The True Value of a Measured Constant and the Theory of Errors

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Abstract

The theory of errors is a discipline indispensable to experimental science at large, and *true value* of a measured constant is one of its main notions. I reject a modern statement which claims that the true value "syndrome" is left behind. I dwell on the history of that notion, – on its heuristic use, informal connection with the arithmetic mean of the pertinent observations, and on its formula (Laplace, Fourier), forgotten perhaps up to the mid-20th century. Von Mises, although not really interested in the theory of errors, effectively connected *true value* with his *frequentist* definition of probability as the limit of the corresponding statistical frequency. Mathematical statistics largely but not completely moved from the true value to the estimation of parameters of functions. Condorcet hesitatingly introduced an intermediate theory of means which studied the determination of both true values and abstract mean values but which became divided between statistics and the theory of errors.

Key words: Experimental science; Frequentist theory of probability; Theory of errors; Theory of means; True value of constant

1. Introduction

From the most ancient times astronomers have been measuring the coordinates of the fixed stars, i.e., of presumably constant magnitudes. Actually, however, this supposition, as will be seen in the sequence, is not really true.

The concept of *true value of a measured constant* had always been inseparably linked with the measurements themselves; only mathematical statistics (almost) changed this situation. Thus, Al-Biruni (1967, p. 83): "Now all the testimonies that we have adduced point out collectively that the [obliquity of the ecliptic] is …" And here is Cotes (1722/1768, p. 22), also without using the term *true value*: "The place of some object defined by observation[s] …"

My second concept is *theory of errors* which I define as the statistical method (statistics) applied to the treatment of observations in experimental science. I only deal with its *stochastic branch*; its *determinate* branch might be related to experimental design.

2. The Arithmetic Mean and the True Value

The first to connect directly these two notions was possibly Picard (1693, pp. 330, 335, 343) who called the arithmetic mean the true (*véritable*) value (of the angle measured in triangulation). The next, and much more outspoken author was Lambert. First, he (1760, §286) stated:

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Da nun Fehler um so häufiger auftreten, je kleiner sie sind, folgt daraus, daß in einem beliebigen gegebenen Fall nach wiederholten Versuchen die häufiger auftretenden Größen dem Mittelwert oder auch dem wahren Wert näher liegen.

[*Since errors happen the oftener, the smaller they are, it follows that in any given case of repeated experiments the more frequently occurring quantities are situated nearer to the mean value, or, also, to the true value*.]

And in §290 he added that the error of the arithmetic mean was much smaller than that of a single observation and that consequently the mean was nearer to the true value. Then, Lambert (1765, §3) argued that, if, in modern terms, the density curve of the observational errors was even,

Das Mittel aus mehrern Versuchen dem wahren desto näher kommen müsse, je mehr der Versuch ist wiederholt worden. Denn unter allen Fällen, die man sich dabey gedenken kann, ist derjenige am möglichsten, wobey gleich große Abweichungen auf beyden Seiten gleich ofte vorkommen.

[*The mean of a large number of experiments ought to move the nearer to the truth, the more is the experiment repeated. Because, among all the cases which might be imagined, the most possible is that in which equally large deviations to both sides occur equally often.*]

He, as well as some later authors, see below, tacitly (but almost directly in his previous case) assumed that the density was unimodal and not *bad* (cf. for example the Cauchy distribution under which a single observation is not worse than the mean) and he certainly had not proved his statements. Thus, only Thomas Simpson, in 1756, proved the essence of Lambert's §290, and, for that matter, only for two distributions.

That the mean tends to the appropriate theoretical parameter is now called, in statistics, the limit property of consistency which holds for linear estimators in general. In my context, however, this remark is hardly of consequence.

My next author here is Laplace. He (1795/1912, p. 161) stated that with an unrestricted increase in the number of observations their mean converged to a certain number, so that

Si l'on multiplie indéfiniment les observations ou les expériences, leur résultat moyen converge vers un terme fixe, de manière qu'en prenant de part et d'autre de ce terme un intervalle aussi petit que l'on voudra, la probabilité que le résultat moyen tombera dans cet intervalle finira par ne différer de la certitude que d'une quantité moindre que toute grandeur assignable.
Ce terme est la vérité même si les erreurs positives et négatives sont également faciles …

[*If we multiply observations or experiments indefinitely, their mean result will tend to a fixed term, so that, taking on both its sides an interval as small as you wish, the probability that the mean result finds itself there will* *finally differ from certitude by a quantity less than any assigned magnitude.*

This term is the truth itself provided that positive and negative errors are equally likely …]

He repeated this statement word for word (1810a/1898, p. 303), and he also repeated it elsewhere, either a bit later, or a bit earlier (1810b/1979, p. 110/272), writing *se confond avec le vérité* [*merges with the truth*] instead of *est la vérité meme* [*is the truth itself*].

And in his *Essai philosophique* (1814/1886, p. LVI) which originated from the *Leçons* of 1795, we find:

Plus les observations sont nombreuses et moins elles s'écartent entre elles, plus leurs résultats approchent de la vérité.

[*The more numerous are the observations and the less they deviate one from another, the nearer their results approach the truth.*]

He added that the optimal mean results were determined by probability theory. Now, it is generally known that he strongly advocated (and furthered) the method of least squares; hence, when discussing the case of one unknown, as above, he certainly meant the arithmetic mean. In the fifth edition of the *Essai* (1825) Laplace also left a similar pronouncement concerning the general case (p. 44 of the English translation (1995) of that edition).

I hasten to add that Gauss had not left anything comparable. When providing his first justification of the method of least squares, he (1809/1887, §177) issued from the hypothesis that the arithmetic mean was the most probable value of the constant sought, or very close to it.

Understandably, Poisson (1811, p. 136; 1824, p. 297; 1829, pp. 12 and 19) followed his predecessors in that he used the term *vraie valeur* and indirectly stated that this value was the mean of infinitely many observations.

3. The Definition

Fourier (1826/1890, pp. 533 – 534) provided the still lacking formal definition:

Supposons donc que l'on ait ajouté ensemble un grand nombre de valeurs observées, et que l'on ait divisé la somme par le nombre m, ce qui donne la quantité A pour la valeur moyenne; nous avons déjà remarqué que l'on trouverait presque exactement cette même valeur A, en employant un très grand nombre d'autres observations. En général, si l'on excepte des cas particuliers et abstraits que nous n'avons point à considérer, la valeur moyenne ainsi déduite d'un nombre immense d'observations ne change point; elle a une grandeur déterminée H, et l'on peut dire que le résultat moyen d'un nombre infini d'observations est une quantité fixe, où il n'entre plus rien de contingent, et qui a un rapport certain avec la nature des faits observés. C'est cette quantité fixe H que nous avons en vue comme le véritable objet de la recherche.

[*Suppose therefore that a large number of observations are added together, and their sum is divided by* [their] *number, m, which provides the* *quantity A for the mean value. We have already remarked that almost exactly the same value A will be found when taking a very large number of other observations. In general, excepting particular and abstract cases which we will not consider at all, the mean value thus derived from an immense number of observations does not change at all. It has a certain magnitude H, and it is possible to say that the mean result of an infinite number of observations is a fixed quantity which never contains anything accidental anymore, and which is in a certain relation to the nature of the observed events.*

It is this fixed magnitude H that we have in mind as the veritable object of research.]

I doubt that his formula was widely noticed and in any case I was unable to find even a single reference to it; perhaps it was thought to be hardly needed. Nevertheless, a number of later authors repeated the same definition independently one from another, and likely, from him, see below. First, however, I turn to Markov (1924, p. 323) who cautiously, as was his wont, began the chapter on the method of least squares of his treatise by remarking that

It is necessary in the first place to presume the existence of the numbers whose approximate values are provided by observations.

A similar statement concerning an unknown probability is on p. 352; his first pronouncement was inserted in the edition of 1908 (perhaps even in the first edition of 1900), the second one appeared in the edition of 1913. Several remarks are in order.

1) Before and after Markov many scholars either expressly mentioned, or indirectly referred to the true value without bothering to define it (Gauss, in all of his writings pertaining to the treatment of observations; Markov himself 1899/1951, p. 250; Poincaré 1912, p. 176; Kolmogorov 1946, title of §7).

2) Probability (Markov's p. 352) is not an entity of the real world, at least not in the usual sense. This generalization of the concept under my study is an important point for a natural scientist, although not for Markov the mathematician. Incidentally, already Gauss (1816/1887, §§3 and 4), a mathematician *and natural scientist*, repeatedly considered the true value of a measure of precision of observations. See also Fisher's relevant statement in my §4.

3) I also note Markov's reluctance to step out of the field of mathematics: he had not mentioned true values at all which was hardly accidental. Recall that he never provided any applications of his *chains* to natural sciences.

Fourier's definition heuristically resembles Mises' celebrated formula for probability; strangely enough, no-one saw fit to mention this fact except Mises himself. Here is what he (1919/1964, pp. 40 and 46) actually stated, largely repeating Fourier:

Der "wahre" Wert der Beobachtung (d. i. derjenige, der sich als Durchschnitt bei einer ins Unendliche fortgesetzten Beobachtungsreihe ergeben müsste) … Der "wahre" Mittelwert ist nicht anderes als die Grösse, die nach der Definition des Wahrscheinlichkeitsbegriffes als arithmetisches

Mittel einer ins Unendliche fortgesetzten Ziehungsserie sich ergeben müsste.
[*The "real" value of the observation (that is, such that ought to occur as the mean value when the series of observations continues to infinity). …*
The "real" mean value is nothing but the magnitude that ought to occur by the definition of the concept of probability as the arithmetic mean when the series of drawings continues to infinity.]

In 1919, the corresponding page numbers were 80 and 87, and it was in that contribution that Mises first introduced his frequentist theory. In other words, the *concept of probability* [*Wahrscheinlichkeitsbegriff*] could have only been his frequentist definition of probability. But to explain the *drawings*. Suppose that an urn contains white and black balls and that *m* white balls and *n* black ones are extracted and returned back one by one. Then, as Mises stated, the ratio *m/n* approached the unknown ratio of the balls contained in the urn. This was his illustration of the connection of the true value and frequentist probability but he had not directly offered it as a formula.

My next author is also interesting because he (Eisenhart 1963/1969, pp. 30 – 31) deals with metrology, an important scientific discipline which statisticians hardly ever discuss when they (also on rare occasions) recall the theory of errors:

The "true value" of the magnitude of a quantity … is the limiting mean of a conceptual exemplar process … 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. I hope that the traditional term "true value" will be discarded in measurement theory and practice, and replaced by some more appropriate term such as "target value" …

And so, first, Eisenhart largely repeated Fourier. Second, here, as had always implicitly been the case before, he clearly stated that the residual systematic error was inevitably included in the true value. Third and last, Eisenhart's *hope* had not materialized, see below, but he was quite right when stating, in addition, that it was impossible to obtain any true value.

To conclude, I mention that Whittaker & Robinson (1924/1958, p. 215n) largely repeated the Fourier definition.

True mean is expectation although different values of a random variable reflect its intrinsic property of change whereas different values of observations of a measured constant are in the first place the result of our helplessness.

4. Mathematical Statistics and the Theory of Errors

Purportedly, mathematical statistics had done away with true values and introduced instead parameters of densities (or of distribution functions). Fisher (1922, pp. 309 – 310) was mainly responsible for this change; indeed, he introduced there the notions of consistency, efficiency and sufficiency of statistical estimators without any reference to the theory of errors or to true values. But then, on p. 311 we read that a

Purely verbal confusion has hindered the distinct formulation of statistical problems; for it is customary [for the Biometric school] *to apply the* *same name, mean, standard deviation, correlation coefficient, etc., both to the true value which we should like to know, but can only estimate, and to the particular value at which we happen to arrive by our methods of estimation.*

So the true value was still alive even in mathematical statistics. A few other examples. The *Dictionary* (Aleksandrov 1962) cites *true* correlation; mean; and value. Bolshev (1964, p. 566) dwells on the "true value of a parameter". His was a commentary on Bernstein (1941/1964) who mentioned a "true probability" of an inequality (in §5, p. 390 in 1964). Then, Smirnov & Dunin-Barkowski (1959/1973, pp. 16 and 17) had chosen to say *true value*.

But what about our contemporaries? Here is an opinion which I oppose (Chatterjee 2003, p. 264): the methods of the theory of errors "were rarely applied outside these narrow fields" [of astronomy and geodesy] and "the true value syndrome" "was ultimately left behind". First, I object to the *narrow fields* and note the author's failure to recognize metrology. And how about measurements in geophysics (of magnetism, or of the acceleration of gravity), or in physics (of the velocity of light in vacuum, or of the mass of electron), etc.?

Then, *syndrome* is usually connected with some abnormal condition. Second, since Chatterjee (pp. 248 – 249) still believes in the existence of the mysterious "well-known" Gauss-Markov theorem, I doubt that he is proficient in the history of statistics (and especially of the treatment of observations).

I am also dissatisfied with Chatterjee's statement (p. 273) that Quetelet was "mentally bound by … the true-value syndrome" and that, implicitly, for Quetelet variations were "of secondary importance". Even excluding meteorology, his important field of research beyond social statistics, Quetelet (Sheynin 1986) studied the change of the probability of conviction for differing groups of defendants (my §4.4 there), held that the *tables de criminalité pour les différents ages* [*tables of criminality for different ages*] merited full attention (p. 304 n 45) and declared that the normal law was *une les plus générale de la nature animée* [*one of the most general of the animate nature*] (p. 313), – especially in anthropometry. More about Quetelet in § 5 where I also dwell on the study of mean values (conditions).

Third, I cite Hald (1998) who described the *History of mathematical statistics from 1750* (when it did not yet exist) *to 1930* on the present-day level. Thus, when discussing the work of Gauss he (p. 353) introduced without explanation the recent notation for a function with an unspecified argument: $f(\cdot)$. He mentions the true value many times, for example in Chapters 5 and 6, and here is how he begins this latter chapter (p. 91): " … we have discussed … the estimation of the true value, the location parameter, in the … model".

I conclude that the term itself, and the notion of true value are still applied to a certain extent even in mathematical statistics.

I defined the theory of errors in § 1. According to its "official" mathematical definition (Bolshev 1984/1989), it is a branch of mathematical statistics beyond whose confines is the *processing of observations* (Bolshev (1982/1991) which studies systematic errors. I do not agree. First, the theory of errors is just unable to divorce itself from such studies. Second, systematic errors are a feature of the structure of statistical data, and their absence or presence should therefore be verified by *exploratory data analysis*, an important chapter of theoretical, even if not mathematical statistics (Sheynin 1999/2006). Third and last, Bolshev's description of the processing of observations is somewhat indefinite and does not mention data analysis at all.

5. The Intermediate Stage

It is usual to credit Galton with breaking away from true value (and the theory of errors in general). In 1908 he (Eisenhart 1978, p. 382) wrote:

The primary objects of the Gaussian Law of Error were exactly opposed, in one sense, to those to which I applied them. They were to get rid of, or to provide a just allowance for errors. But these errors or deviations were the very things I wanted to preserve and to know about.

Deviations together with their respective probabilities, i. e., their densities.

But the intermediate stage between the theory of errors and mathematical statistics began much earlier with Condorcet (1805/1986, p. 604) who introduced

Théorie des valeurs moyennes … un préliminaire de la mathématique sociale … dans toutes les sciences physico-mathématiques, il est également utile d'avoir des valeurs moyennes des observations ou du résultat d'expériences.

[*The theory of mean values … a preliminary to social mathematics … in every physical and mathematical science is equally useful to have mean values of observations or of the results of experiments.*]

On the same page he definitely separated this proposed theory from the "théorie du calcul des probabilités". Nevertheless, he had not elaborated, had not offered a formula of the theory of means. On pp. 555 – 559 Condorcet reasoned on the connection between the arithmetic mean (only in the case of a finite number of observations) and the *vraie valeur inconnue* [*true unknown value*], noted, on p. 555, that *On peut distinguer deux espèces de valeurs moyennes* [*It is possible to distinguish two kinds of mean values*], but still had not explained himself clearly enough, cf. Quetelet's statement below.

Anyway, the emerged theory of means (hardly separated from probability!) was more general than the theory of errors in that it also dealt with mean states; for example, with the mean stature of draftees (Quetelet, his celebrated study). It was Lambert (Sheynin 1971, pp. 254 – 255), who, in 1765, introduced the term theory of errors (*Theorie der Fehler*), but it had not taken root until the mid-19th century; Gauss and Laplace, for example, had not applied it.

I repeat now my quotation (Sheynin 1986, p. 311) from Quetelet (1846, p. 65):

En prenant une moyenne, on peut avoir en vue deux choses bien différentes: on peut chercher à déterminer un nombre qui existe véritablement; ou bien à calculer un nombre qui donne l'idée la plus rapprochée possible de *plusieurs quantités différentes, exprimant des choses homogènes, mais variables de grandeur.*

[*When taking a mean, it is possible to bear in mind two quite different things. We can attempt to determine a number that really exists; or, we can indeed calculate a number that provides the nearest possible idea of many differing quantities expressing uniform objects varying however in magnitude.*]

In the same article I have also cited or mentioned several other pertinent sources from 1830 to 1874.

The study of mean values or states rather than laws of distribution (Galton, see above) had been a necessary stage in the development of natural sciences. Humboldt (Sheynin 1984b, p. 68, n 36), in 1850, mentioned *die einzig entscheidende Methode, die der Mittelzahlen* [*the only decisive method, that of the mean numbers*], and Buys Ballot (Ibidem, p. 55), also in 1850, stated that the study of the mean state of the atmosphere had begun with Humboldt and constituted the first period of the new history of meteorology.

Finally, I refer to Hilbert (1901/1935, § 6) who was perhaps one of the last scholars to mention the *Methode der mittleren Werte* [*method of mean values*]. That the theory of means does not exist anymore is understandable: being an intermediate entity, it became divided between statistics (to which already Quetelet, see the quotation above, had attributed it) and the theory of errors.

Without turning to meteorology anymore, I am giving word to the astronomer, who, in that branch of natural sciences, originated the change from means to frequencies (Kapteyn 1906, p. 397):

Just as the physicist … cannot hope to follow any one molecule in its motion, but is still enabled to draw important conclusions as soon as he has determined the mean of the velocities of all the molecules and the frequency of determined deviations of the individual velocities from this mean, so … our main hope will be in the determination of means and of frequencies.

6. A Conclusion

It is generally known that the development of mathematics has always been connected with its moving ever away from Nature (for example, from natural numbers to real numbers in general to imaginaries) and that the more abstract it was becoming, the more benefit accrued to natural sciences. In particular, the general transition from the true value to estimating parameters of functions in mathematical statistics was also very useful.

I stress however that the science of measuring real objects and treating the collected data does not at all abandon the true value. That Mises (§3) also saw fit to define (not formally) the true value and to link it (indirectly) to his theory certainly lends it some additional support. Of course, in spite of his own opinion, his frequentist theory of probability belongs to natural sciences (Khinchin 1961/2004), but, after all, the theory of errors does not belong entirely to mathematics either. The statements of Chatterjee (§4) and possibly other likeminded statisticians ought to be modified accordingly and *the* *theory of errors must remain to be seen as a worthy scientific discipline*. Together with its *true value*, alive and kicking, it continues to service experimental sciences at large.

To a certain extent, the ideas and methods of mathematical statistics ought to be applied there. Primarily I bear in mind the estimation of precision, which, after all, is not inseparably connected with true values. I ought to mention correlation theory and analysis of variance as well, but these subjects are beyond my scope now. Nevertheless, it is opportune to note that Kapteyn (1912), who was dissatisfied with that theory as having been developed then, introduced his own *astronomical version* of correlation. Without knowing it, he thus quantified Gauss' pertinent ideas and, although his contribution had never been cited (perhaps because of this very fact), geodesists have always kept to his (to Gauss') concepts of dependence and correlation, see Sheynin (1984a, pp. 187 – 189). This does not, however, mean that the "statistical" correlation has no place in the theory of errors.

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References

Al-Biruni (1967), *The Determination of the Coordinates of Positions for the Correction of Distances between Cities.* Beirut.

Aleksandrov, P. S., Editor (1962), *English – Russian Dictionary of Mathematical Terms*. Moscow.

Bernstein, S. N. (1941, in Russian), On the Fisherian "confidence" probabilities. In author's book (1964, pp. 386 – 393).

--- (1964, in Russian), *Sobranie Sochineniy* (Coll. Works), vol. 4. N. p.

Bolshev, L. N. (1964, in Russian), Commentary on S. N. Bernstein paper on the Fisherian "confidence" probabilities. In Bernstein (1964, pp. 566 – 569).

--- (1982, in Russian), Processing of observations. *Enc. of Mathematics*, vol. 7. Dordrecht, 1991, pp. 314 – 315.

--- (1984, in Russian), Errors, theory of. Ibidem, vol. 3, 1989, pp. 416 – 417.

Chatterjee, S. K. (2003), *Statistical Thought: a Perspective and History*. Oxford.

Condorcet, M. J. A. de Caritat de (1805), Elémens du calcul des probabilités, et son application aux jeux de hasard, a la loterie, et aux jugemens des hommes. In author's book *Sur les élections et autres textes*. No place, 1986, pp. 483 – 623.

Cotes, R. (1722), Aestimatio errorum in mixta mathesi per variationes Partium trianguli plani et sphaerici. In author's *Opera misc*. London, 1768, pp. 10 – 58.

Eisenhart, C. (1963), Realistic evaluation of the precision and accuracy of instrument calibration systems. In Ku, H. H., Editor (1969), *Precision Measurement and Calibrations.* Washington, pp. 21 – 47.

--- (1978), Gauss. In Kruskal, W., Tanur, J. M:, Editors, *International Encylopedia of statistics*, vols 1 – 2. New York, single paging, pp. 378 – 386.

Fisher, R. A. (1922), On the mathematical foundations of theoretical statistics.*Phil. Trans. Roy. Soc*., vol. A222, pp. 309 – 368.

Fourier, J. B. J. (1826), Sur les résultats moyens déduits d'un grand nombre d'observations. *Œuvres*, t. 2. Paris, 1890, pp. 525 – 545.

Gauss, C. F. (1809, in Latin), Theoria motus … German translation in author's *Abhandlungen zur Methode der kleinsten Quadrate* (1887). Editors, A. Börsch & P. Simon. Vaduz (Lichtenstein), 1998, pp. 92 – 117.

--- (1816), Bestimmung der Genauigkeit der Beobachtungen. Ibidem, pp. 129 – 138.

Hald, A. (1998), *History of Probability and Statistics and Their Applications from 1750 to 1930*. New York.

Hilbert, D. (1901), Mathematische Probleme. *Ges. Abh*., Bd. 3. Berlin, 1970,

pp. 290 – 329.
Kapteyn, J. C. (1906), Statistical methods in stellar astronomy. [Reports] *Intern. Congr. Arts & Sci. St. Louis – Boston 1904*. N. p., vol. 4, pp. 396 – 425.
--- (1912), Definition of the correlation-coefficient. *Monthly Notices Roy. Astron. Soc*., vol. 72, pp. 518 – 525.
Khinchin, A. Ya. (1961, in Russian), R. Mises' frequentist theory and the modern concepts of the theory of probability. *Science in Context*, vol. 17, 2004, pp. 391 – 422.
Kolmogorov, A. N. (1946, in Russian), Justification of the method of least squares. *Sel. Works*, vol. 2. Dordrecht, 1992, pp. 285 – 302.
Lambert, J. H. (1760, in Latin), *Photometria*. Augsburg. Its German translation in the *Ostwald Klassiker* series does not include the pertinent section. The German quote in my text is from Schneider (1988, p. 228).
--- (1765), Theorie der Zuverlässigkeit der Beobachtungen und Versuche. In author's *Beyträge zum Gebrauche der Mathematik und deren Anwendung*, Tl. 1. Berlin, pp. 424 – 488.
Laplace, P. S. (1795), Leçons de mathématiques. *Œuvres Complètes*, t. 14. Paris, 1912, pp. 10 – 177.
--- (1810a), Sur les approximations des formules qui sont fonctions de très grands nombres et sur leur application aux probabilités. Ibidem, t. 12. Paris, 1898, pp. 301 – 345.
--- (1810b), Notice sur les probabilités. In Gillispie, C. C. (1979), Mémoires inédites ou anonymes de Laplace. *Revue d'histoire des sciences*, t. 32, pp. 223 – 279.
--- (1814), *Essai philosophique sur les probabilités*. *Œuvres Complètes*, t. 7. No. 1. Paris, 1886. Separate paging. English translation (1995): *Philosophical essay on Probabilities*. New York.
Markov, A. A. (1899, in Russian), The law of large numbers and the method of least squares. *Izbrannye Trudy* (Sel. Works). N. p., 1951, pp. 231 – 251.
--- (1924, in Russian), *Ischislenie Veroiatnostei* (Calculus of Probability). Moscow. First edition: 1900. German translation of the edition of 1908: Leipzig – Berlin, 1912.
Mises, R. von (1919), Fundamentalsätze der Wahrscheinlichkeitsrechnung. *Math. Z.*, Bd. 4, pp. 1 – 97. Partly reprinted in author's *Selected Papers*, vol. 2. Providence, Rhode Island, 1964, pp. 35 – 56.
Picard, J. (1693), Observations astronomiques faites en divers endroits du royaume en 1672, 1673, 1674. *Mém. Acad. Roy. Sci.* 1666 – 1699, t. 7, 1729, pp. 329 – 347.
Poincaré, H. (1912), *Calcul des probabilités.* Paris. First edition, 1896.
Poisson, S. D. (1811), Review of a memoir of Laplace. *Nouv. bull. sciences, Soc. philomatique Paris*, t. 2, No. 35, pp. 132 – 136.
--- (1824), Sur la probabilité des résultats moyens des observations. *Connaissance des tem[p]s* pour 1827, pp. 273 – 302.
--- (1829), Second part of same. Ibidem pour 1832, pp. 3 – 22 of second paging.
Quetelet, A. (1846), *Lettres sur la théorie des probabilités*. Bruxelles.
Schneider, I., Herausgeber (1988), *Die Entwicklung der Wahrscheinlichkeitstheorie von den Anfängen bis 1933*. Darmstadt.
Sheynin, O. (1971), Lambert's work on probability. *Arch. Hist. Ex. Sci*., vol. 7, pp. 244 – 256.
--- (1984a), On the history of the statistical method in astronomy. Ibidem, vol. 29, pp. 151 – 199.
--- (1984b), On the history of the statistical method in meteorology. Ibidem, vol. 31, pp. 53 – 95.
--- (1986), Quetelet as a statistician. Ibidem, vol. 36, pp. 281 – 325.
--- (1999), Statistics, definitions of. In Kotz, S., Editor (2006), *Enc. of Statistical Sciences*, 2nd ed., vol. 12. Hoboken, New Jersey, pp. 8128 – 8135.
Smirnov, N. V., Dunin-Barkovski, I. W. (1959, in Russian), *Mathematische Statistik in der Technik*. Berlin, 1973.
Whittaker, E. T., Robinson, G. (1924), *The Calculus of Observations*.

London – Glasgow, 1958.