessel's statement to the contrary a bit below.

9. See Bessel [No. 254/138].

10. First noticed by Richer in 1672, but Huygens hardly did not understand that that should be expected.

11. Zakatov (1950, § 49) indicated that in Helmert's opinion there are no general deviations of the geoid from a rotational ellipsoid, that somewhat earlier F. A. Sludsky had formulated a contrary statement, and that, finally, the existence of *great waves* of the geoid has been proved. It was J. B. Listing who only introduced the term *geoid* in 1873.

12. A city in an island in the Mediterranean Sea.

13. Bessel [No. 306/131] provided the results of these 10 measurements and indicated the most probable value of the mean degree of a quadrant of a meridian, the same as cited in his report. Then, however, he took into account the necessary correction of the French measurement, derived a new value of that degree and, moreover, added terms depending on the mean value of the degree of latitude. Note that Mendeleev (1868) did not indicate the uncertainty of the meter.

Bessel invariably determined most probable values whereas Gauss in 1823 abandoned them in favour of most plausible values. Then, Bessel calculated mean errors, actually having in mind mean square errors. I have seen the latter term in Maievsky (1870) and Chebyshev (1870).

Bessel forcefully declared that natural measures do not exist and indeed, in 1872 the International metric commission abandoned the *natural* meter and defined the meter as the length of the Borda bar. However, a natural measure was found in 1960 when the meter was defined in terms of the length of some light wave.

Gauss expressed his views on the same subject in a letter to Olbers of 8 Dec. 1817:

The outlook on the possibly general introduction of the French system of measures which I find very convenient is indeed interesting. I always willingly apply it and believe that everything or most of what was stated against its general introduction was based on prejudice. I think that serious inconvenience connected with the introduction of a natural system of measures will only occur with the most subtle measurements, for which we will need in addition some other standard. […] *Each arc measurement is directly or indirectly aimed at the determination of the metre. Expressing the length of the arc in metres means that the metre is the length of that piece of iron rather than 1:10,000,000 of the quarter of the meridian.* […] *Endless transformations* (Schwanken) *will follow.*

14. This seems to have happened with regard to the gram. Anyway, many later weighing led to somewhat different values of the weight of water without, however, redefining the gram. F. W. B.

15. Mendeleev (1895/1950) weighed a *definite* volume of water and indicated previous results, in particular those of A. Ya. Kupfer of 1841 but did not mention Bessel. According to his estimate (p. 106), the length of the (standard) meter is determined *comparatively easy* up to 1/200,000 or even 1/10,000,000 (cf. Bessel's estimate at the end of § 14) and the weighing of a kilogram, a hundred or a thousand times more precisely.

16. Elsewhere Bessel [No. 322/135, end of § 9) noted that the first base net introduced by Schwerd appeared in 1822. There also, he described the laying of the centres of triangulations, cf. below his considerations abut the preservation of the measurements in the field. In addition to triangles, braced quadrilaterals and centred figures can also be included in a chain of triangulation.

17. Never say either *always* or *never*! In the 20th century, when triangulation chains had been adjusted, bases and azimuths were not corrected. They were considered much more precise than angle measurements.

18. This requirement seems self-contradictory.

19. See [No. 317/119].

20. Pistor attained his aim so fully that I was unable to find reliably any supposed difference between his measure produced in 1816 and the French measure. In those measurements from which this (?) was concluded (woraus dieses hervorgegangen ist), the measure lay on a flat surface which could not have considerably differed from a plane. F. W. B.

21. It follows that Bessel had read his report not earlier than in 1839.

22. Güldenstein, a castle near Oldenburg in Holstein, Danemark.

23. Here and below, Bessel confused the present and the past tenses, but then it occurred that the bar *is being produced*.

24. A probe (Fühlhebel) measures deviations of conic and cylindrical bodies from a circular form. It was apparently connected with the water level.

Brief Information about Those Mentioned

Kupfer Adolph Yakovlevich, 1791 – 1865, physicist, chemist, metrologist. Fellow of the Royal Society

Baily Francis, 1774 – 1844, astronomer

Bird John, 1709 – 1776, astronomer, constructor of instruments

Borda Jean Charle, 1733 – 1799, physicist, geodesist

Everest Sir George, 1790 – 1866, geodesist, geographer

Fortin Jean Nicolas, 1750 – 1831, constructor of instruments

Gambey Henri-Prudence, 1787 – 1847, inventor, manufacturer of precise instruments

Kater Henry, 1777 – 1835, physicist, metrologist, astronomer

Lambton William, died in 1823, geodesist

Listing Johann Benedict, 1808 – 1882, mathematician, physicist

Mudge William, 1762 – 1820, geodesist

Pistor Carl Philipp Heinrich, 1778 – 1847, mechanician, inventor

Repsold Adolf, 1806 – 1871, constructor of instruments

Richer Jean, 1630 – 1696, astronomer

Roy William, 1726 – 1790, geodesist

Sludsky Fedor Alekseevich, 1841 – 1897, mechanician, geodesist

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VI

[K. T.] Anger

Recollections of the Life and Work of Bessel

Erinnerungen an Bessel's Leben und Wirken. Danzig [1846]

[1] The November issue of von Zach's *Monatliche Correspondenz* for 1804 carried the following letter written to him by Olbers:

My enclosure pleasures me much by introducing to you a young and very gifted astronomer. Still a very young man, Friedrich Wilhelm Bessel serves here [in Bremen] *in one of the best commercial houses. Pity that such a talent cannot be wholly occupied with astronomy*!

His discourse will give you, just as it gave me, a very high opinion about his abilities, knowledge and skill in calculation. If anything should be reproached, it is the expenditure of much more time and effort than Harriot's observations, although valuable, can deserve according to their essence. But since Bessel had undertaken this work, it should be published rather than lost. Perhaps you will decide to honour soon his discovery by rapidly giving it a place in your journal.

I very much wish my young friend such an encouragement. Now, we most exactly know what can be gleaned from Harriet's observations for the theory of that comet.

Von Zach published this letter together with the discourse [No. 1/1] in the abovementioned issue of his journal and added his own Note:

For his own pleasure, a young German described, competently and ably, what many salaried professional astronomers would have been proud to say, and should have said long ago, about an English professor, but preferred to consider such a difficult work unnecessary. Fifteen years ago, the eminent French astronomer Méchain won an academic prize for a similar perfect investigation of the equally notable comet of 1661. Bessel will not win any prize, although he deserves it. However, is an excellent and flattering testimony of an Olbers less worthy? We are not mistaken! Olbers' praise was justified by Bessel's work.

Bessel's memoir, which introduced him into the astronomical world, surprized everyone. A twenty-year-old youth, a worker of a practical trade, acquired knowledge all by himself. This predicted him a worthy place among the representatives of science, and we will see how he surpassed the wildest hopes.

[2] Friedrich Wilhelm Bessel was born in Minden, Prussia, on 22 July 1784. There, his father, a judicial councillor, was a senior civil servant, and his mother, a daughter of the parish priest Schrader in Rehme. A numerous family, three sons and six daughters, obliged the parents to be exceptionally thrifty, so that already early our Bessel was able to understand the need to enter on a career which promised a

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quick independence. After attending the local grammar school (Gymnasium) in his home town up to the middle classes, he quit it of his own intense accord when being only in his thirteen years and continued to be educated privately[1].

Bessel came to Bremen while being in his fifteenth year and went to work at *A. G. Kulenkamp und Sohne* to learn commerce. No one apparently suspected that that apprentice, who took up his place in the office (Comptoir-pulte) on 2 Jan. 1799, will not only grace an academic chair, but reach the highest level in the kingdom of a science. However, he soon showed that he did not belong to ordinary people. After learning the mechanism of business life, he attempted to study its essential interrelations. These efforts proved so successful that he soon won the complete contentment and approval of the boss the more so since even during his first year his work and prudence proved very useful for business.

However, his regular work, which Bessel always fulfilled with utmost devotion, soon ceased to satisfy Bessel's yearning for activity. The office became too narrow for him, and his spirit which was destined for investigating the space of the system of the world[2], craved to come out in the great world. A travel to French and Spanish colonies and China as a freighter (cargadeur) during one of the intended Hanseatic expeditions soon became his most cherished wish.

It was in accord with his personality to prepare methodically his intentions. He began to devote his leisure time to study modern languages and, for being occupied during the voyages, navigation. In those times, the usual manual for studying the astronomical part of navigation was Hamilton Moore (1791) later superseded by Norie. It is known, however, how even now such manuals are compiled: problem – rule – example with not a word about proofs which have been considered as unnecessary luxuries since the student had to be trained rather than educated.

Bessel, however, was not satisfied so easy and tried to justify the correctness of the astronomical instructions. Had mathematics not been completely unknown to him, he would have soon searched for the solution of the problems in the only proper direction.

The popular book on astronomy by Voigt at least indirectly helped him since he found there a reference to the excellent book of Bohnenberger (1795). There he saw that mathematical knowledge was necessary for penetrating astronomy, and this discovery led him to pure mathematics. He delved into that science with tireless eagerness and mostly by reflection came to the point at which the scales fell from his eyes and the astronomical science began to shine in all its lustre and grandeur.

Just to provide one example how he had to go further all by himself, I mention Bessel's first acquaintance with spherical trigonometry. He found its main formulas without understanding their meaning, even without realizing that they concerned a sphere and began to derive them on a plane. He had not arrived at any satisfactory result and surmised to apply them for the three-dimensional space. Only this idea was necessary for him to cope with such an important part of pure mathematics.

When later recommending Bohnenberger (1795) to young mathematicians (?), he apparently recalled his own experience. However, Bessel was not at all satisfied with Bohnenberger's description of methods for determining time and applied his instructions for producing, with the help of a joiner and watchmaker, a wooden sextant and restored an old pendulum clock to working condition.

[3] Nevertheless, work, not leading to a definite result and thus only remaining an exercise, provided him nothing. He, as he himself put it, always should have got something useful from any work done. And so it suddenly happened even when he first determined time with those imperfect aids. Indeed, the determination of time should be applied for determining longitudes, and Bohnenberger's book contained pertinent instructions. So Bessel followed them and issued from his own observations. Indeed, he was able to observe an occultation of a bright star after which he had nothing more urgent than to derive the longitude of Bremen, and, to his joy, his result was very near the known value.

Thus Bessel became acquainted with practical astronomy and became his own teacher and examiner. Such success was destined to inspire him with noble self-confidence, and now he irrepressibly rushed ahead. Not being afraid of the thorny path, his spirit only acquired new vigour, and the more, the greater were the obstacles. The proverbial saying, *per aspera ad astra* (to the stars through hardships) came true.

Driven by the desire to apply invariably the acquired knowledge for the good of science, Bessel carried out burdensome astronomical calculations, and, when it became necessary, enriched the theory. After his abovementioned paper about the Halley comet (?), there soon followed a theoretical work [No. 3/3] which rectified the then painfully felt requirement and left behind the preliminary efforts of Euler and Laplace (eines Euler und Laplace). Then, as Olbers stated [source not mentioned], he calculated the path of the first comet of 1805 during four short hours:

On 1 November, at 8 o'clock in the evening, I sent him both my observations made on Oct. 29 and 31 of that comet and both of the earlier ones made in Paris on Oct. 19 and 20 and asked him to calculate on occasion its path since I myself had no time for it. My note did not find him at home, but he surprised me by coming at 8 o'clock of the following morning with the calculated elements of the cometary orbit for which he had only the time from 10 p. m. to 2 a. m. I can now say with pleasure that our Bessel is now completely won over to astronomy. He quit his commercial profession and came to Schröter in Lilienthal instead of Harding, and this is really a great acquisition for science. I have not yet met such a keen, diligent, insistent and patient genius.

[4] A new period in the life of that remarkable man had begun. For seven years Bessel had been devotedly occupied in a profession remote from science, but now, finally, he is not compelled to restrict his scientific pursuits to night hours. Schröter's activities had been rather directed to that part of astronomy which we may call physical,

since he was more interested in the physical state of the heavenly bodies rather than in their motion in space.

He should have all the more welcomed an astronomer who was an expert in carrying out everything that had to do with determining places in the sky, with managing by properly handling insufficient aids and nimbly calculating everything. Restrictions mean greatness[3]! Bessel kept his own astronomical diaries; observed comets and the new [the minor] planets with a circular micrometer; and, as previously, conducted scientific studies. His insight into investigating instrumental errors and their influence on calculations is surprising. And he always remained his own teacher since he himself comprehensively studied each problem without turning to his predecessors[4].

I can only mention a few of Bessel's works of that period. They include his precise and diligent observations of the comet of 1807 and the calculation of the elements of its orbit; then, the paper on the figure of Saturn which took into account the attraction of its ring. Thus he became closely acquainted with celestial mechanics which proved so useful in his later investigations.

However, aside from the results which Bessel made known as an observer, calculator and theoretician, he busied himself with a new basis for, and a glorious expansion of astronomy. His astronomical work already showed him how inadequate were the foundations on which rested the aids for calculating observations. The places of stars and the elements of the reductions of those places had not been as reliable as was necessary for more precise observations of planets and comets after due separation of the instrumental errors and the unavoidable errors of observation[5]. And the tireless Bessel decided to accomplish a great work, to compile a new star catalogue based on the observations of James Bradley, the greatest observer of his time, and to calculate new elements for calculations [of the reductions].

However, he kept silent about this plan and only made it known after two years. He finally succeeded after continuing this work with characteristic persistence. Bessel's contribution [No. 130] contained the fruit of these immense calculations, and all the European astronomers accepted his gift with surprise and gratitude.

[5] While in Lilienthal, Bessel's circumstances had not been too good, and reality with its various troubles did not imperceptibly pass by. But still his spirit remained cheery. The holy zeal for science which had been living in his heart could not be weakened by the cold world beyond. Again, being used to moderation, he did not require much, and his witty and shrewd reviews provided him with readily paid royalties. Bessel had thus found a means preventing sudden difficulties.

Those same years when the placid occupations of a scientist who thought of conquering the world of the spirit were continuing, the flames of war have been ravaging a large part of the world [of Europe]. The conqueror disturbed Europe and Prussia had also been strongly affected. The times of Friedrich II seemed never to return, and his life was apparently spent for nothing.

Fainthearted and calculating pygmies! You wish to determine beforehand the future of nations, you believe, that since your bodies are shackled, that your spirit is also disgraced? But no oppressor can destroy moral strength; at most, at most, he is only able to screen the world from it. Friedrich Wilhelm III understood the need to give the noblest to his people. When storms are raging out there, men ought to build a world for themselves and thus to become invigorated for defying the tempests.

Supported by excellent councillors, he encouraged in various ways, within tight boundaries of the land still left to him, even those sciences which did not directly influence the needs of life but freshened up the spirit and brought divinity nearer. Astronomy should have also been worthily represented and a temple of Urania[6] built in the former capital of Prussia [in Königsberg].

[6] Wilhelm von Humboldt, that regrettably already extinct star of a Dioscuri[7], understood Bessel's merit, and the good monarch appointed him as Professor of astronomy at Königsberg University. In 1810, Bessel left Lilienthal. Here [in Königsberg], although being for a long time busy with calculating Bradley's observations, he completed his work about the comet of 1807. What was until then unheard of, he took into account its perturbations and precisely calculated its path. The same year there appeared his celebrated work about this comet which won him the Lalande prize.

And now began Bessel's activities in various directions. He spent much time in preparations for the building of the observatory. His clear and lively lectures at the university soon won him a large number of listeners, the more so since he had not restricted those lectures to astronomy but included pure mathematics. And thus Bessel acquired general recognition as a scientist and respect and love in the close circle of his young students. For 35 long years he remained a benevolent teacher and friend of a large number of students. Among them, there possibly was no one at all not sincerely thankful to him for assistance of some kind.

Soon Bessel saw students from abroad. Thune came from Denmark and successfully helped him with the calculations of the star catalogue (§ 4). He possessed a rare ability to instruct young student astronomers by entrusting them with astronomical work in which he himself invariably participated. He thus stimulated interest in science and the desire to extend knowledge whereas the excitement of arriving at a final result protected the students from drowsiness.

Other university instructors of related branches of science later, successfully, as far as I know, adopted his excellent method. And it ought to be stated here that the instruction, which a large number of listeners obtained during the lectures by many oral and written questions and tasks, became lively and infused with a rare charm.

[7] We will soon see how Bessel began his activities in the observatory, but first we ought to describe his private life. Being set up in a carefree position by monarchical favour, he found himself, in his new home city, a marriage partner in the daughter of the medical officer of health and Professor Hagen. During a long time she had with sincerest love shared with him grief and joy, and finally, when

Bessel's death dissolved their union, she paid him her last respects with a gentle hand.

Whoever had an occasion to see the great man in his cosy domesticity should have imagined a pleasant picture of the cordiality of his really happy family life. Such pictures are imprinted in memory and are often recalled after a long time in their blaze of colour and delightfulness. Christmas celebrations were a special feast. Well in advance, Bessel was seen buying all kinds of presents for his children and other dear relations, or making them diligently himself, as though carrying out a scientific research, lovingly and painstakingly.

Who would have been able to describe his boundless joy which shone in his eyes when he surprised others by his presents! Neither did the numerous and far extended family, to which he belonged, fail to prove its love and attachment to him. Boundless good-heartiness coupled with a high scientific culture characterized that man, greatly respected not only in Königsberg, and we are gladly acknowledging that he soon became a member of a fine union of families whose doyen was his father-in-law.

Whenever Bessel's observations and calculations allowed it, his heart reached out for his family. The tender father was glad to see the development of his children. Although previously music had been somehow unpleasant to him, he reconciled himself with it all the more since it should have been an element of education. Bessel cherished the hope that his eldest son Wilhelm will sometime follow the path along which he himself had come so far. Actually, he was glad to notice that, even as a boy, Wilhelm had a strong inclination to mechanics, and thought that it was a favourable sign of his future, since a certain skill in practical things was a very desirable quality for an astronomer. And when Bessel saw that this inclination, had strengthened still more, he bought Wilhelm a complete lathe. And since he thought that each game should lead to a useful result, it was very pleasant for him that Wilhelm decided to produce a pendulum clock all by himself. He achieved it to his own and his father's great joy and Bessel became delighted by comparing the rates of that clock and the main clock of the observatory and announcing a justified opinion about his son's work. Regrettably, we will see that Bessel's wish did not come true because of the intervention of the relentless fate.

[8] In due time Bessel transformed the vacant lot near the observatory into a friendly garden and experienced how the trees which he himself had planted offered him a shade during a hot day. He was often seen there assiduously working with a spade, but it was also pleasant for him to discuss astronomical subjects with his students, answer their questions and allow them to inform him about the results of their work.

Those who had not been close to Bessel could often ascribe his displeasure, which he did not always conceal, when they disturbed him, to his bad mood. Actually, however, this only occurred since he had to interrupt his work. Visitors who had reason to see him often felt whether they came at the proper time or not by the tone of his *Come in*! However, he did not become invariably vexed since any

information about a happy surmount of a difficulty or even about an end of a long calculation, was able to drive away his frustration. We mention this small circumstance since it shows how Bessel participated in the scientific attempts of his students. Indeed, such a man cannot sacrifice more than his time during which he had to interrupt his own work for helping beginners.

Bessel's attractive nature won him respect of wider circles of society as well. He never had any enemies[8], he readily acknowledged worthy attempts and achievements even belonging to alien areas of knowledge[9], and willingly discussed alien items earnestly and wittingly defending his point of view if his opponent did not agree with him. Bessel's deductions did not depend on personalities and could never insult his listeners but often gladdened them even if they kept to very different opinions. Indeed, it became clear at once that he did not reason in the spirit of contradiction but expressed his really deep conviction.

By a stroke of great luck Bessel was very healthy but soon would have succumbed to the unheard of tension had he reasonably not strengthened it by regular walk. In addition, he loved hunting which often pleased him and he rubbed shoulders with people whose interests could have essentially differed from his own. Contacts with businessmen were not unpleasant for him since he was well acquainted with commerce, and an exchange of opinions was therefore possible.

[9] Bessel was really lively, even in his later years. He conducted everything zealously so that the building of the observatory presented him with many difficulties and obstacles and took to heart their rapid elimination. Thus, soon after his arrival in Königsberg he wrote to Bode [source not indicated]:

The news about the departure of the [ordered] *instruments from Rostock was exceptionally pleasant for me. I hope that they will soon arrive and that the building of the observatory will soon begin. Difficulties still occur all the time. They will not persist for a long time anymore, but will be able to paralyze our activities.*

When recalling that that building fell on the period of 1811 – 1813, the appearance of those difficulties will become clear. It was told that in 1812, while visiting Königsberg, Napoleon rode on horseback around the city wall, noticed that construction cite and asked about its aim. Upon finding out that not a log cabin, but an observatory was being built, he exclaimed in wonder:

My God, so has the King of Prussia still time to think about such things at present?

The calculation of the Bradley observations led Bessel to desire for obtaining a similar series of fundamental observations, and now there occurred a possibility of achieving this. His observations at the Königsberg observatory are naturally separated into four periods. The first covers those made by a Dollond transit instrument and a Cary circle. The second period, observations with a Reichenbach meridian circle; the third, with an Utzschneider – Fraunhofer heliometer, and finally, the fourth period, with a Repsold meridian circle.

First observed at the new observatory on 12 Nov. 1813 was the passage of the Pole Star by the transit instrument, and the first volume of observations, of which 20 are now published, appeared in 1815. Here is Bessel's Introduction to that first volume:

Whereas circumstances apparently made it impossible for Prussia to encourage essentially science, the observatory had nevertheless appeared, and astronomers are now receiving the first part of its observations. It was built in 1811, 1812 and 1813 so as to obtain a new auxiliary source for science and no effort was spared for wholly achieving this aim. Owing to the really royal generosity, all the difficulties, though apparently insurmountable, had been overcome. The observatory is therefore a worthy and glorious achievement, a memorial to the spirit that became dominant in Prussia even in these times. Let this institution blossom for a long time; it bears fruit and it reminds our grandchildren of the good deed of our great king and they will be thankful to him. From now onwards, science has a justified claim on the observatory which it ought to satisfy, and will satisfy. Indeed, it is impossible to change the intention of its donation.

It will certainly dawn on everyone that now, more than three decades later, these words prophetically stated, and dared to be stated by the great man, had so thoroughly come true. But thanks also go to him, without whose assistance nothing could have been achieved, since, wonderfully revealing himself, he had lent power to the man, and thus enabled him to attain such enormous success.

[10] Above, we have indicated that Bessel had previously understood that the elements underlying astronomical calculations were imperfectly known, and that he had decided, just as he always did when having the necessary means, to attempt to determine these elements as reliably as was required by science. A good example was the prize of the Berlin academy for Bessel's contribution [No. 104/37].

Because of its great importance, the rotation of the Earth around the Sun became the subject with which Bessel dealt especially thoroughly, and very many of his astronomical activities should be seen as preliminaries to a more precise study of this motion. Not only a new, but a better theory of the Sun seemed to be so desirable to him, that he spared no effort for successfully snatching it from heaven. The precise knowledge of that motion is already necessary because of its influence on the applications of all observations of the planets and comets. Indeed, even the best in themselves observations of these celestial bodies cannot lead to any quite reliable results if at any moment the place of the observer in space is not most clearly known.

But this was not the only circumstance which prompted our Bessel to devote his best efforts over a long sequence of years to accomplish the indicated great goal. He realized that the Newton law of gravitation, according to which the attraction of bodies in space is proportional to their masses, was not the only one[10]. On the contrary, without excluding another kind of attraction, it only belongs to various possible premises. This discovery was very important for the entire physical astronomy and for convincing Bessel himself whether the deviations of the elements of the Earth's path from observations can

be eliminated by improving these elements or not, – that is, whether the Newton law is really a law of nature.

A new calculation of those elements, based on numerous and precise observations, was necessary. Bessel provided future calculators with all the auxiliary means which can lead to the study of that great problem. In addition, he satisfied for a long time the requirements of practical astronomy by improving the Carlini tables of the Sun.

[11] Bessel's observations of the Sun by the Cary circle and the Dollond transit instrument, which lasted for five years, were so precise that they could be applied when investigating anew the abovementioned element[11] which is connected with the errors of graduation of the circles of those instruments. Apart from the fundamental stars and planets he diligently observed stars which had a rapid proper motion, and even with those imperfect aids he got quite useful results.

His wholly original investigation of the errors of graduation of those circles led Bessel to a formula which proved very useful in meteorology and was invariably applied there[12].

In 1820 Bessel began to observe with a Reichenbach, and a Fraunhofer meridian circles. He had most thoroughly investigated the errors of these instruments of unequal perfection and in general he ought to be considered the creator of the new art of observation[13]. Apart from the usual observations of the fundamental stars, the Sun and the planets, he began an immense enterprise, the observation of weak stars down to their ninth magnitude in the belt between – 15° of south and 45° of north declination. He believed that that extremely burdensome work apart from its usefulness will enable to extend systematically the search for new planets. Actually, the star catalogue of the Berlin Academy of Sciences based on those observations made possible the discovery of Astraea[14].

On 19 Aug. 1821 Bessel observed the first zone of that belt, and on 21 Jan. 1833, its last zone. In all, Bessel made 75,011 observations during 536 sessions[15] but he left the belt from 45° north declination to the pole for other observers working in observatories equipped by suitable instruments. Astronomers thankfully acknowledged the great advantage provided by that work.

[12] Still, the realization of such an extensive work did not keep Bessel away from other scientific pursuits. During that same period he calculated the *Tabulae Regiomontanaea* [No. 248] and in 1816 and 1827 determined by numerous observations the length of the simple seconds pendulum[16]. To achieve this purpose Bessel invented an apparatus which eliminated the possible uncertainty about both the middle point of the motion of the pendulum and that length. He observed the period of oscillation of two pendulums the difference between whose lengths was not measured but made equal to the toise of Peru. According to his indications, the eminent mechanic Repsold, who is known to have perished in a fire in Hamburg, had produced an excellent apparatus.

The result of this work proved even more important than its beginning allowed to expect. It occurred that the method of

calculating the influence of the resistance of the surrounding air as applied since the time of Newton was inadequate. Indeed, Bessel noticed that the motive force experienced by a body acts not only on its mass, but in addition on all the particles set in motion, and therefore on the moving particles of the liquid [Flüssigkeit; of the moisture (Feuchtigkeit)?]. By issuing from this circumstance, Bessel based a new theory of calculating the air resistance in case of such experiments[17]. The contribution [No. 219] describing all these investigations appeared in 1828.

Bessel applied the same apparatus for solving the important problem of whether the attraction of terrestrial objects by the Earth was proportional to their masses and repeated Newton's experiments (?) with his auxiliary aids of differing precision. To achieve his aim, he determined the length of a simple seconds pendulum with twelve various substances (gold, silver, lead, iron, zinc, brass, marble, clay, quartz, meteoric iron and stone). In each case he finally obtained a complete confirmation of the proposition that the length of such a pendulum only depends on the attraction of the Earth, but not on the properties of those substances[18].

Bessel also attempted to find out the grounds of our knowledge about the attraction of bodies both in our surroundings and space given the state of our art of observation and of our instruments. Indications, provided by the motions of some heavenly bodies about their incomplete compliance with the known general law of gravitation, provoked a question whether this law all by itself was sufficient for all those motions or are some of its still unknown modifications still necessary.

The experiment with those meteoritic substances of a possible extra-terrestrial origin[19] have perhaps indicated deviations [from that law] including one that can only provide a weak gleam of light on this problem whose interpretation even under most favourable circumstances remains for the attention of astronomers of the next century.

Just as the Keplerian laws had confirmed the Tychonian observations until Newton theoretically modified them whereas more perfect observations justified his work[20], a further step which will essentially supplement the laws (!) of attraction as known to us, is conceivable. Therefore, apart from their own interest, continuous precise observations of heaven are important since only they can extend our view of the true laws of nature.

Taking this into account, we see that Bessel's numerous worries about science obtain a centre in which they meet and a consistent method, which he always applied, and subjected the foundations to sharp criticisms and, if they were not sufficiently fine, laid down a new basis. It thus follows that his method was necessary.

[13] The large heliometer provided him a means to study double stars and to determine the distance to 61 Cygni about whose rapid proper motion he already knew[21]. This determination only occurred after many unsuccessful attempts made by other astronomers[22]. By applying that same instrument Bessel had also penetrated into the systems of Saturn and Jupiter. He began by determining the path of

the Huygens' satellites of Saturn[23], which he attempted 18 years ago when applying a small instrument, although not without obtaining a useful result. Bessel's investigation of the satellites of Jupiter which he observed very thoroughly was very interesting since until now (bis jetzt) their paths are actually unknown.

The precision of the observations with the heliometer was so high, that the meridian observations were unable to come up to them, and Bessel invariably and most diligently attempted to perfect them. The meridian circle produced by brothers Repsold, the sons of their eminent father[24], for the Königsberg observatory ensured at Bessel's hands the highest possible precision.

His discovery of the changes in the proper motion of stars[25] was an excellent result and we may hope that Bessel's observations, although not finished, will soon appear in a special volume edited by Busch in Königsberg and Petersen in Altona.

[14] Finally, we ought to mention Bessel's merit in geodesy and in establishing a Prussian unit of length. Long ago he succeeded to simplify as much as possible calculations of vast geodetic constructions and thus to heighten their precision[26]. The arc measurement in East Prussia, which he carried out with Major von Baeyer [No. 322/135], provided an opportunity for applying those results and became a specimen worthy of imitating.

The government of Prussia commissioned him to put into definitive order the standard of length. Accordingly, in 1835 he came to Berlin and determined there the length of a simple seconds pendulum by his Königsberg apparatus. He had done it in accordance with the desire of the Danish government to even out their own and the Prussian standards whereas other observers determined the length of the pendulum at Güldenstein by the same apparatus[27].

Bessel completely fulfilled the need, felt for a long time, for such a standard suitable even for scientific purposes. In 1839, the Ministry of Finance and Commerce published Bessel's pertinent contributions [NNo. 334; 335/150] and sent his copies of the standard to the Royal government [in Copenhagen?].

[15] It is impossible to mention all the works of the great astronomer or to provide comprehensive information about the ensued progress of science. History of astronomy will accomplish this goal much later than these sketchy pages wilt on the grave of the immortal man. I had only tried to draw a picture of his work insofar as it was necessary for showing how a great talent was coupled with a noblest character.

Bessel's excellent work should have been recognized abroad. Academies and other scientific societies diligently attempted to elect him. In 1820 Friedrich VI of Denmark decorated him with the Danebrog Order, and other monarchs soon followed suit. In 1824 Bessel was awarded the Red Adlerorder of the third class and the title of Privy State Councillor, and, finally, the ruling monarch awarded him the same order of the second class and the Order pour la mérite of the civil class.

The Paris academy conferred on him their highest scientific recognition. For a long time, he had been its corresponding member,

but then became its full member. These testimonies of respect gladdened him, but his happy family life quite especially assisted him by strengthening his scientific efforts and the cheerfulness of his spirit. Old age neared, and from 1839 his health had been shaky, so that this assistance became necessary for him to complete his tremendous investigations.

Bessel was greatly delighted by the marriage of his eldest daughter to his esteemed friend and previous student, Professor Adolph Erman from Berlin. His son Wilhelm whom we mentioned above displayed talent and inclination towards mathematical pursuits. Already in 1835 Bessel introduced him into the scientific world by an astronomical work on the Boguslavski comet[28]. However, the charming and modest young man did not want to devote himself wholly to science, although his highest achievements should have already been intensely expected.

[16] Wilhelm preferred to devote himself to construction industry (Baufache). Upon graduating from the Königsberg University he went to Berlin and entered a general Bauschule. According to the opinion of his instructors and other experts he had been excellently successful, but then, in 1840, soon after passing very fairly an examination for work superintendent (Bau-Kondukteur), he succumbed to a nervous fever. This unexpected blow hit Bessel like a lightning bolt from the blue.

Being in his 56th year, he had experienced many sorrows, but what did they matter as compared with the loss of seeing his only promising son snatched up from him[29]! The parents had been informed about the occurred illness by near relatives living in Berlin, but their last message left no anxiety about the worst. However, Wilhelm's condition suddenly aggravated, and when his death was mourned in Berlin, his parents in Königsberg began to hope once more.

It is difficult to describe the pain which seized the heart of the badly sagged father. But the noble man drew himself up and worthily endured the inevitable. Made happier by the love of his left dear relatives and reassured by the sympathy of the noblest men both in Germany and abroad, he sought and found consolation in those occupations which had been as necessary for his life as his breathing air. On 16 Jan. 1841 he wrote to a friend [neither name nor source mentioned]:

The loss of the only son, a son who had begun as honourably as I did … It is very difficult to endure it. But I ought to endure it. How gladly I would have given the few years still left to me for my dear Wilhelm to provide him 40 or 50 instead! Yes, in all probability they would have been happy years since I do not know what more should he have possessed. I try to distract myself by work and my condition has improved. My health remains rather good; I do not require much anymore since I got used to be content with little.

Bessel's nearest including his scientific surroundings provided most pleasurable consolation; such scientists as C. G. J. Jacobi and Neumann had been his most intimate friends, and we may therefore believe that he had good relations with scientists from other cities. Alexander von Humboldt had always been friendly to him, and corresponded with him over the years. He corresponded all the time

with Gauss, Encke, Struve, Schumacher and Olbers (whose decease he regrettably had to mourn), and he had the pleasure of seeing Humboldt as well as Encke, Struve and Schumacher at his observatory[30]. Bessel had been connected with Schumacher by true devotion and often stated that his friend won himself a high merit by editing the *Astronomische Nachrichten*. Actually, we may presume that without that periodical many Bessel's contributions would have remained unknown. Real chances of rapid publication had been very inviting for willingly and often going that way.

Bessel's high standing in science necessitated a very extensive correspondence with foreign astronomers. From far away, even from overseas, correspondents asked advices or sent their findings for getting his opinion. In previous years Bessel never expressed any desire for going abroad, and it rather seemed that he had been averse to that. Now, however, Bessel decided to go, presumably bearing in mind his health which became ever weaker[31].

Bessel was received everywhere with great honour. In Paris and London, being a member of the academies there, he was showered with expressions of honour. Upon returning home, he was gladdened by a pleasurable event, the marriage of his second daughter to consul Lorck, the son of a family with which his own had long been in friendly relations.

[17] Bessel's health became ever more anxious. In wintertime he developed pathological symptoms but hoped to get rid of them during spring by spa treatment and certainly felt himself better in summer. These symptoms returned in the winter of 1843/1844 and did not completely disappear in summer and he was obliged to stay away from many festivities during the jubilee of the University. In October he suffered so much that his doctor had to forbid him any intensive work, and he obediently complied.

From then onwards, he himself began to consider his condition dubious; indeed, in case of death, that same month he indicated some wishes in writing. On New Year's day Bessel's suffering essentially increased and already then his doctor Kosch understood its cause. All summer Bessel had to struggle with acute pain. Schumacher's visit provided him a pleasant spiritual excitement and the favour rendered him by His Majesty the King[32], who had sent him [for some time] his physician-in-ordinary, touched him so much that he became able to forget for some time the suffering of his body.

At the beginning of this year [1846] it seemed that Bessel had already more or less recovered, and once more a ray of hope warmed his heart. Information about the discovery of a new planet [Astrae, § 11] seemed to have revived him anew, and he was greatly pleased by the mark [by a new mark] of favour from the highly respected king. The king sat for his portrait and on 23 February informed Bessel in his own handwriting that he will soon send him that portrait. It came on 2 March and from that time until his death it became the cause of Bessel's pleasure, although from the end of February he did not dare leave his bed. On 7 March he dictated to his daughter a letter to Schumacher in which he expressed his fantastic joy at the present and

his great gratitude to the king. He wished to express his thanks himself, but was already too weak.

Bessel died on 17 March, at half past six in the evening. He remained completely conscious until the very end and expressed his joy at this circumstance to his spouse and daughter. During his last three days he changed very much. The pulse beat became hardly perceptible, he almost always drowsed [*remained completely conscious*?], his breath gradually weakened. Once he lifted his head and his last breath escaped from him. For a long time they still sat on his bed not daring to disturb the sacred peace by sound or movement.

His death occurred just as he always wished for himself. Foreign newspapers wrote that Bessel's doctors did not understand his illness which was not true at all. An autopsy wholly confirmed Dr Kosch' previous opinion: a spongy growth in the abdomen mechanically pressed the inner parts of his body and disturbed all its functions.

Bessel himself had time to forbid any ceremonies at his funeral. In the lecture hall of the observatory in which he had aroused to spiritual life hundreds of listeners now a coffin with his mortal remains had lain. Numerous orders with which monarchs had decorated him, as well as a laurel wreath presented by Europe (?) were displayed.

The funeral train went along the embankment running around the observatory to the churchyard nearby. His assistants, Dr Busch and Wichmann, carried the orders, national and foreign. All the city authorities had sent deputations, and huge numbers of people, even those little known to him, but having been fond of him, followed the coffin.

The location of his grave was chosen so that it was exactly opposite the observatory, and nothing hindered its view from the meridian hall.

Notes

1. The thrifty parents paid for his education, but did not pay anymore for his attendance at the grammar school (or was public education free?).

2. Bessel did not at all restrict his later investigations to the system of the world.

3. *In der Beschränkung zeigt sich erst der Meister* (Goethe).

4. Bessel did not turn to his predecessors likely because he had not known them.

5. Instrumental errors are unavoidable as well. Anyway, Anger should have mentioned random and systematic errors.

6. Urania, the muse of astronomy.

7. Dioskuroi: the twin sons of Zeus. In this case, the brothers Humboldt (not twins at all!). Wilhelm Humboldt followed the advice of Olbers and Tralles (Bruhns 1875, p. 562).

8. On Bessel's relations with Gauss and Encke see Biermann (1966) and Repsold (1920, pp. 194, 200 – 202) respectively.

9. Acknowledging someone's merits in an alien field is not difficult.

10. In § 12 Anger indicated that Bessel had even established that the motion of some heavenly bodies did not wholly comply with the Newtonian law. This is doubtful, but neither was John Herschel (1829) sure that that law was the only pertinent one. Then (see below), Newton's law would have remained a law of nature even when supplemented by another one.

11. Anger did not mention any single element.

12. The usage of thermometers and barometers hardly involves the application of any such formula.

13. Anger had not mentioned Gauss at all!

14. Astrae was discovered by K. L. Hencke in 1845.

15. Bessel had apparently reduced his observations as well. It is opportune to mention Newcomb (Benjamin 1910) who studied and treated more than 62 thousand observations (made and reduced by others) of the Sun and the planets which was always considered a great work.

16. The length of seconds pendulums depend on the place of observation.

17. The motion of pendulums is now observed in vacuum (Bomford 1952/1962, p. 344).

18. Bessel had apparently verified the constancy of the acceleration of gravity (and therefore of the length of a seconds pendulum). He then apparently had to produce 12 pendulums including two made from meteoritic substances, which is difficult to understand. And the application of the *auxiliary aids* (Anger) was not necessary at all.

19. In those times meteorites had not been considered as extra-terrestrial objects (Blazko 1947, p. 363).

20. Crass ignorance.

21. Anger hardly realized the great importance of that discovery.

22. Schumacher and Bessel himself (Repsold 1920, pp. 196 – 197).

23. Huygens had only discovered one satellite of Jupiter (Blazko 1947, p. 492).

24. Actually, the Repsold dynasty consisted of grandfather (perished in a fire in 1830), father, and grandson (born in 1838) whereas Bessel received that heliometer in 1829 (Engelmann 1876, p. XXVII, left column).

25. Bessel theoretically explained the irregularity by the existence of considerable dark masses in the neighbourhood of those (bright) bodies, which meant that both those irregular bodies were real double stars. Later observations by other astronomers confirmed his conclusion (Engelmann 1876, p. XXVIII, left column).

26. This is wrong, see Sheynin (2001c, pp. 171 – 172).

27. Güldenstein: a castle near Oldenburg in Holstein, Denmark. Other observers: see Note 22.

28. In his note Bessel [No. 284] stated that his son Wilhelm had participated in observing that comet and reduced some of Boguslavski's observations.

29. Bessel had two sons and three daughters, but only one son, Wilhelm, lived to become an adult (Engelmann 1876, pp. XXIX, right column and XXX, left column).

30. Concerning Encke see Note 8.

31. Bessel was invited to participate in a conference of the British Association for the Advancement of Science in Manchester (Repsold 1920, p. 210.

32. In § 5 Anger mentioned Friedrich Wilhelm III (who died in 1840). Now, it was Friedrich Wilhelm IV.

Brief Information about Those Mentioned

Baeyer Johann Jacob, 1794 – 1885, geodesist

Bode Johann Elert, 1747 – 1826, astronomer

Boguslawski Palm Heinrich Ludwig von, 1789 – 1851, astronomer

Busch August Ludwig, 1804 – 1855, astronomer

Dollond John, 1706 – 1751, optician

Encke Johann Franz, 1791 – 1865, astronomer

Erman Georg Adolf, 1806 – 1877, physicist, geophysicst

Fraunhofer Joseph von, 1787 – 1826, physicist

Harding Karl Ludwig, 1765 – 1834, astronomer

Harriot Thomas, 1560 – 1621, astronomer

Humboldt Wilhelm von, 1767 – 1835, philologist, philosopher, linguist, statesman

Jacobi Carl Gustav Jacob, 1804 – 1851, mathematician

Lalande Joseph Jerome François, 1732 – 1807, astronomer

Neumann Franz Ernst, 1798 – 1895, physicist

Petersen Adolph Cornelius, 1804 – 1854, astronomer

Reichenbach Georg Friedrich, 1771 – 1826, constructor of scientific instruments

Repsold Adolf, 1806 – 1871, constructor of scientific instruments

Repsold Johann Adolf, 1838 – 1919, constructor of scientific instruments.

Repsold Johann Georg, 1770 – 1830, constructor of scientific instruments, astronomer. Father of A. R., grandfather of J. A. R.

Schröter Johann Heronymus, 1745 – 1816, astronomer

Tralles Johann Georg, 1763 – 1822, mathematician, physicist

Utschneider Joseph, 1763 – 1840, engineer, businessman

Zach Franz Xaver von, 1754 – 1832, astronomer

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--- [No. 3/3] (1805), Über die Berechnung der wahren Anomalie in einer von der Parabel nicht sehr verschiedenen Bahn.

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--- [No. 248] (1830), *Tabula Regiomontanae*. Königsberg.

--- [No. 284] (1835), Beobachtungen und Elemente des Boguslavski'schen Kometen. Actually, Bessel's note was entitled Schreiben an den Herausgeber. *Astron. Nachr.*, Bd. 12, p. 271. Engelmann obviously preferred to *explain* this *Schreiben*.

--- [No. 322/135] (1838), *Gradmessung in Ostpreussen*. Berlin.

--- [No. 334] (1839), *Darstellung des Untersuchungen und Massregeln welche* […] *durch die Einheit des Preussischen Längenmasses veranlasst worden sind*. Berlin.

--- [No. 335/150] (1840), Über die preussische Längenmass und die zu seiner Verbreitung durch Kopien ergriffenen Massregeln.

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VII

Joh. A. Repsold

H. C. Schumacher

[1] Heinrich Christian Schumacher, a son of a Danish medium-level civil servant and chamberlain Andreas Schumacher (1726 – 1790) and Sophie Hedwig Rebecka, née Weddi (1752 – 1822)[1], was born 3 Sept. 1780 in Bramstedt, a small town in Holstein, Denmark. Already in his seventh year, as he himself stated (to Gauss on 17 Jan. 1840), his father introduced him to King Friedrich VI of Denmark, the Duke of Holstein. Until his death at the end of 1839, the King had been holding a protective hand over Schumacher.

After the death of her husband, his mother moved to Altona where her sons, Christian[2] and Andreas Anton Frederik (1782 – 1823), who became an officer, attended a grammar school (Gymnasium).

From 1799, being destined to jurisprudence, H. C. studied in the universities in Kiel and Göttingen. There [where exactly?], 7 June 1801, he greeted Goethe, who journeyed through the city, as the speaker for the students (*Goethe's Annalen* 1801). In 1804, upon graduating, H. C. took over a position of home teacher in a respected family in Livonia. Then, in 1805 or 1806, he moved to Dorpat [Tartu] to settle there as a Dozent of jurisprudence, but at the same time attempted to study mathematics and astronomy under Professor Pfaff[3], the Director of the observatory there. Indeed, these sciences ever stronger attracted him.

For actually becoming a Dozent, in July 1806 Schumacher received a doctorate in absentia in Göttingen, and, having overcome many delays and formalities, he began to lecture. However, just after that, in 1807 he was benevolently invited to the court in Copenhagen to take up a position in the pension chamber (his letter to Gauss, 4 Sept. 1850). The bombardment of the city by the British navy led to serious disruption, but Schumacher, in spite of his assumptions, became extraordinary Professor of astronomy there. In the meantime, while awaiting that position, he returned to Altona, where his mother had still been living, and began translating Carnot (1803).

That same year he became acquainted with J. Georg Repsold in Hamburg, who had then been busy with producing a new object glass for his meridian circle and turned to Gauss to find out a better form for that glass. In October 1807 Schumacher prepared some drawings which possibly had to do with determining its refractive index as awaited from Gauss.

[2] In April 1808 Schumacher began to correspond with Gauss, and later their exchange of letters became extensive. Schumacher asked an advice about integrating the Pedrayes differential equation[4], but a much later and essentially declining answer crossed with Schumacher's second letter in which he stated that that question was

not anymore important for him: he had just received from Copenhagen an approval of a donation for studying astronomy. So now he asked Gauss about the possibility of help in continuing his studies of mathematics and astronomy.

Gauss agreed, and in October Schumacher came to Göttingen. Gauss thought that a formal instruction stipulated their common observations and unquestionably became the leader. Schumacher, however, was more worldly-wise which led to Gauss' excitement and favourable attitude to him. Their contacts soon became friendlier, the more so since they only differed in age by three years.

The donation was granted for a year and its extension, which Schumacher hoped for, was not allowed. In September 1809 Schumacher wrote Repsold about his intended visit to Paris with a possible return in October. And so it really happened and besides he went with Gauss who had recently lost his [first] wife and, to distract himself, wished to see Repsold as he previously had in mind. They went through Bremen to meet Olbers and the observatory in Lilienthal from which, on 2 November, Bessel set off to meet them (Schumacher 1889, p. 120). Gauss spent about a a week in Hamburg – Altona.

At the end of 1809 began the correspondence between Schumacher and Bessel, but only in the 1820s did it become livelier[5]. After Schumacher, who looked for a position, unsuccessfully attempted to succeed Pfaff in Dorpat, he applied once more for a professorship in Copenhagen, but stayed temporarily in Altona. He taught [privately?] mathematics, completed his translation of Carnot (§ 1), wrote his *Mathematische Geographie* (1812) and reduced Repsold's observations of 1804 for Gauss. He led a sociable life but wished to restrict it.

[3] Then, in August 1810, Schumacher was *suddenly* invited to Copenhagen as an extraordinary professor of astronomy. However, Bugge, the professor there, wished to remain in the observatory all by himself and Schumacher was asked to take a leave of absence, and for the time being to continue his observations at the Repsold observatory which began in December 1809. And so he stayed in Altona, where Repsold, who, following Gauss' calculations, had just produced a new object glass for a meridian circle, willingly allowed him to use that instrument. So Schumacher began a series of observations of circumpolar stars. Repsold participated, and during this common work these very differently disposed men got closer to each other and became bosom friends. In spite of serious trials, their friendship persisted without weakening until Repsold's death in 1830.

Schumacher took lodgings in Hamburg[6], not far from Repsold, but his *main home remained in Altona*, in his mother's house. Already in the spring of 1811 the common observations had to be ceased since under the French rule the fate of the instrument was uncertain, and the observatory itself soon became a new fortification. Under these circumstances Schumacher strongly desired, following Gauss' advice, to receive a call to become director of the Mannheim observatory, although his absence from Copenhagen was only allowed under the condition that he returns in case of Bugge's death.

In August 1812 Schumacher left Altona after marrying Christine Magdalena, née von Schoon, and his mother accompanied him [them] to Mannheim. Schumacher found out that the observatory in Mannheim was in a deplorable state. The old instruments (Klüber 1811) were terribly neglected and only could be made usable by great efforts (letter to Gauss of 6 Jan 1814), and there was no room for the recently delivered Reichenbach repeating circle. Schumacher even thought of moving the observatory (letter to Repsold of 17 May 1814 [source not indicated]) but meanwhile began to observe diligently with Sisson's zenith sector and Bird's mural quadrant (Schumacher 1816). For diversion and rest he *painted many portraits* (his letter to Bessel of 31 Oct. 1836). On occasion, his letters contained small sketches which show that he had a skilled hand and a keen eye.

Schumacher's stay in Mannheim did not last long. Bugge died already in the beginning of 1815 and he was called back to take over the vacant professorship and the direction of the observatory in Copenhagen. A trip to Italy together with Reichenbach which he hoped for did not realize, and in July 1815 he went back (his letter to Repsold of 9 July 1815).

Horner was proposed as his successor at Mannheim but declined the offer; Struve hurried from Altona where he had recently celebrated his marriage but came too late: the Grand Duke had already decided in favour of Nicolai. Schumacher remained in correspondence with Fraunhofer and Reichenbach whom he visited while in Mannheim.

[4] The observatory in Copenhagen was not well equipped; in 1815, its condition was hardly better than in 1802, when Horner had visited it and described it to Repsold. Bugge was then absent and Horner found out that the rooms for observation in the *Round Tower* were so badly closed that he was able to enter without being accompanied. In three rooms he saw a six-foot long telescope, a quadrant with a six-foot radius, and a *meagre transit instrument* six foot long with a four-foot axis, all of them produced by Ahl, whose work Horner could not praise, and smaller English instruments.

This observatory could not satisfy Schumacher (in a letter to Bessel he called it *one of the most pitiful in Europe*). Perhaps mostly for undertaking a fruitful effort without its instruments he proposed to measure an arc from Skagen [the Danish northernmost town] to Lauenburg [a small town in Schleswig – Holstein] (4.5° long). With diplomatic skill he made his plan acceptable to the King who was benevolently disposed to him.

Meantime, he observed the pole altitude of Copenhagen with a Reichenbach universal instrument (Schumacher 1827), but already in the beginning of 1816 confidentially informed Repsold that means will probably be allocated for that arc measurement and asked him to measure the angles from St. Michaelis tower between the neighbouring triangulation stations[7].

In June Schumacher was fully busy with preparations for the arc measurement but, according to his opinion, they did not proceed sufficiently fast. He would have already begun, as he wrote to Gauss on 8 June 1816, had not Reichenbach let him down with the instruments. At the same time he asked whether Gauss, and possibly

Lindenau, can connect their triangulation to his, so that he will reach the triangulation in Bavaria.

Gauss was at first doubtful (?) but on the occasion of his trip to Göttingen Schumacher was able to dissipate all his misgivings. With his help it became possible to gain the assistance of the Hannover government. Now the work has begun, and Repsold helped with the instruments by word and deed. Since Schumacher still (?) did not live in Altona, Repsold also essentially helped him with problems of management.

For measuring a base Repsold had already begun producing a device[8] (Schumacher's letter to Olbers; Altona, 1821) and a suitable place (Braack) was found near Ahrenburg, a few hours from Hamburg. It was also possible to connect such a base with the Hannover triangulation. Schumacher found a competent auxiliary team for the measurement among the officers of the Danish army (Caroc, Nehus, Nyegaard, Zahrtmann and others) and called in Hansen, Olufsen, Nissen and Clausen, and, later, Peters and Petersen for calculations[9]. His work was made much easier by the most generously allocated moneys.

For determining the latitudes of the main stations (?), Schumacher thought it advisable to borrow a Ramsden zenith sector from the British government, and, in the spring of 1810, after successfully preparing this deal by diplomatic efforts, he went through Paris to London to collect the instrument. He also ordered a zenith sector from Troughton, then, however, discovered that it was not at all equal to the Ramsden instrument (his letter to Gauss of 30 Dec. 1823). The voyage gave him an occasion to establish relations with many influential men which later proved very beneficial for him.

[5] Soon after returning back Schumacher had been glad to spend some days with Bessel who came for the first time from Königsberg. He went with wife, child and sister, who had been living in his house, to visit his old home town. They met in Lauenburg where Schumacher and Gauss (who had again left) just finished their common observations with a sector. Until then, Bessel and Schumacher had only fleetingly come across each other, but this longer meeting had neared them.

In the winter of 1820 Schumacher observed in Copenhagen, where the king allowed him to build a small observatory for the sector and the Reichenbach circle. In summer, he worked with both these instruments in Skagen, then once more in Lauenburg. In October, the base at Braacken was measured with the participation of Gauss and Repsold. Struve also came as an onlooker to acquaint himself with such measurements.

It is understandable that, living in Copenhagen, it was difficult and time-consuming for Schumacher to obtain the instruments and to work together with Gauss, so he rent two rooms in Altona and was able to come there in June 1821 since, for the first time, he was completely relieved from his duties in Copenhagen and allowed to live permanently in Altona, but to deliver instead yearly reports.

He immediately bought an imposing house and in autumn occupied it with family. At the end of November 1821 he was able, for the first

time, to greet Gauss who came as his guest to take the sector. In the first (the ground?) floor Schumacher fitted his study. From its three windows Elbe was seen for many of its miles, and, further, even some triangulation stations. Next situated was his thoroughly selected reference library with a built-in bed and a spiral staircase to the so-called barometer room, a storage space for many smaller instruments and devices.

In the middle of the garden sloping from north to south was built a small observatory (Jahn 1834, I, 1), and at the south end of the lot there was a smaller, later bought house with auxiliary rooms. Gauss lived there when he came to Altona in 1827 to observe with the Ramsden sector (Schumacher's letter to him of 2 Febr. 1827) which Schumacher had set quite near the house. On other occasions Gauss apparently lived in the main house (Schumacher to Gauss, 6 June 1846). In 1823 Bessel lived in a room with a splendid view to the south. Schumacher often willingly invited guests and kept an enjoyable kitchen and a well-stocked cellar.

[6] In 1821, apart from geodetic measurements, Schumacher took upon himself a topographic description of the duchy of Holstein[10]. Partly owing to its too precise execution, this led to a very considerable increase in the work and a delay in the measurements. In the autumn of the same year the base near Braacken was measured for the second time [why?], but in March 1822 Schumacher wrote Bessel:

Our arc measurement came to a standstill and probably will not be resumed for a whole year since the map of Holstein ought to be first prepared[11]. *[…] Then I will return to my beloved business.*

Pendulum observations on a large scale were also planned. A pendulum of an invariable length should have been observed on a number of stations along the meridian from Skagen to Italy, but this plan had to be abandoned since Bessel, on whose participation Schumacher reckoned, was unable to devote the necessary time. He declined although Schumacher had even stated that he was

In such a happy independent position, able to go whenever you (Bessel) *wish.*

In spite of all the activities, many trips connected with them and the often occurring obstacles occasioned by his sickliness, Schumacher had been very active as an author. From 1820 until 1829 he published auxiliary tables for astronomical calculations, and, at the instigation of the Archive of Sea Charts in Copenhagen, a series of *Ephemeris of the distances of the four planets, Venus, Mars, Jupiter and Saturn from the Moon's Centre* for 1821 – 1831.

In June 1821 he founded the *Astronomische Nachrichten* and became its editor which proved to be his main scientific achievement. After the demise of von Zach's *Monatliche Correspondenz* and the *Z. f. Astronomie* of Lindenau and Bohnenberger that journal alleviated a deeply felt requirement and the government willingly granted means for its publication. *Our finance minister all but asked me to publish an astronomical journal in Altona*, as Schumacher wrote to Gauss. His journal was probably meant for raising the reputation of the new observatory (?), and Schumacher, with his extensive circle of acquaintants, was certainly the right man for achieving that goal.

Indeed, he had also enlisted the support of Gauss, Bessel and Olbers which he repeatedly made use of. Lengthier contributions had to be published as *Astronomische Abhandlungen*, although only three of them had appeared. Then, in 1836 – 1841 and 1843 – 1844 appeared Schumacher's *Jahrbuch*, mostly for generally understandable communications.

[7] In 1823, Repsold mounted a Reichenbach meridian circle in the small but suitable observatory in Schumacher's garden but many changes had to be done. Schumacher fand Bedenken, bei grober Einstellung des Fernrohres dieses, am Ocular-Ende, oder eine Speiche des Kreises anzufassen; es wurde deshalb am Cubus ein langer, bis fast zum Ocular reichender leichter Arm angebracht. Für das Nivellieren der Achse ließ Repsold anstatt des bisher üblichen, durch die Speichenöffnungen des Kreises zu steckenden Setz-Niveaus ein langes Hänge-Niveau herstellen, das ohne weiteres angehängt werden konnte; Schumacher findet es sehr bequem und lobt das neue, von Repsold hergestellte Niveau, das bei gleicher Feinheit der Teile sehr viel rascher zur Ruhe kommt als die Reichenbach'schen. Repsold glaubte auch die Sicherheit der Angaben des Alidaden-Niveaus dadurch zu steigern daß er es mit dem Kreise nicht in unmittelbarer Verbindung ließ, sondern es zu einem Setz-Niveau umgestaltete, das auf einem am Kreise befestigten Zylinder umzusetzen war. Endlich wurde der Versuch gemacht, die Spinnfaden zu vermeiden, weil sie trotz Repsold's Verfahren, sie sich in einem Wasserbade strecken zu lassen, nicht immer ganz straff blieben. Schumacher verschaffte sich von Wollaston feine in Silber hülle gezogene Platindrähte zum Ersatz. Die Behandlung derselben beim Auflegen proved, however, very difficult. It apparently seemed dirty and not sufficiently rectilinear, and soon was not used anymore.

In the summer of 1824 the meridian circle was applied during an English chronometric trip for determining the longitudinal difference between Altona – Helgoland and Greenwich[12]. Schumacher feared, however, that that trip will be to little avail since the *time in Greenwich is determined too poorly*.

How well was Schumacher's observatory equipped, becomes evident from Bessel's letter of 16 May 1825. He wrote it just after his return from Altona when putting in order his meridian hall:

I have once more convinced myself in that I do not dare think of accomplishing something similar to the attractive and pleasant to look accomplished in your observatory. […] *Now I will not spare time to maintain everything thoroughly and orderly although I ought to forget entirely your established ideal.*

Schumacher highly esteemed his Miren-Fernrohr which he put up in 1827 on a well protected pillar beyond his observatory[13]. He was certainly compelled to apply it since no remote azimuth mark (which Bessel would have preferred *had it been possible*) was possible either to the north or to the south.

And he especially esteemed his Biegungs-Fernrohr produced by Repsold in 1828: a small telescope put up im Horizont perpendicular to the meridian circle. It had two as exactly as possible thick rings

upon which it rested on two solid bearings and carried a refined striding level (*Astron. Nachr.*, 44.1).

Schumacher than combined his investigations with observations of the mercury horizon in the nadir and made many experiments, first with a Kater floating, then with a Repsold suspended zenith collimator (*Astron. Nachr.*, 4.311 [44.311 (?)]). It seems, however, that he had not attained satisfactory results in either case.

[8] Already in 1823 the Berlin academy asked Bessel to continue the investigation of the length of the seconds pendulum which Tralles, their late member, had begun. Bessel, however, was unable to reach an agreement with the Academy since it had not allowed him sufficient independence. Then it dawned upon him that he can investigate all by himself, and Schumacher had to ask Repsold at once whether he was prepared to produce the necessary device.

Repsold had begun the production and Schumacher with great interest kept an eye on that work since he had recently taken upon himself the regulation of the Danish weights and measures. He stipulated that the Danish foot will be connected with the length of the seconds pendulum and hoped to apply the future Repsold device essentially different from those currently applied. He repeatedly informed Bessel about the progress of the work. Bessel in real earnest awaited its completion but remained patient since he wished that Repsold does everything just as it seemed to him most expedient during their initial discussions.

And when finally the completion was nearing, Repsold advised Bessel to take upon himself the delivery of the complicated device and to coordinate with him the best method of applying it. This had indeed happened and moreover in April 1825 Bessel came to Schumacher for two weeks of a very desirable stay. Schumacher had geared everything to Bessel's wishes and habits so that his guest felt himself really at ease. Preliminary investigations were made in Schumacher's basement.

In the summer of 1828, when Bessel's pendulum observations in Altona had been completed, Schumacher went to Königsberg to see and study the application of the device. His own observations should have been made at first in Copenhagen but he found there no suitable room, and finally the castle Güldenstein near Oldenburg in Holstein (Denmark) was chosen: it was peacefully located and solidly built. And there, in late autumn of 1829, after Repsold had made some minor changes, Schumacher began *to pendulum*. In spite of his pressing requests Schumacher regretfully had to go on without Bessel's help. Furthermore, after the first series of observations he fell ill and, until the end of November, lieutenant Nehus, with whom he came there, had to observe instead although, when possible, under Schumacher's supervision. A repetition of the observations had to be postponed until next year.

Meantime, Schumacher hoped to acquire from Repsold a reversible pendulum to be produced according to Bessel's indications *as soon as Repsold somewhat recovers* (*now he is sickly*), he wrote to Bessel on 14 Dec. 1829. In four weeks, however, being deeply shaken, he had to inform Bessel about the sudden violent death of his old friend[14], with

whom shortly before it happened, he had been speaking cosily but forebodingly.

A week later he added:

I feel myself as though transplanted to foreign parts where I cannot yet collect myself, or realize that Repsold is dead which I still regard as a bad dream.

In the same letter Schumacher once more implored Bessel to come for pendulum observations since he himself, as it seems, cannot quite properly find his feet there. In mid-June, Bessel had indeed taken to the long trip with wife and daughter. He first went to Güldenstein where Schumacher was working once more since mid-June.

[9] After completing the observations, they had gone together to Altona where Bessel stayed until the 12th of August. In spite of the great difference of their dispositions, revealed by Bessel's rash and energetic nature as against Schumacher's thoughtfulness and careful caution, the friends moved nearer to each other. Already in 1828 Bessel wrote:

I would prefer very much if you will not so often leave my letters without any notice. The same with questions, although I do not really mean it. You should not restrict yourself to a few lines and many promises which will not be kept! Nice, just like a talk of a coffee-loving lady.

And in 1831:

We virtually became old and grey and we tested each other repeatedly and passed the tests. And so must it continue until one of our hearts prefers to stop beating.

For his part, Schumacher repeatedly uttered such phrases as in 1831: *I have no one nearer to my heart*. They poured out their hearts to each other over the most intimate matters. Schumacher also sent wine and cigars which he chose carefully whereas Bessel sent firs and dogs asked for by Schumacher, but the scientific communication did not suffer.

After concluding the pendulum observations in whose reductions Bessel had participated, Schumacher had for a long time been very busy with final measurements and weighing. It came to light that the Danish foot as derived from the length of the pendulum at Güldenstein was very near to the Prussian measure, and Schumacher and Bessel agreed to ask their governments to ascertain legally this fact. Their request could have naturally only been granted after extensive diplomatic preparations.

Meanwhile even in the autumn of 1830 Schumacher intended to go to Paris for precisely comparing his kilogram with the standard there, but he was ill and in the spring of 1831 sent Nyegaard instead.

Summer brought about Schumacher's great agitation because of the visitation of cholera: he thought that Bessel had first of all been endangered since a mortuary was built near his observatory. Bessel must run away, come to him! However, the epidemic also reached Altona but petered out towards winter.

During next years he was also very ill so that his work at compiling the maps (§ 6) had not proceeded according to his wishes, and he was even unable to go on official trips to Copenhagen. Meanwhile

Humboldt became instrumental in initiating the comparison of the Prussian (the Rhineland) foot with the Danish measure and in April 1834 Schumacher and Bessel came to Berlin to discuss that subject in more detail. In addition, Schumacher wished to charge someone with the engraving and publishing of his maps of Holstein whereas Bessel intended to prepare for the pendulum observations in Berlin which he thought of implementing with his device.

On the way back Schumacher visited first Hansen at the Seeberg observatory near Gotha, then Gauss whose second wife had recently died after prolonged suffering. Bearing in mind the sorrow in Gauss' house, Schumacher thought of staying in a hotel, but Gauss insisted that he lived at him just like on other occasions. The reception was worse than cool, and Schumacher wrote Bessel about it really unwillingly and was only able to explain it by his trip[15]: he did not travel at first to Göttingen, and then to Berlin, and stayed in Berlin for 14 days but was only able to stay now for a few days. Then, however, he continued apologetically:

But enough of that! Gauss himself is certainly unhappy about his dissatisfaction with everything in the world and exactly for this reason anyone who associated with him ought not to take amiss if his foul mood sometimes blazes up like a kindling.

Bessel however, concluded that *our friend is a crass egoist*. Then Schumacher remarked that Gauss

Is only dealing with magnetic subjects, much less in astronomy and not at all in observations. […] He is so poorly conversant with the situation in astronomy that he asked me to show him on occasion in the Astron. Nachr. *which astronomical tables of the planets and other celestial bodies are now best. Without often dealing with them it is easy to forget*.

[10] Schumacher ordered a magnetic device from Apel in Göttingen to acquaint himself in more detail about it and from the beginning of February allowed Nyegaard and Petersen to observe with it in Altona. In February 1835 after overcoming a serious attack of dysentery Schumacher went to Copenhagen on an official trip which he had to forgo for some years. It was arranged there that he, together with Bessel and Oerstedt, will precisely establish in Altona the ratio of both measures of the foot. Already by the end of June we find Schumacher in Berlin in preparation for that work. From the beginning of that month, also in Berlin, Bessel had been busy with his pendulum observations and was still very busy for many weeks more[16].

Schumacher, however, only stayed for 14 days and went back through Hannover and Bremen to see Olbers. Once more he had an occasion to arouse Gauss' discontent: a list of arrivals in Hannover which Gauss happened to see in a newspaper included Schumacher, and Gauss reprimanded him since he did not then come for a visit. He had to excuse himself by an indisposition which disturbed his initial plan for going through Göttingen.

Meanwhile Bessel finished his slow work in Berlin and, as agreed with Schumacher, set at once to compare the standards for which Baumann had produced him an apparatus. He concluded that both measures of the foot were identical for all practical purposes and

determined their ratio to his own Königsberg toise which was necessary for scientific applications. His standard was the best defined among the existing measures of the toise and was compared with the Toise of the Archive as thorough as the doubtful state of that French standard allowed it. After that Bessel decided that his three-month work in Berlin was finally concluded.

And now it turned out that Bessel and Schumacher, in spite of their detailed talks, had completely misunderstood each other. Schumacher reckoned that Bessel will still come to Altona where a comparator was waiting for a definitive comparison [of the measures] by him, Schumacher and Oerstedt.

Bessel, however, refused to come since he longed for his astronomical work and domesticity and in addition expected no pleasure in repeating his thorough measurements. Irritated letters had been exchanged until Schumacher, since nothing else came to mind, finally resigned himself to the impossibility of fulfilling his obligation which he had accepted in Copenhagen.

At the beginning of 1836 he had nevertheless asked Bessel to send him a clearly composed letter containing a proposal to conclude in the autumn, in Berlin, the not yet definitively finished comparison together with him and Oerstedt. Bessel (letter of 14 Feb. 1836), however, explained that the ascertainment of the Prussian measure made in the previous years still required a determination of its coefficient of thermal expansion and suggested that they meet in Berlin and in addition *correct and ascertain* the Danish standard.

Schumacher agreed, and only allowed himself, in a letter to Copenhagen, *to interpolate Oerstedt* whom Bessel did not mention. The apparatus was produced later than stipulated and their meeting only took place in the spring of 1837 and the business was completed.

From New Year's Day 1837 Steinheil lived for a few months in Hamburg and Altona to compare measures and weights produced by Repsold with Schumacher[17]. His lively and captivating nature was especially agreeable to Schumacher who very much needed to be cheered up, but still, it was somewhat difficult for him *to be the host*. Already in 1833 they became good friends since Schumacher then made his first observations with Steinheil's prismatic Kreis.

Schumacher now thought of publishing his work.

I really feel that I should have done it long ago. What had stopped me short was the fear that I will not reduce my observations how the extended mathematical knowledge now requires it. If a better way is possible, the worst way is inadmissible. If you can help me, it will certainly be excellent.

Bessel, to whom Schumacher had turned, replied on June the 10th 1837: he was prepared to help if necessary.

I will willingly help you and cut off completely some of your work, or, since you always nicely conclude it, remove the calculations.

First of all, however, it was necessary to speed up the slowly advancing geodetic work. In summer Schumacher made geodetic measurements near Copenhagen (and incidentally became unpleasantly aware of the local general economizing, mostly, however, when pursuing a wrong goal). He was certainly unable

anymore to observe from towers; he remained on the ground and let Nehus and Nyegaard observe from high up. Next year, 1838, Schumacher worked there with Bessel's device[18] although not as intensively as he did. The Danish triangles lain out on the Danish islands Moon and Falster were connected with the Swedish triangles, and through Rügen with Bessel's triangulation.

[11] The unhappy row that developed between Bessel and Encke in the beginning of 1838 involved Schumacher and caused him serious trouble. It is distressing to see that a trifling circumstance can finally lead to such a row. Actually, it only happened since Encke found it difficult to observe with a somewhat sensitive instrument which satisfied Bessel's better knack and art of observation. It is understandable that someone, as uncontrollably lively, frank and willing to work as Bessel, cannot get along properly with a somewhat narrow-minded, ponderous and self-opinionated person like Encke.

Still, for twenty years they had remained in a more or less friendly correspondence. Encke had always recognized Bessel's superiority[19] whereas Bessel well understood his priority and could have expected some consideration from Encke whom he in 1825 proposed as an academician and director of the Berlin observatory, a position which was initially offered to him himself. This state of affairs can be explained: Encke, in the *Berliner Jahrbuch* for 1839, p. 268f, and then in the *Astron. Nachr.* No. 346, had applied a lecturing tone and, moreover, he suggested that Bessel's communications had contained a contradiction in terms.

However, in his letters to Schumacher Bessel expressed himself with such passion against Encke that some other reason for his deep ill-will should still be suspected. An essential reason ought to be looked for in that for Bessel good relations with Encke and their common work over a long period of time were the decisive preconditions for declining the invitation to Berlin and recommending Encke instead (Bruhns 1869, p. 104f). And even apart from this argument it should be noticed that many matters concerning the Academy or the highest administration, with which Bessel thought himself more competent than others, did not reach him through the intermediate Encke.

The relations between Bessel and Encke had not turned out at all as favourably as they both probably expected. Unfriendly friction occurred already in connection with the printing of the Bessel star maps which was undertaken by the Aacdemy. Tension had remained, weakening and strengthening and gradually increasing until Encke's last clumsiness sent Bessel ablaze. He felt himself hindered and discriminated by Encke and ascribed him dishonest motives which, however, was hardly proved. Nevertheless, the correspondence between Gauss and Schumacher (27 June 1846) described a case in which Encke spoke about Bessel in an inexact and distorting way. It can therefore be feared that other similar cases could have happened. Anyway, the relations between Encke and Bessel were essentially wrecked and it was not easy [it proved impossible] to make them good once more.

Schumacher misfortunately intensified still more Bessel's violent infuriation: he published Encke's rejoinder (*Astron. Nachr.*, 1838, No. 346) without consulting Bessel since he did not dare reject Encke's riposte to Bessel's first publication on the same subject. Bessel, however, only compiled his paper in the *Astron. Nachr.* as a reply to Encke's attack published in the *Berliner Jahrbuch*. And now he poured out all his rage on Schumacher's bent head and accused him of being influenced by Encke and his friends, even if involuntarily. Soon, however, he certainly calmed down to such an extent that he was able to write that he *ought to attempt to correct once more what I had done in extreme ill humour*, and the relieved Schumacher cried out: *I have my old Bessel once more*! […] *We both had enough bleak hours because of that damned business and now we wish to leave it alone.*

However, Bessel thought that publication of new papers in the *Astron. Nachr.* will harm his dignity and Schumacher suggested to him to ask the opinion of Gauss and Olbers about this point, but they both answered uncertainly. Then, in July, after Bessel had again but somewhat reservedly sent a paper to the *Astron. Nachr.*, Schumacher had the brain to answer that Bessel ought not to publish anything in the *Astron. Nachr. until you yourself will have no more doubts about it and will not feel any eeriness*. And so the ice was broken.

Bessel did not want to hear about Encke anymore, although Schumacher excused him as much as it was possible for him and Encke himself repeatedly attempted to give way. Even when Encke, in a letter to Schumacher, expressed his heartfelt condolence to Bessel's heavy loss of his son, Bessel answered his friend that he was thankful to Encke but did not trust him anymore and *ought to thank him silently*.

[12] The tests which the friendship between Schumacher and Bessel had passed during those last years only seemed to link them stronger. Already in February 1839 Bessel announced his wish to come in the summer with his son for a visit to Altona, which greatly gladdened and satisfied Schumacher. A little later Bessel decided that they should correspond regularly: *We became too old and cannot be indifferent to each other. We both always ought to have it in mind.* During Bessel's stay in Altona Schumacher arranged to have his portrait painted by the Hamburg painter Herterich. That portrait occupied its place above Bessel's desk.

In December 1839 Schumacher experienced a sensitive blow by the death of his protector of many years, the King Friedrich VI. His successor, King Christian VIII, proved very favourably disposed to him, but the political situation demanded further economizing and the expenditure for geodetic work had to be restricted. This immediately resulted in that the compilation of the map of Holstein was taken away from Schumacher and given over to the general staff except for two almost completed sheets depicting *the environs of Altona*[20] *and the detailed map* (of Holstein?). Personally, Schumacher did not suffer since his income previously based mostly on the cost of food (of subsistence?) was linked for life to its level.

However, misfortune, heavy family worries and other disturbance had harassed him for a long time and wore out his poorly bodily

strength. Accordingly, as it seems, his interest in geodetic measurements had declined, and he actually left them unfinished[21].

Schumacher became ever more satisfied by his lively correspondence, which connected him with all cultivated countries, and with editing his *Astron. Nachr.,* which won general recognition. How high did Bessel regard is already seen in his letter to Schumacher of 30 Jan. 1831. There, he complained about its undelivered issues and wrote:

You know that in my opinion the <u>Astron. Nachr</u>. *is […] a necessary condition for a happy blossoming of our astronomy. Previously the von Zach's journal and then the periodical of Lindenau* [and Bohnenberger] *had played a similar role. Our astronomy therefore came to the fore and our neighbours can now learn much from us.* <u>Astron. Nachr</u>. *is a step higher than its predecessors since we ourselves have risen a step. In addition, the* <u>Astron. Nachr</u>. *is advantageous in that it is being sent by separate sheets* [1 sheet = 16 pp.?] *and it can replace correspondence for those who do not practise it. All this is lost if you do not look after its regular sending.*

On another occasion he remarked in connection with the state of astronomy in Germany and the *Astron. Nachr*:

Astronomers ought to learn German[22], and you can compel them to do so.

At the beginning of the reign of Christian VIII Schumacher was asked to return to Copenhagen and Bessel used this occasion for opposing that decision in a letter of thanks for the Commander Cross of the Dannebrog order. *It is a vital matter for you to remain in Altona*, he wrote to Schumacher in August 1840.

In his calm and comfortable house Schumacher felt himself best. His windows opened up on the spread of the whole southern sky and a friendly patch of land, and, pleasantly remote, on the invariably active shipping on the Elbe.

But still, Schumacher undertook two long voyages thought to be important, certainly in his own cosy coach and servant and the expenses were not extensively spared. In August 1840, responding to the wish of his king, he visited the Pulkovo observatory which opened in 1839 and reported about it in the *Astron. Nachr*., 18.33. Then, in July 1842 he went to Vienna to observe there a total Solar eclipse. Bessel, whom he willingly induced to come as well, answered:

My health is rather good, but my courage is broken. I feel that I am not young anymore, that only striving for work has remained.

(Half a year ago he lost his only son.)

Still, shortly afterwards, responding to a wish of his king, Bessel intended to visit a conference of the British Association for the Advancement of Science in Glasgow[23]. The worried Schumacher advised against that voyage, warned about the danger of sea voyages, continuing restlessness and the strain at the conference. Bessel, however, held on to his plan and in addition he intended to stay a few days at his brother in Saarbrücken.

[13] Schumacher then asked Bessel to arrange his voyage so that it allows him to visit Gauss as well. But the friends were unhappy in spite of their attentiveness, based on high respect, to the Grand Master

in Göttingen. For seeing Gauss, Bessel had to go a long way round, but his reception was not better than that accorded to Schumacher in 1834 on his way back from Berlin. After receiving Bessel's report about that visit, Schumacher wrote:

He is one of the most unusual men in the world with whom, in spite of all his rough edges, visitors cannot be really angry, although they often feel annoyed. Attention, as you remark, and as I myself know from my own repeated experience, is usually met with an expression of foul mood. And I therefore find that it is better just to remain exactly within the boundaries of usual politeness.

Weber (whom Schumacher had recently visited – J. A. R.) *thinks that Gauss' foul mood sets in mostly because of corns […] but that when the pain disappears he becomes amiability itself. I know, again from experience, that Gauss can indeed be amiable, although not often.*

Bessel had not received this information too seriously *With a head so heavy and sickly legs, how can stable equilibrium always remain*?

On another occasion he remarked:

Incidentally, it is a pleasure to deal with Gauss: never a single petty word. Everything is honest, clear truth.

And Schumacher appropriately commented:

Mind you, he is a queer sort of a fellow [written in English – O. S.] *and somewhat more of an egoist than necessary for a pleasant contact, but at the same time he is exceptionally honest and incapable of any mean slyness or evasion.*

Nehus, Schumacher's loyal colleague and friend of many years, died in April 1844; very unpleasant news came from his eldest son; and Bessel's progressing illness much worried him. At the middle of the year all that heavily depressed Schumacher. Moreover, Bessel remarked with a sense of foreboding: *I have so much before me which I do not want to leave, and I will not therefore grieve to live some years more.*

Schumacher was unable to suppress the following reply:

It is an honest truth that I will not gladly outlive you. You and Collin[24] *are my only remaining real friends. Collin, however, is extremely remote from my pursuits and in case of your death I will be quite alone among strangers.*

He felt himself ill and weak and next year even feared for the future of his *Astron. Nachr.*:

Jahn and Mädler intend to set up a new astronomical journal. I strike my colours.

Was that a mocking joke? He was sure about his connections with Gauss and Bessel, so that it was not easy to become his successful competitors.

By the end of 1844 began the lingering and heavy suffering of his old friend Bessel, and Schumacher's life as though shattered. He wished to know everything about the illness, asked even more than could have been pleasant to the sufferer, thought of distracting him by news but was unable to refrain from complaining about his own imminent loss. During that time, his activity had only been slight.

After Bessel's death Schumacher even more closely sided with Gauss to whom he wrote on 23 March 1846:

Allow me to find now an ersatz for the great loss in your love and friendship for the time left to us both[25].

[14] In the 1840's, rebellious feelings developed in the Schleswig and Holstein duchy against the attempts of the Danish government to join it under the same conditions with Denmark and form a united state. That attempt manifested itself even in the Open Letter enacted (erlassenen) by King Christian VIII and culminated after it was at once proclaimed by his successor.

Schumacher hardly understood properly these events, perhaps did not even try. He was led too much through life by royal favour and patronage and was unable or unwilling to leave this track. He had remained very remote from political life, but found it impossible to free himself completely from its influence.

After 23 March 1848 the *Provisional Government* in Kiel took upon itself the management of the German part [of the duchy], Prince Noer in a surprise attack on Rendsburg initiated an armed resistance, and [his] guerrillas pitched their camp near Schumacher's house. Its location became very embarrassing. Schumacher was a Danish civil servant with his own house in Copenhagen and until then had directly been discussing there his matters with the highest authorities, but now he was cut off from them.

It was difficult for a subject loyal to the king, and so much thankful to his rulers, to obey the alien new rule. As a consequence, the rich means which until then had always been allocated him, now were mostly denied. Schumacher's age and sickliness made his dire situation twice more difficult. He attempted, as shown by his letters to Gauss, to encounter bravely all the hardships and found distraction from his gloomy mood in correspondence and continuation of his *Astron. Nachr.* Already on 10 Nov. 1848 he wrote [to whom?]:

It is remarkable how it is possible to blunt most serious troubles. If someone had forewarned me that for almost a whole year I will not know wherefrom I will next year only obtain a meagre maintenance, I would not have believed to live to the end of this year. And still, the really sorrowful year had only little worsened my health.

In March 1849 he spoke of *restless and unhappy times*, and in February 1850 even about his periodical: *Right now, I attempt to support it with my last financial means*.

By the beginning of 1849 Schumacher had overcome himself and printed a number of letters addressed to highly respected scientists and scientific bodies. He stressed the importance of the Altona observatory, and especially the worth of the *Astron. Nachr.,* and hoped thus to improve his situation. It seems, however, that he was not really successful since the political relations were very dangerous. Peters' popular journal, *Z. f. populäre Mitteilungen,* reproduced three letters by Schumacher and his widow to privy councillor Francke in Kiel, the then head of the finances of the Holstein government. They show that people there sincerely went to trouble for him.

Schumacher was able to live in his old house although all by himself. His poor bodily strength gradually languished and he died on 28 Dec. 1850.

[15] Schumacher was of small stature, and tall men were unpleasant to him. He was tenderly built and very lively at younger age but in weak health. Regrettably, his poor bodily strength had not always been up to his active interest in science, and he was unable to conclude all his undertakings[26]. He was a fine observer, but not easily satisfied with conditions of work: he had to feel himself comfortable and cared when choosing a proper place for his snuffbox. At the time, astronomers thought that they ought to smoke during observation.

Being versatilely educated, he was a man of the world (and somewhat of a playboy), and had elegant manners. After getting on in years, Schumacher became a refined small gentleman with a high, somewhat nasal voice, friendly and with a tendency towards slight jokes. In his judgement, he (letter to Gauss, 29 Aug. 1823) *Adhered to a certain tact which seldom leads me* [him] *astray*. Or (letter to Bessel, 23 Apr. 1833) he is guided by an

Unfortunate but certain instinct. Until now, I never erred about someone against whom I felt something without knowing what exactly, but I had been often mistaken in the opposite case.

Schumacher's correspondence with Gauss over 42 years and with Bessel for 37 years provides a clear picture of his whole nature. However, their letters ought to be cautiously examined so that the judgement about him does not damage his image. It should be taken into account that he quite openly and frankly expressed himself to his respected friends (and especially to Bessel) who exceeded him in science in general, since he (letter to Gauss, 31 March 1840) was convinced that the

Letters exchanged between friends will never be exposed to the danger of appearing in strange hands.

And when this did partly happen[27], we can only regret that Schumacher's image seemed damaged indeed by the light, shone by his more significant friends.

However, the ties of friendship which linked him with them both had remained heartfelt and were willingly maintained by the three friends until the very end. This shows how high these great men esteemed him, which increases his worth.

Incidentally, Schumacher regarded his correspondents in very different ways. Gauss was older [only *by three years*, see end of § 2], and, when Schumacher got closer to him, was already in an exalted position, solemn and reserved, and plagued with many domestic misfortunes but aware of his greatness. In spite of all the free and easy relations between them, Gauss always remained for Schumacher an unattainable judge and leader, witness his expression during the twelve last years of their correspondence. Indeed, he invariably ended his letters of that period with the formula *Yours thankful forever*. Between them there always remained some civil formality and they did not feel themselves evenly matched[28]. Gauss worthily followed advice in business matters which Schumacher on occasion was able to offer, the more so since now and then it was given deferentially.

On the contrary, Bessel and Schumacher met each other when each was still carving his way. Bessel, who was younger, had earlier established himself. Being brought up in a large commercial house, he acquired life and commercial experience and in a short time, all by himself, occupied a leading position in the astronomical science which ensured him a well founded self-satisfaction.

Schumacher readily acknowledged his superiority, but had later been able to become very beneficial to him by publishing many of his enthusiastically compiled contributions in the *Astron. Nachr.* Some equilibrium had therefore occurred. Furthermore, as their correspondence became more active, Bessel's frank and lively nature withdrew all formality ever remoter, and the cooler Schumacher, warmly sensitive in the more important ways, accepted this change. And so their relations became wholeheartedly friendly and their letters covered not only scientific matters, but all the joys and sorrows of human life.

Already on 12 May 1825, just upon returning from Altona, Bessel wrote:

I would have liked to be with you all the time! When a period of tranquillity comes, we will perhaps work together more than now. We stand each other since I have recently seen still clearer that we have the same way of thinking.

Their letters, 1131 in all, are duly ordered by Auwers. Those who read them ought not to forget that Schumacher had written only to Bessel, and should restrain from making any indiscreet insights.

Schumacher was buried in a small cemetery in the then Palmaillenstrasse and which can still be found in the Hehnstrasse park barely 100 steps to the north from the house in which he had lived in good times and in bad. The gravestone is near the street.

Hamburg, June 1918

Notes

1. Andreas apparently married a widow.

2. Repsold indicates how Schumacher signed his name (with the first name or without it, in the Roman alphabet or Gothic script) and concludes that his usual main name was Christian.

3. Johann Wilhelm Andreas Pfaff, the brother of the renowned mathematician Johann Friedrich Pfaff. His *Astrology* (Nuremberg 1816) puzzled Gauss (letter to Olbers, 28 Apr. 1817) although he had not specified that it was not J. F. Pfaff.

4. A complicated differential equation with sixteen terms.

5. The Berlin Academy of Sciences had kindly sent me their correspondence for studying it. I. A. R. It was never published. O. S.

6. The Hamburg Directory for 1811 and 1812 called Schumacher Doctor of Science and translator. I. A. R.

7. In 1818, an illuminated window of that tower inspired Gauss to construct the heliotrope (Biermann 1991b, p. 329).

8. This device is usually called after Bessel. See Note 18.

9. It was wrong to include Hansen, the great scholar, on a par with the others.

10. In § 14 Repsold properly mentioned the Schleswig and Holstein duchy. Until 1864 Holstein belonged to Denmark.

11. The charts were supposedly compiled in a small scale. Preliminary triangulation was not therefore needed, but some more or less precise measurements were still required.

12. Helgoland was possibly an intermediate station.

13. A mira or mark was obviously needed to check, now and then, the invariability of the circle of the telescope. However, I do not understand the Fernrohr (the telescope). Was it the second, less powerful telescope of the instrument? But the mira was then still necessary. Such checking telescopes had been in use when observing in some regions of the Soviet Union.

14. Repsold died in an accident occasioned by a fire.

15. Schumacher described the reproaches actually expressed by Gauss (Biermann 1966, p. 14).

16. Mikhailov (1939, p. 213) stated that Bessel had essentially perfected the then generally accepted Kater pendulum device. On p. 404 Mikhailov placed its photo. And I repeat [v] that he also called Bessel's metrological work *classical*.

17. Cf. [v].

18. Its main components were the measuring bars put one after another and wedges for measuring the space between them [No. 322/135, Chapter 1]. Here and below, Repsold only mentions triangles of triangulations although other figures were also included (for example, braced quadrilaterals).

19. In 1828 Encke wrote to Bessel: *Let heaven grant me the privilege to live for a long time before your eyes* (Bruhns 1869, p. 272). In 1830 he wrote to Gauss that he had come back from Königsberg somewhat depressed since *The little I have* [he had] *adopted disappears when compared with what Bessel had long ago been achieving* (Ibidem, p. 277). Finally, in 1835 Encke complained [to whom?] that each letter from Bessel contained a description of a completed investigation whereas he has nothing worthy of mentioning (p. 281). I. A. R.

Encke became not only a member of the Berlin academy, but the permanent secretary of its physical and mathematical class (Repsold 1920, p. 194). O. S.

20. Repsold described those maps as communicated to him by the Director of the Danish *Arc Measurement*.

21. As communicated by that same person: After Schumacher's death it turned out that most [observations] of the chain of triangulation through Jutland were missing and those [extant] were not definitively treated or published, see Andrae (1872 – 1884). I. A. R.

22. After Daniel Bernoulli and Lambert, in 1776, had published their astronomical contributions in German, Lalande (1802 – 1803/1985, p. 539) noted that *astronomers ought to learn German*.

23. This Association was established in 1831, which means that the conference celebrated its ten years of existence. Bessel himself [No. 354] named Manchester rather than Glasgow. The conference was possibly held in both cities (Manchester and Glasgow) in turn.

24. A Konferenzrat (conference councillor) dealing with finances. I. A. R.

25. Gauss (letter to Schumacher, 25 March 1846) was *painfully shaken* by Bessel's death and concluded: *So let us, dear Schumacher, all the more hold together* (Biermann 1966, p. 19).

26. See Note 21.

27. In 1880 – 1885 the correspondence between Gauss and Schumacher was first published.

28. Witness Wagner (Biermann 1991a, p. 3):

My friends and acquaintances will attest that we never regarded our great mathematician as a colleague, but always as a superior endowed with wholly unusual spiritual power before whom one always stepped a few paces aside. I will not be misunderstood if I say that in our scientific republic he played about the same role as the lion in the animal fabled world.

For his part, Sartorius tells us:

We never saw a man with a more impressive outward appearance. All the other ones seemed on a par with us, but he stood as an unearthly being, as a priest at his post by the throne of the Deity.

Brief Information about Those Mentioned

Auwers Georg Friedrich Julius Arthur von, 1838 – 1915, astronomer

Bird John, 1709 – 1776, astronomer, constructor of scientific instruments

Bohnenberger Johann Gottlieb Friedrich von, 1765 – 1831, astronomer

Bugge Thomas, 1740 – 1815, astronomer

Clausen Thomas, 1801 – 1885, mathematician, astronomer

Encke Johann Franz, 1791 – 1865, astronomer

Fraunhofer Joseph von, 1787 – 1826, optician, phisicist

Hansen Peter Andreas, 1795 – 1874, astronomer, mathematician

Horner Johann Caspar, 1774 – 1834, physicist, mathematician, astronomer

Jahn Gustav Adolf, 1804 – 1857, astronomer

Kater Henry, 1777 – 1835, physicist, metrologist, astronomer

Lindenau Bernhard August von, 1780 – 1854, astronomer, jurist, politician

Mädler Johann Heinrich von, 1794 – 1874, astronomer

Nicolai Friedrich Bernhard Gottfried, 1793 – 1846, astronomer

Noer Fürst von, Friedrich Emil August Prinz von Schleswig-Holstein-Sondenburg-Augustenburg, 1800 – 1865

Oersted Hans Christian, 1777 – 1851, physicist, chemist

Olufsen Christian Friedrich Rottboll, 1802 – 1855

Pedrayes y Foyo Augustin, 1744 – 1815, mathematician

Peters Christian August Friedrich, 1806 – 1880, astronomer

Petersen Adolph Cornelius, 1804 – 1854, astronomer

Pfaff Johann Friedrich, 1765 – 1825, mathematician

Reichenbach Georg Friedrich, 1771 – 1826, constructor of scientific instruments

Repsold Johann Georg, 170 – 1829, businessman, constructor of scientific instruments

Sartorius Waltershausen Wolfgang von, 1809 – 1876, mineralogist, geologist

Sisson Jeremiah, 1720 – 1783, constructor of scientific instruments

Steinheil Carl August von, 1801 – 1870, physicist, inventor, engineer, astronomer

Throughton Edward, 1753 – 1835, constructor of scientific instruments

Weber Wilhelm Edmund, 1804 – 1891, physicist

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VIII

O. Sheynin

A Little Known Side of Gauss

Unpublished

1. The Marble Statue

Biermann (1991a) traced the change of our image of Gauss: his marble statue gradually became a human being with his *contradictions, doubts and attempts, not free from his moods, sufferings and struggles*. The sculptors of that cold statue belonged to the inner circle of Gauss' surroundings during the last two decades of his life, but the main sculptor was Sartorius von Waltershausen. Biermann (p. 5) also stated that Gauss had *conscientiously or otherwise powerfully assisted* those attempts. I somewhat differ.

First, Gauss would have been unable to conceal the encountered difficulties and troubles or his helplessness in everyday life. Second, even when restricting Biermann's conclusion to the realm of science, there is much to say about it. In 1801, Gauss published the *Disquisitione arithmeticae* which immediately made him one of the first (if not the best) mathematician of the whole world and in 1809 appeared his *Theoria motus*, a masterpiece of astronomy.

Understandably, Gauss did not wish to lower the scientific level of his work and indeed, on 30 July 1806 (even before the *Theoria motus* was published) he made known his motto in a letter to Olbers: he intended to be *either Caesar or a nonentity*. Then, Gauss is known to have been collecting information, non-scientific as well as scientific, with a view of arranging random or only seemingly random events and discovering some order[1]. Biermann (1991b) reasonably noted that this habit could have well strengthened his desire for perfection. I conclude that Gauss had indeed unconscientiously and *unavoidably assisted* in sculpting that marble statue.

2. Unpleasant Features

Humboldt called Gauss a *scientific despot* (Biermann 1991a, p. 9, without an exact reference) and Bessel (Biermann 1966, p. 14) considered him an *insensitive egoist*. Indeed, in 1833 Gauss published an essential contribution on terrestrial magnetism, *typically acknowledged the help of Weber but did not include him as a joint author* (May 1972, p. 305, right column) and his sons by his second marriage stated (Ibidem, p. 308, right column) that he *had discouraged them from going into science* [since] *he did not want any second-rate work associated with his name*. May (p. 307, right column) also indicated *personal* ambition (along with *intellectual isolation*) and *deep conservatism*. Indeed (p. 309, left column) Gauss was *hostile or indifferent to radical ideas in mathematics*, which, however, was somewhat far-fetched since Gauss is known to have studied the *anti-Euclidian* geometry (although May stated that Gauss had disliked and suppressed it). And here is a sudden comparison of

Gauss and Chebyshev: the latter was a *pathological conservator* (Novikov 2002, p. 330)[2].

3. References to Other authors

Biermann (1966, p. 18) described Gauss' reluctance to refer to other authors. In particular, he (certainly being preceded by other commentators) quoted C. G. J. Jacobi who had remarked that *for over twenty years* Gauss *had never quoted either me or D* [Dirichlet]. At the same time, in his correspondence Gauss, however, put a high value on both these scholars (May 1972, p. 304, right column).

Biermann (1966) also quotes Gauss: he, Gauss, refers to other authors only after convincing himself of their merit, but he has neither time nor inclination for literary studies.

However, Gauss had a few times mistakenly referred to others which could have strengthened his resolve as stated above. Thus, in 1770, Boscovich had offered a certain method of treating observations and Gauss (1809, § 186) mentioned him and mistakenly stated that Laplace had modified that method. There also, in § 177, Gauss attributed to Laplace rather than to Euler the computation of the integral of the exponential function of a negative square. Later, as Börsch and Simon, the Editors of Gauss (1887, p. 207), noted, he revealed his mistake but did not correct it since Euler had not presented that integral in its final form and, which was more important, a correction was undesirable since the material was in print.

4. Imperfect Contributions

The Note of 1810. It appeared in a six-volume encyclopaedia on the history of literature (1805 – 1813) which, however, included items on natural science and mathematics. Its Editor was J. C. Eichhorn, a professor at Göttingen, who asked Gauss to describe mathematics and astronomy in the 18th century Germany. Biermann (1983), who reprinted the note, reasonably remarked that Gauss had to overcome his dislike of writing popular accounts and to satisfy that request.

Gauss almost failed. He insufficiently described the merits of Lambert and Daniel Bernoulli and called Süssmilch a mathematician. Germany (Biermann, p. 427) was then thought to comprise the region of the German language[3], but Lambert called himself a Swiss (Wolf 1860, beginning of essay). I do not know whether Jakob and/or Johann Bernoulli considered themselves German or Swiss, but Euler (whom Gauss highly praised) was partly a Russian scholar. Herschel (see below), whom Gauss also called a German scientist, was after all an English scholar. Moreover, why then Gauss had not mentioned German scholars working in Russia (e. g., Goldbach)?

Gauss (Biermann 1983, p. 426) indicated that during the 18th century four German scholars (he named only three, Herschel, Olbers and Harding) had discovered five planets whereas Herschel had also discovered six satellites of Uranus. The five planets were Uranus and four minor planets (not thus called in those times and discovered in the very beginning of the 19th century). However, Herschel had indeed discovered Uranus, but thought that this heavenly body was a comet. Even now only five of its satellites are known of which Herschel had discovered only two.

The Memoir of 1823. Some places there are still incomprehensible. Here is Stewart (1995, p. 222) about its §§ 12 and 13:

It requires great generosity on the part of the reader to conclude that he [Gauss] *actually proved anything*.

A special point here is that the principle of least squares can be derived without any intermediate considerations (as in §§ 12 and 13). In § 6 Gauss introduced the density (though not the term) calling it the measure of precision for continuous densities. At the end of the memoir he proved, which was not difficult, that the sample variance is proportional to the sum of the squares of the residual free terms of the adjusted system of equations. Gauss thus arrived at the principle of least squares but did not even hint at this possibility. Why? Such was his well known habit, and I need not go here into details. See Sheynin (2012).

The Memoir of 1828. On p. 152 Gauss indicated that he was determining for the second time the latitudinal difference between the observatories in Göttingen and Altona but he did not say anything about its first determination. In several tables of the results of observations 16 stars remained unnamed without any explanation. In two cases (pp. 172 and 189) Gauss calculated the probable error of some results only tacitly assuming the appropriate normal distributions. On p. 161 Gauss called the arithmetic mean the most probable estimator (which it indeed is, but only for normal distributions) although in 1823 he turned instead to most reliable estimators. Finally, Gauss (p. 177) not quite properly equated residual free terms of an initial system of equations with errors. The same, however, can be said about Legendre and Laplace.

5. The Problem of Priority

To Gauss (May 1972, p. 309)

Priority meant being first to discover, not first to publish; and he was satisfied to establish his dates by private records, correspondence, cryptic remarks in publications.

The most important case here was his discovery of the principle (and calling it *method*) of least squares. Gauss indicated that Legendre had priority of publication but claimed it for himself, since he had applied it from 1794 or 1795.

Legendre had protested whereas Gauss, about 25 years younger, did not answer his letter. As a result, for a long time French mathematicians including Poisson but not Laplace did not mention the appropriate works of Gauss. All that could have been different if only Gauss had answered Legendre, or, even better, if Legendre, instead of writing to Gauss, would have remarked at a later occasion, that everyone will agree with him rather than with Gauss. And here is the final stroke (letter of Gauss to Schumacher of 17 Oct. 1824):

With irritation and distress I have read that the pension of the old Legendre, an ornament to his nation and age, was cut off.

Notes

1. On the inductive discovery of arithmetic regularities see Bachmann (1922).

2. And his talented student Liapunov (1895/1946, pp. 19 – 20) called Riemann's ideas *extremely abstract*, his investigations *pseudo-geometric* and sometimes, again,

too abstract and having nothing in common with Lobachevsky's *deep geometric studies*. He did not recall Klein, who, in 1871, presented a unified picture of the non-Euclidean geometry in which the findings of Lobachevsky and Riemann had appeared as particular cases.

3. John Herschel (1829, p. 222) called German all those who were united by language and behaviour. It is difficult, however, to unite thus Gauss and Bessel, or Karl Pearson and Fisher, or Markov and Liapunov.

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O.Sheynin

Statistics and Its Essence

Unpublished

1. Early History

1.1. University Statistics. In the 1660s, Hermann Conring originated a new discipline, the Staatswissenschaft, or university statistics, and by the beginning of the eighteenth century it was taught all over Germany (Lazarsfeld 1961, p. 291). He modestly named Aristotle, Strabo and Ptolemy as the co-authors of the new discipline (Fedorovich 1894, p. 17).

Then, in mid-18th century Achenwall created the Göttingen school of Staatswissenschaft which described the climate, geographical situation, political structure and economics of separate states and estimated their population by issuing from data on births and mortality but did not study relations between quantitative variables. Wordy descriptions rather than numbers lay at the heart of the works of the Göttingen school, but Achenwall advised state measures fostering the multiplication of the population and recommended censuses without which (1763/1779, p. 187) a probable estimate of the population could be still got, see above. He also appropriately defined *the so-called statistics* as the Staatswissenschaft of separate states (Achenwall 1749, p. 1) and (1752/1756, Intro.) left an indirect definition of statistics:

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

On Achenwall see Schiefer (1916). It is appropriate to mention that in a letter of 1742 Daniel Bernoulli (Fuss 1843/1968, t. 2, p. 496) stated that *mathematics can also be rightfully applied in politics.* Citing Maupertuis' approval, he continued: *An entirely new science will emerge if only as many observations will be made in politics as in physics.* But did he understand politics just as Achenwall did later? Or, as Laplace (1814/1995, p. 62), who urged that the method based on observation and calculus should be applied to the political and moral sciences?

Achenwall's student Schlözer (1804, p. 86) figuratively stated that *History is statistics flowing, and statistics is history standing still.* Obodovsky (1839, p. 48) suggested a similar maxim: Statistics is to history as painting is to poetry. For those keeping to Staatswissenschaft Schlözer's pithy saying became the definition of statistics which was thus not compelled to study causal connections in society or discuss possible consequences of innovations. Note however that that saying is unsatisfactory: even Leibniz (Sheynin 1977, p. 224) stated that statistics of different countries or of different

periods in the life of the same country ought to be compared one with another, that, therefore, statistics should also be *flowing*.

By the end of the 19th century the scope of the Staatswissenschaft essentially narrowed, but it still exists, at least in Germany, in a new form. It applies quantitative data and studies causes and effects. It is the application of the statistical method to the life of a state or a region. On the history of Staatswissenschaft see Sheynin (2014).

Knies (1850, p. 24) and John (1883, p. 670) quoted unnamed German authors who had believed, in 1806 and 1807, that the issues of statistics ought to be the national spirit, love of freedom, the talent and the characteristics of the great and ordinary people of a given state. This critic had to do with the limitations of mathematics in general. Here, however, is an ancient example of uniting description with approximate numbers:

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

Tabular statistics which had originated with Anchersen (1741) could have served as an intermediate link between words and numbers (between Staatswissenschaft and political arithmetic, see § 1.2), but Achenwall (1752, Intro.) had experienced a public attack against the first edition of that book (published in 1749 under a previous title) by Anchersen. Tabular statisticians continued to be scorned, they were called *Tabellenfabrikanten* and *Tabellenknechte* (slaves of tables) (Knies 1850, p. 23). In 1734, I. K. Kirilov (Ploshko and Eliseeva 1990, pp. 65 – 66) compiled a tabular description of Russia, but it was only published in 1831.

In the beginning of the 1680s Leibniz compiled several manuscripts on political arithmetic (§ 1.2) and Staatswissenschaft which were only published in 1866. Now, they are available in his collected writings on insurance and finance mathematics (2000). In one of those manuscripts he (1680 – 1683/2000, pp. 442 and 443) adopted unfounded premises about population statistics including a simply fantastic statement: the birth rate can be nine or ten times higher than it actually is.

In his manuscripts devoted to Staatswissenschaft, Leibniz had recommended to compile state tables containing information useful for the state and to compare those of them which pertained to different states or times; to compile medical sourcebooks of observations made by physicians, of their recommendations and aphorisms; and to establish sanitary commissions with unimaginably wide tasks. He mentioned inspection of shops and bakeries, registration of the changes in the weather, fruit and vegetable yields, prices of foodstuffs, magnetic observations and, the main goal, recording of diseases and accidents affecting humans and cattle.

Leibniz (1682) also compiled a list of 56 questions (actually, of 58 since he made two mistakes in numbering them). He left them in an extremely raw and disordered state and a few are even incomprehensible. Their main topics were population statistics in a wide sense; money circulation; cost of living; morbidity. Incidentally,

for some strange reason population statistics at least up to the 20th century had largely shunned medical problems. Graunt was a remarkable exception and Poisson (§ 5) treated them in his lectures.

1.2. Political Arithmetic. Statistics, in its modern sense, owed its origin to political arithmetic founded by Petty and Graunt. They studied population, economics, and commerce and discussed the appropriate causes and connections by means of elementary stochastic considerations. Petty called the new discipline *political arithmetic* (but had not defined it) and its aims were to study from a socio-economic point of view states and separate cities (or regions) by means of (rather unreliable) statistical data on population, industry, agriculture, commerce etc. Petty (1690/1899, p. 244) plainly formulated his denial of *comparative and superlative Words* and attempted to express himself *in Terms of Number, Weight, or Measure …*; Graunt undoubtedly did, if not said the same.

Petty (1927, vol. 1, pp. 171 – 172) even proposed to establish a *register generall of people, plantations & trade of England*, to collect the accounts of all the *Births, Mariages, Burialls […] of the Herths, and Houses […] as also of the People, by their Age, Sex, Trade, Titles, and Office*. The scope of that Register was to be wider than that of *our existing Register office* (Greenwood 1941 – 1943/1970, p. 61).

At least 30 Petty's manuscripts (1927) pertained to political arithmetic. This source (pp. 39 – 40) shows him as a philosopher of science congenial in some respects with Leibniz:

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

Graunt (1662) studied the weekly bills of mortality in London which began to appear in the 16th century and had been regularly published since the beginning of the 17th century. His contribution had been (but is apparently not anymore) attributed to Petty who perhaps qualifies as co-author. For my part, I quote his *Discourse* (1674): *I have also* (*like the author of those Observations* [like Graunt!]) Dedicated this Discourse ...

Graunt used the fragmentary statistical data to estimate the population of London and England as well as the influence of various diseases on mortality and he attempted to allow for systematic corruptions of the data. Thus, he reasonably supposed that the number of deaths from syphilis was essentially understated out of ethical considerations. His main merit consisted in that he attempted to find definite regularities in the movement of the population. For example, he established that both sexes were approximately equally numerous (which contradicted the then established views) and that out of 27 new born babies about 14 were boys. When dealing with large numbers, Graunt did not doubt that his conclusions reflected objective reality which might be seen as a fact belonging to the prehistory of the law of large numbers (LLN). The parameter 14:13 was, in his opinion, an estimate of the ratio of the respective probabilities.

Nevertheless, he had uncritically made conclusions based on a small number of observations as well and thought that the population increased in an arithmetical progression, since replaced by the geometrical progression definitively introduced by Süssmilch and Euler (§ 1.3).

In spite of the meagre and sometimes wrong information, Graunt was able to compile the first life table (common for both sexes). He somehow calculated the relative number of people dying within the first six years and within each next decade up to age 86. According to his table, only one person out of a hundred survived until that age. The very invention of the life (or mortality) table was the main point here. The indicated causes of death were also incomplete and doubtful, but Graunt formulated some important conclusions as well (although not without serious errors). His general methodological (but not factual) mistake consisted in that he assumed, without due justification, that statistical ratios during usual years (for example, the per cent of yearly deaths) were stable. Graunt had influenced later scholars (Huygens, letter of 1662/1888 – 1950, 1891, p. 149; Hald 1990, p. 86):

1. *Grant's* [!] *discourse really deserves to be considered and I like it very much. He reasons sensibly and clearly and I admire how he was able to elicit all his conclusions from these simple observations which formerly seemed useless.*

2. *Graunt reduced the data from several great confused Volumes into a few perspicuous Tables and analysed them in a few succinct Paragraphs which is exactly the aim of statistics.*

1.3. Population Statistics. I discuss medical and juridical statistics separately (§§ 2.2 and 2.3), but I emphasize that those fields are fundamentally important for population statistics. Thus, Poisson (§ 5), in his lectures, treated all these three disciplines.

Halley (1693), a versatile scholar and an astronomer in the first place, compiled the next life table. He made use of statistical data collected in Breslau, a city with a closed population. Halley applied his table for elementary stochastic calculations and thus laid a mathematical foundation of actuarial science. He was also able to find out the general relative population of the city. Thus, for each thousand infants aged less than a year, there remained 855 children from one to two years of age, …, and, finally, 107 persons aged 84 – 100. After summing up all these numbers, Halley obtained 34 thousand (exactly) so that the ratio of the population to the new born babies occurred to be 34. Until 1750 his table remained the best one (K. Pearson 1978, p. 206).

The yearly rate of mortality in Breslau was 1/30, the same as in London, and yet Halley considered that city as a statistical standard. If such a notion is appropriate, standards of several levels ought to be introduced. Again, Halley thought that the irregularities in his data will rectify themselves, were the number of years [of observation] much more considerable. Such irregularities could have been produced by systematic influences, but Halley's opinion shows the apparently wide-spread belief in an embryo of the LLN.

Sofonea (1957, p. 31*) called Halley's contribution the beginning of the entire development of modern methods of life insurance, and

Hald (1990, p. 141) stated that it became of great importance to actuarial science. Drawing on Halley, De Moivre (1725) introduced the continuous uniform law of mortality for ages beginning at 12 years.

In 1701 Halley (Chapman 1941, p. 5) compiled a chart of Northern Atlantic showing the lines of equal magnetic declinations so that he (and of course Graunt) might be called the founders of exploratory data analysis.

It might be thought that statistics and statistical method are equivalent notions (see however § 9), but it is normal to apply the former term when studying population and to use the latter in all other instances and especially when applying statistics to natural sciences. Nevertheless, there also exist such expressions as *medical* and *stellar statistics*, and theory of errors.

Three stages may be distinguished in the history of the statistical method. At first, conclusions were being based on (statistically) noticed qualitative regularities, a practice which conformed to the qualitative essence of ancient science. Here, for example, is the statement of the Roman scholar Celsus (1935, p.19):

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

The second stage (Tycho in astronomy, Graunt in demography and medical statistics) was distinguished by the availability of statistical data. Scientists had then been arriving at important conclusions either by means of simple stochastic ideas and methods or even directly, as before. During the present stage, which dates back to the end of the 19th century, inferences are being checked by quantitative stochastic rules.

In the 18th century, statisticians had been attempting to bring into conformity the speedy increase in population with the Biblical command (Genesis 1:28), *Be fruitful and multiply and fill the earth and subdue it*, and K. Pearson (1978, p. 337) severely criticized them:

Instead of trying, in the language of Florence Nightingale, to interpret the thought of God from statistical data, [they] *turn the problem around and twist their data to suit what they themselves consider the will of the Creator.*

And, on the same page, again about those statisticians who paved the way for the Malthusians if not Malthus himself:

While the Creator would not approve of starvation for thinning humanity, He would have no objection to plague or war.

The most renowned statistician of the second half of the 18th century was Süssmilch although Pearson (p. 347) called Struyck a more influential forerunner in the field of vital statistics. Süssmilch (1741) adhered to the tradition of political arithmetic. He collected data on the movement of population and attempted to reveal pertinent divine providence but he treated his materials loosely. Thus, when taking the mean of the data pertaining to towns and rural districts, he tacitly assumed that their populations were equally numerous; in his studies of mortality, he had not attempted to allow for the differences in the age structure of the populations of the various regions etc.

Nevertheless, his works paved the way for Quetelet (and perhaps Guerry); in particular, he studied issues which later came under the province of moral statistics (e.g., illegitimate births, crime, suicides) and his tables of mortality had been in use even in the beginning of the 19th century, see Birg (1986) and Pfanzagl & Sheynin (1997). After Guerry (1833; 1864), see also Friendly (2007), and Quetelet the domain of moral statistics essentially broadened and includes now, for example, philanthropy and professional and geographical mobility of the population.

Like Graunt, Süssmilch discussed pertinent causes and offered conclusions. Thus, he (1758) thought of examining the dependence of mortality on climate and geographical position and he knew that poverty and ignorance were conducive to the spread of epidemics.

Süssmilch's main contribution, the *Göttliche Ordnung*, marked the origin of demography. Its second edition of 1761 − 1762 included a chapter *On the rate of increase and the period of doubling* [of the population]; it was written jointly with Euler and served as the basis of one of Euler's memoirs (Euler 1767). Süssmilch thought that, since multiplication of mankind was a divine commandment, rulers must take care of their subjects. He condemned wars and luxury and indicated that the welfare of the poor was to the advantage of both the state, and the rich. His pertinent appeals brought him into continual strife with municipal (Berlin) authorities and ministers of the state (Prussia). He would have likely agreed with a much later author (Budd 1849, p. 27) who discussed cholera epidemics:

By reason of our common humanity, we are all the more nearly related here than we are apt to think. […] And he that was never yet connected with his poorer neighbour by deeds of Charity or Love, may one day find, when it is too late, that he is connected with him by a bond which may bring them both, at once, to a common grave.

Süssmilch's collaboration with Euler and frequent references to him in his book certainly mean that Euler had shared his general social views. Malthus (1798) picked up one of the conclusions in the *Göttliche Ordnung*, viz., that the population increased in a geometric progression (still a more or less received statement). Euler compiled three tables showing the increase of population during 900 years beginning with Adam and Eve. His third table based on arbitrary restrictions meant that each 24 years the number of living increased approximately threefold. Gumbel (1917) proved that the numbers of births, deaths and of the living in that table were approaching a geometric progression and noted that several authors since 1600 had proposed that progression as the appropriate law.

Note, however, that it was Gregory King (1648 – 1712) who first discussed the doubling of population (K. Pearson 1978, p. 109).

Euler left no serious contributions to the theory of probability, but he published a few elegant and methodically important memoirs on population statistics. He did not introduce any stochastic laws, but the concept of increase in population is due to him, and his reasoning was elegant and methodically interesting, in particular for life insurance (Paevsky 1935). On Euler see Sheynin (2007a).

Lambert published a methodical study in population statistics (1772). Without due justification he proposed there several laws of mortality (belonging to types IX and X of the Pearson curves). Then, he formulated the problem about the duration of marriages, studied children's mortality from smallpox and the number of children in families (§ 108). See Sheynin (1971b) and Daw (1980) who also appended a translation of the smallpox issue.

When considering the last-mentioned subject, Lambert started from data on 612 families having up to 14 children, and, once more without substantiation, somehow adjusted his materials. He arbitrarily increased the total number of children by one half likely attempting to allow for stillbirths and the death of children. Elsewhere he (§ 68) indicated that statistical inquiries should reveal irregularities.

Population statistics owed its later development to the general problem of isolating randomness from Divine design. Kepler and Newton achieved this aim with regard to inanimate nature, and scientists were quick to begin searching for the laws governing the movement of population (and attempting to fit them to the Biblical command). Moreover, De Moivre thought that exactly that problem constituted the main aim of his philosophy. He dedicated the first edition of his *Doctrine of Chances* (1718/1756, p. 329) to Newton, and here are a few pertinent lines. He thought about working out

A Method of calculating the Effects of Chance […] *and thereby* [of] *fixing certain rules, for estimating how far some sort of Events may rather be owing to Design than Chance* […] [so as to learn] *from your 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.*

De Moivre thus believed that the (future) theory of probability should be applied in natural sciences, but he rigorously demonstrated his theorems. Studies of various distributions had not yet begun. Chance had been certainly separated from design in everyday life. Bühler (1886/1967, p. 267) described an appropriate (for us, unreasonable) example pertaining to the administration of justice in ancient India. Horrible trials with red-hot iron had been widespread.

1.3.1. The sex ratio at birth. The solution of this problem was not practically needed, but the subject itself attracted scientists and provided a possibility of applying mathematical methods.

1.3.1-1. Arbuthnot. He (1712) assembled the existing data on baptisms in London for 1629 – 1710, noted that during those 82 years more boys (*m*) were invariably born than girls (*f*) and declared that that fact was *not the Effect of Chance but Divine Providence, working for a good End.* Boys and men, as he added, were subject to greater dangers and their mortality was higher than that of the females. Even disregarding both that unsubstantiated statement and such [hardly exhibited] regularities as the *constant Proportion m:f and fix'd limits of the difference* (*m – f*), *the Value of Expectation* of a random occurrence of the observed inequality was less than $(1/2)^{82}$, he stated.

Arbuthnot could have concluded that the births of both sexes obeyed the binomial distribution, which, rather than the inequality *m > f*, manifested Divine design; and could have attempted to estimate its

parameter. Then, baptisms were not identical with births. Graunt (1662, end of Chapt. 3) stated that during 1650 – 1660 less than half of the general [Christian] population had believed that baptism was necessary; Christians perhaps somehow differed from other people, London was perhaps an exception. Note however, that during the 18th century philosophers almost always understood randomness in the uniform sense.

One more point. Denote a year by *m* or *f* if more boys or girls were respectively born. Any combination of the *m*'s and *f*'s in a given order has the same probability (2^{-82} in Arbuthnot's case). However, if the order is of no consequence, then those probabilities will greatly differ. Indeed, in a throw of two dice the outcome "1 and 2" in any order is twice as probable as "1 and 1". It is this second case which Arbuthnot likely had in mind.

I note Laplace's inference (1776/1891, p. 152; 1814/1995, p. 9) in a similar case: a sensible word will hardly be composed by chance from separate letters. Poisson (1837a, p. 114) provided an equivalent example and made a similar conclusion. However, a definition of a random sequence (and especially of its finite variety) is still, and will continue to be a subject of subtle investigations, see also § 7.6.2.

Freudenthal (1961, p. xi) called Arbuthnot the author of the first publication on mathematical statistics, see also Shoesmith (1987) and David & Edwards (2001, pp. 9 – 11).

1.3.1-2. Niklaus Bernoulli. While discussing the same subject, he indirectly derived the normal distribution. Let the sex ratio be *m/f, n*, the total yearly number of births, and μ and (*n* – μ), the numbers of male and female births in a year. Denote

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

and let *s* = 0($\sqrt{n}$). Then Bernoulli's formula (Montmort 1713/1980, pp. 388 – 394) can be presented as

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

$$P[-s \le \frac{\mu - np}{\sqrt{npq}} \le s] \approx 1 - \exp[-\frac{s^2}{2}].$$

It is not an integral theorem since *s* is restricted (see above) and neither is it a local theorem; for one thing, it lacks the factor $\sqrt{2/\pi}$.

The context of De Moivre's paper (1733) in which he proved the first version of the central limit theorem, CLT (a term introduced by Polya (1920)) shows that he intended it for studying that same problem, the sex ratio at birth.

1.3.1-3. While investigating the same problem, **Daniel Bernoulli** (1770 – 1771) first assumed that male and female births were equally probable. It followed that the probability that the former constituted a half of 2*N* births will be

$$P = \frac{1 \cdot 3 \cdot 5 \cdot \ldots (2N-1)}{2 \cdot 4 \cdot 6 \cdot \ldots \cdot 2N} = q(N).$$

He calculated this fraction not by the Wallis formula but by means of differential equations and obtained

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

Application of differential equations was Bernoulli's usual method in probability. Bernoulli also determined the probability of the birth of approximately *m* boys (see below):

$$P(m = N \pm \mu) = q\exp(-\mu^2/N) \text{ with } \mu \text{ of the order of } \sqrt{N}. \qquad (1)$$

In the second part of his memoir Bernoulli assumed that the probabilities of the birth of both sexes were in the ratio of *a:b*. Equating the probabilities of *m* and (*m* + 1) boys being born, again being given 2*N* births, he thus obtained the [expected] number of male births

$$\mathrm{E}m = M = \frac{2Na-b}{a+b} \approx \frac{2Na}{a+b}$$

which was of course evident. More interesting was Bernoulli's subsequent reasoning for determining the probability of an arbitrary *m* (for μ of the order of $\sqrt{N}$):

$$P(m = M + \mu + 1) - P(m = M + \mu) \equiv d\pi =$$

$$\pi - \pi\frac{a(2N - M - \mu)}{b(M + \mu + 1)}d\mu, \quad -\frac{d\pi}{\pi} = \frac{\mu + 1 + \mu a/b}{m + \mu + 1}d\mu.$$

The subsequent transformations included the expansion of ln[(*M* + 1 + μ)/(*M* + 1)] into a power series. Bernoulli's answer was

$$P(m = M \pm \mu) = \pi = P(m = M)\exp[-\frac{(a+b)\mu^2}{2bM}],$$

hence (1). Note that Bernoulli had not applied the local De Moivre (– Laplace) theorem.

2. Statistical Startups, Not Yet Explored Topics, Difficulties

Graunt (1662) was not sure whether anyone except *the Sovereign and his chief Ministers* needed statistics, but since then the situation has essentially changed, and especially with the creation of the welfare state and government decision making. Great changes have occurred with regard to natural sciences as well. Mostly in the 19th century a number of new disciplines linked to statistics have originated: medical

statistics (especially epidemiology), public hygiene (the forerunner of ecology), geography of plants, zoogeography, biometry, climatology, stellar statistics, and kinetic theory of gases. Many fundamental problems, such as the influence of solar activity on terrestrial phenomena have been studied statistically.

Just to illustrate the widest scope of statistics I mention two papers: Thornberg (1929) about the trade union movement (which showed an unusual aspect of the application of statistics in industry) and Thorp (1948) who described the use of statistics in foreign relations.

During the first five decades of the 19th century statistical institutions and/or national statistical societies came into being in the main states of Europe and America. International statistical congresses aiming at unification of official statistical data had been held from 1851 onward, and in 1885 the still active International Statistical Institute was established instead.

Throughout the 19th century the importance of statistics had been considerably increasing. By the mid-19th century it became important to foresee how various transformations influence society and Quetelet (§ 7.1) repeatedly stressed this point. Then, at the end of that century censuses of population, answering an ever widening range of questions, began to be carried out in various countries. However,
- Public opinion was not yet studied.
- Sampling had been considered doubtful. Cournot (1843) passed it over in silence and Laplace's sample determination of the population of France was largely forgotten. Quetelet opposed sampling. Much later Bortkiewicz (1904, p. 825) and Czuber (1921, p. 13) called sampling *conjectural calculation* although already the beginning of the century witnessed *legions* of new data (Lueder 1812, p. 9) and the tendency to amass sometimes useless or unreliable data revealed itself in various branches of natural sciences.

I adduce two barely known statements. In 1904, Newcomb had sent a letter to the Carnegie Institution urging it to establish an *institute or a bureau of exact sciences* for developing methods of dealing with the *great mass of existing observations* (*Methods* 1905, p. 180). Neither he, nor Pearson (p. 184), who was one of the several scientists, whom the Carnegie Institution asked to comment on Newcomb's proposal, mentioned sampling. Pearson argued that the situation was certainly bad and held that *at least 50 per cent of the observations made and the data collected are worthless*. Either the conditions necessary for testing a theory were not met or collectors or observers were *hopelessly ignorant* of the conditions required for accurate work. Owing to various difficulties, Newcomb's proposal was not adopted.

In 1915 or 1916, Chuprov mentioned the need to organize after the end of the world war, under the (Russian) Academy of Sciences, the studies of population and its productive forces (Sheynin 1990/2011, p. 130).
On the history of sampling, whose most active partisan was Kiaer, see You Poh Seng (1951) and Tassi (1988).
- The development of the correlation theory began at the end of the 19th century, but even much later Kaufman (1922, p. 152) declared

that *the so-called method of correlation adds nothing essential to the results of elementary analysis*.

● Variance began to be applied in statistics only after Lexis, but even later Bortkiewicz (1894 – 1896, Bd. 10, pp. 353 – 354) stated that the study of precision was a luxury, and that the statistical flair was much more important. This opinion had perhaps been caused by the presence of large systematic corruptions in the initial materials.

● Preliminary (or exploratory) data analysis (generally recognized only a few decades ago) was necessary, and should have been the beginning of the statistician's work.

● Statistical quality control had not been applied until the 1920s.

● Econometrics only originated in the 1930s as a blend of economics, statistics and mathematics (Frisch 1933, p. 1): the main object of the just established Econometric Society was to promote *unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems* and to foster *constructive and rigorous thinking similar to that which has come to dominate in the natural sciences*.

Poincaré, in an undated letter kept in his *Dossier* at the Paris Academy of Sciences (Sheynin 2009b, No. 619) quite positively described the work of Laurent both in probability and actuarial science and noted his *Traité* [1902] on mathematical political economy and lectures *dans un cours libre* at the Sorbonne on the same subject. Poincaré called this discipline a *science nouvelle créé par Walras et ses disciples*. So are Walras and Laurent the forerunners of econometrics?

I can also mention Petty and Bortkiewicz. Petty's essays on political arithmetic were *econometric in its methodological framework, even from the modern point of view* (Strotz 1978, p. 188). And Bortkiewicz

Made the necessary modifications that rendered the Marxian scheme of surplus values and prices consistent. However, his dry presentation prevented the Marxists (except for Klimpt [1936]) from accepting his method. And he had made *the lonely effort to construct a Marxian econometrics* [without applying statistical data] (Gumbel 1978, pp. 25 and 26).

Strotz (p. 189) also argues that econometrics *is disappearing as a special branch of economics*.

In conformity with the situation in the Soviet Union (Sheynin 1998) econometrics had hardly existed there. At an economic conference in 1960 Kolmogorov (Birman 1960, p. 44) stated that

The main difficult but necessary aim is to express the desired optimal state of affairs in the national economy by a single indicator.

Indeed, the prices of commodities had only been administratively established.

I list now the difficulties, real and imaginary, of applying the theory of probability to statistics.

● The absence of *equally possible* cases whose existence is necessary for understanding the classical notion of probability. Statisticians repeatedly mentioned this cause.

● Disturbance of the constancy of the probability of the studied event and/or of the independence of trials. Before Lexis (1879) statisticians had only recognized the Bernoulli trials; and even much later, again Kaufman (1922, pp. 103 – 104), argued that the theory of probability was applicable only to these trials, and, for that matter, only in the presence of equally possible cases.

● The abstract nature of the (not yet axiomatized) theory of probability. The history of mathematics testifies that the more abstract it became, the wider had been the range of its applicability.

In the beginning of the 1820s in a letter to Quetelet, Fourier maintained that the *statistical sciences* will only progress insofar as they were supported by *mathematical theories* (Quetelet 1826, p. 177). Soon, however, Quetelet (1828, p. i) called the calculus of probability (not just mathematics) the *basis of observational sciences*, and later (1869, t. 1, p. 134) *a most reliable and most indispensable companion* of statistics. Bortkiewicz (1904) expressed similar ideas.

For most statisticians all these pronouncements remained alien (see also § 8.3.3). They had not expected any help from the theory of probability. Block (1878/1886, p. 134) thought that it was too abstract and should not be applied *too often*, and Knapp (1872, p. 115) called it difficult and hardly useful beyond the sphere of games of chance and insurance. In 1911, G. von Mayr declared that mathematical formulas were not needed in statistics and privately told Bortkiewicz that he was unable to bear mathematics (Bortkevich & Chuprov 2005, Letter 109 of 1911). Bortkiewicz (1904, p. 822) also mentioned Guerry (1864, p. XXXIII ff) as an opponent of the application of the theory of probability and therefore his opponent as well.

I have noted (§ 2.1) that in 1835 several scientists including Poisson had stressed the connection between statistics and probability. A bit earlier three scholars, again including Poisson (Libri-Carrucci et al 1834), declared that *the most sublime problems of the arithmétique sociale* [see § 5] *can only be resolved with the help of the theory of probability.*

Nevertheless, statisticians never mentioned Daniel Bernoulli who published important statistical memoirs, almost forgot insurance, barely understood the treatment of observations, did not notice either Quetelet's mistakes or his inclinations to crime and to marriage (§ 7.1). After his death in 1874 they all the more turned away from probability.

Two circumstances worsened the situation. First, mathematicians often did not show how to apply their findings in practice. Poisson (1837a) is a good example; his student Gavarret (1840) turned physician simplified his formulas, but still insisted that conclusions should be based on a large number of observations which was often impossible (§ 2.1). Second, student-statisticians barely studied mathematics and, after graduation, did not trust it.

It is not amiss to mention here a pioneer attempt to create mathematical statistics (Wittstein 1867). He compared the situation in statistics with the *childhood* of astronomy and stressed that statistics (and especially population statistics) needed a Tycho and a Kepler to proceed from reliable observations to regularities. Specifically, he

noted that statisticians did not understand the essence of probability theory and never estimated the precision of the results obtained.

Knies (1850 [p. 163]) was *strongly in favour of adopting the name statistics for political arithmetic called by him mathematical statistics* (John 1883, p. 677). Hardly proper, but the term *mathematical statistics* was apparently thus first pronounced.

● A few words about astronomy and meteorology. In astronomy asteroids were understood to form a statistical population: their orbital parameters were studied statistically (Newcomb). From the mid-18th century (William Herschel) statistical reasoning was also applied to studying the arrangement and (later) the movement of stars and Kapteyn (1906) initiated an international plan for a sampling study of the stellar universe. In meteorology, Humboldt (1817) used statistical data on air temperatures to construct isotherms on a world-wide scale and thus to isolate the Earth's main climatic belts (more precisely, to confirm quantitatively their existence, qualitatively suggested by ancient geographers) and originate climatology. The introduction of contour lines for representing statistical information (a brilliant example of exploratory data analysis) was due to Halley (§ 1.3).

In general, Humboldt (1845 – 1862, Bd. 1, pp. 18 and 72; Bd. 3, p. 288) conditioned the investigation of natural phenomena by examination of mean states. In the last-mentioned case he mentioned *the sole decisive method* [in natural sciences], *that of the mean numbers* which (1845, Bd. 1, p. 82) *show us the constancy in the changes*. In other words, he stressed the importance of statistical studies.

Lamarck, the most eminent biologist of his time, seriously occupied himself with physics, chemistry and meteorology. In meteorology, his merits had for a long time been ignored (Muncke 1837), but he is now remembered for his *pioneer work in the study of weather* (Shaw & Austin (1926/1942, p. 130). He repeatedly applied the term *météorologie statistique* (e.g., 1800 – 1811, t. 4, p. 1) whose aim (Ibidem, t. 11, p. 9 – 10) was the study of climate, or, as he (Ibidem, t. 4, pp. 153 – 154) maintained elsewhere, the study of the climate, of regularities in the changes of the weather and of the influence of various meteorological phenomena on animals, plants and soil.

He preferred a *reasoned* rather than an empirical meteorology (Ibidem, t. 5, p. 1) with its own theory, general principles and aphorisms (Ibidem, t. 3, p. 104). At the time, such an approach was impossible but the development of statistical physics in the 19th century somewhat changed the situation (Angström 1929, p. 229).

Buys Ballot (1850, p. 629) noted the appearance of the second stage in the development of meteorology, the study of the deviations of meteorological elements from their mean values or states. He could have mentioned a few other sciences as well (for example, astronomy and geodesy, – and statistics!).

2.1. Medical Statistics. Interestingly enough, the expression *medical probability* appeared not later than in the mid-18th century (Mendelsohn 1761, p. 204). At the end of that century Condorcet (1795/1988, p. 542) advocated collection of medical observations and Black (1788, p. 65) even compiled a possibly forgotten *Medical*

catalogue of all the principle diseases and casualties by which the Human Species are destroyed or annoyed that reminded of Leibniz' thoughts. Descriptions belonging to other branches of natural sciences as well have actively been compiled (mostly later) and such work certainly demanded preliminary statistical efforts. Some authors mistakenly stated that their compilations ruled out the need for theories and, in addition, until the beginning of the 20th century the partisans of complete descriptions continued to deny sampling in statistics proper.

The range of application of the statistical method in medicine greatly widened after the emergence, in the mid-19th century, of public hygiene (largely a forerunner of ecology) and epidemiology. About the same time surgery and obstetrics, branches of medicine proper, yielded to the statistical method.

Public hygiene began statistically studying problems connected with the Industrial Revolution in England and, in particular, by the great infant mortality (Chadwick 1842/1965, p. 228). Also, witness Farr (ca. 1857/1885, p. 148): *Any deaths in a people exceeding 17 in a 1,000 annually are unnatural deaths*. Unnatural, but common!

Epidemiology was properly born when cholera epidemics had been ravaging Europe. Snow (1855) compared mortality from cholera for two groups of the population of London, whose drinking water was either purified or not, ascertained that purification decreased mortality by eight times, and thus discovered how did cholera epidemics spread. The need to combat the devastating visitations of cholera was of utmost importance.

No less important was the study of prevention of smallpox. The history of smallpox epidemics and inoculation, the communication of a mild form of smallpox from one person to another, is described in various sources (Condamine 1759, 1763, 1773; Karn 1931). In his first memoir, Condamine listed the objections against inoculation, both medical and religious.

Indeed, White (1896/1898) described the *warfare of science with theology* including, in vol. 2, pp. 55 – 59, examples of fierce opposition to inoculation (and, up to 1803, to vaccination of smallpox). Many thousands of Canadians perished in the mid-19th century only because, stating their religious belief, they had refused to be inoculated. White clearly distinguished between theology, the opposing force, and "practical" religion.

Karn stated at the very beginning of her article that

The method used in this paper for determining the influence of the death-rates from some particular diseases on the duration of life is based on suggestions which were made in the first place by D. Bernoulli.

Daniel Bernoulli (1766) justified inoculation. That procedure, however, spread infection, was therefore somewhat dangerous for the neighbourhood and prohibited for some time, first in England, then in France. Referring to statistical data, but not publishing it, Bernoulli specified the yearly rates of the occurrence of smallpox in those who have not had it before and of the corresponding mortality and assumed that the inoculation itself proved fatal in 0.5% of cases.

He formed the appropriate differential equation whose solution showed the relation between age in years and the number of people of that age and, in addition, of those who had not contacted smallpox. Also by means of a differential equation he derived a similar formula for a population undergoing inoculation, that is, for its 99.5% which safely endured it and were not anymore susceptible to the disease. It occurred, that inoculation lengthened the mean duration of life by 3 years and 2 months and that it was therefore, in his opinion, extremely useful. The Jennerian vaccination, – *the inestimable discovery by Jenner, who has thereby become one of the greatest benefactors of mankind* (Laplace 1814/1995, p. 83), – was introduced at the end of the 18th century. Its magnificent success had not however ruled out statistical studies. Thus, Simon (1887, vol. 1, p. 230) formulated a question about the impermanence of protection against post-vaccinal smallpox and concluded that only comprehensive national statistics could have provided an answer.

D'Alembert (1761; 1768) criticized Daniel Bernoulli. Not everyone will agree, he argued, to lengthen his mean duration of life at the expense of even a low risk of dying at once of inoculation; then, moral considerations were also involved, as when inoculating children. D'Alembert concluded that statistical data on smallpox should be collected, additional studies made and that the families of those dying of inoculation should be indemnified or given memorial medals.

He also expressed his own thoughts, methodologically less evident but applicable to studies of even unpreventable diseases. Dietz & Heesterbeek (2002) described Bernoulli's and D'Alembert's investigations on the level of modern mathematical epidemiology and mentioned sources on the history of inoculation.

Seidel (1865; 1866), a German astronomer and mathematician, quantitatively estimated the dependence of the number of cases of typhoid fever on the level of subsoil water and precipitation but made no attempt to generalize his study, to introduce correlation.

Already in 1839 there appeared (an unconvincing) statistical study of the amputation of limbs. J. Y. Simpson (1847 – 1848/1871, p. 102) mistakenly attempted to obtain reliable results about that operation by issuing from materials pertaining to several English hospitals during 1794 – 1839. Indeed, physicians learned that the new procedure, anaesthesia, could cause complications, and began to compare statistically the results of amputations made with and without using it.

Simpson (1869 – 1870/1871, title of contribution) also coined the term *Hospitalism* which is still in vogue. He compared mortality from amputations made in various hospitals and reasonably concluded, on the strength of its monotonous behaviour, that mortality increases with the number of beds; actually (p. 399), because of worsening of ventilation and decrease of air space per patient. Suchlike justification of conclusions was not restricted to medicine, cf. Quetelet's study of probabilities of conviction of defendants (§ 7.1).

In the mid-19th century Pirogov began to compare the merits of the conservative treatment of the wounded versus amputation. Later he (1864, p. 690) called his time *transitional*:

Statistics shook the sacred principles of the old school, whose views had prevailed during the first decades of this century, – and we ought to recognize it, – but it had not established its own principles.

Pirogov (1849, p. 6) reasonably believed that the application of statistics in surgery was in *complete agreement* with the latter because surgical diseases depended incomparably less on individual influences but he indicated that medical statistics was unreliable, that (1864/1865 – 1866, p. 20) a general impression based on sensible observation of cases was better. He (1879/1882, p. 40) singled out *extremely different circumstances* and stressed (1871, pp. 48 – 49) the importance of *efficient administration.* Pirogov participated in the Crimean war, in which Florence Nightingale, on the other side, showed her worth both as a medical nurse and a statistician. She would have approved of Pirogov's conclusion above.

Pirogov was convinced in the existence of regularities in mass phenomena. Thus (1850 – 1855/1961, p. 382), each epidemic disease as well as each *considerable* operation had a constant mortality rate, whereas war was a *traumatic epidemic* (1879/1882, p. 295). This latter statement apparently meant that under war conditions the sickness rate and mortality from wounds obeyed statistical laws. Then (1854, p. 2), the skill of the physicians [but not of witch doctors] hardly influenced the total result of the treatment of many patients. On Pirogov see Sheynin (2001a).

A French physician Louis (1825) introduced the so-called numerical method of studying symptoms of various diseases. His proposal had been applied much earlier in various branches of science. It amounted to the use of the statistical method without involving stochastic considerations which. Quantitative data were also collected in agriculture, meteorology, astronomy etc.; astronomical catalogues, for example, fall in the same category. Nevertheless, this line of development was not sufficient. Discussions about the numerical method lasted at least a few decades. Thus, d'Amador (1837) attacked Louis wrongly attributing to him a recommendation to use the theory of probability.

Tolstoy (1884 – 1886/2003, p. 27) apparently mocked that, not generally accepted anymore approach, by stating that physicians only compared the probabilities of various possible illnesses without really caring about their patients.

Gavarret (1840) noted the shortcomings of the numerical method and adduced examples on the comparison of competing methods of medical treatment as also an advice on the check of the null hypothesis (as it is now called), see p. 194. Thus, apart from popularizing probability theory, Gavarret's main achievement was the introduction of the principle of the null hypothesis and the necessity of its check into medicine (actually, in natural science in general).

Laplace (1798 – 1825/1878 – 1882, t. 3, ca. 1804, p. xi; 1814/1995, p. 116) argued that the adopted hypotheses ought to be *incessantly rectified by new observations* until *veritable causes or at least the laws of the phenomena* be discovered. Cf. Double et al (1835, pp. 176 – 177): the main means for revealing the *vérité* were induction,

analogy and hypotheses founded on facts and *incessantly verified and rectified by new observations.*

Gavarret's contribution became generally known and many authors repeated his recommendations. The time for mathematical statistics or for its application in medicine was not yet ripe, but at least the Poisson – Gavarret tradition led to the existence, in medicine, of a lasting drive towards the use of probability based on numerous observations (and the skill of the physician). Indeed, Poisson (1837a, Note to Annotated Contents) stated that *Medicine will not become either a science or an art if not based on numerous observations, on the tact and proper experience of the physicians …*

A few years earlier Double, Poisson et al (1835) also maintained that statistics was *the functioning mechanism of the calculus of probability, necessarily concerning infinite* [?] *masses …* Cournot (1843, § 103) and Rümelin (1863 – 1864/1875, p. 222) were of the same opinion.

A large number of observations! However, at least from the mid-18th century (Bull 1959, p. 227) valuable medical conclusions had been based on very small numbers of them, but it was Liebermeister (ca. 1876) who vigorously opposed Gavarret and Poisson. He argued that it was impossible, in therapeutics, to collect vast observations and, anyway, that recommendations based on several (reliable) observations should be adopted as well. Statisticians have only quite recently discovered his paper written as though by a specialist in mathematical statistics. Then, at least from Gossett (Student) onwards small samples became necessary for statistics. For his life and work see E. S. Pearson (1990).

2.2. Juridical Statistics. Niklaus Bernoulli published a dissertation on the application of the art of conjecturing to jurisprudence (1709/1975). It contained the calculation of the mean duration of life and recommended its use for ascertaining the value of annuities and estimating the probability of death of absentees about whom nothing is known; methodical calculations of expected losses in marine insurance; calculation of expected losses in the celebrated Genoese lottery and of the probability of truth of testimonies; the determination of the life expectancy of the last survivor of a group of men (pp. 296 – 297), see Todhunter (1865, pp. 195 – 196). Assuming a continuous uniform law of mortality (the first continuous law in probability theory), he calculated the expectation of the appropriate order statistic and was the first to use, in a published work, both this distribution and an order statistic.

Bernoulli's work undoubtedly fostered the spread of stochastic notions in society, but he borrowed separate passages from the *Ars Conjectandi* and even from the *Meditationes* (Kohli 1975, p. 541), never intended for publication. His general references to Jakob, his late uncle, do not excuse his plagiarism the less so since he dedicated his work not to the memory of Jakob, but to his father Johann.

Condorcet, Laplace and Poisson actively studied the application of probability and statistics to jurisprudence. Todhunter (1865, p. 352) concluded that *The obscurity and self contradictions* in the work of Condorcet *are without any parallel*, but Poisson (1837a, pp. 2 and 5)

favourably mentioned his ideas. As to Laplace, it seems that his main achievement consisted in drawing Poisson's attention to the administration of justice.

Poisson (1837a, pp. 1 – 2) thought that the study of the probabilities of verdicts and, in general, of majority decisions, was a most important application of the calculus of probability. He (p. 17) perceived his main goal in that field as an examination of the stability of the rate of conviction and of the probability of miscarriage of justice as well as in the comparison of judicial statistics of different countries and (p. 7) in proving the applicability of mathematical analysis *to things that are called moral*.

Poisson was mainly interested in studying criminal offences. Unlike Laplace, he (p. 4 and § 114, p. 318) introduced a positive probability of the defendant's guilt (not to be taken into account in any individual case). One of his statements (§ 136, pp. 375 – 376) is debatable: he thought that the rate of conviction should increase with crime.

Poisson estimated the (beneficial or otherwise) changes in the rate of conviction following changes in the administration of justice (in the number of jurors, in the majority vote needed for conviction). I do not know, however, whether his calculations had been taken into account.

Neither Condorcet, nor Poisson mentioned that they had assumed that the jurors reach decisions independently from each other whereas Laplace (1816, p. 523) only said so in passing.

The application of the theory of probability to jurisprudence continued to be denied. Here are the two most vivid pertinent statements (Mill 1843/1886, p. 353; Poincaré 1896/1912, p. 20):

1) *Misapplications of the calculus of probability* […] *made it the real opprobrium of mathematics. It is sufficient to refer to the applications made of it to the credibility of witnesses, and to the correctness of the verdicts of juries.*

2) People *influence each other* and act like *the moutons de Panurge*.

The higher is a scientist's standing, the more reserved he ought to be when invading an alien field. Even Mill, not to mention Poincaré, should not have categorically condemned a subject of which he was ignorant.

It is opportune to cite Gauss whose opinion was voiced by W. E. Weber in a letter of 1841 to J. F. Fries (Gauss, W-12, pp. 201 – 204): probability can serve as a guide line for determining the desired number of jurors and witnesses. Fries had then been preparing his book on the principles of the theory of probability; it appeared in 1842. Then, juridical statistics effectively applied the notion of errors of both kinds.

2.3. Insurance of Property and Life Insurance. Marine insurance was the first essential type of insurance of property but it lacked stochastic ideas or methods. In particular, there existed an immoral and repeatedly prohibited practice of betting on the safe arrivals of ships. Anyway, marine insurance had been apparently based on rude and subjective estimates.

And here is a quote from the first English Statute on assurance (Publicke Acte No. 12, 1601; *Statutes of the Realm*, vol. 4, pt. 2, pp. 978 – 979):

And whereas it hathe bene tyme out of mynde an usage amongste merchants, both of this realme and of forraine nacyons, when they make any great adventure, […] to give some consideracion of money to other persons […] to have from them assurance made of their goodes, merchandizes, ships, and things adventured, […] whiche course of dealinge is commonly termed a policie of assurance […].

Life insurance came into its own not by a front-door entrance, but by the marine insurance porthole (O'Donnell 1936, p. 78) … It exists in two main forms. Either the insurer pays the policy-holder or his heirs the stipulated sum on the occurrence of an event dependent on human life; or, the latter enjoys a life annuity. Annuities were known in Europe from the 13th century onward although later they were prohibited for about a century until a Papal bull officially allowed it in 1423 (Du Pasquier 1910, pp. 484 – 485). The annuitant's age was not usually taken into consideration either in the mid-17th century (Hendriks 1853, p. 112), or even, in England, during the reign of William III [1689 – 1702] (K. Pearson 1978, p. 134). Otherwise, as it seems, the ages had been allowed for only in a very generalized way (Kohli & van der Waerden 1975, pp. 515 – 517; Hald 1990, p. 119). At the end of the 17th century the situation began to change.

In the 18th, and even in the mid-19th century, life insurance still hardly essentially depended on stochastic considerations; moreover, the statistical data collected by the insurance societies as well as their mortality tables and methods of calculations remained secret. And more or less honest business based on statistics of mortality hardly superseded downright cheating before the second half of the 19th century. Nevertheless, beginning at least from the 18th century, the institute of life insurance which essentially depended on studies of mortality strongly influenced the theory of probability and turned the attention of scholars to medical and social problems.

Tontines constituted a special form of mutual insurance. Named after the Italian banker Laurens Tonti (Hendriks 1863), they, acting as a single body, distributed the total sums of annuities among their members still alive, so that those, who lived longer, received considerable moneys. Tontines were neither socially accepted nor widespread on the assumed rationale that they were too selfish and speculative (Hendriks 1853, p. 116). Nevertheless, they did exist in the 17th century. Euler (1776) devised a tontine with flexible moments of entering it, flexible ages of its members and of their contributions (therefore, of their annual income as well). Such a tontine could have theoretically existed forever rather than disappearing with the death of its last member. Euler's innovation was apparently never taken up.

De Moivre first examined life insurance in the beginning of the 1720s and became the most influential author of his time in that field. Issuing from Halley's table, he (1725/1756, pp. 262 – 263) assumed a continuous uniform law of mortality for all ages beginning with 12 years and a maximal duration of life equal to 86 years.

Hald (1990, pp. 515 – 546) described in detail the work of De Moivre and of his main rival, Simpson (1775), in life insurance. Simpson improved on, and in a few cases corrected the former's

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findings. After discussing one of the versions of mutual insurance, Hald (p. 546) concluded that Simpson's relevant results represented an essential step forward; however, his attitude to De Moivre showed him as an *unblushing liar* (K. Pearson 1978, p. 184).

Daniel Bernoulli (1768b) investigated the duration of marriages for differing ages of man and wife which was important for insurance on two lives. He based his analysis on another study (1768a) of extracting pairs of white and black stripes from an urn with the respective probabilities being equal or unequal.

Laplace (1814/1995, p. 89) compared *free people* to *an association whose members mutually protect their property* and went on to praise institutions based on the probabilities of human life. Markov collaborated with pension funds (Sheynin 1997) and in 1906, in a newspaper, he destructively criticized a proposed official scheme for insuring children (reprinted in same article).

Actuarial science inevitably led to the compilation of life tables and their improvement. Quetelet & Smits (1832, p. 33) stated that separate tables for men and women had *only recently* begun to be published. However (Nordenmark 1929, p. 250), Wargentin compiled such separate tables for Sweden in 1766.

Then, many authors noted that the expectation of life of the general male (say) population is either larger or smaller than that of men from selected populations (e. g., from monks). Note a related remark made by Buffon (1777/1954, § 8, note) in 1762, in a letter to Daniel Bernoulli and thus to some extent foreshadowing Quetelet's Average man:

Mortality tables are always concerned with the average man; that is, with people in general, feeling themselves quite well or ill, healthy or infirm, robust or feeble.

Andersson (1929, p. 239) voiced a serious complaint:

The State does not [do] *much* […] *to protect and forward the sound practice of insurance.* […] *State statistics should pay regard to all the desires of insurance and try to meet them.* […] *No country has as yet suitable fire statistics, no shipping statistics are* [is] *being performed with due attention to the special demands of marine insurance.* […] *The insurance itself* […] *still has not given the due place to statistics in the scientific insurance work.*

2.4. Earliest Stochastic and Statistical Investigations

2.4.1. Pascal and Fermat. In 1654 Pascal and Fermat exchanged several letters (Pascal 1654) which heralded the beginning of the formal history of probability. They discussed several problems; here is the most important of them which was known even at the end of the 14th century. Two or three gamblers agree to continue playing until one of them scores *n* points; for some reason the game is interrupted and it is required to divide the stakes in a reasonable way. Both scholars solved this *problem of points*, see Takácz (1994), by issuing from one and the same rule: the winnings of the gamblers should be in the same ratio(s) as existed between the expectations of their scoring the *n* points. The actual introduction of that notion, expectation, was their main achievement. They also effectively applied the addition and the multiplication theorems.

The methods used by Pascal and Fermat differed from each other. In particular, Pascal solved the above problem by means of the arithmetic triangle (Edwards 1987) composed, as is well known, of binomial coefficients of the development $(1 + 1)^n$ for increasing values of n. Pascal's relevant contribution (1665) was published posthumously, but Fermat was at least partly familiar with it. Both there, and in his letters to Fermat, Pascal made use of partial difference equations (Hald 1990, pp. 49 and 57).

The celebrated Pascal wager (1669/2000, pp. 676 – 681), also published posthumously, was a discussion about choosing a hypothesis. Does God exist, rhetorically asked the devoutly religious author, and answered: you should bet. If He does not exist, you may live calmly [and sin]; otherwise, however, you can lose eternity. In the mathematical sense, Pascal's reasoning is vague; perhaps he had no time to edit his fragment. Its meaning is, however, clear: if God exists with a fixed and however low probability, the expectation of the benefit accrued by believing in Him is infinite. Pascal died in 1662 and the same year Arnauld & Nicole (1662/1992, p. 334) published a similar statement:

Infinite things, like eternity and salvation, cannot be equated to any temporal advantage. […] *We should never balance them with any worldly benefit.* […] *The least degree of possibility of saving oneself is more valuable than all the earthly blessings taken together, and the least peril of losing that possibility is more considerable than all the temporal evils* […].

2.4.2. Huygens. Huygens was the author of the first treatise on probability (1657). Being acquainted only with the general contents of the Pascal – Fermat correspondence, he independently introduced the notion of expected random winning and, like those scholars, selected it as the test for solving stochastic problems. He went on to prove that the value of expectation of a gambler who gets a in p cases and b in q cases was

$$\frac{pa + qb}{p + q}. \tag{1}$$

Jakob Bernoulli (1713/1999, p. 9) justified the expression (1) much simpler than Huygens did: if each of the p gamblers gets a, and each of the q others receives b, and the gains of all of them are the same, then the expectation of each is equal to (1). After Bernoulli, however, expectation began to be introduced formally: expressions of the type of (1) followed by definition.

Huygens solved the problem of points under various initial conditions and listed five additional problems two of which were due to Fermat, and one, to Pascal. He solved them later, either in his correspondence, or in manuscripts published posthumously. They demanded the use of the addition and multiplication theorems, the introduction of conditional probabilities and the formula (in modern notation)

$P(B) = \Sigma P(A_i)P(B/A_i)$, $i = 1, 2, \ldots, n$.

Problem No. 4 was about sampling without replacement. An urn contained 8 black balls and 4 white ones and it was required to determine the ratio of chances that in a sample of 7 balls 3 were, or were not white. Huygens determined the expectation of the former event by means of a partial difference equation (Hald 1990, p. 76). Nowadays such problems leading to the hypergeometric distribution (Jakob Bernoulli 1713/1999, pp. 167 – 168; De Moivre 1712/1984, Problem 14 and 1718/1756, Problem 20) appear in connection with statistical quality control.

Pascal's Problem No. 5 was the first to discuss the gambler's ruin. Gamblers A and B undertake to score 14 and 11 points respectively in a throw of 3 dice. They have 12 counters each and it is required to determine the ratio of the chances that they be ruined. The stipulated numbers of points occur in 15 and 27 cases and the ratio sought is therefore $(5/9)$[12].

In 1669, in a correspondence with his brother, Huygens (1888 – 1950, 1895), see Kohli & van der Waerden (1975), discussed stochastic problems connected with mortality and life insurance. Issuing from Graunt's mortality table (§ 1.2), Huygens (pp. 531 – 532) introduced the probable duration of life (but not the term itself). He also showed that the probable duration of life can be determined by means of the graph (plate between pp. 530 and 531) of the function $y = 1 - F(x)$, where, in modern notation, $F(x)$ was a remaining unknown integral distribution function with admissible values of the argument being $0 \leq x \leq 100$.

In the same correspondence Huygens (p. 528) examined the expected period of time during which 40 persons aged 46 will die out; and 2 persons aged 16 will both die. The first problem proved too difficult, but Huygens might have remarked that the period sought was 40 years (according to Graunt, 86 years was the highest possible age). He mistakenly solved a similar problem by assuming that the law of mortality was uniform and that the number of deaths will decrease with time: for a distribution, continuous and uniform in some interval, n order statistics will divide it into $(n + 1)$ approximately equal parts and the annual deaths will remain about constant. In the second problem Huygens applied conditional expectation. When solving problems on games of chance, Huygens issued from expectations which varied from set to set rather than from constant probabilities and was compelled to compose and solve difference equations. See also Shoesmith (1986).

2.4.3. Newton left interesting ideas and findings pertaining to probability, but more important were his philosophical views (K. Pearson 1926):

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 […]. De Moivre expanded the Newtonian theology and directed statistics into the new channel down which it flowed for nearly a century. The cause which led De Moivre to his

Approximatio [1733] *or Bayes to his theorem were more theological and sociological than purely mathematical, and until one recognizes 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.*

Bayes theorem is a misnomer (§ 2.4.7). Then, Newton never mentioned mean values. In 1971, answering my question on this point, the Editor of his future book (1978), E. S. Pearson, stated:

From reading [the manuscript of that book] *I think I understand what K. P. meant.* […] *He had stepped ahead of where Newton had to go, by stating that the laws which give evidence of Design, appear in the stability of the mean values of observations. i. e.,* [he] *supposed that Newton was perhaps unconsciously thinking what De Moivre put into words.*

Indeed, K. Pearson (1978, pp. 161 and 653) had attributed to De Moivre (1733/1756, pp. 251 – 252) the Divine *stability of statistical ratios, that is, the original determination of original design* and referred to Laplace who (1814/1995, p. 37) had formulated a related idea:

In an infinitely continued sequence of events, the action of regular and constant causes ought, in the long run, to outweigh that of irregular causes.

However, Laplace never mentioned Divine design. And here is Newton's most interesting pronouncement (1704/1782, Query 31):

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.

Newton's recognition of the existence and role of random disturbances is very important. At the same time Newton (1958, pp. 316 – 318) denied randomness and explained it by ignorance of causes.

Newton (MS 1664 – 1666/1967, pp. 58 – 61) was the first to mention geometric probability: *If the Proportion of the chances* […] *bee irrational, the interest may bee found after ye same manner.* Newton then considered a throw of an irregular die. He remarked that [nevertheless] *it may bee found how much one cast is more easily gotten than another*. He likely bore in mind statistical probabilities. Newton (1728, p. 52) also applied simple stochastic reasoning for correcting the chronology of ancient kingdoms:

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.*

Newton derived his own estimate from other chronological data and his rejection of the twice longer period was reasonable. Nevertheless, a formalized reconstruction of his decision is difficult: within one and the same dynasty the period of reign of a given king directly depends on that of his predecessor. Furthermore, it is impossible to determine the probability of a large deviation of the value of a random variable from its expectation without knowing the appropriate variance (which Newton estimated only indirectly and in a generalized way). And here is the opinion of Whiteside (private communication, 1972) about his thoughts concerning errors of observation:

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 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 ...*

2.4.4. Arbuthnot. See § 1.3.1-1.

2.4.5. Jakob Bernoulli. His *Ars Conjectandi* (1713) appeared posthumously; Niklaus Bernoulli compiled a Preface (Jakob Bernoulli 1975, p. 108) where, for the first time ever, the term *calculus of probability* (in Latin) had appeared. The book itself contained four parts. Interesting problems are solved in parts 1 (a reprint of Huygens' tract, see § 2.4.2) and 3 (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.

In the beginning of pt. 4 Bernoulli explained that the theoretical number of cases was often unknown, but what was impossible to obtain beforehand, might be determined by observations. In his Diary (*Meditationes*), whose stochastic considerations were only published in Bernoulli (1975), he indirectly cited Graunt and reasoned how much more probable it was that a youth outlives an old man than vice versa.

Bernoulli maintained that moral certainty ought to be admitted on a par with absolute certainty. His theorem will show, he declared, that statistical probability was a morally certain (a *consistent*) estimator of the theoretical probability. It was Descartes (1644/1978, pt. 4, No. 205, 483°, p. 323) who introduced moral certainty *for regulating our morals* (moeurs).

Actually, Bernoulli strictly proved a proposition that, beginning with Poisson, is called the LLN. Denote the statistical probability of the occurrence of the studied event in a trial by $\hat{p}$ and the theoretical probability of the event by p; assume that n independent Bernoulli trials in which $p = $ Const are made. Then, as $n \rightarrow \infty$,

$$\lim P(\hat{p} - p) = 1. \tag{2}$$

This is an existence theorem and Bernoulli properly stated that it signified that [for the Bernoulli trials] induction (the trials) was not worse than deduction (the theoretically determined p). Had the right side of (2) be a proper fraction, he added, induction would have been worse.

His direct LLN thus determined $\hat{p}$ whereas, as stated above, he initially stated that $\hat{p}$ was a morally certain estimate of p; moreover, he even adduced an appropriate example in which p did not even exist. This initial statement is called the *inverse* LLN, and Bernoulli mistakenly believed that any version of that law led to the other version.

Bernoulli also estimated the rapidity of the convergence of one probability to the other; however, not knowing the later discovered Stirling theorem, his estimation was not good enough. Without noticing the existence theorem K. Pearson (1925) denied Bernoulli's great achievement and even compared it with the wrong Ptolemaic system of the world.

As Cournot (1843, § 86) emphasized, although not really definitely, stochastic reasoning was now justified beyond the province of games of chance, at least for the Bernoulli trials. Strangely enough, statisticians for a long time had not recognized this fact. Haushofer (1872, pp. 107 – 108) declared that statistics, since it was based on induction, had no intrinsic connections with mathematics based on deduction. And Maciejewski (1911, p. 96) introduced a statistical LLN instead of the Bernoulli proposition that allegedly impeded the development of statistics. His new law qualitatively asserted that statistical indicators exhibited ever lesser fluctuations as the number of observations increased and his opinion likely represented the prevailing attitude of statisticians. Bortkiewicz (1917, pp. 56 – 57) thought that the LLN ought to denote a quite general fact, unconnected with any stochastic pattern, of a degree of stability of statistical indicators under constant or slightly changing conditions and a large number of trials. Even Romanovsky (1961, p. 127) kept to a similar view.

2.4.6. De Moivre. For $n \rightarrow \infty$ De Moivre's main result of 1733 concerning Bernoulli trials can be written as

$$\lim P \left[a \leq \frac{\mu - np}{\sqrt{npq}} \leq b \right] = \frac{1}{\sqrt{2\pi}} \int_a^b \exp(-\frac{z^2}{2}) dz. \tag{3}$$

Here μ is the number of successes, $np = E\mu$ and $npq = \mathrm{var}\mu$.

This is the integral De Moivre – Laplace theorem, as Markov (1900/1924, p. 53) called it, – a particular case of the CLT. Neither De Moivre, nor Laplace knew about uniform convergence with respect to a and b that takes place here. De Moivre proved (3) in a short Latin memoir of 1733 which he sent around to his colleagues and then

translated it into English and incorporated in the editions of 1738 and 1756 of his *Doctrine of Chances.*

Laplace (1812) proved (3) simpler and provided a correction term allowing for the finiteness of *n*. De Morgan (1864) was the first to notice the normal distribution in (3) but he also made unbelievably wrong statements about the appearance of negative probabilities and those exceeding unity. More: in a letter of 1842 he (Sophia De Morgan 1882, p. 147) declared that $\tan\infty = \cot\infty = \mp\sqrt{-1}$.

2.4.7. Bayes. I dwell on the posthumous memoir (Bayes 1764 – 1765) complete with the commentaries by R. Price. In its first part Bayes introduced his main definitions and proved a few theorems; note that he defined probability through expectation. There was no hint of the so-called Bayes theorem introduced by Laplace (1812/1886, p. 183)

$$P(A_i/B) = \frac{P(b/a_j)P(A_i)}{\sum_{j=1}^{n} P(b/a_j)P(A_i)},\qquad (4)$$

as Cournot (1843, § 88), actually following predecessors, called it.

Here is the real Bayes' theorem in a simplified description (Gnedenko 1950/1954, p. 366). It is required to determine the unknown probability *r* having continuous uniform density on interval [0, 1] if after *n* = *p* + *q* (independent) trials it occurred *p* times and failed *q* times. Answer:

$$P(a \leq r \leq b) = \int_a^b u^p (1-u)^q \, du \div \int_0^1 u^p (1-u)^q \, du. \qquad (5)$$

Here, [a, b] is a segment within [0, 1]. Bayes derived the denominator of (5) obtaining the value of the [beta-function] B(*p*+ 1; *q* + 1) and spared no effort in estimating its numerator, a problem that remained difficult until the 1930s. The right side of (5) is now known to be equal to the difference of two values of the incomplete beta-function

$I_b(p + 1; q + 1) - I_a(p + 1; q + 1).$

Beginning with the 1930s and perhaps for three decades English and American statisticians had been denying Bayes after which his theorem *has returned from the cemetery* (Cornfield 1967).
The first and the main critic of the Bayes *theorem* or formula was Fisher (1922, pp. 311 and 326). It seems that he disagreed with the introduction of hardly known prior probabilities and/or with the assumption that they were equal to one another.

Bayes had not expressly discussed the case of $n \to \infty$. In another posthumous note published in 1764 he warned mathematicians about the danger of applying divergent series. He had not named De Moivre, but apparently had in mind his derivation of the De Moivre – Laplace theorem as well. De Moivre and his contemporaries had indeed

employed convergent parts of divergent series for approximate calculations, and about a century later Poisson (1837a, § 68, p. 175) stated that that trick was possible. Note that divergent series are now included in the province of mathematics.

Timerding, the Editor of the German translation of the Bayes memoir (1908), managed to consider the limiting case without applying divergent series. He issued from Bayes' calculations made for large but finite values of p and q. Applying a clever trick, he proved that, as $n \to \infty$, the probability of the studied event obeyed the proposition

$$\lim P\{-z \le \frac{r-a}{\sqrt{pqn^{3/2}}} \le z\} = \frac{1}{\sqrt{2\pi}} \int_0^z \exp(-\frac{w^2}{2})dw, \qquad (6)$$

where (not indicated by Timerding) $a = p/n = \mathrm{E}r,\ pq/n^3 = \mathrm{var}r$ so that $r \approx \mathrm{E}r = p/n$.

The assumption of a uniform density is not a restriction; according to information theory, it is tantamount to a statement of ignorance. The influence of a non-uniform density taking place apparently decreases with the increase of n (with the increase in posterior information).

The functions in the left sides of formulas (3) and (6) are different random variables, centred and normed in the same way; Bayes, without knowing the notion of variance, apparently understood that (3) was not sufficiently precise for describing the problem inverse to that studied by De Moivre, who (1718/1756, p. 251) mistakenly thought otherwise (as Jakob Bernoulli also did). Note that, unlike the direct law, its inverse counterpart has less initial data (the theoretical probability is unknown) which qualitatively explains the situation.

A modern encyclopaedia (Prokhorov 1999) contains 14 items mentioning Bayes, for example, Bayesian estimator, Bayesian approach (and many more items are mentioned elsewhere). There also, on p. 37, the author of the appropriate entry mistakenly attributes formula (4) to Bayes. For my part, I believe that, since Bayes had correctly interpreted the inverse LLN, he thus completed the first stage of the theory of probability. Moreover, he and his predecessors, Jakob Bernoulli (§ 2.4.5) and De Moivre (end of § 1.3 and § 2.4.6) provided strict proofs whereas Laplace resolutely transferred probability to applied mathematics (and Poisson followed him).

Bayes was also the main predecessor of Mises (who never acknowledged it). And when a statistician starts working, he invariably has to issue from some statistical probability. If and when justifying this step, he refers to Mises, but he could have mentioned Bayes instead.

3. Treatment of Observations

3.1. The following **explanation** will be needed below. Denote the observations of a constant sought by

$$x_1, x_2, \ldots, x_n, x_1 \le x_2 \le \ldots \le x_n. \qquad (1)$$

It is required to determine its value, optimal in some sense, and estimate the residual error. The classical theory of errors considers independent observations and, without loss of generality, they might also be regarded as of equal weight. This problem is called *adjustment of direct observations*.

Suppose now that k unknown magnitudes $x, y, z, \ldots$ are connected by a redundant system of n physically independent equations ($k < n$)

$$a_i x + b_i y + c_i z + \ldots + s_i = 0 \tag{2}$$

whose coefficients are given by the appropriate theory and the free terms are measured. The approximate values of $x, y, z, \ldots$ were usually known, hence the linearity of (2). The equations are linearly independent (a later notion), so that the system is inconsistent (which was perfectly well understood). Nevertheless, a solution had to be chosen, and it was done in such a way that the residual free terms (call them v_i) were small enough. The values of the unknowns thus obtained are called their estimates ($\hat{x}, \hat{y}, \ldots$) and this problem is called *adjustment of indirect measurements*.

Since the early 19$^{\text{th}}$ century the usual condition for solving (2) was that of least squares

$$W = \sum v_i^2 = [vv] = v_1^2 + v_2^2 + \ldots + v_n^2 = \min, \tag{3}$$

so that

$$\partial W / \partial x = \partial W / \partial y = \ldots = 0. \tag{4}$$

Conditions (4) easily lead to a system of normal equations

$$[aa]\,\hat{x} + [ab]\,\hat{y} + \ldots + [as] = 0, \; [ab]\,\hat{x} + [bb]\,\hat{y} + \ldots + [bs] = 0, \; \ldots,$$

with a positive definite and symmetric matrix. For direct measurements the same condition (3) leads to the arithmetic mean.

There also existed a determinate branch of the theory of errors now partly superseded by experimental design. It studies the process of measurement without applying stochastic reasoning. Here is a simplest example: determine the form of a geodetic figure ensuring optimal (in some sense) results. The real development of the determinate error theory was due to the differential calculus which ensured the study of the sought functions of measured magnitudes, but even Hipparchus was aware that, under favourable conditions, a given error of observation can comparatively little influence the unknown sought (Toomer 1974, p. 131), see also below.

Gauss and Bessel originated a new direction in practical astronomy and geodesy. They demanded and carried out thorough examinations of the instruments and investigations of the plausibility of the methods of observation. This direction belonged to the determinate error theory.

Now, the design of experiments is a branch of mathematical statistics dealing with the rational organization of measurements subject to random errors (*Enc. Math.* 1977 – 1985/1988 – 1994, vol. 3, p. 66). Finney (1960), however, argued that this new discipline does not entirely belong to the *mathematical theory of statistics*, but did not elaborate. I would say, belongs to theoretical statistics, see § 7.2.

The design of experiments ought to include the choice of optimal methods and circumstances of observation, design of instruments capable of using such methods etc. (Box 1964). Many of such problems have nothing to do with randomness; and they undoubtedly belonged to the determinate error theory.

Some Russian authors (Romanovsky 1955; Bolshev 1989) state that the stochastic theory of errors belongs to statistics, but it seems more natural to define it as the application of the statistical method to the treatment of observations in experimental science, see § 9. Romanovsky excluded systematic errors from their consideration; Bolshev agreed and attributed their study to a special discipline, the processing (the treatment) of observations. I categorically deny such opinions. Observers have to take care of both random and systematic errors which cannot therefore be attributed to separate branches (or twigs) of science.

3.2. Ancient astronomers apparently selected point estimates for the constants sought by choosing almost any number within reasonable bounds. According to modern notions, such an attitude is quite proper if the errors of observations are large; moreover, it fits in with the qualitative nature of ancient science.

It was Daniel Bernoulli (1780) who introduced, although in a restricted sense, the notions of random and systematic errors, but ancient astronomers obviously acquired some understanding of both. The influence of refraction, for example, was systematic.

3.3. In **Kepler's** time, and possibly even somewhat earlier, the arithmetic mean became the generally accepted estimator of measurements. Indeed, Kepler (1609/1992, p. 200/63), when treating four observations, selected a number as the *medium ex aequo et bono* (in fairness and justice). A plausible reconstruction assumes that it was a generalized arithmetic mean with differing weights of observations. More important, the Latin expression above occurred in Cicero, 106 – 43 BC (*Pro A. Caecina oratio*), and carried an implication *Rather than according to the letter of the law*, an expression known to lawyers. In other words, Kepler, who likely read Cicero, called the ordinary arithmetic mean the letter of the law, i.e., the universal estimator of the parameter of location.

Kepler repeatedly adjusted observations. How had he convinced himself that Tycho's observations were in conflict with the Ptolemaic system of the world? I believe that Kepler applied the minimax principle which demanded that the residual free term of the given system of equations, maximal in absolute value, be the least from among all of its possible solutions. He (1609/1992, p. 286/113) apparently determined such a minimum, although only from among some possibilities, and found out that that residual was equal to 8′ which was inadmissible. Any other solution would have been even

less possible, so that either the observations or the underlying theory were faulty. Kepler reasonably trusted Tycho's observations and his inference was obvious. Note that this principle did not ensure optimal, in any sense, results.

When adjusting observations, Kepler (Ibidem, p. 334/143) corrupted them by small arbitrary corrections. He likely applied elements of what is now called statistical simulation, but in any case he must have taken into account the properties of usual random errors, i.e., must have chosen a larger number of small positive and negative corrections and about the same number of the corrections of each sign. Otherwise, Kepler would have hardly achieved success.

3.4. Direct Observations. I am now entering the 18th century and, after discussing Lambert, begin with the treatment of direct observations.

3.4.1. The term *Theory of errors* (*Theorie der Fehler*) is due to **Lambert** (1765a, Vorberichte and § 321) who defined it as the study of the relations between errors, their consequences, circumstances of observation and the quality of the instruments. He isolated the aim of the *Theory of consequences* as the study of functions of observed (and error-ridden) magnitudes. In other words, he introduced the determinate error theory and devoted to it §§ 340 – 426 of his contribution. Neither Gauss, nor Laplace ever used the new terminology, but Bessel (1820, p. 166; 1838b, § 9) applied the expression *theory of errors* without mentioning anyone and by the mid-19th century it became generally known.

Lambert studied the most important aspects of treating observations and in this respect he was Gauss' main predecessor. He (1760, §§ 271 – 306) described the properties of usual random errors, classified them in accordance with their origin (§ 282), unconvincingly proved that deviating observations should be rejected (§§ 287 – 291) and estimated the precision of observations (§ 294), again lamely but for the first time ever. He then formulated an indefinite problem of determining a [statistic] that with maximal probability least deviated from the real value of the constant sought (§ 295) and introduced the principle of maximal likelihood, but not the term itself, for a continuous density (§ 303), maintaining, however (§ 306), that in most cases it will provide estimates little deviating from the arithmetic mean. The translator of Lambert's contribution into German left out all this material claiming that it was dated.

Lambert introduced the principle of maximum likelihood for an unspecified, more or less symmetric and unimodal curve (as shown on his figure), call it $\varphi(x - \hat{x})$, where $\hat{x}$ was the sought parameter of location. Denote the observations by x_1, x_2, …, x_n, and, somewhat simplifying his reasoning, write his likelihood function as

$$\varphi(x_1 - \hat{x}) \, \varphi(x_2 - \hat{x}) \, … \, \varphi(x_n - \hat{x}).$$

When differentiating it, Lambert had not indicated that the argument here was the parameter $\hat{x}$, etc.

Lambert (1765a) returned to the treatment of observations. He attempted to estimate the precision of the arithmetic mean, but did not

introduce any density and was unable to formulate a definite conclusion. He also partly repeated his previous considerations and offered a derivation of a density law of errors occurring in pointing an instrument (§§ 429 – 430) in accordance with the principle of insufficient reason: it was a semi-circumference (with an unknown radius) simply because there were no reasons for its *angularity*.

3.4.2. Simpson (1756), see also Shoesmith (1985), applied, for the first time ever, stochastic considerations to the adjustment of measurements by assuming that observational errors obeyed some density law and thus extended probability to a new domain and effectively introduced random observational errors. He aimed to refute some unnamed authors who had maintained that one good observation was as plausible as the mean of many of them. Simpson considered errors obeying the discrete uniform and triangular distributions and effectively applied the proper generating functions.

For both these cases he founded out that the probability that the absolute value of the error of the arithmetic mean of *n* observations was less than some magnitude, or equal to it. He decided that the mean was always [stochastically] preferable to a separate observation and thus arbitrarily and wrongly generalized his proof. Simpson also indicated that his first case was identical with the determination of the probability of throwing a given number of points with *n* dice each having (v + 1) faces. Note that in the continuous case Simpson's distributions can be directly compared with each other: their respective variances are $v^2/3$ and $v^2/6$.

Soon Simpson (1757) reprinted his memoir adding to it an investigation of the continuous triangular distribution. However, his graph showed the density curve of the error of the mean which should have been near-normal but which did not possess the distinctive form of the normal distribution.

3.4.3. Daniel Bernoulli (1769) assumed the density law of observational errors as a *semi-ellipse* or semi-circumference of some radius *r* which he ascertained by assigning a reasonable maximal error of observation and the location parameter equal to the weighted arithmetic mean with posterior weights

$$p_i = r^2 - (\hat{x} - x_i)^2. \tag{5}$$

Here, x_i were the observations and $\hat{x}$, the usual mean. The first to apply weighted, or generalized arithmetic means was Short (1763). Such estimators demanded a subjective selection of weights and only provided a correction to the ordinary arithmetic mean which tended to vanish for even density functions.

In his published memoir Daniel Bernoulli (1778) objected to the application of the arithmetic mean which (§ 5) only conformed to an equal probability of all possible errors and was tantamount to shooting blindly. K. Pearson (1978, p. 268), however, reasonably argued that small errors were more frequent and had their due weight in the mean. Instead, Bernoulli suggested the maximum likelihood estimator of the location parameter. Listing reasonable restrictions for the density curve (but adding the condition of its cutting the abscissa axis almost

perpendicularly), he selected a semi-circumference with radius equal to the greatest possible, for the given observer, error. He then (§ 11) wrote out the likelihood function as

$$\{[r^2 - (x - x_1)^2] \, [r^2 - (x - x_2)^2] \, [r^2 - (x - x_3)^2]\ldots\}^{1/2},$$

where x was the unknown abscissa of the centre of the semi-circumference, and x_1, x_2, x_3, …, were the observations. Preferring, however, to ease calculation, he left the semi-circumference for an arc of a parabola but he had not known that the variance of the result obtained will therefore change.

For three observations his likelihood equation was of the fifth degree. Bernoulli numerically solved it in a few particular instances with some values of x_1, x_2, and x_3 chosen arbitrarily (which was admissible for such a small number of them). I present his equation as

$$\frac{x - x_1}{r^2 - (x - x_1)^2} + \frac{x - x_2}{r^2 - (x - x_2)^2} + \ldots = 0$$

so that the maximum likelihood estimate is

$$\hat{x} = \frac{[px]}{\sum p_i}, \; p_i = \frac{1}{r^2 - (\hat{x} - x_i)^2} \qquad\qquad (6; 7)$$

with an unavoidable use of successive approximations. For some inexplicable reason these formulas are lacking in Bernoulli's memoir although the posterior weights (7) were the inverse of the weights (5) from his manuscript and heuristically contradicted his own preliminary statement about shooting skilfully. It is now known, however, that such weights are expedient in case of some densities.

3.4.4. Euler (1778, § 6) objected to the principle of maximum likelihood. He argued that the result of an adjustment should barely change whether or not a deviating observation was adopted, but that the value of the likelihood function essentially depended on that decision. His remark should have led him to the median although he (§ 7) selected the estimate (6) with posterior weights (5) and mistakenly assumed that Bernoulli had chosen these same weights.

It is not regrettably known whether Gauss had read these two contributions. Indeed, an intermediate formula of Euler heuristically resembled Gauss' choice of least variance as a criterion for treating observations.

3.5. Indirect Measurements. Here, I consider the adjustment of redundant systems

$$a_i x + b_i y + \ldots + s_i = v_i, \; i = 1, 2, \ldots, n \qquad\qquad (8)$$

in k unknowns ($k < n$) and residual free terms v_i.

3.5.1. In case of two unknowns astronomers usually separated systems (8) into all possible **groups of two equations** each and averaged the solutions of these groups. As discovered in the 19th

century, the least-squares solution of (8) was some weighted mean of these partial solutions (Whittaker & Robinson 1924/1949, p. 251).

3.5.2. For three unknowns that method becomes unwieldy. In an astronomical context, **Mayer** (1750) had to deal with 27 equations in three unknowns. He calculated three particular solutions (see below), and averaged them. The plausibility of the results thus obtained depended on the expediency of the separation and it seems that Mayer had indeed made a reasonable choice. Being mostly interested in only one unknown, he included the equations with its greatest and smallest in absolute value coefficients in the first, and the second group respectively. Note also that Mayer believed that the precision of results increased as the number of observations, but in his time this mistake was understandable.

Mayer solved each group of equations under an additional condition

$$\Sigma v_i = 0,$$

where i indicated the number of an equation; if the first group included the first nine of them, then $i = 1, 2, \ldots, 9$. Laplace (1812/1886, pp. 352 – 353) testified that the best astronomers had been following Mayer. A bit earlier Biot (1811, pp. 202 – 203) reported much the same.

The condition above determines the method of averages and Lambert's recommendation (1765b, § 20) about fitting an empirical straight line might be interpreted as its application. Lambert separated the points (the observations) into two groups, with smaller and larger abscissas, and drew the line through their centres of gravity, and into several groups when fitting curves.

3.5.3. The Boscovich Method. He (Maire & Boscovich 1770, p. 501) adjusted systems (8) under additional conditions

$$v_1 + v_2 + \ldots + v_n = 0, \quad |v_1| + |v_2| + \ldots + |v_n| = \min, \qquad (9; 10)$$

the first of which can be allowed for by summing all the equations and eliminating one of the unknowns from the expression thus obtained. The second condition linked the Boscovich method with the median. Indeed, he adjusted systems (8) by constructing a straight line whose slope was equal to the median of some fractions. In 1809, Gauss noted that (10) led exactly to k zero residuals v_i, which follows from an important theorem in the then not yet known theory of linear programming.

Galileo (1632), see Hald (1990, § 10.3), and Daniel Bernoulli (1735/1987, pp. 321 – 322) applied condition (10) in the case in which the magnitudes such as v_i were positive by definition. Just the same, William Herschel (1805) determined the movement of the Sun by issuing from the apparent motion of the stars. The sum of these motions depends on the former and its minimal value, as he assumed, estimated that movement. Herschel's equations were not even algebraic, but, after some necessary successive approximations, they might have been considered linear. In those times the motion of a star could have been discovered only in the plane perpendicular to the line

of vision. When treating direct measurements Herschel (1806) preferred the median rather than the arithmetic mean (Sheynin 1984a, pp. 172 – 173).

3.5.4. The Minimax Method. Kepler (§ 3.3) had apparently made use of some elements of this method. Laplace (1789/1895, pp. 493, 496 and 506 and elsewhere) applied it for preliminary investigations. This method corresponds, as Gauss (1809, § 186) remarked, and as it is easy to prove, to the condition

$$\lim (v_1^{2k} + v_2^{2k} + \dots + v_n^{2k}) = \min, \ k \to \infty.$$

Below, I describe the subsequent history of the theory of errors, but right now I emphasize that beginning with Simpson and until the 1930s it had been the main field of application of the theory of probability and that mathematical statistics had borrowed two main principles from the theory of errors, those of maximal likelihood and of least variance.

4. Laplace

He devoted a number of memoirs to the theory of probability and later combined them in his *Théorie analytique des probabilités* (TAP) (1812). He made use of characteristic functions and the inversion formula, calculated difficult integrals, applied Hermite polynomials, introduced the Dirac function and (after Daniel Bernoulli) the Ehrenfests' model and studied sampling. Issuing from observations, Laplace proved that the Solar system will remain stable for a long time and completed the explanation of the movement of its bodies in accordance with the law of universal gravitation.

He had not even heuristically introduced the notion of *random variable* and was unable to study densities or characteristic functions as mathematical objects, did not bother to prove rigorously his theorems (for example, often issued from non-rigorously proved versions of the CLT, not even properly formulated) which was contrary to the attitude of his predecessors (§ 2.4.7). His theory of probability therefore became an applied mathematical discipline unyielding to development and it had to be constructed anew. Here, indeed, is Poisson (1837a, § 84) who methodically followed Laplace:

There exists a very high probability that these unknown chances little differ from the ratio …

Then, Laplace insisted on his own impractical justification of the method of least squares and virtually neglected Gauss. Many commentators reasonably stated that his contributions made difficult reading.

Here is an interesting problem from *Chapter 2* of the TAP. An interval *OA* is divided into equal or unequal parts and perpendiculars are erected to the intervals at their ends. The number of perpendiculars is *n*, their lengths (moving from *O* to *A*) form a non-increasing sequence and the sum of these lengths is given. Suppose now that the sequence is chosen repeatedly; what, Laplace asks, will be the mean broken line connecting the ends of the perpendiculars? The mean value of a current perpendicular? Or, in the continuous case, the mean curve? Each curve might be considered as a realization of a stochastic

process and the mean curve sought, its expectation. Laplace was able to determine this mean curve and to apply this finding for studying expert opinions.

Suppose that some event can occur because of *n* mutually exclusive causes. Each expert arranges these in an increasing (or decreasing) order of their [subjective] probabilities, which, as it occurs, depend only on *n* and the number of the cause, *r*, and are proportional to

$$\frac{1}{n}+\frac{1}{n-1}+...+\frac{1}{n-r+1}.$$

The comparison of the sums of these probabilities for each cause also shows the mean opinion about its importance. To be sure, different experts will attribute differing perpendiculars to one and the same cause.

In *Chapter 6* Laplace applied the Bayesian approach to problems in population statistics. First, he wrote out formula (6) from § 2.4.7 with *r* being the unknown probability of a male birth and *p* and *q*, the very large numbers of male and female births. He expressed the integrals of *functions of very large numbers* (as Laplace called them) by integrals of an exponential function of a negative square.

In the same way Laplace estimated the population of France (*M*) by issuing from sampling, from the known number of yearly births in France and in some of its regions (*N* and *n*) and the population of those regions (*m*). K. Pearson (1928) remarked that Laplace had mistakenly considered (*m, n*) and (*M, N*) as independent samples from the same infinite population (whose very existence was doubtful) and that his estimate of the achieved precision of sampling (the first of its kind) was somewhat erroneous.

Laplace's theory of errors was based on several versions of the CLT (whose conditions he never really formulated!) and therefore required, first of all, a large number of observations. In geodesy, that number was barely sufficient, and the errors in long series of astronomical observations hardly obeyed one and the same law of distribution. And only the normal distribution became worthy of attention.

Without explanation which appeared in his *Supplement* 2 (1818/1886, p. 571) Laplace (1816) approximated the squared sum of the real errors by the same sum of the residuals and, for the case of *s* observations, arrived at an estimator of their variance $m = \sqrt{[vv]/s}$. Interestingly, he (1814/1995, p. 45) stated that *the weight of the mean result increases like the number of observations divided* [*divisé*] *by the number of parameters*. See below the more precise formula due to Gauss (§ 6.1) and note that *variance* is a modern term.

Curiously, Laplace (1796/1884, p. 504), actually attributed the planetary eccentricities to randomness:

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

gave rise to the eccentricities of their orbits and the deviations of their movement from the plane of that equator.

Curiously, since Newton had proved that the eccentricities were determined by the planets' initial velocities. However, did Newton get rid of randomness? No, not at all: those velocities seem to be random.

5. Poisson

He (Sheynin 1978) introduced the concepts of random variable and distribution function. He contributed to limit theorems and brought into use the LLN, proving it for the case of *Poisson trials*. He devoted much attention to the study of juridical statistics (§ 2.2) and systematically determined the significance of empirical discrepancies. Poisson stressed the difference between subjective and objective probabilities. Cournot (1843) kept to the same attitude and even introduced non-numerical probabilities. They as well as the subjective probabilities are being applied as expert estimates (cf. § 4).

Since Poisson (1837a) consistently checked the significance of empirical discrepancies, for example between results of different series of observations, he, along with Bienaymé, can be called the Godfather of the Continental direction of statistics (Lexis, Bortkiewicz, Chuprov, Markov, Bohlmann, see § 8.3) that mostly studied population. True, his approach was definitely restricted as it became apparent in medicine (§ 2.1).

Poisson's generally known formula (1837a, § 81, p. 206)

$$P = e^{-w}(1 + w + w^2/2! + ... + w^n/n!), \ w = \mu p$$

for an event having probability $q = 1 - p \approx 0$ to occur not more than *n* times in a large number μ of Bernoulli trials had been all but ignored until Bortkiewicz (1898) introduced his law of small numbers, allegedly a breakthrough extremely important for statistics. However, Whitaker (1914) and then, Kolmogorov (1954) had identified it as the Poisson formula. They did not justify that statement, and I (2008) proved it, see also § 8.3.3.

Poisson's (1837a) LLN is his best known innovation. It generalized the Bernoulli trials on the case of variable probabilities p_i of success although many authors have reasonably noted that his proof was not rigorous. For him, the LLN was rather a principle whose scope he exaggerated. Still, he (p. 10) qualitatively connected it with the existence of a stable mean interval between molecules (Gillispie 1963, p. 438). The founders of the kinetic theory of gases had not regrettably noticed Poisson's conclusion.

Poisson's programme of probability calculus and social arithmetic (1837b) devoted serious attention to that latter subject. I quote the appropriate part of the programme:

Des tables de population et de mortalité. De la durée de la vie moyenne dans diverses contrées. Partage de la population suivant les âges et les sexes. De l'influence de la petite vérole, de l'inoculation et de la vaccine sur la population, et la durée de la vie moyenne. […]

That programme also mentioned insurance establishments, annuities, tontines, savings banks and *emprunts* (loans or perhaps

bonds). Social arithmetic therefore meant population statistics, at least some medical statistics and insurance.

Following Laplace, Poisson (see § 4) had often left demonstrations without indicating the boundaries of possible errors and his theory of probability still belonged to applied mathematics. One of his examples (1837a, § 11) led to a subjective probability of the studied event equal to 1/2, and, in conformity with the future information theory, he (Ibidem, § 4) properly remarked that such results illustrate *la perfaite perplexité de notre esprit*.

Poisson (1825 – 1826) applied subjective probability when investigating a game of chance. Cards are extracted one by one from six decks shuffled together as a single whole until the sum of the points in the sample obtained will be in the interval [31; 40]. The sample is not returned and a second sample of the same kind is made. It is required to determine the probability that the sums of the points are equal. Like the gamblers and bankers, Poisson tacitly assumed that the second sample was extracted as though from the six initial fresh decks. Actually, this was wrong, but the gamblers thought that, since they did not know what happened to the initial decks, the probability of drawing some number of points did not change.

When blackjack is played, bankers are duty bound to act the same wrong way: after each round the game continues without the used cards, and, to be on the safe side, they ought to stop at 17 points. A gambler endowed with a retentive memory can certainly profit from this restriction.

Catalan (1877; 1884) even formulated the following principle: If the causes, on which the probability of an event depended, changed in an unknown way, that probability remains unaltered.

6. Gauss, Helmert, Bessel

6.1. Gauss. He was the real, although not the formal discoverer of the method of least squares (MLSq) first publicly proposed by Legendre (1805). Indeed, Gauss had applied it from 1794 or 1795, informed his colleagues about it before 1805 and justified it. Legendre, however, only put forward reasonable arguments and, even so, actually and mistakenly stated that the MLSq also ensured a minimax solution of redundant systems of equations.

Three circumstances greatly impeded the dissemination of Gauss' ideas. *First*, although citing Legendre, he (1809, § 186) mentioned *our principle* (of least squares) which insulted the much older French scientist. That same year, Legendre (Gauss, W-9, p. 380) wrote a letter to Gauss stating that priority is only established by publication. A withdrawn person that he was, Gauss did not answer; for the time being, Legendre could have dropped the subject and repeated his proper remark at the first occasion.

As it happened, however, Legendre, as well as all the other French mathematicians interested in the treatment of observations except Laplace, became infuriated and, to their own detriment, for at least a few decades had continued to ignore Gauss' contributions to the theory of errors.

Second, Laplace (1812, § 24) properly described the situation, but kept to his own version of the theory of errors. *Third*, Laplace

somehow eclipsed Gauss. Innumerable geodetic textbooks only described the MLSq according to Gauss (1809), but even so many scientists barely noticed that work. Tsinger (1862, p. 1), who obviously did not even read Gauss, was the worst perpetrator:

Laplace provided a rigorous [?] *and impartial investigation* […]. *On the basis of extraneous considerations, Gauss endeavoured to attach to* [the MLSq] *an absolute significance* etc.

So what had Gauss achieved in 1809? Gauss (1809, § 177) assumed *as an axiom* that the arithmetic mean of many observations was the most probable value of the measured constant *if not absolutely precisely, then very close to it*. Together with the principle of maximal likelihood, his axiom or *postulate* (Bertrand 1888, p. 176) led to the normal distribution of the observational errors as the only possible law. Gauss was hardly satisfied with his derivation. His axiom contained qualification remarks, other laws of error were possible and maximum likelihood was worse than an integral criterion. It is somewhat strange that Gauss himself only mentioned the last item and only in a few letters. In his letter to Bessel of 1839 (Plackett 1972/1977, p. 287) he stated that the *highest probability* of the value of an unknown parameter was still infinitely low so that he preferred to rely on the *least disadvantageous game*, on maximum weight or minimal variance.

Indeed, Gauss (1823b, § 6) introduced the variance

$$\int_{-\infty}^{\infty} x^2 \varphi(x)dx$$

where the density $\varphi(x)$ was an even unimodal function (which conformed with the properties of *usual* random errors) and selected its minimal value as the criterion for adjusting observations.

He (§§ 18 and 19) also introduced independence of linear functions: they should not contain common observations. Then Gauss (§§ 37 – 38) proved what was practically necessary: for n observations and k unknowns, the unbiased sample variance and its estimator were, respectively,

$$m^2 = E[vv]/(n - k), \; \hat{m}^2 = [vv]/(n - k). \qquad (1a, b)$$

Instead of the mean value, the sum of squares $[vv]$ itself has to be applied. Coupled with the principle of maximal weight, formulas (1) provide effective estimators, as they are now called. Without mentioning Laplace, see above, Gauss (1823b, §§ 37 – 38) noted that his formula was not good enough. Elsewhere, Gauss (1823a/1887, p. 199) stated that its correction was also necessary for the *dignity of science*.

The necessary restrictions for the derivation of (1a) are linearity of the equations (1) of § 3.1, independence of their free terms (of the results of observation), and the unbiasedness of the estimators $\hat{x}, \hat{y},...$ of the unknowns. An extremely important corollary follows: the immediately appearing principle of least squares can be derived

without recourse to sections 7 – 38 of the memoir. Gauss had thus derived the principle of least squares by two independent ways: by the method which he described in those intermediate sections and by the just outlined method.

The first method is so complicated that perhaps up to the second half of the 20th century textbook authors invariably introduced the MLSq in accordance with Gauss' first memoir of 1809, which he no longer acknowledged. Now, however, after my discovery outlined above (Sheynin 2012), the situation has changed.

Gauss (§ 40) calculated the boundaries of the var $\hat{m}^2$ by means of the fourth moment of the errors but made a mistake later corrected by Helmert and then by Kolmogorov et al.

But why did not Gauss even hint at the described possibility? I can only quote Kronecker (1901, p. 42):

The method of exposition in the Disquisitiones [Arithmeticae of 1801] as in his works in general is Euclidean. He formulates and proves theorems and diligently gets rid of all the traces of his train of thoughts which led him to his results. This dogmatic form was certainly the reason for his works remaining for so long incomprehensible.

Later commentators expressed the same opinion. It remains to illustrate the former difficulties which led to the choice of the memoir of 1809 over Gauss' final memoir of 1823: the very existence of that final memoir (Eisenhart 1964, p. 24)

Seems to be virtually unknown to all American users of Least Squares, except students of advanced mathematical statistics.

Laurent (1875) turned to the method of least squares, but without even knowing about the existence of that memoir. And here is Fisher (1925, p. 260):

In the cases to which it is appropriate, this method [of least squares] *is a special application of the method of maximum likelihood, from which it may be derived.*

Quite recently Nikulin & Poliscuk (1999) failed to mention that final memoir. Petrov (1954) perhaps still provides the best description of the properties of estimators derived by the MLSq.

A very short biography of Gauss is Sheynin (2001b).

6.1.1. There are important **additional considerations**: the determination of the necessary number of observations, the rejection of outliers and the so-called true values of the unknowns (Sheynin 2007b). Owing to the unavoidable presence of systematic errors, the number of observations is not really determined by the formulas (1). For the same reason statistical criteria for rejecting outliers are hardly useful and this latter problem remains delicate.

Astronomers, geodesists, metrologists and other specialists making measurements have always been using the expression *true value*. Mathematical statistics has done away with true values and introduced instead parameters of densities (or distribution functions), and this was a step in the right direction: the more abstract was mathematics becoming, the more useful it proved to be.

Fisher was mainly responsible for that change; indeed, he (1922, pp. 309 – 310) defined the notions of consistency, efficiency and sufficiency of statistical estimators without any reference to true values. But then, on p. 311, he accused the Biometric school of applying the same names to *the true value which we should like to know* […] *and to the particular value at which we happen to arrive…* So the true value was then still alive and even applied, as in the lines above, to objects having no existence in the real world.

The same can be said about Gauss (1816, §§ 3 and 4) who repeatedly considered the true value of a measure of precision of observations. And Hald (1998) mentioned *true value* many times in Chapters 5 and 6; on p. 91 he said: *the estimation of the true value, the location parameter…*

So what is a true value? Markov (1900/1924, p. 323) was the only mathematician who cautiously, as was his wont, remarked: *It is necessary in the first place to presume the existence of the numbers whose approximate values are provided by observations*. This phrase first appeared in the 1908 edition of his *Treatise* (and perhaps in its first edition of 1900). He had not attempted to define *true value*, but this is exactly what Fourier (1826/1890, p. 534) had done about a century before him. He determined the *véritable objet de la recherche* (the constant sought, or its *true* value) as the limit of the arithmetic mean of n appropriate observations as $n \to \infty$. Incidentally, he thus provided the Gauss *postulate* with a new dimension.

Many authors, beginning perhaps with Timerding (1915, p. 83) [and including Mises (1919/1964b, pp. 40 and 46)], without mentioning Fourier and independently from each other, introduced the same definition. One of them (Eisenhart 1963/1969, p. 31) formulated the unavoidable corollary: the mean residual systematic error had to be included in that *true* value:

The mass of a mass standard is […] *specified* […] *to be the mass of the metallic substance of the standard plus the mass of the average volume of air adsorbed upon its surface under standard conditions.*

However, even leaving systematic influences aside, the precision of observations is always restricted and the number of observations finite, so that the term *limit* in the Fourier definition (which is in harmony with the Mises definition of probability) must not be understood literally.

Statistics moved from true values to parameters of densities or distribution functions, but still does not entirely abandon them.

6.2. Chronologically, **Helmert** belongs to the second half of the 19th century, but it is better to mention him here. He mainly completed the development of the classical Gaussian theory of errors and some of his findings were interesting for mathematical statistics. Until the 1930s, Helmert's treatise (1872) remained the best source for studying the error theory and the adjustment of triangulation. When adjusting a complicated geodetic net, Helmert (1886, pp. 1 and 86) temporarily replaced chains of triangulation by geodetic lines. His innovation had been applied in the Soviet Union. The chains of the national primary triangulation were there situated between bases and astronomically determined azimuths. Before the general adjustment of the entire

system, each chain was replaced by the appropriate geodetic line; only they were adjusted, then the chains were finally dealt with independently one from another.

Elsewhere Helmert (1868) studied various configurations of geodetic systems. In accordance with the not yet existing linear programming, he investigated how to achieve necessary precision with least possible effort, or, to achieve highest possible precision with a given amount of work. Some equations originating in the adjustment of geodetic networks are not linear, not even algebraic; true, they can be linearized, and perhaps some elements of linear programming could have emerged then, in 1868, but this had not happened. Nevertheless, Helmert noted that it was expedient to leave some angles of particular geodetic systems unmeasured, but his remark was purely academic: all angles ought to be measured at least for checking the work as a whole.

Abbe (1863) derived the chi-square distribution, see also Sheynin (1966) and Kendall (1971), as the frequency of the sum of the squares of n normally distributed errors. Helmert (1875; 1876) derived the same distribution by induction beginning with $n = 1$ and 2 and Hald (1952/1960, pp. 258 – 261) provided a modernized derivation. Much later Helmert (1905) offered a few tests for revealing systematic influences in a series of errors. Among other results, I note that he (1876) derived a formula that showed that, for the normal distribution, [vv], – and, therefore, the variance as well,– and the arithmetic mean were independent. He had thus proved the important Student – Fisher theorem although without paying any attention to it.

Czuber (1891, p. 460) testified that Helmert had thought that (var $\hat{m}^2$)/$\hat{m}^2$ was more important than var $\hat{m}^2$ by itself and Eddington (1933, p. 280) independently expressed the same opinion. Czuber also proved that, for the normal distribution, that relative error was minimal for the estimator (1b).

In addition, Helmert noted that for small values of n the var $\hat{m}^2$ did not estimate the precision of formula (1b) good enough. His considerations led him to the so-called Helmert transformations.

6.3. Bessel. His achievements in astronomy and geodesy include the determination of astronomical constants; the first determination of a star's parallax; the discovery of the personal equation; the development of a method of adjusting triangulation; and the derivation of the parameters of the Earth's ellipsoid of revolution. He (1838a) also proved the CLT, but its rigorous proof became possible, with a doubtful exception of one of Cauchy's memoirs, only much later. Incidentally, Gauss was familiar with the pertinent problem. In the letter to Bessel of 1839 mentioned in § 6, he stated that he had read that proof *with great interest,* but that

This interest was less concerned with the thing itself than with your exposition. For the former has been familiar to me for many years, though I myself have never arrived at carrying out the development completely.

The personal equation of an observer is his systematic error of registering the moments of the passage of a star through the cross-

hairs of the eyepiece of an astronomical instrument. When studying this phenomenon, it is possible to compare the moments fixed by two astronomers at different times. Although Bessel did not explain the situation, it followed from the context that he and another astronomer had only one clock. Consequently, it was necessary to take into account its correction. Bessel (1823), who discovered the existence of the personal equation, had indeed acted appropriately, since apparently (he did not explain the situation) both observers had been using the same clock.

However, in one case he mistakenly presumed that the rate of the clock was constant, and his pertinent observations proved useless; he made no such comment. When studying Bradley's observations, Bessel (1818; 1838a, § 11) he dismissed the deviations of their errors from normality. Or, rather, he decided to save his CLT by stating that, given more observations (more than a few hundred!), those deviations will disappear …

And I (Sheynin 2000) discovered 33 mistakes in arithmetical and simple algebraic operations in Bessel's contributions collected in his *Abhandlungen* (1876). Not being essential, they testify to his inattention and undermine the trust in the reliability of his more involved calculations.

That Gauss had been familiar with the derivation of the CLT could have angered Bessel. Anyway, in 1844, in a letter to Humboldt he (Sheynin 2001c, p. 168) reversed his previous opinion and stressed Legendre's priority in the dispute over the discovery of the MLSq. Moreover, in 1825 Bessel met Gauss and quarrelled with him, although no details are known (Ibidem) and even in 1822 Olbers in a letter to Bessel (Erman 1852, Bd. 2, p. 69) regretted that the relations between the two scholars were bad. Gerling (1861), a former student of Gauss, described Bessel's unwarranted attempts made in 1843 to establish his priority over Gauss in the adjustment of triangulation. See also Sheynin (2001c, pp. 171 – 172).

Bessel's posthumously published collected reports (1848) include an item on the theory of probability (pp. 387 – 407), this being his report to a physical society, written on a low scientific level (apparently occasioned by the poor knowledge of his listeners). Among the applications of the theory of probability Bessel only dwelt on astronomy, but he did not say a single word about the discovery of the minor planets, about the MLSq or Gauss. A distressing impression!

Bessel (p. 401) stated that the *great Lambert* had objected to the use of the arithmetic mean. Actually, Lambert (1760) introduced the principle of maximum likelihood but noted, certainly without proving it, that the appearing estimate does not deviate much from the arithmetic mean, the mean which he never denied. Worse is to come. Bessel (1843) stated that William Herschel had discovered the planet Uranus, saw its disc. Actually, Herschel only saw a moving body and thought that it was a comet. It follows that Bessel did not know the true story and falsely reconstructed it. Then, he (1845), without any statistical data, invented a false picture about Native Americans. See also my note on Bessel (Sheynin 2016).

A great scholar and a deep-rooted fabricator! A case for the psychologist.

7. The Second Half of the 19ᵗʰ Century

7.1. At the beginning of his scientific career **Quetelet** visited Paris and I think that Fourier had mostly inspired him. Quetelet tirelessly treated statistical data and attempted to standardize statistics on an international scale. He was co-author of the first statistical reference book (Quetelet & Heuschling 1865) on the population of Europe (including Russia) and the USA that contained a critical study of the initial data; in 1853, he (1974, pp. 56 – 57) served as chairman of the *Conférence maritime pour l'adoption d'un système uniforme d'observation météorologiques à la mer* and the same year he organized the first *International Statistical Congress*. K. Pearson (1914 – 1930, 1924, vol. 2, p. 420) praised Quetelet for *organizing official statistics in Belgium and* […] *unifying international statistics*. About 1831 – 1833 Quetelet had successfully suggested the formation of a Statistical Society in London, now called the *Royal Statistical Society*.

Quetelet's writings (1869; 1871) contain many dozen of pages devoted to various measurements of the human body, of pulse and respiration, to comparisons of weight and stature with age, etc. and he extended the applicability of the normal law to this field. Following Humboldt's advice, Quetelet (1870; 1871) introduced the term *anthropometry* and thus curtailed the boundaries of anthropology. He was influenced by Babbage (1857), an avid collector of biological data. In turn, Quetelet impressed Galton (1869, p. 26) who called him *the greatest authority on vital and social statistics*. While discussing that contribution, K. Pearson (1914 – 1930, vol. 2, 1924, p. 89) declared:

We have here Galton's first direct appeal to statistical method and the text itself shows [that the English translation of Quetelet (1846)] *was Galton's first introduction to the* […] *normal curve*.

Quetelet (1846) recommended the compilation of questionnaires and the preliminary checking of the data; maintained (p. 278) that too many subdivisions of the data was a *charlatanisme scientifique*, and, what was then understandable, opposed sampling (p. 293). Darwin (1887, vol. 1, p. 341) approvingly cited that contribution whereas Quetelet (1846, p. 259) declared that *the plants and the animals have remained as they were when they left the hands of the Creator*.

Lamarck was the first who attempted to construct a theory of evolution, and Quetelet's statement proves that his thoughts had been more or less discussed. However, Quetelet never mentioned either Lamarck, or Wallace, or Darwin.

He collected and systematized meteorological observations and described the tendency of the weather to persist by elements of the theory of runs. Köppen (1875, p. 256), an eminent meteorologist, noted that *ever since the early 1840s* the Belgian meteorological observations *proved to be the most lasting* [in Europe] *and extremely valuable*. In six letters, in 1841 – 1851, Faraday (1996 – 1999) praised Quetelet's observations of atmospheric electicity and called him *a*

worthy example of activity & power to all workers in science (Sheynin 2009b, No. 371).

Quetelet discussed the level of postage rates (1869, t. 1, pp. 173 and 422) and rail fares (1846, p. 353) and recommended to study statistically the changes brought about by the construction of telegraph lines and railways (1869, t. 1, p. 419). He (1836, t. 2, p. 313) quantitatively described the monotone changes in the probabilities of conviction of the defendants depending on their personality (sex, age, education) and Yule (1900/1971, pp. 30 – 32) called it the first attempt to measure association.

Quetelet is best remembered for the introduction of the Average man (1832a, p. 4 and elsewhere), inclinations to crime (1832b, p. 17 and elsewhere) and marriage (1848a, p. 77 and elsewhere), – actually, the appropriate statistical probabilities, – and for mistaken (Rehnisch 1876) statements about the constancy of crime (1829, pp. 28 and 35 and many other sources) whose level he (1836, t. 1, p. 10) connected with the general organization of the society. The two last-mentioned items characterized Quetelet as the follower of Süssmilch in originating moral statistics. Quetelet (1848a, p. 82 and elsewhere) indicated that the inclination to crime of a given person might differ considerably from the apparent mean tendency and (pp. 91 – 92) and related these inclinations to the Average man, but statisticians did not notice that reservation and denied inclinations and even probability theory. True, many of them, e. g., Haushofer (1872) or Block (1878), only applied arithmetic. After Quetelet's death statisticians (mostly in Germany) had simply discarded him.

The Average man, as Quetelet thought, was the type of the nation and even of entire mankind. Reasonable objections were levelled against this concept. Thus, the Average man was even physiologically impossible (the averages of the various parts of the human body were inconsistent one with another). Then, Quetelet (1846, p. 216) only mentioned the Poisson LLN in connection with the mean human stature. Bertrand (1888, p. XLIII) ridiculed Quetelet:

In the body of the average man, the Belgian author placed an average soul. He has no passions or vices [wrong, see above], *he is neither insane, nor wise, neither ignorant nor learned.* […] [He is] *mediocre in every sense. After having eaten for thirty-eight years an average ration of a healthy soldier, he has to die not of old age, but of an average disease that statistics discovers in him.*

However, that concept is useful at least as describing an average producer and consumer; Fréchet (1949) replaced him by a closely related *typical* man.

Quetelet (1848a, p. 80 and elsewhere) noticed that the curves of the inclinations to crime and to marriage plotted against ages were exceedingly asymmetric. He (1846, pp. 168 and 412 – 424) also knew that asymmetric densities occurred in meteorology and he (1848a, p. viii) introduced a mysterious *loi des causes accidentelles* whose curve could be asymmetric (1853, p. 57)! No wonder Knapp (1872, p. 124) much too politely called him *rich in ideas, but unmethodical and therefore un-philosophical.* Nevertheless, Quetelet had been the central figure of statistics in the mid-19th century.

7.2. Being influenced by his cousin, Darwin, **Galton** began to study the heredity of talent (1869). In a letter of 1861 Darwin (1903, p. 181) favourably mentioned that contribution. Darwin (1876/1878, p. 15) also asked Galton to examine his investigation of the advantages of cross-fertilization as compared with spontaneous pollination. Galton solved that problem by effectively applying rank correlation. Then, he (1863) devised an expedient system of symbols for weather charts and immediately discovered the existence of previously unknown anticyclones. This was the third (after Halley and Humboldt, see § 1.3) example of a wonderful application of a preliminary or exploratory data analysis, the comparatively new stage of statistical investigations. See Andrews (1978) who refers to many authors especially J. W. Tukey. In particular, this analysis aims at discovering patterns in the data (including systematic influences). Tukey (1962/1986, p. 397) remarked on an important feature of that stage:

Data analysis, and the parts of statistics which adhere to it, must […] *take on the characteristics of a science rather than those of mathematics.*

Kolmogorov (1948a, p. 216) unfortunately, as I think, stated that mathematical statistics comprised theoretical statistics and a (preliminary) descriptive part devoted to systematizing mass data and to calculating the appropriate means, moments, etc. He himself (Anonymous 1954, pp. 46 – 47) later maintained that theoretical statistics comprises mathematical statistics and *some technical methods of collecting and treating statistical methods.* Many statisticians seem to share this opinion but he belittled these *technical methods* and denied theoretical statistics. Anyway, I cannot agree with Anscombe (1967, p. 3n) who called mathematical statistics *a grotesque phenomenon.*

Galton (Pearson 1914 – 1930, vol. 2, Chapter 12) also invented *composite photographs* of people of a certain nationality or occupation, or criminals, all of them taken on the same film with an appropriately shorter exposure. Such photographs heuristically showed Quetelet's Average man.

In 1892, Galton became the main inventor of fingerprinting. Another of Galton's invention (1877) was the so-called *quincunx*, a device for demonstrating the appearance of the normal distribution as the limiting case of the binomial law which also showed that the normal law was stable. Galton's main statistical merit consisted, however, in the introduction of the notions of regression and correlation. The development of correlation theory became one of the aims of the Biometric school, and Galton's close relations with Pearson were an important cause of its successes.

7.3. I reconstruct now **Darwin's** model of evolution (1859). Introduce an n-dimensional (possibly with $n = \infty$) system of coordinates, the body parameters of individuals belonging to a given species (males and females should be treated separately), and the appropriate Euclidean space with the usual definition of distance between its points. At moment t_m each individual is some point of that space and the same takes place at moment t_{m+1} for the individuals of the next generation. Because of the *vertical* variation, these, however,

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will occupy somewhat different positions. Introduce in addition point (or subspace) *V*, corresponding to the optimal conditions for the existence of the species, then its evolution will be represented by a discrete stochastic process of the approximation of the individuals to *V* (which, however, moves in accordance with the changes in the external world) and the set of individuals of a given generation constitutes the appropriate realization of the process. Probabilities describing it (as well as estimates of the influence of habits, instincts, etc.) are required for the sake of definiteness, but they are of course lacking.

Mendel's discovery was only unearthed at the very end of the 19th century, and it certainly changed the picture of evolution. Then, the importance of mutation became known (De Vries 1905). Darwin and his teaching inspired the founders of the Biometric school (§ 8.1). See a very short biography of Mendel (Sheynin 2001d).

7.4. In 1855 **Bertrand** had translated Gauss' works on the MLSq into French. The title-page of this translation carried a phrase *Translated and published avec l'autorisation de l'auteur*, but Bertrand himself (*C. r. Acad. Sci. Paris*, t. 40, 1855, p. 1190) indicated that Gauss, who had died that same year, was only able to send him *quelques observations de détail*.

Bertrand's own work on probability began in essence in 1887 – 1888 when he published 25 notes in one and the same periodical as well as his main treatise (1888), written in great haste and carelessly, but in a very good literary style. I take up its main issues and state right now that it lacks a systematic description of its subject.

1) Statistical probability and the Bayesian approach. Heads appeared $m = 500,391$ times in $n = 10^6$ tosses of a coin (p. 276). The statistical probability of that event is $p = 0.500391$; it is unreliable, not a single of its digits merits confidence. After making this astonishing declaration, Bertrand compared the probabilities of two hypotheses, namely, that the probability was either $p_1 = 0.500391$, or $p_2 = 0.499609$. However, instead of calculating

$$[p_1{}^m p_2{}^n] \div [p_2{}^m p_1{}^n],$$

he applied the De Moivre – Laplace theorem and only indicated that the first probability was 3.4 times higher than the second one. So what should have the reader thought?

As I understand him, Bertrand (p. 161) *condemned* the Bayes *principle* only because the probability of the repetition of the occurrence of an event after it had happened once was too high. This conclusion was too hasty, and the reader was again left in suspense: what might be proposed instead? Note that Bertrand (p. 151) mistakenly thought that the De Moivre – Laplace theorem precisely described the inverse problem, the estimation of the theoretical probability given the statistical data, cf. § 2.4.7.

2) Mathematical treatment of observations. Bertrand paid much attention to this issue, but his reasoning was amateurish and sometimes wrong. Even if, when translating Gauss (see above), he had grasped the essence of the MLSq, he barely remembered that subject

after more than 30 years. Thus, he (pp. 281 – 282) attempted to prove that the sample variance (1) of § 6 might be replaced by another estimator of precision having a smaller variance. He failed to notice, however, that, unlike the Gauss' statistic, his new estimator was biased. Furthermore, when providing an example, Bertrand calculated the variance for the normal distribution instead of applying the Gauss additional formula for that case.

At the same time Bertrand also formulated some sensible remarks. He (p. 248) expressed a favourable opinion about the second Gauss justification of the MLSq but indicated (p. 267) that, for small errors, the even distribution

$$\varphi(x) = a + bx^2$$

can be approximately represented by an exponential function of a negative square, – that the first substantiation of the method was approximately valid.

3) Several interesting problems dwell on a random composition of balls in an urn; on sampling without replacement; on the ballot problem; and on the gambler's ruin.

a) White and black balls are placed in the urn with equal probabilities and there are N balls in all. A sample made with replacement contained m white balls and n black ones. Determine the most probable composition of the urn (pp. 152 – 153). Bertrand calculated the maximal value of the product of the probabilities of the sample and of the hypotheses on the composition of the urn.

b) An urn has sp white balls and sq black ones, $p + q = 1$. Determine the probability that after n drawings without replacement the sample will contain $(np – k)$ white balls (p. 94). Bertrand solved this problem applying the [hypergeometric distribution] and obtained, for large values of s and n, an elegant formula

$$P = \frac{1}{\sqrt{2\pi pqn}} \sqrt{\frac{s}{s-n}} \exp[-\frac{k^2 s}{2pqn(s-n)}].$$

He published this formula earlier without justification and noted that the variable probability of extracting the balls of either colour was *en quelque sorte un régulateur*.

c) Candidates A and B scored m and n votes respectively, $m > n$ and all the possible chronologically differing voting records were equally probable. Determine the probability P that, during the balloting, A was always ahead of B (p. 18). Following André (1887), who provided a simple demonstration, Bertrand proved that

$$P = (m – n)/(m + n), \tag{1}$$

see also Feller (1950, § 1 of Chapter 3). Actually, Bertrand was the first to derive formula (1) by a partial difference equation. This *ballot problem* has many applications (Feller, Ibidem). Takácz (1982) traced its history back to De Moivre. He indicated that it was extended to

include the case of $m \geq \mu n$ for positive integral values of μ and that he himself, in 1960, had further generalized that extended version.

d) I select one out of the few problems on the gambler's ruin discussed by Bertrand (pp. 122 – 123). Gambler A has m counters and plays with an infinitely rich partner. His probability of winning any given game is p. Determine the probability that he will be ruined in exactly n games ($n > m$). Bertrand was able to solve this problem by applying formula (1). Calculate the probability that A loses $(n + m)/2$ games and wins $(n – m)/2$ games; then, multiply it by the probability that during that time A will never have more than m counters, that is, by m/n. Conforming to common sense, Bertrand's derived formula shows that in case of a very high p the game will last exceedingly long.

In a brief chapter he largely denied everything done in the *moral applications* of probability by Condorcet (and did not refer to Laplace or Poisson).

In two of his notes Bertrand (1887a; 1887b) came close to proving that for a sample from a normal population the mean and the variance were independent (to the Student – Fisher theorem).

4) I take up Bertrand's celebrated problem about a random chord of a circle in § 7.6.1.

Taken as a whole, Bertrand's treatise is impregnated with its non-constructive negative (and often unjustified) attitude towards the theory of probability and treatment of observations. And at least once he (pp. 325 – 326) wrongly alleged that Cournot had supposed that judges decided their cases independently one from another. I ought to add, however, that Bertrand exerted a strong (perhaps too strong) influence upon Poincaré, and, its spirit and inattention to Laplace and Bienaymé notwithstanding, on the revival of the interest of French scientists in probability (Bru & Jongmans 2001).

7.5. In the theory of probability, **Poincaré** is known for his treatise (1896); I refer to its extended edition of 1912. I note first of all that he had passed over in silence not only the Russian mathematicians, but even Laplace and Poisson, and that his exposition was imperfect.

Following Bertrand, Poincaré (p. 62) called the expectation of a random variable its probable value; denoted the measure of precision of the normal law either by h or by $\sqrt{h}$; made use of loose expressions such as *z lies between z and z + dz* (p. 252).

Several times Poincaré applied the formula

$$\lim \frac{\int \varphi(x)\, \Phi^n(x)dx}{\int \psi(x)\, \Phi^n(x)dx} = \frac{\varphi(x_0)}{\psi(x_0)}, \, n \to \infty \qquad (2)$$

where $\Phi(x)$ was a restricted positive function, x_0, the only point of its maximum, and the limits of integration could have been infinite (although only as the result of a formal application of the Bayesian approach). Poincaré (p. 178) only traced the proof of (2) and, for being true, some restrictions should perhaps be added. To place Poincarè's trick in the proper perspective, see Erdélyi (1956, pp. 56 –

57). I discuss now some separate issues mostly from Poincaré's treatise.

1) The theory of probability. Poincaré (p. 24) reasonably stated that a satisfactory definition of prior probability was impossible. Strangely enough, he (1902/1923, p. 217) declared that *all the sciences* were nothing but an *unconscious application* of the calculus of probability, that the theory of errors and the kinetic theory of gases were based on the LLN (wrong about the former) and that the calculus of probability *will evidently ruin them* (*les entrainerait évidemment dans sa ruine*). Therefore, as he concluded, the calculus was only of practical importance. Another strange pronouncement is in his treatise (p. 34). As I understand him, he maintained that a mathematician is unable to understand why forecasts concerning mortality figures come true.

In a letter of ca. 1899 partly read out at the hearing of the notorious Dreyfus case (*Le procès* 1900, t. 3, p. 325; Sheynin 1991, pp. 166 – 167) Poincaré followed Mill (§ 2.2) and even generalized his statement to include *moral sciences* and declared that the appropriate findings made by Condorcet and Laplace were senseless. And he objected to a stochastic study of handwriting for identifying the author of a certain document.

The interest in application of probability to jurisprudence is now revived. Heyde & Seneta (1977, p. 34) had cited several pertinent sources published up to 1975; to these I am adding Zabell (1988), Gastwirth (2000) and Dawid (2005) who emphasized the utmost importance of interpreting background information concerning stochastic reasoning.

2) Poincaré (1892a) had published a treatise on thermodynamics which Tait (1892) criticized for his failure to indicate the statistical nature of this discipline. A discussion followed in which Poincaré (1892b) stated that the statistical basis of thermodynamics did not satisfy him since he wished to remain *entirely beyond all the molecular hypotheses however ingenious they might be*; in particular, he therefore passed the kinetic theory of gases over in silence. Soon he (1894/1954, p. 246) made known his doubts: he was not sure that that theory can account for all the known facts. In a later popular booklet Poincaré (1905/1970, pp. 210 and 251) softened his attitude: physical laws will acquire an *entirely new aspect* and differential equations will become statistical laws; laws, however, will be shown to be imperfect and provisional.

3) The binomial distribution. Suppose that m Bernoulli trials with probability of success p are made and the number of successes is α. Poincaré (pp. 79 – 84), in a roundabout and difficult way, derived (in modern notation) $E(\alpha - mp)^2$ and $E|\alpha - mp|$. In the first case he could have calculated $E\alpha^2$; in the second instance he obtained

$$E|\alpha - mp| \approx 2mpq\, C_m^{mp}\, p^{mp} q^{mq}, \quad q = 1 - p.$$

4) Without mentioning Gauss (1816, § 5), Poincaré (pp. 192 – 194) derived the moments of the [normal] distribution

$$\varphi(y) = \sqrt{h/\pi} \exp(- hy^2) \tag{3}$$

obtaining

$$Ey^{2p} = \frac{(2p)!}{h^p\, p!\, 2^{2p}} \tag{4}$$

and proved, by issuing from formula (2), that the density function whose moments coincided with the respective moments of the [normal] law was [normal]. This proposition was, however, due to Chebyshev (1887), see also Bernstein (1945/1964, p. 420).

Then Poincaré (pp. 195 – 201) applied his investigation to the theory of errors. He first approximately calculated $E\,\bar{y}^{\,2p}$ for the mean $\bar{y}$ of a large number n of observations having $Ey_i = 0$ and $Ey_i^2 =$ Const, equated these moments to the moments (4) and thus expressed h through Ey_i^2. This was a mistake: $\bar{y}$ being a mean, had a measure of precision nh rather than h. Poincaré (p. 195) also stated that Gauss had calculated $E\,\bar{y}^{\,2}$; actually, Gauss (1823b, §15) considered the mean value of $\sum y_i^2/n$.

The main point here and on pp. 201 – 206, where Poincaré considered the mean values of $(y_1 + y_2 + \ldots + y_n)^{2p}$ with identical and then non-identical distributions and $Ey_i = 0$, was a non-rigorous proof of the CLT: for errors of *sensiblement* the same order and constituting *une faible part* of the total error, the resulting error follows *sensiblement* the Gauss law (p. 206). For Poincaré, the theory of probability was still an applied science as he himself actually stated, see item 1) above.

Also for proving the normality of the sum of errors Poincaré (pp. 206 – 208, only in 1912) introduced characteristic functions which did not conform to their modern definition. Nevertheless, he was able to apply the Fourier formulas for passing from them to densities and back. These functions were

$$f(\alpha) = \Sigma p_x\, e^{\alpha x}, f(\alpha) = \int \varphi(x)e^{\alpha x}dx \tag{5}$$

and he noted that

$$f(\alpha) = 1 + \alpha Ex/1! + \alpha^2 Ex^2/2! + \ldots \tag{6}$$

5) Homogeneous [Markov chains]. Poincaré provided interesting examples which can be interpreted in the language of these chains.

a) He (p. 150) assumed that all the asteroids moved along one and the same circular orbit, the ecliptic, and explained why they were uniformly scattered across it. Denote the longitude of a certain minor planet by $l = at + b$ where a and b are random and t is the time, and, by $\varphi(a; b)$, the continuous joint density function of a and b. Issuing from the expectation

$$\mathrm{E}e^{iml} = \iint \varphi\left(a;b\right) e^{im(at + b)} dadb$$

(which is the appropriate characteristic function in the modern sense), Poincaré not very clearly proved his proposition that resembled the celebrated Weyl theorem (the terms of the sequence $\{nx\}$ where x is irrational and $n = 1, 2, \ldots$ and the braces mean *drop the integral part* are uniformly distributed on a unit interval). The place of a planet in space is only known with a certain error, and the number of all possible arrangements of the asteroids on the ecliptic can therefore be assumed finite whereas the probabilities of the changes of these arrangements during time period $[t; t + 1]$ do not depend on t. The uniform distribution of the asteroids might therefore be justified by the ergodic property of homogeneous Markov chains having a finite number of possible states.

b) The game of roulette. A circle is alternately divided into a large number of congruent red and black sectors. A needle is whirled with force along the circumference of the circle, and, after a great number of revolutions, stops in one of the sectors. Experience proves that the probabilities of *red* and *black* coincide and Poincaré (p. 148) attempted to justify that fact. Suppose that the needle stops after travelling a distance s $(2\pi < s < A)$. Denote the corresponding density by $\varphi(x)$, a function continuous on $[2\pi; A]$ with a bounded derivative on the same interval. Then, as Poincaré demonstrated, the difference between the probabilities of *red* and *black* tended to zero as the length of each red (and black) arc became infinitesimal (or, which is the same, as s became infinitely large). He based his proof on the method of arbitrary functions (Khinchin 1961/2004, pp. 421 – 422; von Plato 1983) and sketched its essence. Poincaré also indicated that the rotation of the needle was unstable: a slight change in the initial thrust led to an essential change in the travelled distance (and, possibly, to a change from *red* to *black* or vice versa).

c) Shuffling a deck of cards (p. 301). In an extremely involved manner, by applying hypercomplex numbers, Poincaré proved that after many shuffling all the possible arrangements of the cards tended to become equally probable.

6) Mathematical treatment of observations. In a posthumously published *Résumé* of his work, Poincaré (1921/1983, p. 343) indicated that the theory of errors *naturally* was his main aim in the theory of probability. His statement reflected the situation in those times. In his treatise he (pp. 169 – 173) derived the normal distribution of observational errors mainly following Gauss; then, like Bertrand, he changed the derivation by assuming that not the most probable value of the estimator of the [location parameter] coincided with the arithmetic mean, but its mean value. He (pp. 186 – 187) also noted that, for small absolute errors $x_1, x_2, \ldots, x_n$, the equality of $f(z)$ to the mean value of $f(x_i)$, led to z, the estimate of the real value of the constant sought, being equal to the arithmetic mean of x_i. It seemed to him that he thus corroborated the Gauss postulate. In the same context Poincaré (p. 171) argued that everyone believed that the normal law was universal: experimenters thought that that was a mathematical

fact and mathematicians believed that it was experimental. Poincaré referred to the oral statement of Lippmann, an author of a treatise on thermodynamics.

Finally, Poincaré (p. 188) indicated that the [variance] of the arithmetic mean tended to zero with the increase in the number of observations and referred to Gauss (who nevertheless had not stated anything at all about the case of $n \to \infty$. *Nothing*, however, followed since other linear means had the same property, as Markov (1899/1951, p. 250) stated. Poincaré himself (pp. 196 – 201 and 217) twice proved the [consistency] of the arithmetic mean. In the second case he issued from a characteristic function of the type of (5) and (6) and passed on to the characteristic function of the arithmetic mean. He noted that, if that function cannot be represented as (6), the consistency of the arithmetic mean was questionable, and he illustrated that fact by the Cauchy distribution. Perhaps because of all this reasoning on the mean Poincaré (p. 188) declared that Gauss' rejection of his first substantiation of the MLSq was *assez étrange* and corroborated this conclusion by remarking that the choice of the [parameter of location] should not be made independently from the distribution. Gauss (1823b) came to the opposite conclusion, but he restricted his attention to practically occurring distributions.

Poincaré (pp. 217 – 218) also stated that very small errors made it impossible to obtain absolute precision as $n \to \infty$. More properly, this fact is explained by the non-evenness of the law of distribution, the variability of that law and some interdependence of the observations.

7) Randomness. See § 7.6.2.

Poincaré's almost total failure to refer to his predecessors except Bertrand testifies that he was not duly acquainted with their work. Furthermore: in 1912 he was already able to, but did not apply Markov chains. At the same time, however, he became the author of a treatise that for about 20 years had remained the main writing on probability in Europe. Le Cam's declaration (1986, p. 81) that neither Bertrand, nor Poincaré *appeared to know* the theory was unjust: he should have added that, at the time, Markov was apparently the only one who did master probability.

7.6. Supplement to § 7.4. I still ought to discuss Bertrand's problem about the random chord and I seize the opportunity to introduce geometric probability (Sheynin 2003) and the notion of randomness (Sheynin 2011).

7.6.1. Geometric Probabilities. These were decisively introduced in the 18th century although the definition of the notion itself only occurred in the mid-19th century. Newton (§ 2.4.3) was the first to think about geometric probability. Beginning with Niklaus Bernoulli (1709/1975, pp. 296 – 297), see also Todhunter (1865, pp. 195 – 196), each author dealing with continuous laws of distribution effectively applied geometric probability. The same can be said about Boltzmann (1868/1909, p. 49) who defined the probability of a system being in a certain phase as the ratio of the time during which it is in that time to the whole time of the motion. Ergodic theorems can be mentioned, but they are beyond our boundaries.

However, it was Buffon who expressly studied the new notion. The first report on his work likely written by him himself was Anonymous (1735). Here is his main problem: A needle of length $2r$ falls *randomly* on a set of parallel lines. Determine the probability P that it intersects one of them. It is seen that

$$P = 4r/\pi a \qquad\qquad\qquad\qquad (7)$$

where $a > 2r$ is the distance between adjacent lines. Buffon himself had, however, only determined the ratio r/a for $P = 1/2$. His main aim was (Buffon 1777/1954, p. 471) *to put geometry in possession of its rights in the science of the accidental*. Many commentators described and generalized the problem above. The first of them was Laplace (1812/1886, p. 366) who noted that formula (7) enabled to determine [with a low precision] statistically the number π.

A formal definition of the new concept was only due to Cournot (1843, § 18). More precisely, he offered a general definition for a discrete and a continuous random variable by stating that probability was the ratio of the *étendue* of the favourable cases to that of all the cases. Now we replace *étendue* by *measure* (in particular, by area).

Michell (1767) attempted to determine the probability that two stars were close to each other. By applying the Poisson distribution, Newcomb (1859 – 1861, 1860, pp. 137 – 138) calculated the probability that some surface with a diameter of 1° contained s stars out of N scattered "at random" over the celestial sphere and much later Fisher (Hald 1998, pp. 73 – 74) turned his attention to that problem. Boole (1851/1952, p. 256) reasoned on the distinction between a uniform and any other law of distribution:

A <u>random distribution</u> meaning thereby a distribution according to some law or manner, of the consequences of which we should be totally ignorant; so that it would appear to us as likely that a star should occupy one spot of the sky as another. Let us term any other principle of distribution an indicative one.

His terminology is now unsatisfactory, but his statement shows that Michell's problem had indeed led to deliberations of a general kind.

Determine the probability that a random chord of a given circle is shorter than the side of an inscribed equilateral triangle (Bertrand 1888, p. 4). This celebrated problem had been discussed for more than a century and several versions of *uniform randomness* were studied. Bertrand himself offered three different solutions, and it was finally found out that, first an uncountable number of solutions was possible, and, second, that the proper solution was *probability equals* 1/2 and I note that it corresponded to *la perfaite perplexité de notre esprit* (§ 5). Thus failed the protracted discussion.

For a modern viewpoint on geometric probability see Kendall & Moran (1963); in particular, following authors of the 19th century (e.g., Crofton 1869, p. 188), they noted that it might essentially simplify the calculation of integrals. Then, Ambartzumian (1999) indicated that geometric probability and integral geometry are connected with stochastic geometry.

7.6.2. Randomness is a fundamental notion which inevitably enters statistics. For a popular discussion of recent mathematical efforts to define it, see Chaitin (1975). The history of that notion begins in antiquity; Aristotle and other early scientists and philosophers attempted to define, or at least to throw light upon randomness. His examples of random events are a sudden meeting of two acquaintances (*Phys*. 196b30) and a sudden unearthing of a buried treasure (*Metaphys*. 1025a). In both cases the event occurred without being aimed at and in addition they illustrate one of Poincaré's explanations (interpretations) of randomness (1907), then incorporated in his popular book (1908) and in his treatise (1912/1987, p. 4): if equilibrium is unstable,

A very small cause which escapes us determines a considerable effect […] *and we say that that effect is due to chance*.

Many authors had been repeating Aristotle's first example and Cournot's (1843, § 40) explanation can also be cited:

Events occurring as a combination or meeting of phenomena which apparently belong to independent series [but] *happening as ordered by causality, are called <u>fortuitous</u>, or results of <u>hazard</u>*.

Poincaré could have mentioned a coin toss. His deliberations (also see below) heralded the beginning of the modern period of studying randomness. However, Poincaré certainly had predecessors who only failed to mention directly randomness. Among them was the ancient physician Galen (1951, p. 202): I*n old men even the slightest causes produce the greatest change*; Pascal (1669/2000, p. 675): *Had Cleopatra's nose been shorter, the whole face of the Earth would have changed*; and Maxwell (1873a/1882, p. 364) who referred to the unstable refraction of rays within biaxial crystals. Elsewhere he (1859/1890, pp. 295 – 296) left a most interesting statement:

There is a very general and very important problem in Dynamics. [...] *It is this: Having found a particular solution of the equations of motion of any material system, to determine whether a slight disturbance of the motion indicated by the solution would cause a small periodic variation, or a total derangement of the motion.*

Given a large number of births, regularities of such mass random events will, however, certainly reveal themselves but Aristotle did not connect such events with randomness. *Corruption of,* or *deviation from laws of nature* also means randomness, and this idea can be traced at least until Lamarck who stated that the deviations from the divine lay-out of the tree of animal life had been occasioned by a *cause accidentelle* (Lamarck 1815, p. 133).

There also, on p. 173, he indicated that the spontaneous generation of organisms was caused by a *très-irrégulière* force but did not mention randomness. When considering the state of the atmosphere, Lamarck (1800 – 1811/1800, p. 76) stated that it was disturbed by two kinds of causes, including *variables, inconstantes et irrégulières.* Again, no mention of randomness, but then he (1810 – 1814/1959, p. 632) denied it: no *part of nature* disobeys *invariable laws*; therefore *that, which is called chance*, does not exist.

Louis Pasteur definitively disproved spontaneous generation, but until then it was apparently always considered random. Witness indeed Harvey (1651/1952, p. 338):

Creatures that arise spontaneously are called automatic […] *because they have their origin from accident, the spontaneous act of nature*.

Harvey did not say anything about the essence of accidents, but it seems that he thought them aimless, identified them with lack of law. Many other scientists denied randomness as Lamarck did.

I will now mention Laplace (1814/1995, p. 9) who stated that the arrangement of printed letters in the word *Constantinople is not due to chance*; all arrangements are equally unlikely, but that word has a meaning and it is *incomparably more probable* that someone had written it on purpose. He equated randomness with lack of purpose. This example shows that human judgement is needed for supplementing mathematical reasoning about randomness; intersection of events (above) can be additionally interpreted as lack of purpose.

Poincaré (1896/1912, p. 1) also formulated a dialectical statement about determinism and randomness much broader than the one following from *deviation from laws of nature*: it legitimizes randomness and indirectly defines it but does not say anything about regularities of mass random events:

In no field [of science] *do exact laws decide everything, they only trace the boundaries within which randomness is permitted to move. According to this understanding, the word* <u>*randomness*</u> *has a precise and objective meaning*.

He thus restricted the action of his pattern *small cause – considerable effect*. Exact laws tolerate randomness, cf. Newton's statement about the system of the world (§ 2.4.3). He recognized randomness, although this time only in its *uniform* version as witnessed by the expression *blind fate*. Whether in English, or in equivalent French and German terms, scientists of the 17th and 18th centuries, if discussing randomness, mostly understood it in this sense. For example, Arbuthnot (§ 1.3.1-1), only compared Design with a discrete uniform distribution of the sexes of the new born babies. Maupertuis (1745/1756, pp. 120 – 121) indicated that the seminal liquid of *chaque individu* most often contained *parties* similar to those of their parents, but he (p. 109) also mentioned rare cases of a child resembling one of his remote ancestors as well as mutations (p. 121, a later term). It seems that Maupertuis thus recognized randomness with a multinomial distribution, but, when discussing the origin of eyes and ears in animals, he (1751/1756, p. 146) only compared *une attraction uniforme & aveugle* [blind] and *quelque principe d'intelligence* (and came out in favour of design).

A chaotic process engendered by a small corruption of the initial conditions of motion can lead to its exponential deviation. Only in a sense this may be understood as an extension of Poincaré's pattern *small cause – considerable effect*. However complicated and protracted is a coin toss, it has a constant number of outcomes whose probabilities persist, whereas chaotic motions imply rapid increase of

their instability with time and countless positions of their possible paths. Their importance in mechanics and physics is unquestionable. My explanation of the comparatively new concept is only qualitative, but I have not seen any better.

In statistics, a random variable should be statistically stable, but in natural science this restriction is not necessary. Lamarck (see above) provided a good example of the latter phenomena: the deviations from the divine lay-out of the tree of animal life. Kolmogorov (1983/1992, p. 515) properly stated:

We should distinguish between randomness in the wider sense (absence of any regularity) and stochastic random events (which constitute the subject of probability theory).

There seems to be no quantitative criteria of statistical stability which apparently characterizes observations belonging to a single law of distribution, to a single population. However, practice often has to work in its absence; example: sampling estimation of the content of the useful component in a deposit. Choose other sample points, and it will be unclear whether they possess the same statistical properties (Tutubalin 1972, p. 7). But, according to scientific folklore, pure science achieves the possible by rigorous methods, whereas applications manage the necessary by possible means.

I provide now an example of a false conclusion caused by lack of statistical stability of the considered deviations. William Herschel (1817/1912, p. 579), who certainly knew nothing either about the size of stars or of their belonging to different spectral classes, decided that the size of a randomly chosen star will not much differ from the mean size of all of them. The sizes of stars are enormously different and their mean size is a purely abstract notion. There are stars whose radii are greater than the distance between the Sun and the Earth. Again, *ex nihilo nihil fit*.

Earlier, De Moivre (1733/1756, pp. 251 – 252) refused to admit randomness in the wide sense:

Absurdity follows, if we should suppose the Event not to happen according to any Law, but in a manner altogether desultory and uncertain; for then the Event would converge to no fixt Ratio at all.

8. The First Half of the 20th Century

8.1. Karl Pearson (1857 – 1936) was an applied mathematician and philosopher and the creator of biometry, the main branch of what later became mathematical statistics.

Pearson studied physics on which he expressed some extremely interesting ideas. Thus, *negative matter* exists in the universe (1891, p. 313); *all atoms in the universe of whatever kind appear to have begun pulsating at the same instant* (1887, p. 114) and *physical variations effects* were perhaps *due to the geometrical construction of our space* (Clifford 1885/1886, p. 202). He did not, however, mention Riemannian spaces whereas it is nowadays thought that the curvature of space-time is caused by forces operating in it. Remarkable also was Pearson's idea (1892, p. 217) about the connection of time and space subjectively expressed as:

Space and time are so similar in character, that if space be termed the breadth, time may be termed the length of the field of perception.

Mach (1897), in his Introduction, mentioned K. P. in the first edition of his book which appeared after 1892:

The publication [of the *Grammar of Science*] *acquainted me with a researcher whose* <u>erkenntnisskritischen</u> [Kantian] *ideas on every important issue coincide with my own notions and who knows how to oppose, candidly and courageously, extra-scientific tendencies in science.*

Again in the same contribution we find Pearson's celebrated maxim (1892, p. 15): *The unity of all science consists alone in its method, not in its material.* I return to this statement in § 9. Here, I indicate that Pearson, a Fellow of the Royal Society since 1896, was unable to take up the invitation of Newcomb, the president of the forthcoming International Congress of Arts and Sciences (St. Louis, 1904), to deliver there a talk on methodology of science (Sheynin 2002, p. 163, note 8).

At the very end of the 19th century, by founding the celebrated *Biometrika*, Galton, Pearson and Weldon (who died in 1906) established the Biometric school which aimed at the creation of methods of treating biological observations and of studying statistical regularities in biology. Pearson became the chief (for many years, the sole) editor of that periodical and. In the Editorial, in its first issue of 1902, we find a reference to Darwin:

[*E*]*very idea of Darwin – variation, natural selection* […] *– seems at once to fit itself to mathematical definition and to demand statistical analysis.*

K. P. compiled contributions on Weldon (1906) and on Galton's life and achievements, a fundamental and most comprehensive tribute to any scholar ever published (1914 – 1930). Incidentally, Chr. Bernoulli (1841, p. 389) had coined the word *Biometric* (in German) which referred to mass observations. The speedy success of the Biometric school had been to a large extent prepared by the efforts of Edgeworth (1845 – 1926); his collected writings appeared in 1996.

The immediate cause for establishing *Biometrika* seems to have been scientific friction and personal disagreement between Pearson and Weldon on the one hand, and biologists, especially Bateson, on the other hand, who exactly at that time had discovered the unnoticed Mendel. It was very difficult to correlate Mendelism and biometry: the former studied discrete magnitudes while the latter investigated continuous quantitative variations. However, in 1926 Bernstein (Kolmogorov 1938, § 1) proved that under wide assumptions the Galton law of inheritance of quantitative traits was a corollary of the Mendelian laws.

Pearson's results in statistics include the development of the elements of correlation theory and contingency; introduction of the *Pearsonian curves* for describing empirical distributions; and a derivation of a most important chi-squared test for checking the correspondence of experimental data with one or other law of distribution, as well as the compilation of many important statistical tables.

Pearson's posthumously published lectures (1978) examined the development of statistics in connection with religion and social

conditions of life. On the very first page we find the statement about the importance of the history of science: *I do feel how wrongful it was to work for so many years at statistics and neglect its history.* However, he provided a false appraisal of the Bernoulli LLN (§ 2.4.5).

Pearson attempted, often successfully, to apply the statistical method, and especially correlation theory, in many branches of science. Here is his interesting pronouncement (1907, p. 613):

I have learnt from experience with biologists, craniologists, meteorologists, and medical men (who now occasionally visit the biometricians by night!) that the first introduction of modern statistical method into an old science by the layman is met with characteristic scorn; but I have lived to see many of them tacitly adopting the very processes they began by condemning.

It is instructing to note the different views held of K. P. by other scientists. Kolmogorov (1947, p. 63) stated that

The modern period in the development of mathematical statistics began with the fundamental works of English statisticians (K. Pearson, Student, Fisher) which appeared in the 1910s, 1920s and 1930s. Only in the contributions of the English school did the application of probability theory to statistics cease to be a collection of separate isolated problems and became a general theory of statistical testing of stochastic hypotheses (i. e., of hypotheses about laws of distribution) and of statistical estimation of parameters of these laws.

Kolmogorov (p. 64 of same paper) had not then duly appreciated Fisher, and here is his possible explanation:

The investigations made by Fisher, the founder of the modern British mathematical statistics, were not irreproachable from the standpoint of logic. The ensuing vagueness in his concepts was so considerable, that their just criticism led many scientists (in the Soviet Union, Bernstein) to deny entirely the very direction of his research.

A year later Kolmogorov (1948b/2002, p. 68) criticized the Biometric school:

Notions held by the English statistical school about the logical structure of the theory of probability which underlies all the methods of mathematical statistics remained on the level of the eighteenth century.

Fisher (1922, p. 311) expressed similar criticisms as did Chuprov (Sheynin 1990/2011, p. 149); Chuprov (Ibidem) informed his correspondents that Continental statisticians (especially Markov) did not wish to recognize Pearson.

Here are some other opinions about Pearson.

1) Bernstein (1928/1964, p. 228), when discussing *a new cycle of problems in the theory of probability which comprises the theories of distribution and of the general non-normal correlation*, wrote:

From the practical viewpoint the Pearsonian English school is occupying the most considerable place in this field. Pearson fulfilled an enormous work in managing statistics; he also has great theoretical merits, especially since he introduced a large number of new concepts and opened up practically important paths of scientific research. The justification and criticism of his ideas is one of the

central problems of current mathematical statistics. Charlier and Chuprov, for example, achieved considerable success here whereas many other statisticians are continuing Pearson's practical work, definitely losing touch with probability theory …

2) Fisher, letter of 1946 (Edwards 1994, p. 100):

He was singularly unreceptive to and often antagonistic to contemporary advances made by others in [his] *field.* [Otherwise] *the work of Edgeworth and of Student, to name only two, would have borne fruit earlier.*

Fisher (1937, p. 306) also accused Pearson: his *plea of comparability* [between the methods of moments and maximum likelihood] *is* […] *only an excuse for falsifying the comparison* […]. Pearson died in 1936, but his son, Egon, kept silent.

3) But there are also testimonies of a contrary nature: Mahalanobis, in a letter of 1936 (Ghosh 1994, p. 96): he *always looked upon* [K. P.] as his *master,* and upon himself*, as one of his humble disciples.* And Newcomb, who had never been Pearson's student, wrote in a letter of 1903 to him (Sheynin 2002, p. 160):

You are the one living author whose production I nearly always read when I have time and can get at them, and with whom I hold imaginary interviews while I am reading.

4) Hald (1998, p. 651) offered a reasonable general description of one aspect of the Biometric school:

Between 1892 and 1911 he [Pearson] *created his own kingdom of mathematical statistics and biometry in which he reigned supremely, defending its ever expanding frontiers against attacks.* […] He *was not a great mathematician, but he effectively solved the problems head-on by elementary methods.*

5) Fisher (1956/1990, p. 3), however, ungenerously criticized Pearson for the *weakness of his mathematical and scientific work.*

In Russia, Chuprov and Slutsky defended Pearson's work against Markov's opposition (Sheynin 1990/2011, §§ 7.4 and 7.6). Chuprov wished to unite the Continental direction of statistics with biometry, but did not achieve real success.

Lenin's criticism of Pearson was in itself a sufficient cause of the negative Soviet attitude towards Pearson. Maria Smit's statement (1934, pp. 227 – 228) was its prime example: his curves are based

On a fetishism of numbers, their classification is only mathematical. Although he does not want to subdue the real world as ferociously as it was attempted by […] *Gaus* [Smit's spelling]*, his system nevertheless only rests on a mathematical foundation and the real world cannot be studied on this basis at all.*

In 1931 this troglodyte (Corresponding member of the Soviet Academy of Sciences since 1939!) declared: *The crowds of arrested saboteurs are full of statisticians* (Sheynin 1998, p. 533, literal translation). She likely participated in enlarging that *crowd.*

However, the tone of the item *Pearson*, in the third edition of the *Great Sov. Enc.* (vol. 19, 1975/English edition: same volume, 1978, p. 366) was quite different: he *considerably contributed to the development of mathematical statistics* and Lenin had only criticized

his *subjective-idealistic interpretation of the nature of scientific knowledge*.

8.2. Markov is known to have opened up a new direction of probability theory dealing with dependent events, and in particular, to have introduced the *Markov chains*. At the same time, he refused to apply his chains to problems in natural sciences, did not apply the allegedly meaningless term *random magnitude* (as it is still called in Russia) and, similarly, the expressions *normal law* and *coefficient of correlation* were absent in his works. Like a student of Chebyshev that he was, he underrated the then emerging axiomatic approach to probability as well as the theory of functions of a complex variable (A. A. Youshkevich 1974, p. 125).

During his last years, in spite of extremely difficult conditions of life in Russia and his worsened health, he completed (perhaps not entirely) the last posthumously published edition of his *Treatise* but insufficiently described there the findings of the Biometric school; such scholars as Yule and Student (Gosset) were not mentioned and he (1900/1924, pp. 10, 13 – 19 and 24) even wrongly stated that he transferred probability to the realm of pure mathematics just by proving the addition and multiplication theorems. Actually, to some extent he became a victim of his own rigidity; he failed, or did not wish to notice the new tide of opinion in statistics, or even probability theory, see also Sheynin (2006).

Markov (1888) compiled a table of the normal distribution which gave it to 11 digits for the argument $x = 0 (0.001) 3 (0.01) 4.8$. Two such tables, one of them Markov's, and the other, published ten years later, remained beyond compare up to the 1940s (Fletcher et al 1946/1962).

Markov included some innovations in the last edition of his *Treatise*: a study of statistical series, linear correlation. He determined the parameters of lines of regression, discussed random variables possessing certain densities and included a reference to Slutsky (1912), whom he previously barely recognized, but paid no attention either to the chi-squared test or to the Pearsonian curves. The so-called Gauss – Markov theorem *invented* by Lehmann (1951), who followed Neyman's mistake (which Neyman himself later acknowledged), is a misnomer since it was due to Gauss alone.

8.3. The Continental Direction of Statistics. At the end of the 19th, and in the beginning of the 20th century, statistical investigations on the Continent were chiefly restricted to the study of population whereas in England scientific statistics was mostly applied to biology.

The so-called Continental direction of statistics originated as the result of the work of Lexis whose predecessors had been Poisson, Bienaymé, Cournot and Quetelet. Poisson and Cournot examined the significance of statistical discrepancies for a large number of observations without providing examples. Cournot also attempted to reveal dependence between the decisions reached by judges (or jurors). Bienaymé (1839) was interested in the change in statistical indicators from one series of trials to the next one and Quetelet (§ 7.1) investigated the connections between causes and effects in society,

attempted to standardize statistical data worldwide and, following Süssmilch (§ 1.3), created moral statistics.

At the same time statisticians held that the theory of probability was only applicable to statistics if *equally possible cases* were in existence, and the appropriate probability remained constant (§ 2.4.5).

8.3.1. Lexis (1879) proposed a distribution-free test for the equality of probabilities in different series of observations; or, a test for the stability of statistical series. Suppose that there are m series of n_i observations, $i = 1, 2, …, m$, and that the probability of success p was constant throughout. If the number of successes in series i was a_i, the variance of these magnitudes can be calculated by two independent formulas (Lexis 1879, § 6)

$$\sigma_1{}^2 = pqn, \ \sigma_2{}^2 = [vv]/(m-1) \tag{1; 2}$$

where n was the mean of n_i, v_i, the deviations of a_i from their mean, and $q = 1 - p$. Formula (2) was due to Gauss (§ 6); he also knew formula (1), see Gauss, W-8, p. 133. The frequencies of success can also be calculated twice. Note however that Lexis applied the probable error rather than the variance and mistakenly believed that the relation between the mean square error and the probable error was distribution-free. He (§ 11) called the ratio

$$Q = \sigma_2/\sigma_1$$

the *coefficient of dispersion*. For him, the case $Q = 1$ corresponded to normal dispersion (with admissible random deviations from unity); he called the dispersion supernormal, and the stability of the observations subnormal if $Q > 1$ (and indicated that the probability p was not then constant); finally, Lexis explained the case $Q < 1$ by dependence between the observations, called the appropriate variance subnormal, and the stability, supernormal. He did not, however, pay attention to this possibility. His coefficient was the ratio of the appearance of the studied event as calculated by the Gauss formula to that peculiar to the binomial distribution.

Lexis hardly thought about calculating the mean value and variance of Q (and in any case that was a serious problem). In 1916, both Markov, and, much better, Chuprov derived EQ and, in a manuscript of 1916 or 1917, Chuprov derived the mean square error of Q.

8.3.2. In 1910 Markov and **Chuprov**, in their letters to each other (Ondar 1977), proved that some of the Lexian considerations were wrong. Then, in 1918 – 1919, Chuprov formulated the shortcomings of Q as a criterion but, strangely enough, he somehow kept to the Lexian theory until 1921. Indeed, in a letter of 30 Jan. 1921 to a friend Chuprov wrote:

One of the most important doctrines of theoretical statistics, which I until now entirely accepted and professed, the Lexian theory of stability of statistical figures is to a large extent based on a mathematical misunderstanding.

Concerning this paragraph see Sheynin (1990/2011, pp. 140 – 143).

The refutation of those Lexian considerations was apparently barely noticed. Bernstein (1928/1964, p. 224) called them *the first important step of the scientific treatment of statistical materials* and even much later Särndal (1971, pp. 376 – 377) who described this subject did not mention any criticisms of Lexis. As it seems, Bernstein also positively although obliquely referred to the non-existing Bortkiewicz' law of small numbers (1898): *Poisson's investigations had been recently specified and essentially supplemented.*

Yes, Lexis thought of basing statistical investigations on a stochastic foundation (although so did Jakob Bernoulli), and he also made a forgotten attempt to define stationarity and trend. In a paper devoted to the application of probability theory to statistics, Lexis (1886, pp. 436 – 437) stated that the introduction of *equipossibility* led to the subjectivity of the theory of probability. He did not say that the existence of equally possible cases was not necessary. This point haunted him (Lexis 1913, p. 2091).

8.3.3. Bortkiewicz had introduced his own test, Q', not coinciding with the Lexian Q, and equal to the ratio of two dependent random variables, call them ξ and η. Unlike Q, Q' cannot be less than 1 (1898, p. 31). Later Bortkiewicz (1904, p. 833) noted that $EQ = Q'$ but mistakenly justified this equality by believing that, for those dependent variables, $E\xi/\eta = E\xi/E\eta$. Then, he (1918, p. 125n) unjustifiably admitted that the equality was only insignificantly approximate. Chuprov (1922) devoted a paper to that subject. See my discussion (1990/2011, pp. 59 – 62) of the Lexian innovation. In particular, I quoted Bortkiewicz' letter to Chuprov of 29 March 1911: *Poisson cannot at all be considered the own father of the law of small numbers* since he, Bortkiewicz, did not regard a low level of the probability of the studied event as the *decisive point.* Rarity, he continued, can mean a small number of occurrences of that event when the number of trials was also small. He thus undermined his alleged law! Delicate Chuprov did not comment.

8.3.4. The Two Statistical Streams. Bauer (1955, p. 26) investigated how the Biometric school and the Continental direction of statistics had been applying analysis of variance and concluded (p. 40) that their work was going on side by side but did not tend to unification. For more details about Bauer`s study see Heyde & Seneta (1977, pp. 57 – 58) where it also correctly indicated that, unlike the Biometric school, the Continental direction had concentrated on nonparametric statistics. Chuprov can be certainly mentioned here. He achieved some important results; for example, he discovered finite exchangeability (Seneta 1987).

However, his formulas, *being of considerable theoretical interest*, were *almost useless* due to complicated calculations involved (Romanovsky 1930, p. 216). In addition, he had not paid due attention to notation. Thus, in one case he (1923, p. 472) applied two-storey superscripts and two-storey subscripts in the same (five-storey!) formula. Hardly has any other author (not even Bortkiewicz) allowed himself to take such liberties, to expect his readers to digest suchlike monsters.

For his part, Bortkiewicz just had not respected his readers. Winkler (1931, p. 1030) quoted his letter (but did not provide its date) in which Bortkiewicz mentioned that he expected to have five readers of his (unnamed by Winkler) publication. Statisticians had not been mathematically educated and despised mathematics; for them, Bortkiewicz remained an alien body.

I myself (Gnedenko & Sheynin 1978/2001, p. 275), probably following other authors, suggested that mathematical statistics properly originated as the coming together of the two streams. However, now I correct myself. At least until the 1920s, say, British statisticians had continued to work all by themselves. E. S. Pearson (1936 – 1937), in his study of the work of his father, had not commented on Continental statisticians and the same is true about other such essays (Mahalanobis 1936; Eisenhart 1974). I believe that English, and then American statisticians for the most part only accidentally discovered some findings already made by the Continental school. Furthermore, the same seems to happen nowadays as well. Even Hald (1998) called his book *History of Mathematical Statistics*, but barely studied the work of that school.

In 1919 there appeared in *Biometrika* an editorial entitled *Peccavimus*! (we were guilty). Its author, Pearson, corrected his mathematical and methodological mistakes made during several years and revealed mostly by Chuprov (Sheynin 1990a/2011, p. 75) but he had not taken the occasion to come closer to the Continental statisticians. In 2001, five essays were published in *Biometrika*, vol. 88, commemorating its centenary. They were devoted to important particular issues, but nothing was said in that volume about the history of the Biometric school, and certainly nothing about Continental statisticians.

8.3.5. Statistics and Sociology in the Soviet Union. Concerning the general situation there, see Sheynin (1998).

Sociology studies society, its institutions, population, existing tendencies and attempts to discern possible developments. Statistics naturally essential for such investigations, and many statisticians from Graunt to Quetelet to modern specialists can be cited here. Here, I am only concerned with the year 1954 and begin by quoting two authors (Schlözer 1804, p. 51) and Truesdell (1981/1984, pp. 115 – 117) who invented two terms, *plebiscience* which describes modern times and *prolescience* of the future:

Statistics and despotism are incompatible.

Prolescience will *confirm and comfort the proletariat in all that will by then have been ordered to believe*. […] *That will be mainly social science*.

Süssmilch attempted to reveal divine order in demography, but official Soviet statistics regarded statistics as a discipline reduced to corroborate quantitatively Marxist propositions. Many participants in a statistical conference held in Moscow in 1954 voiced that opinion (Anonymous 1954; see also Kotz 1965; Sheynin 1998, pp. 540 – 541). Only *the revolutionary Marxist theory* is the basis for developing statistics as a social science (p. 41); statistics does not study mass random phenomena (p. 61) which anyway possess no special features

(p. 74); the LLN is not a mathematical proposition (p. 64); probability is not the necessary basis of statistics, the theory of stability of statistical series is a *bourgeois theory* and even honest *bourgeois* statisticians are compelled to violate their professional duty (p. 46, the notorious and immortal Maria Smit, see § 8.1).

K. V. Ostrovitianov (p. 82), the vice-president of the Academy of Sciences, ignorantly declared that Lenin had *completely subordinated* [adapted] *the statistical methods of research* […] *to the class analysis of the rural population*. And, as he menacingly continued, the same scientific methods cannot be used in astronomy and economics. His latter statement directly contradicted Kolmogorov's (pp. 46 – 47) definition of mathematical statistics who also mentioned several *safe* areas of application of the statistical method (studies of the work of telephone exchanges, management of life insurance, determination of necessary stocks of foodstuffs) but omitted population statistics. This subject was dangerous. The census of 1937 was proclaimed worthless and followed by a decimation of the Central Statistical Directorate: it revealed a demographic catastrophe occasioned by arbitrary rule, uprooting of millions, mass hunger and savage witch-hunt. And the war losses had to be hushed up.

Much later, still in accord with the resolution of the conference, Riabushkin (1980, p. 498) argued that statistical descriptions should be inseparably bound with life's *qualitative content*, i. e., with Marxism. In itself, that requirement was not new at all, see Buniakovsky (1866, p. 154); Chuprov (1903/1960, p. 42); Fisher (1935, p. 1).Ten more years had to pass before Orlov (1990) *rejected* the decisions of that conference, revealed the falsifications of Soviet statistics and its backwardness (certainly known abroad).

9. The Unity of Statistics Consists Alone in Its Method, in Mathematical Statistics

Schlözer (1804) called his book *Theory of statistics*, but it did not contain any theory in our sense. Bearing in mind other authors of the first half of the 19th century, I believe that in those times theory of statistics meant a systematic arrangement of statistical data according to reasonably chosen indicators. Schlözer had not mentioned either political arithmetic or Jakob Bernoulli, did not clearly define the interrelations of statistics, politics and history, and his bibliographic indications were often barely useful. I consider his book unsatisfactory.

Even Achenwall had a theory (of Staatswissenschaft) in that same sense, and, as it seems, so did Delambre (1819, p. LXVII) and Fourier (1821, pp. iv – v) and the London Statistical Society (Anonymous 1839, p. 1). Delambre argued that statistics ought not to engage in discussions or conjectures or to aim at perfecting theories, and that Society declared that statistics does not *discuss causes* nor *reason upon probable effects*. True, these *absurd restrictions* have been necessarily *disregarded* (Woolhouse 1873, p. 39), – I would say, they became obsolete, but no theory of statistics had yet emerged.

The very title of Dufau (1840) called statistics the theory of studying the laws according to which the social events are developing. And, without mentioning any theories, a kindred idea was pronounced

much earlier (Gatterer 1775, p. 15): Just as in history it is necessary *to investigate not only the Pourquoi, but also the Pourquoi of the Pourquoi, so it is necessary in statistics to explain the present state of a nation by its previous states*.

This *Pourquoi of the Pourquoi* likely came from Sophie Charlotte, Queen of Prussia, apparently from her letter to Leibniz (Krauske 1892, p. 682). Cf. Cournot (1843, § 106):

The essential goal of the statistician, just like of any other observer, is to penetrate as deeply as possible into the knowledge of the essence of things.

Perhaps Cauchy (1845/1896, p. 242) can also be cited: statistics was *infallible* in judging doctrines and institutions.

Here is how Chuprov's student and the last representative of the Continental direction, Anderson (1932, p. 243), described the previous situation of the application of probability in statistics:

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

But did they (or Chuprov, whom Anderson later added to them) really overcome the occurring difficulty? Did they convince statisticians? In any case, the situation changed only gradually. Only in the mid-20th century Neyman (1950, p. 4), Mises (1964a, posthumous, p. 1) and Kendall (1978, p. 1093) stated that mathematical statistics (a section of the theory of probability, as the two first authors held) was the mathematical theory of statistics. The relations between probability theory and mathematical statistics does not directly bear on statistics and I only note that Kolmogorov (1948a, p. 216) thought that *the theory of probability must be considered the structural part of mathematical statistics*, but that (p. 218) *statistics only gradually ceases to be the applied theory of probability*. And (p. 216) mathematical statistics is a *science of the mathematical methods of studying mass phenomena.*

Later, however, Kolmogorov (Anonymous 1954, pp. 46 – 47) only declared that mathematical statistics is not an applied theory of probability. Then, *mass phenomena* is too restrictive. Anyway, much later Kolmogorov provided quite another definition of mathematical statistics, see below.

The following two definitions should perhaps be altered by substituting *theory of statistics* instead of *statistics* and *statistical data* instead of *mass observations*; they both will then be in line with the definitions above.

Fisher (1925, p. 1) argued that statistics *is a branch of applied mathematics and may be regarded as mathematics, applied to observational data*. K. Pearson (1978, p. 3) stated that *statistics is the application of mathematical theory to the interpretation of mass observations*.

Alph. De Candolle (1833, p. 334) and Chaddock (1925, p. 26) thought that statistics is a branch of mathematics. Here also, this rather incomplete definition can be altered to conform to those of Neyman, Mises and Kendall.

According to the comparatively new definition of Kolmogorov & Prokhorov (1988/1990, p. 138),

Mathematical statistics is a branch of mathematics devoted to systematizing, processing and utilizing statistical data, or information on the number of objects in some more or less extensive collection that have some specific properties.

They (p. 139) also argued that *the method of research, characterized as the discussion of statistical data,* [...] *is called statistical* and consists in calculating the number of objects *in some group or other*, in discussing the distribution of quantitative indicators, applying the method of sampling and estimating the adequacy of the number of observations etc. (p. 139).

Kolmogorov & Prokhorov's definition apparently excluded the theory of errors and in addition it remains unclear whether the *information* was raw or corrected, either initially or during *systematization* by means of exploratory data analysis, – whether they incorporated that stage of work into mathematical statistics. See § 7.2 on the difference between mathematical and theoretical statistics.

Many definitions are more or less akin to theirs, although their authors sometimes discuss statistics instead of theory of statistics or mathematical statistics. Thus (Butte 1808, p. XI),

Statistics is a science of the art [science and art] *of the knowledge and due estimation of statistical data, of their collection and systematic analysis.*

Zhuravsky (1846, p. 173): statistics is a calculus of categories, which distributes objects among the categories and counts them in each category. He thought that *statistics is a special and very wide science.* Maxwell (1871/1890, vol. 2, p. 253; 1877, p. 242) defined the statistical method as an *estimation of an average condition of a group of atoms*, as *a study of the probable number of bodies in each group* under investigation.

Some modern definitions have been offered by Egon Pearson (Bartholomew 1995, p. 7), Kendall (1950, p. 130), Kendall & Buckland (1971), Marriot (1991), Bancroft (1966, p. 530), Kruskal (1978, p. 1072), Wilks (1968, p. 162), anonymous authors (1968, p. 166; 1985, p. 230) and Dodge (2003, p. 388).

The first two definitions are rather abstract as also, to a lesser extent, is the fourth one; the others have much in common with Kolmogorov & Prokhorov's. And here is Dodge:

Statistics is a science of collecting, analysing and interpreting the data (the *numerical* information *relating to an aggregate of individuals*).

Several authors have preferred a narrower and therefore hardly sufficient definition of statistics. Chuprov, in his unpublished thesis of 1896 (Sheynin 1990/2011, p. 118), as well as Lindley (1984, p. 360) and Stigler (1986, p. 1) believed that it measures our ignorance or uncertainty. And Chernoff & Moses (1959, p. 1) even stated that

Today's statistician will be more likely to say that statistics is concerned with decision making in the face of uncertainty (than with *processing* of data).

Cf. Mahalanobis' statement of 1950 (Rao 1993, p. 339): *The aim of statistics is to reach a decision on a probabilistic basis, on available evidence*. And Bancroft (1966), remarked that *statistical inferences are made in the face of uncertainty*.

Several authors held that statistics is only a method (Fox 1860, p. 331; Miklashevsky 1901, p. 476). Alph. De Candolle (1873, p. 12) reversed his own much earlier opinion, agreed with that statement and even contrasted statistics with mathematics mistakenly arguing that the latter (only) provided deterministic conclusions.

It is time to formulate my own conclusions.

1. Statistics and statistical method: in § 1.3 I noted that these terms are (sometimes) understood as synonyms. More precisely, the statistical method is almost the same as mathematical statistics or theory of statistics.

2. Such expressions as *stellar* or *medical statistics* mean the application of the statistical method to stellar astronomy or medicine.

3. Statistical theory or mathematical statistics rather than statistics as a whole may perhaps be likened to a statistical method or a series of statistical procedures.

4. Sociology or the science of the life of groups of men in a society naturally applies the statistical method.

5. The stochastic theory of errors is the application of the statistical method to the treatment of observations. This statement contradicts the definition of Kolmogorov & Prokhorov, but I believe that their understanding of statistical data may well be generalized to include results of observations or measurements.

6. K. Pearson (§ 8.1) stated that the *unity of all science consists alone in its method …* To a certain extent this maxim is borne out by the essence of statistical method. Kruskal (1978, p. 1082) thought that *statistics has a neighbourly relation with philosophy of science*, but I will argue that *statistics* ought to be replaced here by *statistical method*. Recall also Achenwall (beginning of § 1.1): statistics *belongs to a well digested philosophy*. **Only mathematical statistics can be the theory of that discipline**. Cf. Items 1 and 3.

7. An afterthought: For statistics, the axiomatized theory of probability is useless.

Bibliography

Abbreviation: AHES = *Arch. Hist. Exact Sci.*
JNÖS = *Jarbücher f. Nationalökonomie u. Statistik*
Gauss W/Erg-i = Gauss, *Werke*, Ergänzungsreihe, Bd. i.
Gauss (1816/1887, p. 130) means that I refer to the edition of 1886 of the memoir of 1816.

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