On the History of the Statistical Method in Astronomy

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Contents

1. Introduction

Enhanced precision in astronomical observations and their reasonable mathematical treatment underlie observational astronomy. Beginning with BRADLEY, the need for regular series of observations had become generally accepted[1], and after the work of GAUSS and BESSEL it became common to analyse astronomical instruments and to allow for instrumental errors. It is no surprise that precisely the requirements of astronomy (and geodesy) gave rise to the classical theory of errors and to the method of least squares.

In an earlier work I have considered the mathematical treatment of astronomical observations up to the middle of the 18$^{\text{th}}$ century [145][2]. Together with my other contributions [142–144; 148; 149] this paper provides the ground-work for a history of error theory. My aim here is to describe the penetration of the statistical method itself into astronomy.

LAPLACE's theory of probability[3] grew largely from astronomy. In discussing the application of the theory to natural philosophy in his *Essai philosophique* [105], LAPLACE enumerated a number of astronomical facts whose existence he first determined by a stochastic examination of the data and then ascertained deductively.[4] Regrettably, it seems that LAPLACE in his published works never produced the preliminary stage of the investigation. Only two stochastic astronomical studies by LAPLACE are known, *viz*, his calculation of the mean inclination

[1] At the same time the volume of work in compiling catalogs, determining astronomical constants and so forth sharply increased, thus enhancing the role of the preliminary treatment of data and their adjustment.

[2] NOEL SWERDLOW (*Math. Rev.*, vol. **56**, No. 8262) severely criticized this paper. I do not accept this criticism.

[3] LAPLACE also applied probability theory to population statistics and to jurisprudence.

[4] I have used one of his relevant statements as an epigraph [147], thus emphasizing the trend of LAPLACE's theory of probability towards natural science and, specifically, towards astronomy.

of planetary and cometary orbits (see my §§ 5.1 and 5.10) and his reasoning on the absence of comets with hyperbolic orbits [155, § 925].

A statistical approach to the system of the world is evident even in the works of KEPLER [146, § 8.1] and NEWTON [139, p. 225]. Referring to *the wonderful uniformity in the planetary system* and regarding the planets as elements of a single statistical population, NEWTON asserted that the system *must be allowed the effect of choice* [not randomness].

HERSCHEL was the first to use the statistical method in stellar astronomy. He attempted to determine the extent of the stellar universe, constructed a model of the spatial arrangement of stars and studied the motion of the sun. *De facto*, if not *de jure*, HERSCHEL allowed randomness to play an important role in his model. Subsequent astronomers continued to study the distribution of stars and the sun's motion.

I describe here the application of the statistical method to investigations of the solar system (planetary distances, minor planets, sunspots) and in stellar astronomy. Roughly speaking, my investigation terminates in the middle of the 19th century when, on the one hand, star parallaxes were still largely unknown and ray velocities of stars had not yet been measured, but, on the other hand, stellar astronomy had already opened up the new field of determining the proper motions of large groups of stars. I outline also the essence of the statistical method as used by KAPTEYN and I sketch the prehistory of the penetration of mathematical statistics into astronomy. Some special features of my paper are a first attempt to describe STRUVE's investigation of the completeness of star catalogs (see my § 7.1), a description of PEARSON's efforts to introduce the theory of correlation into astronomy (§ 9.2.2); and of KAPTEYN's 'astronomical' definition of the coefficient of correlation (§ 9.2.1).

Since authors usually begin the history of stellar statistics with KAPTEYN and SEELIGER[5], there is no general literature on my subject.

As regards the general history of the statistical method [151, § 1.1] my study pertains to its second stage, which is distinguished by the availability of statistical data and the nonexistence of quantitative tests to check the inferences made.

The main body of my paper clearly reveals the insufficient level of probability theory in the second half of the 19th century.

2. The Titius-Bode Law

Beginning with KEPLER, modern astronomers attempted to discover numerical regularities in the structure of the solar system. The best known regularity, called the TITIUS-BODE law, characterizes planetary distances. Many authors have described its history; I shall confine myself to NIETO's contribution [116].

The first to formulate the law was C. WOLFF (in 1723) and, in 1766, TITIUS followed WOLFF. However, the law became generally known as a result of the works of BODE that were published in the late 18th century.

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[5] PAUL's unavailable thesis [118] seems to fall in this category.

According to the TITIUS-BODE law, planetary distances, or, rather, quantities proportional to them, can be represented by the formulas

$$a_n = \begin{cases} 4 & (n = 1), \\ 4 + 3 \cdot 2^{n-2} & (n = 2, 3, 4, 6, 7, 8) \end{cases}$$

(the value $n = 5$ roughly corresponds to the minor planets).

Table 2(1). Planetary distances measured in arbitrary units

	Planets	Planetary distances according to actual data [59, p. 635; 60, p. 362]	the TITIUS-BODE law
1	2	3	4
1	Mercury	4.06	4
2	Venus	7.58	7
3	Earth	10.4	10
4	Mars	16.0	16
6	Jupiter	54.5	52
7	Saturn	100	100
8	Uranus	201	196

Note: Number 5 corresponds to the minor planets.

Table 2(1) gives the actual planetary distances and the distances derived from formulas (1). Of course, BODE had no way of estimating the accuracy of the law, but at any rate the fit should have seemed quite admissible to any astronomer.[6]

Opinions about the physical meaning of the TITIUS-BODE law differ greatly to this day [116, chap. 1] but the mere fact that it is the subject of a special book testifies to the lasting importance of the law. I shall restrict myself to GAUSS's view, which the source just referred to does not mention. Incidentally, this view is characteristic of GAUSS's attitude towards empirical formulas in general; see also my § 7.6.

In a letter to VON ZACH, GAUSS wrote [84]:

Sonderbar ist es, dass man vom ... Titius angegebene sogenannte Gesetz als ein Argument gegen die beyden Planeten [Ceres und Pallas] *gebrauchen wollte. Dieses Verhältniss trifft bey den übrigen Planeten gegen die Natur aller Wahrheiten, die den Namen Gesetze verdienen, nur ganz beyläufig, und, was man noch nicht einmahl bemerkt zu haben scheint, beym Mercur gar nicht zu. Es scheint mir sehr ein-*

[6] BODE [61] also investigated the orbits of planets and comets. Thus, studying comets, he compared the distribution of each parameter of their orbits, one by one, with the uniform distribution and made qualitative conclusions. Quantitative investigations of this kind became possible only in the context of mathematical statistics by means of the KOLMOGOROV test. See also my §§ 3.1 and 5.4–5.8.

leuchtend, dass die Reihe

$$4,\quad 4+3,\quad 4+6,\quad 4+12,\quad 4+24,\quad 4+48,\quad 4+96,\quad 4+192$$

womit die Abstände übereinstimmen sollten, gar nicht einmahl eine continuirliche Reihe ist.

Referring to KEPLER, GAUSS nevertheless continued:

Es ist gar nicht zu tadeln, wenn man dergleichen ungefähre Übereinstimmungen in der Natur aufsucht.

In his approving opening remark, VON ZACH called GAUSS a *scharfsinnige Messkünstler.*

3. Minor Planets

3.1. Newcomb

NEWCOMB devoted a few papers to the study of minor planets. Here is a passage from his first contribution [22]:

Another method [of testing a theory relating to the common origin of the minor planets] *is furnished by the method* [!] *of probabilities, and might, perhaps, if the asteroids were sufficiently numerous, approach very nearly to certainty in its results. It is founded on the supposition, that the hypothesis examined will imply a high probability of some general relationship among the orbits of the asteroids* ... .

Referring to a previous note, NEWCOMB [26] soon derived the distribution laws of the perihelia and nodes of the orbits of the small planets.

In 1869 he [27] compared the theoretical parameters (calculated according to uniform distribution) and the actual parameters of the orbits of the minor planets, but of course he was unable to evaluate the results obtained. NEWCOMB studied the possibility that the empirical density of distribution might diverge from the parent uniform distribution. Only in 1900 did NEWCOMB [31] return to the same subject, this time studying the motions of the asteroids:

It was, I believe, first pointed out by Kirkwood, that if the mean motions of the minor planets are arranged in the order of magnitude, gaps will be found at the values which would have a simple relation of commensurability with the mean motion of Jupiter.

NEWCOMB selected m ($m = 354$) planets with mean motions μ ($600'' \leqq \mu \leqq 1{,}000''$) and divided them into n ($n = 40$) groups with $\mu = 600$–610, 610–620, ..., 990–$1{,}000''$. Assuming a law of distribution

$$\varphi(x) = \frac{m!}{x!\,(m-x)!}\left(\frac{1}{n}\right)^x\left(1-\frac{1}{n}\right)^{m-x}$$

(which, true, he did not write down), NEWCOMB asserted that $n\varphi(x)$ is the probable number of groups having x planets each. Actually, it is the mean number of groups.[7] In a qualitative study of the differences between the observed and probable [mean] numbers of groups with x planets each, NEWCOMB agreed to a certain extent with KIRKWOOD (?) and he concluded that

these inequalities of distribution could [not] *have arisen in a group of such* [asteroids] *once uniformly distributed.*

See also my § 5.8.

3.2. Poincaré

POINCARÉ estimated the total number of minor planets (N) [130, pp. 163–168]. Supposing M planets out of N are known, let the number of planets observed during a certain year be n, of which m planets were known before. Denote the probability of the existence of planet i ($i = 1, 2, \ldots, N$) by ω_i. Then POINCARÉ asserted that the probable value of the unknown N would be

$$\omega_1 + 2\omega_2 + \ldots + N\omega_N.$$

Actually, this sum is the mean value of N, *i.e.* EN; see also my § 3.1.

If p_i is the probability of observing n planets, provided their total number is i, the posterior probability of the existence of N planets is

$$\frac{\omega_N p_N}{\sum_{i=n}^{\infty} \omega_i p_i}.$$

Introducing the probability (p) of the observation of a planet if it does exist, and writing $q = 1 - p$, POINCARÉ got

$$p_i = \frac{i!}{n!\,(i-n)!} p^n q^{i-n}.$$

Assuming $\omega_i = \text{Const}$, he then easily arrived at

$$EN = \frac{n+q}{p}.$$

Giving only the right-hand side of this formula, POINCARÉ wrongly called it the probable value of N. Finally, he extended his derivation to the continuous case.

POINCARÉ did not say a single word about the possible deviation of N from its 'probable' [mean] value. On this point he departed from LAPLACE who, for example, not only estimated the population of France but calculated the probability of a certain error in his estimate [147, § 2.5.5]. See also my § 7.1.

I shall not study POINCARÉ's investigation [130, pp. 6 and 150–152; 131, pp. 227–233; 132, pp. 592–596] regarding the uniform distribution of the longitudes of minor planets.

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[7] Evidently, even at the beginning of this century the mean value of a random quantity was not properly distinguished from its probable value. See also ANGER [52, p. 22], KLEIBER (my § 5.8) and POINCARÉ (§ 3.2).

Thus, stochastic considerations regarding the major planets (see my §§ 5.1 and 5.10) were extended to include asteroids; there really seems to be a point in assuming a random character for the parameters of their orbits (§ 3.1) and even for the very existence of one or another minor planet (see above).

4. Sunspots

4.1. Periodicity

The first to suspect the periodicity in the number of sunspots was HORREBOW who, in 1776, entered a remark about it in his diary. In 1859 THIELE published this note [158, p. 654]. Reasonably considering his investigation to be preliminary, and drawing on data pertaining to 1650–1717, HERSCHEL [8] attempted to determine the relationship between sunspots and the price of wheat. It is more natural to compare the solar activity with its direct, *e.g.*, meteorological, consequences; and for this reason alone hardly anyone approved of either HERSCHEL'S or CHAMBERS' [67] findings. LITTROW [106, p. 851; 68, p. 156] made a cautious step in the right direction remarking that

sieht man sie [the sunspots] *gewöhnlich in grosser Anzahl und gleichsam periodisch kommen, während wieder zu andern Zeiten die Sonne lange von ihnen frei bleibt.*

In 1838 SCHWABE [136] published his observations of sunspots during 1826–1837.[8] Neither then nor in his subsequent yearly reports published in the same journal did he say anything about periodicity in the number of sunspots. In 1843 he [137, p. 283] even stated that

noch viele genaue Beobachtungen angestellt werden müssen, ehe man zu einem einigermassen sichern Schluss auf ihr [the sunspots'] *Wesen* [including their possible periodicity?] *kommen kann* ... .[9]

Only in 1844 did SCHWABE [138, pp. 233–234] publish his findings:

Schon aus meinen früheren Beobachtungen [!] ... *scheint sich eine gewisse Periodicität der Sonnenflecken zu ergeben und diese Wahrscheinlichkeit gewinnt durch die diesjährigen noch an Sicherheit.*

Summarizing all his observations between 1826 and 1843, he gave the period as being approximately equal to ten years. However, he prudently qualified his conclusion (p. 235):

Die Zukunft muss lehren, ob diese Periode einige Beständigkeit zeigt ... .

[8] His astronomical and geophysical observations had appeared in print since 1830. In all, he published 109 contributions including papers on physics and geology (*Cat. Roy. Soc. Lond.*).

[9] Note SCHWABE'S general attitude towards scientific research (*ibidem*):
Ich war bemüht hiebei so unpartheiisch wie möglich zu verfahren und keiner Hypothese einen Einfluss zu gestatten.

SCHWABE'S yearly data included the number of days of observation, the number of days when no sunspots were seen at all, and the number of groups of sunspots; see Table 3.1(1). Perhaps the absence of a formal mathematical analysis in SCHWABE'S note [138] was quite proper: it was hardly worth deriving rigorously a ten-year period from data covering 18 years.

Table 3.1(1). The number of groups of sunspots [138, p. 234]

Year	Number of groups of sunspots	Year	Number of groups of sunspots
1826	118	1835	173
1827	161	1836	272
1828	225	1837	333
1829	199	1838	282
1830	190	1839	162
1831	149	1840	152
1832	84	1841	102
1833	33	1842	68
1834	51	1843	34

SCHWABE'S work passed unnoticed. At any rate, even in 1847, when J. HERSCHEL [95, p. 435] advocated the study of sunspots, he did not mention SCHWABE at all. This is what he wrote:

The great importance of a systematic and continuous series of observations of the solar spots cannot be too strongly insisted on. One observer ... is not enough. Many are necessary ... .

As CLERKE [68, p. 156] testified, *the reality and importance of the discovery were simultaneously recognized* only after HUMBOLDT [99, p. 401] described SCHWABE'S work.[10]

The best known student of sunspots after SCHWABE was R. WOLF. Not later than 1856 he introduced the "relative number of sunspots" [156, p. 12][11]

$$R = k(f + 10g).$$

Here f is the total number of all spots and g is the number of their groups. I do not think that this function possesses any special statistical significance.

In 1859 WOLF [157] collected all observations of sunspots beginning with the middle of the 18th century, determined the epochs of their extreme numbers and derived the periodicity of their occurrence (11.1 years). Returning to this problem

[10] Note however that GALILEO had used observations of sunspots to determine the period of rotation of the sun on its axis. SPOERER [152] and, quite probably, other astronomers of the 19th century continued to investigate this phenomenon.

[11] Observations in this source are collected in chronological order. Those pertaining to 1856 are mentioned on p. 27. BRAY & LOUGHHEAD [66, § 1.4] maintain that WOLF introduced the function R in 1848, but they do not supply any supporting reference.

once more, WOLF [77] put on record observations covering 120 years and calculated the period (T), testing, in the course of doing so, nineteen (*sic*!) hypotheses ($T = 9$ years 6 months; 9 years 8 months; 9 years 10 months; ...; 12 years 6 months). He considered the deviations of the mean yearly data from the general mean number of sunspots and applied as a test the range of the deviations or, alternatively, the root of the sum of their squares divided by T. WOLF concluded that there were two periods: $T_1 = 10$ years, and $T_2 = 11.3$ years, their least common multiple being, as he himself noticed, 170 years.

Contemporary authors [66, § 6.3.2] assume that $T \approx 11$ years, but a rigorous periodicity is not thought to exist (*cf.* SCHWABE's prudent remark above!). Even so, the attempts of previous astronomers to determine T have not been fruitless.

4.2. The Influence on Terrestrial Magnetism and Climate

As early as 1850 HUMBOLDT [99, p. 388] described the first (unsuccessful) attempts to correlate the air temperature with extreme values of the number of sunspots. Weather fluctuations, as he noted, obscure the general picture.[12] A contemporary author [111] pointed out that investigations of that kind were really difficult.

The influence of sunspots (or, generally, of solar activity) on earth magnetism is now undeniable,[13] but at one time it did cause doubts. FAYE's articles are significant in this connection. In 1837 he [75] mentioned the periodicities in the origin of sunspots and in magnetic declinations, obviously regarding their coincidence as a well established fact.[14] In 1878 FAYE [76] stated that in actual fact the periods differed by 0.66 years.[15] Moreover, he stated that

> ... *2° les deux phénomènes sont sans rapport entre eux; 3° un ensemble de circonstances favorables, qui se reproduit tous les* 176 *ans* [!], *a fait croire à la connexion de ces deux phénomènes* ... .

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[12] These are his words:

> *Die Endresultate geben aber für die erkältende Kraft, die hier den Sonnenflecken zugeschrieben wird, kaum* 0°, 42 *Cent.*: *welche* ... *den Fehlern der Beobachtung und der Windrichtungen eben so gut als den Sonnenflecken zuzuschrieben sein können.*

[13] SABINE [134, p. 121; 68, p. 158] seems to have been the first to direct attention to the

> *most striking coincidence, that the period, and the epochs of minima and maxima* ... [of the number of sunspots] *are absolutely identical with those which have been here assigned to the* [variations of magnetic declination].

[14] *Ces concordances frappantes,* he added, ... *ne justifient-elles pas pleinement le titre de Météorologie cosmique que j'ai donné à cette Note, pour rendre hommage, à la fois, aux travaux de M. Wolf et à la mémoire de Donati* ... .

DONATI is mainly remembered because of a comet named after him. I do not know which of his memoirs FAYE had in mind but at least DONATI published a number of papers on meteorology.

[15] This being a result of his calculations which consisted in a separate treatment, by the method of least squares, of the data pertaining to the two phenomena.

Finally, in 1882 FAYE [77] did not mention the connection between sunspots and terrestrial magnetism at all.

WOLF [159] also studied the same connection. He derived an empirical relation between the changes in the relative number of sunspots (see my § 4.1) and magnetic declination, and he stated that his formula was in *remarquable* agreement with the data.

In 1872 MELDRUM [112] took notice of a possible correlation between cyclones and sunspots. Without asserting that any such connection does exist, he nevertheless wrote:

it is difficult to avoid the conclusion that ... meteorology, magnetism, and solar physics are closely connected.

In a later work [113] MELDRUM extended his investigation and indicated (p. 218) that

not only the number of cyclones, but their duration, extent, and energy were also much greater in the [years of maximum] *than in the* [years of minimum sunspot frequency], *and ... there is a strong probability that this cyclonic fluctuation has been coincident with a similar fluctuation of the rainfall over the globe generally.*

Almost at the same time LOCKYER [107], an eminent amateur astronomer, voiced a similar opinion regarding cyclones, while BLANFORD [58] pointed out a connection between atmospheric pressure and sunspots.

The essential influence of solar activity on at least some elements of terrestrial magnetism and on meteorological phenomena was thus ascertained. Regrettably, the authors mentioned above merely gave a qualitative comparison of the data concerning sunspots with those pertaining, for instance, to cyclones. Neither uttered a single word on the need for a new statistical theory, *i.e.*, the theory of correlation.

5. Michell's Problem

Even MAIMONIDES [133, p. 123], stated that the *positions, measures and numbers of the stars* are by no means fortuitous.

5.1. Michell

MICHELL was the first modern astronomer to attempt to calculate the probability of two stars being close to each other. Supposing that n stars are scattered *by mere chance* over the sky, the probability that two stars be situated not farther than 1° apart would be [114, p. 429] $p = 1:13{,}131$, while the probability that no such pair be present would be $[(1-p^n)^n]$ (see below). MICHELL similarly concluded (p. 428) that *there is the highest probability, that* ... [the stars] *are collected together in great numbers in some parts of space, while in others there are either few or none.*

MICHELL also used qualitative stochastic reasoning. Thus [following HALLEY], he supposed (*ibidem*) that the brightest stars are the nearest ones and, without elaborating, assumed (p. 431) that the twinkling of the stars can be explained by random oscillations of a relatively small number of particles of light. Similarly, MICHELL began his second paper [115] by stating that visual binaries are mostly real double stars. HARDIN [93] described MICHELL's scientific work in general (not only in astronomy), put on record his influence on HERSCHEL, and outlined the subsequent history of his problem.

Consider now MICHELL's calculations. Suppose the distance between two stars is 1°; then the surface area of the corresponding spherical segment is

$$s = \pi R^2 \frac{1}{57{,}296^2}.$$

The surface area of the whole (celestial) sphere is $S = 4\pi R^2$, and so[16]

$$p = \frac{s}{S} = \frac{1}{4 \cdot 57{,}296^2} = \frac{1}{13{,}131}.$$

MICHELL's subsequent computations are wrong, a fact noticed by many commentators (see for example my § 5.4). Following FISHER [81, p. 38], who analyzed one of MICHELL's examples, using POISSON's distribution I shall calculate the probability sought. See also my § 5.5.

In the earlier notation the expected number of stars situated not farther than 1° apart from each other is $a = pn$ and the probability that there is at least one binary is

$$P = 1 - \frac{a^0}{0!} e^{-a} - \frac{a}{1!} e^{-a} = 1 - e^{-a}(1 + a).$$

For $n = 5{,}000$, it turns out that $a = 0.3808$ and $P = 0.056$. This probability is rather small (though not negligible), but HERSCHEL was to discover a few hundred visual binaries closer to each other than in MICHELL's example. MICHELL could not foresee HERSCHEL's findings, but at any rate MICHELL was in agreement with his final conclusion (see above) concerning the small probability of star clusters.

Did a given phenomenon occur by design or was it produced by chance? At least from ARISTOTLE onwards, scholars asked this question again and again [146, p. 113]. In modern times, philosophers (*ibid.*, p. 134), population statisticians

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[16] It is well worth noting that MICHELL used geometric probabilities. Though they occurred in applications of probability theory even earlier, they came to be generally accepted only in 1777 [147, p. 152].

[143, § 5], and astronomers (D. BERNOULLI [142, pp. 106–107]) have encountered the same problem. Here is BERNOULLI'S reasoning:

Let the inclination of the orbit of planet i be equal to a_i, $0° < a_i < 90°$ and $i = 1, 2, \ldots, 5$. If the values of a_i are independent and uniformly distributed, the probability of $a_i < A$ $(A < 90°)$ for each of the five planets would be equal to $A/90$; with a small A, the probability of the whole series of inequalities, $(A/90)^5$, becomes insignificantly small. Consequently, the quantities a_i could not be independent.

After MICHELL, LAPLACE argued in a similar fashion regarding the planets and their satellites (see my § 5.10). Still, MICHELL'S problem seemed more interesting; no wonder it attracted more attention than the problems due to D. BERNOULLI and LAPLACE. Note that, following an old tradition [146, § 9.1], MICHELL identified randomness with the uniform distribution. I shall show (see subsequent subsections of § 5) that for a long time this point of view held its ground in astronomy.

5.2. Herschel

HERSCHEL [9, p. 203] attempted to solve a similar problem.

The surface of the globe contains 34036131547 [$\approx 3.404 \cdot 10^{10}$] *circular spaces, each of 5″ in diameter*, he noted, ... *each of the 686 stars* [of the seventh magnitude] *will have 49615357* [$\approx 4.961 \cdot 10^7$] *of these circles in which it* [?] *might be placed*; *but, of all that number, a single one would only be the proper situation in which it could make up a* [particular] *double star with one of the 450 given stars* [between the sixth and fifth magnitudes].

Therefore, HERSCHEL maintained, the probability of a 'random' existence of this star whose components possess the magnitudes indicated above, is less than $1/(75.5 \cdot 10^6)$. And, in general (p. 204),

casual situations will not account for the multiplied phenomena of double stars ... their existence must be owing to the influence of some general law of nature ... .

His conclusion is sound, but his calculations are wrong. Indeed, first, the number of surfaces with a diameter of 5″ that make up the celestial sphere equals

$$\frac{4\pi R^2}{\pi R^2} \cdot 57.296^2 \left(\frac{60 \cdot 60}{2.5}\right)^2 = 13{,}131 \left(\frac{60 \cdot 60}{2.5}\right)^2 = 2.7228 \cdot 10^{10} = \frac{3.4036 \cdot 10^{10}}{1.25}.$$

I cannot explain the absence of the coefficient $1/1.25$ (or even of an approximately equal factor $\sqrt{2/\pi} = 1/1.2533$). Second, the fraction $1/(75.5 \cdot 10^6)$ is the quotient of 450 divided by $3.404 \cdot 10^{10}$; thus HERSCHEL did not allow for the number of stars of seventh magnitude.

5.3. *Forbes* (*1849*)

FORBES [82] called in question MICHELL'S conclusions. He noted that

(1) *An equable spacing of stars over the sky would seem to me to be far more inconsistent with a total absence of Law or Principle, than the existence of spaces of comparative condensation ... as well as of regions of great paucity of stars.*

(2) He regarded *with doubt and hesitation an attempt to assign a numerical value to the antecedent probability of any given arrangement of grouping whatever.*

(3) *No bad representation of stars and their distribution may be made by sparking viscid white paint ... upon a dark ground ... ... such an artificial galaxy will present every variety of grouping, with double and treble points innumerable* ... .

5.4. *Forbes* (*1850*)

FORBES [83] repeated his previous arguments. He (p. 420) also asked himself what distributions might be called random. Of course, owing to the insufficiently high level attained by probability theory at that time, he was unable to produce an intelligible answer. Referring to a *mathematical friend*, FORBES also remarked (p. 425) that MICHELL'S calculations were wrong and that, in a throw of n dice with p faces each ($p > n$), the probability of all dice without exception showing faces different from one another is

$$P = \frac{p(p-1)(p-2)\dots(p-n+1)}{p^n}.$$

Suppose, FORBES continued, $n = 230$ is the number of stars of some magnitude, and $p = 4{,}254{,}603$ ($\approx 4.255 \cdot 10^6$) is the number of spherical surfaces each with a diameter of 3.′2 situated on the celestial sphere. Then the probability that there would not be even one double star having a distance of 3.′2 between its components is $1 - P$. (FORBES estimated P.) Actually (see my § 5.1) the number of surfaces is

$$13{,}131 \left(\frac{60}{3.2}\right)^2 = 4.617 \cdot 10^6 = 1.085p.$$

Finally, FORBES (pp. 411–415) studied the distribution of grains of rice falling from a sieve onto the squares of a chessboard. He maintained that this experiment confirmed his previous conclusion in this matter (see my § 5.3), and that therefore MICHELL had proved nothing at all.

FORBES need not have experimented; he could have referred to BUFFON'S celebrated trial of the Petersburg game [155, § 648].

5.5. *Newcomb*

NEWCOMB [21, 1860, pp. 137–138] calculated the probability that some surface with a diameter of 1° would contain s stars out of N scattered *at random* over the celestial sphere. Using POISSON's distribution, he arrived at

$$P = \left(\frac{Nl}{h}\right)^s \frac{e^{-Nl/h}}{s!}.$$

Here, h is the number of surfaces and l, as I see it, is the area of any one of them. At any rate, according to NEWCOMB,

$$\frac{l}{h} = \frac{1}{41{,}253} = \frac{1}{13{,}131\pi}.$$ [17]

The probability for at least one such surface to contain s stars is then naturally $41{,}253P$. NEWCOMB concluded that MICHELL wrongly solved a problem concerning the particular case of $s = 6$ and that, from a logician's point of view, his reasoning was imperfect. As to FORBES's experiment (see my § 5.3) it was (p. 138)

about as decisive as an attempt to disprove the Pythagorean proposition by measuring the squares described on a triangle without knowing whether it had or had not a right angle ... .

NEWCOMB (p. 139) also solved a methodical problem on the scattering of stars once more using the POISSON distribution;

we shall ... determine, he noted, *what law a random distribution may be expected to follow.*

He maintained that a random distribution is an arrangement of mutually independent elements (stars).

5.6. Newcomb (Continued)

Some of the arguments described above are also contained in another of NEWCOMB's contributions [23]. Here he formulated (p. 436) the same opinion about the relation between randomness and independence, pointing out (p. 438) that

A certain calculable amount of irregularity, or grouping, is to be expected as the result of a random distribution

and explained (p. 439) how to calculate this irregularity by means of the POISSON distribution. He also noted (p. 437) that FORBES (see my § 5.4) actually

objects [to] *the very mathematical definition of the word probability.*

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[17] The appearance of an additional multiplier, $1/\pi$ (see my § 5.1), is a mystery.

NEWCOMB devoted a considerable portion of his article to the logical aspect of the application of probability theory, but, just as in another of his writings (see my § 10), the exposition is not clear enough.[18]

5.7. *Proctor*

In connection with MICHELL's problem, PROCTOR [41, p. 99] wished to

determine what peculiarities of distribution might be expected to appear among a number of points spread over a plane surface perfectly at random.

Opening a table of logarithms at random, PROCTOR *brought down the point of a pencil upon the page* ... and recorded the digit he had hit.[19] Each four digits of the series thus obtained determined the coordinates of a point situated in a unit square. For example, digits 7, 3, 2, and 4 corresponded to point (0.73; 0.24). In all, PROCTOR (p. 100) got more than a thousand points *distributed perfectly at random* in the unit square. He therefore maintained that the regularities which he had discovered in a system of about a thousand stars could not be explained away by a freak of chance. His approach, with no reference to any theorem of probability theory, seems rather amateurish. See also my § 9.1.

5.8. *Kleiber*

KLEIBER [101] refuted some of FORBES's conclusions (see my § 5.4).

It is a common error, he remarked on p. 440, *to confound random scattering with uniform distribution. ...... the most probable distribution of points on a surface if scattered at random, is a uniform one, but this is very improbable.*

KLEIBER also analyzed the results of FORBES's experiment incidentally explaining the passage just quoted. Suppose n points (grains) are scattered over m congruent squares ($n > m$). Then the probability for exactly i points to be found inside some

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[18] In 1904 NEWCOMB [34, p. 13] indicated that
a chance distribution [of stars] *will always, in practice, differ more or less from a uniform one* ... .

He also used here the expression *a purely accidental distribution*. In this respect it is possible to recall BOOLE's remark [62, p. 256] which he formulated in connection with MICHELL's problem:
a "random distribution", 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.

[19] Did he exclude the first digits of the mantissas? This is unclear.

square will be

$$P = \frac{n!}{i!\,(n-i)!}\left(\frac{1}{m}\right)^i \left(1-\frac{1}{m}\right)^{n-i} = p_i.$$

Comparing the 'probable' (actually, the expected) numbers of such squares, mp_i, with FORBES's observed outcomes, KLEIBER suggested (naturally, without producing any numerical estimates) that the discrepancies were permissible. (See also my § 3.1.) Uttering an incomprehensible statement (p. 443) about obtaining experimentally *a more uniform rather than an accidental distribution*, KLEIBER described his own investigation into the distribution of the two last digits of a seven-place logarithmic table. He concluded by stating that

The [theory of probability] *gives a sufficient account of the possible irregularities of distribution such as those observed by ... Forbes in his experiments, or those presented by the stars in the sky.*

5.9. Struve

STRUVE's contribution to the study of double stars is widely known. He used simple stochastic arguments in estimating the number of multiple stars in visual systems [44, pp. 36–39]. Elsewhere [46, p. 212] he formulated his opinion about this kind of reasoning:

The physical connection in double stars we have so far deduced from two arguments, one of which was drawn from the slight probability of a purely optical connection, and the other from the proper motion common to the group. These arguments, although very strong, are nevertheless indirect.[20]

STRUVE [43, pp. xxxvii–xxxix] also determined the probability that two or three stars be situated near one another. In particular, he calculated the probability that the distances of a star from two other closely placed stars be not greater than specified.

5.10. The Distance Between Two Random Points on a Sphere

The problem concerning the distance between two such points, which is methodologically related to the one under discussion, can be traced back to LAPLACE [105, p. 261; 155, § 987; 143, pp. 286–287] and even to D. BERNOULLI (see my § 5.1). LAPLACE assumed that

pour chaque orbite, toutes les inclinaisons depuis zéro jusqu'à l'angle droit soient également possibles.

[20] Somewhat later, GAUSS [149, p. 56] expressed a similar idea on the stochastic proof of the diurnal rotation of the earth.

He introduced this assumption in order to ascertain whether or not the orbits of planets were situated randomly in relation to one another.

Consider two randomly placed great circles of a sphere. According to LAPLACE, the probability for the distance between their poles (ξ, $\xi < 90°$) to belong to the interval $[n^0, m^0]$ is

$$P\{n \leqq \xi \leqq m\} = \frac{m - n}{90}.$$

COURNOT [70, § 148] took for granted that the probability of the distance was proportional to its sine, *i.e.* that

$$P\{\alpha \leqq \xi \leqq \alpha + d\alpha\} = \varphi(\alpha) \sim \sin \alpha \, d\alpha.$$

He apparently thought P proportional to the length of the corresponding circumference of a small circle, the locus of poles situated at a distance α from the first of the two given poles.

For his part, NEWCOMB [24], who maintained that LAPLACE'S formula was wrong, thought that

$$P = \cos n - \cos m.$$

The right-hand side of his formula, as he himself indicated, was the ratio of the corresponding zone to the surface area of the semisphere. The formula means that the density of distribution of distance ξ is

$$\varphi(x) = \sin x$$

and it follows that $\cos \alpha$ is distributed uniformly over the interval $[0°, 90°]$. NEWCOMB himself said so many years later [34, p .13].

Without referring to anybody, BERTRAND [57, pp. 6–7] gave the solutions due to LAPLACE and COURNOT. He used this problem along with other ones to argue that the notion 'at random' should be introduced more precisely. For BERTRAND, LAPLACE'S solution could have been wrong only from the point of view of concrete applications of probability theory. BERTRAND (pp. 170–171) also expressed doubts about the possibility of solving MICHELL'S problem. What should we consider to be unlikely, he asked. Small distances between stars, the small area of a circle circumscribed about a star cluster *etc*, or, perhaps, the existence of an equilateral triangle formed by three stars? Besides, BERTRAND continued, randomness did not play an important part in the creation of the universe and it is therefore meaningless to estimate the probability of the existence of regularities in the distribution of stars.

Analyzing a few more problems, including his celebrated one on the length of a chord of a given circle, BERTRAND (pp. 4–7) reasonably asserted that

Les probabilités relatives à la distribution des étoiles, en les supposant semées au hasard sur la sphère céleste, sont impossibles à assigner si la question n'est pas précisée davantage.

BERTRAND's argument is evidently addressed more to astronomers than to mathematicians. BOREL [63, § 42], who did not agree with his remarks,[21] maintained that the difficulty in contrasting design with randomness lies in the separation of events into remarkable and usual ones. See my article [146, p. 125, note 119].

6. Herschel

See above subsection 5.2, also devoted to HERSCHEL. I also refer to him in § 4.1.

6.1. The Extent of the Sidereal System

HERSCHEL was the first to study the spatial distribution of stars. Being equitable elements of a statistical population, the stars in his investigation were either indistinguishable or different only in magnitude.[22] But of course HERSCHEL had no possibility of applying methods of mathematical statistics,[23] which did not yet exist.

In 1784 HERSCHEL [3, p. 162] first reported upon his study of the starry heavens:

> *It* [the method of gaging *the Heavens, or the Star-Gage*] *consists in repeatedly taking the number of stars in ten fields of view ... very near each other, and by adding their sums, and cutting off one decimal on the right, a mean of the contents of the heavens, in all the parts which are thus gaged, is obtained.*

Besides *gages*, HERSCHEL also mentioned *sweeps* (*ibid.*, p. 159) explaining this term elsewhere [5, p. 261]:

> *I drew the* [telescope] ... *so as to make it ... perform a kind of very slow oscillations of 12 or 14 degrees in breadth ... .*[24] *At the end of each oscillation I made a ... memorandum of the objects I chanced to see ... .* [Then] *the instrument was ... either lowered or raised about 8 or 10 minutes, and another oscillation was then performed ... ... thus I continued generally for about 10, 20, or 30 oscillations ... and the whole of it was then called a sweep ... .*

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[21] Elsewhere (§ 35) BOREL accused BERTRAND of unwarranted scepticism.

[22] Witness DE SITTER's opinion [71, p. 35]:

> *W. Herschel is the first to have the idea that the fixed stars form a system having a certain structure ... .*

[23] Only elements of the then nonexistent error theory occur in his works. HERSCHEL compiled three catalogs of binaries and discovered and ordered more than 2,500 new nebulas and star clusters. Undoubtedly this immense work bore a statistical mark.

[24] Subsequently, as HERSCHEL remarked (*ibidem*), he turned to the use of sweeps *with a vertical motion.*

Oscillations were evidently composed of separate gauges. Several questions arise: Is it possible that HERSCHEL distributed the gauges of a certain oscillation at random? That he fortuitously arranged his sweeps over the sky? Or, in short, can we assume that the method of *gaging the Heavens* was a particular version of sampling?

I think that the answer should be negative. First, HERSCHEL wanted to cover the whole sky [the visible part of it], or at least the entire Milky Way with dense gauges [3, p. 163; 4, p. 223]:

(1) *It would not be safe to enter into an application of these ... gages ... till they are sufficiently continued and carried all over the heavens.*[25]

(2) *I have now viewed and gaged* [the Milky Way] *in almost every direction ...* .

Second, it appears that only in one instance HERSCHEL employed an element of randomness in his investigation.[26] As to gauges proper, he indicated [4, p. 227] that

in gaging, a regular distribution of the fields, from the bottom of the sweep to the top, was always strictly attended to.

Of course, HERSCHEL could have combined a regular distribution of fields (gauges)[27] with a random selection of the first one of them, but then he would have certainly mentioned this fact.

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[25] At least once HERSCHEL [7, p. 51] even used overlapping sweeps.

[26] In one section of the Milky Way measuring 30 square degrees the brilliance of a *glorious multitude* of stars prevented HERSCHEL from counting them [3, p. 158]. He therefore counted the stars in six fields selected *promiscuously* and assumed the mean number of stars as an estimate for the whole section which, as he concluded, *could not well contain less than fifty thousand stars*. The six fields included 110, 60, 70, 90, 70, and 74 stars respectively their mean content being 79. The diameter (d) of the fields was 15′ ($d = 15'$), and so

$$\frac{30 \text{ square degrees}}{\pi d^2/4} = 661.2; \quad 79 \cdot 611.2 = 48{,}285.$$

But, then, even if we set aside the obvious rounding-off of the observed number of stars and assume the counts to be precise, the standard deviation of the mean (about which HERSCHEL knew nothing) is 7.4. Multiplying it by 611.2, I get 4,522. This means that the section hardly contained less than *forty* thousand stars. Note that HERSCHEL additionally counted the number of stars in *a most vacant place* [field]. It contained 63 stars and $63 \cdot 611.2 \approx 38{,}500$. HERSCHEL did not give this latter figure. It is important to repeat that a random selection of fields was not at all characteristic of his work in general.

[27] *Cf.* J. HERSCHEL's opinion [95, p. 374]:

it was desirable to ensure an absolute impartiality in the selection of the gauge-points, which could only be done by determining beforehand where they should occur ... .

The standpoint of HERSCHEL the elder seems to have been much the same.

Third, even in 1817 HERSCHEL [12, p. 575] thought that

The construction of the heavens, in which the real place of each celestial object in space is to be determined, can only be delineated with precision, when we have the situation of each heavenly body assigned in three dimensions ... .

It is well worth considering two additional circumstances.

(1) *Where the stars happened to be uncommonly crowded, no more than half a field was counted, and even sometimes only a quadrant ...* [4, p. 227].

Obviously, HERSCHEL doubled, or quadrupled, the counts. It is well known that he used his counts to derive the distances to the bounds of a finite (as he originally thought) universe. Supposing the stars to be uniformly distributed over the heavens, he calculated the cube roots of the number of stars, thus arriving at quantities proportional to the distances sought. It is therefore natural to assume that HERSCHEL tolerated a less accurate determination of larger distances.

(2) In one instance (*ibidem*, p. 246) HERSCHEL dropped a none-too-clear remark:

wherever the stars happened either to be uncommonly crowded or deficient in number ..., the gages were reduced to other forms, such as the border-gage, the distance-gage & c. which terms, and the use of such gages, I shall hereafter find an opportunity of explaining.[28]

I have been unable to find any explanation of these terms but in any case a special procedure for the study of particular sections of the sky was also envisaged in the *plan of selected areas* now being carried out. See also my § 9.2.

As time went on, HERSCHEL deviated from his original belief in the uniform spatial distribution of stars and in his later years he did not regard the star-gauging method as sufficiently accurate. Moreover, he came to understand that his telescope did not penetrate to the bounds of the sidereal system, *i.e.*, that his calculation of the distances to these bounds was simply wrong [98].

6.2. The Arrangement of Stars in Space

In 1817 HERSCHEL [12, p. 577] introduced a model of uniform spatial distribution of stars. He placed the stars of each given magnitude $(i-1)$, $i = 2, 3, \dots, 8$,

[28] In the same memoir, HERSCHEL (p. 223) vividly described the relation between facts and hypotheses in astronomy (actually, in experimental science in general):

If we indulge a fanciful imagination and build worlds of our own, we must not wonder at our going wide from the path of truth and nature ... . On the other hand, if we add observation to observation, without attempting to draw not only certain conclusions, but also conjectural views from them, we offend against the very end for which only observations ought to be made. I will endeavour to keep a proper medium; but if I should deviate from that, I could wish not to fall into the latter error.

between two corresponding concentric spheres (i and $i-1$). Table 6.2(1) shows the actual number of stars and quantities proportional to the volume of shells situated between the spheres.

Table 6.2(1). A model of a uniform spatial distribution of stars [12, p. 577]

Number of shell (i)	Radius of external sphere (r_i)	Stellar magnitude ($i-1$)	Number of stars according to BODE's catalog	The difference between the cubes of radii ($r_i^3 - r_{i-1}^3$)	Discrepancies (4)–(5)
1	2	3	4	5	6
2	3	1	17	26	−9
3	5	2	57	98	−41
4	7	3	206	218	−12
5	9	4	454	386	68
6	11	5	1161	602	559
7	13	6	$\geqq$6103	866	$\geqq$5237
8	15	7	6146	1178	4968

Notes. 1. Shell (sphere) No. 1 has radius $r_1 = 1$, this being an arbitrary unit, and includes only one star: the Sun.

2. Nonallowance for $4\pi/3$ in column (5) is tantamount to the introduction of stellar space density $3/(4\pi)$.

HERSCHEL selected the radii of his spheres in accordance with a law he postulated earlier [1, p. 52]: he indicated that stars of the second, third, fourth, *etc.* magnitudes are twice, thrice, four times, *etc.* farther away than those of the first magnitude. HERSCHEL understood well enough that this law was at best true only in the mean; moreover, he actually violated it in his model which merely fixed the bounds for star distances allowing the stars to be randomly distributed within these bounds.

HERSCHEL did not search for a better fit by changing the radii of the spheres (and thus complicating his model). Neither did he introduce a stellar space density differing from $3/(4\pi)$ (see note 2 to Table 6.2(1)), which would have resulted in a worsening of the fit for stars of the first four magnitudes without any essential improvement for subsequent stellar magnitudes. He remarked that his model provided a fair approximation for the whole set of stars of the first four magnitudes. Indeed, the corresponding sum of discrepancies is only six,[29] but the individual discrepancies are too large and HERSCHEL's model is hardly satisfactory. However, the very idea of stars of a definite kind being situated at random in a certain shell proved fruitful since both STRUVE (see my §§ 7.2–7.4) and KAPTEYN were to use it.

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[29] Thus HERSCHEL referred to a condition reminiscent of the main condition of the method, due to BOSCOVICH, of adjusting redundant systems of linear algebraic equations [144, § 1]. See also my § 6.4.

6.3. *Variations among Stars*

HERSCHEL repeatedly mentioned variations among individuals of a species; he most likely supposed the variations to be random and insignificant. Indeed, he also referred to variations in order to explain small and, as he might be understood to mean, random differences between stars; for example [6, p. 331], between the luminosities of stars of the identical magnitude belonging to the same cluster.

Elsewhere [4, p. 225] HERSCHEL assumed that

we ought perhaps to look upon ... clusters, and the destruction of now and then a star, in some thousands of ages, as perhaps the very means by which the whole is preserved and renewed.

This argument is vague, but at any rate similar to, and, for that matter, not more obscure than some ideas pronounced in biology. In 1772 ADANSON, a famous botanist, asserted [150, p. 334] that

monstruosités & variations ont une certaine latitude, nécessaire sans doute pour l'équilibre des choses ... ,

while in 1775 KANT (*ibidem*) also maintained that randomness was necessary in the organic world.

6.4. *The Movement of the Solar System*

HERSCHEL was the first to determine the apex of the sun's motion. Explaining his approach to the problem, he wrote [2, p. 120]:

We ought ... to resolve that which is common to all the stars ... into a single real motion of the solar system, as far as that will answer the known facts, and only to attribute to the proper motion of each particular star the deviations from the general law the stars seem to follow ... .

Such, HERSCHEL added, were *the rules of philosophizing.*[30] He then (pp. 120–127) applied his principle to adjust the proper motions of seven, and then twelve, stars. Using graphical methods, he managed without calculations.[31] In 1805 he returned to the same problem [10]. Drawing on his own data, he arrived at a redundant

[30] *Cf.* NEWTON (*Math. principles nat. philos.*, Book 3, *Rules of reasoning in philosophy*, Rule 1):

We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.

In 1805 HERSCHEL put forward similar arguments [10, p. 324].

[31] In 1783, in the memoirs of the Berlin Academy, PREVOST published his study of the movement of the sun. Since he referred to HERSCHEL's work of the same year, his own research must have appeared somewhat later. From my point of view, PREVOST offered nothing new.

system of (nonalgebraic) equations and solved it by a method of successive approximation. At each step he determined the apex by a graphical procedure securing a decrease in the residual motions for almost each star so as to minimize the sum of the motions.

After a few approximations, when the apex is almost reached, the equations of the system to be solved may be linearized, at least in principle. This fact suggests that HERSCHEL's criterion can be put into correspondence with the main condition of BOSCOVICH's method for the solution of redundant systems of linear algebraic equations; see also my note 29. Attempting to determine the velocity of the sun's motion, HERSCHEL [11, p. 362] had to choose between the arithmetical mean and the median of a series of observations.

There are two ways of taking a mean of the siderial motions, he noted, *one of them may be called the rate and the other the rank. For instance, a number equal to the mean rate of* ... 2, 6, 13, 15, 17, 19, *would be* 12; *but one that should hold a middle rank between the three highest and three lowest* ... *would be 14.*

Remarking that the difference between the two numbers is not large, he indicated that the rank should be chosen. Once more (p. 358), this time mentioning the *doctrine of chances,* but still without producing any proof, he stated that *a* (?) *middle rank* [the median] *must be the fairest choice* ... .

I think that HERSCHEL followed LAPLACE, who at that time, considering the case of a small number of observations, preferred the median rather than the arithmetical mean [148, pp. 3 and 8–9].

6.5. The Size of the Stars

In 1817 HERSCHEL [12, p. 579] formulated an assertion about the size of the stars. It seems that he was guided by an intuitive notion which was later put into mathematical form as CHEBYSHEV's inequality. Since, HERSCHEL wrote, there are more than fourteen thousand stars of the seven magnitudes,

it may be presumed that any star promiscuously chosen ... *out of such a number, is not likely to differ much from a certain mean size of them all.*

In regard to their size, stars are so extremely different that HERSCHEL was completely wrong. His mistake is an excellent illustration of the fact that the theory of probability, like any other scientific discipline, cannot lead to concrete results in the absence of positive knowledge (in this instance, in the total absence of data).

7. Struve

STRUVE was one of the most eminent astronomers of the 19th century.[32] He also was one of those responsible for an immense meridian arc measurement

[32] See also my §§ 5.9 and 8.3.

and an active participant in it. Finally, STRUVE was one of the first in Russia and, perhaps, in Europe, to deliver lectures on probability theory. My source of information is an article whose author asserts [117, p. 187], without adducing any references, that STRUVE delivered his lectures while in Derpt (Tartu), *i.e.*, before 1839.

STRUVE'S main work is his *Études* [49]. Here he described in detail the research of HERSCHEL and subsequent astronomers[33] as well as his own work [48] then just published. It seems that the *Études* were written somewhat hastily; at any rate, it is rather difficult to follow the general exposition, and the explanations of particular statements are not always sufficient. For the most part I shall restrict myself to a description of this source.

7.1. The Completeness of Star Catalogs

Comparing three 'overlapping' star catalogs with each other, STRUVE [49, pp. 51–57] estimated the completeness of one of them, thus establishing the total number of stars of the first eight magnitudes in the zone he had investigated. He did not calculate the error of his estimate due to the incompleteness of the two other catalogs. STRUVE treated the same subject even before 1847 [48, pp. xxv–xxvii], and he subsequently [49, pp. 54–55] used the results of this earlier investigation to estimate the number of stars of the ninth magnitude in the same zone.[34]

Let [48] a certain zone contain z stars of brightness μ and z' stars of brightness μ' (z is known while z' is not); suppose that among r (r') observed stars of brightness μ (μ') a_1 (a'_1), a_2 (a'_2), ...; a_5 (a'_5) stars were observed once, twice, ..., five times. Then

$$a_1 + a_2 + \ldots + a_5 = r, \; a'_1 + a'_2 + \ldots + a'_5 = r'.$$

(Here and below I have partly changed STRUVE'S notation.)

Finally, suppose the zone is divided into five sections, the first, second, ..., fifth of which were surveyed once, twice, ..., five times with the number of stars of brightness μ being $\alpha_1, \alpha_2, \ldots, \alpha_5$, respectively and $\alpha_1 + \alpha_2 + \ldots + \alpha_5 = z$. Stars of this brightness observed five times can only belong to the fifth section; those observed four times, to the fifth or the fourth sections, *etc.* (STRUVE assumed $\alpha_1, \alpha_2, \ldots, \alpha_5$ to be unknown.) As to stars of brightness μ', he wrote their numbers in the five sections $x\alpha_1, x\alpha_2, \ldots, x\alpha_5$ so that $z' = xz$. He also introduced the coefficient of completeness p ($0 < p < 1$) and denoted $1 - p = q$.

Quantities $a_1, a_2, \ldots, a_5$ and $\alpha_1, \alpha_2, \ldots, \alpha_5$ satisfy equations

$$a_i = p^i \sum_{k=i}^{5} C_k^i q^{k-i} \alpha_k, \qquad i = 1, 2, \ldots, 5. \tag{1}$$

[33] STRUVE also reported (pp. 94–108) on the fundamental not yet published investigation of star parallaxes by PETERS [127] and paid special attention to studies of solar motion. M. S. EIGENSON, the translator of the *Études* into Russian, and A. A. MIKHAILOV, the editor of the Russian edition, justly consider STRUVE a beginner in the history of stellar astronomy [49, 1953, p. 124].

[34] He did not publish the data necessary for this estimation.

Thus

$$a_2 = p^2(\alpha_2 + 3q\alpha_3 + 6q^2\alpha_4 + 10q^3\alpha_5).$$

Solving system (1) with respect to $\alpha_1, \alpha_2, \ldots, \alpha_5$ (I omit the relative formulas), STRUVE introduced the linear forms

$$\begin{aligned} f &= a_1 + 2a_2 + 3a_3 + 4a_4 + 5a_5, & k &= a_4 + 5a_5, \\ g &= a_2 + 3a_3 + 6a_4 + 10a_5, & l &= a_5, \\ h &= a_3 + 4a_4 + 10a_5 \end{aligned}$$

and, denoting by $f', g', \ldots, l'$ and p' the corresponding quantities for stars of brightness μ' obtained the equations $fxp' = f'p$, $gxp'^2 = g'p^2$, $hxp'^3 = h'p^3$, $kxp'^4 = k'p^4$, $lxp'^5 = l'p^5$ in two unknowns, x and p'/p. Since a_3, a_4, and a_5 were too small (*h, k, l et h', k', l' valde exiguus esse*), STRUVE used only the first two of the equations to get

$$x = \left(\frac{f'}{f}\right)^2 \frac{g}{g'}, \qquad z' = z\left(\frac{f'}{f}\right)^2 \frac{g}{g'}.$$

I can repeat the remark I made at the end of § 3.2 concerning a formula due to POINCARÉ.

STRUVE once again formulated the above problem in his *Études* [49, Note 71], where he gave the final answer with no derivation whatsoever. Furthermore, he did not say he had published the derivation before and he did not even explain that different sections of the zone were surveyed an unequal number of times.[35]

7.2. *Maximal Distances of Stars*

Basing his analysis on an inexact, as he himself acknowledged [49, Note 72], assumption of uniform spatial distribution of stars, STRUVE calculated the maximal relative distances of stars of given magnitudes. Suppose for example (*ibidem*) that a certain portion of the sky contains a stars of the first five, and b stars of the first six magnitudes. Then the sphere of fifth-magnitude stars has radius $r = (a/b)^{\frac{1}{3}}$, unity being the radius of the sphere for stars of the sixth magnitude.[36] Like HERSCHEL (see my § 6.2), STRUVE thus left room for randomness in the spatial arrangement of stars. Indeed, in his model, stars of a given magnitude can be situated at random within a sphere of a certain radius. Nothing prevents me from maintaining a similar opinion in regard to the conclusions of §§ 7.3 and 7.4 below.

[35] At best, readers of the main text of the *Études* could suspect this fact.

[36] In those times it was customary to calculate an obviously excessive number of significant digits. GAUSS himself did so in his geodetic work [87, §§ 23–25] and STRUVE followed suit calculating $r = 0.7126$. In this respect his attitude was the same throughout the *Études*.

7.3. The Distribution of Stars in Space

Drawing on HERSCHEL'S data, STRUVE [49, pp. 59 and 70] assumed certain laws for the spatial arrangement of stars. Accordingly, he (pp. 71–72) introduced an empirical function of the type

$$z = \frac{a + b_1 \cos 2\varphi + c_1 \cos 4\varphi}{1 + b_2 \cos 2\varphi + c_2 \cos 4\varphi} \tag{1}$$

for the number of stars visible in HERSCHEL'S 20 foot telescope at an angle φ from the principal plane of the stellar system. To a large extent the conditions imposed on function (1) (p. 33 of the Notes) determined its form; still, STRUVE did not explain why the function was so involved. Indeed, one would think he would have been satisfied by the numerator alone.

Using formula (1), STRUVE (p. 73) also derived a relation for the relative density of stars as a function of their distances from the principal plane

$$\varrho = \frac{1 + e_1 x^2 + f_1 x^4 + g_1 x^6 + h_1 x^8}{(1 + e_2 x^2 + f_2 x^4)^2}, \quad 0 \leqq x \leqq 0.8660$$

(0.8660 = sin 60°). ERPILEV [74, p. 113] remarked that the integral equation which STRUVE had to solve numerically so as to determine ϱ was the first of its kind used in stellar statistics.

Conducting a similar study of stars included in WEISSE'S catalog, STRUVE compared his results and concluded that his formulas were plausible enough. Still, he evidently strove to show only the most general picture of the stellar system. Moreover, he calculated the coefficients of function (1) taking into account five points with abscissas $\varphi = 0(15)60°$ which was hardly enough. Empirical formulas had been in use long before STRUVE. In 1772 LAMBERT [140, p. 247] used an empirical function of mortality, and C. WOLFF formulated the 'TITIUS-BODE' law as early as 1723 (see my § 2).[37] Nonetheless, in STRUVE'S time the tradition of introducing empirical formulas had hardly been established.

7.4. Mean Distances of Stars

Later on STRUVE [49] recalculated the maximal and mean star distances allowing for stellar density. As before [43, pp. xxxiv–xxxv], he [49, p. 80] took the mean distance of stars of a certain magnitude to be the radius of the sphere that included all brighter stars and half the stars of the given magnitude. Suppose that the estimated number of stars of magnitude i is $2n$. Denote their distances by

$$r_1, r_2, \ldots, r_{2n} \quad (r_1 \leqq r_2 \leqq \ldots \leqq r_{2n}). \tag{1}$$

[37] In theoretical astronomy, epicycles "attached" one by one to the PTOLEMAIC system of the world were tantamount to empirical correction terms.

Then, in principle (*i.e.*, without allowing for the change in stellar density with distance), the mean distance of these stars, according to STRUVE, is equal to the distance of star n or, actually, to the median of numbers (1).[38]

PETERS [126, p. 201] made an important critical remark on STRUVE's model:

Man hat gegen Struve's Ableitung der Entfernungen der Fixsterne die Einwendung gemacht, dass die ihr zum Grunde liegende Hypothese, die Sterne von gleicher Helligkeit seien auch gleich weit von uns entfernt, sehr unrichtig sein könnte ... (see also my note 53 and § 9.2.2).

Accordingly, PETERS proved that, if all luminosities from the interval $[0, a]$, $a > 0$, are supposed equally probable for each star, and if the spatial distribution of stars is supposed 'random' [uniform], the mean distance of stars of magnitude i is proportional to the cube root of the total number of stars up to and including that magnitude.

The conditions of this theorem were strong, but it was at least a few decades ahead of its time in spirit.

7.5. *The Extinction of Light*

Drawing on statistical data, STRUVE attempted to prove [49, pp. 83–93] that interstellar space absorbs light. His proof was not convincing [25, p. 377] (although the existence of the phenomenon was in the end ascertained) because it was based on essential assumptions concerning the structure of the stellar universe.[39]

7.6. *Opinions about the Études* [49]

ENCKE [73] was the first to comment on the *Études*. He asserted: (1) that STRUVE did not formulate his assumptions concerning the structure of the stellar system and, furthermore, that STRUVE even denied introducing any hypotheses; (2) the assumptions made were far-fetched and the work as a whole was therefore a failure.

I shall comment on the latter item. As to the former, STRUVE did point out at least some of his hypotheses (for example, those underlying his empirical formula (7.3.1)). STRUVE's statement [49, p. 81]

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[38] Without referring to STRUVE, KLEIBER [102] calculated the mean distance (r_0) of stars situated 'at random' inside a sphere Ω of radius R:

$$r_0 = \iiint_\Omega r\,dv : \iiint_\Omega dv = \frac{3}{4}R.$$

He applied this result to estimate the mean star parallax.

[39] STRUVE (p. 87) thought that these assumptions could scarcely fail to be true.

Le tableau [des distances relatives] *renferme tout ce que notre recherche nous a fourni par rapport aux distances des étoiles, ... par une recherche uniquement basée sur l'observation, sans y employer aucune hypothèse arbitraire,*

which ENCKE referred to, seems to be rather unfortunate.

On October 23, 1847, in a letter to GAUSS, SCHUMACHER [89, p. 379] informed his correspondent about ENCKE's forthcoming review [73] of the *Études*:

Encke sucht zu zeigen dass Struve's ganzes Gebäude ein Kartenhaus sei auf nicht hinlänglich begründeten Hypothesen aufgeführt.

Answering him on October 27th and referring to the review which he had obviously seen in manuscript form, GAUSS pointed out (p. 384):

Sie wissen, dass ich von jeher kein Freund davon gewesen bin, schwach begründeten Hypothesen einen Platz in der Wissenschaft einzuräumen ... .

Finally, on November 7th GAUSS continued (p. 394):

Im Allgemeinen würde ich gegen dergleichen Phantasiespiele nachsichtig sein, und ihnen nur [!] *die Aufnahme in die wissenschaftliche Astronomie ... nicht einräumen. Gehören doch auch Laplace's Cosmogenische Hypothesen in jene Classe.*

Clearly, GAUSS had not yet read the *Études*. Still, STRUVE's work exerted a strong influence on astronomy in the 19th century. Of course, STRUVE could not apply methods, or propound ideas, peculiar to some future period. His time might be characterized by saying that parallaxes were then only known for an insignificant number of stars, the measurement of stellar radial velocities had not yet begun and the notion of the mean distance of stars of a given magnitude was still in use (*cf.* my § 9.2.2). It was therefore inevitable that fresh findings superseded STRUVE's estimates and results rather soon. At the end of the 19th century, KAPTEYN [13, p. 129] maintained that the

arrangement der sterren in de ruimte [according to STRUVE] *ist niet in overeenstemming met de werkelijkheid.*

Roughly twenty years later SCHOUTEN [135, p. 6] even stated that STRUVE's

merit consists in making use for the first time of countings of catalogues of stars. His method and his results however are only of historical value.

Insofar as SCHOUTEN mentioned nothing else, his is of course an extreme opinion. An opposite extreme pronouncement is due to DE SITTER, who thought [71, p. 49] that, in the *Études*.

The discussion of the material is very careful and a model of sound scientific criticisms.

De Sitter raised no objections to Struve's assumptions and he even noted (p. 50) that Herschel could not manage without hypotheses either.

8. Proper Motions of Stars

Investigation of the spatial arrangement of stars continued to be a most important subject of astronomy after Herschel. For example, Struve's *Études* [49] were centred precisely on this topic; see my §§ 7.2–7.4. However, during the 1830's and 1840's, stellar astronomy took a new direction, *viz.*, study of proper motions of stars. In part, this trend was due to a desire to confirm Herschel's discovery of the sun's motion (see my § 6.4) and to determine it more accurately.[40]

8.1. Argelander

Argelander [54, p. 581] considered 560 stars with a perceptible proper motion. Because of difficulties in calculation he divided the stars into three classes according to the magnitude of their motion (p. 586) and assumed that stellar distances are on the whole inversely proportional to their motions. His aim was to determine the direction of the sun's motion and he calculated it for each class separately.[41]

8.2. O. Struve

O. Struve [153; 154] determined the mean proper motion of four hundred stars of the first seven magnitudes; see Table 8.2(1).

8.3. Subsequent Work

Thus O. Struve attempted to prove that on the whole brighter stars possess larger proper motions. Nevertheless, referring to Bessel and Arago, Humboldt [99, p. 267] formulated an opposite opinion:

Die leuchtenderen Sterne haben großentheils ... schwächere Bewegung als Sterne 5ter bis 6ter und 7ter Größe.

[40] Obviously, astronomers understood well enough the fundamental scientific importance of studying the sun's motion. Struve described the findings of his contemporaries precisely from the standpoint of this problem; see note 33. Elsewhere [50] (see my § 8.3) investigating the proper motions of stars, he also calculated the velocity of the sun's motion.

[41] *Cf.* J. Herschel's remark [96, p. 585]:

two courses only present themselves, either, 1st, To class the distances of the stars according to their magnitudes, or apparent brightnesses, and to institute separate and independent calculations for each class ... or, 2dly, To class them according to the observed amount of their apparent proper motions, on the presumption that those which appear to move fastest are really nearest to us.

Table 8.2(1). Proper motions of stars [153, pp. 54–56; 154, pp. 71–72]

Stellar magnitudes	Relative distances of stars	Proper motions of stars during 70 years Observed	Calculated	Discrepancies (3)–(4)
1	2	3	4	5
1	1	36.″1	43.″2	−7.″1
2	1.71	10.9	25.3	−14.4
3	2.57	11.0	16.8	−5.8
4	3.76	8.4	11.5	−3.1
5	5.44	6.7	8.0	−1.3
6	7.86	5.5	5.5	—
7	11.34	4.5	3.8	0.7

Notes. 1. O. STRUVE took stellar distances according to F. G. W. STRUVE [43, p. xxxv], *i.e.* as being proportional to the cube root of the number of stars; see my §§ 7.2 and 7.4.

2. He assumed that for stars of the most numerous group, *i.e.* for those of the sixth magnitude, the calculated proper motion should coincide with the observed motion. For stars of other magnitudes, motions in column 4 are derived from their distances; thus, for magnitude 5, $8.0 = 7.86 \cdot 5.5 : 5.44$.

3. He partly explained the existence of considerable discrepancies [(3)–(4)] by a non-uniform spatial distribution of stars.

In a note included in his translation of HUMBOLDT's work into Russian, GUSSEW (pp. 555–557) pointed out the mistake committed by the great naturalist. He referred to F. G. W. STRUVE (see below) and to his own *nearly completed* study which he had undertaken in 1852 on STRUVE's advice. He published a second version of his note in German [92].

STRUVE investigated the proper motions of 1662 stars. I explicate his results [50, pp. clxxxii–clxxxv] in Table 8.3(1). STRUVE also estimated the velocity of the sun's motion (p. clxxxvii) as being equal to 0.5–0.8 of the mean peculiar motion of 736 single stars. No wonder that ERPILEV [74, p. 177] believed that the study of proper motions had actually begun with STRUVE. FEDORENKO [78, p. 84] stated that

die mittleren Sternbewegungen sind den mittleren Sterngrössen nach der Schätzung der Astronomen umgekehrt proportional.

Table 8.3(1). Proper motions of stars [50, p. clxxxii]

Number of stars	Stellar magnitudes; mean stellar magnitudes	Proper motion during 30 years α	δ
180	1–4.5 (3.15)	4.″64	4.″58
206	4.5–7 (5.66)	1.87	1.41
1276	> 7 (7.34)	1.12	0.82

He repeated his assertion elsewhere [79; 80, p. 13]; in one instance [79] he referred to MÄDLER (*Dorpater Beobachtungen* von 1856). FEDORENKO [80, p. 7] calculated the proper motion of 2,590 stars of magnitudes ranging from 4.5 to 9.25. Apparently, this quantity was not enough, and his conclusion was wrong.[42] And, of course, neither he, nor his predecessors including STRUVE had any possibility of considering the ray velocities of stars. However, the main objection to the findings of FEDORENKO and other astronomers (see § 8.2 and above) concerning the mean proper motion of stars of a given magnitude is that this concept seems to be of slight importance.

PETERS [127, p. 50] sought to determine the mean parallax of stars of the second magnitude. He was able to take into account 35 stars with magnitudes up to 4.5, but he calculated the parallaxes of stars of magnitudes 1(0.5)6, which was rather unjustifiable. His investigation made it possible to calculate [49, p. 102] (true, only to a first approximation) the sun's motion in terms of the radius of the earth's orbit.

8.4. Peculiar Motions and the Normal Law

In adjusting the proper motions of stars while studying the sun's motion, astronomers, beginning with HERSCHEL (see my § 6.4), considered peculiar motions to be random quantities. In particular, STRUVE [47, pp. 132–133] pointed out:

Quant au mouvement particulier des étoiles, il est comme accidentel pour nous ... .

AIRY [51, p. 147] remarked that the probability of a peculiar motion of a certain star following any direction is constant, while FEDORENKO [80, p. 8] even maintained without proof that proper [peculiar?] motions are distributed *according to the law of accidental errors.*[43]

Much later, in a popular lecture, KAPTEYN [15, p. 400] introduced a *fundamental hypothesis*[44] concerning peculiar motions:

The peculiar motions of the stars are directed at random, that is, they show no

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[42] GUSSEW [92] was the first to come out against him.

[43] As far back as 1842 O. STRUVE had formulated a similar assertion more accurately. According to his evidence [153, p. 51],

Bisher hat man den Werth der Praecessionsconstante abgeleitet in der Voraussetzung dass sämmtliche eigene Bewegung gleichsam als zufällige Beobachtungsfehler in die Rechnung hineinkämen und deshalb ihr Einfluss sich bei einer gehörig grossen Anzahl von Sternen aufheben müsste.

However, O. STRUVE continued, the same assumption still (*i.e.*, even after ARGELANDER published his study [54]) persisted, though in regard to peculiar motions.

[44] Quite in LAPLACE's spirit [147, p. 176], he remarked (p. 412) that this *provisional* hypothesis was *to be used for want of a better.*

preference for any particular direction. I shall further on refer to this hypothesis as the fundamental hypothesis.

As a consequence, he continued (*ibidem*), *the sum of the projections* [of these motions] *on any line ... must be zero* (see below). KAPTEYN (p. 418) even called the distribution of motions satisfying his hypothesis *normal.*[45] In a later work [20, p. 310] KAPTEYN assumed that the *motions corrected for both the solar and the stream motions* [see note 45] *with some crude approximation are Maxwellian ... .*

Consider KAPTEYN'S statement regarding the projections of peculiar motions. Denote these motions by

$$v_1, v_2, \ldots, v_n; \tag{1}$$

then their projections on an arbitrary axis L would be $v_i \cos \alpha_i$, where α_i, $i = 1, 2, \ldots, n$, are the angles between the directions of motion and L. For random errors (1), the classical error theory beginning at least with MAEVSKY [108, p. 62], assumed that, according to the law of large numbers,

$$\lim_{n \to \infty} \frac{v_1 + v_2 + \ldots + v_n}{n} = 0. \tag{2}$$

MARKOV [109, p. 249] noticed that equality (2) also holds for linear functions of v_i (and thus for $v_i \cos \alpha_i$).[46] It seems that KAPTEYN actually thought of the mean, rather than the sum, of the products $v_i \cos \alpha_i$ as being equal to zero.

NEWCOMB [32, p. 166] assumed that the projections of stellar motions on an arbitrary axis are distributed according to [proportional to] the normal law:

$$\varphi(x) = e^{-x^2/a^2}.$$

Then, as he stated, regrettably without proof, the projections of these motions on an arbitrary plane follow the density law [proportional to]

$$\psi(x) = x e^{-x^2/b^2}, \tag{3}$$

where b is a *simple* function of a, while the motions themselves are distributed according [proportional] to the law

$$\zeta(x) = x^2 e^{-x^2/c^2} \tag{4}$$

[45] In the same lecture KAPTEYN (pp. 416, 418, and 419) reported that a graphical study (by means of star charts and a celestial globe) of the peculiar motion of the stars led him to believe in the existence of two star streams. At present, astronomers do not accept this statement.

[46] Exactly for this reason MARKOV opposed substantiation of the method of least squares by appeal to the law of large numbers. Earlier BOUNIAKOWSKY [64, pp. 269–270] derived equality (2). He commenced from the central limit theorem as proved (non-rigorously) by LAPLACE. However, BOUNIAKOWSKY's derivation contained an error.

(NEWCOMB did not specify c). Note that the transition from the normal law to functions (3) and (4) is explained elsewhere [141, p. 328]; the latter functions are connected with the chi-square distribution.[47]

Thus an intuitive feeling that peculiar motions might be treated as a random quantity prevailed. Some astronomers, without even a heuristic substantiation of their belief, thought that the motions were subject to the normal law.

9. The Statistical Method

For HERSCHEL (see my § 6.1), individual stars (or stars of the same magnitude) were equitable elements of a statistical population, while his model of the spatial distribution of stars (§ 6.2) left room for randomness. The same can be said about STRUVE (§§ 7.2–7.4) who also applied the statistical method to the solution of special problems (§§ 7.1 and 7.5) and whose model of stellar distribution was more elaborate than the one due to HERSCHEL. But then, almost all the conclusions which he arrived at in his *Études* [49] were of a determinate nature.[48] Consider for example the maximal and mean distances of stars of some magnitude i. Allowing for the stellar density that he determined, using HERSCHEL'S data, STRUVE calculated the distances drawing on the number of stars up to and including the i^{th} magnitude. And it really seems that he considered all his initial information to be determinate. At any rate, he did not mention any laws of distribution of stellar distances or even possible errors in the mean distances. I think that this fact is justified. Indeed, astronomers of that time studied stars only of the first few magnitudes and the estimation of distances was only preliminary. Besides, astronomers, and even natural scientists in general, did not then use empirical laws of distribution; it would have been unnatural to expect STRUVE to describe the stellar system in terms of distributions, as KAPTEYN did about fifty years later.

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[47] I do not know why NEWCOMB did not refer to his own study [30] in which he, unlike FEDORENKO or KAPTEYN, compared the distribution of centennial proper motions in declination with the normal law and noted considerable disagreement between the two. He actually used the terms *normal law of error, normal curve of errors.* On the history of these expressions I should mention the following:

In 1905 PEARSON [120, p. 189] indicated:

On the whole my custom of terming the curve the Gauss-Laplacian or normal curve saves us from proportioning the merit of discovery between the two great astronomer mathematicians.

PEARSON'S contributions to probability theory started to appear in 1893 and even in 1894 he [119, p. 72] wrote:

A frequency curve, which ... can be represented by the error curve, will for the remainder of this paper be termed a normal curve.

In 1873 PEIRCE [125, p. 206] used the term *normal least-squares curve* and NEWCOMB possibly followed suit. KRUSKAL [103, p. 99] discovered PEIRCE'S expression and also credited GALTON with subsequent use of the terms *normally* and *normal in shape.*

[48] This fact is true even in regard to the calculated completeness of stellar catalogs (see my § 7.1), and consequently, in respect to STRUVE'S (obviously methodological) estimation of maximal stellar distances based on the derived completeness.

A new feature of stellar astronomy in the second half of the 19th century was the study of statistical regularities inherent in stellar ensembles (see my § 8)[49] though no fitting substantiation of the approach had been offered.

COURNOT, to whom the first pronouncement on the statistical method in astronomy is due [70, § 145], did not notice this fact. He merely remarked that

S'il est une branche de la philosophie naturelle à laquelle ce genre de recherches puisse s'approprier avec chance de succès, c'est assurément l'astronomie.

COURNOT continued:

la statistique des astres (s'il est permis de recourir à cette association de mots) doit servir un jour de modèle à toutes les autres statistiques.

His work, partly popular science and not addressed specifically to astronomers, appeared at a time when the development of the classical (pre-CHEBYSHEV) probability theory had just ended. COURNOT could not have influenced astronomy appreciably, in particular because he himself only applied stochastic reasoning to the study of planetary and cometary orbits without even mentioning investigations in stellar astronomy.

Only towards the end of the 19th century appeared statements correctly formulating the (statistical) aims of this discipline. CLERKE [69, p. 9] noted that

The stars in their combinations demand inquiry no less than the stars in themselves. ... statistics are wanted of the distances and movement of thousands, nay millions of stars.

In the sequel (p. 311) she referred to HILL & ELKIN [97, p. 191] who expressed themselves even more directly:

The great Cosmical questions to be answered are not so much what is the precise parallax of this or that particular star, but:

1. What are the average parallaxes of those of the first, second, third, and fourth magnitude respectively, compared with those of lesser magnitude?

2. What connection does there subsist between the parallax of a star and the amount and direction of its proper motion or can it be proved that there is no such connection or relation?

9.1. Proctor

PROCTOR denied the statistical method. In 1872 he [38] compiled charts of stars of the first six magnitudes showing their proper motions. By means of

[49] In this sense astronomy overtook physics, which the statistical method did not penetrate until the middle of the 19th century and even then at first only because it was impossible to study the motion of individual molecules.

these charts PROCTOR claimed (pp. 147–148) to have discovered stellar streams.[50] He obviously had no possibility of checking his finding by analytic methods. Elsewhere [40, p. 544] PROCTOR contrasted his graphical procedure with statistical studies:

I can conceive no general statistical process absolutely free from hypothetical considerations. Statistics can be satisfactorily applied to inquiries suggested by other and less deceptive processes; but at the beginning we cannot count except on accordance with some prearranged plan, and such plan must necessarily be based on hypothesis.

For example, PROCTOR continued, STRUVE [49, p. 56]

counted the number of stars in given hours of right ascension; but the result was meaningless, except on the assumption that the distribution of stars over a given hour ... possessed a certain significance.

PROCTOR plotted 324 thousand stars on his charts and thus he got along without any theories on the structure of the stellar system (pp. 545 and 547) ... These obscure utterances are unconvincing. PROCTOR could not have studied star distances by means of his charts; besides, their very compilation was evidently founded on a serious, even if elementary, statistical study. PROCTOR was right only insofar as fundamental difficulties are indeed inherent in sampling.[51] But without statistics, without sampling, astronomy could not have developed in any case. PROCTOR himself actually spoke out in favor of sampling [39]:

Great exactness in enumeration is by no means necessary. ... What is required is a complete but rapidly effected survey bearing the same relation to the actual charting of stars, that the reconnaissance of a land region bears to trigonometrical survey.

A few decades before PROCTOR started his work, French physicians hotly debated the expediency of applying the statistical method in medicine. PROCTOR's opinion corresponded to the standpoint of the proponents of the so-called numeri-

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[50] He reported on the streams even in 1869 [36], long before KAPTEYN (see my note 45). PROCTOR [38, p. 147] also concluded that *the average proper motion of the brighter orders of stars is barely equal to that of the three lower orders* (see my § 8.3).

PROCTOR was an active advocate of popular science. In particular, he published a lot of popular articles on probability theory and its applications [42]. At the same time, his knowledge of the theory was slight. Thus studying the difference between the number of *lucid* stars in the northern and southern parts of the sky he [37] directly estimated the sum of the corresponding binomial coefficients of the development of $(1 + 1)^n$ where n was the total number of *lucid* stars. He did not apply the normal approximation to the binomial distribution.

[51] *Cf.* NEWCOMB's remark [33, p. 303]:

All scientific conclusions drawn from statistical data require a critical investigation of the basis on which they rest.

cal method [151, § 4]. In a more general way PROCTOR's attitude illustrates the fact that statistics always seems to be at odds with the specific branch of science to which it is applied. To illustrate, advances in surgery such as the introduction of anesthesia and antiseptics [151, § 6.1] made useless all previous studies of the mortality of amputations; in turn, the investigation of anesthesia led to the need for a statistical study of deaths from bronchitis after amputation, *etc.* Being restricted by my upper chronological boundary, I am not in a position to offer an astronomical example as telling as the one pertaining to surgery. See however § 9.2.2.

9.2. Kapteyn

KAPTEYN'S scientific activity was in essence directed towards a statistical description of the stellar system as a whole. In his popular reports [15; 18][52] he vividly depicted the stellar universe describing it by means of the laws of distribution of parallaxes and peculiar motions of stars. He thus regarded both parallaxes and motions as random quantities.[53] KAPTEYN [15, p. 397] explained his standpoint thus:

Just as the physicist ... cannot hope to follow any one particular molecule [of gas] *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.*

He could have added that neither the physicist, nor even the astronomer, had any need to study the isolated objects of their systems; *cf.* § 9.

NEWCOMB [33, p. 302] offered a generally correct estimate of KAPTEYN'S work:

In recent times what we may regard as a new branch of astronomical science is being developed, showing a tendency towards unity of structure throughout the whole domain of the stars. This is what we now call the science of stellar statistics. ... In the field of stellar statistics millions of stars are classified as if each taken individually were of no more weight in the scale than a single inhabitant of China in the scale of the sociologist.

The statistics of the stars may be said to have commenced with Herschel's gauges of the heavens ... . The subject was first opened out into an illimitable field of research through a paper presented by Kapteyn to the Amsterdam Academy of sciences in 1893 [13?].

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52 He continued his work for a good twelve years more during which stellar astronomy experienced essential progress. Thus the connection between absolute magnitudes of stars and their spectra was established.

53 In particular, KAPTEYN concluded [18, p. 310] that no real meaning could be attached to the notion of the mean distance of stars of a given magnitude. *Owing to want of other data,* KAPTEYN maintained, STRUVE *saw himself compelled* to place all the stars of the fifth magnitude [why only the fifth?] *at one and the same* [mean] *distance.*

And, further (p. 303):

The outcome of Kapteyn's conclusions is that we are able to describe the universe as a single object ... .

NEWCOMB could have well referred here to STRUVE.

One of KAPTEYN'S important achievements in stellar statistics was the initiation of an international plan for a study of the stellar universe [16] by sampling. Explaining his point of view, he wrote (p. 14):

whereas all the astronomers consulted agree in the demand for a uniform distribution of a good part of the areas, some would absolutely restrict them to such uniformly arranged positions, on the ground that only such a plan might be expected to lead to the knowledge of the general laws governing the structure of the sidereal system and that these must be found out before divergences from the rule are to be studied. Others urged that in this way just the most interesting parts of the sky ... would be excluded.

KAPTEYN (p. 67) also published a letter he received in 1904 from PICKERING in which the distinguished astronomer maintained:

As in making a contour map, we might take the height of points at the corners of squares a hundred metres on a side, but we should also take the top of each hill, the bottom of each lake, ..., and other distinctive points.

KAPTEYN did not mention sample surveys of population which had come into general practice at the turn of the 19th century.

Determining the characteristics of faint stars is now basically done in some areas uniformly distributed over the sky and additionally at places of special interest. In other words, faint stars are studied according to (PICKERING'S) scheme of stratified sampling.

9.2.1. Coefficient of Correlation. KAPTEYN [19] was not satisfied with GALTON'S definition of the correlation coefficient[54] which came to be used, for example, by biologists. He aimed at a quantitative estimate of the connection between two functions depending on partly coinciding measured arguments. Suppose, KAPTEYN reasoned,

$$x = \varphi(a_1, a_2, \ldots, a_k, b_1, b_2, \ldots),$$

$$y = \psi(a_1, a_2, \ldots, a_k, c_1, c_2, \ldots)$$

[54] In 1865–1866, the astronomer and mathematician L. SEIDEL [151, § 7.4], in a study of the occurrence of typhoid fever, quantitatively estimated the connection between the prevalence of the disease and meteorological factors. SEIDEL also published a few articles on the theory of errors. I could not find any trace of his contribution *Über das Wahrscheinlichkeitsgesetz der Fehler bei Beobachtungen* which he proposed to publish *in einer Fachzeitschrift* (*Sitz. Ber. Bayer. Akad. Wiss.*, Math.-Phys. Kl., Bd. **14**, 1884 (1885), p. 194).

are the coordinates of a moving point on a plane with elements a_i, b_i, and c_i being characterized by normally distributed errors α_i, β_i, and γ_i, respectively. Taking into account only the first terms of the corresponding power series and appropriately translating the system of coordinates, KAPTEYN got

$$x = G_1\alpha_1 + G_2\alpha_2 + \dots + G_k\alpha_k + B_1\beta_1 + B_2\beta_2 + \dots,$$

$$y = H_1\alpha_1 + H_2\alpha_2 + \dots + H_k\alpha_k + C_1\gamma_1 + C_2\gamma_2 + \dots$$

so that x and y also became normally distributed errors. Passing on to mean [square] errors, he arrived at

$$\varepsilon_x^2 = G_1^2\varepsilon_1^2 + G_2^2\varepsilon_2^2 + \dots + G_k^2\varepsilon_k^2 + B_1^2\mu_1^2 + B_2^2\mu_2^2 + \dots,$$

$$\varepsilon_y^2 = H_1^2\varepsilon_1^2 + H_2^2\varepsilon_2^2 + \dots + H_k^2\varepsilon_k^2 + C_1^2\nu_1^2 + C_2^2\nu_2^2 + \dots$$

The joint probability for quantities x and y to belong to intervals $[x, x + dx]$ and $[y, y + dy]$ respectively is equal to

$$P = \frac{1}{2\pi\sqrt{1 - r^2}} \exp\left[-\frac{1}{2(1 - r^2)}\left(\frac{x^2}{\varepsilon_x^2} - 2r\frac{xy}{\varepsilon_x\varepsilon_y} + \frac{y^2}{\varepsilon_y^2}\right)\right]\frac{dx\,dy}{\varepsilon_x\varepsilon_y},$$
$$r = \frac{1}{\varepsilon_x\varepsilon_y}[G_1H_1\varepsilon_1^2 + G_2H_2\varepsilon_2^2 + \dots + G_kH_k\varepsilon_k^2]. \tag{1}$$

BRAVIAS [65] knew an equivalent formula though he did not introduce quantity r. Recording this fact, KAPTEYN nevertheless called BRAVAIS the originator of the correlation theory.

Continuing his exposition, KAPTEYN assumed without loss of generality that

$$\varepsilon_x = \varepsilon_y = 1.$$

He then noticed that distribution (1) persisted if, first,

$$x = \sqrt{G_1H_1}\,\alpha_1 + \sqrt{G_2H_2}\,\alpha_2 + \dots + \sqrt{G_kH_k}\,\alpha_k + \dots,$$

$$y = \sqrt{G_1H_1}\,\alpha_1 + \sqrt{G_2H_2}\,\alpha_2 + \dots + \sqrt{G_kH_k}\,\alpha_k + \dots$$

where the dots stand for errors produced by independent elements and if, second, the coefficient of correlation (r) is defined as the common part of the mean square error:

$$r = G_1H_1\varepsilon_1^2 + G_2H_2\varepsilon_2^2 + \dots + G_kH_k\varepsilon_k^2.$$

Finally, KAPTEYN considered the case

$$x = f_1 + f_2 + \dots + f_k + m_1 + m_2 + \dots + m_s,$$

$$y = f_1 + f_2 + \dots + f_k + n_1 + n_2 + \dots + n_s$$

with the errors of each element being equal to e. Then

$$\varepsilon_x^2 = \varepsilon_y^2 = (k + s)\,e^2 = 1, r = ke^2 = k/(k + s).$$

Thus, the coefficient of correlation was here equal to the ratio of the number of common elements to the total number of elements, a fact which KAPTEYN regarded as especially interesting.

KAPTEYN'S contribution evidently remained unnoticed. I think it is extremely important and for this reason I shall describe the corresponding ideas of GAUSS and PEARSON. GAUSS had repeatedly stated that observations should be independent [85, § 175; 86, §§ 15 and 18; 87, § 22];[55] he resolutely spoke out on this point in a posthumously published work [145, p. 112]. In two instances [87, § 3; 88] he noted that the dependence between observations was caused by conditional equations.

Consider an elementary example. Let the observed values of angles A, B, and C of a plane triangle be α, β, and γ respectively, so that

$$\alpha + (\alpha) = A, \beta + (\beta) = B, \gamma + (\gamma) = C$$

where (α), (β) and (γ) are the errors of observation. The conditional equation is

$$(\alpha) + (\beta) + (\gamma) = 180^\circ - (A + B + C) \equiv w$$

whence, according to the method of least squares (as well as following common sense), the errors are

$$(\alpha) = (\beta) = (\gamma) = \frac{w}{3}.$$

GAUSS'S remark should be understood in the sense that, in contrast to the observed angles α, β, and γ, the adjusted angles $\alpha + w/3$, $\beta + w/3$, and $\gamma + w/3$ are mutually dependent.

Finally, GAUSS [86, § 18] formulated two more statements:

(1) Two linearized functions of the same [independent] errors *werden offenbar nicht mehr von einander unabhängig sein* ... .

(2) The same functions *würden ... nicht mehr von ein ander unabhängig* if [at least] one of the errors is common to both of them.

KAPTEYN might have mentioned GAUSS as his precursor. Though errors of observation need not follow the normal law, KAPTEYN'S definition of the correlation coefficient (2) would remain valid even in this general case.

PEARSON devoted one of his articles [123] to the history of the correlation theory. PEARSON maintained that GALTON, not BRAVAIS, originated this theory. He also noted (p. 187) that, according to GAUSS, observations are independent [only if they do not contain common errors; see above], while *for us* they are correlated.

LANCASTER [104] seems to have supported this point of view. For my part, I simply do not understand it.

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[55] Astronomers and geodesists undoubtedly adhered to the same opinion long before GAUSS, but it was precisely GAUSS (and LAPLACE [148, p. 11]) who expressly indicated it.

9.2.2. Kapteyn and Pearson. In 1904 KAPTEYN published a booklet [14] on the application of skew distributions in biology[56] and statistics. In the Introduction he asserted that the PEARSONIAN theory of skew curves was imperfect and, moreover, since it was purely empirical, did not connect the shape of the curves with the action of corresponding [random] causes. Further on in the Introduction KAPTEYN explained the action of the central limit theorem. Apparently he was not sufficiently acquainted with probability theory; at least, he did not mention LAPLACE (or POISSON, or CHEBYSHEV) even once and his explanations were only qualitative. KAPTEYN also described his *skew-curve machine* which functioned like GALTON'S well-known device for the demonstration of the normal distribution.

Except for two short remarks, I shall not dwell any more on KAPTEYN'S contribution. The second edition of his booklet does not contain the statements dealt with above; and KAPTEYN was not the only one who failed to recognize PEARSON'S achievements. In a letter to CHUPROV, dated 1910, MARKOV [110, p. 12] maintained that PEARSON accomplished *nothing which deserves attention.* Only later, possibly under the influence of CHUPROV, did he relax his stance somewhat (p. 154).

PEARSON [120] was quick to criticize KAPTEYN [14]. Answering him, KAPTEYN [17, p. 216] admitted unintentionally following EDGEWORTH *without acknowledging his priority.* In substance, however, KAPTEYN did not repent and in his turn (p. 218) accused PEARSON of an inaccurate description of his (KAPTEYN'S) work [14] remarking that his opponent *has largely profited by the exposition of the theory which he refutes.*

I shall leave the matter at that. It is more important to note that precisely the same period (the very beginning of this century) witnessed an unsuccessful attempt to establish correlation theory in astronomy.[57] WINIFRED GIBSON, whose contribution [90] was communicated by PEARSON, applied elements of this theory to the study of the relation between some stellar characters. Later on GIBSON & PEARSON [91, p. 415] referred to this contribution, pointing out that

modern statistical methods were used [by GIBSON] *for the first time to determine the numerical relationships between various star characters. The object of the present paper is to deduce further similar relationships, and to deal with some of the same relationships on the basis of wider data.*

In about two years GIBSON, this time together with PEARSON, returned to the same subject aiming [91, pp. 447–448] *to indicate the directions in which closer relationships* [between star characters] *may be found ... .* GIBSON'S paper [90] became the subject of discussion between PEARSON and the astronomer A. H. HINKS. PEARSON [121, pp. 517–518] maintained that

astronomers have been guilty of a considerable amount of circular reasoning. They start from the hypothesis that magnitude is very closely related to parallax, and

[56] About ten years later he published an article on the growth of trees in response to meteorological factors.

[57] Qualitative studies of star distribution [72] were still being published in 1895, just when PEARSON started his scientific career.

when the statistician shows that the ... parallaxes show no continuous relationship between parallax and magnitude, they turn round and say: "Yes, but our stars were selected because they had big proper motions." They thereby screen entirely the fact that the fundamental hypothesis that the brighter stars are much the nearer as yet awaits statistical demonstration.

And, further:

I would venture to ask whether it may not be that the mass, the chemical constitution, and the life-history of a star, as evidenced in its spectroscopic character, have sensibly more to do with the magnitude than its mere distance?

PEARSON'S conclusion seems reasonable. However, he was not sufficiently acquainted with astronomical literature. Thus he failed to point out that astronomers had started to doubt the connection between stellar distances and stellar magnitudes long before him (see my § 7.4 and note 53).

Continuing the discussion, PEARSON wrote (pp. 613–615):

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 methods 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.

From 1908 to 1910 PEARSON, partly in collaboration with JULIA BELL, published six papers in astronomical periodicals (*Monthly Notices Roy. Astron. Soc.* and *Observatory*) applying correlation theory and elucidating its essence. Two of the articles were written in response to critical comments due to PLUMMER [128; 129]. In his first note the latter stated (p. 349) that

[The astronomer] *will certainly not be attracted by modern methods in statistics if they lead to results from which no useful deductions can be made ... .*

In his second contribution PLUMMER (p. 5) asked a rhetorical question:

Do they [the results attained by statisticians] *give us new and useful information which would otherwise have escaped us? ... Does the method of derivation give them a rigorous certainty which would otherwise be lacking?*

BELL herself studied the connection between the colors of stars and their spectral classes [55] and magnitudes [56]. In the first instance she noted that

correlation is only a single and not very long chapter in the complete theory of association: the chapter on contingency may often be of greater service.

BELL [56] was probably one of the first to use the PEARSONIAN test for goodness of fit.

In 1910 PEARSON [122] derived a test for checking uniformity in the spatial distribution of stars. Suppose the stars of a certain population possess magnitudes up to and including m. Then (p. 61), if the distribution is uniform, the standard deviation of the magnitudes does not depend on m. Also (p. 69), the nonuniformity of distribution increases with m; uniformity may persist, but then either a connection between stellar distances and stellar luminosities takes place, or extinction of light [really] exists.

PEARSON and his associates often used non-GAUSSIAN curves of density (at least in one instance [124, p. 534], a PEARSONIAN curve of type vii). It can only be regretted that their work, at least initially, remained unnoticed. In particular, the remark due to GIBSON & PEARSON (see above) to the effect that statistics can indicate direction for further study [in astronomy proper] proved barren of results. KAPTEYN, for one, did not support PEARSON: as shown above, he criticized the latter's work while his own definition of the correlation coefficient suitable for application in the mathematical treatment of observations was patently unfit for studying connections between stellar characters (see § 9.2.1 above). After 1910 PEARSON, who was then 53 years old, left astronomy for good. Of course, he always had his own extremely wide field of scientific activity.

10. Appendix: Newcomb and the Theory of Probability

In the field of probability theory, even without considering its application to astronomy (see §§ 3.1 and 5.10 above), NEWCOMB published

1. A popular article on the theory.[58]

2. A lengthy methodological paper [21], which included an explanation of the principles and ideas of the theory and its application to the study of testimonies, to the solution of urn problems and life insurance.

3. Two popular notes on life insurance. A specialist in the field levelled criticism at the second note, maintaining that NEWCOMB based his conclusions on insufficient data.

4. A note on the frequency of different digits [28]. Noticing that the first pages of logarithmic tables wear out faster than the last ones, NEWCOMB formulated and solved a problem pertaining to the stochastic branch of number theory. The distribution of digits in numbers is such, he concluded, that all the mantissas of their logarithms are equally probable.[59]

5. A contribution on the ratio of male and female births [35]. NEWCOMB wanted to show how to apply the statistical method so as to *reach conclusions on questions which elude all direct investigation.* In essence, he (p. 21) explained qualitatively the origin of one or another sex by the action of a random process.

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[58] References which I did not supply here may be readily found in ARCHIBALD's bibliography of NEWCOMB's works [53].

[59] This note is too short and some of its passages remain obscure. (I made a similar remark in § 5.6.) Though he did not say so, the author evidently had in mind numbers expressing the results of measurements. Supposing that each such number must provide the same information, NEWCOMB's conclusion seems to be correct.

Finally, NEWCOMB formulated a problem in the theory of urns. A mathematician solved it in 1907.

In regard to the mathematical treatment of observations NEWCOMB is known primarily in connection with his 'generalized' theory of the combination of observations [29; 100, pp. 298–305; 94, p. 74]. NEWCOMB assumed that the errors of observation follow a normal law with an unknown parameter of precision. Introducing a mixture of such laws and basing his conclusions on a quadratic loss function (this, of course, is a later term) he got a certain posterior estimator instead of the usual arithmetical mean. NEWCOMB's theory demanded complicated calculations and found little practical application, but subsequent astronomers have followed up his general ideas. NEWCOMB's contribution could have helped incorporate error theory into mathematical statistics earlier. That this occurred only about half a century ago was, I suppose, due to the slow development of the theory of probability in the second half of the 19^{th} century.

As NEWCOMB himself remarked [29, p. 344], difficulties inherent in *dealing with ... abnormal errors* prompted the appearance of his article. STIGLER [160] briefly discussed NEWCOMB's contributions on least squares. NEWCOMB also offered mechanical devices for calculations by least squares. Finally, I remark that [53, p. 54]

Among his MSS there is considerable material on least squares. This seems to be preparatory to a text ... .

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Abbreviations:
AHES = *Arch. Hist. Exact Sci.*
AN = *Astron. Nachr.*
MNRAS = *Monthly Notices Roy. Astron. Soc.*
Phil. Mag. = *Lond., Edinb., Dublin Philos. Mag.*

W. HERSCHEL

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S. NEWCOMB

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